especially where effect on human beings is concerned, experimenta-
tion apparently becomes extremely slow. We can speed up a given experiment a little but not much. Improve-
ment in this regard must lie in carrying on multiple experi-
ments by an army of trained observers. Trained observers are surely learning to reduce their observations to writing so that others may compile the results with the work of their fellows. To my mind this is the portal to the rapid progress which will be made in the future.

It will astonish some to be told that insufficiency of reward is delaying progress. Our patent system is designed to reward progress and doubtless has been of great aid, but does the inventor and pioneer really get a reward adequate to stimulate research and the application of research to a degree commensurate with the enormous benefits to human-
ity?

In a case the details of which are known to me personally, a company employed men for several years and finally brought out a new product which saved the public many millions of dollars. Out of each $100 saved by the invention $97 were passed to the public, $2.70 were passed to the pioneer company, and only 3 cents to the men doing the work in recogni-
tion of their success. This case is probably fairly repre-
sentative, though the public believes that both the company and the men instrumental in bringing out the product have been most generously rewarded.

Why didn't the company charge a greater profit on the novelty? Purely because imitations immediately appeared and consumers would give no material preference to the original article. If the public would willingly pay a reasonable price for an article that saves it money, we would doubt-
less find a more rapid increase in the development of in-
ventions.

Progress is dependent on painstaking experiments and ob-
servations which require time and money. We are living in a period when wealth has enormously increased and which is, therefore, well adapted for the making of progress. Indi-
cations are that the application of the scientific practices already known will materially increase this surplus wealth which is available for research and that this in turn can fur-
nish the means to make faster progress in the future. How-
ever, prosperity can be easily upset. If those opposed to the capitalistic system were to come into power in our Govern-
ment, the creation of wealth would decrease so rapidly that progress in research would be throttled.

It may not seem germane to the subject to point out that, notwithstanding the phenomenal gain in knowledge in chem-
istry and other natural sciences, there has been no correspond-
ging gain in the science of government. In fact, an unpreju-
diced spectator might say that even here in the United States we are retrograding. We have given up our Republic and taken on a Democracy. We determine by popular vote how ends should be accomplished rather than what ends should be accomplished. Our Representatives and Senators in Congress are instructed by their constituents how to vote on questions. What hope would there be of advancement in chemical research or the applications of research to produc-
tion if the method of application was voted on by the popul-
ace? Is law-making, then, so simple that the average mind is more expert than specialists? This question is propounded here because to my mind the greatest danger to further and phenomenal progress in chemistry is degeneracy in govern-
ment.

Let the public, then, do their part by selecting the best men available to represent them in our Government; let those engaged in chemical industry invest more freely in research, and especially reward more liberally the successful scientist. Let the chemists themselves be even more frank in disclosing the results of their work to their fellows, thereby obtaining the good results of team work. The dyestuffs industry will then prove to be the forerunner of the chemistry of human well-being.

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**How I Have Been Led to the Direct Hydrogenation Method by Metallic Catalysts**

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HAVING arrived at the end of my career as a chemist, I have thought that it might be of interest, and at the same time of some utility, to relate how I was led to develop the direct hydrogenation method by finely divided metals, which won for me the Nobel Prize in 1912.

Coming from l'Ecole Normale, where I had followed the lectures of Henri Sainte Claire Deville and performed my first laboratory work, I became an assistant of Berthelot at the College of France and was directed by him at first toward thermochemical matters. I naturally was obliged; under the influence of these two illustrious masters, to direct my researches toward general chemistry—thermochemistry, chemical equilibriums, reaction velocities, absorption spectra—and toward mineral chemistry—sulfides, selenides, chlorides, and hydrochlorides, nitrates, nitric acids, basic salts, etc.; and it would have astonished me greatly if, during the first twenty years of my scientific career, it had been pre-
dicted that I should work principally in organic chemistry and that in this field I should obtain the most important results.

In 1890, Mond, Langer, and Quincke announced that by the direct action of carbon monoxide on very finely divided nickel, prepared by reduction from its oxide, there was obtained a volatile compound, nickel carbonyl, resulting from the fixation on the metal of incomplete molecules of gas, and they indi-
cated that reduced iron yielded an analogous compound. I was very much impressed with these results, and I asked myself if other incomplete gaseous molecules—nitric oxide, nitrous oxide, nitrogen peroxide, acetylene, ethylene—could not, in the manner of carbon monoxide, be fixed on nickel or on reduced iron, in giving well-defined, stable, and volatile products, comparable to nickel carbonyl.

**Experiments with Nitrogen Oxides**

With the collaboration of one of my pupils, M. Senderens, who had just undertaken his doctorate thesis, I tried first of all nitric oxide, NO, the incomplete character of which is so clearly affirmed by its spontaneous combination with oxygen. But our tests with nickel, cobalt, iron, and copper were negative and, at temperatures more or less high, would give only reduction of nitric oxide to nitrogen, with formation
Work with Acetylene by Moissan and Moureu

We ended this work in 1896, and we were prepared to try the fixation of acetylene on the same metals, when we learned from the Comptes rendus of the Academy of Sciences that the experiments had just been attempted by MM. Moissan and Moureu. Having turned a current of acetylene on slivers of iron, nickel, or cobalt recently reduced from their oxides by hydrogen and chilled in this gas, they observed a brilliant incandescence. The high temperature thus produced decomposed the greater part of the acetylene into hydrogen and a large amount of carbon which was not long in filling up the tube. The rest of the acetylene yielded liquid hydrocarbons, which closely resembled those which Berthelot had obtained by heating acetylene to dull redness in a bell inverted over mercury.

According to the two scientists, the porous metal absorbs the acetylene with production of sufficient heat to cause its spontaneous destruction. As it is very endothermic, incandescence is reached and is maintained as long as the acetylene enters, and this incandescence determines the polymerization of the acetylene into liquid products. However, they neglected to analyze the free gas, which they judged to consist of hydrogen, and they examined the liquids only sufficiently to recognize the presence of benzene.

This publication left me greatly disappointed and tended to destroy my hopes of fixation. But the explanation of the phenomenon which the authors gave appeared to me inexplicable, and in my opinion they should be interpreted entirely differently.

On the mechanism of catalytic actions in general, whatever might be their nature, my ideas were very different from those usually admitted—ideas which I owed certainly to the influence of my master, Berthelot. I thought, and I still think, that in all cases the catalyst acts with one of the agents of the reaction, or of products which could result from it, in yielding a temporary unstable compound. For example, in the case of porous platinum sponge acting on a mixture of hydrogen and oxygen, its catalytic activity for combining them is due, not to a physical condensation of the gas in the pores of the metal, causing a local elevation of temperature sufficient to unite them, but to a truly chemical combination at the surface of the platinum: with the oxygen in a sort of suboxide, which acts immediately upon the oxidizable matter, here hydrogen, in producing some water and regenerating the suboxide, which acts immediately upon the oxidizable matter, here hydrogen, in producing some water and regenerating the metal, which indefinitely continues the same effect.

In the experience of Moissan and Moureu, the cause of the phenomenon seemed to me to be the affinity of metal—nickel, cobalt, or iron—either for acetylene or for its constituents, carbon or hydrogen, extracted by them from the endothermic molecule of gas.

Moreover, Moissan, who on account of his brilliant work upon fluorine and on the electric furnace had withdrawn from organic chemistry, was not greatly interested in this reaction, which he considered solely as a curious experience. If he had analyzed the liquids, he would have discovered that the hydrogen is only present in a small proportion and that there is found a large proportion of ethane. Moreover, if he had taken the precaution, after reduction of the oxides by hydrogen, to cool them, not within this last gas but within a current of pure nitrogen, to eliminate the hydrogen condensed or combined in the metal, he would have found that the flow of acetylene no longer determined a spontaneous incandescence, and that to release it he would have to heat it to 300° C. This proves that the determining factor is hydrogen condensed on the porous metal, or rather the hydride which covered its surface.

Fixation of Ethylene

I did not wish, by scientific courtesy, to take up immediately the experience of Moissan and Moureu. Having learned from an authoritative source that they did not think of pursuing it, I decided the following year, in 1897, to begin it, not at first with acetylene, but with a hydrocarbon less violent in its reactions, ethylene, bivalent carbide, which permitted me to expect a fixation resembling that of nickel carbonyl.

With M. Senderens, I directed a current of ethylene upon slivers of reduced nickel. No action was produced in the cold, but if the temperature was raised progressively, around 300° C., a brilliant incandescence of the metal was noticed, which disappeared in a voluminous deposit of black carbon, proving the destruction of the ethylene. But the gas which issued from the apparatus contained only a little hydrogen and was mainly ethane. The latter could only proceed from a hydrogenation of ethylene not destroyed, and this hydrogenation had been without doubt provoked by the presence of nickel. This permitted us to predict that the reduced nickel possessed the property of hydrogenizing ethylene.

This we immediately verified. If a mixture of equal volumes of ethylene and hydrogen is conducted through a bundle of thin slivers of reduced nickel, very lightly heated (30° to 40° C. is sufficient), one-half the volume of pure ethane is obtained, and the reaction continues indefinitely without the necessity of heating and without an appreciable modification of the metal. At a higher temperature, around 150° to 190° C., it is still more rapid, and a few centimeters of metallic slivers are sufficient to accomplish it.

Nickel obtained by the reduction of its oxide is also found to be a remarkable catalyst of hydrogenizing ethylene.

Acetylene Experiments

The following year we discovered that it possesses the same power with respect to acetylene, which starting from the ordinary temperature is changed to ethylene, then to ethane, according as in the mixture of acetylene it is treated with its own volume or a double volume of hydrogen.

We have, moreover, proved that reduced cobalt, iron, and copper, as well as finely divided platinum, sponge or black, possess an analogous but less energetic action.

Hydrogenation of Benzene

This special power of reduced nickel seemed to us so perfect that we thought of generalizing it, and we attacked a case of hydrogenation, which up to that time had not been realized by any method, that of benzene. Berthelot had attempted to effect it with his universal agent of hydrogenation, a concentrated solution of hydric acid at 250° C. But instead of cyclohexane, which boils at 81° C., he had prepared only its isomer, methylcyclopentane, which boils at 89° C. Under the influence of the hydricide acid the hexagonal nucleus was transformed into a substituted pentagonal nucleus.

It was with a real emotion that we proceeded to this decisive test. The benzene vapors, carried along by a current
of hydrogen in excess, were directed over a bundle of reduced nickel, maintained at 200° C; the gases issuing from the tube were sent to a U-tube surrounded by ice, within which the vapors of cyclohexane should be expected to condense to a liquid state. The operation seemed to proceed well from the beginning, the velocity of hydrogen issuing from the condenser tube seeming very little less than that at the entrance. But after boiling for a rather short time the gas ceased to pass, the U-tube was stopped up by the condensation of a solid product, which it seemed could only be benzene, solidifiable at 4° C—that is to say, within the ice—whereas we had seen in the literature that cyclohexane, extracted from Caucasian petroleum by Markovnikov by means of sodium in alcohol, melted at -11° C. But our despair was of short duration and was changed to a very lively joy, when on opening the U-tube we perceived, instead of the odor of the original benzene, the special intermediate odor between that of chloroform and that of the rose, which belongs to cyclohexane. The condensed product was cyclohexane, entirely pure, melting not at -11° C but above 6° C, absolutely un-attacked by the sulfonitric mixture. The transformation of benzene had been complete.

Experiments with Other Compounds

The power of finely divided nickel as a catalyst of direct hydrogenation was also found demonstrated in a clear manner and, therefore, we had then only to verify this power for the most diversified compounds.

Thus all the compounds possessing a double ethylene linkage—hydrocarbons, alcohols, acids—take up very easily hydrogen under 180° C. a molecule of hydrogen under the action of reduced nickel. All the unsaturated hydrocarbons of cyclic nuclei, homologous with benzene—polyphenyls, naphthalene, anthracene, etc.—easily take up hydrogen at their double bonds and are thus converted to the corresponding saturated hydrocarbons. The phenols furnish the cyclohexanic alcohols. Aniline and its homologs fix in the same manner six atoms of hydrogen for generating the cyclohexylamine and similar amines. The aldehydes and the aliphatic acetones take up very easily hydrogen in the presence of reduced nickel, and their formation accompanies the simultaneous formation of water, synthesis of methane starting from the carbon monoxide or carbon dioxide, preparation of the amines starting from the nitro derivatives, from the amides or the oximes. Either with M. Senderens or with two others of my pupils, MM. Mailhe and Murat, I have been able to accomplish with success more than two hundred different hydrogenations.

Precautions Necessary

These direct fixations of hydrogen in the presence of reduced nickel in the neighborhood of 180° C. are usually very easy and frequently give very high efficiencies without any difficulty. But this is accomplished only by using carefully prepared nickel and pure hydrogen, and avoiding the presence of any substance capable of forming with the nickel a combination irreducible by hydrogen, thus diminishing the active surface of the catalyst. The role of such a substance is analogous to that of living microorganisms in fermentations. A poison introduced into the liquid kills the ferments and suppresses their useful action. In the same way the metallic ferments, which is the nickel catalyst, is killed by traces of chlorine, bromine, sulfur, and thus neglect of sufficient precautions explains the failure that so many chemists experience in certain hydrogenations, such as those of benzene and phenol.

I should acknowledge that in this respect M. Senderens and I have had very good fortune. Naturally we were ignorant, at the beginning of our labor, of the extreme susceptibilities of the metal catalyst, and from the first, instead of ourselves preparing the oxide destined for the reduction, we made use of an oxide bought on the market, which fortunately was found good. By good fortune also, our benzene and our phenol, bought it is true as pure, were so in reality. But if there had been in the benzene a small portion of thiophene or of chlorobenzene, we would not have discovered any hydrogenation and, discouraged by this accidental failure, we would perhaps have attributed it to the direct impotence of nickel in this particularly difficult case of hydrogenation.

Let me cite a very striking example. We prepared some cyclohexane by the direct hydrogenation of phenol. The reaction proceeded regularly for two days, interrupted each evening but resumed the following morning. The slight oxidation which the nickel was able to undergo during the night in the chilled tube was not a disadvantage, since at the temperature of operation, around 180° C., the oxide of nickel is converted to the metallic state. Suddenly we discovered that the phenol was no longer transformed, and the preparation resumed with new nickel and the same phenol gave again a negative result. I remember that during the operation, a little while before the check of the catalysis, I had uncorked a bottle of bromine not far from the vessel which contained the phenol. The traces of bromophenol dissolved by this latter rendered it incapable of being transformed into cyclohexanol.

The various hydrogenations are, moreover, accomplished with very unequal care. Some demand perfectly pure materials, nickel prepared with the greatest possible care, and a temperature maintained within very narrow limits; whereas others, as the saturation of the double ethylene linkage or the reduction of nitro derivatives into amino compounds, are simple and require only moderate precautions. As an example of a particularly delicate hydrogenation I can cite that of benzoic ethers, which I vainly attempted to realize, first with M. Senderens, then with M. Mailhe, and which on the other hand, I accomplished easily with M. Murat in working on a mass of bronze at a temperature well regulated.

Theory of Metallic Catalysis

Since the activity of the metal catalyst is in direct relation to its porous state and disappears when the metal becomes compact, there has usually been given, as well as I recall just now, a purely physical explanation of the phenomena of catalysis by metals, based on the adsorption which they exercise on the gas. It may be considered that hydrogen and the vapor of hydrogenizable material are simultaneously absorbed with a great energy in the cavities of nickel, and that the compression and the heating which also take place within the cavities suffice to combine them, the products of the combination being immediately eliminated and replaced by a new portion of the reacting mixture.

I have not seen within these actions a succession of purely chemical phenomena. The hydrogen coming in contact with the nickel forms very quickly on the surface of each grain a very slight coating of a very definite hydride, such as NiH or NiH2, dissociable and capable of yielding rapidly its hydrogen to hydrogenizable substances in regenerating the nickel, which continues indefinitely the same effect. The activity of this nickel should, moreover, be all the stronger as the surface of the grains increases in proportion to their mass.

The liquid state of hydrogenizable material seems incompatible with the physical theory of the cavities where the compression of the gas determines the combination. On the
Another result of the theory of the temporary hydride is that hydrogens and aldehydes; the secondary alcohols form acetones. This was the beginning of the great industry of the hardening of oils, which has been so greatly developed in certain countries and particularly in the United States.

The hypothesis of the hydride of nickel formed directly by the hydrogen on the surface of the grains was remarkably supported by the observation of Schlenk and Weichselfelder, who were able, by the action of hydrogen on a colloidal solution of nickel, to prepare a brown flocculent hydride, NiH₄, which dried in the hydrogen becomes NiH₂₄, and which possessed remarkable abilities as a direct hydrogenizing agent. Another result of the theory of the temporary hydride is that it tends equally to produce at the beginning some substances which retain hydrogen only slightly. The hydride thus produced dissociates very quickly in liberating free hydrogen and regenerating nickel capable of reproducing indefinitely the same effect. We also foresaw that the nickel catalyst of hydrogenation would also be a catalyst of dehydrogenation, and that it would be the same with the other finely divided metals—copper, cobalt, iron, platinum. Experience has proved that this is very much the case.

The hydride of naphthalene, produced by the direct hydrogenation of naphthalene on nickel at less than 200° C., reverts to naphthalene at above 300° C. in contact with the same nickel.

The primary alcohols in contact with nickel, and still better with reduced copper, at above 200° C. divide sharply into hydrogen and aldehydes; the secondary alcohols form acetones in the same way. These are, as M. Senderens and I have indicated, the most advantageous procedures for preparing the aldehydes and the acetones. But at a lower temperature and in the presence of an excess of hydrogen, the reaction would be the reverse on the same catalyst.

My chemical theory of catalysis on pulverulent bodies has found very important support in the decomposition of alcohols and that of methanes acids. The ordinary vapors of alcohol directed at 230° C. on reduced copper, split exactly into hydrogen and aldehyde. On replacing the copper by another pulverulent material, alumina or thoria, the division is entirely different and gives solely water and ethylene. Some of the other pulverulent oxides furnish at the same time hydrogen and aldehyde, water and ethylene; that is the case with titan oxide and uranium oxide. Manganous oxide, recently been able to verify this at a temperature sufficiently high with respect to nitrobenezene.

The sense of the reaction then necessarily depends on the chemical nature of the catalyst—that is to say, to the quality of its temporary unstable compound; hydride in the case of hydrogenizing metals, and the same without doubt of certain oxides, hydrates, or alcoholates in the case of dehydrating oxides.

The catalysis of formic acid gives results not less conclusive. The finely divided metals, copper, nickel, platinum, split it cleanly into hydrogen and carbon dioxide. The titan oxide, on the contrary, separates it into water and carbon monoxide. The oxide of zinc, a white powder similar in appearance to titan oxide, behaves exactly like the metals and acts as a dehydrogenizer. We cannot well see how the physical theory of the compression of the gas within the cavities of the catalyst, can explain these differences of effect according to the nature of the powder.

In the case of alcohols subjected to alumina, I have been led to think that the temporary combination which determines the catalysis is a sort of alcoholic aluminate similar to an ethereal salt, such as ethylsulfuric acid, and capable of reacting in the manner of this latter upon the vapors of acetic acid to give acetic ester, upon hydrogen sulfide to produce thiol, and upon ammonia gas to yield amines. In every case the results obtained by my pupil, M. Mainhe, have verified completely my expectations. The action of hydrogen sulfide on the vapors of alcohol in contact with thoria or alumina is a very advantageous practical method for preparing the mercaptans, in the same way that amines are easily obtained by submitting a mixture of alcohols or of ammonia gas to the same catalyst at 350° C.

It is the faith in the theory of temporary compounds furnished by the catalyst which has constantly guided me in these various labors; it is these indications to which I owe all my new results. I shall retain them until the day when I find them irreconcilable with the well-established observed facts. Theories should never have the pretension of being indestructible. As has been said by Henri Poincare, "A theory is good as long as it is useful."

Bureau of Mines' Films Tell Story of Mining

The largest collection of educational motion picture films depicting the mining, treatment, distribution, and utilization of the numerous essential minerals ever compiled is in the possession of the Bureau of Mines. The bureau's motion picture library comprises more than fifty subjects visualizing the operations of the mineral and allied industries of the nation. The more than 2 million feet of motion picture film now possessed by the bureau represent an expenditure of almost $1,000,000. The entire expense of making the films has been borne by private industrial enterprises.

A number of highly interesting films depicting the production, refining, and distribution of petroleum have been made. The utilization of that important refined product of petroleum, gasoline, is portrayed in thirty-one films. The Bureau of Mines is undertaking an intensive campaign of safety in the oil industry of the nation, and two films show the safe and unsafe ways of producing oil. "The Story of Oil," made in cooperation with the National Coal Operators' Association, has proved to be one of the most popular films in the bureau's library.

"The Story of Sulfur" shows the ingenious processes by which sulfur is brought from the deposits lying 1000 feet below the earth's surface.

The production of motion picture films is conducted under the supervision of M. Leopold, safety engineer, and the distribution is centered at the Bureau of Mines Experiment Station, Pittsburgh, Pa. Sixteen distributing centers have also been selected with regard to accessibility. The final censoring of a film is done by a board of review selected from the technical staff of the bureau.

All costs, such as express charges, postage, etc., in connection with the shipment of films must be paid by the borrower. The films are loaned free of charge to all desiring to use them, with the understanding that they will receive the best possible care and that all damages and loss other than nominal wear and tear will be replaced by the borrower. A revised list of the bureau's motion pictures has been issued, and a copy may be obtained by addressing the Bureau of Mines, Department of Commerce, Washington, D.C.