arguments, also of a technical nature, which, on the contrary, confirm the hypothesis of cementation by simple contact.

As to the diminution of the carburizing efficiency of carbon when it has been used as a cement (a diminution which Caron attributes to the volatilization of the alkalis contained in the ashes), he explains it, instead, by the known "change of state" which carbon undergoes when subjected to prolonged ignition, a change which renders it less reactive—either with iron "by contact" or with carbon dioxide to "regenerate" the carbon monoxide; this would confirm precisely the opinion that the direct action of carbon and that of carbon monoxide are preponderant in the process of cementation. Margueritte points out then that Caron himself, in his last note, is beginning to admit (which he would not do at first) that pure carbon and carbon monoxide can produce, under certain circumstances, "a slight cementation."

After the publication of a note[1] in which, from the study of the cementing action of retort carbon, he maintains that he can deduce new confirmations of his theory of cyanides, Caron resumes the discussion with Margueritte. In the meantime Jullien, director of a steel works, had entered the discussion, citing[2] some facts observed by him in industrial practice and which, while they confirmed the assertions of Margueritte concerning the carburizing efficiency of carbon alone, demonstrated that pure carbon monoxide can not cement iron.

In his next reply Caron,[3] first of all, calls attention to the fact that his experiments are in full accord with the absolutely negative attempts made in 1859 with minute care by Percy to cement iron with pure carbon monoxide. Then he refutes the hypothesis of Margueritte, that the diminution in carburizing efficiency suffered by carbon with use is due to the increase of its density and the diminution of its combustibility, by observing that oak charcoal, used in industry as the most efficient cement, is precisely the most dense and least combustible. Such maximum efficiency of oak charcoal, while evidently incompatible with the hypothesis of Margueritte, is a new confirmation of Caron's theory, since numerous analyses have shown that oak charcoal is the richest (especially for equal volumes) in alkaline ashes.

Furthermore, Caron cites as evident proof of his theory the fact that carbon, freed from alkalis by washing with an acid, has hardly any carburizing efficiency, and he repeats the observation that carbon which has lost its efficiency can again become an active cement by means of the addition of an alkali.

This last fact manifests itself not only when the alkali added is in the form of carbonate (in which case its efficiency might be attributed to the formation of carbon monoxide), but also when the alkali (for example, dry gaseous ammonia) is such as to exclude absolutely the possibility of the formation of carbon monoxide.

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[1] *Comptes Rendus*, LIX, p. 819.

[2] *Comptes Rendus*, LIX, p. 915.

[3] *Comptes Rendus*, LIX, p. 953.

The note ends with a reference to some experiments of Percy which show that pure sugar carbon, freed from occluded gases by a strong preliminary ignition, does not carburize iron except at a temperature sufficiently high to *melt* the product of the carburization (steel or cast iron), but in such a case the process of carburization is no longer a cementation. As to the experiments made by Margueritte with diamonds, they do not allow of drawing any conclusion concerning the true processes of cementation, for there is no authority for establishing *a priori* that the diamond should behave toward iron in the same way as ordinary carbon.

Finally, this first polemic concerning the function of pure carbon and carbon monoxide in the process of cementation is concluded by a last note of Margueritte.[1] In this, Margueritte begins by maintaining that the negative results of Percy's experiments, cited by Caron, are due to the fact that the English metallurgist had worked with too slow a current of carbon monoxide (three-fourths of a liter in three hours), so that the presence of carbon dioxide prevented the cementation.

As to the other experiments of Percy, likewise cited by Caron, Margueritte declares that the fact that two sheets of iron heated in the same tube in a current of hydrogen, one in contact with sugar carbon and the other free, are both cemented proves, to be sure, that under these experimental conditions hydrocarbons are formed which also cement the sheet which is not in contact with the carbon; but Caron has forgotten to say that in all of Percy's experiments the iron placed in contact with the carbon is always much more intensely cemented than that exposed only to the action of the gases, which constitutes a clear proof that carbon also exercises a marked direct carburizing action *by contact*. And the farther fact observed by Percy, that the same sugar carbon, heated to the melting point of the iron, no longer cements iron by contact, confirms Margueritte's hypothesis as to the cause of the diminution in carburizing efficiency of carbon as the result of intense ignition, for in this case the action of the ignition could not consist in the volatilization of cyanides, from which the materials used were totally free.[2]

There is no doubt that the interesting polemic which I have briefly summarized, considered only on the basis of the scientific data known at the time when it developed, leaves the suspicion that Margueritte, notwithstanding the eminently decisive character of his cementation experiments *by contact*, by means of the diamond and those carried out with pure carbon monoxide, had not been able to refute clearly and completely a part of the

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[1] *Comptes Rendus*, LIX, p. 1043.

[2] Margueritte summarizes and coördinates the results of his experiments, and the conclusions which he draws from them, in a Memoir published in Vol. VI, Series IV, of the *Annales de Chimie et de Physique* (1865, pp. 55–85).

arguments cited by Caron and supported by his theory of *cyanides*. But it is a far cry between such a suspicion and the clear and precise assertion with which Captain Nicolardot, in the memoir to which I have referred, concludes his summary of Caron's investigations on cementation: "Ainsi, se trouvaient réfutés tous les arguments de Margueritte qui, l'un des premiers, avait affirmé que le carbone pur (le diamant) et aussi l'oxyde de carbone pouvaient transformer le fer en acier." ("Thus are refuted all the arguments of Margueritte, who was one of the first to affirm that pure carbon (diamond) and also carbon monoxide could transform iron into steel.")

That the assertion of Nicolardot is too absolute and not wholly justified becomes still clearer when we study the memoirs of Caron and of Margueritte on the basis of the experimental and theoretical data recently acquired by metallurgical science. We shall see, in fact, that it would be easy to-day to find, together with some correct observations contained in the works of Caron, in the works of Margueritte a large number of experimental data constituting the basis of a valid refutation of Caron's theories. Of this we shall speak later. For the present it suffices to recall that a series of analyses of gases contained in the cementation boxes, published in 1865 by Cailletet,[1] did not reveal the presence of either cyanogen or cyanides.

In the meantime, and in the years following the publication of the memoirs summarized above, a series of accurate investigations due, above all, to H. Sainte-Claire Deville, Troost and Cailletet, showing how iron, heated at a high temperature, is easily permeable to various gases, made it more than ever probable that the penetration of carbon into the mass of the iron is due to the diffusion of the carburizing gases into this mass.

The problem of the course and nature of the process of cementation was again taken up a few years later by R. Mannesmann, who brings together the results of his numerous and accurate investigations in a memoir published in 1879 under the title: "*Studien über den Zementstahlprocess.*"[2]

While we must consider somewhat exaggerated the assertion of Wedding,[3] that the investigations of Mannesmann cleared up all the doubtful points in the theory of cementation, yet these researches have certainly contributed, by furnishing more copious and somewhat more accurate experimental material, toward the subsequent development of present theories.

It is well, therefore, to give here a brief summary of Mannesmann's work, to which I shall have occasion to come back later.

Mannesmann first premises that an exact and complete study of the process of cementation can certainly furnish most useful data for the theory of the constitution of steels, because, while it has been shown that the best steels are those composed essentially of iron and carbon, it is also now certain[1] that the process of cementation consists of the simple diffusion of the carbon into the iron. Mannesmann then subjects to a brief critical examination the experimental data already established at the time and the various theories founded on them. From this examination he concludes that it is only owing to the fact that Margueritte's experiments on cementation *by simple contact* with the solid carbon (which had furnished the complete proof of the theory of cementation based on the hypothesis of the molecular migration of the carbon) were not extensive and deep enough, and received no confirmation for many years, that the tendency has become general to accept the other theory, better studied from both the theoretical and the experimental point of view, of the necessity of the intervention of gases in the process of the carburization of iron.

With this premise, Mannesmann proposes to determine:

1. Within what limits of temperature and of carburization cementation can take place.
2. With what means cementation can be obtained experimentally.
3. Through what process it really takes place, in practice, in the cementation furnaces.
4. What cementation agents are best adapted for any given temperature and how steels of different hardness behave in cementation.

In order to avoid a repetition of the contradictions so frequently manifested in the earlier researches between the results of experiments carried out in laboratory apparatus and of those executed in apparatus employed industrially, Mannesmann's experiments were all carried out in the works and under conditions as near as possible to those under which the industrial process is actually executed. I shall have occasion, later, to make some observations on this method adopted by Mannesmann and later followed by other experimenters.

To answer the first of these propositions, Mannesmann performs a series of experiments consisting in heating in a crucible *at a very high temperature* (but below the melting point of cast iron) iron bars surrounded by powdered wood charcoal. By varying the conditions of the experiments he obtains, in the iron bars, external carburized strata having the characteristics of a large number of varieties of white and gray cast irons, containing from 2% up to 4.76% of carbon, and he concludes that the maximum concentration of carbon which can be made to penetrate into the iron by means of cementation proper (carburization of the iron without fusion) is the same as that which can be obtained by melting the two substances together.

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[1] *Comptes Rendus*, LX, p. 344.

[2] *Verhandlungen des Vereins zur Beförderung des Gewerbfleisses*, Vol. LVIII, p. 31.

[3] *Handbuch der Eisenhüttenkunde*, 2nd Ed., Vol. I, p. 1105.

[1] Noteworthy contributions to the exact knowledge of the composition of steels in general, and especially of steels obtained by cementation, were made by the accurate investigations of Boussingault, published a few years before the Memoir of Mannesmann (see *Comptes Rendus de l'Académie des Sciences*, LXXVIII (1874), pp. 1458–1464; and *Annales de Chimie et de Physique*, Series V, Vol. V (1875), pp. 145–265).

The examination of the products obtained in these first experiments established also two interesting facts: the first is that the successive layers of cast iron and of steel are distinctly separate from each other in the sense that the concentration of the carbon, while remaining approximately constant for any given layer, changes suddenly in passing from the layer of cast iron to that of steel and, sometimes, directly from cast iron to iron. The second fact (which Mannesmann maintains is proved by his experiments) is that as the temperature rises the process of cementation becomes more and more rapid and the concentration of the carbon in the cemented zones increases, so that to any given temperature corresponds a definite maximum (or *saturation*) concentration of carbon. These facts are represented graphically by Mannesmann in two diagrams, reproduced as Figs. 7 and 8.[1]

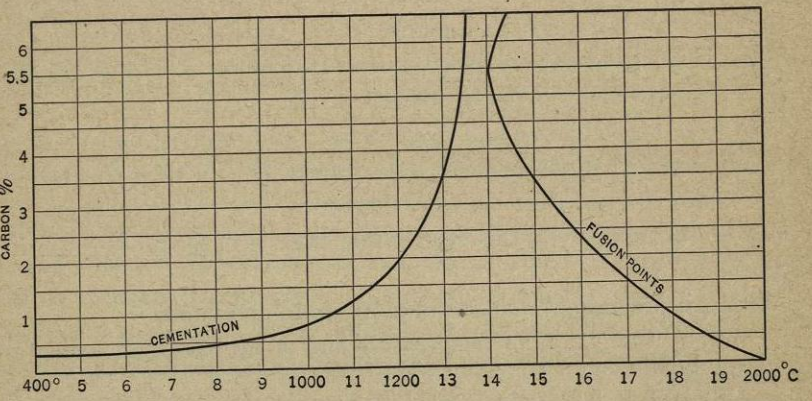

FIG. 7.—Diagram of the formation points and of the fusion points of cemented iron.

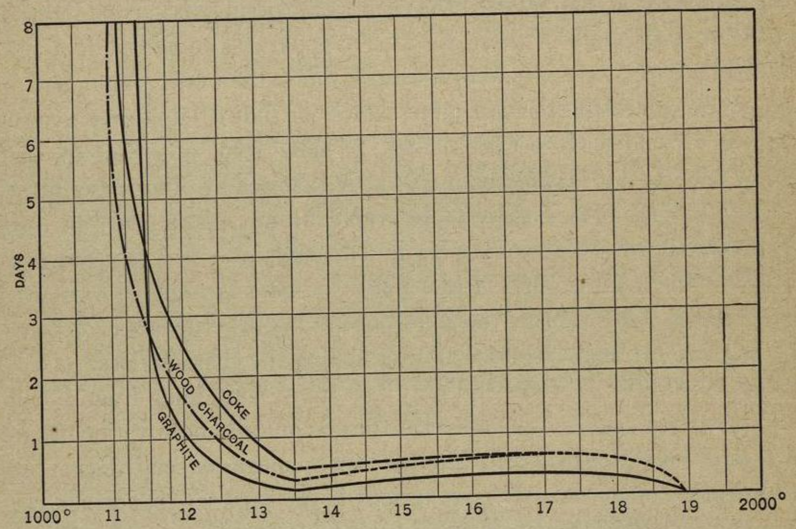

FIG. 8.—Duration of the cementation of 12 mm. iron bars.

As to the lower limit of temperatures at which cementation can take place, Mannesmann cites the observations of Caron, according to whom cyanogen can commence to carburize iron at temperatures much lower than red heat.

Passing to the experimental study of his second question, Mannesmann carries out many experiments, the results of which I summarize briefly:

(*a*) Experiments carried out with petroleum and asphalt vapors enabled him to obtain intense cementations, showing thus the carburizing efficiency of hydrocarbons even in the total absence of nitrogen;

(*b*) Numerous tests showed that potassium ferrocyanide can effect good superficial cementation, but that it is ill-adapted to the production of deep cementation;

(*c*) Numerous experiments performed by strongly heating in glazed crucibles, at various temperatures and for varying periods of time, bars of iron half immersed in pure graphite powder, with the other half surrounded by inert refractory material reduced to grains the size of lentils, showed always that only the iron placed in contact with the graphite is strongly cemented, while the portion surrounded with inert material remains unchanged. The same fact also always manifests itself when the part of the sample of iron not in contact with the graphite is allowed to project into an empty space in the crucible instead of being surrounded with inert material which might have exerted a harmful influence in the "regeneration" of carbon dioxide formed from the carbon monoxide when the latter yields carbon to the iron. In this case also, the only part of the bars of iron which was cemented was that which was placed in contact with the graphite.

The same experiments, performed by substituting for the graphite sugar carbon or lamp-black, previously strongly ignited, gave identical results.

From these first experiments Mannesmann maintains that he can already conclude that the gases which can be formed in the cementation boxes containing carbon interpenetrated with air are so dilute as to render negligible their carburizing action, even if some of them (for example, carbon monoxide) can be proved experimentally to have a strong carburizing action when used sufficiently concentrated. While such conclusions proved (according to Mannesmann) the preponderance of the direct action of carbon *by contact* in industrial cementation, and therefore the necessity of admitting the *molecular migration* of the carbon into the mass of the solid steel, nevertheless Mannesmann tried to add to these more direct proofs of the correctness of his views, by means of the following later experiments:

[1] The temperatures indicated in the work of Mannesmann can not be considered as exact. To become convinced of this, it is only necessary to examine the fusing temperatures of iron-carbon alloys represented by the points in the second curve of Mannesmann's third diagram (see our Fig. 7).

(*d*) On shaking in a glazed and perfectly closed crucible, heated at a very high temperature (but below that at which cast iron melts) pieces of iron together with turnings of gray cast iron, Mannesmann *always* observed a marked cementation of the iron. Now he considers it improbable that the same gas contained in the crucible should on the one hand remove the carbon from the cast iron, to yield it, on the other hand, to the steel, at the same temperature, so that these experiments prove that the carburizing action of the cast iron is due simply to contact. We shall see later the weak point of the reasoning upon which is based this conclusion of Mannesmann, and we shall see also how the deductions drawn by him from another series of experiments, intended to confirm the preceding ones, carried out in a way similar to those of Series *c* by substituting in these (as in those just cited) cast-iron turnings for graphite, are not exact. In these experiments, too, the bars of iron were cemented only in the portion placed in direct contact with the cast-iron turnings, while they remained unchanged in the part surrounded with the inert granular refractory material.

(*e*) Various samples of iron immersed in *spiegeleisen* heated only high enough to make it pasty, without melting it, were always distinctly cemented to depths of 1 or 2 mm. Since the sharp angles of the samples of iron showed that they had not suffered even a trace of fusion and since the "pastiness" of the *spiegeleisen*, which Mannesmann found could not penetrate into spaces 1/2 mm. wide between a series of sheets of iron immersed in it, excluded the possibility of its penetration into the "extremely fine pores of the compact iron," these new experiments constitute a further confirmation of the possibility of cementation *by contact*, and therefore of the "molecular migration" of the carbon into the solid steel.

(*f*) In order to exclude entirely the action of gases, the experiments of Series *d* were repeated by filling the crucible with finely powdered salt which, melting at the temperature of the cementation, completely filled the interstices between the fragments of cast-iron turnings and the particles of refractory material. In these experiments also, and in other analogous ones in which the salt was replaced by molten lead, cementation was effected only at those points of the samples of iron where they were in direct contact with the cast iron. It is well to note, however, that in these experiments it took from three to four days to obtain cementations analogous to those obtained in the preceding experiments in one or two hours of heating at the same temperature (yellow heat).

From all these experiments, Mannesmann considers his conception conclusively confirmed, that in the practice of cementation the carburizing action of solid carbon by simple *contact*, and hence also the process of diffusion of the carbon by *molecular migration* into the mass of the solid steel, must play a large part.

As to the latter process of molecular migration of the carbon into the solid steel, this does not appear, according to Mannesmann, in any way "theoretically inexplainable," which he proposes to demonstrate by a series of interesting considerations based on the absence of a limit of precise demarcation between the solid state and the liquid state.

Mannesmann observes that iron at the temperature of cementation must be considered as an extraordinarily viscous true *liquid* in which the mobility of the molecules is shown by the evidence of the tendency which they manifest to assume the disposition corresponding to coarsely crystalline structure, characteristic of iron kept for a long time at a high temperature.

We shall see later how the considerations developed by Mannesmann are partly confirmed by recent studies, also based on phenomena which manifest themselves in many other metallic alloys. Mannesmann further notes that to an analogous process of molecular migration must be attributed the variations in concentration which (especially as a result of the studies of Boussingault) it has been shown are undergone by various of the other foreign substances contained in steel, such as sulphur, phosphorus, silicon, arsenic, etc. This is especially evident in the case of silicon, where the intervention of gaseous compounds cannot be assumed.

Having exhausted thus the examination of the first two questions, Mannesmann begins that of the third, proposing to determine in what way the process of cementation is actually effected in practice.

After having confirmed his conception of the preponderance of the direct action of carbon by contact, he proposes, by means of a new series of experiments, analogous to those which I have just cited in Series *c*, but carried out under conditions nearer to those which prevail in practice, to study the individual phases of the process of cementation. To this end he placed in a cementation box eight bars of iron, removing them one after the other within the space of thirteen days and a half. The examination of the specimens thus treated allowed him to establish how the concentration of the carbon in the cemented *crust* varies with the progress of the cementation, and how the *velocity of penetration* of the carbon varies in the successive phases of the cementation. The data relative to the concentrations of the carbon are summarized by Mannesmann himself in the accompanying diagram (Fig. 9); he attributes the sudden variation in concentration manifested on the eleventh day to a rise in the temperature of the furnace. The diagram in Fig. 10 summarizes the data relative to the variation in the velocity of penetration of the carbon, and the fact that this velocity gradually increases is, according to Mannesmann, a new proof that the cementation is not due solely to the action of the gases, for he considers it self-evident that in this case the cementation should proceed with increasing slowness as the deeper layers of the mass of steel are reached. Mannesmann treats then the question of the *exhaustion* which carbon employed as a cement suffers with use, an exhaustion which (as we have already seen) Caron attributed to the volatili-

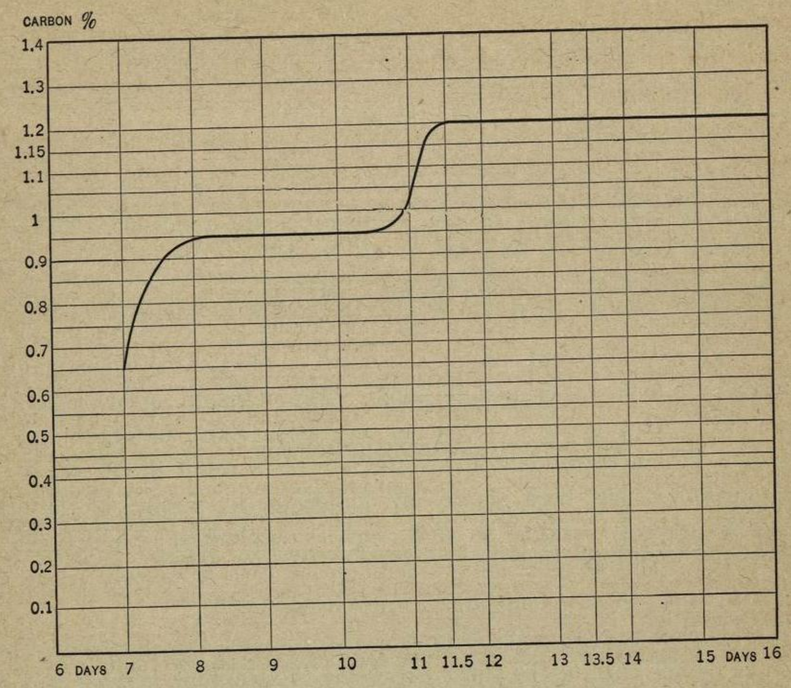

FIG. 9.—Carburization in the various phases of the cementation.

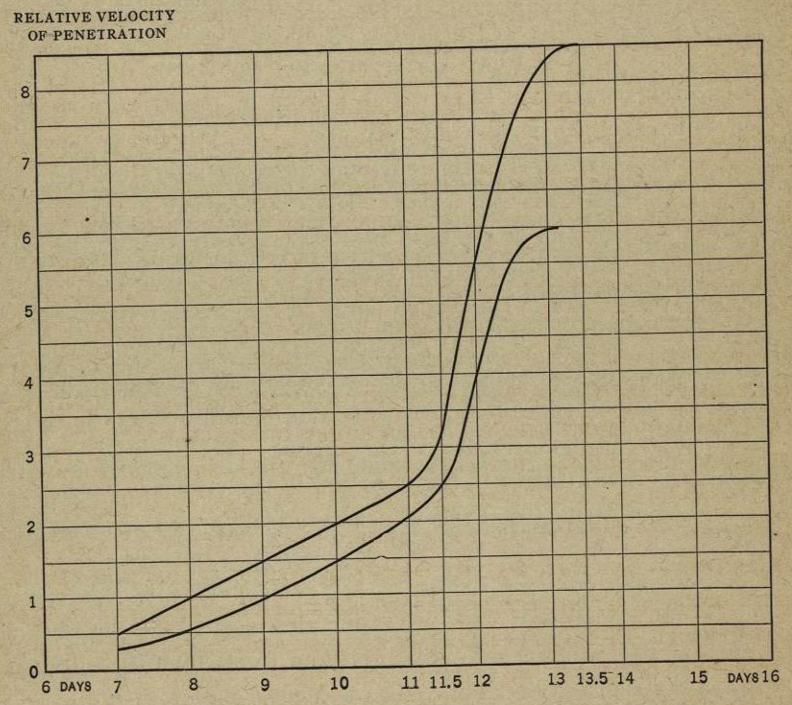

FIG. 10.—Progress of the uniformly carburized cemented layer.

zation of the alkalis necessary to form cyanides, which, according to him, are essential for the production of cementation, while Margueritte explained it by a more compact "atomic structure" which the carbon assumes as the result of prolonged heating. From a series of experiments, quite debatable, however, based on the varying effects which are obtained in the treatment of wood charcoal with nitric acid, according as *new* carbon or carbon which has already been strongly heated for a long time is used, Mannesmann believes that he confirms the hypothesis of Margueritte.

Finally, passing to the treatment of the fourth question, Mannesmann begins by observing that possible improvements in the industrial process of cementation must tend essentially toward a diminution of the cost of the process and the securing of the maximum homogeneity of the cemented zones.

This granted, he takes as a starting point a conception which, suitably transformed and limited, has, we shall see further on, recently been confirmed.

According to Mannesmann, when the process of cementation of steel is not effected by the use of free carbon but with a compound of carbon, the *degree of carburization* is proportional to the difference between the *affinity* of the carbon for iron and the affinity of this carbon for the substances with which it is combined, so that, at a given temperature, no compound of carbon can yield as "high" carburization as that which can be obtained with free carbon, by means of a sufficiently long contact with the iron, at the same temperature. And, in confirmation of his assertions, Mannesmann cites two examples, to which we shall have to refer later: that of cyanides, already studied by Caron, and that of carbon monoxide.

To these considerations he adds others, in great detail, on the practical consequences of the fact, considered by him as evident, that in cementation due to the direct carburizing action of gases the *regeneration* of the active gases within the pores of the metal can be effected only with great slowness when the carburization has extended to a considerable depth, on account of the great friction opposed to the circulation of the gases in the fine pores of the steel. Gaseous cementation proper, therefore, can not reach to a great depth, unless a very high temperature is used,[1] while the raising of the temperature, by greatly increasing the mobility of the molecules, would have the effect of strongly accelerating the cementations produced principally by the direct action of contact of the solid carbon. On the basis of these and many other considerations of the same kind, Mannesmann maintains he can conclude that liquid cements (for example, fused potassium ferrocyanide) and those which owe their activity essentially to the carburizing action of gases are specially adapted to obtaining thin superficial cemented layers, or to cases in which it is desired to work at relatively low temperatures, at which the "molecular migration" of carbon takes place with great slowness. The

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[1] We shall see how more recent experiments show that this hypothesis is totally erroneous.

use of cements with a solid carbon base, on the other hand, is best and most economical in the cases where deep cementations and high carbon contents are to be obtained.

As to the varying carburizing action of different kinds of carbon, Mannesmann cites only the few data (which he himself considers uncertain) represented in the diagram reproduced in Fig. 8; these refer to the lengths of time necessary to completely cement bars of iron 12 mm. thick, using as cement coke, wood charcoal or graphite.

The memoir of Mannesmann also contains the results of some investigations on the formation of "blisters" in steel subjected to cementation; on the basis of these results, the author considers it proved that the blisters are due solely to the action of the carbon which "migrates" into the steel, upon the particles of slag contained in the metal.

Then follow some interesting applications of the conceptions developed in the preceding pages to the study of the best technical conditions under which it is advisable to carry out the process of cementation in various practical cases. To these considerations we shall have occasion to return later.

Finally, treating the last part of the questions proposed, Mannesmann studies the behavior of steels of different hardness subjected to cementation. Starting from the conception, already enunciated, that to any given temperature corresponds a definite concentration of carbon at which the steel is saturated with this element, and that the affinity of the steel for carbon (and hence the intensity of the process of carburization *by contact* which results therefrom) is smaller the nearer the steel is to the point of "saturation," Mannesmann maintains that the velocity of cementation is smaller the greater the initial concentration of the carbon in the steel subjected to cementation, and since, on the other hand, it is clear that with equal velocity of cementation the *work of cementation* necessary to reach *saturation* will be less the higher the content of carbon started with, it is therefore necessary to conclude that steels of different hardness must behave similarly in the cementation which is effected by the molecular migration of the carbon. However, the course of the process is modified by the phenomenon of *softening* which the steel undergoes with rise in temperature, to a degree which is the greater the higher its content in carbon, and since this phenomenon greatly facilitates the migration of the carbon, it is clear that the cementation will be effected more rapidly the harder the steel started with. These considerations explain the well-known fact that steel is cemented more rapidly than iron, even in cases in which the cementation takes place only by molecular migration, while the explanation which is usually given of this fact—based only on the consideration that steel does not have to absorb as much carbon as does the iron—is not valid for these cases (as results from the reasoning just developed), but only for *gaseous cementation proper*.

The memoir of Mannesmann concludes with some observations on the reciprocal action at high temperature of layers carburized to different degrees, observations which make evident the tendency which, at high temperatures, the carbon of contiguous zones, carburized to different degrees, has to migrate from the more highly carburized zone to that which is less highly carburized. It is not necessary to repeat here the minute considerations which Mannesmann develops in regard to this phenomenon.

Finally, Mannesmann summarizes the consequences which we have seen result from his way of interpreting the many experiments performed, placing foremost the conclusion that in the process of cementation the penetration of the carbon into the deep layers of the iron is due essentially to the *molecular migration* of the carbon, while the carburization due to the diffusion of the carburizing gases into the iron plays but a small part in the process of cementation and is limited solely to a superficial zone of small depth.

It is well to note that the same conclusion had been reached four years before by Boussingault. The latter, in the course of those investigations already referred to,[1] in which he had observed the variations in concentration of the other elements (especially sulphur) in the iron subjected to cementation, had called attention to the fact that although Cailletet[2] had found in the gases contained in the cementation chambers from 14% to 15% of carbon monoxide (a gas of which Boussingault says *its carburizing property is well known*), yet, "considering the quantity of carbon necessary to transform into steel the 13,000 or 14,000 kg. of iron contained in the cementation boxes," it must necessarily be assumed that the carbon absorbed by the iron during the cementation comes for the most part from the fixed carbon of the wood charcoal. We shall see later how this argument of Boussingault is not supported by a more accurate examination of the phenomena which take place in the cementation boxes.

The researches of Mannesmann may be considered as the conclusion of the first period of investigations on the cementation of steel carried out with scientific data and methods. To this period, which comprises essentially the years 1860 to 1880, belong several other memoirs which I have not cited. It does not seem opportune to me, however, to give more than a brief summary of their contents here, for, while this would oblige me to dilate beyond measure on questions which have to-day lost all interest whatsoever, I would be led, in reality, to expose simply a series of theoretical speculations (now shown to be without foundation) upon *the same experimental facts* brought out in the memoirs which I have already summarized. Now, the importance of setting forth briefly the facts observed by the first experimenters—those facts for which any modern theory of cementation *must* absolutely furnish as clear an explanation as for the experimental facts more recently observed, is evident, and this justifies the exposition contained in the preceding pages

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[1] *Annales de Chimie et de Physique*, Series V, Vol. V (1875), pp. 145–265.

[1] *Comptes Rendus*, LX (1865), Vol. I, p. 344.

and which to some may appear too minute. But the uselessness of summarizing memoirs which, containing no facts essentially new, simply set forth considerations to which can be attributed only a historical interest, is also evident.

To the same period of time belong various works of an essentially technical nature and several patents. Of these, though of little value, I shall give an account later, when speaking of the industrial applications of the process of cementation.

CHAPTER II

STUDIES ON THE PROCESS OF CEMENTATION DURING THE LAST TWENTY YEARS OF THE NINETEENTH CENTURY

The first period of scientific investigations on the process of cementation, which I have briefly summarized in the preceding pages, a period notable more for the activity of the researches and the vivacity of the polemics than for clear and certain results, is followed by a second, also of twenty years' duration, during which but few new experimental data of real importance were brought out in support of one or the other of the various opposing hypotheses which in the preceding twenty years had been considered by their adherents as conclusively proved.

Leaving aside what concerns the advances in the technique of cementation, with which we shall deal later, I shall summarize briefly in this chapter the results of the principal scientific studies on the process of cementation published during the last twenty years of the nineteenth century.

A first work, due to Alb. Colson, is published in the *Comptes Rendus de l'Académie des Sciences.*[1] It contains no data essentially new as far as the process of cementation proper is concerned, that is, the diffusion of carbon into iron. The author tries, instead, to study the reciprocal action of iron and of carbon from a wider point of view, and believes that he can establish with certainty that, in the process of cementation, the diffusion of the carbon into the iron is accompanied by the reciprocal phenomenon of the diffusion of the iron into the carbon. According to the investigations of Colson, at relatively low temperatures (the phenomenon manifests itself even at 250° C.) the process of diffusion of the iron into the carbon is predominant, while at high temperatures the reciprocal process of diffusion of the carbon into the iron, that is, cementation proper, prevails. Since the author occupies himself especially with the first process, which takes place to a marked degree only at temperatures at which the process of cementation certainly does not occur to an appreciable extent, his work (in which he uses not very convincing reasoning by analogy) is of but slight interest for the study of the process of cementation.

In an interesting work published in the same year, by Forquignon,[2] under the title "Recherches sur la fonte malléable et sur le recuit des aciers,"

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[1] 1881, 2nd sem., Vol. XCIII, pp. 1074–1076.

[2] *Annales de Chimie et de Physique,* Series V, Vol. XXIII (1881), pp. 433-554.