

ON THE ESTIMATION OF SULPHUR IN PYRITES.

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IT has taken Mr. T. S. Gladding six months to reply to my last paper on the above subject. I will not take much more than six days from the date of receiving the May number of the Journal of before dispatching my final reply to that gentleman.

Mr. Gladding avoids any mention, and of course offers no refutation, of the charges I had brought against him, but he again puts me into a totally false light, by saying that I "attempt no further support of my position by chemical experiment." This suppresses the fact that I had referred to my more than sufficient experimental proof for Mr. Gladding's and his assistants' inability to handle my process, which has been in daily successful use by scores, if not hundreds, of chemists for a number of years past, and is that employed in Fresenius' own laboratory, as I hear from his son-in-law and laboratory chief, Dr. Hintz. Mr. Gladding now exacts a further reply from me, more especially on the strength of some new comparative tests of what he states to be the main point at issue, namely the necessity of a very slow addition of the barium chloride.

I am convinced that our readers are as tired of this dispute as I am, but as some of them might construe my silence into the admission that Mr. Gladding is right on this point, and might saddle themselves with a total unnecessary complication in their daily work, I will not shirk a further reply, although I think it unnecessary after having quoted already in March, 1895, eleven experiments by entirely independent chemists, refuting all Mr. Gladding's assertions.

In his former paper Mr. Gladding states that the error caused by the rapid addition of the barium chloride solution is from two-tenths to three-tenths per cent. of sulphur, and according to his last paper it is even one-half per cent. He appeals to independent chemists to settle this discrepancy between his statements and my own. I have taken this up in the following manner: I instructed one of my assistants, Mr. U. Wegeli, a skilled

worker, but entirely ignorant of the above dispute, to make a series of very careful tests of a sample of pyrites, just arrived for analysis and belonging to an important commercial case. I enjoined him to give me absolutely unvarnished results (which in our laboratory it would not have been at all necessary to say), and I told him, as we must be quite sure of the matter, he must not merely employ all the ordinary precautions, but also try both the usual quick addition of the barium chloride and a process recently very much recommended, namely, the very slow addition of the precipitant; I did not express any opinion of my own upon that point, and left it entirely for him to find out what there was in the matter. I had just then to undertake a short journey, and on my return he handed to me the following results.

A. *Quick* addition (*i. e.*, pouring in the hot barium chloride solution in about ten portions, occupying about half a minute in all, and stirring the mixture all the time, as every chemist would do).

1. 39.83 2. 39.65 3. 39.65 per cent. sulphur.

B. *Slow* addition from a burette, one drop per second (exactly as described by Mr. Gladding).

4. 39.63 5. 39.69 6. 39.44 per cent.

This means: In No. 2 and 3 the quick addition has given *identical* results with the slow addition in No. 4 and 5. No. 1 shows a little more, No. 6 a little less. I have suppressed nothing, and I give these results as well, although they are evidently not as reliable as the other four, entirely concordant, results; but even if we admit the less reliable results in striking an average, we find a difference of only one-tenth per cent. between the quick (39.71) and the slow (39.59) process. Such a difference is evidently within the limits of ordinary experimental error.

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[This discussion closes with the present paper.—ED.]