In obtaining this factor of solubility of $\frac{1}{3}$ we would call attention to the fact that the investigation, if it has erred on either side, has been possibly too severe in the conditions of the experiment. All analyses tend to show that the gastric juice of the dog is stronger than that of the human being; the degree of acidity, on which especially the solubility of the hydrate and phosphate of alumina would depend, being much higher. The total solids in the gastric juice of a dog amount to more than 20 parts per 1,000, and while we would admit that the analysis of Bidder and Schmidt giving but 6 parts per 1,000 in human gastric juice was made on an abnormal secretion, we see no reason for doubting the figure determined by Berzelius, *i.e.*, twelve (12) parts. There is reason, therefore, for supposing that human gastric juice would have given us (could it have been procured) a smaller coefficient of solubility than the figure reported.

In brief, the results obtained for the solubility of alumina residues during the process of mastication and stomachic digestion (and leaving aside the action of intestinal juices), is somewhat less than $\frac{1}{3}$ of the alumina present when canine gastric juice is used.

This represents, we think, a maximum figure. From a comparison of published analyses human gastric juice is probably about one-half as strong. If solubility is approximately proportional to strength, then our figure should be changed to $\frac{1}{5}$ or $\frac{1}{6}$ for the solubility in mouth and stomach of man.

From the impossibility of procuring human gastric juice at will, a more exact approximation is probably not now possible.

"WATER" RESULTS AND THE PUBLIC.

By Prof. William P. Mason.

In the *Analyst* for April, 1883, Dr. Dupré writes an excellent article on "standards" for water analysis, in which he states that the laying down of a general standard is impossible. He also adds:

"This difficulty as to standards is certainly by no means confined to water analysis, but comes up whenever a standard is laid down for a natural product liable to variation."

Now my experience is that one is more frequently annoyed by a demand for "standards" in water analysis—more liable to be called upon to explain, translate and re-explain his results in that line of investigation than in any other.

Water analysts are treated very unfairly. There is an irritability manifested over their results usually unknown in other fields of analytical chemistry; the hasty critics forgetting, or being ignorant of, the fact that "water analysis," so called, is really less of an analysis than a series of experiments made with a view to assist the investigator in formulating his opinion as to the purity of the sample.

The analyst's "*opinion*" is the thing, after all, and yet how many water boards in the country would rest satisfied with that, and that alone.

The question that forces itself on my mind is, whether or not a water analyst pledges himself to educate his patron to the point of understanding analytical methods. Possibly it may be urged that the latter, without attempting to criticize the results himself, might, in his desire for an unprejudiced opinion, hand them to another chemist for his judgment. But even then there might be chance for misinterpretation; for how could the second chemist know the smaller details of the analysis, for instance the rate at which the ammonias have come over in Wanklyn's process, and upon which, as we all know, so much depends. How can he be aware of the dozen or more points, the noting of which goes in with the factors that make up the total "opinion," all of which are important and none of which appear upon the "report"?

Water analysts, again, are often called upon to report upon waters the history of which they do not know, and to base their opinions upon the analyses alone.

Such a demand is manifestly unfair. Recently a water was sent me for analysis which, upon being submitted to distillation for free ammonia, gave the enormous quantity of 1.18 parts per million.

What chemist would regard such a water as pure? And yet pure it really was. I found, upon inquiry, that it had been obtained by melting a cake of pure artificial ice. The melting had been conducted in an atmosphere more or less charged with the ammonia

used in the process, and the ice had taken up ammonia as a sponge would absorb water.

Again, I condemned a water recently which ran moderately high in nitrates, because of the sloping nature of the ground leading from a large cow yard towards the spring whence the sample was drawn. Had I not seen the ground I should have reported the water as wholesome. Subsequent investigation proved the correctness of the opinion.

Except in the instances of an exceedingly good or an exceedingly bad water, no one can safely venture an opinion as to quality unless he be admitted to all the facts in the case, and it is utter folly for the civil engineer to fail to furnish them as so frequently happens. Of course the ground taken is, that if observation be all, the engineer can furnish that himself; but observation is not all, nor nearly all, and yet the chemical side of the question cannot, as yet, get along without it. Where the necessary information is withheld I can see but one way open to the chemist, and that is for him to furnish the bare analysis, with full notes, and let the engineer make his own interpretation.

I have been so annoyed by being obliged to explain and re-explain water results, that I am seriously thinking of eliminating figures from my reports altogether, and writing the opinion only, or else attaching a printed slip, reading somewhat as follows:

"The writer of this report has arrived at the opinion herein expressed, after a most careful analysis of the water in question, and after having thoroughly weighed all evidence bearing in any way upon the result. He regrets that he cannot undertake to explain to anyone but a chemist his reasons for arriving at his conclusions."

RENSSELAER POLYTECHNIC INSTITUTE,
TROY, N. Y., Feb. 16, 1887.