

days ago, however, I have sent a small article on the matter to Scheel [editor of *Verhandl. Deut. Physik. Ges.*] . . . For the greater part, I already had these results in my possession in May, but it was only your valuable work on the heats of solution which gave me cause to publish. I make this point very clear in my communication. In the meantime Haber has also found several of these results, partially by completely tangled ["wirren"] routes; he will believe subjectively in his authorship. You see that I am in a dilemma, I do not wish to hurt or put anyone at a disadvantage, but on the other hand, these consequences are so obvious that I would have been very foolish had I not immediately arrived at them. Only Haber's and Nernst's objections against my theory moved me not to publish any consequences until the foundation of the theory had been checked from another side. What is now to be done for the best in order that everyone gets his just rights? I think we should wait until the proofs of my article arrive. As soon as we have them (I have arranged for them to be sent to you and Haber), we will discuss whether my article should be extended by remarks from you and Haber (e.g. more exact values of the electron affinities) or be transformed into one joint paper from the three of us. It would be best if we could all come together and have a verbal discussion . . . I shall write to Haber to find out whether a meeting of the three of us can be arranged perhaps here [Frankfurt a. M.] or in München . . ."

Haber visited Fajans in München (Munich) late in October (Born was unable to attend) and reported the conversation in a letter written to his associate J. Franck on November 3, 1919. This letter ends with the remark that he could not see real grounds for a joint publication of the three authors and that Fajans had also decided against it.

Thus the three separate papers on the thermochemical correlation were published simultaneously. It is clear that Haber, like Born, was precipitated into publishing

his contribution by Fajans's appreciation of the physico-chemical applications of the lattice theory. Haber states in his paper . . . "Since the subject, on account of the participation of Fajans in extending the Born theory, has turned into a torrent, the communication of these considerations cannot be postponed . . ."

We thank Professor Kasimir Fajans for providing us with copies of his correspondence and for helpful remarks. We also thank Sir Lawrence Bragg for reading the first draft of the manuscript and Professor G. V. R. Born for permission to quote from the letters of his father, who is in very poor health.

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Chemical Warfare

SIR EDWARD THORPE, in his review of Victor Lefebure's book, "The Riddle of the Rhine", in *Nature* of November 10, p. 331, quotes a passage which deals with my own work during the initial stages of the war, and that of the Kaiser Wilhelm-Institut für Physikalische Chemie, of which I am the principal. The intention is to make the world believe that the materials for gas warfare were prepared by the German military authorities and chemical industry for the intended war, and that experiments with this end were carried out in my institution, if not previous to the war, at least from August 1914 onwards.

It is always dangerous to attempt to form a correct estimate of the intentions of others from the traces of events they have left behind them. But the greatest errors must necessarily arise if an outsider tries to deduce from his own impressions the intentions of men whose ways of thinking he does not know and cannot understand.

Perhaps there might have been some ground for suspicion if Germany could have foreseen the trench warfare, and if we could have imagined that the German troops could ever be held up for weeks and months before the enemy's wire entanglements. But previous to the war, and up to the Battle of the Marne, everyone in Germany imagined that the course of the war would be a succession of rapid marches and great pitched battles, and what use would gas have been to a field army in such a war of movements? I think I may safely say that during the course of the war I became acquainted with every man of any importance in the army, in industry, and in science, who had anything to do with chemistry as applied to military offensive and defensive operations, and that I am well informed regarding the development and the course of chemical warfare. Yet among all these men I have never met one who, previous to the war or during the first two months of its course, had conceived

the idea of providing the field army with gas, or had made experiments or preparations for such a purpose. We had actually first to read in the French, Italian, and English Press—as, for instance, in the *Pall Mall Gazette* of September 17, 1914—of the terrible things that were in preparation for us before we began to make similar preparations in view of the commencement of the war of position.

As regards my own institution and its work during the first months of the war, that intelligent person who, according to the passage in Lefebure quoted by Sir Edward Thorpe, observed my activities in my institute from behind a wall, lacked the gift of interpreting correctly what he saw and heard. Visitors in grey Headquarters motors did indeed come to my institution in August 1914, though not to see me upon the subject of chemical means of warfare, but because Headquarters were very anxious to know how motor spirit could be made proof against the cold of a Russian winter without the addition of toluol. The question of gas as means of warfare did not begin to engage our attention until the first three months of war had passed.

In war men think otherwise than they do in peace, and many a German during the stress of war may have adopted the English maxim, "My country, right or wrong", but that German science and industry before the war made preparations with deliberate intent for gas warfare against other nations is an assertion that, in the interest of the necessary interdependence of the nations in the realms of science and industry, must not be allowed to go uncontradicted in so serious and respected a journal as *Nature*.

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December 17.