Hypnotism : including a study of the chief points of psycho-therapeutics and occultism / Albert Moll.

Contributors

Moll, Albert, 1862-
Harvey Cushing/John Hay Whitney Medical Library

Publication/Creation

London : Walter Scott Pub. Co. ; New York : C. Scribner's Sons, 1913.

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HYPNOTISM.

HYPNOTISM:

INCLUDING A STUDY OF THE CHIEF POINTS OF PSYCHO-THERAPEUTICS AND OCCULTISM.

BY

DR. ALBERT MOLL.

TRANSLATED FROM THE FOURTH ENLARGED EDITION BY

ARTHUR F. HOPKIRK.

THE WALTER SCOTT PUBLISHING CO., LTD.
PATERNOSTER SQUARE, LONDON, E.C.
CHARLES SCRIBNER'S SONS
597 FIFTH AVENUE, NEW YORK
1913

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PREFACE

TO THE FIRST EDITION.

In writing this book I was guided by the wish to offer to the reader a survey of all that is most important in the whole province of hypnotism. While in the numerous and detailed works on this subject which have lately appeared, sometimes its therapeutic and sometimes its forensic significance have been exclusively brought forward, I, for my part, have endeavoured to treat hypnotism broadly and from various points of view, avoiding irrelevant matter; and, being aided by my own experiments, I was in a position to add much that was new to what was already known.

I here express my hearty thanks to Professor August Forel, Director of the Cantonal Lunatic Asylum in Zürich, who placed several of his most valuable experiments at my disposal for this book; also to Dr. Max Dessoir, of Berlin, who has assisted me both with his wide acquaintance with the literature of hypnotism and with much good advice; finally, to all who in other ways helped me in the work.

A. MOLL.

Berlin,
April, 1889.

PREFACE

TO THE FOURTH EDITION.

My numerous other duties have, unfortunately, delayed the appearance of this edition of my book, in spite of the fact that the third has long been sold out. In the present, fourth edition, I have completely revised my former work and made many important additions thereto. I have endeavoured to bring it into line with our present-day knowledge, and have laid special stress on the universal importance which has become attached to hypnotism and suggestion during the last ten years. I have given the narrowest limits possible to the concept suggestion, with the view of better differentiating suggestion from other psychic process than was formerly done. Relatively speaking, the fewest alterations have been made in the chapters on symptomatology and post-hypnotic suggestion. Very little has been added to our knowledge of these questions during the last few years, and it would appear that this branch of hypnotic research is fairly exhausted, though, of course, it may one day happen that it will have to go through a searching revision which will prove instructive.

In the chapters which treat of the various theories of hypnotism I have endeavoured to throw fresh light on the experiments I originally made for the purpose of explaining the phenomena. But I adhere to the position I primarily assumed—to wit, that certain premises must be accepted if we are to explain

hypnotism. We shall be far more likely to arrive at a proper conception of hypnosis by reasoning from analogy than by paying any attention to misty psychological concepts or physiological experiments which our present knowledge of ultimate cerebral processes is quite unable to explain. I have added a chapter dealing with the general influence that hypnotism and suggestion have had on medicine: the first part of it is devoted to theoretical medicine and the far-reaching effects of hypnotism on this branch of the question; the second part contains a study of the salient points of psycho-therapeutics. I have done this because psycho-therapeutics seems to me to be a developmental outcome of hypnotic and suggestive therapy, and ought, therefore, to be distinguished from hypnotic therapeutics, which only constitutes a small branch of general psycho-therapeusis. Attention is also drawn to the connection that subsists between hypnotism and the science of psychology, especially with regard to the important part played by suggestion in all psychological investigations. I have also discussed the influence of suggestion on other questions, such as art, superstition, ethnology, etc., much more fully than in the earlier editions of this work. My reason for doing this is the tendency nowadays to overlook the influence of modern hypnotism on the most varied branches of science and the different phenomena they present. The revised chapter on the legal aspect of hypnotism will be found to contain much fuller details than in former editions. I have shown in it the close historical connection that subsists between the psychology of testimony and hypnotism.

The last section of the book—that dealing with the

most important points connected with occultism—has been considerably enlarged. I felt bound to extend this chapter: first of all, because hypnotism has brought to light many sources of error in this connection, and secondly, because real criticism affords us the best means of stemming the tide of the uncritical advocacy of occultism. It cannot be denied that belief in occultism has increased in recent times. I do not assume this merely from the increase in the number of occultistic societies and periodicals, but rather because private conversation has convinced me of the fact. I have also observed an increased tendency on the part of the public to mystery-mongering. I need only recall the epidemic of "faith-healing," the sensation caused by the so-called "sleep-dancers," the way in which many people were upset by the doings of the horse "Clever Hans," the uncritical praise bestowed on the divining-rod, and the medical miracles of such individuals as Kneipp, etc., etc. The fact that so many worthy men of science have taken to this mystery-mongering is not calculated to make future generations have much respect for the present age. That a man like Crookes should believe that Home could overcome the force of gravity without employing any mechanical means, that Lombroso should believe that Eusapia Palladino could move objects by the action of her will alone, that Stumpf should believe that a horse could be educated like a child and be influenced by telepathy, that Richet should believe that the murder of the Servian royal family was foretold in Paris by occult means—all these things are but instances of the errors that otherwise competent investigators may make. For they are nothing but cases of error, not because the investigators attempted to explain the impossible, but because they

based their conclusions on imperfect data, and did not see the pitfalls before them. These scientists and others like them, prove that a man may be proficient in his own special branch and yet quite incapable of criticizing other methods of research. In spite of these and other authors who express a belief in occultism and spiritism, I can safely say, not only as the result of my own experiments but also from a careful study of numerous occultistic and spiritistic works, that I have never come across even one single experiment carried out under strictly scientific conditions that could be said to justify the assumption that occult forces exist. One of the biggest swindles perpetrated by occultists is the way in which they promise beforehand strict adherence to scientific conditions, and then do their utmost to prevent such conditions being observed.

In spite of my most earnest endeavours, I have never been able to detect even the slightest approach to occult phenomena, provided strict conditions were observed; in all these investigations the assumption of animal magnetism, telepathy, clairvoyance, etc., was altogether superfluous. I am, of course, just as willing in the future as I always have been in the past to investigate, under the conditions enjoined by science, all cases of mediums, magnetizers, etc., etc., who profess to possess occult powers, for I consider *à priori* negation just as unscientific as those swindles and frauds connected with occultism which I have so strongly condemned.

ALBERT MOLL.

St. Helier, Jersey.

apparently written in 1913

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HYPNOTISM.

CHAPTER I.

THE HISTORY OF HYPNOTISM.

In order to understand the gradual development of modern hypnotism from animal magnetism, we must distinguish two points: firstly, the belief that there are human beings endowed with a power not acting by suggestion, but by means of which they can exercise an unwonted influence over others, either by direct contact or even from a distance; and secondly, the fact that a particular psychical state, which we term hypnosis, can be induced in human beings by means of certain actions. This second fact has long been known; it was frequently interpreted in a fallacious manner, and was utilized, more especially by Oriental peoples, for religious purposes and superstitious customs. Kiesewetter attributes the early sooth-saying by means of precious stones to hypnosis, which was induced by steadily gazing at the stones. This is also true of divination by gazing into vessels and crystals, as the Egyptians have long been in the habit of doing (Rossi), and as has often been done in Europe—by Cagliostro, for example. Bonfigli believes that his essay on Cagliostro proves that the well-known adventurer was fully acquainted with the means of inducing hypnosis, and surmises that Cagliostro had been taught in his youth how to hypnotize by Althotas, since the latter was versed in physical science, Oriental languages, and the hypnotic art of the fakirs. The hypnotic phenomena are also found to have existed several thousand years ago among the Persian Magi, as well as up to the present day among the Indian yogis and fakirs, who throw themselves into the hypnotic state by fixation of the gaze. Relying on a statement of Stein, Preyer believes that the condition of a Japanese religious leader, who lived long before Christ, was also an auto-hypnosis, and that this kind of

devotion came to Japan from India. Gumpertz, too, has given detailed proof that hypnosis played an important part in the religious philosophy of ancient India. Many Indian priests and monks earnestly endeavour to acquire a knowledge of religious philosophy and the power of self-inducing a state of trance, *i.e.*, two apparently heterogeneous things. Gumpertz, therefore, set to work to fathom these two phenomena, and at the same time discussed the question as to whether the self-induced trance has anything to do with hypnosis or not. He has come to the conclusion that, even if slight mention is made of external suggestion, we have here to deal with conditions set up by auto-suggestion. Various investigators maintain that the state produced by hypnosis is expected to facilitate the acquisition of a higher degree of knowledge. In his graduation thesis published in 1893, Hermann Walter deals with the literature concerning the method of inducing yoga sleep, from which Schrenk-Notzing concludes that the means employed are in part the same as those used in inducing hypnosis—preparatory steps, abstention from anything calculated to excite the emotions, concentration of the thoughts, etc.; but he considers that the Indian ecstatic carried out these measures to a superlative degree. Regnier also, in his book on *L'Hypnotisme et les Croyances anciennes*, gives much authentic information concerning the part played by hypnosis and auto-suggestion in the religious exercises of the Indians, Persians, Egyptians, and Jews of old. On the strength of information communicated to him by Adolf Harnack, Preyer concluded that the self-induced insensibility of the Montanists, a Christian sect of the second century after Christ, were of auto-hypnotic character. The Montanists were also called Taskodrugites (τασκός forefinger, and δροῦγος nose), because they were accustomed to hold the finger to the mouth and nose during prayer. Brugsch Pasha also tells of genuine hypnotic conditions which were produced in the Egyptian Gnostic schools of the first century during divine worship. At such times the eyes were closed and various hallucinations appeared, especially apparitions of gods. The same thing has occurred since the eleventh century in many convents of the Greek Church (E. L. Fischer). Among the best known are the Hesychasts, or Omphalopsychists, of Mount Athos, who hypnotize themselves by gazing at the umbilicus. I take this opportunity of pointing out that the fact has often been verified

in popular opinion, that it is possible to induce sleep by looking fixedly at the tip of the nose. Apart from the yogis and fakirs, hypnotic conditions are found among various peoples in the present day. In a recent report from Abyssinia we are told that the Lebascha[1] children are trained to detect crime. These children, when hypnotized, are said to be able to detect a criminal even at a very great distance. One of them is stated to have run for sixteen miles without stopping, and then to have unearthed the criminal directly it arrived at his hiding-place. Michaud informs us that the Annamites employ hypnotism for religious purposes; in these cases hypnosis is attained by the subject fixing his gaze on two burning sticks attached behind one ear of the magician, who turns his head. Hypnotic conditions appear often to occur among uncivilized peoples, as is clearly to be gathered from the information of many travellers. That eminent ethnologist the late Professor Bastian, of Berlin, pointed out long ago the near relationship of many phenomena among uncivilized populations to hypnotism. Bastian believed that a more exact study of hypnotism by individual travellers would be of great service to ethnological psychology; the phenomena which occur spontaneously among uncivilized peoples could then be more carefully examined and brought in closer relation to artificial hypnosis.

The ethnologist Stoll also, in his comprehensive work, *Suggestion und Hypnosis in der Völkerpsychologie*, a second edition of which appeared in 1904, gives many details concerning hypnotic conditions among various peoples and in various ages. Certainly the book does not treat specially of hypnosis, but deals more with suggestion in general, to which Stoll gives a very wide range. Still, numerous instances are given of states which we cannot but consider hypnotic, or auto-hypnotic. For example, he mentions hypnosis among the Mongols and Chinese. According to him, the auto-hypnotic state is brought into play by the Chinese soothsayers of West Borneo for clairvoyance, and in the treatment of disease. The auto-hypnotized soothsayers give verbal or written answers to any questions which may be propounded to them. Stoll also tells of the hypnotic manipulations of the Tagals. A lady of his acquaintance, long resident in Manila, allowed herself to be hypnotized for insomnia by a Tagal woman, who had acquired

[1] Stoll writes "Lebascha"; in many newspapers it is spelled "Labascha."

her knowledge of mesmerism from her mother and her grandmother. In this instance verbal suggestions, such as "Don't speak" and "Shut your eyes," were also employed to promote the onset of sleep. Like many other investigators, Stoll holds the view that the distinction which has so often been made between the civilized and uncivilized, in respect to auto-hypnotism, is not possible, since even among civilized races the characteristics of savages still lie at the foundation. Nietzsche also expressed the opinion that certain dreamy conditions, which among the civilized occur during sleep, among savages often occur in the waking condition. The perfect clearness of dream ideas, which presupposes an unconditional faith in their reality, recalls conditions of primitive humanity when hallucinations were extraordinarily prevalent.

Independently of this production of particular psychic states, there has existed at all times in many quarters the belief that particular individuals could influence their fellows by the exercise of certain powers. This influence could be used as well for good as for evil. Of the first use we are reminded by the laying on of hands in benediction; also by the healing by touch which was obtained by the old Egyptians and other Oriental nations; numerous old monuments testify to this. The Ebers Papyrus also, which represents the state of Egyptian medicine before the year 1552 B.C., and of which Joachim has made a complete German translation, contains a statement according to which the laying on of hands on the head of a patient plays a part in his treatment. We see the same thing later in the cures which King Pyrrhus and the Emperor Vespasian are said to have effected. It is known that Francis I. of France, and other French kings up to Charles X. (Perty), healed by the imposition of hands. As Vierordt mentions in his work, *Medicine in Universal History*, kings of England performed similar acts and thereby aroused a feeling of jealousy between the two nations. An Englishman named William Tooker upheld the priority of the English kings; but Andreas Laurentius, Physician-in-ordinary to Henry IV. of France, proved but a very mean champion of his sovereign's claims. It was customary to attribute the action of this individual power to contact; however, this appears not to have been always necessary, as is witnessed by the widespread and continued belief in sorcerers, who can bewitch other persons, not only by contact, but even from a great distance.

This belief in the special powers of certain individuals still exists among uncivilized and even civilized peoples. Healing by breathing and by contact, as Barrère described it in Guyana in the eighteenth century, as well as similar processes described by Father Bryert in California, must here be reckoned. Numerous facts of the same order may be found in the writings of travellers, and especially in the works of Bastian and Stoll. Among the Mohammedans also there are persons who enjoy the reputation of possessing special powers of healing. Whoever has seen the howling dervishes of the East will have observed that the Sheik, in attempting to heal the sick, especially children, breathes on them, or touches them, and often treads with the foot.

The question here is only of solitary facts, in which no scientific system is discoverable; and it was not till after the end of the Middle Ages that an attempt to set up such a system was made. It was developed out of astrology, the doctrine which teaches the influence of the stars upon men. Even nowadays we find remains of it, especially in the belief in the influence which the moon is supposed to exercise. Many people expect warts to disappear as the moon wanes, while more modern specialists in mental disease have been known to call in the influence of the moon to explain special periodical mental disturbances.

About 1530 Theophrastus Paracelsus came forward with the theory of the effect of the heavenly bodies on mankind, more especially on their diseases. Out of this the theory gradually developed that not only did the stars influence men, but that men mutually influenced each other. Van Helmont (1577-1644) taught with more precision that man possessed a power by means of which he could magnetically affect others, especially the sick. Perhaps Helmont obtained the main features of his system from Goclenius (1572-1621), a learned physician and professor of physics at Marburg.

Later on (about 1665) the Scotsman Maxwell advanced a theory of magneto-therapeutics of his own, based on the work of Robert Fludd, Andreas Tentzel, and others. According to this theory, everybody is supposed to emit rays evincing the presence of the soul, and these rays are endowed with a vital spirit by means of which the soul performs its actions. Now, the animal excreta contain a portion of the vital spirit; hence Maxwell attributed to them an effect upon human beings; they

could be utilized for the curing of diseases (sympathetic cures); also men could cure themselves of diseases by transferring them to animals and plants. A remnant of this system developed by Maxwell still exists in country places, where people occasionally apply excreta to their wounds. Adolf Witke, in his work on popular German superstitions of to-day, treats in detail of the transference of disease from one person to another; as, for example, the prevalent belief among Thuringians that if a person suffering from nasal catarrh wrap up a copper coin in a piece of paper into which he has blown his nose, and throw it backwards over his shoulder into the street, then the cold will be transferred to the individual who may happen to pick up the packet. Maxwell also assumed the existence of a vital spirit of the universe (*spiritus vitalis*), by means of which all bodies are related to each other; a theory we meet later on in Mesmer's universal fluid. In the beginning of the eighteenth century we find Santanelli in Italy asserting a like proposition. Everything material possesses a radiating atmosphere, which acts magnetically. Santanelli, however, recognized the great influence of the imagination (Avé Lallemant).

Although the foundation of the doctrine of animal magnetism was thus laid, universal attention was first drawn to it by Mesmer,[1] a Viennese doctor (1734-1815). He studied in his dissertation the influence of the planets on human bodies. In the year 1775 he sent out a circular-letter particularly addressed to several academies. In this he maintained the existence of animal magnetism, by means of which persons could influence each other. He, however, distinguished animal magnetism from the magnetism of minerals, which he at first used in the treatment of diseases, but later on ceased to employ. The only academy which replied to him was that of Berlin, at Sülzer's instigation, and its reply was unfavourable. However, about this time Mesmer was nominated a member of the Bavarian Academy.

Mesmer made much use of "animal magnetism" in the treatment of disease. He cured at first by contact, but

[1] The name is often written "Messmer," instead of "Mesmer"; the latter spelling is, however, decidedly the correct one. At least it is so found in the book which Mesmer himself brought out, *General Explanations of Magnetism*, by Mesmer; Carlsruhe, 1815. Mesmer's friend, Wolfart, and his biographer, Justinus Kerner, also write the name with one "s."

believed later on that different objects of wood, glass, iron, and so forth, were also capable of receiving the magnetism. Consequently he made use of these as means for conveying his magnetism, especially later in Paris, where he went in 1778, chiefly in consequence of the enmities he had aroused in Vienna. In Paris, Mesmer constructed the *baquet*, which was magnetized by him, and which was supposed to transmit the magnetism. Bailly represents it as a very complicated apparatus: an oak chest with appendages of iron, etc. Mesmer found many adherents in Paris, but he also encountered many opponents. Dr. Deslon, a Parisian physician, became one of his chief adherents, and was ruthlessly attacked by his colleagues in consequence. The Faculty of Medicine, in particular, was most severe in its attacks on the new teaching, and when thirty physicians continued to practise magnetism in spite of warning, issued the following circular-letter:—"In future no doctor will be allowed to write favourably of animal magnetism, or practise the same, on penalty of losing his professional privileges" (Ochorowicz). From this we perceive how intense the intolerance of the medical schools of those days was, and with what bitterness heretics were persecuted. Several scientific commissions which examined the question pronounced, in 1784, against the existence of animal magnetism—more particularly that of which Bailly was reporter. One of the members of the commission, Jussieu, made, however, a report that was not considered decisive. No one, however, denied that far-reaching effects were produced by imagination; it was only denied that there was a physical force resembling the magnetism of minerals. In spite of all attacks Mesmer made disciples. His pupils and successors are generally called mesmerists, and the doctrine of animal magnetism is also called mesmerism, vital magnetism, bio-magnetism, therapeutic magnetism, or zoo-magnetism.

I do not wish to join the contemptible group of Mesmer's professional slanderers. He is dead, and can no longer defend himself from those who disparage him without taking into consideration the circumstances or the time in which he lived. Against the universal opinion that he was avaricious, I remark that in Vienna, as well as later in Mörsburg[1] and Paris, he always helped the poor without reward. I believe that he erred in his teaching, but think it is just to attack this only, and not his personal

[1] Also spelt Meersburg.

character. Let us consider, however—for I deem it right to uphold the honour of one who is dead—more closely in what his alleged great crime consisted. He believed in the beginning that he could heal by means of a magnet, and later that he could do so by a personal indwelling force that he could transfer to the *baquet*. This was evidently his firm conviction, and he never made a secret of it. Others believed that a patient's mere imagination played a part, or that Mesmer produced his effects by some concealed means. Then, by degrees, arose the legend that Mesmer possessed some secret by means of which he was able to produce effects on people, but that he would not reveal it. In reality the question was not at all of a secret purposely kept back by him, since he imagined that he exercised some individual force. Finally, if he used this supposititious force for the purpose of earning money, he did nothing worse than do modern physicians and proprietors of institutions who likewise do not follow their calling from pure love of their neighbour, but seek to earn their own living, as they are quite justified in doing. Mesmer did not behave worse than those who nowadays discover a new drug, and regard the manufacture of it as a means of enriching themselves. Let us be just and cease to slander Mesmer, who did only what is done by the people just mentioned, against whose procedure no one raises a word of protest, even when the drugs they extol possess no therapeutic properties whatever. Further, Colquhoun, who is thoroughly conversant with the events of the period, opines that Mesmer never made nearly as much money as he is said to have done (Sinnett). That those who defame Mesmer know the least about his teaching and the particulars of his life, is very clearly shown by a whole series of modern books on hypnotism.

A follower of Mesmer, Chastenet de Puységur, discovered in 1784, a state which was named artificial somnambulism. Apart from some falsely interpreted phenomena (thought-transference, clairvoyance, etc.) the chief characteristic of this state was a sleep, in which the ideas and actions of the magnetized person could be directed by the magnetizer. Whether Mesmer knew of this condition or not is uncertain, but it seems to me probable that he did. About the same time Pétetin, a doctor of Lyons, occupied himself with magnetism; besides catalepsy, Pétetin describes phenomena of sense-transference (hearing with the stomach). The French Revolution and the wars repressed the investigation of magnetism in France until about the year 1813.

In Germany, animal magnetism was recognized at the same time in two different places—on the Upper Rhine and in Bremen. In the year 1786 Lavater paid a visit to Bremen, and exhibited the magnetizing processes to several doctors, particularly to Wienholt, through whom Albers, Bicker, and later on Heineken, were likewise made acquainted with magnetism (Sierke, Wienholt). Bremen was for a long time a

focus of the new doctrine ; the town was often even brought into bad repute in the rest of Germany on account of the general dislike to animal magnetism. About the same time the new doctrine spread from Strassburg over the Rhine provinces; Böckmann, of Carlsruhe, and Gmelin, of Heilbronn, occupied themselves with it ; later on they were joined by Pezold, of Dresden. Getting encouragement from Bremen, people began to make experiments in other parts of Germany. Selle, of Berlin, brought forward, in 1789, a series of experiments made at the Charité, by which he confirmed a part of the alleged phenomena, but excluded all that was supernatural (clairvoyance). In Berlin magnetism was taken up by the Court. According to Vehse, magnetizers flocked to the palace where Frederick-William II. lay ill; and one of them in particular, a Parisian named de Beaunnoir, tried to induce Countess Lichtenau to obtain his admission to the sick-chamber. He advised the imposition of a magnetic hand to ensure the king's recovery, and asserted that his own, or the Parisian de Puyégsur's, or Count Brühl's would suffice.

Notwithstanding the early dislike to it, magnetism finally gained ground in Germany. It flourished very much during the first twenty years of the nineteenth century, and many journals were devoted to its advocacy. In Austria only it made no progress; indeed the exercise of animal magnetism was forbidden in the whole of Austria in 1815. In the rest of Germany, however, many doctors began to occupy themselves with the question, and at first in a thoroughly scientific manner. I do not enter more fully into the details of the teaching of different individuals, as they have no close connection with hypnotism. In the main two different tendencies can be distinguished—one critical and scientific, and the other mystical. While the first had the preponderance in the beginning, later on the last came to the fore and proved fatal to magnetism. Besides the scientific investigators already mentioned I may name Treviranus, Kieser, Passavant, Kluge; also Pfaff, who attacked clairvoyance in particular; and further, Stieglitz, Fr. Hufeland, and C. W. Hufeland. The last, who was at first an opponent, acknowledged certain facts later on, but excluded all that was supernatural, and thus drew upon himself the hatred of the mystics. Even in 1834 C. W. Hufeland expressed himself as recognizing the existence of animal magnetism and its value in healing. Among the

mystics I may mention Schelling, Ziermann, Eschenmayer, Justinus Kerner, the well-known poet and editor of the *Seeress of Prevorst.*

In the year 1812 the Government sent Wolfart from Berlin to Mesmer at Frauenfeld, in order that he might there make himself acquainted with the subject. Wolfart came back a thorough adherent of Mesmer, introduced magnetism into the hospital treatment, and afterwards became a professor at the university. A prize which was offered by the Berlin Academy of Sciences, at the request of the Prussian Government, for an essay on Animal Magnetism was, it appears, withdrawn. However, animal magnetism flourished to an extraordinary extent at that time in Berlin, and Berlin physicians placed a monument on Mesmer's grave at Mörsburg. The well-known physician Koreff, also, of whom Cuvier said later if he were not already in Paris he must be entreated to come there, interested himself much in magnetism, and often made use of it for healing purposes so long as he lived in Berlin. Virchow complained in his address as Rector of the University, in 1893, that the Chancellor of State, Hardenberg, had, in 1816 and again in 1817, with the full concurrence of William v. Humboldt, expressed an earnest wish to promote Wolfart and Koreff, the chief representatives of animal magnetism in Berlin. The extent to which animal magnetism had gained ground in Berlin at that period may be judged from the fact that theological students received instruction in physiology, pathology, and the treatment of sickness by vital magnetism. It was Mesmer's idea to teach magnetism to the clergy, and this may account for the assumption on the part of a few individuals—Nicolai and Biester in Berlin, for instance—that the only object of Mesmer's teaching was to restore their lost power to the Church and the Jesuits.

The attitude of the Catholic Church to animal magnetism has often been discussed. Clerical authorities were frequently asked if the use of animal magnetism in the treatment of disease were permissible. Such a question was propounded in Rome in 1842, and, according to Gousset, the Grand Penitentiary Castracane replied in the following somewhat evasive terms:—A hasty decision might endanger the honour of the Holy Chair, and the question was not of a nature to necessitate an immediate conclusion being come to, because no danger would be run by postponing judgment. The Catholic Church had raised no general objection to the use of animal magnetism in individual cases, but had tacitly permitted it, though she had protested against many abuses.

In the rest of Germany, also, many investigators occupied themselves with animal magnetism; in several universities a knowledge of the phenomena was spread by means of lectures —for example, by Wolfart in Berlin, and by Bartels in Breslau. In 1821, Fritze, the Magdeburg physicist, began to occupy himself with magnetism, and in 1853 Varges, who had commenced his investigations at the same time as Fritze, published the result of his experiences since 1821. In South Germany also the importance of animal magnetism began to be better recognized, and in 1818 Haberl, of Munich, employed it in the treatment of disease in the hospital of that town. As many authors inform us, a royal order in February 1817 made magnetization in Prussia the privilege of physicians only; but in the official code of laws nothing is to be found on the subject. At the same time such laws were enacted in other countries. Magnetism was introduced everywhere, especially in Russia and Denmark, in which countries medical men were bound to report periodically to the authorities the results they had obtained with animal magnetism (Brandis). In Holland such distinguished physicians as Bakker, Wolthers, and Hendriksz devoted themselves to the study of animal magnetism; but in Switzerland and Italy it was at first received with less sympathy.

After Mesmer had left France in the time of the Revolution, in order, after prolonged travels, to settle himself at his native place on the Lake of Constance, magnetism only regained its importance in France at the beginning of the last century. In Germany it was chiefly the medical profession which turned to the study of animal magnetism. True, a number of French doctors experimented with it, and Esquirol states that in 1813-16 he made use of it in eleven cases of mental disorder, but without producing even the slightest improvement in the condition of the patients. Nevertheless, in France mesmerism for the most part fell into the hands of laymen. Here Deleuze may be mentioned as one of the earnest investigators. But the whole doctrine received a great impetus through the Abbé Faria, who came to Paris from India. In 1814-15 he showed by experiments, whose results he published in part in 1819, that no unknown force was necessary for the production of the phenomena: the cause of the sleep was in the person who was to be sent to sleep—all was subjective. This is the main principle of hypnotism and of suggestion, of which Faria made

use in inducing sleep. General Noizet allowed the Abbé to experiment on him, and even if he did not fall into a deep sleep, a condition which we nowadays call the lighter degree of hypnosis was induced. The General relates that he was unable to open his eyes until Faria allowed him to. Shortly afterwards the Abbé was suspected of fraud, simply because he was tricked by an actor who had been persuaded to feign sleep while pretending to submit honestly to the process of hypnotizing. Thus Faria, a thoroughly honourable man, was set down a swindler, in spite of the fact that for a long time he alone, almost, held the only true view of mesmerism; while, as Ochorowicz reproachfully states, not a single scientist gave himself up to a study of the question. Later on, in France, Noizet, whom we have mentioned above, and a physician named Bertrand, paved the way for the doctrine of suggestion, in spite of much inclination to animal magnetism. In 1820, experiments were begun in the Paris hospitals, chiefly under the direction of Du Potet. At the proposal of Foissac, and at the recommendation of Husson, the Paris Academy of Medicine in 1826 appointed a Commission to examine the question of animal magnetism. The Commission worked for six years, and pronounced a favourable opinion in 1831; but the Academy was evidently not convinced. In spite of several further experiments—for example, those of Berna—no other result was obtained. Particularly because the chief emphasis was laid on the mystical side of the question, the struggle was made substantially easier to the opponents of mesmerism, among whom Dubois was prominent. The candidates for the celebrated Burdin prize for clairvoyance, Pigeaire, Hublier, and Teste, failed to obtain it; and in 1840 the Academy declined to discuss the question further. Nevertheless, animal magnetism retained numerous adherents in France, particularly in lay circles; and in the following years several works were published on the question. I may mention those of Aubin Gautier, who made many valuable contributions to the history of animal magnetism, and Ricard's exhaustive treatise on the theoretical and practical bearing of the doctrine. Baron du Potet, too, must be mentioned. In brief, the doctrine retained many adherents, not only in Paris but in other French towns as well—for example, Havre.

Meanwhile, in Germany a few investigators still busied themselves with mesmerism. I find that in 1818 the University of

Leipzig published a graduation thesis by Wendler, entitled *De Magnetismi animalis efficacia rite dijudicanda;* and another in 1826, by Volkmann, *Observationes biologicæ de Magnetismo animali.* But in the main, after about 1820, the belief in animal magnetism declined. This retrogression was caused as much by the rise of the exact natural sciences as by the unscientific and uncritical hankering after mystical phenomena, which could not but revolt serious investigators. Mesmerism flourished relatively the longest in Hamburg and Bremen, where Siemers was its advocate; and also in Bavaria, where Hensler and Ennemoser, between the years 1830 and 1840, still represented it; and as late as 1857 Wurm, a Munich physician, published an enthusiastic book on mesmerism in the treatment of disease. In other towns we likewise find a number of thoughtful inquirers, who allowed themselves to be influenced neither by the passion for the wonderful nor by the attacks of the principal opponents of magnetism, and who sought to defend their position in a thoroughly scientific manner; Most, Fr. Fischer, and Hirschel may be mentioned. A series of philosophers and philosophical writers also has believed firmly and persistently in the reality of the phenomena, although not much regard has been paid to this fact; for example, Schopenhauer, Carus, Pfnor, and Wirth.

About the middle forties of last century the waning fire of animal magnetism burst somewhat more strongly into flame in several towns simultaneously. In Vienna, on Eisenstein s recommendation, a Commission of Investigation was appointed, on which Güntner, Schuh, Dumreicher, and other Viennese physicians sat; but according to the report published by Gouge, the commission expressed itself as vigorously against the existence of animal magnetism as Czermak had done a short time before. The excitement also caused at that time by Reichenbach's theory of the "Od" could not help bringing fresh adherents to the cause of animal magnetism. Fechner, in his reminiscences of the last days of the theory of the Od, thus defines the Od itself:—"According to Reichenbach, the Od is an imponderable force, analogous to electricity and to magnetism, but differing more or less from the latter in the phenomena it exhibits, and in following its own special laws." Considering the close relationship that subsists between the theory of the Od and that of animal magnetism, it is easy to

understand that the promulgation of the former necessarily brought fresh friends to the latter.

Although magnetism gradually lost nearly all its adherents in the scientific world, among the people the belief in the mysterious force continued prevalent. In Germany, however, as well as Austro-Hungary, where Counts Szápary and Mailáth were well known in this connection, but more particularly in France, a whole series of laymen continued to use animal magnetism for healing purposes. The more science drew back, the louder became the clamour of the quacks. But the more intentional fraud and cheating increased, the less inclined were seriously-minded persons to interest themselves in these questions.

In England, in spite of the efforts of many physicians, particularly Elliotson and Ashburner, the theory of animal magnetism could get no footing in the scientific world, as it had done on the Continent. A succession of experimenters and writers, however, actively pursued the matter; for example, Townsend, Scoresby, and Edwin Lee. When the French magnetizer, Lafontaine—a grandson of the poet, according to Ochorowicz—exhibited magnetic experiments in Manchester in 1841, Braid, a doctor of that place, interested himself in the question. He showed, like Faria, that the phenomena exhibited by the person experimented on were of subjective nature, and were not induced by any magnetic fluid. By carefully fixing the eyes on any object a state of sleep was induced, which Braid called "hypnotism."[1] Braid did not straightway consider the hypnotic state to be identical with mesmerism, but for a time, at least, left the latter in an independent position by the side of hypnotism.

In the foregoing I have followed the phenomena of animal magnetism down to the middle of the last century. The historical development, as I have traced it, begins with the popular opinion that, in the first place, there are human beings who can exercise a personal influence over others, and that, in the second place, peculiar psychical conditions can be called forth by means of certain manipulations. The scientific development of hypnotism now begins. In this we see the relationship of modern hypnotism to animal magnetism, in

[1] The name was not, however, altogether new, as already Hénin de Cuvillers had talked of "hypnoscope" and "hypnobat," with reference to magnetic states (Max Dessoir).

that both are called forth by the influence of one man on another; but nowadays this influence is held to be psychical, and in no way connected either with a magnetic fluid or the mineral magnetism.

Braid now set to work to investigate the symptomatology of hypnosis, and in 1843 published his *Neurypnology*, a treatise on the subject. He was acquainted with the cataleptic phenomena and certain suggestions, and used hypnotism therapeutically; in particular, he used it to perform painless surgical operations, just as mesmerism had been made use of in former times. Compared with his earlier works, Braid's later writings show great progress; in them he lays far greater emphasis on the psychical significance of hypnosis than had been his wont. In this connection I may mention his work entitled *Magic, Witchcraft, Animal Magnetism, Hypnotism, and Electro-Biology*, a third edition of which appeared in 1852. In the result we see Braidism, as the state investigated by him is sometimes called, occasionally used for therapeutic purposes, but more particularly for the performance of painless surgical operations. Among those who used hypnotism in surgery, the following deserve to be mentioned:—Loysel, Fontan, and Toswel in London; Joly, Ribaud, Kiaro (according to Max Dessoir), Varges, and Herzog. The last-named had acquired his knowledge of animal magnetism from Colonel Bruce-Bey, of Stockholm, who had long resided in the East. Yet in spite of these individual efforts, and although distinguished men of science like the well-known physiologist Carpenter, as well as Laycock, James Simpson, Mayo, and others, confirmed the facts, hypnotism found no general acceptance either in medicine or in any other branch of science. It remained an isolated phenomenon.

The doctrine of animal magnetism was not, however, entirely forgotten: in lay circles, at least, it retained many permanent adherents. This was particularly the case in England, for Carl Scholl tells us that, somewhere about 1852, his attention was drawn to the meeting-places of mesmerists by bills and notices posted at the street corners. One Haddock, by name, acquired a certain amount of distinction among English mesmerists by reason of a book which he wrote on the phenomena of vital magnetism. This work was translated into German by Merkel, who, in a preface, advised his readers to send either their autograph or a lock of their hair to Haddock,

who would then be able to put them *en rapport* with Emma, the clairvoyante. Dr. George Barth, also, was a well-known London magnetizer. A similar state of affairs obtained on the Continent. In Paris the doctrine of animal magnetism continued to flourish extensively in lay circles. Scholl, whose writings have already been mentioned, describes the public sessions of one of the societies of mesmerists. In Germany also a few individual adherents of the old doctrine were always to be found, and from time to time some magnetizer appeared there in public—as, for example, the French magnetizer, Laurent, who, with Miss Prudence, the lady upon whom he operated, created a certain amount of excitement in Wiesbaden in 1850.

In America, meanwhile, animal magnetism had gained adherents; New Orleans was, for a long time, its chief centre. Kiernan has collected a number of historical notes treating of the position of American psychiatry in respect to animal magnetism. In his book on the *Treatment of Insanity*, which appeared in 1846, Galt, of Virginia, expresses very sceptical views as to the value of animal magnetism in the treatment of mental disease; and Brigham states that in 1841 he experimented on five patients, but without success. Mitchell also, whose reports are more detailed, found only one-seventh of the persons experimented on responsive to mesmeric manipulation. No serious case was cured, though it was found that temporary improvement could be obtained in some nervous states. In 1843, Estes, of Columbus, Miss., and others made similar experiments.

A few years later than Braid, Grimes appeared in North America, and, independently of Braid, obtained like results. The states produced by Grimes were called electro-biological. Among his adherents Dods and Stone must be mentioned. Electro-biology was introduced into Europe by Darling and a French physician named Durand de Gros. The latter had lived in America, where he wrote under the pseudonym Philips. He returned to Europe in 1853.

Braid's discovery was first made known in Bordeaux by Azam, in 1859. Encouraged by Bazin and mocked at by others, Azam made some hypnotic experiments; he communicated the results to Broca, in Paris. The latter discussed hypnotism before the *Académie des Sciences*. It was soon made use of to perform painless operations; Velpeau, Follin, and Guérineau in particular made experiments. Other physicians, Demarquay

and Giraud-Teulon, as well as Berend in Berlin, Pincus in Glogau, and Heyfelder in St. Petersburg, showed the slight value of hypnotism for surgery. In consequence of this it found no acceptance in medicine at that time. The experiments of Laségue in 1865, when he obtained cataleptic phenomena by closing the eyes, aroused no particular interest. In the following years we only find here and there, especially in some English scientific works, brief notices of hypnotism or mesmerism, as in Thomas Watson's *Lectures* (1875), in Tanner's *Practice of Medicine* (1875), and in Quain's *Dictionary of Medicine*, in which Bastian wrote on Hypnosis, and urged the need for further investigation (Felkin).

Meanwhile, Liébeault, who later removed to Nancy, had made himself familiar with the phenomena of hypnotism and animal magnetism. The last he endeavoured to refute, and he became the real founder of the therapeutics of suggestion. His book published in 1866 (*Du Sommeil, etc.*), which is even to-day very well worth reading, contains his ideas; it remained little known, and the author was much laughed at.

Suggestion had undoubtedly been applied before Liébeault's time, in the waking as well as in the hypnotic condition. We find in the literature of mesmerism many indications that the followers of animal magnetism frequently regarded speech as the bearer of magnetism; for example, Ennemoser and Szápary. As Bramwell very properly reminds us, there is frequent reference in Braid's writings to the great influence of verbal suggestion. It was, however, Liébeault who first utilized suggestion methodically. It must not be forgotten also that it was through Liébeault, as we shall see, that Bernheim was induced to turn to the study of hypnotic suggestion, while through Bernheim the attention of many other investigators was called to hypnosis. The later historical development of hypnosis begins with Liébeault. That some earlier writers, however, knew much that he more fully worked out cannot be doubted.

Independently of Liébeault, Charles Richet came forward in Paris in 1875 to contend for the real existence of hypnosis, which he called "Somnambulisme provoqué." In the year 1878 Charcot began his demonstrations, in which he directed attention to the physical states of hystero-epileptics during hypnosis; in 1881 Paul Richer published, in his book on *La grande Hystérie*, many experiments performed on the lines of Charcot. Among the pupils of Charcot I may name, in addition, Binet, Féré, Gilles de la Tourette, Babinski, Barth, Bourneville, Regnard.

About 1880 many investigators in Germany—particularly Weinhold, Opitz, and Rühlmann in Chemnitz, Heidenhain and Berger in Breslau—occupied themselves with the question; and Rosenbach, moreover, pointed out the psychical character of the phenomena of hypnosis at about the same date. Other contemporary investigators to be mentioned are:—Möbius, Benedikt, Eulenburg, Senator, Adamkiewicz, Börner, Meyersohn, Bäumler. The investigation of hypnosis in animals, published by Czermak in 1872, and after him by Preyer, aroused no lasting interest. The movement of 1880 also soon ceased, although Preyer often pointed out the importance of hypnotism.

Many opinions of early investigators in the field of hypnotism have been refuted in later times. Of some authors scarcely a single conclusion has been left standing. Even Charcot must be reckoned among these. Nevertheless, I consider we owe thanks to all the serious early investigators of hypnotism, on account of the attention they drew to the matter, even if all their conclusions are refuted. It is much easier to push on a work which is already well advanced than to lay the first stones on which the structure must be erected. Among the investigators who, in my opinion, deserve enduring gratitude, although a greater part or nearly all their results are surpassed by later workers, must be reckoned Charcot and Heidenhain. It will, I am sure, be admitted, that recent investigators have a right to demand exemption from spiteful attack and calumny on the part of those of their forerunners whose opinions they have refuted. Benedikt, for example, though an early inquirer into the phenomena of hypnosis, has offended in this respect ever since his views were upset by the Nancy school of investigators.

The researches of Charcot likewise had little effect upon the further pursuit of the inquiry—as little as had the book of Prosper Despine on somnambulism, which appeared in 1880. It is true that in some hospitals investigations were undertaken, particularly by Dumontpallier in Paris, by Pitres in Bordeaux, also by Ladame in Geneva, and later by Binswanger in Jena. These researches were, however, sporadic.

Only when a second medical school in France—that of Nancy—approached the subject did the interest become more general. Professor Bernheim, of Nancy, who, incited by Dumont, had studied the question with Liébeault, and had accepted the latter's views, published a book, *De la Suggestion, etc.*, in 1886. He gave in it examples of the curative effects of hypnosis, the phenomena of which, he says, are entirely of a psychical nature. Besides which, at Nancy, Beaunis worked at the physiology of hypnosis, and Liégeois at the forensic side

of the question. Then followed in France the contest between the schools of Charcot and of Nancy, in which the latter, however, has gained ground more and more, and has at length received just recognition at the hands of the Academy of Medicine, Paris, thanks to the exertions of Proust. Whereas Charcot, and Heidenham in Breslau, emphasized the importance of the somatic symptoms, the Nancy school and its adherents held that all the phenomena were caused by suggestion; according to the latter, hypnosis presents no purely physical changes which are not produced by suggestion. As already stated, the importance of the psychical element in hypnosis, particularly in respect to suggestion, continued to gain adherents, and Charcot's views were almost totally superseded. In only a few rare instances—for example, Schaffer of Buda-Pesth, Paul Magnin of Paris, Micheline Stefanowska in Russia—do we find views expressed which approximate to the somatic conception of hypnosis put forward by Charcot and Heidenhain. One thing is certain, Charcot never adequately recognized the importance of suggestion.

People began to busy themselves with hypnotism in other countries as well as in France, chiefly on the lines of the school of Nancy. It is true that, as has already been mentioned, the study of hypnotism had been begun in various countries in connection with the work of Charcot. As, however, in consequence of the rather one-sided standpoint of these investigations, the different inquirers failed to find any lasting satisfaction, even the name of Charcot was powerless to establish a lasting interest in the study of hypnotism. Only when the school of Nancy created a surer basis for hypnotism by a profounder psychological conception could people begin to devote themselves on a larger scale to the study of it. Among French investigators I should name A. Voisin, Jules Voisin, Bérillon, Déjerine, Luys, Cullerre, Nizet, Laloy, Regnault, Paul Farez, Lebailly, Grasset. Numerous other experimenters occupied themselves with the question, and even those who had at first considered the experiments of Charcot to be of higher value later on turned in large numbers to the school of Nancy. Of course, attention was not directed exclusively to medical questions; the psychological and forensic aspects of hypnotism were studied as well. Among those investigators who worked at the physiology of hypnosis I must mention Pierre Janet, Ribot, and in more recent times Paul Farez and Regnault.

On the forensic side we have Mesnet, who investigated offences against morality committed on persons in the hypnotic state, and many others to whom I shall refer in the section dealing with the legal aspects of hypnotism. How existing French law affects the practice of hypnotism has been worked out by Halgan in his graduation thesis, *L'Hypnotisme devant la loi*; Paris, 1901.

Hypnotism found an entrance to many other countries. In Switzerland it gained numerous adherents, among whom I may mention Bleuler, Ringier, Bonjour, Liengme, but more particularly Forel, who, as the most determined follower of the Nancy school, enthusiastically supported the theory of hypnotic and non-hypnotic suggestion. In Belgium the eminent psychologist Delbœuf, of Liège, smoothed the way for it. Several lawyers, like Bonjean and Mallar, interested themselves in the new science, especially in its forensic aspects; and among physicians may be mentioned Moreau, Velsen, Maës, Crocq, and Spehl. Perhaps greater progress was made in Holland, where Stephan's objective treatment of the question threw light upon a subject which might have been obscured by Cattie's opposition. Numerous physicians made use of hypnotism in Holland for curative purposes. Among the Dutch pioneers of hypnotism we must reckon Renterghem and Eeden, of Amsterdam, and de Jong, of The Hague. Of the numerous Dutch physicians who made practical use of hypnotism, I may mention Reeling Brouwer, of The Hague; Breuking, of Scheveningen; Hekma, of Groningen; and Stigter, of Leyden. Other advocates of hypnotism to be named are Deventer and Jelgersma. At the second Congress of Hypnotists, held in 1900, Renterghem published a list of Dutch physicians who practise hypnotism for curative purposes, from which I have extracted some of the details given above. In Denmark, Sweden, and Norway we find also a series of inquirers—Johannessen, Sell, Fränkel, Carlsen, Schleisner, Reiersen, Velander, Geijerstam, and most particularly Wetterstrand, of Stockholm, who uses hypnotism therapeutically to a very great extent; also Lehmann, of Copenhagen, an earnest advocate of the psychology of hypnotism; also in Russia, where Stembo, Michailow, Tokarski, Bechterew, Rossolimo, Meyer, Rybakoff, Orlitzky, Brodowski, Repman, Matveef, and Wiazemsky of Saratoff, are to be mentioned, although the Government have put many difficulties in the way of studying hypnotism. In Greece and Spain, where

Pulido used suggestion therapeutically many years before Bernheim, hypnotism has gained in importance. E. Bertrán Rubio, of Barcelona, has published a comprehensive work entitled *Hypnotismo y Suggestion*. Among Italian investigators may be mentioned Lombroso, Belfiore, Morselli, Tonoli, Ottolenghi, as well as Olinto del Torto, the editor of *Magnetismo e Ipnotismo*, Ellero, Cantani, and Ehrenfreund.

In England there exists a scientific society—the Society for Psychical Research—which, besides examining certain mystical phenomena, also does much to promote the study of hypnotism. The members of this society are all men of high scientific and social standing, of whom I need only mention Ramsay, Stanley Hall, and the late Professor Sidgwick. Gurney and Frederick Meyers must here be especially mentioned as promoters of the study of hypnosis in the Society for Psychical Research. Before this Hack Tuke had often called attention to hypnotism in England. He was, however, unable to excite any enduring interest in the matter; nor were Gamgee, who in 1878 reported on his observations of Charcot's experiments, and Whitehead, who in 1885 wrote concerning some experiments at Manchester, more successful (Felkin). At the same time, as Felkin remarks, numerous works dealing with hypnotism appeared from time to time in England; as by Gasquet in 1887, by Karl Grossmann in 1888, by Foy, and especially by Preyer, who spoke on hypnotism at the British Medical Association meeting in 1880. Among more recent investigators in England I may mention Lloyd Tuckey, of London, who has made many therapeutic applications of hypnosis on the lines of the Nancy school; as well as Kingsbury, Hart, Vincent, and Coates. Special mention must here be made of Bramwell, who by a series of contributions extending over a number of years, and more particularly by his recent book *Hypnotism*, has done so much to promote the study and therapeutic application of hypnosis. In Scotland, Felkin has done much for hypnotism in a small but careful and interesting book on the question; also George Robertson, who advocated the use of hypnosis in the treatment of mental diseases. As Bramwell mentions, Sir Francis Cruise remained about the only writer on hypnotism in Ireland. At all events, in spite of numerous opponents, among whom Norman Kerr and Burney may be named, hypnotism has thus won citizenship in England. Moreover, at the Birmingham meeting of the British Medical

Association in 1890, a committee of physicians was appointed to test hypnotism psychologically, physiologically, and therapeutically. This committee, which included among its members Hack Tuke, Langley, Needham, Broadbent, Kingsbury, and Clouston, presented its report in 1892. In this report not only was the reality of hypnotism recognized and its symptoms described, but hypnosis was warmly recommended for therapeutic purposes, especially for insomnia, pain, and numerous functional disorders. The results in dipsomania were mentioned as peculiarly encouraging.

In other quarters of the globe, especially in America, it had also awakened great interest. Beard had already long ago occupied himself with the question. Unfortunately, his investigations are not known to the extent they deserve. In 1881 Beard also attempted, at the International Congress of Physicians in London, to interest European physicians in hypnotism. The results he obtained were the opposite of those he desired, as may be seen from the writings of various eye-witnesses, Mortimer Granville, Donkin, and Crichton Browne, in the *British Medical Journal*, although the first-named, Granville, at the International Medical Congress in London in 1881, had referred to the possibility of hypnotizing the insane. Although Beard's exertions were at first fruitless, at a later period many in America occupied themselves with the problem of hypnosis. Among recent investigators may be named Funkhouser, Hamilton Osgood, William Lee, Howard, Pope, Gerrish, Fitzgerald, Clark Bell, Hulst, Hammond, Dana, Vermeren, Axtell, Booth. Sidis made special investigations into the psychology of suggestion as a means of studying personality, and in this was to an extent under the guidance of William James, of the University of Harvard; he published a monograph on the *Psychology of Suggestion*, but the work is not based entirely upon observations made upon persons in the hypnotic state. In various universities and colleges of the United States the study of hypnotism has been carried on; for example, at Wellesley College, as Whiton Calkins reports. A scientific association, the American Society for Psychical Research, now affiliated to the English Society, has also been formed in the United States. In several of the South American States serious inquirers have turned to the study of hypnotic phenomena; for example, Octavio Maira and David Benavente in Chili; Barreto, Fajardo, and Jaguaribe in Brazil. In Cuba

the physicians Villamonga and Diaz may be named. Damoglou, of Cairo, also has studied hypnotic suggestion.

As was to be expected, hypnotism very soon began to arouse greater interest in Germany. Although the investigations incited by the exhibitions of Hansen had left no lasting impress, yet from time to time individual inquirers, such as Obersteiner of Vienna, Fränkel of Dessau, and Möbius, had endeavoured to draw attention to hypnotism in Germany by clear and impartial reports. Experiments in therapeutics had also occasionally been made; for example, by Creutzfeldt, Wiebe, E. L. Fischer, Berkhan. But no general interest was aroused until 1887, when I delivered an address on the question before the Medical Society of Berlin, in which I related my own experiences and certain observations I had made at Nancy. Certainly the address was not at all favourably received, and two men, who were obviously only acquainted with Charcot's investigation and not with those of the Nancy school, opposed me. Ewald objected altogether to such a method of treatment being designated medical, and Mendel attacked hypnosis on account of its manifold dangers. As a natural consequence an exaggerated mistrust of hypnosis was engendered, and only gave way when a more objective conception of the question made itself felt. This occurred when Forel, who had been giving instruction in hypnosis in Switzerland in connection with the Nancy school, insisted on the importance of the subject, and at the same time especially denounced the manner in which Ewald and Mendel sought to settle the question.

A really stirring activity now set in in Germany also. The importance of suggestion for hypnosis was recognized; and many physicians, following the example of the Nancy school, commenced therapeutic experiments with hypnosis in Germany. Among them may be named Sperling, Nonne, Michael, Hess. I must further especially mention Schrenck-Notzing, who was one of the very first pronounced advocates for the therapeutical application of hypnosis; also Hösslin and Baierlacher, who discovered the reaction of degeneration, but who unfortunately died shortly after turning his attention to hypnotism. Among those who, in Germany, either employed or recommended the therapeutical application of suggestion may also be mentioned Corval, Schuster, Hirt, Ad. Barth, Brügelmann, Hecker, Max Hirsch, Scholz, Gerster, Stein, Seif, Tatzel, Stadelmann, Placzek, Gumpertz, Delius, Steiner, Schütze, Herzberg, Sjöström, Steg-

mann. We must also remember Lœwenfeld, on account of his various contributions to the therapeutical side of hypnosis and kindred questions, as well as for his detailed treatise on hypnotism. I must here mention several other Berlin physicians who, by their individual investigations, furthered the therapeutical side of the question, and by so doing were able to illuminate the broader domain of psychology and psychotherapy—for example, Vogt and Hirschlaff. The former, aided by several of his pupils, notably Brodmann, essentially improved the technique of medical hypnotism. Others to be named are Georg Flatau, of Berlin; Georg Wanke, of Friedrichroda; Hilger, of Magdeburg; and Dölken, of Marburg.

We find, likewise, a number of physicians in Austro-Hungary active in the same field. Here Obersteiner continued his earlier investigations, but special mention must be made of Krafft-Ebing and his pupil Alfred Fuchs, who, like Schrenck-Notzing, laid stress on the importance of hypnotic suggestion in the treatment of cases of sexual perversion. I may also mention Freud and Breuer, who recommend a peculiar method of treatment, the cathartic; also Frey, Schnitzler, F. Müller, Donath, Mosing.

Ziemssen, Nothnagel, Seeligmüller, Benedikt, Köberlin, Richter, Schultze, Windscheid and others set their faces most decidedly against the therapeutic use of hypnosis. Some emphasized its dangers, while others gave prominence to its uselessness. The cursory nature of the work upon which many of these assumptions were based was soon demonstrated. For example, it was shown by Schrenck-Notzing that Friedrich, a pupil of Ziemssen's, who had particularly animadverted on the dangers of hypnotism, was himself "a transgressor against the most elementary demands of those who advocated hypnotherapeutic interference in the treatment of disease."

Putting aside the numerous works which deal exclusively with hypnotism, we find this subject discussed in many books chiefly concerned with other themes. I may mention the various works on nervous and mental diseases. Hirt and Möbius, likewise Gowers and Oppenheiner, have inserted more or less comprehensive chapters on hypnotism in their works. The same is true of many writers on psychiatrics; for example, Krafft-Ebing and Kraepelin, both of whom mention the therapeutic value of hypnosis in their books. The value of hypnosis when other means fail is admitted by Sommer

though Kirchhoff, in his *Psychiatrie*, treats hypnosis as being more a psychological phenomenon.

We also find hypnotism discussed in works dealing specially with nervous diseases; in Müller's *Handbuch der Neurasthenie* there is an extensive chapter by Schrenck-Notzing on hypnotic and especially suggestive therapeutics. Borel, also, in his book, *Nervosisme ou Neurasthénie*, deals briefly with treatment by suggestion, which he conditionally allows; as also does Lœwenfeld in his book on hysteria and neurasthenia. Hypnotism is fully dealt with by Pitres in his work on hysteria and hypnotic treatment, by Binswanger in his great treatise on the same subject, in which he justly only covers a portion of psycho-therapeutics.

Occasionally we find hypnosis thoroughly discussed in other medical works; for example, in Eulenburg and Samuel's comprehensive treatise, *Allgemeine Therapie*, in which the section on psycho-therapeutics is written by Ziehen. The question is similarly treated in Eulenburg's *Real-Enzyklopädie*, and in the *Enzyklopädischen Jahrbücher*, which are supplemental to the former. For these Corval and, later on, Schrenck-Notzing have contributed diligent and detailed criticisms of the most recent works on the subject. In Penzoldt and Stinzing's *Handbuch der Speziellen Therapie*, the section in question has been prepared by Liebermeister, who writes with reserve on hypnosis, and gives preference to suggestion applied when the patient is awake.

Other authors worked at the particular subjects which have a relation to hypnotism without laying special stress on its therapeutic value, as we have already seen was the case with many French investigators. As far as Germany is concerned the works of Lilienthal, Rieger, Drucker, Heberle, Loos, and Neumeister must here be named, which inquired into the legal side of the question. Krafft-Ebing published an extremely detailed experimental study of two cases; Max Dessoir compiled a valuable bibliography of modern hypnotism, with appendix; further, Nussbaum, Nonne, Bleuler, Otto Effertz, Hückel, Kocks, Maack, D. Weiss, Sallis, Binder, Dreher, Moravcsik, Hebold, Hitzig, William Hirsch, Straaten and Trömner must be named. A short but useful book on hypnotism was long ago produced by Minde, who rescued from oblivion many little known facts contained in ancient and modern literature.

Here, too, we must specially mention various works on psychology in which hypnotism is also discussed, such as Wundt's *Grundzüge der Physiologischen Psychologie*, and the same author's *Grundriss der Psychologie*; likewise James's books on psychology; also the new edition of Volkmann's *Lehrbuch der Psychologie* by Cornelius, and the works of Paul Carus, Külpe, Hœfler, Münsterberg, Lipps, and finally Heilmann and Jahn's *Psychologie als Grundwissenchaft der Pädagogik.*

It would be altogether a mistake to fix the therapeutic value of hypnosis as the standard by which it is to be judged, for that would lead to the neglect of other factors; for example, the psychological importance of the subject. As a matter of fact, a series of investigators have recognized the great value of hypnotism, particularly in this direction—above all, Krafft-Ebing, Forel, Max Dessoir, and Ribot. In Germany many scientific societies have made valuable contributions to this branch of the subject. Such are the Psychological Society in Munich and the former Society for Experimental Psychology in Berlin, to which we owe a series of remarkable works by Max Dessoir, Bastian, Hellwald and Bentivegni. Later on, Vogt, of Berlin, called special attention to hypnosis as a means of psychological research. By some psychologists—Wundt, for example—it is denied that hypnotism is of value in experimental psychology. At the same time Wundt admits that, like dreaming and insanity, it is a proper object for psychological observation; but its value to the psychologist is not equal to its high value to the physician.

I may here briefly mention that considerable space is devoted to hypnotism in many philosophical works, and in such as deal with the history of civilization, but particularly in those treating of occultism and superstition. To the latter category belong the works of Lehmann and Hennig.

The theologians have not been able to leave hypnotism alone altogether, although they sometimes attribute it to the agency of the devil. Among the authors who have dealt with hypnotism from the standpoint of the Church, I may name Franco, Méric, Finlay, Haas, and Cocconier, of whom the last named, though condemning hypnotism as a rule on moral grounds, justifies its use in a good cause. I may further mention that Rohnert, an Evangelical pastor, condemns hypnotism from the Christian standpoint even when it proves

an effective therapeutic agent, and Schütz describes it as illegal and objectionable on rational grounds. On the other hand, Ziegler would grant its use to physicians expert in hypnosis.

The domain of suggestion, though not identical with hypnosis, is intimately and historically connected therewith. Formerly the two were studied together, but of recent years suggestion has been dealt with separately. It is a matter of common knowledge that treatment by suggestion has been developed from the therapeutical employment of hypnosis. I may here mention one of the larger treatises in which the important part played by suggestion in the causation and cure of disease is discussed—*Les Phénomènes de Suggestion et d'Auto-Suggestion* (1903), by Lefèvre, a Belgian army surgeon. Other inquirers also have investigated the domain of suggestion apart from medical considerations. The general significance of suggestion for social life as well as for art and science, has been treated in *Die Psychologie der Suggestion* (1892), by Schmidkunz, assisted by Gerster, a physician. Although the book betrays mystical tendencies, it is replete with valuable suggestions and historical references. In a small work entitled *Personality-Suggestion*, published in 1894, J. Mark Badwin called attention to the difference in the suggestive influence exerted by different persons on children. The educational value of suggestion in general has been discussed by T. Félix Thomas, in his book, *La Suggestion, son Rôle dans l'Education*, published in 1895; and the general social importance of suggestion by Becterew. Grohmann has dealt with suggestion by letter, and demonstrated the dangers of character-reading by advertizing graphologists. In 1900, Binet published a book, *La Suggestibilité*, in which the susceptibility of children to non-hypnotic suggestion is discussed; the author also furnishes historical data on the gradual differentiation of suggestion from hypnotism, and at the same time demonstrates that the classification of personal characteristics, as given by Tissié, Bolton, and Lapouge, is based entirely upon suggestibility. Numerous other investigators have dealt with suggestion and suggestibility from the psychological point of view; among them I may mention Hellpach, who dwells upon the connection subsisting between suggestibility and hysteria. But it is to Lipps that credit is particularly due for having, in a lucid and stimulating dis-

course, attempted to give a psychological basis to, and a delimitation of, the problem of suggestion. I must here again mention the American investigator, Boris Sidis, whose work, *The Psychology of Suggestion*, is directed to the elucidation, not only of hypnotic, but more especially of non-hypnotic suggestion.

In order to facilitate a general discussion of the most important questions in the domain of hypnotism, a congress met in Paris in August 1889, at which nearly all civilized nations, including Germany, were represented, and at which many important matters were cleared up. In general, it may be said that the views of the Nancy school carried the day. A second congress met in Paris in 1900. Raymond, Charcot's successor, attempted in his introductory address to represent the congress as a reconciliatory meeting of Charcot's school with that of Nancy, and many speakers—Bérillon, Crocq, Magnin—emphasized, on the lines of Charcot's teaching, the similarity subsisting between certain phases of hysteria and many of the phenomena of hypnosis. Still, on the whole, the views of the Nancy school prevailed at this congress. More recently many congresses and scientific assemblies have occupied themselves with hypnotism. Only a few need be mentioned. At the Olten meeting of the Swiss Medical Association in 1888, Forel delivered an address on the therapeutics of suggestion. At the International Congress for Psychiatrics, held at Paris in 1889, Ladame spoke of the therapeutic value of suggestion, but was opposed by Benedikt. At the Congress of Russian Physicians in St. Petersburg in 1889, Tokarski and Danillo introduced an interesting discussion in the neurological section. In 1890, Bérillon discussed the therapeutics of suggestion at the International Medical Congress in Berlin. At the meeting of the British Medical Association held at Bournemouth in 1891, A. Voisin addressed the Section of Psychiatry on the criminal importance of hypnotic suggestion; and at the International Medical Congress, Moscow, 1897, Bernheim called attention to the importance of hypnotism and suggestion for medical jurisprudence. At the three International Congresses for Experimental Psychology, interesting communications on hypnotism were brought forward; the London Congress of 1892 was divided into two sections, one of which was specially devoted to hypnotism, F. Myers being its secretary, and Eeden read

a long paper on the principles of psycho-therapeutics. Hypnotism was also discussed at the third International Congress of Psychology, in Munich in 1897; and at the Congress of Psychology held in Rome in 1905, both hypnotism and suggestion were dealt with. At the Congress of Criminal Anthropology held in Brussels in 1892, hypnotism was discussed. In 1894, at the International Medical Congress in Rome, Hirt introduced the subject of hypnosis; Sollier and Benedikt appeared as opponents, and the chief advocates of the value of hypnotic therapeutics, besides Hirt, were Hitzig and Bérillon. In 1897, a Congress of Neurology, Psychiatry, Electro-therapeutics, and Hypnology met in Brussels. Liégeois addressed the congress on criminal suggestion, Bramwell on the therapeutic value of hypnotism, Castelain on suggestion in everyday life, and Aimé on the value of hypnotic suggestion in the waking state. The question of using hypnotism and suggestion in the treatment of children considered criminals, but in reality psychopathic, was discussed by Jules Voisin and Bérillon at the Congress of Criminal Anthropology held at Amsterdam in 1901, and the value of hypnosis and suggestion in relation to the psychology of crowds was outlined in the reports handed in by Jelgersma and Sighele. At the Congress of the South-Western Association of German Alienists which was held in Stuttgart in 1902, a discussion on hypnotism and psycho-therapeutics followed on an address by Franck and a communication by Bezzola, in which Krehl and Hecker took part. Hypnotism was also discussed at the Thirteenth Congress of French-speaking Alienists and Neurologists in Pau, in 1904.

Under the title of *La Société d'Hypnologie et de Psychologie*, a scientific society was formed in 1889, in Paris, for the special cultivation of hypnotism; its first president was Dumontpallier, a post now held by Jules Voisin. A similar society has recently been founded in Moscow. The existence of other scientific societies interested in hypnotism may be briefly mentioned.

Various journals are now devoted to hypnotism. While in France the *Revue de l'Hypnotisme*, edited by Bérillon, has been published for nineteen years, in Germany a *Zeitschrift für Hypnotismus* was founded in 1892, but ceased to appear on the completion of the tenth volume in 1902. Simultaneously with the demise of the *Zeitschrift*, the *Journal für Psychologie und Neurologie* began to appear; it is published by Forel and

Oskar Vogt, edited by Brodmann, and treats the domain of hypnotism from a broad point of view. It embraces a wider field than the *Zeitschrift*, taking in those special psychological questions which are of interest to the neuro-biologist. Certainly a succession of interesting and valuable articles has appeared in these periodicals. There have been, of course, other periodicals devoted to hypnotism, but most of them have gone under after a brief existence. Another French journal, the *Annales de Psychiatrie*, must here be mentioned. In Italy we find *Magnetismo e Ipnotismo*, a journal edited by Olinto del Torto, which was at the same time the organ of an Italian scientific society occupied with the questions of animal magnetism and hypnosis. America also has produced numerous periodicals dealing with hypnotism, but like many published in Europe, they cannot be accredited with much scientific worth. When founding the *Zeitschrift für Hypnotismus* Sommer referred to the danger lest the connection between hypnosis and psycho-pathology generally be lost sight of. The manner in which Vogt and Bérillon have edited, and continue to edit, their respective German and French journals shows that this assertion is not well grounded.

Hypnotism has, moreover, been frequently made the study of medical students; from 1888 to 1890 lectures were delivered about it in Berlin by the late Professor Preyer, and in Freiburg-in-Baden by Münsterberg. It was more frequently mentioned in lectures on other subjects, as, for example, by Jolly in his clinical discourses on psychiatrics. But as a rule hypnotism did not maintain a prominent position in university lectures, any more than psychology, psycho-therapeutics, and medical psychology in general. Hypnotism and suggestion were for the most part taught in classes held independently of the universities. Among lecturers on the subject in Berlin I may mention Hirschlaff, Vogt, Georg Flatau, and J. Grossmann.

In the winter of 1904-05 I myself gave a series of lectures on psycho-therapeutics in connection with the course promoted by the central committee for post-graduate students; in those lectures I dealt with the methods of treatment by hypnosis and suggestion.

In other countries also lectures on hypnotism have been given at universities, as at Zürich by Forel. In Paris, Bérillon has delivered lectures on hypnotism in conjunction with Jennings and Farez. Lehmann, a distinguished psychologist

at Copenhagen, has, in his lectures there, dealt with the psychological significance of hypnotism. Joire has lectured on hypnology in Lille, and Tokarski in Moscow. In short, we find universal and strenuous endeavours to throw the light of science on hypnotism, and attract fresh investigators to this field of inquiry.

Much documentary evidence in favour of hypnotism has been collected in Germany. The propriety of utilizing hypnosis in the production of a literary work has been discussed by Franzos, who collected and published a number of opinions on the subject. Unfortunately, Franzos, to further his object, made use of many highly-esteemed names in the German professional world, among others that of Helmholtz, who openly admitted that he had never troubled his head about the matter. At the same time we must not lose sight of the instructive and detailed work which we owe to Forel, Eulenburg, Krafft-Ebing, and Preyer. Another collection of documents, dealing more especially with the therapeutic side of the question, was published by J. Grossmann. This, I am sorry to say, omits some names—for example, Preyer's—but several of the papers contained in it are of great value. In 1902 the Prussian Minister of Education invited the Council of Prussian Physicians to institute an inquiry into the therapeutic value of hypnosis. At the same time the presidents of the various governmental districts were requested to report on the extent to which hypnosis was carried on by unqualified persons.

It cannot be denied that hypnosis is frequently practised by laymen, and that, too, in a very unscientific manner. I need only point to the public exhibitions of hypnotism which take place from time to time in spite of all prohibitive measures. Advertisements announcing instruction in hypnotism constantly appear in the daily press, both at home (Germany) and abroad. There is an American institute which has, undoubtedly, been the greatest offender in this respect; for years past its widespread advertisements have held out to prospective pupils a means of learning the art of hypnotizing, whereby they should be able to influence their fellow-men in wondrous wise. This institute offers its instruction in written and in printed form. But putting on one side this unscientific treatment of hypnotism, we must not forget that numerous popular, and at the same time excellent, treatises have appeared on the subject, and popular exposition must not be confused with unscientific

exploitation. Many of the authors of these popular works are scientists of some repute, as, for instance, Eisler, the author of *Zür Psychologie der Hypnose*, an essay which appeared in a South-German paper; there are many other popularizers of hypnosis who could be mentioned, but I need only name such men as Max Hirsch, Sjöström, and Newbold of Philadelphia.

Hypnotism has not been without its votaries in literature. In former days animal magnetism formed material for romances. I need only call to mind a short play by Iffland, called *Der Magnetismus;* or to the writings of Alexandre Dumas and Balzac, the latter an enthusiastic upholder of animal magnetism, as evinced in his story of Ursule Mirouët. According to Witkowski, Ben Jonson's comedy, *The Magnetic Lady*, was produced one hundred years before Mesmer was thought of, and in it a somnambulistic *clairvoyante* played a prominent part. Novelists of a later date have also brought hypnotism into play. This we see in Claretie's *Jean Mornas*, Belot's *Alphonsine*, and Achille's *Un Raffiné.* Others to be mentioned are Epheyre and Valdès, the latter for his *La Prise du Regard.* But without doubt, George du Maurier's *Trilby* caused most stir. Of German productions of a similar nature I may mention Meding's *Unter fremden Willen* and Pröll's *Moderner Totentanz.* Haas tells us that Catholic literature has been influenced by hypnotism, but according to our authority the *modus operandi* seems to have been somewhat incomprehensible. Hypnotism has often been utilized by playwrights; for example, in Sardou's *Sorcière*, a piece in which Sarah Bernhardt appeared. Other plays to be mentioned in this respect are Kurt Abel's *Der Hypnotiseur* and Dr. Feodoroff's comedy, *Die Hypnotische Suggestion*, a play produced in 1896 for a Russian charity, and in which all the doctors connected with the fund appeared. It stands to reason that caricaturists and satirists have not failed to make use of the humorous side, and certainly it would be difficult to find a subject lending itself more readily to caricature. This fact has kept many a person from studying hypnosis. A number of pretty songs depicting hypnotism from a humorous point of view is to be found in Korb's well-known *Song-book for German Physicians and Scientists.*

In art, also, hypnotism has played a certain part. Charcot and Richer in their work, *Les Démoniaques dans l'Art*, have given illustrations depicting attacks of hysteria, and considering

the close connection between hysteria and hypnosis maintained by those authors, it is not surprising to find them attempting to establish a similar connection between art and *grand hypnotisme.* I may here mention that in the Paris Salon, not long since, a picture was exhibited by Brouillet called "Une Leçon clinique à la Salpêtrière"; and I will take this opportunity of calling to mind that when mesmerism flourished, it was made use of in illustrative art, but more particularly for satirical purposes. K. F. H. Marx mentions in his treatise on the connection of illustrative art with the art of healing, a work by Chodowiecki entitled, *A Magnetic Manipulation*, and a satirical sketch of Mesmer's magnetism by Fr. Sergent. Of a somewhat different nature is the connection between hypnosis and art, as recently discussed by Rochas and Otto Schultze, with whom Schrenck-Notzing and Lœwenfeld agree to some extent. The three last-mentioned inquirers, writing in reference to the exhibitions given by the "sleep-dancer" Magdeleine, give prominence to the question as to how far the power of expressing the emotions during hypnosis may be utilized for artistic purposes.

Certainly all the references to hypnotism and the numerous works on it, mentioned in the last paragraph, demonstrate the general nature of the interest which has been brought to bear on the question; and it may not be superfluous to draw attention to the fact that hypnotism has not remained without influence on our daily speech. When we say that a man seems "hypnotized," we mean that his whole interest is so concentrated on one point that he neglects every other important question. Similarly we employ the word "suggestion" to describe the means by which one person may exercise undue influence over another.

We thus see how hypnotism has gained in significance, how it has gradually developed itself out of animal magnetism, and what an important branch of modern science it has become. Finally, it would be an omission on my part if I did not mention that animal magnetism, as distinct from hypnotism, has retained some of its adherents in the scientific world—Ochorowicz, Myers, and Richet. Naturally, I ignore the numerous uncritical and unscientific persons who express a belief in this magnetism.

CHAPTER II.

GENERAL CONSIDERATIONS.

In order to give the reader an idea of the phenomena of hypnotism, it will be best, first of all, to describe a few experiments. The phenomena will in this way be made more comprehensible than by means of any number of definitions.

First Experiment.—I begin the experiments with a young man of twenty. I request him to seat himself on a chair, and give him a button to hold, telling him to look at it fixedly. After three minutes his eyelids fall; he tries in vain to open his eyes, which are fast closed; his hand, which up till now has grasped the button, drops upon his knee. In answer to my question as to how he feels, he replies that he is tired. I assure him it is impossible for him to open his eyes. (He makes vain efforts to open them.) I now say to him, "Your hands are stuck fast to your knee; you cannot possibly raise them." (He raises his hands, however.) I continue to converse with him; I find that he is perfectly conscious, and I can find no essential change in him whatever. I raise his right arm; directly I let go he drops it as he pleases. Upon which I blow upon his eyes, which open at once, and he is in the same state as before the experiment. The young man remembers all that I have said to him.

The only striking thing is, therefore, that he could not open his eyes, and that he felt a certain degree of fatigue.

Second Experiment.—This is a woman of fifty-three. When she has seated herself on a chair I place myself before her; I raise my hands and move them downwards, with the palms towards her, from the top of the head to about the pit of the stomach. I hold my hands so that they may not touch her, at a distance of from two to four centimètres. As soon as my hands come to the lowest part of the stroke I carry them in a wide sweep with outspread arms up over the subject's head. I then repeat exactly the same movements—that is, passes from above downwards, close to the body, and continue this for about ten minutes. At the end of this time the subject is sitting with closed eyes, breathing deeply and peacefully. When I ask her to raise her arms, she raises them only slightly; they then fall down again heavily. When I ask her how she feels, she explains she is very tired. I forbid her to open her eyes. (She makes useless attempts to open them.) Now I lift up her right arm; it remains in the air even after I have let go. I command her to drop her arm. She drops it. I lift it again, and again it remains in the air; upon which I request her to drop her arm, declaring at the same time that she cannot do

it. She now makes vain efforts to drop her arm, but it remains in the air. The same thing happens with the other arm; when I forbid her she is unable to drop it. She cannot pronounce her own name directly I have assured her she is dumb. She only makes movements with her mouth, without producing any sound. I tell her now she can speak. She speaks at once. I say to her, "You hear music." The woman shakes her head to show she hears no music. I wake her by passes from below, upwards, over the surface of her body, turning the back of my hand towards her. She opens her eyes, and can control her movements as before the experiment.

We see here, then, that not only are the eyes closed during hypnosis, but that all sorts of different movements become impossible to the subject when I forbid them.

Third Experiment.—Here is a boy of sixteen, whom I have hypnotized several times. I request him to look me straight in the eyes. After he has done this for some time I take him by the hand and draw him along with me. Then I let go, but our eyes remain fixed on each other's. Then I lift up my right arm. (The boy does the same.) I make him understand by a gesture that he must kneel down. (He does so.) He tries to rise, but does not succeed so long as I look at him, and fix him to the floor by a movement of the hand. Finally, I cease to look at him; the charm is at once broken.

We see here, then, a young man whose movements take the character of imitation, and whose eyes at the same time are wide open and fixed upon mine.

Fourth Experiment.—Mr. X., forty-one years old, seats himself on a chair. I tell him he must try to sleep. "Think of nothing but that you are to go to sleep." After some seconds I continue: "Now your eyelids are beginning to close; your eyes are growing more and more fatigued; the lids quiver more and more, and get gradually closer. You feel tired all over; your arms go to sleep; your legs grow tired; a feeling of heaviness and the desire for sleep take possession of your whole body. Your eyes close; your head feels duller; your thoughts grow more and more confused. Now you can no longer resist; now your eyelids are closed. Sleep." After the eyelids have closed I ask him if he can open them. (He tries to do so, but they are too heavy.) I raise his left arm in the air. (It remains in the air, and cannot be brought down in spite of all his efforts.) I ask him if he is asleep. "Yes." "Fast asleep?" "Yes." "Do you hear the canary singing?" "Yes." "Do you hear the concert?" "Certainly." Upon this I take up a black cloth and put it into his hand. "You feel this dog quite plainly?" "Quite plainly." "Now you can open your eyes. You will see the dog clearly. Then you will go to sleep again, and not wake till I tell you. (He opens his eyes, looks at the imaginary dog and strokes it.) I take the cloth out of his hand and lay it on the floor. (He stands up and reaches out for it.) Although he is in my room, when I tell him he is in the Zoological Gardens he believes it, and sees trees, the water, the children playing, and so on.

We have here a case in which a man is thrown into the hypnotic state by my arousing in his mind an image of the sleep. This manner of hypnotizing was introduced by the Nancy school of investigators, and may be termed the method of Nancy. It is not only possible in his case to prevent the most various movements by a mere prohibition, but I can also control his sense-perceptions. On my assurance he thinks he hears a canary, or hears music. He takes a black cloth for a dog, and believes himself to be in the Zoological Gardens when he is in my room.

But the following phenomenon is still more striking. X. hears all that I say to him, and allows himself to be influenced by me in every way. Yet two other men, A. and B., who are present, appear not to be observed by the hypnotic at all. A. lifts up the arm of the subject; the arm falls loosely down, and when A. desires the arm to remain in the air the subject takes no notice. He obeys my orders only, and is *en rapport* with me only. In order to wake him I now call to him: "Wake up!" He wakes up at once, but only remembers going to sleep; of what happened during the sleep he knows nothing.

Fifth Experiment.—The woman seated on the chair is thirty years of age. She is highly hysterical. Directly she stares intently at any glittering object, and I at the same time speak to her as I did to X. in the last experiment, she falls into a kind of sleep. Her eyes close, and she sits there in an apparently passive condition. When called upon to open her eyes, she attempts to, but cannot so long as I assure her it is impossible for her so to do. I suggest that she is on board ship. (The suggestion takes effect and she immediately feels unwell; she declares she is sea-sick.) I let her sit still for a few seconds, when she suddenly jumps up and asserts that fire has broken out. She can only be calmed with difficulty. Her breathing is very rapid, and every expression of her features betokens dread of the fire. It is not possible to explain how she came by this idea, as nothing leading to it was said to her. You will observe that although the patient has hardly recovered from a state of abject fear, her face now assumes a look of contentment; she begins to laugh, and when asked the cause of her hilarity, explains that a tramcar has just passed, and it was so funny to see an elegantly dressed gentleman stumble in the mud. It is anything but easy to free the patient from auto-suggestive influence, and it has cost me much time and trouble to bring her into a state of quiet, and apparently dreamless, sleep. I now ask her to wake up, but she declares that she is terribly tired and does not want to wake up yet. Further remarks addressed to her lead to her opening her eyes, at first partially, then completely. She was told that on awaking she would be quiet, cheerful, and contented; nevertheless she gives one the impression of being exhausted and worn out. Her eyes close; she sleeps again; it takes an hour before she is thoroughly awake and free from lassitude. She only complains that her head troubles her.

We have here a case of hypnosis in which auto-suggestion plays an important part. It is true that the woman resembles X. of the previous experiment in that she can be influenced by suggestion; but left to herself, even for only a short time, auto-suggestion exerts itself and produces the scenes we have witnessed. Also, it is to be noticed, that the woman cannot be so speedily and surely awakened as in the case of X. Further, on waking, the woman does not feel quite well, and it takes considerable trouble to efface the phenomena set up by hypnosis.

I interrupt here for a time the description of the experiments; I shall describe others in the course of this work, and shall occasionally return to those already depicted. To sum up, in all these experiments, however different they might be, the voluntary movements were always inhibited, that in the last two cases hallucinations of the senses could be induced, and that it was possible for me in all cases to converse with the subject, and we could understand each other. I wished to bring forward these examples in order that the reader might understand to a certain extent, in spite of the absence of living subjects, what different states are included in the idea of hypnosis—how those states are induced and how terminated. The experiments described above are typical; they can be reproduced by any one who knows how to experiment correctly.

At the conclusion of these experiments I add a short terminology, which, however, is by no means complete, as some particular ideas can only be made clear in the further course of the work.

By *hypnosis* I mean the state into which the subjects were thrown during the experiments described above.

Hypnotism is not, as Braid defined it, the name of the state itself, but of the whole science which deals with the phenomena of this state.

A person in the hypnotic state is called a *hypnotic*, or *subject*.

A *hypnotist* is a man who hypnotizes for scientific purposes. A *hypnotizer* is a man who makes hypnotism a profession.

Hypnosigenesis is the technical term for the act of inducing hypnosis. *Hypnosigen* is the means employed. Hypnogen, a term frequently used, only leads to confusion. It is derived from ὕπνος—sleep—and is often used for "sleep-producing." *Hypnosigen* is derived from hypnosis.

The different commands which are given to the subjects in the experiments described, the prompting and persuasion, are called *suggestion*. I shall use the phrase "to suggest" for the giving of these hints and promptings. If the suggestion takes effect it is said, from the point of view of hypnotism, that the subject is under the influence of *suggestion*.

As is often the case in other branches of science, the terminology of hypnotism is very defective. Preyer pointed this out long ago. Many terms, such as post-hypnotic, are hybrids, and have been justly condemned. The second Congress of Hypnotists, held in Paris in 1900, appointed a committee to devise a sound terminology. Oskar Vogt was a member. The work of the committee does not appear to have been over-fruitful, for Dr. Vogt tells me that although five years have elapsed since its constitution he has failed to hear anything of it.

There are several methods of inducing hypnosis, as can be gathered from the above examples. In order to make a systematic survey, we divide these methods into two groups—the mental and the physical.

The mental methods induce hypnosis by giving a particular direction to the subject's imagination; this is done either by concentrating the attention on an arbitrary point (Braid), or by raising an image of the hypnotic state in a patient's mind. The latter is most easily done by speech, as we have seen in the fourth of the examples given above. This process deserves particular attention, as by the use of it unpleasant accompanying phenomena are more surely avoided. As a matter of course, the methods are slightly modified to suit special cases, because individual character plays an incomparably larger part in mental states than in ordinary physiological investigations. It is, of course, quite possible to call up the image of the hypnotic state, and thereby induce hypnosis by other means than speech; upon this fact depends the influence of imitation. The hypnotic state is occasionally induced by the mere sight of others in that condition, as well as by speech. The recollection of earlier hypnosis has the same effect; upon this fact depends the induction of hypnosis by means of letters or by the telephone (Liégeois).

In many instances the best results are obtained by taking the patient by surprise (Sperling, Forel, Eeden, Renterghem).

Vogt prefers a method which he terms fractional hypnosis. This consists in the rapid repetition of experiments, allowing the patient short intervals in which to relate his experiences and feelings, and thereby enabling the experimenter to modify his procedure if necessary. This method is said to have greatly increased the roll of somnambulists. Lœwenfeld also has developed a particular method of his own. The person to be hypnotized is first of all requested to keep on counting from 1 to 100, slowly and gently. Lœwenfeld then proceeds to verbal suggestion on the lines of the Nancy school, but, as a rule, precedes this by a brief fixation of the gaze. He does not straightway suggest the symptoms of fatigue as evinced by the eyes, but rather attempts to bring about a condition of rest and tiredness.

It is certain that these mental influences play a large part in hypnosigenesis. It is equally sure that they suffice in many cases to produce hypnosis, particularly when the person concerned has already been hypnotized. Many even consider the mental factor as indispensable to hypnosis; they hold the opinion that all the other methods mentioned below only succeed when they are of a kind to call up the picture of hypnosis. Yet the other point of view has many adherents, a fact which may easily be overlooked nowadays. Crocq, the most decided exponent of the view that every case of hypnosis is not necessarily the result of suggestion, supports this contention on the corroborative opinions of many other investigators, among whom may be mentioned Charcot, Richer, Pitres, Boirac, Sanchez Herrero, Marot, Azam, A. Voisin, Beaunis, Brémaud, Lajoie, David, and others.

Faria formerly made use of a mental method to obtain hypnosis. After he had strained the attention of the subject as much as possible he called out suddenly, "Dormez!" ("Sleep!"). Liébeault substantially developed and completed this process; Bernheim made it more universally known.

Here also belongs auto-hypnosis, or self-hypnosis. In this the idea of hypnosis is not aroused by another person (hetero-hypnosis), but the subject generates the image himself, either voluntarily or involuntarily. Just as the will is otherwise able to produce particular thoughts, so it can allow the idea of hypnosis to become so powerful that finally hypnosis is induced; this is, however, rare. Hypnosis generally takes

place in consequence of some incident by means of which the idea of hypnosis is induced; this often happens when the subject has been frequently hypnotized. Certainly it is not always possible to make a sharp distinction between auto-hypnotism and hetero-hypnosis. It is possible that some states of sleep which are generally considered pathological, belong to auto-hypnosis.

I will now speak of the physical means, which for a long time were the only ones used. They consist of certain stimuli of sight, hearing, and touch. Taste and smell (Binet, Féré) have rarely been tried, and have generally given negative results.

The best known is the so-called method of Braid, in which hypnosis is caused by a fixed gaze at some object or other. It is of little consequence whether the object is bright or not (Gigot-Suard, Durand de Gros). Later, Braid gave up placing the object so close as to cause convergence. It is considered advantageous to hold the object so high that the eyelids are strained as much as possible in keeping the eyes open. Instead of a lifeless object, the experimenter can make use of his finger for the purpose, or, as the professional magnetizers prefer to do, of his eye (du Potet).

Luys proposed the use of a rapidly revolving mirror, in order to produce speedy and extreme fatigue of the eye. Lemoine, Joire, and others have pointed out that Luys's method enables us to hypnotize some hysterical and mentally afflicted patients, in whom hypnosis cannot otherwise be induced. The original apparatus designed by Luys has since been subjected to many modifications, more especially at the hands of Bérillon and Pau de Saint-Martin. Bérillon states in his brochure on the Psycho-Physiological Institute of Paris that he has exhibited various other instruments designed for the purpose of inducing hypnosis, such as that of Ch. Verdin; also, Aubry's magnesium lamp and Gaiffe's *Casque vibrant*. Bellemanière introduced a further modification by means of which sight and hearing could be stimulated simultaneously.

Just the same effect can be produced by hearing as by sight; for this the ticking of a watch is preferred (Weinhold, Heidenhain). Among uncivilized races particular instruments are used to produce analogous states; for example, the sound of a magic drum among the Lapps; among other races the monotony of uniform rhythm in song (Bastian). Instead of these continuous, monotonous, weak stimulations of the senses, we also see sudden and violent ones made use of; for example, in the Salpêtrière, the field of Charcot's work, the

loud noise of a gong or a sudden ray of the Drummond light. However, it is more than doubtful whether these sudden strong physical stimuli, without any mental effort, can induce true hypnosis. Perhaps we have here to do with states in which paralysis from fright is intermixed with hypnosis; at least, subjects thus hypnotized often wear an expression of fear (Richer). The effect can also be produced through the sense of touch, even by a gentle stroking of the skin, or by pressure upon it. We know that Celsus was acquainted with this latter fact. Some have also sought to induce hypnosis by the stimulus of heat—*e.g.*, warm plates of metal (Berger).

I here mention in particular the so-called mesmeric, mesmerizing, or magnetic passes, upon which Richet sets great value. I have already shown and described above, in the second experiment, how they are made. It is not certain whether the stimulation of temperature, as Heidenhain and Berger suppose, or the slight motion of the air, or the mental influence, is the efficient agent in this case. I myself consider it most likely that the various agencies combine, but that the mental factor is the most important. It is not necessary to assume the existence of any peculiar force, such as the magnetic fluid of the mesmerist.

I may here mention that the method of so-called "mesmeric passes" was not used by Mesmer; it is true that he endeavoured to influence by touch, but these peculiar, monotonous, long-continued passes which I have described above were unknown to him.

Pitres maintains that certain portions of the body are particularly sensitive to stimulation of the skin. The *zones hypnogènes* described by him sometimes exist only on one side of the body, sometimes on both. Stimuli applied to them are said to produce hypnosis in certain persons, as is indeed maintained of other parts. Among these parts of the body the crown of the head, the root of the nose, the elbows and the thumbs are mentioned. Crocq, of Brussels, is in agreement with Pitres as to the "zones"; but he says he has found other spots which vary with the individual. Crocq thinks that this enables him to eliminate "suggestion."

According to Chambord and Laborde a gentle scratching of the skin of the neck induces hypnosis. I myself have seen many persons who maintained that they became hypnotized only when I touched their foreheads. It is often stated that touches on the forehead induce a peculiar

sleepy condition in many persons (Purkinje, Spitta). An Englishman named Catlow magnetized by means of gentle stroking of the forehead (Bäumler). I also know some persons who, in order to go to sleep more easily, cause other parts of the body to be gently stimulated—the head, or soles of the feet, for example. Eulenburg maintains that pressure on the cervical vertebræ induces hypnosis. Boyd asserts that he once saw hypnosis induced by the introduction of a catheter into the male urethra; this was, of course, only a case of drowsiness in which sleep supervened later on. Herzog, commenting on Boyd's statement, has very properly pointed out that there could be no question of hypnosis in the case, which he explains as I have done. Petersen includes the ovaries in the hypnosigenetic zones.

Finally, I mention the action of the electric battery, whose influence, according to Weinhold, has the same effect as mesmeric passes; but Weinhold, however, does not consider that mental influences are in this case excluded. Eulenburg obtained a lethargic condition by galvanizing the head; but the person experimented on had already had attacks of lethargy. More recently Harrison Low states that he has seen hypnosis induced by the Röntgen rays. But we are justified in assuming that in all these cases in which hypnosis is supposed to have been caused by the means just mentioned, the hypnosis has only come on because the subject believed that the means employed induced hypnosis. Hirt often uses electricity in this way, but it is at the same time perfectly certain that it is not the electricity, but the subject's belief in its effect, that produces the hypnosis.

To conclude, I further mention stimulations of the muscular sense, such as the cradle-rocking used to send little children to sleep; I leave on one side the question as to whether hypnosis can be attained by this means. Similar states are said to be produced among uncivilized people by violent whirling or dancing movements: the movements are, however, accompanied by music and other mental excitations. The best known are the Aïssaouas, in Algiers (Figuier, Bert, Delphin). "They carry on their business chiefly in the Algerian town of Constantine. They are able by means of dancing and singing to throw themselves into a state of ecstasy difficult to describe, in which their bodies seem to be insensible even to severe wounds. They run pointed iron into their heads, eyes, necks, and breasts, without injuring themselves" (Hellwald). The same thing is related of the Buddhist convents in Thibet (Hellwald, Gabriel Huc). It

has often been pointed out that the monotonous singing and uniform whirling movements of the dervishes induce hypnosis in the performers. I have, however, myself often watched the howling and dancing dervishes at Cairo and Constantinople without being able to detect any indication of hypnosis.

I have hitherto only spoken of sense-stimuli among the physical methods. But it must also be mentioned that the absence of these stimuli is likewise specified as an expedient for hypnosigenesis. Jendrássik, of Buda-Pesth, expresses the opinion that fixed attention is only effective because it causes fatigue of the nerves of sight, and consequently produces insensibility to stimulation. Strümpell observed a case in which a person fell asleep immediately on the cessation of sense-stimulation. A case of Ballet's, in which sleep and suggestibility were induced by closing the eyes and stopping the ears, probably belongs to the domain of hypnosis induced by suggestion; and I think that the same is true of the method described by Hartenberg in Paris, in 1900, by means of which hypnosis is supposed to be induced without suggestion being called into play. Hartenberg lets the patient be comfortably seated, and then applies one electrode to his forehead and the other to his chest. The faradic current is then started, but without including the patient in the circuit. The subject is then told to take deep inspirations, and at the same time to concentrate his whole attention on his breathing; as soon as he begins to blink the experimenter closes his eyelids. All these details of the procedure—the enforced rest, relaxation of the muscles, the closing of the eyes, the monotonous tapping of the induction hammer, but more particularly the deep inspirations are intended to induce hypnotic sleep without the aid of suggestion. As far as the deep inspirations are concerned, Hartenberg considers that they set up a certain amount of hyperæmia of the thoracic viscera with concomitant cerebral anæmia, which latter promotes the onset of sleep. Although Hartenberg avoids using the words sleep and hypnosis, the action of suggestion is not excluded from his procedure, as will be seen from the considerations given below.

The classification of hypnogenetic expedients into psychical and physical is merely theoretical (Forel, Levillain), and that for two reasons. In the first place, we cannot regard body and mind as two factors which are independent of one another.

Sense-stimuli, which affect the body, nearly always exercise a certain influence on the mind; the mind, on the other hand, can act upon nothing that has not previously entered it by means of the organs of sense. In the second place, in practice several hypnosigenetic processes are used at the same time. This will become perfectly clear if the hypnotized person is watched; let him be told that he must concentrate his whole attention on the idea of sleep, and he will then, in order to obey the command, look steadily at some point, or at once shut his eyes, in order as much as possible to prevent distraction of thought.

Thus Bernheim occasionally uses fixed attention in addition to the mental methods. Braid, again, who made use of fixation almost entirely, considered a particular mental activity also necessary. This is to be particularly noticed, because some people nowadays believe that they are using the method of Braid when they tell the subject to look steadily at something. In reality, Braid considered a steady attention as well as a steady gaze indispensable if hypnosis were to be attained; the subject must think steadily of the thing he was looking at, and must not allow himself to be diverted from it. According to Braid, one can hypnotize even in the dark.

But even theoretically we cannot always keep these things apart. Closing of the eyes, with perhaps slight pressure upon them, often leads, as Lasègue showed, to hypnotic states. How these come about, whether through the cessation of the sense-stimulation or through the idea of sleep, which the closing of the eyes certainly easily calls up, cannot be decided.

After these details, the much discussed and disputed question must be answered, whether a person can be hypnotized without his knowledge; whether any one can be thrown into the hypnotic state merely by sense-stimuli, without these arousing an image of the hypnosis. For a long time such an occurrence was held to be possible, until the Nancy school demonstrated an important source of error—viz., the possibility that suggestion may be used quite inadvertently. *At all events I know of no well-authenticated case in which sense-stimulation has produced hypnosis by a purely physiological action.* Most people upon whom such experiments are made know that an attempt is being made to hypnotize them; they have been already hypnotized, and the stimuli arouse conscious or unconscious mental images of the hypnosis; or they have seen the same experiments with others, or have heard of them. Even when

this is not the case, the objection raised by Bernheim and Forel remains to be considered—that the sense-stimuli induce a feeling of fatigue, and through this induce the hypnosis.

Which of the above methods, or which combination of them is the best for practical use, is a question the answer to which cannot readily be supplied. When we find that Richet thinks he can throw nearly everybody into the hypnotic state by means of mesmeric passes, that Liébeault, Bernheim, and Forel hypnotize nearly all their patients by the Nancy process, Vogt by his fractional method, and that Braid hypnotized ten out of fourteen by means of fixation, we see that different methods bring about nearly identical results. From this it follows that the success or failure of an experiment does not depend solely on the external influences which may be brought into play. The mental susceptibilities of the individual to be experimented on are of far greater importance, and consequently in each individual case that method should be selected which is most suited to the mental condition of the subject, for some persons appear refractory to one method while another succeeds. I have found persons insusceptible to the use of fixed attention, or to the method of Nancy, while I obtained results by mesmeric passes. Evidently this proves nothing against mental action, for many persons believe they can only be influenced by some particular process. On the other hand, I have seen that intense fixity of gaze sometimes induces hypnosis when other methods are useless, perhaps because the subjective expectation of the hypnosis is sooner aroused by the long, intense stare than by verbal orders.

Chambard reckons chloroform, ether, etc., among hypnosigenic agents. Certainly many phenomena analogous to those of hypnosis have been observed in the sleep induced by these agents; but F. Myers, however, considers it better to distinguish the conditions thus produced from hypnosis. Attempts have recently been made, more particularly by Wetterstrand and Schrenck-Notzing, to study these phenomena. They conclude that by chemical substances like chloroform, morphine, haschisch, hypnosis can be attained in persons who are insensitive to other methods. Farez advocates the use of somnoform—a mixture of ethyl chloride, methyl chloride, and methyl bromide—for the purpose of inducing narcosis, during which suggestion may be used. Bernard, Feuillade, and Wiazemsky report good results from the use

of somnoform. It would, however, be necessary to distinguish between cases in which deep sleep is first obtained by the use of drugs, and hypnosis from this condition, as Coste de Lagrave advises, and those cases in which the hypnotic phenomena are primary. I have myself often produced hypnotic phenomena with post-hypnotic suggestions by the use of chloral hydrate.

Stoll has given detailed accounts of the connection between drugs and suggestion as used by uncivilized peoples. It is not my intention here to deal with the part played by suggestion in the therapeutic action of drugs, but I must certainly refer to the hallucinations and conditions resembling hypnosis which are set up by certain vegetable substances. Stoll, in the course of his dispute with Dünschmann, has proved conclusively that such vegetable substances do not necessarily cause a state of intoxication, but frequently induce a condition resembling hypnosis by an action which can only be described as purely suggestive. Stoll's authority, the botanist Schinz, told him, amongst other things, that when he smoked Indian hemp in quantities not employed by the natives, he did not experience any toxic effects. On the other hand, a puff of a pipe filled with hemp invariably produced such effects on the Bushmen, driving them at times to distraction. Stoll gives an even more convincing proof of his contention: "Schinz tells me that when the supply of hemp runs short the Bushmen smoke the dung of elephants and antelopes, substances to which even Dünschmann will hardly ascribe a toxic action; and yet the smokers pass through the same phases of intoxication as when smoking hemp. My friend Schinz is therefore equally convinced with me that suggestion plays the chief part in producing the symptoms mentioned above, and that tradition and a greedy anticipation of enjoying the hemp are the active factors in inducing the hypnosis." But the question still remains, are we justified in referring to hypnosis all those cases in which we find toxic symptoms accompanied by the phenomena usually met with in hypnosis? Somewhat analogous to the instances mentioned by Schinz and Stoll, are those cases in which patients who are about to be chloroformed fall asleep before they have hardly inspired one breath of the anæsthetic.

According to Landouzy, Proust, and Benedikt, the magnet also has a hypnotizing action; but my own numerous experiments in this direction have been altogether unsuccessful. Ranschburg, nevertheless, opines that the magnet is frequently of value for inducing hypnosis by suggestion; in fact, he regards it as a thoroughly reliable implement when used in this respect. From time to time new methods of hypnotizing have sprung up, in which some artifice is employed which seems to facilitate the induction of hypnosis in some cases. Many of these artificial means have been already described in the older literature of animal magnetism, a study of which teaches us that many of the so-called modern discoveries were well known more than a hundred years ago. To this class belongs, for instance, the method of hypnotizing described by Moutin, who seizes the finger of the subject seated opposite to him, then presses the knees close together, etc., a method often used by the mesmerists of former times.

Must even tried out the Magnet!

The waking[1] from hypnosis (dehypnotization) can also occur in two ways—through immediate action on the imagination, or through sense-stimulation. It is nearly always possible to put an end to the hypnosis by mental means—that is, by the command to wake up at once, or to wake up at a particular signal. It is hardly ever necessary to use other means, such as forcibly opening the eyes, fanning, sprinkling with water, excitation by means of the faradic current, loud calls, etc. Just as the mesmerizing passes induce hypnosis, so the de-mesmerizing passes—as I used them in the second experiment described above—cause it to disappear. Even if the cool current of air, which is nearly always thereby generated, expedites the awakening, the belief of the subject that he must wake is the most important factor in the process. Other processes which have been given, and which were supposed to induce awakening by physical means, such as bringing charcoal near the patient, have only a mental effect, as they are understood as a command to awake. In rare cases these artificial means of awakening do not succeed quickly; a feeling of fatigue then continues, as we occasionally experience when waking out of a natural sleep. After deep and long hypnosis a temporary state of drowsiness often supervenes, in which certain hypnotic phenomena continue. The latter condition, however, occurs exclusively, or almost exclusively, in the case of hysterical patients (*cf.* p. 37).

If the awakening is not brought about by artificial means, persons in a light hypnotic state such as is described in the first two experiments, habitually wake of their own accord after a few minutes, or even seconds; this especially happens when the continuance of the state has not been expressly ordered. Some people wake directly the experimenter leaves them, as they then no longer think themselves under his influence. Others wake of their own accord even out of deep hypnosis if they hear an unexpected and loud noise, or have exciting dreams. Thus, I once saw a grown-up person wake

[1] I shall adhere throughout to the terms waking, awakening, and the waking state. They have not yet been superseded by any better expressions. It should not, however, be forgotten that hypnosis is not invariably a state of sleep. Consequently, when I speak of a person waking from hypnosis, it must be understood that I imply that an end has been brought to a state in which a possibly conscious person was unable to perform certain actions.

herself by screaming, because in the hypnotic state she had believed herself to be a little child, and in that character had begun to cry. The awakening which comes about without any apparent cause is remarkable (*mouvement psychique*). The same thing is sometimes observed in natural sleep, especially at the beginning; O. Rosenbach traces it to increase of the reflexes. Generally, however, the deep hypnoses continue for some time when they are not artificially terminated. Sometimes many hours pass before the subject wakes.

The old mesmerists (du Potet, Lafontaine) describe as a rare occurrence in hypnotic experiments a state of lethargy in which artificial awakening was impossible. After some time there was a spontaneous awakening, and no evil consequences were to be observed. Guermonprez described lately how a person had remained three days in hypnosis, nobody being able to wake him. These incidents have only been observed among hysterical subjects. Again, many of these cases have nothing whatever to do with hypnosis—are more probably instances of a state of lethargy. One thing is certain: there can be no question of hypnosis when *rapport* of any kind and all possibility of suggestion are wanting. Even a lethargic state occurring in a person being hypnotized must be held as something quite distinct from hypnosis. If we wish to make a rational comparison between two conditions we must consider the similarity of the symptoms they present, not the nature of the cause which has produced them.

Who is hypnotizable? In order to settle this question without hypnotic experiments, Ochorowicz has invented a special instrument—the hypnoscope; it is an iron magnet in the form of a ring, which the person to be tested puts on his finger. Hypnotizable persons are supposed to experience certain sensations in the skin or twitchings of the muscles, while with the insusceptible nothing of the kind takes place. The researches of other investigators have not confirmed this (Obersteiner, Gessmann, Grasset, Bottey). Other signs which are supposed to indicate susceptibility to hypnotism I consider untrustworthy.

Neither neurasthenia nor pallor, neither hysteria nor general feebleness of health, produce a disposition to hypnosis. Our ordinary hysteria, with its variable characteristics of headache and the feeling of a lump in the throat (globus), combined

with the general hysterical desire to be interesting and to exaggerate the sufferings endured, produces, according to my experience and that of others, no special disposition to hypnosis. Certainly Charcot held that hysteria predisposed a sufferer from that malady to hypnotic influence, and some more recent investigators (Hirschlaff, Gumpertz) have expressed a similar opinion with respect to the deeper hypnotic state. I consider such opinions erroneous. The mistaken notion that hysterical or nervous patients are particularly susceptible to hypnotism results from the fact that most physicians have experimented with them only; besides which it is very easy to discover in all persons something which may be explained as a hysterical symptom if we only try to do so. This reminds one of the hereditary taint which we so easily detect in nearly every one. If, however, we consider every one who submits himself to a hypnotic experiment to be "nervous" (Morand), then, naturally, only nervous persons can be put into the hypnotic state. In reality, as Sperling has rightly pointed out, if we are to take a pathological condition of the organism as a necessary condition for hypnosis, we shall be obliged to conclude that everybody has a mental twist—is not quite right in the head. For the rest, the old mesmerists (Brandis, Lichtenstädt, Wirth, and others) maintained that a healthy individual could not be mesmerized. In opposition to the assumption that general weakness is a predisposing factor, I may mention that Hansen always preferred muscular persons for his experiments, and I have myself hypnotized many muscular individuals, in some instances men of athletic build.

With regard to mental aptitudes, Forel believes that every mentally healthy human being is hypnotizable. In Liébeault's opinion heredity plays a great part in the disposition to hypnosis. It is universally agreed that the mentally unsound, especially idiots, even if not wholly insusceptible, are still very much more difficult to hypnotize than the healthy. However, A. Voisin succeeded in hypnotizing ten per cent. of the mentally unsound, by exercising the necessary patience. But apart from this I do not believe that intelligence plays any important part. Of course we are justified in assuming that the dull and stupid are not easily influenced, just as there are others who let their imagination come into play on the slightest provocation. On the other hand, I think that susceptibility to

hypnotic influence should not be considered a gauge of the patient's intelligence. Mental excitement frequently prevents hypnosis. Inhibition is also often brought about by the subject's urgent desire to be hypnotized. Emotional influences may also account for the fact that persons who are occasionally refractory, at other times readily submit to hypnotism.

It is altogether a mistake to consider the disposition to hypnosis a sign of weakness of will. Without doubt the ability to maintain a passive state has a predisposing effect. This is why soldiers are in general easy to hypnotize. The ability to direct one's thoughts in a particular direction is also very favourable. This ability to give the thoughts a certain prescribed direction is partly natural capacity, partly a matter of habit, and often an affair of will. Those, on the contrary, who can by no possibility fix their attention, who suffer from continual absence of mind, can hardly be hypnotized at all. It is specially among the nervous that many of this class are to be found—persons in whom a perpetual wandering of the mind predominates. The disposition to hypnosis is also not particularly common among those persons who are otherwise very impressible. There are plenty of people who believe all that they are told, yet they often offer a lively resistance when an effort is made to hypnotize them.

Hilger has attempted to ascertain the bearing of distinct mental factors on the general susceptibility of the individual to hypnotic influence. He hopes by these means to determine the possibility, or otherwise, of hypnotizing any particular person. Among the many factors incidental to the induction of hypnosis which Hilger has examined, the amount of confidence displayed by a patient in his doctor, and the treatment pursued, may be mentioned. Hilger examined 295 cases in this respect, and found that an increase in confidence was invariably accompanied by a rise in the percentage of those persons susceptible to hypnotism, especially where deep and rapid hypnosis could be attained. Moreover, he did not neglect the question of habitual or temporary docility on the part of his patients. The percentage of those hypnotizable, or susceptible of deep hypnosis, appears to have increased in 283 cases examined by him. Hilger puts his somnambulists into three catagories, in the lowest of which we get 10.64 per cent., in the second 34.07 per cent., and in the third 50.49 per cent.

The old mesmerists attempted to fix on certain signs as indicative of susceptibility to magnetic influence. Any lack in this respect was explained on various grounds. Dechambre tells us how Prince Henry, the brother of Frederick the Great, once came to Paris and attended the *séances* given by a French officer who indulged in magnetism. Mesmer appeared on the scene and proposed to magnetize the prince, but the latter "no more slept than he did at Friedberg or at Breslau. He did not even experience any of those sensations which magnetization is supposed to induce." Of course the ill-success of the experiment was finally attributed to the countervailing influence of "royal blood."

Nationality (Ewald), or local surroundings (Brugia), have no influence upon susceptibility to hypnotism. Forel in Zürich, Renterghem in Amsterdam, and Wetterstrand in Stockholm, have shown that Teutonic peoples are as easy to hypnotize as Latin. Besides, Braid's experiences in London show nearly the same thing; on one occasion in London, he was able to hypnotize sixteen out of the eighteen persons he experimented on. Recently it has been pointed out in many quarters that Russians are more easily hypnotized than any other people. At all events the fact remains that susceptibility to hypnosis is not a special characteristic of the Latin races. It is also to be noted that Ringier and Terrien have shown that rural populations are easily hypnotizable.

With regard to age, infants under three years of age can hardly be hypnotized at all, and even up to six years of age children can only be hypnotized with difficulty. Although children are otherwise easily influenced, their thoughts are so readily distracted that they cannot fix their minds on a prescribed picture, such as that of hypnosis. Old age is by no means refractory to hypnosis. According to the experiences of the Nancy school, with which mine agree, older persons more often remember, after hypnosis, all that has happened than do younger ones. Sex has no particular influence; it is a mistake to suppose that women are better adapted than men; though Lœwenfeld may possibly be right in stating that deep hypnosis is more easily induced in woman than in man.

Besides this, individual observers (Brémaud, Maack) mention some points which may be favourable or unfavourable. Brémaud, for example, mentions alcohol as favourable, Maack as unfavourable. But universal conclusions should not be drawn from a few isolated observations. For the same reason I question the accuracy of some of Ringier's statements, though the rest of his remarks are of great practical value. According to him hypnosis is less easily induced in winter than in summer, because cold is supposed to be unfavourable; thus persons who were easily hypnotized in summer became refractory in winter.

The frequency with which an attempt should be made on the same person is of considerable importance. While, according to Hähnle, only one person in ten proves susceptible on the first attempt, the proportion increases with the frequency of the sittings. This is not to be wondered at, from the excitement shown by many people in the beginning. And as it is most important to hypnosis that the attention should not be distracted, many people are first of all obliged to learn to concentrate their thoughts. I doubt whether continued attempts render everybody hypnotizable, though some earnest investigators hold such to be the case. I have myself made forty or more attempts with some persons without obtaining hypnosis. Perhaps by even longer continued efforts a result would have been obtained, as indeed has happened to me many times even after forty vain attempts. Any one may possibly win the big prize in the lottery provided he lives long enough and keeps on buying fresh tickets. In other cases the exact opposite occurs, and the oftener the attempt is made, the less successful it is; by a process of auto-suggestion a person persuades himself that he is not hypnotizable. There are cases in which we get evanescent symptoms of hypnosis. Similarly, there are people who at first willingly submit to hypnotic experiments, but later on become intentionally refractory and thereby exclude the possibility of being hypnotized.

Besides these subjective conditions there are some objective ones. Thus, for example, disturbing noises at the first experiment have power to prevent hypnosis: they draw off the attention, and thus interfere with the mental state necessary for hypnosis. When once hypnosis has been induced, noises are less disturbing in subsequent experiments. Further, the environment of the subject must be considered. Any sign of mistrust on the part of lookers-on may easily spoil the experiment; and it is well that the subject should live, as it were, in an atmosphere of suggestion. It is well known that many persons can be speedily hypnotized when the above conditions are fulfilled, and that, as a rule, such precautions enhance the possibility of hypnotizing new subjects. Again, the susceptibility of a whole town or populace may be influenced to a remarkable degree by the manner in which the hypnotizer presents himself to the general public. This, in itself, accounts for the great variations in susceptibility to hypnosis which have shown themselves at different times and places. It is, conse-

quently, not surprising that on one occasion ten persons, one after the other, are hypnotized, while on another occasion ten other persons all prove refractory.

Experience and a knowledge of the mental conditions of mankind are indispensable for the hypnotizer, for he has to gauge each individual subject and work upon his susceptibilities. In this respect practice naturally plays an important part. Practice and a gift for observation enable the right stress to be laid at the right moment either on fixed attention or on the closing of the eyes. The experienced experimenter knows how to judge whether it is best in any particular case to attain his aim by speaking, or whether, as sometimes happens, speech would be a hindrance, and the chief stress would be best laid on mesmeric passes, etc. A person who is easily hypnotized can be hypnotized by any one, but one who is hypnotized with difficulty can only be thrown into hypnosis by a good experimenter. The factors which play a part in inducing the hypnosis cannot always be recognized at once. It sometimes happens that a person A. can be hypnotized by B., while he remains refractory to the efforts of C. On the other hand, it may happen that D. can be influenced by C., but not by B. This shows that the influence of one person over another depends on the individuality of both. We find the same in life, in the relation of teacher to pupil, and of pupil to teacher, in the reciprocal relations of friends, or lovers.

That there exists an individual aptitude for hypnotization, and for making the suggestions—to which I lay no claim—is certain. It is true we must not think of this ability as did the older mesmerists, who considered it a peculiar physical force possessed by certain persons. We must rather represent this natural ability to ourselves as we do many others, when we have to do with particular mental aptitudes. I need only remind my readers of the preacher who brings a whole community under his influence; of the doctor whose presence in the sick-chamber immediately inspires the patient with confidence; of the great men in history, such as Napoleon I., the magic of whose personal influence has led him to be called a great master in the art of suggestion. It is possible to analyze some of the personal qualifications which confer an aptitude for hypnotization—patience, calm, presence of mind; others elude analysis.

. . . . . .

The question whether hypnosis can be induced against the will of the subject is one of importance. We must distinguish here whether the subject complies with the prescribed conditions or whether he does not. If he does—if, for example, he sufficiently concentrates his attention—then hypnosis may be produced at the first attempt, even against the wish of the person experimented on. However, it must be remembered that a person who does not intend to allow himself to be hypnotized will hardly place himself in the necessary mental state; he will fix his eyes, but will allow his attention to wander. Nevertheless, I think that, granted special conditions, many persons can be hypnotized at the first attempt, even against their will. This occurs most readily when the general surroundings are of a nature calculated to suggest hypnosis. Some persons can be speedily hypnotized against their wish, if only taken unawares directly after witnessing a public exhibition of hypnotism. Heidenhain hypnotized soldiers in the presence of their officers, who had strictly forbidden them to sleep. Such a command, coming from an officer, would have as much effect on a soldier as the personal wish not to be hypnotized. It is not to be doubted that many people who have been frequently hypnotized can be re-hypnotized against their will. Post-hypnotic suggestion, of which I shall speak later, is also a means of sending persons to sleep against their wish. There is a third possibility—namely, that no wish should exist in either direction. The conditions necessary for hypnosis may occur occasionally by chance, without the subject being conscious of them (Max Dessoir). For example, some one over his work is obliged to look fixedly at a certain point; this suffices to induce hypnosis (sometimes after earlier unfit experiments) without the person thinking of it. In this case the will is neither interested for nor against it. The statement of Preyer, that persons being photographed sometimes remain sitting rigidly still after the taking of the photograph is finished, may be referred to a hitherto unsuspected hypnotic state, brought on by the fixed stare necessary to the process. I have already mentioned that some of the inmates of the Salpêtrière in Paris suddenly fall into catalepsy in consequence of some loud unexpected noise. There is an interesting case of a girl who had often been hypnotized by loud noises, and who went to a drawer to appropriate some photographs out of it. The casual beating of a gong threw her into a cataleptic state,

so that she stood motionless in the act of carrying out her theft, and was discovered. Hack Tuke remarks that it is a pity all thieves cannot be taken as easily.

Certain mysterious and not altogether credible reports have recently reached Europe concerning hypnotism in Japan. It is said that the Japanese have a special method, called jiu-jitsu, by means of which hypnosis is induced. It is taught to police officials and others in a special school in Yokohama. The details of the method are kept a secret. According to these accounts, the greater the opposition offered by an unskilled person to the expert pitted against him, the more easily does the latter vanquish the former; a simple touch and the victim is paralyzed—rendered absolutely helpless. It will not cause very much surprise if we are presently told that the great military successes of the Japanese were due to their skill in the art of hypnotization. Is it possible that a diminutive policeman can, by means of a simple jiu-jitsu touch, render a sailor of colossal proportions incapable of further resistance?

As Bertrand related, with certain persons natural sleep can be transformed into magnetic sleep. Many attempts have been made to do this in later times. Baillif, Gscheidlen, Berger, Bernheim, Forel, and Manfronie, but more particularly Farez, have made such experiments, occasionally employing the therapeutics of suggestion at the same time. Some investigators certainly mention the suggestions they made use of during the subjects' ordinary sleep; one thing is certain, natural sleep can only be transformed into the hypnotic state when the suggestions are made in the manner mentioned. Some of the experiments in which natural sleep was transformed into hypnosis were made on persons who had never before been hypnotized, or who on previous occasions had proved refractory to hypnotic influence. I myself have been able to make some observations of this kind. One person concerned was a gentleman whom I had already frequently hypnotized, and whom I often threw into the hypnotic state while he was taking his afternoon sleep, without waking him. In another case I succeeded in producing various movements, as raising of the arms, through slight suggestions, to a gentleman who was in his afternoon sleep. I was obliged to speak in a whisper to avoid waking him. It is doubtful whether such experiments would succeed with persons who had never heard of hypnotism. Schrenck-Notzing reports a case in which hypnosis was produced from post-epileptic coma. Cases in which conditions of hysterical sleep have been led on into hypnosis have been described by Lœwenfeld and others. I may refer here to the

question already raised, whether hypnotic states can be produced by chemical substances, such as chloroform, etc.

In any case, however, previous consent is not absolutely necessary to the production of hypnosis, and, on the other hand, there are people who are refractory in spite of a decided wish to be hypnotized. In general, however, the intentional resistance of the subject hinders hypnosis, because a person who is willing to be hypnotized complies more easily with the necessary conditions than another. Consequently, it is not astonishing that patients who come to a doctor on purpose to be hypnotized, particularly when they come with full confidence, are more easy to hypnotize than others. These others often allow an attempt to be made with them, with the silent resolution that "they are not to be caught," or they submit themselves, as Nonne says, "only for fun."

As so many different circumstances influence the induction of hypnosis, it is not surprising that the proportion of hypnotizable persons should be differently stated. If Ewald in the Women's Hospital at Berlin can only hypnotize two persons, while Liébeault hypnotizes 92 per cent. of his patients, the reason of this enormous difference must lie in the disparity of the conditions. The insufficient mental preparation of Ewald's subjects is particularly to blame for his failure. Bottey gives 30 per cent. as susceptible, Otto Binswanger more than 50 per cent., Morselli 70 per cent., and Delbœuf more than 80 per cent. The latter's data appear to me of great value, having been evidently collected with critical care. As he excludes simulators, he appears to me more sceptical than the investigators at Nancy. The highest percentage of successes was obtained by Vogt, who states he did not find one thoroughly refractory case in 119 subjects experimented on by him. Bramwell makes a similar statement with regard to a particular section of his patients. Bernheim refuses the right to judge of hypnotism to all hospital doctors who cannot hypnotize at least 80 per cent. of their patients. Forel fully agrees with him.

An international investigation on aptitude for hypnosis has been made by Schrenck-Notzing, who applied to numerous investigators in various countries—England, Sweden, Germany, France, Algiers, Canada, and Switzerland. Among 8,705 persons covered by the investigation, only 6 per cent. were completely refractory. The remaining 94 per cent. were

divided by Schrenck-Notzing into three groups, according to Forel's classification, of which I shall soon have to speak: 29 per cent. attained to somnolence, 49 per cent. to hypotaxis, 15 per cent. to somnambulism.

The oftener hypnotic experiments are made the sooner is hypnosis generally induced. The first attempt often takes five minutes or more, although on many occasions a few seconds suffice. When the experiment has succeeded a few times, a few moments are nearly always enough to attain the result. This is because the remembrance of the earlier hypnosis essentially favours its return. Besides this, the strongest hindrance has been overcome by the earlier hypnosis—which is the belief of the subject that he is not hypnotizable, a belief which often prevents hypnosis. The certainty with which well-known hypnotizers hypnotize people, rests partly on the fact that these subjects believe that some hypnotizers possess a greater aptitude for hypnotizing them than others do. On the other hand, Rosenbach and Eschle go too far in their contention that "the nimbus of the hypnotizer" is the essential factor in producing hypnosis. "It is to the mysticism of the subject to be hypnotized, in the form of a naïve but firm belief, and not to that of the hypnotizer, that we must look for an explanation of the fact that the former person submits to the influence of the latter." This is Eschle's opinion, but auto-hypnosis forbids an all-round acceptance of this view.

Although we have seen that the disposition to hypnosis increases the oftener experiments are made, it may also disappear when the experiments have been discontinued for a long time. But the disposition to hypnosis can generally be reproduced if a few attempts are made. Still, I have seen cases in which subjects who were at one time easily hypnotized later on become altogether refractory, and for no apparent reason.

From the above examples it appears that the various hypnotic states differ much from one another, especially where the depth of the hypnosis is concerned. This suggested that in order to obtain a general survey an attempt at classification must be made.

A well-known classification is that of Charcot, who supposes three stages—the cataleptic, lethargic, and somnambulic. I shall go into more details as to these later, but will remark here that this classification has no universal value. Pitres

partly follows Charcot, but accepts so many different kinds of hypnotic states that a general view is thereby rendered only more difficult. The classification made by Gurney—alert and deep stages—is only accurate for a few cases. In the same way the three stages supposed by Richet, Fontan, and Ségard are not sufficiently well defined for practical use. According to Delbœuf, there are two stages in hypnosis, one with and the other without analgesia. As, however, complete insensibility to pain is very rare, and as the transitions are vague, insensibility to pain does not seem to me to be a suitable distinguishing characteristic. The classifications made by Liébeault, Bernheim, and Forel are well known. As they agree in the main, only differing in the number of stages—according to Liébeault, 6; according to Bernheim, 9; according to Forel, 3,—I shall only cite that of Forel:—

Stage 1. Drowsiness: the patient can resist suggestions only with great effort.

Stage 2. Hypotaxy ("*charme*"): the eyes are fast closed and cannot be opened; the subject is obliged to obey various suggestions.

Stage 3. Somnambulism: it is characterized by a loss of memory on waking, and also by post-hypnotic phenomena.

The classifications of Liébeault, Forel, and Bernheim rest chiefly on loss of memory, as a particular group (Forel's 3rd stage) of hypnotic states with loss of memory is placed in contrast with the others (Forel's 1st and 2nd stages), in which no amnesia exists.

Those hypnotic states in which loss of memory exists are called somnambulism by the authors above named. Wienholt also has already said that the magnetic states with ensuing loss of memory may be called somnambulism.

It would be better, however, not to make our estimate of the stages of hypnosis dependent on loss of memory, but on the phenomena which appear during the hypnosis itself. Memory after hypnosis is dependent on many other factors which have nothing to do with the depth of the hypnosis. A chance view of any external object will suffice to arouse a whole chain of mental images implanted in the memory; further, we shall see that memory is influenced by suggestion. Delbœuf, who often experimented with profound hypnoses, declares that the subjects after the awakening were able to give an account

of all the hypnotic incidents. I should consequently prefer to judge of the depth of the hypnosis only from the phenomena of the hypnosis itself.

The numerous subdivisions given by Liébeault and Bernheim are not easy to utilize, because there exists no uniform principle for such classification. For example, one stage is distinguished by the complete closing of the eyes, and a deeper stage by motor disturbances in the arms. As these last, however, can also occur when the eyes are open, they cannot be regarded as a sign of the deepening of the stage in which the eyes are closed; for in the deepening all the phenomena of the previous and lighter stages must appear (Max Dessoir).

In order to avoid these difficulties, Max Dessoir has published a classification of the stages of hypnosis which is as simple as it is comprehensive and clear, and which I still consider to be the best, just as I did many years ago. According to this the states fall into two large groups, which are divided from each other by the extent of the functional disturbances. In the first group merely the voluntary movements show changes; in the second group abnormalities in the functions of the sense organs are added. The principle of this classification was already known to Kluge. If we accept Dessoir's classification, the minority of subjects belong to Group II.; thus, assuming 75 per cent. to be susceptible, 55 per cent. belong to Group I. and 20 per cent to Group II. According to Kron this latter percentage is perhaps too high. He conjectures that through practice and other factors these figures might vary considerably. The results which Vogt has obtained are simply marvellous—99 cases of somnambulism in 119 subjects experimented on.

It will be understood that in Dessoir's two groups many stages and types can be distinguished. For example, we see that many a hypnotic state belonging to Group I. is merely characterized by the closing of the eyes, which the subject cannot voluntarily open, as in the first experiment (p. 34). This state used to be considered as a particular stage of hypnosis, but according to the explanation given above it takes its place as merely a particular form of Group I. It was frequently represented as a particular stage of hypnosis, because in many cases hypnosis is ushered in by a closing of the eyes, while other muscles are only affected later on. However, this may be a pure accident (Max Dessoir); we have accustomed ourselves more and more to induce hypnosis by

affecting the eyes, and to provoke a closing of them as quickly as possible; but this is nothing but a habit, resulting from the identification of hypnosis with sleep. Hypnotists exist who induce hypnosis when the eyes are wide open, as is the case in "fascination," which will be discussed later (*cf* 3rd experiment, p. 35). Lœwenfeld, also, has elaborated a method in which any effect on the eyes is only of secondary importance. I myself have met many people in whom it was impossible to bring about any abnormality in the movements of the eye, while other muscles were easily affected. For this reason, I think the assertion of Michael that hypnosis can only be proved when the eyes are completely closed is entirely mistaken. Certainly he is perfectly right when he says that we should not ascribe to hypnosis the states of fatigue and giddiness which ensue after long attention, unless other phenomena typical of hypnosis also appear.

It is clear that the two groups cannot be sharply divided from one another. Also the transition from a normal state to hypnosis is gradual, and certainly not so sudden as some think. We find many stages even before we arrive at the closing of the eyes, which certainly does not indicate a deep hypnosis; at first only heaviness of the eyelids, then a desire to close the eyes, then a difficulty in opening them, and finally their complete closure. All possible stages are displayed. Further, a deep hypnosis is not always attained at once; the light states are often passed through before the deepest appears. It is naturally difficult, through all these different transitions, to decide the exact moment of the appearance of hypnosis. A deep sigh, which often ushers in the beginning of hypnosis, is by many wrongly considered as diagnostic of the moment, particularly as this symptom is easily spread by imitation (Delbœuf). The movements of swallowing which appear, especially after long fixation, have equally little significance.

As already shown, the word "hypnosis" is derived from the Greek ὕπνος, and this might induce one to conclude that the term hypnosis should only be used to describe a condition resembling sleep. In reality, however, the cases in the first group show no resemblance to sleep or a loss of consciousness. And yet there is a very prevalent notion amongst laymen that there can be no hypnosis without sleep and loss of consciousness, and persons holding this view do not refer the conditions exhibited by the first group to hypnosis. But this is not a

question of what the layman thinks or believes; as Vogt has rightly pointed out what may or may not be ascribed to hypnosis can only be decided by those persons who have devoted themselves to a critical study of the whole subject. To prevent any confusion which might possibly arrise from the etymological resemblance of the words "hypnos" and "hypnosis," it has been proposed to distinguish those cases in which there is no semblance of sleep and in which consciousness remains normal, from deep hypnosis, by a distinct appellation. Max Hirsch has proposed for such superficial conditions the term Captivation. Hirschlaff also would like to see the aforesaid conditions distinguished from hypnosis, and terms them pseudo-hypnotic. We might even agree with Hirschlaff on this point in spite of the fact that some of his theoretical explanations of the question raise considerable doubts in one's mind. It must, however, be pointed out that it is quite usual to find that the etymological signification of a word no longer covers all the uses to which it was originally applied, or that in time it gains a much wider meaning. I may refer to the word electricity, which now scarcely reminds any one of electron, amber. We need not stay to consider Bernheim's somewhat oracular statement that there is no such thing as hypnosis, but only suggestion. Pierre Janet has justly pointed out that because there are many transitions from the normal state to the hypnotic, we are not therefore entitled to say that there is no special condition of hypnosis. A condition which, on waking, leaves no recollection behind, as in the fourth case, and in which the subject becomes a mere puppet in the hands of the experimenter, must be considered a condition altogether distinct from the normal.

One peculiar quality of consciousness we shall often find in hypnosis: what is called suggestibility; or, better, increased suggestibility. I shall so often use the word, and words connected with it, that it will be well to define exactly what is meant by it. For this purpose I must make a little digression, remarking at the same time that many works—excellent in their way—which have been written for the purpose of giving a clear definition of what is meant by suggestion, will not be dealt with by me at this juncture; but I may as well mention the names of certain authors in this connection:—Bernheim, Forel, William Hirsch, Bergmann, Lipps, Schrenck-Notzing, Parish, Vogt, Brodmann, Lœwenfeld, Döllken, Hellpach,

Hirschlaff, Sidis, Bechterew. The works of these and other authors are valuable from a psychological point of view, and I shall refer to them later on in the chapter dealing with the theory of hypnotism. It is not necessary for me to discuss them here at any length, as my object is to arrive at the briefest definition of suggestion possible.

Every concept in human beings has a particular action, which is to be recognized by an external or internal effect. For example, by the laws of association, one concept calls up another. The idea of St. Helena at once awakens that of Napoleon I.

This peculiar arousing of ideas by other ideas was called the law of suggestion by a great school of Scotch psychologists (Thomas Brown and others), and Paul Janet thinks that this expression induced Braid to introduce the term "to suggest" for an analogous phenomenon—*the suggestion d'attitude* which we shall discuss later on,—though Brandis used it earlier in his *Psychische Heilmittel*, to denote processes which ideas arouse during magnetic sleep.

A concept can, moreover, produce an effect by arousing feeling; if any one thinks of a dead relative, he feels grief, and the thought of a joyful event awakens a feeling of happiness. Inclinations are called up in the same way; the idea of an object for which one has a great longing awakens the desire to possess it. Sensations can also be produced in the same manner. We have an example in the itchings which many persons feel directly fleas are talked of. These ideas, feelings, sensations, and desires, aroused by another idea, form internal processes, which we recognize by internal experience. But an idea can have an effect which displays itself externally—for example, thoughts call up certain movements.

Let us here consider a proceeding which is called thought-reading, which, as the "willing game," was for a long time a favourite society game in England in a somewhat modified form, and which became popular in Germany through the exhibitions of Cumberland. A person A. is made to leave the room; among those who remain, B. is chosen to think of some object present, which A. is to find. A. comes back, takes B.'s hand and demands that he shall think steadfastly of the chosen object; let us say it is the lamp. B. thinks steadfastly of it, and it is seen that A. and B. go together towards the lamp, till A., pointing to it, says, "That was the object thought of." Simple as this process—explained by Beard, Gley, Richet,

Obersteiner, Preyer, and known about sixty years ago to Chevreul and Braid—may be, it appeared enigmatical to many at first. The usual explanation of thought-reading is the following:—B. thinks steadily of the place of the lamp, and has at the same time slight movements of the body, and particularly of the muscles of the arm, in the direction of the lamp. A. feels these muscular movements and follows them, he permits himself to be directed by them, and in this way finds the object thought of. B., naturally, did not make the movements intentionally, consequently they were involuntary and unconscious. All the same, the movements were strong enough to show A. the way. This example shows us the following:—B. had a certain idea (namely, that of the lamp) in his head, and this concept called up the movements. The movement of the lips which occurs where one thinks intensely of a word, is of analogous character (Stricker).

We see, then, from the foregoing, that ideas aroused in us have an effect which sometimes shows itself as other concepts (ideas, sensations, and so forth), and sometimes externally as movement; in many cases, perhaps in all, there is both an internal and an external effect. What effect appears, what idea, what feeling, what movement will be induced by the first concept, depends upon the individuality of the person, upon his imagery, upon his character, his habits, and upon the species of the concept.

In many cases a person, A., is able to attain some particular effect intended, by rousing in B. a definite concept; and this effect is often obtained independently of B.'s will, or even against it. We see an example of this in a juggler. He wants to take some object or other in his right hand without being seen by the public; to attain this he looks at another point—for example, his left hand. The eyes of the spectators involuntarily follow his. By glancing at his left hand the juggler has caused the spectators to look in the same direction. He has aroused in the spectators the idea that something is going on in his left hand; and this idea has had the effect of making the spectators look at his left hand. It is not at all necessary that the spectator should be conscious of the idea which is being impressed on him. Habit impels him to look in any direction indicated, without further consideration; and thus spectators are often induced to look in the direction desired by the juggler, in order that he may be able to change or hide some object unobserved.

Naturally, the juggler takes great care not to direct the spectators to look in any particular direction. If he were to do this the spectators would discover his object, and would not look at the spot which the juggler wishes, and the latter would not attain his end.

On the other hand, there are also cases in which a desired effect is attained simply by assuring the person concerned that the effect would appear. In most cases he is able to present arbitrarily the appearance of such an effect; but not always, however. An example brought forward by Bonniot should make this clear. One says to a person who is embarrassed, "You are getting red in the face now!" It is well known that many people really blush when the conviction that they are blushing is aroused in their minds. Now, a proceeding of this nature is called a suggestion, and should enable us to form a conception of what "suggestion" really means. We have seen that an effect (blushing) has been produced by merely suggesting the idea of it. If we base our conception of suggestion on the process just mentioned, then suggestion itself is a method of producing an effect by calling the imagination into play. This definition, which I laid down long ago, considerably restricts the whole conception of what is meant by suggestion, when compared with other definitions. For example, when Bernheim so defines suggestion as to include *l'acte par lequel une idée est introduite dans le cerveau et acceptée par lui*, we cannot under any circumstances in the present day accept such a wide concept. Other authors also, such as Bérillon, go much too far in their definition of suggestion: according to them we should eventually be compelled to include every possible kind of influence—reason, tuition, enlightenment—in our idea of suggestion. In spite of this, Hirschlaff objects that my definition is much too wide in its scope. Hirschlaff thinks it would include the source of many convictions which are brought about in a perfectly normal manner, and which have nothing whatever to do with the process of suggestion. "For example, if I say to an acquaintance, 'Some one has just told me that fire has broken out in your house during your absence,' my friend's motor and emotional spheres are both affected by the conviction which my statement has brought home to him. But that is not a case of suggestion." This last remark is perfectly correct, but in nowise contradicts the definition given above. In Hirschlaff's case the motor and emotional results did not occur *because*

external influence had aroused the notion that such results would occur, but rather that the person in question was logically convinced from previous experience that what he had been told could be believed, his informant having no interest in telling an untruth, being therefore, so far, trustworthy. As Bechterew also has insisted, the most important point in suggestion is that the influence must be of a direct nature, because it is not the *Ego* of the subject which brings about logical convictions and the like. Consequently, if the above definition be properly interpreted, there is no necessity to add to it. It must, of course, be admitted that misapprehensions are always possible: this we have seen from the very nature of the objections raised by Hirschlaff. Still that is no reason why we should not join William Hirsch and Lipps, and include the production of an effect, even when the conditions are inadequate, among the characteristics of hypnosis. If I tell a person on whose forehead a fly has settled that he feels an itching in that region, and he does really experience the sensation, that is not a case of suggestion. For here we have the adequate condition —the irritation caused by the fly. Even if in the case just stated, the person concerned were so lost in thought that he did not feel the itching until I called his attention to it, we should not be justified in speaking of suggestion. The mere fact of attracting his attention, coupled with the irritation caused by the fly, would be quite sufficient to make him feel the itching. Similarly in Hirschlaff's case, the man's belief that fire had broken out in his house was aroused by conditions which must be considered adequate; on the other hand, if he had been sitting hypnotized in his room and had believed the room to be on fire because he was told that such was the case, the physical conditions would not have been adequate. Anyway, in order to avoid any misunderstanding, we may formulate our definition as follows:—Suggestion is a process by means of which an effect may be produced, even when the necessary physical conditions are absent, by arousing the notion that the desired effect is about to be produced.

What is understood by inadequate conditions has been discussed by Lipps in his well-known essay, *Zur Psychologie der Suggestion*. "Sense-stimulation is undoubtedly the adequate condition for arousing perception. As far as the conclusion arrived at is concerned, there are various adequate conditions or means. Thus perception, reflection, or the persuasion of

another may all be considered adequate conditions for my arriving at a conclusion. I can sum all this up in single expression—the adequate condition for arriving at a conclusion is 'a rational basis.' Consequently, when a conclusion is induced by suggestion the rational basis is wanting. Finally, consciousness of the value of any object—the pleasure it may produce, its utility, or its ethical or æsthetic worth—is sufficient to bring about a voluntary action. It is possible that custom and some other factors may here play a part."

It is not difficult to perceive that there is no definite limit to "inadequate conditions," since they depend so much on the individuality of the subject. As Lipps points out, the special value of anything may prove an adequate means of inciting a voluntary action. But the concept of value differs with different individuals. In spite of all this, it must be admitted that by recognizing these "inadequate conditions" we can more easily grasp the notion suggestion process.

Many other attempts have been made to define suggestion, but in some instances theory and definition have been confused. To a certain extent this objection applies to those definitions in which the associative disposition is held to be the chief characteristic of suggestion. The definitions given by Schrenck-Notzing, Schaffer, Lœwenfeld, and Wundt belong to this category, although they contain much that is valuable from a theoretical point of view. There are also other definitions which do not differ very much from that laid down by Lipps. For example, Hirschlaff considers the production of an effect without a tangible cause to be the most important point in suggestion. He lays stress on the absence of motive, while others, like Dubois, consider that the effects of suggestion are produced in a somewhat mysterious manner. Vogt holds suggestion to be the abnormally powerful action of the desire to attain an end. I myself believe that the definition I have given will suffice to carry us through our further investigations.

We shall now see that suggestion plays an extraordinarily great part in hypnosis, and it may be remembered I have already given numerous instances of suggestion in cases I have referred to. The Nancy method of inducing hypnosis is of a similar nature; an effort is made to induce the patient to believe that he is going to be hypnotized, and when the effort is successful hypnosis follows.

Now, there are cases in which the notion of an effect may

apparently arise spontaneously, without the intervention of a second person, and the effect itself be produced, even when the subject is unwilling. We often meet with this in disease. I have already mentioned the case of a man blushing because he was firmly convinced that he was blushing. But there are cases in which people blush without anybody interfering with them. The notion of blushing is to a certain extent personal, and is called by pathologists the "fear of blushing." Fear and imagination here run hand in hand. As soon as such a person imagines that he or she may blush, then he or she blushes. Here the idea of blushing is not aroused by a second party; it is purely personal, and we therefore term it a case of auto-suggestion in contradistinction to a notion aroused by some other person, which Bentivegni calls external or hetero-suggestion. Auto-suggestion of this nature plays an important part in certain pathological conditions. There are many stammerers who only stammer when they think they are going to stammer, but who can speak quite well when they do not think about stammering. Many an impulsive idea may be ascribed to auto-suggestion, although Lipps attempts to draw a distinction between the two. For example, a patient suffering from agoraphobia is filled with the fear that he cannot cross an open space alone. No reasoning is of avail here; the patient acknowledges its justice without permitting it to influence him. Often—but not always—logic is for the most part powerless over these auto-suggestions. Many hysterical paralyses are also auto-suggestions; the patient cannot move his legs because he is convinced that movement is impossible. If this conviction can be shaken, movement is at once practicable.

Auto-suggestion may be called up by some external cause. Charcot referred some isolated traumatic paralyses to something of this kind. According to this view, a violent blow on the arm, with its resultant disturbances of sensibility, may produce in the person concerned a conviction that he cannot move his arm. As the conviction was called up by the blow, this case stands somewhere between external suggestion and auto-suggestion. We will call all cases in which the auto-suggestion did not arise spontaneously, but was the secondary result of something else, such as a blow, indirect suggestions, as opposed to direct suggestion, which arouses a certain idea immediately, of which I have already given several examples. It is not always necessary that there should be a conscious

mental act in suggestion; individuality and habit sometimes replace this, and play a great part in the training of the subject, of which we have still to speak. If some external sign, such as a blow on the arm, has several times, by means of a conscious mental act, produced the auto-suggestion that the arm is paralyzed, then the auto-suggestion may repeat itself later mechanically at every blow without any conscious thought of the effect of the blow.

A particular psychical state, disposing to suggestion, is a necessary condition of its appearance. The disposition to suggestion is called "suggestibility."

We shall now see that we can in this way obtain many effects by employing suggestion during hypnosis. We shall also see that we can produce these effects not only during hypnosis (hypnotic or intra-hypnotic suggestion), but that they extend to the time following hypnosis. This is post-hypnotic suggestion. By means of this we can tell the person in the hypnotic state that after his awakening a particular result will follow. We can also distinguish another kind of suggestion: something may be suggested to the subject before the hypnosis which is to follow in that state. This is pre-hypnotic suggestion (Maack, Stembo).

CHAPTER III.

THE SYMPTOMS OF HYPNOSIS.

I NOW come to the symptomatology of hypnosis. In order to make as complete a survey as possible, and only for that reason, I must arrange the subject-matter under the headings of Physiology and Psychology. It must not be thought, however, that we have to do with a real division; of that there can be no question. For the bodily functions show a deviation from the normal purely as a consequence of changes in the psychical state. Just as a man paralyzed by fright cannot move in consequence of a mental shock, and not from any injury to his muscles, so people in a state of religious excitement have visions, not because their eyes are abnormal in visual function, but because they are in an abnormal mental state; thus in hypnosis the muscles, the organs of sense, etc., are abnormal in function only because the mental state is altered. Only from this point of view is the division made in what follows. As we shall see later on, it is doubtful whether there exist in hypnosis any primary bodily abnormalities.

In consequence of the close tie which exists between the mental and bodily phenomena, it will not be surprising if, in discussing the latter, I am often obliged to refer to the former and vice versâ; a thorough separation is not possible. In order not to destroy the inner unity for merely external considerations, I shall occasionally deviate from the purely tabular arrangement.

(1) *Physiology.*

We will now pass to a discussion of the functions of the individual organs. The alterations which we find in hypnosis affect the voluntary and involuntary muscles, the organs of sense, common sensation, the secretions, metabolism, and in rare cases also the cell power of organization.

The voluntary muscles show the most frequent abnormalities, and suggestion[1] exercises an extraordinary influence over their functions. When no ideas are aroused by suggestion during hypnosis, we find the greatest differences in the behaviour of the voluntary muscles, according to the method of hypnotization selected, and according to the character of the subject. Some are able to move with perfect freedom till the command of the experimenter inhibits some particular movement: many, on the contrary, look as if they were asleep from the commencement. In this case we see no movements, or very rare ones, which are slow and laboured. When we discuss the phenomena of suggestion, we shall see that this incapacity for movement cannot in certain cases be removed by the command of the hypnotist. Between these extreme cases there exist all sorts of transitional stages. It is all the same which of these characters has the preponderance; muscular activity can nearly always be influenced in a high degree by suggestion. By means of it we can make movements impossible, or else induce movements.

I have already shown (page 34) how easily I can make the second subject's arm powerless to move simply by arousing in her the conviction that her arm is powerless. In just the same way the movements of the legs, trunk, larynx, etc., escape the subject's notice. "You cannot raise your arm; cannot put out your tongue." This suffices to make the forbidden movement impossible. In some cases the inability to move arises because the person cannot voluntarily contract his muscles, and his arm consequently hangs limp; while in other cases a contracture of the antagonistic muscles makes every attempt at a voluntary movement useless (Bleuler). In the same manner the leg will lose the power of motion at command. We have seen (page 34) in the second experiment how the power of speech can be taken away. And it is even possible to allow the muscles to contract for one particular purpose only. If we say to a hypnotic subject, "You can only say your own name; for the rest you are absolutely dumb," the desired effect will be produced. In the same way it is possible to prevent movements of the arm for any particular purpose. Thus we can make it impossible for a person to write, though he will be able to do

[1] For the sake of brevity I shall for the future use "suggestion" for "external suggestion" when the contrary is not expressly stated, and I shall treat cognate terms in a similar manner.

any other kind of work. The subject can sew, play the piano, etc., but all efforts to write are vain. As a rule, the movements only become possible when the experimenter gives permission. It is remarkable that in some persons one set of muscles is easier to influence by suggestion, and in others another set. We can make a person dumb by suggestion, while all the other muscles obey his will in spite of suggestion. Another, again, loses the power of moving his arms, while his speech remains unaffected.

In just the same way as muscular movements are prevented by suggestion, so can movements be induced by it against, or without, the will of the subject. We have seen (page 35) how the subject in the third experiment knelt down, followed me, and so forth. I say to another person, "You are lifting your right arm to lay it on your head"; this happens at once. I would insist that it must be decided whether these movements take place without or against the will[1] of the subject, as in the latter case an increase of sensibility is already demonstrated. I say, "Your left arm will now rise up in the air"; and the arm rises as if drawn up by a string, although the subject makes no voluntary movement; but neither does it occur to him to resist. The movements without the subject's will can often be distinguished from those against it by a certain steady ease. These last are nearly always characterized by strong muscular contractions and by trembling—evidences of antagonistic forces, the hetero-suggestion of the hypnotizer, and the will of the subject. This shows how the hypnotized person fights against the suggestion to keep his arm in the air, and also gives a clue to the tremblings.

Just in the same way the hypnotic subject is obliged to cough, laugh, talk, jump, etc., at command.

It is further possible to generate by suggestion the idea of a paralysis of one of the extremities. These isolated paralyses have a great resemblance to the psychical paralyses arising without hypnosis, such as Russell Reynolds described in 1869 as "paralysis dependent on idea," and Erb later on as "paralysis by imagination." Charcot's pupils have tried to find objective symptoms of these paralyses that depend on suggestion. It

[1] It is not my intention to employ expressions other than those generally understood; hence my use of the word "will" here and in some other places. From a strictly psychological point of view it would possibly be more correct to use "arbitrary volition" or "wish."

cannot be doubted that such objective changes may occur through a particular association of symptoms; this hypothesis is supported by Krafft-Ebing also. We must, however, recognize that this is not the rule. According to Lober, Gilles de la Tourette, and Richer, the clinical characteristics of these paralyses are marked by the absolute loss of motor power and sensation, increase of the tendon reflexes, ankle clonus, at times wrist clonus, complete loss of muscular sense—*i.e.*, of the ability to control perfectly the action of the muscles and to be certain of the position of the limbs,—changed electrical excitability, and vasomotor disturbances; these last are particularly said to show themselves by a bright flush of the skin on slight stimulation. Vogt also found that the sensory and motor phenomena were not altogether unconnected. When investigating the nature of a case of anæsthesia of the arm produced by suggestion, he observed that the limb had lost all power motion. But directly the patient was firmly impressed with the idea that he could move his arm, there was return of the power of movement, and of sensation as well. Paralysis produced by suggestion may be of hypnotic or post-hypnotic origin. Besides these paralyses in which the muscles are completely relaxed, other paralyses, in which the muscles are persistently contracted, can be induced by suggestion.

With these subjects who are deprived of will, besides the movements described above, complicated movements, or even performances (if I may be allowed the expression), also take place by suggestion. I say to the subject, "You will spin round three times," or, "You must lift that thing off the table; you must go and do it; you cannot help it." The subject does as he is told.

The suggestion itself is made in different ways. The main point, and all turns upon this, is that the subject should thoroughly understand what the experimenter wishes. Each of the organs of sense is a door of entrance for suggestion. The most common is naturally our habitual means of communication—speech (verbal suggestion)—by means of which we tell the subject what we wish. But it is very important, and often much more effective than words alone, that the experimenter should perform the movement which the subject is intended to execute. Professional magnetizers in particular habitually induce movements by imitation.

Imitation appears particularly in a hypnotic state which certain authors (Brémaud, Morselli, Tanzi) have studied, and which Descourtis calls fascination or captation. I have shown (p. 35) in the third experiment a case of this kind. A professional magnetizer, Donato, has demonstrated this state completely; and Morselli and others have on this account called this form of hypnosis Donatism. As I saw in Paris, Donato uses a particular process to bring about this state. This process aims at a primary forced contracture of all the muscles of the body, in order, by this means, to limit the voluntary movements as much as possible. In this case the eyes of the hypnotist and the subject are firmly fixed on one another from the beginning. The subject finally follows every movement of the experimenter. If he goes backward, the subject follows; if he comes forward the subject does the same. In the same way the latter imitates every movement of the experimenter—only on the condition, however, that he knows he is intended to do so; that is the main point. We saw in the third case that fascination may be a primary form of hypnosis. But it can also be originated secondarily from other hypnotic states; and this is more usual. When the experimenter has hypnotized the subject in some other way, and has made him open his eyes, he can fix his own steadily on them, and thus induce the same phenomena. A variety of this fascination is to fix the eyes of the subject on some other object—for example, on the finger of the experimenter. In this case the fascinated person follows every movement made by the experimenter.

But imitation plays an important rôle in hypnosis, as well as fascination. This results from the fact that the sight of a movement arouses a much more vivid picture of it in the hypnotized person than does a mere command.

Verbal suggestion is also made easier by other gestures. In order to compel some one to kneel down, an energetic movement of the hand accompanying the verbal suggestion is very effective, as in the third experiment. With this fact is connected one of the phenomena which magnetizers are fond of demonstrating—namely, the drawing of the subject after the experimenter, who makes movements with his hand which show the subject that he is intended to approach. The experimenter can also repel the subject in the same way. This succeeds in particular by means of movements of the hand,

indicating that he is to go away. It is not at all necessary, in this case, that the subject should see the movements of the experimenter; it is sufficient that he should divine them either from a noise or a current of air; thus the hypnotic obeys the experimenter even when he has his back turned towards him. Upon suggestion also depend the attraction and repulsion of single limbs of the subject, which happen in the same way through the hypnotic's perception of the experimenter's gestures. Without uttering a word the experimenter can make the subject raise and drop his hand merely by gesticulating with his own; he can also obtain many effects by a glance only. It is not necessary to look steadily in the eyes of the hypnotic, as in fascination. The operator looks at the subject's leg—it at once becomes powerless to move. The hypnotic is going away—the experimenter suddenly looks at a spot on the floor, and he stands chained to the spot.

We all know the effect which may be produced by a look, even when the person looked at is not hypnotized. A tutor will look fixedly at a pupil whom he suspects of lying in the hope of producing an effect. The look with which a tamer of wild beasts fixes his animals may be included here, in spite of the many fabulous statements which have been made about it; also the fixed gaze by means of which a serpent renders its prey incapable of moving. I may further remind my readers of the "fascinating gaze," and the "evil eye" by means of which an evil influence was supposed to be exercised. In Southern Europe, as well as among the Jews of Northern Africa, the dread of the evil eye is strongly marked. Various symbols here serve as a protection against it (Fitzner). In Italy people who believe in the evil eye hold their hands in a particular position when they fear its effects, as I have often had the opportunity of observing. During my residence in Palestine I was frequently able to verify that the belief in the evil eye is still widely spread among Mohammedans and Christians (Preyer, Einszler). Siegfried has collected various passages from the Bible and the Talmud which prove a belief in the evil eye.

Not only articulate speech and gestures, but music also has a suggestive effect. Mesmer long ago recognized the influence of music, and used a then newly-invented instrument, the bell-harmonica, to obtain the necessary effect. If dance music is played, the subject will dance, following the rhythm; and when the dance is changed to another, he alters his step to correspond. I shall have occasion to refer to the influence of music again later on.

The muscular sense which keeps us informed of the position of our limbs, and by means of which "catalepsy by sugges-

tion"[1] is induced, requires particular consideration as a way of entrance for suggestion. It is also to be found in other states than hypnosis—for example, in some cases of typhoid (Bernheim)—and frequently occurs in hypnosis. I lift the arm of a hypnotic, hold it in the air, then let it go; the arm remains as I placed it, although I say nothing. Why does this happen? Because the subject believes that the arm must remain thus. Another person lets his arm fall; I raise it again, and say at the same time, "Your arm keeps still"; this happens; but only because the person now knows that this is intended, while he did not understand the simple raising of the arm. Let us return to the first subject. I raise the arm again, saying at the same time, "Now the arm falls down," which, in fact, happens; but only because the person believes that he is to let it fall. The legs, head, trunk, and so forth can be put into different postures and maintained there in the same way; the muscular sense here suffices to transmit the suggestion. The inclination of the subject to maintain cataleptic positions is so great that Heidenhain considered the hypnotic state to be a catalepsy artificially produced. Catalepsy by suggestion has nothing whatever to do with physical changes in the muscles.

The main point for the attainment of catalepsy is that the subject should accept the idea of the corresponding attitude. Consequently the idea must be allowed to act for a considerable time before the desired result can be obtained. Words answer the purpose as well as other signs; many persons can only be thrown into catalepsy when the experimenter insists on the attitude required being maintained for some time.

The mesmeric passes (p. 34) which we have already become acquainted with as a method of producing hypnosis, here deserve especial mention. These mesmeric passes can be used locally in hypnosis—for example, over an arm, in order to make it cataleptic. Cataleptic attitudes which cannot be produced by verbal suggestion may often be obtained in this

[1] As the most different views exist as to what "catalepsy" means, I remark here that, for the sake of brevity, I shall so name any state in which voluntary movements disappear and the limbs remain as they are placed by the experimenter—without having regard to the length of time which elapses before the limbs move freely again, or fall from their own weight.

way. As far as I have been able to study these phenomena,[1] it is unnecessary in their case to imagine an especial force as an explanation; according to my view, the efficiency of the mesmeric passes results from the fact that by means of them the whole attention of the subject is directed to his arm for a long time. By this means the idea has time to take root. Let any one allow his arm or his leg to be mesmerized in this fashion and he will find that his whole attention is directed to this part of his body, and much more forcibly than if the attention was concentrated on the limb in any other manner. From this it follows that contractions often only appear when the mesmeric passes have drawn the attention for some time to the part of the body concerned. Passes with contact act in exactly the same way as passes without contact. In any case it must be remembered that the effect only appears when the individual has an idea of what is intended to follow. That centrifugal passes call up contractions, and centripetal ones dissipate them, has often been averred; but here we have to do with unintentional suggestions. I have as often been able to do the same thing with centripetal passes as with centrifugal.

We thus see in what manner suggestion affects the voluntary muscles. It is often observed that muscular activity aroused by suggestion has a tendency to persist. Thus a certain state of contraction is continued for a long period—there is, in fact, a contracture; or a long-continued movement may set in; or, finally, when the muscles are relaxed, a contraction of them can only be obtained with difficulty or not at all. Sometimes when the particular muscular activity has been primarily aroused by suggestion, the experimenter cannot at once counteract the effect of a primary suggestion by a second.

These cases, in which the action of the original suggestion cannot be easily inhibited, recall certain forms of insanity, such as melancholia cum stupore. Bancroft has shown that the disturbances of movement in this and other insanities can be brought about by a primary psychic process, even by a delusion, but that when the cause has passed, the disturbance of movement may persist automatically.

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[1] I have published many experiments bearing on this point in a book of mine, *Der Rapport in der Hypnose, Untersuchungen über den tierischen Magnetismus.* Publications of the Society for Psychological Research, Parts 3 and 4; Leipzig, 1892.

Vincent denies that there is any tendency for the suggestion to become fixed in hypnosis; he has always been able to remove the suggestion at once. Nevertheless, he remarks in a subsequent passage that he has sometimes found opposition during several seconds; but that is only to be regarded as an auto-suggestion; whether one refers it to auto-suggestion or not is a matter of indifference. The phenomenon is found in a certain group of cases.

I order a person to stretch out his right arm stiffly. The arm is stretched out, and the subject is unable to bend it of his own accord. In most cases, directly I command the arm to be bent it can be done; but there are some cases in which the experimenter is unable to put an end to the contracture at once, but the effect of the earlier idea continues.

A particular movement can also be continued for a long time in the same way. The so-called automatic movements described by Liébeault and Bernheim, or continued movements, as Max Dessoir more properly terms them, belong to the same category. If we turn the arms of an hypnotic round and round each other, the tendency to continue the movement persists, even after the experimenter has ceased to compel it by stopping turning his own arms. This also happens when the subject believes he has to go on turning. In some cases he continues turning his arms passively, while on other occasions he makes the strongest possible effort to keep them still, particularly when requested to do so. Such resistance is, however, useless; but in most cases a new suggestion of the experimenter that the arms shall stop is enough to arrest the movements at once. Sometimes, however, the experimenter finds it impossible to obtain an immediate arrest by a counter-command. I have often observed that a movement has continued for some time in spite of my order. We can also induce alternate movements of drawing up and stretching out in the arm and leg, and nodding and shaking of the head, etc. A particular movement can also be continued for a long time in the same way, when once started. Again, I lift up an arm and bend it gently at the elbow-joint; directly I let go it repeats the movement. If it is desired that the hypnotic shall walk and he does not obey the command, let him be pulled forward a little; he will then when left to himself continue to walk (Heidenhain). The involuntary laughter, which I have often heard, is connected with this; it begins on command, or on a slight provocation. There are also pathological cases of uncon-

trollable laughter, showing decided resemblance to these cases of hypnosis; Feodoroff, who has published several observations, refers it to a weakening of the will and, consequently, to a similar condition to that found in hypnosis. This laughter in hypnosis also resembles the cheerfulness produced by haschisch; under the influence of this drug, expressions which are quite insignificant excite involuntary laughter (Moleschott). Obersteiner has observed the hypnotic laugh in his own case, and has described it.

In some cases the passivity of the subject is so great that the suggestion of the experimenter is unable to overcome the muscular relaxation. Subjects of this kind let their arms drop after they have been raised, in spite of all suggestions. Questions are not answered, or only slight movements of the lips show that they have been heard at all. Two different types of hypnosis, which are called active and passive, may be distinguished by the absence or presence of this muscular relaxation. The passive form has a great external likeness to natural sleep, while the latter might be taken for a normal state on superficial observation. Passive hypnosis is not considered by some authors (Braid) as a form of hypnosis, but is considered to be a sleep, because the especial symptom is wanting which those investigators regard as the necessary characteristic of hypnosis—namely, catalepsy. This does not appear to me absolutely necessary in order to show hypnosis. Hypnosis often shows itself as passive at the beginning: as soon as the eyes are closed the head drops forward or backward, because the supporting muscles of the neck are relaxed. There are many transitional states between active and passive hypnosis, and one often passes into the other.

The motor disturbances which appear in the eye must here be particularly discussed. We have already seen that many hypnoses are characterized only by the closing of the eyes—a fact already known to Heidenhain—while in many cases this is added to other symptoms. But closing of the eyes can also be influenced by suggestion, and an order of the experimenter is enough in most cases to cause their instantaneous opening. Closing of the eyes greatly favours the appearance of other hypnotic phenomena, but is not absolutely indispensable. There are persons who can be thrown into the deepest stage of hypnosis by a deep gaze, without the eyes closing at all (Gurney).

Although, as we have seen, closing of the eyes is not a necessary preface to hypnosis, yet the eyes are in most cases closed, and it is often impossible to permit them to open without ending the hypnosis at once. Even when the eyes open during the continuance of hypnosis, there is in many cases a certain heaviness in the lids and a desire to close them. Much depends, however, upon the method employed; and primary fascination in particular always occurs while the eyes are wide open. The closing of the eyes is sometimes very gentle, and not spasmodic; though in a number of cases the muscles which close the eye contract spasmodically. Braid and Heidenhain already pointed out that when the lids close, even in the deepest hypnosis, the closing is not always complete. There is often a little chink of opening, and this is not unimportant, because many experiments in "clairvoyance," and also pretended "reading with the pit of the stomach," may be explained by the ability to see through this small opening.

While the eyes are closed the lids not unseldom have a vibratory movement; but this symptom is of no real importance for diagnosis, as on the one hand it is sometimes wanting, and on the other hand often appears without hypnosis. The eye-balls often roll upwards as the eyes are closing. While in some cases this position of the eyeball is maintained, in others the eye-ball resumes its natural position directly the eyes are closed. If this is not the case, the white sclerotic only is visible when the lids are artificially raised. I have only been able to find the convergence of the pupils during hypnosis described by some observers in one case of hystero-epilepsy and in a few others of severe hysteria. Ancke, an ophthalmologist, has recently described this convergence in the case of the so-called "sleep-dancer" Magdeleine as caused by a squint in the right eye during hypnosis. Borel also affirms that convergence can occasionally be obtained by suggestion.

As we have seen, the voluntary muscles are entirely under the influence of external suggestion during hypnosis. A further peculiarity is, that a particular movement or state of contraction of the muscles cannot always be controlled at once, and finally we have seen that in some cases muscular contraction can only be brought about with difficulty or not at

all. *One of these two functional abnormalities of the muscles exists in all hypnotic states.* Though it is occasionally confined to inability to open the eyes, in other cases the functions of other muscles of the body are affected.

The different phases result from various combinations of the above-mentioned abnormalities, and from their different localization in the muscles. The various kinds of catalepsy arise in this manner. Bernheim distinguishes several forms of this catalepsy, according to the facility with which the cataleptic position can be changed. Sometimes this is very easily done, sometimes it is more difficult, as in tonic contracture; the *flexibilitas cerea* forms an intermediate stage. These different kinds of catalepsy are matters of training and suggestion (Berger). I have hardly ever clearly seen a typical *flexibilitas cerea* in hypnosis, except when the training of the subject had been directed to that point. Nonne appears to have collected other experiences regarding it.

One of the best known features in hypnosis is the rigidity of the whole body. There is here tonic contracture of numerous voluntary muscles, by means of which the head, neck, trunk, and legs become as still as a board. A well-known experiment can be carried out in this state: the head can be placed on one chair and the feet on another, and the body will not double up. A heavy weight, that of a man, for example, may even be placed on the body without bending it. It is not astonishing after what I have said of the effect of the mesmeric passes, that this stiffening should be more easily induced by their means; it cannot always be induced by mere verbal suggestion. A command or sign of the experimenter generally suffices to put an end to the rigidity.

We must now ask whether any further abnormalities appear in the voluntary muscles during hypnosis. Changes which are not supposed to be of a psychical nature have often been assumed; for example, the English committee mentioned on page 22 detected an increase in the motions made in swallowing. It is frequently maintained that reflex action is altered in hypnosis, and that reflexes appear which do not occur in normal conditions. Heidenhain, Charcot, and Obersteiner are to be mentioned among those who first expressed this view, and later on Schaffer.

Charcot based his classification of the hypnotic states upon the alteration of the reflexes. He distinguished a *grand hypnotisme* and a *petit hypnotisme*. The last he does not describe in detail; in the first, which is found in hystero-epilepsy, he distinguishes three stages:—1. The cataleptic stage, which is produced by a sudden loud noise, or results from the opening of the subject's eyes, while he is in the lethargic stage; in this stage the position of the limbs is easily changed, while the hypnotic's eyes are open. Every position which is given to the limbs is maintained for some time, but is also easily changed by the experimenter without resistance on the part of the subject; there is therefore no wax-like flexibility (*flexibilitas cerea*). No tendon reflex, no increase of muscular irritability. There is analgesia, but it is often possible to exercise a certain influence over the subject through sight, hearing, and the muscular sense. 2. The lethargic stage. It can be induced primarily by fixed attention, or secondarily out of the cataleptic stage by closing the eyes. The patient is unconscious and not accessible to external influences, and there is analgesia. The limbs are relaxed and fall by their own weight; the eyes are closed, the tendon reflexes increased. There is increased excitability of the muscles, the so-called neuro-muscular hyper-excitability. These increases are demonstrated by mechanical stimulation of the muscles, nerves, or tendons. For example, if the ulnar nerve is pressed, a contraction of all the muscles which it supplies follows, so that a characteristic posture of the fingers results. This contraction is not brought about by direct irritation of the motor fibres, but is a reflex, the sensory fibres of the ulnar nerve conveying the irritation to the central nervous system, where it is converted into a motor impulse. If a muscle is mechanically stimulated, it alone contracts. The same thing is attained by this as by local faradization in normal states, which was shown by Duchenne. While at the extremities the contraction passes into contracture—that is, becomes permanent—a stimulation of the facial nerve only causes a simple contraction in the face, which soon ceases. The resolution of the resulting contracture is produced by exciting the antagonistic muscles; thus, for example, a contracture of the wrist is put an end to by exciting the extensors, the contraction of one sternocleido-mastoid by stimulation of the other. According to Charcot, the motor regions of the cerebral cortex can be stimulated through the cranium by means of the galvanic current, so that the muscles in connection with them contract. 3. The somnambulic stage. In some persons it arises primarily by means of fixed attention; it can be induced in all by friction of the crown of the head during the lethargic or cataleptic stages. The eyes are closed or half closed. By means of gentle stimulation of the skin the underlying muscles can be put into rigid contraction, but not, however, by stimulation of the muscles, nerves, or tendons, as in the lethargic stage. Also, the contracture does not disappear on stimulation of the antagonistic muscles as in that stage. The posture of the limbs produced by contracture in somnambulism cannot also be so easily altered as in catalepsy; a certain resistance appears as in *flexibilitas cerea;* Charcot calls it the cataleptoid state. The same stimulation of the skin which induced the contractures is employed to resolve them. In somnambulism many external influences are possible by means of suggestion, of which I shall speak later on in their proper connection.

With regard to these stages of Charcot, most investigators think that they are only an artificial product, the result of an unintentional training process. It is certainly striking that since the Nancy school pointed out the many sources of error that should be avoided, the stages of Charcot are less and less frequently observed. Wetterstrand never found them at all among 3,589 different persons (Pauly). I also have never been able to find them, even in hystero-epileptics, in spite of Richer's opinion that every one who experiments on such persons will obtain results identical with those of Charcot.

Even those authors, who on the whole accept Charcot's stages, admit there are many exceptions. For instance, Dumontpallier and Magnin consider that the increase of neuro-muscular excitability is by no means confined to the lethargic stage, but appears in all of them. They have likewise described certain conditions in which the symptoms, partly of the lethargic and partly of the cataleptic stages, show themselves. Jules Janet, also, has produced a fourth stage in Wit,—one of the best-known of Charcot's subjects—which is distinguished from the three others, both physically and mentally. Gilles de la Tourette describes a *léthargie lucide*, in which there was no loss of consciousness. In any case, the idea of the stages has become more and more confused as attempts have been made to include everything possible within them. Every one looked for the stages, and when he could not find them exactly as Charcot did, he believed himself obliged to add certain new characteristics to them.

It is perfectly certain that we cannot recognize Charcot's three stages as a general principle on which to base a classification of the various conditions met with in hypnosis. Even granted that the stages do exist, it must be admitted that they are confined to a few cases of severe hysteria. Opinions also differ as to whether, in such cases, the stages may not be an artificial product. Certainly Raymond maintains that all three stages were described by Despine as far back as 1840, under the names catalepsy, *somnambulisme mort*, and *somnambulisme vivant*, and that it is therefore improbable that the conditions observed by Charcot in his investigations were merely artificial products. Recently, too,—for example, at the second International Congress of Hypnotists, 1900,—several investigators have admitted, with more or less reserve, the possibility of Charcot's stages presenting themselves in some cases of hysteria; and Micheline Stefanowska has gone so far as to express the opinion, based on numerous experiments, that the *grande hypnose* as understood by the Salpêtrière investigators can be induced in frogs, in whose case there can be no

question of suggestion. In particular, M. Stefanowska thinks she has clearly established both the lethargic and cataleptic stages.

But even supposing we hold the opinion that Charcot's three stages were solely produced by artificial means, there still remains the question whether in hypnosis the muscles are responsive to a physical (not suggested) stimulus, which would produce no effect in the non-hypnotic state. Heidenhain stated the same thing much earlier; he believed that a gentle stimulation causes the underlying muscles to contract, and that the contracture produced spreads in proportion to the strength of the stimulation. In this manner Heidenhain considers the rigidity which is seen in hypnosis, to be a reflex. Like Born and others, Heidenhain believed he had discovered new reflexes in the course of his experiments.

Without wishing to maintain that specific physical reflexes do not occur during hypnosis, I certainly think that no definite proof of their existence has been brought forward. For instance, Lehmann reports that Dr. Hütten, who has submitted more than 6000 persons to hypnotic treatment, has never observed anything comparable to hyper-excitability, unless he had suggested that phenomenon. Nowadays, when we know that these phenomena can all be brought about by suggestion, the latter must first of all be excluded. The best way of accomplishing this end is to establish a very strict supervision of the first experiment performed on a subject; for it cannot be avoided, that when the same experiments are repeated many times certain intimations—some remark or motion made by the experimenter—should be given, from which the subject draws conclusions as to what he is expected to do. We have to take into account the quick perception which a hypnotic possesses and his desire to carry out every suggestion made by the experimenter. Certainly, Charcot's pupil, Richer, maintained that in their experiments, which were varied a thousandfold, the results were always identical, and that imitation was excluded. But we may conclude from a statement of Vigouroux, who excludes the deltoid muscle from the law of neuro-muscular activity, that the thing is not so plain. Gilles de la Tourette also states that the results were only attained after long previous experiment. But, however great may have been the tendency occasioned by the rise of the Nancy school to deny the occurrence of the physical

reflexes mentioned above, defenders of those reflexes continue to crop up from time to time, and quite distinct from Charcot's own pupils.

Among these we must reckon Obersteiner, who describes the following experiment performed on himself:—"When I was in light hypnosis and the skin of my hand on the ulnar side was stroked, I expected that the little finger would be bent; instead of that, to my surprise, it was abducted. As a matter of fact, beneath the irritated portion of the skin lay not the flexor, but the abductor digiti minimi, of which, in my half-sleeping state, I had not thought." However exact this statement may be, I would not accept it as an argument against the suggestive origin of contractures. That Obersteiner, after hypnosis was over, believed he had not thought of the abductor proves nothing, for there might very well have been a deception of memory. Even if we admit that this was not the case, Obersteiner knew the exact position of the muscle, although the fact never came into consciousness during hypnosis. I may here refer to the sub-consciousness which I shall have to speak of more particularly later on. We shall then find that experiences that we have once gone through, but which have momentarily become unconscious, may still influence our action. For this reason Obersteiner's experiment is not an unconditional proof of his view.

Schaffer, also, has attempted, in a very exhaustive work, to demonstrate the existence of physical reflexes in hypnosis. He was able to produce contractures by various sensory stimuli, by stimulation not only of touch, but of the ear, and especially of the retina. In unilateral stimulation the contractures always appeared on the stimulated side. Schaffer believes that he has excluded suggestion. In bilateral stimulation the contractions appeared on both sides. When one side was made anæsthetic, sensory stimuli on that side were inoperative. On the other hand, Schaffer could also produce contractions by suggesting any sensory impression. The same thing then happened as if the sensory stimulus had really been applied. On the ground of this observation, Schaffer concluded that the reflex path is through the cerebral cortex; yet he does not consider himself justified in admitting suggestion. Schaffer also points out that Laufenauer and Högyes have established the existence of neuro-muscular and senso-muscular hyper-excitability under conditions in which suggestion was excluded. The mere use of the term "suggestion" is certainly no proof of the inaccuracy of all these investigations. For even if, as often happens, due attention is not paid to suggestion as a possible source of error, we have no right to ascribe everything straightway to suggestion; on the other hand, Schaffer gives us no definite proof of the absence of suggestion in the production of the reflexes in question.

There is another point which I must also discuss, and which seems to tell against the mental origin of many of the phenomena. Schaffer endeavours in his work to weaken one of the arguments brought forward by me against the conception that the phenomena are induced by physical means. Now, we saw in the fourth experiment (p. 35) that during deep hypnosis the subject could only be influenced by one person, the experimenter with whom he was *en rapport*, as the phrase goes. As we here saw, the experimenter alone could induce contractures, attempts at

stimulation by other persons were unavailing. Charcot's pupils also mention this phenomenon. They state that during the somnambulic stage only the few persons who are *en rapport* with the hynoptized subject can influence the latter's muscles by stimulating his skin. That the contractures should arise without participation of consciousness would be incomprehensible. Against this, Schaffer asserts that in his experiments any one could induce the contractures. That would apparently tell in favour of the upholders of physical influence. To this we may reply as follows:—All the cases in which contracture can only be induced by the experimenter prove that mental influence is at work. It very far from follows that suggestion plays no part in all those cases in which, in spite of the *rapport* existing between subject and experimenter, other persons can also induce contractures. Here, as is so frequently the case, we have to deal with the fact that a hypnotized subject may be trained to respond unconsciously—or, to express it better, subconsciously—to certain influences which may be exerted by all those present.

Schaffer having with particular acumen brought about a discussion of the question whether we have to do with reflex action or suggestion, it is necessary, if we would avoid any misunderstanding, to point out that physiologists very often associate two sorts of reflexes—the physical and the mental.

In order to make this clear I must say a few words about reflex action. We understand by reflex action of the muscles that action which is induced by excitation of a sensory nerve, without the co-operation of the will. When an insect flies into the eye, the latter closes; this closing is reflex because it is involuntarily. Let us take the following case:—I touch the eye of a person, A.; the eye closes in consequence by reflex action, without the participation of A.'s will. I now bring my hand near to the eye of another person, B.; long before it is touched it closes, and does so against B.'s will. The closing of B.'s eye is also reflex action; the stimulation here affects the nerves of sight. And yet there is a difference between the two cases. While in the case of A. no mental action is necessary to produce the reflex, in the case of B. it is otherwise. He shuts his eye because he imagines it will be touched—at least, this is the general opinion. If B. puts his own finger near his eye it does not close, because this idea does not then arise. In any case, a mental action takes place in B., but not in A. On this account we call the closing of B.'s eye a mental reflex, and A.'s a physical one. The mental reflexes are extremely common; stooping at the whistling of a bullet, laughing at the sight of a clown, sickness produced by a disgusting smell, are mental reflexes. The involuntary muscular action is caused by a stimulation of the eye, ear, or sense of smell, after the stimulation has been interpreted in a particular way by the consciousness.

The classification of the reflexes into physical and mental is not valueless for us; I think it better at present to keep to this classification, although it is only schematic, and although Lewes supposed a mental action in all reflexes. Gurney, Max

Dessoir, and Hückel have directed attention to the importance of mental reflexes for hypnosis. Heidenhain and Charcot denied any mental action in the contractures they induced; the Nancy school, on the contrary, believes that it occurs, that the subject knows what is intended to result, but that his will is unable to prevent the contracture; this is called a suggestion, and is therefore only a kind of mental reflex. Consequently the question put forward above, whether Heidenhain's and Charcot's contractures are reflexes, may be thus modified: Have we to do as these authors suppose with physical reflexes, or with mental ones?

The phenomena of echolalia (imitative speech), described by Heidenhain and Berger, belong to this action. Berger says that hypnotics will repeat everything that is said before them, like phonographs; even what is said in foreign languages is repeated with some exactness. Heidenhain and Berger believed that only particular tracts of the bodily surface (the stomach or the neck) are suitable for the production of this imitation by means of stimulation. They came to this conclusion because they were not as yet acquainted with the significance of suggestion. In reality the hypnotic echoes what he believes he is intended to echo. It is certain that some persons are able to perform great feats in this way, imitating a hitherto unknown language quickly and correctly, particularly after the necessary practice.

Other reflexes, also, which are induced by touching certain parts of the cranium, the appearance of aphasia, or of twitchings or contractures in the arm or leg, should be understood in the same way. Statements of this kind were made by Heidenhain, and have been repeated lately by Silva, Binet, and Féré. These last even believe that they can place single limbs in the somnambulic state by stimulating the parts of the head which correspond to the motor centres of the limbs concerned. They also refer to the phrenology of Gall, and Chalande even wishes to study the physiology of the brain in the same way (Delbœuf). Braid described similar phenomena, which he called phreno-hypnotic, and conjectured that there was a kind of reflex stimulus. By pressure on a portion of the skull a nerve was stimulated which by reflex action excited a part of the brain, and by this means aroused feelings of benevolence, for example; by stimulating another spot another nerve was excited, which by reflex action produced an expression of piety, etc.

Hypnosis of one side (hemi-hypnosis) may also be explained as a phenomenon of suggestion. Braid thought that by blowing on one eye the corresponding side could be awakened. Descourtis, Charcot, Dumontpallier, Bérillon, Lépine, Strohl, as well as Grützner, Heidenhain, and Berger, who were under Kayser's influence, carried on these experiments in various modified forms; Berger later on changed his views. Though

these authors regarded hemi-hypnosis as a physiological condition induced by the closing of one eye or by friction of one half of the scalp, their statements do not now prove their point. We know that we can produce all these states by mental influence, and suggestion must be excluded before the experiments can be considered conclusive. The statements made by the various authors are so contradictory on this point that they only increase one's doubts. Sometimes stroking the right side of the head was supposed to make the left half of the body hypnotic; sometimes the result followed on the right side. The main point is that the subject shall know what is intended to happen to him, and what effect is expected from the processes. Hemi-hypnosis has even been utilized for drawing such far-reaching conclusions as that the two cerebral hemispheres act independently of each other. Grasset has recently assumed this view, and considers the assumption justified even if the hemi-hypnosis is attributed to suggestion. I do not deny the relative independence of the functions of the two hemispheres; but hemi-hypnosis proves nothing that is not equally well demonstrated by every pianist who plays somewhat differently with his right hand than with his left, or by any individual who moves his right arm but keeps his left still, or tosses his right leg about while the left is allowed to remain at rest. I think it is better to have nothing to do with this sort of physiology.

It is evident, from the preceding considerations, that in hypnosis the influence of suggested ideas on the voluntary muscles is very extensive. Let us now see whether certain other peculiarities in the functions of the muscles, due either to suggestion or some other cause, appear in hypnosis.

The cataleptic posture of the limbs is sometimes maintained for a very long time, even for several hours. One person remained for seventeen hours in a cataleptic posture. Berger mentions the case of a young girl who maintained this condition without perceptible change for seven hours, during which she was continually watched. In these cases the fatigue and pain which ordinarily follow on great muscular exertion do not ensue. Great fatigue rarely results even when the same position is maintained for so long as an hour; still, this is not necessarily the rule. Certain differences which Binet and Féré think they have found between the curve-tracings in suggested

catalepsy and those obtained in the simulated form will be dealt with by me later on, in the section on Simulation.

Dynamometric investigations—that is, measurements of the muscular force—have often been undertaken during hypnosis. I myself have made a number of such experiments, which for the most part agreed with the results of Beaunis. The most important part appears to me to be that in most cases the muscular force is lessened in hypnosis. I have made these investigations during the different hypnotic states, and have but rarely found an increase. However, there are variations, and I have occasionally seen the strength of one hand increase while that of the other diminished. I have also obtained different results at different times with the same person. When there were such variations they were always of small amount, and they are the less important because all dynamometrical investigations suffer from certain sources of error. In opposition to the conclusions which have been drawn from the investigations just detailed, Luys points out that in hypnosis the muscular force sometimes increases simultaneously with the loss of sensibility—the increase being at times two-fold. But it would be far-fetched to conclude from this that the increase in the muscular force is dependent on the disturbance in the sensibility. Carpenter reports that the muscular force may often be increased by suggestion. One of Braid's patients, who had been so weak for years that he could not lift even a twenty-pound weight, was enabled, by means of suggestion, to pick up a twenty-five pound weight with his little finger and swing it easily round his head. On another occasion the same subject raised a fifty-pound weight to his knee on the last joint of his fore-finger (Bramwell).

Let me take this opportunity of discussing the muscular sense. This sense instructs us as to the position and movements of our limbs. Its existence is consequently necessary for the performance of any useful function. There could be no artistic skill without the muscular sense. Braid relates that a hypnotized girl once imitated some of the songs of the famous Jenny Lind perfectly, which she was quite incapable of doing in the waking state. Braid attributes this fact to the delicacy of hearing and of the muscular sense in the hypnotic state. Some cases of imitative speech (echolalia), which can be cultivated to a high degree of perfection, would belong here. But whether the delicacy of the muscular sense is increased in

hypnosis without suggestion is more than doubtful. Döllken, indeed, holds the contrary view. He experimented on the subject's capability of denoting the position of his (the subject's) limbs, and found the answers inexact or confused unless the hypnotic was given time for reflection. Döllken found that a short pause for reflection sufficed to produce the right answer. Those alterations in the handwriting which Döllken and others have noticed in hypnosis are perhaps referable to a diminution in the muscular sense. Anyway, the muscular sense can be influenced by hypnosis. It is interesting to learn that absence of the muscular sense has often been observed in cases of total anæsthesia of one arm, although voluntary movement of the limb in question has been retained. We have here a condition like that met with in locomotor ataxy. Sufferers from the latter disease can perform any operation, such as writing, correctly, provided their eyes remain open, but can only do such actions imperfectly directly their eyes are closed (William James and Carnochan).

The muscular sense enables us to estimate the weight of any object placed in our hand, because we adjust the activity of the muscles called into play to the weight of the object dealt with. Bramwell and Alcock made certain experiments for the purpose of testing the muscular sense in hypnosis. They used little boxes of different weight and experimented for the purpose of ascertaining if hypnotic suggestion enabled a subject to detect minuter differences in weight than he could in the waking state. The experiments seemed to point to such a possibility; but Bramwell expresses no definite opinion on the question, and, moreover, does not appear to consider the experiments conclusive.

We may here consider the electric excitability of the muscles and nerves, to which little attention has hitherto been paid. Moritz Rosenthal finds an increase of electric sensibility in hypnosis. Tereg also found changes in one case, which, however, was investigated without the galvanometer; and Marina has done the same in the case of a person in the waking state, who, however, had often been hypnotized. I, for my part, like Heidenhain, Berger, and Rieger, have been unable to discover anything of importance in this direction. I have tried more than a hundred different experiments without finding a perceptible difference on this point between the hypnotic and waking states. I made my experiments with the galvanic

and faradic current; I used Hirschmann's galvanometer, and made most of the experiments on the ulnar nerve just above the elbow. I have already said that the electric excitability is decreased in certain suggested paralyses. I may here just mention that according to Morselli and Mendelsohn the muscles contract more quickly from stimulation in hypnosis than in the waking state.

I have thoroughly discussed above the question whether new reflexes appear in hypnosis, and it consequently now remains for us to consider how the ordinary reflexes behave in that state. I have already spoken of the tendon reflexes, which are said to be increased in the lethargic stage of Charcot, and in certain paralyses by suggestion. Berger has also observed an increase of the patellar reflex. On the other hand, Vogt found, in his investigations on the influence of certain mental conditions on the knee-jerk and muscular tonus, that the knee-jerk lost in intensity. Vogt experimented on his wife, Frau Cécile Vogt. In these investigations it was found that the muscular tonus was diminished as well as the knee-jerk, and Vogt considers that his researches have thoroughly demonstrated the dependence of the strength of the knee-jerk on that of the muscular tonus. Like Berger, I have often noticed changes in the knee-jerk. But it seems that the type of hypnosis and the kind of suggestion play a great part in this respect. We are thus enabled to explain an increase of tendon reflexes in cases of suggested paralysis and when the muscles are completely relaxed, and a decrease of those reflexes in cataleptic postures. Of course this increase must not be regarded as a phenomenon peculiar to hypnosis, since apart from hypnosis the tendon reflexes are more perceptible when the muscles are relaxed than when they are contracted.

With regard to the pupil of the eye, Braid has already mentioned a difference between its states in hypnosis and in sleep. In sleep there is a contraction of the pupil, but Braid, and later on Heidenhain, often found it dilated in hypnosis. I have never observed the latter phenomenon except when I have employed the method of fixed attention; at other times I have more often found contraction of the pupil. I can confirm Braid's assertion that oscillations of the pupil appear not infrequently in fixation; contraction and dilatation here alternate rapidly. Spasm of accommodation is also often

mentioned (Heidenhain, Cohn, Rumpf). The assertion that the pupil reflexes are abnormal in hypnosis is often met with (Luys, Bacchi). It is said that a ray of light does not invariably cause a contraction of the pupil during hypnosis. I have never observed a complete absence of the reflex, but I have often remarked very slight reaction when I have used the method of fixed attention for a considerable length of time to induce hypnosis. Whether this was an effect of fixation or of the hypnosis is doubtful; I am inclined to consider the prolonged fixation as the cause. Sgrosso noticed dilatation of the pupils in his two subjects on the appearance of hypnosis, followed by diminished contraction during the state.

We shall see later on that the reflexes can be affected by the action of suggestion on sense-perception. For example, the conjunctiva of the eye can be rendered insensitive by suggestion, so that it may be touched without producing the corresponding reflex.

Up to this point we have hardly studied any but those changes which appear in the voluntary motor system during hypnosis. The hypnoses of the first group (p. 59) are characterized by these changes, which, however, are invariably found in the second group also. The hypnotic states belonging to this group are, however, distinguished by an increase of susceptibility to suggestion; the functions of the organs of sense in particular are influenced by it. The statements of different authors as to how these functions are performed in hypnosis without suggestion are very contradictory. Some investigators (Beaunis, Lajoie, David) assume that in hypnosis without suggestion sense-perception does not deviate from the normal; but others hold the opinion that hypnosis alone—*i.e.*, without suggestion—exerts an influence on the organs of sense. But the views of the various authors differ very considerably on the question of details. Some will not for one moment admit that any hard and fast rules can be laid down. Ochorowicz, for instance, opines that any kind of combination between inhibition and increase, may take place, and that, in this respect, there are just as many forms of hypnosis as of subjects hypnotized. Others, again, think they are in a position to set forth certain definite laws on the changes in sense-perception. Braid distinguishes two grades in hypnosis. In one an increased activity of sense is shown, except in the

case of the sense of sight, and in the other a diminution of it. It is evident that Braid not only found that sense-perception in hypnosis varied according to the depth of the hypnosis, but that he made a distinction in this respect between the various organs of sense, contrasting that of sight with the others. We also find analogous views expressed by more recent investigators, although their statements as to the individual organs of sense differ essentially from Braid's. Liébault found changes in the functions of the organs of sense during deep hypnosis. According to him, the senses of sight and taste diminish first, then the sense of smell, and finally hearing and feeling disappear. But when the method of fixed attention is used, sight is the last to go. Crocq considers that the extent of the changes under discussion is determined by the depth of the hypnosis; he, moreover, made inquiries of different investigators as to the condition of sense-perception, mental power, and more particularly memory, in the subjects whom they had hypnotized, and came to the conclusion that we may lay down the general rule—subject, of course, to some exceptions—that the deeper the hypnosis, the greater the diminution in the sensibility of the skin and in the functions of the other organs of sense. Crocq makes a general exception in the case of the sense of hearing, because the experimenter is in verbal communication with the subject, and involuntarily suggests to the latter that he must continue to hear. De Jong also thinks that the state of the sense-perceptions depends on the depth of the hypnosis, and that one is justified in saying that as a rule these perceptions are diminished in the somnambulic stage.

This branch of the question has been rigorously investigated by Döllken, who has published his results in a valuable work. He was unable to detect any quite definite and regular sequence of the phenomena as far as the individual organs of sense are concerned. The individuality of the subject, and the method of hypnosis employed, each plays a part in this respect. The perceptive faculty of the visual organ is usually the first function diminished, a result which is certainly brought about by hypnotic suggestion. Suggestion of this nature can only very rarely be avoided. Döllken found that by excluding any possible action of verbal suggestion on the other senses that of touch remained longest intact, though in some cases the same held good for the sense of hearing. Döllken did not

fail to perceive a diminution of the perceptive faculty in any of his cases, not even in his own personal experience of being hypnotized for the first time, when he only fell into a somnolent state. "First of all the various articles in the room became less distinct, their outlines less clearly defined; then I became indifferent to the 'passes,' which I had found very unpleasant at the commencement of the experiment. At about the same time I became no longer able to detect the smell of a medicament which permeated the room. Finally the noise in the street appeared less loud." Döllken also investigated the phenomena of deep hypnosis; but he was unable to discover any law connecting the depth of the hypnosis with any particular phenomenon produced. The only thing he could establish was that the deeper the hypnosis in one and the same case, the greater also the diminution in the perceptive faculty. This condition, however, often improved if the experiments were continued for some time. Döllken believes that he excluded all disturbing influences of the nature of suggestion. With regard to the eye, all degrees of amblyopia were met with, even to deep amaurosis. In one case he found the vision $\frac{6}{24}$ on one occasion, but $\frac{6}{18}$ on another, whereas it was normally $\frac{6}{6}$. In another case he found that in several experiments vision was first of $\frac{6}{12}$ and then $\frac{6}{6}$. With regard to the sense of hearing, as the hypnosis proceeds the noises in the street appear more distant and at last inaudible, then the sounds in the room die away—for example, the ticking of the clock. Finally, the experimenter's voice becomes less audible to the subject, even if he raises his voice. More detailed investigation showed that whispering which could be heard at a distance of 6 metres in the non-hypnotic state, was inaudible at 3 metres or less. The ticking of a watch, which can normally be heard at 1.8 metres, was inaudible when placed close to the subject's ear. Döllken also tested the sense of smell, using for that purpose soap, ether, roses, cloves, and violets. The most frequent phenomenon here noticed was failure to distinguish accurately between the perfumes of the flowers. In some cases no notice whatever was taken of the proximity of the object. Döllken did not examine the sense of taste, but his investigations of that of touch were very thorough. In the first place, he found a diminution just to the extent that greater pressure is required to produce the sensation of touch in hypnosis than in the waking state. This implies a diminu-

tion in the sense of pressure. The power of localization is also less than in the waking state. If a patient was asked to state at once the spot on which he was touched, the error was from 1 to 5 cm. more than under normal conditions; for example, 5 cm. on the forearm and 3 cm. on the back of the hand of one subject. While the error in the case of the cheek was less than 1 cm. under normal conditions, it increased to 2-3 cm. when the subject was hypnotized. Certainly Döllken thinks that the source of error is increased in hypnotics, because hypnosis renders their movements somewhat slower. The "compass test" also showed variation. The two points of a compass were recognized as two tactual impressions on the back of the hand at 45 mm. (normal, 30-35 mm.); on the forearm, 60 mm. (normal, 36-39 mm.); on the chin, 15 mm. (normal, 8-11 mm.); on the cheek, 15 mm. (normal, 10-13 mm.). After a series of experiments the above values underwent a change and rapidly approached the normal. Döllken was unable to discover any essential difference between the impressions of "pointed" and "blunt," but he certainly occasionally found a decrease in the perception of temperature.

Differences of opinion also have been expressed as to whether the sensation of pain can diminish, or disappear, without suggestion. Berger found an increased sensitiveness to pain in some cases. Still, in the present day, we are certainly justified in assuming that the Breslau investigator could not possibly have known at the time he made his experiments how carefully the influence of suggestion must be excluded. Döllken never observed a complete analgesia without suggestion, but he assuredly did see some cases in which the prick of a needle either produced no pain, or the sensitiveness to pain was diminished.

Up to this point I have discussed the condition of the sense-perceptions when the influence of suggestion is excluded, and I have already mentioned that some investigators only admit changes in sense-impression in those cases in which suggestion plays a part. On the other hand, as I pointed out very many years ago, we possess many data which show that where variations in sense-perception occur during hypnosis, sufficient attention is not paid to the difference in the way the hypnotic behaves in respect to different objects and people. For example, it often happens that the hypnotic hears the voice, and feels the touch, of the person who has sent him to

sleep, but not of any one else. Marot has observed—and this observation has been made by many other investigators—that if any one runs a needle into a hypnotic the latter first of all feels it, but ceases to do so as soon as it is suggested to him that the person who pricked him has gone away. This selective factor has great influence on the perceptions of a hypnotic. Certainly it may be objected that this selective factor is only a product of suggestion. We shall come across it again, and in its most conspicuous form, when dealing with *rapport*. In fact, we should here bear in mind that suggestion, with its many forms, may act without our intending it to do so, or observing its effects. We must also take into consideration the great part played by auto-suggestion; and we must bear firmly in mind, that it is just in auto-suggestion that many factors exert their influence when the subject is on the borderland of consciousness, so that the hypnotic does not always clearly understand the connection between the idea produced by suggestion and the way in which suggestion itself works. But even if we admit this, we must bear in mind that we are not justified in attempting to explain all the processes concerned by merely using the word "suggestion"; we should then have to attribute to that word a wider meaning than we ever think of doing in the present day. It is quite possible that when any one is in a state of deep hypnosis, analgesia or some other diminution of sense-perception may occur spontaneously, without the influence of suggestion. This symptom would then have to be considered an essential factor in hypnosis, and not the result of a specially suggested idea. To persist in calling this a case of suggestion because the hypnosis was produced by suggestion, would be to attribute to the word suggestion a meaning far beyond permissible limits. In what we call suggestion there must be at least a direct connection between the effect produced, and the idea of that effect. When this connection is wanting we are not justified in ascribing other mental connections—association, for example—to hypnosis, quite apart from the fact that there may possibly be processes of a purely physiological nature which lead to a decrease of sense-perception without the intervention of any mental activity being demonstrable. At all events, I consider that no proof has yet been adduced to show that all the diminutions of sense-perception met with in hypnosis are due to suggestion, as defined by me on page 66.

No matter what opinion we may hold on the origin of those diminutions of sense-perception which we have described up to this point, there can be no doubt that many changes of sense-perception can be brought about by suggestion in hypnosis. Analgesia, for example, often exists to such a degree that the severest surgical operations can be performed during the state. It is also known that needles may be run into some persons during hypnosis, without their feeling the pain, though they feel the touch. And yet a complete analgesia is rare in hypnosis. There is an immense difference between pricking a person with a needle and using the faradic brush. The pain caused by the use of the latter is so great, especially when the current is sufficiently strong, that very few persons in hypnosis can endure it, even when they show no pain on being pricked with a needle. At all events, decreased sensitiveness to pain can often be induced by suggestion, although an absolute, complete analgesia is rarely attained by that means. Many cases described as completely analgesic —for example, those of Tamburini and Seppilli—proved on closer examination not to be so, as a strong faradic current finally produced pain. I will just remark that all kinds of pain can be induced by suggestion—the pain caused by a needle, as well as that caused by a knife or burn. The face of the subject expresses pain in such a manner, that an impartial person can hardly decide whether the pain is real or suggested.

Tactual sensibility, the sense of temperature, etc., can also be anæsthetized by suggestion as well as the sense of pain. The mucous membranes, too, can be rendered insensible. The fumes of ammonia in the nose, and tickling the throat, are not felt; the conjunctiva can be touched without producing the corresponding reflex; even the cornea may become insensitive, either spontaneously or by suggestion (Bramwell). Preyer quotes the cynical experiment of an American physician, Little, who thrust a needle through the cornea of a hypnotic whom he suspected of simulation, in order to test its insensibility. But in my experience these last-mentioned phenomena are uncommon. When this anæsthesia of the conjunctiva and cornea exists the eye no longer closes on reflex stimulus; but this is a consequence of the anæsthesia, and not an independent phenomenon (Gurney).

Attention must be drawn to another phenomenon, which,

however, cannot be straightway ascribed to suggestion. Döllken, like Bechterew, Lannegrace and others, has observed that when there is a general decrease in the sensibility of the skin, the function of the eye and ear are also impaired. But he never found the opposite to be the case. Döllken, as well as Schaffer, further found that when one side of the body was made anæsthetic by suggestion all the organs of sense were affected; there were also motor disturbances. For example, in such cases of hemianæsthesia by suggestion he observed the following changes on the anæsthetized side:—Cessation of the functions of the senses of pain, touch, and temperature; incapacity for carrying out delicate movements with the hand, extending in some cases to loss of movement of the upper extremity, also locomotor disturbances and loss of the power to walk. The muscular force was nil, the field of vision apparently concentrically narrowed, the hearing less acute, the sense of smell and the muscular sense appreciably diminished; finally there was decrease, and in one case cessation, of the patellar reflex, even when the muscles were completely relaxed. Further, amaurosis induced by suggestion was invariably accompanied by a decrease of the functions of the senses of smell and hearing—a fact already established by Schaffer. Amblyopia, hyposmia, and slight hypæsthesia were observed in cases of deafness brought about by suggestion. Since Döllken excluded the action of suggestion in the production of these secondary symptoms, they are very remarkable as sequelæ of primary suggestion. If we compare these secondary symptoms with the loss of sensation which directly follows the loss of motor power in paralysis by suggestion (p. 72), the similarity of the two phenomena strikes one at once. In such cases as those with which we have just been dealing, it is quite possible that processes may be at work which we are not justified in ascribing to suggestion, even if there naturally remain a suspicion that the secondary symptoms may have been induced by ideas unintentionally suggested.

I have shown above that perceptions of each sense by itself can be prevented by suggestion; but hyperæsthesiæ of the organs of sense likewise exist in hypnosis. Whether these come on from suggestion, or in other ways, is, as mentioned above, not always to be distinguished exactly. Although

they are not on the whole very common, I shall here add some of these very remarkable cases. It is exactly these rarer cases which deserve the most careful consideration, for they often offer us a key to a natural explanation of many mysterious phenomena, such as transposition of the senses, and clairvoyance.

An increased sensitiveness to touch has often been observed. The two points of a compass are used for measuring the least distance between them at which they may be felt as two separate points. In this way it has been found that the points can be distinguished at a less distance in hypnosis than in the normal state (Berger). I have made a series of experiments on this point and can confirm Berger's statements—at least, I found that suggestion caused a considerable increase of sensitiveness. Bramwell and Alcock, who experimented together, obtained similar results. For example, without suggestion the two points were distinguished on the hand at one inch[1] distance; under the influence of suggestion the distance was reduced to a quarter of an inch. Without suggestion the distance on the left eyebrow was one inch; with suggestion, half an inch. I have also experimented on pathological subjects. In cases of locomotor ataxy, with profound anæsthesia, increased sensitiveness has been found when the patients were under the influence of suggestion; the state may continue some time post-hypnotically. In one case of locomotor ataxy, I found that, before hypnosis, the two points were distinguished at a distance of 6.1 cm. During hypnosis the separate points were perceived at 4.9 cm. distance, and after waking, even at 4.1 cm. I have recently repeated the experiments, always with the same results. Lœwenfeld protests that these experiments prove nothing, because variations of sensitiveness occur in cases of locomotor ataxy, without suggestion being called into play. This is a point which I have certainly not disputed, but it does not tell against the results of my experiments. For, if immediately after suggestion there is an increase of sensitiveness which was not demonstrable before, and which also disappears after the lapse of a certain amount of time, and if the same phenomenon is frequently observed in the same subject, then we are certainly justified in assuming that suggestion has exerted its influence in such a case.

[1] 1 inch = 2.54 cm.

The senses of pressure and temperature sometimes become much more delicate. The hypnotic recognizes things at a certain distance from the skin, and this simply by the increase and decrease of temperature (Braid). He walks about the room with bandaged eyes or in absolute darkness without striking against anything, because he recognizes objects by the resistance of the air and by the alteration of temperature (Braid, Poirault, Drzewiecki). D'Abundo produced enlargement of the field of vision by suggestion.

Bergson has described one of the most remarkable cases of increased power of vision. This particular case has been cited as a proof of supersensual thought-transference, but Bergson ascribes the result to hyperæsthesia of the eye. In this case the hypnotic was able to read letters in a book which were 3 mm. high; but the reading was made possible by a reflected image of these letters in the cornea of the experimenter. According to calculation, the reflected image could only have been 0.1 mm. ($\frac{1}{250}$ inch) high. The same person was able without using the microscope, to see and draw the cells in a microscopical section, which were only 0.06 mm. in diameter. Sauvaire, after some not quite irreproachable experiments, supposed the existence of such a hyperæsthesia of sight, that a hypnotic recognized non-transparent playing-cards by the rays of light passing through them. A case of Taguet's, in which an ordinary piece of cardboard was used as a mirror, is said to have shown quite as strong a hyperæsthesia. All objects which were held so that the reflected rays from the card fell on the subject's eye were clearly recognized. A great increase of the sense of smell was also noticed in the case in question. A visiting-card was torn into pieces, which pieces were professedly found purely by the sense of smell; pieces belonging to another card were rejected. The subject gave gloves, keys, and pieces of money to the persons to whom they belonged, guided only by smell. Hyperæsthesia of smell has often been noted in other cases. Carpenter states that a hypnotic found the owner of a particular glove among sixty other persons. Sauvaire relates another such case, in which a hypnotic, after smelling the hands of eight persons, gave to each his own handkerchief, although every effort was made to lead him astray. Braid and the older mesmerists relate many such phenomena. Braid describes one case in which the subject on each occasion found the owner of some

gloves among a number of other people; when his nose was stopped up the experiments failed. This delicacy of the different organs of sense, particularly of the sense of smell, is well known to be normal in many animals; in dogs, for example, which recognize their masters by scent. Hypnotic experiments teach us that this keenness of scent can be attained by human beings in some circumstances.

I will take this opportunity of quoting an experiment which is often repeated and is wrongly considered as a proof of increased keenness of the senses. Let us take a pack of cards, which naturally must have backs of the same pattern, so that to all appearance one cannot be distinguished from the other. Let us choose a card—the ace of hearts, for example—hold it with its back to the subject and arouse by suggestion the idea of a particular photograph on it—his own, let us say. Let us shuffle the cards, including, of course, that with the supposed photograph on it, and request the hypnotic to find the photograph, without having allowed him to see the face of the cards. He will often find the right one, although the backs are all alike. The experiment can be repeated with visiting cards, or with sheets of paper, if the selected one is marked, unknown to the hypnotic. This experiment makes a greater impression on the inexperienced than it is entitled to; for most people are able to repeat the experiment without hypnosis, and hyperæsthesia is not generally a condition for its success. If the backs of these cards and papers are carefully examined, differences which may easily be discerned will be discovered. The experiment has no bearing on the question of simulation. Naturally, I do not contend that a hypnotic cannot find a paper in such a case better than a waking man. I only wish to point out that although this experiment is often used to demonstrate the presence of hyperæsthesia, the latter is not generally necessary for its success. I have seen men of science show astonishment when a hypnotic distinguished apparently identical sheets of paper. They did not understand that there were essential differences in the sheets, which suffice for distinguishing them even without hypnosis. The experiment is to be explained thus:—The minute but recognizable differences (*points de repère*) presented to the hypnotic at the moment when the idea of the photograph was suggested to him, recall the suggested image directly he sees them again. The points are so closely associated with the image that they readily call it up. Binet and Féré have rightly pointed out that the image only occurs when the *points de repère* are recalled to the memory; they must first be seen. Consequently, if the paper is held at a distance from the subject's eyes, the image will not be recognized, for the *points de repère* are not visible.

Some of Binet and Féré's experiments are interesting. They have caused photographic impressions to be made of white papers on which a portrait had been created by means of suggestion. It was shown that the hypnotic always took the copies for the original, because the photographed *point de repère* aroused the same image in his imagination. Jendrássik has observed the same sort of thing; if a "d" is drawn with the finger on a sheet of white paper, and if it is suggested that the "d" is real, the subject sees the "d." If the paper is turned upside down he sees "p,"

and in the looking-glass "q." This is because certain points on the paper were remembered by the subject, and when the paper was placed in different positions the points appeared in different positions also.

In addition to increase and decrease of sense-perception, it is possible to induce all kinds of sense-delusions. The images produced are so remarkable that any one who sees them for the first time is justified in doubting whether the phenomena are real or not. We have accustomed ourselves to depend so completely on our organs of sense, to think them such trustworthy witnesses in all cases, that we are astonished to find that a word suffices to place a hypnotic among utterly different surroundings.

Sense-delusions are divided into hallucinations and illusions. The first is the perception of an object where there is really nothing; the second is the false interpretation of an existing external object. If, for example, a book is taken for a cat, or a blow on the table for the firing of a cannon, we talk of an illusion; but if a cat is seen where there is nothing, we call it a hallucination. We have thus to do with a hallucination when an external object causes a perception by means of association. A chair on which a particular person has often sat may by association call up an image of that person; this is a hallucination called up by an external object.

We observe numerous hallucinations and illusions in hypnosis. We have seen in Case IV. (p. 35) that it suffices to assert that a dog is present, and a dog will apparently be seen by the hypnotic. A handkerchief was in this case taken for a dog, consequently this was an illusion. An illusion is more easily induced than a hallucination; in the absence of an external object, such as the handkerchief, the suggestion very often fails. When I do not offer some such object the hypnotic often finds it for himself. Hallucinations of sight are more easily caused when the eyes are closed; the subjects then see objects and persons with their eyes shut, as in dreams. They think at the same time that their eyes are open, just as we are unaware in dreams that our eyes are shut. If we wish to cause a delusion of the sense of sight at the moment of opening the eyes, it is necessary to make the suggestion quickly, lest the act of opening the eyes should awaken the subject. I advise the use of fixed attention while the suggestion is being made (*cf.* Exp. IV., p. 35), so that the patient may not awaken himself by looking about. The other organs of sense may also be deluded. I knock on the table and give

the idea that cannon are being fired; I blow with the bellows and make the suggestion that an engine is steaming up. A hallucination of hearing something—*e.g.*, the piano, is induced without the aid of any external stimulus. In the same way smell, taste, and touch may be deceived. It is well known that hypnotics will drink water or even ink for wine, will eat onions for pears, will smell ammonia for Eau-de-Cologne, etc. In these cases the expression of the face induced by the suggested perception corresponds so perfectly to it that a better effect would scarcely be produced if the real article were used. Tell a person he has taken snuff, he sneezes. All varieties of the senses of touch, of pressure, of temperature, of pain can be influenced. I tell a person that he is standing on ice. He feels cold at once. He trembles, his teeth chatter, he wraps himself in his coat. Even "goose-skin" can be produced by the suggestion of a cold bath (Krafft-Ebing). In like manner, itching and so forth can be induced. I say to a gentleman, "To-morrow at three o'clock your forehead will itch." The post-hypnotic suggestion proves true; the forehead itches so much that the subject rubs it continually. I find that the senses of taste and touch are more easily influenced than the others. The suggestion of a bitter taste, or of increased sensitiveness to temperature on the part of the skin, takes effect much sooner than the suggestion of a delusion of sight or hearing. Certainly we do know that even without hypnosis the senses of taste and temperature are more readily affected by suggestion than those of sight and hearing. Similarly, delusions of the senses of taste and temperature appear in the first stage of hypnosis (just as they do in the waking state), whereas conspicuous delusions of sight and hearing are only observed in deep hypnosis. Moreover, it sometimes happens, especially in cases of light hypnosis, that a subject tastes the bitterness or feels the warmth suggested, although he is perfectly aware of the delusion. On one occasion a gentleman said to me: "I know there is nothing bitter in my mouth, and yet I have a bitter taste in it."

Sense-delusions can be suggested in various ways. We tell a subject that he sees a bird, and he does. We can suggest the same thing by gesture—for example, by pretending to hold a bird in the hand—particularly after the subject has received some hypnotic training. The chief point is that the subject should understand what is intended by the gesture.

Naturally, several organs can be influenced by suggestion at the same time. I tell some one, "Here is a rose"; at once he not only sees, but feels and smells the rose. I pretend to give another subject a dozen oysters; he eats them without it being necessary for me to say a word. The suggestion here affects sight, feeling, and taste at the same time. In many cases the muscular sense is influenced in a striking manner by such suggestions. I give a subject a glass of wine to drink; he lifts the pretended glass to his lips, and leaves a space between hand and mouth as he would if he held a real glass. I am not obliged to define the delusion for each separate sense; the subject does that spontaneously for himself. The subject in this way completes most suggestions by a process resembling the indirect suggestion described on page 67. The results of the external suggestion naturally depend on the character of the subject. I remember a case of chronic alcoholism being brought to me for treatment by suggestion. The patient had been accustomed to consume enormous quantities of alcohol daily. On the first few occasions that I hypnotized him, he made frantic efforts to drink any glass of wine I suggested. In the course of time, however, I not only succeeded in making him a total abstainer, but even created in him such an aversion to alcoholic drinks that his friends could not but laugh at his quaint behaviour. The change was also very noticeable during hypnosis. At first he drank every glass of wine suggested with avidity, but later on he would push the imaginary glass of wine away with a gesture of disgust, sometimes even attempting to dash it to the ground.

Besides which, the deception, if it is thorough, is clearly reflected in the subject's expression and gestures. No gourmand could wear a more delighted expression over some favourite dish than does a subject over a suggested delicacy. The fear depicted on the face of a subject when he believes he is about to be attacked by a tiger is most impressive. A subject will drink several glasses of wine by suggestion, will become red in the face, and then complain of his head. I give a piece of cork to a subject for an onion; he smells it and his eyes fill with tears.

By means of suggestion, we can place a subject in any situation we please, and from his behaviour draw conclusions as to what his conduct would be under analogous circumstances, and also as to his character (Morselli). But it is

necessary to exercise great caution in drawing such conclusions, since the subject nearly always has some dim consciousness of his real surroundings, however completely he may seem to be transported into the imaginary ones.

Some authors (Dumontpallier, Bérillon) have particularly directed attention to the suggestions which take effect on one side of the body only. For example, we can cause a bird to be seen on the right side, a dog on the left; but this appears to be only a matter of suggestion and training. The case mentioned by Magnin is connected with this: a person affected by weak sight of the left eye, of hysterical origin, believed that he saw with the right eye things which he really saw with the left, and so thought they were on his right side when they were really on his left (allochiria).

In contrast with the delusions of sense hitherto described, which are sometimes called positive, there are also negative delusions of sense. The older mesmerists (Deleuze, Bertrand, Charpignon) published many observations of them. When the delusion is positive the hypnotic believes he sees what does not exist; when it is negative he fails to recognize the presence of an object really placed before him. For example, Mr. X. is in hypnosis. Two other persons are present besides myself. I tell him: "From this moment you will only be able to see me; you can no longer see the other men, though they are still here." The experiment is successful; X. replies to every question addressed to him by these gentlemen, and can feel them, but he cannot see them. This is a negative hallucination of sight only. But a negative hallucination of several senses can be induced as readily as a positive one. I say to X.: "The two men have gone away; you and I are now alone." From this moment X. neither sees nor hears them, nor perceives them by means of any sense. When I ask him who is in the room he replies, "Only you and I." The whole or part of any object or person can be made invisible in the same way. We can cause people to appear headless and armless, or make them disappear by putting on a particular hat, as in the story of the Magic Cap. The situation may be varied in any way we please. Forel has pointed out that the insane often have these negative hallucinations. He has also shown that hypnotics complete, and even extend, negative hallucinations. Thus, I say to X., while A. is sitting on a chair: "A. has gone away; there is nobody on that chair." X. examines the chair, and as he feels something there, he imagines that a shawl has

been laid upon it. We see here how a suggested negative hallucination gradually passes into an illusion, through the auto-suggestion of the hypnotic; this is very common. To be exact, we can regard every illusion as the sum of a positive and a negative hallucination, as in each illusion something present is not perceived and something not present is perceived.

Further, it is also possible to prevent recognition of certain colours, and to induce colour-blindness by suggestion. But we have here only to do with a defective perception of colours, and not with an alteration of the stimulus affecting the eye; the disturbance is purely mental (Schirmer). Cohn's assertion that, on the other hand, colour-blindness sometimes disappears in hypnosis has been contested by Königshöfer. At all events, this could only be a hysterical disturbance of the sense of colour, and not a disturbance founded on peripheral alterations.

An entire cessation of the functions of any sense organ can be induced in the same way as a negative hallucination. "You can no longer hear," "You are deaf," or "You are blind": these words suffice to deprive the hypnotic of the corresponding sense-perceptions. Not only does he cease to recognize any particular object, but the sense organ affected is insusceptible to anything. A command suffices to restore the functions (sight, hearing, etc.). It is certain that the blindness and deafness induced in this way are of a mental nature, for the corresponding organ of sense performs its functions, though the impressions do not reach the consciousness. In the same way the sight of one eye can be prevented, though the other can see as usual; a one-sided amaurosis can be created (Borel), or even hemianopsia (Willy).

Common sensation is influenced in hypnosis in the same way as the functions of the organs of sense, though it is a moot point whether disturbances occur without suggestion. It cannot be doubted that the excitement alone sometimes causes a feeling of oppression, probably of mental origin, but not necessarily attributable to suggestion. At all events, we can influence common sensation very materially by suggestion in hypnosis. A feeling of fatigue often appears in the lightest hypnosis, and may also exist in the deeper stages. In other respects also we are able to influence common sensation in hypnosis. This is not surprising when we consider that it is

exactly the common sensations which are most under the influence of mental processes. Just as looking down from a tower causes giddiness, as the thought of repugnant food produces disgust, so we can call up these and related phenomena, or cause them to disappear, by suggestion. It is in this direction that the physician has to record the most striking successes, since the common sensations, of which pain is one, are the cause of most of the complaints we hear of. Anything that we can induce by a mental process, we can banish by suggestion. I say to a subject who complains of want of appetite, "The loss of appetite has disappeared; you are hungry." I can cause another to feel thirst. Debove, on the other hand, has induced loss of appetite by suggestion to such an extent and for so long a period that the person concerned took no solid food for fourteen days. Further, it is possible up to a certain point to satisfy the hunger and thirst of subjects in deep hypnosis by merely suggested food and drink, as Fillassier informs us. It is a pity, however, that this result can only be obtained with a few persons and in a limited measure, for otherwise our politicians would no longer need to puzzle their heads over social questions and the feeding of the masses. Sexual feeling can also be produced by suggestion. Leopold Casper tells of a case in which Tissié hypnotized a patient and suggested to him that the right ring-finger should indicate sexual desire and the left abstinence. When the patient awoke, contact with the right finger caused sexual excitement; contact with the left subdued it. Once Tissié forgot to remove the suggestion, and the consequence was that for twenty-four hours the patient was unable to refrain from coitus and masturbation, as well as spontaneous emissions. Indirect methods often produce the desired result. For example, a feeling of disgust can sometimes be induced by suggesting to the patient that repugnant food has been placed before him. An easy way of inducing a feeling of suffocation is to suggest that the patient is drowning.

Abnormalities of voluntary movement apart, nearly all the phenomena of suggestion hitherto described are the exclusive privilege of the second group of hypnotic states. I come now to some other physical functions which require a deep hypnotic state if they are to be influenced. I mention, first of all, the phenomena of that part of the muscular system which is normally independent of the will.

We will here, first of all, consider the circulation of the blood and the respiration. A large number of physiological investigations has been made in this field during hypnosis, in order to decide what is the state of the pulse and respiration without suggestion, but the statements are so contradictory that we only dare receive them with caution. Some believe that they have discovered objective symptoms in changes of the action of the heart and the respiration, but we cannot doubt that there has been considerable exaggeration. Tamburini and Seppilli found the respiration accelerated, often irregular, and the heart-beat accelerated and stronger in the transition stage from the waking to the hypnotic state. There was no change in the type of the pulse, but the wave was as a rule higher, and the curve of the pulse-tracing was affected by the respiration. Hauer and others—Richer, Magnin, etc.—also studied the pulse and respiration in connection with Charcot's stages, but the methods employed did not lead to the discovery of any sharp line of demarcation between the cataleptic and lethargic stages. Of other investigations I may mention those on respiration made by Isenberg and Vogt, who experimented on one another. They found that when the eyes close in response to suggestion, the number of respirations diminishes, and that the subject at the same time experiences a feeling of restful calm. These authors also found that as the hypnosis proceeded the breathing became slower and more superficial, but was deepened at the commencement and termination of the hypnosis. Beaunis occasionally found that the tension of the pulse was increased, which he does not, however, think of much importance. Horsley found no alteration in the curve of the pulse-tracing. I shall deal with the investigations of Francke and others later on. Döllken found that when hypnosis sets in suddenly, the patient experiences a subjective feeling of warmth, and as a rule the face flushes and is covered with beads of perspiration; but two cases of sudden deep hypnosis were characterized by facial pallor, which persisted until the experiment was over. According to Lœwenfeld, Lloyd Tuckey almost invariably found the capillaries and smaller arteries so contracted in deep hypnosis that even somewhat deep wounds caused no hemorrhage.

A great acceleration of the pulse and of the respiration has been often observed when the method of Braid, or fascination (Brémaud), or mesmeric passes were employed (Ochorowicz), the respiration, which was normally

18 per minute, rising to 50 or even more. I have myself made a number of experiments on this point, and fully agree with Bernheim and Preyer that these changes are not brought about by the hypnosis, but by the fixed attention. I believe that it is only the effort made and the excitement which cause these abnormalities; the irregularities in the respiration which are observed at times should be ascribed to the same cause. Preyer mentions that the respiration of a person looking at a microscopic object often changes; in the same way it displays abnormalities when a person believes himself watched. An experienced doctor, therefore, prefers to examine the respiration unobserved by his patient. At all events, I have seen a material acceleration of the pulse and respiration set in after a long strain of attention without a trace of hypnosis. If there is hypnosis, in a little while the irregularity and acceleration cease either spontaneously or by suggestion. I have only seen a few cases in which they persisted, but am by no means inclined to think this a sign of hypnosis, as some persons show an acceleration of pulse and breathing on the slightest provocation. Even a conversation is enough to induce acceleration, and changes of pulse and respiration have been known to be brought about by an uncomfortable sitting posture. Besides which it must be added that in many people there is an important acceleration of pulse and respiration in the strong muscular contractions of the cataleptic phenomena (Braid), and also in tonic contracture (Rumpf). If I made such persons lie quietly down, and avoided conversation, physical effort, and mental excitement, I never observed any acceleration.

In some instances I have found a deepened and somewhat long-drawn inspiration, and also a slight slowing of pulse, in hypnosis. These were the cases which bore an external resemblance to sleep, and in which, as I have already mentioned several times, no important spontaneous movements take place. It is also very difficult to induce movements by suggestion in these cases. Respiration, which holds an intermediate position between voluntary and involuntary movement,[1] can also be influenced by suggestion. I have always been careful never to prolong such experiments for more than half a minute. Whenever I suggested to a subject that he could not breathe, an apparent pause in his respiration occurred. Jendrássik mentions a case in which, by means of suggestion, he stopped respiration for three minutes. According to a report published by Beesel in 1853, a "magnetized" subject apparently ceased to breathe for from six to eight minutes, during which time his mouth remained so wide open that the onlookers thought he was dead. Still, shallow respiration is easily overlooked.

[1] Respiration is, as a rule, an involuntary act, but it can be influenced by the will to a certain degree—accelerated or retarded.

Numerous investigations have shown that suggestion exerts a certain amount of influence on the blood-vessels and the heart. Forel, Beaunis, and F. Myers have observed local flushing induced by suggestion. There is nothing astonishing in this, for we know quite well that the vasomotor system is easily influenced by mental processes. I have already mentioned (page 64) that embarrassment will cause blushing, and dread, pallor. I also mentioned some experiments of Dumontpallier, who induced by suggestion a local increase of temperature of as much as 3° C. To these may be added Krafft-Ebing's experiments, in one of which any body-temperature could be induced—for example, 36° C. Marès and Hellich also made some very interesting experiments on this point; they frequently found it possible to reduce the temperature of a hypnotic from 37° C. to 34.5° C. in the space of twenty-four hours. This reduction of temperature was not a direct result of suggestion, but was brought about indirectly by suggesting a diminished sensitiveness to heat and cold. I may also remind my readers that Lehmann and one of his friends found that they could increase the temperature of the palms of their hands to the extent of 0.06° C. and 0.02° C. respectively, by vividly imagining a feeling of warmth at the spot in question. Lœwenfeld also relates that he placed a thermometer in each hand of a person in the somnambulic stage, pointing out to the subject that his right hand was warm and moist, but the left hand cold; in an hour's time the temperature of the right hand had risen 1° C. Lœwenfeld further states that the subject was closely watched to see that he kept the thermometer firmly grasped. Unfortunately, this experiment, like so many others, proves very little. Lœwenfeld should have shown that the grip was constant throughout the experiment, for a change, small enough to escape the notice of those controlling the experiment, will easily produce a variation of temperature. As a curiosity I will mention the local reddening of the skin which has often been observed in spirit mediums (Carpenter, du Prel), and which has been explained as a supernatural phenomenon. As such mediums are often in a state of trance, which is possibly identical with hypnosis, this phenomenon admits of a perfectly natural explanation.

Some observations have also been made on the influence of suggestion on the action of the heart. I myself have often been able to produce a slowing of the pulse in cases of

palpitation. However, we should be cautious how we draw the conclusion that the suggestion has affected the nerves of the heart directly; the effect is doubtless an indirect one. For, independent of the fact that the action of the heart is to a certain degree dependent on the respiration, it is likewise under the influence of ideas, which affect the emotions. Such ideas have the power of quickening or slowing the heart's action; it is possible that the suggestion which retards a quick pulse only produces this result indirectly by a removal of the mental exciting cause, or, vice versâ, quickens the pulse by excitement. My observations of the quickening and slowing of the heart's action by suggestion lead me to take this view rather than that of a direct influence of suggestion on the nerves or nerve-centres of the heart. In any case, it would be difficult to exclude this indirect action. No matter how the result was brought about, Beaunis has seen several cases of momentary changes in the pulse without the respiration being affected by suggestion. He has seen the pulse fall from 98 beats to 92, and then rise to 115 beats. He infers a direct action on the inhibitory centre of the heart, and thinks himself also obliged to exclude ideas which affect the mental state, since the effect of the suggestion was always momentary. But his conclusions are not altogether borne out by his statements. We must also carefully bear in mind that the method of suggestion employed may so effect the emotions that the imagination can play no direct part in the result produced. To retard the pulse we must address the subject in slow and soothing tones; sharp and hasty remarks only conduce to acceleration. Bérillon also found that the pulse could be accelerated or retarded by suggestion. On one occasion he experimented on a case of pronounced tachycardia, and found that the pulse which was first of all accelerated by hypnosis from 132 to 138, could be reduced to 114, 102, and even 84 by suggestion. Bramwell also found that he could accelerate or retard a patient's pulse by suggestion. In one case, in which the subject when awake had a pulse of 80, hypnosis brought the beats to 100, but subsequent suggestion reduced them to 60 per minute. As Bramwell has rightly pointed out, indirect suggestion sometimes affects the pulse. For example, tell a man he must hurry up or he will miss the train, and his pulse is immediately accelerated.

It is not necessary to say much of those other functional abnormalities of the involuntary muscles which occur without the influence of suggestion. Max Rosenthal saw a case of vomiting which he attributed to irritation of the cerebral cortex; and nausea is often observed in persons who are easily excited or frightened (Friedmann).[1]

There are other ways in which suggestion can affect the involuntary muscles. Peristalsis is frequently influenced in this manner. I have had several experiences of the facility with which the bowels of some hypnotics are affected by suggestion. I say to one of them, "In half an hour after you wake your bowels will act." This is certain to act. "To-morrow morning at eight your bowels will act." The effect follows. "To-morrow between eight and nine your bowels will act three times." Exactly the same result, though the subject remembers nothing of the suggestion on awaking. It is interesting to note that the action of aperients can be arrested by suggestion, though this does not often happen. A patient takes a dose of castor-oil which is sufficient to procure copious action of the bowels. He is told in hypnosis that the medicine will only take effect in forty-eight hours. The suggestion is effectual, although with this person the dose habitually acts promptly and abundantly (v. Krafft-Ebing). Or let a few drops of water be given to the hypnotic with the assertion that it is a strong purge; motion of the bowels follows. Suggested emetics act in the same way. This is not very surprising, as we know that these and other functions, even though they are independent of our will, are yet under the influence of the mind. On the other hand, Murell reports that a hypnotic, Mr. X., was induced to drink tartar emetic for sherry. So long as X. was hypnotized he did not suffer from nausea, retching, or any of the usual results of a dose of tartar emetic. As soon as X. awoke he began retching, which could only be stopped by hypnosis. Vomiting at the sight of disgusting things, and the celebrated *mica panis* pills administered as aperients show that there may be suggestibility without hypnosis.

We find but scanty accounts of physiological investigations into the processes of secretion during hypnosis. Perspiration, which was noted by the earlier mesmerists—for example,

[1] This was communicated to me privately.

Heineken, Kluge, etc.—has also been observed by later investigators (G. Barth, Demarquay, Giraud-Teulon, Heidenhain, Preyer). I doubt if the secretion of sweat depends on the hypnosis; I believe that it is rather a result of the straining and excitement of fixed attention. We know something more about the influence of suggestion on secretion. Burot shows that the secretion of saliva can be induced by suggestion, and Bottey demonstrates the same thing of perspiration. Charles Richet says that erection and emission of semen can be effected by awakening in the subject's mind the impression of sexual intercourse. I have mentioned above that I have myself seen a hypnotic's eyes water when it was suggested to him that he was smelling an onion. By producing emotion it is possible to influence the secretion of tears. A gentleman who believed he was a child again, imagined he had just been disobedient to his parents, and as he asked forgiveness in the hypnotic condition he shed many tears. In a case of increased secretion of the gastric juice, Bergmann believes that he has exerted therapeutic influence by suggestion and rendered the gastric juice normal.

The secretion of milk is also under the influence of suggestion. A case, which only shows, however, the indirect influence of suggestion, has been reported by Hassenstein. In a wet nurse in whom the secretion had ceased, it again flowed copiously by suggestion. It had ceased, however, owing to excitement over the child's condition, and was renewed by suggesting away the excitement. J. Grossmann reports a case in which the secretion of milk was produced by direct suggestion, and Bramwell states that Esdaile and Braid knew of the influence of suggestion on the secretion of milk.

The literature of the question contains a few statements which seem to show that hypnosis influences the secretion of urine; but, undoubtedly, in some of the cases reported, the act of micturition, and not the secretion of urine, was influenced by suggestion. It is often a very easy thing to make a subject empty his bladder. One has only to say to a subject in deep hypnosis, "You must make water directly you wake up," or "You must make water five times during the first hour after you wake up," and in many cases the suggestion proves effectual. Few investigations have been made as to whether the secretion of urine can be influenced

by suggestion. However, Wetterstrand mentions results produced in diseases of the kidneys which almost justify the conclusion that in certain persons it is possible to influence the kidney secretions of suggestion. This is not so strange when we reflect that many diseases in which there is increased secretion of urine are of nervous origin, and that anxiety and fear may influence it at any time.

Krafft-Ebing draws conclusions as to the increase of intestinal secretions from one experiment. He suggested to his subject a profuse watery evacuation of the bowels, which followed. As the bladder had been emptied shortly before, and only a small quantity of water had been found in the urine, Krafft-Ebing thinks himself obliged to consider the fluid as an increase of the intestinal secretions.

Some investigations have been made as to how metabolism is affected during hypnosis, but we should certainly hesitate to draw any hasty conclusions from them. Brock finds that in a short hypnosis of twenty minutes' duration, with partial catalepsy of the extremities, the sum of the solid constituents, and also the phosphoric acid in the urine, decreases, as Strübing has described in catalepsy. But as Brock forgot to examine his patients under analogous circumstances—*i.e.*, sitting quietly without hypnosis (Preyer), his experiments prove nothing. In any case, no conclusion as to the action of the brain must be drawn from them, though Brock concludes that the activity of the brain is lessened, because the quantity of phosphoric acid is decreased. Gürtler also found that the quantity of phosphoric acid varies, but as he did not make any comparative experiments with the same subject in analogous circumstances without hypnosis, he refrains from drawing final conclusions; because, to justify these the evacuations of the bowels and the respiration must be investigated also. A. Voisin and Haraut conclude from their investigation of the urine of hypnotic subjects that assimilation is carried on better during hypnosis than in the waking condition, and that hypnosis is not a pathological condition. Gilles de la Tourette, who made similar experiments with the assistance of Chatelineau, holds the opposite opinion. According to him, the secretion of urine keeps on diminishing as the hypnotic sleep is prolonged. Both these investigators concluded from their experiments that hypnosis

and hysteria are closely related, even as far as metabolism is concerned.

I now come to some phenomena which almost invariably awaken mistrust. I mean the anatomical changes effected by suggestion during hypnosis. No matter how sceptical we may be on this point, it would be perverse to deny the possibility of such phenomena. We certainly do know that organic changes can be brought about by mental processes. I need only recall the physiognomy of certain professions—for example, the type of the clergy shows how a spiritual and mental avocation gradually exercises an influence on the physiognomy. In the hypnotic experiments which I shall now proceed to describe the process is only somewhat more acute.

Among the experiments in this direction I will first of all mention the cases in which menstruation is affected, more especially those in which menorrhagia is induced or arrested by suggestion. It is not to be doubted that this is practicable in the case of certain persons. Forel has made a whole series of experiments on this point, and has also partly confirmed the accuracy and the effect of suggestion by personal investigation. Many other experimenters have also been able to confirm the effect of suggestion on menstruation (Liébault, Brunnberg, Sperling, A. Voisin, Gascard, Briand). The influence of suggestion in menorrhagia seems less wonderful when we reflect how very much psychical influences otherwise change it. It is well known that the periods often become irregular in women who are about to undergo a surgical operation.

I have mentioned the influence of suggestion on menstruation in this place in spite of the fact that these experiments do not, properly speaking, demonstrate an organic change. We may be concerned here with a vaso-motor disturbance, which secondarily induces the organic changes. This appears to me probable.

I may further mention the experiments of Bourru, Burot, and Berjon, who induced bleeding by suggestion in the same subject as Mabille, Ramadier, and Jules Voisin. Puységur had witnessed the same thing. Bleeding of the nose appeared at command in the above-mentioned subject, and later on bleeding from the skin at a time decided on beforehand.

When the skin had been rubbed with a blunt instrument in order to direct the suggestion, bleeding of the skin is said to have appeared at command, the traces of which were visible three months later. It is interesting that in the case of this person who was hemiplegic and anæsthetic on the right side, the suggestion would not take effect on that side. Mabille's observations of this subject are particularly interesting, because they show that a person in hypnosis can cause these bleedings by auto-suggestion.

Unfortunately, the accounts we possess of this case do not tell us whether contact induced bleeding under other circumstances (F. Myers). But any possible error is excluded by the fact that the bleeding did not follow closely on the contact, which would have been the case if the effect were mechanical. Berjon reminds us also that precautionary measures were taken to prevent the subject from touching his own arm and thereby accidentally causing a wound. Artigalas and Rémond later on published the case of a woman of twenty-two in whom tears of blood appeared. By suggestion it was also possible to induce bloody sweat on her hand. Laguerre and Bardier have dealt with this woman's illness, but in the abbreviated report which alone I have seen, it is not stated whether the sweat was subjected to a microscopical and chemical analysis. Everybody will here call to mind the stigmatics of the Roman Catholic Church. Bleeding of the skin is said to occur in them, generally in spots which correspond to the wounds of Christ. I shall return to this question in a later chapter. At the present moment I may just mention that Hebold once successfully treated a case of hysteria by suggestion, and that by the same means he caused red patches to disappear and a wart to fall off.

The experiments of Delbœuf, who worked in common with Winiwarter and Henrijean, also belong to the class of organic lesions. Delbœuf produced symmetrical burns, and made one of the wounds painless by suggestion. It was observed in this case that the painless wound showed a much greater tendency to heal, and, in particular, that the inflammation showed no tendency to spread. As, however, there were some slight anomalies, the experiments are not fully convincing.

Jendrássik and Krafft-Ebing obtained marks like burns on one of their subjects by means of suggestion. If some object, such as a match-box, a pair of scissors, a snuff-box, a linen-

stamp, etc., was pressed upon the skin in the morning, and the subject was at the same time told that his skin was being burned, a blister in the form of the object resulted in the afternoon. The marks remained a long time visible. If the object was pressed on the left side of a patient who was anæsthetic on the right, the burn appeared symmetrically on the right almost as if reflected in a glass, as could be especially seen if letters were used. Jendrássik maintains that deception was absolutely excluded in these cases of suggested burns; and a dermatologist, Lipp, at one of the experiments, declared that it would be impossible to cause the suggested lesion by any artificial means, either mechanical or chemical. Burns by suggestion have often been observed in the Salpêtrière. Pierre Janet experimented on a patient who showed wounds like those of Christ. Schrenck-Notzing also observed that a wound which this same patient had in the cardiac region bled on Good Friday. In one case Janet suggested stigmatization of the right instep; the skin turned red at the spot suggested, and a blister followed which healed by scabbing. During the experiment the patient was very carefully watched, the foot being enclosed in a sheet of copper having an aperture in which a watch-glass was inserted, so that the changes which the skin might undergo at the spot suggested could be observed (Schrenck-Notzing). Ryvalkin and Wetterstrand have also seen burns induced by suggestion, and Podiapolsky states that he succeeded in producing blisters in the case of a peasant-girl, aged eighteen. The girl was thrown into hypnotic sleep at half-past nine one evening, and the suggestion made that she should be affected by a blister like that produced by a burn; at a quarter past twelve the next day there was a blister filled with serous fluid; but the details given do not suffice to show that the experiment was properly controlled.

The experiments made by Schrenck-Notzing on one patient belong to this category. Flach, of Aschaffenburg, experimented on a girl of twenty, and succeeded in producing erythema by suggesting that the cold key held to her skin was red-hot. The erythema was visible three weeks after the experiment was made, and healed by superficial exfoliation. Wheals have often been produced by contact with a solid substance, although the subject experimented on had never previously suffered from any spontaneous form of nettle-rash. Flach, who first of all experimented in conjunction with Offner,

tested the same patient by some experiments which he made with Parish's assistance for the purpose of ascertaining whether severe pain, followed by itching and the formation of blisters, could be induced by suggestion, without the skin being directly stimulated. As a matter of fact, a watery blister appeared the next morning. In further experiments in which it was suggested that a burn was caused by the thermo-cautery, erythema and blisters of various sizes were produced which could hardly have been caused by artificial means, as the spots selected were covered with wadding and plaster and sealed up. An experiment was made in Munich in the presence of a number of specialists. The subject was watched constantly, and the possibility of any mechanical injury obviated by means of a bandage. In this case it was suggested that a particular spot on the back of the fore-arm had been burned by the thermo-cautery, but when the bandage was removed the injury was found to be in front. As, however, the bandage had been pierced in several places and a hair-pin was found in it, it was suspected that the injury was self-inflicted. To avoid any possibility of doubt, another experiment was made in which a plaster-of-paris bandage was used; when the bandage was taken off it was found that nothing had happened to the skin.

The fact that the results in this case diminished in direct proportion to the precautions taken is not an off-hand proof that suggestion produced no effect. It is very possible that the extraordinarily strict precautions taken so affected the subject's mind that any reaction to suggestion was excluded. On the other hand, the case does not conclusively prove that organic lesions can be induced by suggestion.

I now come to some experiments in which the hypnotic was told that a blister had been applied to him, which blister was only an ordinary piece of paper. As Binet and Féré inform us, this experiment was made as long ago as 1840 by an Italian doctor, Préjalmini, and Du Prel tells us that in 1819 a sloughing of the skin was obtained by an ordinary piece of linen in the case of a somnambule who had been magnetized by Celicurre de l'Aupépin. Focachon, an apothecary of Charmes, has recently repeated the experiment. Sometimes alone, and sometimes in company with the Nancy investigators, he applied pieces of paper, suggesting they were blisters. He is said to have often produced blistering. Beaunis has published an exact report of some experiments of this kind.

After the experiment had lasted for twenty-one hours the paper was taken off, and it was found that the skin was thickened, dead, and of a yellowish tint; later, perhaps as a result of the pressure of the clothes, several small blisters appeared. The reverse experiment has also been successfully made by the Nancy investigators, who were able to counteract the effect of a real blister by suggestion. Meunier has published an account of such an experiment made at Nancy. Forel, also, often tried to produce organic changes by suggestion. In one attempt to produce blisters little pustules of acne appeared. Besides this, Prof. Forel has made some other experiments, the results of which he has kindly allowed me to publish.

The experiments were made on a nurse, twenty-three years old, who was not in the least hysterical. She was the daughter of plain country people, and had been for a long time an attendant in the Zürich Lunatic Asylum which Forel directs. Forel considers she was a capable, honest person, in no way inclined to deceit.

A gummed label was fixed upon her chest above each breast; its shape was square, and in no case was an irritating gum used. At midday Forel suggested that a blister had been put on the left side; and at six o'clock in the evening a moist spot appeared at this place; the skin was swollen and reddened around it, and a little inflammation appeared also on the right side, but much less. Forel then did away with the suggestion. On the next day there was a scab on the left side. Forel had not watched the nurse from midday till six o'clock, but had suggested that she could not scratch herself. The other nurses said that the subject could not raise her hand to her breast, but made vain attempts to scratch. Forel repeated the experiment later; he put on the paper at 11.45 a.m., and ordered the formation of blisters in two and a half hours. Little pain was suggested, and therefore the nurse complained but little. At two o'clock Forel observed that the paper on the left side, which had been used for the experiment, was surrounded by a large red swelling. The paper could only be removed with difficulty. A moist surface of the epidermis was then visible, exactly square like the paper. Nothing particular appeared under the paper on the right side. Forel then suggested the immediate disappearance of pain, inflammation, etc. Nevertheless the place wept and suppurated for a week, and the scab lasted for some time. Even when Prof. Forel related this

to me, seven weeks later, the place was still brownish. The nurse was a little annoyed and uneasy about the experiment, and she was not strictly watched while it lasted.

A few days after this experiment Forel drew two very light crosses with the point of a blunt knife on the same person. They did not bleed. Another cross (Fig. 1) was made on the inner side of each fore-arm. Several doctors were present.

Fig. I

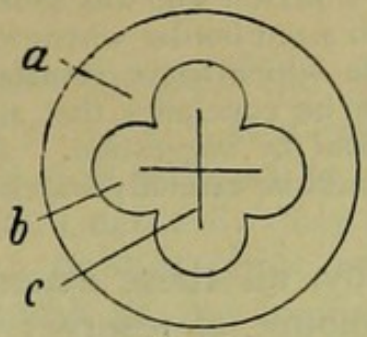

Fig. II

Forel suggested the appearance of blisters on the right side. Even at the end of five minutes, during which Forel watched the subject, a considerable reddish swelling of the skin appeared (Fig. II., *a*). A wheal, *b*, had formed itself round the cross, *c*, somewhat in the shape of a cross. On the left side nothing was to be seen but the cross that had been drawn, unaltered, as in Fig. I. The wheal on the right side resembled a vaccination pustule, in the form of a cross: but it was simply a dry papular swelling, as in nettle-rash. Forel then suggested the disappearance of the swelling and the wheal, and, further, the appearance of a drop of blood at the end of an hour. At the end of this time a very small drop of blood was to be seen; but the wheal, redness, and swelling had disappeared. But as Forel had not watched the subject during this hour, he attached no importance to the drop of blood, which might have been caused by the prick of a needle.

Later on it was only possible to cause a slight reddening of the skin. Forel only lays weight on the experiment in which the papular swelling was produced, but even that he does not consider absolutely conclusive. Still, the fact that the wheals only appeared on one side tells against the theory of mechanical irritation. Of course it may be objected that both sides were not scratched alike; but, as Forel points out, the nurse was not one of those persons who get a wheal whenever their skin

is slightly stimulated. Except when bitten by gnats she never showed any wheals. When her skin was scraped it showed a disposition to redden, but wheals never formed. She had, besides, often been scratched by insane patients, but no remarkable result had ever been observed.

It should be added that there are people who develop wheals under mental excitement without hypnosis. A trustworthy observer told me of the case of a person who was so frightened of thunderstorms that he showed wheals with a red border whenever one was approaching. D. Weiss tells of a case in which fright invariably caused an attack of *herpes labialis*, from which he concludes that structural changes in the tissues may be brought about by suggestion. At the same time he considers that the subjects should be carefully watched before drawing any conclusion.

Naturally, all these experiments must be received with a certain amount of reserve; not that we have a right to deny that anatomical changes can be produced by suggestion, but because the evidence of such changes having taken place must be unimpeachable before we can accept it. Otherwise we should show ourselves as incapable of conducting a critical investigation as the quacks who pretend in their advertisements to be able to cure any and every organic disease by magnetism or some other universal specific. We must also participate in Schrenck-Notzing's scepticism as to the conclusiveness of the experiments described above. Most of the objections raised by the sceptics are to the point, especially those which refer to inefficient supervision of the subjects experimented on. But Lœwenfeld thinks that Schrenck-Notzing has carried his scepticism too far, because Charcot did prove in one of his experiments that trophical processes can be influenced by suggestion. On five consecutive days he suggested to a hysterical patient, in hypnosis, that his right hand should swell and become larger than the left; that it should turn bluish red in colour, and, further, become hard and also colder. The suggestion is said to have resulted in the right hand swelling to twice the size of the left. The hand also became cyanotic and hard, and the temperature fell about three degrees. At all events, no matter how sceptical we may be, we are not justified in straightway denying the possibility that suggestion may induce organic changes in the skin merely because we have never seen such changes, or because their occurrence is rare. Nevertheless, rarities do occur, such as

miscarriages which cannot be accounted for, triplets, and millionaires. We believe in their existence, although we may have never seen them ourselves. Because a phenomenon is rare, or that we have never observed it, is no argument against the possibility of its occurrence. Consequently the observation of any rare phenomenon is valuable.

(2.) *Psychology.*

In the foregoing sections we have studied the physical symptoms of hypnosis, and have only casually touched upon some psychical phenomena. We will now proceed to investigate the latter more closely, but I shall naturally only discuss such as are characteristic from our present point of view. For practical reasons I must first speak of the memory, because it determines the other psychical activities. Without memory no action of the understanding is possible, and all the higher mental functions depend upon the memory. Memory during and after hypnosis has been specially studied by Richet, Delbœuf, Dichas, Beaunis, and Pitres.

Memory, in its broadest sense, consists of three parts: firstly, of the power of retaining ideas; secondly, of the power of reproducing these ideas; thirdly, of the power of recognizing the ideas and of localizing them correctly in the past. To make this clear, let us take any event which we remember—for example, a severe scolding given to us by a teacher. The memory in this case acts in three ways: in the first place, what is said is received and retained in it; in the second place, the memory can reproduce the lecture; and in the third place, we can place it in its correct position in time by recalling its relation to other events, such as being at the school, etc. But the power of retention is only made evident by the ability to reproduce an impression; consequently the first two faculties are apparently interdependent. Still, it is customary to make a theoretical distinction, and William James calls retention primary memory, and reproduction secondary memory. At all events, although reproduction may be the only proof of the retention of ideas, the two processes can easily be distinguished. This is easily demonstrable. There are times when we cannot recall certain events, although at other even more remote periods we can do so. Which means that although in the first case the impression could not be immediately reproduced, it had nevertheless been retained because it was reproduced later on; a fact which shows that the two processes must be differentiated. The same holds good of the third power—that of correctly localizing past events. That a name should occur to me without my being able to connect it with some past event shows that the third of the powers which we have discussed is distinct from the simple reproduction of an impression.

The retention of ideas in hypnosis has been little investigated. Beaunis has found no essential difference in this respect between hypnosis and waking life. Max Dessoir has also made experiments, the results of which he has communicated to me. From these it appears that memory is weakened in deep hypnosis, when this is not prevented by suggestion. Max Dessoir repeated a number of syllables which the hypnotic was to try to remember; a suggestion of improved memory was entirely avoided. Under these circumstances the hypnotized subject remembers fewer syllables than did the same person when awake. The older mesmerists (Wienholt), on the contrary, believed that the memory was intensified in the magnetic sleep; poems could be learned by heart in a much shorter time than in the normal state. However, these investigators did not altogether avoid suggestion.

Is the chain of memory in ordinary life broken by hypnosis or not? It was formerly supposed that the subject always forgot on awaking what had happened during hypnosis; but this view has not proved correct.

In the lighter hypnotic stages, especially in the first group, there is rarely any abnormality of memory; the subject remembers everything in the hypnosis of which he was conscious in normal life, and after hypnosis recollects all that had occurred during that state. In the deeper hypnoses it is very different; they belong for the most part to the second group, and only a few to the first, and there is loss of memory (amnesia) after the hypnosis. The subject is much astonished when he hears what he has been doing during the hypnosis—that he has been running about, that he has had hallucinations. Sometimes, however, a dim memory persists, like the memory of a dream. I suggest to some one the hallucination of a bird flying about the room; the hypnotic tries to catch it, amuses himself for a long time with it, gives it sugar, puts it in an imaginary cage, and so forth. After waking he dimly recollects that he has seen a bird, but that is all; he certainly does not believe that he has left his seat. However, there are certain people who recall everything after being told what they have done during the hypnosis.

In other cases, associations of ideas will call up memory. A hint is given to the hypnotic after the awakening and everything recurs to him (Heidenhain). There is something of this sort in dreams; we very often remember a whole dream when we

see some object that is in any way connected with it (Delbœuf). The same thing happens when a person is able to repeat a quotation or a poem directly he has heard the first words. Let us consider an example in hypnosis. I suggest a concert to a hypnotic; he hears various pieces, and among them the overture to *Martha;* meanwhile he eats his supper at the concert, drinks his beer, and talks to imaginary people. After the awakening there is no trace of memory. I ask him then if he knows the opera of *Martha;* this suffices to recall nearly all the events of the hypnosis. Sometimes memory is aroused in the same way by pure chance. X. believes in hypnosis that he sees a number of persons he knows at my house whose presence I have suggested to him. He goes through several scenes with them, but remembers nothing on awaking. Only when he meets one of the persons several days later does the whole thing recur to him. Delbœuf draws attention to one method of making the memory last: he thinks that subjects remember any hypnotic event if they are awakened in the middle of it; but this is certainly not universally true (Gurney). On the other hand, it often happens that the first or last occurrences are remembered, while all the others are forgotten. It has often been observed that memory after awakening can be produced by a special effort of the hypnotist (Bleuler, Pierre Janet). Bernheim even thinks that memory can be produced in all cases by means of a strong suggestion after awakening, especially if the suggestion be strengthened by causing the subject to place his hand on the experimenter's forehead. Some persons remember all the hypnotic proceedings during their nightly sleep; it is not rare for the hypnotic dream to be repeated in natural sleep. It is also possible in all cases to prevent post-hypnotic amnesia by suggesting during the hypnosis that the patient on awaking is to remember all that has happened; on the other hand, post-hypnotic amnesia can sometimes be produced by suggesting to the hypnotic that he must forget all that has happened during the hypnosis. But such cases are comparatively speaking rare. Bernheim observed cases in which, without any suggestion being made, the subject on awaking had forgotten not only what had happened during the hypnosis, but events which had immediately preceded its onset.

But if we avoid using any kind of suggestion, then, in some cases, chiefly in the deepest hypnosis, memory of even the

hypnotic proceedings cannot be recalled after awakening. In such cases the person does not generally know how long he was in the hypnotic state. On the other hand, the subject remembers in hypnosis all that has happened in previous hypnoses. Things that happened in hypnoses many years back, even as many as ten, may be recalled, although they are completely forgotten in the waking state. Wolfart relates the case of a woman who remembered in the magnetic sleep all that had taken place in a magnetic sleep thirteen years before, although in the meantime she had never recollected it.

Events of the normal life can also be remembered in hypnosis, even when they have apparently been long forgotten. This increased power of memory is called hypermnesia. Benedikt relates a case of it. An English officer in Africa was hypnotized by Hansen, and suddenly began to speak a strange language. This turned out to be Welsh, which he had learned as a child, but had forgotten. Breuer and Freud point out that many cases of hysteria are called forth by some psychic moment that the patient cannot recall in the waking condition, though hypnosis may again bring it back to memory. Vogt and Brodmann have given reasons for employing hypnotic hypermnesia in therapeutics, and the former has also used it in psychological analysis. Hirschlaff, however, who has made many investigations on the subject, feels compelled to deny the existence of any increased power of recollection in hypnosis. He is convinced that the apparent increase of recollection depends either upon the suspension of an emotional form of inhibition which existed in waking-life—for example, a hypnotic will relate something which modesty would compel him to withhold in waking-life—or upon auto-suggestion brought about by the verbal suggestions of the experimenter.

Such cases of increased recollection recall others which are mentioned in the literature of hypnotism; for example, the famous one of the servant-girl who suddenly spoke Hebrew. She also, in an abnormal state of consciousness, spoke a language which she did not know, but which she had often heard when young in the house of a clergyman. We hear of like cases of hypermnesia in dreams. Maury, whose investigations on the subject of dreams are classic, relates a number of things which returned to him in dreams, although when awake he knew nothing about them. The heightened faculties of hypnotic subjects of which we so often hear, and which we can observe in auto-hypnosis also, are a result of this increased power of reproducing ideas. Many apparently supernatural facts can be explained in this way. I shall refer to this later on.

Dreams, also, which have occurred in natural sleep are sometimes reproduced in hypnosis, although they may have been forgotten on waking. It is naturally very difficult to judge of the accuracy with which dreams are reported. But as dreams sometimes leads to talking in sleep, it is then possible to make observations. I know of many cases in which persons betrayed their dreams by talking in their sleep; in several instances I was able to show that the loss of memory which followed on waking disappeared in hypnosis, and the dream was remembered. In one case a bed-fellow was able to confirm the accuracy of the recollection. The occurrences of a pathological condition may be reproduced in hypnosis just as we have seen those of a dream. Bramwell mentions a case reported by Morton Prince, in which a hypnotized person remembered many things—especially those which had occurred during the delirium of fever—which could not be recollected when the subject was in a normal state. In recent years this question has occupied the attention of several authors, among whom may be mentioned Naef, Gräter, Hilger, Muralt, Heilbronner, Binswanger, and, more especially, Riklin. They endeavoured to restore in hypnosis, memory, the loss of which had been caused by epilepsy or hysteria. Binswanger and Heilbronner consider that the amnesia of hysteria can be dispelled in hypnosis, but not that caused by epilepsy; on the other hand, Riklin concludes from his experiments that the amnesia of epilepsy may also disappear. Bramwell experimented for the purpose of ascertaining if in hypnosis a person could recollect what had happened to him when under the influence of ether or laughing gas, but only obtained negative results. Other experiments in which attempts were made to cause subjects to recollect what had gone on around them while they were in natural sleep were equally unsuccessful. A sentence was read to X. while he was asleep and repeated several times, but he was unable to recollect it when he was hypnotized. Brodmann tells us that Vogt was able to restore in hypnosis the memory in some other cases of amnesia; for example, in the acute delirium arising from neurasthenia, in post-epileptic amnesia, and loss of memory arising from some infective disease; but never in the case of a real epileptic fit.

It sometimes happens that the hypnotic does not remember all that occurred during the hypnosis. Thus matters of no interest to him escape his notice just as they would if he were

awake. In some cases the cause of the amnesia cannot be ascertained. I said to a hypnotic, "In five minutes time you will call out 'Ha!' three times." He did as he was told, but did not afterwards remember that he had called out. But apart from this, several states of consciousness may occur, so that a subject in one state does not know what occurred when he was in another. From this Gurney supposes two stages of hypnosis, distinguished from each other by completely different memories. The old magnetizers described such stages. Gurney distinguishes two stages, *a* and *b*. In stage *a* the subject knows nothing of stage *b*; and in *b* nothing of *a*. It cannot be disputed that in some persons several sharply divided states of consciousness may exist, apart from the waking consciousness; this is also affirmed by Krafft-Ebing, Max Dessoir, Pierre Janet, and others; but I think it erroneous to speak of it as universal. I cannot take upon me to decide whether Gurney, who was an excellent experimenter, may not have used suggestion unconsciously. But I consider it necessary to emphasize the fact that I have recently seen cases in which separate states of memory have apparently arisen spontaneously; they may, of course, have been brought about by auto-suggestion or indirect suggestion. We shall see later on, when discussing change of personality, that such chains of memory can be easily induced directly by suggestion.

But apart from these cases of hypermnesia it is characteristic that in the deeper hypnotic states not only the events that have taken place in earlier hypnoses are remembered, but also the events of waking life. On the other hand, in the waking state the events of that state alone are remembered. This state of things is named "double consciousness" (*double conscience* in the broad sense of the term). It was evidently well known to the old mesmerists—Kluge and Deleuze, for example—and was also observed by Braid.

The state of double consciousness is also found under pathological conditions. One of the best-known cases was published by Azam. The life of the patient, one Felida by name, for nearly thirty years was divided into certain periods—a, b, c, d, e, f. In the periods a, c, e (normal condition) she remembered only what had happened in them; in the periods b, d, f (second condition) she remembered what occurred in these periods, as well as what had happened in the periods a, c, e. The normal state was a, c, e, while the pathological was b, d, f. Osgood Mason has published a similar case which was watched for ten years; a remarkable feature of this case was that the patient, a woman, was thoroughly

acquainted with the medical history of Felida (Azam's case). This may, perhaps, account for her illness. Many other similar cases have been published. Max Dessoir's thoughtful work on the "Doppel-Ich" contributes much to the elucidation of this question of double-consciousness; he shows that indications of such a splitting of the consciousness are much more common than has hitherto been believed; he refers us to examples in dream-life and in pathological states. I shall return to the double consciousness with more detail in the theoretical part of this book.

One phenomenon which I have often observed depends on memory in the later hypnoses. If a whole series of scenes is suggested to a subject in hypnosis a very slight impulse suffices to cause the whole panorama to pass before him again in a later hypnosis. A hypnotic imagines himself hunting a lion; he kills the lion and devours it; and then by suggestion he is turned into a general, and then into a child. On awakening he forgets all these things. In a later hypnosis he hears an unexpected noise, which he immediately believes to be the roaring of a lion. In consequence he goes through all the scenes again, without omitting the smallest detail. This incident may be accounted among the indirect suggestions, since the auto-suggestion was aroused by an accidental circumstance.

The case observed and quoted above by Mabille, in which a person induced hæmorrhage by auto-suggestion, after it had once been induced by external suggestion, belongs to the same category. The subject separated herself, so to speak, into two persons, one of whom made suggestions to the other, as is proved by the conversation which she carried on with herself.

The subject's recollection of all that he has experienced in earlier hypnoses is most important. The possibility of hypnotic training depends upon the strength of the recollection, which is also a frequent source of error in new experiments, since they are easily spoiled by memory of the earlier ones. I say to the hypnotic (X.) "You will now raise your left leg." X. does so. While I make the suggestion I unintentionally take hold of his right hand. When, in a later hypnosis, I again take hold of his right hand, X. lifts his left leg. Evidently he remembers the first event, and regards the taking of his hand as an order to lift his leg. It is probable that the new reflexes which Born and others thought they had discovered, and which I have mentioned before, came about in this way.

Although opinions may differ as to whether hypermnesia may be induced in hypnosis, by means of suggestion, the possibility of inducing errors of memory (paramnesia), or failures of memory (amnesia), can hardly be doubted; Bertrand collected many observations on these points. These memories

may consist of former perceptions; the suggestive influence of these former perceptions has often been observed: by means of them the subject may be completely deluded about his former experiences. Since these suggestions have a certain retroactive force, they are called retroactive suggestions; or as they are concerned with supposed sense perceptions, they are sometimes termed retroactive hallucinations, either of a positive or negative order, according as a new memory is created or an old one annulled.

I say to a subject: "Of course you remember that we went to Potsdam yesterday, and took a drive on the Havel?" The suggestion takes effect, and he at once begins to relate all that he believes we did in Potsdam. This is a retroactive positive hallucination, because the hypnotic believes that he has experienced something that never really occurred. The following would be a retroactive negative hallucination, as the hypnotic forgets something which did happen. I say to him: "You have not had any dinner; you have not had any breakfast." Upon which he immediately feels hungry, as he thinks he has had nothing to eat since he got up.

Many motor disturbances of which I have before spoken may be reckoned as related to amnesia, or loss of memory. For example, when I tell somebody that he cannot lift his arm, or that he cannot speak, I am sometimes dealing with loss of memory, because a movement is made impossible if the memory of it cannot first be called up. This is the case in those paralyses which some French authors (Binet, Féré) call *paralysies systématiques*—a paralysis for a special act. Such a paralysis is not followed by total functional incapacity of a whole group of muscles; the function is rather interfered with for one particular use only. The incapacity to say *a*, or to sew, for example, would be a paralysis for a special act; if the person could not speak or move his arm at all, this would be a complete paralysis. It is possible in this way to deprive the subject of all memory of the letter *a* for a considerable time, so that he can neither speak nor write it. It is possible to produce almost all kinds of aphasia experimentally, as Kussmaul, Arndt, and others have demonstrated. We can cause any one to forget a language he has learned—French, for example (Forel, Frank); we can make writing impossible (agraphia). By a suitable suggestion a hypnotic can be deprived of the power of making himself understood by facial expression (amimia). Drawing, sewing, every form of activity, in fact, can be prevented by suggestion.

There is a particular group of disturbances of speech in which the power of speaking is more or less lost, though the perception of words remains intact (motor aphasia). It is easily induced by suggestion. But we can go farther, and not only deprive a subject of the power of speaking but also of the perception of speech, or even of any particular word or letter. It is possible to deprive him of the very idea which he attaches to a letter—*a*, for example. This difference will become clear if we observe the behaviour of a person under the different circumstances. If he retains the idea of the letter he is conscious of his inability to utter it; he is aware that he is speaking or writing nonsense, because he has no letter *a*, and even tries to avoid words in which that letter appears (Max Dessoir). But if he is deprived of the conception or idea of a letter, he is no longer surprised that he cannot write or speak it. This becomes still more interesting in post-hypnotic suggestion. It is possible to cause a post-hypnotic loss of memory, and to make the subject at the same time replace one letter by another. I told a hypnotic that after he was awakened he would always say *e* instead of *a*. I woke him, and asked, "Are you awake?" "Je" (Ja), he replied. When asked what he had been doing, he replied: "Ich heb' geschlefen" (Ich habe geschlafen). The subject laughed, but was at the same time slightly annoyed because he could not utter the letter *a*, and was perfectly aware that he was talking nonsense. But if the idea *a* were also missing, or had been replaced by the idea *e*, the subject would say *e* instead of *a* without observing it.

I have shown above that subjects may be made to forget certain of their experiences (negative retroactive hallucinations). In the same way, whole periods can be made to vanish from a subject's consciousness. Mr. X., who is forty-three years old, was told, "You no longer remember anything that has happened to you since you were thirty." This sufficed to cause a blank in X.'s consciousness. He was unable to answer any questions about this period; he did not know how he made my acquaintance, nor how he got into my room; when such questions were put to him he invariably shrugged his shoulders and answered, "I don't know."

It is possible to carry this still farther, and transport the subject back to an earlier period of life. In this case the subject finds no gaps in his memory; he believes that he is

living in this earlier time, and brings his present surroundings into relation with it.

Here is the case of a man who fought at St. Privat in the French War. I suggested to him that he was younger, and in the battle. He stood up at once, gave military orders, and commanded the artillery to open fire. When I asked him if he knew Dr. Moll, he said, "My doctor's name is R——. I do not know Dr. Moll." He knew nothing that had happened since the day of the battle; he was totally unaware of the rheumatism for which I was treating him; he said he was quite well. When I asked him who I was, he replied he did not know. It was interesting that he could not be induced to retreat; I tried to make him take a few steps backward, but he replied, "I will not retreat one step without orders." I suggested that the enemy was approaching, but nothing would induce him to retreat. When I drew his attention more and more upon myself, and told him that he must know who I was, the situation suddenly altered. He recognized me, and knew his real age, but had no notion of what had just passed.

A lady, aged thirty-four, whom I caused to believe that she was eight years old again, asked for her doll, spoke in a childish voice, cried when she thought I was about to take her doll away, and called for her mamma.

Finally, it is possible to make a person believe that he has never been born. Even this suggestion will be accepted, and the consciousness will be an absolute blank.

By placing a subject back in some earlier period of life—childhood, for example—Krafft-Ebing endeavoured to obtain an experimental solution of the question as to whether events which have disappeared from consciousness can be brought back during hypnosis. He considers that his experiments have settled this question in the affirmative, and further thinks that the changes in handwriting and style of expression noticed in such cases are typical of what the subject must have been when a child. But a different interpretation of Krafft-Ebing's experiments has been given by other investigators. Jolly, Köhler, and Lœwenfeld assume that when such a subject behaves like a child he does not really depict his own childhood, but taking the experimenter's suggestion to mean that he should act like any child, he behaves as he imagines a child would. I think that this interpretation may be taken as generally accurate. Still, we must not exclude the possibility of that form of hypnotic hypermnesia being occasionally met with; a suitable subject could easily be influenced in that way, with the result that a mnemonic image would be reproduced.

New memories can be created at the time the old ones are cancelled. This is the case with the phenomenon which Charles Richet describes as *objectivation des types*. In this case the subject believes himself another personality, another being; not only do many memories connected with his own *ego* disappear, but he also endeavours to connect the remaining memories with his suggested personality. Durand de Gros was acquainted with these phenomena; he appears to have come across them in America, where they were already observed in 1840.

I told a certain Mr. X. that he was Dr. Moll, and that I was Mr. X.; upon which he asked me to take a seat, so that he might hypnotize me. He attempted to do so; he went carefully through the process which I go through with him, and did not forget to make several pleasant suggestions.

I experimented with another man in whom these phenomena are very easily produced. He would represent with dramatic vividness any character which was within the grasp of his ideas. I told him, "You are Napoleon I.," upon which he immediately assumed the famous position of Napoleon after the battle of Waterloo, but spoke German, as he did not know French. As Frederick the Great, he walked with a crutch in the well-known gait, and knew nothing about railroads. Subjects can be made to believe they are animals; they will bark like dogs, or croak like frogs. They can even by suggestion be changed into inanimate objects, such as stoves, chairs, tables. When X. thinks himself a chair he crouches down on both legs; when it is suggested that the chair has a broken leg, he sinks his knee to the ground and rests on one leg; when he is a carpet he lies flat and motionless. These experiments in suggestion may be carried still further. "You are made of glass," I say to a hypnotic; he stands perfectly still. When I tell another that he is made of marble, he stands stiffly and cannot be moved; but directly he believes himself to be made of wax he becomes plastic and allows himself to be placed in any attitude.

It should be remarked that the subject always obeys the experimenter, even when he believes himself an inanimate object. Moreover, hypnotized subjects are not always consistent in these experiments; they often forget their part, though this may be generally prevented by training. For example, another person whom I had changed into Frederick the Great travelled contentedly in a railway carriage, evidently not reflecting that there were no railways in those days. Another, whom I had carried back into the year 1864, spoke of the new German Empire, of the Emperor William I. In spite of such inconsistencies, the mental images are much more consistent with hypnotics than with many lunatics who believe themselves to be kings and prophets. The inconsistencies of lunatics are much greater, and hypnotics sooner get rid of them. Besides this, when they represent a new personality, memories of former experiences disappear more completely than is the case with lunatics (Cullerre). In hypnotic change of personality each change is usually accompanied by loss of memory of that which preceded it. One person whom I hypnotized was unable to remember as Napoleon what he had done as Frederick the Great.

The change of personality in hypnotics has often been compared with the performances of actors. It is the opinion of the actress Dumesnil

that the actor who himself creates the idea of his part and allows himself to be governed by it will play his part the best; others—for example, the famous Clairon—held a different opinion on this point. In any case, few actors are able to accommodate and assimilate their own idea of a character —*e.g.*, that of Julius Cæsar, as thoroughly as a hypnotic can do. The subject is not distracted by sense-perceptions, while the actor cannot avoid being affected by them. Some actors, in order to play their parts as naturally as possible, call up imaginary objects by force of imagination, so as to place themselves amongst suitable surroundings.

Graphological investigations have been undertaken in several quarters in order to decide whether the handwriting of the hypnotized subject changes with the personality, and if the change bears any relation to the personality. Changes have been observed (Lombroso, Ferrari, Héricourt, Richet, Varinard, Mayeras). The expert Hoctès, however, thinks that the subject's handwriting is never altered to such a degree as not to be recognizable. I have never seen distinct changes of handwriting follow on change of personality; only in some cases when I placed the subjects in different periods of life has the handwriting altered. As children they wrote awkwardly and made mistakes in spelling; as old people they wrote shakily. The trials made with Krafft-Ebing's patient, who wrote different hands, corresponding to the different earlier periods of her life, are very interesting; unfortunately, the writing could not be compared with true writing of the subject at those periods. Sidis has published similar experiments. In one case, however, Krafft-Ebing was able to compare the handwriting of a hypnotic placed in an earlier period of life with the subject's actual handwriting at that period, and thinks he could trace a certain similarity between the two. Nuel's statement that in hypnosis the writing always differs from the subject's normal hand, and that consequently hypnotic signatures may always be distinguished from others, seems to me too general. He is probably right when he says that in many cases the writing of hypnotic subjects is irregular and spasmodic.

We have now studied the memory, which is a chief condition for the continuance of mental activity. This shows no abnormalities in superficial hypnosis, though such may be easily induced in deep hypnosis, at least by suggestion. But a certain adherence to rule in the chain of ideas, conditioned by the laws of association, exists in deep

hypnosis, as in the waking state. Certainly associations may assume the character of hallucinations when the state of consciousness is altered. I suggest to X. that he has a pack of cards in his hand. He believes it; and the mental picture of the cards arouses the idea that he is playing a game of "Skat," and that he is in a restaurant with his friends Y. and Z. The single suggestion of cards has sufficed to call up a whole scene by association of ideas. Certainly in this case the associations are merely mechanical—*i.e.*, no high degree of mental activity is concerned.

Nevertheless, the mental activity, thought, appears as soon as we deprive X. of the existing associations, and he exerts himself to create a new sequence of ideas. I tell X., while he thinks he is holding the cards, that he is in the train, and the chain of ideas connecting the cards and the restaurant is put an end to. In order to establish a connection between the cards and the train, the subject now declares he has bought the cards for a birthday present for a friend whom he is travelling to meet. The consciousness and will of hypnotics also express themselves in the case of indeterminate suggestions (*suggestions indéterminées*, as Beaunis calls them). In such suggestions no definite action is commanded, but the subjects are left to choose among a number. Here is a man with a violent bronchial catarrh. I suggest to him in hypnosis to do something or other which will benefit his health. He at once gets up, and goes to a box containing catechu for the purpose of taking some. Indeterminate suggestion can also be made to act post-hypnotically. I tell a hypnotic, who is sitting with me one evening in my room, to do some thoroughly foolish action after he wakes. He wakes and blows the lamp out.

The fact that the subject sometimes allows himself to be persuaded to do something, if a reason is given to him for it, shows even more plainly that the thinking process is not arrested in hypnosis. Nothing can induce X. to spill a glass of water in my room; but directly I tell him the room is on fire he does it at once so as to extinguish the fire.

It is important to know how the hypnotic subject sometimes makes a logical use of slight external impressions which anybody else would hardly think about. Much apparent "clairvoyance" is due to this. Many subjects are

helped also by the hyperæsthesia of their organs of sense, which enables them to recognize things which would be overlooked in the waking state.

Let us take a very common experiment, often made to prove the existence of animal magnetism. The magnetized subject knows whether he has been touched by his magnetizer or another person. It is astounding to observe the accuracy with which such subjects, when their eyes have been bandaged, can distinguish one person from another. Ochorowicz, who believes in animal magnetism on other grounds, gives a number of interesting examples of this. The hypnotic subject observes the smallest details—the differences in the strength of pressure, in the temperature of the hand, in the posture of the person touching him, in the sounds he makes with his shirt-cuffs; nothing is overlooked, and a logically exact conclusion is drawn. It may very well happen in such cases that the subject is not clearly conscious of drawing his conclusions from these details. This phenomenon is very common in normal life. How often it happens that at first sight one person draws a conclusion as to the character of another, and is at the same time unaware of the details from which he draws it. We often divine the meaning of a face without knowing how; we think that it is a clever or a stupid face; we recognize an expression of happiness or sadness at once, without realizing the details of our impression. Thought-transference, of which I shall speak later, may often be explained in this way; the subject reads the wish and thought of the experimenter even in a gesture, in the direction of his eyes, in the involuntary movement of his lips (Carpenter), particularly when he has had some training in this line.

The prophecies and predictions of somnambules and other such persons often depend upon the logical utilization of such insignificant impressions. A peculiar mental quickness is not always necessary, as is shown in the case of a man who was told by a soothsayer that he had lately suffered a loss in his family. This was true. The man was astonished till a friend drew his attention to the fact that he was wearing crape—a fact which gave the soothsayer a clue (Fonvielle).

In hypnosis, just as in the normal state, mental activity depends upon the attention paid by the subject; indeed, in the deep hypnoses the subject's attention plays a particular part, being easily diverted from the experimenter. In deep hypnosis the subject's attention is first directed to one point only—*i.e.*, to the experimenter, so that other objects hardly exist for him. When this phenomenon is clearly marked, we speak of *rapport;* or of isolated *rapport* when the subject is in hypnotic connection with only one individual. This is an important phenomenon of hypnosis. We saw in the fourth experiment (p. 35) that the subject only answered me, and apparently ignored the other persons present. Isolated *rapport* is a common phenomenon of deep

hypnosis. It can undoubtedly be induced by suggestion; whether it can only arise in this manner or may be brought about by other means, is to be questioned. Döllken only once saw a case of isolated *rapport* without the necessary suggestion being made. I have often seen cases of this kind in which I do not think I lent any assistance either by word or deed. It may of course be objected that in such cases auto-suggestion plays a part, because the hypnotic thinks of the experimenter when falling asleep, and so, by auto-suggestion, isolates himself from the other persons present. This is a possible explanation, though it appears to me doubtful whether the concept suggestion suffices to explain the phenomenon, because there may be associative processes at work which are not included in that concept. The old mesmerists were acquainted with isolated *rapport*. It struck them that the magnetized person appeared not only to hear, but also to feel the magnetizer. This form of *rapport* was to them a proof that the magnetizer exercised a physical influence on the person magnetized. I have, however, demonstrated in my large work on *Rapport in Hypnosis* that isolated *rapport* occurs as a psychic phenomenon without the aid of magnetic manipulations. Up to the present no one has made any serious attempt to refute my demonstration. The phenomena of magnetization are exactly the same as those observed when a subject is sent to sleep by suggestion. As suggestions are most easily made through the muscular sense and the hearing, isolated *rapport* is made most clearly evident by means of these senses. A subject, X., is hypnotized. I lift up his arm; it remains raised in suggested catalepsy. Another person, A., makes a similar attempt with the other arm, but without result; the arm always falls down loosely. A. now tries to bend the cataleptic arm, but is prevented by its rigid contracture, while I easily succeed. The command of the experimenter suffices to put other persons—A., for example—in *rapport* with the subject. The circumstances are analogous in verbal suggestion. The experimenter says when he has lifted the arm, "Now it bends, now it falls, now it is stretched out," and the effect at once follows. The commands of others are not obeyed if they have not been put in *rapport* with the subject by the experimenter. This shows the importance of *rapport* if a subject is to be in-

fluenced. On the other hand, I must point out that persons who are not in *rapport* with the hypnotic are sometimes only apparently ignored. It can be shown in various ways that sense-impressions coming from other persons are perceived, although they do not cross the threshold of consciousness. I have been able in some cases to demonstrate the existence of perception by means of automatic writing, to which I shall refer later; in others I was able to produce the necessary proof by persistently suggesting that the subject should do something which A., who was present, told him to—for example, put his hand to his forehead. Being a case of isolated *rapport*, the patient ignored A.'s command, but subsequently obeyed when I suggested that he should do so. Phenomena exactly like those of *rapport* in hypnosis have been observed in spontaneous somnambulism (Macario). Finally, I must mention that in superficial hypnosis also, in which others besides the experimenter are distinctly felt and heard by the hypnotic—a fact he readily admits—he can sometimes only be influenced by the experimenter. The ideas suggested by the latter alone lead to the realization of suggestion; attempts at verbal suggestion made by others are heard and may be repeated by the hypnotic if requested to do so, but they produce no effect.

As we have already seen, isolated *rapport* depends entirely on the attention paid by the subject to the experimenter, and just as the *rapport* may be transferred to another person by suggestion—*i.e.*, the subject's attention directed to that person, so we can force the hypnotic to concentrate his attention on any point we please. The increased mental activity which is occasionally observed in hypnosis is often referred to the fact that the subject's attention is directed to one point exclusively, from which nothing distracts it, and, as Hirschlaff points out, the same result may be induced by suggestion. Hirschlaff found by experiment that, with the same subject, the reaction-time is shorter in deep hypnosis than in the waking state, and thinks this explains the phenomenon mentioned above.

By reaction-time we mean the time that elapses between the moment of making a sense-impression and the moment when the impression manifests itself by some external sign (Wundt). It is known that a number of different processes take place in the consciousness during the time of reaction. I shall the less enter into them, that the researches

which have hitherto been made into the time of reaction during hypnosis have given contradictory results. Stanley Hall found the time of reaction considerably shortened in hypnosis. He found—

Before hypnosis	0.328 secs.
During hypnosis	0.193 ,,
Half an hour after hypnosis ..	0.348 ,,

The time of reaction during hypnosis is thus sensibly diminished here; but William James's experiments have not confirmed Stanley Hall's. He nearly always found an increase of time of reaction during hypnosis, sometimes to an important extent. He gives this as an average on one occasion :—

Before hypnosis	0.282 secs.
During hypnosis	0.546 ,,
After hypnosis	0.166 ,,

But as there are many contradictions in James's different experiments, no definite conclusion can be drawn. He himself believes that the contradictions are to be ascribed to the fact that so many different states are included in hypnosis, as Braid already knew, and that we should be careful not to generalize from single observations. Beaunis, who has also made these experiments, is equally cautious. The only conclusion he draws from his partially contradictory results is that the time of reaction in hypnosis may be shortened by suggestion. Similar results are given by the experiments of Henika, Worotynski, and Bechterew. These observers also found a lengthening of reaction-time in hypnosis as compared with the waking condition. Marie and Azoulay have measured the time of reaction for suggested sense-delusions in hypnosis; they found it longer than when the object was a real one. Perhaps this is because the points of recognition (*points de repère*, p. 100) have to arouse the suggested picture before it can be perceived. The time of reaction,[1] according to my experience, may last so long—to return to the experiment with the photographs on page 100—that we might even speak of a search for the picture. The subject looks till he finds the points of recognition, which at once recall the suggested picture to his memory. This search may be united with a dim consciousness that the whole thing is a delusion, but there can be no question of simulation.

Other investigators also attribute increased activity in hypnosis to increased concentration of the attention. Brémaud is quite right in thinking that the increased power of vision and of hearing in hypnosis are to be ascribed to an increase of attention. Quite a young lad, a relation of his, solved problems in his sleep which he could not solve when

[1] I am doubtful if in this case we ought to talk of time of reaction, as this expression is generally used only with regard to perceptions of real objects.

awake, a phenomenon which Brémaud ascribes entirely to greater attention, and not to quickened intelligence (Crocq). Concentration of the attention also explains the increased functional activity in hypnosis observed by other investigators. Ach, for instance, investigated the question of mental activity. To keep his subjects' minds continuously employed in one direction he set them the task of adding up column after column of simple digits, and found that under this abnormal and intensified concentration of consciousness the work done was one-fifth more than would have been accomplished under normal conditions. Bechterew found that the time occupied in counting simple numbers, or in associating ideas, was shortened when the subject was requested to carry out the processes in question with greater rapidity.

In hypnosis the feelings also are subject to the influence of suggestion. Desire and dislike can be very easily suggested, particularly in deep hypnosis. Similarly, the whole mental tone, which is so closely connected with the feelings and with common sensation, is readily amenable to suggestion. But as to whether the feelings as a whole exhibit abnormalities in hypnosis without suggestion being brought into play opinions differ, just as they do on so many other questions in which suggestion is concerned. We occasionally find the view promulgated that the hypnotic is extraordinarily grave. I cannot admit this. Many people, on the contrary, seem particularly comfortable in hypnosis, an observation which Richet also has made. Of course we can hardly exclude the possibility that this state of feeling may have been brought about by auto-suggestion. In some cases the emotions are easily controlled. It is consequently very easy to induce either sadness or cheerfulness in deep hypnosis, and these emotional states often alternate very quickly. It is even easier to call up such emotions as love and hate, anxiety, anger, and fear by means of suggestion; for example, a hypnotic can easily be thrown into a passion by suggesting the presence of a particularly offensive personal enemy. Sometimes a simple command suffices to call up a particular emotion; fear, for example, may be induced in this way, the subject putting himself in the fear-inspiring situation by auto-suggestion. At all events, in many cases a very lively expression of emotion can be extremely easily induced by suggestion, either directly or indirectly. Under such circum-

stances, the facial expression, attitude, and posture of the hypnotic clearly show what is passing within him.

The impulses also may, to a certain extent, be controlled by suggestion. Observations which have been made in the field of sexual perversion show the great power of suggestion in this respect. I shall return to this question in Chapter VIII., in which we shall see that under certain conditions ordinary hetero-sexual love can be influenced by suggestion.

Of course verbal suggestion is not the only means of implanting those ideas which influence the feelings, emotions, and impulses. We have already seen that the sense of sight and the muscular sense can serve for the purpose of suggesting ideas, and this is also the case when the feelings, etc., are to be influenced. The suggestions made through the muscular sense, observed by Braid and Charcot, are founded on this (*suggestions d'attitude*, or *suggestions par attitude*). If a subject's arms are put into the attitude of prayer, the face soon wears an expression of religious devotion. The following is a favourite experiment of Charcot:—If the subject's hand is raised to his mouth as if he were throwing a kiss, he smiles. If the fist is closed and raised in a threatening attitude, he looks angry. Charcot and Richer maintain that the experiment may be reversed. If, for example, the muscles used in laughter are stimulated by faradization, so that a laughing expression is induced, the movement of throwing a kiss with the hand follows. If the muscles which produce an angry expression are stimulated in the same way, then, according to Charcot, the arm is raised in anger. It is very probable that suggestions of this kind are affairs of hypnotic training. From this it follows that one movement may cause another by suggestion, but not that the movement necessarily calls up the emotion corresponding to it. It appears, however, that in a series of hypnotic cases one movement not only caused another by suggestion, but called up the corresponding emotion as well.

I may here mention that movements may be used with advantage to help the induction of sense-delusions. I give an imaginary glass of a very bitter liqueur to a subject, X. He says that there is no glass of liqueur, and that he has nothing in his hand. Without noticing this objection, I cause him to raise his hand to his mouth by suggestion, and order him to drink the liqueur. He obeys slowly and hesitatingly; but when his hand reaches his mouth he makes drinking movements, and the expression of his face shows that he has a disagreeable taste in his mouth. When

I ask him what is the matter, he answers that he has an unpleasant taste, as if he had just drunk something bitter. Here at first the true situation was recognized by the subject; the suggestion took effect during his compulsory movement. In another case I make a subject move his fingers as if he were playing the piano, and suggest at the same time that he is playing. He does not believe it, but continues the movement. While he does this the idea of piano-playing really arises by degrees in his mind, and at last he makes the movements in the firm belief that he is playing the piano. I have often observed that it was easier to induce sense-delusion by accompanying movements than by verbal suggestion alone, and I would recommend this as a means of deepening the hypnosis in suitable cases. It is often impossible to define the exact moment when sense-delusion supervenes; it is impossible, therefore, to decide whether the delusion was really in existence before, or whether it was called up by the compulsory movements.

I take this opportunity of pointing out that particular movements have been observed to excite mental process in waking life as well as in hypnosis, a fact to which Dugald Stewart, Gratiolet, and others called attention long ago. I may mention an example from ordinary life: an attitude expressing anger is assumed; a real feeling of anger very often follows, especially if words are also used. The expression, "To talk one's-self into a passion," is a proof of this.

We thus see that a particular movement exerts an influence on the emotions and ideas in waking life. This can, of course, be considered merely an instance of auto-suggestion. Still, it is doubtful whether the term suggestion in its present-day sense is permissible in this case; and we are as little justified in considering "colour-hearing" an auto-suggestive process, because the phenomenon may be the result of a number of processes which have no direct connection with suggestion. The chief point is this: does any particular muscular action in hypnosis call up emotions without any previous special training which it fails to do in the waking state? Let us take one of Charcot's examples: if I make the necessary suggestion, and also stimulate the muscles used in laughter by faradization, the subject makes the movement of throwing a kiss with the hand. According to the views held by Charcot's school, the movement of throwing a kiss is not brought about in this way; it is rather the result of an associative process which has nothing to do with any external suggestion. This appears to me more than problematical. Bouchard and Pitres think they have discovered that contraction of the muscles used in laughter calls up jovial feelings or hallucinations—*i.e.*, such as are intimately connected with facial expression. Moreover, it is well known that a normal person can be made to laugh by

forcing his mouth to assume a laughing expression. Any one can easily demonstrate this on himself—hold the cheeks firmly with the fingers, pull up the corners of the mouth, and a laugh follows. Whether cheerful feelings result from this procedure is another question. Any way, the authors we have mentioned assume that in hypnosis cheerful hallucinations ensue from contraction of the muscles used in laughter. In his work *Le Rire*, etc., Raulin records similar experiments made by various investigators; he thinks that in the cases mentioned external suggestion was apparently excluded. Yet the examples he adduces for the purpose of proving that cheerfulness, for example, can be induced by the assumption of a particular posture, are not convincing; at least there is no proof of the absence of suggestion and training in the experiments to which he refers.

We have something similar to the above in the *zones idéogènes* of Pitres. I have already mentioned that according to Pitres stimulation of certain portions of the body induces hypnosis, of other parts terminates it. Pitres also states that stimulation of particular portions of the body which he includes in his *zones idéogènes* calls up definite mental processes, particularly emotions. In one case friction of the temporal region is said to have caused cheerfulness; but this form of stimulation is not invariably effective unless the subject is hypnotized. There is only one case on record in which stimulation of the zone in question induced laughter when the subject was awake. Pitres has also described *zones idéogènes* for ecstacy and other mental states, but there is no necessity for me to discuss these questions, because we have no guarantee that external suggestion was really excluded in any instance.

I will now mention a particular method of influencing subjects which has recently attracted a certain amount of attention. It acts through the ear like ordinary verbal suggestion, but music, and not speech, is the agent. In 1894, Warthin published experiments dealing with the question. His subjects were five men and two women, all of whom were stated to be in good health. Four were physicians and tutors, the rest students, and none of them had ever been the subject of a hypnotic experiment before. They were all more or less fond of music, although it did not excite any of them particularly or cause any noticeable physiological action when they were awake. All this is said to have been changed in hypnosis,

which leads me to remark that the whole account reads like a fairy tale. The "Ride of the Valkyries" was played upon the piano, and although only one of the subjects knew of the connection between the music and the wild ride, the idea of riding is said to have been called up in each of them, the concrete idea being derived from some recent experience. The only one who understood the music even imagined himself one of the riders. The magic fire, also, caused all of them to experience a sensation of fire and flames. But when the imaginary ride was at its wildest, and the music changed from B major to B minor, the effect was even more intense. All were thrown into a terrible state of collapse; the pulse fell from 120 to 40 beats in the minute, was irregular, soft, and small, and the respiration was retarded and distressful. The same experiment was often successfully repeated with other subjects.

Some little time ago a "dream-dancer," Frau Magdeleine, attracted much attention in Germany. Others soon followed in her wake. When hypnotized, and under the influence of specially selected music, she was able to express any emotion to perfection, by facial play and pantomime. Her talent for dancing was also a factor. The mere mention of an emotion, or any other form of impression—for example, the recital of a poem—called up the emotion in question. The commotion which this "sleep-dancer" caused was brought about by two things: (1) her talent for acting; (2) her incapacity to display the same except when hypnotized. It was also stated that she had never received any special training for her performances. I shall enter into a criticism of the whole question later on in the section dealing with Art in relation to Hypnosis.

From all the phenomena hitherto discussed it must have been gathered that there can be no question of loss of consciousness in hypnosis. Of course, I mean loss of consciousness as it is understood in psychology, and not in the penal code. In the latter, according to Schwartzer, Casper, and Liman, it means about the same as abnormalities of consciousness; but according to the view more generally held (Krafft-Ebing and others), it implies abnormalities of self-consciousness. Of course we can only talk of loss of consciousness from a physiological point of view when no kind of psychical process takes place, as is the case in a deep faint, or

coma, or the death-agony. We have seen that the subject in hypnosis remembers the events of earlier hypnoses. Consequently impressions were received into the consciousness in these earlier hypnoses. We cannot, therefore, talk of loss of consciousness because loss of memory exists after the awakening (Forel), apart from the fact that post-hypnotic suggestion in hypnosis will prevent the loss of memory. This temporary loss of memory is often erroneously considered a proof of loss of consciousness. It is just as erroneous to assume that the fact that a hypnotic sometimes does not obviously respond to external influence is a proof of unconsciousness. We have only to consider the case in which the hypnotic is in *rapport* with one person only, but at the same time is quite unconscious as far as other persons are concerned. The fact that a hypnotic responds promptly to the suggestions of the hypnotizer only proves how intensely the attention of the former is directed to the latter.

Even the states mentioned on page 78, in which no response could be obtained to questions and demands, do not prove loss of consciousness; for (1) post-hypnotic suggestions could be made, and were effectual, which proves that there was consciousness; (2) these subjects woke up directly they were told to do so (Bernheim), which also shows that they were conscious. The forms of lethargy also have to be considered—the lethargy which Charcot describes as such, and the lethargy termed hysterical, of which I have already spoken (p. 48). As concerns the latter, it must be absolutely distinguished from hypnosis; it has nothing to do with the phenomena of hypnosis. With Charcot's lethargy the case stands thus: apart from the numerous cases of lethargy here described, and which even the pupils of Charcot admit are associated with movements caused by command, there remain very few cases worthy of consideration. I doubt, however, whether there is the loss of consciousness in these cases which Charcot describes. The cases which I saw in Paris convinced me of the contrary. The quickness with which these lethargic subjects fell into catalepsy when Charcot touched their eyelids, showed that these apparently unconscious persons had been attentively waiting for the moment in which they were expected to become cataleptic. Consequently the loss of consciousness seems to me more than questionable. This point is of great importance, because Charcot's pupils maintain that the phenomena of the

muscles and nerves in the lethargic state are not induced by suggestion.

Of course, we shall occasionally come across cases in which any evidences of consciousness are too obscure to be readily demonstrated, but that does not justify the assumption of loss of consciousness. At all events, loss of consciousness has nothing whatever to do with hypnosis.

But though we cannot speak of a loss of consciousness in hypnosis, we must, however, often suppose an abnormal state of consciousness; for if some one believes he sees things that are not present, or fails to see things that are present, he is certainly in an abnormal state of consciousness. If a man forty years old believes he is ten years old, or somebody else, his consciousness is certainly abnormal. We find such phenomena continually among the second group of hypnotic subjects, and we must consequently here suppose a material abnormality of consciousness.

But even in such cases we occasionally find a certain degree of consciousness. For instance, many hypnotics have a distinct feeling that they are asleep, or in an altered state (Richet, Pierre Janet)—a phenomenon of ordinary sleep, for we are occasionally conscious in dreams that we are asleep and dreaming. Many hypnotics of the second group have this consciousness of being asleep, and when they are asked if they are asleep or awake, they give the right answer. When, as sometimes happens, the awakening is incomplete, they also rightly say that they are not quite awake. Many people have a feeling of deep hypnosis if they are incapable of resisting certain suggested ideas. I say to X., "You cannot lift your arm!" "Yes I can," he answers, and experiment shows that he is right. But the contrary sometimes happens; the subject often knows exactly the minute when his power to resist is at an end, when he must obey. X. announces after a time that he is at this point: "Now the hypnosis is deep enough," he says. I say to a person thirty years old, whom I have often hypnotized, "Now you are a little child." The subject replies, "It is not enough yet; you must wait a little." After a time, when I ask, he says that he is now at the right point. In other cases a remarkable amount of judgment is displayed in hypnosis. Many hypnotics, even those of the second group who are open to sense-delusions, say that they know quite well how suggestion works and that the influence exercised on

them is a purely mental one. One in whom all kinds of sense-delusions can be induced, said to me, "I know that you do not exercise any extraordinary magnetic faculty; it is my own imagination which deprives me of my will and obliges me to obey you; but I cannot help it."

Even delusions of the senses brought about by suggestion are sometimes corrected purely by a reasoning process. A subject declines to believe that there is a wolf in my room; or, rather, he explains that he sees an image of a wolf plainly enough, and could point out the exact spot. But he knows quite well that it must be a delusion, as I should certainly not allow a wolf to come into my consulting-room. Macnish says that people can guard themselves against bad dreams and control them in sleep by a process of thought. This control often causes delusions of the senses to disappear more quickly; in other cases they may persist, in spite of the correction made by the reason. If the correction is complete, the delusions will have no results; they will not influence the actions following.

All the phenomena which I have just described may be observed both in hypnotic and post-hypnotic suggestion. I ask a man before I hypnotize him to tell me of something which, in his opinion, would never be found in my room. He says he would never believe there was an owl in my room. In hypnosis I make him the post-hypnotic suggestion that there will be an owl in my room. He wakes and says he sees the owl plainly; it is chained by the foot. Although he knows and says that the owl is only a hallucination, it is so real to him that he hesitates to put his finger on the spot where he imagines it to be.

It is not always easy to recognize the mental state of a hypnotic subject in suggested sense-delusions. If in many cases all thought and action is dependent on the delusion, in other cases the effects are less complete. I even believe that most subjects retain a dim consciousness that they are in a fictitious, and not a real, situation. For example, I suggest to a patient that he is in battle and must fight. An imaginary struggle begins at once and he hits the air. When I suggest that a cloth on the table is an enemy, he strikes at it. I suggest that one of the persons present is an enemy, but in continuing the fight the hypnotic takes care not to strike this person. Naturally this looks like simulation, and an inex-

perienced person would take such to be the case; but it was quite possibly a real typical hypnosis, in which, in spite of the sense-delusions, there was a dim consciousness of the true situation which influenced the actions of the hypnotic. This dim consciousness of his real surroundings prevented the subject from striking a human being, but left him free to hit a cloth. This behaviour of the hypnotic reminds one forcibly of automatism. As we when walking in the street and reading a newspaper, automatically avoid knocking against passers-by, so the hypnotic avoids hitting another person, although he is only dimly, or not at all, aware of his existence.

It is the same with negative hallucinations. Binet and Féré have said about this that the object must be recognized in order not to be perceived. At all events, in negative hallucinations the subject has a dim consciousness of the true situation. The authors mentioned above made a series of experiments in support of their assertion, which I have been able to repeat with success.

If ten sheets of white paper are taken and one of them marked on the back, the subject can be made to believe that he only sees nine sheets (negative visual hallucination), even when the sheet whose invisibility was suggested is among them. If he is asked to give up the nine sheets, he picks out the nine unmarked ones and leaves the other, guided by the mark. Consequently he is able to distinguish it from the others, although he is unconscious of making the distinction.

A series of experiments made by Cory are even better. I was able to repeat them in part, and came to the same results. I took a sheet of paper and drew a rather irregular line on it. I then suggested to the subject, X., that the paper was blank. X. agreed that he saw nothing. I then drew fifteen straight lines on the paper and asked X. what he saw. He said, "Fifteen lines." I recommenced the experiment, but made the first line straight instead of crooked, and then suggested its invisibility; upon which I added twenty more lines exactly like it, and made X. count them. "There are twenty-one," he said. Therefore the line suggested as absent was only invisible to X. when he could distinguish it from the others. The following experiment resembles this:—I took a match and marked its end with a spot of ink. I then suggested that the match was invisible. I took twenty-nine other matches and put the whole thirty on the table in such a manner that X.

could see the ink spot. To my question, X. replied that there were only twenty-nine matches on the table. I then, while X.'s eyes were turned away, moved the marked match so that X. could not see the ink spot. He looked at the matches and said there were thirty of them on the table. Thus the marked match was only invisible so long as X. could distinguish it from the others.

From these and other such experiments it may be concluded that the subject recognizes the object of a negative hallucination, even though there is no perception of it. The automatic writing, of which I shall speak farther on, also demonstrates this (Pierre Janet), as I can prove by numerous experiments I have made. The results of the suggested negative hallucination depend upon the influence exerted by the real object, in spite of the opposition brought to bear by suggestion. I suggest to a subject, X., that a table which is between him and the door is no longer there. X. goes carefully to the door, but avoids hitting against the table. I suggest that the electrode, which is armed with the very painful faradic brush, is invisible. After closing the current I touch the subject with the brush, and he shows great pain. When I ask X. what has hurt him, he says he does not know, for my hand is empty; but at the same time he takes care not to touch the place where the brush is lying, or does it hesitatingly, and with evident signs of fear. I tell another that I am going out of the room; he apparently neither sees nor hears me. Yet every suggestion that I now make to him is executed. I order him to take the cushion from the sofa and throw it on the floor; the order is obeyed, though after some hesitation. To another subject, who also believes that I am out of the room, I suggest sense-delusions—the presence of a dog, etc. All the suggestions succeed, evidently because the subject hears what I say, though he believes me absent. I tell another that he is deaf, upon which he ceases to do what I tell him. But after I have several times repeated "Now you can hear again," he obeys every command. We see in these cases, which I could multiply, that the organs of sense act normally, that a certain effect is produced, but that the impressions are not received into clear consciousness. I naturally do not maintain that this is the case in all positive or negative hallucinations; on the contrary, in some the delusion is complete. This depends on character, on training, and to a great extent on the manner in which the suggestion is made.

I wished merely to describe the more incomplete and by far the most common cases, because they are often mistaken for simulation.

The cases detailed above provide us with noteworthy instances of the persistence of consciousness, and to a certain extent of self-consciousness as well, in hypnosis. It is obvious that where self-consciousness is so strongly expressed the activity of the hypnotic's will cannot be entirely suppressed. Let us now inquire in what other ways the activity of a hypnotic's will is displayed, bearing in mind that we have to deal with an external and an internal activity. The former governs the movements, now starting, now inhibiting them; similarly, the latter rules the ideas, feelings, and emotions, which are, within certain limits, influenced by the will, the latter sometimes arousing and sometimes suppressing them.

When we investigate the activity of the will in hypnosis, care must be taken to distinguish between the two groups which I described on page 59. In the first group the external activity alone is diminished, and the consciousness presents no abnormality. The subject knows exactly where he is; he knows what is being done with him; he makes the movements commanded because he cannot help it; his limbs are paralyzed at command. Catalepsy may be induced by suggestion, and yet the subject will be fully aware of all that goes on. Some of Hack Tuke's subjects—for example, North, a physiologist in London—have given very interesting information with regard to the interference of the will during the experiments, which makes them unable to resist, though otherwise fully conscious.

It is very different in deep hypnosis. Here also the external activity of the will is limited, as we have already seen. Moreover, the experimenter can very considerably influence the internal activity of the will by suggestion. But it is exactly the quickness with which the subject can be transferred from one situation to another, and with which he accepts the suggested idea, which demonstrates that he is only the plaything of the experimenter. Just as the ideas of dreams transport us from one situation to another, so do suggested ideas. Pleasure is changed into pain in a moment; the moods change as quickly as they usually only do in children and the sick. The subject now thinks he is in my room; the next

moment he believes he is in bed; directly after he is swimming; now he believes he is ninety years old; and in the next second he is back in his tenth year. Now he is Napoleon I., then a carpenter, then a dog, etc. This change of ideas often takes place in a moment; the corresponding ideas arise at once through association. The quick change of ideas, feelings, emotions, and of the whole mood is so common that I was astonished to read (in Malten) that a legal specialist in Vienna, Ferroni, has been led by it to conclude that the thing is simulation.

In spite of this, it would be a great mistake to think of the subject as an automaton without a will. On the contrary, the will of the subject expresses itself in manifold ways, as may be seen from the expressions of self-consciousness noted above, and each of the two forms of volition has its own expressions of will.

Often the decreased power of will shows itself merely in slow and lingering movements. In these cases any movements can be made, but the subject takes longer to perform them than he does in normal circumstances. An inexperienced person has a tendency to fail to recognize hypnosis in such cases; he generally thinks the experimenter mistaken in calling them abnormal. Further, it has been already said that in many persons only certain muscles can be controlled by suggestion (page 71). In other cases it is necessary to repeat the suggestion often before the result is obtained. For example, a subject can raise his arm in spite of the command of the hypnotizer, but repetition of the command ends by making the movement impossible. These are examples of the way in which resistance to suggestion expresses itself.

Expressions of the will which spring from the individual character of the patient are of great psychological interest. The more an action is repulsive, the stronger is his resistance (Forel, Delbœuf, Jong). Habit and education play a large part here; it is generally very difficult to successfully suggest anything that is opposed to the confirmed habits of the subject. For instance, suggestions are made with success to a devout Catholic, but directly the suggestion conflicts with his creed it will not be accepted. The surroundings play a part also. A subject will frequently decline a suggestion that will make him appear ridiculous. A lady whom I easily caused to make suggested movements could not be induced to put out her

tongue at the spectators. In another such case I succeeded, but only after repeated suggestions. The manner of making the suggestion has an influence. In some cases it must be repeated often before it succeeds; others interpret the repetition of the suggestion as a sign of their own ability to resist. Thus it is necessary to take character accurately into account. It is often easier to induce some action by suggesting each separate movement than by suggesting the whole action at once. For example, if the subject is to fetch a book from the table the movements may be suggested in turn; first the lifting, then the steps, etc. (Bleuler).

Resistance is sometimes expressed in other ways. Beaunis has observed that an attack of hysteria is sometimes the answer to a repugnant suggestion. I myself have observed that subjects have asked to be awakened when a suggestion displeased them. The fact that subjects are not nearly so inclined to discuss their private affairs as is sometimes stated is another proof of the activity of the will. Döllken found that whole tissues of lies were as readily invented in hypnosis as in the waking state ; andLœwenfeld observed that hypnotics spoke the truth—hesitatingly, perhaps,—about matters which they would have gladly kept to themselves when awake. But he also admits that subjects are prone to make false statements when it is to their interest to withhold the truth. The statement, so often made, that a hypnotic is always ready to tell the truth unreservedly, is not quite right as far as my experience goes, though many subjects are readier to make admissions in deep hypnosis than when awake.

We shall see later on that the assumption that the truth can be extracted from a hypnotic has induced some people to advocate the use of hypnosis for forensic purposes. I will only mention here that apart from legal cases it was assumed that the truth could be ascertained by hypnosis, especially in the days when animal magnetism flourished. About that time a certain Rahel Herz for years deceived her medical attendant by allowing him to excise hundreds of needles from various parts of her body when she was in a state of hysterical analgesia. Like many stigmatics she apparently required no food. Brandis, who was at that time physician to the court, determined to clear the matter up, and visited Herz at the request of the queen. Taking off his coat, he informed Herz that he intended to magnetize her, which would compel her to admit everything. She refused, and Brandis left the room declaring that her refusal told him all he wanted to know (Rieks). According to Malfatti, an Italian army-surgeon, Franchini, has recently proposed the hypnotization of recruits suspected of simulating epilepsy. Franchini thinks that the truth could be ascertained in this way.

In other cases the resistance can only be overcome by suggesting a false premise, as I mentioned on page 133. The order will then be more easily obeyed. I will choose an example from Liégeois. A subject was to be induced to steal a watch. He refused. But when it was represented to him that the watch was his own, and that he would only be taking it back again, he immediately obeyed the command. Or the subject may be told that the laws are altered, that stealing is no longer punishable, etc.

The explanations I have already given do not exclusively concern movements and actions, but delusions of the senses and other suggestions as well. I have often seen unpleasant and improbable suggestions resisted when contrary ones succeeded.

I once told a subject who was forty years old, "You are now thirteen years old." He answered, "No, I am forty-one." But directly after he accepted the suggestion that he was twelve or fourteen years old. However, I failed to make him believe he was thirteen years old; he refused the suggestion. He was superstitious and dreaded the number thirteen. His notion that thirteen was an unlucky number accounted for his resistance; on that account he would not be thirteen years old.

The experimenter may unconsciously increase the resistance. Fontan and Ségard rightly maintain that many hypnosis may be continued or put an end to by the tone in which the operator speaks. If we say to a subject, "Try to open your eyes; they are fast closed, you cannot possibly open them," the kind of emphasis may alter the effect. If the emphasis is laid upon "Try to open your eyes," the last part of the suggestion is more easily overcome, and vice versâ. It is just these cases which show clearly the gradual transitions from the lightest stages to the deepest. I raise a man's arm; the arm remains raised so long as I say nothing. Directly I tell him that if he tries to drop his arm he will not succeed, he does it nevertheless, though at first with some stiffness. This alone shows that the state was not quite a normal one. In this case, as in many others, the subject passively allows his arm to remain as it was fixed; he makes no effort of will either for or against. But the moment I induce him by verbal suggestion to make an effort of will, he does so, and shows that he can exert the will against my orders, even though the hesitating movement plainly shows that he was somewhat influenced. It is the same thing with continued movements, which are sometimes made

passively without an act of the will, and sometimes cannot be inhibited by the strongest effort of will, as I have explained above (page 77).

Although the above examples show that there is no complete loss of will in hypnosis, yet in all of them the will was set in action by some external impulse. Let us ask whether spontaneity, an independent activity of thought and will, the presence or absence of which was utilized by Durand de Gros in his classification of somnambulists, may not exist in hypnosis, apart from external impulse. This question must be answered in the affirmative, so far as the first group of hypnosis is concerned. But the hypnotic often shows independent activity of the will in deep hypnosis, hallucinations even arising without external suggestion. But the question is complicated by the fact that we are not always able to exclude external stimuli. For example, without any suggestion from me, a hypnotic suddenly jumps up and says that he has seen and heard a mad dog. The cause of this is the unintentional creaking of the boots of one of the people present. I had not observed the creaking, but as often as it was repeated the same result occurred. The subject misinterpreted an impression of hearing, which aroused a certain chain of thought in him. I have often observed such phenomena in impressionable and lively persons.

But I have found spontaneous hallucinations in the deepest hypnoses, which I was unable to refer to any stimulus of the senses. In particular, any events which had much occupied the subject in the waking state, continued to affect him in hypnosis. One of them, for example, related anecdotes which he had recently heard. While his mind was full of them no experiment could be made with him; he was as uncomfortable as a diner-out who only feels at ease when he has got rid of his whole stock of stories. In this and other such cases there must have been independent mental activity; at least I could never discover any external stimulus. Of course I cannot mathematically prove that these spontaneous actions did not arise from some external impulse; for the external impulse might have been some faint sound which I failed to hear, and even the slightest friction of the skin by the clothes may act as a stimulus and induce apparently independent actions in the subject. I do not believe this, but have rather gained the impression that hypnotic subjects in the deep stage

often have independent currents of thought. Brügelmann has published the case of an otherwise very chaste woman who became filled with sexual ideas when hypnotized. Similar cases have been reported by other authors, and Vogt specially points out that sexual feeling often occurs spontaneously in hypnosis. I take this opportunity of mentioning that dentists are well aware that lady-patients are often convinced that they have been assaulted when anæsthetized, although there were no possible grounds for the accusation. It is quite certain that some persons are liable to such auto-suggestions when hypnotized. In recent works dealing with pathological hypnosis such cases are put down to hysteria. Hirschlaff, for example, does so.

Hirschlaff distinguishes several kinds of hypnosis entirely among the deeper grades—*i.e.*, the second group in Max Dessoir's classification.

(1.) A form in which the subject has a power of discrimination. Here auto-suggestion outweighs external suggestion, but the auto-suggestion is not, as otherwise often occurs, a result of external suggestion; so far from having any direct connection with the experimenter's intentions, it is opposed to them. In such cases the hypnotic, for example, refuses to carry out experiments with which he is already acquainted and which he has prepared himself to resist by pre-hypnotic auto-suggestion. For instance, when told that his arm will become quite stiff, a subject will reply jeeringly, "No it won't; I am not going to make any experiments." Or a hypnotic is given a piece of paper and told that it is a tablet of chocolate; but he replies with a laugh, "It is a piece of paper. I knew quite well before that you were going to repeat this experiment, but made a firm resolution not to be caught again."

(2.) Abnormal hypnotic sleep. This is characterized by the hypnosigenic means and methods employed producing a normal or abnormal state of sleep instead of the hypnosis which was expected. The sleep may set in with loud snoring. Such persons cannot be influenced by suggestion, and wake either spontaneously with a terrified start, or when spoken to gently. In another group of cases the awakening is not so easy because the subjects are in a deep, unconscious sleep exactly like the pathological sleep which is also observed to occur spontaneously in hysteria. Awakening is difficult, as even strong stimuli produce no impression.

(3). The hystero-hypnoid state, as Hirschlaff terms certain conditions, basing his contention on Freud and Breuer, though he uses the term in a somewhat different sense to those authors. In this case instead of normal hypnosis the hypnosigenic measures produce more or less severe hysterical conditions, palpitation of the heart, hiccoughs, convulsive screaming and crying, attacks of hysteria of the severest nature, cataleptic and lethargic states accompanied by loss of memory. Occasionally these phenomena only appear as a complication of hypnosis.

(4.) Spontaneous somnambulism. Here the hypnosis is apparently normal at first; then there is a more or less sudden, spontaneous, but circumscribed outburst of excitement, generally of an erotic cast, in which

the subject often refers to some remote event that had at one time occupied his mind and caused him great mental excitement.

According to Hirschlaff, all these abnormal states are only met with in hystericals. The chief changes which differentiate these from normal deep hypnosis are, (*a*) decrease or entire cessation of suggestibility, (*b*) the spontaneous appearance of certain phenomena, (*c*) increased difficulty in awakening the subject, and (*d*) post-hypnotic *malaise*, an almost invariable result.

I myself believe that some of the states which Hirschlaff ascribes to abnormal hypnosis have nothing whatever to do with hypnosis, even if they do occur in hypnotic experiments. For a person who is being hypnotized to have an attack of hysteria (Hirschlaff's third group), or to fall into a lethargic state, or for another person, who is also being hypnotized, to fall asleep and snore but wake up at the slightest noise (Hirschlaff's second group), is not hypnosis, and we are therefore not justified in considering such states abnormal hypnoses. But we certainly might look upon those cases in which the hypnosis is complicated by hysteria as abnormal hypnoses: though an attack of hysteria alone must not under any circumstances be considered a case of hypnosis. We might also agree to include among the abnormal hypnoses those cases in which there is pronounced auto-suggestibility, the subject either acting in direct opposition to the hypnotist's suggestions (Hirschlaff's first group), or else only allowing his own auto-suggestion to be effective (Hirschlaff's fourth group). I have described a case of this nature on page 36. Such a person will jump up suddenly without a word having been said to him, merely because he is possessed with the hallucination that a tiger is about to spring upon him. When this phenomenon is more fully developed we are faced by a condition which is hardly distinguishable from spontaneous somnambulism.

I think it is doubtful whether the cases which Hirschlaff describes in the first group only occur in hystericals; but I should certainly contest the assertion that the auto-somnambulic states mentioned in the fourth group only occur in cases of hysteria. The form which hypnotic phenomena may take depends very much on the temperament of the subject. A very lively temperament often leads to auto-somnambulism without there being any reason to consider the patient hysterical.

We must certainly agree with Hirschlaff that pronounced auto-suggestibility during hypnosis is far more frequent among hysterical subjects than others. But it does not necessarily

follow that it is only met with in hysteria. We must take into consideration that external suggestion is limited by auto-suggestion in other cases as well. Nearly every hypnotic's susceptibility to suggestion is, to a certain extent, under his own control. A highly moral man rejects a criminal suggestion which one whose ethics are of a lower order would accept without hesitation. Similarly, evidences of erotic feelings during hypnosis are more often observed in subjects whose sexual desires are easily aroused, than in others. Such persons disclose their feelings more readily when hypnotized than when awake, because the restraints exercised in the latter state disappear in the former. But I do not consider it proved that we are therefore justified in assuming hysteria in all such cases. Even the phenomena which Hirschlaff includes in the hystero-hypnoid category, in which hypnosis is complicated by hysterical phenomena (trembling, palpitation, etc.), are not necessarily of hysterical origin; to describe them as such would be giving far too wide a scope to the concept hysteria. Sufferers from ordinary neurasthenia exhibit similar phenomena when being hypnotized; this is accounted for by the excitement which the act of hypnotizing sets up in such persons.

In dealing with auto-suggestion we must also bear in mind that there are certain variations in susceptibility to the experimenter's suggestions. One declares at one moment that his name is Moll, and does what I command him; directly after he is himself again, without any certain or apparent cause. Like many other subjects, he says afterwards that he perceives two opposing wills in himself, and that sometimes one, and sometimes the other, conquers.

I have been careful to distinguish between auto-suggestion proper and those forms which are really external suggestions. This is a distinction upon which Hirschlaff has rightly insisted. Many cases of external suggestion *seem* to be instances of auto-suggestion, a phenomenon which plays a great part in training, to which I shall now direct my remarks. The slightest sign suffices to make a subject repeat, later on, any action which has once been induced in hypnosis by means of external suggestion—*i.e.*, he responds just as he did to a definite suggested idea. When a hypnotic has been trained it is hardly necessary for the experimenter to state what he wishes

to be done—a gesture will suffice. For example, let a man's arm be paralyzed by verbal suggestion, then, later on, the experimenter will only have to give the slightest sign and the paralysis will reappear without being specially suggested. It may happen that the experimenter, either by his voice or by some slight movement, unintentionally directs the subjects to exhibit certain phenomena which could only be primarily induced by definite verbal suggestion. In this we have one of the chief sources of error, because the subject is inclined to obey the experimenter's intentions, and thus unintentionally misleads him. The subject is also greatly influenced by his surroundings, and by watching other subjects (Bertrand). Imitation is also of great importance here. I hypnotize X., and suggest that he cannot speak, at the same time inadvertently touching his left shoulder with my right hand. Y., in hypnosis, sees this, and every time I touch his left shoulder with my right hand he, too, is unable to speak. Y. believes this is the signal for loss of speech. In this case I gave the signal (touching the shoulder) unintentionally. We often give the signal unintentionally, but easily overlook the fact that we are at the same time suggesting something, and this leads to the phenomenon produced being erroneously attributed to the signal instead of to the idea suggested. Consequently at each subsequent experiment the hypnotist is, without suspecting it, educating the subject to respond more and more readily to a given signal—*i.e.*, there is unintentional training, if I may be allowed the expression, and, moreover, the particular symptom induced by the training becomes more and more pronounced.

We must, therefore, invariably consider the question of training. All the phenomena of hypnosis may be interpreted falsely by any one who overlooks this point. This refers in particular to on-lookers at hypnotic experiments. When hypnotic experiments are shown to outsiders, subjects are as a rule selected who have gone through a hypnotic training in some particular direction, and as the directions are various, the results also are various. The experimenter A. keeps in view a particular symptom, *a*, and reinforces it at each experiment; in the same way experimenter B. cultivates symptom *b*. In the first case *a* is fully developed, and *b* receives little attention; and in the second case the reverse happens. The Breslau investigators, for example, developed the imitative movements, while others did the same with the effects of the movements on

the feelings (*suggestions d'attitude*). He who only regards the final results and pays no attention to their gradual evolution will be inclined to believe that the two parties of investigators are engaged with different things; though it is in reality only differences in training which give a different appearance to states which were primarily identical. Each experimenter now only demonstrates such symptoms as he has cultivated by training, especially as this training commonly produces most interesting phenomena; the heightening of certain faculties in particular. The outsider is unaware that this is a mere result of hypnotic training, and is misled. Children who repeat to strangers the piece of poetry they know best, do exactly the same thing. Experimenters produce certain objective symptoms by means of training, and any one seeing them for the first time is apt to make mistakes. But every experimenter produces different objective symptoms—one, for example, a lasting catalepsy, another a perfect echolalia. These things strike the stranger who knows nothing about the previous training. The question of training is of immense importance. Many have suspected simulation because of the apparent variety of hypnotic states. This variety is really only the result of different training, if we put aside differences of character. In this respect the experimenter influences the development of the hypnosis. Unimportant phenomena such as echolalia are developed as much as possible, and are at last wrongly considered to be essential hypnotic phenomena. By training the subject learns, as it were, to "read" the experimenter's thoughts.

We meet something very similar, but under different circumstances, in the training of animals, in which, as we know, it plays an important part. It was through overlooking this fact that Stumpf, a well-known Berlin psychologist, was led to attest in the case of the horse, "Clever Hans," that the only possible explanation of the animal's power to calculate, read, etc., lay in the admission of telepathic communications passing between the owner and the horse. As we shall see later on, Stumpf failed to see the tiny signals to which the horse responded.

A long training is not at all necessary; Delbœuf artificially induced the stages of Charcot in one of his own subjects in a very few hours. My object in making these remarks is *to warn against attributing too great importance to demonstrations, particularly when these offer symptoms apparently objective and*

impossible to imitate. It should always be kept in mind that many such symptoms can be produced by training, and can, perhaps, be imitated by practice without hypnosis.

In addition to the artificial cultivation of certain symptoms, "training" also means the production of such particular modifications of hypnosis as are seen after frequent repetitions of the state. As has been said already, it is sometimes necessary to make several attempts before the hypnosis appears. Husson, in 1831, said this with regard to the magnetic sleep. In other cases hypnosis is produced very quickly, though it may take several sittings to produce deep hypnosis. In one case which I have seen, hypnosis with sense-delusions only resulted after eighty attempts, though lighter states had been attained earlier. Training not only makes the hypnosis deeper, but makes it appear more quickly. But, undoubtedly, a deep hypnosis may occasionally be induced at the first attempt; and Forel is right when he warns us against overestimating the value of constant repetition. I have often seen a subject fall into so deep a hypnosis in a minute or two on the first trial that post-hypnotic negative hallucinations could be induced at once.

But in most cases it is necessary to give the subject a hypnotic training in order to make the state as deep as possible. For this a particular method is advisable, as otherwise the deepening is not always attained. The first suggestions should be possible, and progress should be gradual. More will be attained in this way than by suggesting impossible situations at first which the patient will decline. And if a suggestion is often declined, there is apt to arise in the subject the auto-suggestion that he is refractory to this suggestion, or perhaps to any other suggestion. I therefore strongly recommend such a method for post-hypnotic suggestion. A man is in the hypnotic state for the first time. I suggest that when he wakes he shall call me an insulting name. He does not do it, but is perfectly ready to carry out another post-hypnotic suggestion; for instance, to tell me that he was quite well. Here there is only a slight degree of suggestibility at first, but it is quite possible by frequent repetition and gradual increase to get much more complicated suggestions carried out.

This concludes my review of the symptomatology of hypnosis. We have seen that the symptoms are of manifold kinds, and I may add that they are hardly ever identical in two different persons. In spite of conformity to law, one human body is

never exactly like another, the mental state of one man is never exactly like another's. It is the same in hypnosis; one man displays this symptom with greater clearness, another that. We shall never be able to find a subject in whom all the symptoms are united, just as we cannot find a patient who has all the symptoms of an illness as they are theoretically described.

CHAPTER IV.

POST-HYPNOTIC SUGGESTION.

I HAVE already spoken several times of post-hypnotic suggestion. This is a point of such importance in medicine and psychology that it must be examined in detail. No serious observer can nowadays doubt the reality of post-hypnotic suggestion.

The old mesmerists observed some cases of it. In 1787 Mouillesaux ordered a lady in the hypnotic state to pay a visit to a certain person next day; the command was exactly obeyed (Du Prel). Kluge, Noizet, and Schopenhauer mention other cases. Post-hypnotic suggestion has been much studied lately, particularly by Liébeault, Richet, Bernheim, Delbœuf, Gurney, and Forel.

Any suggestion that takes effect in hypnosis will also take effect post-hypnotically in suitable subjects; movements and delusions of the senses—itching, pain, action of the bowels, hunger, thirst, etc., can be induced. Dreams can be influenced. "To-night you will dream that you are at Swinemunde; you will go on the Baltic in a boat with six people; the boat will be upset, and you will fall into the water and wake at the very moment." The subject dreams this in detail. Dreamless sleep can be induced in the same way; or at least the subjects do not remember if they have dreamed. The memory can also be influenced by post-hypnotic suggestion; for example, a person can be made to forget on waking some event in his past life. Later hypnoses can also be influenced by post-hypnotic suggestion. We have already seen that subjects remember the events of earlier hypnoses in later ones: this can be prevented by post-hypnotic suggestion. In the same way post-hypnotic suggestions may be obeyed in a new hypnosis. For example, I suggest to a man that in three days' time he will again fall into hypnotic sleep and will then believe he is Napoleon I.

These post-hypnotic suggestions at first aroused much incredulity, and yet they do not present any features for which analogies are not to be found in normal life: I shall deal with this point in the theoretical section. Of course they do present certain peculiarities which I shall also return to later on.

Post-hypnotic suggestions may be divided into two groups; but I make this division merely for practical convenience in considering them. In the first the suggestion is remembered on awakening, in the second it is forgotten. It will be explained in the theoretical section that the loss of memory in the second group is only apparent. I shall discuss the second group much more thoroughly than the first, because the phenomena of the latter follow from those of the former.

It is possible to carry on suggestions from hypnosis into waking life; they are then called continuative suggestions. I suggest to X. that my photograph is on a visiting-card, and add that he will continue to see it after awakening. When X. wakes he is firmly convinced that the photograph is there. According to Londe an illusion of this kind has lasted for two years. This carrying on of suggestion into normal life happens sometimes by chance, when the suggestion has not been cancelled before the awakening.

I suggest to X., in hypnosis, that he is drinking peppermint water; I awake him, and he says that for an hour after he has a taste of peppermint in his mouth. To another subject, Y., I say, "You know that we drank two bottles of wine just now, and that we had roast goose for supper." When Y. answers "Yes," I tell him that after he wakes he will remember all about it. He wakes and relates it all; he declares that he has eaten too much and that the wine has made his head heavy; he even thinks himself slightly intoxicated from the quantity of wine he has drunk. This is a purely imaginary intoxication produced by suggestion. Hütten relates an even more interesting case; he says he has cured real intoxication by suggestion.

These phenomena may occasionally last for weeks and months. However, in many cases I have seen them disappear spontaneously a short time after waking. A man who directly after waking believed that he had seen his mother at my house before the hypnosis, forgot all about it after a few minutes. We had spoken of other things in the meantime, and this probably caused the rapid oblivion.

The following oft-repeated experiment belongs to the continuative suggestions:—I say to the subject, "Count up to

ten, and wake up when you get to three." He counts up to ten, but is awake while counting from four to ten.

In other cases the suggestion only takes effect after waking. I say to the subject, "You will not be able to move your right arm after you wake." He wakes, and is unable to move it, though otherwise in a normal state. Exactly the same effects may be produced after an interval of hours, days, weeks, and months. I say to a subject, "When you come to see me this day week, you will not be able to speak when you come into the room." He comes to see me in a week, and is fully awake when he enters the room; I ask him his name, but he is unable to say it or anything else. Here we have an example of fulfilment of suggestion after an interval, or *suggestion à échéance*, deferred suggestion, as it is called.

The moment for the fulfilment of the post-hypnotic suggestion can be decided in several ways. Here is a subject to whom I say, "An hour after you wake you will hear a polka played; you will believe you are at a ball, and will begin to dance." To another, whom I wake at eight o'clock, I say, "When the clock strikes nine, you will take the water-bottle from the table and walk up and down the room three times with it." The moment of fulfilment is decided differently in these two cases. In the first case an abstract term, an hour, is fixed; in the second, the moment is decided by a concrete external sign.

When it has been decided that the moment of the fulfilment of a post-hypnotic suggestion shall be determined by an external sign, it is as well to choose a stimulus which can easily be repeated.

I try a subject, X., with the post-hypnotic suggestion that he is to call one of the spectators a blockhead directly the clock strikes. X. does not obey; the moment the clock strikes he has an idea of what he is intended to do, but refuses to put it into action. Should I, however, instead of choosing the striking of the clock select some other stimulus which also arouses the idea and at the same time is sufficiently lasting to make it continuous, the desired result will then be attained. For example, the suggestion will be carried out if I say to a subject, "Directly I rub my hands together after you wake up you will call that gentleman a blockhead." As soon as X. is awake I rub my hands together, and the idea of what he is intended to do immediately arises in his mind; but he succeeds in resisting it for a time. I then keep on rubbing my hands together—perhaps for a minute or more—X.'s resistance gradually weakens, and in the end he obeys the command.

A post-hypnotic suggestion will be more readily fulfilled when the moment for its execution is determined by an

external sign; but successful cases in which no such sign has been employed are anything but rare. There is often a certain amount of unpunctuality in the fulfilment of a suggestion when no concrete external sign is used; for example, the suggestion will be carried out in three-quarters of an hour instead of an hour. Occasionally punctuality is very marked. Delbœuf made post-hypnotic suggestions to various persons by telling them to perform a certain act after so many minutes—say, a thousand minutes. In many cases, even with persons who were otherwise not able to estimate time rightly, striking punctuality was shown. Bramwell has made numerous experiments for the purpose of fixing the time in this respect. For example, he told a young lady aged nineteen to make the sign of the cross after the lapse of 4,335 minutes. In spite of the fact that she had forgotten all about the suggestion she fulfilled it accurately. Further experiments of this nature gave strikingly accurate results, no error exceeded five minutes, and Bramwell ascribed all to hesitation on the part of the subject. The subject was requested, in hypnosis, to transpose the hours and minutes, and did make miscalculations; but she nevertheless carried out the primary suggestion correctly, sometimes even when asleep at night. At Hansen's instigation Sommer made a few experiments of the same kind, but on a smaller scale. A student was told that he was to fall asleep when ordered; then, two minutes later, he was to talk about skating; after this he was to sit still for a minute, and finally jump up and shout "Hurrah! for Giessen." The suggestion was fulfilled to the second. The subject stated subsequently that he only remembered counting up to thirty-one or thirty-two, although he wanted to count up to one hundred and twenty to complete the two minutes. Sommer considers that counting would enable the subject to keep to the time-limit, but that loss of memory intervenes.

I will point out a frequent source of error in experiments of this nature: this is the behaviour of the spectators. They look at the clock at the appointed time, or make some other unconscious signal that the right moment has arrived. I call particular attention to this so that the time may be accurately observed.

The older mesmerists, Nasse and Eschenmayer for example, made investigations about this faculty of somnambulic subjects for exactly reckoning time. But it is an exaggeration to maintain that as a rule the

time is reckoned accurately. The punctuality displayed by hypnotics is very like that of people who can voluntarily awake from their usual sleep at any hour they please. According to statistics collected by Childe many people are capable of doing this. The ancient Hindoos studied this subjective faculty for reckoning time very industriously. It is sometimes called the mental clock ("Kopfuhr," Du Prel).

A third way of fixing the time at which a post-hypnotic suggestion is to be fulfilled has been thoroughly investigated by Gurney and Pierre Janet. For example, I say to a subject, X., "When I shuffle my feet for the tenth time after you wake up, you will burst out laughing." X. wakes up quite oblivious of my order, and I engage him in conversation and shuffle with my feet several times without his taking any notice; at the tenth shuffle he gives a loud laugh. I repeated the experiment on another occasion, and when I had shuffled my feet four times I asked X. if he heard the shuffling; he replied "No." I went on quietly talking, and at the same time gave six more shuffles with my feet, upon which the suggestion was carried out. In most cases, however, the result was not quite so accurate—the post-hypnotic suggestion was certainly fulfilled, but not at the right signal.

Many deferred suggestions resemble those suggestions in which the moment of fulfilment is determined by counting (Gurney). Post-hypnotic deferred suggestions can be made in two ways; for example, on the 3rd of May I say to a person who calls on me every day, "On the 6th of June, when you come into my room, you will see me with a black face, and you will laugh at me." The suggestion succeeds. But here a fixed date is named which helps the subject to carry out the suggestion in the same way as the striking of the clock in the case first quoted. Delbœuf, in particular, has pointed out the importance of this. It would have been another matter if I had made the suggestion thus: "On the thirty-fifth day, reckoning from to-day, you will come into my room and see me with a black face, etc." According to Gurney's observations, suggestions of this kind succeed, and my own experiments confirm him. An example may make this sort of suggestion clearer. I suggested once to X., "You will come to my house on the sixteenth Tuesday, reckoning from last Tuesday, and will abuse all the people present," etc. This suggestion succeeded completely, although no fixed date was named.

. . . . . .

I have as yet only discussed the manner of determining the point of time for the carrying out of the post-hypnotic suggestion. I now come to the question, What is the condition of the subject while carrying out the post-hypnotic suggestion? Dumontpallier, Beaunis, and Liégeois observed that post-hypnotic suggestions were certainly not carried out in the waking state, even if the action took place after the awakening from hypnosis. The question has led to a lively discussion; and Forel and Gurney have shown that the post-hypnotic suggestion may be carried out in very different states. A few examples will make this clear to the reader.

A man (X.), thirty years old, is in the hypnotic state. I say to him, "When you wake, directly I cross my knees you will take the inkstand from the table and put it on the chair." He wakes at my order, and I talk to him. After a time I cross my knees; he begins to stare at the inkstand and hardly answers me. He goes to the table, takes the inkstand and puts it on the chair; upon which I suggest to him that he sees his brother, that he is eating his dinner, etc., all of which suggestions he accepts. I am obliged to re-awaken him to put an end to this new state of suggestibility. After waking he remembers absolutely nothing.

This case is characterized by loss of memory of all that happened during the post-hypnotic state, and further by susceptibility to suggestion. I do not know how this state is to be distinguished psychologically from a true hypnosis, and to my mind Delbœuf is right when he says that to make a post-hypnotic suggestion is really to order a new hypnosis at a fixed moment and the carrying out of the suggestion in this new hypnosis.

There are other very different cases. We have here a man in hypnosis and I say to him, "When you awake, directly I rub my hands together you will forget your name. When I separate my hands you will remember it again." Everything happens as ordered; we talk to one another, but when I bring my hands together the subject forgets his own name. He is, however, completely awake, and incapable of accepting any further suggestion. When I separate my hands he knows his own name, and knows also that he had forgotten it a moment ago. He goes away, and in a few days we meet again; but now he remembers his name however I hold my hands. But he remembers perfectly well that the other day he was several times unable to say his own name. He maintains that he was awake all the time.

We are not justified in calling this case one of hypnosis. There was no mental symptom of hypnosis, no loss of memory,

no suggestibility, no fatigue; the subject did not think he had been asleep; nothing remains but to consider the state a perfectly normal one, except on one point. Whether such a state may be regarded as normal, generally speaking, is another matter. I shall discuss this when I come to the legal question for which these cases are very important, according to Bentivegni.

It appears from these examples that post-hypnotic suggestions may be carried out in various different states. Between the two extremes—the one case in which there were all the mental symptoms of a new hypnosis, and the other in which there were none—there are many degrees which I will now discuss.

Here is a third example. A woman is hypnotized, and two men A., and B., are present. I say to the subject, "When A. speaks to you after you wake, you will laugh at him. When B. speaks to you, you will put your tongue out at him. Wake!" She wakes. A. speaks to her and she laughs. I ask, "Why did you laugh just now?" "I did not laugh." A. speaks to her again; she laughs, and again at my question she denies having laughed. She puts out her tongue at B. when he speaks to her, and the moment after, when I question her, she says that she did not do it. I suggest that she hears a barrel-organ, but she says she does not, and is insusceptible to other suggestions. She remembers everything else that has happened, and knows perfectly well what I have said to her. All that is forgotten is the post-hypnotic act and what is immediately connected with it—*i.e.*, the words which A. and B. spoke to her. She can repeat what I said to her, and her replies; everything, in fact, unconnected with the post-hypnotic suggestion. She knows nothing about the brief space during which she carried out the suggestion; at the same time she recognizes no gap in her memory.

In this case there is complete loss of memory for the post-hypnotic act, and no further suggestibility; the loss of memory extends simply to the post-hypnotic act. This is, then, a third way in which hypnotic suggestion is carried out, and it is not rare.

In other cases the subject not only forgets the post-hypnotic act he has just performed, but becomes susceptible to a fresh suggestion while he is carrying it out. It is difficult to distinguish these cases from those just described; on that account I shall not make a separate group of them; for it seems that subjects like the person described in the last example are really always susceptible to suggestion while they are carrying out the act, but that in many cases the act takes

place too quickly to allow of a fresh suggestion being made. The post-hypnotic act is completely forgotten, while the state of the subject before and after the action is quite normal. Liégeois thought this a separate state, which he called *condition prime.* He gave this up later, and now calls the state *condition seconde provoquée;* Beaunis calls it *veille somnambulique;* Gurney, "trance-waking." I, however, agree with Delbœuf that these states must be considered true hypnoses. Evidently the suggested idea is so powerful in them that it produces a state analogous to that in which it was first implanted. When the idea vanishes, the abnormal state also vanishes.

I will now proceed to give a fourth case. I suggest to X. to take a chair and put it on the table five minutes after he wakes. The suggestion is carried out. While he is putting the chair on the table I call out suddenly that a dog is biting him. He believes it, kicks the imaginary dog away, and wakes spontaneously. X. remembers moving the chair and remembers the dog, but says the whole thing was like a dream.

Consequently this state is characterized by suggestibility during the carrying out of the post-hypnotic suggestion; and there is also memory. It is true X. feels as if he dreamed it. He has a consciousness of having slept through the performance, and of having waked when it was ended. This consciousness of having slept is very important (Delbœuf). We often have some life-like experience in a dream and yet know directly we wake that it was a dream. I think the last described post-hypnotic state must be considered a hypnosis.

Amongst the post-hypnotic states we have studied—(1) a state in which a new hypnosis characterized by suggestibility came on during the carrying out of the suggestion, loss of memory afterwards, and no spontaneous waking; (2) a state in which there was not the slightest symptom of a fresh hypnosis, although the suggestion was carried out; (3) a state in which the post-hypnotic suggestion was carried out with complete forgetfulness of the act, with or without fresh susceptibility to suggestion, and from which the waking was spontaneous; (4) a state of susceptibility to suggestion with retention of memory following, but a feeling of having been asleep. In judging of these states the chief symptoms are—firstly, the fresh suggestibility; secondly, the retention of memory; and thirdly, the feeling of having been asleep. Whether the subject wakes spontaneously

or has to be again awakened is of secondary importance, as spontaneous waking is observed in ordinary hypnosis.

Gurney has directed attention to certain devices for estimating the mental state at the moment a post-hypnotic suggestion begins to act. We have seen that the renewed suggestibility is of great importance in deciding whether a fresh hypnosis has been induced or not, and Gurney has made use of this post-hypnotic suggestibility for solving the question. The subject, X., is shuffling the cards (post-hypnotically). He is told while he is shuffling that when the clock strikes he will jump up three times. He has finished shuffling and is quite awake. There is nothing to show that he is still in hypnosis; he is not susceptible to suggestion. He does not remember shuffling the cards, and contends that he has not done it; but directly the clock strikes he jumps up three times. From this post-hypnotic susceptibility to suggestion we conclude that X. was not in a normal state when he was shuffling the cards. Whether this state was hypnotic, or was another mental state, as Beaunis and Gurney suppose, is another question. I incline to think it a true hypnosis.

Gurney thinks that in order to properly estimate this post-hypnotic state we must take the memory into consideration also. We have seen that subjects in later hypnoses remember what has occurred in earlier ones. If, now, the events of earlier hypnoses should be remembered in the post-hypnotic state, we should consider it a fresh hypnosis. Now, I have often found that there was a complete recollection of the events of earlier hypnoses while the post-hypnotic suggestion was being carried out. This fact also favours the supposition of a fresh hypnosis.

Finally, I may add that there are cases in which physical symptoms are found. The fixed look and blank expression often seen during the carrying out of the post-hypnotic suggestion also justify the assumption of a fresh hypnosis.

It may be concluded from what I have said that post-hypnotic suggestions may be carried out in various different states. This, I may add, is the case not only when we compare one subject with another, but when we observe the same subject under different suggestions. The questions upon which it all hinges are—(1) Does the subject remember later on what he has done, and does he remember the events of earlier hypnoses while carrying out the suggestion? (2) Does

he feel after carrying out the suggestion as though he had just been asleep? (3) Whilst doing what has been suggested is he susceptible either to suggestions to be carried out at once, or to new post-hypnotic suggestions? (4) How does the subject look? Has he the appearance, the manner, the physical symptoms usual in hypnosis, or not?

The question becomes even more complicated when we consider the following of Forel's experiments. Forel said to a nurse, "Whenever you say 'Sir' to the assistant-physician, you will scratch your right temple with your right hand without noticing it." The nurse did so, talking clearly and naturally all the time. She did not notice that she was scratching her face.

Here the subject behaves normally, and yet the post-hypnotic suggestion is executed during the conversation with complete loss of memory. When a subject performs one act with loss of memory, is this state hypnosis or some other state? I think it should be regarded as a part of normal waking life, for it would be a mistake to conclude a hypnosis from the mere forgetting of one act, without susceptibility to suggestion. Gurney points out that loss of memory alone cannot be taken for proof of an abnormal state, because in normal life we perform actions and see objects without remembering them afterwards. If the action is a purely mechanical one, such as winding a watch, we often remember nothing about it.

I have purposely in the last section only discussed those movements and acts executed post-hypnotically; but all sorts of delusions of the senses, positive and negative, can be induced hypnotically at pleasure. We can cause whole scenes to be gone through; the subject will go to a ball, or partake of an imaginary dinner, etc. The state of the subjects during the realization of a post-hypnotic suggestion may differ considerably, but in my experience it is almost a rule that the induction of a post-hypnotic delusion should induce a fresh hypnosis with susceptibility to suggestion and subsequent loss of memory.

It is possible, besides, to influence subjects in these states in any way. For example, we may make the suggestion thus: "You will see a dog five minutes after you wake; but you will remain awake and not allow anything else to be suggested to you." The subject may in this way be protected from further suggestion; he will then carry out the first suggestion, but for

the rest will appear fully awake. X. and Y. are at my house. I hypnotize Y., and say to him, "When you wake, X. will be sitting on this chair; you will remain awake." When he wakes he believes that he sees X. on the chair, and talks to him, etc. I call his attention to the real X. and say, "Which is the real X.? You see one on the chair and one standing before you." Y. feels the chair and the real X. to find out which is air and which is reality. He finally concludes, "He is on the chair." And yet Y. is not susceptible to suggestion on other points.

But even if we are thus able to influence post-hypnotic acts and sense-delusions by means of particular suggestions, it does not necessarily follow that all the details of a post-hypnotic suggestion depend entirely upon the experimenter's influence. I am much more inclined to think that the state during the carrying out of a post-hypnotic suggestion may vary without any such influence. Undoubtedly much depends upon the purport of the suggestion. The more absurd the suggestion, the more it clashes with the subject's normal way of thinking, the more likely will a fresh hypnosis set in during the carrying out of the post-hypnotic suggestion. For example, X. was in a perfectly normal state when carrying out the post-hypnotic suggestion that he was to rub his hands together occasionally when talking to me later on; but the suggestion that he was to take my purse out of my pocket induced a fresh hypnosis. The nature of the action suggested undoubtedly exerts a great influence in determining the state induced, and to it we must add the character of the subject. I certainly do not think we are justified in exclusively attributing all the various states observed to the influence of the experimenter, or to training.

In what precedes I have discussed the state of the subject during the carrying out of the post-hypnotic suggestion. It will not take long to consider the state between waking and the execution of the suggestion. The subject is then nearly always fully awake, and the state is, in fact, as if he had been wakened without previous post-hypnotic suggestion. However, there are some cases in which the wakening is not complete so long as the effect of the suggestion lasts; this occurs particularly when the suggestion is repugnant to the subject's character. Such subjects look tired and sleepy, and when questioned, often say that they are not quite awake but still half-asleep. I have had cases in which I was obliged to cancel the suggestion before I could completely awaken the subject.

In other cases I have observed a subjective discomfort instead of a feeling of fatigue, until the suggestion was executed. This subjective discomfort is sometimes felt without the suggestion being carried out. One lady to whom I had suggested that she should put a book on the floor woke in great discomfort, but it did not occur to her to put the book on the floor. She recovered herself, however, when, at my request, she had put the book on the floor in the waking state. Another subject complained of twitching in the arm after waking; I had suggested to him to give me his hand when he woke. He did not do it till I asked him again in the waking state; before that he was aware of nothing but the twitching. I have, however, never observed evidences of discomfort when the post-hypnotic suggestion had a therapeutic aim. It is obvious that the discomfort is caused by the conflict waging in the subject's mind, even when at the threshold of consciousness; and, of course, there can be no question of such a struggle when the suggestion has a therapeutic aim. Köhler declares that between waking and the execution of the post-hypnotic suggestion, the subject is invariably in one or other of two abnormal states—the one an obvious, but exceptional, form of hypnosis, the other a peculiar state of intermittent hypnosis. By which Köhler means that the patient remains hypnotized from the beginning of the experiment to the execution of the suggestion, or else in an exceptional condition in which he can carry on a rational conversation until he suddenly falls into a fresh hypnosis. The abnormal state terminates directly the post-hypnotic suggestion is fulfilled. Nevertheless, I consider Köhler's views on this question erroneous—at least they as little agree with the results of my own observations as they do with those of Lœwenfeld and others.

There are, of course, numerous cases of post-hypnotic suggestion in which the suggested act is not performed, many persons being able to offer a more or less successful resistance. It sometimes even happens that a hypnotic rejects a suggestion during hypnosis. Many carry out only the suggestions to which they have assented (Pierre Janet). Scripture reports a case observed at Brown University. The post-hypnotic suggestion was given to a person to pronounce *a* always like *ee*—*e.g.*, "feether" instead of "father"; on being awakened she was often asked about her parents, but always avoided using the word "father," speaking instead of "the husband

of my mother." Pitres relates an interesting case of a girl who would not allow him to wake her, because he had suggested that on waking she would not be able to speak. She positively declared that she would not wake till he gave up his suggestion. But even when the suggestion is accepted as such, a decided resistance is often expressed during its post-hypnotic execution. This shows itself as often in slow and lingering movements as in a decided refusal to perform the act at all. The more repugnant the action, the more likely is it to be omitted.

As in all the above cases of post-hypnotic suggestion the command was not remembered, it is particularly interesting to observe how the subjects try to account for their execution of the suggestions. Naturally, we shall only here consider those cases in which the action is not immediately forgotten; in the others, subjects do not try to find reasons for actions which they have forgotten.

Let us take an example. I say to a hypnotized woman, "After you wake you will take a book from the table and put it on the bookshelf." She wakes and does what I told her. When I ask her what she has been doing, she answers that she has moved the book from the table to the shelf. When asked for her reason, she answers, "I do not like to see things so untidy; the shelf is the place for the book, and that is why I put it there."

In this case my command led to a definite action being carried out; but the subject does not remember my suggestion, believing that she has so acted of her own accord, from love of order.

Let us go on with our experiment. I suggest to the re-hypnotized subject to take the book from the shelf and lay it under the table, which she does. I ask her why she did it; she can give no reason. "It came into my head," she answers. I repeat the experiment several times. To a new request for her reason she finally replies, "Something made me feel as if I must put the book there."

In this case the subject, who at first believed she was acting freely, came by degrees to recognize the restraint put upon her; she, perhaps, suspected the suggestion, but was not sure of it.

Another case. I suggest to a hypnotized man to use an insulting expression to me when he wakes. He wakes, and after a pause of a

few seconds, during which his face expresses an inward struggle, he calls out, "Donkey!" When asked why he so insults me, he makes many excuses, and explains, "I felt as if I must say 'Donkey!'"

Here we have to do with a paradoxical action; the man knew at once that constraint was being put upon him; the woman who performed the simpler act above only perceived the constraint after several experiments. However, in a great number of cases the result is different.

We have here a hypnotized subject to whom I say that when he wakes he is to take a flower-pot from the window-sill, wrap it in a cloth, put it on the sofa, and bow to it three times. All of which he does. When asked for his reasons he answers, "You know, when I woke and saw the flower-pot there I thought that as it was rather cold the flower-pot had better be warmed a little, or else the plant would die. So I wrapped it in the cloth, and then I thought that as the sofa was near the fire I would put the flower-pot on it; and I bowed because I was pleased with myself for having such a bright idea." He added that he did not consider the action foolish, he had told me his reasons for so acting.

In this case the subject carried out an absurd post-hypnotic suggestion; he was unconscious of the constraint put upon him, and tried to find good reasons for his act. Most experimenters have observed that their subjects try to find reasons for having carried out even the most foolish acts suggested. This mental process so frequently follows the execution of a post-hypnotic suggestion that some experimenters have come to look upon it as the rule—but such an assumption is undoubtedly erroneous.

We thus see that when subjects are questioned as to their motive they make different answers; they either believe that they have so acted of their own accord, and invent reasons for their proceedings, or they say they felt impelled to act so; or they only say, "It came into my head to do it." We can use suggestion here also. When the original suggestion is being made, it may, at the same time, be suggested to the subject to believe that he has acted of his own free-will (Forel), or to believe that constraint was put upon him. When such a suggestion is not made, it depends upon the subject's power of self-observation which reason he gives—whether he perceives the restraint, or invents false reasons for his conduct. Something also depends upon the frequency with which the experiment is made, and particularly on the greater or lesser absurdity

of the suggested act. This endeavour of certain subjects to find a motive for their apparently free acts is very instructive, and has, as we shall see, a certain value in determining our conception of free-will from a psychological and philosophical point of view.

Post-hypnotic suggestions are of especial value for the induction or prevention of future hypnosis. In this way an easily hypnotizable person may be prevented from allowing himself to be hypnotized by another subject. Post-hypnotic suggestion is an excellent means for protecting susceptible people and guarding them against unexpected hypnosis, as Ricard pointed out for the somnambulic state. Mr. X., whom I had often hypnotized, had also often been hypnotized by Mr. A. I suggested to X. that he should in future only allow himself to be hypnotized by doctors, but on no account by Mr. A. After this Mr. A. could no longer hypnotize him. However, I do not believe that this is a perfect protection in all cases. But the chief danger, which does not arise from susceptibility to hypnotism, but from susceptibility to hypnotism against the subject's will, is thereby guarded against. On the other hand, it is possible to throw a subject into an unexpected hypnosis by means of post-hypnotic suggestion. I say to a subject, "Directly I say the word 'to-day' you will fall into a fresh hypnosis." I then wake him, and he remains awake until I say "to-day"; upon which he is instantly thrown into a fresh hypnosis.

It is difficult to say for what length of time the carrying out of a post-hypnotic suggestion may be successfully deferred, since this depends on the subject's character and the method employed. The longest post-hypnotic suggestion I have seen was executed at the end of four months; no hint had been given to the subject in the meantime. The longest which has ever been described, as far as I know, was in a subject under Liégeois and Liébeault; in this case exactly a year elapsed before the suggestion was carried out. The case of the photograph, mentioned on page 161, in which the photograph remained visible for two years is rather different, as it appears that the suggestion was often recalled to the subject's memory. The case mentioned by Dal Pozzo is, perhaps, of the same kind: a person who was afraid of thunderstorms was cured of the fear by post-hypnotic suggestion. The effect is said to have lasted twenty-six years (Belfiore).

I have hitherto only discussed those post-hypnotic suggestions in which there is loss of memory after waking from hypnosis. This loss of memory greatly favours the carrying out of the suggestion; but it is not a necessary condition, for post-hypnotic suggestions are often executed in spite of the memory remaining intact. These cases are highly interesting, because the compulsion can be better observed in them. The subject knows that his action was the result of a hypnotic suggestion from which he could not escape. Sometimes suggestion only succeeds with difficulty and after a long struggle, in consequence of the subject's resistance and control of his consciousness.

One of my colleagues, a doctor, was in the post-hypnotic state; I suggested abnormal movements to him with success; sense-delusions did not succeed. I told him that after he woke he would be unable to say his name whenever I laid my hand on his forehead, and further that instead of his own name he should always say mine. When he woke from the hypnosis, whenever I put my hand on his forehead he said his name was Moll; he knew his right name also, but was unable to say it. He remembered my order about it, and did not believe in any supernatural force; he knew that the effect was mental, but could not help himself.

It is the same thing with sense-delusions, they can also be produced post-hypnotically, in spite of the fact that the suggestion is remembered. It is true that the effect of the sense-delusion in such cases is often not to be seen, because, as the order is remembered, reasoning is possible, and thus the suggestion is negatived. Nevertheless, sense-delusions with remembrance of the suggestion are rarer, because loss of memory more frequently follows hypnosis with sense-delusions, even though the loss of memory can always be prevented by post-hypnotic suggestion.

CHAPTER V.

COGNATE STATES.

We can often advance the study of a state which has hitherto been little known and examined, by comparing it with other states with whose symptoms we are better acquainted. We will, therefore, try to find points of correspondence with hypnosis. The very name shows that there is a resemblance between sleep (*hypnos*) and hypnosis, and some investigators (Liébeault, Bernheim, Brullard, Forel, Vires), consider hypnosis an ordinary sleep. They think that a person who falls asleep spontaneously is in *rapport* with himself, while a hypnotized subject is in *rapport* with the person who hypnotized him; in their view this is the chief difference between sleep and hypnosis. I believe, however, that we cannot so easily agree to such an identification of the states; we must begin by distinguishing the light and deep hypnoses.

We see that in light hypnosis there is merely an inhibition of voluntary movement; consciousness and self-consciousness are unaffected, and what happens during hypnosis is usually remembered. Now, in sleep there is always a great decrease of self-consciousness. But it is just this self-consciousness which remains intact in light hypnosis; and in this state the subject is perfectly aware of all that goes on, and, as a rule, forgets nothing on waking. Consequently, I do not think we can make a close connection between sleep and superficial hypnosis; nor do I think it possible to make a fruitful comparison between these light hypnoses and the states of drowsiness and fatigue which precede sleep. In any case, a feeling of fatigue is not uncommon in these hypnotic states. But this is not always the case, and we have seen that the loss of voluntary movement, or its subjection in a greater or less degree to the influence of the experimenter, is one of the chief phenomena in hypnosis. There is hardly a hint of this in the drowsy state; certainly there is a general heaviness in the limbs, but it

is not of a nature to inhibit every voluntary movement; and the loss of voluntary movement in sleep is not subject to the influence of the experimenter as it is in hypnosis. Further, these light hypnotic states are distinguished from the earlier stages of sleep by the decreased activity of consciousness in these latter. The current of ideas, of images of memory, is less under the control of the will; sense-impressions do not develop into conscious ideas in the usual way; much that generally excites our interest and attention is unnoticed and overlooked, while there is often reverie independent of will. But almost all this is entirely absent in the light hypnotic states, in which only the voluntary movements suffer change.

I cannot bring myself to consider a subject merely "asleep" so long as his consciousness and self-consciousness remain intact. There are, certainly, many investigators of a different opinion. Vogt, for example, even goes so far as to call anæsthesia induced by suggestion a state of partial sleep. According to him, all sleep depends upon some form of inhibition, which may set in and disappear with equal suddenness, may be lessened or terminated by a peripheral stimulus, but never attains the intensity of a real outbreak such as we meet with in cerebral disease. The anæsthesia induced by suggestion often presents these characteristics, and Vogt, therefore, calls it a state of partial sleep. Sollier holds similar views on the phenomena of hysteria. In his opinion, every hysterical person is more or less a somnambulist. For example, the somnambulic state in such cases is the result of anæsthesia, and Sollier calls the termination of the anæsthesia waking. According to him different regions of the brain participate in the production of sleep. But, as I have already mentioned, I do not think we are justified in associating such conditions with sleep, although in common parlance we speak of a limb being asleep. Sollier's views, which are also held by some other investigators, may be accounted for to a certain extent by the fact that he and his adherents attribute all functional activity to definite cerebral processes. The holders of such views are consequently inclined to speak of partial sleep when certain portions of the body cease to functionate. But, from a psychological point of view, we must not speak of sleep unless there is some essential disturbance both of consciousness and self-consciousness.

The case of deep hypnosis is essentially different. It is

characterized by sense-delusions which are just the same thing as our nightly dreams. In order to carry out the comparison, it will perhaps be well to consider the mode of origin of dreams in ordinary sleep. Dreams are divided into two classes, according to the manner of their origin (Spitta): (1) dreams induced by nerve-stimulation, and (2) dreams induced by association of ideas. The first—by far the most numerous—are induced by a peripheral stimulus of the nerves, affecting the brain. Here the stimulus is certainly felt; or a memory-image arises as well, and a perception results. This perception does not, however, correspond to the actual stimulus. What memory-image will be aroused, and what dream will result, depends upon many factors which for the most part escape our observation. Consequently, the memory-picture aroused by a stimulus attaches itself in a number of cases, but not invariably, to a previously existing dream. "When an orator dreams he is making a speech, he takes every noise for the applause of his imaginary hearers." Dreams which are called up by nerve stimulation often occur accidentally. Spitta relates that he once dreamed he was a gymnast. In the dream he was thrown to the ground, bound, and boiling water poured on his left foot. The irritation thus produced was so unbearable that he awoke. His explanation is that he was very tired that night and forgot to take off his left sock, and the weather being very warm, the irritation set up by the sock caused the dream which frightened him. That dreams can be artifically called up by nerve-stimulation has been demonstrated by Gregory, MacNish, Maury, Leixner and many others. If a sleeping man is sprinkled with water he will dream of a shower of rain. Maury states that when Eau de Cologne was held to his nose he dreamed he was in Farina's shop at Cairo. Mourly Vold put the limbs of certain of his patients in a fixed posture before they fell asleep, by bandaging their hands and feet together, and was thereby enabled to demonstrate that an enforced position influences the nature of the dream. This is in accord with an observation previously made by Child, that dream-activity is influenced by artificial changes in the muscular sense. I have taken these statements from the writings of Sante de Sanctis, who made many investigations into the question of artificially induced dreams. In some of his experiments, Sante de Sanctis placed a musical clock under the pillow of his sleeping subjects—the latter his son, a girl, and

an imbecile. Pleasant dreams resulted, and the subjects smiled in their sleep. A melancholy air caused the imbecile to ask on waking what had been done to him; and, on one occasion, the girl dreamed the clock was playing martial music. Delicious perfumes (violets, heliotrope,) gave the experimenter's son pleasant dreams. According to Sante de Sanctis, dreams can undoubtedly be influenced in the manner indicated, although a particular stimulus does not invariably produce the same results when repeatedly applied to the same subject, or similar results when a different subject is chosen.

The second kind of dreams are dreams from association of ideas; they are supposed to follow on a primary central act. The memory-image is supposed to be caused by some primary central activity, and not by a peripheral stimulus. We may certainly place by the side of dreams from association of ideas the forms of auto-suggestion which I have frequently mentioned in the foregoing sections, and which I have thoroughly discussed on page 154, and such a comparison is especially permissible in the case of those auto-suggestions by which Hirschlaff considers abnormal hypnosis specially characterized.

Between these two classes of dreams there is another, which I may call suggested dreams. In these no stimulus is applied to the nerves of the subject which he may work out at his fancy; but a dream is suggested to him verbally. An acquaintance of mine told his daughter that she saw rooks, upon which she dreamed of them and related her dream on waking. On other occasions the attempt failed. This was already known to the old mesmerists, and their knowledge must be in nowise underrated. Kluge gave an account of such observations:—"Thus mention is made in many places of an English officer who could be made to dream anything wished, by softly whispering to him. On one occasion, he was made to dream of all the phases of a duel, from the beginning of the quarrel to the firing of the pistols which had been put in his hands for the purpose. The report of their discharge woke him." Sante de Sanctis made several experiments on his nine-year-old son when asleep, by whispering certain words in his ear. On three distinct occasions the word "pale" was used and the child woke up a minute after. In two of the three cases there was nothing in the dream which could be attributed to the word employed, with certainty. Once, however, the child woke up frightened, and when asked what he

had dreamed, replied that he had had a horrid dream. "Papa, I thought you were scolding me, and I trembled with fright, because you were quite pale with anger." On three other occasions, Sante de Sanctis whispered the word "task" into the boy's ear. Again, there was no result in two of the cases; but in the third, the boy dreamed that it was time for him to go to school and that he had not finished the very long task that had been set him the previous day. It is also comparatively speaking easy to call up dreams artificially in the case of persons who habitually talk in their sleep; here, we must take it, that the waking person insinuates himself, so to speak, into the sleeper's chain of thoughts, which he is then able to influence in their course. This often succeeds, as we shall see later on. It appears that as far as ordinary sleep is concerned, certain stages are more fitted for suggested dreams than others. The transitional stage between waking and deep sleep, which the French call the hypnagogic state, is particularly noteworthy in this respect. Havelock Ellis, Manacéïne and others have called attention to the suggestibility of subjects in this state, and Delbœuf ascribes to it a rôle of particular importance in the causation of nervous and mental diseases. He supposes that such maladies are of auto-suggestive origin, and that they develop themselves like post-hypnotic suggestions.

As regards the mode of origin, these suggested dreams are identical with the suggested sense-delusions of hypnosis.

But the mode of origin of other dreams in sleep occurs in hypnosis also. I have already spoken of dreams from association of ideas, which are analogous to the auto-suggestions of a hypnotic subject. This is particularly clear when we compare the hallucinations induced by nerve-stimulation on p. 152 with them; these hallucinations are identical as to mode of origin with dreams induced by nerve stimulation in ordinary sleep. I hypnotize X., and repeatedly blow with the bellows close to him, without speaking to him. The blowing causes a central excitation, and X. believes he hears a steam engine. He dreams he sees a train, and believes he is on the platform at the railway station at Schöneberg. This is exactly the same thing as a dream produced by nerve-stimulation, in which the falling of a chair makes the dreamer think he hears a shot fired, and dreams he is in battle. Besides, in hypnosis as well as in sleep, such stimuli are, as a rule, enormously overestimated,

as Tissié points out; a slight noise is taken for the report of a gun, and a gentle touch with the hand for the bite of a dog. I drum on the table without speaking; the subject hears and dreams of military music, thinks that he is in the street, and sees soldiers, etc., etc. Tissié mentions that in sleep visual impressions seldom lead to dreams, since we usually sleep in the dark and with our eyes closed. We can, however, produce dreams in ordinary sleep with the help of any source of light. It is interesting to find, nevertheless, that many investigations on sleep show that the nervous stimulation comes preferably through the ear, as in hypnosis (Mary Whiton Calkins).

One thing is clear from the comparisons I have made: it is a mistake to think as many do, that all intercourse with the outside world is cut off in sleep. Indeed, the opinion that by far the greater number of dreams are induced by sense-stimuli has its adherents (Wundt, Weygandt). This receptivity to stimuli which reach the brain, unregulated by the consciousness and mistakenly interpreted, is a phenomenon of both sleep and hypnosis. Further, it is evident from what has been said that the method employed to make external suggestion in hypnosis often suffices to induce dreams in sleep. At the most, there is only a quantitative difference, since most sense-delusions are directly suggested in hypnosis, while in sleep dreams are caused by some peripheral stimulus which undergoes a special elaboration in the brain of the sleeper. A qualitative distinction is not here possible, although Sully separates sleep and hypnosis on the ground that dreams arise in the former differently from hallucinations in the latter.

Consequently, the purport of dreams, as well as the way they originate, is alike in sleep and hypnosis. But, as in sleep we believe ourselves in another situation, and encounter all sorts of sense-delusions, so is it in hypnosis. And as a subject in hypnoses can be replaced in earlier periods of his life, so in dreams also. Many habitually dream that they are again undergoing the final examination at college many years after. Complete changes of personality also take place in dreams. An officer who greatly admired Hannibal, told me that he had dreamed he was Hannibal and had fought an imaginary battle in that character. Another man was even less modest; he once dreamed he was God and was ruling the world.

We cannot decide whether there is more dreaming in hypnosis than in sleep, because we can never know with

certainty how many dreams happen in sleep. While some say that dreams only occur during a short period of sleep, others, like Kant, Forel, Exner, and Simonin, go so far as to deny that there is any sleep without dreaming; they say that dreaming is continuous, but that most dreams are forgotten. Jouffroy, also, considers that we invariably dream when asleep. Bigelow does not think that dreaming has been proved to be continuous, but he is convinced that the mental activity is as unbroken in sleep as it is in the waking state. Vaschide, with whom Näcke agrees, likewise thinks that sleep without dreaming hardly ever occurs.

In spite of all this, we can find a difference between the phenomena of deep hypnosis and of sleep in several points—(1) in the apparently logical connection between the suggested idea and the hypnotic subject's own thoughts; (2) in the movements of the subject, and particularly in his speech, since there may be a conversation between the experimenter and his subject.

With regard to the first point, we have seen (p. 133 *et seq.*) that a series of ideas sometimes links itself logically to another particular idea. Consequently, the difference between hypnosis and sleep is not a fundamental one. Even if this linking is, on the whole, merely mechanical and the result of habitual association of ideas, it must be admitted that the power of discrimination is not entirely in abeyance in hypnosis, but can, as we have seen, display a certain amount of activity. Still, the whole connection can be broken at any moment by suggestion, as I have shown; in the same way the whole current of ideas may change at any moment. It appears, at once from this, that the independence of a hypnotic subject is very limited. The logical connection mentioned above lasts only as long as the experimenter permits. In the dreams of the night, which Radestock refers entirely to the pause in logical thought, there is not usually such a logical connection, because it but rarely happens that they centre in a definite idea as in hypnotic suggestion. In hypnosis the attention of the hypnotic subject is directed to the experimenter; the ideas given by the latter are accepted, and retain a certain amount of supremacy. In sleep the most diverse sensations are conducted to the brain; as the sleeper's attention is not usually directed to a special point, it is much less easy for a definite idea to gain supremacy. Giessler points out how

easily a dreamer's personality can be changed. When a change of character in hypnosis is brought about by suggestion, the subject does his utmost to play the part suggested; on the other hand, as Giessler again points out, a person who is merely dreaming immediately transfers the experiences, characteristics, titles, functions, and occupations of others to his own dream *ego*, and without effort. I will not go into details of examples. It is known that Voltaire wrote poetry in sleep, that mathematicians sometimes solve problems when asleep, and that the celebrated physiologist, Burdach, worked out many scientific ideas in sleep. It is said of Agassiz that he solved the problem of fossil fish which he was engaged upon, in a night-dream (Bigelow). Hack Tuke reports that the utterances of a person who is dreaming that he is disputing with some one are not invariably illogical; and he relates that one night, after having taken part in a lively discussion on spiritualism, he had a dream in which he worked out a series of experiments with considerable acumen.

I mentioned the movements in hypnosis as a further contrast between this state and sleep; but this assuredly forms no qualitative distinction, since it is known that people move in sleep. The activity of the muscles in sleep is often an automatic continuation of movements begun awake. This happens, *e.g.*, with people who fall asleep in making one particular movement; they continue the movement in sleep. For example, coachmen will go on driving, and riders will hold the bridle without falling off. Birds fall asleep standing, and aquatic birds go through the motions of swimming slowly with one foot when asleep, which shows that a group of voluntary muscles can be in a constant state of activity. In all these cases the muscular action is very like the contractions and continuous movements described on p. 77. Moreover, Henle has pointed out in his Lectures on Anthropology that muscular action is almost invariably present in sleep. Physiologists term this activity tonus; it enables a sleeper to assume and maintain a position which could not be controlled without muscular action. Slipping down in bed is a symptom of extreme exhaustion in typhoid fever.

Besides this, external stimuli may cause movements during sleep. It must not be assumed that they happen apart from mental activity. If part of a sleeper's body is uncovered, he will draw the cover over it; if he is tickled, he will scratch the

place. Even if these are regarded as physical reflexes without any accompanying mental action, which is not proved, the case is essentially different from the movements which children make in sleep, at command. If a child is told to turn over, it will do so without waking. This is an act which, as Ewald remarks, may be fairly compared with the phenomena of hypnosis, in which movements of the same kind, if greater in extent, are made at command.

Such movements are much more frequently caused by dreams. It is well known that children often laugh in pleasant dreams. A lady I know dreamed that she was blowing out a lamp; she made the corresponding movements with her mouth. She was awakened, and related the dream which had no doubt caused the movements of the mouth. Every one knows that children in especial often scream when they are dreaming of something exciting.

These movements are much more evident in the case of the persons we call somnambulists, sleep-walkers, night-walkers, with whom they are characteristic. The resemblance between hypnosis and somnambulism is so great that the name somnambulism[1] is used for both (Richet). Hypnotism is called artificial somnambulia, or, better, spontaneous somnambulia, since artificial somnambulia is really as natural as the other, as Poincelot insists. As a rule, somnambulia is divided into three stages according to the extent of the movements exhibited:—(1) that in which the sleeper speaks; (2) that in which he makes all sorts of movements, but does not leave his bed; (3) that in which he gets up, walks about, and performs the most complicated actions. In my experience the first two stages are found in persons of sanguine temperament who are certainly not in a pathological condition. It is not yet finally decided whether the third state appears under pathological conditions only, as many still assume. From my own experience I am inclined to think that it is occasionally observed when there is no constitutional weakness, especially in children. If we want to show these states, we can invariably

[1] Bentivegni and Wundt have very properly termed the condition somnambulia instead of somnambulism, and other authors, Hirschlaff, for example, have recently accepted the change. The termination "ismus" when applied to other foreign words signifies an occupation, science, or the like, and not a state or condition. In the following pages I shall adhere to the terminology employed by the authors I have mentioned.

do it with the healthiest subjects. As regards the movements in sleep, my own experience is that the persons who are most restless in natural sleep, who talk, or throw themselves about, are the most inclined to lively movements in hypnosis. In any case, the movements are also displayed in sleep. Consequently, the movements of subjects in hypnosis do not offer a fundamental contrast to sleep, especially when they are caused by suggested delusions of sense.

Spontaneous somnambulia is obviously very near akin to deep hypnosis, even if we maintain that the non-occurrence in hypnosis of those wild and illogical flights of fancy which occur in dreams constitutes a difference between the dreams of sleep and hypnotic suggestions, and it is exactly in spontaneous somnambulia that we find something analogous to hypnosis. It is evident, if we draw conclusions as to the contents of a somnambulist's dreams from his movements, that wild flights of thought may be absent in dreams; for surely we know that a subject of spontaneous somnambulia often commences an action which is quite logical, and carries it out in his sleep.

The fact that a subject in hypnosis can carry on a conversation is not enough to mark off hypnosis from sleep, as Wernich erroneously supposes, for many persons answer questions and obey in sleep (Lotze, Bérillon). According to my experience and that of others, certain persons easily answer in sleep when some one they know well speaks to them. A child will speak to its mother, and bedfellows to one another. A conversation is easily carried on when the waking person follows the sleeper's chain of thought and insinuates himself, so to speak, into his consciousness (Brandis). A lady I know, A., dreamed aloud of a person B. When Mrs. A.'s husband talked to her as if he were B., he was answered, but when he spoke in his own person he was ignored.

Finally, there are many persons who can hardly be induced to move in hypnosis, though they can be made to dream anything. Here the resemblance of sleep to hypnosis is particularly striking.

I hope that what has been said makes it clear that deep hypnosis need by no means be sharply distinguished from sleep.

As regards post-hypnotic suggestion, which is a very important phenomenon of hypnosis, we find that sleep presents

not only many points of resemblance but even apparently identical phenomena (Liébeault, Exner, Sante de Sanctis). Of course the effect of night-dreams upon the organism is not so easy to observe as the effect of suggestion, as most dreams are forgotten. Still there are exceptions. People who dream of a shot, and wake in consequence, continue to hear the reverberation clearly after they wake (Max Simon). Others after waking feel a pain of which they have been dreaming (Charpignon). Aristotle maintained long ago that many of our actions have their origin in dreams. To this class belongs a case reported by Sauvet and Moreau de Tours in 1844, in which a man in ordinary sleep had visions which gradually influenced him in waking, and induced him to abandon his home. Tonnini mentions a rather inconclusive case of a woman who was induced by a dream to do something. Of course, such phenomena are very difficult to observe, but it is very probable that dreams have an after-effect on even thoroughly healthy people. I will merely mention certain phenomena which resemble these — the dreams that are continued into waking life, which may be compared to continuative post-hypnotic suggestions. There are well-known vivid dream-images which are not recognized as dreams, and which are taken for reality even after waking (Brierre de Boismont). It is certain that even the most enlightened persons are influenced by dreams. Many are out of humour after having been annoyed by unpleasant dreams. The experiments lately made by Heerwagen have proved that persons who have dreamed much are in an unpleasant frame of mind the next day. I know of patients who are much worse after dreaming of their complaints; a stammerer will stammer more after dreaming about it. It is probable that erotic dreams belong to this class, because even when they terminate with the emission of semen, they stimulate, rather than inhibit, sexual desire. We find analogies with post-hypnotic suggestion everywhere. There are well-known cases in which persons have dreamed of taking an aperient with effect. Perhaps a case mentioned by Ferré may also be referred to here. A girl dreamed for several nights that men were running after her. She grew daily more exhausted, and the weakness in her legs increased until a hysterical paraplegia of both legs declared itself. Näcke insists on the legal significance of dreams. Hysterical girls have often declared that they had been raped

although they had only dreamed it; and in a similar way chronic drunkards have been led to accuse themselves or others of murder. According to Näcke, whenever a hysterical or neurasthenical or otherwise nervous person, or more particularly a drunkard, makes a definite statement we must invariably be on our guard, and bear in mind that there is always the possibility of a night-dream being continued in waking life. Näcke adds that Schmitt refers the acts of pyromaniacs to their dreams. To dream of fire has such an influence on them that they feel compelled to set fire to something when they are awake. Näcke, however, doubts whether this view is justified. In other cases the connection between a dream and the subsequent phenomena of waking life is different, and the phenomena could only be erroneously attributed to the dream. We shall see that there are pathological phenomena which are more readily perceived in dreams than in the waking state. I shall return to this point later on. Occasionally, there is a connection between the phenomena of waking life and dreams, but it has nothing to do with the phenomena described above. A young lady tells me that she is always in a bad temper in the morning if she has been awakened in the middle of a pleasant dream. The interruption makes her irritable.

Post-hypnotic suggestion finds a special analogy in those dreams which influence the first appearance of mental disease. Trenaunay has pointed out this connection in his work *Le Rêve prolongé*. Like Klippel, he calls attention to the case in which the dream is continued in waking life and there causes delirium by disturbing the normal course of ideas. Onirismus is the name given by Régis to those states in which a person is the victim of a prolonged dream. When onirism occurs at night the symptoms are disturbed sleep, nightmare, delusions of the senses—especially of vision—excitement, delirium. As a rule these phenomena disappear when the patient wakes, but in severe cases of onirism they continue after waking. Long ago, moreover, alienists were struck by the connection between dreams and mental disease. Griesinger relates cases of delirium which began in dream and did not show itself in waking life till later on; and in acute mania it has been observed (Esquirol) that the patient thinks he has been ordered in a dream to do something which afterwards he actually does. Hohnbaum reports that the first outbreak of mania often dates from a horrible and alarming dream, and that the predominant idea

is connected with that dream (Freud). Of course, it is explicable that in many such cases the dream was but a symptom of the disease; nevertheless, Sante de Sanctis has in recent times directed his attention chiefly to the question whether the dream might not be the cause of the mental disorder. He points out that Baillarger has observed cases of delirium arising from hypnagogic hallucinations, and that statements to the same effect have been made by Brierre de Boismont, Maury, Falret, and others. The Salpêtrière school, also, has adduced many facts which help to explain the connection between dreams and the delirium of an attack of hysteria. Chaslin, too, cites cases from his own experience and from the literature of the subject, and concludes that although it is true that every case of delirium does not originate in a dream, and is not necessarily influenced by one, yet such cases occur much more frequently than we think.

As the result of his own experience and a careful study of the material to hand, Sanctis has come to the conclusion that dreams and mental disorder are very closely connected etiologically. An exciting dream may so perturb the mind of a predisposed individual that he will appear distracted for a time, or a dream may set up a case of melancholia or a phase of alternating (circular) insanity. According to Guislain, a maniacal condition may have its inception in a dream, and an insane idea or visual hallucination be developed unconsciously from dream-life or the hypnagogic period. Something dreamed may be held to be an experience of waking life and thus become the origin of the preponderating idea in megalomania, persecutory mania, or religious mania. It seems certain that more than one psychopathic state can be called up by dreams.

In discussing the manner in which dreams induce mental disorders Sanctis distinguishes two cases. In the first a dream may act like a mental trauma. Here the mental disturbance which ensues is to be counted a traumatic neurosis or psychosis, or a state of exhaustion. We must bear in mind with Féré that even if the dream-images are false, the dream-feelings are true, and that the physical changes wrought by a dream are so great that a dreamer may be very powerfully influenced by them. The consequences of the excitement thus produced may easily persist in the subsequent waking state, even when the feeling itself has disappeared. Toulouse, consequently,

likens the way in which a mental disorder follows a dream to a powerful agitation. Sometimes, according to Sanctis, it is the cerebral exhaustion caused by a dream, and not the excitement during the dream, which is the etiological moment in producing mental disturbance in waking life. According to Sanctis, the second way in which dreams may produce mental disturbance extends over a much greater area. These are the cases in which the waking consciousness takes over the "dream-stuff." It is not necessary that the morbid state should immediately follow the dream; there may be an interval of greater or lesser duration, though in other cases dream-images may be continued into the waking state, just as we have seen happen in continuative post-hypnotic suggestions. Such cases have been described by Tissié, Manacéïne, Maury, Brierre de Boismont, Baillarger, and others. Cases in which a definite belief is acquired during a dream bear even a greater resemblance to post-hypnotic suggestion. Although the condition produced is as a rule only a passing one, it sometimes happens that the "belief" induced takes root in the waking state and leads to acts corresponding to its nature.

What has already been said should suffice to show the close connection between sleep and hypnosis, a subject upon which no mean light is thrown by the close resemblance of post-hypnotic suggestions to the after-effects of many a dream.

Although, as we have seen, deep hypnosis and ordinary sleep are closely related, and especially as regards dreams stand very close to one another, we must not therefore accept the identity of sleep and hypnosis. Dreams are only *one* symptom of sleep; we cannot, consequently, regard a comparison of dreams and suggested hallucinations as sufficient to demonstrate identity. I believe that even in deep hypnotic states we have clear grounds for concluding that the condition is not identical with sleep; for in order to assert an unconditional identification we must take into consideration not only the psychological image, but also the physiological symptoms. Now, we know that during sleep the pulse, the respiration, and other bodily functions are changed, and exhibit a greater regularity and prolongation. If we do not find this in deep hypnosis, it is a proof that the physiological condition in hypnosis is not identical with that in sleep. Francke finds, indeed, that pulse, respiration, and skin secretion show greater regularity, but his observations are not convincing, since he

tells us nothing more definite concerning the other symptoms of hypnosis as he observed it. In any case, however, Francke saw a remarkable resemblance between the curves of sleep and hypnosis. I have only seen in a few cases of hypnosis the slowing of the respiration and pulse observed in sleep; I believe therefore that the state in which this is not observed cannot, on the grounds already mentioned, be identified with sleep. Certain pathological conditions argue an even wider separation of sleep and hypnosis. The spasm of chorea and the tremulous movements in paralysis agitans cease in ordinary sleep. I have hypnotized patients suffering from these diseases without causing the movements or tremors to cease. Certainly other observers state that they have seen the spasm of chorea stop in hypnosis, and this circumstance is utilized as a proof of the identity of hypnosis and sleep; still, that is not sufficient. I have never asserted that choreatic twitches can never be brought to a standstill in hypnosis. My remark refers to typical chorea at its acme. In such a case I have never seen the spasms cease in hypnosis, any more than I have seen the tremors of palsy stop in the numerous cases in which I have attempted to bring hypnotic influence into play. That the spasm of chronic chorea, or of an acute attack which is subsiding, can be influenced in hypnosis is beyond all manner of doubt; but the same result is often obtainable when such patients are awake. If, however, sleep and hypnosis are to be considered identical, then the spasms of typical chorea which cease in ordinary sleep ought invariably to stop in hypnosis also, and without any form of suggestion being employed. Up to the present I have never seen this happen. If other authors have obtained other results, any objection to the identity of hypnosis and sleep fails as far as these cases are concerned, but does not in the least justify us in assuming a general identification of sleep and hypnosis.

From various sides (Bernheim, Delbœuf, Max Hirsch) it has been asserted that the hypnotic subject is not asleep, but that he believes he is asleep, that the illusion of sleep produces the increase of suggestibility. A somewhat different, and perhaps, more correct view, has been expressed by Schrenck-Notzing. He divides hypnoses into those in which there is no sleep, those in which there is an illusion of sleep, and those in which actual sleep is present.

The similarity of the means used to induce sleep and

hypnosis is often insisted upon as a proof of their identity. But a distinction must be made. It is said that monotonous stimuli induce both sleep and hypnosis. Purkinje, therefore, thought that Braid's methods would produce sleep. But we should never conclude an identity of states from the identity of their causes. We should observe whether their symptoms are identical. I have seen cases in which the subjects fixed their gaze, but did not concentrate their attention. The subsequent state was an ordinary sleep, out of which the subjects awoke when I made verbal suggestions to them, however softly I spoke. It is the same thing when we wish to decide whether a tedious speaker hypnotizes his audience. Many people grow sleepy or even fall asleep, in such a case. I consider the state one of ordinary sleep produced by the subject failing to concentrate his attention. If he concentrates his thoughts on the speaker, he will not go to sleep; in this case his state of partially strained attention much resembles hypnotism. If the state is strongly marked, negative hallucinations may arise, for instance, with regard to noises, as in hypnosis. In many works on oratory, even in Cicero's, this effect of a fine speech is referred to.

Similarly, those states of loss or disturbance of consciousness, induced by vertigo—*e. g.*, by spinning round quickly, should not be reckoned as hypnoses. Erdmann has identified the states induced by vertigo and by tedium in his well-known ingenious manner.

Once more I must repeat my own view: it does not matter how the states are produced; the point is whether their symptoms are alike.

Hypnosis has often been compared to mental disorder as well as to sleep. Rieger and Semal, as well as Hack Tuke (so far back as 1865), called hypnosis an artificially induced mental disorder of short duration. In the first place I would remark that it is of no consequence what hypnosis is called; a name goes for nothing. Even in therapeutics this is a matter of no moment; otherwise we should have to refrain from using morphia because it is a poison, and because the sleep induced by morphia is an effect of poisoning. Freud is right when he says that meat does not lose its flavour when a rabid vegetarian calls it carrion; why should a mental influence, such as we have found hypnosis to be, lose its value or interest because it

is sometimes called a mental disease? A remark of Griesinger shows how capriciously all such terms are used; he thinks a somnambulia of short duration is a sleep, and a longer one a mental disorder.

I should raise no objection to our calling hypnosis a mental disorder if we also regarded sleep and dreams as such. And we find that when alienists wish to discover analogies to mental disorder they always have recourse to dreams; but no investigator has maintained that in order to lose one's sanity it is only necessary to go to sleep. To illustrate the resemblance which dreams bear to mental disorders, Freud, following Radestock, points to statements made by various philosophers. Kant called the insane dreamers who were awake; and Schopenhauer declared that a dream was a short mania, and mania a long dream. Wundt, in his *Physiological Psychology*, expresses the view that when we dream we experience all those phenomena met with in a madhouse. For this reason even Radestock concludes that mania, an abnormal morbid phenomenon, should be considered an exacerbation of the periodically recurrent dream-state. Trenaunay points out in a work I have already mentioned that a number of alienists have expressed similar views. Moreau de Tours has written on the identity of the dream-state and mental disorder, and Delasiauve has observed dreams which approached *confusion mentale*. Schüle considers mania the analogue of the physiological dream-state. Delirium of toxic origin—*e.g.*, alcohol, has recently been classified among dreams by various writers—Laségue, Klippel, Régis. The two latter lay special stress on the toxic origin of many forms of delirium. Régis, for example, considers the delirium of infectious diseases the delirium of a dream caused by toxic action: for him it is a kind of *état second*, analogous to the hypnotic state. Like somnambulists, these sleepers, as our authors call them, on waking often retain but a confused recollection of what has happened: sometimes the whole dream is forgotten. Régis finds even greater justification for comparing toxic delirium with the state of consciousness in hypnosis in the fact that although the memory of the events of such delirium is lost on the patient's waking, it can nevertheless be restored in hypnosis.

It follows from the foregoing considerations that dreaming and mental disorder are analogous in many respects, and from this point of view there can be no objection to our comparing

hypnosis to mental disorder. But the most dissimilar mental disorders have been compared to hypnosis, which shows what confusion there is about it. For example, Rieger and Konrád say that hypnosis is nothing but an artificial madness. Meynert maintains that it is an experimentally-produced imbecility. Luys compares it to general paralysis of the insane, Bevan Lewis to stupor. These different comparisons show the want of unanimity among the authors, for the forms of mental disorder we call imbecility and mania are as unlike as a pea and a rose, which are both plants, but of utterly different kinds.

When hypnosis is thus compared to mental disorder it is generally forgotten that susceptibility to suggestion is the chief phenomenon of hypnosis. But it is a mistake to think that susceptibility to suggestion is an essential phenomenon of mental disorder; if it were, mental disorders could be cured by suggestion, but this is hardly ever possible. By means of suggestion in hypnosis, forms of hypnosis may be induced which resemble mental derangement—*e.g.*, spontaneous mania, or melancholia attonita, besides forms of imbecility, etc. But we can also induce paralysis and stammering by suggestion, and yet hypnosis is not a state of paralysis or of stammering. We can suggest pain in hypnosis, yet hypnosis is not a state of pain. And how the light stages of hypnosis in which only motor effects are caused by suggestion can be called states of mental disorder is not clear to me, unless a person is to be called mentally unsound simply because he cannot open his eyes. But even the susceptibility to suggestion which exists in such mental disorders as delirium tremens (Moeli, Pierre Janet), or the katatonia of Kahlbaum (Jensen), must not be without further ceremony identified with the susceptibility we find in hypnosis; and we have just as little right to reckon mental disorders in which there is a fixed idea as hypnosis merely because of the fact, to which Kornfeld and Bikeles have called attention, that such fixed ideas in cases of megalomania are very easily influenced in hypnosis. We often see that a fixed idea can be guided as we please. But none of these states has anything to do with the susceptibility to suggestion met with in hypnosis. I need only say "Wake!" to the hypnotized subject, and at once the state ends; but there is no mental disease which can be ended at a moment's notice like hypnosis.

It is no new thing to see hypnosis brought into connection

with, and regarded as, an artificial hysteria or neurosis. Demarquay and Giraud-Teulon have pointed out analogies, and Charcot later on called his three stages a *grande névrose hypnotique*. Dumontpallier also thinks that hypnosis is an experimental neurosis. I would make the same remark upon this as upon the mental disorders. In hardly a dozen cases has Charcot called up the complete type of a neurosis, and specially of hysteria, by suggestion. This was comparatively easy in his cases of *grande hystérie*, because phenomena which are common in the subject in waking life are more easily induced in hypnosis than others (Grasset). Effertz's description of hypnosis as induced hystero-epilepsy is to be met with the same objection. I repeat, it would be easy to suggest stammering, and then draw the conclusion that hypnosis is a state of stammering. We might possibly recognize a somewhat closer relationship between the hypnotic state and *one* of the phenomena of hysteria—viz., somnambulia, provided, of course, that we consider the latter a hysterical phenomenon. I have already pointed out the striking resemblance of spontaneous somnambulia to deep hypnosis, the only form of hypnosis of which there can be any question here. Nevertheless, as I have already explained, the assumption of Charcot's pupils (Guinon, Stéphanie Feinkind) that spontaneous somnambulism only occurs in hysteria, or perhaps epilepsy, has not been established. The spontaneous somnambulia of persons who are neither hysterical nor epileptical presents exactly the same analogy to hypnosis as does the somnambulia of the hysterical. For this reason any comparison between the somnambulia of an attack of hysteria and hypnosis has no significance.

Other states have also occasionally been compared to hypnosis. I may mention catalepsy, a disease, or symptom of disease, in which the limbs keep any given position; and lethargy, a strange state of sleep, in which artificial awakening is difficult or impossible. That the unconsciousness in lethargy, as in Charcot's lethargic stage, is only apparent, is shown by an observation of Encausse, who in a case of this kind was able to produce awakening by suggestion. Thomsen's disease, in which a contracture follows voluntary movement, is also compared to hypnosis, and so are epileptical disturbances of consciousness. I may also mention the phenomena of intoxication by alcohol, chloroform, ether, opium, and haschisch, which are

often compared to hypnosis on account of the delusions of sense which occur in them. Haschisch intoxication has lately been described in detail by Régla. There are many phenomena in this state which we call hypnosis, and suggestibility can go so far as to produce a change of personality. Régla has made experiments on himself, and on one occasion, for instance, believed that he was a pregnant woman on the eve of confinement. Narcolepsy must also be mentioned. In this disease there are periodical attacks of sleepiness. It has been described by Gélineau, Rousseau, Ballet, and others. Certain cases of what Drosdow calls *morbus hypnoticus*, whose resemblance to hypnosis is unmistakable, may be included in this tolerably undefined narcolepsy. These states might be regarded as auto-hypnoses, as intimated on p. 40. Vizioli has published an account of an auto-hypnosis in which he succeeded in making even post-hypnotic suggestions. Naturally, the terminology is very arbitrary in these cases; these states might be ascribed to spontaneous somnambulia arising directly out of waking life, and not in sleep, as usual. The famous case of Motet, which was so important from the legal point of view, would then belong to this class. A man committed a criminal act in a state of auto-hypnosis, to which he was subject. On Motet's recommendation he was acquitted. A case of Dufay's is nearly identical. It would be extremely illogical to call hypnosis a morbid state merely because a morbid imitation of it is found in many forms of *morbus hypnoticus*. It would be as great a mistake as if we were to take yawning for a disease because there are people who suffer from attacks of yawning (Ochorowicz). Cases of disturbance of consciousness (delirium, partial amnesia) after accidents have also been published; they would be described by some (MacDonald, Wright) as cases of traumatic hypnotism, although there could be no question of any typical hypnotic phenomena. *Lata* often resembles hypnosis (Bastian, O'Brien, Forbes). The word *lata* properly means the sufferers from this complaint, not the disease. The disease is found among the Malays; the patient imitates every movement made in his presence, as in "fascination." The same thing has been seen in Maine among the "Jumpers" (Beard), and in Siberia, where the sufferers are called "Miryachit" (Hammond). Obersteiner mentions the interesting observation of Kaschin that a troop of Siberian soldiers repeated all their commander's orders

without obeying them, and even imitated the threats which he uttered against them.

I lately had an opportunity, at the large Arab hospital at Cairo, of seeing a case which doubtless should be mentioned in this connection. The subject was a negress, some thirty years of age, then in the hospital. Besides her own negro tongue she understood some French. The chief symptom of her disorder was that while she sat passively in the same place all day long she repeated whatever was said to her, whether in her own language, in French, or in any other language of which she knew nothing whatever. I made trials with German and other languages; even difficult sentences she repeated mechanically, the first time usually with omissions, but when she had again gone over one, every syllable was clearly uttered. She also imitated movements to a certain extent. If I put out my tongue she did the same, if I showed my teeth she also showed hers, if I clapped my hands she clapped hers, though after a longer interval. It was, however, generally first necessary to attract her eye before performing any action.

Once more, the chief feature of hypnosis is increased susceptibility to suggestion. By means of this we can induce counterfeits of all sorts of diseases, which appear identical with the real thing. But none the less, hypnosis should not be identified with these diseases. The two characteristics of hypnosis are suggestibility and the power of ending the state at pleasure. We do not find them united in psychoses, nor in neuroses; but we find them in sleep, in which many dreams are induced by suggestion, and from which the subject can be awakened at any moment by an external stimulus. Although no identification of hypnosis and sleep would be justifiable on the above grounds, I must again point out that they are closely related, at least so far as hypnoses of the second group are concerned.

The various phenomena of hypnosis have also been observed in normal waking life, and this makes a comparison of the hypnotic states with other abnormal states considerably more difficult. For example, a symptom which A. shows in hypnosis he does not show in his normal state; but it may be observed in B.'s normal waking life. People differ greatly in their susceptibility to suggestion in waking life; I have spoken (p. 64) of suggestions in waking life from which hypnosis

cannot be concluded. Besides which, a number of phenomena of suggestion, which are generally regarded as a peculiarity of hypnosis, have been found in waking life. Braid, the American electro-biologists, Herzog (1853), Heidenhain, Berger, Richet, Lévy, Bernheim, Beaunis, Liégeois, and Forel are among those who have made observations in this field.

These phenomena are shown in waking life by subjects who have been hypnotized as well as by those who have not. Contractures, paralyses, dumbness, and other kinds of motor disturbances can be induced by suggestion in the waking state. According to some authors, it is even possible to induce hallucinations without hypnosis.

Nevertheless, many of the experiments, and particularly the conclusions drawn from them, seem to me to have two defects. Those who talk of suggestions in the waking state (*suggestions à veille*) forget, first, that sleep is by no means always indispensable for many hypnotic suggestions. Authors often confuse hypnosis with sleep. We have seen that the light hypnotic stages do not much resemble sleep; consequently, we must not conclude that a state of contracture, etc., is, or is not a hypnosis because it resembles sleep or not. The second point which these authors generally overlook is this: they think that hypnosis is excluded in these cases of waking suggestion, because none of the usual methods of inducing hypnosis have been used. But the methods are not absolutely necessary for the induction of hypnosis. We cannot make the question, whether hypnosis is present or not, depend upon the means employed; we must always consider the state and its symptoms separately. For if we take a certain degree of suggestibility, loss of memory, etc., for a symptom of hypnosis, nothing remains but to regard as hypnoses many states which are frequently described as suggestions without hypnosis. The chief phenomenon of hypnosis is, as we see, that a certain accepted idea has a tendency to lead to a movement or a delusion of the senses, etc. We have further seen that the experimenter can change the subject's dominant idea very quickly—*i.e.*, he can suggest one thing quickly after another. If, then, we can do the same without apparently previous appearance of hypnosis, we must call the state a hypnosis all the same, particularly if there is subsequent loss of memory.

In reality, a hypnosigenic method is employed, because in such experiments the subject generally remembers an earlier

hypnosis, and the idea of hypnosis is enough to induce it. Therefore we often need only to repeat a suggestion made in an earlier hypnosis to cause a new one (Bentivegni, Marin).

The fact that paralyses, contractures, etc., can be quickly produced by suggestion in this new hypnosis shows that it is as real as the first. In the deeper states, when delusions of sense can be induced, loss of memory usually follows. The changed expression of the subject's face also shows there is hypnosis. Finally, the presence of a real hypnosis is proved in many cases by the *rapport* between subject and experimenter.

For the reasons above mentioned, I think we should call many of these states true hypnoses, not suggestions without hypnosis. The school of Nancy, and particularly Liégeois and Beaunis, have only partially acknowledged this. They thought many of these states were intermediate forms between hypnosis and waking, which they identified with the *veille somnambulique* described above (p. 167).

It must not, of course, be concluded from what has been said that all these suggestions only succeed in hypnosis. I am far from saying that. My explanation aims only at pointing out that there may really be hypnosis in the so-called *suggestions à veille*, though none of the usual methods (*e.g.*, those of the Nancy school) have been employed to bring it on. Moreover, we should always endeavour to prevent suggestions in waking life, and especially to make delusions of the senses impossible.

It is often difficult to decide whether there is hypnosis or not, because the question is sometimes more one of subjective perception than of objective proof. I may refer here to a case of Carpenter's mentioned by Finlay. A gentleman placed his hands on the table, and for half a minute directed his attention to them. When the suggestion was made to him, in decided tones, that he could not remove his hands, he was in fact unable to do so. I have frequently observed the same thing, especially after hypnotic exhibitions. I have, for instance, seen inability to move a limb, to withdraw the outstretched tongue, or to close the mouth. New suggestions, however, only succeed after the attention has been drawn to them for some time.

There are even delusions of the senses without hypnosis, sleep, or mental disorder, when circumstances influence the mind in a particular way. The common hallucination of smell

is an example. People often imagine that they still smell things which have been removed. Delusions of sight are just as common. Many people have taken trees for men when walking through a wood in the twilight. Goethe's self-induced hallucinations of sight are well known. Delbœuf also describes a waking hallucination of sight; he thought he saw his dead mother, but corrected his impression by reason. If there are even delusions of the senses without hypnosis, it is evidently difficult to argue the presence of hypnosis from a single symptom.

The following are the chief points to be considered in settling the question whether a suggestion is made in hypnosis or not:—1. Of what kind are the suggestions? Are they of such a kind that they rarely occur normally? 2. After one suggestion has succeeded, can other suggestions be made as quickly as in hypnosis, or is a long preparation necessary for each suggestion? The quick success of the following suggestion would be in favour of hypnosis. 3. After the suggestion has succeeded, can the subject prevent a further suggestion by an act of will, or not? If he cannot, it favours the supposition of a hypnotic state. 4. Is there *rapport?* That is, can the subject be influenced by only one person, or by anybody? *Rapport* favours hypnosis. 5. Are there bodily symptoms of hypnosis? 6. Are the events subsequently forgotten? Loss of memory also favours the supposition of hypnosis.

The many transitional states between waking life and hypnosis will often make the question difficult to decide; none of the points above mentioned will alone suffice to settle it. The cases of fascination and analogous states of normal life mentioned on pages 73 and 74, show that there is no very sharp line of distinction between hypnosis and waking life. From this we can see, on the one hand, the resemblance of many of the phenomena of hypnosis to those of waking life, and, on the other, how difficult it is to decide exactly where hypnosis begins.

States resembling, or perhaps identical with hypnosis, are also found in animals, and can easily be experimentally induced. The first experiment of this kind is usually attributed to the Jesuit father Kircher;—the so-called *experimentum mirabile Kircheri.* Kircher described this experiment in 1646; but according to Preyer, it had been made by Schwenter several

years earlier. A hen is held down on the ground; the head in particular is pressed down. A chalk-line is then drawn on the ground, starting from the bird's beak. The hen will remain motionless. Kircher ascribed this to the animal's imagination; he said it imagined it was fastened, and consequently did not try to move. Mach mentions Kircher's experiment in his book, *Erkentniss und Irrtum*, as a proof of the ease with which an experiment may be erroneously interpreted. For a long time the chalk-line was held to be the essential part of the experiment, producing some far-reaching mental action on the part of the hen; later on, however, it was shown that the experiment could be successfully performed by merely holding the hen down on the ground, and the chalk-line was consequently but of secondary importance. Czermak repeated the experiment on different animals, and announced, in 1872, that a hypnotic state could be induced in other animals besides the hen. Preyer shortly after began to interest himself in the question, and distinguished two states in animals —cataplexy, which is the state of fear, and the hypnotic state. Regnard observed that when dynamite explosions took place in the water, fish that were not in the immediate neighbourhood of the charge would lie as if dead, though a slight touch would restore movement. Laborde found the same true of trout, which could thus be caught. Heubel, Richet, Danilewsky, Rieger, Gley, Verworn, and Micheline Stefanowska, besides the authors mentioned above, have occupied themselves with the question.

Most of the experiments have been made with frogs, crayfish, guinea-pigs, and birds, and by Verworn with the hooded snake. I myself have made many with frogs. This much is certain: many animals will remain motionless in any position in which they have been held by force for a time. There are various opinions as to the meaning of this. Preyer thinks many of these states are paralyses from fright (cataplexy), produced by a sudden peripheral stimulus. In any case, they recall the catalepsy of the Salpêtrière, also caused by a strong external stimulus. It is said a sudden Drummond limelight produces the same effect on a cock that it does on hysterical patients (Richer). But in general, the external stimulus used with animals is tactile, as in suddenly seizing them. Heubel thinks that these states in animals are a true sleep following on the cessation of the external stimuli, and Wundt seems to agree

with him. Rieger has shown that the frog will remain rigid when upright, if kept from falling, as well as when lying on its back. The hind leg of a frog lying on its back may be pulled out, and the animal will not draw it in again as it usually does. Richet, however, says that it is drawn in again at once if the spinal cord is divided below the *medulla oblongata*. It is interesting that when a "hypnotic" frog is placed in a certain position it will at first move after a short time, but the more often the experiment is repeated the longer the frog lies without moving. I have seen frogs lie on their backs in this way for hours, and have even often seen them die without turning over. The deeper the state is, the less the animal responds to external stimuli; it ends by not moving to tolerably loud noises, or even stimulation of the skin. Danilewsky made a series of experiments, from which he concluded that there were regular changes of reflex excitability; but Rieger was unable to confirm this. According to Danilewsky, when the cerebral hemispheres are removed the frog assumes cataleptoid postures, and further that the rotatory movements caused by injury to the semi-circular canals of the ear disappear in hypnosis. Harting's experiments also deserve mention; after repeated hypnotic experiments with fowls he observed hemiplegic phenomena in them, according to a communication by Milne-Edwards to the Paris Academy of Sciences. I may here recall the fact that Lodder once reported a case in which he considered that hypnotic phenomena were associated with an attack of cerebral hemiplegia.

In 1898, Verworn propounded a physiological theory of his own in a work in which he dealt with the "so-called hypnosis of animals." He has formed the opinion that the states observed in animals have nothing to do with hypnosis in man; at the same time he certainly considers suggestion of chief import in the case of human beings. In his opinion, we must base our conclusions upon the attitude of the animal and the condition of its muscular system, which alone are decisive. For any particular position in which an animal may be placed, it assumes a characteristic attitude corresponding to the reflex which tends to restore it to a normal posture. The muscles which participate in this reflx action remain in a state of tonic contraction. The same characteristic complexus of symptoms occurs when the animal's cerebrum has been removed, and from this very fact Verworn concludes that there can be no

question of suggestion. But the phenomenon is made up of two components. The chief of these, and that which causes the characteristic symptoms, is the tonic state of excitation in that cerebral sphere which controls the reflex in question; a subordinate component is the inactivity of the motor spheres of the cerebral cortex, as shown by the absence of spontaneous efforts to rise. The latter phenomenon is brought about by the excitement produced by the experimenter's manipulations, and is also made manifest by acceleration of the respiration and heart-beat. This view, therefore, has something in common with that held by Preyer on paralysis from fright. Micheline Stefanowska, who has made numerous experiments on frogs and considers a frog in a state of inanition particularly predisposed to hypnosis, holds an essentially different opinion. According to her, all these states are hypnoses, and she even thinks she can recognize the symptoms of Charcot's stages in the case of frogs; yet her experiments in this direction, and their results, have not produced any convincing impression on my mind, in spite of their interest and value and the many new facts concerning the life and physiological characteristics of the frog, which they have brought to light.

Another series of observations which were chiefly made for practical purposes may be mentioned here. They also may be regarded as hypnotic phenomena. For example, the so-called "Balassiren" of horses, introduced by the cavalry officer Balassa. This process has been introduced by law into Austria for the shoeing of horses (Obersteiner). It consists chiefly in looking fixedly at the horse, just as in "fascination." Other authorities—Glanson, for instance—have stated that restive horses may sometimes be checked by hypnotism. Lépinay gives a detailed account of hypnotism in respect to horses, and thinks that hypnotic influence can be brought to bear upon them. In particular, he thinks that vicious and restive horses can be soothed by music, and in this he is supported by Guénon. I shall deal later on with the case of "Clever Hans," which recently proved such a painful pitfall for Stumpf in Berlin, Hans' feats being attributed to hypnotism. Bruno is said to have hypnotized cats and pigeons, and Stoll believes that in other ways we work by suggestion on our domestic animals. He regards the influence of the rider on the horse or the mule, especially when particular tricks have to be combated, as suggestive, since scarcely anything can be accomplished

here by brute force. A kind of counter-suggestion appropriately brought to the animal's intelligence would thus oppose his idiosyncrasies, which are of the nature of auto-suggestions. The numerous experiments of Wilson should also be mentioned; he is said to have hypnotized a number of animals—elephants, wolves, horses, etc., in London, in 1839. Fascination is used by many animal-trainers, whose very first principle is to stare fixedly into the eyes of the animal they wish to tame. Many think that the charming of small animals by snakes is fascination. Hart and Lysing, however, believe that the animals are not hypnotized, but that, as the snake gazes at them, they hypnotize themselves. A case is reported in the *Revue de l'Hypnotisme* in which the opposite occurred—a snake was said to have been hypnotized by a cat. The story comes from Madras.

Of course, in many of the cases related above, imagination plays so great a part that absolute credence is not to be placed in all the details. There is a fable of Lafontaine's in which a number of turkeys took refuge in a tree so as to escape from a fox. The latter so fascinated the poor birds, which were watching him, by the glitter of his tail which he waved in the moonlight that they fell into his jaws. Thomas Willis tells a similar tale of a fox circling round a tree in which a cock was perched. The cock kept his eye on the fox, but finally fell from the tree and was devoured. In a communication to a French scientific journal, Guimbal ascribes all such cases to fascination.

Liébeault and Forel think that the winter sleep (hibernation) of animals is an auto-hypnosis; and so, perhaps, is the strange sleep of the Indian fakirs, which sometimes lasts for weeks and months (E. L. Fischer).

A number of trustworthy witnesses and authors (Jacolliot, Hildebrandt, Hellwald) tell us even stranger things about these fakirs, which set any attempt at explanation on the basis of our present scientific knowledge at defiance. Hildebrandt, among other things, relates that he saw a fakir sitting in a Hindoo temple; he was crouching down with his left arm stretched upwards; the arm was dead and so perfectly dry that the skin might easily have been torn from it. Another fakir had held his thumb pressed against the palm of his hand till the nail had grown deep into the flesh. It is said, besides, that some of these people can make plants grow very quickly.

Görres mentioned this. These fakirs are also said to have been apparently buried for weeks and months, and yet have returned to normal life. Kuhn holds this to be an undoubted fact, the condition of the fakirs being that of hypnotic catalepsy. Of course, these things must be listened to with sceptical reserve. Yet even so scientific an investigator as Hellwald thinks that though no doubt there is a great deal of jugglery, yet some of the phenomena remain at present inexplicable. Lœwenfeld thinks that the observations which have been made show that in the so-called Yoga sleep the respiratory and circulating functions are not nearly as much in abeyance as has been assumed hitherto. Still, I cannot help doubting the genuineness of the Yoga sleep in the case of those natives who have been on exhibition in Europe. I have it on good authority, that two of these people who were supposed to be asleep, and "strictly watched by a committee of scientists," were playing cards with a third man a good way from the place in which the committee thought they were "sleeping." The agent admitted this swindle to my informant.

Many other observations recorded by ethnologists and travellers show striking resemblance to auto-hypnotic conditions. Stoll records many such facts; as, for instance, the auto-hypnotic state of the shamans or priests of various Siberian peoples, as recorded in the travels of Pallas and Gmelin. An Armenian physician, Vahau Artzronny, mentions a disorder which attacked a whole race, the Ezidi, in Armenia. When any of the people were brought to a spot and a circle drawn round them with a stick, they would rather die than step out of it. There would seem to be some suspicion of a superstition in this case, but it may have been a matter of fascination.

I have purposely made but brief mention of these matters and of the experiments with animals; details would take me too far.

CHAPTER VI.

SIMULATION.

As is well known, hypnosis has only lately been generally recognized as such. Sinnett has pointed out for how long a time the most childish objections were raised against it. For instance, when Clocquet performed a painless operation on a patient whom he had magnetized, Mabru immediately raised the objection that, so far from the patient having been magnetized, he was in a state of coma which had nothing to do with the "passes." The opposition even went so far as to accuse persons anæsthetized by hypnosis of simultation. But in the end the most obstinate doubters—or, at least, those of them of any account—were compelled to admit that there is such a thing as the hypnotic state. Consequently, when discussing simulation I have not to consider whether there is such a thing as hypnosis at all, but whether there is simulation in any particular case.

Those who believed in hypnosis were for a long time regarded as deceivers or deceived. It was occasionally less harshly supposed that a man who busied himself with hypnotism must be suffering from some loss of mental balance. Latterly, Mendel, in a popular lecture, has expressed himself in this sense. He thinks that many men have given proof of their own pathological state in their autobiographies. Mendel certainly mentions no names, but judging from the context it is evident that he refers to those investigators who have described their own personal experiences of hypnosis. His charge would apply to such men as Forel, Bleuler, Obersteiner, as well as to Wundt, who described his own case of auto-somnambulism, and to many others. If Mendel's diagnosis is correct, then all the men mentioned must be looked upon as pathological specimens whose opinions are not to be taken seriously. Such a method of attacking unpleasant adversaries is a mere invention on Mendel's part, and cannot be too severely stigmatized. Even a newspaper of very moderate views put down Mendel's method of controversy as outside the bounds of legitimate scientific discussion. The newspaper in question added the ironical remark that it was strange that Mendel, who considered all persons who carried out hypnotic experiments to be of unsound mind, should have related how he had performed many such experiments himself.

In the first place, I think that simulation is much rarer than is generally believed. It has been too much the habit to look for one physical or objective symptom which could not be simulated, and settle the question of fraud from its presence or absence. And yet this is exactly the opposite of what is generally done in judging of mental states. When we want to diagnose a case and decide whether it is insanity or not, no authority on mental disorders would suppose fraud simply because some bodily symptom was absent. He will consider and weigh the case as a whole. Even when each symptom taken separately might be fraudulent they would be weighed against one another and a diagnosis formed from them. If the doctor also finds some symptom which cannot be simulated, he will weigh this too, but he will not conclude fraud from its absence. It is true that in this way the conviction may be only subjective, or rather it will be clear only to those who have studied mental disease. The outsider may often be able to raise the objection that this or that symptom may be feigned; but no specialist would allow himself to be influenced by this.

If we apply this to hypnosis, which is also a mental state, it follows that only he who has studied hypnosis practically is in a position to diagnose it, although many a person who has no knowledge whatever of hypnotic experiment considers that he is able to judge of hypnotism, express an opinion on it, and demand consideration for his views. Kron and Sperling have very rightly contested this assumption. It is not correct to diagnose fraud in hypnotism from the absence of a certain physical symptom. Even if each separate symptom may be feigned, the experienced experimenter will diagnose by summing up the different symptoms and comparing their relation to each other. It is satisfactory if he finds an unfeignable symptom besides; this is an objective proof, convincing even those who have no practical knowledge of hypnosis. But we must bear in mind that objective physical symptoms are more seldom found in hypnosis than in mental diseases. The first is a transitory mental state, in which objective physical change is less likely to occur than in mental disorders, which last for months and years.

However, we must, of course, try to find physical symptoms in hypnosis. Many authors have done so, Charcot in particular. Not only did he lend the weight of his name to the acceptance

of hypnotism, but he is also to be credited with having searched for objective symptoms. We must, however, remember that the school of Nancy sought for objective symptoms and found them, though different from Charcot's; I mean the blisters, etc., produced by suggestion. It is, consequently, erroneous to consider that the essential difference between the two schools is to be found in the question of simulation. To exclude fraud we look for symptoms which cannot be voluntarily simulated; it is indifferent whether these are produced by suggestion or not. There are symptoms which are produced by suggestion, and which are independent of the subject's will. Herein lies the chief difference between the school of Nancy and that of Charcot. The Nancy school believes that *all* the symptoms are caused by suggestion, even those independent of the will; while the school of Charcot finds bodily symptoms which are independent of the will and of suggestion—*e.g.*, the increased neuro-muscular irritability met with in the lethargic state. Consequently, *suggestion* is the main point on which the two schools differ.

That there is a practical distinction between the questions of suggestion and fraud is shown by a whole series of observations. The case of Siemerling teaches us this. His subject was hemianæsthetic both with regard to sight and feeling—*i.e.*, the power of sight was limited on the side on which the skin was without feeling. The field of vision was concentrically narrowed, so that anything beyond a certain distance from the point on which the eyes were fixed could not be seen. Now, in hypnosis the sense of feeling on the hemianæsthetic side was restored by suggestion, and as soon as this happened the eye on the corresponding side became normal, without direct suggestion. Westphal and Siemerling thought this an objective proof of hypnosis; and, as a matter of fact, I think that we must agree with them.

We are told that in this case the power of sight was restored by indirect and not direct suggestion, the sense of feeling being first of all rendered normal by suggestion. The whole process was probably one of indirect suggestion such as I have described on p. 67. It is possible that in a case of Krafft-Ebing's, objective symptoms were produced by suggesting mental paralysis. Such cases have been described by Charcot's pupils. When the subject is told that his arm is paralyzed, vasomotor disturbances set in, which we may consider objec-

tive symptoms. But even if we assumed—not unjustifiably—that there could be no question of indirect suggestion in all these cases, but rather of processes which cannot nowadays be included in the idea of suggestion, the objection would be without meaning; for we know that objective symptoms can be produced by suggestion. Let us take a case similar to Siemerling's, one of concentrically narrowed field of vision. If we say to such a subject in hypnosis, "Your sight is now normal," and it becomes so, then the restoration of sight which cannot be feigned is evidence of an objective symptom produced by suggestion. It is the same in the case of the vasomotor disturbances already mentioned, and especially where organic changes are produced by suggestion.

From this we see that the difference between the two schools is not the question of simulation, but that of suggestion. Both schools have sought for objective symptoms. The difference is this: they either think they have discovered different objective symptoms, or that they must attribute the symptoms to a different origin. According to the Nancy school everything observed in hypnosis, even the objective bodily symptoms, results from the influence of suggestion. Charcot's school, on the other hand, thinks that the special symptoms it deals with are physical reflexes, and have nothing to do with suggestion.

With regard to the different symptoms which have to be considered in discussing the question of simulation, Charcot and his pupils lay great stress on the curves of muscular contraction and respiration in the cataleptic stage. There is no essential difference in the duration; a cataleptic person cannot hold his arm up longer than an impostor. But when the curve-tracings from the raised arm and the respiration are noted, there is an important difference: the impostor soon shows that he is tired by irregularity in the arm and respiration curves; the hypnotic subject, on the contrary, breathes calmly and evenly from beginning to end, and there is no perceptible trembling in his arm, which sinks gradually and uniformly. Other people say that a cataleptic posture is sometimes maintained for a very long time, and therefore offers an objective proof.

Charcot mentions increased neuro-muscular irritability as a particular characteristic of lethargy. It is not to be supposed that a person can thus bring single muscles, or groups of muscles supplied by single nerves, into contraction without constant practice; but these contractions could only be important if they appeared instantaneously from the first, because it is undoubtedly possible by practice to cause isolated groups of muscle to contract. Moreover, we must remember that this symptom upon which Schaffer places great value, is met with in hysterical patients when awake, and is not, therefore, unconditionally objective for hypnosis.

Charcot does not think that the contractures induced by stimulation of

the skin in the somnambulic state are of much value in deciding the question of simulation, and, in fact, they might easily be brought about by voluntary action.

Let us now consider those symptoms which must guide us in discussing the question of simulation. In the first place we must notice how the eyes close; how this takes place, and how the subject tries to open them is of importance. This closing of the eyes is difficult to describe. The gradual falling of the lids, and the action of the muscles of the forehead when opening the eyes, in a way like that after sleep, are important details; and the rolling upwards of the eyeballs must also be noted. The fibrillary twitching of the eyelids is, on the contrary, of no importance, as it often happens without hypnosis. In cases where the eyes are open their expression is most important, as the look is often blank and meaningless. The mask-like expression and the attitude of the subject are often characteristic also. The subject moves his limbs slowly and heavily when commanded, but there are cases in which this symptom is wanting, and the movements in particular are quick and lively.

In the case of the "dream-dancer," Magdeleine G., which has already been mentioned, the oculist Ancke made very detailed observations, which convinced him that the hypnosis was genuine because the symptoms could not have been produced voluntarily. The subject's right eye converged at an angle of 45°, and both pupils were contracted to the maximum; the lids failed to close for 11½ minutes—the duration of the hypnosis, during which the eyes remained open and the pupils fixed. Ancke does not think that any one could keep up such a degree of convergence of one eye for 11½ minutes—the left eye kept straight all the time.

The expression during sense-delusions is also very important. Every one knows how difficult it is to place oneself in a particular position so that the expression, the attitude and the actions should correspond to the idea. To represent such a situation as naturally as possible is the greatest art of the actor, but is very seldom altogether realized on the stage; but it is still more difficult to change the mood in a moment and pass from one situation to another in a few seconds. The hypnotic, however, often does so easily. It is astonishing that outsiders should regard this very ability of accomplishing a complete and rapid change a sign of fraud, as a competent judge once did at Vienna (*cf.* p. 149). This change is, surely, one of the most difficult things to accomplish, and it would be wonderful

that all the suspected persons should devote themselves to the thankless part of fraud, when with such talents for acting a very different career would be open to them. The expression of pain, the smiles, the chattering of the teeth and shivering at different suggestions of pain, pleasure, cold, etc., would be no easy task to the supposed impostor. The waking in many cases is just as characteristic: the astonished face with which the subject looks round as if to find out where he is. His behaviour in post-hypnotic suggestion is likewise important. The impostor generally exaggerates like a person feigning madness. In spite of the variability of all the symptoms of hypnosis, there is a certain conformity to rule in its development. The impostor usually accepts all suggestions very quickly, while the experienced experimenter knows that susceptibility to suggestion increases with a certain uniformity. Analgesia to slight feelings of pain is a favourite form of fraud; and although an unexpected pain causes the usual reflexes in the face and eyes, the impostor will declare that he felt no pain. It is the same with sense-delusions, where the suggestion generally requires to be emphasized before it takes effect. The impostor usually exaggerates here also.

But, apart from the symptoms of Charcot's stages, certain abnormalities of the muscular system have been utilized in judging of fraud. On the face of it, it would hardly be expected that abnormalities which are supposed to exclude fraud should be capable of being induced by suggestion. If we consider that an idea suggested to a hypnotic differs in no respect from a voluntary idea of a person who is awake, it should be difficult to conceive that the idea in the first case should produce objective changes which do not appear in the second. And yet that is the case. Even in waking life an idea awakened by another person has by no means the same effect as one voluntarily produced. The difference is probably most perceptible in pathological cases. A patient suffering from the fixed idea that he is insane can generally be pacified if the doctor assures him that he is not insane. In such a case it is not merely the doctor's *dictum* which prevails, because it happens often enough that the patient is fully aware that his own idea is erroneous, and that the doctor in assuring him of his sanity is only repeating a statement which he has already made on previous occasions. Nevertheless, the repetition of the assurance again pacifies the patient. The case of blushing which I

mentioned on page 64 is to the point. When A. told B. he had got to blush, the latter frequently did so, although the voluntary idea of blushing did not cause B. to do so. Tickling, also, is a well-known example of the difference in the results produced when a simple stimulus is self-applied or applied by somebody else. Let somebody else tickle you, and you laugh; tickle yourself, and you do not. A number of other examples could be cited, and all tend to show that when another person calls up an idea in my mind the result is different from that which would be produced by the self-same idea voluntarily induced by myself. Experience shows the same to hold good in hypnosis. Superficial observation led to the conclusion that objective changes could not be brought about by suggestion, but a closer study has now shown that conclusion to be erroneous. But, as I have already shown, there may be other processes at work in hypnosis besides suggestion. It is possible that the physical symptoms which are sometimes associated with suggested paralyses, and which I have dealt with in page 72, belong here.

At all events, experience teaches us that suggestion in hypnosis can bring about muscular phenomena which cannot be produced voluntarily. For example, the cessation of the staggering gait in locomotor ataxy, which Berger described, and I also have observed, and other like phenomena. All other abnormalities of the muscular system may be used as arguments against simulation. If a person holds out his arm for a long time without trembling to any extent, this may be held to exclude fraud to a certain extent. It is also possible to produce such abnormalities at times by special methods. A heavy weight placed in a hypnotic's hand will often be held longer and more steadily than it would be possible for a waking man to. As Wilkinson and Braid have pointed out, directly the hypnotic shows signs of giving way, any tremors can be suppressed for some time by suggesting that he has only bits of cork in his hand. Similarly, I have seen a hypnotized person, whose arm was beginning to get tired and trembled, hold it out quite still directly it was suggested that his arm was resting on a cushion or some other support. All these points must be considered when judging of fraud.

Binet, Féré, Parinaud, and others have made particular investigations on the sense-delusions of sight. They say that a prism doubles the hallucinatory object as it would a real one; and in hallucinations of colour, the

complementary colour is said to be seen afterwards, just as in a normal act of vision. But Charpentier and Bernheim have very properly submitted these statements to criticism. They have shown that the hallucinatory object was only apparently doubled. The subject first saw some real object doubled by the prism, and concluded from this that the suggested hallucination should be doubled also. Such a conclusion can, of course, be drawn unconsciously. For this reason, it may also happen that the doubling of the sense-delusion is secondary. In any case, the great point is that the prism only produces a doubling when a real object is seen through it. If this is not the case—if, for example, the experimenter is in a dark room, or if he shows the subject a perfectly blank white screen—the doubling does not occur. According to Charpentier and Bernheim, the experiments with complementary colours were not more exact; and the same is the case with other experiments of Binet and Féré in colours, from which they drew the conclusion that in suggested perceptions of mixed colours the effect was the same as with real optical images.

We must, consequently, give up any thought of using these experiments when judging of fraud, even when we have to deal with uneducated persons who know nothing of the doubling of images by prisms, or of complementary colours, or of mixed colours.

The phenomenon presented by the pupil of the eye, which Binet and Féré mention, seems more valuable. In suggesting a hallucination—*e.g.*, that of a bird—the suggested approach of the object causes contraction of the pupil, and vice versâ. At the same time there is often convergence of the axes of the eyes, as if a real object were present. But it must be remembered that some persons are able to produce this phenomenon by an effort of will, as Hack Tuke and Budge pointed out long ago. Lefèvre quotes the experiments of Beer de Boon, who was able to cause his pupils to contract by imagining that he was in a place where the light was very bright. Piltz, also, has recently published a work on the influence of the will on the pupil-reflex; he lays particular stress on the fact that the idea of light produces contraction, of a dark object dilatation of the pupils. It follows that the phenomenon presented by the pupil of the eye must only be used with great caution in judging of fraud.

Bernheim lays great weight on the analgesia of hypnotic subjects. If a completely analgesic subject is touched with a faradic brush he shows no trace of pain. There are no impostors who could repress the expression of pain under these circumstances, particularly if the contact were unexpected. But we must consider that such a high degree of analgesia is very rare in hypnosis. The anæsthesia of the mucous mem-

branes—*e.g.*, of the membrane of the nose—with regard to ammonia, is to be tested also.

There is no need to say that certain rare phenomena—*e.g.*, secretion of tears and sweat, changes in the heart's action, and organic changes, produced by suggestion are of the greatest value.

Finally, I wish to call attention to the absence of those movements which I should prefer to call the movements caused by tedium (Langweiligkeit). As is known, a waking man is unable to retain any posture for a long time, even when all his muscles are relaxed. In the latter case the movements cannot be caused by fatigue of particular muscles; it is rather that when one position is long maintained, a lively feeling of discomfort is produced, that is subjectively felt as tedium. This, it seems to me, leads to certain irresistible movements, the movements of tedium. Their absence is a strong evidence of hypnosis, and I think this an important, but almost entirely unknown, symptom. They are best observed when the subject has been left for some time to himself, without any notice being taken of him.

I have as yet only spoken of such symptoms as take the form of bodily functions; but according to Pierre Janet these bodily symptoms are of much less importance to the question of simulation than the mental ones; the memory in particular. Gurney also held this view. The assumption from which these authors started is that there is loss of memory on waking from hypnosis, and that consequently the subject remembers nothing that has happened during the state. Now, this loss of memory is to be used to decide the question of fraud.

I tell X., whom I have hypnotized, that when he is going to bed he is to dip a towel in warm water and wrap it twice round his throat. When he wakes he seems to remember nothing about what I had said to him while he was hypnotized; upon which I repeat the command, but omit the doubling of the towel. When I ask him what he is to do on going to bed, he answers, "I am to dip a towel in warm water and wrap it twice round my throat." It will be seen that I gave the order differently during and after hypnosis; yet X. repeats the command as it was given in hypnosis.

According to the views of Pierre Janet and Gurney, this would very likely be a case of fraud; for X., who had apparently completely forgotten everything after waking, mentions the one point which was omitted in the second command, and of

which he could have no knowledge unless he remembered what had happened during the hypnosis. But must we really consider this a case of fraud? I believe not, and I appeal to a long series of experiments with perfectly trustworthy subjects, in whom I often observed objective bodily symptoms. A second question to be considered is, How is the proceeding brought about? Here, of course, we must first of all think of the association of ideas mentioned on page 122, by which in this, as in other cases, a hypnotic event may be reproduced after waking. It may happen that the subject adds the word "twice" to a certain extent automatically, and without remarking it; in other cases he may make it consciously, as a previously forgotten idea may be suddenly called into consciousness.

The following case is somewhat similar. I hypnotize X., take hold of his scarf and disarrange it. In spite of this he sits quite still, and the hypnosis is undisturbed. But directly he wakes he puts his hand to his scarf to straighten it, although he is not supposed to remember what had happened. I would not have taken even this as a proof of fraud without further evidence, for the action in question could be just as well performed automatically, because of the subconscious idea that the scarf was awry.

Such cases naturally make one think of fraud, and the sharper the distinction between the subject's chains of memory, the more are we justified in accepting the hypnosis as genuine. On the other hand, we must not straightway discern a proof of fraud in acts which may possibly have been performed automatically.

From two points of view, the somatic and psychic signs of hypnosis which have so far been mentioned have only a relative value in deciding the question of fraud. In the first place, we are never justified in concluding fraud from the absence of one or all of these signs. For example, in some cataleptic postures there are considerable tremors, in many cases there is no analgesia, etc. In the second place, we must always consider whether any symptom might not be produced voluntarily without hypnosis. Here we must remember that the symptom can, perhaps, be acquired by practice, and also that there may possibly be a special capacity for the voluntary production of this sign.

The important point, whether an apparently objective symptom does not occur without hypnosis, is often overlooked; for it is not yet decided whether by practice some persons might not produce all the above-mentioned phenomena without hypnosis. There is, perhaps, no symptom of hypnosis which has not already been observed without hypnosis. I have already mentioned that neuro-muscular hyperirritability is said to be found in hysterical patients who have not been hypnotized; and the most strained cataleptic attitudes can be produced by gymnasts by means of practice. When Hansen, the well-known hypnotizer, was appearing in Vienna, no small stir was created by a certain Klein who declared he could voluntarily imitate rigidity of the whole body. I may further mention that cases have been reported of persons who could influence the action of their hearts without a change of breathing; though, according to Beaunis, a distinction can be found here: the hypnotic obeys suggestion at once, while out of hypnosis a short time must always elapse before the will can exercise its influence. Local flushings are another symptom apparently impossible to simulate; yet Mantegazza says that at one time in his life he was able to induce local reddening of the skin simply by thinking intently of the spot; he even adds that wheals sometimes appeared. It has often been asserted that people can perspire at any place they please. Delbœuf says that he can influence the secretion of saliva by his will or ideas. It is well known that this last is under the influence of ideas which some persons can call up when they please.

I have purposely made these remarks, because a superficial observer will often take a symptom to be objective which a more careful investigator would not. Of course, we must always take these symptoms into consideration, because they have a relative value; but no more attention should be paid them than scientific criticism permits.

On account of their practical importance, I shall speak of other symptoms which, according to experience, are often wrongly considered by outsiders as proofs of fraud. The outsider believes that hypnosis must invariably present an ideal and complete picture, and he consequently assumes there is fraud when some symptom appears which does not fit into that picture.

Let us take the laughter of hypnotic subjects. In the light

stages the subject is quite aware that he is playing a somewhat absurd part—*e.g.*, he makes all the movements of eating an apple when told to, although he really has nothing to eat. He feels compelled to make the movements, but knows quite well that the situation is ridiculous; therefore it is not odd that he should laugh. But there is often a trace of consciousness even in deep hypnoses; the subject separates himself, so to speak, into two parts, one of which acts the suggested part and the other observes it and laughs. We have observed something similar in dreams; sometimes we dream and yet feel we are dreaming.

I have already spoken of the trembling of cataleptics. The subject sometimes makes quite unexpected movements which interrupt the suggestion. I stretch out a subject's arm and suggest that he cannot move it. It remains as I placed it. But now a fly settles on the subject's forehead, and he moves his arm at once to scratch the place. This and similar movements are of common occurrence in hypnosis, and have nothing to do with fraud. Scratching when one is tickled is sometimes a kind of impulsive act. We often see a person who is awake attempt to resist the desire to scratch some spot, but finally succumb to the impulse, and we can understand that a hypnotic subject may be similarly influenced. It is true that the experimenter's assurance that the subject cannot move his arm suffices to inhibit voluntary flexion of the limb; but when a stronger impulse, like itching, acts upon the subject in a manner that stimulates movement, it becomes evident that the suggested inability to move can thereby be very easily removed. We may consider the whole effect produced, the resultant of two forces—not absolute magnitudes—of which now one, now the other, preponderates. In many cases the scratching is not an irrepressible impulsive movement, but a pure reflex action, as rapid and unconscious as in waking life. Here, also, it is easy to understand that if at the moment the itching begins the subject is not thinking of the order given him, the new and sudden impulse to move causes movement. I have seen people put their hands to their faces when they sneezed, as we habitually do, though their hands had just previously been made motionless by suggestion. Besides, many movements which have been prevented by suggestion become possible when the subject does not think of the suggestion. If a subject is forbidden to say "*a*," he

can often use it in conversation, and pronounces words containing it without hesitation; he only cannot say it when he thinks about it (Laverdant, Hack Tuke, Max Dessoir).

I will further point out that the eyes sometimes open very quickly. I have frequently seen this, and can safely assert that it happens in genuine hypnoses. An impostor will also often open his eyes when he thinks he is not observed; the hypnotic subject does it sometimes, whether he thinks he is observed or not.

A series of similar phenomena must be included here. I say to a hypnotized subject, X., "You are a rope-dancer, and are on the rope." He believes it, and I pretend to cut the rope, on which he falls down; but he falls so as not to hurt himself. This, of course, is the natural result of a perfectly normal, mechanical, nearly unconscious process which is always going on in us. We always use our hands to shield ourselves when we fall. This habitual mechanism works on in hypnosis regardless of any suggestion. Hysterical paralytics—and drunkards, too, at times—for this reason seldom hurt themselves when they fall. Hack Tuke told a subject he was dead; the man invariably fell, but without hurting himself.

Forel once had two dormice under observation during their winter sleep. He took one of them and put it at the top of a fir-tree, and as soon as the sole of the sleeping animal's paw touched a thin branch of the tree a reflex action was set up, and the claws grappled the branch just as they would have done instinctively in waking life. Forel then let the dormouse hang by one foot, and the animal gradually fell fast asleep again. The muscles of the foot by which it was hanging slowly relaxed; its paw extended slowly until it was only hanging on ly the extremity nearest the attachment of the claws. Forel thought that the dormouse must fall. Instead of that, a kind of instinctive shock seemed to flash through its nervous system, and another paw seized the nearest underlying branch, so that the animal only took one step downwards. The whole scene then commenced over again. Once more the animal fell asleep and its paw lost its grip, and once more another paw stretched out and grasped a lower-lying bough. The dormouse thus gradually descended the tree until it reached the foot, where its cage was, in which it remained asleep. Forel repeated the experiment several times with both animals, and the result was always the same. Neither of the animals ever fell.

I have cited Forel's observations in detail because they show that reflex action and automatism persist during hibernation. From this it is perfectly evident how wrong it would be to assume fraud because a hypnotic performs automatic movements.

I must again direct attention to those sense-delusions in which, as I have already demonstrated, a dim consciousness

of the true situation persists. In this way situations are created which arouse the suspicion of fraud—as, for example, in the case mentioned on page 145, where a subject fought with a suggested enemy, one of the spectators, but took pains not to hit him.

Further, a complicated suggestion may be misunderstood or only partially retained, in which case it will be carried out imperfectly. As memory is the first condition for the success of a suggestion, it follows that the more highly cultivated a person's memory is, the more likely will he be (*ceteris paribus*) to carry out a suggestion. If a post-hypnotic suggestion is imperfectly remembered it will be imperfectly carried out, for hypnosis does not produce supernatural results. Obvious as this must appear, I have yet heard the existence of hypnosis doubted because such mistakes have been made. To a man whom I have hypnotized in the presence of A., B., C., and D., I make the post-hypnotic suggestion that when A. speaks he is to say "Ha!" when B. speaks, "He!" when C. speaks, "Hi!" and when D. speaks, "Ho!" As the command is only given once the task is not an easy one, and it is not surprising that the subject is confused and makes the wrong exclamation to each person. To the class of imperfectly realized suggestions a case of Joire's also belongs. He suggested to a person that the name Marie was written on a piece of paper. When the paper was turned upside down he seemed to see the letters backwards—eiraM. One would, however, have expected not only the word to appear backward, but the letters upside down also. Obviously, neither hallucination nor illusion was sufficiently developed in the subject to permit of this. And we must remember that these things depend on strength of memory, and on the strength with which sense-delusions make themselves manifest.

There are, moreover, certain transitional forms of hypnosis which suggest fraud, but unjustifiably. A subject will go through every movement I command him to make. I tell him to eat an imaginary beefsteak, and he goes through all the motions of eating a steak just as if one were before him. I tell him to drive the dog away, and he kicks as though to do so; but when I ask him where the dog has gone, he replies that there was not any dog there. So, too, when I ask him how the steak tasted, he says he has not had one. To the outsider these things suggest fraud, but in reality we are deal-

ing with a case of hypnosis of the first group (*cf.* p. 59). The subject *had* to move as told, but there was no sense-delusion. We may form a correct judgment of these states in two ways: (1) by following the experiment; (2) by examining the subject's memory after hypnosis. From what subjects have told me, I think that sense-delusion must be excluded. The careful observation of subjects points to this. The movements were not of that rapid and immediate nature associated with hallucination; they were much more the outcome of compulsion. Even the facial expression of a subject is no criterion of simulation. When a subject says, "There is not any steak there," or shakes his head, it is enough to prove that any movement is the result of suggestion. But these very contradictions seem to confirm the outsider's suspicion of fraud.

In other cases the subject is so passive that he makes no opposition to any suggestion made by the experimenter. Should the latter suggest a hallucination, such as the presence of a tiger, the subject, when questioned, declares that one is there, but he does not run away, show fear, or behave as though the animal was really present. In this case there is neither hallucination nor any act corresponding thereto, only an affirmative answer, and the subject subsequently remembers this fact. Still, outsiders often confuse passive hypnosis with simulation.

In other cases the patient is only too ready to do all he is told, and this easily leads to simulation being suspected. It is sometimes difficult to distinguish whether the subject is acting from complacency, or whether he is deprived of will-power. At the most, we can only ask a subject to exert his whole will against that of the experimenter, and thereby prove the futility of his, the subject's, exertions and his loss of will-power. There are, of course, cases in which the subject's anxiety to conform to the wishes of the experimenter spoil the experiment; but, as Hirschlaff rightly points out, that does not justify the assumption of fraud. In this connection Hirschlaff mentions the experiments which have been made to produce blisters by suggestion. But, as Hirschlaff points out, although accessory rubbing was suspected in the case of one subject, that is not sufficient to justify the assumption of fraud. We must always remember that the subject may think he is in duty bound to do anything to bring about the desired result.

Hirschlaff's distinction is certainly quite right from a psychological point of view, although it may not be of great importance in deciding in any particular case whether the organic lesion was of mechanical or mental origin.

In discussing abnormal hypnoses, Hirschlaff also very properly points out that an outsider may easily take the phenomena presented to be the result of fraud and acting. For example, a subject when hypnotized for the first time will accept every suggestion made, but in subsequent hypnoses he will refuse to carry out any experiment against the performance of which he has prepared himself by pre-hypnotic auto-suggestion. When told his right arm is powerless, he replies, "No, my arm is not going to be stiff; and I am not taking part in any experiments." When the subject is given a piece of paper and assured that it is a chocolate tablet, he replies, "That is a piece of paper; I knew quite well beforehand that you would repeat this experiment, but I have made up my mind not to be taken in again." Naturally, no one but an outsider would conclude that such remarks point to simulation.

Finally, I must mention that a subject will sometimes confess to imposition, or to having acted to please others. Such a confession must be judged with caution. Many who have made hypnotic experiments have observed that subjects will often say after the hypnosis that they have been pretending, though their actions were really compulsory. Much has been written on the significance of confession in criminal cases, and Ernst Lohsing has recently published a small monograph on the subject. He has come to the very proper conclusion that a prisoner should never be found guilty of a crime merely because he has confessed to it. If the criminal law is satisfied that confession to a crime never justifies the assumption that crime has been committed, it would be in the highest degree erroneous to consider a subject's assertion that he has simulated hypnosis as a proof of fraud on his part. Lohsing points out that in judging the value of a confession in criminal cases we must examine the motive, taking care at the same time to distinguish between the motive and that which underlies it. The same procedure must be followed in hypnosis. For example, a patient receives hypnotic treatment from a doctor, but without success; he is annoyed because the doctor demands payment for his trouble, and declares that he only pretended to

be hypnotized. Another person is vexed because he has been a mere plaything in the hands of a professional hypnotizer before a number of spectators. He had gone on to the stage to unmask the hypnotizer, but had proved himself one of the best subjects. He is so annoyed and so anxious not to admit that his actions on the stage were compulsory, that he asserts he was pretending all the time. In other cases a confession may be extorted by compulsion or fear. We can easily imagine such a case as the following:—A. is in a position of authority over X., but fails in every attempt to hypnotize him, whereas B. succeeds at once. This annoys A., and the next time he meets X. he brings his authority into play, partly intentionally and partly unintentionally, and thereby leads X. to confess to fraud. Here, of course, the dread that a denial of imposture might bring about unpleasant consequences may also play a part. Take another case: The head physician of a hospital fails to hypnotize a patient, but another doctor succeeds in doing so. The next time the patient visits the hospital he is so afraid—possibly quite unjustifiably—of all sorts of little unpleasantnesses if he does not gratify the head physician's vanity, that he denies that the other doctor had ever hypnotized him.

In cases like those cited above, the subjects tell untruths unconsciously; but it is much more interesting from a psychological point of view when untruthfulness is the result of self-deception. Here the self-deception is the same as we have found in some cases of post-hypnotic deception. The subjects think they could have acted otherwise if they had so pleased (F. Myers). Heidenhain mentions such a case: a doctor said, after the hypnosis, that he could have opened his eyes if he had wanted to; but when the hypnosis was renewed he could no more help himself than on the first occasion. I have made a number of similar observations myself. One case was that of a doctor who at first almost invariably stated after hypnosis that he could have behaved otherwise, only he did not wish to open his eyes, etc.; but in each fresh hypnosis his will was inhibited. Finally, he himself became aware of his loss of will-power. In another case, I hypnotized X. at least ten times before he would admit that the suggested paralysis of his arm had really made him unable to move it; he previously believed that he had so behaved to please me. I may here mention that many a man who has done something stupid when drunk

for the first time, is often convinced on the following day that he could have controlled himself if he had wished to. The self-deceptive process here is obviously just like that in the other cases mentioned.

All this makes it evident how difficult it is to decide the question with regard to fraud. It seems to me to occur relatively more often with children, but the transition from simulation to true hypnosis is so gradual that even an experienced experimenter is sometimes uncertain. For example, when a subject shuts his eyes to be obliging, it is not the same thing as if he shut them to deceive; or he shuts them because he is tired of fixing them on something, but could open them by a strong effort, though he keeps them shut because it is more comfortable. It would be a great mistake to identify this with simulation. Others do what the experimenter wishes, to please him, but not to deceive him. This, as I have already mentioned, is not pure fraud either; we can only speak of that when there is the deliberate intention to deceive.

There is yet another complication: people in hypnosis sometimes pretend exactly as insane persons do. Thus, a hypnotic will say he sees something when he does not. It is, naturally, difficult to say where deceit begins and ends in such a case; but, generally speaking, practice will enable us to judge the mental state of the subject with some certainty, or at least great probability. It occasionally happens that the most experienced deceive themselves or are deceived; the most experienced alienist or neurologist is in the same case. But that is no reason why we should deny the reality of hypnosis. Obersteiner justly observes: "A group of morbid symptoms, such as an epileptic fit, may be so exactly reproduced by clever simulation that even the most skilful expert (Esquirol, for example) may be deceived. And yet, unfortunately, we must still unconditionally recognize the existence of epileptic fits." The fear of being deceived has prevented many from interesting themselves in the subject; yet no other principles need be followed than those which guide us in the study of other mental states—psychopathic states, for example. Each case must be treated with scientific reserve, as mental cases are treated. We must not make impossible demands in order to exclude imposition; to do so would be to overstep the bounds of scientific scepticism, and would, in truth, only display an

unscientific mind. I have heard a "cultivated" man, who thought himself scientifically sceptical, say, when watching a hypnotic subject, that he would only believe in the reality of the hypnosis if the subject could see through a non-transparent substance—*e.g.*, if he could see through a man as if he were a piece of glass!

CHAPTER VII.

THE THEORY OF HYPNOTISM.

We have learned in the preceding chapters that the phenomena of hypnosis are extremely complex, and the question now is, "Can these phenomena be explained?" We must not demand too much in this connection. To explain a hitherto unknown thing, we must trace it back to what we do know. If we adhere to that, and also bear in mind that we know nothing of the real nature of our mental processes, it is evident that any explanation of hypnosis must be a limited one. Our knowledge of mental processes is confined to certain concomitant phenomena and their symptoms, and these are often but inadequately apprehended, while the real nature of such processes is debarred us. Under these circumstances we must be satisfied by such an explanation as may be got by demonstrating that hypnosis presents phenomena parallel to those of non-hypnotic life. We must settle what are the true, and what the apparent, differences between the two states, and then we must find a causal connection between the peculiar phenomena of hypnosis and the hypnosigenic method employed. An example will make this clearer. I will suppose that we want to find an explanation of a hypnotic negative hallucination of sight. We must first of all find an analogous phenomenon in a non-hypnotic state. If we find a case in which, without hypnosis, an object is not perceived, though the eye must have seen it, we must then ask what is the difference between this phenomenon and the same phenomenon in hypnosis. We shall then find that in hypnosis objects are not perceived only when the experimenter forbids the perception; but that to forbid the perception of an object in waking life would be to ensure its being perceived. This point of difference must be kept in view for a proper explanation. It will be explained by the existence in the one case of

a peculiar state of consciousness—dream-consciousness; and we must then ask how hypnosigenesis explains the formation of this dream-consciousness. It is a conspicuous flaw in many theories of hypnotism that they attempt to explain more than can be explained in the present state of our knowledge, and more than we are justified in demanding of them. An explanation of hypnosis is not called upon to explain the real nature of the process by which an idea is aroused; that is a problem for psychology in general to solve. Similarly, we have no right to demand that an explanation of hypnosis should explain the real nature of dream-consciousness as well. Certain facts must always be taken for granted. Only superficial considerations could lead any one seeking to explain hypnosis to expect an explanation of the facts just mentioned. In studying psychological questions I always start from certain postulates, and never have anything to do with theories based on the theory of cognition. Similarly, any one who desires to explain hypnosis must accept certain postulates which he may, with a tranquil mind, leave to be explained later on by some other branch of psychology.

I think we can now explain many of the hypnotic phenomena, if "explanation" is taken in the above sense. About fifteen years ago I attempted to explain post-hypnotic suggestion in this way. In any case, such numerous analogies to the phenomena of hypnosis have already been found that it has been rescued from the domain of mysticism and occultism once and for all. We need no longer think the methods of hypnotism incomprehensible, as was the case formerly. This has, to an extent, been brought about by more careful methods of observation, by means of which it has been demonstrated that waking life, sleep, and other states present so many phenomena analogous to those of hypnosis, that the latter can hardly be said to present a symptom exclusively its own. Much progress has also been made by following the method recommended by Obersteiner; *i.e.*, by studying the transitional states between normal life and hypnosis. In this way we have found many more connecting links with normal life than was originally expected. Self-observation when practised by such intelligent investigators as Wilkinson, Bleuler, Forel, Obersteiner, North, August Heidenhain, Wundt, Döllken, Marcinowski, Straaten, Frau Bosse, Vogt, and Frau Vogt, has done much to further our comprehension of hypnotic phenomena.

Fr. Fuchs, it is true, has asserted that hitherto hypnotic experiments have only proved successful when the subjects have been priggish young men or young women, and not strenuous male adults. This shows how thoroughly Fuchs is acquainted with the literature of the subject. If he includes such men as Obersteiner, Bleuler, Forel, Wundt, etc., among the prigs, then an impartial observer will not have much difficulty in deciding who is the prig when he has to choose between Mr. Fuchs of Bonn and the gentlemen I have named.

We must never forget when endeavouring to explain hypnosis, that a uniform explanation is impossible, because the term hypnosis includes states that differ very much from one another. If we consider the remarks on the classification of hypnoses made on page 59, it at once becomes evident that Max Dessoir's two groups represent two totally different states, and Hirschlaff excludes the first group altogether from the category of hypnoses. At all events, when a subject retains consciousness and self-consciousness, there is an essential difference between his being unable to perform certain movements and his believing that he has been transported to another situation differing both as to time and place from the one in which he really is. But we could easily produce other types, apart from the two states just mentioned. For this reason I think it is better to discuss the chief symptoms of hypnotical phenomena simply, than to attempt to discover a uniform explanation for them all. I shall follow this plan and devote my attention to (1) the phenomena of suggestion as regards voluntary movement; (2) positive and negative delusions of the senses; (3) *rapport;* (4) the phenomena of memory; (5) post-hypnotic suggestion.

We shall understand the different symptoms of hypnosis much more easily if we recognize certain facts in the mental life of human beings. They are of immense importance to psychology, physiology, medicine and jurisprudence, as well as to hypnotism. The facts to which I refer are—(1) Men have a certain proneness to allow themselves to be influenced by others, and at the same time to believe much without making conscious logical deductions; (2) a psychological or physiological process tends to appear in a man if he believes it will; (3) a man's susceptibility to influence based on the two preceding facts is often specially marked in respect to some particular person; (4) capability of being influenced in this way is generally increased if a person has learned by previous experience that he has a tendency in this direction;

(5) besides the ordinary state of waking consciousness there is another, which we may term dream-consciousness; it is brought about by certain definite physiological and pathological conditions, is capable of developing those characteristics of the human mind which we are now discussing very considerably, and is specially noticeable for its influence on sense-perception.

Let us begin by considering the first point. There are people who believe that they can escape external mental influences; but they are wrong. Life is full of such influences, and they will work as long as there is mental activity among men. The desire for society, the necessity of exchanging opinions, show our proneness to influence others or be influenced by them. In this way we often attempt to convert a scientific or political opponent.

In such a case, of course, we generally endeavour to attain our object by producing logical proofs. But, that men have a tendency to believe things without complete logical proof cannot be doubted for one moment. We will call this quality credulity. There is no man who believes only what has been logically proved to him. A conscious logical conclusion is hardly ever the immediate result of a sense-perception, yet we form our judgment of external objects as if that were really the case. Most people confuse the subjective idea of an object with the object itself (Spencer); whereas the latter, the "thing in itself," as Kant calls it, is quite unknown to us. But apart from the delusions of judgment which we make with regard to our sense-impressions, we find that human credulity ranges over a very wide field. It is emotional emphasis, in particular, that causes the processes which exert an influence on credulity. We easily believe anything we want to hear, and it is a matter of common experience that a patient is much more ready to believe the quack who tells him he will get well than the doctor who declares his malady incurable. This is to a great extent the result of the influence of the quack, who, knowing full well the weakness of his patients in this respect, promises them recovery from even absolutely incurable diseases, a promise which no honourable medical man could possibly make. Also, the constant repetition of an assertion facilitates belief in its accuracy. The success of those clever advertisements from which no one escapes, least of all the very people who consider themselves secure against such allurements, makes this particularly clear;

and the history of hypnotism shows plainly what a power the constant repetition of an assertion has. Twenty years ago most people believed that there was really no such thing as hypnotism and that any belief in it was attributable to self-deception on the part of the experimenter or fraud on that of the subject. Since that time a complete reaction has set in and opinion has changed, influenced to no slight extent by the constant accessions to the ranks of those investigators by whom the reality of hypnotism was originally maintained. Doctors and others have changed their opinions about hypnotism, not because it has been definitely demonstrated to them, but because they have been influenced by constantly hearing and reading the same assertions about it. To this we may add that faith in authority has also played a definite, though not exclusive, part. For even after Charcot, Heidenhain, and a number of other authorities had admitted the reality of hypnosis, many people still believed the whole thing to be a swindle and that those investigators had been deceived. Certainly, the personality of any one who wishes to influence another's belief plays a special part. I shall return to this point later on.

I will now proceed to discuss the second of the facts of human mental life given above—*i.e.*, the physiological and psychological action of belief. Carpenter, Hack Tuke, and other English investigators long ago ascribed great importance to strained expectation—"expectant attention," they termed it. The second of these factors must be strictly separated from the first. It is quite possible to believe something without the action corresponding to that belief taking place. A man may believe that he has taken an anodyne powder without the pain for which it was taken subsiding. For belief to be effective a particular mental predisposition is certainly requisite. We cannot analyze this predisposition in all cases; it may be present on one occasion, absent on another. We find exactly the same thing apart from hypnosis—*e.g.*, in pathology. I have already mentioned that people who suffer from dread of blushing are particularly liable to blush when they most dread they will do so. Other pathological cases present similar phenomena. At present we are only able to establish the fact that in ordinary life—*i.e.*, outside hypnosis, there is occasionally a predisposition to be influenced psychologically and physiologically by belief in something.

A few examples will illustrate this effect of belief. It often occurs that people vomit if they think they have taken an emetic, although they have not taken anything of the kind. A person who goes to bed believing he has taken a sleeping powder often falls asleep, even if the powder only contains some inert substance. These phenomena are particularly noticeable in pathological cases. Hysterical paralysis is often cured at the exact moment the patient expects. Many apparently mysterious effects may be explained in this way. Hysterical patients can occasionally foretell an improvement in their paralysis. That recovery should follow need not surprise us if we remember the foregoing remarks, for the connection is totally different from what believers in the gift of prophecy think. The hysterical patient is cured at a particular moment because he expects to be, and the prophecy is thereby apparently fulfilled. Pathological phenomena also frequently occur when expected by the patient—for example, *impotentia coëundi* of mental origin, stammering and other disorders of speech. Let me here call attention to a mistake that is frequently made: people confuse the expectation of a state with the wish for it. No patient wishes to be impotent, but he becomes so because he expects the calamity; it is the same with stammering.

We can readily understand that the fulfilment of an expectation may be hindered, especially by impediments of organic nature. However much a sufferer from severe myelitis may expect his paralyzed legs to move they will not do so, because the anatomical changes in his spinal cord present an impediment which cannot be overcome by expectation. There are other impediments which hinder expectation from taking effect. But this does not disprove the fact that belief has a tendency to produce an effect. The efficacy of a tendency may be impaired, but the tendency as such will remain unaffected.

The influence of belief—the phenomenon we have just been discussing—is not confined to its effect on the voluntary muscular system. Its range is much wider than could be gathered from the examples already given. Menstruation, for instance, is affected by it. Forel mentions that there are certain popular methods of retarding the catamenia. In one town many of the young women tie something round their little finger if they wish to delay menstruation for a few days in order to go to a ball, etc. The method is generally effectual.

It has also been observed that belief influences the organs of sense under particular circumstances. The following case of Carpenter's is related by Bentivegni:—A judicial disinterment was to be made; the grave was opened and the coffin raised; the official present said that he already smelt putrefaction; but when the coffin was opened it was found to be empty. Here expectation caused a distinct sense-perception. Archibald tells of a teacher who described various perfumes to the children in his class, and then told them that he would sprinkle something about the room. Although he only used pure water, 95 per cent. of the children declared that they could smell scent. Somewhat older children were not quite so susceptible to suggestion, though not altogether wanting in susceptibility. Yung has made a series of experiments and has proved that the sense of touch and the sense of temperature, particularly the latter, are subject to delusion, and that certain perceptions occur when they are expected without external stimuli. I myself have often repeated the following experiments of Braid, Weinhold and others:—I blindfolded certain persons, doctors among the number, and then told them that they were going to be mesmerized. Even when I did not mesmerize them they generally imagined they felt the current of air caused by the passes, and believed they could tell the exact moment when the passes were begun. Here we see expectation produce a perception. Many people begin to feel the pain of an operation almost before the knife has touched them, simply because their whole attention is fixed upon the pain and the beginning of the operation.

It is upon the simultaneous development of the two characteristics of the human mind which we have just described—viz., the tendency to believe without logical proof, and the influence of belief on the human organism—that suggestion depends. The phenomena they present occur often enough in non-hypnotic states; and even if we are obliged to admit that any inordinate intensification of their activity is only observed in hypnosis, we should have to desert the safe ground of reality if we wished to limit that activity to hypnosis alone. I have already discussed the various definitions of suggestion (p. 64 *et seq.*), and I must refer the reader to the explanations which I then gave for a proof that influences which a superficial observer considers only effectual in hypnosis may be equally so in every-day life. Höfler thinks that we should only

speak of suggestion when a judgment is formed or a wish executed in a way that is not quite normal, the power of judgment and the energy of the will being for the time partially in abeyance. For example, he ascribes the effect of a doctor's assuring a patient, in a tone conveying complete personal conviction and truthfulness, "You will be well," to suggestive influence, because the patient cheerfully believes the statement without any proof of its accuracy. This view is quite justified. Only, we should remember that it is quite a common occurrence for a judgment to be formed or a wish fulfilled "in a way that is not quite normal"—a fact which psychologists are apt to overlook. We can also see that all that Bechterew, Lipps, William Hirsch and others have written on the "concept" suggestion still leaves suggestion a wide field of operation outside the domain of hypnosis. An examination of those theories which put the associative processes in the foreground, or of Dubois' and Vogt's definitions mentioned on page 66, shows at once that suggestion is not limited to hypnosis. No matter what definition we select, it will always be found that non-hypnotic states present processes analogous to those of hypnosis. Sidis thinks that there must always be more or less resistance to suggestion; but this is wrong, whether the suggestion be hypnotic or post-hypnotic. As Hirschlaff very properly insists, those phenomena of waking life which arise from stupidity, superstition, feeble-mindedness, and fanaticism bear a great similarity to the phenomena of hypnosis. In all such cases there is suggestion, and the suggestion is accepted and carried out without hypnosis because of the subject's mental predisposition, and there is certainly no resistance. Lipps lays stress on the inclusion of the extraordinary in the concept suggestion, but that should not lead us to exaggerate. Many things appear extraordinary which are not so in reality. And we must admit that suggestive processes are of daily occurrence in ordinary life, unless we would dissociate phenomena which really belong together. Hellpach gives the following example: —If a man is told when he sits down to dinner that the food placed before him is unsavoury or dirty, and he experiences a momentary feeling of aversion, that is an ordinary phenomenon, and not a case of suggestion; if, however, this aversion is not dispelled by the food being of good quality, but increases to loss of appetite and nausea, then suggestion is at work.

I now come to the third fact in the mental life of man—viz.,

the susceptibility which an individual may evince to the influence of some particular person. The latter need not be an all-round authority, but may yet influence people who are in no way mentally deficient. It is well known, for example, that great and learned men are often under the influence of a person who is their intellectual inferior. We know well enough that even lunatics and weak-minded persons are sometimes able to influence not only certain individuals, but even large numbers of people. Many a political or religious movement owes its success to the influence of mentally-deranged persons. And when we consider our behaviour with regard to dogmatic assertion, human credulity is made particularly clear. As children are particularly credulous of dogmatic assertions, and as such credulity is strongly marked in hypnosis, this state has often been compared to childhood (Copin, Miescher, Cullerre, Wernicke). I was told at school that the North Cape was the most northerly point of Europe. This was not logically proved to me, yet I believed it because it was in the book, and more especially because the teacher said so. Dogmatic assertion influences not only children, but adults, for the latter believe in the existence of land near the pole, not that they have ever seen it, but solely because they have been told that it is there. Certainly we are unable to explain upon what the influence which some people exert depends, especially when there is no question of authority. Still, we must take this influence to be an actuality, confirmed by many instances in daily life. We cannot explain sexual love. The man who falls in love with a woman allows her to domineer over him. Sexual influence may even go so far as to induce a state of "sexual subserviency," as Krafft-Ebing tersely terms it. This is characterized by unconditional surrender of the will. But we find this influence of one individual over another quite apart from sexual love. This is well exemplified in school life; one master has greater influence over his pupils than another, because the influence exerted has usually no logical basis. Anyway, it would be altogether wrong to deny the existence of personal influence. And I do not consider that Lœwenfeld is right in his view that in hypnotic suggestion the personal influence of the experimenter is put in the background by other methods of influence—entreaty, advice, command.

The circumstance that we are unable to analyze the cause of personal influence in numerous cases has led to all kinds of

mystical theories being propounded, especially that of animal magnetism, by which it is attempted to prove that the influence in question is due to some kind of physical action, although in reality it depends on processes that are entirely mental. The fact that we are not always able to understand these processes often leaves a mysterious impression, and Eschle, consequently, agrees with Rosenbach that there is a mystic factor in suggestive influence. We must not, however, call a phenomenon mystical because we do not understand all its details; otherwise we should have to term sexual love, the influence of teacher on pupil, and many other similar human relations mystical also.

Many experiences of daily life also confirm the fourth of the facts under consideration—viz., that if one person commences to exert an influence over another, that influence is increased by subsequent repetition. It frequently happens that when two people fall out neither will give way, no matter how trivial the subject in dispute may be. Each disputant fears that if the other gain no matter how small a success, he will demand more. In the winner the consciousness of victory, which is an essential part of the success gained, is increased, while the loser feels a lessened power of resistance. It is evident that the increase of influence on the one hand, and of susceptibility on the other, has a logical basis. This may be observed in the case of patient and doctor; the latter's influence is sometimes increased on account of the success of his treatment.

The fifth fact of human mental life concerns a consciousness completely distinct from the waking consciousness. Following Eduard v. Hartmann, we will call this the state of dream-consciousness. In it feelings and perceptions do not occur in the same way as in the waking consciousness; but I will not enter into details on this point, as it has been thoroughly discussed in the chapter on "Cognate States" (p. 178 *et seq.*). The chief point is that we are able to distinguish dream-consciousness from waking-consciousness simply by recollection. When we wake from sleep in which we have been dreaming, we know that what we dreamed was only a dream, and was not real (Bentivegni).

We may take these five facts in connection with the mental states of human beings for granted. As I have pointed out in the preceding remarks, there are many respects in which they may be rendered more comprehensible, but we do not possess any thoroughly satisfactory explanation of these five facts, and

such is not necessary as far as theories of hypnosis are concerned. Still, the study of them renders the phenomena of hypnosis much more comprehensible. It may be urged—in many respects justifiably—that the limits I have placed on the possibility of explanation prevent me from giving an "explanation." But we must admit that many a so-called explanation may in the same way be considered only a circumlocution; though many authors put their supposed explanations before the world with such assurance that if we would only believe them there remains hardly anything unexplained either in the whole of mental life or in hypnosis. I hold the opinion that the possibility of explanation goes no further than I have here intimated; when other authors express the belief that they have explained more by means of their psychological theories, ideas of attention, contraction of consciousness, etc., I must enter a protest. I shall be silent here on the physiological theories, as I shall come to them later on; they may be looked upon as most unsatisfactory hypotheses. At all events, if we are to understand the phenomena of hypnosis, it is essential first of all to establish the fundamental facts of mental life, and to remember at the same time that now one, now another, of these facts, now several of them together, will have to be considered.

After these preliminary remarks I go on to discuss the separate phenomena of hypnosis; the functional disturbances of voluntary movement first, because there is no hypnotic state without them. They are almost always the first symptom, even when there are other changes. The effect of expectant attention explains their onset. But to understand this more readily the hypnosis should be induced by slow degrees, as in this case the motor disturbances are plainer.

To produce any motor disturbance by suggestion in a subject, X., who is in a perfectly normal state, we must first of all make him believe in the possibility of such an effect. Consequently we shall the sooner attain our end if X. has seen a number of similar experiments performed on other persons. The possibility of influencing people in this way is, for example, much greater just after they have witnessed hypnotic performances. But belief in the possibility alone of the effect taking place does not as a rule suffice. It is more generally necessary to fix the subject's attention as far as possible on the coming

on of the motor disturbance; or, as Fechner and Wundt express it, to place the expectation of the disturbance in the range of his inner perceptive. The better we succeed in doing this, the easier will it be to induce motor disturbances by means of suggestion. It is certain that some people have a peculiar disposition which facilitates the attainment of the desired result. But such a disposition can be created by external conditions; in this respect the surroundings, moral influence, and the manner and appearance of the experimenter play a great part.

Supposing we have succeeded in paralyzing the subject's arm. The appearance of one symptom facilitates the following of others, because it increases the subject's conviction of his susceptibility to suggestion. A mental state ensues which Pierre Janet calls *misère psychique*, a feeling of weakened will-power which favours the acceptance of later suggestions—for example, paralysis of the speech, the legs, etc.

This enables us to understand the gradual development of the disturbances of the muscular functions. This development is in many cases identical with that of hypnosis, which is often merely an inhibition of the voluntary muscular functions. Many methods used to induce hypnosis are alike in one particular—they direct the subject's attention to some change in the functions of the muscles. The method of the Nancy school consists chiefly in making the subject expect the closing of his eyes as strongly as possible, though certainly this method also aims at producing the dream-consciousness. However, the latter is an additional moment, and is not an absolute necessity. As we saw in our classification of hypnoses, there are numerous cases without any evidence whatever of dream-consciousness. On the other hand, it is evident that when special stress is laid on the closing of the eyes, the dream-consciousness may be unintentionally suggested as well. We may begin with any other member of the body just as well as with the eye. For example, an arm or a leg loses its power to move when I concentrate the attention of the subject on the loss of power to move. In fact, it is quite unnecessary to begin with the eyes, as the school of Nancy does; we can begin with any member, as Max Dessoir rightly insists. Of course, we should naturally begin with that abnormality which is most easily induced, because the acceptance of later suggestions is favoured by the abnormality already induced.

This principle of the effects of expectant attention illustrated

above is nowhere shown more plainly than in the voluntary movements. Modern psychology teaches us how easily a movement is induced by the idea of it, without any voluntary action. Here we have apparently the same effect produced when the subject himself has the idea of the movement as when the experimenter arouses the idea in hypnosis. But in the latter case not only is the idea aroused, but the expectation of its fulfilment as well, and this further favours the movement being carried out.

I now come to the discussion of sense-delusions; first of all, of the positive kind. Are we not exposed to such delusions otherwise than in hypnosis? Take first a very simple example of Max Dessoir's. If a man who is wide awake is told, "A rat is running behind you," he will have a mental image of a rat for a moment—*i.e.*, there is already a trace of hallucination, even though he is convinced that there is no rat.

Modern psychology, following such men as Dugald Stewart and Taine, generally supposes that every idea includes an image—*e.g.*, the idea of a knife includes the image of a knife. As, further, every central image tends to externalize itself, as Stuart Mill in particular has explained, when an idea is aroused, there is always a tendency to externalize the corresponding image—*i.e.*, there is a tendency to hallucination. We have thus a tendency to take remembered images for real objects (Binet, Féré). Many interesting details on this process are given by Sourian in *La Suggestion dans l'Art*.

But even if the ideas of waking life are associated with a trace of hallucination, there is a great difference between the effect of an idea suggested in hypnosis and that of the corresponding idea in waking life. In normal waking life a man can convince himself of the inaccuracy of a statement by means of his senses; and, apart from this, an idea in itself has not the same tendency that it has in hypnosis to develop into a hallucination which dims the judgment. The difference may only be quantitative, but must not be disregarded. It is a necessary condition for the production of a sense-delusion in hypnosis that an idea be connected with the corresponding image. "The power possessed by hallucination of producing conviction depends upon the fact that we invariably refer the reproduction of our impressions to the sensory region from which they sprang; indeed, we not only localize them in this way, but even to a certain extent thus project them into space."

(Jodl). A further condition necessary for the distinct development of hallucinations in a hypnotic subject is a complete change in his state of consciousness. Here the fifth fact of human mental life—viz., that we have a dream-consciousness completely distinct from the waking consciousness. These states of consciousness differ from one another in two respects (Wundt). In the first place, the remembered ideas in dream-consciousness have a hallucinatory character—*i.e.*, we try in dreams to objectify the images of memory; we do not recognize that they are images of memory as we do in waking life, but believe that we see, feel, etc., the real object to which they correspond: in the same way sense-impressions do not produce normal perceptions, but illusions. In the second place, in dreams apperception is changed—*i.e.*, the power of judging the experiences of which we are conscious is essentially altered. It is just this peculiarity of the dream-consciousness mentioned by Wundt which is found in the consciousness of such hypnotic subjects as are accessible to suggested sense-delusions. I need not enter into details on this point, as it has been thoroughly discussed in the chapters on "Symptoms" and "Cognate States." The chief point is the hallucinatory character of the images of memory; faintly imitated in normal states, in dream-consciousness it is extremely plain, and appears in hypnosis in connection with illusions, to which dream-consciousness is also favourable. But we must remember that there is nothing strange in such a dream-consciousness, since it is often found in ordinary sleep and is always a potential phenomenon of sleep.

The production of this peculiar dream-consciousness is one of the chief points in hypnotizing. An explanation of hypnosis need not include an explanation of the state of dream-consciousness in general; to make such a demand would be unreasonable and impossible of fulfilment as well, because we are quite unable to explain even the dream-consciousness of ordinary sleep. An explanation of hypnosis is only called upon to demonstrate the connection between the means employed to induce the hypnosis and the dream-consciousness of the person hypnotized. For this purpose I need only mention that apart from hypnosis dream-consciousness is often brought about in a similar way. Since we know that children may be talked to in sleep, and in adults dream-consciousness in hypnosis only appears when some similar

influence is at work, the whole process should appear less mysterious. Indeed, if we further consider that the ordinary sleep of adults in which dream-consciousness is almost habitual is in many cases induced mentally—*e.g.*, by auto-suggestion—then we have made considerable progress towards understanding this particular state in hypnosis. I do not, however, go so far as Forel, Liébeault and many others who say that natural sleep is the immediate result of auto-suggestion. I much more readily admit that without any such mental intervention sleep may have a chemical origin—*e.g.*, the accumulation of the products of tissue-waste in the body, or may be produced by purely physiological processes. But it is a fact that we sometimes fall asleep because we are convinced by a purely mental process that we shall sleep. I have already mentioned that people often fall asleep when they believe that they have taken a sleeping draught. That this mental process is very often the result of another person's influence is explained by the first and second facts of human mental life stated by me earlier in this chapter. And here I will again point out that one person is frequently able to exert an immense influence on the whole state of consciousness of another in other ways as well. The priest and the popular orator, for example, sometimes under the influence of strong emotion, often produce an effect upon their audience analogous to hypnosis.

It is true that in many cases dream-consciousness in hypnosis can be apparently induced by means which have nothing to do with the induction of sleep. When a hypnotic subject fixes his gaze and his eyes finally close, this does not appear to be the induction of a state of sleep. On this point, however, I refer the reader to my remarks on hypnosigenesis in Chap. II. It is very probable that the idea of sleep is induced by such physical means, even when it is not purposely suggested. Sleep may be brought about by the feeling of heaviness in the eyes, through association of ideas (Forel); for we know that some people are in the habit of staring fixedly at some point in order to tire their eyes out and bring on ordinary sleep. For these reasons, I believe that when a hallucination happens in hypnosis some means of inducing dream-consciousness have always been used, and that such means apparently need not have anything to do with the induction of sleep. Even the fact that sense-delusions sometimes happen without previous

closing of the eyes does not contradict this, since the dream-consciousness is not necessarily connected with the closing of the eyes. It sometimes comes on when the eyes are open, as is seen in cases of spontaneous somnambulism.

From what has been said we are able to find an explanation of sense-delusions in the analogy between these hypnotic states and the dream-state. Without entering into any discussion of the theories that have been advanced to explain the sense-delusions of ordinary sleep, we shall find that it will provisionally help us in examining hypnosis if we take the state of consciousness of the hypnoses in which there are pronounced sense-delusions as completely corresponding to the dream-consciousness of ordinary sleep. In both states certain impressions (memory-images, or mere stimulation of the senses) induce sense-delusions.

These conclusions lead to the discussion of *rapport*, especially *isolated rapport*. This *rapport* causes the subject to be more influenced by certain impressions than by others, and to respond to them by corresponding sense delusions. I shall speak of *rapport* briefly, as I have elsewhere[1] dealt with it at length. According to Noizet and Bertrand, who have been joined lately by Liébeault, Bernheim, Forel, and others, *rapport*, as already mentioned, is a state of sleep in which the attention of the subject is fixed exclusively on the hypnotizer, so that the idea of him is constantly present in the subject's memory during the hypnosis. Bernheim compared these processes to the falling asleep of a mother by her child's cradle. She continues to watch over it in sleep, but over it alone; she wakes at the least sound it makes, but hears no other sounds, even the loudest. An analogous phenomenon may be observed in waking life when several mothers are present at a children's party. Each mother's interest is centred in her own child, and it is particularly noticeable that, though deaf to the prattle of other people's children, she never misses any of her own little one's clever (?) sayings. Of course a careful study of the negative hallucinations will help us to understand *rapport*, as will be seen from my remarks on those hallucinations; but the most essential consideration in respect to *rapport* is the individual influence which certain people can exert over others, which I

[1] *Der Rapport in der Hypnose;* Leipzig, 1892.

have already described as the third fact of human mental life. For there is a simpler form of *rapport* in which, without there being any question of isolation, the hypnotic subject is amenable to the suggestions of one individual, but not to those of any other. We can well understand that an experimenter's influence should increase as soon as he has established a kind of authority over his subject; for the fourth fact of human mental life teaches us that influence increases with the expectancy of its being exercised. No new psychic law is to be found in hypnosis.

When we go on to discuss the negative hallucinations, two things at once strike us: firstly, that the subject does not see certain objects or hear certain noises; secondly, and more particularly, that the objects he does not see are just those he is forbidden by the hypnotist to see. Of course, we know from experience that many things are not seen and heard in normal circumstances when the attention is diverted from them. I almost invariably find jugglers well worth studying from a psychological point of view; and I think we may utilize the juggler in our present case. Let us watch him carefully, and we shall see how he hides things, how he makes a change, how he substitutes one card for another under the very eyes of the spectators. But he knows how to draw off their attention by clever talk, so that even those who have watched him are unable to give an account to themselves of his proceedings. For example, cards are changed in the spectators' field of view; sense stimulation takes place, but does not penetrate to the consciousness. We find analogous occurrences in ordinary life. It has happened to everybody to look for something which is before his eyes. In this case the thing is not perceived although it is in the seeker's field of view and he is actually thinking about it. Attention may be so far diverted as to induce a state not unlike hypnosis. I recall the case of the motor-cyclist seated behind his motor in a race: "The rider is fairly asleep; he is carried along by *his machine* without knowing what is happening. Nothing wakes him but a puncture or a fall." Warthin thinks that some Wagner enthusiasts auto-hypnotize themselves at the sound of the master's music; and the story of Archimedes' death is another instance of the influence exerted by diversion of the attention. The facts of negative sense-delusions in

hypnosis are not astonishing, but in respect of such delusions there is a marked difference between hypnosis and waking life. If I tell a waking man who has a chair in front of him, "There is nothing there, neither chair nor table," he will see the chair in spite of what I say; but the hypnotic subject will not see it—at least, if he is at all susceptible to negative hallucination.

Now, we can regard this process in the hypnotic as a diversion of the attention, like that in the waking man who fails to perceive things because his attention has been diverted from them. Many observations seem to substantiate this view. We know that negative sense-delusions sometimes disappear the moment the attention is re-directed to the object. I say to a subject, "When you wake, X. will have gone away." When he wakes and is asked how many are present, he says, "Two; you and I." I then point out X., and tell the subject to look at him. Then he sees X., and the suggestion has lost its effect.

Parish has pronounced against this view of negative hallucinations. He considers that a negative hallucination in hypnosis is nothing but the subject's individual interpretation of a positive hallucination. For example, to tell a man that he does not see a glass which is on the table is synonymous to a command to see something else in the place of the glass. Still, the proofs that Parish brings forward do not suffice to establish his point. Parish's chief objection is to my view that a negative hallucination is a diversion of the attention; this he holds to be erroneous. Of course diversion of the attention does not afford a complete explanation; it should be considered a kind of psychological scheme representing the subject's mental state at the moment a negative hallucination takes place. For the rest, in many cases Parish's view that a negative hallucination is an auto-suggestive interpretation of a positive one is certainly not right.

Of course, although we may interpret a negative hallucination as a diversion of the attention, that alone does not explain its mode of origin; for if I tell a man in the normal state that there is no chair where there really is one, he will be all the more certain to see it. How can we explain the completely opposite result with the hypnotic subject? According to Binet and Féré, diversion of the attention is attained in this case because a conviction that the chair is not there has been first established in the subject. It must be admitted that such an established conviction hinders the perceptive faculties. If a man is at work in a room which is generally quiet he is far more likely to miss hearing a slight unexpected

sound than one he is prepared for. Probably the production of negative sense-delusions in hypnosis is facilitated in a similar way. It must be remembered that the experimenter has acquired ascendancy over the subject and has become an authority for him. The subject is consequently inclined to believe everything he is told by the experimenter; and it is conceivable that negative hallucinations are thereby favoured.

Nevertheless, these two factors, the diversion of the subject's attention and the conviction established in him, do not suffice to explain negative hallucinations. However much he believes the hypnotist, without such motives as would induce belief under normal circumstances (as Bentivegni rightly points out) this does not alone explain such mistakes of sense-perception as are found in negative hallucinations. A completely changed state of consciousness must be added if we wish to understand negative hallucinations; the dream-consciousness again, which helped us to understand positive delusions of the senses. For dream-consciousness is not only distinguished by the reappearance of former memory-images as hallucinations; it is also characterized by the fact that sense-impressions, which under normal circumstances become feelings and perceptions, induce in it no feeling or perception.

Hence, negative hallucinations depend upon the co-operation of various factors: firstly, dream-consciousness which creates the tendency to negative sense-delusions; secondly, the subject's belief in everything the experimenter says, which favours those delusions; thirdly, the mental state which results from this, and which may be regarded as analogous to diversion of the attention.

We can explain the analgesia of some hypnotic subjects in a like manner. It is known that an expected pain is more acutely felt than an unexpected one. The effect of a stimulus may vary very considerably according to the mental attitude of the subject. We see this in operations; the subject feels much more pain when he expects the stroke of the knife than when it takes him unawares; in the latter case he feels hardly any pain at all. It is the same thing with analgesia in hypnosis; but it is still doubtful whether there is ever an entirely spontaneous analgesia without suggestion. In any case, analgesia is more usually induced by suggestion. Here we may take it the subject's mental state has been brought about by his implicit belief in the experimenter, and is much

the same as in diversion of the attention. To explain analgesia, however, it is necessary to call in the aid of dream-consciousness, just as we did in the case of negative sense-delusions; for dream-consciousness has a decided tendency to prevent impressions, which would otherwise be painful, from becoming feelings and perceptions. Be this as it may, the following essential difference exists: as soon as a painful stimulus has acquired a certain degree of intensity it wakes a person from ordinary sleep in spite of dream-consciousness, but does not as a rule terminate a hypnosis; and Rosenbach pointed out long ago in his work, *The Reflexes in Sleep*, how differently sensory stimuli act in health and disease. We can more readily understand the analgesia of hypnosis when dream-consciousness is associated with the other factors mentioned above.

I now come to the discussion of some phenomena of memory. Only those cases will be considered in which there is a derangement of memory due to hypnosis—*i.e.*, cases in which the subject after waking from hypnosis remembers nothing of what has happened. It is a well-known fact that we forget certain events in ordinary life. We entirely forget mechanical actions, such as the winding of a watch. But some things done with reflection and in perfect consciousness are occasionally forgotten even though we particularly intended to remember them. I will choose an example from my own experience, a thing which we have all doubtless observed in ourselves. I take a book and put it in a particular place so that I may find it when I want it. At last I want it, but I cannot remember where I put it. I think in vain. Only when I replace myself in imagination at the moment when I put it away (a method which every one knows) do I remember where it is. And yet in spite of temporary loss of memory I did not put the book away when I was in a state of loss of consciousness; it was rather that I was at the time in another state of consciousness. This is in many respects analogous to hypnosis, the events of which are remembered only when the subject is again in the same state of consciousness—*i.e.*, in a fresh hypnosis. Of course these cases in ordinary life may deprive the mental derangements which occur in hypnosis of much that is strange and mysterious, but they do not afford a satisfactory explanation of the phenomena. I mentioned when discussing the memory before that the subject in hypnosis sometimes

remembered all the events of preceding hypnoses, and of his waking life. If we suppose the life of such a being as divided into several periods *a*, *b*, *c*, *d*, *e*, *f*, in the periods *a*, *c*, *e*, only the events of those periods will be remembered; so that in period *c* he will only remember what happened in *a*, and in period *e* what happened in *a* and *c*. On the other hand, in the periods *b*, *d*, *f*, both what has happened in them and in the periods *a*, *c*, *e*, will be remembered. A phenomenon such as this calls for an explanation.

Max Dessoir endeavours to explain it by his theory of the "Doppel-Ich," or double Ego. He supposes that human personality is a unity merely to our own consciousness, but that it consists really of at least two distinguishable personalities, each held together by its own chain of memories. According to him many actions are performed mechanically though of mental origin—*i.e.*, the agent acts unconsciously for the moment. For example, rubbing the hands when they are cold and even more complicated actions are performed automatically. Max Dessoir relates the following personal experience:—"A friend calls on me with a communication which means that I must go with him at once. I dress myself to go out while he relates the details of a case that is evidently urgent. I put on a clean collar, turn my cuffs, button them on, put my coat on and my latch-key into my pocket although the questions I put to him from time to time show that my attention is directed exclusively to what he is saying. As soon as we get into the street I am seized with the firm conviction that I have left the key behind. I go back and hunt for it in every nook and corner in vain; suddenly I put my hand in my pocket, and there is the key." This shows that an action which is quite intelligible can be performed unconsciously—*i.e.*, without the agent noticing what he is doing or breaking off the conversation he is engaged in. The experiment made by Barkworth, a member of the English Society for Psychical Research, is much more complicated than this. He can add up long rows of figures while carrying on a lively discussion, without allowing his attention to be at all diverted from the discussion. Recently some American investigators—Speir, Armstrong, and Child—have brought forward interesting statistics of unconscious cerebration. It is shown that during this activity, though it goes on in the lower consciousness, most people have a distinct feeling of effort. If, for instance, one

cannot recall a name and purposely refrains from trying to do so, these statistics show that there is still very frequently a certain sense of effort. This shows that, in the first place, there is an unconscious intelligence in men, as is seen in the mechanical rubbing of the cold hands, and that, in the second place, there is an unconscious memory; Barkworth, for example, must have at least two groups of figures in his memory to make a third out of them; he must retain the third to add a fourth. But this chain of memory is independent of the other chain by means of which he carries on the conversation. Max Dessoir thinks that we have here the elements of a second personality. At least we can picture to ourselves consciousness as consisting of two halves, a primary and a secondary consciousness, which act independently. The mental processes which take place consciously to the individual are called the primary consciousness and those which go on without his knowledge the secondary consciousness. Thus in Barkworth's case the primary consciousness carried on the conversation, while the secondary one mechanically performed the addition.

From the above it will be seen that what we call primary consciousness has hitherto habitually been called consciousness. Generally speaking, "consciousness" means the mental processes that are subjectively perceived. In future we shall give it a wider meaning, so as to indicate the sum of all our mental processes. Consciousness thus falls into two halves, primary and secondary. To avoid confusion I shall in future only use the word "consciousness" in this latter sense.

With regard to the existence of a secondary consciousness, much that is instructive may be learned from a study of automatic writing. I owe my knowledge of this to Dr. Max Dessoir, whom I again thank for his unselfish and scientific help in the preparation of the previous editions of this book. Automatic writing has also been observed among uncivilized peoples (Doolittle, Bastian). We will now proceed to examine it carefully.

There are men who habitually strum on the table or do something similar while they are talking or thinking. When such people take a pencil in their hand they make all sorts of scribbled marks without observing it. This scribbling may be regarded as the beginning of automatic writing. It may, however, develop into something more than mere scribbling. Schiller says that when reflecting he has often covered whole sheets of paper with little horses (Max Dessoir). Other

persons also automatically write letters and words, and this process is called automatic writing; it is evidently guided by a species of intelligence, as without it no rational words could be written. But this intelligence resides in the writer, though it may not be conscious in the ordinary sense of the word; it is the secondary consciousness which carries on movements and actions as does the primary consciousness, although the person concerned does not remark them. Spiritualists imagine that this writing, which they call mediumistic, is the work of some external force or spirit.

I now ask the reader to follow me through some experiments with automatic writing. I give X. a pencil and ask him to answer some question in writing—for example, what he had for dinner yesterday; he is, however, to leave his hand passive and not to write on purpose; at the same time I put the point of the pencil on paper. It would not be strange that X. should write down something he is thinking of. It would remind us of the experiments in thought-reading described on page 62. X. thinks of roast veal, and the hand makes corresponding movements. But the process becomes rather different when I talk to the writer meanwhile. We talk about the theatre, the weather, etc.; in the meantime the hand writes "roast veal." It appears that this was yesterday's dinner. In this case the hand wrote without any concentration of thought on the writer's part; and this is already very different from the usual thought-reading.

Now, though X. did not know that he was writing, he knew the fact which he unconsciously wrote down; *i.e.*, he knew that he had had roast veal the day before. But people often write automatically about things they do not know. For example, when X. is asked what he had for dinner every day last week, he will automatically write down the whole list of dishes correctly, although he cannot give a correct answer by word of mouth.

Such experiments can be made in hypnosis with good results, and many phenomena, especially negative sense-delusions, are made more intelligible by them. I suggest to X., in hypnosis, that A. and B., who are really present, have gone away. X. ceases entirely to respond to A. and B.; he neither hears nor sees them, apparently. When I ask him who is present, he says, "only you and I"; upon which I give him a pencil, the point of which I put on a piece of paper, and ask him to answer the question in writing. He writes down, "Dr. Moll, Mr. A., Mr. B., and myself." Consequently he has given a correct answer automatically—*i.e.*, without knowing that he is writing. This shows that he can give the right answer by means of automatic writing to questions to which he cannot reply correctly in the ordinary way by word of mouth.

Max Dessoir also makes use of automatic writing to prove his theory that two mental processes can go on simultaneously in the one individual in such a manner that we might almost refer them to two distinct personalities. Objections have, certainly, been raised to this theory. Schrenck-Notzing, in

particular, thinks that if Max Dessoir does assume that a chain of processes in the primary conscience co-exists with one in the secondary he quite overlooks the fact that we are not here dealing with the question of processes in the two consciousnesses running their respective courses simultaneously. In reality the point is that the attention should be directed now to the one series, now to the other. Even if Schrenck-Notzing does not deny the occurrence of automatic acts, he considers that only such are possible as may result from practice. Lœwenfeld, however, rejects this objection, and with perfect right. "Schrenck-Notzing has never attempted to explain how it is possible for two series of ideas, whose members rapidly alternate in the consciousness, to continue so separate that the ego can only take cognizance of one of them with certainty." But other objections can be raised to Schrenck-Notzing's views apart from this. He does not give us the least explanation how it is that post-hypnotic suggestions are carried out in spite of the loss of memory. I shall deal with this point in detail later on. Moreover, Schrenck-Notzing has done nothing whatever to elucidate those cases in which the two chains of memory are not simultaneous conscious processes.

There are cases in which the chains of memory follow one another, instead of both existing together in the way we have already seen. Max Dessoir tells of a person who took up his dream on a second night where he had left it off on the first. Here then, the dream-consciousness tended to form a new chain of memories. The same author puts the following case of Macario's with the last:—A girl who was outraged during an attack of spontaneous somnambulism knew nothing about it when she woke, and only told her mother of what had happened in her next attack. I have already mentioned (p. 126) that similar cases occur under pathological conditions. Gumpertz published a very interesting case a short time ago. A girl, nine-and-half years old, presented the phenomenon of double consciousness. At times she was transformed into her aunt who was dead but was supposed to appear as a spirit on such occasions. On returning to her normal condition, the child was quite oblivious of what had occurred and remained so until she again fell into a trance. It has also been observed that during an epileptic fit the patient sometimes remembers what happened in previous seizures, though he knows nothing about them at other times; and a drunkard occasionally

recollects the events of a previous carouse in a subsequent fit of drunkenness, but not when he is sober. It cannot, therefore, be denied that two distinct chains of memories are met with apart from hypnosis.

We have seen that in the double consciousness—also termed doubling of the consciousness—of hypnosis, the subject, when in the hypnotic state, remembers the events of preceding hypnoses and of waking life, but that in waking life he only remembers the events of waking life. But there is also another form of splitting of the consciousness. In this the life of the subject, X., is likewise divided into several periods—*a*, *b*, *c*, *d*, *e*, *f*. In the period *e*, X. only knows what happened in *a* and *c*, and in *f* only what happened in *b* and *d*, etc.—*i.e.*, in each period X. only remembers the events of the corresponding period, whereas, as we have already seen, in hypnosis and similar abnormal states the memory remains intact not only for the events of the abnormal state, but for those of waking life as well. Dic-May has described such a case of splitting of the consciousness in his story *The Allard Case*, which induced Paul Lindau to write a play entitled *The Other One*. In this piece a lawyer plans various crimes while in the somnambulic state, and finally breaks into his own house. But we see that the lawyer has not the slightest knowledge of the existence of the criminal, nor the criminal of that of the lawyer, though at times there appears to be a kind of bridge connecting the two states of consciousness.

As regards the objections which have been raised by some investigators—*e.g.*, that of Wundt and Hirschlaff, who think they are justified in placing the theory of the double ego on a level with the assumption of demoniacal possession—we must point out and emphasize the fact that when the theory is applied with just limitations it has nothing whatever to do with such assumptions. It must, of course, be understood that we cannot assume, as is done by some foreign psychologists, that the individualism is made up of several separate personalities—that, for instance, a gentleman whom we usually know as Mr. M—— carries with him also the personality of Mr S——. Any one who so conceives the theory of the double ego can only arrive at an absurd conception of human personality. For it stands to reason that the two chains of memories belong to one individual, although we are sometimes able to fix their boundaries. There need be no exaggeration with this theory.

We must consider it merely a diagram to demonstrate the fact that mental processes may go on within us unobserved, only at times giving evidence of themselves in a chain of memories which in point of time is distinct from the ordinary processes of the primary consciousness; it also serves to demonstrate the fact that when they occur simultaneously, though separated from the processes of the primary consciousness, those of the secondary consciousness often appear as though connected by a chain of memories of their own. The fact that under certain conditions we can prove the existence of a whole series of such chains of memories which are partially independent of one another favours the view that the whole theory should only be considered schematic. We see this, for example, under pathological conditions in the case of the insane, who sometimes represent different personalities at different periods of their disease, thus enabling us to distinguish more than two chains of memories. But the same phenomenon may also be observed in the sane, in whom it sometimes happens that several chains of memories exist together in the secondary consciousness quite distinct from the chain of memories in the primary consciousness. We are also able to demonstrate a similar condition in hypnosis. If we suggest to a hypnotic subject, first of all, that he is Napoleon, then, shortly afterwards, Frederick the Great, and, finally, restore his own personality, also by suggestion, we find that each of the different chains of memories goes on independently within him; thus Frederick the Great knows nothing about Napoleon, the latter nothing about the reality, and the real person himself is quite unaware of the other two states. In short, we must invariably bear in mind that the theory of the double ego is only a diagram.

As regards double consciousness in relation to the hypnotic state, Max Dessoir thinks that hypnosis represents experimentally that half of our mental life that is usually hidden; the part which is called secondary consciousness, something of which association occasionally enables us to observe in ordinary life, but which in abnormal states appears as a connected whole held together by its own chain of memories. According to Max Dessoir's theory, double consciousness as it appears in hypnosis is no absolutely new phenomenon, but is the experimental representation of a psychic faculty latent in man. Considered within these limits the theory is intelligible, although it does not explain everything. Max Dessoir, whose

adherents include Pierre Janet, Sidis, F. Myers, Gurney, W. James, and Lœwenfeld, endeavours to support his theory of the double ego in a special manner which depends upon his proving that the state of consciousness in hypnosis is identical with the secondary consciousness of waking life. For this purpose he cites the following case:—One day he was chatting with some friends among whom was Mr. W., when a Mr. X.'s name was suddenly mentioned. This appeared to interest W. particularly, for he turned round and asked what had happened to Mr. X. When questioned W. did not know what the previous conversation had been about, so Max Dessoir hypnotized him, and again questioned him while he was in a state of deep hypnosis. W. then repeated every word that had been said before X.'s name was mentioned—*i.e.*, the conversation that was hidden in his secondary consciousness before hypnosis, came into consciousness during that state. To support his theory still further, Max Dessoir mentions certain experiments in which Pierre Janet was able to induce hypnosis by *suggestion par distraction.* In describing one of these experiments Pierre Janet writes:—"The subject, a woman, was thoroughly awake and talking to M. Binet. I stood behind her and caused her to obey my orders unconsciously—to move her hands, write down a few words, answer my questions by signs. Suddenly she ceased talking to M. Binet; and turning round, continued *vivâ voce* the conversation which she had unconsciously begun with me by signs. On the other hand, she no longer spoke to M. Binet or heard him speak, and we had to wake her from the fresh hypnosis that had been induced." Max Dessoir thinks that he has a right to interpret this case also on the lines laid down by him. For the induction of hypnosis by automatic acts suggests that the hypnotic self merely represents a developmental phase of the secondary consciousness.

Of course the assumption that hypnosis is simply the demonstration by experiment of the pre-existing double consciousness does not make the explanation complete, for the question of the causal connection between hypnogenesis and this demonstration remains to be answered. This point must be considered in the manner described by me when discussing the theory of suggested sense-delusions. Dream consciousness is very closely related to secondary consciousness; indeed, according to Eduard von Hartmann it directly represents the second state of consciousness. Delbœuf also identifies the

hypnotic phenomena with nightly dreams as far as the memory is concerned. At all events, these states present many analogies not only in general symptomatology, but as regards the memory as well. I have already mentioned that processes suggested and carried out in hypnosis are occasionally gone through again in sleep. If we accept the identity of dream-consciousness and hypnotic consciousness, I need only refer the reader to my previous discussion of sense-delusions for a further explanation of the causal connection between such consciousness and the induction of hypnotic sleep.

I now come to the discussion of post-hypnotic suggestions, which I partially explained by analogy a long time ago. For theoretical purposes I will begin by choosing some action induced by post-hypnotic suggestion, and will suppose it to be a case of hypnosis without loss of memory, the subject consequently remembering on waking all that has happened. Whether the hypnosis belongs to the first or second of Max Dessoir's groups (p. 59)—*i.e.*, whether there is normal waking consciousness or dream-consciousness—is a distinction I do not intend to make; we have certainly seen cases in which the appearance of dream-consciousness did not cause subsequent loss of memory.

We will begin with a case in waking life. I give a letter to X., who has called on me, and ask him to post it on his way home, if he passes a letter-box. Later on X. goes away, and posts the letter. Here we see that the commission I gave was duly executed later on. From this case let us turn to a similar one with the subject Y. in a hypnotic state without loss of memory. I give him the same commission that I gave to X.—*i.e.*, to post a letter on his way home if he should see a letter-box. This commission also was executed.

Now the question is, What is the difference between the two cases? When we compare the second, or hypnotic, case with the first, one circumstance strikes us—*i.e.*, that Y. executed the commission without, or perhaps, against his will. Of course the fact that Y. had posted the letter without *willing* it would not distinguish his case from X.'s. X., for example, walked home with a friend and passed a letter-box. While they were talking X. apparently did not notice the box, but he threw the letter into it without interrupting the conversation. Later on it occurred to him that he had a letter to post; he had only a

faint recollection of having done it. He did not know for certain, but by feeling in his pocket for the letter he convinced himself that he had executed the commission entrusted to him. We thus see that he performed a specified act without the intervention of his will.

It would be more striking if X. should do some such action against his will. He would certainly not have executed the commission if his will had opposed it. What is striking in post-hypnotic suggestion is exactly the fact that it is carried out against the subject's will.

The question now is whether we can find an analogy to this in waking life, whether an idea can in this case cause a motor or other effect in spite of the will. The answer must be, "Very commonly."

We will suppose that X. has lost a dear relation. X. is in consequence sad and depressed, and cannot refrain from tears. Months pass, and he grows calm; but when the anniversary of the death arrives he falls again into the same state of grief and tears, which he cannot conquer. The vivid idea has been enough to throw him, against his will, into a particular condition.

But we also observe the same sort of thing in the motor sphere. I showed, when discussing suggestion in the waking state, that an idea is sometimes enough to cause a movement in spite of the subject's will. But the post-hypnotic actions and movements carried out in spite of the will have a very great likeness to the instinctive movements well known in psychology, which also occur in spite of the will. I may here call attention to the fact already mentioned, that you cannot keep your eyes open if any one suddenly and unexpectedly brings his hand close to either of them. It is the same with other movements which are partly mental and partly physical reflexes, as, for example, the raising of the hand to ward off danger. In these instinctive movements there may very well be an idea of the movement; the will is unable to inhibit the movement. The same thing occurs in the case of many post-hypnotic actions. This irresistible impulse is also found in pathological cases. I refer to those cases of mental disorder in which the patients' actions "appear to be impelled by instinct, though they are consciously carried out" (Schüle). Uncontrollable acts, especially when accompanied by great emotional disturbance, play an important part in pathology.

There are, of course, cases in which a motive underlies the impulsive act; for example, a patient who fears that everything he touches conveys contagion wears gloves on every possible occasion or else keeps on washing his hands. But there are numerous others in which the uncontrollable action is the direct result of the idea of that action; and it is exactly these that bear the greatest resemblance to cases in which post-hypnotic suggestions are carried out. There are patients who feel an impulse which they cannot control, especially when alone, to use certain words. That cases occur which are not pathological is proved by the actions of many gamblers; but here the boundary between the normal and the pathological is elastic. A gambler who calmly reflects may very well be able to foresee the ruin that awaits him, yet the moment he thinks of the game or sits down at the table he is unable to control himself. Great men of business are in the same case. As far as many great financial enterprises are concerned, it is altogether wrong to assume that the people who start them are animated solely by the desire to make money; it is far rather an irresistible impulse to lead a strenuous life that is the true cause. I know of a multimillionaire who, though nearly seventy years of age and childless, is embarking on a project involving many millions, in which he is risking much money and is bound to be beset by constant worries. In these great financial undertakings it is only the impulse to lead an active life that is the true incentive, and that is what the great man of business cannot suppress. Such processes impelling men to actions that are motiveless are to be found everywhere in daily life, which, if we look around us, presents manifold analogies not only for post-hypnotic suggestions, but for all other hypnotic phenomena as well.

The enigma, therefore, does not lie in the fact that post-hypnotic phenomena are carried out at all, but in the fact that the hypnotist is able to implant an idea that produces a definite result, just as an irresistible idea produces an uncontrollable action. Now, if we bear in mind what I said at the beginning of this chapter, especially in regard to that feeling of dependence on the experimenter which dominates the subject and does not cease with the termination of the hypnosis, and also the subject's feeling of inability to resist which goes on increasing, and if we further pay particular attention to the second of the facts I then mentioned, namely, that anything the subject be-

lieves is capable of inducing the corresponding psychological and physiological effects; if, as I say, we remember these facts, then post-hypnotic suggestion cannot be so enigmatical. And it must appear even less so if we bear in mind that an idea which is accepted in hypnosis has, by means of the association of ideas, a natural tendency to create a state of consciousness and will analogous to that which obtained when the idea was implanted.

I have now considered why post-hypnotic suggestions are carried out without or in spite of the will. I supposed a case in which the subject remembered the order given him in hypnosis after he woke. It is a more enigmatical question why post-hypnotic suggestions are carried out when the subject after waking has no recollection of having received the command.

For explanation let us return to the case of waking life, where X. was to post a letter. Now X. did not keep the request continually in his consciousness, for we certainly saw that he apparently posted the letter unconsciously; yet he would not have performed the action at all if he had not really remembered my request. It is the same in post-hypnotic suggestion. All post-hypnotic suggestions really remain in the memory, and are merely apparently forgotten between waking and fulfilment. Here, again, we must remember that our mental processes are divided into two groups, that of the primary consciousness in which they are subjectively perceived, and that of the secondary consciousness in which they are unperceived. We must further bear in mind that the state of the primary consciousness is not uniform, but, on the contrary, subject to constant changes. In one period we are conscious of ideas which are wanting in others. One period comprises more than another. Now, if we call the sum of mental processes subjectively perceived at a certain time the sphere of primary consciousness, we may suppose a number of such spheres. But not to complicate the subject too much, we will only suppose two spheres.

We saw, when discussing the memory, that the hypnotic who forgot the events of hypnosis in waking life remembered them in later hypnosis. But he remembered the events of waking life also in hypnosis, though in waking life he was only conscious of the events of that life. We have, then, two different spheres of primary consciousness here; one comprises the events of

hypnosis and of waking life, the other only those of waking life. We must further remember that the two consciousnesses are not schematically separated. Impressions made on the secondary consciousness very often rise to the primary, and *vice versâ*. The restoration of memory through the association of ideas, which I spoke of on p. 122, is an example of this, and alone suffices to show that the events of hypnosis are firmly established in the mind, even when there is loss of memory after waking; otherwise it would be impossible for the association of ideas to call up recollection. The events of the hypnosis were unperceived until raised to the primary consciousness by the association of ideas.

But, in addition to this, there are other ways in which it can be proved that the events of hypnosis are really only dormant in the secondary consciousness; and it is automatic writing, of which I have already spoken, and which has been investigated by Gurney, F. Myers, Pierre Janet, Binet, Patrick, Max Dessoir, Flournoy and others, that provides a special proof. For example, I hypnotize X., make him go through all kinds of scenes by suggestion and then wake him. When I ask him what happened during the hypnosis, he declares he does not know. No matter how much I press him to recollect what he experienced during the hypnosis and tell me of it, he is unable to do so. I now order him to write down automatically the events of the hypnosis. X. does so, and writes down everything that was suggested to him during the hypnosis. He could not do this if the events of the hypnosis were banished from his mind. Hence recollection was dormant, as the automatic writing proved.

We have thus seen that it can be proved by automatic writing and other methods that post-hypnotic commands are firmly lodged in the secondary consciousness, and it is now easy for us to show why the post-hypnotic suggestion is carried out in spite of loss of memory after waking. As we have seen, the command lies quiescent in the secondary consciousness, and the loss of memory is so far only apparent. But much goes on in the secondary consciousness often of a very exact kind, and there is no confusion in its thoughts; this explains why the subject carries out a suggested order correctly, even when after waking he has no recollection of having received it.

The foregoing explanations show, firstly, why a post-hypnotic

suggestion is carried out without the will, or in spite of it; and secondly, why this happens in spite of the apparent loss of memory. A second question is this: Why is a post-hypnotic suggestion carried out at the right moment? We already know (p. 162) that the moment may be appointed in numerous ways; either by a concrete external signal—*e.g.*, the striking of the clock, or by fixing an abstract period, or by counting signals or days.

In the case of the striking clock we shall find no new mental law; we find the same process quite commonly in normal life; it is the result of the association of ideas. The striking of the clock often reminds us of something we wanted to do at a particular time, and we then proceed to do it.

The same thing happens when we tie a knot in our handkerchief to remind ourselves of something. It occurs to me that I must write a letter to-morrow; I make a knot in my handkerchief to remind me of it. The knot and the letter are then associated in my consciousness, and when I see the knot next day the idea of writing the letter rises from my secondary into my primary consciousness. Now, we see the same thing in post-hypnotic suggestion (p. 162). The striking of the clock made the idea of taking the water-bottle and walking up and down with it rise from the secondary into the primary consciousness. This process of association is so powerful that it often takes effect even when the suggestion is not punctually carried out. I hypnotize X. on Saturday and tell him, "When you come in early on Tuesday I shall cough three times; you will then give me your hand and remark 'That is too stupid.'" X. does not come till Thursday, but the suggestion is carried out, merely because I cough.

We will take the second case, where an abstract period of time was given instead of a concrete sign. Here the idea lay in the secondary consciousness until it resulted in the corresponding action. This was carried out because work goes on in the secondary consciousness. But the calculations which take place in the secondary consciousness are not always quite exact; hence it often happens that the suggestion is not carried out punctually when an abstract period of time is given. For this also many analogies may be found in ordinary life. I say to X., "Remind me in an hour to write a letter." X. is busy, and thinks no more of the letter, but nevertheless reminds me of it after some time. But as he has not looked at the clock,

he is not punctual: the case is quite analogous to post-hypnotic suggestions, where there is generally no perfect punctuality. Some people suppose that in the few cases of striking punctuality some unconscious calculation of time takes place, like the unconscious regularity of our pulse and breathing. This would imply the existence of unconscious mental activity quite independent of the secondary consciousness; the unconscious regularity of the pulse is never directly perceived, whereas the processes that go on in the secondary consciousness occasionally rise into the primary. But there is no necessity to assume any unconscious activity in our case. We know, in the first place, that there are persons who can calculate time with some exactitude when they are awake, and, in the second, that others can do the same in sleep—*i.e.*, they can wake themselves at a definite time without hearing the clock strike. For further information on this point I refer the reader to pp. 162 and 163. In any case, the secondary consciousness of a person who carries out a post-hypnotic suggestion after a definite lapse of time has no greater task to perform than might be expected of it, considering what we already know concerning the primary and the secondary consciousness. Here, again, the most important point is that we need not assume any special faculty on the part of the hypnotic subject.

The third way of fixing time is by counting signals or units of time (minutes, hours, days, etc., *cf.* p. 164 *et seq.*). Gurney's explanation of this is grounded on the division of the consciousness into primary and secondary. While the primary consciousness is busy talking to the experimenter, the secondary works on independently, marking the signals—*e.g.*, the shuffling of the feet, etc. When the tenth signal is given the suggestion is carried out, just as other suggestions are carried out at an appointed signal.

Gurney endeavours to explain many long-deferred suggestions just in the same way. As we have seen, in these also the execution of the suggestion may be ordered at the end of a series of minutes or hours or days, etc., instead of a definite date (p. 163). This may be explained in two ways. Perhaps the subject calculates the date after he has been given the number of days or weeks. Against this there is the fact that the subjects, when hypnotized in the intervening time, cannot give the date. We have the same sort of thing in Bramwell's experiments which I described on p. 163. In those of

Gurney's cases in which the subjects were hypnotized in the intervening time they could count the days which had elapsed, or were to elapse, before the suggestion should be carried out, though they did not know the exact date. On this account Gurney supposes an action of the secondary consciousness in such cases. He thinks that the hypnotic subject's sub-consciousness calculates days just as the waking person's primary consciousness does, and that is why the suggestion is carried out.

By accepting these different spheres of consciousness and also an independent activity of the secondary consciousness, we are better able to understand those hypnotic suggestions which are carried out in a state of complete loss of memory, for the suggested command remains fixed in the consciousness, even if it only be in that consciousness which we have described as the secondary. The punctual execution of such a suggestion is only comprehensible if we admit that the two states of consciousness are similarly equipped; and the explanations we have already given show that this is no mere hypothesis.

The preceding explanations are chiefly intended to approximate as much as possible post-hypnotic suggestion to certain habitual occurrences. There is no question of a complete identification of them. Still, I think I have proved that those properties which we are prone to consider characteristic of post-hypnotic suggestion are also met with out of hypnosis. There is one thing more which I must certainly point out as I have not hitherto mentioned it, and it might pass for a characteristic of hypnosis. I refer to the fact that it is not the post-hypnotic command itself—*i.e.*, not what was said to the subject, but the idea of carrying out the command, that later on rises to the primary consciousness. If I suggest to a subject in hypnosis to ask for an apple an hour after he wakes, he will do so; it is not my order, but idea of carrying it out, that rises into his primary consciousness. We must always carefully distinguish between these two points.

But there are many analogies for this, also, where there is no question of hypnosis. We are reminded, for instance, of those dominant ideas which often result in actions, and whose origin is for the most part "unconscious" (Bentivegni). The source of the idea cannot be discovered by questions or by

any other means. But we need not refer to Lock's principle, *Nil est in intellectu, quod non prius fuerit in sensu,* to justify the assumption that a dominant idea is the result of some external event that has formerly influenced the mind of the person concerned. Lœwenfeld has very properly pointed out the determining influence which certain mental impressions possess. A sudden fright, for instance, may produce a lasting dominant idea. Freud mentions the case of Pascal, who from the time he nearly fell into the Seine was constantly pursued by the idea of falling into an abyss. But even events that do not directly affect the emotions—for example, quite ordinary occurrences in normal life—can by suggestion produce such ideas. It is quite a common occurrence for a patient to feel convinced that he is suffering from severe cardiac trouble, because he has overheard some casual remark about heart disease. Friedmann mentions the case of a girl who heard a scream just as she was going out of the room and was told that a neighbour's child had fallen out of the window. If ever after that she wished to open the door, the recollection of what had happened overcame her; this placed her in a most painful position, as she was never able to go out of the room, even to answer the calls of nature. It may very well happen that the original cause—the determining factor—is forgotten, yet the dominant idea will arise directly the same prompting impression occurs. This is analogous to post-hypnotic suggestion, where, as we have seen, it is the suggested idea, and not the command, that rises to the primary consciousness. The same sort of thing occurs when imperative ideas lead to uncontrollable acts—*e.g.*, murder, suicide, incendiarism, etc. We may here include a possible cause of hysteria described by Breuer and Freud. They attribute the appearance of hysterical symptoms to some injury to the sexual organs which the patient has received in early youth but does not remember when in a normal waking state. As we shall see in the medical section, Breuer and Freud employ hypnosis to bring back a forgotten injury to the patient's recollection, in the hope of affecting a cure thereby.

But it is not only under pathological conditions that some externally induced idea influences our actions and feelings without our being able to remember how the idea was implanted in us. Let us suppose that a child two or three years old is often in the society of A. and B.; A. is kind and gentle,

B. hard and unkind, so that the child gradually learns to like A. and dislike B. Let us further suppose that the child sees neither for a long time; nevertheless, when it does meet them accidentally it will still like A. and dislike B. The child, who is now several years older, will not know its own reasons; it will not remember the former conduct of A. and B.; no questions will bring this back to its memory, yet the effect of the old impressions remains. It is certain that this is a common occurrence in childhood. Shrewd observers think it likely that a man may owe his preference for some profession—painting, for example—to some childish impression, such as dabbling with colours; in this case also the early impression is forgotten by the adult.

So far from this occurrence being confined to childhood we frequently observe it in adults. We are often influenced by unimportant expressions we have heard, though later on we cannot trace the effect to its cause. Our conduct with regard to experiences and theories is often the effect of early unconscious impressions. It is by no means an uncommon occurrence that a remark which has apparently passed unheeded has really produced a profound effect.

Finally, we find something similar in the association of heterogeneous ideas. Recent studies in sexual perversion have drawn attention to the fact that the concurrence of sexual desire and some chance experience—witnessing a flogging for example—may lead to lasting sexual perversion. It may happen that the original experience is forgotten, and yet the link between flogging and sexual excitement remains fast, so that witnessing the former invariably induces the latter. Even when the association is of the simplest kind, depending on the law of the association of experiences, there is no necessity whatever for the original experience to be remembered; the effect corresponding to it invariably occurs. When two mental processes have once occurred together, recurrence of the one calls up the other. Here also, without any recollection of the concrete case in which the original linking together took place being necessary, a corresponding and similar linking invariably takes place. In short, these cases are analogous to post-hypnotic suggestions, which, as we have seen, are carried out although the command has apparently been forgotten.

Moreover, we find that something similar happens in the

case of animals. Indeed, it is mainly on this that the training of animals is based. Smugglers train their dogs to avoid frontier-guards by having them constantly beaten and otherwise maltreated by men wearing the uniform of such officials. The consequence is that the dogs run away directly they see a man in the uniform of a frontier-guard approaching. Loiset describes a trick-act which used often to be given in circuses—the little hump-backed tailor who tries to mount a horse but cannot, because it always lashes out at him, and bites and chases him as would a rabid dog. The horse chosen for this spectacle was one that had been teazed from a foal, more especially by a supposed tailor clad in a quaint costume, who maltreated it in various ways. Consequently, whenever any one similarly clad approached it, the horse lashed out, etc., but was quiet and obedient to persons in ordinary dress. There is no reason to suppose that an animal, any more than a man, recollects all the details of former injury because of the costume of its torturer. Much has been said in this connection of the sagacity of animals, but such remarks are merely the outcome of superficial observation. In reality, these are but mechanical associations in which, as in the case of the horse and the tailor where the sight of the latter in his quaint costume caused the former to bite and kick, one process calls up the other corresponding process without there being any recollection of the earlier experiences from which this linking together resulted.

In short, we have no occasion to consider it particularly enigmatical that the original command in post-hypnotic suggestion is forgotten, and that only the idea of carrying out the command rises to the primary consciousness. The process is here exactly the same as in the cases just mentioned. It is the idea of what is to happen, and not the source from which that idea springs, that is remembered, and this, as we have just seen, is a process which has its analogies in many cases which are outside the domain of hypnosis.

I have hitherto spoken only of post-hypnotic movements and actions, and have endeavoured to explain the most important phenomena by means of analogy. I have still a few words to say about post-hypnotic sense-delusions, which are less easy to explain. It is true that those which occur in a fresh hypnosis hardly present any substantial difficulty. We have seen that the subsequent loss of memory is only apparent,

and that the idea really remains in the secondary consciousness. Consequently, it is not surprising that the suggested idea should at an appointed time transform itself into a sense-delusion in a fresh hypnosis, which hypnosis comes on through association when the idea reappears. We must then explain the sense-delusion by means of the dream-consciousness as I have shown above.

It is quite another thing when the sense-delusion appears without a new hypnosis. For example, I say to some one in hypnosis, "When I cough after you wake, you will see a pigeon sitting on the table; you will remain thoroughly awake." The suggestion takes effect; the subject sees a pigeon where no pigeon is. But it is impossible to make him accept a further suggestion; that one point excepted, he seems perfectly normal. Whether the total mental state of such people is really normal, is a question on which Bentivegni speaks very clearly, and will be discussed in the legal section of this book. Now, how can we explain this particular sense-delusion? Dream-consciousness does not afford a satisfactory explanation, although Eduard von Hartmann believes that it always co-exists with waking consciousness. But even if we admit this it brings us no nearer a solution, for we should still have to explain how it happens that dream-consciousness is only manifest in respect to one point, waking consciousness being present in all others. But even if the dream-consciousness does not provide a satisfactory explanation we find like occurrences under different circumstances. I do not mention the hallucinations of insane persons, because it is exactly the addition of other disorders to their sense-delusions which distinguishes them from the above case. But we find isolated sense-delusions in persons who for some reason or other "are disinclined to correct the creations of their own imagination." Krafft-Ebing mentions the delusions of several famous men—the case of Socrates, who conversed with his Dæmon, and Luther, who threw an inkstand at the devil. Statistical investigations on hallucinations among normal persons have lately been carried out by the English Society for Psychical Research. These results were presented by Sidgwick at the Congress for Experimental Psychology in London, in 1892, and they were discussed in detail by Parish in his work *Ueber die Trugwahrnehmungen (Hallucinations and Illusions)*. Parish holds that sense-delusions in themselves are no indication of disease, but that usually when they

are present an abnormal psychic state may be demonstrated. Such delusions are often caused by strong expectant attention, of which I have already spoken. This is very clearly seen in spiritualistic manifestations, which may be ascribed in great part to hallucinations of the spectators, who think they see spirits or other things in consequence of abnormal processes in their own brain. The visions of religious enthusiasts, which sometimes take an epidemic form, belong here. In these latter cases the sense-delusions result from a particular mental state which may be called a state of expectation. It thus appears that the induction of sense-delusions by means of post-hypnotic suggestion brings about a mental state when the idea reappears, which, although the subject is otherwise awake, has a great resemblance to this state of expectation, and is perhaps even identical with it.

Again, too much weight should not be laid on solitary cases of post-hypnotic sense-delusion, as it is always very rare for the subject to remain quite awake and unable to accept fresh suggestions. As we have already seen, a fresh state of susceptibility to suggestion, which we can only ascribe to hypnosis, readily sets in even while post-hypnotic actions are being carried out. At all events, in these cases of post-hypnotic suggestion the more absurd the latter is and the more opposed to the subject's natural disposition, the more readily does a fresh hypnosis arise. We can explain this process as the result of associations which create a state of dream-consciousness when the process in question does not harmonize with a state of waking consciousness.

Several attempts have been made to explain hypnosis from the point of view of psychology; but they are generally marred by two defects; (1) the assumption that more has to be explained than is really called for—a point I have discussed on p. 225—and (2) an attempt to explain everything by one concept, or rather by some catchword. For this purpose the concept "attention" was formerly much used, because the change in it is most striking in hypnosis; recently, however, it has had to give way to "change in the process of association." I thought, at first, that hypnotic phenomena could be explained by the one word attention, but gave up the idea more than fifteen years ago when I published my own theoretical considerations on the question in an earlier edition

of this book. It is not by the use of any term that we shall gain a clearer insight into the phenomena of hypnosis; that can only be achieved by methodical analysis and a careful consideration of all kinds of analogies drawn from non-hypnotic life. At all events, a diversity of processes such as hypnosis presents cannot be explained by a catchword. Hirschlaff, also, has recently insisted with great justice that if we are to arrive at a definite explanation of hypnotic states we must at least distinguish between the two great groups (*cf.* p. 61).

As the different theories which depend on diversion of the attention are often met with, I shall develop them shortly in what follows. The ensuing explanations are not in contradiction with what has already been said, but are, on the contrary, supplemented by it in many essentials.

We have seen that susceptibility to suggestion is the chief phenomenon of hypnosis. The externally suggested idea of a movement induces the movement, the idea of an object causes a corresponding sense-delusion. However strange and paradoxical the symptoms of hypnosis may appear to us at first sight, there is, as was pointed out on p. 231, no absolute difference between hypnotic and non-hypnotic states. As I mentioned in the passage just referred to, a certain degree of susceptibility to suggestion is normal; but it is not merely confined to sense-delusions, but extends to various other kinds of processes as well. Lipps expresses a very similar view in his *Theory of Self-Projection*,[1] which he considers closely related to suggestion. "Every consciousness of any condition of relativity in another consciousness necessarily implies the specific tendency to a corresponding experience. This tendency is, however, most direct, and consequently most active, whenever such a state of consciousness is conveyed by visual or audible manifestation of the other, or to speak more precisely, when it stands in direct connection with the aforesaid phenomena of the senses." But apart from the case in which the other person gives direct evidence of a condition, an

[1] Lipps's *Theorie der Einfühlung*. I have rendered "Einfühlung"—a word recently introduced into German—by "Self-Projection." Although the latter is an ugly term, and the older writers on æsthetics spoke of "Inner Imitation," I cannot think of a better. A simple example will make Lipps's meaning clear. A spectator at a football match often so "feels himself into" the actual position of some particular player that he participates in that man's individual game.—NOTE BY THE TRANSLATOR.

analogous effect is produced, for example, by a command or an assertion. If A. tells X. to lift his arm, X. is inclined to do it, but he controls the impulse by his own will, by arousing the opposite idea. The following example may make this clearer. When two people look at each other they both often begin to laugh if one assures the other he is going to laugh. This is a favourite joke among children. But the idea of laughing is a necessary condition for its appearance, and the stronger the idea the quicker will laughter ensue. The laughter may be prevented by arousing in ourselves the contrary idea, and if the will alone is not sufficient it must be supported by some sense-impression. Probably many of my readers have made the same observation in their own cases that I made when at school. We had a master who often talked such nonsense that we were obliged to laugh. One day he asked me why I was laughing, and I told him the reason truthfully. Of course he could do nothing to me as I was in the right. After that, he never asked any boy why he was laughing, but we noticed that he invariably gave us bad marks for our exercises when we had been laughing. It was a petty revenge. To avoid that unpleasant consequence of our laughter, we then took to pricking ourselves with a pin directly we felt we could not help laughing. The pain drove away the idea of laughter, and so prevented it. This is an example of the prevention of laughter by indirect means—*e.g.*, the sense of feeling. In other cases the idea of laughter may be suppressed directly by means of voluntarily produced opposing ideas.

Now, it appears that this process often takes place in ordinary life; the idea of a movement, for example, results in a movement (Joh. Müller) if it is not opposed by a contrary idea. Thus the idea of a movement called up in a subject in or out of hypnosis has a tendency to induce the movement. But in waking life this idea can be made ineffectual by other ideas that are inhibitory. Thus we may say that the hypnotized subject has lost the power of arousing certain inhibitory ideas—*i.e.*, in hypnosis the inhibitory ideas are inhibited. We have to thank Heidenhain for having first pointed out the importance of inhibitory processes in hypnosis. Münsterberg also thinks that the sole characteristic of a suggested idea is its power to inhibit an opposing idea, and he considers this applies as much to suggestions in daily life (education, art, politics), as to the phenomena of increased susceptibility to suggestion in hypnosis.

The case is, of course, the same with suggested paralyses. Here the idea of inability to move is suggested. In ordinary life we can make this idea ineffectual by means of others opposed to it; but not so in hypnosis, for here idea of inability to move transforms itself into a real inability.

Let us see if the process in sense-delusions can be looked at in the same light. When we hear some one say, "There is a dog," we are inclined to believe it, as I have pointed out above. But our waking consciousness prevents the suggested idea from becoming a perception, so that we decline to believe in the dog. But in deep hypnosis waking consciousness is supplanted by dream-consciousness, the particular characteristics of which I have already discussed. Sense-impressions are not transformed in the same way as in normal life; the normal course the ideas would take in waking life is inhibited, and this allows the suggested idea to become the corresponding sense-perception. It is the same with negative hallucinations, which we may consider as caused by the inability of the normal course of ideas to correct the suggestion.

We may, then, consider every hypnosis as a state in which the normal course of the ideas is inhibited, no matter whether they have to do with muscular activity, or with the external world. It is impossible for the subject to control the external ideas or to put forward his own; the external ones dominate his consciousness. Now, by attention we mean the power of giving prominence to certain ideas and other mental processes; consequently we may say that there is an alteration of attention in hypnosis.

But attention may be either spontaneous or reflex (Ed. v. Hartmann). When by one act of will we choose one of several ideas and fix our attention upon it, this is spontaneous attention; but when one idea among several gets the upper hand through its intensity or for some other reason, and thus represses other ideas, and draws exclusive attention on itself, this is reflex attention. Now, it is only spontaneous attention that is altered in hypnosis—*i.e.*, the subject's ability voluntarily to prefer one idea to another is interfered with, while reflex attention is undisturbed. Many investigators conceive hypnotism in this way. The works of Durand de Gros, Liébeault, Beard, Richet, Schneider, Wundt, Bentivegni, and Stadelmann are in the main directed to this point, as latterly were Braid's also, after he gave up his original physiological conception of hypnosis.

More recent theories, also, which depend on the doctrine of association, approximate somewhat to this view. Schaffer looks for the chief characteristic of hypnosis in an alteration in the associations, but he does not distinguish between susceptibility to suggestion and reflex excitability in the rough and ready way so many have done. For him suggestions and reflexes are phenomena caused by one and the same change in the mechanism of association; and in both cases the controlling associations are excluded. It is only by direct association that every idea that arises can be at once transformed into an action. As this is not only the mechanism of suggestion but of ordinary typical reflex action as well, Schaffer holds that suggestion is a mental, and anatomical, cortical reflex. From this he concludes that the cardinal symptom of hypnosis is not suggestion, but that alteration in the mechanism of association which manifests itself in direct—so-called primary—associations. A whole series of other authors also perceives that hypnosis, or at least suggestion, depends essentially on a change in the associative processes, though in some cases with more or less modification. I need hardly mention that in giving this prominence to the processes of association we are doing nothing in contradiction to the importance of attention, for both these mental processes are very intimately connected.

It should already be clear from the foregoing explanations that the phenomena of hypnosis have many more points of contact with ordinary life than would be concluded from the discussions and articles written to satisfy a mere longing for sensation. Some of the phenomena of hypnosis, *e.g.*, motion without will, only appear mysterious on the most superficial observation, for we have seen that an idea of a movement is enough to cause a movement without an act of will. These explanations have to a great extent approximated hypnosis to waking life, as well as to the nightly state of dream. Indeed, every one will have recognized that phenomena which were often considered the prerogative of hypnosis very frequently occur in daily life. All good observers will find "hypnotic phenomena" in daily life; they result spontaneously from the chance concurrence and grouping of the necessary conditions.

Although a psychological explanation has thus been given for many of the phenomena of hypnosis, it is hardly likely that every one will be content with it. Thus Wundt has referred

to the defects in the attempts at psychological explanation hitherto given. He specially points out that the various analogies between hypnosis and waking life which I have given certainly exist, and show that hypnosis is a less strange phenomenon than was imagined, but that they do not suffice to explain it; the main point is why the one-sided concentration of the attention, or, as Wundt now prefers to call it, the contraction of consciousness, comes about. Psychology has hitherto been unable to offer an explanation of this point; and Wundt believes that psychology is not to-day able to offer any explanation without the aid of physiology. We must also admit that both the monist and the materialist have a right to put further questions on this subject, especially the following:— 1. What is the state of the central nervous system and the other organs during hypnosis? 2. What is the causal connection between this state and the phenomena of hypnosis? 3. What is the causal connection between this state and the methods which induce hypnosis and put an end to it?

Unfortunately our knowledge of the physiology of the central nervous system is so incomplete that we cannot expect much from it. In spite of the great progress which physiology has made, we must admit that we know much less about psychical functions of the different elements of the brain than would appear from our physiological text-books. As far as the brain is concerned, Hirschlaff thinks that all we can assume is that it, and especially its cortex, must be intact for mental processes to be carried out. The assumption of some investigators that all conscious processes take place in the cortex and the sub-conscious ones in the subcortical centres, is very justly opposed by others who point out that such an assumption lacks proof. According to Flechsig it is anything but certain that the activity of the secondary consciousness is not due to a minor degree of stimulation; and in both cases any stimulus operates through the same cerebral elements (Lœwenfeld). Considering how very fragmentary our knowledge of the central nervous system is, we cannot expect that assistance from experiments on animals that Heidenhain did; for hypnotism is essentially a psychical process, and to draw conclusions from animals about mental action in men would be very daring. The investigation of mental processes may, as we have seen, be undertaken in two ways—(1) by observing individuals, and (2) by calling the subject's memory to our aid. This last could not be done in

the case of animals. But any observations of animals must be very elementary, for we can only obtain a glimmering of the processes of their consciousness from external signs. Further, many physiologists make the grievous error of assuming that the successful stimulation of any portion of the brain proves that an act of will originates in that spot. Heidenhain and Bubnoff have made numerous experiments in electrical stimulation of the cortex of the brain on dogs poisoned by morphia. But the conclusions which these authors attempted to draw about the action of will in men from such experiments must be pronounced mistaken till it is proved that the impulse of the will is an electrical stimulation. For the above reasons I consider Heidenhain's endeavours to draw conclusions on hypnosis in man from experiments on dogs too hazardous.

In spite of all these weighty doubts many attempts have been made to give a physiological explanation of hypnosis. Heidenhain must here be mentioned first. He supposes that the cause of the hypnotic states is an inhibition of the action of the ganglion cells of the cerebral cortex, induced by continuous weak stimulation of certain nerves, and he thinks that this inhibition is analogous to reflex paralyses, as in these also the functions of the ganglion cells are impaired by peripheral stimuli.

But even if we take the inhibition of the action of the ganglion cells for granted, Heidenhain's theory does not explain the connection between this and the means used to induce hypnosis. For (1) Fixation unaccompanied by mental effort does not lead to hypnosis. Braid and Berger considered that there must be concentrated attention as well; (2) in any case there would be no causal connection here between the purely psychical methods and hypnosis.

Besides this, Heidenhain starts from a mistaken premise when he supposes an inhibition of the ganglion cells. He concludes this inhibition from the lowered state of consciousness during hypnosis. But consciousness expresses itself in many ways during hypnosis. The processes of consciousness seem merely to be concentrated on one point, which is chosen by the experimenter and is removable at his pleasure. Heidenhain maintains, like Despine, that the subject is not conscious of the external stimuli. Heidenhain was led to this erroneous view, which has lately been taken by Landois and

others, by his almost exclusive observation of the movements of imitation (*cf.* Exp. III., p. 35). He supposed that the subject received a sense-impression of a movement and copied it, though it did not result in a conscious idea. But he should have proved this first. From what does Heidenhain conclude that the sense-impression was unconscious? From the loss of memory after waking? But loss of memory does not prove that we have to do with an unconscious movement. The fact that subjects remember in the hypnosis, and also in later hypnoses, the imitative movements they have made, renders it far more probable that consciousness persists. Besides, there is often no loss of memory whatever after waking, and finally, a suggestion made during hypnosis will invariably cause subsequent memory. As far back as 1880, when Heidenhain declared his views about the imitative movements, O. Rosenbach explained that the processes were certainly mental, and not as Heidenhain thought, unconscious somatic reflexes. Unfortunately Rosenbach did not at that time explain his own views in detail. Berger and others agreed much later that these processes were mental.

I also was enabled to study the imitative movements. They only take place when the hypnotic subject has an idea of them, and knows that he is to make them. If they were unconscious reflexes, the subjects would imitate any person's movements. But they only imitate the one person who exists for them, *i.e.*, the experimenter, and him only when they know they are intended to do so. A clear idea of the movements to be imitated is the first condition. I do not contest that when such experiments are often made, the imitation may not become mechanical in later hypnoses, as happens in waking life. However, at first a clear idea is necessary; but physiology localizes the seat of ideas in the cerebral cortex, and there is no reason for placing them in another part of the brain in hypnosis, so that there is no proof of the cortex becoming inactive. But perhaps there are mental processes in the subcortical centres during waking life, about whose extent we know nothing. We know just as little whether, or in what degree, the cortex takes part in unconscious activity in waking life. But be this as it may, there is no reason for assuming that the cortex is inactive in hypnosis. Pathology gives us some information on this point. Many partially paralyzed persons whom I hypnotized, and in whom the *capsula interna* had been

injured by apoplexy, made no imitative movements in hypnosis with the paralyzed side, any more than they made voluntary movements either in, or after, hypnosis. But in this case exactly that part was excluded in which we place the conscious ideas of movement—*i.e.*, the cerebral cortex; the centres which cause the unconscious reflexes were not excluded. As, however, there were no imitative movements, this shows that without that part of the brain in which ideas are produced, there are no imitative movements. For these reasons I reject the comparison between hypnosis and the state of Flourens' pigeon when its brain was removed. As is well known, Flourens experimented on pigeons, whose cerebrum he had removed. Untouched they remained quiet, but when excited they made all sorts of purposive movements, as if to walk, fly, etc. But this behaviour of the pigeons must not be compared, as has been done, with the imitative movements of a hypnotic subject. These imitative movements only occur when the subject knows he has to make them, and that implies a mental process that is excluded in the case of the pigeons.

My reasons for not completely rejecting this assumption of inhibition of the cerebral cortex, in spite of Heidenhain's erroneous views, are as follows. Although single ideas, single processes of consciousness, are not absent in hypnosis, yet the influence of the will on their course is limited. According to many physiologists the cause of this decrease in the power of the will is to be sought in a functional disorder of the cerebral cortex; but here we are treading on the delicate ground of hypothesis. Cullerre, supported by Ferrier's experiments, thinks there is a functional disturbance in the anterior half of the cerebral cortex in hypnosis. He thinks that though this is not the seat of the motor centres, the centres here have a regulating influence on the motor centres, but that this influence is removed in hypnosis.

The works of many doctors unfortunately lose in value, because their authors expect too much of the physiology of the central nervous system, especially as regards the localization of functions. This applies to Landmann's work, *Die Mehrheit geistiger Persönlichkeiten in einem Individuum*, in which he attempts to localize the processes of hypnosis in various regions of the brain. Others do not try to localize the hypnotic subject's loss of will; Bennett, for example, put forward a physiological theory as early as 1851. He had already recog-

nized that it is not the genesis of separate ideas which is prevented in hypnosis, but the voluntary synthesis of them. And as the ideas originated in the ganglion cells, Bennett supposed a functional disturbance during hypnosis in the nerve fibres—the so-called fibres of association—which connect them. Jendrássik took somewhat the same view later on. It is easy to see that this theory is only the physiological theory of association in another form.

Other investigators went further: they did not merely ask what parts of the brain are inactive; they tried to find the anatomical substratum of the activity. Schleich has applied his theory of sleep, which he bases on the function of the neurolgia, to hypnosis as well. He considers the neuroglia an organ which diminishes or increases the insulation of the ganglion cells, and he assumes that augmentation of the volume of its plasma increases its insulating power. He then proceeds to explain how this augmentation of the plasma in hypnosis is brought about by reflex action, but in ordinary sleep is the necessary result of external causes which are periodically at work (onset of night, absence of sunlight). To this and other physiological theories Hirschlaff rightly objects that they do not explain how this inhibition of a subject's mental functions invariably undergoes an immediate change at the experimenter's pleasure.

I will now give Hirschlaff's account of a further histological theory of sleep, which has been put forward by Duval, Lanoitte, and Pupin. These authors base their theory on the discovery made by Golgi and Ramon y Cajal that nerve-cells come into communication with one another simply by the contiguity of the terminal arborization of the axis cylinder of one nerve cell to the protoplasmic processes of another. Anything which tends to loosen or break this contact must therefore tend to bring about inhibition or cessation of the functions of the cells. But according to the authors mentioned, the work of all nerve elements is carried out by currents which are propagated in a manner similar to that of electric induction. This would justify the assumption that all functional disturbances of the nervous system, including hypnosis and normal sleep, depend upon obstruction or interruption of conduction by contact; or, to put this in other words, contraction or total destruction of those finer terminal arborizations of the neurons which, under normal conditions, establish contact, renders the transmission

of stimuli impossible. This assumption is all the more plausible if we remember that nerve-cells are originally nothing but amœbæ which protrude or retract pseudopodia. Like tea, coffee, or morphia, a psychic moment may stimulate or concentrate the activity of some mental region or other, and may so act on the amœboidism of those nerve-terminals that are in contact as to produce a functional change in the nervous system. This is the sense in which suggestion and hypnosis are to be assumed to act. According to this theory, hypnosis has the power of causing protrusion and retraction of the protoplasmic processes, and can thus create inhibitory centres, block morbid paths, and restore interrupted connections; in short, it can make the contact necessary for nervous induction, or break it when abnormal. Hirschlaff thinks that this theory, which counts numerous adherents in France, has the advantage of explaining all the enigmatical phenomena of hypnosis and suggestion. But it suffers from the misfortune of being based on false premises. The theory of the contiguity of the neurons was long accepted by physiologists, but recent investigations have shown that fusion of the neurons does take place. But apart from this, Hirschlaff thinks that it requires a great stretch of imagination to conceive how innumerable protoplasmic processes can lay themselves together, approach, or recede from one another, especially when one takes into consideration the modern view that all mental processes are located in the cerebral cortex, and that an idea, or definite mental act, is not confined to one ganglion cell, but depends upon a whole series of such cells whose orderly co-operation is necessary for the production of the simplest mental function.

Change of the circulation of blood in the brain plays a great part in physiological theories of hypnosis. Braid thought of this, and Carpenter supposed cerebral anæmia, just as Hack Tuke has more recently imagined a partial spasm of the vessels. Heidenhain also at first supposed that anæmia of the brain was the cause of hypnosis. He soon gave up this opinion, for two reasons. (1) The investigations of Förster with the ophthalmoscope disclosed no sort of change in the vessels at the back of the eye in hypnosis. The changes here noticed correspond to those which take place in the brain when its blood-supply is altered. I can confirm this by my own observations. (2) Heidenhain saw hypnosis appear in spite of inhalation of nitrite of amyl, which causes hyperæmia of the

brain. Salvioli and Bouchut have, on the contrary, found cerebral hyperæmia during hypnosis. Krarup finds a narrowing of the internal carotids, an enlargement of the external carotids and of the vertebral arteries during hypnosis. Regnier studied the carotid pulse with the sphygmograph during and after hypnosis. He concludes from the changes in the carotid pulse that there is retardation of the circulation and congestion of the cerebral capillaries during hypnosis. He thinks that his view is confirmed by the ophthalmoscopic investigations of Luys and Bacchi who found hyperæmia of the back of the eye Laker concluded that there is a change in the amount of blood in the cerebral cortex during hypnosis, because he once observed œdema of the face after hypnotic sleep. This he assumed to be an analogous phenomenon to the facial œdema observed in the angioparalytic forms of hemicrania. But he was more careful in his other conclusions. Tamburini, Seppilli, and Kaan also investigated the circulation of the blood during hypnosis, but only in connection with Charcot's stages. In the same connection Meynert investigated circulation in hypnosis; he speaks of a strong cramp of the musculature of the vessels in hypnosis. The three other last-named investigators used several methods:—(1) Mosso's method, which determines the volume of an extremity, and concludes from a decrease in the mass of blood contained in it, an increase in the mass contained in the brain. (2) The action of cold and hot compresses on the head (Kaan), which cause anæmia or hyperæmia. From the resulting changes, *i.e.*, from the cessation or modification of the hypnosis, a conclusion is drawn between this and the mass of blood in the brain. (3) Ophthalmoscopic investigation of the vessels of the retina. I do not enter into details of the different experiments, because they are valid for the stages of Charcot alone, and therefore only have a historical interest. Apart from this, these are quite untrustworthy methods for ascertaining the mass of blood in the brain. Brodmann made a series of excellent investigations on a subject who had been trepanned, and proved that there is no antagonism between the circulation in the brain and in the arm, either in sleep or in the waking state. Mosso's method may, therefore, be dismissed as of no account. But Hirschlaff points out that apart from this, Brodmann has shown that the relative conditions of the circulation of the blood in the brain are as yet of no general

use in discussing the theory of sleep, and of course we must be just as cautious in regard to hypnosis.

Döllken has put forward a theory based on the condition of the blood and the state of the nerve elements. According to him arbitrary reduction of the activity of the sense-organs accompanied by reduction of the associative activity to a minimum produces anæmia of the brain. The cortex and cortical paths participate in this process, but what share the subcortical elements have in it, has yet to be shown. According to Döllken a certain tonicity of the nerve-elements results from this anæmia, rendering them responsive to stimuli far below the normal limit in strength. The nerve-elements are thus enabled individually to acquire a better state of nutrition and the increase of functional capacity arising therefrom. Still this particular tonicity might also be considered primary.

Failure to distinguish clearly between cause and effect is one of the sources of error frequently found in those theories of hypnosis which are based on the state of the circulation of the blood in the brain. Even when there is a change of circulation in the brain in hypnosis, it is a mistake to think that the changed circulation causes the changed functions. Haas very properly points out that when a person who feels ashamed blushes, it is not the blushing that causes the feelings of shame. As a muscle needs more blood when it is at work, but does not work more because more blood flows to it; as the stomach when digesting needs more blood than when it is inactive, it is also not improbable that the brain, or portions of it, when they are active need much blood, and when they are inactive but little. Then if we take the vasomotor disturbances as proved, it is by no means proved whether they are the cause or the effect of hypnosis.

In fact, Cappie takes the opposite view. He thinks that the increased activity of the motor centres in hypnosis draws too much blood to them, thereby causing anæmia of the other portions of the brain which are necessary to consciousness. But this theory does not give a satisfactory explanation, for it arbitrarily opposes the motor centres to the parts of the brain necessary to consciousness, and there is always consciousness in hypnosis. The principle from which Cappie starts is the one put forward by Brown-Sequard. He thinks that hypnotism is the sum of dynamo-genetic and inhibitory acts—*i.e.*, that the increased action of certain parts of the brain (dynamo-

genetic act) causes decreased action of others (inhibitory act). That Fechner recognized these principles very clearly is shown by the fact that he spoke of the antagonism existing between the various spheres of psychophysical activity. He was also fully aware of the important parts played by nutrition and the circulation of the blood. The theory of an antagonistic distribution of the blood was used later on by Meynert to explain morbid mental states.

Just as Cappie assumed that there is during hypnosis an altered activity of certain centres and associated changes in the circulation, so Wundt has suggested as the physiological basis of the phenomena of hypnosis a double interaction, neurodynamic and vasomotor. The irritability of any central element depends not only on its own condition at the moment of stimulation, but also on the condition of the other elements with which it is in association, in such a way that excitation of the neighbouring element lowers its own excitability, while a condition of inhibition favours discharge of energy. This is the neurodynamic reaction. At the same time, according to Wundt, there is a vasomotor reaction, for the blood contents and functions of the organs stand in such a relationship to one another that increase of function produces increased flow of blood, decrease of function, depression of the blood flow. Wundt further argues that neurodynamic compensation favours vasomotor compensation and vice versâ, and seeks in this way to explain the phenomena of hypnosis by viewing the chief symptoms from this standpoint. He then refers to the centre for apperception, which, hypothetically, he regards as the substratum of the process of apperception. He believes that we have here an essential difference between dreaming and hypnosis, though he does not ascribe absolute importance to this difference. Certain phenomena of inhibition of the processes of the will and the attention are common to both, also a tendency to increased excitability of the sensory centres leading to hallucinatory interpretation of sense-impressions. But there are distinguishing characteristics; attention is only partially altered by suggestion in hypnosis, but the inhibition of the will in sleep affects both apperceptive and motor processes. It is from this psychological difference that Wundt develops his physiological theory. In dreams those central regions which are associated with the process of apperception are more or less in a state of inhibition, and

nearly all the compensatory excitation is conducted to the sensory centres, but under certain circumstances in hypnosis a compensatory increase of irritability arises in the apperception-centre in opposition to the existing partial inhibition.

As I have already mentioned, Wundt has raised against my attempts at a psychological explanation, the objection that I have not answered the question why in hypnosis consciousness is not contracted. I have, however, put the questions which must be met by physiological answers (p. 268). The third question I put is this: What is the causal connection between this state and the methods which induce hypnosis and put an end to it? I do not find that this question is in any way answered by Wundt's physiological theory, and against Wundt's physiological explanation I must raise the same objection as he has raised against my psychological speculations. In addition to this, the centre of apperception as supposed by Wundt, is little more than a hypothetical assumption.

Vogt's theory is based on similar principles to Wundt's, but it contains a series of comprehensive and elucidatory ideas, especially that which he terms "constellation." By this Vogt understands the whole cerebral mechanism corresponding to a mental process, and he considers it the resultant of conscious and unconscious processes. He thinks that hypnosis is brought about by an alteration of this constellation, and he refers the phenomena of suggestion partly to inhibition, partly to increased irritability, and partly to the opening up of fresh paths. "Let us, for example, consider the mechanism of catalepsy. I lift up a hypnotized subject's arm. This passive movement causes a sensation of movement. The associative paths leading from the centre in which this idea of movement is localized are rendered non-conductors by the dissociative effect of hypnosis. In consequence of this the nerve-wave (neurokym) caused by the passive movement of the arm is, for the most part, transferred to the centripetal paths leading from the centre in which the idea of the movement was aroused, and thus sets up muscular contraction corresponding to the position passively imposed on the arm." Vogt also endeavours to explain other phenomena by the action of the neurokym (nerve-wave)—*i.e.*, by the nervous excitation that reaches the cerebral cortex. A further part of Vogt's theory bears on the origin of sleep, ordinary and hypnotic. He attempts to prove that sleep is caused by the

stimulation of certain centres, more especially the reflex centre for shutting the musculus orbicularis occuli, by the action of the neurokym or nerve-wave. But certainly Vogt thinks that a vasomotor reflex centre here plays a more important part, its stimulation causing an increasing anæmia of the brain and thereby drowsiness and sleep.

I shall not enter into any criticism of Vogt's theory, as the same objections apply to it and nearly all other physiological theories, as were raised to Wundt's.

Finally, I mention the theory of Preyer, who puts the matter thus:—An activity of one hemisphere of the brain results in hypnosis; fixed attention causes a rapid accumulation of waste-products in the parts of the brain which are active, and by this a quick local consumption of the oxygen of the blood is caused. In consequence of this, favoured by the failure of the ordinary change of stimulus of the nerves of sense, there is a partial loss of the activity of the cerebral cortex. The partial loss of activity of one region would then explain the increase of activity of the other, because the inhibition would disappear. Bernheim objects to this that it does not explain a rapidly induced hypnosis, for it is hardly conceivable that waste-matter should accumulate so rapidly. Similarly, the sudden termination of hypnosis is not consistent with this. As we have seen, the one word "wake" is enough to end the hypnosis at once. We should be obliged to suppose that the simple idea of waking was able to dissipate the waste-matter or make it of no effect.

I do not think that any of the physiological theories hitherto propounded can be considered in the least degree satisfactory. This does not imply any reproach to physiology in general. But it should be remembered that as far as physiological theories go we have about reached the limit of our knowledge. The connection between mind and body is still purely hypothetical. The fact that stimuli which differ to but a trifling extent here produce such different effects is a favourite objection to materialistic theories and physiological explanations of hypnosis. Ludwig Busse has recently called attention to this in his excellent work, *Mind and Body*. The owner of a ship who receives a telegram from the captain, "Reached the Cape," would be very differently affected if the telegram were "Beached the Cape," yet the physiological stimulus differs but

very slightly in the two cases. We must admit that similar considerations show the value, or rather the valuelessness, of physiological theories. I certainly think that as long as we are totally unable to understand how an idea, roused for example by the word "wake," changes the subject's whole state, we must be very sceptical as regards physiologists' theories. Even so unbiassed an observer as Lotze has ironically stated that, according to his own private statistical reckoning, the great discoveries of physiology had an average existence of four years. There may be some exaggeration in this, and I do not think it should be applied to all branches of physiology. But the endeavours of some investigators to explain mental processes by means of our present knowledge of the central nervous system point to a disquieting tendency to over-estimate physiology; and I think that Meynert's assertion that cerebral physiology is no longer a problem will cause many to share my doubts. But I think I can best show how devoid of all value physiological theories of hypnosis are, by calling attention to the contradictions between the views of Mendel and Ziemssen. Mendel explains that in hypnosis we have to do with a strong stimulation of the cerebral cortex, while Ziemssen declares that the cerebral cortex is too little stimulated and the subcortical centres too much! Under these circumstances we may, surely, be allowed to hope that in future less will be asserted and more will be proved. Such contradictions as those between Mendel and Ziemssen would be inconceivable if it were not for the presence in their works of just such speculations as those with which medicine is in the habit of reproaching philosophy.

CHAPTER VIII.

THE MEDICAL ASPECTS OF HYPNOSIS.

It is certain that the present interest in hypnosis depends upon its therapeutic utility. According to the generally accepted view, hypnosis is a state of increased susceptibility to suggestion, although, as we have seen, other factors play a part in it. We shall see that in its therapeutic application other properties of hypnosis may also have to be considered. At all events increased susceptibility to suggestion plays an important part. It follows from this that suggestibility exists apart from hypnosis, and that the therapeutics of suggestion in, and out of hypnosis, are complementary. It is the Nancy school which has pointed out that there are many suggestions without hypnosis, and it was the first of all to recognize the therapeutic value of purely empirical suggestion. The Nancy school, also, has never denied that many were cured or relieved by suggestion long before hypnotic suggestion came to be studied. A patient's conviction that his condition will improve has always contributed to such improvement. Every capable practitioner uses this suggestive treatment, which is as old as disease. Most of the miraculous cures one hears of we may now consider the results of *the unconscious employment* of empirical suggestion. There must in such cases be some means of conveying the suggestion; and this can be accomplished equally well by the influence of some distinct personality or by an object. The first is the case when, for example, special powers are ascribed to a particular person, as in the healing of Jeroboam, whose hand, the Bible tells us, was motionless, but recovered the power of movement through the prophet's words. In the second case an object conveys the suggestion—for example, a spa, a particular medicine, etc.

The great point in the therapeutics of suggestion is to implant in the patient's mind the conviction that he will be cured. But the physician is not always able to achieve this

even when the patient has great confidence in him. In such a case hypnosis is a good means of implanting the idea and causing it to take root, provided the sleep be deep enough. If we admit that confidence occasionally facilitates the cure of disease, we must also admit that hypnosis is a valuable accession to therapeutics.

We have to thank the late Dr. Liébeault, of Nancy, for having been the first to use hypnotic suggestion methodically in therapeutics. It is true that verbal suggestion was occasionally used by the old mesmerists, Kluge, Lausanne, Jobard, and many others, as Du Prel and Pick justly point out. But method was entirely wanting. Braid also recognized and used suggestion, but he did not recognize its fundamental significance in hypnotic therapeusis; this fact has to be mentioned in spite of the opposite view held by some investigators, Bramwell, for example, and it in no wise detracts from Braid's great services. Some investigators in Breslau, for example, Friedberg, and more especially Berger, in 1880, concluded that hypnosis was a therapeutic agent. Berger saw a hemiplegic patient make movements in hypnosis which he could not make awake. He saw sufferers from locomotory ataxy cease to stagger during hypnosis and for a short time after. But Berger, to whom the simplified method of Liébeault was unknown, also overlooked the great importance of suggestion. Many people, who had never heard of Liébeault had seen that, from a medical point of view, a state in which contractures and paralyses, analgesia and pain, etc., could be induced and removed, must be of great importance; but Liébeault was the first to find the right path, while Bernheim, Wetterstrand, Forel, and others, developed the methods and made them known.

It is not astonishing that objections have been made to the therapeutic use of hypnosis. The history of medicine teaches that hardly any essential progress has ever been made in that science without a struggle. Every one knows how the use of quinine, of vaccination, and of emetics, was contested; how the water-cure was treated with contempt, and how R. Remak was attacked in Germany before the galvanic battery was accepted in the medicine-chest. Ewer relates that when Lingg laid his method of treatment before the highest officials in the land he was dismissed with the remark that there were quite enough jugglers and mountebanks about without burdening the country

with fresh ones. And Ewer himself, who had done so much to introduce massage into Germany, had often enough to put up with a supercilious shrugging of shoulders and deprecatory smiles when he first ventured to talk about massage before physicians. And yet in the present day all these methods are reckoned among the most highly-valued treasures of the medicine-chest. Certainly some people now try to prove that it was only hypnotic treatment and not mental treatment that was opposed. Now, since psycho-therapeutics is almost universally recognized a legitimate branch of medicine, whereas the importance of hypnosis is still under discussion, those who opposed hypnosis, but accepted mental treatment, might consider themselves justified. Still, to prevent any fairy tales creeping into the history of psycho-therapeutics, I must lay the strongest stress possible on the fact that, in reality, the attack was directed more, or at least quite as much, against psycho-therapeutics in general, than against hypnosis in particular. *In 1880 Ewald*, for example, *who was at first opposed by no one but me*—Forel and others certainly joined the opposition soon after—*distinctly stated that psycho-therapeutics was only a form of quackery, and quite unworthy of being called medical treatment.* Hypnosis was *not* specially singled out for attack; it was the acceptance of psycho-therapeutics as a legitimate form of treatment that was opposed.

Although some people may at first have ascribed too much importance to hypnotic treatment, the attack on it was a failure. I am not going to point out that it is often impossible to draw a sharp line between hypnotism and suggestion, or between the latter and mental treatment in general. But one thing must not be forgotten; it was the study of hypnosis that first proved how much can be achieved by mental influence in therapeutics. Although, as already pointed out, suggestion and mental treatment had often been used before, the full extent of the efficacy of these methods was only established by hypnotism, which thereby essentially brought about the development of modern psycho-therapeutics. It was hypnotism that first drew general attention to suggestion in waking life, to the questions of work and occupation, to medical measures in education, to instructing the patient, to diversion, the power of the will, and many other branches of psycho-therapeutics which had all been too long neglected. And even if we can to-day dispense with hypnosis in many cases in which it was used

before general mental treatment was sufficiently understood, we must not forget that we owe our knowledge of these accessory therapeutic methods to hypnosis. We are now, therefore, able to dispense with hypnosis in many cases in which it formerly appeared indicated. Still, I think that in the present day it will be found an essential adjunct to other mental methods of treatment in many cases. I cannot altogether agree with Heller, Jolly, and others that exactly the same results can as *invariably* be obtained without, as with, hypnosis. We should rather, in the present day, employ hypnosis in those cases in which mental treatment is indicated but cannot be efficiently carried out with the patient in the waking state. Hypnosis is, therefore, of value to medicine in two ways: (1) it has provided an inducement to the further study of psychotherapeutics in general, and (2) it is a remedy in itself.

It has often been asked why so many authorities have pronounced against suggestive therapeutics. There are three answers:—(1) Even an authority may be wrong—indeed, a real authority does not believe in its own infallibility; (2) all so-called authorities are not necessarily authoritative; (3) many who are authorities in one field are just for that reason not so in another. Much injury to science, and particularly to medicine, has arisen through these three points being overlooked. Let us consider the last two farther.

In all sciences, besides the real authorities there are men who are mistakenly supposed to be so. Fashion often makes "authorities" out of those who have no real scientific greatness. A man is called an authority; but when it is asked what he has done there is shrugging of shoulders, for often he has done nothing. Such pseudo-authorities—there have always been such persons—are much inclined to pass judgment on questions they have not examined. Their position and credit is due to a faculty, which a clever writer, Karl von Thaler, a short time ago called the art of putting oneself on the stage. Their judgments are of no value, but it is interesting to note that no small number is adverse to hypnosis.

But I do not mean to say that all who have opposed the therapeutic use of hypnosis are pseudo-authorities; on the contrary, true authorities, such as Meynert and others, have expressed themselves decidedly against it. But as mentioned above, it does not follow that because a man is an authority on one matter he has a right to claim authority on another. A great historian or astronomer is not in a position to pass judgment on medicine. Now, many of those who have objected to the therapeutic use of hypnosis are authorities on matters that have nothing to do with therapeutics. Physicians, as well as laymen, often lose sight of this. A man may be eminent in the histology of the brain, and yet be incompetent in therapeutics. And there is, nowadays, no more connection between the art of healing and the histology of the brain than there is between it and astronomy. If I may call the art of healing a science, the histology of the brain is something quite apart from it—at least in the present day. Feuch-

tersleben, whom no one will accuse of dislike to medicine or anatomy, since he was their most ardent admirer, has expressed the opinion that the art of healing should not be confused with the knowledge of anatomy. Therefore I consider the judgment of a man who may be an authority on some science which, like the histology of the brain, is necessary and valuable, but has no intimate connection with the healing art, is of as little weight here as the judgment of an astronomer would be.

We should always bear the foregoing considerations in mind when dealing with other cases. As I mentioned on page 31, the committee of the Prussian Medical Board was requested by the Kultusminister [1] to institute an inquiry into the therapeutic value of hypnosis. From this it might appear that medical boards have a just claim to sit in judgment on hypnotism. It would be better if medical boards confined their attention to matters that concern them; they do not constitute courts of reference on purely scientific questions. The report on hypnotism submitted by the Berlin-Brandenburg Medical Board shows in the clearest manner possible how little importance attaches to such documents. It has been vigorously criticized by Forel, Lœwenfeld and others. Lœwenfeld very properly points out that no one who wished for a competent opinion on the electric light treatment would apply to medical men, as they are not specialists on the question. Now Mendel, Munster, Gock, and Ascherborn sat on the commission, but up to the present no one has heard that the three last-named ever had any experience of hypnotism. As regards Mendel, his hostility in public to hypnotism has long been known, but it is also a fact that he does not disdain to use it in his private practice, even recommending a specialist when he deems it necessary. "From this it appears that, in Mr. Mendel's opinion, hypnotism is only a scientific and permissible method when sanctioned by Mr. Mendel." Gumpertz was quite right in bringing the contradiction between Mendel's public utterances and private practice to light. Perhaps those persons who consider the report of the above-named board of medicine authoritative, may be induced by these explanations to modify their opinion somewhat. The East Prussian Medical Board set to work in a much simpler but more scientific way. A list of questions was sent out to medical practitioners, and the result of the inquiry which was

[1] Note by translator.—Kultusminister = Minister of Ecclesiastical Affairs, Public Instruction, and Medical Affairs (all three offices combined in one Minister).

published showed that only a few doctors had used hypnosis therapeutically, that some cures had been effected, and that no injurious effects had been observed.

Of course, the foregoing considerations are not in any way intended as an attack on the right of opposition. Besides, really scientific opposition always tends to advance science; only the opposition should not be allowed to take the form of an *à priori* decision, as some of the opponents of hypnotism have done. The therapeutic importance of hypnotism should be tested, as Virchow, who was certainly sceptical, recommended; not rejected absolutely, as was done by some "scientific investigators."

We will now consider singly the objections made to hypnotic treatment or to suggestive therapeutics.

A chief objection was made by Ewald of Berlin, who "decidedly protested against calling suggestion medical treatment." He did this in the interest of physicians. Forel's reply to him will make it clear what he meant.

"Ewald protested against the expression 'medical treatment by hypnotism.' He said that medical treatment meant medical art and medical knowledge, and that every shepherd-boy, tailor, and cobbler could hypnotize; only self-confidence would be necessary. I think we have much more right to protest against this way of treating a scientific question. Has not medicine drawn a countless number of its remedies from the crudest empiricism, from the traditions of the shepherd-boys? Cannot every cobbler inject morphia, apply blisters, and give aperients if he has the material? Yet we do not despise these remedies, nor baths, nor massage, etc. But Professor Ewald deceives himself greatly if he believes that a delicate agent like hypnosis, which directly affects and modifies the highest and most refined activities of our minds, could be manipulated by a shepherd-boy and ought to be handed over to him. Medical science and psychological knowledge, the ability to diagnose and practise, are all necessary to its use. It is true that laymen have succeeded with it, just as charlatans have succeeded, and continue to succeed, in all provinces of medicine. Should we on that account leave the practice of medicine to them? Long enough, much too long, science has left the important phenomena of hypnosis to 'shepherd-boys and their like'; it is high time to make up for the delay, and to devote ourselves to a thoroughly scientific examination of the series of phenomena

which can complete our views of psychology and of the physiology of the brain. Medical therapeutics must not remain behind when great results are to be obtained. But these results can only be obtained by a thorough study of the proper hypnotic methods."

Ewald's objection amounts essentially to this: hypnosis should not be called medical treatment because it is unscientific and perhaps unprofessional. But this conviction is easily aroused in the case of a remedy we wish to rescue from the charlatans. The novelty of the remedy makes it appear alien to the practices of the medical profession. I have already discussed this point in detail in another work.[1] In any case we cannot fail to recognize that they who endeavour to gain the sole use of such a remedy for the medical profession are thereby fighting against quackery, whereas men who, like Ewald, simply set down the use of such remedies as quackery, and therefore to be excluded from medical treatment, are in reality aiding quackery, although perhaps unintentionally. As a matter of fact when doctors emphasized the dangers of hypnosis and claimed that the practice of hypnotic treatment should be restricted to members of the medical profession only, Emil Muschik-Droonberg disposed of their claim by referring to Ewald's statement that any shepherd-boy could hypnotize.

Benedikt's objection to the use of hypnotic treatment, because of the mysterious impression it causes, belongs to the same category as Ewald's. But apart from the fact that there is less mystery about the matter than was formerly supposed, it would be perfectly indifferent to a practitioner whether a remedy took effect from the mysterious impression it made, or through suggestion, or through chemico-physical influence. The point is that it does act, not in what manner it acts. Certainly Rosenbach has protested against the use of suggestion in therapeutics, and he is a rational investigator and thoroughly recognizes the importance of psycho-therapeutics, which he was led to appreciate by studying hypnosis. Rosenbach also lays stress on the mysterious character of the impression produced, but his objection to suggestion lies essentially in the fact that he expects better results from other therapeutic measures. Besides, I think I have shown in the theoretical section (p. 267)

[1] Moll, *Ärztliche Ethik*, Stuttgart, 1902, p. 274 *et seq.*

that a thing is often considered mysterious which is really a phenomenon of every-day occurrence. Moreover, Benedikt recommends, that in order to lessen the impression of mystery, hypnosis should be induced by the use of a magnet. But as a magnet only acts by suggestion, according to present-day opinion, Benedikt has unwittingly recommended the use of a mysterious agent.

If I believed that in some cases a mysterious agent would be useful to a patient I should not hesitate to use it; for were I to do so I should be neglecting my duty as a doctor, which is of more importance than any scientific signboard. Naturally a physician should not make use of a remedy the employment of which is contrary to medical ethics; but I am firmly convinced that he has no right to deprive a patient of the benefits of a remedy because he thinks it acts mysteriously. Consequently, I should not hesitate in certain cases to send patients to some miracle-working spring—Lourdes, for example; and, in fact, fifty to sixty patients are yearly sent to Lourdes from the Salpêtrière (Constantin James). Charcot has expressed the same opinion in his well-known work *La Foi qui guérit.* When questioned about faith-healing, he replied among other things: *Elle intéresse d'ailleurs tout médecin, le but essentiel de la médecine étant la guérison des malades sans distinction dans le procédé curatif à mettre en œuvre.* It cannot be denied that faith and emotional excitement produce many results at Lourdes. We may well believe Rommelare's statement that the water from Marseilles cured a patient who believed in it. But even if hypnosis were only effectual from its mysteriousness, its use would not be thereby contra-indicated.

The temporary loss of will can hardly be considered an objection to hypnotic therapeutics from the ethical standpoint, though it has occasionally been brought forward. If it were, we should have to give up the administration of chloroform, for there is loss of will in chloroform narcosis. The main point is to choose a trustworthy experimenter. We only take chloroform from a person whom we can trust to administer the anæsthetic without danger, and whom we believe will take no advantage of the loss of will induced.

But a far more important objection than any of the above is the danger of hypnosis. Even if we cannot consider hypnosis absolutely safe, the dangers should not be exaggerated. "*The best assertion that can be made about a remedy or method of cure*

is that it might also do damage; for what can never do positive harm can never do positive good." These words, which were used by Rust in speaking of artificial somnambulism, should always be remembered if we wish to establish the true value of a therapeutic method. What would remain of our medicine-chest if we threw away everything that might occasionally do harm? How often has a drug that might prove fatal been administered even in a case in which there was no danger to life? I need only mention deaths from chloroform; Thiem and P. Fischer say that according to an old calculation there is one death to every thousand administrations of chloroform, but this estimate has since been modified. In many surgical operations there is a remote possibility of a fatal issue, if only from infection with erysipelas. With regard to other means and methods, I must refer the reader to my work on *Medical Ethics*. In addition to drugs that are generally known to be dangerous, such as morphia, strychnine, and belladonna, others, such as potassium chlorate, trional, potassium iodide, antifebrin, quinine, etc., which are often considered harmless, have frequently produced fatal results. Eduard August Schröder has collected hundreds and hundreds of cases which have terminated fatally through the use of these drugs. Billroth also in his day called attention to the great dangers from carbolic acid; and Lewin, in his work *Die Nebenwirkung der Arzneimittel*, gives a long list of serious, though not fatal, after-effects which have been observed from the use of such drugs. Such accidents are not confined to the internal administration of drugs; they are met with in all other kinds of medical procedure. Mendel's treatment by suspension has not only caused great injury, but even death.

Whether or not there are dangers in the use of drugs is not the question. We must ask:—(1) Do we know under what conditions the danger appears? (2) Can we remove these conditions and the consequent danger? (3) And if we cannot, does the advantage to be gained by the patient outweigh the danger he runs? Now, we can answer these questions as regards hypnosis, and at the same time institute a comparison between the latter and other remedies. We know under what conditions hypnosis is dangerous, which we do not know about many other remedies, such as morphia, chloroform, etc. Hypnosis may prove harmful if we employ improper methods or disregard contra-indications. We may therefore answer the

first two questions in the affirmative, since we know the contra-indications. With regard to the third question, it is possible that unpleasant concomitant effects may be produced when methods that are not quite so harmless have to be employed if we are to attain our object. But I think that if a physician is conscientious in his efforts, the discomforts to which the patient is exposed—a short headache, watering of the eyes, etc.—are almost invariably slight compared to the advantages which may result from the hypnosis. I also think that those authors who publicly disown hypnosis on theoretical grounds agree with me on this point; otherwise it would be impossible to explain how such men as Gilles de la Tourette, Mendel, Rieger, and Binswanger, who have said so much about the dangers of hypnotism, by no means refrain from hypnotizing. The danger of hypnotism has been greatly exaggerated. The inhabitants of a little town once left off eating potato-soup, because a woman fell downstairs and broke her neck after eating some. The terrified inhabitants were convinced that the accident was caused by the soup, because it happened after the woman had eaten some. Such reasoning is rather out of place in scientific discussions. We should be unable to apply any remedy if the principle *post hoc ergo propter hoc* were admitted in this way. Mr. X. had an apoplectic fit a fortnight after he returned from Carlsbad, therefore Carlsbad causes apoplexy; Mrs. Y. had an attack of rheumatism while being treated for hysteria in a sanatorium, therefore sanatoria cause rheumatism; Mr. Z., who was sent to the hills for insomnia, returned with symptoms of cancer, therefore hilly districts are to be dreaded as conducive to cancer. It cannot be denied that a causal connection between hypnosis and the subsequent appearance of morbid symptoms has often enough been manufactured in this way—for example, by Binswanger, Ziemssen, and others, although Pauly long ago warned observers against drawing such false conclusions. What drug has not been followed by severe symptoms after it has been in use for some time? And where would it lead to if we assumed a causal connection in such a manner? In all the cases I know in which hypnosis is supposed to have had injurious results, and which have been published in detail, it can be shown—as in the cases of Séglas, Briand, Lwoff, Jolly—either that important precautions were neglected, or else that a connection between hypnosis and the disease was assumed without any proof.

Let us now consider the dangers of hypnosis in detail, and at the same time the means of preventing them.

Mendel maintains that hypnosis induces nervousness; that nervous people grow worse, and sound people nervous through its use. But he was only led into this error because he was unacquainted with the harmless method of hypnotizing and making suggestions. It is quite true that prolonged fixed attention, as practised by Braid, may produce unpleasant sensations; but such discomforts are of no great significance. To avoid making exciting suggestions is of far greater importance, as Bertrand already knew. Whoever has seen the difference between a subject who has received an exciting suggestion and one who has received a therapeutic one will recognize how differently the two suggestions act. Judicious observers are right in warning against such sports, as Sawolshkaja did a long time ago. A man who makes an alarming suggestion—*e.g.*, an imaginary fire—just to satisfy his own curiosity, need hardly be astonished if unpleasant results ensue. In this way we can explain the very serious effects that have often been observed, for example, by Nolan, Lombroso, Brémaud, Finkelberg, etc., after public exhibitions of hypnosis. Hirschlaff justly remarks in his essay entitled *Laienhypnotismus und Ärztliche Heilkunde* that charlatans readily undertake experiments that cannot be carried out without a certain amount of danger to the subject, because they hope by so doing to impress the patient and gain his confidence. Experience also shows that patients are often worse on days following bad dreams. Consequently we can hardly be astonished when terrifying suggestions made in hypnosis produce like results. Such suggestions should not be made at all, or with the greatest caution, care being taken to do away with all suggestions that are not quite harmless before the waking. Even if a mistake is made during hypnosis, it is of little consequence provided the subject is properly wakened in the manner used at Nancy and by all who follow the prescriptions of that school. At the time when so much nonsense was talked about the dangers of hypnosis, most people knew nothing about removing a suggestion. They thought it enough to blow on the subject's face to waken him, and were astonished that he did not feel well afterwards. I am surprised that more mischief has not been done in consequence of insufficient technical knowledge. That is the danger—not hypnosis.

To show how a suggestion should be done away with, I will suppose that an exciting suggestion has been made to a subject. One should say something like this: "What excited you is gone; it was only a dream, and you were mistaken to believe it. Now be quiet. You feel quiet and comfortable. It is easy to see you are perfectly comfortable." Only when this has succeeded should the subject be wakened; and this should not be done suddenly, as it is better to prepare the subject for waking (Sallis). Of course the experimenter will use his own method. I generally do it by saying: "I shall count slowly up to three. Wake when I say three. You will be very comfortable and contented when you wake."

With technical skill and care on the part of the experimenter there is no danger of rendering a subject nervous nowadays; and under such conditions there is less danger to be feared from hypnosis than from many methods of treatment that are termed harmless.

It is asserted that one of the special dangers of hypnosis is that it causes hysteria (Guinon), or at least brings it out in persons who have a latent tendency thereto (Jolly), or sets up hysterical convulsions even in persons who have never had them before (Anton). It is true that hystero-epileptics are sometimes thrown into hysterical convulsions in hypnosis, but I decidedly contend that the convulsions are not caused by the hypnosis. Like other similar occurrences, they are probably due to the mental excitement which often accompanies hypnosis. Some persons fall into them whenever anything excites them, such as a slight noise, for example, a falling book, a bell. Timid persons sometimes faint when they are electrified (E. Remak); others, especially hystero-epileptics, may even fall into convulsions. I also think it quite possible that exciting impressions may bring about such attacks in persons who have never had one before. Gorodichze tells of a lady who had her first and only attack of hystero-epilepsy while being chloroformed. Consequently we must not deny that the excitement caused by hypnosis may occasionally bring on such attacks. Cases of this nature were described by Charpignon long ago, and later by Solow, Finkelnburg, Verneuil, Drosdow.

Although the danger of hysterical attack has to be taken into consideration, it ought not to be exaggerated. The chief question is not whether such an attack occasionally occurs or not, but rather whether or not permanent hysterical attacks are

caused, or attacks to which a patient is subject are permanently aggravated by the process. Up to the present all known experience shows that such is not the case. Certainly we occasionally hear of a case in which permanent attacks have apparently resulted from the treatment, in spite of the use of proper methods. The nearest approach to such a case seems to be one published by Jolly, in which a girl who was suffering from progressive muscular dystrophy became permanently subject to attacks of hysteria after a hypnotizer had treated her by suggestion. But a careful examination of the case puts it in a somewhat different light; and Jolly himself admitted that the fatigue and exhaustion which follow hypnosis probably also helped to bring on the hysterical convulsions. It cannot be doubted that the exercises and suggestions employed to induce movements in cases of muscular dystrophy may prove extremely exhausting.

Instead of supposing hypnosis to be a cause of permanent attacks of hysterical convulsions, we are far more justified in assuming that when once a complete hypnosis has been obtained we have in our hands a trustworthy means of permanently lessening the convulsions. In reality, an attack of hysteria is not nearly so important as some would have us believe; and we certainly know that hysterical attacks are sometimes artificially induced during lectures for purposes of demonstration.

I am satisfied from certain cases of my own, that hysterical attacks do not indicate the necessity of stopping the hypnotic treatment. This agrees with the experience of Sperling, Krakauer and others, who have also had cases in which, though there were attacks at first, yet cures were subsequently obtained. These cases also show that the attacks are by no means permanently increased in number, even when they take place at the first or second attempt to hypnotize. The old mesmerists—*e.g.*, Noizet, Puységur, Mesmer, Deslon—were well acquainted with these convulsions, which they called *crises*, and even thought them a favourable sign, which was certainly a mistake. A man who is so very much afraid of being confronted with an attack of hysteria should make it a principle never to attempt to hypnotize a patient who is either excited or afraid of hypnosis. We shall see later on that dread of hypnosis does not contra-indicate its use. It is also said that mental disorders, and even morbid delusions, may result from hypnosis.

When any one who has frequently been hypnotized becomes the victim of delusions, we must be on our guard not to assume any causal connection too hastily, especially if a long interval has elapsed since the hypnosis was practised. People without expert knowledge may be misled on the question of causal connection by the nature of a delusion. It is well known that delusions are influenced by the ideas prevalent at the time; it is therefore not surprising that in recent years we have frequently met with the delusion of being magnetized or hypnotized. When this delusion occurs in a person who has been hypnotized at some time or other, it does not necessarily follow that the hypnosis has had even the slightest injurious effect on him. The most we can assume is that the nature of the delusion, which would otherwise have appeared in some other form, was in some way determined by the hypnotic experiments on the subject. Delusions of thought-transference effected by persons at a distance occur in a similar way. Of course we should never think of denying the possibility of mental disorders being caused by the unscientific use of hypnotism; such cases have repeatedly been reported, especially after some public hypnotizer has made his appearance—for example, by Finkelnberg in connection with Hansen's public experiments, by Lombroso in connection with Donato's; also by Weinbaum, Schmitz, and others. And we must not overlook the fact that the enormous excitement that prevails at such exhibitions and its emotional effects are liable to cause at least temporary mental aberration in persons who are that way predisposed. There is also a certain amount of connection between hypnosis and those forms of mental derangement which follow spiritualistic séances. A state of trance—*i.e.*, an auto-hypnosis—is often caused by such séances; and as Charcot, Gilles de la Tourette, and Henneberg have shown, such intense excitement may occasionally lead to mental disorder in persons of a highly emotional disposition.

It has been mentioned by some as a further danger of hypnosis—Sioli, for instance—that after repeated hypnotism an inordinate desire for its repetition may be set up. Rechtsamer, of St. Petersburg, reports that a lady who had undergone hypnotic treatment subsequently made one of her friends continue hypnotizing her, because it was so pleasing. Such a result of hypnosis can be avoided by taking proper precautions, but especially by making counter-suggestions.

I may here mention some slight ailments which are occasionally found after hypnosis, especially in cases of hysteria. They are often the result of a bad method or of auto-suggestion, and occur after both deep and light hypnoses. The chief symptoms are fatigue, heaviness of the limbs, drowsiness, and faintness. In some cases it is very difficult to combat these symptoms when there is great hysterical auto-suggestibility, and it may even be necessary to discontinue the hypnotic treatment. In most cases we can prevent these auto-suggestions by employing the proper technique, but as a rule this is only possible if we suggest from the beginning—*i.e.*, at the first experiment—that any feeling of fatigue or sleepiness will disappear after waking. It is often advantageous to get rid of the fatigue before the awakening. There is no necessity to overrate these attendant phenomena of hypnosis, even if they are unpleasant, and certainly no one will do so who remembers that suggestion is often responsible for temporary after-effects in the case of other remedies, especially where there is hysteria.

Although the dangers of hypnotism which we have hitherto described are of no great practical importance, there are others that are much more serious in that respect. I mean the increased tendency to hypnosis which includes a greater liability to auto-hypnosis, and the heightened susceptibility to suggestion in the waking state. Möbius maintains, that apart from a wilful craving for hypnosis, the danger of involuntary hypnosis is quite the only danger from hypnotic treatment that need be considered. Cases have been described in which spontaneous hypnosis has occurred a short time after a subject has been hypnotized injudiciously, and even all the events of the previous hypnosis have been reproduced by auto-suggestion. Brémaud has described one such case, and Solow another. But the cases in which, as I suspect, unscrupulous hypnotizers sometimes induce such hypnoses intentionally are far more serious, for subjects become filled with a feeling of complete dependence on the hypnotizer, are in constant dread of falling under his influence, and even when no real form of insanity is developed they lose all freedom of action and feeling of independence. Lloyd Tuckey has published a case of this kind; and the somnambulists employed as clairvoyants by so-called mesmerizers would about come in here. I firmly believe these dangers are much more serious than those previously mentioned.

The safest way of guarding against these dangers is to make some such post-hypnotic suggestion as follows to the subject before waking him:—"Nobody will ever be able to hypnotize you against your will or without your consent; you will never fall into hypnosis against your wish; nobody will be able to suggest anything to you when awake; you need never fear that you will have sense-delusions as you do in hypnosis," etc. The antidote to such dangers is counter-suggestion. Permission to hypnotize should certainly only be granted to persons whose character and knowledge afford a guarantee that they will do no harm, either intentionally or unintentionally.

But it may be objected that though an occasional use of hypnosis may not be hurtful, a long one, involving a repeated induction of the state, might be so. The objection is justifiable; but it might also be made against the use of various other remedies, since we do not yet know whether a long use of them might not endanger health. Experience is the only way to decide such questions. I myself, as well as other investigators, have watched cases in which persons were repeatedly hypnotized for several years without evil results. But apart from this, it will hardly ever be found necessary to hypnotize frequently in such cases; even when the treatment has to be carried out for years, an occasional hypnosis will suffice. Even when for special reasons a patient has to be hypnotized repeatedly for years, a conscientious and experienced physician will be quite able to guard against any possible dangers. We shall always find counter-suggestion the surest preventive of danger.

I do not intend to discuss purely theoretical dangers in detail. Mendel fears stimulation of the cerebral cortex, while Ziemssen and Meynert fear a loss of power of that part. To pay any attention to such a combination of theoretical dangers which are always mutually contradictory, would only land us in fruitless speculations.

In the foregoing I have discussed two objections made to the therapeutic use of hypnosis and suggestion; first, Ewald's assertion that hypnotism should not be called medical treatment; and secondly, that it is too dangerous to allow of its practical use. A third objection to be mentioned is that hypnotic treatment is superfluous. Certainly it is seldom denied that patients do occasionally get better, and are even cured by hypnotic treatment; but it is none the less objected

that the same results can be obtained without hypnotic treatment, or that a lasting improvement never ensues.

It is true that many cases in which hypnosis used to be considered necessary can be treated without it nowadays. Other methods of mental treatment constitute the most important substitute for hypnosis, and include not only suggestion in the waking state, but the special instruction of the patient, etc., as well, to which I shall return in the next chapter. And here we must note that psycho-therapeutic treatment without hypnosis is essentially an outcome of hypnotism. It was not until the results of hypnotic suggestion had enabled us to recognize the extent to which human beings, particularly patients, are susceptible to psychic influence that the importance of almost all of these methods of mental treatment was made clear. That this often enables us to dispense with hypnosis in cases in which it was formerly employed, is nevertheless a result of hypnotism. But apart from this, hypnosis is still in a series of cases the quickest and best means of obtaining satisfactory results; and even if new remedies have rendered hypnosis superfluous in many cases, there still remains a no inconsiderable number in which that treatment is indicated. And it is certainly a fact that even where other remedies prove successful, hypnosis often produces the same results much more speedily, so that if we adhere to the old principle *tuto cito et jucunde*, hypnotic treatment frequently has the advantage.

The assertion that hypnotic treatment does not produce any lasting cures may be answered as follows. The results are by no means transitory; on the contrary, a large number of lasting cures have been observed and published. I have myself seen many cases where there was no relapse for years. One cannot ask for more. The objection that the improvement may be only temporary is thus not justified. But even were this so we must nevertheless be glad that we have found a way of procuring even temporary relief (Purgotti, Schuster). For instance, in difficulties of menstruation it is a great thing if we can succeed in subduing pain for a time, although we may not be able to prevent its recurrence. If pain returns a new hypnosis may be induced. In any case, therapeutics is not yet so far advanced as to give us the right to reject a remedy because it has often merely a temporary value.

Another objection, closely related to the foregoing, is that hypnotic treatment only affects symptoms, but does not cure

the underlying disease. Discussing this point with Binswanger, Richard Schulz says: "If, as in the case before us, we can enable a woman who has been paralyzed for two years to walk, then, even though the hysteria which caused the paralysis remains unaffected, we have obtained an important result, especially for the patient. There are many other internal complaints—for example, diseases of the heart and chronic diseases of the kidneys accompanied by severe dropsy in which we cannot remove the cause of the trouble, but we can remove the distressing symptoms produced by dropsy and thereby make the patient's sufferings bearable." The objection raised by Binswanger and others that hypnotic treatment does not cure but only produces a temporary improvement can only influence a superficial observer. A man who has acquired his knowledge of therapeutics by the practice of medicine, and who has kept his mental vision unobscured, knows how seldom disease is cured by any of the so-called scientific remedies. Or does Binswanger perhaps think that treatment in an institution, as recommended by him for cases of hysteria, cures the disease? We know, on the contrary, that although in numberless cases some distressing symptoms disappear during a stay in hospital, they reappear with renewed vigour directly the patients return to their old surroundings. Still that is no reason for rejecting hospital treatment. If we were to reject the remedies which only act symptomatically, and such that merely relieve the symptoms of disease for a time, we should have to abandon the largest part of therapeutics.

An objection occasionally made (Bruns) to hypnotic treatment or treatment by suggestion, is that in many cases cures apparently due to suggestion in reality occur spontaneously. In many cases this objection is justifiable, but it applies equally to every other therapeutic method. I even believe that in the present day many really spontaneous cures are erroneously ascribed to the suggestive action of drugs. To many people the word "suggestion" has become a catch-word that will explain anything. In this way suggestion is accredited not only with any spontaneous improvement in cases of functional disorder, such as neurasthenia, but also with instances of temporary improvement where the disease is organic and progressive. In addition to spontaneous recovery, we have also to deal with certain fluctuations which may occur in cases of both functional and organic disease. A neurasthenic may be in a state of

excitement for weeks, then quieter for weeks; his sleep may be disturbed at nights, and then he may recover spontaneously without any treatment. Such fluctuations, which sometimes have a regular and periodic character, may very easily be mistaken for the suggestive action of any drug that has been exhibited. The same is also the case in incurable organic diseases in which the symptoms by no means invariably present a regularly progressive character; on the contrary, essential symptoms sometimes abate for a time, without the organic disease showing any tendency towards improvement or cure. There are cases of tabes dorsalis in which the ataxy seems to abate for a long time, but that does not justify the conclusion that the organic disease is being cured. We observe the same sort of thing in chronic deformative rheumatism of the joints—the pains are at times less intense, but the swellings do not subside. It often occurs quite spontaneously and can easily be mistaken for the action of some remedy, including the action of suggestion if that has been employed at the same time. It cannot be doubted that such erroneous conclusions have often been drawn in medicine,—in electro-therapeusis, for instance. Paralysis of the muscles of the eye, an early symptom of tabes dorsalis, as a rule disappears spontaneously, and yet how often the improvement is attributed to electric treatment! And it has been pointed out very justly that we must carefully guard against such sources of error when judging the therapeutic value of suggestion. But in spite of all this, no really objective investigator can doubt that there are numerous cases in which success is not due to any spontaneous improvement, but rather to the direct influence of hypnosis or hypnotic suggestion. If we take the case of a man suffering from insomnia, and his nightly sleep at all stages of the disease is improved by hypnotic suggestion, we cannot call that a mere coincidence. No matter how sceptical we may be, there is always an essential something in the result, that is justly attributable to hypnosis.

Another objection to the therapeutic use of hypnosis is that it cannot be generally applied because everybody is not hypnotizable. To which I may add that in many cases, even when a hypnosis is induced, it is not deep enough to be used therapeutically. This, of course, reduces the number of cases in which hypnosis proves successful; but, on the other hand, the fact remains that hypnosis is practicable in many cases. We have only to consider the number of people who should,

from a medical point of view, take a prolonged rest and yet are prevented from so doing by pecuniary or social reasons. Many people cannot afford the sea voyage, long stay in a sanatorium, healthy dwelling, etc., which would greatly improve their health.

In the early days a naïve attempt was made to introduce an element of national feeling into the struggle against suggestive therapeutics and hypnotism. It was considered justifiable to speak ironically of "French professors of medicine," as Friedrich, for instance, did. A German author had previously made a chauvanistic protest that hypnotism originated in France, and it was frequently maintained that susceptibility to hypnosis was something French and could not possibly occur among Germans. This latter assumption was disproved long ago. But apart from this, Forel pointed out very properly that such talk is not permissible in scientific discussions. I have often known a French professor show himself up as Friedrich did on the question of hypnosis. The attempt to bring other nations into ridicule by ascribing to them quaint idiosyncrasies is a very old mania. As far as we are here specially concerned, I need only point out that in Mesmer's time the Germans accused the French of making mesmerism the basis of nothing but the wildest swindles. In those days Mirabeau answered such attacks by calling attention to the so-called "moon-doctor," the stocking-knitter Weissleder, who caused such a stir in Berlin from 1780 to 1781: "As if we Frenchmen had not received the notorious Mesmer from the hands of the Germans, and the latter were not anxious to have their 'moon-doctor,' who was supposed to cure all diseases by the influence of that planet, forgotten" (Avé-Lallemant).

What therapeutic factors play a part in hypnotic treatment? Some think that hypnosis is in itself beneficial; this is the opinion of Beaunis, Obersteiner, and Wetterstrand, who employed hypnosis in the treatment of epilepsy, hystero-epilepsy and other states. Later on Hirschlaff, Vogt, Brodmann and others also expressed the opinion that the rest afforded by hypnosis is curative, because it strengthens the nervous system. Brodmann distinguishes three methods of employing hypnosis: (1) Prolonged sleep, (2) periodical sleep to prevent exhaustion or a pathologically increased tendency to fatigue, and (3) occasional sleep to ward off threatened

attacks or suppress such as may exist (convulsions, pain, emotion).

Another factor to be mentioned is the heightening of memory, to which Vogt and Brodmann, but Breuer and Freud specially, have called attention. I shall return to this when I come to deal with the cathartic method recommended by Breuer and Freud.

But the most important point in connection with hypnotic therapeutics is the direct action of suggestion, or, to use Vogt and Brodmann's words, the subject's heightened susceptibility to psychic influence. Both these authors prefer the latter expression because it is not only the subject's susceptibility to suggestion that is increased in hypnosis, but to other psychotherapeutic agencies as well—for example, to praise or blame, to logical argument, emotional stimulation, therapeutic exercises, etc. Still, increased suggestibility is undoubtedly the most important factor, and we will now proceed to discuss it in a more detailed manner so that we may understand its true therapeutic significance.

Let us take as a simple example the case of a woman suffering from a functional headache. We wish to cure the headache by suggestion—*i.e.*, by arousing in the subject the idea that the headache is gone. Spontaneous reflection would prevent this in most waking people, but in hypnosis it is relatively easy to induce, or suppress, sensations by means of suggestion. Consequently, the suggestion that the headache is gone will be more readily accepted in hypnosis, and the first result will be that the patient feels free from pain while in the hypnotic state. But the great point is to prevent the return of the pain after waking. Either external post-hypnotic suggestion or auto-suggestion will do this. As we have already seen, suggestions can be made to persist post-hypnotically. It is, therefore, not surprising that we can do as much with the idea that the pain is gone after waking. Of course, the patient need not be conscious of this idea in the sense of remembering it; on the contrary, the less conscious the idea is, the more effect it will probably have. Auto-suggestion is the second plan. The patient, finding herself without pain in hypnosis, may convince herself that pain is not a necessary consequence of her state, and this idea may under some circumstances be strong enough to prevent the return of the pain.

The more easily an idea can be established in a subject the

quicker a therapeutic result can be induced. And the deeper the hypnosis the more easily therapeutic suggestions can be established. But we must certainly agree with Vogt that the persistence of suggestions that are momentarily successful needs a subject of a different character to that which is necessary for a suggestion only to prove temporarily successful. Hence it often happens that the therapeutic result only amounts to a temporary disappearance of the symptoms of a disease without preventing their return shortly after the patient wakes. On the other hand, there are cases in which the acceptance and continuance of a therapeutic suggestion is brought about by superficial hypnosis without any very strong suggestibility. It follows from this that superficial hypnoses must be taken into consideration, quite apart from the fact that they often lead to deeper hypnoses. However, we may take it to be the rule that suggestibility, and also the persistence of suggestions, increases with the depth of the hypnosis.

Suggestion provides us with the key to suggestive therapeutics. When the hypnotized subject does not accept the suggestion, or refuses it, which sometimes happens, it will hardly ever be possible to obtain a therapeutic result. In addition to external suggestion we must certainly also take into account auto-suggestion on the patient's part, for its action does not merely take effect after the termination of the hypnosis in the manner described above. Even during the hypnosis itself, auto-suggestion may be the essential influence that produces the result, although, of course, this auto-suggestion proceeds indirectly from the external suggestion. A person who has himself hypnotized in the belief that the hypnosis will cure him, often suggests the cure himself, without any external suggestion during the hypnosis being really necessary. I have already mentioned pre-hypnotic suggestion (p. 68). Since pre-hypnotic suggestion becomes auto-suggestion during hypnosis, it plays an important part in therapeutics in the manner just described, and the results obtained which are ascribed to hypnosis as such—*i.e.*, without suggestion, are often due to pre-hypnotic suggestion. Similarly, emotional influences are closely related to auto-suggestions. The patient's expectant attention, and in some cases also the apparent mystery attaching to the whole proceeding and the excitement which that causes, may tend to support the influence of suggestion.

We find on investigation that the number of diseases in which hypnotic treatment is indicated is very great. This is not meant to imply that hypnosis is a universal panacea. There are numerous diseases in which the employment of suggestion comes in question, but in which it is sometimes necessary first of all to ascertain by experiment whether a hypnosis can be induced and hypnotic suggestion thereby rendered possible. Ewald, who wanted to leave suggestive therapeutics to shepherd-boys, likewise refused to concede the same rank to hypnotic treatment as to other methods, because it was impossible to establish definite indications for its use. Let us see how other therapeutic methods stand in this respect. When we find that the same disease can be influenced beneficially in one case by cold water, in another by warm, in one case by douches, in another by hip-baths, in this case by the galvanic current, in that by the faradic, sometimes by static electricity and sometimes by electric light baths, then all these facts should make us somewhat more tolerant in our attitude to hypnotism. We know from experience that patients suffering from the same disease get relief by totally different methods of treatment. What we do not know is why a warm bath is beneficial in one case and a cold one in another, why static electricity succeeds in one case and the galvanic current in another, quite apart from the question whether these methods have only a mental action or not, or whether spontaneous improvement may not be mistaken for the effect of the remedy applied. Certainly these considerations do not agree with the fairy tales which many authors tell us about "exact indications," and which Ewald seems to have believed in when he tried to place hypnotic treatment on so low a grade. Medicine consists to a great extent in the careful selection, by trial, of that treatment which seems most suited to each case. This by no means disparages the functions of the physician; at the most it militates against medicine's claim to being considered an "exact science." It is just because the indications are so often indefinite that the physician is necessary; it is for him to decide from his own observations whether the remedy employed is acting beneficially, and should therefore be persevered with, or not.[1] It is the same with hypnosis. We can put forward general indications for its use, but we cannot guarantee a cure

[1] For further details see *Ärztliche Ethik*, by Albert Moll; Stuttgart, 1902, p. 476 *et. seq.*

in any particular case. Ewald overlooked the fact that there are rarely definite indications in internal diseases, as may be clearly seen by comparing various text-books, and from the numerous contradictory statements made by different doctors. He also overlooked the fact that the indications for hypnotic treatment are quite as clear as those for treatment by electricity, massage, drugs, and baths.

Before proceeding to discuss the general indications for hypnotic treatment, I must say a few words on the idea conveyed by the term "hysteria." Unfortunately, several authors—chief among them Mendel, of Berlin—have done much to obscure hypnotism through the very vague meaning that attaches to "hysteria." Binswanger admits in his great monograph on the subject, that no definite idea has yet been constructed that would enable us to recognize hysteria as a clinical entity, a well-defined nervous complaint. In reality, the question as to what is meant by hysteria is considered from two totally different points of view in the present day, the theoretical and the clinical. "All morbid bodily changes caused by ideas are hysterical." This is Möbius's conception of hysteria, and it is accepted by many other authors. Eulenburg takes essentially the same view, which also agrees to a certain extent with that finally held by Charcot. There is justification for Möbius's definition from the theoretical standpoint, and also from Binswanger's not quite identical view that all hysterical morbid phenomena are indissolubly connected with functional disturbances in the cerebral cortex, though Binswanger explicitly explains that he does not mean solely disturbances of mental origin. No matter how well-grounded these theoretical definitions may be, we have to reckon with the fact that a theoretically constructed conception of a disease does not as a rule cover the clinical conception of the malady, unless investigation has led to a definite result—*i.e.*, unless the clinical material has been sufficiently examined and classified from the theoretical point of view. Unfortunately, we cannot say that this has happened in the case of hysteria. But the confusion of such theoretical definitions with clinical ideas has led to much misunderstanding. It is not so long ago that we in Germany described hysteria clinically as a disease of an essentially functional nature, and then again as typified by the multiplicity and variability of its symptoms. Certainly there has been a change in this respect during the last ten to fifteen years, and we no longer consider that the multiplicity and variability of symptoms justify the diagnosis hysteria. But this by no means implies a fusion of the clinical picture of hysteria as recognized to-day with the theoretical definitions given above, and it may well happen that when several authors write of hysteria each means something different from that discussed by the others. Möbius tries to avoid this difficulty by pointing out that hysteria, in his sense of the word, may be accompanied by symptoms which do not belong to hysteria; he considers such symptoms—*e.g.*, the hysterical character and hysterical mental troubles—complications, and not symptoms, of hysteria. Other observers think these phenomena essential symptoms of hysteria. From all of this it is easy to see how great a difference there is between the clinical and the theoretical idea of hysteria.

Indeed, as already pointed out, the word hysteria is variously employed

in a clinical sense, and by using the term in one sense or another at pleasure erroneous conclusions are drawn which even many doctors fail to recognize. We have seen that it was at one time almost universal in Germany—it is so, to an extent, in the present day—to consider hysteria a functional disease which has numerous and variable symptoms—to-day one symptom, to-morrow another predominating; now headache, now ovarian pain, now pain in the side, and now weakness in the legs, etc. The patient is called "hysterical" as well as the symptoms. As such patients are sometimes obstinate and capricious, this word "hysterical" has a somewhat unpleasant after-taste; some authors go so far as to say that a tendency to falsehood and hypocrisy is a chief symptom of such hysteria. This is evidently an unfair generalization. At all events the multiplicity and variability of the symptoms are the main characteristics of hysteria taken in this sense.

In another sense the word "hysterical" has quite a different meaning. It is used to describe morbid symptoms which have no anatomical basis and which are therefore merely "nervous"—*e.g.*, headache, pains in the muscles, certain tremors, vomiting, etc.; even when the symptom is solitary and constant. Now, if in such a case the patient, as well as the symptoms, is to be called "hysterical," we have two totally different meanings for the clinical conception of a "hysterical patient."

These points have to be taken into consideration when discussing the connection between hysteria and hypnosis. I have already (p. 49) spoken of the supposed connection between hysteria and hypnotizability, denying its existence, and I based my negation on the clinical meaning of hysteria. But some cautious German investigators—*e.g.*, Hirschlaff, Hellpach, Gumpertz, Vogt—have recently attempted to establish a close connection between hysteria and hypnosis, or at least a certain psychological relationship. This would be quite right from Möbius's point of view, according to which both states are equally influenced by ideas. But it is quite different if we accept the climical conception of hysteria, for that does not admit a close connection between hysteria and hypnotism, or, more particularly, the connection between hypnotizability and hysteria which used to be sometimes assumed.

But it is necessary to insist upon the various meanings given to the term hysteria if we are to ascribe a definite position to hypnotic and suggestive therapeutics, or, indeed, to the whole of mental therapeutics. If we accept polysymptomatic hysteria with the varying pathological picture it presents then a remedy that allays the morbid phenomena in such cases is often considered of minor importance because the term hysteria, used in this sense, is too easily associated with the ideas of exaggeration and simulation, and because, moreover, such hysterical symptoms are themselves liable to undergo variation. Let us now take the second clinical meaning of hysteria, and examine the case of a person suffering from a severe pain in a muscle—the biceps, for example—but without any other symptom of hysteria, then if the patient is freed by suggestion from the pain the people who want to make out that hypnotic treatment is only beneficial in cases of hysteria at once exclaim, "You see that was only another case of hysteria." They carefully suppress the fact that the patient was not hysterical in the first meaning of the word. This is the way in which the word "hysterical" is juggled with to prove that only hysteria can be influenced by hypnosis. Such methods can only tend to obscure

the whole question; and it is easy to see how much more likely this is to occur, if at one time the clinical meaning and at another Möbius's definition of hysteria is made the basis of the discussion.

How easily a confusion of these meanings may lead to misunderstanding, is exemplified by a letter which Charcot once addressed to Guttmann, in which he asserted that only hysteria can be treated by hypnosis. But, as Nonne remarked, Charcot understood much more by "hysteria" than German physicians did in those days. In contradiction to two German authors, Oppenheim and Thomsen, Charcot distinctly declared that the variability of the symptoms is not the chief characteristic of hysteria. Hence, according to Charcot, the indications for hypnotic treatment are not so limited as those authors assumed from their antiquated German notion of hysteria. We shall see later on that Charcot understood much more by hysteria in other respects; he was satisfied that hysteria was a concomitant of many organic diseases in which its presence was denied by other authors.

The most suitable maladies for hypnotic treatment are the neuroses—*i.e.*, nervous complaints in which no anatomical cause is demonstrable. But, as pointed out, there must be no confusion with hysteria or neurasthenia. The nomenclature is certainly somewhat arbitrary. Whether an occasional headache is described as hysterical, or nervous, or even functional, often depends more on the individuality of the physician than on the nature of the pain, and in the same way a patient suffering from imperative ideas may be described either as a neurasthenic or a psychopathic subject. Dubois, for example, arbitrarily classified neurasthenia, hysteria, hystero-neurasthenia, mild forms of hypochondria and melancholia, and, finally, certain severe disturbances of mental equilibrium which border on insanity, under the heading psycho-neurosis or neurosismus. Considering the arbitrary way in which all such designations are used, I shall not place too much value on a scientific terminology; but to avoid any misunderstanding, I must maintain that cases of polysymptomatic hysteria are not invariably suitable for hypnotic treatment, especially when the symptoms vary very rapidly. This is obviously caused by the auto-suggestibility of the patient, which in such cases counteracts the effect of the external suggestion. We can often remove such a hysterical symptom as pain in the head or the leg by suggestion, but another symptom readily takes its place as the result of auto-suggestion. On the other hand, it not infrequently happens, as Kraepelin has pointed out, that cases which come under the monosymptomatic conception of hysteria are often suitable for hypnotic treatment; this we shall see from the following

summarization of the indications for such treatment. I will now proceed to enumerate the most important states in which hypnotic treatment has to be considered.

All kinds of pains that have no anatomical cause—*e.g.*, headaches, stomach-aches, ovarian pain. Rheumatic pains, even with effusion in the joint, according to Block; but it is possible to confuse with hysterical effusion. Many forms of neuralgia, also, are influenced beneficially by hypnotic treatment; Forel observed good results in a case of neuralgia of traumatic origin.

All kinds of other sensations of nervous origin, as, for example, pruritus cutaneous nervosus, paræsthesiae; nervous ringing in the ear.

Nervous ocular disturbances (Forel, Möllerup, Chiltoff).

Various kinds of local spasms—*e.g.*, blepharospasm (Forel Ritzmann); vaginismus (Barbaud). All kinds of tics (Meige and Feindel, Wetterstrand, Renterghem, Féron, Vlavianos). Paramyoclonus (Scholz). Neglected cases of chorea (Dumontpallier, Leroux); hemichorea (Farez).

Writer's cramp, nervous tremors. Here, also, belong those affections which Bérillon designates by the name *bégaiement graphique*, in which the patient is only unable to write when he thinks he is being watched. Legrain likewise lays weight on the mental excitement in writer's cramp, and recommends hypnotic suggestion as treatment.

Stammering (von Corval, Ringier, Wetterstrand, Pauly). Lefèvre traces the origin of stammering essentially to suggestion caused by imitation.

Hysterical attacks of various kinds—for example, spasms, convulsions.

Hysterical paralyses of the extremities; hysterical aphonia and mutism; astasia and abasia (Stembo).

Catalepsy (Viviani). Georges-Gaston Pau de St. Martin published in his medical dissertation (Strassburg, 1869) a case in which catalepsy was successfully treated by hypnosis. He thought the improvement was due to rubbing the limbs during hypnosis, and maintained that the simultaneous use of hypnosis was necessary in his case, in opposition to an earlier publication by Puel, who also employed such rubbings successfully in cataleptic fits.

Enuresis nocturna. Ringier reports that a little girl became subject to nocturnal incontinence of urine after other children

had told her that she would wet the bed because she had plucked a certain flower, the meadow crowfoot. The flower in question, *Ranunculus acris*, is in Switzerland commonly called *pisse-en-lit.*[1] It is easy to see that a disorder which can thus be caused by suggestion may most easily be cured by suggestion.

Any kind of disturbance of menstruation (Liébeault, Bernheim, Forel, Wetterstrand, Bugney, Brunnberg, Journée, Marandon de Monthyel, Gascard). The most different kinds of disturbance of menstruation may be influenced by hypnotic suggestion, amenorrhœa as well as menorrhagia and dysmenorrhœa. It is worth mentioning that Liébeault was never able to cause abortion by hypnotic suggestion.

Attacks of eclampsia are thought by Le Menant des Chesnais to be influenced beneficially by hypnosis. Reports as to the results obtained by the hypnotic treatment of epilepsy vary, but are not favourable on the whole. A few observers—Wetterstrand, for example—report cases of improvement from treatment by prolonged hypnosis; but Hilger is very reserved in his remarks, though he also found improvement in two cases. I have never seen a successful result that I could attribute to hypnosis as such with any degree of probability; certainly a successful result is easily simulated in many cases of epilepsy.

Sleeplessness, uneasy dreams, spontaneous somnambulism.

Gastro-intestinal disturbances of nervous origin; loss of appetite; hysterical vomiting (Freud); vomiting of pregnancy (Choteau, Anuforiew, Pobedinski); chronic constipation (Forel, Bénard, Schmidt, Farez, Delius). Forel has rightly pointed out that many cases of chronic constipation are brought about by there being no direct innervation current from the brain to the bowels. It is exactly in such cases that purgatives are not merely useless, but injurious.

Hysterical polyuria (Mathieu, Babinski, Debove); nervous asthma (Brügelmann).

All kinds of neuroses of emotion—*e.g.*, fear of blushing (Friedländer, Bechterew); fear of being unable to pass water, dread of diarrhœa, agoraphobia (Jong), and similar obsessions. To the obsessions belong also cases of nosophobia, in which the patient is dominated by the fear of disease. We know that in such cases the symptoms of the disease dreaded may be

[1] In the well-known French lexicon by Sachs-Villatte the dandelion is called *pisse-en-lit.*

produced by auto-suggestion, as, for example, the phenomena of tabes, or attacks like those of epilepsy. In some cases suggestion should be employed to remove the feeling of fear, in others to combat the symptoms produced by auto-suggestion. Julius Althaus specially recommends suggestion in nosophobia, which includes many cases ascribed to rabies. Ch. Ph. Pinel thinks that there are cases of pseudo-rabies which sometimes terminate fatally although the patients have never been bitten. In such cases, as well as in those of pseudo-rabies in which the patients have been bitten, hypnosis with suggestion is indicated. Pinel has treated and cured a case of this kind by hypnotic suggestion.

Many authors advocate the use of hypnosis where there is a tendency to the misuse of stimulants and narcotics, especially in cases of chronic alcoholism (Forel, A. Voisin, Ladame, Widmer, Lloyd Tuckey, Wetterstrand, Corval, Knory, Neilson, Bushnell, Stegmann, Tokarski, Wiazemsky, Ortizky, Rybakoff, Farez, A. Marnay). Corval pointed out that in alcoholism the injurious effects of abstinence can sometimes be suppressed by suggestion, and Bramwell thinks that he has obtained successful results in cases of dipsomania. Morphinism (Wetterstrand, Marot), nicotinism, and similar drug manias have been treated by suggestions, sometimes successfully. Experience shows that better results are obtained in alcoholism than in morphinism, though in the latter the injurious effects of abstinence can also be suppressed by suggestion. Opinions differ whether a gradual or sudden disuse of the drug should be produced while the patient is undergoing hypnotic treatment. Bérillon and Tanzi are in favour of the gradual method. R. Binswanger disputes the great efficacy which Wetterstrand attributes to suggestion. Landgren, a Swedish physician, has hereupon published his own history; in consequence of acute pains due to inflammation of the joints, he had become accustomed to the use of morphia. Wetterstrand succeeded in curing him with remarkable rapidity, and Landgren states that the severe pains which assuredly followed every attempt to discontinue morphia were remarkably shortened. Sigmund A. Agatson also reports that he was able in the same way to avoid the injurious effects of abstinence in a case of morphinism.

Hypnotic suggestion is also recommended in affections of the sexual impulse, and has sometimes met with success. Most suitable for hypnotic suggestion are the various forms of sexual

perversion, including homosexuality, as well as masochism, sadism and fetishism; and, further, perverse inclination towards the immature of the other sex. Krafft-Ebing, Schrenck-Notzing, Kraepelin, Alfred Fuchs, Ladame, Tatzel, Naret, Renterghem, and Wetterstrand have observed good results in the most different forms of sexual perversion. The treatment has also occasionally proved successful in masturbation.

Hirst recommends hypnotism in the neuroses of traumatism and emotion.

Whether suggestion can be of any essential use in neurasthenia is a question that has often been raised, but the views expressed on it differ. Bernheim has seen no good results; but this is not the experience of Eeden. Many believe that neurastheniacs are not so very suggestible, an opinion which is not shared by Bérillon, Mezeray, Mavroukakis, and others. The successful results reported by Bérillon have been sharply criticized by Schrenck-Notzing.

In reference to an experiment by Heim, in which suggestion was successfully used to ward off sea-sickness, Forel mentions a similar case. Other authors also (Farez, Hamilton Osgood, and Bonnet) state that it is possible to prevent sea-sickness by suggestion, or to stop it at the onset. On theoretical grounds Bonnet thinks that it would only be possible to stop sea-sickness that has once really started if the patient were very highly suggestible; but from his own experience he admits the value of suggestion as a prophylactic. I believe, with Rosenbach, to whom we owe an admirable treatise on sea-sickness, that we have here to distinguish two causes, one of which is fear and excitement. In this respect a favourable influence can certainly be exerted by suggestion. But the second is made up of physical causes, the movement of the ship and of objects on it which are watched. That suggestion may sometimes render these physical influences inoperative appears to me conceivable; but one can well understand that it only rarely succeeds. I may here mention that as far back as 1793 a woman who had an unconquerable aversion to trusting herself afloat, once crossed the water in the somnambulic state while under the influence of so-called animal magnetism (Ferret).

Affections which can scarcely be called diseases may also yield to hypnotic suggestion. In a case of David's, a lady for many years had been liable to burst into tears at every occurrence, however slight; this condition improved under hypnotic

suggestion, and five years later there had been no relapse. The dread of thunderstorms, which in some cases is almost pathological, can occasionally be combated by hypnotic treatment. Lloyd Tuckey relates that he was consulted about a young girl who had fallen in love with an unprincipled man and become engaged to him. Lloyd Tuckey was requested to bring about a rupture between the lovers by means of hypnotic suggestion. At first he refused, but finally consented on hearing the details of the case. The girl was hypnotized ostensibly for neuralgia, and proved to be a somnambulist. After the third sitting he began to suggest cautiously that she would let herself be guided by her mother and would break off the connection. She gradually acceded. In a second case a young widow of thirty was nearly letting herself be ruined financially by another woman for whom she had a romantic attachment. She was treated ostensibly for insomnia and a weakness for alcoholic drinks. In this case also Lloyd Tuckey succeeded in suppressing the perverse tendency by means of hypnotic suggestion.

I must here append a case that came to my knowledge in Berlin, in which a young girl had fallen in love with a poor gentleman. The services of a hypnotizer were requisitioned to turn the love to hate. He succeeded, so I was told. Then the poor man who had been freed from the bonds of love suddenly inherited a fortune; so the hypnotizer was again sent for, this time to reverse the former process and turn the hate to love. I have had similar propositions made me in a whole series of such cases; but I am of opinion that on principle a physician should never do anything in hypnosis to which the patient has not given his assent while in the waking state, provided, of course, that the person in question is of sound mind.

There are, besides, many cases in which suggestion has been used in a somewhat fantastical manner. A Russian physician once told me that he was able to influence the sex of an embryo by means of suggestion. But apart from such fanciful notions, it has also been proposed to employ hypnosis for practical purposes in many other cases which are not strictly medical. I shall return to this question later on when I come to discuss the use of hypnotism in education.

Many attempts have been made to treat mental diseases by hypnosis, but certainly without any great success. This is

partly because insane persons are often anything but good subjects for hypnosis (A. Voisin, Percy Smith, A. T. Myers), but is also due to the fact that such essential symptoms of mental disease as insane ideas, delusions of the senses, melancholic frame of mind, etc., are difficult to remove. It is easier to remove nervous symptoms, such as sleeplessness and headache, although the mental malady as such continues (Forel, Robertson, Kraepelin). However, in the lighter forms of mental disease—*e.g.*, melancholia and mania, remarkable improvements have been obtained (Forel, Burckhardt, A. Voisin, Séglas, Burot, Dufour, Frank). Hysterical mental disorders have often been influenced beneficially by suggestion (Majewska). Anglade is quite satisfied that hypnosis should be used in mental cases, but only for the purpose of treating hysterical disturbances; Sturgis reports good results in cases of fixed ideas; A. Voisin, Repoud, and Locojano say they have seen good effects produced in cases of severe mental disorder. Tokarski rightly protests against the forcible hypnotization of children, criminals, and the insane as carried out by some, such as A. Voisin, Herrero, and Caryophilis.

Forel has recommended another special method for making use of hypnosis in psychiatrics; it consists in hypnotizing the attendants in asylums so as to make them more careful with their patients. Walter Inhelder has collected and published Forel's experiences, from which it appears that after being treated by hypnosis and suggestion the attendants certainly sleep peacefully, but wake at the slightest suspicious noise made by a patient whom they are then quite able to protect from himself, or prevent injuring others. Inhelder thinks that in this way victims of melancholia who are dangerous to themselves are better looked after. Such patients could not sleep if placed in the attendants' room on account of the noise, and to provide special attendants for them would cost too much; but by employing hypnotized attendants both these difficulties can be got over.

Hypnotic treatment has also often been successfully used in cases of organic[1] disease. Liébeault and Bernheim, the earliest

[1] I here include among the organic diseases some that are usually classified as neuroses, because we are as yet unaware of the nature of the anatomical lesions that produce them; but such anatomical lesions must certainly be assumed to exist, for example, in paralysis agitans, facial

investigators, and others who have studied the therapeutic value of hypnotic suggestion have demonstrated this. Later on others—such as David, Grossmann, Stembo, Lloyd Tuckey, also Bechterew and, quite recently, his pupil Pewnizki—have likewise emphasized the importance of hypnosis in the treatment of organic disease. Lloyd Tuckey observed the severe pains in a case of tabes dorsalis disappear under treatment by hypnotic suggestion; Stembo subdued pains arising from cicatrices; Pewnizki saw the spontaneous pains decrease in a case of syringomyelia, but those caused by peripheral stimulation increase under the influence of hypnotic suggestion. Bernheim saw an apoplectic paralysis rapidly improved by suggestion. The objection that the diagnosis was mistaken was contradicted by the autopsy when the patient died of disease of the lungs later on. Martin, also, has expressed a decided opinion against limiting hypnotic treatment to functional diseases. He has described, among others, cases in which the vomiting of pregnancy and pains in the hips and thighs arising from the pressure of the gravid uterus were subdued by hypnosis. He also succeeded by means of hypnosis in making patients retain food and medicines which they otherwise invariably returned.

A superficial examination of the question might very easily lead to the view that the employment of hypnotic suggestion in the treatment of organic disease is but the result of an uncritical over-estimate of the value of that method, because it is not the organic lesion that is cured by suggestion. Such a conclusion would be altogether erroneous. If we believe that Fränkel's therapeutic exercises have a beneficial effect on the symptoms of tabes dorsalis, then there is no reason to contest the efficacy of other psycho-therapeutic agencies in the treatment of organic disease. For Fränkel's method is a mental remedy; it is an attempt to restore voluntary co-ordination of certain movements by making special uses of the sense of sight. So that even if we admit the efficacy of this remedy, we are not bound to assume that the morbid anatomical changes in the spinal cord are done away with by it. Sperling pointed out

cramp, acute chorea, etc. Of course, some organic change is the basis of every neurosis. Still we are justified in making certain distinctions, as between paralysis agitans and hysteria. If we take into consideration the progressive course of the disease and the immutability of its chief symptoms, then the organic lesion in palsy must be of a much more stable character than is the case in hysteria.

long ago that the part of the brain injured in apoplexy is not restored by electricity; nevertheless the treatment of apoplexy by electricity is considered a scientific method. In the same way psycho-therapeutic measures are often effective in cases of organic disease, without the organic lesion being thereby cured. This view does not lead us into the realms of the miraculous; on the contrary, it enables us to understand the mode of action of such remedies, even if the explanations given *do* differ somewhat. They all depend upon the following fact, established by correct observation:—*In many organic diseases, the functional disturbance, which we usually consider the symptom of the organic disease, extends much farther than the direct influence of the organic lesion justifies.* In the following considerations, an endeavour will be made to explain this fact from the theoretical point of view; they do not refer exclusively to hypnosis and suggestion, but to psycho-therapeutics in general as well. To prevent any artificial disconnection of subjects that belong together, and at the same time to avoid repetitions I should otherwise have to make in the next chapter, I shall discuss this question with reference to psycho-therapeutics in general.

In a whole series of cases, Bernheim has watched the influence of hypnotic suggestion on the symptoms of organic disease, and thinks that in many organic diseases the functional disturbance lasts longer than the organic lesion, and that the functional disturbances are sometimes much more extensive than the organic lesion justifies even during its presence. In both cases it is possible to obtain improvement by means of psycho-therapeutic influence, also when the disease is organic. Let us take a case in which the functional disturbance lasts longer than the organic change. When a muscle has been injured it may happen that the pain lasts after the anatomical results of the injury—for example, extravasation of blood, inflammation—have passed away. We can understand that this after-effect of the cured organic lesion can be done away with by psycho-therapeutics. Bernheim mentions apoplexy, in which the functional disturbance is far more extensive than the lesion. He thinks that the lesion acts on the adjacent parts like "shock," setting up functional disturbance. We can quite understand the benefit derived from mental treatment in this case also. Indeed, psycho-therapeusis sometimes satisfies the *indicatio morbi*, at least in

the first case, in which the functional disturbance lasted longer than the organic lesion, and in the second case (apoplexy) it was indicated as symptomatic treatment.

From Charcot's neuro-pathological standpoint, the successes obtained in cases of organic disease require a different explanation. As already mentioned, Charcot's conception of hysteria was totally different to that which originally obtained, especially in Germany. In particular, he held the opinion that hysteria supervenes in many organic diseases, the latter often being the exciting cause of the former. But he thought that in such cases we should make a strict distinction between the symptoms of the organic disease and those of the accompanying hysteria. According to Charcot the local anæsthesia met with in cases of lead-poisoning is often improperly described as a symptom of the intoxication, whereas it is really a symptom of the accompanying hysteria. Similarly, he thought that the tremors observed in mercurial poisoning should in most cases be considered symptomatic of the accompanying hysteria and not a phenomenon of the poisoning. In common with his pupils and many other investigators, Charcot considered hysteria an after-effect of many infectious diseases—typhoid, for example. His pupil, Gilles de la Tourette, states that malaria may bring about a recurrence of hysteria, which then takes an intermittent character. Charcot also thought he could detect the exciting cause of hysteria in many nervous diseases. The symptoms of organic nervous disease and of hysteria may become associated in many ways, partly because organic nervous disease is an exciting cause of hysteria, and partly because the two sometimes appear simultaneously without there being any causal connection between them. But it is often very difficult to decide in any particular case which symptom should be ascribed to the hysteria and which to the organic disease. Thus according to Charcot hysteria and multiple sclerosis are not infrequently observed together. Now, the intentional tremor observed in both diseases is very much the same; hence it is difficult to decide in the case of a person so afflicted whether the tremor is of hysterical or of organic nature. The way in which we conceive psycho-therapeutics to act in cases of organic disease is in accord with this theory of organic disease combined with hysteria. The patient is relieved of the hysteria, but not of any symptom of the organic disease itself.

We can see that there is a difference between the theoretical views of Charcot and those of Bernheim. But I shall not discuss that question any further, as it would practically only lead to a contention about words. Considering the difficulties in the way of differential diagnosis, and the conflict of opinion as to hysteria, the question, whether a symptom that has been relieved by hypnotic suggestion was referable to the organic disease or to the accompanying hysteria, could hardly be made the basis of a profitable discussion. As far as the practical value of psycho-therapeusis in the treatment of organic disease is concerned, such a discussion would be meaningless.

A few further considerations will serve to show the importance of psycho-therapeutics in organic disease, but I shall not discuss any further the question whether hysteria or something quite different is present, because the exact meaning that should attach to the term "hysteria" is still a matter of dispute. In many organic diseases a functional disturbance may supervene, provided the disturbance caused by the organic disease be augmented by auto-suggestion. Paralysis agitans is a case in point. It often happens that patients suffering from palsy are unable to walk properly, or fall when they attempt to get about. One fall suffices to make the patient feel even more insecure, and thus considerably diminishes his power of locomotion. We can easily understand how these troubles may be lessened by suggestion or other therapeutic measures without the organic disease being done away with. Again, let us take a case of polyarthritis deformans in which the knee-joint is affected as well as others. Motion in the joint is essentially inhibited both by anatomical changes in the joint and by pain. In addition to this there is the fear of the pain, which increases the functional impairment of the joint even when there is no real pain present. In such a case auto-suggestion may make certain movements impossible, thereby rendering the functional derangement persistent even when there is neither a mechanical obstacle nor a sense of pain. This enables us to understand how such patients come to stand up when momentarily excited; an outbreak of fire suffices to make them jump up and run out. But when peacefully seated and undisturbed, the patient cannot voluntarily rise from his chair. That disturbances which are not the direct outcome of the organic lesion should be done away with by psychic remedies is quite comprehensible,

How readily mentally-determined functional pains follow organic lesions is shown by the fact that people who have had a limb amputated often feel exactly the same pains after, as before, the ablation. Attempts have been made to refer this to irritation of the nerve-stumps in the cicatrix. This explanation may apply in some cases, but there is much to be said against it; indeed, it is much more probable that in many cases the original peripheral pain is reproduced centrally. This view is supported by the fact that the patient experiences the same pain, at the same spot, as before the amputation, which can be better explained by central reproduction than by the physical stimulation of the peripheral nerves. Of course the pain experienced by the subject in such a case could be explained by the law of the pheripheral ramification of the nerves; but that would not explain why the patient feels exactly the same pain in exactly the same spot as before the operation. Let us take as an example the case of a person suffering from a painful ulcer on the leg. The patient feels severe pains at a particular spot on the leg; he also feels that the pain is of the particular kind caused by a peripheral affection. The local affection is then removed by amputation, and yet long after the operation, often years after, the patient experiences a sensation of pain in exactly the same way that he did before. Does irritation of the nerve-stump at the place where the amputation was performed explain this? Certainly the patient *thinks* he feels the pain at the same spot as before, and not in his heel or his toes; but that is better explained by central reproduction of the pain than by peripheral stimulation; and this view is quite in accord with many other psychological experiences, for mental processes primarily set up by peripheral stimulation acquire a tendency to be reproduced centrally.

The efficacy of psycho-therapeutic measures in the treatment of organic disease is further rendered intelligible by the fact that organic troubles are more acutely felt by neurotic subjects than by those whose nervous system is in a healthy state. Maximilian Sternberg cites tooth-ache as an example "of the connection between the sufferings caused by a disease and the irritability of the central nervous system." If a person whose nerves are in a healthy condition forgets his toothache at the dentist's door, it goes to show that toothache in cases of neurosis is particularly dependent on the general state of the subject's health. Sternberg advances caries as an example of

this, because it is often connected with hysterical tooth-ache and with tooth-ache as observed during pregnancy. In these cases the general neurosis causes the pain to be felt more acutely than the caries justifies. The neurosis, and not any change in the circulation, is to blame. Consequently all kinds of suggestive remedies—mouth-washes, hypnosis, etc.—are effective in such cases, though of course local treatment should not be neglected.

But there are further considerations that elucidate the influence of mental processes on the symptoms of organic diseases. We know that pain often disappears, or becomes less appreciable, when the patient's attention is diverted from it. We know, further, that hypnotic suggestion can render a subject insensitive to the prick of a pin; indeed, minor surgical operations can be carried out painlessly in this way, although there certainly must be a certain amount of organic irritation which would otherwise cause pain. We must remember that a peripheral stimulus alone does not suffice to set up a sensation of pain; that only happens when central processes are set going by the peripheral stimulus. We know, further, that central processes mutually influence each other; the sensation of pain can be just as easily prevented by hypnotic or non-hypnotic suggestion as by diverting the subject's attention. These processes which have been observed after operations may also be expected to take place in cases of organic disease; and this may sometimes explain the symptomatic improvement which follows hypnotic suggestion and other psycho-therapeutic remedies in spite of the persistence of the organic disease.

Finally, many successful results can be explained by the fact that central processes (ideas, feelings, emotions) have a peripheral effect; the idea of a tasty dish causes a flow of saliva, shame leads to blushing, and fright to pallor; fear often has a great influence on peristalsis—we know that diarrhœa is a frequent consequence of dread and anxiety. It is sometimes epidemic among soldiers when they have to face fire. These things being so, we can understand that psycho-therapeutic processes at least produce a change in peripheral functions.

The attempts that have hitherto been made to explain these successes depend on the improvement that has resulted in many organic diseases from psycho-therapeutic measures, and must be kept quite distinct from the view that mental influence produces an anatomical change in a diseased organ directly.

I certainly do not deny that mental processes may set up organic change; in fact, I readily admit it. The facial expression of a subject mentioned by me on page 114 is a case in point. We further know that chronic emotions affect both personality and physiognomy—grief and care alter the facial expression. Dr. Weiss, also, holds that we should not curtly dismiss the possibility of hypnosis producing organic changes, and supports his view by referring to instances of the hair turning white from fright, and also to a case in which he saw a woman suffer from herpes labialis as the result of a sudden shock. Many other instances could be adduced to bear out the fact that such effects arise from mental influence. We accept mental strain, worry, and care as causes of mental disease; yet mental diseases are organic diseases of the brain, although we are seldom able to establish the nature of the organic change. I may further mention those cases in which a blister, or some other organic change, is produced by the suggestion that an epispastic has been applied. But in spite of all those objections raised by sceptics, I do not deny the influence of mental processes on the anatomical structure of our organs. Unfortunately, we are unable to make any practical use of this influence, because it has not yet been proved that we can arbitrarily bring about those anatomical changes that are necessary to ensure the cure of organic disease.

A totally different view has certainly been expressed on this subject. Many earnest investigators—such as Delbœuf, Braid, and Hack Tuke—have expressed the opinion that organic diseases may be improved by suggestion, and they have given cases in support of their view. Recent investigators, especially Bernheim, have kept aloof from this view. I only mention this because Binswanger and Seeligmüller mistakenly represent Bernheim as having maintained that the original organic injury is done away with by suggestion. Uncritical investigators have certainly reported many cures of organic affections by suggestion. Among such stories may be mentioned the supposed removal of warts by sympathetic remedies. Whether in such cases the organic affection is cured by mental influence or the warts disappear spontaneously appears to be a matter of detail. But we have much more right to doubt the professed cure of incurable diseases as related by charlatans. All depends upon the question whether the disease really

existed or not. When quacks announce the miraculous cure of "cancer, tabes dorsalis," and similar affections by such methods as animal magnetism, we are compelled to ask how was the diagnosis arrived at? Quacks often assert that the diagnosis was made by a scientific doctor. But that is no proof that such a diagnosis was ever made. Statements made by patients are anything but conclusive on this point; such people seem to take a delight in representing that they had been given up by some doctor. One thing is certain: it has never been proved that psychic methods bring about those changes necessary to cure an organic lesion.

Even if we cannot engender a trust in the psycho-therapeutic treatment of organic disease, it would nevertheless be a great mistake to ignore the value of certain mental methods. To satisfy the *indicatio causalis*, psycho-therapeusis must be taken into consideration in those cases in which mental influence has been a contributory cause. This does not merely refer to functional diseases such as neurasthenia, but also to organic affections. The injuriousness of mental influence in cases of progressive paralysis is recognized even by those who consider syphilis the prime cause of the disease. C. Wollenberg includes mental strain among the causes of paralysis; also the exhaustion that is caused by the restlessness of modern life—competition, increased personal responsibility, and all the worries and deceptions of business undertakings. Kraepelin thinks that the attendants in lunatic asylums are often injured by their employment which tends to render them subject to degenerative psychoses. The same author also thinks that "emotional excess" has its significance. As a matter of fact, if we are to satisfy the *indicatio causalis* when treating progressive paralysis we must insist on absolute mental rest from the moment premonitory symptoms of the disease appear, not merely because the diseased organ requires rest, but because mental influences may have helped to cause the disease. As already pointed out, psycho-therapeusis is specially indicated for treating the symptoms of organic disease. But we have further to consider how such diseases react on the patient's mental life. We must endeavour to minimize the feeling of ill-health—for example, by recommending a tabetic patient to engage in some congenial occupation, not merely because that is desirable from a domestic point of view. And mental treatment is all the more indicated when we have to

deal with such concomitant troubles as sleeplessness, loss of appetite, and an unhappy frame of mind.

Having thus thoroughly discussed the indications for hypnotic treatment, I must now add a few words on the contra-indications. In some cases the treatment may be contra-indicated if auto-suggestion produces unpleasant results which cannot be counteracted, and which outweigh the benefits to be expected from hypnosis. Fear of hypnosis often prevents hypnosis, or puts it off until the fear has subsided. As we have already seen, a patient who is afraid of being hypnotized often exhibits unpleasant symptoms. In the same way care should be exercised when hypnotizing excited or weak-minded patients, and we should even desist when the subject is hysterical and has a tendency to morbid auto-suggestion. Of course I do not profess that any unimportant disturbance of auto-suggestive origin should prevent us from employing hypnosis. On the contrary, the good results which we expect from hypnosis must be compared with the possible evils of auto-suggestion, and a definite conclusion thereby arrived at in the manner customary in medical practice. It should be evident from what I have already said, that such *contretemps* as a feeling of vertigo caused by auto-suggestion, or an occasional attack of hysteria, should not lead us to abstain from hypnotic treatment. But it is only the medical specialist who can appreciate the significance of such concomitant symptoms and draw a correct conclusion from them. The fact that medical men have described hypnotic treatment as harmless does not justify its use by quacks. The harmlessness of hypnosis depends on the special knowledge and experience of the physician—his capacity for appreciating the significance of auto-suggestions.

I shall now proceed to give cases in which I have observed hypnotic treatment prove of use.

Case 1.—Cephalalgia and cardialgia. Mrs. X., *æt.* 30; her mother and sister often suffer from headache; similar trouble herself, and also attacks of cramp in the stomach since she was sixteen. She is said to have vomited blood once. The whole region of the stomach is painful on pressure, but there are no typically sensitive spots on the head. Certainly several nerve trunks seem sensitive to pressure on both sides of the head, but not to a greater extent than is met with in nervous persons. The attacks of headache affect the whole head; they are accompanied by nausea, though vomiting is rare. The attacks of headache do not seem to synchronize with the pains in the stomach. The head is seldom entirely free from pain, and the patient has an almost constant feeling of oppression in the head.

For a long time she was treated by drugs and also by static electricity, but without effect. The good effects of hypnotic treatment were soon apparent; moderately deep hypnosis removed the feeling of pressure, and post-hypnotic suggestion suppressed the attacks of pain. Even when an attack did occur it was possible to suppress it almost momentarily by means of suggestion. I have followed this case for many years, and although it has occasionally been necessary to repeat the hypnosis every two or three months the immediate benefit has always been extraordinary. The patient has not had a severe headache for three years, and all attacks of pain in the stomach are easily cut short by suggestion.

Case 2.—Nervous cough. Miss X, *æt.* 22; temperament nervous and somewhat hysterical. Has suffered from nervous cough for the last eight months; I could hardly see her for a quarter of a minute without her coughing. The coughs are short, sharp, and regular in sequence. The patient had been treated with many medical preparations. Neither local treatment of the larynx, nor cold rubbing, nor the pack applied to the body produced any effect. A water-cure at Lippspringe and electricity were also ineffectual. But after a few days of hypnotic treatment the cough began to yield and had quite disappeared at the end of a fortnight.

The patient has been two years without treatment, and the cough has not reappeared.

Case 3.—Attacks of hysteria. X., a labourer, *æt.* 25; has suffered from convulsions for seven years. According to his own account he partially loses consciousness during the attacks, but not completely; he has a semi-notion of what is going on around him. He does not bite his tongue, which is free from scars. He once seriously injured one of his eyes during an attack. When the attacks are on he lies flat on his back and his whole body is convulsed; the attacks last from three to ten minutes, and generally recur once a week. Drugs and other remedies have proved useless. The patient can be thrown into deep hypnosis; during the second stage he has sense-delusions, and there is loss of memory on waking. He has not had an attack since the first day of hypnotic treatment, and was still free when I saw him two years later.

Case 4.—Traumatic paraplegia. X., a girl aged ten. Fell downstairs six weeks ago, and is said to have lain unconscious for a quarter of an hour. Shortly afterwards she was found to have lost her speech; a day later spasmodic movements of a slight nature were observed, chiefly of the tongue and legs, the hands being unaffected. Ten days after the fall there was complete paralysis of both legs, and the child doubled up directly she tried to stand. Hypnosis was then tried, and although only the first stage was reached the child was able to walk in a quarter of an hour, and is still free from paralysis although many years have elapsed.

Case 5.—Hysterical aphonia. X., *æt.* 17, is a member of a nervous family and has suffered from aphonia for four months. He cannot speak out loud, only in a whisper. The history of the case mentions catarrh of the larynx, but examination determines the diagnosis hysterical aphonia. The patient could be hypnotized into the first stage, and although simple suggestion proved ineffectual good results were obtained by combining suggestion with forced expiration. Four days later the patient was able to articulate simple syllables distinctly, and six days later his speech could be pronounced normal. There was no recurrence for a year, and even then the trouble yielded to hypnotic treatment.

As far as hysterical aphonia is concerned I could recount many cases in which loss of voice has been cured by hypnotic suggestion.

Case 6.—Nervous sickness. The patient, a pale young girl aged 18, and a member of a healthy family, has suffered occasionally from headache. Her chief trouble is sickness, which frequently comes on when she has eaten anything. Retching and vomiting often occur within a minute of swallowing even a drop of water. When she came under treatment the trouble had lasted more than a year. The patient was very much run down, and it was almost impossible to stop the vomiting without narcotics. All kinds of treatment were tried, and it was sometimes found possible to suppress, or at least delay, the usual sickness by taking energetic measures to distract the patient's attention when she had had a morsel to eat. Nevertheless it was not often possible to distract the patient's attention sufficiently, which shows how much the patient's mental condition affected the vomiting. I then tried hypnotic suggestion and found that the patient could be easily hypnotized to a deep stage. She was very much exhausted by the first hypnosis, but this discomfort disappeared as the hypnosis was persevered with, and she was finally able to eat and drink with only occasional attacks of vomiting. Post-hypnotic suggestion gradually made this improvement perceptible in the patient's waking life, and in fourteen days time she was able to take food without vomiting. There has been no alteration in her condition for a year and a half.

Case 7.—Somnambulism. This patient, 18 years of age, is the son of a violent father and an epileptic mother. It was noticed that from his childhood he talked aloud in his sleep, and sometimes got up and wandered about his bedroom, but he had no recollection of these occurrences. His parents were afraid to speak to him when he was walking in his sleep, because of the popular notion that it would be dangerous to do so. His somnambulism sometimes led him from the upper story in which his bedroom was situated to his grandparents' apartment on the ground floor. This occurred on an average once a fortnight. No convulsions were ever observed, and I think this case must be considered one of somnambulism, although a nice distinction from mentally produced epilepsy was not quite possible. The suggestibility of the patient was very marked during hypnosis, and the attacks of somnambulism were easily suppressed. There have been no attacks for eight months. The patient often talks at night; but he never gets out of his bed, although he has not been hypnotized for six months.

Case 8.—Narcolepsy. The patient is eighteen years of age and suffers from attacks of somnolence. His father was also similarly afflicted. The patient has observed that close reading makes him quickly tired and fall asleep. This occurs almost daily, so that he has hardly the courage to read. The trouble has lasted for nine months, and electric treatment has proved unsuccessful. But hypnotic treatment has not only stopped the tendency to fall asleep when reading, but has also suppressed the tired feeling otherwise experienced. I was able to follow this improvement long after the patient had ceased to be treated.

Case 9.—Asthma of mental origin. Mrs. X., *æt.* 48; formerly a heavy drinker; has suffered from attacks of dyspnœa for eight years, especially in the afternoon. The attacks have often been so severe that the patient has been compelled to get up and open doors and windows so as to get fresh air. The attacks are almost of daily occurrence. The lungs are healthy; but the

first mitral tone is not quite pure but somewhat broken. Otherwise there is nothing abnormal—no bronchial catarrh and no emphysema. The patient was subjected to all kinds of hydro-therapeutic treatment; electric treatment was employed; her nose was examined and given local treatment, but all to no purpose. The patient was then treated hypnotically; the first experiment produced the second stage of hypnosis and almost complete anæsthesia. From the first day there was no attack of asthma. The patient was first of all hypnotized daily for a fortnight, then every second day, and finally once a month. In the end it was possible to do without hypnosis as the attacks did not recur, and after many years of observation I am satisfied that the cure is complete.

Case 10.—Chronic constipation. Mrs. X., *æt.* 38, a somewhat corpulent member of a healthy family; suffered from chlorosis when she was fifteen years of age, but had not apparently suffered from any other trouble. At present she is occasionally troubled with a sense of numbness in the head but no direct pain; also some minor troubles such as lassitude and heaviness of the limbs. As a rule she only has a motion every third or fourth day, and then it is very hard. She has taken Carlsbad salts in increasing doses, also aloes pills, and other such medicaments. She was obliged to take such large doses that she finally agreed to undergo hypnotic treatment, especially as weeks of massage had done her no good. At the first attempt she fell into deep hypnosis, and a copious evacuation on the following morning was successfully suggested. She was hypnotized on ten successive occasions, and then at longer intervals, and when I last saw her two years after she had stopped the treatment, her motions were normal—a copious evacuation every morning without the use of drugs.

Case 11.—Enuresis nocturna. The patient is a well-developed boy aged ten; has always suffered from enuresis nocturna, and wets the bed nearly every night. Electricity, drugs, and the sound have no effect. At the second trial the boy was deeply hypnotized. Post-hypnotic negative hallucinations were induced, and the boy was ordered to wake up at night directly he felt that he must pass water. The time for waking was gradually postponed by suggestion until the morning. Suggestion was then less frequently employed, and now after five months there is no trace of enuresis nocturna.

Case 12.—Pruritus cutaneous nervosus. X., *æt.* 42, has suffered from nervous itching of the skin for four years. The itching is generally so severe that the patient has to get up at night and scratch himself till he bleeds, or else to get his wife to rub him down with a scrubbing brush. In consequence of the loss of sleep—nervous itching is always worse at night—the patient is very run down and poorly. On examination I find him pale and thin. He has tried antipyrin and other drugs, also electricity and baths, but all in vain. One week of hypnotic treatment not only set up an improvement in a malady of years' standing, but even put a stop to most of the symptoms. The patient could again sleep quietly, although the itching sometimes recurred once a week but in a less intense form. The patient was discharged in a fortnight, and when I saw him some years later there had been no recurrence of the trouble, all traces of which had disappeared.

Case 13.—Chronic chorea. The patient, a girl aged 16, had a bad attack of chorea eight months ago. Six weeks later the spasms were less severe, though often considerable at the time she came to me for treatment.

Drugs and electricity had proved useless, so hypnotic treatment was decided on, with the result that the patient who had hitherto been unable to hold a glass or a cup or do any manual labour, was enabled in three days' time to take up her household duties. A fortnight later every trace of spasm had disappeared.

I have often seen neglected cases of chorea improved by suggestion; but I have never observed any noticeable improvement in acute cases even when the subjects have been specially susceptible to deep hypnosis.

Case 14.—Tremor nervosus. X., a mechanic, 26 years of age, belongs to a family with hereditary taint; is of a very excitable temperament, and has various neurasthenic troubles. He is specially afflicted with trembling of the hands, which takes the form of small and rapid movements and is particularly inconvenient because a steady hand is necessary in his business. The trembling is very marked when he stretches his hands out, and increases when he is engaged on fine work. After six attempts deep hypnosis followed by amnesia was induced, and after this I was always able to suppress the tremor which has not recurred after the lapse of two and a half years.

Case 15.—Agoraphobia. The patient is 36 years old. His mother, and also his brothers and sisters, suffer from migraine. From his twenty-second year he has always felt discomfort when crossing an open space. This increased from year to year until he is now unable to cross an open space. After a few steps he is attacked with trembling and vertigo; his sight becomes blurred, he perspires and is obliged to retrace his steps. As is usually the case with such patients, he is able to cross a square if accompanied even by a child, and he is also able to gain the other side of the square by taking a roundabout way, such as keeping close to the houses. His condition has remained almost invariably the same, and no method of treatment—water-cure or other—has been of any use. Hypnotic suggestion brought about marked improvement after three sittings. The patient was at once able to cross a small open space, and the improvement has now so increased that he can cross a large square without assistance. I have observed that the improvement has been maintained for several years.

Case 16. Imperative ideas. The patient, Mrs. X., is 42 years of age, and a member of a somewhat neuropathic family. Her relatives are all described as being nervous. Eleven years ago she suffered from severe impulsive ideas which lasted for three years. When she came to me for treatment she had been suffering in the same way for more than a year. She was overwhelmed by the idea that she was suffering from an incurable disease, and was very much depressed. She was often troubled with thoughts of suicide and was tired of life. She complained that her illness prevented her from associating with her family and that she took no interest in her relations. She complained that she had lost all interest in painting, nature, the theatre, poetry and everything that had formerly interested her. Baths, drugs, the galvanic current and static electricity were tried in vain; but hypnotic treatment speedily produced good results. Deep hypnosis was produced at the first sitting, and was followed by post-hypnotic negative hallucinations. For the first three weeks the sittings were

repeated daily; the impulsive ideas were always much weaker directly afterwards, and the patient's condition was improved. After about four weeks the patient's condition was so much improved that the treatment was stopped. After the lapse of a year there has been no recurrence of the former troubles.

Case 17.—Fear of blushing. The patient, *æt.* 25, had suffered from an uncontrollable tendency to blush since he was fifteen. It occurred more frequently when he was with other people than when alone. This inconvenience had continued to get worse. As is usual in such cases the blushing generally occurred when it was most dreaded. The patient was in official employ, and the attacks were most likely to come on when he was in the presence of his superiors, which rendered his position most painful. He tried letting his beard grow solely to render the blushing less noticeable, and also various other artifices, such as turning his face away to conceal its redness. Patient was in despair and begged for relief, as he had tried all kinds of remedies. I found it easy to induce deep hypnosis, and by suggestion cured not only the blushing but more especially the dread of it. Suggestion was at first employed frequently, and then at longer intervals. The old fear of blushing gradually subsided, and he now very seldom blushes, and even then only at times when it hardly causes him any inconvenience.

Case 18.—Ischuria of mental origin. X., *æt.* 25, of a generally nervous disposition, but free from any organic disease, had long experienced difficulty in passing water. He found it difficult to make water voluntarily even when his bladder was full. The trouble particularly affected him in public conveniences. Even when alone he could only pass water after exposing himself for some time. The trouble, which had lasted many years, was all the more painful as he felt the desire to make water but could not do so. I found it easy to induce deep hypnosis, and a few sittings sufficed to effect a cure, and when I saw him some months later there had been no recurrence of the trouble.

I have already mentioned that great improvement can be obtained in the symptoms of organic disease, and I now append a few cases.

Case 19.—Multiple sclerosis. Mrs. X., *æt.* 34, had for years suffered from increasing tremors, which only occurred when she attempted to carry out some voluntary movement. Although her head was not quite free, the tremors were particularly noticeable in the arms and legs Movement of the lower limbs obviously weakened; gait uncertain and spastic in character. Her speech was scanning, there was evident nystagmus, and she sometimes suffered from retention of urine. Sensory disturbances were not very great but easily demonstrable, the skin in particular being less sensitive to temperature and touch. Hypnotic treatment was directed essentially to the tremors in the arms, and to the urinary troubles. There was very considerable improvement in the former directly after the first application of hypnotic suggestion. This could not be considered an accidental coincidence; for although the patient had previously been unable to lift a glass or a spoon to her lips, she re-acquired the power of so doing

for at least some considerable time, as a direct result of suggestion. There was also a noticeable decrease of the urinary trouble. When the necessary hypnotic suggestion had been made, the patient was invariably able to pass water comfortably, and the improvement sometimes lasted a few days, occasionally even longer—as much as three weeks. After that it became necessary to repeat the suggestion. Of course a cure was neither expected nor obtained. On the contrary, the symptoms of the progressive nature of the disease were evident after a time.

Case 20.—Deformative polyarticular rheumatism. The patient, aged 49, had suffered from severe articular rheumatism for eight years. There were large bony protuberances in the finger-joints and also in the knee-joints, rendering movements of most of the affected joints very limited, or quite impossible. The pains were indescribable, making it impossible for the patient to sleep at night or rest during the day; he therefore desired hypnotic treatment so as to obtain relief from some of his pains. It was found that deep hypnosis could be induced and the patient thereby almost invariably relieved from pain for a certain time. The patient's ability to move gradually returned, and he was finally able to walk a little, even to go downstairs. The treatment lasted four weeks, and when I saw the patient subsequently I found that the improvement had been maintained, except for occasional attacks of pain which always speedily gave way to suggestion. As a rule the loss of pain was so marked after suggestion that there could be no doubt as to its cause.

Of course there can be no question of curing osseous and cartilaginous deformities, but it is always possible to modify a whole series of morbid symptoms in such cases as the above.

Among other diseases accompanied by organic injury, I have seen a very painful eczema of the ear, in a child of eight, made completely painless by post-hypnotic suggestion. I observed this case in the company of my friend and colleague Friedemann, then of Köpenick but now of Berlin, whom I have to thank for a number of interesting experiences in hypnotism. The child in question had so painful an eczema that he could not bear the slightest touch. An order given in his first hypnosis had such an effect that he could afterwards endure even strong pressure on the affected spot.

We have now to consider how hypnotic therapeutics can best be installed as a practical method of treatment, and this at once brings up the question: To whom shall hypnotic treatment be entrusted? There can be but one answer: solely to the scientifically trained medical man. He alone is in a position to establish a diagnosis and thereby determine the indications and contra-indications in any case; he must watch the effect of the treatment on the patient and decide whether or not the

hypnotic method should be continued. He alone can decide on diagnostic grounds whether hypnosis should be supplemented by other methods of treatment or be stopped. I do not deny that there are laymen who are well acquainted with the technique of hypnosis, perhaps even better so than the average medical man. Delbœuf was one of them; but we need not take such exceptional cases into account when discussing the practice of hypnosis. But apart from that, such an investigator, even if possessed of the requisite knowledge of the technique of hypnosis, would be unable to determine what was indicated by the diagnosis in any particular case; and it is even more important to oppose the therapeutic use of hypnosis by ignorant laymen, such as charlatans. For this reason Warda has entered a protest against Lœwenfeld's proposal that attendants should at times be allowed to hypnotize patients for the purpose of alleviating certain symptoms. Warda certainly thinks that a therapeutic hypnosis should never be entrusted to any one who is not a medical man, otherwise we should be encouraging quackery.

On the other hand, I do not think that every medical man is fit to undertake the hypnotic treatment of patients. Just as in other special branches special knowledge and experience are requisite, I am also convinced that the experimenter in hypnotism must have a special faculty for hypnotizing and suggesting, and to avoid any misunderstanding I may add that I do not lay any claim to the possession of this special faculty. Frank regrets that suggestive therapeutics have been so greatly neglected by doctors, and traces this to the fact, among others, that everybody does not possess the necessary ingenuity, as it were, for employing the method successfully. Just as many *imponderabilia* play a part in hypnotic treatment as in mental therapeutics in general; and this prevents the majority of experimenters from obtaining the best results. The gift of individualizing, which we so often hear of in medicine, is given to few as far as hypnotic treatment is concerned. But this power is all the more necessary because men are no more alike mentally than they are physically. Since each of us does not possess this gift, we have no right to deny the successes of others because of our own failures. An eminent Swedish alienist—Oedmann—says that he recognizes the good effects of suggestion in alcoholism, but that as he is unable to produce them he sends such patients to Wetterstrand (Corval).

In any case, it is a mistake for doctors, who have no aptitude for mental therapeutics and who moreover lack experience, to deny the successes of others.

But even if every one is not a hypnotic therapeutist by nature, it does not follow that specialists alone have a right to practise hypnotic treatment. In simple cases it is not always necessary to call in a specialist; and in addition to specialists there will, very properly, always be some medical men who occasionally practise hypnotic treatment. It is much the same here as with other special branches. A country doctor, or one in a small town, often treats cases or employs methods, which in a large town would be left entirely to a specialist. But even in large towns we can hardly desire that all hypnotic treatment should be carried out exclusively by specialists. Sometimes no advantage whatever would be gained by the patient leaving the doctor who had been treating him and seeking the services of a specialist for the purpose of some hypnotic sitting or other. The patient's circumstances have also to be considered in such a case. In short, it is quite wrong to assume that only a medical specialist should hypnotize. Of course a patient has a right to expect that a doctor who undertakes to treat him hypnotically has had a certain amount of training in the practice.

But I do not think it right that when a patient places himself in the hands of a specialist for hypnosis the latter should confine his treatment exclusively to hypnosis. Specialism is not without its dangers. The specialist who only treats particular diseases, like the specialist who only employs certain methods, has a tendency to become biassed. The mere fact that we cannot dispense with these two forms of specialism is no reason why we should shut our eyes to their dangers. Specialization, especially as regards methods of treatment, should be carried no farther than is absolutely necessary. For this reason I do not think it right to consider hypnotic treatment an exclusive speciality. The man who devotes himself to hypnotic treatment must cultivate psycho-therapeutics in general as well. There are so many details and so many combinations to be considered, that it would be irrational to separate hypnotic treatment from the rest of psycho-therapeutics. I will give an example. The assertion that alcoholism can only be cured in an institute is a fairy-tale; many alcoholists can be made abstainers by proper mental measures outside an institute. In

some cases hypnosis renders the task easier, but alone it seldom leads to a cure. Wherever drinking is a social custom, the patient is always liable to be led astray by his companions. It is consequently necessary, especially in the earlier stages of the treatment, to modify the patient's surroundings so as to keep him out of temptation. The beneficial effects of temperance societies and of doctors who are abstainers depends on this. Bonne lays special weight on the doctor being an abstainer, because his example and the conviction it brings are most effective methods of suggestion. Without wishing to exaggerate its importance, I must certainly point out the necessity of giving some advice to the patient's *entourage*. In many cases it happens that a patient who is convinced he can do without alcohol thinks he can also resist temptation; but at first he requires protection, which the doctor must endeavour to provide him with. It would be foolish for the doctor to leave this to a colleague, and merely confine his own attention to the hypnotic treatment. Legrain reports that a number of dispensaries have been established in Russia, at the instigation of the Government, which are visited by numbers of alcoholic subjects in search of free treatment. Here hypnosis is the chief, but not the only, remedy employed. And such is the only right way. It is almost invariably necessary to bring a number of other mental influences into play simultaneously with hypnosis. Mental treatment is not quite such a simple matter that "every tailor, cobbler, and shepherd-boy" can carry it out, as Ewald thought.

I go still farther on the question of specialism in hypnosis. Psychology teaches the physician the unity of man's being, and that should be taken into consideration in the treatment of a patient. For a doctor who is treating an emaciated neuropathic patient for sleeplessness to consider himself so much a specialist in hypnotism that he cannot attend to the patient's diet himself, but must call in a specialist in dietetics every day, is not only ludicrous but injurious as well. It is just from the psychological standpoint that uniformity of treatment is so necessary. For this reason I agree with Forel that, as far as possible, only one doctor should treat the patient by methodical suggestion. I do not mean by this that a medical hypnotist should not occasionally call in a colleague such as a specialist in diseases of nutrition, or that he should not busy himself specially with hypnosis, but he should not do the latter exclusively. I may here remark that apart from the patient's interests, it is not to

a doctor's interest to let himself appear merely the agent of a colleague at whose orders he confines himself exclusively to applying hypnotic treatment. This would be just as degrading as for a surgeon to perform an operation at a colleague's request and then leave any medical after-treatment of the patient to another medical man. In the same way, a doctor should never let a patient tell him what is indicated; he only places himself in an undignified position by so doing. There is great danger of this happening when a doctor devotes himself entirely to hypnosis to the conclusion of all other methods of treatment.

Granted these restrictions, we would wish that there should be specialists in hypnotic treatment. Most doctors' day's work is so arranged that they are prevented from practising hypnotic treatment systematically. That requires a considerable amount of time to be reserved for special cases; also the doctor when so engaged should not be disturbed every minute by the front-door bell, the ring-up of the telephone, the impatience of clients in the waiting-room, etc., etc. A man has to arrange the nature of his practice and map out his day accordingly. Many doctors are able to do so; others will not. A suitable arrangement of the hours for treatment is all the more necessary, because the erroneous notion is still very prevalent that hypnosis cures in one or two sittings.

It would take too long to give a detailed description of all the rules for hypnotic treatment. Works devoted exclusively, or essentially, to hypnotic or suggestive therapeutics, give many details and may be consulted by persons interested in those questions. I make special mention of the works of Bernheim, but refer more particularly to the articles by Brodmann which appeared in the *Zeitschrift für Hypnotismus*, 1897–1900. As a rule every doctor gradually develops his own method. In this as in the methods of hypnotizing there are also numerous individual differences. Nearly every one who practises hypnotic therapeutics has some device that he considers particularly useful. Liébeault and Bernheim, Forel and Vogt, Schrenck-Notzing, Grossmann and Hirschlaff, Döllken—nearly every one of them has described some artifice or other that he has found effective.

It is of importance that the patient be given a certain amount of preparation; any feeling of dread of hypnosis must be removed first of all, then the nature of his malady must be

made clear to him, and finally it must be explained that mental influence can often be employed to a patient's benefit. As Bourdon very properly remarks, the hypnotist should not begin by telling the patient that he has no pains. Considering the dangers that may arise from fear of hypnosis, it is as well to desist from hypnotizing the patient until the fear has passed. Forel and Tokarski have expressed themselves in this sense.

We must, further, make a distinction between preparatory and therapeutic experiments. First attempts at hypnosis may fail but later ones prove successful, and only a superficial hypnosis may be induced when a deeper is desired. Certainly opinions differ as to what should be the depth of the hypnosis; but I agree unconditionally with those investigators who consider that suggestion is a much more powerful therapeutic agent in deep hypnosis than in superficial, and I cannot understand how anybody can maintain the contrary. Fuchs also thinks that deep hypnosis is not necessary when treating sexual perversion. I take a totally different standpoint, and advance the following views for consideration. Changes that occur in the voluntary muscular system are the chief characteristics of superficial hypnosis, and how should that enable one to influence impulses and emotions in the manner requisite for the successful treatment of sexual perversion? On the other hand, the essential characteristic of deep hypnosis is that it enables us to influence the patient's mental life, and not merely his voluntary muscles. I do not deny that light hypnosis has proved successful in some cases of sexual perversion, but I think that in such cases other factors were possibly of greater influence than any specific action of hypnotic suggestion. Many things quite independent of hypnosis are of great therapeutic value—such as the avoidance of unseemly pictures. Further, auto-suggestion on the part of the patient, arising from confidence in the treatment, may also act beneficially. Confidence in hypnosis may alone prove beneficial, but the success should be ascribed to the patient's trust in the remedy employed, and not to any specific action of hypnotic suggestion. We can understand that this differs essentially from the action of hypnotic suggestibility. Of course, when a patient has confidence in superficial hypnosis we must utilize that confidence. But there are cases in which deep hypnosis holds out better prospects, if it can be attained. I have often had to regret my inability to produce deep hypnosis in cases in which it promised good results.

Practice and experience enable us to decide in each case whether the hypnosis is deep enough for therapeutic experiments, or whether the patient's receptivity for therapeutic suggestion should be increased by preparatory measures.

In many cases the object of treatment can best be attained by pursuing a slightly roundabout way. Laurent mentions cases in which persons have been weaned from tobacco by suggesting that the smell of tobacco is unpleasant. In this way better results are often obtained than by suggesting that the patient should give up smoking. In many cases I have found it an excellent plan to place the hypnotic subject back into earlier periods of life. I have sometimes found that suggestion, even in deep hypnosis, did not remove acute pain; but if I placed the patient back in a period when he suffered no pain, it was possible not only to remove the pain during hypnosis, but also to keep him free from pain when he returned to his own time of life, and after awaking. Russel Sturgis recommends, at all events in such cases of "fixed ideas" as he treated, that the suggestion should be noted down before hypnosis and communicated to the patient, in order to exclude any misunderstanding of the suggestion; he further recommends frequent repetition of the suggestion during hypnosis, and its communication in a present, not a future, form. But there are points that have to be considered in connection with each individual case; no general rule applies to them all. In many cases it is better not to make any suggestion during hypnosis, because the auto-suggestion arising from pre-hypnotic suggestion acts more powerfully when verbal suggestion is left out. I have often seen the advantage of this when the hypnosis was superficial and consciousness retained. A patient knows what is expected to happen when he is hypnotized, and the expected happens without verbal suggestion. The latter is often superfluous, may even be injurious. Unintentional resistance on the part of the patient may easily make itself felt and thereby bring about exactly what is not wanted.

Since intense pain often renders hypnosis impossible, it is sometimes necessary to make the preparatory experiments during a lull in the pain. Later on hypnosis can be induced even when acute pain is present.

Nothing definite can be said as to the frequency and duration of the sittings. There are cases in which hypnosis has to be repeated frequently, and it is sometimes necessary to have

recourse to hypnosis to prevent relapses. In other cases it is as well to limit the sittings from the first, because, as Forel has very properly pointed out, the experimenter's influence may gradually diminish as the training of the patient increases. In other cases, again, it may be necessary to continue the treatment for some time. Sometimes we have to proceed slowly and methodically when the malady to be cured by suggestion is deep-rooted. I cannot understand how some doctors invariably expect immediate success from hypnosis. A doctor is often satisfied if he obtains good results after many weeks or months of electric treatment, and in an establishment a cure has to be carried out for very many months. Why, then, should hypnotic treatment be expected to cure disease in one or two days?

Various authors have recommended the practice of collectively hypnotizing several patients, as Liébeault did many years ago. The psychic contagion is said thereby to become stronger and hypnosis rendered much easier. Other hypnotists—for example, Wetterstrand, Grossmann, Vogt, etc.—have made the experiment more recently, but as Max Hirsch has shown, it has both advantages and disadvantages. Among the latter may be mentioned possible breaches of professional secrecy. I recollect, for instance, the case of a doctor who by such collective hypnoses enabled some patients to know what others were suffering from; that this one was a morphinist, that a dipsomaniac, that one patient was a masturbator, another a victim of sexual perversion, and so on. Schrenck-Notzing further remarks that people of the better social classes very often object to such collective hypnoses. There is also the danger that the personal influence of the hypnotist in individual cases may be lost. Mavroukakis also refers to the possibility of what he calls indirect suggestion.[1] It would also easily be possible for a patient, A., to be influenced by a suggestion to B. But if A. is suffering from narcolepsy, and B. from insomnia, the suggestion to B. to sleep more would hardly work favourably on A. But such action cannot always be excluded on all these grounds. I conclude that while hypnosis in common has certain advantages, such as sparing the physician's time, it cannot be recommended in all cases.

[1] This is a different kind of indirect suggestion from that mentioned on p. 54.

As I have already mentioned, the employment of hypnotic suggestion should not preclude the use of other remedies, when their application is indicated; and I have also called attention to the fact that other mental influences besides suggestion may be operative during hypnosis. The action of suggestion may, for example, be supported in a case of hysterical vomiting, by telling the patient during hypnosis that she is not suffering from any organic disease of the stomach. In cases of stammering, in which the patient's mental state of course plays a great part, we shall sometimes have to supplement suggestion by exercises in talking. In a case of aphonia following laryngotomy, published by Hilger and Sänger, the action of hypnosis was supplemented by systematic exercises of that kind. And it is also just as necessary to avoid anything that might spoil the prognosis or render suggestion ineffectual as it sometimes is to employ other remedies in conjunction with hypnosis. It is, therefore, sometimes as well to explain to those present that they should not make heedless remarks or put stupid questions that might counteract the suggestions made to the patient. Just as people can be made ill by constantly telling them how poorly they look, so a cure may be prevented by making the patient believe that it is impossible or by putting him in constant dread of the remedy—hypnotic suggestion, for example.

It is often said that hypnosis may be used, but only as a sort of last hope. I consider that not only an unjust view of the importance of hypnosis but impracticable as well. Considering the large number of remedies and methods at our disposal, some patients would have to attain the age of Methuselah before hypnotic treatment would be permissible on such principles. It is the duty of every one who believes that hypnosis is harmless when properly applied, to use it where it is indicated. It often happens that the longer a disease has lasted the more difficult it is to cure, and some diseases become incurable because they were not rightly treated at first. We might hesitate to make long preparatory experiments with people difficult to hypnotize (Grasset). But where one or two experiments demonstrate that a sufficiently deep hypnosis can easily be induced, it would be a mistake to postpone hypnotic treatment until a hundred other methods, all disagreeable to the patient, had been tried in vain.

I have hitherto discussed the use of hypnotic suggestion to

remove morbid symptoms, but, as I have already said, the importance of hypnosis to medical practice is not limited merely to that. For instance, I briefly mentioned prolonged hypnosis and touched on the prophylactic use of hypnosis where morbid states have a tendency to recur at times. I also mentioned seasickness, and I may add that hypnotic suggestion has been recommended as a prophylactic in cases of attacks of migraine. It has also been specially proposed that the increased power of memory exhibited by hypnotics should be utilized for therapeutic purposes. This has occasionally happened in close connection with suggestion. Naef used it to remove a temporary total, and partially retrograde form of amnesia. The patient, who had lived in Australia at the time his memory was beginning to fail, was methodically treated in hypnosis with the suggestion that he would at once regain his memory for details of all kinds, and would retain it on waking. The possibility of increasing the power of memory in hypnosis has been used by others—Brodmann, for example—for the purpose of ascertaining the pathogenesis of certain disturbances. Various authors also state that in cases of organic lesion it is possible to carry out movements in hypnosis which cannot be executed in the waking state. Stembo thinks this must be ascribed to a heightening of memory; he believes that the memory-images of movements that have fallen into disuse are lost in the waking condition. Consequently the movements cannot be carried out even when the lesion is repaired. In hypnosis, however, there is a heightening of memory by which the mechanism of movement is again recollected and so set in action. On the other hand, a few doctors—Vogt and Stadelmann, for example—have employed suggested amnesia for therapeutic purposes.

Special mention must here be made of two authors, Breuer and Freud, who have put forward the possibility of heightening the memory in hypnosis as a recommendation for a special form of treatment which they term the cathartic method. They started from the hypothesis that hysterical symptoms are often caused by an arrested emotion. The process which produced the hysterical symptoms must be made quite clear so that the emotion which it arrested may be released. For this purpose the patient should be made to give as detailed an account of that process as possible, and should also be made to express the emotion in words. Both investigators assert that they have

caused the immediate and permanent disappearance of single hysterical symptoms in this way. Freud found later on that he could influence other neuroses not usually considered hysterical, and especially imperative ideas, in the same way. Freud gradually extended his investigations to other neuroses, with the result that he became convinced that, if we may speak of a cause in connection with acquired neuroses, then the etiology of such complaints must be sought in some sexual moment. I do not intend to discuss the further development of these theoretical considerations, but will merely remark that what Freud says on the etiology of neuroses is undoubtedly one-sided, and will not, I trust, meet with general acceptance, for surely we do not study human beings solely from the standpoint of sexual life. Freud then proceeded to develop the cathartic method, which he calls Breuer's, still farther, and applied it to non-hypnotic states in particular. For example, under cover of all kinds of pretexts, a waking patient was set the task of recollecting what it was that had brought about his troubles. He was ordered to exercise his memory, concentrate his thoughts and shut his eyes; his forehead was then touched, in short all kinds of devices were resorted to to heighten the memory of a waking patient. The point was, that the patient should recall the process that, in Freud's opinion, had acted morbifically. In this Freud started with the special assumption that a sexual process was the original cause of the mental injury and thereby gave rise to the hysterical trouble. In addition to hysterical troubles, imperative ideas are said to be caused in this way. But general experience has not justified the hopes that were placed in the Breuer-Freud method. Krafft-Ebing, Lœwenfeld, Seif, Warda and many others with whom I agree, have not observed any great benefits from it. Krafft-Ebing, in particular, was quite right in saying that even if heightening the memory suffices to cause the original mental lesion to rise once more into the primary consciousness, it by no means follows that the patient is cured. To which I think we must add, that much that is assumed to be pathogenic was unintentionally suggested to the patient by Freud in the exercise of this method; this also is a source of error that can hardly be avoided. When good results are obtained, they are not necessarily the effect of the cessation of the reaction—*i.e.*, the reproduction of the idea on the outbreak of the emotion. Here, also, it is far more probable that unintentional suggestion

on the part of the physician employing this method is the source of the error.

I have hitherto only discussed the therapeutic importance of hypnosis, but a certain amount of importance has also been ascribed to it for purposes of diagnosis, by Bernheim first of all, as far as I know. There are cases in which the diagnosis between functional and organic disease is not clear. If the morbid symptom—aphonia, for example—disappears under suggestion, then the affection is functional, but if it persists the trouble is more probably organic—*i.e.*, the diagnosis is to be established *ex juvantibus*. Delbœuf, Brunon and Ernould have published cases in which the diagnosis of organic disease has been rendered easier by hypnosis. A case mentioned by Schibbye of hysteria localized in the urinary sphere was diagnosed in this way. Similarly, an error in diagnosis which had been made in Czerny's Clinic, Heidelberg, was corrected by Starck by means of suggestive treatment. The patient, a woman aged twenty-two, was suffering from suppuration of the right middle-ear. In addition to deafness there was severe unilateral headache, vomiting after each meal, giddiness, inability to walk, absolute sleeplessness. A cerebral abscess was diagnosed, but operative measures were postponed for a time. In the meanwhile hypnotic treatment was resorted to, with the result that the sickness, sleeplessness, giddiness and inability to walk disappeared, and the patient was very soon able to attend to her household duties. Although the patient continued liable to passing attacks of headache, we are justified in assuming that the original diagnoses was erroneous and that it was subsequently rectified by the treatment employed. At least this assumption seems to me much more justifiable than that the abscess was cured by suggestion or had healed spontaneously. A. Westphal, also, has published a case belonging to this class; in it the psychogenic origin of contractures of the eye (convergence, and contraction of the pupils) was proved by the arbitrary removal or production of the contractures in hypnosis. The question has also been discussed whether epileptical or hysterical amnesia can be removed by suggestion. Riklin, who thinks that he can remove epileptical amnesia by hypnosis, is nevertheless opposed to the assumption that the disappearance of the amnesia points to hysteria, or its persistence to epilepsy. On this question I refer the reader to what I said on page 125.

Gumpertz has also called attention to hypnosis as an aid to diagnosis. He distinguishes two ways in which hypnosis may be employed for this purpose. In the first place, the result obtained by suggestion is of importance, as in the cases already mentioned. Gumpertz thinks that in a case in which the diagnosis between paralysis agitans and a traumatic hysterical affection was not clear, he was justified in excluding paralysis agitans, because suggestion acted beneficially on the tremors. He also thinks he was right in describing a case of paralysis of the obliquus enternus as hysterical paresis, because of the beneficial influence of suggestion on the double vision. The third case was that of an elderly maiden lady suffering from contractures of the muscles of the hands and feet; a single hypnotic sitting sufficed to remove the contractures, and it was shown both by the anamnesis and the influence of hypnotic suggestion that a subsequent attack of aphasia might well be put down to hysteria. Secondly, Gumpertz considers that, besides the results produced by suggestion, the course of a hypnosis may be used for diagnostic purposes, because deep somnambulism with hallucinations makes one suspect hysteria, just as a brief period of spontaneous oblivion after waking does. Hirschlaff also, who holds that somnambulic hypnosis occurs in some forms of chronic intoxication of the nervous system, especially alcoholism and morphinism, thinks we may use hypnosis as an aid to diagnosis on this assumption.

I do not deny that hypnosis can be used as an aid to diagnosis, but I hold the opinion that this should only be done with the greatest reserve. First of all, as far as the opinion of Gumpertz and Hirschlaff, who use deep hypnotizability in the diagnosis of hysteria, is concerned, I think we must be very cautious in this connection. I must here refer the reader to what I said on p. 303 *et seq.*, concerning the meaning that attaches to "hysteria." But we must in general be very careful about using hypnosis for diagnostic purposes. Take the case of a person suffering from severe pain in which the diagnosis lies between a tumour and a hysterical pain. It is easy to assume that a differential diagnosis can be made because suggestion removes functional, but not organic pain. But from the practical point of view there are many difficulties in this respect. I do not lay any weight on the fact that there is very often a combination of functional and organic pain; but what follows is well worth considering. There are

functional pains that cannot be removed by suggestion; therefore the persistence of pain might be no proof that the disease is organic. In addition to this the pain caused by organic disease can, as we have seen, be removed by suggestion, at least for a time. Seeing that a degree of analgesia sufficient to render the prick of a needle or some otherwise painful operation painless may be induced by suggestion, it follows that the beneficial influence of suggestion does not exclude the organic nature of a pain. At any rate, when a pain disappears for a considerable length of time through the influence of suggestion we may assume that very probably the disease is not of an organic nature; but it is only to this limited extent that we can consider hypnosis an aid to diagnosis. All that I have here said in respect to pain applies equally to other organic and functional symptoms—*e.g.*, paralyses. I must here refer the reader to Bernheim's remarks on the influence of suggestion on organic diseases, and may add that I quite agree with Gorodickze, who long ago protested against any exaggeration of the diagnostic importance of hypnosis.

I now come to the use of hypnotism in surgery. Hypnotizing for the painless performance of operations is not new; one inventive genius even imagines that God took the rib from Adam while he was in a hypnotic sleep, since he would certainly have waked had it been a natural one. In the days of animal magnetism surgical operations were often performed in the magnetic sleep; for instance, by Récamier in 1821. Such operations were also performed in several of the Paris hospitals under the direction of the Baron du Potet. Cloquet used it in 1829. He related his experiences to the French Academy of Medicine, but Lisfranc, the celebrated surgeon, put him down for an impostor or a dupe. Oudet was no better received in 1837, when he told the Academy of the extraction of teeth in the magnetic sleep. In 1840 Esdaile performed a number of operations during mesmerically induced sleep in the Mesmeric Hospital at Calcutta. The wounds are said to have healed very quickly. Hellwald, also, has recently drawn attention to the quick healing of the wounds of the Arab pilgrims, which are made in the hypnotic state. In 1852, Vogler, although very sceptical as to animal magnetism, made Esdaile's results known in Germany. Elliotson at the same

time was using mesmerism in surgery, in London. Braid, who was much struck by Esdaile's results, also used hypnosis in surgery. At that time the opinion was expressed—it has some adherents even now—that mesmeric passes induce analgesia better than Braid's method. Azam brought Braid's method of analgesia to Paris, as we saw on p. 16; from thence it passed to Germany, but found little support. Nussbaum, who had studied animal magnetism in Paris, thought that Germans were not suitable subjects either for magnetism or Braid's methods. None of his attempts to substitute hypnosis for chloroform met with success. Preyer says that military doctors and others appear often to have used empirical hypnotizing methods to induce analgesia for small operations, such as tooth-drawing. Bonwill observed that after a succession of deep respirations a brief anæsthesia appeared; this was confirmed by Hewson. Possibly this is an auto-hypnotic condition, or an auto-suggestive anæsthesia. Recently Forel, Voigt, Tillaux, le Fort, Grossmann, Bourdon, Howard, Wood, Toll, Schmeltz and Starck have used hypnotic analgesia in surgical practice. In 1890 Haab used hypnosis in the operation for cataract; extraction of the lens was preceded by iridectomy; on both occasions the patient was hypnotized and anæsthesia produced by suggestion, and he smoked an imaginary pipe as the iris was cut through. Lauphear, Aldrich, and others have even used hypnosis for amputations, and Hulst for gastrotomy.

Some years ago I once hypnotized a patient in order to open a boil painlessly. I did not succeed in inducing analgesia, but the patient was almost unable to move, so that I could perform the little operation with ease.

Analgesia has also been induced by post-hypnotic suggestion and an operation performed without difficulty in the waking state (Boursier).

It has been specially for the purpose of extracting teeth that hypnotic suggestion has been so frequently used (Bramwell, Andrieu, Hivert, Klemich, Sandberg, Moiroud). Glogau has even recommended its general use in dentistry. On the question of its practical importance the same remarks would apply as in the case of the use of hypnosis in general surgery.

Besides this, I cannot approve of dentists who have neither general medical training nor experience being allowed to hypnotize their patients. There are cases in which hypnosis is contra-indicated, but that is a point

that can only be decided by a properly trained medical man. Just as a doctor is not in a position to treat every kind of dental trouble, so, too, a dentist is not fitted by his training to determine when hypnosis is contra-indicated or to carry out the process of hypnotizing in a technically correct manner. We may take it as quite certain that the ill-effects said to be brought about by hypnosis are really due to incompetence. When a dentist, Beisswenger, tells us that a highly intellectual and talented young man whom he had frequently hypnotized subsequently developed an aversion to work, showed symptoms of persecutory mania, and formed a determination to murder him (Beisswenger), then we may declare straightway that either the patient was a psychopathic individual, or that Beisswanger, in spite of his asseverations to the contrary, did not go to work in a proper way. In any case we can only advise Beisswenger very strongly never again to undertake a method of treatment that he does not understand. Dentists in the present day complain often enough about quackery; they should strenuously oppose those of their colleges who meddle with matters with which they have as little right to deal as a quack has with dentistry.

The value of hypnosis in obstetrics is about the same as in surgery. Lafontaine and Fillassier, among the mesmerists, have magnetized women during labour. As Freyer reports, Jörg certainly thought birth impossible in the magnetic sleep without a quick awakening. Liébeault has also used his method of hypnotizing in obstetrics. A series of such cases has lately been published (Pritzl, Mesnet, Secheyron, Auvard, Thomas, Varnier, Voigt, de Jong, Schrenck-Notzing, Tatzel, Grandchamps, Luys, Cajal, le Menant des Chesnais). A case in which Delbœuf and Fraipont hypnotized a woman in labour belongs here. The results were not unfavourable. The pains were regular and strong, and could often be made almost insensible by suggestion. Khovrine used post-hypnotic suggestion to prevent attacks of hystero-epilepsy in the case of a woman in labour. Directly the pains started, an attack threatened but was prevented in the manner mentioned. Indeed, towards the termination of the labour the patient lost consciousness at each pain, but only for a few moments, and just before the birth terminated pain was again experienced.

The value of hypnosis for producing analgesia must not be exaggerated. Sometimes analgesia cannot be induced at all, and sometimes it is only possible to obtain complete analgesia after repeated trials. The excitement before the operation increases the difficulty. The cases in which hypnosis can be used to make a severe surgical operation painless are very rare; the care with which every such case is registered by the daily

press shows this. Besides, we have at present many means of inducing analgesia that are much surer in their action than hypnosis. In addition to ether, chloroform, and the like, there is Schleich's method of local anæsthesia; further, the modern method of spinal anæsthesia by injection. When by chance a person who is to undergo operation is found to be very susceptible, there is no reason why hypnotism should not be used. Hack Tuke and Forel think that hypnosis should be used instead of chloroform in all cases where that anæsthetic would be particularly dangerous. Forel believes, besides, that analgesia, sufficient for operative purposes, is more easily induced than I suppose. I certainly think it possible that a clever hypnotist may obtain better results in this direction than I have been able to do.

CHAPTER IX.

THE MEDICAL ASPECTS OF HYPNOTISM (*continued*).

1. *Theoretical Considerations.*—In the foregoing chapter I have discussed the medical importance of hypnosis, but only as far as its practical application is concerned. But the medical aspect of hypnosis is not thereby exhausted. Hypnosis has proved of far greater value to medical science, and its indirect assistance to therapeutics has been more valuable than anything produced by its more practical employment. As far as the latter point is concerned, hypnosis has led to the development of a new branch of the healing art, psycho-therapeutics, which I shall discuss in the second section of this chapter.

Hypnosis has also proved of importance in medical research. It has thrown light on a source of error in judging the *modus operandi* of therapeutic measures, which, although not unknown, had been very much underrated. The good results obtained by the use of certain remedies used to be erroneously ascribed to chemical or physical action, whereas it was really suggestion that produced the results. This applies to innumerable therapeutic measures, quite as much to physical methods as to the products of chemical factories. It has also often happened that while one doctor has seen nothing but failure with some drug, another has imagined that it produced brilliant results. As there could be no question of bad faith in such cases, the contradictions they presented necessarily appeared enigmatical until suggestion supplied the key to their solution. We must also bear in mind that a patient's confidence in the advice given him by his doctor very often suffices to bring about the good result, and sets the suggestive action going. If the same remedy acts differently when administered by different doctors, we are justified in asking the question, to what extent was the difference in its action due to psycho-therapeutic influence? We have to consider that the doctor's conviction of the efficacy

of a drug is transferred to the patient. A doctor does not only employ suggestion consciously, but often without either knowing or suspecting that he is doing so. Let us take an example of recent date. Some investigators—Joh. Fred. Fischer, for instance—have asserted that in numerous cases *enuresis nocturna* in children is brought about by adenoid vegetations. Fischer, indeed, thinks that in the vast majority of cases in which these two symptoms are found to occur together the children cease to wet the bed as soon as the adenoid vegetations are removed. Another doctor—Viktor Lange—disputes the existence of this connection, because in his experience adenoid vegetations had the opposite effect. But when we consider that mental influence plays a most important part in suppressing *enuresis nocturna*, and that the doctor's confidence in the method he employs is transmitted to the patient, the *raison d'être* of the different results obtained by the operation is at once evident. We are justified in applying the same criticism to many other methods which are said to cure *enuresis nocturna*. Maximilian Hirsch asserts that he has obtained brilliant results with Chatelin's epidural injections; but this is contested by Götzl on the ground of his own contrary experience. When we see that a number of other doctors who have busied themselves with hypnosis—Liébeault, Ringier, Hackländer, for instance—obtain just as good results with hypnotic suggestion as Joh. Fred. Fischer, Maximilian Hirsch, and others do with their methods, may we not take it that suggestion is the common basis of all these methods?

Many also of the opponents of hypnotism have often underrated the suggestive moment in their remedies in an illogical manner, and have thereby proved that they would have done better to study hypnotism than to oppose it. To pick out only one of these opponents, I mention Mendel, who attempted to introduce the suspension-treatment of *tabes dorsalis* into Germany. (The attempt is nowadays only of historical interest.) If Mendel had studied the question of suggestion in connection with suspension, there would have been no epidemic of suspension-treatment in Berlin. I may remark, by the way, that as far as my experience goes yohimbin, which was so warmly recommended by Mendel in the treatment of impotence, has no other importance than that derived from the suggestion that accompanies its exhibition. At least, among all the cases that I have treated with yohimbin I have been unable to find

a single one in which the effect produced could be traced to the somatic influence of the drug with any degree of probability. Other opponents of hypnotism, too, who have recommended chemical remedies, have made very similar mistakes because they were unable to form an adequate estimate of the action of suggestion.

We shall, under certain circumstances, be able to avoid falling into any grave error as to new remedies and their action, provided we recognize the significance of suggestion. How comes it that so many remedies are not only widely advertised but even enthusiastically recommended by some doctors, remedies that so soon prove to be useless? How many remedies have been proclaimed hypnotics, how many appetisers, only to be forgotten immediately? And on what does their transient success depend? On suggestion alone, often enough. When a doctor is convinced of the narcotic action of a certain drug, that conviction is readily transmitted to the patient who is under his suggestive influence, and there is therefore no cause for surprise when the remedy does produce sleep. That is why Rosenbach has given a proper way of testing new hypnotics. It is impossible to determine the value of a hypnotic remedy scientifically, when the patient knows he is taking such a remedy. Hypnotism has distinctly proved that. And it is just the same with a number of other remedies, such as purgatives, astringents, anodynes, etc., etc. All these substances can only be tested as to their true somatic action when every form of suggestive action is scrupulously excluded, and the first requisite in this respect is that the patient should know nothing about the expected effect. The significance of electro-therapeutics has frequently been discussed from this point of view. Möbius has traced many electro-therapeutic effects to suggestion, and, in an exhaustive work on the question, I have expressed the view that, in very many cases at least, the action is mental; in other cases I concluded that the improvement or cure was spontaneous. This view has found both opponents and followers. Whereas Delprat came to the conclusion, on statistical grounds, that electricity made no difference, the cure being no more rapid, other observers have not relinquished the physical influence of electricity in electro-therapeutics. Among such observers are Eulenberg, Sperling, Lœwenfeld, Müller, Laquer, Remak, Wichmann. Eulenburg nevertheless admitted that in a great number of cases the

action was of an essentially mental nature, but that at the same time we must not deny that there is often an action independent of suggestion.

It is very interesting to observe how often the would-be discoverer of some particular method thinks he can put aside the possibility of suggestive influence without producing even a trace of proof for his assumption. Thus Fleury, on the ground of theoretical considerations, regards the infusion of salt as the specific agent in the treatment of neurasthenia, and thinks that in so doing he excludes the influence of suggestion. When Nägeli, who treats neuralgia and neurosis by a process of manipulation, concluded a lecture with the words "suggestion is excluded," Forel very properly pointed out how little is proved by such assertions. In fact, any experimenter of experience in the domain of suggestion must often be astonished at the unthinking manner in which suggestion is assumed to be excluded. As Forel has rightly pointed out, Brown-Séquard's testicular injections must be considered from the same point of view. Massolongo regards their operation as purely suggestive, and Martinet is convinced, on the ground of numerous experiments, that it is a matter of indifference whether testicular fluid or distilled water is injected. Save the peripheral stimulus imparted by the injection, there is no other essential moment worth minding but the operation of suggestion. Of course this is no reason for denying animal substances all somato-therapeutic action. I would only point out that the results obtained by these and other similar therapeutic measures can be completely explained by suggestion.

At all events, there are numerous cases in which this explanation is much more satisfactory than any of the complicated and pseudo-scientific attempts at explanation that are so frequently made. A striking example of this fact is supplied by balneo-therapeutics. Formerly the efficacy of such treatment was ascribed to salts in the springs visited, and the enormous influence which leaving home and all business troubles must have on a patient who visits a watering-place was ignored. But in order to attribute a thoroughly specific action to springs, special value has recently been laid on radio-active substances. And yet an unbiassed investigator will often find a perfectly adequate explanation in spontaneous improvement, and in numerous other cases in the influence of suggestion—an explanation which makes him feel those

"scientific" interpretations mere triflings which remind one of the wood that cannot be seen for the trees.

Even in the case of operations we must always be on the watch for mental influence. I call to mind the discussion that ensued on the introduction of castration for hysteria and mental affections, and I cannot do better than quote the excellent remarks made by Julius Friedemann in his thesis for the degree of Doctor of Medicine:—"Only to think of the number of women who have, in reality, been mutilated since clitoridectomy was introduced as a cure for neuroses and psychoses! But neither in hysteria, nor in epilepsy, nor in onanism, nor in any psychosis connected with sexual processes will any good result from removing a healthy clitoris. It is now said that the *arcanum* has been discovered in the ovaries which are to blame for all the lamentable symptoms which characterize those diseases." Even at that period Friedmann, Israel, and Landau opposed the views held by Hegar and Tauffer, and rightly drew attention to the mental influence of such operations. Unfortunately, a general appreciation of mental influence was impossible in those days; but time has wrought a change in that respect.

Of course, there are many questions in this connection still unsolved, and even now we are at times unable to decide whether some particular therapeutic action should be ascribed to suggestion or to somatic influence. But undoubted proof of the exclusion of suggestion should always be given when its influence is denied in any particular case. To assert apodictically "suggestion is excluded" does not furnish such proof.

On the other hand, we must beware of tracing everything to suggestion after the manner of people who delight in catch-words. Since the influence of suggestion was underrated at first, it easily came to be occasionally exaggerated later on; for example, the chemical and physical action of remedies was denied, and results were traced to suggestion. How far we may be led astray in this manner is shown by the reception which was given to Schleich's method of inducing local analgesia. The fact that local analgesia could be induced by suggestion—Barth, for instance, had induced a sufficient degree of anæsthesia for minor operations on the throat by persuading the patient that the solution of common salt with which his throat was painted was cocaine and therefore rendered the mucous membrane insensitive—led to the

erroneous conclusion that Schleich's method was suggestive, though Schleich had shown that the injection of water produced such swelling of the tissues that a surgical operation might be painlessly performed. In the case of internal remedies, also, the action of suggestion has occasionally been assumed in the wrong place, not only in respect to chemical or physical action, but where the action of a drug has been simulated by a spontaneous cure or spontaneous improvement. I have already discussed the latter point in detail on page 297. Here I will only call attention to a point that shows how careful we should be in the use of catchwords *à la* suggestion. In recent times homœopathy has come to be looked upon as a form of suggestive therapeutics; but when we consider that homœopathy also plays a great part in veterinary medicine, and that it is impossible to understand how an animal so very low down in the intellectual scale as a horse should be influenced by suggestion, it follows that there are other sources of error to be considered in the case of homœopathy. I think that both in veterinary and medical practice many a really spontaneous cure is put down to homœopathic treatment. Of course, such real exponents of homœopathy as Roth, Sperling, Lorbacher, Pfander, and Julius Fuchs distinguish between the efficacy of homœopathy and suggestion and spontaneous restoration to health. Karl Gerster, a physician intimately acquainted with homœopathy, gave a scientific demonstration in a discussion he had with the homœopaths that homœopathy requires revision from the standpoint of suggestion.

Hypnotism has also enriched our knowledge by enabling us to understand the pathogenesis of certain diseases. I refer here to the importance of auto-suggestion as a factor in the genesis of disease. Charcot already had admitted that paralyses of traumatic origin often depend on the patient's auto-suggestion that some limb could not be moved. In recent years there has been considerable discussion as to the extent to which auto-suggestion may be responsible for other nervous phenomena of traumatic origin. It was recognized that when the victim of an accident is convinced that his injuries are bound to bring on an illness, some symptom or other of disease will appear. Krafft-Ebing and Wichmann are among those who take this view, and even if others—Meynert, for instance—have contested the importance of auto-suggestion, the number of investigators who ascribe essential importance to

this psychological factor in the production of the neuroses which follow accidents is permanently on the increase. Hypnotism has also contributed indirectly to our knowledge of the pathogenesis of traumatic neuroses by placing the general importance of mental influence in a right light. Thus, it has been pointed out that the quarrels and litigation nowadays forced on a person who has been hurt in an accident often prove more injurious than the accident itself. Finally, many people are unable to fight against possible pathological *sequelæ*, because they hope to derive a permanent income from the accident. That these psychological moments play a chief part in the causation of traumatic neuroses had long been recognized by Strümpell, especially as regards the wish to acquire an annuity; later on, Jolly, F. C. Müller, Lauenstein, O. Binswanger, F. Schultze, Mittelhäuser, and others expressed the same view. It is particularly in Germany that this question plays so great a part, on account of the Law of Compulsory Insurance against Accident. The cruel fact of being driven to work often enough restores a man to health when he has been injured in an accident, but the possibility of obtaining an annuity without working for it diminishes the desire for work and the capability of carrying it out.

But hypnotism has thrown more light on hysteria than it has on neuroses of traumatic origin. I have already mentioned (p. 303) that Möbius considers all those bodily pathological changes hysterical that are caused by ideas. In recent times other authors also have attempted more and more to put hysteria on a psychological basis. When one reads the works of authors like Möbius, Charcot, Hellpach, Eulenburg, Crocq, and others, it is not difficult to understand how the investigation of hysteria has been influenced by hypnotism. Many of the authors who have recently studied this branch of the subject have put forward suggestibility as a common characteristic of hysteria and hypnosis, which is generally taken to mean that hysteria is characterized by auto-suggestibility, and hypnosis by external suggestibility. Of course it must not be assumed that the exclusively psychological view of hysteria is the only correct one; that is a point which cannot as yet be decided. But there is one point on which we may rest assured, and that is, that psychological factors play a great part in these diseases, and that their recognition will be essentially advanced by the study of hypnotism.

Of course, this question possesses only a theoretical significance. When a trouble that is considered to be hysterical or neurasthenical is caused by some malignant psychic influence, we know how to combat it prophylactically and therapeutically. Daily experience teaches us that an uncongenial environment will cause or aggravate a disease. There are few people who are not affected by being constantly told on all sides that they look ill, and I believe that just as much injury is done by an accumulative mental process of this kind as by chemical poisons. Suggestion produces suffering, just in the same way that it cures it. Doctors who are incapable of understanding mental influence aright may easily cause unpleasant results by making thoughtless remarks. Forel mentions a case in which a patient suffered from headache for years, because it was, as he believed, said of him when suffering from inflammation of the lungs accompanied by headache that this would never pass away. Whether or not the patient misunderstood the doctor is immaterial—the working of suggestion appears here. Forel succeeded by hypnosis and counter-suggestion in rapidly removing the headache. Lœwenfeld mentions similar cases; for example, one in which a patient believed he was suffering from a severe heart trouble, because of a thoughtless remark made by his doctor. Brügelmann shows that many attacks of asthma are caused by the patient's belief that he cannot breathe; the patient awaits with anxiety the moment for the attack to appear, and this anxiety brings on the attack. A powerful diversion of the attention may sometimes suffice to diminish the intensity of the attack.

There are many cases in which it is uncertain whether the action of a remedy is suggestive or somatic, and it is the same with the pathogenesis of certain diseases. Some doctors favour a suggestive origin, others a somatic. For some years past attention has been drawn to the connection between affections of the nose and certain phenomena presented by the womb. Some say that nasal treatment influences menstruation by suggestion, and others oppose this view. Even if we assume that the action here takes place by the way of natural reflex paths, and that the process should be distinguished from suggestion, the possibility still remains that suggestion has a great influence.

Hypnotism has also thrown light on the nature of many

idiosyncrasies, for such at times arise from auto-suggestion. There are people whom coffee does not constipate but purges, and patients on whom drugs and other remedies have the contrary effect to that expected. A lady who was given bromide as a sedative, remarked: "Strange, I've had that before and it only excited me." Morphia only increased pain in her case. Many of these cases are the result of auto-suggestion, and hypnotism in giving us the key to a large number of idiosyncrasies has at the same time taught us many an important lesson. But we must, of course, refrain from ascribing every idiosyncrasy to auto-suggestion, because idiosyncrasies may depend on purely somatic conditions. Many people suffer from urticaria after eating crab; but they also have urticaria when they do not know that they have had any crab. Naturally, we cannot, even in the present day, invariably decide whether the idiosyncrasy is of mental or physical origin.

Although there are many points on which we must still remain in doubt, hypnotism has put a check on exaggerated notions of morbid anatomy as a cause of disease and given freer scope to the neglected study of diseases of functional and mental origin. We must certainly take up the position that there is no disease and no subjective trouble without an anatomical substratum. But we must not forget that the latter is in many cases so fine and so unstable, that not only are we unable to detect it *post-mortem*, even with the help of the microscope, but also it is impossible for us to form even the slightest notion of the nature of the abnormality without getting lost in a maze of fruitless hypotheses. We must bear this firmly in mind if we wish to understand what hypnosis has done for us. It has taught us to diagnose functional disease in cases where there was formerly a tendency to assume the presence of stable morbid anatomical changes. We now know that, where formerly organic disease of the heart or stomach was suspected, such disturbances are often enough of a functional nature, and very frequently of mental origin. Hypnotism has therefore greatly improved diagnostics.

We have seen what a source of error the action of suggestion has proved in medical investigations. This fact brings us to the importance hypnotism has acquired in the study of the history of medicine. How are we to explain the fact that so few of the remedies that played so great a part in old books on

medicine are retained in the modern medicine-chest? We need not go back to Hippocrates and Galen, but merely take up a medical book of the beginning of the nineteenth century to note how the remedies of former times interest us to-day as curiosities. Ought we to assume that remedies which were formerly effective have nowadays lost their power? or that diseases have undergone a change? Is it not much more probable that our colleagues of former times made the same errors of observation that we are still apt to make? There are two sources of error that recur at all times; the one consists in ignoring the spontaneous cure of disease and leads to an exaggerated value being placed on remedies, the other in overlooking the action of suggestion and ascribing the good results obtained to chemical and physical action. In many cases the novelty of a drug or treatment provides a stimulus that increases the action of suggestion. There are certain diseases whose symptoms recur from time to time—hysterical headaches and paralyses, for instance—in which a remedy that removed the morbid symptom the first time it was applied proved a failure when that symptom recurred. The stimulus of novelty, which can only occur when the remedy is changed, was wanting. There are many cases affecting both sexes in which this may be observed, and not merely in those in which we are accustomed to assume hysteria. That which here applies to the individual holds good for medicine in general, because anything to which we are unaccustomed makes a greater effect than that with which we are familiar. This explains how many a remedy soon becomes forgotten because it has lost its suggestive power. At one time women were castrated for hysteria, and in many cases obtained temporary relief; at another, metallo-therapeutics was in great favour; then this or that drug had its day; at one time nerve-stretching was recommended for *tabes dorsalis*, later on treatment by suspension. But for all such cases the suggestive action that attaches to a novelty provides an ample explanation. We can thus understand that a drug comes into vogue when it conveys suggestion. But it also happens that a remedy that has long been forgotten will later on regain its old efficacy, provided it again conveys the impression of novelty. This also enables us to understand how many a remedy that has long disappeared from the medicine-chest again comes to be recommended. It operates like a new drug because its existence was forgotten.

If we turn to the history of medicine we find, to mention only one example, that many of the marvels nowadays attributed to organo-therapy had already been observed long ago when organo-therapy was employed in a much simpler manner and animal substances were used as remedies without the intervention of the chemical factory. I need only mention the renown that of old attached to the vaginal discharge of a mare that was horseing as an aphrodisiac, concerning which Hugo Magnus gives many instructive examples in his work, *Die Volksmedizin*.

I have spoken of the stimulus of novelty. To avoid any misunderstanding, I must make a few additional remarks on this question. When it is not clear whether the action of a remedy is mental or physical, it is sometimes assumed that mental and suggestive action should be considered excluded when the action on the same individual is invariably the same. This cannot be explained by suggestion. It may possibly be assumed by some that a remedy which acts mentally very soon disappears from the medicine-chest, and that this gives us a kind of test by which we may decide whether the influence is mental or physical. Such a view is, however, wrong. Even if the stimulus of novelty is essentially a mental moment, it is not the only one in question. There may be cases in which the use of a drug is so closely connected with its suggestive action that the latter becomes a constant phenomenon of its exhibition. But the same phenomenon is observable in the psychology of crowds—*i.e.*, the phenomenon presented by the crowd is analogous to that presented by the individual. A remedy may, therefore, under certain circumstances retain its efficacy, and that, too, without any fresh stimulus of novelty, for a long time, not merely for years but for decades and centuries as well. We can see this from the length of time the temples of Æsculapius flourished, and also from the fact that even in the present-day shrines possessing miraculous springs are visited by pilgrims year after year for decades.

Hypnotism has, moreover, become of great importance in the critical examination of charlatanism, and here again because it has taught us the true significance of suggestion. People were formerly only too fond of calling every case of improvement reported to be due to the manipulations of a charlatan, a swindle; at the most it was occasionally admitted that a charlatan might possibly produce an effect on hysteria

by mental means. Hypnotism has shown us that in other cases besides those ordinarily termed hysterical, symptomatic improvement may be obtained by mental action, and that this occurs even when progressive organic diseases are concerned. Recognition of this fact has thrown a new light on charlatanism. But it is particularly on the question of curative magnetism, a branch of charlatanism, that hypnotism has enlightened us. By presenting the scientific doctor with psycho-therapeutics, and thereby rendering it possible for him to be successful in cases in which patients formerly had recourse to the charlatan, hypnotism has made the fight against quackery easier. This fact must not be underestimated. First and foremost, hypnotism has given what O. Binswenger calls psychological guidance to medical thought. There was formerly far too great a tendency to search for a material focus of disease for every disorder, and when that was not found to put the trouble down as unimportant or exaggerated or even to lying. And the patient who knows his doctor doubts him feels injured by his mistrust, "he turns his back on him and seeks help elsewhere, only too often to become the prey of unscrupulous adventurers who make the most they can out of the weakness and helplessness of the sick and their need of practical assistance." The more doctors learn to think psychologically the less likely will they be to fall into this error, and they will thereby be rendering a great service to their patients. For although the mental influence of a charlatan may sometimes benefit a patient, that is nothing compared to the dangers of quackery. The possibility of a person's health being improved by psycho-therapeutic means cannot make up for neglect of the diagnosis (Lobedank) and omission of the necessary physical methods of treatment; and this is what almost invariably happens where charlatanism is concerned.

Hypnotism has also shed light on many a superstition in the same way that it has on quackery. When we find that in some parts of Germany the superstition obtains that insomnia can be prevented by placing one's shoes with the toes towards the bed, or by leaving two pots turned upside-down on the table (Wuttke), we can well conceive that this may act just in the same way as suggestion does. The study of the action of suggestion has also thrown fresh light on medicine as practised by uncivilized peoples. It enables us to understand the effect produced by the medicine-man, and the study of hypnotism

has become of importance to the student of ethnological psychology, as Bastian, Stoll, and other ethnologists long ago surmised it would.

Those miraculous cures which so often occur without the intervention of a scientifically trained doctor or a professional charlatan belong here, such, for instance, as the cures observed at Lourdes and similar places. Medical men usually object that such cures only affect hysteria, and it must be admitted that the vast majority of the cases that terminate successfully are hysterical, but not all. When we read reports from Lourdes impartially, such, for instance, as those contained in Boissarie's book, *Great Cures at Lourdes*, we find a large number of cases that we have no right to put down to hysteria. That book recounts miraculous cures at Lourdes of lung troubles, of tuberculous inflammation of the joints, of wounds, of gastric ulcers, of eye-diseases, of cases of deafness and deaf-mutism, as well as of all kinds of paralyses, internal inflammatory processes and nervous diseases. In going through these cases I have become more and more convinced that many were real instances of severe organic disease in which the patient's condition was improved by psycho-therapeutics. I must here refer the reader to the remarks I made on this question on page 314 *et seq.* Believers in Lourdes certainly assert that otherwise incurable diseases, such as cancer, are cured there; but what I have read of the medical history of such cases has given me no scientific ground for accepting the diagnosis as accurate. On the contrary, the superficial manner in which the diagnosis is often arrived at is very striking to the critical reader. Charcot admitted that organic lesions had been cured at Lourdes, but he thought that this was only so in the case of organic changes of hysterical origin, the cure of which by psychic means can be explained. He mentioned, among others, Fowler, who reduced tumours of the breast by mental influence, but he considered such tumours merely trophical disturbances of hysterical origin. The supposed miracles of Lourdes can be easily understood without giving so wide a meaning to hysteria as Charcot did. In many cases it will be found that the remarks I made on quack-diagnosis on page 319 hold good.

When we take the foregoing considerations into account, hypnotism acquires great significance in its bearing on the history of medical culture. For there have at all times been

just such miraculous places as Lourdes is to-day. Ancient medicine, which was so much in the hands of the priests, is full of records of miraculous mental influence of this nature. The temple sleep of the old Greeks and Egyptians had much in common with hypnotism. In Greece it was in the temple of Æsculapius that the sick lay down to sleep and were told in their dreams by the god of the remedy that would cure them. It is easy to understand the auto-suggestive influence of this temple sleep. And at other times certain human beings have had the same renown for curing the sick as was here ascribed to the Deity. I may here mention the well-known Greatrakes,[1] whose cures astonished all England in the seventeenth century. He healed by laying on of hands, but seems also to have used verbal suggestion. I may mention, also, Gassner, the celebrated exorcist, at the end of the last century. The reports make it clear that Gassner used suggestion, for though he spoke Latin it is evident he made his patients understand him; nobody misunderstood his famous "Cesset"; they knew that the malady or the pain was to stop. I find in Sierke that Gassner occasionally sent a patient to sleep by command. He told her to walk when she was asleep, when to wake up, and in fact produced what we should at present call a regular hypnosis. Lichtenberg reports that during the eighties and nineties of the eighteenth century a certain Frau Starke in Osterode created some excitement by performing cures through stroking and touching the patient's body and by so-called charming. I may further mention Prince Hohenlohe, a Catholic priest, who aroused much attention by his cures in Bavaria, after 1821. The mesmerists reckoned him a magnetizer, but others ascribed his cures to religious faith. One school of mesmerists, that of Barbarin of Ostend, took up a peculiar middle position. Barbarin maintained that the influence was purely a spiritual one, and that the right way to induce sleep was to pray at the patient's bedside (Perty). Many modern adherents of magnetism hold the same views; Timmler, for instance, thinks religious faith valuable and necessary for obtaining the result. This tendency of animal magnetism brings us to one of the recent forms of epidemic mental disorder that hail from America, viz.—faith-healing or

[1] Lichtenberg writes the name "Greatraks," as does his authority Robert Boyle. Others write it "Greatrix."

Christian Science. A Mrs. Eddy passes as the foundress of this creed. The male, or female, patient is told to sit in a chair and think that his, or her, illness is due to sin, that God is averse to sin and will heal those who believe in Him. The faith-healer sits on another chair, and is supposed to concentrate her mind on similar thoughts. Faith-healing is also used in distant treatment. There are many analogies for the latter. Weil mentions the case of a doctor who lived in a large town in Saxony, and who cured many patients by strictly ordering them to go to bed at a certain hour, at the same time telling them that they would perspire profusely and that this would cure them.

Numerous other cases that belong here could be mentioned, and they would show that many a phenomenon observed in the domain of medicine has first had true light thrown on it by hypnotism. With every one of the workers of miracles of whom we hear from time to time—Pastor Kneipp's is a recent case—mental action plays an extraordinarily great part. Science, doctors, and the sick have long enough suffered from the mental factors in disease being underrated.

2. *Psycho-therapeutics.*—I have shown in the foregoing that, apart from the practical uses to which it may be put, hypnotism has become of importance to medicine, inasmuch as it has shed light on many branches of theoretical medicine. But this does not exhaust its importance in medicine. It has, on the contrary, acquired an almost fundamental significance in a certain direction by bringing into prominence a new branch of the healing art—viz., psycho-therapeutics, and although this branch has not yet attained full development its progress has been so great that its extraordinary importance is recognized. In this respect hypnosis has become of much greater importance to medical practice than its direct application would justify. We must carefully distinguish between psycho-therapeutics and hypnotic treatment, for the latter is but a small part of the former. But hypnotism has given us the key to psycho-therapeutics by showing us how powerfully mental influences may operate on human beings. Appreciable light was first shed on the importance of mental influence by hypnotic experiments; for until susceptibility to such influence had been demonstrated in the case of hypnotic suggestion, it was not understood that many forms of suggestion prove effective even without hypnosis.

General suggestive therapeutics was thus evolved from the method of treatment by means of hypnotic suggestion. But it was gradually recognized that so far from suggestion exhausting the possibilities of psycho-therapeutic influence there are other mental remedies to be considered. The psycho-therapeutics of to-day is a development of suggestive therapeutics just as the latter is of hypnotism.

It may, perhaps, be here objected that able practitioners employed many and various forms of mental influence long before modern hypnotism was known, and that the latter is therefore of no such great importance in psycho-therapeutics. But to this we may reply that modern psycho-therapeutics, which is based on hypnotism, has made us acquainted with a whole series of mental influences of which even capable practitioners of former days had hardly any knowledge. Apart from this, modern psycho-therapeutics would not have the employment of mental influence confined to a few specialists, but is more concerned to see it made the common property of all practitioners. Finally, it must be added, many practitioners who formerly used psycho-therapeutic methods did not really understand what they were employing. They assumed a chemical or physical action in the case of many remedies where the cure was really due to mental influence. Other cases we read of belong more to the domain of the marvellous, as, for instance, that in which a doctor used a thermometer and the patient very soon declared himself cured, because he believed the thermometer was the remedy. At all events there is a considerable difference between the occasional use of a mental remedy and the scientific investigation of psycho-therapeutics.

Nevertheless, we must admit that theoretically, at least, psycho-therapeutics was by no means entirely ignored in the past. There are intimations of it in the works of Hippocrates, Celsus and Galen. We find advice of a psycho-therapeutic nature in Seneca, and as a general rule in the Stoics; also in the writings of many other philosophers of antiquity. The teaching of the old Stoics that physical ailments should be combated by the soul, in particular, presents ideas that connect it with psycho-therapeutics. Later philosophers, also, of whom I may mention Descartes—I shall come back to Kant later on—recognized the importance of mental processes in the cure of disease; so also many theological authors. Similarly, the

Arabian and Jewish doctors of the Middle Ages, and also the school of Salerno, often took psycho-therapeutics into account in their works. Coming to later times, I may mention in addition to Boerhaave in the beginning of the eighteenth century, Alberti, who taught forensic medicine in Halle. In several dissertations for the doctor's degree, which emanated from his school, and which were for the most part written by himself, divers branches of psycho-therapeutics are discussed. I may mention Papai's dissertation *De Therapia morbum morali*, which appeared in 1714; and also Süssenbach's *De Therapia imaginaria*, and Moosdorf's *De Valetudinariis imaginariis*, both of which were published in 1721. Several works of a similar nature also appeared about that time, written by Hilscher of Jena; for instance, *De Strategematibus medicis*, 1738, and *De Animi Laboribus, egregio sanitatis praeside*, 1742. Mention must also be made of a dissertation by Lemmer of Langguth's School at Wittenberg, *De Animo sanitatis praeside atque custode optimo*, 1758; likewise of a work by Gaub, *De Reginime mentis quod medicorum est*, 1763. In spite of the growth of natural philosophy, the end of the eighteenth century produced many investigators in the domain of empirical psychology. Both medical men and philosophers did much at that time to promote psycho-therapeutic investigations. I may mention C. W. Hufeland and Kant. The latter in his well-known work, *Von der Macht des Gemüths, etc.*, which appeared at the end of the eighteenth century, showed that he was not merely a speculative philosopher. The *Magazin zur Erfahrungsseelenkunde*, which was published by Karl Philipp Moritz, is a treasury of information on individual psycho-therapeutic observations. The psychic treatment of mental diseases, which began to be recommended towards the end of the eighteenth century, shows that psycho-therapeutics was by no means entirely unknown at that time, and those investigators who took up the inquiry into animal magnetism about the year 1800 were evidently acquainted with the value of the power of the imagination. Bailly, for instance, in 1784, ascribed Deslon's phenomena to the power of the imagination, and about the same time John Hunter expressed similar views on animal magnetism. Even many believers in animal magnetism, such as Kluge and Eschenmayer, were acquainted with the effect produced by the imagination.

Throughout the whole of the nineteenth century, and down to the present day when the study of hypnotism has directed general attention to psycho-therapeutics, there have always been able individual investigators who have pursued their own course without regard to the teachings of scholastic medicine or the opinions held by the general body of medical men. To make an arbitrary selection from the middle of the nineteenth century, I may mention Wilde, who in 1830 recommended a little wholesome dissipation as medical treatment; Brigham, who wrote on the influence on bodily health of intellectual culture and the exercise of the mental faculties; Traiber with a dissertation, *De Cura psychica;* Domrich, who wrote a treatise entitled *Die psychischen Zustände, ihre organische Vermittlung und ihre Wirkung in Erzeugung körperlicher Krankheiten.* I further mention Moore's book, *The Power of the Soul over the Body;* Sadler on *The Power of a Doctor's Personality in Alleviating and Curing Disease;* Padioleau's excellent little work, *De la Médicine morale*, 1864; and finally that profound and exhaustive work, *The Influence of Mind on Body*, by the English alienist, the late Hack Tuke.

Although there were from time immemorial individual investigators who recognized the value of mental influence, no organic connection between psycho-therapeutics and medicine was thereby created. It was the study of hypnotism, which since the rise and progress of the Nancy school had shown the value of suggestion either with or without hypnosis, that first called general attention to other remedies of a mental nature. No matter how much one may be opposed to hypnotism itself, the fact that it has led up to modern psycho-therapeutics and caused the latter to be incorporated in medicine cannot be denied. We are compelled to arrive at this conclusion even when we admit that other authors have developed their views of psycho-therapeutics independently of hypnotism, for they, also, were not recognized until the importance of mental impressions had been demonstrated by means of hypnotism. Before that, they were ignored by official science, because they had strayed from the beaten track; and their works were more frequently considered curiosities than scientific productions. I need here only recall the case of Rosenbach, who endeavoured to develop therapeutic views to a great extent in opposition to the theory of suggestion, but who nevertheless admitted that the incentive to his studies

came from hypnotism. At all events, it is almost entirely due to the general interest in psycho-therapeutics aroused by hypnotism that the psycho-therapeutic works of Rosenbach and others were recognized. This is shown by the numerous works that have appeared on the influence of the emotions on the body—for instance, those of Mosso and Lehmann, and more recently, Paul Cohn and H. Berger. It was already known how much menstruation is influenced by excitement and fear. Many of the effects produced by the imagination were also known, such, for instance, as imaginary pregnancy. But there was nothing systematic in all this; the occasional observations made were disparaged and contemptuously referred to cases of hysteria. It was too much the custom to look on a case as merely interesting and rare, not as a clear indication of the existence of a great branch of therapeutics.

A comparison between modern works on nervous diseases, hysteria, and neurasthenia, and those that appeared five-and-twenty years ago at once shows how the value of psycho-therapeutics has risen in medical estimation. Let any one who thinks that hypnotism has opened up no new paths just take up one of the medical text-books of those days, and he will find as far as hysteria is concerned, to say nothing of other diseases, all kinds of therapeutic measures thoroughly discussed, but that the most important—viz., mental treatment, though not entirely ignored, receives but cursory mention. He will find exact instructions for the application of leeches to the *portio vaginalis*, what waters are to be drunk, what bathed in, and what form of electricity is indicated. But that all such matters are of insignificant importance in comparison to mental treatment was unknown, although the importance of mental remedies was occasionally admitted, as in the case of hysteria. As far as diseases are concerned—I make no mention of mental maladies, which only received a place in psycho-therapeutics at the commencement of the nineteenth century—psycho-therapeutic influence was almost totally ignored. It must also be admitted that many new branches of psycho-therapeutics are not directly linked to hypnotism, though they have been indirectly influenced by it; to such belongs, for instance, attendance on and care of the weak, which is a psycho-therapeutic factor of the first order. But admitting even this, it cannot be denied that hypnotism has exerted a

thoroughly fundamental influence on the development of psycho-therapeutics.

That it was hypnotism that essentially gave the impulse to the study of modern psycho-therapeutics is evident from the works of the chief investigators of the latter subject. The very first among them—even those opposed to hypnosi-therapeutics, as, for instance, Rosenbach, Dubois, and Marcinowski—have unhesitatingly admitted that their incentive to the study of psycho-therapeutics was due to hypnotism; and it was from hypnotism that I myself first of all learned how extensively we can influence human beings by means of psycho-therapeutics. In cases in which for some reason or other hypnosis could not be induced, I have sought, and often found, a substitute in some other form of mental influence. O. Binswanger also, who is one of the opponents of hypnosi-therapeusis, admits the importance of hypnotism in this respect. He certainly also thinks that the hypnosi-therapeutists, by holding that hypnosis is the only recognized branch of psycho-therapeutics, have hindered the development of the latter. It must be admitted that there has been both exaggeration and one-sidedness. But any one who blames the elaborators of hypnotism in general with this misses the chief point; for as long as hypnosis was almost the only known branch of psycho-therapeutics, investigators of hypnotism naturally considered it the heart of psycho-therapeutics. To say there is anything blameworthy in this would be like reproaching the men who introduced antisepsis with having delayed the development of asepsis, for the latter is the outcome of the former. O. Binswanger has, indeed, a strange way of appraising the value of services rendered to science. The man who originally prepared and recommended a serum for the treatment of a particular infective disease is not to blame when his serum is improved and perfected later on, for without the original preparation there could have been no improvement. To anybody who thinks with O. Binswanger that he is justified in attempting to minimize the importance of hypnosis in such a manner I put the following question:—Where were these critics, such as Binswanger, when Ewald said that hypnotic treatment should be left to tailors, cobblers, and shepherd-boys? Where were they when Ewald declared that suggestive treatment oversteps the bounds of medicine and trenches on the field of psychology? *Ewald distinctly opposed psychological to medical*

treatment, and the adherents of the Nancy school of hypnotism *alone* protested. Yet it was particularly O. Binswanger who held his peace; he did not declare that psychology must be made as useful to medicine as physics and chemistry have been, and he failed to point out that Ewald in his assertions has shown a total misconception of the duties of a doctor and the problems that face medical science.

I take this opportunity of mentioning a few works on psycho-therapeutics that have appeared in recent times—*i.e.*, since modern hypnotism has played a part. Some of them are but little known, and for that reason I think they deserve some slight mention. It is not claimed that the list is by any means complete, and I entirely omit those works in which hypnotism is the main theme and psycho-therapeutics only a side issue. I mention Alphandery's thesis, *La Thérapeutique morale et la Suggestion*, Paris, 1885; Lindsay's essay in the *Glasgow Medical Journal*, 1886, on "Faith in Relation to Medical Treatment"; and Charcot's *La Foi qui guérit.* In 1891 Taylor wrote a work entitled *The Mental Element in the Treatment of Disease.* F. C. Müller's *Handbuch der Neurasthenie*, 1893, contains a detailed account of the "Mental and Suggestive Treatment of Neurasthenia" by Schrenck-Notzing. I may also mention Laurent's *La Médicine des Ames*, Paris, 1894; and from Germany, Hallervorden's *Abhandlungen zur Seelengesundheits-lehre*, which appeared 1896-97. In a Bordeaux thesis, 1898, Bellet discussed *The Therapeutics of Imperative Ideas;* and in 1902 Arthur Hermann wrote a thesis on *The Suggestive Treatment of Headache in School-children.* In 1901, Paul Hartenberg discussed the psycho-therapeutic treatment of timidity in a monograph, *Les Timides.* Zbinden published a small work, *Neurasthénie et Psychothérapie*, in 1902, and also another that belongs here, *Conception psychologique du Névrosisme.* The psycho-therapeutic treatment of *The Tics* has been exhaustively treated by Meige and Feindel in a mono-graph. Among other authors to be mentioned are Krehl and Dunin, also Oppenheim with two works, *Zur Prognose und Therapie der schweren Neurosen* and *Psycho-therapeutischen Briefe.*

I now proceed to mention some of the larger works specially devoted to psycho-therapeutics, taking first of all Rosenbach's *Nervöse Zustände und ihre psychische Behandlung*, a book of which the first edition appeared in 1897 and the second in

1903, and which is possibly the best and most thoughtful work we possess on psycho-therapeutics. In 1897 Lœwenfeld published his *Text-book of General Psycho-therapeutics*, which contains a series of excellent essays on medical psycho-therapeutics. I also mention Vogt, who has given a more theoretical than practical review of psycho-therapeutics, somewhat in the form of a programme, in the *Zeitschrift für Hypnotismus;* further, Ziehen, who has given advice on practical therapeutics in the *Lehrbuch der allgemeinen Therapie*, edited by Eulenburg and Samuel, but without exhausting the whole field of psycho-therapeutics or treating it completely. In the year 1904 an exhaustive treatise by Camuz and Pagnier appeared entitled, *Isolement et Psychothérapie;* it contains the records of a series of carefully-observed cases, which show the value of psycho-therapeutics, in addition to which it includes some very neat historical comments on psycho-therapeutics. I must also mention Dubois' book, *Les Psychonévroses*, a new edition of which appeared in 1905, as well as a German translation. It contains a number of useful hints. It would certainly have been better had the author paid more attention to the earlier works of other writers, especially Rosenbach, which contain much that Dubois has to say, but expressed in a better form. Evidently Dubois was unacquainted with Rosenbach's works. This is also the place to mention Eschle's book, *Die Krankhafte Willenschwäche und die Aufgaben der erziehlichen Therapie*, that appeared in 1904; and, further, the exhaustive chapter on psycho-therapeutics in O. Binswanger's work, *Die Hysterie*, also published in 1904. I must also mention Marcinowski's book, *Im Kampf um gesunde Nerven*, 1905; and Levy's work, *L'Éducation rationelle de la Volonté*, which was widely read in France. I myself have thoroughly discussed psycho-therapeutics from the standpoint of ethics in my *Aerztlichen Ethik*, 1902, and have also contributed a comprehensive sketch of psycho-therapeutics to Eulenburg's *Enzyklopædischen Jahrbücher*, 1906.

Finally, there is in connection with this branch of the subject a whole series of more or less scientific books that have recently appeared in the market in various countries—Germany, France, and also in America. I shall not mention them separately, but will remark that they are for the most part of no scientific importance. Some of the authors do indeed attempt to grasp the problem after the manner of Feuchters-

leben in his *Diätetik der Seele*, but they never come up to the standard of their prototype.

When once the importance of hypnotic suggestion had been established by hypnotism, an increased significance was attributed to suggestion in the waking state, and efforts were made to employ it in therapeutics. An intermediary is, however, necessary to bring it into play, and for this purpose all kinds of means can be employed. Ziehen recommends that we should, as far as possible, choose such as have a physical action of the nature indicated; and undoubtedly this advice is sound in the main. Many authors have thought out and recommended special methods of suggestion. Electricity has been used for this purpose for a long time past, and has recently been specially recommended by Altschul. Loeb recommended that in cases of hysterical aphonia the patient should press his epiglottis down with his forefinger. The means employed for the suggestion must be suited to the individuality of the patient. In one case it will be found necessary to use electricity, in another pharmaceutical preparations, in a third a journey to a watering-place will be indicated, and so on. A doctor who had a large country practice was in the habit of adding violet-syrup to certain drugs, because the "beautiful green medicine" always gave special satisfaction. A pungent odour may act in the same way as a striking colour, and the high repute in which asafœtida so long stood was essentially due to its stink. The price of a drug may also play a part in suggestion, and I think that the reputation of yohimbin was chiefly due to its high price. In other cases, strong sensory stimuli, such as the faradic brush or the application of moxa, have a suggestive action. Even the fact that the treatment is complicated, as in the case of some hydro-therapeutic "cures," may help to convey the necessary suggestion. It is occasionally desirable that the patient should have no notion of what drug he is taking. For instance, a patient who has taken antifebrin without deriving any benefit from it, who even thinks it made him worse, and who, by auto-suggestion, easily sets up unpleasant results when he knows that he has been given antifebrin, will take it without any unpleasant results if it is prescribed as acetanelide. Many a doctor is a master in inventing all kinds of new ways of bringing suggestion into play, whereas others are invariably

satisfied with one and the same vehicle. Remedies that many a doctor does not consider professionally correct—for instance, a journey to Lourdes or some other shrine—may prove of great service in conveying suggestion. Suggestion can be made just as effective by the personality of a physician whose every word acts as a suggestion, because of the trust placed in him, as by the definite instructions he gives. Moreover, in many cases much talking is superfluous, even injurious. It is sometimes erroneously assumed that the patient must be talked into believing in the efficacy of the remedy prescribed. This may, of course, be true in many cases, but there are others in which the suggestion affects the patient more readily when the remedy is prescribed without any remarks. The old truth that suspicion acts as an inhibitant is very applicable to suggestion. We must here include many hysterical cases with a tendency to suggestion in which a kind of counter-suggestion follows verbal suggestion. On this point I refer to my remarks on hypnotic therapeutic suggestion (page 338).

I take this opportunity of pointing out that it has been proposed to make the same use of ordinary sleep as of hypnosis for the purpose of suggestive therapeutics. This proposal was specially brought forward by Sante de Sanctis, who, however, distinguishes between suggestion made in ordinary sleep and when the latter has passed into the hypnotic condition. Certainly de Sanctis does not restrict the method merely to suggestion; on the contrary, he recommends that all kinds of sensations should be aroused, and even chemical substances be used. In the same way he thinks it right that Corning's proposal should be further investigated, and music played in certain cases of neurosis both before and after the patient falls asleep. Sante de Sanctis thinks that dream-therapeutics would benefit nervous and psychopathic children, and also hystericals —for instance, such as suffer from hysterical paralyses; it should also be of use in those states that he calls dream-psychoses. Indeed, it should be as easy to induce the desired sensation in a sleeping person as in one going to sleep. I must here refer to what I said on page 178 concerning artificial dreams, which naturally play an important part in respect to this proposal.

Our knowledge of the importance of suggestive therapeutics is essentially due to hypnotism. But just as suggestive therapeutics has proceeded from hypnotism, so has psycho-

therapeutics proceeded from suggestive therapeutics. But this historical development has occasionally caused a confusion of ideas. As I have already mentioned, hypnotic therapeusis was long supposed to include psycho-therapeusis. Such a mistake should not be made in the present day, and Lœwenfeld has therefore very properly blamed Lloyd Tuckey for calling his text-book of hypnotic therapeutics *Psycho-therapeutics*. In the same way suggestive treatment is often confused with psycho-therapeutics. Even doctors engaged in this branch easily make this mistake. Font has reported a case from Desjerine's Clinic, in which suggestion was applied in the waking state to a twelve-year-old girl suffering from contraction of both legs. The child was rigidly isolated during the treatment and threatened that she should not see her parents again until she could walk properly. Although it is hardly possible to exclude the action of a suggestive factor in this case, the treatment cannot be described as suggestion in the waking state, for threats and isolation differ considerably from suggestion.

We shall now see that there are other branches of psycho-therapeutics that must be distinguished from suggestion.

First and foremost comes the instructional and explanatory system of treatment. It has received the special advocacy and support of Rosenbach and Eschle, while Marcinowski, Dubois, Oppenheim and others have lent their aid. The method consists in explaining to the patient the nature of his complaint, the treatment to be pursued, and everything connected therewith, in the hope of thereby conducing to his cure or at least improvement. I will quote one of Rosenbach's examples. A man who thinks he is suffering from heart-disease, because of occasional irregularities in his pulse, should be told that such irregularities occur under normal conditions and may be caused by intense self-observation, change of position, or even some slight muscular action. It should be explained to him that a doctor prefers to examine the frequency of the pulse or respiration when the patient is not noticing, lest some change should take place in these functions. To give a few other examples. The dread of some hereditary taint has reached an extraordinary pitch in the present day, and it is certain that this dread of hereditary taint does more harm than the taint itself. Such patients should be informed that nearly every great family boasts a disease that may be termed hereditary, more particularly since the number of such hereditary diseases has been

found to be continually on the increase. Such timorous persons should, if necessary, be referred to works presenting the opposite view—for instance, to the works of Jenny Koller, who has proved that there is more likelihood of inherited taint existing in the case of healthy people than is generally assumed, that the regenerative factor is sometimes extraordinarily strong, and that many diseases supposed to carry an hereditary taint are of no importance whatever in that respect. A recent work of Wagner's may also be referred to, as its author has arrived at similar conclusions. In other cases—when, for instance, a patient thinks he is suffering from lung trouble because of pains in the chest—it should be pointed out that it is the muscles and not the lungs that are affected, and proof of this should be given by showing that the muscles are painfully sensitive to even the slightest touch. Many a person who imagines that he is suffering from serious stomach trouble and that he cannot digest anything, can be easily freed from apprehension by washing out his stomach and thereby proving that his digestion is normal. Rosenbach pointed this out long ago. Of course it is relatively easy to give this satisfaction when the patient is a doctor, but even in the case of a layman it is sometimes possible to demonstrate the error by comparing the contents of his stomach with those of a healthy man. It occasionally requires an educational process to induce a patient to submit to treatment. Many people erroneously consider their malady incurable, and refuse treatment on that account. Bérillon and Jennings have pointed out that many morphinists refuse to submit to treatment because they are convinced that their affliction is incurable. It is sometimes even necessary to explain the theory of his disease to a patient, so that he may help to bring about its cure. Many cases of sexual perversion can be cured, and some prevented, by giving the necessary explanation. This, of course, refers particularly to the time when the sexual impulse is still undifferentiated. As Max Dessoir has shown, perversion appears in the earliest days of puberty and often leads to "perverse" tendencies, but the latter disappear altogether when taken in time and properly treated. It is of the greatest importance that such young people should be warned against wretches who would lead them astray and endeavour to make them believe that their perverse tendencies are incurable. For this purpose it is sometimes necessary to explain how sexual perversion comes

about and all that appertains to it, if the victim is to be rendered amenable to medical advice. A similar method of procedure is necessary in some other cases; Oppenheim, for instance, was only able to make a sufferer from *akinesia algera* follow his instructions after he had explained the origin of the disease and the nature of the underlying mental factor. Sometimes it is as well to convince the person who comes for advice that he has nothing the matter with him. There are patients who imagine they are impotent merely because their powers are not the same as others. It is beneficial to let such people know that men boast of powers they do not possess. Similarly, sensitive young men who have never tried their prowess on ladies of easy virtue should be taught that that is no reason why in married life they should prove impotent in the arms of a modest woman.

In other cases it may be essential to prove that the disease from which the patient thinks he is suffering—cancer, softening of the brain, spinal disease, kidney disease, etc.—does not exist. There are times in which such fears are epidemic; this is especially the case when a disease is the subject of much discussion in the papers because some public person is suffering from it, or that medical investigations or some other circumstance have led to its discussion. When the Emperor Frederic was suffering from cancer, thousands of people went to their doctor to know if they were not developing cancer of the larynx. Many of them were, of course, suffering from some organic affection of the larynx, such as catarrh, but very many others had not even that trouble. The same thing is observed when an epidemic like cholera is raging; people take a harmless diarrhœa for the severe disease cholera. We must also include here people who think they are suffering from a disease because they have misunderstood the description given of it. This fear of disease frequently arises from reading nasty popular works on medicine, but it also happens to young medical students when they first hear diseases minutely described. The best method of quieting and curing such patients is to submit them to a searching examination; that will generally satisfy them that their fears are groundless. This method does not, of course, succeed in serious cases; it is then necessary to leave it to the ingenuity of the doctor to devise some means of satisfying the patient. A doctor once took severe measures with a patient who would not believe that he

was *not* suffering from hydrophobia; the doctor kissed the patient on the lips as a proof that *he* had no fear of infection.

It must be explained to patients who occasionally suffer from headache or loss of appetite that such things happen to the healthiest people and are of no importance. Special care must be taken to do this with patients who have at one time suffered from a severe disease and who dread its return every time any trouble reminds them of it. A patient who has suffered from ulceration of the stomach will generally be very frightened when he has a slight pain in the region of that organ. He should then be told that such pains are not as a rule the result of organic changes.

In many cases explanation should be directed to prognosis and therapeusis. A patient who is suffering from a chronic disease should be told that it is seldom cured spontaneously, and that there may even be exacerbations of the malady while the curative process is gradually going on. It is often as well to point out these matters beforehand, so that the patient may be prevented from being frightened by anything that may occur and from drawing unfavourable conclusions therefrom. It is sometimes advisable to give the patient a proof that the treatment is doing him good—to show a diabetic, for instance, that the analysis of his urine points to a diminution in the excretion of sugar. This is calculated to make him more inclined to follow his doctor's advice in the future. It will also sometimes be necessary to point out to patients that the remedies they have proposed are useless, perhaps injurious. For example, people who suffer from insomnia should be told that the constant use of hypnotics is injurious and may set up a condition even worse than the loss of sleep.

Of course, doctors who practise psycho-therapeutics must be as careful to avoid overdoing it as when using any other method of treatment. It is just this explanatory system of treatment that so easily gives rise to such errors. Many a doctor thinks he is making matters clear to a patient when he is doing nothing of the kind. For this reason I will point out an error that was formerly very common, though it does not occur so frequently nowadays. It should never be drummed into a patient that he is not ill because he has no organic lesion and that his malady is consequently imaginary. Folks frequently make such remarks; but a psychologically trained doctor should scrupulously avoid anything of the kind at all

times. He should know that the expression "imaginary pain" is false. Such "imaginary pains" have been excellently compared with hallucinations. Now, we can say that the hallucinatory object is imaginary; but it is false to say the perception is imaginary; it has a central cause, whether the object is imaginary or not. Similarly, a pain that is felt is the result of a definite central process. It is a matter of indifference whether the central process is caused by a peripheral one such as a prick, or by suggestion by a spontaneous mental act. The pain exists in both cases, and is not imaginary. If in the latter case the patient were to refer it to an external stimulus he would be wrong. But the doctor must take the pain the patient says he feels to be real. To combat and remove such pains is just as much the duty of a doctor as the healing of a wound. A doctor may be able to detect and explain the functional nature of a pain, and even trace it to its mental origin, but he should never say that it is imaginary. It may possibly not be invariably easy for him to avoid mistakes; for there are patients who think their disease is not understood when a doctor assures them of its purely functional nature. A doctor must not only take into consideration the education a patient has received, he must also think of the degree of intelligence possessed by the latter. He must remember that a patient's power of thinking logically is influenced by disease in so far as the patient's own conclusions as to his disease are concerned. There are well-educated people, people who as a rule think logically, and who are yet unable to understand that a functional complaint is just as much a disease as an organic one, and may even bring about more serious consequences than the former. Experience and tact are the best aids a doctor has for getting over such difficulties.

As years go on I am more and more convinced that uprightness, truth and candour afford the best help a doctor can give his patient, and that the reserve that used to be displayed is only too often inimical to the patient's interests. Even when we tell a patient truthfully that we cannot cure his disease, the truth is often of use. Kant tells of himself that his flat and narrow chest, which gave but little play to his heart and lungs, predisposed him to hypochrondriasis. He suffered from cardiac oppression, but the conviction that the cause of the oppression could not be removed soon made him cease to

dwell upon it. "The oppression is still there, for it is caused by the build of my body, but I have mastered its influence on my thoughts and actions by distracting my attention from it, as though it had nothing to do with me." A man suffering from hemiplegia following an apoplectic fit, with no sign of improvement as time goes on, will often be greatly benefited by a doctor who teaches him to take no notice of the paralysis, provided, of course, that the chance of curing him is very small or *nil.* Such a method of treatment will be more advantageous to the patient than sending him from one expensive doctor to another or from this to that institution and finally landing him in the hands of a quack. When the chance of obtaining a cure is small, the question of expense is generally important. It is better for a doctor himself to inform a patient that he is suffering from *tabes dorali's* than to let him be brutally told the truth by some one else. It is just in cases of *tabes* that a truthful explanation lessens the patient's dread of his disease. Recent statistical investigations have shown that the average duration of life is not affected by locomotor ataxy. There are cases in which the name of the disease terrifies the patient, and yet good may be done by explaining its nature; as, for instance, when a doctor explains that diabetes is an inclusive term and does not mean a definite disease, and that there are forms of diabetes which are in nowise dangerous.

It will be seen from the foregoing that I rate explanation and instruction very highly. But this must not be overrated as a curative factor, as some have done—for instance, Dubois. People forget that suggestion plays a great part in all explanations, and cannot be altogether excluded. The patient who trusts the doctor from whom he receives instruction and explanation is subject to the influence of that doctor's suggestion. Much that Dubois and others put down to instruction is really due to suggestion. But apart from this, there are cases in which suggestion is more effective than instruction. Many forms of auto-suggestion belong to this category. A patient who suffers from diarrhœa after taking black coffee may be relieved of this inconvenience by explaining that it is the result of auto-suggestion. In other cases counter-suggestion proves much more effective. Explanation will always improve the condition of a person suffering from imperative ideas. The patient clearly recognizes the foolishness of the idea, but is unable to resist it. When it is possible to induce deep

hypnosis in such cases good results can be more readily obtained than by instruction. It sometimes happens that explanation given at a wrong time is absolutely dangerous. I can recall the cases of patients who were weaned from the drug habit by sham injections—indifferent solutions that they imagined contained morphia. But when such patients are told the truth, they invariably return to their old habit. Experience, at least, shows this.

Of course, many patients feel flattered when they are told that they will not be treated by suggestion but that their own intelligence, powers of thought and will, will be brought into play. Suggestive treatment usually appears easier to the patient, but his vanity is flattered by the explanatory method. Nevertheless I think there are many cases in which it is carrying suggestion too far to tell a patient that he is being cured by instruction, whereas in reality the improvement in his condition is mainly due to suggestion. Unintentional suggestive therapeutics may consist in the suggestion to a patient that he is being cured by instruction and his own will. People who in the present day discard suggestion as a therapeutic method go too far. In the same way that Rosenbach formerly did, Grasset has recently held up suggestive-therapeutics as *psycho-thérapie inférieure* in contradistinction to *psycho-thérapie supérieure* as generally accepted. Nevertheless, catchwords should be avoided, and it should never be forgotten that what is supposed to have been brought about by *psycho-thérapie supérieure* is often the result of veiled suggestion, the patient being convinced that his own will and intelligence have restored him to health.

Of course we must make all possible use of every psychotherapeutic factor, especially of the explanatory method as I have described it. The last remarks I made on this subject were merely intended as a warning against exaggeration.

I now come to the discussion of volitional therapeutics. This is often confused with explanatory therapeutics, and even Rosenbach and Eschle do not make a proper distinction between the two. Although both methods are sometimes used together as "educational therapeutics"—there are many cases in which the patients can only be induced to exercise their will by first of all instructing them as to the necessity of this procedure,—they may be employed independently. As far as volitional therapeutics is concerned, we must remember that

the activity of the will is of two kinds—the external or objective, which is shown in the external movements and actions that depend upon the will, and the internal or subjective, which can arbitrarily arouse mental processes, such as ideas, feelings, and emotions. Both the external and the internal activity of the will may excite or inhibit; the will can cause movements and actions, or check them; in the same way it can, within certain limits, arouse ideas, feelings, and emotions, or suppress them. One part of volitional therapeutics is, historically, closely connected with hypnotism, but some of its branches have developed independently. We come across the external activity of the will in the form of active mental gymnastics long before the days of modern hypnotism; and that, too, as a branch of physical therapeutics, although it might equally well be included in psycho-therapeutics. For instance, the attempts that have long been made to improve the conditions set up by apoplexy by means of exercises really constitute an employment of the activity of the will. This branch of psycho-therapeutics has long been used for disturbances of speech following apoplexy, the patients being instructed to repeat sentences and go through other exercises. Whether the compensation produced is organic or functional in such cases—we are following Luciani's scheme—has no bearing on the inclusion of this method of treatment in psycho-therapeutics. Moreover, hypnotism has not been without influence on the external gymnastics of the will. The name of this branch of treatment is occasionally associated with hypnotism. Under the name of suggestion-gymnastics Lehmann has described a method of gymnastics for the paralyzed with the object of rendering non-conductive but undestroyed paths permeable by the will. The chief point is for the patient to concentrate his will as much as possible on the movements to be performed, and to learn to believe in the results of his own activity. Frenkel has also recommended that the treatment of ataxy should be considered a branch of psycho-therapeutics. Our object should be to restore the lost power of co-ordination by exercises in which the mind plays the greatest part. Of course it is of the greatest importance that the patient should concentrate his whole will on the correct co-ordination of the movements he is told to carry out.

Granted that the influence of hypnotism on muscular gymnastics is not to be denied, it is of much greater importance

where the inhibitive effect of the external activity of the will is concerned. It has frequently been used in recent times to suppress convulsive movements, and often gives good results in cases of chronic chorea, though it is less effective when the complaint is acute. But it is particularly useful in all other kinds of convulsions, especially such as are termed tics. Meige, Feindel, Brissaud, Oppenheim, and others have given examples of the great efficacy of will-gymnastics. The influence of the will must be methodically strengthened by at first limiting the suppression of the unpleasant symptom to only a few moments, indeed only to seconds, and then gradually increasing the duration of the effort. It will always be possible to increase the effect by using all kinds of accessory measures, such as making the patient look in a mirror while he is performing the exercise, as he would otherwise fail to notice the involuntary movements he makes. It is also important to let the patient rest for a time after his attention has been kept fixed. Although this branch of psycho-therapeutics has been greatly furthered by hypnotism, it is not altogether new. In former times people were often enough somewhat hazy as to the psycho-therapeutic character of this method of treatment. I remember the treatment of writer's cramp that was recommended by the writing-master Julius Wolff a long time ago, and which consisted in a combination of massage and gymnastics. The treatment of stammering also has its place here. The exercises in breathing and speaking that are set a stammerer likewise serve to strengthen the influence of the will on the motor sphere of speech. The treatment of stammering, moreover, shows how little we are able to separate the suggestive element from other influences. Apart from the fact that a patient is often influenced suggestively when the doctor who is treating him holds out the possibility of his being cured by exercises in talking, we have to consider that such exercises as well as the whole *milieu* tend to strengthen his trust in his own capabilities.[1]

But it is especially in its relation to hypnotism that the importance of the internal activity of the will has been recognized as a mental remedy. Certainly it had already met with popular commendation. Anybody suffering from pain was told

[1] It often happens that the effect of suggestion is favoured by telling the stammerer that the doctor who is treating him formerly suffered in the same way and was cured by the method he advocates.

he should not think of it, but pull himself together. But no methodical efforts were made to strengthen the patient's energy. In this respect hypnotism cleared the way. By showing how much can be done by hypnotic suggestion, it raised the question whether the same results could not in many cases be obtained by the action of the patient's own will. In many cases this question must be answered in the affirmative, although it may often happen that a methodical strengthening of the will is first of all necessary. I must here refer to what Payot, who has thoroughly discussed the therapeutic employment of the will, has said of the influence of the latter on the sequence of ideas. He has rightly pointed out that every recollection that is not from time to time repeated gradually fades, and finally disappears from memory. We are to a great extent master of the attention we pay to anything, and we might easily eliminate a memory-picture by striving to resist its recurrence. But as the patient has frequently no tendency to do this, his doctor must guide him. In many cases of imperative ideas, either with or without a feeling of dread, the evil may be combated by gradually and methodically increasing the pressure brought to bear on the patient. In a case of agoraphobia the doctor should recommend the patient to gradually increase the length of his walk, or to take a companion with him at first—this generally does away with the feeling of fear,—and then to increase the distance from the companion gradually. I have often seen this method succeed with people who were afraid of railway-travelling. This enables the patient to take longer journeys, and it is often noticeable that people who have made some little progress in this way get on rapidly afterwards. For instance, when a patient who could only remain a few minutes in a train gradually becomes able to stay in one for half an hour, the transition to journeys lasting days and nights will be very rapid.

Another phenomenon, often closely connected with imperative ideas, may also be beneficially influenced by gradually increasing the patient's energy. Many patients who suffer from imperative ideas feel a constant impulse to talk about their malady. This is very trying to those about them, and gradually alienates any sympathy. In such cases it will prove beneficial to the patient and his surroundings little by little to teach him self-control. I have often observed that patients who are given permission to talk about their complaints for a

specified time, such as an hour twice a day, never take advantage of the permission, even in those cases where they were formerly in the habit of complaining about the imperative ideas that troubled them, every time they met a relative. Even if this treatment fails to bring about any improvement in the patient's disease it is none the less of importance, for it prevents him from annoying his relatives with constant complaints, and thus enables him to retain their sympathy. This circumstance must not be underrated, for there is a limit to the forbearance that relatives can be expected to exhibit.

Volitional therapeutics, especially the exercise of the internal activity of the will, of course often greatly taxes the patient's powers. From this it should be evident that it will generally be as well to explain to the patient the purport of the treatment he is to undergo, especially when the disease that he is to assist in eradicating is not precisely irksome to himself. This refers particularly to the affections included in the generic term sexual perversion. I shall enter into a somewhat detailed discussion of this point, because, in the first place, the treatment of sexual perversion by the influence of the activity of the patient's own will constitutes a paradigm for therapeutics of this nature; and, in the second place, because the importance of volitional therapeutics in combating such affections has been very much underestimated. When a person suffering from perversion—a homosexual, for instance—is to be cured by the activity of his own will, he must be addressed in something like the following terms:—"All sexual thoughts, whether normal or perverse, may be divided into two groups according to their mode of origin—the voluntary and the involuntary. The latter often arise without its being possible to state the cause. An accidental meeting with a sympathetic person, a sentence in an otherwise harmless book, or an organic stimulus such as the accumulation of semen will often arbitrarily arouse sexual ideas. But sexual ideas are frequently produced voluntarily. People try to create voluptuous feelings by recalling past events of an exciting nature and giving free scope to their imagination. It is hardly possible for there to be a normal man who has not at times let his fancy run on such subjects. And this applies equally to the perverse as to the normal individual." The method by which perversion is to be combated must depend upon a distinction being made between voluntary and involuntary sexual ideas. The victim of perversion cannot fight

involuntary ideas successfully, but he should concentrate all his energy on avoiding the voluntary creation of perverse fancies. Indeed, whenever perverse ideas crop up involuntarily, he must endeavour to suppress them by an effort of will. It is immaterial, in this respect, whether the patient endeavours to create normal sexual pictures in his mind or betakes himself to some occupation that will distract his attention, provided he does his utmost to suppress perverse mental images. Although the vast majority of patients assert that the perverse thoughts arise involuntarily, there can be no doubt that the voluntary ones must first be got rid of if we are to succeed in suppressing such as are involuntary. But in addition to this we must exert ourselves to make the patient sensitive to normal sexual stimuli. For this purpose he should either be led from time to time to direct his attention to normal sexual processes, or else should be occasionally given an opportunity of testing his capacity to react to normal sexual stimuli. This can, for instance, be accomplished in the case of a homosexual man by placing him in the right kind of female society. The chief point, however, is not, as some occasionally assume, that the homosexual individual should seek intimate heterosexual intercourse, but rather that he should learn to react to the charms of persons of the opposite sex by frequently associating with them. There can be no manner of doubt—and this refers equally to the homosexual, the pædophile, the fetishist, the masochist, and the sadist—that many persons can be brought in this way to lead a normal sexual life without any suggestive treatment. The central idea of the treatment is that the patient should avoid the voluntary induction of perverse sexual notions, and should also attempt to combat any that may arise involuntarily; on the other hand, he ought to force himself to create normal sexual ideas. I have often found it of great assistance that the patient should form sexually normal ideas shortly before going to sleep. This frequently appears to act beneficially in bringing about dreams that are normal from the sexual point of view, which, it is well known, hardly ever occur in cases of perversion.

It is perfectly clear that the activity of the will can only be employed in combating sexual perversions where the patient is endowed with moral strength of a high order. A person who is sexually perverse will be as little inclined to avoid dwelling on lascivious ideas as is the normal individual to turn away

from agreeably voluptuous thoughts. The real danger consists in the disinclination of the patient to give up sexual fancies congenial to himself, and to substitute for them a set of ideas which are just as unsympathetic to him as would be the perverse notions to a normally constituted man. But experience teaches us that many of the perverse do develop the necessary energy. And experience also proves that in many of those cases—especially of young people—in which the patient co-operates with his medical adviser, the result is thoroughly satisfactory. When the patient avoids all voluntary perverse fancies, normal sensation not infrequently ensues.

I have only given sexual perversion as an example; there are many other cases in which much can be accomplished by voluntary efforts at suppression—such, for instance, as idiosyncrasies of auto-suggestive origin, and all kinds of psychogenic disturbances.

The foregoing considerations show the close connection that subsists between volitional therapeutics and habit. We are able to attain the power of influencing all kinds of mental processes by regularly and methodically employing the will; we are further able to modify the functions of the body very considerably by increasing the influence of the will, as we saw, for instance, in the case of muscular activity. It is even possible to acquire an influence over functions not controlled by the conscious will—those of the bowels, for instance. It is possible by inducing a habit to bring about action of the bowels in the chronic constipation from which so many nervous people, and sometimes even whole families, suffer. A remedy that is often recommended consists in advising the patient only to go to the closet at a specified hour every day and also to resist to the utmost any inclination in that direction at other times. We often observe when employing this method that although, in many cases, the patient may not at first have a motion at the specified time, he gradually gains such control over his bowels that one very soon occurs regularly at the time intended. On the other hand, many people bring on constipation from a false sense of modesty; this occurs, for instance, with school-girls who are often ashamed to let their companions know that they could possibly be troubled with a natural function of that kind. Frequent suppression of the natural desire to defecate gradually sets up a sluggish action of the bowels and finally brings on chronic constipation.

Diversion of the attention is of the greatest importance in pyscho-therapeutics. It is related of Pascal that he tried to obtain relief from toothache by occupying himself with the solution of arithmetical problems, and Kant had a special method of diversion that he recommended for insomnia. There are two ways in which a doctor may recommend the use of diversion of the attention for therapeutic purposes; he may either tell the patient in general terms to divert his attention from his troubles, or he may give exact instructions intended to bring about the diversion. He may, for instance, advise the patient to go to the theatre, read some book, or travel. There are other cases in which the nature of the distraction must be left to the patient and only the necessity for it be insisted on. But experience teaches that exact detailed instructions are more readily followed than general advice. A patient who is told to read a certain book, or to translate a passage from a foreign language, or to go for a ride at a specified hour, is far more likely to obey the order than he would be to attend to mere general advice. Any one suffering from severe pain, or from imperative ideas, cannot have sufficient faith in diversion to try it readily, unless he be given definite instructions.

It is sometimes desirable that the method employed for diverting the patient's attention should be under the doctor's control. Oppenheim refers to cases of *akinesis algera*, in which he diverts the patient's attention from his trouble to something else, so as to preclude the inhibitive impulse. A patient of mine, who declared he could not walk without experiencing very severe pain in his back, was cured directly I took a few walks with him during which I distracted his attention by conversation. This convinced him that the pains were not necessarily associated with the act of moving; but since the belief that the latter is the case, and brings on or, at least, increases the pains auto-suggestively, we are often able to be of great use to the patient in the manner indicated.

There are many cases in which diversion of the attention is only intended to render the patient's troubles less appreciable; but in others it is an intentional adjunct to the general dietetics of mind and body. I shall return to this latter point when I come to discuss the treatment of disease by giving the patient some occupation. The importance of diverting the attention is very often underrated, and this has led to many

a person being misjudged. There are patients—not only hysterical women—whose troubles disappear directly they become interested in anything; an enjoyable theatrical performance, for instance, an agreeable book, or animated conversation. But only a layman can consider it a sign of want of energy, or even an evidence of simulation, when a patient who was previously full of complaints declares that such little matters render his pains less appreciable. We are here really dealing with a perfectly intelligible psychological phenomenon—viz., a reciprocal relation, that puts some elements of our consciousness in the background when we direct our attention to other matters. On the other hand, the value of diversion is sometimes exaggerated, and laymen often think that they can benefit patients by employing the method when it is really quite out of place—as, for instance, in melancholic states. It is quite wrong to persuade such a patient to seek diversion in travelling. Journeys were recommended long ago by Seneca as an excellent means of diverting the attention, especially on account of the number of other influences they bring into play, such as change of climate, the joys of nature, the pleasure of contemplating works of art and of meeting with other people. All these things may possibly put a stop to even serious nervous phenomena. Only, the value of travelling should not be overrated, as, for instance, very frequently happens in the cases of melancholia alluded to above, for rest is then the best remedy and not diversion.

The effects produced by diverting the attention indeed show how great a part is played in psycho-therapeutics by the feelings, and we shall also see when we come to treatment by occupation that we must be guided by the patient's inclinations. The easiest way of diverting the attention is to do something that is agreeable and decidedly amusing. Attempts have also been made to give a very wide scope to the therapeutic use of those influences that can be brought to bear on the inner and emotional spheres of life. Sydenham certainly recognized the importance of this remedy when he expressed the opinion that a whole town would benefit more by a visit from a Jack-pudding than by the importation of ten mule-loads of drugs. But unpleasant sensations likewise have their therapeutic value. They spur a patient on to defend himself and render him capable of resisting such daily influences as constantly

prove harmful to a pampered individual. These facts must be taken into special consideration where children are concerned, for it is only in this way that the latter can be made resistant.

Powerful or sudden emotions have also been known in many cases to exert a favourable influence on health. Hysterical paralyses have been cured by horror. A well-known historical instance is that of the son of Crœsus, who recovered his speech when he saw a soldier attempting to kill his father. Fright is also sometimes of service. Hack Tuke relates that an epidemic of somnambulism in a school stopped directly a couple of buckets of cold water had been thrown over one of the pupils just as he was beginning to walk in his sleep. Boerhaave brought an epidemic of convulsions to an end by threatening to use the actual cautery. Every practitioner of experience has observed some similar occurrence, though often only by accident. But we must, as a rule, be very cautious in the use of sudden emotions, especially horror, for the consequences may be serious and cannot always be foreseen. It is certainly more frequently possible to utilize fear as a therapeutic, at least indirectly. Many patients only follow the advice given them when the consequences of disobedience are brought home to them. A patient of mine who was suffering from alcoholic neuritis, and who was at the same time convinced that he could not do without alcohol, became an abstainer directly I told him, in strict accordance with the truth, that otherwise his paralysis would get worse and locomotor disturbances set in. The faradic brush, so often used in therapeutics, in many cases only has a mental influence, and that either by suggestion or, as Rosenbach points out, by the patient's fear of the pain that a repetition of its use would again bring on. It cannot be doubted that the hope of reward or the fear of refusal in respect to some special wish may induce the patient to exert greater energy in the direction desired. This may be observed even in the case of functions that take place involuntarily—nervous vomiting, for example. A patient troubled in this way sometimes ceases to be sick when she is told that she will have to be fed in a very unpleasant way artificially, perhaps *per anum*, should the vomiting continue. Again, a patient suffering from all kinds of tics will work all the harder to suppress them if it be hinted that he will be rewarded by a visit to the theatre or some other form of amusement. Nevertheless, we must not

fail to recognize that ethical considerations impose certain restrictions on a medical man. The psycho-therapeutic use of fear must never partake of the character of a punishment, for no doctor is ever justified in inflicting that. No matter how far a medical man's rights may be thought to extend, we must at least exclude from the sphere of his activity the power of inflicting anything that bears even the remotest resemblance to an indignity, under which head the question of the infliction of punishment by a doctor certainly comes. Such matters must be left to the proper authorities.

Volitional therapeutics and diversion of the attention have already given us an opportunity of mentioning treatment by occupation. Although we occasionally find mention made of it earlier, it is only in quite recent times that it has come to be seriously recognized as an accessory therapeutic agent that should be used methodically. Occupation not merely provides the patient with a temporary diversion from his troubles, but very frequently proves a true, indeed, the only remedy. Health in the case of most people presupposes well-regulated and active habits. Even in cases of incurable disease it is often of the greatest importance, since it provides the quickest means of suppressing the feeling of being ill. It is of far greater benefit to a sufferer from locomotor ataxy to keep him suitably employed than to leave him to his fate. "Even in cases of real sickness we must carefully distinguish between the illness and the feeling of being ill. For the most part the latter greatly exceeds the former" (C. W. Hufeland). It is just the very feeling of being ill that is soonest lessened by regular work.

Of course, we must individualize as much as possible in choosing the occupation. Many kinds of work are precluded by the nature of the disease. But apart from this, the capabilities, interests, degree of education, social position, pecuniary circumstances, age, sex, nationality of the patient, besides the tendencies of the times and many other matters, may any one of them determine what particular occupation is to be recommended. In that satirical but very instructive book entitled *Stolpertus, the Young Doctor at the Bedside*, that appeared more than a hundred years ago, the author expresses the opinion that many a fine lady would be better off with a besom in her hand than a fan. However right this may be, and however justified we may be in not laying too great a

stress on the patient's social position when choosing him an occupation, each of the factors mentioned above has to be taken into earnest consideration. We cannot employ an educated person merely with cleaning rooms, and on the other hand we should hardly recommend to a simple, uneducated woman the study of the history of Italian art as an occupation. A doctor must also avoid basing the advice he gives on his own personal tastes. Because he is himself an enthusiastic Wagnerite, he must not therefore recommend a patient who has no ear for music to attend Wagner's operas or sit out symphony concerts. Even though an interest in works of art is often only aroused by being brought into contact with them, the question of natural disposition yet plays an important part in the selection of the work. The colour-blind can never see things as the normal do, and there is an analogous distinction between persons of different mental constitution. A doctor should also never allow himself to be influenced by the patient's relatives; and he should therefore pay no attention to those mothers and husbands who still believe they have a right to deprive young girls and married women of every book dealing with the woman-question or similar problems. Times have changed, and any one who wishes to be convinced of this fact need only listen to the conversation that goes on between young men and young women nowadays. At all events, we have no business to withhold such books from women of a properly earnest disposition or to deny them a suitable sphere of activity.

In certain cases the doctor will lay the chief value on the mental, in others on the bodily activity caused by the occupation. It is often necessary to prescribe visits to the theatre, concerts, museums, a particular course of reading, to the importance of which B. Laquer, Lœwenfeld, Oppenheim and others have drawn attention. The further education of adults has also to be attended to by giving instruction either in languages or in other subjects. It sometimes has a very good effect if the doctor superintends the patient's work; recommends him, for instance, to read some book and then makes him repeat what he has read, or else gives him a written exercise—a translation, for example—and himself supervises the performance of the task. About the only thing a doctor had to do formerly was to write prescriptions, but his duties nowadays are very diversified.

B. Laquer has attempted to classify the books that should prove useful to patients according to Dunin's three chief types of the molimina presented by the sick. He has consequently made the selection of a book depend upon whether diversion, soothing, or encouragement is indicated. But I hardly think that a patient's reading can be arranged in this way according to types of disease. This method of differentiation will not, as a rule, hold good in practice, the other aforesaid factors playing too great a part. Similarly, it is very often futile to attempt to improve the frame of mind of a patient suffering from very low spirits by giving him humorous or farcical books to read. And I think the old rule that a *malade imaginaire* should never be allowed to obtain possession of a book of the kind called humorous still holds good in many cases of hypochondriasis.

Although we may consider the reading of suitable books a remedial measure, we must remember, on the other hand, that unsuitable books are injurious. Every practitioner knows the amount of misery that popular works on the "terrible" consequences of masturbation have caused. The discussion of mental diseases plays an important part in modern literature, and Ibsen's works in particular must be considered inimical to the public good. Of course we must admit that forbidden fruit tastes sweetest, and it is therefore often better to give direct advice as to what should be read—*i.e.*, the patient should not be forbidden to read dangerous books, but rather recommended useful ones. It is often prohibition that first leads a patient astray.

Moreover, the dangers connected with the nature of the book read were pointed out many years ago. In 1839 there appeared a work by Bird, entitled *Mesmerism and Belles Lettres*, in which the author made a special attack on Justinus Kerner for trenching on the domain of psychiatrics in a poem and a novel. Kerner's productions were compared with Shakespeare's; and Bird made out that Kerner, in contradistinction to Shakespeare, had done harm because his descriptions were wrong.

Muscular activity must also be included in treatment by occupation. It as frequently satisfies the indications of psycho-therapeutics as it does those of physical therapeutics. Here also the individuality of the patient must, of course, be taken into account. All kinds of physical sport belong here, such as cycling, riding, skating, tennis, gymnastics, and likewise

ordinary walking exercise. We need not hesitate to recommend rough work occasionally, and too great attention should not be paid to the question of its suitability to the patient's station in life. It is the man engaged in head-work who so often derives great mental benefit from physical labour—tree-felling, sawing, grass-mowing, for instance. Cutting down trees did the English statesman, Gladstone, an immense amount of good. The way in which muscular activity influences the patient's mind must be judged from the standpoint of psycho-therapeutics. Cycling, for instance, from both the mental and physical standpoint of therapeutics. The speed with which long stretches of ground are covered and the relatively small amount of muscular energy expended have a salutary effect on the consciousness. Fürbringer lays great value on the way in which the work is divided among the cerebral centres. "The cyclist whose attention is chiefly directed to the road and the surrounding country is compelled to make very great calls on the lower centres, the organs of sense, and as the heavy thoughts which cloud his brain disappear cuts off those factors that were depriving him of mental rest." To be able to rest from mental labour of an exhausting nature is certainly an essentially remedial factor in many cases. On the other hand, Monnier has rightly pointed out that many kinds of work—*e.g.*, turning the ergostat, the "hygienic promenade," the use of dumb-bells, knitting and the like are not to the purpose, because they only exercise the lower cerebral centres. It is far better to attempt the cure of habitual abnormal activity of the brain by diverting the activity into centrifugal paths, and we should therefore select some kind of work that requires constant application of the attention and can only be carried out by the primary consciousness. Both authors are correct in their advice, though they seem to contradict one another somewhat. They entirely agree that a form of muscular activity should be chosen which has some definite object in view. We should hardly think of using a treadmill, for instance, as a therapeutic agent, since its employment is aimless. Other kinds of labour may be of much greater use—even chopping up blocks of wood, for that produces tangible results.

Much attention is paid to praxi-therapeutics nowadays, especially in institutes. We have certainly here to distinguish between cases in which the patients have only to work a short

time—*e.g.*, an hour or two in the garden, and those in which the work constitutes the essential part of the treatment. The latter form of treatment plays a great part in those institutions for nervous diseases among the poor which were first erected on Benda's recommendation. The form of ergo-therapeutics employed in such establishments is that recommended by Krafft-Ebing, Eulenburg, Möbius, but more especially by Grohmann and Schwarz. Its object is to render the patient once more a useful member of society. The system is worked on economic principles, the patient's work being utilized for the benefit of the establishment, and a portion of the expenses are thereby covered. Quite recently, training patients to work has come to be looked on as an essentially remedial factor even in establishments for those who are better off. Marcinowski deserves special mention in this respect. Although great importance must be ascribed to the use of ergo-therapeutics outside such institutions, the latter afford special advantages for its employment. The supervision is very much better, provided the doctor really troubles about his patients and does not conduct the establishment as though it were merely an hotel. Monnier very properly insists that only a few patients at a time should be placed under the guidance of a specially qualified director, otherwise the treatment degenerates only too easily into a mere matter of routine. Very much depends on the personality of the director, who must understand how to permanently increase the patient's trust in himself and his confidence that his morbid symptoms are disappearing, by advice and suggestive influence. Such institutions are also specially adapted for praxi-therapeutic treatment, because good example is just as infectious as bad. A patient will be much more inclined to work when he sees others employed, and also observes how well their work makes them feel. As a rule no compulsion is necessary, and Möbius would even forbid the use of any such in an institute. It is generally quite unnecessary, because patients who are at first disinclined to work very soon follow the good example given them.

With regard to the curative value of work, views differ somewhat according to the results achieved. Although any differences in the results obtained may partly be due to the doctor, the nature of the disease also conditions such diversities. Schwarz has pointed out that better effects are produced

with nervous people than with psychopathics. Monnier has attempted to make the indications even more definite. It is specially in quite fresh cases of hysteria that imperative ideas and impulses, tendency to brooding and auto-suggestion, paræsthesiae of all kinds and the like, also the phenomena of neurasthenia, can be removed. Where the patients are weak-minded, psychopaths, or drunkards, the results depend essentially on hereditary disposition and the duration of the disease. To succeed in training such people to work has the very best influence not only on the patient himself but on his companions as well. The work-cure is not so beneficial in cases of hypochrondiasis and paranoia; in the former, restlessness and the constant desire for change, and in the latter the persecutory ideas, prevent the patients from persisting in the treatment.

Treatment by occupation is also of great economical importance to many patients, as it enables them to be trained once more for professional work. To practice a profession has a very salutary effect on a human being's health. Unfortunately, medicine has usually underrated the importance of this question of following a profession, and even when this has not been the case the matter has been judged on false premises. Physically weak people with a tendency to tuberculosis have before now been recommended to betake themselves to gardening or agriculture, regardless of the fact that these occupations make such severe calls on a person's physical strength as to nullify the advantages derived from working in the open air. Nevertheless, it must be admitted that the advisability of being engaged in some calling has for some time past obtained at least casual recognition from the standpoint of somato-therapeutics, whereas the psycho-therapeutic importance of occupation has been almost totally ignored. Forel has pointed out that numbers of people pine away because they are not allowed to choose a calling suited to their talents and inclinations. I have frequently noticed the quarrels that have arisen in this way. A doctor must always endeavour to overcome the prejudices of parents and relatives. This is often observable in the case of daughters. Many young girls desire serious occupation, but their parents, who were brought up with old-fashioned ideas and who are disinclined to adapt themselves to the ideas of the day, refuse their consent for fear of damaging the reputation and social position of the

family. The result is that a girl who is thus thwarted in her desire to engage in an occupation congenial to her mental disposition falls ill, becoming more especially hysterical and nervous, or when either of these two morbid states already exists their cure is impeded. In nearly every case the healthiest course is that of allowing a woman to occupy herself in whatever manner best imparts fulness to her life. This, of course, is not intended to run counter to the view that a happy marriage generally fulfils the object of a woman's life. But when financial, social, or other reasons prevent such a marriage taking place, or even when a married woman fails to find that marriage entirely fills her life, we have to think of some form of activity to make good the deficiency.

Naturally, these considerations in the choice of a calling do not only apply to the female sex but to the male as well, though prejudice plays a greater part in coming to a decision in the former case. Of course when a doctor has to take part in such a dispute he must make a most careful study of the individuality of the person in question. Forel, who warmly recommends professional occupation as a therapeutic measure, utters a word of warning against considering every one an unappreciated genius whose parents do not at once let him have his own way. Psychopathic persons, and especially the weak-minded and the very hysterical, are just the people who so very frequently misjudge and overrate their powers. Such people always want to imitate any person who has made a public success. One wants to be a singer, a girl to become an actress, and a third, who has perhaps been a passable business man, suddenly thinks he has a call to reform electro-technics or the policy of the country. The psycho-therapeutist must carefully distinguish between the moods of such insignificant persons and tendencies that have to be taken seriously. Individuals of the former type only look to results; they object to devoting years of arduous study to attain the end. It is different with people who are really in earnest. It is not so much the desire to pose as leaders of thought that actuates them, but rather the instinctive impulse to engage in some occupation for which they feel themselves to be naturally fitted. It often falls to the lot of a doctor to smooth their way and overcome any opposition their relatives may offer. Of course, he will not invariably be in a position to decide which of the two cases he is dealing with in any particular instance. Know-

ledge of human nature, earnest study of the individual in question, consultation with other specialists, and often a lucky shot, will lead to a correct decision, the importance of which can hardly be overrated.

We have seen that in many cases the patient himself asks for some professional occupation, and the doctor's assistance is then only necessary to overcome the opposition of his family and thereby render the way smooth. But in other cases the very thing the doctor has to do is to combat the patient's opposition to work by explaining to him that he can only maintain or regain his health by taking on some serious occupation. These considerations also apply to elderly persons whose tendency to hypochrondia and brooding can often only be overcome by their being engaged in some form of regular work. The impulse to work which this brings rejuvenates them and has a salutary effect on their disposition and health. Even the occasional cares and excitement attached to business are often far less injurious to the nervous system than doing nothing and the absence of any impulse to work. I must here refer to what I said concerning traumatic neuroses on page 349, where I pointed out that the certain prospect of a permanent income and the absence of any compulsion from without have proved injurious to many people.

Of other psycho-therapeutic measures I may mention treatment in an institute, to which reference has already been occasionally made. Hospital treatment was recommended for many affections long ago, but its field of activity has been greatly widened in recent times. It is often advisable that a patient should be admitted into an institute because many therapeutic measures can only be properly carried out there—a complicated hydro-therapeutic treatment, diet cures, operations, etc. Treatment in an institution is also often necessary on psycho-therapeutic grounds. The patient *has* to be removed from his former surroundings and relieved of his business and domestic cares. He requires relief from the injurious influence of over-anxious friends, and from everything tending to counteract the doctor's advice or upset his plans; or it may even be that it is advisable to place the patient under the doctor's continuous influence. Anything of this sort can rarely be accomplished at home, though it may very well be in a good institute. There are many other influences to be considered in this connection, and Wiedeburg has called attention to them in a

pamphlet. Sometimes a patient is benefited when he hears that he is to visit an establishment of which he has heard good accounts. Many establishments possess the advantage of a good position, the opportunities they afford for excursions, for enjoying the pleasures of nature and indulging in sports and harmless games. Work-cures, to which I have already called attention, are best carried out in institutes. But one point on which special weight must be laid, and to which attention has already been drawn, is that a doctor is better able to bring his personal influence into play inside an institute than out. The discipline that so many patients require can be best exerted by a conscientious and energetic hospital doctor. Every one, even the rich and pampered, should submit to it. It is just this compulsory subjection that proves beneficial in so many cases.

It is certainly easy to understand that every institute is not suitable for the purposes mentioned above, especially when we come to consider that the personal influence of the doctor is greater the smaller the establishment. All large institutions conducted on the lines of an hotel must therefore be rigidly excluded when it is intended that such personal influence of the medical director is to play the chief part in the treatment. In the same way, when rest is essential for the patient we must be careful not to select one of those badly-planned establishments in which the position of the rooms is calculated to endanger the patient's peace and quiet. Unfortunately, in many establishments the thoughtlessness of the servants and the constant din of music frequently disturb the patient's rest at night. It should be the duty of every doctor to avoid recommending any establishment in which a patient is likely to be annoyed and disturbed. This does not imply that large establishments are not without their advantages, especially for patients who are more in need of the stimulus of social intercourse than of the personal influence of the doctor. But even in such cases an establishment should be selected that is built and conducted in a way calculated to save the patient from any kind of annoyance. As Edinger has rightly pointed out, many a one is more benefited by a trip to the hills than by the treatment he was advised to undergo in an establishment. In the latter form of treatment special care must be taken to give prominence to the psycho-therapeutic moment. Patients who enter an institute in the hope of obtaining rest, only to find

that the whole day is occupied in exhausting hydropathic treatment, massage, etc., may easily be seriously injured by such processes. In any case, even where somatic treatment is concerned, the importance of mental influence should never be overlooked.

The psycho-therapeutic effect of treatment in an institute may often be considerably increased by strictly isolating the patient. That can hardly ever be done outside. Attempts to isolate a patient in his own home scarcely ever succeed. Even when the patient's relatives promise to do so, the doctor may assume with a degree of probability bordering on certainty that there will be no really strict isolation, and that either the curiosity or anxiety of the relatives, or else their desire to prevent the patient becoming *ennuyé*, will soon lead to his instructions being evaded. The good effect of solitary treatment, especially in cases of hysteria, has long been known. Charcot was a particularly warm advocate of the method, and he was quite right in considering isolation the working principle in the Weir-Mitchell treatment. When discussing that system Charcot called attention to the interesting historical fact that Weir had already recommended isolation for the treatment of demoniacal obsession. From the psycho-therapeutic standpoint isolation may be just as necessary for preventing unfavourable influences being brought to bear on a patient as it sometimes is from the standpoint of somato-therapeutics for safeguarding a patient from the acquisition of noxious drugs—morphia, for instance.

Of course, strict isolation is only possible in relatively speaking few cases. It also presents certain dangers: the possibility of a patient devoting too much attention to his morbid fancies calls for special consideration in cases of hysteria. But then there are contra-indications in every form of therapeusis, and they have also to be thought of in the case of treatment in an institute. It is rarely possible for a doctor entirely to prevent patients discussing their maladies, no matter how firmly he forbids it, and we can understand how injurious such conversation is to very impressionable people. How great these dangers really are is proved by the fact that when I was in Paris I often heard the *Salpêtrière* referred to as a *Fabrique d'hysterie*, notwithstanding the authority exercised there by Charcot. We must therefore be most cautious before placing an impressionable girl in an institution where she is likely

to get into conversation with hystericals, victims of fear, etc. Morphinomaniacs may also be dangerous companions, and I know of patients who, having stayed in an institute on account of some neurasthenic trouble, have there become perverted to the morphia habit. The essential thing in every institute is the spirit that pervades it, and that depends more especially on the doctor who has the post of medical director. But even where the conditions are most favourable we must not expect miracles to be worked. There are many cases in which the disappointment is all the greater because the symptoms that disappeared while the patient was being treated break out afresh when he leaves the place. A balance must be struck between the advantages and disadvantages of treatment in an institute, and it is for the doctor to find out which is the right establishment in each particular case.

Just now I made mention of the danger of mental infection occurring in institutions. It may also happen outside such establishments, especially in the consultant's waiting-room. I will take traumatic neurosis as an example. It only requires one such patient to describe his sufferings and their cause for a number of others at once to imagine they are suffering in the same way, because they have met with an accident at some time, or at least think they have. In the same way we often observe that many a waiting-room in the out-patients' department of a busy hospital, for instance, is a very hot-bed of "traumatic neuroses." Of course there are many other opportunities for morbid symptoms to be created in a similar way; we can never entirely exclude this possibility; but we are bound, as doctors, to bear it in mind and do our best to avoid the risk of any such danger arising. We are sometimes able to do this by requesting patients who, for instance, are likely to prove dangerous to others, only to visit us at times when they will find nobody in the waiting-room. Of course, we should not go so far as to attempt to prevent every possible kind of injurious influence; we are not able to do this, and it would be opposed to a wise system of psycho-hygienics.

Finally, I must mention religion as the most effective psycho-therapeutic remedy we possess. I do not mean those forms of religion that involve superstitious practices, because they may prove injurious by causing the necessary somato-therapeutic to be neglected, although they do at times have a therapeutic effect; what I really mean is the religion that

enables a patient to face even the most terrible situations with equanimity. There must always be cases in which the disease is either incurable or to an extent stationary or progressive, where the patient can no longer be buoyed up by any influence his doctor can bring to bear. In these cases religious belief is the best medicine for the patient. When a patient says, "The Lord our God must indeed love me, or he would not inflict such suffering on me," a doctor cannot but reflect, as Lœwenfeld rightly insists, that he can never provide a patient with such consolation from any other source.

I have intentionally abstained from going any farther into the indications for the employment of psycho-therapeutics. The examples I have already given show that it is as well-calculated to satisfy the *indicatio causalis* as it is to fulfil the *indicatio morbi* and the *indicatio symptomatica*. But we must be very careful in our delineation of these indications. It is frequently assumed that in cases of functional disease all that is requisite is to satisfy the *indicatio causalis* or even the *indicatio morbi*.

It is generally admitted in the present day that the use of psycho-therapeutics is more frequently indicated than was the case twenty years ago, when it was thought desirable to limit its application entirely to hysteria. There is hardly any disease in which it does not play a part. This applies as much to functional diseases as to organic ones, to curable as well as to incurable, or to stationary or progressive, acute or chronic maladies. The way in which psycho-therapeutics has come to be looked on in the present as an important factor in the treatment of exactly the severest diseases, proceeds from the recognition that the care of the sick now enjoys, the influence of which on a patient's mind is of the greatest importance. Even in surgical cases in which a superficial judge would consider psycho-therapeutics superfluous, it really plays a great part. Klaussner has called attention to this in a small pamphlet. A very essential point is how a surgeon makes his preparations for an operation; whether he selects his instrument in the presence of the patient, or whether the latter hears the cries of pain emitted by others or even sees the blood of those who have just been operated on. All these matters come within the scope of psycho-therapeutics and have been much too long neglected. Of course, the object for which a doctor employs

psycho-therapeutic remedies will be different in each particular case. Sometimes psycho-therapeutics, for example, praxi-therapeutics, will be employed to prevent the patient being conscious of his disease, sometimes to cure the disease itself. The form of mental influence to be used must be so chosen as to suit not only the nature of the disease but the patient's individuality as well. Age and sex, financial conditions and social position, nationality, temperament, character and degree of culture, religious belief, and many other facts are all determinants in the selection of the kind of influence to be employed. A simple talking-to by the doctor without any other accessory suffices in the case of a child (Comby), but it may be much more difficult to make an impression on a grown-up person. With weak-minded persons we shall have to pursue a different course to that we should take with the intelligent; but no doctor has ever any right to neglect psycho-therapeutics. From the age at which a human being becomes susceptible to mental influence down to the hour when he is lying unconscious in the death-agony, psycho-therapeutics has to be taken into account. Indeed, in respect to the latter point, I must draw special attention to the fact that a dying person is in most urgent need of the psycho-therapeutic influence of the doctor and of those at the bedside, and that those somatic influences—injections of camphor, for instance, and all the other tortures to which the dying are so often subjected—are, in numerous cases, quite opposed to the fundamental principles of psycho-therapeutics and humanity. I also take this opportunity of giving a word of warning against assuming that the death-agony is accompanied by unconsciousness in cases in which the patient may possibly still be susceptible to the influence of those around him. Solicitude for the dying person's peaceful passing (euthanasia) opens up a wide field to psycho-therapeutics. I have discussed this point in great detail in my work *Ärztliche Ethik.*

It follows from the foregoing explanations that the use of psycho-therapeutic influence is not limited solely to specialists. I must here refer the reader to what I said about medical specialists for hypnotic treatment on page 326 *et seq.* But there are certainly many cases in which a doctor who has made a speciality of psycho-therapeutics is better suited to carry out the treatment than another, but every other doctor, whether he is engaged in general practice or in some special branch of

medicine, is bound to have many opportunities for employing psycho-therapeutic influence. In many cases certain quite definite factors also play a part. Thus there are cases in which a doctor who is almost a stranger is able to exert a greater influence than one who is a friend of the patient. It is an old story that when a doctor is on intimate terms with his patient, his influence on the latter is frequently lessened, though it sometimes happens that old acquaintanceship increases the influence. Take the case of a family doctor who has attended the same family for twenty years, seen its members in happiness and in suffering, watched the children grow up, been consulted on all serious matters of health, and without whose knowledge no other doctor has ever been called in. We can well understand that such a family doctor is often able to exert the very strongest influence on such patients. On the other hand, a doctor who is a family doctor in name only, but who in reality occupies the degraded position of advertising agent to specialists, hydropathic establishments and sanatoria, can never under any circumstances be calculated to exercise such influence. It often happens that a doctor, whose reputation for special skill has preceded him and whose extensive practice forbids the devotion of sufficient time to each patient, obtains the best results; his every word seems a revelation to the patient. There are many other cases in which it is much better for a patient—a neurasthenic with all kinds of hypochondriacal troubles, for instance—to seek the advice of a doctor who can devote sufficient time to his case to go into all his complaints, and who can also direct him to some occupation and activity. But this will generally have to be a doctor who is not very busily employed.

Of course, a doctor should not bide by a mere schematic use of the psycho-therapeutic remedies that have been described, or even think that he has only to use one of them at a time. It has only been my intention to give a general sketch of the question, and I have consequently omitted many details. As a rule, a doctor will not merely combine mental with psychic treatment, but will employ several mentally curative factors simultaneously. I have already repeatedly touched on this question. When a doctor intends, for instance, to use volitional therapeutics or praxi-therapeutics he ought, almost invariably, to explain to the patient the importance of such methods in the treatment of his disease—*i.e.*, instructional therapeutics

should precede volitional therapeutics. It often happens that hypnotic treatment cannot be employed until its character has been explained to the patient. Also we are not always able to separate the action of the emotions, more especially that of expectant attention, from the action of suggestion. For this the numerous holy shrines visited by so many pilgrims, and of which Lourdes is the most famous in Europe, afford an admirable field of observation. It was at La Bonne Sainte Anne in Quebec, where sick people have resorted for more than two hundred and fifty years, and which is even now visited yearly by several thousand Catholic pilgrims, that I was able to obtain the clearest insight into the manifold nature of the influences at work. For a long time beforehand the patients are prepared for the journey and filled with hopes of its results. Intercourse with the other patients, the influence of religious exercises, especially of prayer and the impressive services of the Church, each of these produces its effect. Patients may be seen praying fervently before the numerous *ex voto* crutches offered by their lame and spectacles by their blind predecessors.

Similarly, we shall often find it necessary in scientific medicine to combine mental remedies with others.

In many cases it will be found necessary to use psychotherapeutics for preventive purposes in the form of psychohygiene. O. Binswanger rightly advises that children who are disposed to hysteria should early be subjected to a process of mental hardening. We should begin as early as possible to combat their timidity and nervous fears, and carefully avoid any but class-instruction, since it is constant emulation with companions of their own age which will best combat their morbid egoism and self-will and reduce their hyper-sensitiveness to a normal degree. Much modern agitation against over-pressure in schools may easily lead to very serious results. I have already alluded to this in my work on *Der Einfluss des grosstädtischen Lebens und des Verkehrs auf das Nerven-system* (The Influence of Public Life and Business on the Nervous System). Perpetual public discussion of the so-called over-pressure in schools must in the end enervate the pupils and diminish their powers of resistance. There are cases known to me in which children have excused their laziness under the pretext of over-pressure, the dangers of which they knew had been recognized by medical men! Without entering into the question of the injury done to education by thus undermining the respect in which the

scholars should hold their school, I for my part consider that the published accounts of such discussions, which young people only too readily read, do them on the one hand more harm by debilitating them, than they can on the other ever repair. Instead of considering external stimulus a danger and exaggerating its importance, it is a far better plan to endeavour to render children, and adults too, capable of offering resistance by accustoming them to the action of certain stimuli.

I must here point out the dangers to psycho-hygiene that generally arise from modern hygiene. It frequently happens that the good hygiene was going to do results in evil, and for this many a bacillus-hunter is to blame. Eschle reminds us that Rosenbach gave warning of this danger fifteen years ago. Every opportunity seems to be taken of harassing the public with the fear of infection; now it is books from the lending library, now combs, or knives and forks that the hygienists denounce as dangerous. But apart from the fact that these dangers are often exaggerated and frequently unavoidable, since human beings must do business with one another and each of us cannot shut himself up in his own isolation-hut, this creation of scares is very reprehensible from a psycho-hygienic point of view. It has brought about such a dread of infection as to amount to a veritable mental epidemic. Of course we ought not to ignore the teachings of hygiene, but we should never press them so far as to make it impossible for a man to touch anything or transact any business without nervously inquiring "What danger am I running into?" The care of the mind is as much the purport of a true system of hygiene as that of the body, and that is a point which our hygienists should take into consideration. Instead of so doing, some of them are much too addicted to giving the greatest publicity to the results of laboratory-research, with the result that the public is thrown into an unhealthy state of constantly dreading infection.

We must as carefully guard against any exaggeration where psycho-therapeutics is concerned as in the case of any other therapeutic measures. Finot thinks we ought to be able to prolong life; it is auto-suggestion produced by constantly thinking of death that causes people to die earlier than they should. He mentions Spitzka's observation that many people die after starving for two or three days, although investigation of the cases of fasting men like Succi and Tanner has shown that it is possible to live without food much longer than that.

Cases have, indeed, been cited in which people are said to have died because they feared that they were going to be killed. In those cases in which people quickly succumb to starvation, Finot ascribes the early death of the subjects to auto-suggestion and fear. But we should be very careful in drawing such conclusions, and it is always a great mistake to generalize from isolated observations. Experience has shown that even when the conditions are very unfavourable, as in cases of shipwreck or of explosions in mines, where the fear of death puts everything else in the background, life may be maintained for a very long time even without food. But although these facts prove that the mental influences in question have no such general tendency to shorten life as people are inclined to assume from the reports they have read of isolated cases, we ought not to fall into the error of seeing perchance in auto-suggestion an essential means of prolonging life. And further, our antagonism to exaggeration and capricious fancies should never lead us to disregard the therapeutic importance of mental influence.

It is, of course, impossible to mention all the details of psycho-therapeutics in a single chapter, and out of the question to attempt to give instruction in them. The space at our command would not suffice for the former purpose, and the latter can never be fully accomplished. But it must be said that the personality of the practitioner plays an essential part, and the characteristics that go to make a good psychotherapeutist are partly innate, partly acquired. They may be developed later on, but cannot be created. It is upon such characteristics—patience, quickness of perception, presence of mind, knowledge of human nature, power of individualizing—that much of the success obtained by laymen depends. There are personal characteristics that make a man a born psychotherapeutist. This is not merely a question of the suggestive force that emanates from them, but of the far-reaching nature of the influence they exert. This is often as impossible to analyze as many other reciprocal human relations. Certainly, suggestive influence begotten of confidence plays a great part here: but we are not always able to trace the origin of the confidence. It often happens that the confidence of new patients is due to the doctor's scientific reputation having preceded him; in others, to his successes being known. But confidence need not be due to success. A doctor often gains

a great reputation for skill without his knowing the reason. He is often told, to his astonishment, that cases he has considered failures were successes, or that little impression has been produced by what he considered a brilliant result. Patients' minds, and more especially their logic, are often the most enigmatical things a doctor has to deal with. I remember a patient whom I tried to cure of a nervous gastro-enteritic trouble, not only by means of all kinds of physical and chemical remedies but also by mental treatment. I considered the case a complete failure, and yet shortly afterwards a number of people came to me from the district in which that patient lived and begged I would help them as I had helped their neighbour. They said my treatment had cured him of neuralgia. But the man had never complained to me of neuralgia. Whether he really suffered from neuralgia in the first instance, or, as is easily conceived, imagined so later on, through some misconception, I cannot tell.

The wider a doctor's knowledge of human nature the greater his presence of mind and the better he is able to individualize —the latter is an art that is also given to but few—the greater will his psycho-therapeutic successes be. Things that apparently have no great significance become important remedies in the hands of an able doctor. Many a patient—a man, for example, suffering from the fixed idea that he is going out of his mind—requires to be told frequently by the doctor that the whole thing is merely an utterly groundless fixed idea; whilst in another case the constant repetition may prove injurious and only weaken the impression intended to be made. An occasional visit to the doctor should be insisted on in the case of many patients merely to report progress, and not for the purpose of securing fresh advice. I have thus found it a very salutary measure in many cases of alcoholism to insist on the patients paying me a regular visit on a stated day, every quarter or half-year, in order to let me know how they had been going on in the interval. The value of this procedure may also be observed in cases of sexual perversion. The sense of moral responsibility awakened in the patient by the doctor's display of confidence may act in these cases as a strong preventive against temptation. I am told that in one establishment for inebriates the patients, on obtaining their discharge, receive a small ribbon, which they pledge their word of honour to return directly they relapse into their former

drunken habits. Even such an apparently unimportant matter as wearing a bit of ribbon may exert a very great influence. The relative importance of verbal and written instruction should also be carefully borne in mind. It may at times be desirable that medical advice should be imparted in the form of a written communication. If, for instance, one wishes to induce the patient to engage in serious work, a much better result will be obtained by a letter to that effect than by any verbal instructions given in the consulting-room. Similarly, experience teaches that written instructions as to diet are carried out more implicitly than mere verbal advice on that question. In other cases, instruction by word of mouth proves more effectual. Even here, things that are apparently of no moment are really of the greatest importance. To tell one of the people who accompany the patient what the latter should do often creates a greater impression than addressing the patient himself. In short, things that appear insignificant from the standpoint of physiology and physics are often of fundamental import to the psychologist. This is the reason why every attempt hitherto made to give a physiological explanation of mental processes was foredoomed to failure. I refer the reader to what I said on this point on page 278. The more medicine takes this to heart, the better able will it be to perfect psycho-therapeutics, and this is an absolute necessity. Even if it be not possible to teach all the details of the system, every doctor should be as well acquainted with its fundamental principles as with those of other methods of treatment. When these facts come to be fully recognized, we shall achieve results utterly unattainable at a time when the medical profession neglected psychology and psycho-therapeutics.

CHAPTER X.

THE LEGAL ASPECTS OF HYPNOTISM.

Some of the old adherents of animal magnetism—Kieser, for instance, and later on Charpignon—already recognized the legal importance of the subject. Thus the commission which investigated the matter in Deslon's time, besides their official verdict, sent in a private report to the king, brought to light apparently by the Revolution, in which they point out the special dangers by which it seemed to them morality was threatened. Liébeault also thoroughly discussed this question from the standpoint of hypnotism in his book in 1866, and his explanations are very valuable even now. Gilles de la Tourette, Liégeois, and especially Forel, Lilienthal, Bentivegni, Drucker, Heberle, Loos, Reden, Bonjean, Reese, Mesnet, Neumeister and Halgan have since studied the legal side of the question.

The first point I shall consider is the relation of hypnosis to crime, and I shall, as is customary, distinguish between crimes committed on, and by, hypnotic subjects.

Of course all those crimes that can be committed on a person in a waking state are equally perpetrable on the hypnotic. But some crimes are particularly interesting in this respect, and of such I must give the first place to offences against morality. F. C. Müller supposes that the fact that but few such cases have hitherto come to the notice of the law is accounted for by loss of memory, the subject being usually unaware of them. But Forel's supposition seems to me more probable; the experimenters know quite well that the permanence of the loss of memory is not to be relied on. They are also well aware that the subject may unexpectedly remember the occurrences of former hypnoses. Liégeois, who certainly seems to have exaggerated the danger, suggested a kind of moral preventive inoculation. According to him, everybody

should be tested as to their susceptibility to deep hypnosis, and the susceptibles should have it suggested to them that no one will ever be able to hypnotize them.

From time to time a whole series of such cases have been the subject of judicial investigation. In a case published by Wolfram in 1821, a doctor was accused of having assaulted a woman during the magnetic sleep, and of having endeavoured to avoid the consequences by procuring abortion, but when brought to justice he was acquitted. Liégeois has collected in his book, *De la Suggestion*, a series of cases that occurred in France; others are to be found in Golddammer's *Archiv* for 1863, and in F. C. Müller's book, *Die Psycho-pathologie des Bewusstseins*. The number would be slightly increased if some cases of auto-somnambulism were counted among them.

I will only mention a few of the more interesting cases.

One case mentioned by Liégeois is that of a professional magnetizer of Marseilles, who, in 1853, assaulted a girl in the magnetic sleep. The experts, Coste and Broquier, with whom the well-known authorities on medical jurisprudence, Devergie and Tardieu, agreed, gave their opinion that a magnetized subject might be assaulted against her will and without her consciousness. The case of Castellan in 1865, reported by Prosper Despine, is better known. Liégeois refers the case to suggestion; Castellan must have suggested to his victim, Joséphine H., to love him, trust him, etc. But we can quite well understand what happened without referring to any special suggestive influence; it was merely a case of rape committed on a hypnotic. Castellan was condemned to twelve years' penal servitude upon the report drawn up by Roux and Auban, with whom the doctors Hériart, Paulet, and Théus were associated.

The Lévy case, in 1869, is also interesting. A dentist of Rouen, named Lévy, was charged with assaulting a girl, B., in the magnetic sleep. The case is remarkable, because the girl's mother was present and noticed nothing. Lévy had placed his dentist's chair so that what he was doing could not be seen. Brouardel gave his opinion on the case and Lévy was imprisoned for ten years. But the case certainly gave rise to much conjecture. Lévy admitted the intimacy but denied that he had hypnotized the girl: she was perfectly willing. As Krafft-Ebing remarks, "it is impossible to decide on scientific grounds whether she was hypnotized or not." Maschka also

thinks that this case was never properly cleared up, and Brouardel evinced some doubts at the trial.

The Mainone case, of which Schrenck-Notzing has given a detailed account, has recently attracted considerable attention (1901). Mainone, who advertised in the papers as a magnetopath and nature-doctor, treated a certain girl, Marie B. He was accused of inducing her to illicit intercourse while in the hypnotic state. Although all the specialists called in, including Schrenck-Notzing, expressed the opinion that the girl was an unwilling participant in the act, the jury acquitted the defendant. Obviously the weak-minded Marie B. did not strike the jury as being a particularly reliable witness, especially concerning events supposed to have taken place while she was hypnotized. Quite recently a magnetopath in Hanover was accused of assaulting two girls whom he had deprived of will-power by hypnosis. The trial, at which Bruns, Schwabe, and Delius appeared as experts, ended in the prisoner being condemned for an attempted offence against morality. In a case reported from Buda-Pesth, in 1901, a woman was said to have been seduced by being shown a photograph, with the result that she became neurotic. A few years ago a manufacturer of bicycles in Vienna was punished for seducing several girls while they were in the hypnotic state. A case of this kind occurred in Berne, in 1903, in which a nature-doctor, R., was accused of committing a number of assaults on girls. R. was convicted of hypnotizing and assaulting, while they were unconscious, girls and married women who came to him to be treated for some disease or other. Some of the witnesses in the case had been assaulted by R. years before, and when asked by the judge why they had not complained to the police at the time, replied that R. had forbidden them to do so. As a proof that such a command may retain its force for a time, at least, without the aid of hypnosis, I should like to call attention to the number of cases in which children are forbidden by servants to tell their parents certain things. Parents would often be very much surprised if they were only aware what their children know but conceal from them at a servant's bidding. Even a grown-up person sometimes feels so swayed by another that he obeys the latter's order not to disclose anything.

I was once called in as an expert in the case of a charlatan who advertised that he treated disease by suggestion and

hypnosis. He was accused of having forced several women to sleep with him. In one of the cases the examining magistrate had so strong a suspicion that the victim had been hypnotized, that he asked me at the outset of the examination to make a very careful investigation of all the circumstances of the case. After taking every point into consideration, I was only able to state that in all probability an attempt had been made to hypnotize the woman, but that I could not assert that hypnosis had been induced. The accused, who was very importunate the first time the patient visited him, had on the second occasion suddenly seized her by the shoulders, placed her hands on his shoulders, then stared hard at her, forced her towards the sofa, and finally thrown her on it. At the same time she stared at him. The whole procedure was reminiscent of fascination (*cf.* p. 73). When we consider what the accused did, and all the other facts that came out during the investigation, we can only conclude that at the most an attempt at hypnosis had been made.

There are, moreover, cases in which girls assert they have been assaulted although nothing of the kind has taken place. Some of these appear to be the result of auto-suggestion. It may have been so in a case in which the public prosecutor referred to me about a report sent in by my *locum tenens*, Dr. Hirschlaff, during my absence, and that led to the matter being discussed. A girl had told Dr. Hirschlaff that she had been hypnotized and rendered *enceinte* by some man. Hirschlaff then hypnotized her himself and was convinced from the detailed statements she made in hypnosis that there was no objective ground whatever for the charge. It is quite possible that the girl believed the charge to be true even when she was in the waking state. The case reported by William Lee Howard was probably of a similar nature. Two girls who were employed by a travelling hypnotizer in his experiments, fell into a condition of hysterical auto-hypnosis, and accused Dr. Picken of seducing them while they were in the hypnotic state. Judge Bailey applied to Howard, who came to the conclusion that the case was one of auto-suggestive self-delusion.

But there are other cases in which there is nothing of the kind last mentioned: the woman invents the hypnosis, or at least the rape, simply to hide a *faux-pas* she has made, to extort money, to make herself appear interesting, or for some

other reason. Tardieu had already seen a case of that kind in which a sixteen-year-old girl brought an obviously false charge. Another case of this nature was reported on by Ladame in Geneva in 1882. The supposed offender was acquitted, as the accusation was probably false. I have frequently seen such cases, and have found that it is not always quite easy to explain them. A case that came before the court in a town of South-west Germany not long ago was obviously difficult; a doctor was charged with hypnotizing a young girl by stroking her forehead and then behaving indecently to her. Edinger, who was called as an expert witness, admitted that the case might possibly be one of auto-suggested delusion. Schrenck-Notzing has also published a case in which a hypnotized child was supposed to have been used for immoral purposes. But Schrenck-Notzing came to the conclusion that it was a case of retro-active falsification of the memory, or perhaps even of conscious simulation. Certainly we must exercise great care before assuming that there is conscious lying on the part of the accuser in such a case. The confused notions of hypnosis and suggestion that are still so prevalent make it quite possible for a woman to mistake intense sexual excitement for hypnosis, and this appears all the more likely when we come to consider that sexual excitement, when artificially aroused, renders a girl quite as incapable of offering resistance as hypnosis or suggestion. Of course, from the human point of view we may be charitably inclined in such cases, but as experts we must rigidly distinguish between them and hypnoses. Czynski's case, tried at Munich in December 1894, belongs here. Czynski, who had studied hypnosis and animal magnetism, made the acquaintance of Baroness X. He was charged with seducing her by means of post-hypnotic suggestion. This was followed by a sham marriage ceremony performed by one of his acquaintances, a man named Wartalsky. The jury acquitted Czynski of having committed an offence against morality, but he was condemned to three years' imprisonment for his conduct in respect of the mock marriage. The opinions expressed by the experts engaged in the case were somewhat at variance. Grashey took up the standpoint that the baroness's love was not normal, but hypnotic; her love was aroused by Czynski's declaration of love, which he made while she was in hypnosis. Although Schrenck-Notzing and Preyer expressed the same view, I think that Hirt was right in ascribing very little im-

portance to hypnosis in this case. To my mind the most probable explanation is that the baroness *did* fall in love with Czynski, and that the question of hypnosis was only introduced into the case later on when it became known that the accused had devoted much time to the study of that subject. He may possibly have made use of post-hypnotic suggestion; but I do not think that was necessary, considering the terms on which the parties stood. But attention must certainly be called to the dangers of exaggeration where hypnosis is concerned, and this view is in nowise affected by the fact that the relatives of a girl who has been criminally assaulted, and very often the girl herself, are firmly convinced that she was hypnotized. Just as some strong perfume used to be considered the overpowering agent in cases of criminal assault in the train, so nowadays hypnosis is unjustly blamed; though we must at once admit that such assaults may be, and frequently have been, made during hypnosis. All I want to warn against is the tendency to lend too ready an ear to such reports.

When the facts of any such case are clear, the legal decision to be arrived at should present no difficulties. Here the following provisions of the Criminal Code would apply:—

§ 176, *Sec. 2.*

(2) Any man who has criminal connection with a female who is deprived of will-power, or who is unconscious or insane, shall be punished with penal servitude up to ten years.

§ 177 *of the Criminal Code.*

Anybody who by force or threats enhancing danger to life and limb forces a female to have criminal connection with him, or who has criminal connection with a woman whom he has deprived of will-power or rendered unconscious for his purpose, shall be punished with penal servitude.

§ 178 *of the Criminal Code.*

Should any of the acts mentioned in §§ 176 and 177 lead to the death of the injured party, the punishment shall be penal servitude for life, or for a period of not less than ten years.

These paragraphs give us the punishment that enforced illicit intercourse with a person deprived of will or in an unconscious state entails. But even if, as we have seen, there is no question of unconsciousness in hypnosis, but only of a disturbance of consciousness, and also that the deprivation of will

has its limits, we must abide by the phraseology of the Criminal Code, which differs from that of psychology. Different meanings are given to the idea "deprivation of will." Casper and Liman thought that the law intended by the term "deprivation of will" to protect persons who are mentally incapable of understanding the criminal nature of certain acts. This would include persons who are easily hypnotized. Krafft-Ebing certainly thinks that deprivation of will as mentioned in § 176, sec. 2, should be limited to those cases in which it is physically impossible for the person assaulted to offer resistance. But since a physical impediment may be of mental origin—as, for instance, would be the case in paralysis caused by suggestion—this interpretation would place many hypnotic states within the meaning of the paragraph. Nevertheless, other cases might present difficulties. The question of deprivation of will has recently been discussed in all its details by Aschaffenburg in Hoche's *Handbuch der gerichtlichen Psychiatrie.* Aschaffenburg contests Becker's definition of deprivation of will as a morbid state of mental activity which is not produced by mental disease, but in which free-will cannot be exercised in respect to certain actions (sexual misuse in this case), and he interprets the idea of deprivation of will as follows:—A woman who cannot be described as mentally diseased, but whose bodily and mental condition renders her incapable of exercising her will in respect to sexual advances, is to be considered as deprived of her will-power. At the same time, Aschaffenburg gives a definition of unconsciousness that implies that the inhibition of a person's power of resistance must depend on the state of his consciousness. At all events, there should be no difficulty in including hypnosis in the state of deprivation of will in the sense of the above-mentioned paragraphs. In those cases of deep hypnosis in which, as we have seen, there is disturbance of self-consciousness, a state of unconsciousness of the nature implied in those paragraphs would necessarily have to be admitted.

It is somewhat more difficult to decide how far sexual excitement that is artificially produced should be considered a condition of deprivation of will within the meaning of §§ 176 and 177. The above question was also discussed by me in the case I have already mentioned, in which I was called upon to express an expert opinion on a criminal assault that had apparently been committed on a girl who was hypnotized, on account of the manipulations to which the girl had been subjected by the accused, apparently for the purpose of exciting her sexually. One of the witnesses

even expressly declared that the accused had only made digital explorations for the purpose of rendering her amenable to his desires. There can be no manner of doubt that when sexual excitement has reached a certain degree the will becomes unable to resist the *libido sexualis* or prevent the *actus* itself. But it must be left to jurists to decide whether the paragraphs mentioned apply in such cases. At all events, this kind of deprivation of will has nothing to do with hypnosis.

But there are cases that seem to us offences against morality and yet are not deemed punishable according to the Criminal Code. A case of this kind actually occurred in Berlin, or at least was reported to the authorities as such. A boy aged fifteen asserted he had been hypnotized by a gentleman, who had then committed an unnatural offence on him while he was in the hypnotic state. The authorities came to me for my opinion, and I expressed my conviction that the boy was lying; apparently he had stayed away from home too long, and then had invented the yarn as an excuse for his absence. Still, such a case might really happen, and would come under paragraph 185, which deals with defamation of character. It could hardly be considered a punishable offence against morality, for § 176, sec. 3, of the Criminal Code only provides for punishment up to ten years' penal servitude in cases of unnatural offences committed on persons under fourteen years of age, and sec. 2 of that paragraph prescribes the same punishment for any one who forces a woman to illicit intercourse while she is deprived of will or in an unconscious state. It would therefore appear from the paragraphs that unnatural offences committed on hypnotized or narcotized persons who are over fourteen years of age are not punishable.

Among other punishable offences against hypnotics I may mention bodily injury, which in some cases might be caused intentionally—by post-hypnotic suggestion, for instance. We have seen that paralyses, loss of memory, etc., may be thus brought about. Jendrássik saw a case in which post-hypnotic paralysis persisted for several days. It is not exactly probable that these suggestions will ever be important from a legal point of view, and Lafforgue's supposition that a man might try to evade military service by causing a disease to be suggested to him seems to me even more improbable. At all events, the provision of the law as to intentional bodily injury would apply in such cases. It is much more likely for bodily injury to be caused by inattention to the proper precautions. Weinbaum

published a case of this kind. A "suggestor" named Welmann, who was giving a performance at Insterburg, experimented on L., a schoolboy in the first form at the local high school. Even while the performance was going on L. behaved very strangely. Insomnia followed, and in three days' time L. was found to be mentally deranged and suffering from attacks of acute mania, sense-delusions, and megalomania; and at the end of a year he was still uncured. Although Embacher, Meschede, and Weinbaum, the experts called in, came to the conclusion, with which the Court concurred unconditionally, that there was a causal connection between the experiments and the subsequent mental derangement, the defendant was acquitted, because it could not be proved that he had been guilty of negligence. Special attention was drawn to the fact that the President of the Local Government and the police authorities had sanctioned the performance, and that the defendant, therefore, had a right to consider himself justified in undertaking such experiments. On the other hand, a hypnotizer and professional healer, who used to give "suggestion-parties" at his house in Hanover, in 1905, was condemned for causing one of his subjects bodily injury through neglect. At these entertainments he had performed most unwarrantable experiments on a work-girl, suggesting among other things that a man would undress in front of her to bathe, then jump into the water and be drowned. As the girl was taken seriously ill after the experiments, the experimenter was charged and convicted.

The quack use of hypnotism also may possibly lead to injury to health. Hirschlaff has communicated numerous observations he has made in this respect. I was called as an expert witness in a criminal case in which a former railwayman who had taken to hypnotizing had apparently brought on an attack of acute mental disease in a patient whom he had hypnotized. Although there was much in favour of the view that the mental disorder was produced by hypnotizing, I was unable to state so with absolute certainty. On the strength of my report the case was not proceeded with.

It is still doubtful whether hypnotic suggestion can be used to procure abortion. Although Liébeault's experiences in this respect were negative, Laurent has reported a case in which a student hypnotized his cousin whom he had got into trouble, and succeeded in procuring abortion by suggestion.

It has also been asked (Roux-Freissineng) whether suicide might not be caused by suggestion; on theoretical grounds and from practical experience I agree with Krafft-Ebing that it would be possible provided the suggestion were adroitly made. There was a case reported from Russia a short time ago of a young married woman who had admitted to her doctor while she was in the hypnotic state that her husband had suggested to her that she should commit suicide on a certain day, and it was, moreover, to take place after her husband had heavily insured her life in his favour. Of course we need not believe this or any other story that is not properly substantiated, but at the same time we must admit that there is nothing to preclude the possibility of such a suggestion being made. A case described by Näcke under the heading "Suicide by Suggestion" was obviously somewhat different. A Parisian student fell in love with a married woman and his love was returned. He received a letter from the woman telling him to commit suicide, and he did so. Näcke has properly criticized this case, which was very imperfectly reported, and I think it very doubtful whether any suggestion was the essential cause of the suicide.

The question has also been frequently discussed whether a person might not be murdered by means of hypnosis. In one experiment Bramwell observed the pulse-rate drop to 40 beats and then rise to 150. He thinks that these limits might have been exceeded, only he was afraid of endangering the subject's life. The possibility of death being produced by hypnosis was specially discussed in connection with the case of Fräulein Ella v. Salamon, who died during hypnosis. Fräulein v. Salamon was employed by a layman named Neukomm for clairvoyant experiments in the hypnotic condition. She was to diagnose symptoms of disease, and as is well known, people hypnotized for this purpose often feel the symptoms they diagnose in others. In this case Fraulein v. Salamon had to describe the disease of a man who in her opinion was dying, and this produced a strong emotional effect on her, which, by its influence on the vasomotors and the heart, caused the brain to be emptied of its blood and death ensued. "Ella v. Salamon was a victim—and certainly neither the first nor the last victim—not of hypnosis itself, but of that truly modern mixture of notoriety-hunting and refinement of nerve-torture, of humbug and mysticism, deceit and self-deception, frivolity

and delirium, which seems utterly inseparable from all hypnotic entertainments and experiments" (Eulenburg). Another death supposed to be the result of hypnosis caused great excitement in America in 1897. A nigger boy, Spurgeon Young, died suddenly under circumstances that led the authorities to institute inquiries. It appeared that he had frequently been the subject of hypnotic experiment, and that he had latterly suffered from glycosuria or diabetes. An attempt was apparently made to establish a connection between the hypnotic experiments and the diabetes, so that the lad's death might be ascribed to hypnosis. But in spite of all the exertions of Bowen, who conducted the official inquiry, and in spite of the investigations of Clark Bell, to whom he applied to throw light on the question, the matter was never explained. No proof could be adduced that the subject's death was even probably due to hypnosis.

In the older literature of animal magnetism we also find mention made of a few cases in which serious injury to health, and even death, was ascribed to magnetizing. I have already mentioned the *crises* on p. 292. Severe cases of collapse are also frequently mentioned. In the later years of mesmerism Varges published such a case (in 1853) in which the subject suffered severe collapse, pulse imperceptible, etc., during the magnetic sleep. The case presented a certain amount of resemblance to one of Krafft-Ebing's observations in which the patient, who was fully awake, thought she had been poisoned by belladonna. A dangerous collapse resulted from this auto-suggestion and was only cured by hypnotic suggestion. Lafontaine expressly condemns the practice of magnetism by inexperienced persons, as it might lead to the production of insanity, epilepsy, paralysis, idiocy, and even to sudden death.

The hypnotic state might be used to get possession of property illegally. People can be induced hypnotically and post-hypnotically to sign promissory notes, deeds of gift, etc. I reported to the Society of Prussian Medical Officers a case of a man who in the post-hypnotic state promised a donation to the society, and carefully explained in writing that he did it of his own accord, after I had suggested to him that he should think so. Testamentary disposition might be influenced in the same way. A case of this kind occurred in England a few years ago. A lady left her doctor, who was a hypnotist, a large fortune. The will was contested on the ground that its provisions had been suggested by the doctor while the testatrix was under his hypnotic influence. But the validity of the

will was finally recognized, because it was discovered that the patient had never been hypnotized at all! I shall speak later on of the significance of such acts in civil law, when quoting Bentivegni. As far as the criminal law is concerned it would be difficult to obtain a conviction in such cases. The paragraphs dealing with fraud would probably apply in some cases, and occasionally those that treat of embezzlement—when, for instance, a certain sum of money is obtained by suggestion but is not returned—but in other cases it would be difficult to establish a punishable offence. The paragraphs that deal with coercion would hardly be applicable, although the idea of force here includes *vis compulsiva*. Many people will probably consider this a defect in the criminal law. Bérillon in France, in a note dealing with a communication of Merlier concerning the influence of "waking suggestion" on testators, even describes it as a flaw in the law that suggestion so exercised entails no punishment on the perpetrator. But if we were to make the use of every suggestive influence in life a punishable offence, we should be even worse off than we are now.

The question has also been discussed whether it would not be considered an infringement of the rights of the individual to hypnotize any one against his will. In such a case § 239 of the Criminal Code would have to be considered; it prescribes imprisonment for any one who intentionally or unlawfully confines or deprives another of his or her personal freedom. Now, a hypnotic *is* deprived of his personal freedom, and therefore in any concrete case it would have to be decided whether the unlawful action of the hypnotizer was not punishable. A case of this kind was tried in Bavaria in 1905; a fourteen-year-old boy had been used for hypnotic experiments without his parents' knowledge, but the indictment for deprivation of liberty fell through. In an earlier case that occurred at Nürnberg a similar charge was brought against a commercial traveller who had hypnotized a waitress. He also was acquitted because the court "was not satisfied that the accused had been conscious of the illegal nature of his action; he might have been of the opinion that the waitress was fully aware of the consequences of hypnosis, since he had often carried out such experiments in her presence, and she was therefore a consenting party" (Heberle).

Every medical man who has had any considerable experience in the domain of hypnosis has probably come across laymen

who endeavour to ascribe to hypnosis anything they find very peculiar, or for some reason or other unpleasant, or that they cannot quite understand. At times it is a case of seduction or a mysterious love affair, at others the provisions of a will or the exploitation of some business, that puzzles them. When any lady of rank—a princess, may be—falls in love with a man of the lower classes, there are always plenty of people ready to ascribe the episode to hypnosis, though it is precisely in such cases that ladies are led astray by the influence of sexual love. And we must make a careful distinction between cases of hypnotic or suggestive influence and those which have been so tellingly described by von Krafft-Ebing, and so aptly termed sexual bondage by him, in which an individual of the one sex becomes entirely dependent on one of the other through the influence of sexual impulse. As I have already mentioned, Hirt very properly opposed the notion that there was any question of hypnotic influence in the Czynski case. The position seems to have been somewhat similar in the case published by Preyer under the heading, *A Remarkable Case of Fascination.* A young woman who was married to a Herr v. Porta was induced by a man named Pander, who was a friend and relative of her husband, to leave the latter and blindly follow her seducer. Unless we shift the line that demarcates hypnosis, a case of this kind can only be described as analogous to hypnosis, not as hypnosis itself. When we see that many a woman is strangely affected by the foreign appearance of a man, by his broken speech and peculiar complexion, when we further observe that many women fall in love with bull-fighters and lion-tamers, that actors are dangerous to some females, and that the male sex affords analogous cases, we must be very careful how we ascribe such processes to the action of hypnosis. And the word "suggestion" must not be turned into a catch-word to be applied to any remarkable case in which influence may have been exerted. "To speak of waking suggestion in such cases would be to *do away with* the whole idea of hypnotism. There would then only remain the old experience that there are people who are easily influenced, which would have to be considered in some criminal cases but could have no psychiatric significance" (Strassmann).

Some time ago the parents of a young girl came to me because their daughter was altogether under the influence of

an unprincipled man, X., who knew how to get all her money from her. X. was not accused of sexual intimacy with the girl, but only of extorting money from her, and this the parents could only ascribe to hypnotic influence. Of course, when once such a suspicion crops up, every action, every movement of the person suspected is considered from that point of view alone. Directly the parents observed that X. was looking at their daughter they assumed he was exerting hypnotic influence. It is, of course, quite right to investigate the details of such cases, but we should not think that every striking case of influence is due to hypnosis. There are so many other forms of influence that one human being may bring to bear upon another that we must be very careful before we assume that hypnosis has been employed. And we must place even less reliance on those newspaper reports that are only written to produce a sensation. I recall a case that occurred in Paris, in 1894, in which a married woman was reported to have found her husband lying motionless at home, and it was said that a burglary had been committed by men who had first hypnotized the husband. Another case of this kind was reported in 1890: a well-known English author was said to have filched a work from a brother literary man whom he had hypnotized, and then published it as his own.

There are important differences of opinion about the offences which hypnotic subjects may be caused to commit. Liégeois thinks this danger very great, while Delbœuf, Gilles de la Tourette, Pierre Janet, Benedikt, Ballet, Foveau de Courmelles, and Kötscher deny it altogether, and others, Joire, Forel, Eulenburg and Dalley take an intermediate position. Liégeois thinks that about 4 per cent. can be influenced by criminal suggestion; this would give about 80,000 people in Berlin. Others, as already observed, deny the danger entirely. In any case we must not be too ready to believe the stories of robbery we find in the newspapers; they are written rather to "make people's flesh creep" or create a sensation than to advance science.

There is no doubt that subjects may be induced to commit all sorts of imaginary crimes in one's study. I have hardly made any such suggestions, and have small experience on the point. In the first place, the continuous repetition of the same experiment is superfluous. If the conditions of the experiment are not changed, it is useless to repeat it merely

to confirm what we already know. In the second place, these criminal suggestions are repugnant to me, although I do not believe they injure the moral sense of the subject, for the suggestion may be negatived and forgotten. Thirdly, experiments carried out in the study prove nothing, because some trace of consciousness always remains to tell the subject he is playing a comedy (Franck, Delbœuf), consequently he will offer no resistance. He will more readily try to commit a murder with a piece of paper than with a real dagger. These experiments carried out by Liégeois, Liébeault, Foureaux and others in their studies do not, therefore, prove the danger.

Certainly Liégeois has made some such experiments in all apparent earnestness, and in the presence of officers of the law, by hypnotic and post-hypnotic suggestion, and even by suggestion in the waking state. He made a girl fire a revolver, which she thought was loaded, at her mother; and another put arsenic in the drink of a relation. Delbœuf shows good reason for not considering these experiments convincing. Yet we must admit the possibility that a crime may be committed in this way, though we must be on our guard against any exaggeration in this respect. Few people are so suggestible as to accept the suggestion of a criminal act without repeated hypnotization. It is also true that many would refuse to commit a crime even after a long hypnotic training (Delbœuf). If Kahler really thought that imperative ideas produced by hypnotic suggestion resemble impulsive ideas of pathological origin, particularly on account of their violence, we cannot agree with his conclusion that post-hypnotic imperative ideas never lead to acts of violence, since pathological impulsive ideas do sometimes lead to such acts. According to Gilles de la Tourette we are specially protected from such crimes being committed by the fact that a criminal who suggested any such offence would be no more protected from discovery than if he committed the crime himself. On the other hand, Forel insists that the greatest danger is that at the time the criminal suggestion is made the subject may be induced to believe that he is acting on his own initiative, and is unaware of any constraint. Still, as most investigators assume, only people whose general moral character renders them capable of committing criminal acts could be influenced in this way. Forel, however, does not admit this unconditionally. He made various experiments for the purpose of enlightening a lawyer named Höfelt, who

was writing a dissertation on the connection between hypnotism and criminal law. In one case, for instance, he induced a hypnotic subject to fire several shots at Höfelt with a revolver that was capped but not loaded. According to Forel, the experiment was so arranged that the hypnotic could not have been conscious that he was only playing at shooting, though I think we cannot straightway accept this explanation. It was different, however, in the case of an otherwise modest girl, who was yet induced by Forel to strip to the waist in the presence of a strange man. Forel thinks that this experiment disposes of the objection that only such acts can be successfully suggested in hypnosis as are agreeable to the subject's moral disposition. I myself think that we must admit that in exceptional cases it is possible for a person to be induced to commit acts that are contrary to his disposition, but that there is not any great general danger from criminal suggestion. In any case the sphere within which such suggestion would work must necessarily be a very limited one. "We may take it as a general rule, that the criminal acts we should have to deal with here are those which demand no special aptitude on the part of the agent—no particular qualification, that is to say—nor any special preparation, nor such complicated manipulations as would involve deliberation and perspicacity, nor yet the co-operation of others" (M. Weiss).

In remarking just now that the general importance of suggestion in criminal cases did not appear to me very great, I would yet make it clear that we must carefully distinguish between its general practical importance and its significance in any special case. For the above-named reasons the general importance is slight. But it is quite another question whether hypnotic suggestion must not be taken into serious consideration in a concrete case in which, for instance, the accused person has not only been constantly hypnotized, but the hypnotizer is also known to derive considerable advantages from the crime, whilst other circumstances point to his participation. And this question we must certainly answer in the affirmative.

The question as to whether hypnotic suggestion can play a part in the commission of a crime has frequently been before the courts in recent years. To these belongs the case of Eyraud and Bompard, who were accused in 1890 of murdering an employé of the courts, named Eyraud. Sacresta, the family doctor of the Bompards, informed the court that he had frequently hypnotized the accused, and Liégeois, who was called as an expert, declared,

in opposition to the opinion expressed by Brouardel, Motet, and Ballet, that the woman Bompard had committed the crime under the influence of a hypnotic suggestion she had received from her lover Eyraud. Both of the accused were convicted. Eyraud was executed, and Bompard condemned to twenty years' penal servitude, from which she was released after serving thirteen years. After she came out of prison, Liégeois submitted her to a series of hypnotic experiments, the result of which was to strengthen the opinion he had formed in the first instance. Although she at first resisted, he finally succeeded in making Bompard, while in the hypnotic state, re-act the scenes in which the crime was originally suggested to her. The Weiss and Chambige cases also excited a considerable amount of interest. In the former, a Madame Weiss, in Algiers, attempted to poison her husband, and was condemned to twenty years' imprisonment, whereupon she committed suicide by poison. Liégeois believes this was a case of post-hypnotic suggestion received by the woman from her lover; but the possibility of this was not seriously investigated by the court. In the other case a married woman who had previously been a model wife and mother was killed by a man, Chambige, who had fallen in love with her, and who afterwards attempted suicide. Liégeois's explanation is that the woman was hypnotized by Chambige, and then by suggestion induced to forget her husband and children and fall in love with him, Chambige. Liégeois supports this argument by the defence put forward by the advocate Durier.

In another case, A. Voisin expressed the expert opinion that a certain woman accused of theft had been induced by suggestion to commit the crime, and was therefore irresponsible. It is impossible to decide from the official report of this case whether it was one of hypnotic or of waking suggestion.

Hypnotic suggestion has also often been made a pretext in other criminal cases. A few years ago a woman was charged in Liegnitz with attempting to murder her husband. Here also the possibility of hypnosis being in some way connected with the crime was discussed. It seemed at first as though it was intended to present the accused lover in the light of a victim to the hypnotic suggestions of the woman, because he had often been hypnotized, but as the case proceeded it was shown that hypnosis was only used to prove that his mental capacity was of a very low order. There was a case a short time ago in Berlin in which a magnetopath, Reichel, and a rich widow, with whom he was on intimate terms, were accused of suborning witnesses. One of the persons involved in the case expressed the opinion that the widow was under the hypnotic influence of her fellow-prisoner. In a case that occurred in Vienna, Caroline Ullrich, a former dancer, who was accused of slander, asserted that she was hypnotized by her husband and wrote the slanderous letters while she was in that state. It was this very case that showed what confused notions about hypnosis obtain in the public mind.

A few years ago I was called in as an expert in a case of this kind in Germany. A young girl, X., had fallen into the clutches of a procuress, Y., who gave spiritualistic *séances*, and who drove the girl to prostitution and crime by means of automatic (?) writing. The woman Y. used to write down the commands of the spirits, and a number of documents were impounded minutely detailing what X. was to do. The seed fell on fruitful ground, for the girl was a spiritualist and therefore believed that she was bound to do all that the spirits ordered. Y. was greatly assisted by the

artistic ambition of the girl, who had been trained as a singer. Fame, honour, and riches were promised the girl if she would only obey the spirits' commands. The girl consequently became intimate with all kinds of men, and Y. always took the money. Finally, X. took a blackmailing letter to the man who had first seduced her. That letter also was instigated by Y. Similarly, X. let herself be induced to attempt to procure abortion, to be guilty of breach of contract, etc., at the instigation of the spirits. This uncanny influence, as we must call it, that Y. exerted over the girl X., caused many people to think of hypnotic influence, particularly in respect to the spiritualistic *séances*. X.'s own father finally gave information to the police, so as to release his daughter from the overwhelming influence of the woman Y. But careful consideration of all the circumstances of the case gives no ground for considering that the girl had been hypnotized; it was rather a case of the superstitious tendencies of an obviously nervous girl being artfully utilized by a procuress for her own gain.

There are many other criminal actions in connection with which the possibility of hypnotic suggestion having played a part has been discussed. This happened some years ago in the charge of murder brought against two men in Kansas, named Gray and Macdonald. Gray, who found a certain Patton an irksome witness in a case in which he was interested, artfully induced Macdonald to murder Patton by representing the latter to be an enemy of his, Macdonald's. Macdonald was acquitted, but Gray was condemned to death. It was frequently urged at the time in the press that hypnotic suggestion had been employed, though in reality the case was of a totally different nature. The court did not accept the plea of hypnotic suggestion, but assumed that another kind of influence had been brought into play (William Hirsch).

Since we cannot unconditionally deny the possibility of a crime being brought about by hypnotic or post-hypnotic suggestion, it behoves us to consider what the legal position would be in such cases; and we must at the same time distinguish between an action carried out in hypnosis and one that is the result of post-hypnotic suggestion.

It is certainly less likely that such an act will be performed in hypnosis than post-hypnotically. But the former possibility is by no means entirely excluded. We have only to think of spiritualist mediums who in a state of trance, which is to be considered an auto-hypnosis, deceive their fellow-men, no matter whether they do so at the special suggestion of their accomplices or spontaneously. (I will, moreover, take this opportunity of adding that I am convinced that many cases of supposed trance are only simulations of that state.) Nevertheless, we have to discuss the question of a criminal action being carried out by a person who is in the hypnotic state. Par. 51 of the Criminal Code would apply here:—

An action shall not be punishable when the agent at the time he committed it was in a state of loss of consciousness, or of such a morbid disturbance of his mental faculties as to render him incapable of free volition.

According to Schwartzer, Casper, and Liman, loss of consciousness includes abnormalities of consciousness, but according to Krafft-Ebing it means abnormality of the self-consciousness. It was the intention of our law-givers to include in the idea of loss of consciousness certain conditions that cannot be straightway considered morbid disturbances of the mental activity—such as states of drunkenness, certain emotional states, somnambulic conditions, etc. (Casper, Liman, Krafft-Ebing). There would therefore not be the slightest difficulty in including hypnosis here. But the position would be different if the incriminating action were performed through the agency of hypnotic suggestion. We have already seen (p. 165 *et seq.*) that the mental states in which post-hypnotic suggestions are realized differ. Now the state is normal, now an abnormal one. Whether we consider the latter an ordinary hypnosis, or a special condition, as Liégeois, Beaunis, and Gurney do, is immaterial from the forensic point of view, since § 51 might apply at any time.

Whether § 52, which declares that an action is not punishable when the agent is driven thereto by some irresistible force, would include those cases in which a suggestion is carried out in the waking state must be left to jurists to decide, and their views as to whether irresistible force only applies to physical influence differ considerably. Olshausen has expressed himself in favour of the "physical force" view, whereas Neumeister would apply § 52 to cases of hypnotic suggestion. Krafft-Ebing had much earlier come to the conclusion that many cases of impulsive action should be considered as the effect of compulsion by irresistible force in the meaning of § 52.

I have here, and also previously (p. 166), called those states waking states in which a hypnotic suggestion was carried out when the mental state was not perceptibly abnormal, except on the one point. But I only did this to avoid making the discussion too complicated. This question was at first passed over as unimportant, but Bentivegni has recently called special attention to it. I will therefore now consider whether there is a mental state which may be called normal in spite of irregularity on one point, as would be the case when suggestions are carried out in an apparently normal state.

We will take a simple case. I say to X. in hypnosis, "When you wake you will give A. a blow in the ribs." X. wakes and performs the suggested action; and he will accept no other suggestion either before, during, or after the act. Thus it appears that he is quite normal except on the one point. But modern psychiatry, and forensic psycho-pathology in particular, say that a man cannot be mentally abnormal on one point only; they rather suppose a mental disturbance showing itself on one point, which is a symptom of general mental disturbance (Krafft-Ebing, Morel, Maudsley, etc., etc.). Therefore the state, in carrying out a post-hypnotic suggestion, would really be abnormal, though it appeared normal, as Bentivegni insists. But this author further thinks that this certainly cannot be supposed in all cases of post-hypnotic suggestion, otherwise we should be obliged to think every man who accepted a therapeutic post-hypnotic suggestion was in an abnormal state when he carried it out. Take the following case, for instance: Y. is hypnotized in my very warm room, and I tell him to say in half an hour, "Your room is frightfully hot." Now supposing it is really hot in my room, the carrying out of this post-hypnotic suggestion would by no means suffice to prove that the subject had again fallen into an abnormal state.

The question we have now to consider is how we are to decide whether the subject is in a normal or an abnormal state. A diagnostic point is difficult to find, but it seems to me that Bentivegni's is the only one we have to guide us, although it at times depends on subjective distinctions. He says, "The state while carrying out a post-hypnotic suggestion can only be thought normal when the motive force developed by the suggestion is such as can also be explained by the normal disposition of the subject, and when it is not so opposed to reality that the normal individual would at once discover and correct it." According to the last clause, post-hypnotic sense-delusions without a renewed state of suggestibility would at once prove an abnormal mental state, and particularly so in those cases of sense-delusion to which Bergmann ascribes a physiological and not a pathological character. An abnormal state of consciousness would also have to be assumed for the carrying out of numerous post-hypnotic acts, but not for all, even when there is no renewed state of suggestibility. Let us, for instance, consider the two cases mentioned above. One post-hypnotic suggestion was that X. should give A. a blow in the ribs. Let us suppose that X. is a peaceful man and not ill-disposed towards A.; then the motive of X.'s act would be inexplicable from his normal disposition; consequently, according to Bentivegni, his post-hypnotic state would be abnormal. Y.'s remark about the heat was different. It was a natural remark, supposing that the room was really warm. Consequently, we have no reason to conclude that the carrying out of a post-hypnotic act presupposes a generally abnormal state. In many cases the question is no doubt difficult to decide, because "normal disposition" is hard to define. However, Bentivegni has at least given us an essential point of view from which these post-hypnotic suggestions may be judged, and one which in many cases will enable us to decide whether the subject's mental state is normal or abnormal.

Having learned how to judge post-hypnotic states in which there are no signs of a fresh hypnosis, we will return to the

criminal importance of such cases. We have seen that post-hypnotic crimes may be committed in an apparently normal state, but that § 52 of the Criminal Code would not save such persons from punishment, although they were driven to commit the acts by some irresistible power. But according to Bentivegni's explanations, we might perhaps even here assume a state of loss of consciousness within the meaning of § 51, but then we should have to make it quite clear that we were giving a wider meaning to the idea contained in the paragraph than was originally intended. Still, from a practical point of view such an interpretation of the paragraph is a rational one.

Desjardins, in France, expresses the opinion that any person who commits a crime by hypnotic or post-hypnotic suggestion is punishable, because he might have seen the possibility of such a suggestion. Yet according to Lilienthal such a position is quite untenable in Germany. It would certainly be quite contrary to the whole spirit of the German Criminal Code to punish a person for an act committed when he was in a state of irresponsibility and without intention.

The case would be different if the subject had caused the criminal act to be suggested to him in hypnosis, perhaps with the view of carrying it out more courageously. Lilienthal thinks that in this case the hypnotic would be punishable. The power of self-determination would be normal at the moment of decision. The induction of the hypnosis would be the cause of the act, and therefore the hypnotic ought to be punished.

Campili, who has thoroughly discussed the different legal questions connected with hypnosis, distinguishes between the standpoints of two schools—the classical and the anthropological.[1] According to the former, there is no guilt in the last-mentioned case, as there can be no reflection when the crime is committed; according to the latter, the criminal must be punished because such people as he are dangerous to society.

Of course, every one who accepts a criminal suggestion in hypnosis and then carries it out would not receive the benefit

[1] These are the names of the two schools of criminology in Italy; the classical recognizes freedom of will, and the anthropological does not. However, the last-named also agrees to the punishment of the criminal; but only because he is dangerous to society, not because his will is free when he commits the offence.

of §§ 51 and 52 of the Criminal Law. It would be essential that the depth of the hypnosis and the suggestibility of the subject satisfied the relative conditions contained in § 51, which demand the exclusion of free volition, or a morbid disturbance of the mental activity if the action is to go unpunished. A light state of hypnosis would in nowise satisfy those conditions; and similarly, when applying § 52, we should have to consider whether the power was really irresistible.

In the foregoing I have discussed the question whether and to what extent a subject is responsible who commits an offence either in hypnosis or from post-hypnotic suggestion. Another question to be considered concerns the responsibility of the person who makes the suggestion. The legal decision of this question would depend upon whether the subject were responsible or not according to either § 51 or § 52. If the subject is not responsible, the person who made the suggestion would have to be considered the agent. Any person who causes a lunatic to commit a crime is considered the actual agent (Liszt) and not mere inciter, because a lunatic, being irresponsible, cannot commit a crime. It would be the same, of course, with a subject who commits a suggested crime either in hypnosis or post-hypnotically, and is not considered answerable. It is only when the subject who commits the crime is punishable—as, for instance, when the hypnosis is not deep enough, or the suggestibility insufficient, to exclude responsibility—that the person who made the suggestion would have to be considered merely the instigator of the crime (Preyer, Krafft-Ebing, Neumeister). Therefore any person who orders a hypnotic who is still in a condition of responsibility to take anything away from a third party would, if the theft were carried out, be punishable as the instigator of the crime. Further, as Preyer has pointed out, § 49A of the Criminal Code would probably be applicable; according to it, it is a punishable offence to instigate the commission of a crime.

The importance of hypnosis in civil law was not seriously considered at first. Most investigators passed it over, supposing that hypnosis could only be important in criminal law. However, Bentivegni has put forward the contrary in his work, *Die Hypnose und ihre zivilrechtliche Bedeutung* (Leipsic, 1890), and Drucker has also expressed a similar opinion. I know from my own experience that it is no uncommon thing for the

loser in a doubtful legal transaction to ask for a written expert opinion to the effect that his signature, etc., was obtained by hypnotic suggestion. The main points of what follows are on the lines of Bentivegni's work, which touches the root of the matter even in the present day, although it appeared before the civil law was codified. Bentivegni, in discussing hypnotism in its relation to civil law, distinguishes between responsibility in business and liability for damages. The first means such a degree of freedom of will as is necessary for the transaction of business in connection with legal affairs. Liability for damages means that degree of freedom of will which causes responsibility for unlawful acts.

Responsibility in business is dealt with in § 105 sec. 2 of the Civil Code:—

> A declaratory act or transaction is also invalid if effected while in a state of loss of consciousness or of temporary disturbance of mental activity.

According to Bentivegni, a state of hypnotic suggestibility is enough to exclude responsibility in business; but this certainly pre-supposes a certain depth of hypnosis. Bentivegni also points out that not only such acts as are carried out through hypnotic suggestion are invalid, but that the mere existence of hypnotic suggestibility is enough under some circumstances to exclude business responsibility, even when the acts are not suggested.

Bentivegni thinks the same about many post-hypnotic states in which anything of the nature of a post-hypnotic suggestion is carried out. Here he makes several distinctions. All transactions are invalid which are effected in a post-hypnotic state in which there is renewed suggestibility. We may certainly agree with this by reason of § 105 sec. 2 of the Civil Code. Also, according to Bentivegni, the state during the carrying out of a post-hypnotic suggestion, if it is united with forgetfulness of the post-hypnotic act, excludes responsibility, provided the state shows no signs of suggestibility. Certainly there are distinctions to be made here. We saw (p. 166) that a person may be apparently awake and yet carry out a post-hypnotic suggestion without remarking it, without falling into a new hypnosis, and calmly talking meanwhile. Whether such post-hypnotic suggestions affect business responsibility is decided by Bentivegni according to the nature of the

suggestion. When the post-hypnotic suggestion is merely a movement or action, which the subject often does automatically at other times, there is no reason to question the responsibility. There are people who have a habit of scribbling on paper. Now, according to Bentivegni, if a person does this post-hypnotically he is not unfit for business. But he is unfit when he does post-hypnotically what he would not do under normal circumstances. Bentivegni thinks that when the post-hypnotic act is done without renewed suggestibility and without loss of memory, the question becomes very difficult. He thinks that in such a case all depends on the nature of the suggestion. Are the suggested acts, and their possible motives, of such a nature as to be willingly received into the consciousness of the subject and to be generally compatible with the context of his consciousness, or not? Bentivegni gives the two following examples:—1. A. owes B. £25, but has forgotten it; in hypnosis he is told to pay B. the money at the first opportunity, which he does post-hypnotically. 2. C., who is not in good circumstances, is told in hypnosis to make a present of his whole personal property to D., whom he does not like. He wakes, and the idea occurs to him when he sees D.; he resists at first, but finally formally obeys the order. According to Bentivegni, in example 1. neither responsibility for the particular act nor the capacity for business in general need be doubted, because the suggestion was acceptable to the motives pre-existing in the subject's consciousness. But in example 2. there is a difference; here there must be a revolution in the subject's consciousness before he will obey a suggestion so contrary to his interests. Therefore, Bentivegni thinks the responsibility is doubtful, at least as far as the single act is concerned.

In other cases the business incapacity is much more extensive, because delusive ideas may be post-hypnotically suggested, which may cause incapacity for business so long as they last, in the same way as do the delusions of the insane. Bentivegni thinks it should be provisionally supposed that a subject who is under the influence of a post-hypnotically suggested idea must be considered unfit for business when this idea is of such a kind that its spontaneous recurrence would partially or wholly do away with his responsibility.

Finally, besides the post-hypnotic suggestions which do not interfere with consciousness, and those which alter conscious-

ness, as insane ideas do, Bentivegni discusses a third category of suggestions. For example, a subject might be told in hypnosis that a particular engraving was an oil-painting. In such a case the error, the inability to perceive the real facts, would have to be considered from the legal point of view—*i.e.*, as laid down in § 119 of the Civil Code, which allows the validity of a statement to be disputed by the person who made it, if there be a presumption that he did so when he was not fully aware of the nature of the case. It is also possible that § 123, which deals with intentional malicious deception, might apply.

But, as a general rule, the objection in such a case would have to be based on the above-mentioned § 105 sec. 2 of the Civil Code. Only, let it be remembered, this clause of the law must be intelligently interpreted. For even if it only treats in a general way of a state of unconsciousness or of disturbance of the mental activity, and not of one that excludes free volition, we must remember—and this is a point referred to by the most different interpreters of the law—its provisions must be considered in conjunction with other laws bearing on the question, and consequently an infinitesimal disturbance of the mental activity, such as may, indeed, be brought about by post-hypnotic suggestion or some other occurrence, does not straightway render a declaratory act void.

Besides capacity for business, Bentivegni discusses liability for damages. This implies an illegal act committed in a responsible state, for which the civil law prescribes indemnification. But § 827 of the Civil Code states:—

> A person who in a state of unconsciousness or in a state of morbid disturbance of mental activity that precludes free volition causes injury to another, is not responsible for the injury. Should the person have placed himself temporarily in a state of this nature by the use of spirituous drinks or similar means, then he is responsible for any damage done by him while in this state, as he would be for an act of negligence; there is no responsibility when the state was not of his own creating.

The conclusions drawn above consequently hold good for acts entailing liability for damages, but, naturally, all the special provisions of the code have to be considered as well. For instance, § 829 also under certain circumstances holds a person liable for damages, who, while in a state of unconsciousness or in such a state of morbid disturbance of his mental activity as precludes free volition, yet causes damage. At all events, there is otherwise no liability for damages for acts done in

hypnosis, provided the hypnosis is deep enough. The same holds good for post-hypnotic states in which, on the grounds already mentioned, there is no capacity for business. On the other hand, any man who causes himself to be hypnotized only that he may not be responsible for his misdeeds, must make reparation for every damage. Bentivegni has pointed out that the Prussian common law contained clauses applicable in such cases, and the new Civil Code makes no exceptions, as may be seen from § 827, cited above. Ernst Schultze has pointed out that in its original form the paragraph only states that a man who got drunk intentionally would be liable for damage committed by him while in that condition However, Mendel, Liszt, and Schmidt had already advised that it would not be right to limit the provisions of the clause to the effects of alcohol, and Liszt, in particular, had proposed the same treatment for any one acting in the hypnotic state or while under the influence of any kind of narcotic, since omission may just as well be the object of an illegal action as commission. And this is the conclusion which has been arrived at in the present day. Therefore, any man who lets himself be hypnotized so that he may injure another must make reparation for the damage.

Just as the common law formerly held the hypnotizer responsible in certain circumstances for the damage done by the hypnotic in hypnosis or during the states following it, so in the present day would those clauses of the Civil Code that deal with illegal acts. Bentivegni recognized in a strict application of the provisions of the common law an effective preventive of those improprieties which might arise from frivolous hypnotization or the possible employment of hypnosis as a society pastime, and we may say the same for the Civil Code.

Of course, I have been unable to enter into detail on all points; but I have discussed the most important questions from the point of view of the civil law in connection with Bentivegni's *Die Hypnose und ihre zivilrechtliche Bedeutung*, which treats the subject exhaustively, and is still, in spite of the new Civil Code, an illuminating treatise on all points in question.

There is another direction in which hypnosis might be of importance in law; it can be used to falsify testimony. By means of retro-active hallucinations, which we discussed in an

earlier chapter, subjects can be made to believe, even after they wake, that they have witnessed certain scenes or perhaps even crimes. At any rate, there is no theoretical bar to hypnosis being some day used for the purpose of procuring false testimony. Nevertheless, there is no need to overrate this danger, which, indeed, is open to the very same objections as were raised against the employment of hypnotics to commit crimes. It is difficult to conceive a case in which a deeply hypnotized subject would prove a very valuable witness for the hypnotizer in a court of law. And here also we must particularly bear in mind that some detail might easily be omitted when the witness is receiving his instructions during hypnosis, and then the whole fabric would fall to pieces during the hearing in court. But even if the direct importance of hypnotism in influencing testimony is not very great, hypnotism has acquired the very greatest significance as an indirect agent from this point of view. The retro-active hallucinations induced by hypnosis certainly led to the investigation of the question whether it is not possible to bring about falsification of memory without hypnosis, and Bernheim, who was first in the field, proved, about twenty years ago, how easy it is to do so. In this way people can be made to believe that they have witnessed certain episodes—thefts, for example, which only existed in their own imagination.

Bernheim, also, pointed out long ago what precautions should be taken to prevent a judge accepting evidence procured by suggestion. He proposed that witnesses should be tested as to their suggestibility, and that, too, by attempting—of course without hypnosis—to suggest an answer the inaccuracy of which could be easily demonstrated. If it should appear that a witness was readily susceptible to such suggestions, then the judge should be very cautious in accepting his testimony. This advice has received the support of Forel; but although it may appear self-evident to many people, any one who has been in the habit of attending the courts will admit it is seldom followed in the cut-and-dried manner in which most evidence is treated. As soon as the retro-active suggestions produced by hypnosis became known, many other authors called attention to similar phenomena in daily life; it is true that the existence of such phenomena was already known, but they had not as a rule been properly appreciated. Lilienthal referred to the training of witnesses, Forel to the management

of the different parties in a lawsuit by counsel to a process that is analogous to retro-active suggestion in hypnosis. Max Dessoir expressed a similar view. Bernheim and Motet compared Moritz Scharf, the chief witness in the Tisza-Ezlár lawsuit, to a hypnotized person. When he declared he saw his father kill the Christian children he must have been suffering from a falsification of memory resulting from the influence that had been brought to bear on him. Daily life, also, provides us with similar phenomena in other respects. Two parties often assert the exact opposite in ordinary life in perfectly good faith. A well-known proverb says, "The wish is father to the thought." Each party imagines what he wishes, and a gradually-developed delusion of memory is the consequence. We must here include those people who keep on telling a lie until they finally believe it to be the truth. Every time the lie is told the mental image connected with it is reproduced afresh, and the more frequently this occurs, the more vivid does the image become. Similarly there are people in whom we can induce falsification of memory while they are awake, and without their ever having been hypnotized. It is only necessary to impress upon them that some event was really an experience of their own, and they immediately become unable to distinguish truth from fiction. It was particularly pointed out long ago by many authors—Motet, Bézy, Bourdin, Liégeois, for instance—that children are unreliable witnesses. The same was observed of the hysterical long before the evolution of modern hypnotism. But it was the latter that first showed how carefully we must take retro-active suggestion into account, and that much that used often—certainly not always—to be put down to conscious lying, is really attributable to falsification of memory. This is sometimes due to auto-suggestion, sometimes to external suggestion. Certainly the psychological processes leading to false testimony are often much more complicated. Hans Gross, Möller and others have called attention to the errors of perception which have to be considered in connection with false testimony, but which are underestimated in favour of errors of memory. There may even be real delusions. This is best shown in the case of those people whom Delbrück describes as mentally abnormal swindlers. In his work, *Die Pathologische Lüge und die psychisch abnormen Schwindler*, Delbrück deals very thoroughly with such pathological cases, and Kraepelin has also discussed the question in his *Lehrbuch*

der Psychiatrie. Henneberg, Köppen, Hinrichsen, Fritz Hartmann, and others have thrown further light on the question by their analyses of cases.

As the name indicates, pathological lying is a morbid process, but the investigations of other seekers have shown that there is also an immense amount of involuntary lying perpetrated by normal, healthy people. A number of works on this question have appeared during the last few years, partly in scientific journals and partly also in the daily press, in which the question of witnesses being influenced by suggestion has frequently been discussed in connection with some sensational trial. But it is particularly the *Archiv für Kriminalanthropologie* (edited by Hanns Gross) and the *Beiträge zur Psychologie der Aussage* that have contained a number of papers dealing with some special items of the question, as, for instance, the differences arising from age and sex, the influence of the degree of culture attained, the way in which the impression affects the trustworthiness of the testimony, etc. The psychology of testimony has become a very wide field of inquiry for the investigator. Binet, Henry, L. W. Stern, Wreschner, Placzek, Jaffa, Lobsien, Lipmann, Heilberg, E. Bernheim, Hanns Gross, Minnemann, L. W. Weber, Bogdanoff, Stooss, Cramer, Plüschke, Marie Borst, Schneickert, Radbruch, Agahd, R. Sommer, Rodenwaldt, Siemens, Claparède, Schott, Gmelin, Rauschburg, and a number of other investigators have contributed materials to the construction of the fabric. Many of these works deal very particularly with the evidence given by children, and I must not miss this opportunity of mentioning the valuable investigations made by the Berlin Society of Child-Psychology at the request of Kemsies. The lying and the testimony of children have been made the subject of special investigation. Special mention must here be made of Kemsies himself, of Piper, Marcinowski, K. L. Schaefer, and again, quite particularly, of O. Lipmann. These works have laid the foundation of great distrust in evidence. Let us hope that Sommer may be right in his assumption that the minute analysis of each individual case will enable us to recognize, in the apparently confused mass of false testimony, that which is psychologically legitimate and thus to create a critical diagnostic.

I take this opportunity of correcting an historical error that has recently been creeping more and more into Germany. Representations are made

which would lead me to believe that Binet and L. W. Stern were the first to make experiments of this kind in the psychology of testimony. Thus O. Lipmann states:—"The question whether and in what respect the context of a reproduced idea differs from the corresponding percept is no new problem in psychology. Yet these investigations referred almost invariably to experiences of colour, sound, and the like, and are consequently as good as inapplicable to daily life, in which we certainly have invariably to operate with manifold and complicated memory-images. Binet alone once attempted to institute such experiments in a way that might not 'too far remove them from the actualities of life' ('lebensfern')—*i.e.*, so that the results obtained might be to a certain extent applicable to practical life." Lipmann mentions Stern as a second originator in this respect. He goes still further and says:—"This problem of testimony is, as we see, first and foremost a question for the jurist and the historian, and Stern was the first to point this out thoroughly." L. W. Stern himself commences his work *Zur Psychologie der Aussage*, 1902, with the words, "It is to a problem in applied psychology that this dissertation is intended to be a first contribution." All these historical statements are in so far false that a number of investigators had made experiments relating to the psychology of testimony long before Binet and Stern did so. Stern's work appeared in 1902, whereas the works I refer to appeared in the eighties and nineties of the previous century. Not only were such experiments made in those days, but their application in the administration of justice was thoroughly discussed at the time. There were investigators in the domain of hypnotism who made such experiments long before Stern. It is true that hypnotism was their starting-point, but they very soon recognized that very similar falsifications of memory could be produced without calling in the aid of hypnosis. Thus, Bernheim, of Nancy, had already published in his book on suggestion that appeared in 1886 several experiments of that kind which he had been induced to undertake by the Tisza-Eszlár trial. Even as early as that he and Liégeois discovered that many very complicated scenes could be suggested to subjects as their own personal experiences, even without the induction of hypnosis. Among the other investigators who at that time worked in this field I may further mention Bérillon (*Revue de l'Hypnotisme*, 1890-92) and Joire (1896). Also, at the Congress of Criminal Anthropology held at Geneva in 1891, Bérillon had brought forward a proposition to the effect that in legal investigations a contradictory procedure should be followed instead of the present secret method, so as to prevent as far as possible the testimony delivered being influenced by unconscious suggestion. In consequence of the researches that had already been made in hypnotism and suggestion, a man, who was tried at the Aisnes Assizes in 1892 for murder and attempted murder, was acquitted, because the Court held that suggestion had a great deal to do with the evidence. Consequently this field of research is not quite so new as Lipmann and Stern represent. If they had only looked through the literature of hypnotism and the back numbers of the *Revue de l'Hypnotisme*, they would have straightway discovered that a great number of experiments had already been made in respect to the question of falsification of memory without the use of hypnosis. It is certain that these falsifications of memory came about in two ways; in some cases the retro-active suggestion caused the subjects to relate almost spontaneously as personal experience of their own something that had never occurred, but in others the falsifica-

tion only occurred when some suggestive question had been put to the subject. We must always bear in mind that a sharp distinction has to be made between an untrue answer that is the result of suggestion and a falsification of memory.

The foregoing remarks were not only necessary in the interests of historical truth and justice, but also because they show the inner connection that subsists between researches in psychology and in hypnotism. It was also probably no mere accident that led Binet, to whom modern German investigators gave a certain degree of priority, to study hypnotic suggestion for a long time before he took up experiments in suggestion on children in the waking state. May he not have been led thereto by his studies in hypnosis? Schrenck-Notzing, who proved the influence of suggestion on the evidence in Berchtold's trial, also began with hypnotism.

The correction set forth in the above paragraphs is also instructive in another respect. It shows that professional psychologists were not the first to work at a certain important province of psychological research; it was medical men, more particularly Bernheim and Bérillon, who made investigations in it long before professional psychologists did so. This affords a fresh proof of the fact recognized by Hellpach that modern psychology has received great encouragement from medical men.

The foregoing explanations have not been given for the purpose of minimizing the services of modern investigators, those of Stern and Lipmann in particular; they are important enough, even if it has been proved that the problem was most industriously studied before they took it up. These modern investigators have attempted to answer many a question which has not been dealt with by investigators of hypnotism. They endeavoured to establish as sharp a distinction as possible between spontaneous and artificial falsification of memory. But it proved impossible to do this thoroughly in all cases, for there is a gradual transition from the one form to the other and *vice versâ*, just as there is with auto-suggestion and external suggestion. Modern investigators have also distinguished better between errors of memory and errors of perception, and their numerous experiments have contributed greatly to our knowledge of the whole question. Only we must deny that they have any claim to priority in this field of research—no priority even in the practical application to daily life of the results obtained, much less where jurisprudence is concerned. It is therefore all the more necessary to remember that modern research has led to no essential, practical conclusions which had not been drawn by the investigators of hypnotism ten or twenty years ago. The way in which these facts can be utilized is to make us careful how we accept evidence, especially that given by children, hystericals, and even normal individuals. The practical conclusions that are occasionally drawn go much too far and depend upon a hasty estimate of insufficient *data*. But quite apart from this, many of the conclusions of modern investigators can only be considered precipitate. Specht has very properly denounced modern investigators for their lack of system in arranging experiments, for their misinterpretation of results, and for their too hasty deduction of important conclusions from results that are, at all events, partially hypothetical. I myself once had occasion during a discussion on Lipmann's experiments to point out that he had altogether ignored one of the chief points in connection with children's evidence—viz., the question already raised whether the false answer was given by the children themselves, or

they were the victims of a wrong idea induced by suggestive questions. A rigid distinction must be made between these two points. And no matter how difficult it may be to arrive at a decision, the rigid distinction between the two will always be a chief psychological factor.

I now come to the question whether hypnotism can be made practically useful in a court of justice. No matter how remote this question may appear, it is possible for it to gain importance every day. Indeed this has already been the case on several occasions. Is hypnotization in a court of justice allowable at present? Certainly, under some conditions and for some purposes (Lilienthal).

It is interesting to note that as early as 1869 Erckmann-Chatrian, in their play *Le Juif Polonais*, represented hypnosis in a law court, in order to bring the murderer of a Polish Jew to confession; the authorities allowed this experiment to be crowned with success. Schrenck-Notzing mentions a statement made by Wissmann, according to which accused persons in the country of the Muschilangs are brought to confession in the following manner:—The accused is narcotized by being made to smoke a pipe of haschisch, and then, while he is in that state, a confession is elicited from him or else forced on him by suggestion.

The question occasionally asked, whether hypnotism may be used to obtain from the accused or from witnesses testimony which they decline to give in the waking state, must, according to German criminal procedure, certainly be answered in the negative. This will not cause any great gap in the proof adduced, since such a proceeding has no great practical value. As I explained on page 54 *et seq.*, it is only under certain conditions that people can be hypnotized against their will; and it is not probable that these favourable conditions would be present in the case of an accused person, or a witness addicted to lying. But apart from this it is a mistake to suppose that a hypnotic subject straightway lets out his secrets. This supposition is always supported by a few well-known cases; for instance, that of Giraud-Teulon and Demarquay, who had to wake a lady out of hypnosis because she began to tell secrets; and a similar case of a magnetized lady is related by Brierre de Boismont. Though I do not contest the truth of these cases, I must insist that spontaneous chattering is a rare phenomenon. The hypnotized subject keeps his individuality, and he is silent on matters he does not choose to discuss. A further question, whether the hypnotic subject can be induced to tell by suggestion, must be answered in the affirmative in a

few cases. I have hardly a personal experience in this direction. I once observed a case of lock-jaw when the subject feared some word would escape him. The spasm was so strong that it was impossible to end it artificially.

Lichtenstädt declared in 1816 that he knew of no case of somnambulism in which indiscreet questions were answered. Delbœuf goes further, and says that when a hypnotic betrays what he wants to conceal, the apparent confession is false. A woman who confesses infidelity under hypnosis, but denies it in the waking state, Delbœuf would certainly regard as faithful. At all events, it is a fact that when such questions are put in hypnosis only the answer is easily suggested, not the betrayal of a real secret. According to Danillo, the statements made by hypnotic subjects are so untrustworthy that he would on principle refuse their being admitted in a court of justice.

It is much easier to attain the end in a circuitous way than by simple suggestion; by suggesting a false premise, for example, as I have mentioned on p. 151. Let the subject be told, for instance, that some person is present in whom he would confide, or that the people he does not wish to tell are absent. But all such statements must invariably be received with caution. For it is perfectly certain that hypnotized people can tell falsehoods as well as if they were awake and that subtle webs of falsehood are invented in hypnosis. Lombroso tried in one case to obtain a confession of a crime that had been proved, though the subject had always denied it. The attempt was useless; the subject told the same tissue of lies as when awake. Delbœuf, Algeri, and Laurent relate similar experiences. In any case, a statement made in hypnosis must be received with caution; it might be an indication, but never a proof.

Du Prel relates the case of a man named K., who was arrested on a charge of murder. There was another prisoner in the same cell, and K., who talked in his sleep, described the murder, and stated he had committed it, but when he awoke he declared he knew nothing of what had occurred. The other prisoner was then induced to question K., while asleep, as to all the details of the crime. K. complied. But he was acquitted of the crime of robbery attended by murder, because the confession he made in his sleep could not be used against him. Howard has reported several American cases in which confession made in hypnosis was used to obtain conviction.

A case of this kind occurred in Indianapolis; in another case that happened in California, Gardener was induced to hypnotize a man accused of murder, and is said to have obtained a confession of the murder in that way.

I must take this opportunity of remarking that the rest of Du Prel's statements in his work on hypnotic crimes will not bear serious criticism. He proposed to employ clairvoyant somnambulists in the detection of all kinds of crime, simply because he believes in any and every occult phenomenon. Kron justly thinks that such views indicate a golden age of criminology.

Interested by Max Dessoir's experiments in automatic writing, I tried to arrive at results in the same way with a subject whose consent I had previously obtained. I put a pencil into his hand with the point on a sheet of paper, and ordered him to answer certain questions, but not to write purposely. The subject answered every question in writing, and betrayed every secret. In this way he told me many family secrets without knowing it or wishing it.

Another way in which hypnosis might possibly be used in law, would be to decide whether a person were hypnotizable or not, or to obtain a statement which the accused or the witnesses cannot give in the waking state. Such a case may occur, and has already been of practical importance.

Such statements in hypnosis would be valuable because subjects remember in later hypnoses all that has occurred in earlier ones. Now, if it is suspected that the subject has been the victim or the instrument of a crime which he forgets in the waking state, it is evident that hypnotism should be used when there is no other means of clearing up the case.

But according to Lilienthal there is certainly a legal limitation here. He thinks an accused person or witness may be hypnotized if he consents. But hypnotization is only permissible to confirm the fact of hypnotizability, whereas a judicial examination in hypnosis is illegal. Lilienthal thinks that a deposition made in hypnosis is inadmissible, because the testimony of an unsworn witness is only allowed in certain cases, and an oath could not be administered to a hypnotized subject, and it would also be impossible to make him swear to his statement after waking. The statements of an accused person in hypnosis are likewise inadmissible, because he should not be compelled to make statements against his will.

But I certainly think that Lilienthal's view must be essentially modified. In the first place as far as the judicial examination of a hypnotized witness is concerned, the exceptional circumstances in which the testimony of unsworn persons is allowed are mentioned in § 56 of the Rules of Criminal Procedure. But as the case of the hypnotized person is not mentioned therein, it would appear that his evidence is actually excluded. But in reality the position is somewhat different. Certainly every witness should be sworn, whether the decision is based on unsworn testimony or not. But as Dalcke has specially pointed out, the High Court has not been invariably consistent in this respect; it has on more than one occasion only granted a fresh trial because the verdict was based on unsworn testimony. It is therefore quite possible for a hypnotic witness to be examined without being sworn, provided the verdict is not made to depend on his statements. But in addition to this there are other points to be considered. A case may be so thoroughly cleared up by the hypnotization of a witness at the preliminary investigation that no charge is preferred. But at a preliminary investigation the oath may only be administered under very exceptional circumstances. Finally, specialists when giving evidence at a trial are oftened questioned on matters which are not the special subject of the trial. It is therefore quite conceivable that a specialist may make hypnotic experiments before the trial and subsequently relate the results in the witness-box. In the case already mentioned, in which Hirschlaff sent in a written opinion in the same terms he had used at the preliminary investigation, the question was one of a woman who had made statements to him in hypnosis. There is certainly the possibility of the statements of a hypnotized witness being worked into the evidence in a circuitous way.

As far as the accused is concerned, I certainly think that Lilienthal has overlooked the fact that an accused person cannot be said to testify against his will when he asks to be hypnotized so that he may give evidence that he cannot give in the waking state. At most this would be testimony given without the subject's conscious will, but not against his will. Whether there are any legal objections to this I cannot venture to decide, but I wish to point out that according to § 242 of the Rules of Criminal Procedure the accused ought to be further examined in the manner laid down in § 136, according to which he should be given every opportunity of refuting the

evidence against him and of putting forward all the facts that tell in his favour. In a case in which hypnosis is indicated, it would be a contravention of the above paragraphs to omit hypnotizing the accused.

We will now discuss what should be done when the accused person pleads that he has committed the offence through hypnotic or post-hypnotic suggestion, or when he says he has been the victim of a crime in hypnosis. Unless such a plea is made hypnosis will never have to be judicially considered. The point requiring consideration, as Forel points out, is that when the crime is successfully suggested it may also be impressed upon the subject that he shall think he has acted freely. If any regard were paid to it, we should be obliged to take into consideration that every case of crime might be a result of hypnotic suggestion. According to Delbœuf, indeed, this is especially the case when the crime does not in any way benefit the accused. But in reality the question whether the patient was hypnotized or not will only have to be investigated when hypnosis is pleaded. But we must not straightway reject the plea merely because it is, as Riant thinks, of great advantage to the accused.

Let us suppose that the accused asserts that he acted under the influence of hypnotic or post-hypnotic suggestion. He may perhaps add that he felt a subjective constraint, and that he has often been hypnotized, but that he does not remember any criminal suggestion.

It would then have to be judicially decided—(1) whether the accused was really hypnotized; (2) whether the commission of a crime was suggested to him in this state; (3) who was the originator of the suggestion; (4) to what degree he was suggestible (Max Dessoir).

Now, if the statements of witnesses were insufficient, the accused could be hypnotized. Very little, however, would be gained by that; but the hypnosis might be employed in further endeavours to ascertain the truth—I am, of course, assuming that examination and statements made in hypnosis are legally admissible. Making use of the memory in hypnosis we should, first of all, ask the accused whether he had been hypnotized previously and by whom, and who had made the criminal suggestion. If no answer was obtained—since the instigator might also have suggested loss of memory—an indirect method must be used, such as Liégeois mentions; the instigator might

be discovered by means of association, if any one were decidedly suspected. He might, for instance, be told to cough, laugh, etc., when he saw the instigator or his photograph, or heard his name. I believe the instigator might be detected in this way, and that some such devices would prove successful even when suspicion attached to no one in particular. Automatic writing might also be used—in some cases with certainty of success. I think it certain the aim could be attained by repeated efforts, in spite of loss of memory; for a suggested loss of memory must eventually disappear before repeated contrary suggestions, cleverly made.

Finally, we must endeavour to ascertain the degree of suggestibility by making fresh experiments in suggestion.

But the case would be much more complicated if the instigator of the crime had suggested that the subject should not be hypnotizable by anybody but himself. Still my own experience makes it seem probable that even such a suggestion might be made ineffectual by repeated opposed suggestions in new hypnoses, supposing, of course, that a repetition of the original suggestion could be prevented. Delbœuf has expressed the same view. He proposed that a more indirect method of hypnotization should be chosen. The subject should be prevented as far as possible from thinking of the hypnotization, and the necessary suggestions should only be made casually.

Goltdammer relates that this question of the use of hypnotism in law called up a discussion in a court of justice between the defending counsel and the counsel for the crown, in a suit at Verona thirty-seven years ago. It was a case of rape in magnetic sleep. There was loss of memory in the waking state. The defending counsel opposed the counsel for the crown, who proposed to re-magnetize the assaulted person, but the court agreed to his doing so, as it considered the induction of the magnetic sleep merely as a method of proof. The victim made important statements in the sleep artificially induced, and in consequence of these the accused was condemned. A case of Motet's was somewhat different. Motet hypnotized a person accused of unnatural offences so as to prove that the accused was probably in a state of unconsciousness when he committed the incriminating acts.

The case would be the same if a person asserted he had been the victim of a crime; here also new hypnosis must under certain circumstances be induced, and if there was loss of

memory the question must be cleared up by examination in hypnosis, supposing the law allowed it.

All this shows what difficulties stand in the way of turning hypnosis to practical account in law. Hypnotization would only result in a certain degree of probability, since, as we have already seen, (1) there is intentional falsehood in hypnosis; (2) the statements may be influenced by previous suggestions; (3) there is danger that answers may be influenced by the way in which the questions are put; (4) previous suggestion may make new hypnosis very hard to induce.

As a matter of course, we should take similarly into consideration all the details of the case, such as the act itself, and the question, who might expect to benefit by the crime. This would be the only way when the person supposed to have been influenced by suggestion is already dead, as is conceivable in a will case. Such a case does not seem to be unlikely, and would be very difficult for an expert to clear up. In all cases of legal hypnotization the possibility of simulation must be considered as well as the possibility of a purposely false accusation (Ladame).

Many proposals have been made for avoiding the possible dangers of hypnosis to health as well as to morality. As early as 1880 Friedberg demanded that hypnotic experiments should only be allowed in the presence of a doctor. Grasset and others joined him in this contention later on. Delacroix, in France, propounded a law making hypnotization legal only for doctors, and then only when at least two are present. It would be difficult to carry out such regulations: to begin with, the exact limitation of the idea "hypnosis" is still a matter of dispute, and then again, as Preyer thought, other difficulties would be raised by the fact that many people can hypnotize themselves.

All experts—medical men, psychologists, lawyers (Drucker, Lilienthal, Liégeois, etc.)—agree that difficulties should not be thrown in the way of the use of hypnosis by doctors for therapeutic purposes. This is recognized in the law that was passed in Belgium at the special instigation of the Minister of Justice, Le Jeune. It certainly does not restrict the permission to hypnotize to doctors. The original draught contained a paragraph according to which the hypnotizing of minors and the insane was only to be entrusted to physicians. When this question came to be discussed Merveille expressed

the opinion that the courts might wink at an evasion of the law should a layman hypnotize such a patient at the latter's request. Finally, on the motion of Soupart, it was agreed that by special authorization an exception might be made in favour of those who were not physicians. The first exception made—it was in favour of Astère Denis—certainly raised a storm of indignation in the Belgian Academy of Medicine, Brasseur having described the authorization in a pamphlet as state support of Charlatanism.

Public exhibitions of hypnotism are, however, forbidden by law in Belgium. In other ways also the representatives of science have generally opposed such exhibitions. They ought to be prohibited on both moral and hygienic grounds. It is perfectly true that at one time such public exhibitions served to draw the attention of scientists to hypnotism, but nowadays they are more calculated to repel people from the scientific study of that question, since they degrade hypnosis into an object of vulgar curiosity, instead of elevating it to one of research. For this reason alone it is a good thing that the Prussian Government, acting on the advice of the members of the Scientific Committee on Medical Affairs, permanently suppressed public exhibitions of hypnosis in 1881. Unfortunately, hypnotizers know of many ways in which they could evade the law. Since it was only public exhibitions that were prohibited they removed their entertainments to the premises of private societies to which members and their guests have free access. Now, if such a society is only used to screen an evasion of the law, the authorities would have as much right to interfere with a prohibited exhibition on its premises as if the performance were publicly advertised. It is difficult to understand why the authorities do not make requisite use of the power entrusted to them. How important this point is may best be judged by the fact that, in a case we have already mentioned, a "suggester" named Weltmann, who was charged with inflicting bodily injury on a subject, was acquitted by the court because the competent authorities had given him permission to perform his experiments. The prohibition as to public exhibitions also applies to the female "dream-dancers" and "sleep-dancers," so many of whom have appeared of recent years. It is obvious that the police authorities, and perhaps also the medical officials who work with them, are even now tricked by the actors who

advertise their performances as experiments in suggestion in the waking state. The hypnotizer is nowadays called a "suggester." Under the pretext of only making suggestions in the waking state, the "suggesters," Weltmann, Viebig, Ignot, and Krause have carried out ordinary hypnotic experiments in public, and the police authorities have apparently let themselves be deceived by the words "suggestion in the waking state." Forel also called attention to this on account of a statement made by Speyrs that in the case of the "suggester" Krause the experiments were entirely hypnotic, and similarly Neustätter has shown that this was the case and that the subjects were, in a manner, surprised into hypnosis which was not induced in the ordinary way.

If the authorities and their medical colleagues are not sufficiently acquainted with these questions they would do well to consult specialists, just as the courts often do. It is impossible for a man to know everything. Cramer proffers the advice that doctors who are without personal experience in the domain of hypnotism had best decline to report on such cases, and Ernst Schultze says the advice is excellent and should be followed implicitly. For instance, he declares he would never pass an opinion on the value of hypnotism in civil law, since he lacks the necessary experience. If he were asked to do so he would request to be excused from undertaking the duties of a specialist. I strongly advise all forensic physicians and psychiatrists to take these words to heart. It will hardly increase the authority of our police and medical officials if they let themselves be so deceived by sham waking suggestions that they fail to see the real hypnoses.

Public exhibitions have also been objected to in other countries. They were prohibited in Austria as early as 1880, chiefly on account of Hansen's appearance in public. A commission, of which Hofmann was a member, expressed a fear that such performances were not unattended with danger to health. The matter was again investigated by the Chief Sanitary Board, and Krafft-Ebing drew up a report. On this occasion it was pointed out that as far back as 1795 the Court Chancellor of Vienna had issued a decree placing essential limits to experiments in animal magnetism, and that in 1845 it was agreed, at the request of Professor Lippich, that the employment of animal magnetism should only be sanctioned in the case of qualified medical men. Another official note was published by the Austrian Sanitary Council in 1895—Krafft-Ebing again acted as reporter—dealing with the use of

hypnosis by spiritualist societies. This note was in favour of the suppression of spiritualistic societies on account of a report sent by Schlager in 1883, in condemnation of such bodies. In Italy public exhibitions were forbidden in consequence of Donato's performances. Notices of prohibition were also issued in Switzerland—at the request of the sanitary authorities of the Canton of Aargau, among others. The first person to bring about the prohibition of public exhibitions in America was Prendergast, a sanitary officer in Cincinnati, etc.

Though I consider the public exhibitions of hypnotism that take place in the present day mischievous, I do not wish to depreciate the services of those who have drawn attention to hypnotism by public exhibitions. Just as I refuse to join in the general condemnation of Mesmer, I try, and recommend others to try, to judge such men as Hansen, Böllert, and Donato, fairly. Their motives may have been selfish, but they have certainly been of great service to science. *Que les savants n'oublient jamais que si l'on supprimait de l'histoire de l'hypnotisme les quatre ou cinq vulgarisateurs étrangers à la médecine qui ont eu la force et le courage de prouver les faits aux quatre coins de l'Europe depuis cinquante ans, cette histoire s'écroulerait entièrement.*[1] To the honour of those mentioned, it should be expressly stated that all three of them were invariably ready to help the representatives of science in the most straightforward way. Heidenhain, Michael, Delbœuf, Wernicke, Morselli, and others have emphatically recognized this. None the less, such exhibitions are reprehensible in the present day, and Delbœuf's supposition that they are the best means of spreading a knowledge of hypnotism, and thus lessening its dangers, in nowise justifies them. They are accompanied by danger to health, as I have often explained. And it must be added that, from the moral point of view, there is no justification for putting people into a condition which deprives them of will, for no higher object than for the amusement of the uncultured persons who constitute the major portion of the audience at such exhibitions.

Many Bills dealing with this question have been promoted

[1] "It should never be forgotten by scientists, that if from the history of hypnotism we wiped out the work of vulgarization done by four or five outsiders, who with courage and perseverance have proved its main facts throughout the length and breadth of Europe during the past fifty years—that history itself would entirely fall to pieces."

in Germany. Still I think that the law as it stands is able to prevent any abuse in this respect. A few years ago Lentner asked whether it would not be advisable to insert in the Criminal Code a clause making the use of hypnosis by swindling quacks and money-grabbing showmen a punishable offence, as constituting a danger to health and personal safety. Rosenfeld also considers the existing regulations adequate. At all events it would be difficult to justify the inclusion of such a clause in the Criminal Code unless it were made applicable to those cases in which performers are compelled to risk their lives. Hanns Gross proposes that it is the people who organize such performances who should be punished.

Attempts have occasionally been made to restrict the misuse of hypnotism by means of administrative measures. This happened, for instance, in Hamburg, where a well-known hypnotizer was fined for advertising his method of treating disease. The Senate of that town issued a decree on 1st July, 1900, dealing with the practice of medicine by unqualified persons, and § 2 of the decree prohibited the advertisement of methods of treating human complaints, should such methods be calculated to endanger health. In a case at the Hamburg Assizes, I was called in to express my opinion, as an expert, whether hypnosis, as advertised in the case before the court, was to be considered dangerous to health. In the end, no importance was attached to this point, since the court held that the decree of the Senate was inconsistent with the general trade regulations, and therefore null and void.

This completes our consideration of the bearing of hypnotism on jurisprudence. We have seen the direct practical importance of hypnotism to jurisprudence, and also that it must not be exaggerated. The legal importance of hypnosis is similar to the medical. It is not the fact that a hypnotic can be made the victim or the instrument of a crime; it is not the fact that we can induce retro-active suggestions hypnotically and thereby falsify testimony, that is all important in this respect, but rather the number of instructive lessons in jurisprudence that hypnotism has indirectly supplied us with. By teaching us the importance of suggestion it has opened up many a fresh field of view to the science of jurisprudence. In the instigation of crime, factors that are very similar to those employed in hypnotic suggestion often play a part; indeed, it may be that

suggestions in the waking state, as well as other mental influences, are used. I must again call attention to the case already mentioned, in which a girl's superstitious ideas were used for the purpose of making her a prostitute and a criminal. Of all things, it is superstition that is capable of producing as powerful an influence on the will as hypnotic suggestion can. I have already discussed in detail the importance hypnotism has acquired in respect to its influence on testimony.

All these are questions for the jurist to consider. He must be able to think psychologically and know how far an accused person's culpability may be lessened, if not nullified, by his suggestibility, and more especially by the ease with which he can be influenced. On the other hand, he must know how to weigh the value of evidence. Unfortunately, our professional jurists are not sufficiently schooled in the psychological way of thinking. In future, more will have to be demanded in this respect, since incapacity to think psychologically is calculated to make a jurist the agent of injustice, not of justice.

CHAPTER XI.

HYPNOSIS AND PSYCHOLOGY.

I HAVE, in the foregoing part of this book, already discussed the psychological phenomena of hypnosis, and in the last three chapters I have considered the close relation of those phenomena to medicine and jurisprudence. All these considerations show the importance of hypnotism to the science of psychology itself. For all that, I am now going to mention briefly some particulars concerning the importance of hypnosis in certain branches of psychological research.

Certainly professional psychologists are not agreed in this respect. Villa thought that experiments on hypnotics have no psychological value because of the totally abnormal conditions under which they are performed. He considered the first requisite in such an experiment wanting—viz., clear consciousness on the part of the subject of himself, of his actions, and of the changes that take place in him. Any one who agrees with this view of Villa's must reject child-psychology and animal psychology, and also forfeit all right to make use of the psychology of savage races or of the results of psycho-pathology. In all these cases self-observation is just as faulty as in hypnosis, and in spite of that these domains of research are considered valuable portions of psychology. Indeed, self-observation is not at all impossible in hypnosis; on the contrary, we owe many instructive communications on hypnosis, and the theory of hypnosis, to the self-observation of hypnotized investigators. In this connection Obersteiner, Forel, Bleuler, Marcinowski, Vogt, Wundt, and many others have made material contributions. Villa's standpoint is really not one for discussion. Vogt rightly points out that Durand de Gros fifty years ago, and Charles Richet much later, have shown that sense-delusions can be induced by suggestion without any change whatever taking place in the subject's consciousness. We are able by means of suggestion to cause inhibition of a perfectly circumscribed form, such, for instance, as anæsthesia

of a single finger, or even of one phalange. We can induce loss of memory in respect to a single word or a whole system of ideas, and that, too, without producing any other change in the subject's consciousness or in the least degree influencing his critical faculties (Vogt). When we consider how readily psychologists have at all times made use of dreams in the investigation of mental life, but that hypnosis is accessible for purposes of experiment in quite a different way to night-dreams, and that when suitable suggestions are made a hypnotic is more capable of self-observation than a person who is dreaming, it follows that Villa's dogmatic decision can only be ascribed either to total ignorance or lack of reflection.

Wundt has ascribed at least a limited amount of importance to hypnotism—about as much as to other abnormal states. He puts it down as a phenomenological method of research. More recently—in his *Grundriss der Psychologie*—he has gone so far as to admit that partial hypnosis may, under certain circumstances, be considered a general method of research. Beaunis ascribes even a still greater importance to hypnotism in respect to psychology. According to him, hypnotism is to psychologists what vivisection is to physiologists. Krafft-Ebing and Forel have expressed similar views, and Max Dessoir and Münsterberg also gave timely recognition to the psychological importance of hypnotism. Ribot and Wizel have used the hypnotic state for purposes of experiment, and have studied the mental life of persons in that state; they also made use of it for comparing the hypnotic with the non-hypnotic state. They found that hystericals give more accurate answers in hypnosis than in the waking state.

Vogt has certainly gone much farther. He perceives in hypnotic experiments a possible means of arriving at psychological results of a more universal character. Two conditions are necessary for a hypnotic state to be suitable for this purpose: (1) The subject, in order to present as high a degree of suggestibility as possible, must be thrown into as deep a sleep as is compatible with the removal at pleasure of this sleep-inhibition;[1] (2) there must be a preservation of the waking state within a certain compass, so as to facilitate self-observation on the part of the subject. These conditions are best

[1] By sleep-inhibition Vogt means the inhibition *produced* by sleep; this might easily prove misleading, since according to the ordinary rules of etymology sleep-inhibition would mean the inhibition of sleep.

satisfied by that systematic partial preservation of the waking state, which, while affording a thorough waking state for all those elements of consciousness that appertain to the special experiment, causes a deep sleep for all others. In addition to this, Vogt further recommends the normal waking state in which there is a single realized suggestion; that is to say, a partial systematic state of disassociation. He obtains the systematic partial preservation of the waking state either by partially waking the subject from a general sleep or by putting him partially to sleep. And he does not even think it either practical or desirable to procure deep sleep, since a lighter degree will give us conditions more advantageous than are to be had in the waking state. Here a state of contracted consciousness, in which the only noticeable thing is a marked dulness in response to any commotion, is quite sufficient. By concentrating his attention, a hypnotic, while in these states, is capable of making a very different analysis of mental processes to that which would otherwise lie in his power. Thus, Vogt thinks that we are better able in these states to concentrate our attention on the problem that has to be analyzed in complicated mental processes than we should otherwise be; if, for example, a tuning-fork is struck we first hear a composite, not a simple, sound; we further feel a tension in our ears, an agreeable or disagreeable feeling, our frame of mind is affected, respiration altered, other organic sensations take place, we may perhaps be able to fix associations called up by the sound, etc., etc. All these phenomena require analyzing, but that can only be done by concentrating the attention on one phenomenon at a time to the exclusion of all others. It would be difficult to find a better way of doing this than by employing a state of partially preserved wakefulness, systematically maintained.

That is the way in which Vogt has made a qualitative analysis of feeling. Wundt, in consequence of his more recent researches, has now rejected the generally accepted notion that our feelings may be classified as those of like and dislike. In his opinion, where phases of feeling are concerned the differentiation lies not merely between like and dislike, but between excitation and inhibition, tension and relaxation as well. Vogt now thinks that Wundt's view is substantiated by the self-observations of hypnotic subjects. According to Vogt, there are two couples of qualities, the one "agreeable and disagreeable," the other "elevating, exhilarating, buoying up,

relaxing, depressing, saddening." He calls the latter accentuation of feeling sthenic, in contradistinction to the hedonistic form. In one of Vogt's subjects, the feeling of tension released a certain feeling of volition. Vogt thinks that he is able to refute the charge that he suggested the analysis to the hypnotized subject in this case by the fact that he was originally an opponent of Wundt's method of analysis, of which he only became an adherent in consequence of the analyses made by hypnotics. Certainly this would not suffice to refute that objection, which, moreover, may also be raised with regard to analyses made in the normal waking state. It would then have to be proved that the subject experimented on had no notion of Wundt's analysis, and could not receive, either by word or sign from any one present, any intimation of that analysis. Lœwenfeld has raised the further objection, that many of the states used by Vogt in these analyses and ascribed by him to hypnosis have nothing to do with that condition. Vogt, by including all mental states in which unemotional suggestions are carried out, gives far too wide a meaning to the concept hypnosis. This objection of Lœwenfeld's is fully justified. According to Lœwenfeld, Vogt also exaggerates the value of hypnosis as a means of psychological research, in that he fails to give due consideration to the question of the possibility of the persons concerned acquiring the power of making such analyses solely by learning and practising the art of observing, without any hypnotic measures being necessary. Finally, we should, according to Lœwenfeld, still have to decide whether the analyses, as made by Vogt's subjects, apply generally, or only to the few persons whom they concern. Certainly some of the objections are justified; at least, they have not yet been refuted by Vogt. Consequently, we must leave the question, whether valuable analytical results have as yet been obtained by means of hypnosis, as provisionally unanswered. But we certainly have no right to reject the method as such; we must, on the contrary, admit that further investigation is called for.

We have to take up the same standpoint in respect to other investigations; such, for example, as those of Ach, who, prompted by Vogt's works, has investigated the capabilities of persons in the state of contracted consciousness about which I spoke on page 139. The performances of the dream-dancer, Magdeleine G., led F. E. Otto Schultze to submit her to a

series of acoustic, psychological, and æsthetic investigations. In this way Schultze was going to examine the chief elements of music and speech separately, and to endeavour to obtain, by psychological experiments in tone-perception, an answer to the question whether all reactions present the same conformity to law in hypnosis that they do in normal life. But the value of the experiments is very considerably discounted by the fact that we are unable to decide what was due to training and what was spontaneous on the part of the subject. Several investigators, Farez and Vogt in particular, have investigated the hypermnesia of hypnotics psychologically. Vogt studied associations in this way, and thinks that associations, for which the connecting link is wanting in waking life, may be explained by increased power of memory in hypnosis. Hirschlaff certainly denies the existence of hypermnesia in hypnosis *in toto*, and considers the results as more probably due to unintentional suggestion. Other investigations of hypermnesia in hypnotics certainly belong to the domain of psychopathology. I may here recall the experiments of Breuer and Freud, who tried to discover an etiological moment in the production of hysteria by using the increased power of memory exhibited by hypnotics (*cf.* page 335). I also refer to what I said (page 349) concerning modern researches in hysteria, to the advancement of which hypnotism has greatly contributed, especially from the psychological point of view. I may further mention the impulsive actions and imperative ideas which we are able to induce experimentally by means of post-hypnotic suggestion. The presumption that what here occurs as the result of post-hypnotic external suggestion appears in other cases as a pathological phenomenon produced by auto-suggestion, is certainly justified. Bentivegni has called attention to this affinity between post-hypnotic suggestions and many imperative ideas. When the signal for a post-hypnotic action to be performed is perceived, the subject experiences certain impulses in the same way that a morbid person feels impelled to commit murder or suicide at the sight of a knife or of water (Cullerre). Höfler has pointed out the importance of post-hypnotic suggestion to normal psychology as well. He showed how it certainly stimulated research into the after-effects of conclusions. I must here further refer to the creation of feelings and moods by means of post-hypnotic suggestion. We are able to arouse feelings of like and dislike

without the subject being even conscious of the ideas connected with those feelings. A man's whole frame of mind may be made either pleasant or unpleasant without his ever knowing what influenced him, since he does not recollect the command he received. This likewise throws light on those cases in every-day life in which people are unable to explain why they feel in a good or bad frame of mind, as the case may be. Hypnotism and the theory of suggestion have also exercised a great influence on our estimation of synopsia, colour-audition, etc. At the present day some of these phenomena are very properly considered the result of suggestion; others may be referred to associations of a purely psychological character, but not on that account attributable to suggestion. Others may possibly have a different origin—one of a purely physiological nature. At all events, the possibility of such phenomena originating in auto-suggestions was first put in its true light by hypnotism. Hypnotism has also done much to promote the study of sleep, and especially of dreams. By means of post-hypnotic suggestion we are certainly able to induce any dream we will.

There is another direction in which hypnotism has acquired importance. This brings me to the theory of primary and secondary consciousness. That there *are* subconscious mental acts was known long before the advent of modern hypnotism. But what we owe to hypnotism is a new, almost ideal method of putting such acts to the test of experiment. In this connection hypnotism has proved most fruitful. Post-hypnotic suggestion shows us how delicate the workings of the secondary consciousness sometimes are, and that, too, without there being any question of the presence of those automatic processes that practice unconsciously produces. A hypnotic is told that when he wakes he is to do something or other. He wakes, does not remember what has happened, and yet at the expiration of the hour performs the act more or less punctually. We are able to observe that the process of counting goes on in the secondary consciousness of our subjects; on this point I refer the reader to the experiments described on page 164. How accurately time is sometimes calculated in the secondary consciousness—*i.e.*, how the secondary consciousness executes a complicated task—is shown by Delbœuf and Bramwell's experiments (p. 163), in which thousands of minutes were counted and estimated. To the superficial observer, or the man who has no experience

of hypnotism, many of these reports must appear incredible; yet these are just the very experiments that have been made, and their details recounted, by experienced and critical experimenters—by such a man as Delbœuf, in particular. Even if such experiments only succeed in a few cases, hypnotism has nevertheless provided us with an excellent method of testing the secondary consciousness experimentally.

A few investigators went even farther, and thought they were able to prove by means of hypnosis that the composite nature of human personality is the result of consciousness consisting of two halves, each of which operates more or less independently of the other. As Clemens Sokal and S. Landmann pointed out, these experiments have formed the essential part of French psychology for a number of years. I mention Binet as first and foremost in this respect, then F. Myers and Gurney in England. In Germany, Max Dessoir, who dedicated his *Doppel-Ich* to the subject, once held similar opinions, but he has latterly modified his views on the plurality of human personality very essentially.

In the same way, hypnotism has shed light on a problem that is usually considered to belong to philosophy, but that yet presents an essentially psychological side; I refer to the question of free-will. Post-hypnotic suggestion has shown the relative worthlessness of the feeling of free-will as a proof of its existence.

Spinoza's saying, "The illusion of free-will is nothing but our ignorance of the motives which determine our choice," appears to find striking support in post-hypnotic suggestion. If we suggest to a subject in hypnosis to move a chair when he wakes, he very frequently does as he was told. When asked to find a motive for the act, the subject sometimes replies that he felt compelled to act so, or sometimes says he believes he acted so of his own accord, and gives some reason or other for his action —anything but post-hypnotic suggestion. This feeling of freedom of will when there is actually an irresistible impulse at work has frequently been used as an argument in support of determinism. We see here that a state has been induced by experiment, in which the subject has the feeling that he is acting freely, while in reality his will has already been directed in a particular manner. Ribot, Forel and others have called attention to the light that post-hypnotic suggestion has thrown on the problem of free-will. The following experiment that I have

frequently made, and that can be repeated by any one on a suitable subject, illustrates the illusion of free-will very clearly. I suggest to a hypnotic subject, X., to lay an umbrella on the ground after he wakes. He wakes, and I now tell him to do anything he pleases, but that he is to act entirely of his own free-will; at the same time I give him a folded paper on which I have written what he is to do. X. does what I suggested, and is then much astonished to find written on the paper the action he has carried out and in the performance of which he firmly believed he acted of his own free-will.

Of course, we must not exaggerate the importance of these hypnotic experiments, neither should we draw general conclusions about free-will from them; for we may be nearly certain that there will be a feeling of free-will only when the action is agreeable to the individuality of the subject. If, for instance, a morally normal man were told to take another's watch or to commit some other improper act, the feeling of aversion would be strong enough to suppress the feeling of free-will. A subjective constraint would then be felt. The feeling of free-will is much more likely to be aroused by post-hypnotic suggestion when the acts suggested are indifferent and unimportant than when they are momentous and at the same time opposed to the subject's individuality. We come across something similar in the normal waking state; in that, as Freud has rightly pointed out, the feeling of free-will arises soonest when the proceedings are unimportant, while when more important decisions are concerned the feeling of being unable to act in any but one way almost invariably predominates. Finally, we must also beware of exaggerating the importance of hypnotic experiment, since we do not by any means find these deep hypnoses and subjective delusions of the judgment in all subjects. Such subjects are invariably very few in number, and after repeated experiments most of them begin to observe themselves, which makes them conscious of the constraint put upon them. Finally, I must not omit to mention that hypnosis has taught us nothing absolutely new in this respect; for in every-day life, also, we are often able by an adroit manipulation of circumstances and conditions to give definite guidance to the will of some people without their knowing it. Goethe's saying, "Du glaubts zu schieben, und Du wirst geschoben,"[1] expresses this fact. I will, moreover, take

[1] Roughly, "You think the force is yours,—and you are forced."

this opportunity of referring to the art of conjuring. A well-known trick of the conjurer is to allow a card to be drawn from a pack and to guess it at once. The trick is thus explained: the spectator thinks he has freely chosen the card, but in reality the conjurer has directed him to one in particular, and compelled him to take it. The conjurer attains this end by putting the card he wishes chosen where it will naturally be the first to be taken up. We have as little right to assert that the hypnotic experiments already described disprove the doctrine of free-will, as we have to consider that the above experiments in waking life do so. Few can have made such experiments often without fresh doubts of freedom of will arising, but from these doubts to scientific proof is an immense step.

In one very particular respect hypnotism has given us a lesson of the greatest importance to psychology: it has proved that special precautionary measures must be taken in planning psychological experiments. It has shown that an essential source of error in psychological experiments was formerly overlooked or considerably underrated. The training of hypnotics has thrown light on this source of error. I must refer the reader to what I said on this question on page 156 *et seq.*, here I will only point out once again that a hypnotizer may, often without knowing it, by the tone of his voice or by some slight movement, cause the hypnotic to exhibit phenomena that at first could only be produced by explicit verbal suggestion, and that altogether the signs used by the hypnotizer to cause suggestions may go on increasing in delicacy. A dangerous source of error is provided by the hypnotic's endeavour to divine and obey the experimenter's intentions. This observation has also proved useful in non-hypnotic experiments. We certainly knew before the days of hypnotism that the signs by which A. betrays his thoughts to B. may gradually become more delicate. We see this, for example, in the case of the schoolboy, who gradually learns how to detect, from the slightest movement made by his master, whether the answer he gave was right or not. We find the same sort of thing in the training of animals—the horse, for instance, in which the rough methods at first employed are gradually toned down until in the end an extremely slight movement made by the trainer produces the same effect that the rougher movements did originally. But even if this lessening in the intensity of the signals exists independently of hypnosis, it is the latter

that has shown us how easily neglect of this factor may lead to erroneous conclusions being drawn. The suggestibility of the hypnotic makes these infinitesimal signals specially dangerous in his case. But when once this danger was recognized, greater attention was paid to this source of error in non-hypnotic cases than before. It is certain that many psychological experiments are vitiated by the fact that the subject knows what the experimenter wishes. Results are thus brought about that can only be looked upon as the effects of suggestion; they do not depend on the external conditions of the experiment, but on what is passing in the mind of the subject. Just as we saw on page 343 *et seq.*, that to certain medicaments chemical activity is only ascribed because the doctor expects a therapeutic action which he intentionally suggests to the patient, so is it with psychological experiments.

Perhaps many of the results obtained experimentally in connection with the injurious effects of alcohol belong here, since the subject is only too inclined to present the condition expected by the experimenter. Many experiments have been made—Kohlschütter's were the first—to measure the depth of sleep by the intensity of the sound necessary to wake the sleeper. Forel has pointed out that these experiments prove nothing, since a noise to which a person is accustomed soon loses its power of waking him, whereas gentle but unwonted noises wake one at once. In addition to this, we have to consider auto-suggestion as a source of error, particularly the subject's expectation that he will be wakened by some noise or other, a circumstance that considerably enhances the probability of his being awakened. As early as 1831 Kohlschütter himself had pointed out that the subject cannot get rid of the feeling of expecting the signal agreed upon. Naturally, these objections call for earnest consideration. Certainly Michelson, who has also studied the question, thinks that the unanimity of the results already obtained disposes of any importance that might attach to this source of error. There is, indeed, much to be said in favour of Michelson's point of view, although his contention may not repose on an entirely sound basis. We must also admit that the experimenter is frequently in anything but a position to exclude the action of suggestion. Consequently, it would be wrong to consider that all the earlier psychological experiments in which suggestion possibly played a part were simply superfluous;

many of them would probably give the same results if suggestion were excluded. If, however, one is unable in every single instance to carry out the most extreme precautionary measures, one must at least be careful so to do before venturing on far-reaching conclusions. We should take Laplace's words to heart, and examine phenomena with all the greater care in proportion to the difficulty we have in accepting them. This principle should guide us in respect to the exclusion of the action of suggestion. An event which at the time of its occurrence created a considerable commotion (I refer to the case of Clever Hans), will show how far we may be led by neglecting the above lesson taught us by hypnotism. If the Berlin psychologist Stumpf, the scientific director of the committee of investigation, had but taken into consideration the teachings of hypnotism he would never have made the fiasco of admitting that the horse, Clever Hans, had been educated like a boy, not trained like an animal.

Clever Hans answered questions by tapping his hoof on the stage; and the observers, more particularly the committee presided over by Stumpf, believed that the answers tapped out were the result of due deliberation on the part of the horse, exactly as spiritists believe that the spirits hold intelligent intercourse with them by means of "raps." One tap denoted *a*, two taps *b*, three taps *c*, etc.; or, where numbers were concerned, one tap signified 1, two taps 2, etc. In this way the animal answered the most complicated questions. For instance, it apparently not only solved such problems as 3 times 4 by tapping 12 times, and 6 times 3 by tapping 18 times, but even extracted square roots, distinguished between concords and discords, also between ten different colours, and was able to recognize the photographs of people; altogether, Clever Hans was supposed to be at that time about upon a level with a fifth-form[1] boy. After investigating the matter, Stumpf and the members of his committee drew up the following conjoint report, according to which only one of two things was possible—either the horse could think and calculate independently, or else he was under telepathic, perhaps occult, influence:—

The undersigned met together to decide whether there was any trickery in the performance given by Herr v. Osten with his horse—*i.e.*, whether

[1] Lowest form but one in a German gymnasium.

the latter was helped or influenced intentionally. As the result of the exhaustive tests employed, they have come to the unanimous conclusion that, apart from the personal character of Herr v. Osten, with which most of them were well acquainted, the precautions taken during the investigation altogether precluded any such assumption. Notwithstanding the most careful observation, they were unable to detect any gestures, movements, or other intimations that might serve as signs to the horse. To exclude the possible influence of *involuntary* movements on the part of spectators, a series of experiments was carried out solely in the presence of Herr Busch, Councillor of Commissions. In some of these experiments, tricks of the kind usually employed by trainers were, in his judgment as an expert, excluded. Another series of experiments was so arranged that Herr v. Osten himself could not know the answer to the question he was putting to the horse. From previous personal observations, moreover, the majority of the undersigned knew of numerous individual cases in which other persons had received correct answers in the momentary absence of Herr v. Osten and Herr Schillings. These cases also included some in which the questioner was either ignorant of the solution or only had an erroneous notion of what it should be. Finally, some of the undersigned have a personal knowledge of Herr v. Osten's method, which is essentially different from ordinary "training" and is copied from the *system of instruction employed in primary schools.* In the opinion of the undersigned, the collective results of these observations show that even *unintentional signs* of the kind at present known were excluded. It is their unanimous opinion that we have here to deal with a case that differs in principle from all former and apparently similar cases; that it has nothing to do with "training" in the accepted sense of the word, and that it is consequently deserving of earnest and searching scientific investigation. Berlin, Sept. 12th, 1904. (Here follow the signatures, among which is that of Privy Councillor Dr. C. Stumpf, University Professor, Director of the Psychological Institute, Member of the Berlin Academy of Sciences.)

Any one who has done critical work in the domain of hypnotism, after the manner insisted on by the Nancy school, cannot help considering Stumpf's method of investigation erroneous from the very outset. I had visited Herr v. Osten, the owner of Clever Hans, more than a year before Stumpf wrote his deplorable report; and, on the ground of my experiences in the domain of hypnotic experiment and suggestion, I had at once thought of those signs, which we make unintentionally and sub-consciously, as primary sources of error. I therefore at once so arranged the conditions of the experiments that no one present, not even myself, could know the correct result. In consequence of this every experiment turned out a failure, as I explained to the Psychological Society of Berlin at a time when Stumpf had not yet retreated from his erroneous position. A first source of error that had to be considered was that some one present—it might have been Herr v. Osten

or it might have been any one else—unintentionally gave the horse a sign when to stop tapping. It cannot be considered sufficient, as stated in Stumpf's report, that Herr v. Osten did not know the answer; no one should be present who knows it. This is the first condition to be fulfilled when making such experiments. Anybody who has been engaged in training hypnotized subjects knows that these insignificant signs constitute one of the chief sources of error. Some of the leading modern investigators in the domain of hypnotism—Charcot and Heidenhain, for instance—were misled by them at the time they thought they had discovered new physical reflexes in hypnosis. But in 1904, by which time suggestion had been sufficiently investigated to prevent such an occurrence, a psychologist should not have fallen into an error that had been made more than twenty years previously. It is necessary to make these remarks openly, no matter how highly we may prize Stumpf's services to psychology in other respects. As may be seen from the above report, Stumpf made the mistake of believing that he must see the signs if any were given. Since this was not the case, he simply excluded the presence either of voluntary signs—*i.e.*, tricks—or of involuntary ones. But a psychologist like Stumpf could not help knowing that it is especially the involuntary signs that may be so delicate that not only the person who makes them is unaware that he is so doing, but even the spectators also are unable to detect them unless they employ special instruments for the purpose. But the main point is this: signs that are imperceptible to others are nevertheless perceived by a subject trained to do so, no matter whether that subject be a human being or an animal. Stumpf's mistaken report has led to the propagation of the erroneous notion that a horse can be taught to extract square-roots, to perform the most complicated calculations, and be generally educated by a method of instruction like that employed in primary schools. The chief point for the experimenter is not, as Stumpf thought, that he ought to be able to perceive signs, but that he should work under conditions that render it impossible for signs to be made. Stumpf ought from the very first to have made use of that critical system of research that alone has been employed for the last twenty years by persons making experiments in connection with telepathy, clairvoyance, and the action of drugs at a distance—a domain I shall treat of in the last chapter. Since he had failed to

draw from the experiences of the investigators of hypnotism and suggestion those conclusions requisite for the purposes of his investigations, he was bound to come to grief over Clever Hans.[1]

I have intentionally gone into the case of Clever Hans in detail, because it shows in the clearest manner possible what hypnotism has taught those who are conversant with that subject. It is certain that the investigation of many other experimental researches made by some psychologists would bring to light the existence of the same source of error that I have demonstrated in the case of Clever Hans. Only, in the latter case, the error produced such serious consequences; for the horse that was supposed to be able to calculate not only created an immense sensation, but even led a well-known professional psychologist to pen one of the most absurd expert reports ever drawn up.

Hypnotism has, moreover, helped us to understand many erroneous scientific theories by the light it has thrown on the effects of auto-suggestion on investigators. Soury has published a study of a case of scientific auto-suggestion affecting several Italian physicians. Rainaldi and Giacomo Lombroso had believed that it was possible to excite certain brain centres and contract the related muscles, by mechanically irritating the scalp above those centres. Soury shows convincingly how doubtful the doctrine of localization is, and that every author always succeeds in causing contractions in accordance with his own views as to motor centres. Similar errors were very prevalent in the early days of hypnotic research. But we find a very similar auto-suggestive process outside the domain of hypnotism, as, for instance, when authors discover exactly what they want. Auto-suggestion is a hindrance to objective contemplation.

[1] Later on, after I had given him the correct explanation privately and publicly, Stumpf relinquished his erroneous views when he was a member of a second committee. Unfortunately, he did not make a public admission of his error, as he was in duty bound to do; on the contrary, he fogged the issue by representing that in his first report he only denied that voluntary signs had been made. That he was also on the look-out for involuntary signs, and did most emphatically deny that any such were made, is irrefutably proved by the report reproduced above, in which I have italicized the sentences relating to this point. In making the statement that he only dealt with the question of tricks—*i.e.*, intentional signs—in his first report, Stumpf has said that which is substantially untrue.

It was also the influence of modern researches in hypnosis and suggestion that first shed a brighter light on that domain which we call the psychology of crowds. Among the investigators who have worked in this domain I may mention Tarde, Sighele, Sidis, Lombroso, Friedmann, Bechterew, Binet, Lœwenfeld, Le Bon, Bleuler, Straticò. The soul of a crowd is not merely a combination of the souls of the individuals composing it; on the contrary, it presents certain peculiarities. So, too, the suggestibility of a crowd is of a different nature to that of the individual. Even if opinions differ as to how it happens that a crowd is often influenced by factors that have no effect on the individual, still all agree that the suggestibility of a crowd is something special. Lœwenfeld thinks it is elective in that it is not absolutely greater than that of the individual, but, at the most, heightened in a definite direction. Moral contagion, which had already been discussed long before hypnotism, is closely connected with suggestion; and, as Tarde has rightly pointed out, suggestibility is the condition necessary for it to prove effective. Sighele expresses similar views:—"It is quite clear to me that this suggestion is the cause of the movements and actions of crowds, and that a cry raised by some individual in the midst of a crowd, or the word of a speaker, or the act of some desperado, so seizes the whole mass in its suggestive grasp, that it is carried away to acts of destruction like a wild and will-less herd." Certainly the catchword "suggestion" alone does not explain the psychology of crowds. Numerous other psychological factors play a part, and these have been pointed out in a more or less convincing manner by the authors mentioned above. But whether we are dealing with social or political events, or with artistic tendencies and scientific currents of thought, the suggestibility of the crowd invariably makes itself felt in a particular direction. The same thing occurs where questions of religion or civilization are concerned, even should, in the latter case, the question be merely one of fashion in dress. The part that is also played in religious questions by the suggestibility of crowds is evident from the enormous influence exerted on huge multitudes by the religious hallucinations of any hysterical female who imagines she has seen the Madonna. We find the same thing occurring with those founders of sects, who have known how to infect thousands and thousands of people with their own peculiar hallucinations. Similarly, the suggestibility of crowds

throws light on many phenomena recorded in universal history and the history of civilization, no matter whether we take the Crusades or those mental epidemics known as St. Vitus's dance, dancing mania, and the flagellation mania, which at one time raged so furiously in Germany, the Netherlands, and Italy, and processes analogous to which are reported as also occurring in Abyssinia (J. F. C. Hecker); it matters not, I say, whether we are dealing with the obsessions of the Middle Ages or with epidemics of convulsions, none of these phenomena is comprehensible from the psychological point of view unless we take the suggestibility of crowds into consideration. Flechsig endorses Bechterew's statement that history, and more particularly the history of civilization, affords such striking instances of the mighty effects of suggestion that it should hardly be possible for any historian to pass them by unnoticed.

Hypnotism sometimes accomplishes a marvellous feat in providing us with an explanation of mental epidemics, as, for instance, in those cases in which whole districts have been infected with lycanthropia, or zoanthropia--*i.e.*, their inhabitants suddenly imagined they were turned into wolves or other animals. Epidemics of zoanthropia occurred frequently in the Middle Ages, and even later. People usually believed they were changed into wolves, and they then attacked and tore others, and displayed the fierceness and instincts of wild beasts. This phenomenon was usually supposed to be the work of the devil. Herodotus and Pliny mention the occurrence of like phenomena in ancient times, and Johann Wier has given us details of later epidemics. Baret reports that in a form of insanity occurring in Japan the subject believes that he has been changed into a fox. It is popularly believed that such a person is possessed by a fox, and the condition is called *Kitsune-tsuki*. This at once recalls those cases in which hypnotized persons are induced by suggestion to believe that they are transformed into animals.

The foregoing considerations should show the extent to which hypnotism has enriched the most different domains of psychology. But we shall see from what follows that recognition of the psychological value of hypnosis has helped to advance other branches of research—ethnology and the history of civilization, for instance. This is solely due to the fact that the psychological character of hypnosis has become more and more recognized, but this has only taken place gradually.

I may here remind the reader that in 1880, when Heidenhain considered hypnosis a purely physical state, Rosenbach pointed out the psychological importance of hypnosis and was almost completely ignored! Heidenhain thought that the imitative movements were simple reflexes caused by the subject seeing similar movements made; catalepsy and echolalia were mere physical reflexes, and psychic influence was entirely ignored. Charcot, also, did not escape making similar mistakes. Since then, the importance of psychic influence for inducing the symptoms of hypnosis has been recognized to such an extent through the efforts of the Nancy School that the physical symptoms are probably even underrated. This change, which gradually led from the former physical to the present-day psychological conception of hypnosis, could not help having an important effect on psychology in general, since it brought with it hitherto unsuspected proofs of the importance of certain psychical processes, especially suggestion. To-day, impartial examination shows that in many places psychology contains traces of hypnotism and of the theory of suggestion. Let the reader take a modern work—such, for example, as the *Leitfaden der Psychologie* of Lipps, who is one of the few independent psychologists of the present day—and he will there find evidences of hypnotic research in sections other than those specially devoted to that domain. The theory of self-projection is so intimately connected with suggestion that without the latter Lipps would have been quite unable to establish the former. We find something similar in other provinces of modern psychology (*v.* Ribot's works on Memory, Personality, Will, etc.). Psychology has also benefited by the fact that many persons first acquired a general interest in psychological processes through studying hypnotism. And by providing psychology with so many fresh fields of view, hypnotism has helped to prevent numerical psychology from exercising supreme power to-day in the manner the representatives of this domain of research hoped only twenty years ago.

Of course, we must also avoid exaggerating the importance of hypnotism. Some people thought they were able even to solve the chief problems in cerebral physiology by means of hypnosis, but this turned out to be erroneous. Mistaken interpretations have given rise to these errors. I may here refer to what I said about phreno-hypnotism on page 86.

Nevertheless, such errors recur occasionally. Just as Bérillon did formerly, so Grasset in the present day still believes that hemi-hypnosis proves that the two cerebral hemispheres act independently. This is but an instance of the kind of exaggeration met with in every domain of research (*cf.* p. 87). Similarly, the theory of the Double-Ego has nothing to do with any disturbance in the equilibrium of the two cerebral hemispheres, as some assume to be the case. Krauss, in his biography of Ribot, lays stress on the fact that the latter has refuted this theoretical deduction. There are individuals who think they possess a triple personality; thus, a priest got himself so involved in the mystery of the Trinity that he believed himself to be three distinct persons, and wanted three places laid for him at table. Here we see the above-mentioned physiological interpretation of hypo-consciousness carried at once *ad absurdum.*

Hypnotism has, nevertheless, had its influence on physiology, if only more in a negative way, in that physiological explanations have had to give way to psychological ones. At the same time, it must certainly be admitted that psychical influences are ultimately physiological processes, only we are to-day so far off any possible means of explaining this, that it is better to keep our conception of them untrammelled by physiology. In former times the possibility of explaining such states as sleep by means of chemical and physical theories was often exaggerated. Nevertheless, it is impossible to establish a satisfactory theory of sleep without taking psychology, and suggestion in particular, into consideration. This is not to be taken to mean that we look upon sleep itself as a phenomenon of auto-suggestion—a view to which some people incline—but that the psychological factors must invariably be considered in conjunction with the physiological; both play a part. The fact that excitement will banish the severest symptoms of fatigue instantaneously, discloses the psychological factor; and the fact that sleep becomes a necessity after a long vigil probably has a physiological explanation. It is the same with muscular fatigue. Vignolle has recently published a work on fatigue, in which he very properly separates the subjective moment in the production of fatigue from the objective. Even if muscular fatigue is brought about by chemical and physical changes, it is, nevertheless, greatly influenced by psychical processes. The thought of soon getting tired may

bring on the feeling of fatigue. It is, for example, in consequence of psychical processes that people who are thoroughly worn out by a long tramp lose the feeling of fatigue without taking the rest necessary for the elimination of the "fatigue-stuffs"—a phenomenon often observed, for instance, when the goal is nearly reached. There are many other cases in which it could be shown that the study of hypnotism has diminished the domain of physiological theories in this connection, in that it has demonstrated the importance of suggestion, and especially that of auto-suggestion.

CHAPTER XII.

SOME FURTHER ASPECTS OF HYPNOTISM.

It is evident from explanations given in the earlier parts of this book, but more particularly from the last chapter, in which I spoke about the effect of suggestion on masses of people, that hypnotism has contributed very essentially to the elucidation of the history of civilization. This refers most particularly to that domain of the history of civilization that we know as superstition. I have already discussed in the ninth chapter what hypnotism has done towards rendering medical superstition intelligible. In the final chapter I shall return to the connection between hypnotism and some of the phenomena ascribed to occultism. But there are, moreover, numerous other domains of superstition into which hypnotism has given us a clear insight.

I am here going to call attention to one of the most remarkable chapters in the history of civilization, I mean the superstitious belief in witches that obtained during the Middle Ages and the first centuries of modern times. Not only has hypnotism enabled us to understand the universal diffusion of the belief in witches by demonstrating the suggestibility of large masses of people, but it is also due to modern researches in hypnotism and hysteria that light has been thrown on the mental state of the witches themselves. We are now justified in tracing to auto-suggestion in auto-hypnotic and similar states the witches' belief in their intercourse with the devil. Macario thinks that the fixed ideas and sense-delusions of the witches were the result of erroneous sense-perceptions during sleep, and that the witches were so firmly convinced of the reality of their delusions that they faced torture and even death without flinching (Sante de Sanctis). Lovatelli points out in her *Roman Essays*, one of which is devoted to the dreams and hypnotism of antiquity, that the belief in *incubi* and *succubæ*—male and female dæmons

whose imaginary embraces were due to disturbing dreams—already existed among the old Chaldæans and Babylonians; the *amours* with devils reported in the Middle Ages were nothing but a revival of this ancient belief. Paul Blum also touches on the antiquity of such phenomena in his dissertation on psychical anæsthesia. Greeks, Romans, and also Buddhists, would have exhibited these phenomena just as witches, magnetized persons, and hypnotics have done.

The lethargic, cataleptic, and somnambulic states, which hypnotism likewise explains, and whose victims have often enough acquired a reputation for special sanctity, provide a kind of counterpart to the witches damned by the Catholic Church. One has only to peruse the literature of the bygone days to see what an impression a girl made who lay in a deep sleep for weeks and months, and how any one who fell in convulsions before a sacred picture was looked upon as a being favoured of God. Or let us take those cases of auto-somnambulism which have so frequently obtained for their subjects the fame of being divinely inspired. Felix Rocquain has devoted a chapter of a recent work, *Notes et Fragments de l'Histoire*, to the question of hypnotism in the Middle Ages; he thinks that hypnotic states would account for phenomena that have hitherto been disputed and ascribed to fraud, as, for example, the case of Marie d'Oignies, who lived in Belgium in the thirteenth century, and acquired great celebrity by falling into states of ecstasy, in which she was so animated with a burning desire to give a representation of the wounds of the Saviour on her own body that she hacked herself with a knife. A reputation for special sanctity was even more common in the case of those auto-somnambulists who delivered addresses which conveyed to pious minds the impression of divine, or at least supernatural inspiration. Believers in religion and spiritualists are alike in this respect. We know with what earnestness the poet Brentano listened to and recorded the sayings of Katarina Emmerich, the stigmatist; and the speeches of mediums are believed by spiritualists to emanate from spirits, whereas they are in reality nothing but the rhapsodies of persons in a state of trance—*i.e.*, in hypnosis, as Thoma very properly assumes from his analysis of a special case. The circumstance that these people often speak quite fluently, in spite of their lack of education, conveys, to uncritical audiences, the impression of special inspiration, although we have no

difficulty in explaining that all mediumistic eloquence is due to the fact that a hypnotic subject is often much more loquacious than a man who is awake. Some cases also present an increased power of recollection, like that occasionally described as occurring in hypnosis, and hyperæsthesia of the organs of sense. Bastian has pointed out that a similar heightening of the faculties is also observed in the auto-somnambulic states of uncivilized peoples. Nowadays, hypnotism certainly enables us to understand these observations, but we must add that most of the reports dealing with the heightened faculties of auto-somnambulic individuals are so devoid of criticism that they afford no basis for scientific discussion. An example of this:—A blacksmith's apprentice, Köhn, aroused considerable attention during the middle of last century at a place near Dantzig, by delivering religious addresses in which he repeated passages from the Bible with literal accuracy when in the somnambulic state. Beesel furnished a report of this case to a medical journal in 1853, and remarked how wonderful it was that a man like Köhn, who was unacquainted with the Bible, should deliver such addresses, but he adduced no proof that the somnambulist had not previously read the Bible. It is just the same with many other cases in which special capabilities have been attributed to somnambulics. Lehmann's contention that the existence of higher intellectual power in somnambulism has never been attested by persons whose discrimination and judgment could be relied on, is at least true in the majority of cases. The literature of the question shows that the miraculous mental performances of somnambulists were hardly ever submitted to real scientific control. Such control is often quite out of the question—as, for instance, when a medium describes what it looks like on the moon or the planet Mars. That credulous spiritualists should believe such statements is not to be wondered at. But it is not to be denied that people occasionally exhibit somewhat greater capability in hypnosis than when they are awake. But even if this is the case, it can be explained by the special characteristics of hypnosis which have just been mentioned, more particularly the greater freedom of movement, the increased power of memory, and hyperæsthesia of the organs of sense. We can likewise trace to hypermnesia those cases in which a medium makes a speech which he would be incapable of delivering if he were awake; he remembers things that are

completely forgotten in the waking state. I may here refer the reader to what I said about this on page 126 *et seq.* There is no credible case on record in which a medium has been proved to have delivered a speech which he had not learned beforehand.

There are, in this connection, many other phenomena that can be explained by hypnosis. Of these I may mention the assertion that fakirs and ecstatics do not bleed when pricked with a needle. Lloyd Tuckey says he has observed the same thing in the case of deeply-hypnotized subjects (*cf.* p. 107).

In the foregoing paragraphs I have purposely discussed some phenomena which belong to the domain of spiritualism, but which hypnosis explains in the simplest manner. It used to be assumed that mediums simulated a state of trance on the ground that no such state really existed, but hypnotism has shown this assumption to be erroneous, though it must certainly be admitted that a state of trance is very often simulated. The foregoing remarks ought also to show that hypnotism has contributed in many other ways to the refutation of spiritualism. And it ought not to be considered a mere matter of chance that spiritualists, and more particularly mediums and their accomplices, find critical investigators the most unpleasant experimenters they have to deal with—people who should be kept away from spiritualistic *séances* at any cost. Mediums know quite well that a man who understands hypnotism and is also a critical observer—there are people who understand hypnotism and yet suffer from the same auto-suggestions as spiritualists—is best able to detect those sources of error which have to be considered.

I will now make brief mention of another phenomenon which has been wrongly interpreted and ascribed to fraud, because it did not appear easily explainable—I refer to the bleeding of the skin, which has played a part in the mysteries of the Catholic Church. It is reported of stigmatics that they exhibit bleeding of the skin in places that correspond to the wounds of Christ. It was first observed in Francis of Assisi. Bournet, who gives a detailed account of the case, asserts that the assumption that the phenomenon was the result of heightened imaginative power was first put forward by Jacobus de Voragine in the thirteenth century. As time went on, the number of stigmatics increased very considerably. According to Imbert-Gourbeyre, there had been one hundred and forty-five

stigmatics up to 1873, in which year, he adds, eight were still alive. The best-known case of modern times is that of Louise Lateau, of Bois d'Haine, near Mons, who was much talked of in 1868. The anatomical process in her case was rather a complicated one (Virchow, Lefebvre). Blisters first appeared, and after they burst there was bleeding from the *corium* (true skin) without any visible injury. In the descriptions given of other stigmatics, we also find very complicated processes described as stigmatization. In the case of the stigmatic of Gendringen, described by Welscher in 1844, the bleeding was preceded by the formation of blisters. Delbœuf and others believe that the phenomena of stigmatization were due to auto-suggestion. Louise Lateau directed her attention continually to those parts of her body which she knew corresponded to the wounds of Christ, and the anatomical lesions resulted from this strain of attention, just as they did from external suggestion in other cases. Virchow thought that fraud or miracle were the only alternatives; but it must be added that Louise Lateau was supposed to have refused all nourishment. Warlomont decided, after personal investigation, that fraud was impossible. It must be admitted that no exact investigation, such as science demands, was undertaken in the case of Lateau, and my own examination of other such cases has never revealed one which had been submitted to the unbiassed investigation of true science. At the same time, it must be admitted that the mechanical production of such hemorrhages is within the limits of possibility. On the other hand, if we take the results of modern hypnotic research into consideration, it cannot be denied that the bleeding might have been the result of auto-suggestion, as pointed out above, but even then we should have to remember that the ecstasy of the stigmatic bears a great likeness to hypnosis—both states are possibly identical (Mantegazza). Nevertheless, the auto-suggestive interpretation must be considered justified. And Nussbaum thinks it does not run counter to the teachings of the Catholic Church if we consider that the pious maidens who became stigmatics were so deeply embued with the impression that they felt the pains and exhibited the five wounds of the Saviour, that real drops of blood oozed through their skin.

The Catholic clergy, many of whom, like Sancha Hervas, condemn hypnotism altogether, object to the identification of stigmatization with bleeding from the skin. Méric asserts that

the stigmatics, so far from being in an abnormal state, were quite awake. But apart from the fact that this would not traverse the assumption that auto-suggestion produced the bleeding, Méric's supposition is undoubtedly wrong. It is perfectly clear from the descriptions given by various stigmatics that their state was abnormal. As far as Louise Lateau is concerned, she was in a state closely related to hypnosis; that is, if we take it for granted there was no fraud in the case. There was even some *rapport*, for Lateau, like a hypnotic, spoke to certain persons only. Other objections to the explanation afforded by suggestion are also unwarranted. Gombault considers stigmatization a supernatural (*préternaturel*) process. He is opposed to Surbled, who, like himself, did not admit the psychical origin of stigmatization, but, nevertheless, thought that although a scientific explanation is still wanting, the future may well be expected to supply one. Such a standpoint, which is in itself thoroughly logical, Gombault considers mistaken; he thinks that if stigmatization cannot be explained in the present day, for that very reason the view that it is of supernatural origin is the only one that is warranted. In total opposition to this view, we are to-day much more justified in asserting that hypnosis and suggestion have provided us with a possible explanation of the phenomenon.

The same remarks apply to the reports of other miracles, even if believers very frequently controvert the explanation provided by suggestion or other similar psychical influences. Lelong, for example, does this with regard to the psycho-therapeutic view of the cures which have occurred at Lourdes; but what he advances as disproof will hardly convince a scientific investigator—viz., a child that was supposed to be dying was plunged into the ice-cold spring in February and got well. For Lelong this can only be explained by a miracle. Now, it is exactly the study of hypnotism and suggestion that proffers numerous inducements for accepting a scientifically psychological view of the action of Lourdes on patients. One of these inducements—viz., the psycho-therapeutic factor observable in the action of Lourdes, I have already discussed on page 355. As the result of a thorough investigation of original sources of information, Rouby has recently published *A Study of Lourdes*, in which he describes how that place came to acquire its renown. He traces everything to the fact that in the case of Bernadette Soubirous hysteria was overlooked at

the time. It is usually related of this girl, that in 1858, when she was fourteen years old, she saw the Virgin Mary in the grotto. Rouby proves from authoritative sources that one day when the child was gathering sticks she stayed somewhat behind her companion, so as to take off her shoe, and hearing a rustling noise, looked up. When asked by her sister what was the matter, the child at first only said she had seen something white. Later on this developed into a marvellously beautiful lady of medium stature, clothed in a white dress with a blue girdle, and to this story fresh details were gradually added from time to time. A simple noise had caused a sense-delusion in an obviously morbid child! It is not exactly improbable that the girl did at first actually see something white, and that afterwards all the other details were worked into the story by means of suggestive questions. It is extremely instructive to observe how a harmless incident has made Lourdes what it is to-day, and naturally the suggested therapeutic action of a locality so glorified was bound to keep the renown of the place ever on the increase amongst emotional believers. It is probable that the Oracles, Temples of Sleep, and other holy places of antiquity were created in a very similar manner.

In hypnotism, we may, perhaps, yet discover the key to many other miracles. Among such is the changing of a serpent into a rod, mentioned in the Bible. Verworn reports in his work on the so-called hypnosis of animals, that when experimenting with an Egyptian spectacle-snake, he observed that the snake lay motionless if adroitly seized at the back of the head and the nape of the neck pressed with the fingers. He connects this state of the snake with the changing of a serpent into a rod. Moreover, he himself, in contradistinction to other investigators, does not reckon these states of animals as true hypnoses. If these and other miracles mentioned in the Bible admit of an easy explanation through researches based on hypnotism, the attacks which some people have on that account made on investigators of hypnotism are surely quite unjustified. Rohnert and Ziegler, for example, are particularly opposed to any attempt being made to explain the miracles of the Bible by hypnosis. They consider it disparaging to Holy Writ. Even if, as is only natural, nobody's feelings should be wounded by the scientific explanation of miracles, we ought not to prevent investigators to-day explaining in a scientific

way things which at one time passed for miracles. The piety of people who after earnest deliberation can yet see in this any danger to religion, must be only very superficial. For certainly some people, and perhaps not the most irreligious, consider it the most miraculous work of the Deity that all things follow immutable laws. From the standpoint of religion, the scientific explanation of miracles—*i.e.*, the explanation according to the law of causality, will appear much more sublime than the dogmatic acceptance of insignificant wonders. At all events, the assumption that hypnotism is undermining religion by explaining miracles, is quite untenable.

In the foregoing paragraphs we have become acquainted with the connection of hypnotism with the history of civilization, and there is just such a connection with ethnology. Moral contagion, and the suggestibility upon which its efficacy depends, plays a great part in the morals, customs, and actions of uncivilized races. Vierkandt denotes it absolutely as an important advance in ethnology, that more attention is paid to the action of emotion, of suggestion, and of the prevailing habits of thought than was done formerly; moreover, he openly expresses the opinion, suppressed by so many people, that our own convictions also, and the actions that proceed from them, are frequently on no higher level than those of yore. But there is another special part that hypnotism plays with respect to ethnology, and to this Vierkandt has likewise called attention. He ascribes to suggestion an essential influence in producing the belief in sorcery, that certainly constitutes so essential a portion of the domain of ethnological research. "Human beings, especially when in a state of great excitement or of mental disturbance caused by the prevailing idea, often believe themselves really changed into animals. People who have unwittingly eaten tabooed fruit have sometimes died when their offence was subsequently made known to them. It has frequently been proved that the knowledge of being bewitched can make people really ill. On the other hand, we see even in the present day how belief in the powers of sorcery can free from disease." Bartels, also, in his *Medizin der Naturvölker* gives many details concerning the suggestive power of magic practices. These and many other considerations prove, without further trouble, how hypnotism has assisted ethnology, and, more particularly, ethnological psychology.

Long ago, Stoll had supposed that ethnological psychology

would be furthered in a twofold respect by hypnotism and the study of suggestion; (1) suggested sense-delusions in the waking state would be explained, (2) wholesale suggestion would be rendered more readily comprehensible. And Regnier had expressed similar views in his *Hypnotisme et Croyances anciennes.* He thought that if hypnotism were studied, our comprehension of ancient magic, in particular, would be essentially facilitated. In hypnosis those phenomena are artificially produced which played so great a part in ancient magic in cases of somnambulism with its accompanying sense-delusions. Here also would belong the marvellous things we hear reported about fakirs and yogis, about which I have already spoken on page 203 *et seq.* It is, especially, certain states of sleep presented by these people that we may count as phenomena of auto-hypnosis. How far the other marvels we occasionally hear reported of these people—for instance, their being buried alive, power to make plants grow artificially, etc.—depend on truth I will not here decide. I will only mention that, according to communications I have received quite recently, probably all the miracles of the fakirs and yogis would be as little able to withstand an investigation conducted on modern lines as are the miracles of our spiritist mediums. The mysteriousness of India and its remoteness are obviously extraordinarily favourable conditions for the cultivation of legends.

One such fakir, who enjoyed a certain amount of celebrity, appeared on closer acquaintance to prove a very good man of business. I have received the following report about him. The miracle performed by this fakir was said to consist in his lying perpetually on a bed made of nothing but thorns. It appeared on closer inspection that there were no thorns in the bed—or, more correctly, that the thorns were very blunt. Also, the man himself never lay on the bed except when paid for so doing. As a rule, he raised the objection that it was not quite his time for lying down. But even when paid he invariably remained but a few minutes on the bed. The mystery of the thorns that did not penetrate his skin is therefore very easily explained. At all events, my informant has never yet spoken to any one who had seen the man lying on the bed except when he had been specially requested to do so. The same gentleman, who knows India thoroughly, told me he considered it quite out of the question that fakirs would ever really submit to being watched under the strictest scientific conditions; such a thing would be quite contrary to the Indian national character.

The foregoing explanations alone ought to show what a great advantage we may expect to gain in the fight against superstition

by studying hypnotism and suggestion. When Virchow was as afraid of the blossoming forth of hypnotism as he was of that of animal magnetism and homœopathy, it was obviously because he was too much accustomed to pathological anatomy to be always able to find the right standard by which to test psychological questions. In the final chapter still further material will be produced to show that it is exactly the theory of suggestion that has exposed the most important sources of error in investigating occult phenomena. For this reason, Henning in his book *Wunder und Wissenschaft*, like Lehmann in his *Aberglaube und Zauberei*, makes a thoroughly appreciative use of hypnotism and suggestion in explaining the most diverse forms of superstition.

The use of hypnotism in education has also been frequently discussed. Cases of masturbation have been treated by hypnotic suggestion. Bérillon employed suggestion to cure the habit of biting the nails (onychography), which is said to be particularly prevalent among degenerates, and, according to J. Voisin, is often associated with masturbation. Here also would belong cases in which exaggerated bashfulness and timidity, particularly the dread of being looked at, about which Hartenberg, Bechterew, and Soukhanoff have written, were treated by hypnotic suggestion. Most of the cases set down as coming within the province of the educational use of hypnotic suggestion might with equal justice be reckoned medical cases. The distinctions here made are rather arbitrary. When a child is attacked with involuntary movements through imitating choreatic movements, it is difficult to say where the evil habit ends and the disease begins. It is indifferent whether we say that hypnotism is used in such cases to cure disease or in the interests of education; the point is to know what is meant. There is nothing opposed to this in the view held by Forel, Dekhtereff, and probably all other serious investigators, that the use of hypnosis for educational purposes should be reserved for medical men experienced in this domain, and that laymen should not be allowed to hypnotize for this purpose, as was proposed by Decroix. When an anonymous German author thought he made the question ridiculous, or refuted the adherents of the educational use of hypnotic suggestion by banishing hypnotism from the schools, he was simply combating a proposal that had never been made. Bérillon,

Hérment, Netter, Leclerc, Ladame, Brunnberg, A. Voisin, Collineau, Sinani, Natanson, Pamart, and Pigeaud, who devoted his thesis, *La Suggestion en Pédagogie*, Paris, 1897, to the question, merely mean by the educational use of hypnosis that certain faults in children, which many people consider actually pathological, should be cured by medical hypnotic suggestion. According to Bérillon, the chief value of this is that it enables us to combat automatisms by the influence of suggestion on the inhibitory centre. Whether hypnotic suggestion produces great results in such cases is another question. Binet was probably right in severely criticizing the enthusiastic report in which Luckens recounted his impressions of a visit to Bérillon. Nevertheless, we shall be able to obtain good results from hypnotic suggestion in some cases, if we only apply it as indicated above, either for pathological phenomena or for such as lie in the borderland between education and therapeusis.

Only, we must avoid all exaggeration. Some people have even imagined that the hypnotic state could be used to learn a language quickly, because the accompanying hypermnesia would prove of great assistance. And an American doctor named Quackenbos asserts that he has cured certain defects of character by means of hypnotic suggestion: untruthfulness, kleptomania, alcoholic tendencies and murderous impulses, want of respect for superiors and uncontrollable impulse for play, all these can be overcome by suggestion. Low impulses and dirty tendencies can be transformed into noble characteristics. Shakiness in syntax can be changed into correct grammatical English, and a tendency to slang into elegant speech. I have only given a small selection from all that Quackenbos has achieved! In 1903, the Dutch Society for the Protection of Children applied to several investigators — Winkler, Schuyten, and Renterghem—for their opinion on the question. Renterghem replied that he had seen good results obtained in cases of various bad habits (onychography, masturbation), but Winkler expressed a fear that if it were suggested to a child, "Thou shalt not steal," only the word steal would be remembered. Schuyten declined to answer the question on the ground of inexperience, but stated he was very sceptical as to the educational use of hypnosis.

Even if I believe that the educational use of hypnosis only concerns us in the manner mentioned above—*i.e.*, that its province is a very limited one—I nevertheless believe, on the other hand, that the frequent objection (Blum, Seeligmüller) that hypnosis would turn children into machines instead of human beings is erroneous. Hypnotic suggestion and suggestion out of hypnosis, and also education in general, have all

the same aim—to determine the subject's will in a certain direction. Just as we endeavour in ordinary education to lead the subject to do right of his own conscious will, and not on compulsion, so is it with hypnotic suggestion. In the latter we endeavour to let the external suggestion become an auto-suggestion. Even if people often oppose the teaching of suggestion, experience and unprejudiced investigation show that numerous suggestive factors co-operate in every system of education, even the best. In a work devoted to the physiology and psychology of attention, Nayrac has discussed, among other things, the cultivation of the attention where it is morbidly impaired. Even if he is very reserved in his remarks on hypnotic suggestion, he nevertheless ascribes a prominent part to waking suggestion in such cases. I, also, believe that hardly anything will ever be accomplished without the latter.

The relations of hypnotism to Art have often been discussed, and the hope has been expressed that it would lend its help as an incentive to art. Braid discussed the influence of music on hypnotics in his *Neurypnology:* music enabled the hypnotic to move most gracefully and dance to perfection. Hypnotics were also enabled to maintain a definite posture without any exertion, and Braid even thought that the Greeks owed the perfection of their artistic skill in sculpture to hypnotism. The achievements of the Bacchantes, also, were due to the hypnotic state in which they must be assumed to have been, because, as Ovid said, *non sentit vulnera Maenas.* Ordinary people of no education moved in hypnosis with the grace of the most accomplished ballet-dancer. Braid went so far as to construct a connecting-link between the art of dancing in his day and the dance executed by hypnotized persons in the ancient Greek mysteries.

Braid's observations should arouse interest, especially considering the appearance of sleep-dancers in recent years. As far as these sleep-dancers are concerned, Madame Magdeleine G. has caused the most sensation. Endeavours were made to utilize her for the study of certain problems, sometimes of a scientific and sometimes of an artistic nature, and stress was laid on the following details as being particularly noteworthy. In the first place, the gracefulness and expressiveness of her dancing, and particularly of her mimicry, were said to be so perfect that the like had hardly ever been seen before;

secondly, it was said that the talent for this only came to her in hypnosis, and that it was, in fact, aroused sometimes by the influence of suitable music, sometimes by the influence of suitable words, which, for example, expressed the emotion to be depicted; thirdly, it was expressly stated that the lady had no knowledge of music when in the waking state; and fourthly, that the very perfection of her movements first appeared quite spontaneously in hypnosis, and had not been taught her in any way.

When calmly considered, the only thing that remains of all these assertions is that most people acknowledged that the lady knew how to express emotions in the most exquisite way by mimetic and other movements. With regard to the investigations of experts, we may also take it as proved that the lady is hypnotizable, and at least sometimes showed her artistic skill when in the hypnotic state. In this respect, at all events, the investigations of experts are more trustworthy than the *à priori* decisions of laymen, whether the latter sport a doctor's hood or not. Of course it does not follow from these investigations that Madame Magdeleine G. was always in hypnosis when she appeared in public. But apart from these two facts (hypnotizability and an exquisite skill in the portrayal of emotions), everything else that has been reported about her achievements is to be considered unproved. It has not been proved that she only possesses this artistic skill when in the hypnotic state, for Schrenck-Notzing's apodictical assertions on this point cannot take the place of proof. One is so easily led astray. I myself remember a subject who, when hypnotized, showed marvellous skill in representation in response to the most diverse suggestions--changes of character, for instance. For a long time I thought that it was only possible for the subject to represent the most diverse characters and emotions when in hypnosis; but one day it turned out that the subject possessed an extraordinary talent for this sort of thing in the waking state as well, and that all that was added in hypnosis was the possibility of situations being called up by suggestion. Everything else was just as possible in the waking state. Now, when we come to consider that to make a sensation of the whole affair by laying stress on the hypnosis was always to the interest of Madeleine G. and her impresario, the magnetizer Magnin, it follows that we must greatly distrust all unsubstantiated statements to the effect that the woman did not possess

the same talents when in the waking state. It has just as little been proved, and, moreover, was never asserted by Schrenck-Notzing, that the lady is not musical, or is less sensitive to music in the waking state. There is, also, no proof whatever that she had not received a long training in the art of expressing various emotions. Certainly Schrenck-Notzing has accepted these assertions of the impresario, Magnin, as accurate. Schrenck-Notzing says:—"No instruction had ever been given in dramatic art or in ballet-dancing. . . . Magnin tried the effect of music on the sleeping somnambulist; and at the very first of these experiments she passed into a state of active somnambulism, and accompanied the sensations aroused in her by the music with the most exquisite plastic poses and a dramatic skill in expressing emotion that far surpassed anything that the highest order of acting is capable of achieving. Her phenomenal dramatic skill is consequently a ready-made gift of nature. In all she can do she has received but little assistance from practising and developing her latent talents." So many sentences, so many assertions, which is all the more striking in the case of Schrenck-Notzing, since he never seems able to be suspicious enough when dealing with the experiments of other investigators. I call to mind, for instance, his thoroughly justifiable criticism of the statements made about the production of organic changes by suggestion. Schrenck-Notzing will doubtless understand that we do not yet consider his confidence in the trustworthiness of his subject a proof of that trustworthiness.

As far as his statement that she had received no instruction whatever is specially concerned, it has nevertheless certainly been proved that she comes of a dancing-master's family. I am inclined to doubt whether that exactly proves that she had received no instruction. Moreover, Lœwenfeld also takes it for granted that at least Madame Magdeleine G.'s capabilities were specially cultivated when she was in the hypnotic state.

Thus much for the actual material, which has not brought us any essential scientific benefit. For it was known long before Madame Magdeleine G.'s much puffed performances took place, that music, either with or without hypnosis, could cause susceptible persons to display emotion. I here refer the reader to what I said on page 143 *et seq.*, to which I may add a recent observation made by Pamart. The latter saw a lady, who was listening to a pianist playing a piece by Beethoven, approach

the performer with staring eyes and sink down weeping beside him, in a state of complete catalepsy. Numerous other cases of sleep-dancing have also not proved of any greater importance to science. At the most, we need only feel interested in the infection which one such case can spread. Madame Magdeleine G. was followed by a whole series of sleep-dancers. We also heard of a lady who could draw in her sleep, and another who could ride in her sleep. The latter, who had never been on a horse before, suddenly developed into an accomplished horse-woman in the somnambulic state. In conversation with me, an expert to whom the lady had referred certainly denied *in toto* that she possessed any such skill.

As far, then, as the relation of hypnosis to art is concerned, we must pay no attention to the case of Madame Magdeleine G., which caused such an unreasonable sensation, but we must discuss the question generally, for the very reason that it has never been proved that Madame Magdeleine G. had to be in hypnosis for her to represent emotions and feelings.

In itself, there is no objection to the opinion that hypnosis may be used for artistic purposes. Not only are we able to succeed in the arbitrary suggestion of emotions and feelings—*i.e.*, to produce such experimentally—in the case of a suggestible person in hypnosis, but we have also to consider that the inhibition which is often caused by the surroundings is less likely to occur in the case of a hypnotic than in that of a waking person. We can therefore understand that under circumstances a hypnotic may be able to pose as a good model for the representation of feelings and emotions. That hypnosis itself can be used for artistic representation goes without saying, and has already been mentioned by me (p. 32). At all events, the possibility of utilizing hypnosis cannot be denied. The representation of the emotions given us by Charcot and Richer, Luys, Rochas and others should certainly favour this possibility.

As far as the further relations of hypnosis to art are concerned, Lœwenfeld mentions a case of Dufay's, who sent an actress to sleep and suggested that she should undertake, for a sick colleague, a part she had never studied but only seen played. The actress played the part excellently, and it was necessary for Dufay to wake her after the performance was over. However, one would hardly be so ready to venture on such an experiment. But, as Lœwenfeld also insists, there is

another direction in which the use of hypnosis for artistic purposes might well be considered, and this brings us into touch with a domain that is closely connected with medicine and education. Actresses and other artists have not infrequently expressed a wish to be relieved of their fear of "going on" by means of hypnotic suggestion. It is undoubtedly possible to do this in a whole series of cases, and we certainly have a right to accede to such a request even when the nervousness has not quite reached a pathological stage. Like many others, I am convinced that a very great service might be rendered some persons in this way.

Thus much about the relation of hypnosis to art. I may add that I have not here discussed the use of hypnosis in belletristics, because I have already mentioned the chief points of that question in the first chapter (p. 32 *et seq.*).

CHAPTER XIII.

OCCULTISM.

In this chapter I shall treat of the so-called phenomena of occultism, which, notwithstanding the absence of all internal affinity, are constantly mentioned in connection with hypnotism, the connection being for the most part merely determined by their historic development. Though I for my own part consider the deductions drawn from such observations to be inexact, I am yet of opinion that they demand unprejudiced investigation, all the more because of the greater service which by a scientific refutation we may render truth, than by simply dismissing the question on *à priori* grounds. I am strengthened in my conviction of the expediency of investigating these phenomena by my further belief that this course alone will afford us the means of successfully opposing the uncritical occultism of the present day. It cannot be denied that the occultic movement has now become very widespread throughout the civilized world; no less in America than in Europe, and in Protestant as much as in Catholic countries; to the same extent among nations accounted liberal, such as England and the United States, as among those which, like Russia, still seem to belong to the Middle Ages; everywhere occultism has been steadily gaining ground within recent years. Further, this interest pervades the most widely differing strata of society. Among the aristocracy of birth, in the first place, we find it largely represented. Occultic, and more especially spiritistic tendencies extend to the highest circles, and now—as under King Frederick-William II. and the Emperor Napoleon III., for instance—find at many of the Courts of Europe powerful support. Next, we must mention the plutocracy, in this as in so much else a close imitator of the aristocrat nobility. Let it not be thought, however, that leanings to occultism are confined to these upper ranks of society. On the contrary, a host of small officials,

schoolmasters, and tradespeople are to be met in its camp, whose forces are, in a word, recruited among all social classes.

It is a noteworthy circumstance that some really eminent scholars may be found among them. Just for this reason I advocate careful scrutiny of the problems and exposure of all sources of error. The names of these individual men of learning being constantly invoked by other occultists as a guarantee of their testimony, this circumstance, together with the natural bent of mankind to accept authority in matters of belief, constitutes a very real danger. The fact is perpetually overlooked that a man may be an authority in his own sphere without having the slightest claim to be considered an expert in any other, and that the foremost scientists, should they momentarily betake themselves outside their special department, are often utterly at a loss how to avail themselves of methods of research so different to those employed in their own daily work. Mere superficial observation does not take this diversity of method at all into account. Even the scientists in question are apt to be themselves completely unaware of having entered a foreign domain. In reality, however, their ordinary methods may differ fundamentally from those which they are suddenly called upon to apply. A few examples will serve. Lombroso, in dealing with Eusapia Palladino, fancied himself specially qualified for the detection of imposture, on account of the experiences concerning simulation which his work as a psychiatrist led him daily to collect. As if the trickery of spiritistic mediums had anything in common with the simulation of persons supposed to be mentally afflicted. With regard to such tricks, conjurers are the proper experts, not alienists. The same may be alleged against Wallace, who has affirmed the trustworthiness of spiritistic phenomena. Specially convincing appeared to him the experiment of a certain Mrs. Marshall, in which a blank sheet of paper placed under a table showed, when withdrawn, inscribed upon it the name of a person with whom the medium was supposed to be wholly unacquainted. It was the impossibility of producing the writing mechanically which at the time chiefly impressed Wallace. And yet even here it may be objected that Wallace's statement that a blank sheet of paper was laid under the table by no means suffices to prove that it had not already been written on. The gentlemen who occupy themselves with these phenomena should only take the trouble to acquire a little of

the prestidigitator's art, and they would soon find out into what an entirely unknown world they have stepped.

Stumpf, too, whose report concerning the horse Clever Hans I discussed on page 457, must be reckoned in this category. I reproduced (page 455) this document. It admitted of a two-fold interpretation: either, in the first place, that the horse really did solve most complicated arithmetical problems, that it knew the clock, recognized different people from their photographs; in a word, gave proof of real intelligence, of a mental proficiency which Stumpf described as having been acquired by something akin to the system of instruction at an elementary school. One loop-hole was left—the statement admitting the possibility of occult agency, of a telepathic influence being exerted upon the horse. Stumpf thought himself, as a psychologist, necessarily an expert in the investigation, whilst in reality the method of inquiry requisite here totally differed from any that he was accustomed to follow. Had he but considered the matter objectively from the first, had he but once carefully read through any single piece of the evidence drawn up, not from the point of view of the scientist, but with a man's sound common-sense, he must at once have recognized his error. Must he not have thought it strange that the horse, then only eight years old, should already know more than a child of the same age; that it appeared, in fact, to have mastered the whole German language, which human beings spend long years in acquiring? A current joke of the time was that the horse stood on the level of a good pupil in the fifth class of a German grammar-school—one about to be removed to the fourth. In reality, however, we should have to place the animal on a still higher stage,—unless we accept the theory of a very grave delusion. A protocol placed at my disposal contains, for instance, the following questions:—"Look, Hans, there is the general you know so well; what are the colours of the order he is wearing? tell us the first!" "Hans, a silver five-mark piece is being shown you; how many marks are there in the next smaller silver coin?" Many such questions were put to the horse and correctly answered. If we take further into consideration that it recognized melodies, distinguished one chord from another, could extract roots or square numbers, in a word, solve quite complicated arithmetical problems—remembering all this, it seems impossible that Stumpf should not see the necessity of proceeding

with the very greatest caution before putting his name to such a report. The mere fact of overlooking so important a point as the horse's complete knowledge of the German language is a characteristic example of the ease with which scientists may be deceived, directly they enter a field in which the method of inquiry is quite new to them.

That which happened here to Stumpf took place also in the case of those investigators who entered on observations with the spiritistic medium Slade, concerning, for example, the deflection of the magnetic needle. I shall have to return to this, and will merely remark here that these men were also obliged, even though they may themselves not have been aware of it, to deal with this matter according to methods of inquiry which were perfectly new to them. These investigators were most assuredly most admirable workers both in the study and the laboratory, but there they had merely to contend with error, never with fraud. And now, where it was at all events within the bounds of possibility that they were about to witness a sort of conjuring trick, their science was utterly at fault; they formed a circle, holding one another's hands, doing everything, in fact, that the trickery of the medium demanded of them. The observer, far from making his own conditions, allowed them to be imposed on him, and did not even perceive that he was doing nothing but just what seemed good to the medium. The same thing occurred with Crookes, whose belief it was that spiritistic mediums, and Home in particular, were able by means of a psychic force at their command to make material objects lighter or heavier, and consequently to set them in motion. Never once did Crookes remark that he had entered a sphere of observation in which he was not at least at home. It is Lehmann's greatest merit to have pointed out that those experiments of Crookes, so often brought forward in support of spiritism, really prove nothing at all. From the accounts of the proceedings published by Crookes himself, it is perfectly evident that not only did he insist on no objective conditions at all, but that he simply allowed himself to be completely directed by Home. And there are such fundamental differences between the two accounts, that Lehmann was able from the second publication to furnish proofs of the impossibility that the *séance* should really have taken its course in the manner depicted in the first. He shows that Crookes had not the vaguest conception of the extreme

importance of those particulars which he passed over in his first report. The two descriptions, according to Lehmann, differ so entirely, that had Crookes himself realized those discrepancies he could not have given the one "without laying himself open to a charge of conscious fraud."

It has already been hinted in the above that it is not from men of science, who believe themselves a final court of appeal in these investigations, that a decision is as a rule to be sought, but rather, in very many cases, from the conjurer. I am, however, perfectly well aware that conjurers have been found to maintain the genuine character of the phenomena. Zöllner, for instance, invoked the testimony of Bellachini. But in according this unquestioning recognition to Bellachini's evidence, Zöllner himself entered a territory with which he was totally unacquainted. In the first place, not every conjurer is necessarily a competent judge of his own art. And just with regard to Bellachini, it is well known that he was often quite taken aback by the performance of some new trick. In the profession he was never looked upon as a really quick-witted conjurer. We must, therefore, not consider all conjurers properly qualified to examine occultistic phenomena. The question further depends not merely on the professional competency of the conjurer, but also on his personal character, and more particularly on his regard for the interests of science. Let it not be forgotten that the conjurer regards as a colleague the medium engaged in the service of occultism, and, as we know, *clericus clericum non decimat.* As an instance of the lengths to which *ésprit de corps* may be carried in these matters, let me cite the following:—About fifteen years ago a so-called magnetic lady, Mrs. Abbott, made her appearance in Berlin. The strongest men tried in vain to lift her from the ground; their united efforts were equally unavailing to lift the chair on which she was sitting. In one special attitude it was found impossible to make her fall back an inch; together with more to the same effect. I pointed out at the time that the whole thing turned on a very clever application of the laws of mechanics relating to the lever. To remove all remaining doubt the famous wrestler Abs was called to Berlin, and he declared in public that he found it impossible to raise the woman from the ground. I do not think I am guilty of the slightest indiscretion if I now state that the chief actor in the scene has long since acknowledged the accuracy of my explanation, and admitted

the imposture. And just the same motives which prevented him from exposing it would prevail with very many conjurers under similar circumstances. All this one must remember in judging of the value of evidence from these sources.

I am, of course, not at least opposed to the serious investigation of the phenomena of occultism by real inquirers; I even go so far as to consider this most desirable. Only let one beware of believing oneself an expert, unless one is really so. I hold the impartial examination of these matters perfectly legitimate, and think that all those who, like Wundt, would forbid it, utterly in the wrong. It is customary with *à priori* reasoners to perceive in the mere fact that any one is making a study of either occultism or spiritism a sufficient proof that he is himself a spiritist or occultist. With just as good logic, retorts Dessoir, might we argue that all criminologists must of necessity be criminals. It, unfortunately, happens that in point of fact a really unprejudiced inquiry into occultistic phenomena hardly ever takes place. On the one hand, we have investigators who either overlook or underrate the most important sources of error, and therefore themselves become duped; whilst, on the other, are those who, like Wundt, simply disdain to apply any test at all. Neither course is justifiable. It must, however, be conceded that the occultists themselves hardly ever allow their phenomena to be subjected to impartial observation, and that notwithstanding their loudly proclaimed readiness to court inquiry beforehand, during the *séance* itself some pretext will always be found, either by the medium or his assistant, or by some occultist or spiritist, to upset every attempt at scientific investigation. And yet, whatever the difficulties, it is the scientific method of dealing with such phenomena which alone can enable us to disprove them. In the year 1877, Wieland wrote a little tract on "Magnetism" well worth reading. In it he blamed the general tendency to turn into ridicule everything alike that had to do with magic, apparitions, or mesmerism, and to treat as fools, mad enthusiasts, or impostors all those who occupied themselves with such matters. It is the bounden duty of science, as Wieland at that date already clearly perceived, to give these things due attention; wherever the cry of the were-wolf is raised let the monster be hunted down, and it will invariably turn out to be an ordinary wolf among other wolves, or perhaps rather bigger than the rest of the pack. The task

incumbent on science has been sketched by Wieland in a few decisive words, which to this day retain all their force. Let us not now be too optimistic as to the results of scientific investigation of these phenomena. Mysticism is too deeply implanted in human nature, and as a contemporary current of thought, forms, perhaps, a too inevitable reaction against recent materialism for us to be over sanguine on this score. The fact, however, remains that rational inquiry and explanation are the only weapons with which we may hope successfully to combat uncritical occultism.

The phenomena which I am about to discuss are as follows: (1) animal magnetism, (2) the influence of mineral magnetism on human beings, (3) super-sensual thought-transference (telepathy, *suggestion mentale*), (4) clairvoyance, (5) transposition of the senses, (6) the effect of drugs on approach or contact, (7) occultism in general.

In animal magnetism a leading part is played by the personal influence exerted by one individual over another, an influence brought about neither by suggestion nor by any other psychic agency. The following examples will make this clear.

An individual, A., says, for instance, to another, B., "You cannot speak." B. hears this and is mute. This is suggestion. If, now, A. makes mesmeric passes over B.'s arm, and analgesia results in consequence, this may also be attributed to suggestion, as B. is perfectly aware that A. has made the passes. Let us now suppose that C. also makes passes over B.'s arm, without analgesia ensuing. This, too, may be explained by suggestion, by the belief on B.'s part that A. alone can produce analgesia, in consequence of which C.'s manipulation remains ineffective. Suggestion then will account for all these phenomena. But the case is a different one if B. does not know whether it is A. or C. who makes the passes. According to the believers in animal magnetism, the so-called mesmerists, B. does then experience analgesia when magnetized by A., but not when it is C. who magnetizes. It is just by virtue, they maintain, of a peculiar force residing in him that a personal influence is exercised by A., which neither suggestion nor any psychic process will account for. They alone who are endowed with this force are able to magnetize others. These illustrations may serve to show what is at the

present day understood by the expression animal magnetism (vital magnetism, bio-magnetism, zoo-magnetism, mesmerism). There is, however, a great difference of opinion among believers in this power as to the precise meaning to be attached to it. Some consider it a common property of all mankind, which, however, under unfavourable conditions in many cases remains simply latent. To some its possession implies the capability of influencing all people alike, whilst others, again, hold certain individuals alone to be receptive to this influence. Here, as in the case of suggestion, there are those who admit the theory that an individual who has proved refractory to the experiments of one magnetizer may, after all, quite well be magnetized by another.

They who are endowed with magnetic power are supposed to be enabled thereby to produce certain active results, such as local or general analgesia, or contractions, also either partial or general, on the persons of those magnetized by them. According to Rochas, the subject may occasionally, in spite of analgesia, perceive some stimulus, such as a prick, for instance, at a certain distance from the epidermis. Boirac cites similar instances, without, however, adducing satisfactory evidence that in the carrying out of his experiments proper precautions were taken. Voisin's testimony is negative, whilst Crocq goes so far as to deny the feasibility of utilizing such experiments as the basis of any theory of animal magnetism, pointing out the wide scope for error they afford, it being impossible to control the vibrations of the air and fluctuations of temperature by means of which the magnetized person could become aware of the approach of the needle.

Among the further results of mesmerism must be noted the healing powers to which it lays claim. Magnetism has the power, we are told, of restoring the sick to health. In reply to the possible objection that these cures might be attributed to the influence of suggestion, special stress is laid on the fact that quite small children, infants less than a twelvemonth old, have been successfully magnetized. Liébeault, who published a refutation of animal magnetism in 1866, subsequently, in 1883, recanted, the change in his opinions being chiefly brought about by his observation of the susceptibility of small children to magnetic influences, to which his attention had been drawn by the magnetizer, Longpretz. The cures effected among children had, Liébeault avers, completely convinced

him of the existence of animal magnetism. Even if the greater part of that which was set forth among the claims of mesmerism were easily to be explained by suggestion, there yet, he thought, remained something inexplicable except by the hypothesis of animal or zoo-magnetism,—to adopt the name that had been used by Athanasius Kircher, and afterwards by Bartels at the very beginning of the nineteenth century. Later on, Liébeault certainly gave up his belief in animal magnetism. That in these experiments on children wrong conclusions are often drawn may surely in a great measure arise from the fact that the frequency of spontaneous cures is so constantly overlooked. I have already referred (p. 297) to this very fruitful source of error, which plays a much greater part in misleading us than is commonly supposed.

In support of their view that animal magnetism is something entirely distinct from hypnotism or suggestion, Du Prel and others of its adherents bring forward the following arguments. It is possible, they say, to magnetize animals, whilst with them suggestion is unavailing. I am certainly quite of the opinion that suggestibility, in the sense of the word in which we use it with regard to human beings, is not to be looked for among animals, for the simple reason that suggestion always implies a very complicated psychic process. It assumes the capacity for consciously undergoing a decided change, and I do not believe that the understanding powers of animals—and more particularly of those, such as the cat, dog, horse and lion, supposed to be specially susceptible to the magnetic influence,—are sufficiently developed for this to take place. But I am also equally firmly convinced that certain occurrences, reported as having taken place among animals, and accepted as furnishing proofs of magnetism, in reality afford no proof at all. Here, again, evidence of the healing powers of magnetism has been constantly produced, and here, just as with human beings, the simple fact of spontaneous healing has been too much neglected. Other results cited, such as the soothing effect of patting and stroking a horse or dog, or the fascination exercised by the eye of the rattlesnake, have in reality nothing to do with magnetism at all. For however inacceptable we may find the idea of suggestion as applied to animals, the possibility of very many psychic processes yet remains to be considered. In the results obtained by reward or punishment we see these at work. And

in the guise of magnetism the very same thing often takes place. When we quiet an excited animal by stroking it, the success of our effort is partially to be accounted for by the fact that the animal has never been in the habit of connecting such treatment with impending danger. And in addition to this there are inherited reflex-mechanisms of a physiological nature. Preyer relates in his book, *Die Seele des Kindes* (The Soul of the Child), that an infant, little more than a fortnight old, was instantaneously quieted in a violent fit of screaming by being laid face downwards, on a pillow. He speaks also of the soothing effect of singing, whistling, or a gentle touch of the hand, even on infants yet unweaned. Nor does he at all see in this a reflex-inhibition, but merely the driving out of an unpleasurable sensation, with its motor consequences or reflex activity, by a new sensation. Pflüger had already cited the similar instance of a new-born, brainless infant, when screaming violently, being easily quieted by having a finger given it to suck. In any case, we do not require, in order to explain the soothing influence of the touch of the hand, etc., to assume the intervention of some mysterious magnetic force. With regard to the supposed magnetizing of animals, just the same holds good. The results are to be traced in part to physiological reflex movements, in part to psychological influences. And however gradual may be the transition from the physiological to the psychological action, we are in no way justified in accepting the theory of magnetic intervention in this "magnetizing" of animals.

At all events, there is no series of experiments extant that would compel us to admit the magnetizing of animals. I have myself sedulously read through all the literature bearing on the subject, without being able to find, either among the old mesmerists or their successors, anything that could be termed a scientifically conducted experiment. Everywhere we have the same casual observation, the same detached experiments, lacking all exactness and serious control, everywhere the same disregard of those pitfalls (*e.g.*, spontaneous cures, etc.), against which I have so often warned. In the eyes of the general public, magnetic influence is very important in the relations of man to the animal world. This arose partly from the air of mystery in which trainers of performing animals were wont to invest their craft. They were often led to this by the very natural wish to prevent some very clever trick from being seen through, and

in order to throw dust in the eyes of the spectators they began by ascribing to themselves mysterious powers. I need only recall the extraordinary sensation excited by the performances of the horse-trainer Rarey, some fifty years ago. In his book on the training of unmounted horses, Loiset advised the trainer to insist on being quite alone with his horse while putting him through certain trials, lest the animal's attention should be diverted from its task. This momentary isolation—which is, by the way, a feature in the training of nearly all performing animals—gave rise to innumerable vague allusions to the secret powers and mysterious methods employed.

Du Prel sees a further proof of animal magnetism in the experiments carried out on individuals during sleep. All possibility of suggestion is, he thinks, excluded by the sleeper's own unconsciousness that he is being experimented on. To this argument it must, however, be objected, in the first place, that sleep and unconsciousness are not synonymous terms, as the mere act of voluntarily waking from sleep (p. 164) clearly proves; and secondly, that, as we have also seen (p. 181) in the case of dreams induced by nervous stimulation, a person in sleep may be perfectly susceptible to external influences. With regard to the whole question of experiments on sleeping persons, the same remarks apply which I above made concerning experiments on animals. Any number of sweeping general assertions are made on the subject of influences exerted over people during sleep, and incidentally even experiments quoted with much fulness of detail; but for anything resembling a series of experiments, subjected to strict scientific control, and therefore admissible as trustworthy evidence, we may look in vain. We are constantly assured that such experiments have been successfully carried out, but for my own part, I have been quite unable to find any that afforded exact proof of that which was to be demonstrated.

We are also told that it is quite possible to magnetize people who are themselves unaware that this is taking place, as, for instance, at a great distance. Magnetism must exist, it is argued, to account for this, as for the kindred phenomena of telepathy (super-sensual thought transference) to which I shall return shortly. Then, again, we have the magnetizing of plants, with its stimulating action on their growth, the transmission of the magnetic force to inanimate substances, such as water, which then carries on the action of the magnetizer,—all

these are given as proofs of magnetism. In the same category we find are ranged the following:—the creation of somnambulic states, during which the marvellous phenomena of clairvoyance, transposition of the senses, etc., etc., occur; the attraction exercised over the magnetized person by the magnetizer, the perception of the magnetic fluid by the former, his sudden acquaintance with languages he has never learned, variations in the weight of bodies, said to become heavier and lighter under the influence of animal magnetism, the setting in motion of objects without mechanical means being employed, fluctuations of the magnetic needle, power of destroying animal life by a look,—and much more of the same kind.

Some of these questions I must still deal with. Very nearly all the assertions of the mesmerists I have myself minutely tested, and have given an account of some portion of my observations in my book, *Der Rapport in der Hypnose*, published in Leipzig, 1892. Several other investigators had associated themselves with me in this inquiry into the existence of animal magnetism. On critical examination, the result of all our experiences was negative. The mesmerists themselves constantly misinterpret their own observations. That a magnetized person may at times discern "magnetized" water, is correct. It has, however, nothing on earth to do with magnetism. In the first place, it is very often impossible to prevent a slight rise in the temperature of water that has just been magnetized. Secondly, it is highly probable that in the act of magnetizing, which is generally accompanied with the gesture of flourishing something in the direction of the water, chemical substances may be introduced into the latter, and may bring about an alteration in its taste. But chemical dissociations have nothing in common with magnetism, which is supposed to represent a physical force. This intentional confusion between chemical agencies and the magnetic force is a good proof of the want of clearness prevailing on the subject amongst most mesmerists. Just the same want of clearness distinguishes the attempt to fix magnetic impressions by photography. It cannot possibly be denied that the atmosphere immediately surrounding the body may exercise an infiuence on the photographic plate, that heat rays and chemical rays—*i.e.*, rays imperceptible to human vision—may very often come into play here. But what all this has to do with animal magnetism it is not so easy to see. As a specimen of the utter confusion of ideas prevailing among

mesmerists in general, I need only give my own experience of the trial of the Tilsit quack-doctor, which I attended in the character of professional expert. During the whole proceedings, Schröter, the mesmerist in question, kept trying to convince me that he had a certain magnetic force under his control. The argument on which he chiefly relied consisted in taking in one hand an india-rubber bladder filled with water, which he pressed against the window-pane; he then placed his other hand flat against the pane, and showed that round this one a light film formed, not, however, round the bladder. To this vaporous deposit, produced by cold, Schröter pointed triumphantly as a proof of his own magnetic powers. Exactly the same confusion of ideas attends the photography of irradiations and similar experiments.

For the present only this much can be said: the statements of most mesmerists are extremely confused; things that have no possible connection with magnetic force are cited by them in proof of its existence. Nowhere do we find any series of exact experiments reported, in the conduct of which all sources of error were so carefully excluded as to furnish a satisfactory proof of the existence of animal magnetism. On all sides only unproved assertions.

Mesmerists have propounded quite a long list of theories, only the principal of which I can allude to here. According to Mesmer himself, whose theory has been made the subject of much misrepresentation, the whole universe is filled with a fluid, more subtle than the ether, just as this is more subtle than air, and air more so than water. Vibrations, he maintained, take place throughout this fluid, just as they do throughout the ether, air and water. And just as light is transmitted by the vibrations of the ether, so, he concluded, are phenomena of another nature constantly produced by the vibrations of this all-pervading fluid. On these vibrations he believed the mutual attraction and repulsion of the heavenly bodies to depend, and that they also determine the interchange of attraction and repulsion between bodies endowed with animal life. In the vibrations of this fluid Mesmer considered animal magnetism to reside. Mesmer's theory of the magnetic fluid has often been confounded with another, according to which the nerves of the human body contain a fluid that may be conveyed to the surface by motion from within. This was propagated by the celebrated physiologist of the eighteenth century,

Albrecht von Haller, who even went so far as to set forth against Mesmer claims of priority of discovery, although, as is perfectly obvious, the two theories are quite distinct the one from the other. That the activity of the sensitive nerve-fibres may be exercised at a certain, though perhaps very limited distance, was admitted also by Alexander von Humboldt, and his opinion was concurred in by the well-known anatomist and clinician, Reil. More than once the hypothesis has been put forward of electric activities being called up by the mesmeric passes (Rostan, J. Wagner). Tarchanoff has demonstrated that the application of gentle stimuli to the skin will excite in it slight electric currents, and that, moreover, a strong effort of concentration of the will, with the muscular contraction by which it is invariably attended, will also suffice to produce the same. Now, since mesmerists always insist on the necessity of strong tension of the will on the part of the mesmerizer while making his passes, may not a peripheral development of electricity be induced in his person and passed on to that of the individual he is mesmerizing?

Contenting myself with mere reference to the fact that certain mesmerists (Nasse, Barbarin, and others) do not believe in the existence of the magnetic fluid, I pass over various theories, practically valueless from the scientific point of view.

In support of their contention that the magnetic conditions are in no way connected with hypnotism, mesmerists often bring forward the argument that sleep is by no means requisite for the exercise of magnetic influence. In reply to this, we may remark, that also with persons being hypnotized it is only in the case of a comparatively small minority that sleep is induced (*cf.* p. 60). And in any case, suggestion may, as we have seen, be exercised independently of either sleep or hypnosis. As a further distinction, they are fond of pointing out the important part played by the personality of the magnetizer, in the means he employs. The very use of magnetized objects shows, however, that this is not absolutely correct, even were the magnetizing of the same as essential as, without sufficient evidence, they constantly assert. Not that mesmerists are in the least embarrassed to account for these discrepancies. Should an object, as in following Braid's method, not have passed through a magnetizer's hands, then, as Moricourt explains, it will suffice for the subject to gaze fixedly at it so

that his own magnetic fluid may be reflected therein, and by this he will in turn himself be influenced.

Nor must the simple fact that it is in the power of another person to provoke physiological or psychological results, which we ourselves by the use of apparently precisely similar means cannot obtain, be interpreted as an argument in favour of animal magnetism. Take the well-known example of the laughter occasioned by tickling. We can none of us make ourselves laugh by tickling. And there are plenty more examples of the same kind. I had earlier (p. 85) occasion to remark, how if another person's hand approaches our eye, we have a natural tendency to close it, even if we do not in the least dread actual contact. In another place (p. 400) I pointed to the special results obtained by the frequent repetition of the same words on the part of the doctor; these results the patient is quite powerless to produce, however carefully he may repeat the same words to himself. It is an undisputed fact that by the touch of a sympathetic person a pleasurable sensation may be evoked, and by that of an unsympathetic person a disagreeable one. In the case of local pains and certain other sensations, the touch of a sympathetic person may have a beneficial effect,—not that of an unsympathetic one. It thus becomes evident, that the very same physiological and psychological stimuli, when applied by ourselves, may be very far from giving the results obtained by them when they originate with another person. The whole question still demands much elucidation, although endless physiological and psychological theories have been started for the purpose. That of Demonchy would attribute the sleep-inducing power that lies in the touch of a hand to a merely thermal influence. And yet this would most assuredly not suit all cases, the application of warm compresses, for instance, having very varying results. It is undeniable that purely physiological processes often play here a highly important part. This appears most clearly in sexual intercourse, in which the very same physiological stimulus and the very same psychological process produce quite different results, according to the degree in which they correspond to the feeling of the person employing them. Normal sexual intercourse, even at the supreme moment, never produces complete gratification in a homo-sexual man. And yet here the peripheral stimulus cannot be said to be at fault, but simply the fact that it does not correspond to the feeling in question.

In this, as in other examples given of difference in the results brought about by similar physiological agencies, animal magnetism plays no part. We have in reality to deal here with innate tendencies and psychological processes, a detailed analysis of which certainly at present exceeds our powers. The assumption of the existence of animal magnetism is, however, utterly superfluous, and it furnishes no explanation at all of the phenomena under consideration, whether the reflex-closing of the eyelids, or the effect produced by the touch of the hand or persistent repetition of the same words.

It is surely a somewhat wrong-headed proceeding, if, in order to account for phenomena which are not quite clear to us, we drag in the agency of a perfectly hypothetical force, when we know the whole time that this force, even if its existence were proved, would be incapable of explaining the phenomena in question. Already some time ago, Lemoine, in his book, *Du Sommeil*, laid stress on the fact that the phenomena of magnetic somnambulia, clairvoyance, transposition of the senses, etc., cannot, even if we accept them, be explained by animal magnetism. The attempt, therefore, to make these phenomena, even if we admit their existence, the basis of a theory of animal magnetism, must be distinctly negatived.

To convey magnetic force from one person to another, various manipulations, and more especially the magnetic passes, are constantly employed. At times mere contact, the interchange of a fixed gaze, or light breathing of the magnetizer over the subject, may prove efficacious (Baréty). In conjunction with such manipulations, or even without them, according to some of the older mesmerists (Puységur, Nasse), entire concentration of thought and of the will on the result aimed at is specially recommended, and there are not wanting in the younger school of mesmerists those who consider that this should in itself suffice. In all books dealing with animal magnetism are to be found a number of precepts concerning the best method of magnetizing. Much information is also given about the direction of the magnetic passes. Accordingly as they are made upwards or downwards, and with the palm or the back of the hand, their effect is said to vary. There is also supposed to be a difference observable in their action on the right and left side of the body. Reference to magnetic polarity is also made. The principle had been admitted with

regard to man by Fludd, Heller, Mesmer, and a little later by Scoresby, and has been defended in our day by Chazarain, Dècle, Durville, Rochas and Baréty. But they contradict one another so flatly concerning the divergence of these poles, that I feel constrained for the moment to look upon the supposed polarity as an involuntary outgrowth of professional training,—in other words, as a piece of unconscious self-deception on the part of the experimenter. Baraduc even alludes to an instrument by means of which he thought it possible to measure the exact magnetic relations of human beings one to the other, and to reduce them to a formula.

The doctrine of animal magnetism has been turned to practical account in its use in therapeutics by the so-called magnetopaths or medical magnetizers. Already some time ago, Göler von Ravensburg and others called attention to the numerous sources of error in this field, and these are equally prolific at the present day. Magnetopaths claim that the existence of animal magnetism is demonstrated by their cures, overlooking the very important point that these cures are partially assisted by suggestion, partially by spontaneous improvement in the health of the patient. They also pretend, it is true, to effect cures in cases of such diseases as cancer, tabes dorsalis, etc., incurable by scientific medical treatment. But in spite of all their noisy self-assertion, the true position of affairs is this: not one single well-authenticated case exists of a disease of this nature having been cured by magnetic treatment. To start with, it would be a necessary condition of such cures that a proper diagnosis of cancer, or whatever the disease might be, should be established in an unassailable fashion by scientifically qualified medical practitioners prior to the magnetic treatment. The absence of a serious scientific diagnosis (*cf.* p. 319) can never be compensated for either by those furnished by the patients themselves, or by the magnetopaths, quite capable of taking an inflammation for cancer, or neurasthenia for tabes dorsalis. As to the manner in which their diagnoses are made, a single instance may suffice. A good many years ago I had a visit from a Fräulein von X., "qualified medical magnetizer," as was stated on her card, who wished to convince me of her magnetic powers. When I inquired how she proposed to accomplish this, she assured me that she had quite recently cured a case of erysipelas in the face by a course of magnetizing. To my next

inquiry as to how her diagnosis had been made, she replied:—"It was a red spot that turned white on pressure; consequently it must have been erysipelas." And this is the sort of diagnosis we are asked to accept as evidence of the curative powers of magnetism! The very names given to some of the illnesses are enough to arouse suspicion. Thus one patient is said to have been cured by magnetism of "swelling of the cardiac valves." Another is described as having suffered from "gout in the head"; while in a third case, that of a patient one of whose lungs was seriously affected, a three weeks' treatment is reported to have caused the diseased organ to "scab over." Another patient had "serious oppression of the stomach," and one woman's organs were "all of a wobble." The more appalling the diagnosis, the greater should be our caution in accepting it as correct. Nor must it be forgotten that magnetopaths very often employ other methods simultaneously with their own. They prescribe, for instance, rules of diet which are in reality the principal factors in their treatment, the magnetizing itself having no specific action. But it is to magnetism that the good results are ascribed, which are, of course, in fact due to diet. An alcoholic patient while being magnetized, received the injunction from the magnetopath to abstain from drink. It will be very readily believed that the condition of a man suffering from alcoholism will improve as long as he gives up drinking. Only this is no argument whatever for the existence of animal magnetism.

Magnetopaths constantly complain that science does not recognize their powers. In point of fact one magnetopath does not, as a rule, recognize the power of a fellow magnetopath. The magnetopath A. hears with a pitying smile of the magnetic power over which the magnetopath B. claims to hold control. It may not be uninteresting to my readers if I here reproduce a letter written to an acquaintance of mine by a magnetopath. It shows how these gentry make their diagnoses, and also what one magnetopath thinks of another. The gentleman, who had once been a patient of mine, wrote a description of his malady to the magnetopath and received the following reply:—

"Dear Sir,—I have received your esteemed communication, and will make an exception in your case, a thing I otherwise never do without a fee of twenty marks (£1), because I have been taken in too often by the general public. As your complaint is neither dilatation of the heart nor poverty of blood, kindly give up taking iron, or your stomach will be

completely ruined. Your whole trouble is the result of a perfectly *normal* circulation of the blood caused by the *abdomen*, which does not functionate properly. The best I can say and advise is come to me, then you will *certainly* be cured of your complaint. You should write to me beforehand, so as to enable me to make an appointment, as my services are in *great request.* Yours faithfully, [here follows the signature]. I do not know of a magnetizer named N., in the locality you mention, and I must warn you to be very careful whom you take to be a magnetizer !!!"

This magnetopath also exhibited the virtue of being a friend to his own family. When he was again questioned about a magnetizer in Magdeburg or Münster, he replied, he did not know of any such in those towns: "There are very few trustworthy magnetizers in Germany." To this communication there was also a postscript: "My son Henry can also cure diseases at a distance. You may apply to him here" (address given) "with perfect confidence."

Magnetopaths certainly assert that they can prove their magnetic powers in another way. As a rule, the proof is supposed to be in the results they say they have obtained. As I have already mentioned, the experiments I made in conjunction with other investigators only gave negative results. But since 1892, I have repeatedly expressed my willingness to experiment with persons who believe that they possess magnetic powers, provided the conditions are stringent. Although it is frequently asserted by occultists, and also by magnetopaths, that they are ignored by doctors, hardly any one of them has placed himself at my disposal. The few who have done so came completely to grief. One of them was the well-known Herr Scheibler. When proper precautions were taken, Herr Scheibler could never prove that he possessed any magnetic power whatever. These experiments, to which I shall again revert, could only be explained in other ways. A second gentleman, a well-known Berlin magnetopath possessing an honorary diploma, also placed himself at my disposal. I experimented on him in conjunction with three of my Berlin colleagues, Drs. Gumpertz, Leo Hirschlaff, and Fritz Koch. The experiments led to nothing, a fact which the subject himself had to admit. He had, for example, asserted that he could produce any sensation he wished—warmth, cold, twinges, etc. But every experiment failed when the subject was unaware of the sensation that was intended to be produced. Recently, a doctor who called himself a magnetopath came to me for the purpose of making experiments; but he turned out to be just like other magnetopaths. To him the simplest suggestive influence only meant animal magnetism.

I once received a communication from abroad, in which a magnetopath invited me to make certain experiments. But I must candidly admit that the letter, which I reproduce *verbatim*, did not inspire me with much confidence in my correspondent: consequently I did not see my way to carry out experiments which would be free from reproach. This letter should show the mental level of many of these magnetizers.' The letter[1] runs as follows: —

DEAR SIR,—I have been possessor of your book Hypnotism about a year. In case it may afford you satisfaction, I will bring you a proof of the telepathy p. 323 of your work, that I really have a special power at my command—*i.e.*, even if I only possess such but nevertheless cure the sick of every kind without selected medicines and other manipulations but solely by mere words suspending amulets or as Christ the Lord also did, go and wash thyself seven times in the river Jordan (meaning the same as in the Spree, Seine, Rhine, Danube, or the tub at home) again to another go thy faith hath helped thee, but only where such is certainly present can I assure this but specially expressly for each particular case further this is all on condition the sick person tells me all the circumstances and accessory circumstances upon which I tell him whether a cure can possibly be made by me for death and constant sickness also demand their victims. Moreover it must be mentioned that in certain diseases the patient is not at once convinced I mean, that he is cured, but this differs about eight days to one month or several or even beyond that. This is one and the same power with which Christ healed but it necessitates a specially individual call selection by the Deity which I have specially underlined for your better comprehension in the prospectus enclosed herewith. That I am called I am firmly convinced because the gift of clairvoyance has enabled one to foresee the events of my life for years that something of this kind would become very powerful as it progresses that in time princely equipages will make way for me in the street, even if only spiritually understood and then worldly authority will want to punish me and will not be able to do any harm and all this from the beginning up till to-day lies in the same power described above. Similarly a man of the same kind prophesied of me when I was to be apprenticed to a joiner in answer to my mother's question whether she should put me to it the man mentioned above said yes, but that I would not always remain at it, which proved true in not quite a year. This power I have achieved, the foundation must be innate which must be developed by earnest endeavour reading the Bible this I call light from above for otherwise the words are true they have eyes and see not, ears and hear not, understanding and do not understand and it just the same with that beautiful work 2 volumes Prevorst mentioned by you which moreover perfected clairvoyance, but what does J. Kerner say of the countess who was cured; vol 1. p. 208. Behold here intelligent reader the power of mental correspondence of prayer and child-like faith at the end of his narrative. Now if you would like to have convincing

[1] I do not pretend to be able to do justice to this letter.—TRANSLATOR.

proof of what I have stated about myself you might request any sick person you choose to write to me and you would discover what you cannot now believe and I remark in addition that you will be allowed to read every thing I communicate to the patient, and also test and examine everything. Further I will add about what you write on p. 207 (Post-hypnotic Sense-delusions) about that most wise and just man Socrates, that I also have developed this gift but that there can be no question of hallucinations and delusions but of the most convincing reality, but to this must be added that any one who wants to undertake such things must first of all have complete power over his body and be very much like the noblest character god as far as it is possible for a human being if he does not wish to come to temporal and eternal grief. For the devil can quite well be reasoned with, only one must know how to do this as well and better than with many a neighbour for he is a very clear-sighted gentleman and as the Bible already tells us only quarrelled with Purity *i.e.* Heaven because he could not stand anything superior to himself. Therefore it may also be obvious to you that it is very much better to reason with the Prince of Darkness than with stupidity *i.e.* poverty of mind and supper-cleverness with which the world of to-day is crammed where the gods themselves strive in vain. If you my dear Sir doubt this and refer it to the realm of hallucination you must do just the same with Kerner's Seeress, but I am convinced this is most profoundly true from my own experience, but how to understand this is disclosed by no science only with 99% belief are the profundities of all knowledge disclosed as Göthe already said.

Awaiting your kind agreement and reply.

I remain yours faithfully.

(*Signature*).

The assertion made by magnetopaths and similar persons that they are totally disregarded by doctors, is in any case incorrect. Apart from the fact, that during the period of its greatest prosperity animal magnetism played a part in officially recognized science, magnetopaths have at all events had opportunities for having their supposed powers tested scientifically. I have offered to make such tests, and undoubtedly others would do so also. Considering the constant complaint of magnetopaths that they are ignored by medical men, one would think they would take advantage of every opportunity given them for having their powers tested; but this is far from the case, for during the fifteen years that have elapsed since I offered to carry out such experiment only three of these gentlemen have allowed their supposed magnetic power to be tested.

I now come to the question of mineral magnetism. Belief in the action of the magnet on human beings is very old. The magi of the East used the magnet to cure disease, and

the Chinese and Hindoos did so long ago. Albertus Magnus in the thirteenth century, and later Paracelsus, Helmont and Kircher also used it. So did the astronomer and ex-Jesuit Hell, of Vienna, at the end of the eighteenth century. It was from him that Mesmer is said to have learned the influence of the magnet on human beings. As we saw on page 6, Mesmer used the magnet at first, though even then, some doctors—Deimann of Amsterdam, for instance—denied the therapeutic action of the magnet and asserted that brass plates did as well. Reil also used the magnet. In the year 1845 Reichenbach asserted that some sensitive persons had peculiar sensations when they were touched with a magnet. He also said they saw light—the so-called *Od* light; an assertion that was long supposed to be disproved, but which has lately been again made by Barrett in London, and by Luys in Paris, on the ground of fresh experiments. According to Schrenck-Notzing, Jastrow and Pickering have specially opposed the revival of the *Od* doctrine. Still, the opinion that the magnet exerts a specific, and, more especially, a therapeutic action on the human body, has many adherents. Maggiorani, in Italy, has lately contended for the therapeutic use of the magnet (Belfiore), and the school of Charcot has asserted the influence of the magnet on certain individuals. Benedikt also, in opposition to a few American investigators—Peterson, Kennelly—who had attributed the therapeutic action of the magnet to suggestion, advocated its specifically physiological curative action.

But some people suppose that there are further affinities of mineral magnetism to man. Formerly, it was occasionally assumed that some people could deflect the magnetic needle by merely approaching it, and more particularly by putting a finger near it. Wolfart relates of himself in his *Erläuterung zum Magnetismus*, that he could cause the magnetic needle to deflect and incline downwards by bringing his finger near either pole, and that the needle was so firmly fixed in its new position that it did not at once react to a strong magnet, whereas Wolfart was able to restore its normal equilibrium by making a few counter-passes with his finger. He consequently supposed that there is a definite connection between mineral and animal magnetism. Further, Du Potet reports, that a subject named Angelika Cottin could deflect the magnetic needle by merely bringing her arm near it (Perty). Fechner, who was very sceptical with regard to Reichenbach's *Od* doctrine, and who

was more inclined to look upon the deflection of the magnetic needle as a disproof and not a support of that doctrine, experimented on one of Reichenbach's subjects, a Frau Ruf, and was convinced that this lady could deflect the magnetic needle. When Frau Ruf waved her finger over one of the poles, the needle oscillated just as if a bar-magnet were being waved over it. Although the strictest conditions were not imposed—Fechner wanted to do so, but the magnetic power of the lady abated—Fechner did not consider himself justified in assuming fraud on the part of the subject, and declared he was convinced by the experiments. He added, with that modesty which is so characteristic of the real *savant*, that he had thought of the possibility of hallucination, but he considered he must discard that suspicion because another investigator, Professor Erdmann, witnessed the experiments and also observed the same phenomenon. But we are quite justified in thinking that Frau Ruf produced the effects by means of some steel object which she had concealed about her. Moreover, Fechner does not claim that other people were convinced by the experiments. Only, at the end of his book he strongly recommends investigators to be on the look-out for some such fraudulent procedure. As Ulrici reported in his work on so-called spiritualism, Slade was able to deflect the magnetic needle. In the presence of W. Weber, Scheibner and Zöllner, Slade deflected the needle 40° to 60° until it finally made several complete revolutions, although his hand was quite a foot from the compass. He is also said to have magnetized steel knitting-needles. The Tilsit magnetopath, Schröter, asserted when on trial for quackery, that he could deflect the magnetic needle himself without employing any friction. In connection with Harnack's experiments, which I shall presently discuss, I may here mention that Max Breitung states that he also has employed a thin knitting-rod, such as is used in making fishing-nets, to test his own magnetic power, and that he first magnetized the rod with a horse-shoe magnetic and then hung it up. He states that when he laid the thumb and first two fingers of his right hand together and brought them near the needle which was freely suspended, the needle followed his hand to the right no matter which pole was approached. The needle literally ran after his hand. On using the fingers of his left hand the needle was deflected to the left. But since Breitung himself states that he cannot deflect the needle of a

compass by means of his finger-tips, it would be as well to find out whether there was not some source of error in his experiments with the knitting-needle, such as draughts, etc.

It has also occasionally been pointed out that there are some people who are so electric that they may even emit sparks. Du Bois-Reymond doubted these statements and thought, moreover, that the whole question was of but little interest. It has also been stated that steel becomes magnetic in certain persons' hands. Eisenstein relates of his subject Leopoldine R., who was chief subject in the Viennese experiments mentioned on page 13, that she could turn ordinary steel needles into magnets simply by touching them. She told the audience and Dr. Hümmel, who was controlling the experiments, that she only possessed this power when she was tired and worn out, but not when she felt quite well. But when the experiments were more carefully controlled, her assertions were not substantiated. The magnetizer Lafontaine also stated that he could render iron magnetic, and could produce a north or south pole at will, according to the passes he made; he further stated that he was able to deprive a steel magnet of its magnetism by a proper process of magnetizing. He also further asserted that he could render water so magnetic that it would cause a galvanometer to show a deflection of from 10° to 20°. With one of his patients, Thilorier, he approached the *Académie des Sciences* with a request that this fact might be tested. On June 10, 1844, the Academy appointed a commission consisting of Pouillet, Dutrochet, Becquerel, Chevreul, Regnault, and Magendie. The experiments proved unsuccessful. Lafontaine blamed Thilorier on the ground that the latter did not understand the business; but he refused to make any further experiments, asserting that the commission was prejudiced.

Certain experiments that have recently attracted attention are of a somewhat different nature. Erich Harnack, for instance, is convinced that he can deflect the compass-needle by rubbing the glass-plate with his finger. Harnack assumes that the friction produces statical electricity, that we have to deal with a process that is not merely mechanico-physical, but with one that is distinct and peculiar to certain individuals. He came to this conclusion, because (1) apparently stronger currents of frictional electricity did not deflect the needle, (2) the finger-tips of numerous individuals did not possess this faculty, (3) he only possessed it at times. Harnack thought

he refuted the objection that the process was only mechanico-physical, because he tested the tension and found it over 1000 volts. This assumption has been disputed, notably by Bethe, who experimented with his own finger and also on the corpse of a man sixty-nine years of age. Bethe was able to get a tension of 1000 volts with the finger of the dead man when it had been dried in the exsiccator.

But in spite of these objections, Harnack's experiments seem to me to be of such great importance, and so interesting, that we cannot but earnestly desire that they should be put to a further objective and scientific test. But the question whether the processes were physiological or merely mechanical does not seem to me solved. The rapidity with which Harnack obtained such a high tension—600 volts with one pass—is certainly extremely remarkable. Experts must examine this question further, and objectively. This is particularly necessary so as to prevent occultists and magnetopaths making an unscientific use of Harnack's experiments, as they might otherwise do.

Of course, experiments, the results of which can be made use of, should only be carried out under the strictest conditions. I think I may assume from Harnack's own publications, that he was not invariably as careful as he might have been. He speaks of cases in which people were supposed to cause deflection of the magnetic needle by merely approaching it, though he himself had to rub the glass-plate. He was able to experiment with two people who were said to possess this power. With one of them the experiment failed—he assumed the subject was only a magnetopath in her leisure hours. Harnack writes, "I obtained better results in the second case, that of a woman of the educated classes. It was only by accident that she became aware that she possessed this peculiar power. She told me at once that she did not always succeed in deflecting the needle by approaching and withdrawing her finger (she never rubbed the glass-face)." In his experiments with this lady, Harnack used a rather large, but simply constructed compass, with a glass-face; the needle was about 4 inches long, and very sensitive. When the experiment had been going on for about ten minutes, the needle suddenly deflected in the direction of the subject's finger-tips. The deflection gradually increased to from 20° to 25°. Harnack considers this experiment, which he often

repeated, thoroughly convincing. During the experiments "the lady wore nothing that was made of steel, and no stays," and Harnack thinks that this excluded all possibility of fraud, because if the subject had had anything made of steel about her, the needle must have been affected directly she approached the compass, but nothing of the kind ever happened. Harnack further thinks that the lady and her husband took no special interest in the matter, but were solely concerned in serving the cause of truth.

But the experiments Harnack made with this lady are not convincing. His assumption that if she had had anything steel about her it would have caused the needle to deflect directly she approached the compass, is incorrect. That would depend upon whether the steel object were freely movable, so that she might weaken its action when she wished. At all events, I have not been able to discover anything in Harnack's publications showing that those precautions were taken which are absolutely necessary, if the possibility of some steel object being concealed about the subject is to be precluded. It would have been all the better if the proper precautions had been taken in this case, as it is the only one in which Harnack saw the needle deflected without the glass plate being rubbed; *i.e.*, supposing the observation was free from error, we have here a proof that friction is unnecessary. Unfortunately, Harnack did not take the necessary precautions in the case of this lady; perhaps he was unable to do so, for it is especially difficult to get any one, particularly a lady, to observe all the necessary conditions of a scientifically controlled experiment. Moreover, Harnack's great confidence in the veracity of the lady and her husband is merely subjective, and does not fully prove their truthfulness.

I have good authority for stating that this lady, who is supposed to be able to deflect the magnetic needle, is the very same person as the spiritist medium called the *femme masquée*, about whom Wilhelm Winkler has written in his work, *Zur Reform des sogenannten Spiritismus*, Leipzig, 1905. I have experimented on her on three different occasions during the last eleven years. These opportunities have also enabled me to follow up the "evolution" of the lady's powers. I first of all investigated the "raps" which caused such a stir in the spiritistic world. A *feuilleton* of the *Hamburger Nachrichten*, of March 29th, 1905, contains an account of one of the sittings I then held with this lady; it was written by Eugen Reichel, who was present. The experiments were made in my house, and a number of vigorous and loud raps were heard as long as the lady wore her

boots, but directly I got her to wear list-slippers the sounds were gentle and dull, and when I held her feet down there were no sounds at all. There can be no doubt that she made the raps with her feet, toes, etc., etc. I again experimented with her about three or four years ago. At that time the question investigated was the supposed deflection of the magnetic needle, and also whether other objects, such as wooden ones, or a ring suspended by a thread, would move in her presence without any mechanical aid. A whole series of sittings that I held with the lady—who, moreover, came to me most willingly and charged no fee—gave none but negative results. I now come to the third period in which I experimented with this lady. In the years that had intervened she had greatly improved as a medium. She could see spirits, and there are numbers of photographs of spirits extant, of spirits whom she is said to have materialized by means of her mediumistic power. She was now also said to be able to remove objects from cases that were firmly fastened and sealed, without any mechanical assistance. Unfortunately, when this experiment was tried in my house, it proved a complete failure; even the spirits did not appear, though the voices of a few supposed spirits were heard. I have never doubted for one moment that this was nothing but ventriloquism on the part of the medium, who may, or may not have been in a trance. Test experiments—*i.e.*, experiments carried out under strictly scientific conditions, which the husband had promised I should have the opportunity of making, were not made after this. Indeed, Mr. X. wrote to me and said that it was generally thought that I was quite unable to provide a proper *milieu* in my house for the occurrence of occult phenomena. I can only hope that the lady's spouse, who put me off to some later date, will not defer the fulfilment of his promise *ad Calendas graecas*. In any case, Harnack must admit, that if spirits appear to a lady, and her presence is said to render solid substances penetrable—the experiment failed when tried before me—we ought to be particularly cautious when dealing with experiments made with the magnetic needle.

I have expressly given a somewhat detailed account of this case, as it affords a warning against the *naïveté* and credulity with which scientists so often enter a domain of research that is new to them. On this point I refer to what was said on page 482. It is an unfortunate necessity, but men of science must always mistrust persons who are strangers to them.

I am not for one moment asserting that it is utterly impossible for human beings to deflect the magnetic needle without the aid of contact or friction. *A priori* negation is an error often committed in scientific circles, unfortunately. There can hardly be any question of *impossibility* where science is concerned. Only, we must demand the most exact demonstration where such far-reaching assertions are made. And we will do so in this case to prevent discredit falling on the extremely interesting experiments Harnack has made on himself. It is necessary

that Harnack should experiment on others, and only under the strictest conditions.

The greatest caution is necessary for the very reason that occultists and magnetopaths are only too ready to put their own interpretation on all such things. As a matter of fact, magnetopaths certainly obtain no support from the experiments which Harnack made on himself. For if we consider the capacity for generating electricity a proof of the existence of curative magnetism, Harnack must be a very good healing magnetizer, but then the magnetopaths would first of all have to prove that they possess this capacity. Apart from this, we can quite easily generate the same quantity of electricity by means of the electrical machine, which means that persons possessing magnetic power are quite superfluous.

I have hitherto only discussed the mental relations between human beings and mineral magnetism. But it has been asserted that certain special relations subsist between hypnosis and mineral magnetism. I have already spoken of the application of the magnet for inducing hypnosis (p. 46), as well as of the action of the hypnoscope (p. 48). But the magnet is also said to produce special effects during hypnosis. The phenomena of transference must first be mentioned here. According to the school of Charcot, transference means that certain phenomena, influenced by some æsthesio-genetic expedient, particularly the magnet, change the place of their appearance. Charcot says that such phenomena occur in hysterical patients. Thus, contractures on the right side can be transferred to the left, so also can anæsthesiæ. But most experimenters supposed that the subject's expectation produced the effect, and not the magnet. They also found that sealing-wax, bones, etc., produced the same result, provided only that the subject expected it (Westphal). The school of Charcot say that phenomena of transference, similar to those observed in the case of hystericals, also take place in hypnosis. Binet and Féré think they have discovered laws governing the course of the transference in each particular hypnotic state. When lethargy on one side of the body and catalepsy on the other have been induced by closing one eye, the approach of a magnet causes catalepsy on the lethargic side, and on the cataleptic side lethargy. In the same way, when the state is somnambulic on one side and cataleptic or lethargic on the other, the magnet causes transference. But also, in each

particular hypnotic state, symptoms can be transferred from one side to the other—*e.g.*, contractures in lethargy, particular postures of the limbs in catalepsy, hallucinations of one side, and hemi-anæsthesiæ in the somnambulic state, etc., etc. Binet and Féré say that when hypnotic subjects write with the right hand, they write with the left hand under the influence of the magnet, and at the same time reverse the direction of the writing.

Another method of influencing hypnotic subjects with the magnet is called polarization. It is a reversal of a functional state (Belfiore). Binet and Féré are the authors of the experiments on polarization, which are confirmed by Bianchi and Sommer. It is probable that these are all cases of unintentional suggestion. It is said that in some cases the magnet resolves a contracture induced by suggestion (motor polarization). It can banish a suggested hallucination, and can change the mental pictures of colours into their complementaries; if a subject believes he sees blue, he thinks he sees yellow (sensory polarization). Sometimes there is an arbitrary change in the colour-sensation—for example, yellow changes into red; this is called dispolarization (Lombroso Ottolenghi). The magnet is also said to change a happy frame of mind into a sad one (mental polarization). A committee of the Medical Congress at Padua was unable to confirm these phenomena. Tanzi was quite right in referring them all to unintentional suggestion. The same may be said of analogous assertions concerning matters which partially belong to the domain of therapeutics. Venturi and Ventra used mental polarization therapeutically, and they say they conquered a fixed idea, an auto-suggestion, by applying the magnet in the waking state. The reverse of this has been observed. Raggi thinks that the approach of a magnet in hypnosis often causes subjective discomfort. In other cases the magnet is said to have put an end to the hypnosis.

Tamburini and Seppilli state that when a magnet is brought close to the pit of the stomach it influences respiratory movements in hypnosis. Later on, Tamburini and Righi found that other metallic bodies produced the same effect; the strength of the effect, however, depended on the size of the piece of metal. The electro-magnet is said to have the same effect whether the current is open or closed. For this reason Tamburini supposed later that it is only the temperature of

the metal which has the effect, and that the magnetic force may have no influence.

Lastly, I must mention Babinski's and Luys' experiments. If a hypnotized subject and a sick person are set back to back, a magnet put between them will cause the sick person's symptoms to pass over to the hypnotized subject. Hysterical contractures and dumbness have been thus transferred, as also the symptoms of organic disease—*e.g.*, multiple sclerosis. The transference is said to take place even when the hypnotic has no notion what the sick person's symptoms are—*i.e.*, when suggestion is excluded. Luys went even farther. When he places a magnet first on a sick person's head and then on a hypnotic's, the morbid symptoms of the first person are supposed to appear in the hypnotized person.

In these experiments of Babinski and Luys we have an obvious combination of the phenomena of mineral and animal magnetism. It is a significant fact that such assumptions as these have hardly ever been made in recent times by men who must be taken seriously. We are, therefore, justified in now assuming that the results obtained by Babinski and Luys in those experiments were due to suggestion—*i.e.*, that there was self-deception on the part of the experimenters, who at the time were not so well acquainted with suggestion as a source of error as we are to-day. Of course, all this does not prove that it is *impossible* for the magnet to influence human beings. Obersteiner supposes that there may possibly be a special magnetic sense, which may come into activity with many people during hypnosis, and which is, perhaps, localized in some terminal organs whose functions are still unknown.

I have hitherto discussed the influence of the magnet on human beings, and *vice versâ*. I have mentioned cases in which the magnet is said to have influenced human beings, and others in which human beings are said to have influenced the magnet and also steel. To make this summary complete, I may further mention that there are said to be persons who can not only attract or repel the magnetic needle, but other bodies as well, even such as are made of wood. This at once leads us into the domain of spiritism, whose adherents may be divided into two groups: (1) spiritists in the narrower sense, persons who trace all the supposed phenomena to spirits; (2) the animists or psychicists, who assume that a force which emanates from the psyche of certain persons, the mediums,

is able to make objects move, and the like. Innumerable cases of this kind are to be found in the literature of the subject. But I have never seen anything of the sort happen when the strictest conditions were enforced. Everything that I have seen in this domain—*e.g.*, in the case of Eusapia Palladino, was undoubtedly the result of purely mechanical action, and, therefore, I can only deeply regret that a man of science like Lombroso should have let himself be victimized by the frauds which such persons perpetrate.

The following was recently told me of a medium in Berlin, a youth of sixteen, who is supposed to possess special magnetic powers:—Objects hang on to his fingers when he has magnetized them; sticks and other objects remain in an oblique position in spite of gravity, because of his magnetism. I was allowed to take this medium in hand, and the results were very different. It is quite true that plates and ash-trays stuck to his fingers, and that sticks apparently disobeyed the laws of gravity. But nothing of the kind happened when I took the precaution of dressing the young man in a long coat and covering the objects with a cloth so as to cut off all connection with them. The medium certainly stated he was not accustomed to that sort of thing, and that his magnetism did not suffice to overcome such obstacles. But the magnetizer was unable to sustain his explanation when I produced the string that ran from the leg of one of his stockings to the other. He admitted the fraud, said it was the first time he had been caught, and left our "circle."

The phenomena of supersensual thought-transference, *suggestion mentale*, or, as Mayerhofer fittingly calls it, telæsthesia, are closely related to animal magnetism. Telepathy means the transference of thoughts, feelings, sensations, etc., from a person A. to a person B. by some means other than the recognized sense-perceptions of B. (Consequently such thought-reading is excluded in which, as described on page 62, one person guesses the thought of another by means of the tremors in his muscles—*i.e.*, by a recognized kind of perception). B. is to feel A.'s sense-perceptions; if A. is pricked B. feels it; if A. tastes salt, B. tastes it. It is also said that A. can make B. act, merely by concentrating his thoughts on what B. is to do. Others think that it is the concentration of A.'s will on B. which causes the action. Perronnet even maintains that it is possible to influence the pulse and cause vasomotor changes

telepathically by an effort of will. Numerous experiments have been made in this way—for instance, in guessing numbers. The agent A. concentrates his thoughts on a number which the subject B. is to find out. In many cases the number is written down and A. gazes at it, concentrating his thoughts the while. Or the same sort of thing is done with cards. A. picks out any card he chooses, looks at it earnestly, and B. then has to name it. A further series of experiments deals with movements. A. makes a movement, or thinks of one which B. is to carry out. Telepathic experiments are also very frequently made with drawings. A. makes a drawing, or concentrates his thoughts on a particular one, such, for instance, as of a circle, a square, or a human being, and B. then has to execute a drawing of the same.

In many of the experiments in thought-transference the passive party—*i.e.*, the recipient—was first of all hypnotized, as this is supposed to make the transference easier. But experiments have also been made when both persons were quite awake, by Guthrie, for example. Sometimes, also, both were hypnotized. We can understand that the recipient being in hypnosis largely increases the number of successes, because a hypnotic has a much greater tendency to pay attention to the smallest sign made by the experimenter, than a person who is awake has. But it is just in this that one of the chief sources of error lies, because what in reality depends on the influence produced by such insignificant signs is very often taken to be the result of telepathic influence.

Telepathy is to an extent connected with animal magnetism, some magnetizers seeing in it a proof of the existence of animal magnetism. But there is still another connection between the two, to which Ochorowicz, in particular, has drawn attention. The mesmerizing of B., who has to find out the thought, by A., who transfers it, is said to have a successful result essentially, and more particularly when B. falls into a magnetic sleep.

This is jocularly, though perhaps somewhat drastically, described in *Pudenda*, Leipzig, 1817:—"You know from the writings of Gmelin, Wienholt, and Kluge, that when a magnetizer puts pepper or salt in his mouth, his clairvoyante makes a grimace, but gives signs of gratification when he drinks good wine; further, that when he pricks himself, she feels it in the same part of her body; when he has diarrhœa, she gets an attack of it. Why, there is even the case of the lady, who, although only in

natural *rapport* with her married sister, felt a sensation in her nipples when her sister was suckling her child." Probably for the purpose of making the whole subject appear ridiculous, the author of the article further states that he also practised magnetism on a well-behaved young girl for a considerable time. She was extraordinarily virtuous, and so was he. Yet when he kissed his wife, his clairvoyante felt the kisses energetically, and, finally, when his wife was confined, the girl suffered from severe pains because he had put the two women *en rapport*. Conversely, it is sometimes asserted that the magnetizer's magnetism renders him clairvoyant in respect to disease in others, or makes him feel the disease himself. He then feels the pain in the same place as the patient, without the latter telling him where. In the law case which has already been mentioned, the magnetizer declared, for instance, that when a patient was suffering from liver trouble, he, the magnetizer, also felt pains in his liver, upon which a specialist asked him whereabouts he would feel the pain when he was trying to diagnose the case of a woman suffering from a disease of the womb. It is always a very good thing to consider what the consequences of any particular assumption may be; for, when the consequences are absurd, one's distrust in the accuracy of the assumption is, of course, particularly strengthened.

The transference of thought is usually said to be brought about by A. firmly concentrating his mind on the thought to be transferred. The nearer A. is to B. the better, but the phenomena are also said to have been observed when subject and agent were separated by several kilomètres. It is said to be even possible to hypnotize certain people at great distances by concentration of thought; such experiments are said to have succeeded at Havre.

The supposed revelations of dying people are also often referred to some such action at a distance. It is reported that dying people, at the moment of death, or just before it, appear to some near relative or friend who is far away. Adherents of telepathy refer this to some such mental action at a distance being facilitated by the dying person's intense thoughts of loved ones who are away from him. The English Society for Psychical Research has studied this domain thoroughly. The Society made an inquiry as to the frequency of hallucinations in the waking state (the appearance of some particular person), and also, in a second question, asked whether the waking hallucination corresponded in point of time with the death of the person who appeared in the vision. The English investigators endeavoured to meet the objection that the events of a waking hallucination are entirely independent of the visible processes accompanying death, by comparing

the numerical results provided by the answers to the two questions, so as to ascertain the probability of the waking hallucination coinciding with the death of the person supposed to be seen in the vision. The inquiry showed that the number was much too small to admit of any coincidence being accepted.

Parish, however, has criticized all the available material, and thinks that all the results admit of a different explanation. The first thing to be considered is retroactive hallucination. As soon as the news of a death is communicated to a person, he thinks he has had a vision of the event. But the second objection, the adaptability of the memory, is much more important. Supposing a person has a hallucinatory perception of an event at the time of its occurrence, his memory will later on retain the hallucination as though it were the recollection of something really experienced. A third objection is raised by Parish—he thinks that there is no question of waking hallucinations, but that we have, rather, in many cases to deal with the phenomena of a dream-state in which the memory is much less clear. Finally, he opines that the waking hallucinations are sometimes called up by circumstances which have nothing whatever to do with the death of a person who is far away. There might have been factors which led to the hallucination in some way or other. It is often very difficult to discover how the hallucination came about. But it is extremely dangerous to assume straightway that such an event as a death at a distance is a cause, instead of proving that no other contributory causes have been at work.

I have just explained that Parish has criticized the spontaneous occurrence of the perception of things distant, as far as the large amount of material at his disposal permitted. Against this criticism, we can hardly oppose the reports that occasionally reach us of cases which tell in favour of such action at a distance, and in which not only dying persons are concerned but other important events, such as serious injuries, as well. The only cases demanding earnest consideration are those in which the person concerned has written down the nature of the phenomenon. In such cases there would be no question of delusion or adaptation of memory. Still, the last of the sources of error mentioned by Parish would have to be considered. Isolated cases of this kind, in which all that happened was put down in writing at the time, have been

reported to me. Thus, a Mr. X. who lived in America wrote to me as follows:—"One day when I was half asleep, I seemed to see my father, who appeared rather 'elevated,' come round the corner of a house I did not know, and walk across the courtyard; I saw him slip on a large flat stone at the foot of the stack-pipe leading from the gutter attached to the roof, and he lay there with the blood streaming from him over the stone." As X. recognized the second face, he at once wrote down all he had seen in his notebook, so as to be able to verify the day and hour later on. When he saw his relatives in Europe a year later and visited his parents' new home, he went into the courtyard, and "there I saw the stone exactly as I had seen it in my dream. I at once called my mother, pointed out the stone, and told her what had happened. As it had been intended to keep the accident a secret from me, she asked who had told me about it. I told her what I knew, and showed her the entry in my notebook. The hour given in my note was perfectly correct—*i.e.*, after making due allowance for W. longitude. On the day in question, my father had completed a large business transaction for a capitalist, and the latter had passed the bottle freely, making my father drink more wine than he wanted to." The man who told me this thought his father had never been drunk on any other occasion. He did not think the coincidence accidental. In spite of this detailed communication—I have received others of a similar nature—I must add that in this case, particularly, the fourth of the sources of error mentioned by Parish is not excluded. At all events, any one who does not wish to be lost in a maze of miracles, must carefully consider these sources of error when dealing with such statements as are occasionally made.

As we see, the cases of thought-transference that have been observed fall into two groups—the spontaneous, which I have just noticed, and the experimental, which I mentioned first of all. Those produced experimentally do not call for any serious criticism. All that Du Prel, Mensi, Welsch, and many others have published on this hardly requires even moderate criticism. Among those who vouch for the reality of telepathy I mention Charles Richet, Ochorowicz, Pierre Janet, Gibert, F. Myers, A. Myers, Gurney, Lombroso, Birchall, Guthrie, Eeden, Glardon, Schrenck-Notzing. I had an opportunity of being present at Mrs. Sidgwick's very neat experiments at Brighton. Two persons were in the hypnotic condition, and one had to

indicate a number thought of by the other. The proportion of correct answers was extremely large. As, however, the two persons experimented on, though separated by a folding screen, were somewhat near together, the experiments were not conclusive. Mrs. Sidgwick admitted the justice of this criticism. Also the experiments made by the other persons I have mentioned do not stand serious criticism. My own experiments, especially those I made some years ago in conjunction with Max Dessoir, only gave negative results when the necessary precautions were taken. Still, I agree with Lœwenfeld that we cannot deny the possibility of there being such a thing as telepathy, or at least the possibility of their being ways of influencing others about which we know nothing in the present day. But up to the present no proof of this has been forthcoming.

I intend, in conclusion, to give all the sources of error to which occultism is liable, in a connected form, but here I will just mention a principal source of error in connection with thought-reading, which is not invariably properly considered, to wit, involuntary unconscious or subconscious signals. I discussed thought-reading on page 63, where I mentioned that steadfast concentration of the thoughts on an object or a place causes muscular contractions that guide the subject to the place where the object is. In any case, it is a fact that steadfast concentration of thought sets up involuntary, and for the most part unconscious movements, that are often so slight that they can only be detected by means of very fine instruments, as, for example, Preyer, Richet, and Sommer have stated. These very slight movements are often tactually perceptible without being visible. It also frequently happens that certain persons can see them while others cannot. A certain amount of practice, perhaps also a special capacity, enables some persons to perceive signs that are so slight that others overlook them. This is particularly the case with hypnotics; their whole attention is so fixed—possibly subconsciously—on these signs, that they are able to perceive signs of the existence of which the spectators have no notion. The signs can be made in ways that differ very considerably. One is inclined to look at an object which one is thinking of steadfastly. Any one who has drawn a card and looks at it hard, is inclined to make some corresponding movement with his lips. Stricker mentioned in his work, *De la Parole et des*

Sons intérieurs, an *articulation faible si peu perceptible que nous ne la remarquons pas d'habitude.* It is from such movements that A., provided he has had practice, can sometimes guess the card that has been drawn, because, for example, B. who has drawn the queen of spades moves his lips as though he were going to say queen of spades. The subject sometimes even whispers gently, as Lehmann and Hansen have pointed out. Similarly, adequate movements are also made with other parts of the body. When anybody thinks steadfastly of a number he is inclined to make the necessary movements with his fingers for writing down that number. I mentioned when dealing with thought-reading that other muscles occasionally participate in such movements. The whole body is impelled towards the object of which the person is thinking (Tarchanoff).

Sometimes the signals given are somewhat different. We may observe in cases of thought-reading, that when the reader is taking a wrong direction the person who is concentrating his thoughts, or often some one else who is present, will involuntarily give a sign that at once tells the practised thought-reader that he is on the wrong tack. A loud breath, for example, will do this. A rapid and distinctly audible inspiration will very often tell the thought-reader that he is making a mistake. Also, two Italian authors, Guicciardi and Ferrari, have, as Gley informs us, ascribed an important part to changes in respiration during experiments in thought-transference. In other cases some rougher movement of the body is noticed by the thought-reader if his eyes are not bandaged; but it may quite well be audible.

In any case, the movements just described guide the thought-reader in the right direction, and this may, indeed, occur even when he is not touching the person who is concentrating his thoughts. Now, as a rule the thought-reader generally has his eyes bandaged. As we saw on page 63, it is not necessary that the thought-reader should be able to see when he is in direct contact with the subject, because such direct contact enables him to feel the movements that are made. But, since the involuntary movements just described are also audible, we can understand that the thought-reader can solve the problem that is set him correctly even when his eyes are bandaged and he is not in direct contact with the subject experimented on. Now, as a matter of fact, a whole series of cases has occurred in which thought-reading

was accomplished although the reader's eyes were bandaged and he was not in contact with the subject. Consequently, such a case is not necessarily an instance of telepathy, although uncritical experimenters would probably ascribe it thereto. It is very much like the game of hide-and-seek which most of my readers probably played when they were children. One of a number of children is sent to a distance while the others hide something. The child sent away is then recalled and told to find the hidden object; so long as the child is far off the object it is looking for the others cry out, "cold," when it gets nearer, "warm," and when quite close to it, "hot." Just in the same way the thought-reader, even when his eyes are bandaged and he is not in contact with the person experimented on, finds out whether he is performing his task correctly or not. He can tell from the sighs and changes in the respiration of his audience, and also from other signs, whether he is going in the right direction or not.

It is possible that with certain people tactual sensibility plays a special part—at least in some of the cases. It sometimes happens that the thought-reader finds himself quite close to the person who is concentrating his thoughts, and that the latter stretches out his hand while the thought-reader's is close to his hand or arm. Now, supposing the subject makes some movement with his hand or whole body in the direction of the object to be discovered—it is easy enough to show that such movements may be considerable—and supposing the thought-reader possesses a very fine sense of temperature, then we can readily understand that the thought-reader's hand should tell him the direction in which the movements are being made. The amount of resistance offered by the air may also have something to do with this. There are persons who are quite able to perceive in the dark whether they are near a wall or any other solid substance. The resistance of the air to movements is different near a wall to what it is in the middle of a room (*cf.* p. 99). Consequently, it is possible for the thought-reader to tell from the resistance of the air the direction in which the subject's hand is moving, and this fact helps him to solve the problem that has been set him. Nevertheless, it is probable that less weight should be laid on this circumstance than on perception by means of the sense of hearing.

Lively discussions have recently taken place as to whether the blind possess a special sense or not. One author, Ludwig

Cohn, who is himself blind, has asserted that such is the case; while Brandstaeter, an instructor of the blind, has denied its existence. A special point that has been raised in this discussion is how it comes about that the blind are quite able to orientate, or take their bearings as to the position of things about them, either in a room or in the street: they can even distinguish whether they are close to a wall or not. Some ascribe this to the sensation produced by the pressure of the atmosphere, which differs when the surrounding space is clear from what it would be in the proximity of a wall. At all events, the sensibility may be so delicate that a blind person shut in a room can tell the size of it or the position of the furniture in it without touching anything. Opinions differ as to whether normal individuals can accomplish as much. Important authorities, Zell and Hauptvogel, for example, have answered this question in the affirmative. We will certainly take this source of error into consideration in all cases of telepathy and also of clairvoyance.

Bénoit's supposition that delicacy of the sense of smell is of importance in thought-reading seems to me more than problematical. Wernicke, who is a very objective investigator, has expressed a similar view to Benoit's, but with all reserve. He thinks that concentration of the will, coupled with a cheery expectation that the experiment will succeed, or a depressing feeling that it will fail, may, so to speak, bring about a change in the atmosphere surrounding the hypnotizer and thereby act indirectly on the subject's extremely delicate sense of smell. Consequently, it might be possible for the subject to detect from some change in the sensation of smell the direction in which the experimenter's will is working, and to act accordingly. Wernicke therefore proposed that experiments should be made with experimenter and subject separated by a glass-plate so as to exclude the possibility of the sense of smell being affected.

I have discussed these sources of error in some detail, because their importance is so frequently underrated. At all events, let us firmly bear in mind that the possibility of recognized sense-impressions leading to the correct solution of a problem is far greater than superficial observation would lead us to imagine. Thus, some persons think that when the subject is blindfolded and is not in contact with the experimenter, supersensual thought-transference takes place, whereas, as I have just explained, there are numerous other sources of perception besides whispering. Consequently, it would be very rash to conclude that such experiments establish the

existence of supersensual thought-transference, or that the latter in any way supports the theory of animal magnetism.

It is also on these grounds that I have already mentioned that Scheibler could not prove the existence of animal magnetism. Herr Scheibler pretended he could do so by means of thought-transference. The experiments were carried out as in ordinary thought-reading. Not only was Scheibler blindfolded, but he was also not allowed to touch the subject experimented on. Scheibler left the room, and it was arranged during his absence what he was to do—for example, to fetch a book from the table and put it on a chair. Scheibler was then brought in blindfolded, and his friend A. was told to think steadfastly of the allotted task. Scheibler did not touch A., but the latter's hand was quite close to him. As it turned out, Scheibler certainly did sometimes perform the task set him correctly, though, as a rule, only after some failures.

Carefully conducted controlling experiments showed that the successes were not due to any magnetic or unknown power. Directly Scheibler was made to wear gloves, and had his ears stopped as well as his eyes bandaged, the experiments failed. From this it follows, at all events, that the experiments did not prove successful when the recognized sense-impressions were excluded. I think it probable that auditory impressions were responsible for the successful cases. Any one who has the opportunity for making or seeing such experiments, and who at the same time carefully watches the spectators, will be astonished to find how distinct and loud the sounds sometimes are, that the uncritical occultists consider excluded.

I have here intentionally discussed but one source of error in telepathy; I shall come to the others later on. I will only further remark, that the exclusion of one source of error alone is never sufficient. On the contrary, all sources of error must be excluded if we are to arrive at an unimpeachable conclusion.

I now come to clairvoyance, which is the perception of things distant in time or space. In the former, forthcoming events are foretold, or past ones, about which the clairvoyant could have learned nothing by normal means, recounted; in the latter, things are seen which are so placed in space as to be invisible—*i.e.*, they are either separated from the clairvoyant by some non-transparent substance, or they are too far off to be seen. The somnambulic state[1] induced by magnetizing is

[1] The magnetic states in which such phenomena as clairvoyance, thought-transference, etc., occur, are specially called somnambulia by mesmerists. Consequently the word somnambulia is used in several senses: 1. One of Charcot's stages is often called somnambulia (*cf.* p. 81); 2. The Nancy school calls that state somnambulia in which there is loss of memory after waking; 3. Some identify hypnotism with somnambulia; 4. Somnambulia is a sleep in which there are actions and movements (p. 184); 5. The mesmeric state described above is called somnambulia.

said to favour clairvoyance. For a long time clairvoyant somnambulia was a special profession, and is so to the present in many towns. The belief in clairvoyance goes back as far as history; I need only refer to the Bible and the Greek Oracles. It seems that the state of the Pythia was like a hypnosis, although toxic methods were probably also used. Kluge, Ed. von Hartmann, and others, think that the state was somnambulia. It was the same thing with the Sibyl of Cumæ. Other phenomena of antiquity must also be included here—for example, the reports of Apuleius about the prophesying of boys; as well as many phenomena reported in recent times from various countries. Stecker tells us that in the camp of King John of Abyssinia, 1882, a boy in an apparently somnambulic condition was employed to discover a thief (*cf.* p. 3).

When animal magnetism blossomed forth at the end of the eighteenth century, clairvoyance became more and more associated with it. It is not certain whether Mesmer himself believed in clairvoyance; but it would appear from one of his letters, published by Du Potet, that he did, but did not go deeply into the question. The numerous scientific commissions which have investigated clairvoyance have failed to establish the occurrence of the phenomena. Nevertheless, many people had a lasting belief in it, especially philosophers—Schopenhauer, for instance. Even Braid, about whose views there are so many mistaken opinions, believed in clairvoyance, at all events at first. There is a passage in his *Neurypnology* (p. 22), which I can interpret in no other way. Braid thought clairvoyance proved, though he had never seen it and could not induce it himself; but he thought that a number of those who vouched for its reality were scientific and truth-loving enough to be believed.

Gilles de la Tourette has given a number of details concerning the treatment of the sick through the agency of Parisian somnambulists in his work on the forensic importance of hypnotism. However, it is not to be imagined that such things only occur in Paris. Hirschlaff tells of a magnetopath in Berlin, who employed a somnambulist, and I have not the least doubt that the same thing is done in many other German towns. A Bavarian medical official, Wetzler, who thought he was suffering from rheumatism, treated himself with medicines ordered by a somnambulist. A man named Jost, who had

formerly been a tailor, while in a state of assumed hypnosis prescribed cures for hundreds of sick people. On the testimony of the medical expert, Fürstner, he was tried and condemned for fraud and simulated hypnosis.

Somnambulists are, moreover, also made use of to diagnose their own disease, predict its course, and indicate the necessary remedies. When a somnambulist describes the appearance of his own internal organs, he usually does so in such vague, general terms, that his statements are not worth the trouble it would take one to substantiate or disprove them. But the foretold onset of morbid symptoms occurs with extreme punctuality. I remember being called to a lady who often suffered from attacks of hysteria. When I visited her, she declared that a voice had told her that the last attack would occur the following night. A colleague and I advised her to refrain from taking any further measures to combat the attacks, as the prophecy would possibly be fulfilled; and it was. The most natural explanation—and one based on our present-day knowledge—is, that the patient caused the onset or cessation of certain morbid symptoms by auto-suggestion, in consequence of which the prophecy proved correct. Here there are, perhaps, other circumstances to be taken into consideration as well. F. Myers pointed out the importance of the secondary *ego*. He thought that the latter obviously possessed a profound knowledge of the organism, and could consequently make a more reliable diagnosis than the primary *ego*. He also thought that auto-suggestion would account for the prediction of the course of a disease; indeed, he even considered that death might be caused by auto-suggestion, the patient becoming so anxious and depressed from the auto-suggestion of approaching death as to cause his vitality to fail gradually. But in his opinion real cognition of the secondary *ego* was of far greater importance. We know of something analogous in dreams. As far back as 1866, Liébeault pointed out that many morbid symptoms give warning of their approach in dreams. I will give a few instances of this from my own practice. A lady dreamed she was suffering from severe toothache. In the daytime she was free from pain, and nothing could be discovered the matter with her teeth. A few days later one of her teeth was found to be diseased. Again, a gentleman of my acquaintance frequently complained of dreams in which he suffered from pains on the right side of his chest. On one occasion he

dreamed he was fighting with burglars, one of whom struck him on the chest with a hammer. During this period he also dreamed of inflammation of the lungs. A good many days later he had symptoms of inflammation of the pleura on the right side. Ribot has reported similar cases.

Those diagnoses which are made from objects belonging to sick persons or, more especially, from their hair, also belong to the domain of clairvoyance. Many years ago, in conjunction with Max Dessoir, I made a whole series of experiments with a woman who at the time had a great reputation in Berlin for making such diagnoses by means of patients' hair. The investigation proved a complete failure. Not one single disease was correctly diagnosed, and the number of cases in which such details as age, sex, etc., were correctly given did not exceed the numerical value of the probability of chance success. I have given the results of these experiments in my work, *Über den Rapport in der Hypnose.* In a number of other cases in which I succeeded in getting to the bottom of the matter the diagnosis invariably turned out to be incorrect. A Dutch woman also, who was said to be able to diagnose diseases from patients' hair, made a wrong diagnosis in a case that was carefully controlled. I observed the same thing in the case of a man in Germany, who had a reputation for diagnosing diseases from objects belonging to patients. He also failed utterly in his attempts.

There are various sources of error to be reckoned with where such diagnoses are concerned. In the first place there are vague diagnoses, such as nervousness, a weak stomach, headache at times, occasional sleeplessness, that would apply to any number of cases. In addition to this, the diagnosis often includes a number of morbid symptoms—I came across one in which nearly a dozen were given, such as headache, weakness, bad digestion, weak kidneys, excitability, weakness of the stomach, etc.—some one or other of which would most probably be present. Most people, but more especially uncritical individuals and marvel-mongers—and these form the bulk of the people who consult clairvoyantes—are much more impressed by one success than by many failures. Any one who knows how to criticize can correct these errors, but not so a person in whom that faculty is wanting. Consequently, even when the clairvoyante is only right as to one of ten symptoms that she has described, but is wrong as to the other nine, many people

think that on the whole the case is a proof of clairvoyance. Another important point is, that as a rule such patients subsequently ponder long over the question whether they did not at some time suffer from one or other of the complaints mentioned to them; and with a little reflection any one complaining of sleeplessness will soon discover that they once suffered from some gastric affection. Finally, much is straightway accepted as true without any investigation. For example, suppose a woman who is suffering from pains in her back is told by a clairvoyante that she has a uterine complaint, she will take the assertion for an established fact, and when a doctor tells her later on that her womb is perfectly healthy, she will in all probability put her trust in the clairvoyante. Many of the diagnoses made by clairvoyantes are of such a nature that it is almost impossible to submit them to revision. Thus, stagnation of the blood, impurity of the lymph, etc., play an important part in such diagnoses. At all events, I have gone through the literature of the subject carefully, but nowhere have I found a series of experiments that would bear criticism, and would prove that a number of correct diagnoses had been made by means of clairvoyance in excess of the numerical value of the probability of chance success.

Moreover, I have never obtained a positive result in my other experiments in clairvoyance, although my investigations were frequently made with that object, especially during the earlier years of my researches. When the ordinary sources of error—I shall deal with these together later on—were avoided, the experiments gave none but negative results. As a rule, so little mention is made of scientific conditions in the reports other authors have published of the positive results they say they have obtained, that we cannot consider such reports proofs of the assertions they contain. Or careful investigation shows that the results have been artfully interpreted, and we know that by the exercise of a little ingenuity anything can be proved. In any case, artful interpretation, chance, fraud, signs made by spectators, and similar sources of error, play such an important part in the reports hitherto made, that at present we are perfectly justified in considering clairvoyance a product of fraud and self-delusion. Clairvoyantes who cause such a sensation among the "faithful," hardly ever submit to scientific investigation.

We must here include, for example, the "seeress" Frau Ferriëm, of Berlin. I was invited to experiment with her in November 1904, and I at once expressed my readiness to do so under scientific conditions. I had hardly accepted the offer when I received the following communication :— "The seeress is for the present prevented from placing herself at your disposal, but I have no doubt that she will do so later on." At the same time an invitation to the seeress's lectures was promised me; but the promise was not fulfilled. As the seeress had been silent for more than a year, I reminded her in December 1905 of her promise, and I received the following reply :—"Unfortunately, the seeress is at present still unable to be at your service. In consequence of the protracted illness of one of the members of her family, her head is, in a manner, full of trouble which prevents her bringing the necessary interest to bear on the experiments in question, at present." I was promised a further communication in a few months, but I have never received it. Clairvoyantes and their followers behave in exactly the same way as spiritist mediums; they promise to submit to be experimented on under strict conditions; but when it comes to *Hic Rhodus, hic salta*, a convenient attack of *migraine* sets in, or some member of the family meets with an accident, and so forth.

Another Berlin clairvoyante also beat a retreat at the very moment in which she ought to have proved her powers. This was the case of a lady who was apparently of a very religious disposition, and who was supposed to be able to see forms so clearly in visions that she could at once draw them and then write down a description of them in her diary. It turned out that the lady invariable met the person who had appeared to her on the same day or shortly after. Her drawings and descriptions were usually so accurate that her husband could at once recognize the person from them. One night I went to a lecture at which I was to meet this lady. After the lecture was over I was introduced to her, and directly she heard my name she declared that she had known that morning that she would meet me in the evening. Also she had written a description of my personal appearance in her diary. I begged her to allow me to have a look at her diary, and she promised to do so. But when we (the lecturer and I) asked her to let us drive home with her at once—precautions appeared necessary in the interest of truth—the lady first of all explained that her house was some way off, and when we persisted in our request her confidence abated considerably and she gradually admitted that although she had seen me quite distinctly in a vision, on this occasion she had not written down anything about it. It is obvious that falsification of memory and a habit of romancing played an extraordinarily great part in this lady's case.

The following case shows how easily simple things may be misinterpreted and expanded into something wonderful, how easily a perfectly straightforward process may be turned into an act of clairvoyance, and on the other hand how simply this can be explained. One day a married woman was missed from a village in North Germany. About three days later her son said he had dreamed that his mother had been murdered at a spot lying between the villages A. and B. The boy mentioned his dream to several people, who related it farther. A search

was made for the body, and it was found at a short distance from the high road running from A. to B.—*i.e.*, where the boy had seen it in his dream. The authorities who were conducting the investigation now sent to tell me the facts of the case and to ask my advice, particularly with respect to the boy's dream. The official who called on me, explained in a very matter of fact way that the authorities considered the affair incredible and very remarkable. As they wished their investigations to be as complete as possible, they were anxious to have the advice of an expert, and therefore sent to me for my opinion. The details of the case that came out were extremely interesting. In the first place, it was discovered that the boy had given different accounts of his dream. Thus, he said he had also seen the murderers in his dream, but the description he gave of them, even as to their number, varied at each recital. He also gave different versions of the way in which his mother was murdered, and the only thing he adhered to was, that in a dream he had seen his mother murdered at a spot between the places A. and B. But this riddle admitted of a very simple solution: the day before he had the dream, the boy had heard people say that the woman could only have been murdered between A. and B. The investigation, therefore, proved conclusively that it was known that people were on their way to look for the body between A. and B. the day before he had the dream. All the other details of the dream proved false, and the boy gave a contradictory account of them in each successive statement. In spite of this, the story was at once spread about that the boy had had the power of clairvoyance in his dream, and the authorities ought to follow the matter up in the direction indicated. It is an old experience that, if one unimportant point in an event fits into a prophecy, people who have a mania for the miraculous straightway take everything else to be correct, and that, too, without making any investigation whatever. So it was in this case. At all events, the whole miracle was reduced to the following simple fact: the boy had had a dream in which he had dreamed of something he had heard the previous day. All the rest was due to imagination.

Transposition of the senses bears a certain amount of resemblance to clairvoyance. In it, stimuli, which normally would only affect a particular organ of sense, affect some other part of the body. For example, letters are said to be read by

means of the skin instead of the eyes, without any heightening of the sense of touch such as is found in the blind. On the contrary, the part of the skin concerned is supposed to be stimulated by the light rays, even without direct contact. One of the most commonly mentioned phenomena is reading with the pit of the stomach, or hearing with the stomach. In Paris, I saw a woman who was supposed to read with the lateral cartilages of her nose, even at a distance of several feet. But when those parts were covered with wadding the experiment failed. It is tolerably certain that she saw with her eyes; for though they appeared to be covered with wadding and bandaged, Braid has pointed out that such bandaging is of very little use.

I will here mention some experiments of Heidenhain's which are generally misunderstood, and which at any rate may be easily misunderstood. Heidenhain maintained that his hypnotized subjects repeated whatever he said to them when a stimulus was applied to their stomachs; it was necessary to speak close to the stomach to stimulate it. According to him the vagus nerve was set vibrating, and the phonetic sound-centre was stimulated, and thus a sound was made which corresponded to the one heard; but he thought the sound was heard by the ear and not by the stomach, the nerves of which merely stimulated the sound-centre and thus induced imitation of what was heard by the ear. It might be concluded from many accounts that Heidenhain thought his subjects heard with their stomachs, but nothing was further from his thoughts. I have already mentioned on page 86 that Heidenhain was wrong in many of his other conclusions. This was probably due to the fact that he ignored psychological factors, and was dominated by the desire to explain everything he could by means of some physiological diagram.

The law of the individual capacity of the sense-organs would be violated by transposition of the senses. According to this law each organ of sense has its own specific stimulus, which has no effect on any other organ—*e.g.*, the eye is stimulated by light, but not the sense of touch, or the stomach. Of course, the fact that transposition of the senses would be a violation of the above-mentioned law does not justify our denying that such transposition has ever occurred; here also we must demand proof-positive of its occurrence. Of all the innumerable cases that have been reported, I do not know a single one in which the various sources of error were satisfactorily excluded. Here again, the trifling signals which are given by spectators, and which act as a guide, constitute the chief source of error.

I now come to the action of drugs at a distance. At the present moment this is generally supposed to be disproved, though some authors still assert it. It would not be surprising if the number of its adherents were to increase. And certainly the belief in the divining-rod, which has recently gained fresh supporters, can only be considered an analogous phenomenon.

For, the divining-rod also implies action at a distance, in this case that of water or metals, on certain persons. Formerly, the divining-rod was often used for the purpose of finding springs or veins of metal, whereas in the present day the possibility of at least discovering springs by means of it is still believed in by a few trustworthy people. Heim and Franzius, in particular, have expressed their belief in the efficacy of the divining-rod, and many others—*e.g.*, the district councillors v. Bülow-Bothkamp and v. Uslar—are active water-finders. Heim is very cautious in his statements, and considers the scientific geologist a better water-finder than the man with the divining-rod. Nevertheless, he believes that there are persons who can detect the presence of water hidden deep beneath the earth's surface, and he thinks that their success depends upon either a psychological or a physiological process. In his opinion, the divining-rod is merely a detail. It is the movements and slight contractions or twitchings of the diviner's muscles that are all important—the rod merely acts as a lever of contact, and its movements are due to the movements of the fingers and hand caused by the slight muscular contractions just mentioned. The reason why the excursions of the distal end of the rod are greater, and therefore more readily perceived than those of the fingers, is to be found in its greater length. According to Heim, the process may be a psychological one, the diviner's knowledge of the *terrain* leading to the idea of the presence of water directly he comes to stand on ground under which it is lying. This idea would then cause involuntary muscular movement as in thought-reading, and the rod would sway in consequence. He certainly thinks that the process is purely physiological in other cases. He believes that there are persons upon whom the proximity of water acts as an excitant and produces a kind of tremor, the presence of which may be evinced by the rod swaying; but in this case, also, the rod is only of secondary importance.

Although at the present time some people express their appreciation of the divining-rod, others are strongly opposed to

it. Wolff and Weber, for example, have pointed out the number of failures connected with its use. In any case, it is a fact that exact proof of its efficacy has not yet been brought forward. At least, no one has hitherto shown that the number of successes obtained exceeds the probability of chance success. Even Heim's work in this connection is not convincing. He quotes almost entirely from memory, and admits that he did not make notes systematically, because at first he attached too little importance to the question. This is a pity, and compels us to receive his statements with reserve, because experience shows that positive results are much better retained by the memory than negative ones. Consequently, although Heim only estimates the number of successes at 10 per cent., we are compelled to ask whether this percentage is not much too high, and also whether the results might not equally as well be attributed to chance and falsification of memory as to the action of the divining-rod. In addition to this, we have also to consider the possibility of intentional fraud. Heim himself relates, that it sometimes happens that a person whose knowledge of the ground enables him to detect the presence of water will use the divining-rod, because of the greater impression thereby produced. In any case, I do not think that Heim paid sufficient attention to these sources of error, especially those arising from falsification of memory. Here, also, objective investigation is the only correct method of procedure; neither *à priori* negation nor uncritical assent is justifiable.

As I have already mentioned, the divining-rod was formerly used to find veins of metal, just as it is nowadays to find water. It has long been supposed that some metals have an influence on certain persons, and the assumption has been made use of in medicine. We must here include Burq's metalloscopy and metallotherapy, in which, however, there was contact with the metals. Certain persons were supposed to be influenced by particular metals—copper, for example—which even caused symptoms of disease to disappear. Such assertions were made long ago. Brandis, for example, observed pain and convulsions result from contact with gold and silver.

The later investigations on the action of drugs at a distance apparently proved that certain drugs in hermetically closed tubes would, when brought close to human beings, act in the same way as if they were administered internally. Thus,

strychnine was supposed to cause convulsions, ipecacuanha vomiting, opium sleep, alcohol drunkenness.

Experiments of this kind were also made even earlier. In the eighteenth century Pivati of Venice asserted that if odorous substances were shut up in glass tubes, the fragrance would penetrate the glass and exert a specific influence on human subjects as soon as the tubes were rendered electric by friction. Verati and Bianchi found this correct, and so did Winkler, professor of philosophy at Leipzig. As a result, such tubes were much used at Leipzig for the treatment of disease during the middle of the eighteenth century. There were anti-apoplectic tubes, anti-hysterical tubes, etc. Abbé Nollet then went to Italy to investigate the matter, but he was unable to confirm the phenomena. He found that the statements that had been made were the outcome of inaccurate observation, exaggeration, and fraud. Bianchini, professor of medicine at Padua, came to the same conclusion (Lichtenberg).

Similar experiments were more recently made by Grocco in Italy, and Bourru and Burot in Rochefort. They experimented with both waking and hypnotized subjects; Luys and Dufour repeated the experiments with hypnotized subjects, and confirmed them; so did Duplouy, Alliot, and Peter. The last-named even asserted, on the strength of experiments he had carried out on one subject with Caron and Martinet, that contact with gold would produce a burn of the second degree, without the subject knowing what had touched him. So far as can be seen, the experiment was not very carefully carried out. Luys went farther; he even found distinctions, according as the ipecacuanha was applied to the right or left side. These experiments have been repeated by many other investigators—*e.g.*, by Jules Voisin, Forel, Seguin, and Laufenauer, without result; Luys brought the subject before the French Academy of Medicine, which appointed a commission (Brouardel, Dujardin-Beaumetz, and several others) to test the question in the presence of Luys; they came to a conclusion opposed to his. Seeligmüller also attacked Luys' experiments, and proved that the conditions were not sufficiently stringent; the Nancy school came to the same conclusion. Suggestion and training were shown to be the chief sources of error. The subject's great suggestibility enables him to gather from the experimenter's manner what is wanted of him. For this reason the Nancy school raised the objection that no experiments on the

action of drugs at a distance could be considered conclusive if any one acquainted with the contents of the tubes were in the room. I am unacquainted with any series of experiments in which these conditions were strictly adhered to and the subject nevertheless exhibited the correct reaction.

In the preceding part of this chapter I have discussed questions that are usually included in the domain of occultism. I had already mentioned some points in this connection in the previous chapter. Many believers in these occultistic phenomena go much farther. This is particularly noticeable in their attitude towards spiritism, of which many occultists are followers even if they do not always admit the fact. Of course one may be a believer in animal magnetism, thought-reading, or even in spatial and previsional clairvoyance, without that necessarily making one a spiritist. Spiritism is the doctrine or belief that spirits, more especially those of the dead, appear to human beings under certain circumstances. The presence of certain persons, called mediums, who act as intermediaries between the spirits and human beings, is said to be a preliminary condition. Sometimes the manifestations of the spirits consist of raps which can be heard at different parts of the room, on tables, chairs, the walls, etc., and by means of these raps the audience and the supposed spirits are often able to carry on a conversation; at other times, objects in the room begin to move of their own accord—*e.g.*, the furniture jumps about without any perceptible mechanical assistance. But the most important manifestation is the materialization of spirits. The spirit manifests itself not merely by raps and the like, but its astral body appears—*i.e.*, the body which under normal conditions constitutes the ethereal frame of the soul. It is easy to understand that a person may believe in other occultistic phenomena without being a spiritist and tracing such phenomena to the spirits of the dead, and, as I have already mentioned, the mentalists are opposed to spiritism in the narrower sense of the term. In spite of this, it cannot be denied that there is a connection between occultism and spiritism (in the restricted sense). Spiritists readily admit that clairvoyance is a means of obtaining glimpses of the spirit-world which is hidden to the view of human beings in a normal state. Conversely, it is asserted that the only explanation of clairvoyance is that the spirits inform the medium of

the things to be seen. We generally observe that whoever believes in the phenomena of one domain of occultism—*e.g.*, animal magnetism—takes the phenomena of all the other domains for granted. This will not surprise any one who has watched spiritists and occultists when close to them, more especially when they are joining hands at some séance held by one of their "circles." It is the peculiar frame of mind of the participants that plays such an important part in such proceedings.

I have never asserted that any one of the occultistic phenomena hitherto described is impossible. Although we have no right to say that a phenomenon is impossible, there is an immense difference between the recognition of a phenomenon contingent on its being observed when the necessary precautions are taken and the uncritical acceptance of all the phenomena of occultism.

Of course, we are bound to admit the reality of phenomena observed in experiments carried out under the strictest conditions, and with the various sources of error excluded. But we must insist that these sources of error really are excluded. Laplace's remarks, mentioned on page 455, would apply here. He stated that neither animal magnetism nor the influence of an ordinary magnet, nor the impression that may be produced by the proximity of metals or flowing water, was impossible. He was also perfectly correct in his statement that we have no right to say that these phenomena are impossible, because they are not observed in every case. Least of all should we deny their existence, because the science of the day fails to explain them. And he added, that for this very reason, all the more vigilance should be exercised by those investigating such phenomena.

I have never observed anything of an occultistic nature occur during my own experiments, provided the necessary precautions were taken. A favourite reply of occultists, and more especially spiritists, to this is that the experiments will not work in the presence of sceptics, who disturb that harmony of the "circle," which is so necessary if successful results are to be obtained. I can very well imagine that, if there really are any such subtle psychic processes, the constitution of the environment is of importance. *But if such phenomena cannot be exhibited under scientific conditions, no pretence should be made of proving their occurrence scientifically; rather should it be openly*

admitted that the whole question is a matter of belief. Science has nothing whatever to do with questions of belief, and every man is free to believe what he wishes. I must also state, that the longer and oftener I have devoted my energies to the investigation of occultism and spiritism, the more have I become convinced that conscious fraud on the part of the mediums and their accomplices plays a far more important part than I formerly imagined. But I am also convinced that many occultists and spiritists, even if they are not guilty of intentional fraud, nevertheless attempt to impede the investigation of the phenomena by raising frivolous objections and employing all the arts of evasion, and that they thereby facilitate the perpetration of fraud.

I used to think that it was extremely wrong of men of science not to investigate spiritistic phenomena, and I still maintain that it is far better to subject such matters to the test of scientific investigation than to leave them severely alone. But I must admit that we have no right to blame men of science seriously in this respect, for it is hardly possible to obtain an opportunity for investigating the phenomena scientifically. As a rule, the so-called scientific séances are merely caricatures of scientific investigation, and in nearly every case the whole affair is nothing but a farce. Directly an attempt is made to proceed on scientific lines, the medium usually begins to weep and wail, or feels injured at being distrusted, and it is almost certain that some of the spectators will support and condole with the poor, dear creature. Occultists and spiritists are generally present at these séances; indeed, their presence is necessary for the "Harmony of the Circle." A scientific séance, at which Eusapia Palladino was the subject experimented on, was described to me by a person who was present. My informant told me that when he took a tight hold of Eusapia Palladino, so as to prevent her getting her hands free, the doctor who was directing this "scientific" meeting shouted to him that he must not grip so firmly. In the case I mentioned above, in which I proved that the magnetism of the magnetizer was due to a string fastened to him, many of the audience were filled with pity for him and called out, "Good heavens, how terribly hot it is!" directly I put a cloak over him to prevent him from using the string. Such are the methods employed to nullify scientific precautions. Indeed, most mediums have no need to submit to scientific conditions,

they have enough faithful followers as it is. A thorough-going medium is more spoiled than a first-class *prima donna*. A whisper of doubt, and the medium stops the séance. That is what happened, for example, in the case of Mrs. Abbott, the magnetic lady I mentioned above. I once attended a so-called test séance given by her, and directly it was proposed to take scientific precautions she declared the proposition was an insult, and left the room. It was only at the earnest request of an ardent spiritist that she could be induced to return.

As Max Dessoir very properly points out, scientific investigation should be confined to one simple phenomenon at a time. This phenomenon should be investigated under varying conditions, but the medium should never be allowed to prescribe the conditions. As long as such investigations are impossible or are declined, and as long as investigators continue to allow the mediums to prescribe the conditions—*e.g.*, whether the room is to be darkened, or whether a screen is to be placed in front of the medium—we shall only be dealing with assertions that cannot prove anything.

Schrenck-Notzing, also, would like to see mediumistic phenomena investigated. He strongly objects to the credulity, fanaticism, and lack of criticism displayed by the spiritists. Like Lodge, he advocates the founding of a kind of psychological laboratory, with all the necessary equipment for carrying out experimental researches in every branch of psychology and psycho-physics. Mechanical recording-instruments should be used, so as to keep the results as free as possible from errors that might arise from delusions of the organs of sense. I also agree with Schrenck-Notzing, that persons who call themselves mediums should not straightway be treated as impostors when under examination. There are some who do not intentionally deceive. With them it is a case of self-deception, either in or out of the auto-hypnotic state. Although I agree with Schrenck-Notzing with regard to the methods of investigation, I cannot help blaming him for not having taken his own very clear instructions sufficiently to heart.

The way in which he intervened in the case of Mme. Magdeleine G., the sleep-dancer, was calculated to make the public think there was something occultistic about her performances, and was very reprehensible. I am referring to his unjustifiable assertion that, in the first experiment, the effect of the music on the somnambulic lady was such, that she developed

a power of dramatic expression "far beyond the possibilities of the actor's art." Still more reprehensible was the way in which he foisted this lady, whose performances contributed nothing new to science, upon an unsuspicious public. In my opinion, the way in which he stage-managed the lady's performances in the presence of large audiences was an insult to science, and such methods should be rigidly excluded from the laboratory of the psychologist. Considering the strict conditions under which Schrenck-Notzing would have the investigation of mediumistic phenomena carried out, he ought to be particularly careful himself, and refrain from puffing the scientifically unimportant proceedings of such a person as Mme. Magdeleine G. in a manner which, according to Willy Hellpach, constitutes a downright misdemeanour.

I must also blame Schrenck-Notzing for his conduct on a recent occasion. In spite of his repeated protests that all such matters should be investigated under the scientific conditions that obtain in the laboratory, he nevertheless attempted to prevent the scientific examination of a medium in Berlin a few months ago. He had heard that certain sceptical Berlin investigators, whose names were given, had been invited to the séance of a medium. He stated that this was unfortunate and that the meeting ought to be prevented as it might injure the lady's mediumistic powers, which were still in the developmental stage. He said he was induced to take this step, because professors at the University of Munich, and noted neurologists, had declared that a promise made to a mediumistic "circle" need not be kept, and that the only scientific way of getting at the truth is to catch the phantasm and hold it fast. I am unaware that Munich scientists have ever advocated perjury. It is hardly possible to find any other motive, as Schrenck-Notzing calls it, for his action than his spiritistic tendencies, of which he may, perhaps, be unconscious. In an earlier work, Schrenck-Notzing attacked the experts who had pronounced against thought-transference, and he also expressed the opinion that many of their assertions were due to *à priori* aversion, and were not based on experimental research. "I am convinced this is the most unscientific way of getting rid of questionable proceedings. Consequently we are spared the necessity of submitting such verdicts to a searching criticism." Here we have Schrenck-Notzing blaming the men who do not experiment, and yet on other occasions he does his best to hinder those who are willing to investigate the question experimentally. I very much doubt whether a man who deprecates scientific séances—and it is only with such that we are concerned—has any right to set himself up as an objective investigator of mediumistic phenomena. But all this can be easily explained if we remember that Schrenck-Notzing is an honorary member of the German Society of Spiritists; that in the very article in which he advocates the investigation of mediumistic phenomena, he also indulges in a hopelessly uncritical disquisition on the "unexplained residuum," and that he also poses as the champion of Eusapia Palladino and similar persons. In reality, Schrenck-Notzing is so entangled in the meshes of spiritism, that he has apparently lost the power of observing and thinking objectively.

I have already on previous occasions referred to the numerous sources of error which we must not overlook in our inquiry into occultistic experiments. These sources are to be found

either in the person of the subject of the experiment, or in that of the experimenter or of some one else present.

Let us first deal with those contained in the person of the subject. The following important points have all to be taken into consideration:—

1. Intentional deception on the part of the subject with or without hypnosis. Simulation of hypnosis is in this connection of little moment; for it is evident that if any one professes to see without using his eyes, the whole question turns on the truth of that assertion, and that it matters little whether or not it is supposed to occur during hypnosis. But we must also remember that the deepest hypnosis does not exclude the possibility of fraud. An instance of deception practised during hypnosis may perhaps be found in the case of Eisenstein's somnambulist, who prophesied that she was about to spit blood. Czermak, Voigt, and Langer subsequently showed, microscopically, that this was bird's blood, which, of course, the somnambulist had previously placed in her mouth.

2. Unintentional delusion, if such an expression may be used. The subject may receive impressions through the ear, for instance, without being conscious of it. This occurs in transposition of the senses, the subject perhaps believing that it is with his stomach he hears. Or it may be in consequence of previous training that on the approach of the magnet transference takes place. It being assumed by the subject that the approach of the magnet suffices to produce transference, he acts in perfect unconsciousness that he is leading the experimenter astray. In the same way the percipient in telepathy guesses the other person's thoughts by signs, but himself thinks that the thoughts are transferred to him directly and without any signs being made.

3. Hyperæsthesia of the subject's organs of sense. We have already come across this in hypnosis; it sometimes permits the hypnotic to perceive things imperceptible to others. Let us, however, bear in mind that the term "hyperæsthesia of the organs of sense" is not quite accurate, for the processes here concerned are really central. This so-called hyperæsthesia can be produced by practice, and also without hypnosis. By neglecting to take this circumstance into consideration we may easily be led into drawing erroneous conclusions.

4. Increased power of the intellectual faculties—*e.g.*, the memory—may also prove misleading. I recall the cases of

people who spoke in languages they had never learned (*cf.* p. 466). We must also here include the increased power of drawing conclusions. This sometimes takes place in the secondary consciousness. Hennig relates the following case:— "A friend of mine, head-master of a school, once dreamed that he was being touched by the cold hand of his father, who had just died. The shock woke him up, and he heard a hissing sound; but after a careful search he found that the gas was turned on and escaping. His secondary consciousness had perceived this, recognized the danger, and warned the sleeper in a symbolical dream." In any case, hypnotics and people who are dreaming often come to such subconscious conclusions, but part of the conclusion may also be arrived at subconsciously in the waking state. Sometimes, also, an increased power of the intellectual faculties, a telepathic influence or the like, is erroneously assumed when we are really only dealing with a combination of circumstances well within the experiences of daily life. It may easily happen in thought-reading, as well as in telepathic experiments, that when the percipient is led in some way or other close up to a box of cigars or a book-shelf, he will take a cigar or a book, as the case may be, and that this was what was wanted of him. The process is so simple, that we need not look to any heightening of the subject's intellectual faculties or other extraordinary influence for an explanation.

I have hitherto spoken of the sources of error that lie in the person of the medium. I will now discuss certain others that are external to the medium.

1. We must first of all consider the mental state of the "believers." I have already intimated that most occultists exhibit a peculiar turn of mind. I do not mean to imply that there is any question of insanity in their case, though spiritism may be the result of mental disease. Aural delusions sometimes lead their victims to refer the words they hear to the voices of spirits. But this is an exception. In the case of spiritists, a peculiar weakness of mind is of somewhat more frequent occurrence. But this also is, relatively speaking, rarely the case. On the other hand, we much more frequently find unbounded credulity and a tendency to exaggerate their own powers of observation on the part of spiritists. With regard to the latter point, it is interesting to observe how fond occultists and spiritists are of accusing men of science of over-

weening self-confidence and exaggerating the value of science, although it is hardly possible to find in any branch of science men who suffer from overweening self-confidence to the same extent that the overwhelming majority of spiritists and occultists do. The latter believe that they cannot be deceived, because of the precautions they take, and also because their powers of observation safeguard them from deception. But these precautions do not satisfy the requirements of objective investigators. Let me give an example. A spirit appears after the medium has been bound with cords. This sets a careful investigator thinking whether the medium may not have got loose and be representing the spirit, whereas the spiritist thinks that the medium could not get free, because he bound him himself. He takes no notice whatever of the fact that the medium was only bound to his own liking. The real man of science is fully aware that he is liable under certain circumstances to sense-delusions, and more especially to delusions of memory. This fact, also, is never considered by occultists. It is astonishing what different accounts people who have taken part in such séances give of what occurred, when they are subsequently questioned separately. This applies equally to cases of spiritistic phenomena and clairvoyance. Nevertheless, occultists are firmly convinced that their memory never plays them false.

As might be expected, numbers of occultists and spiritists are led by their exaggerated mystical tendencies to recognize occult phenomena. Every one may possibly have a touch of mysticism in him. Even men of science are not necessarily exempt, but they should be extremely cautious before accepting anything as scientifically proved. *It is their duty to draw a sharp line between the domain of Belief and that of Science.* Occultists do nothing of the kind; their mystical tendencies utterly prevent the spirit of scientific research having anything to do with the investigation of these phenomena, and they therefore believe, with a child-like credulity, anything they choose. In addition to this, many occultists and spiritists hate science, because it includes them in one category. Instigated by the desire to affirm anything that science refuses to recognize, they accept the most impudent performances of fraudulent mediums as proofs of clairvoyance, transposition of the senses, animal magnetism, Od-radiations, and the like. The fact, already mentioned, that most of the people who

ascribe a special domain to occultistic phenomena also believe in any other kind of occultistic phenomena, no matter how disconnected they may be, is extremely characteristic. I have already mentioned that animal magnetism cannot explain clairvoyance; nevertheless, we find that most believers in clairvoyance are also upholders of animal magnetism. The converse is also the case. It is also noticeable that many occultists are also believers in homœopathy, nature-cures, vegetarianism and Jäger's all-wool régime, although these questions are in no way connected and have nothing to do with occultism. To a magnetopath, who describes himself as a representative of homœopathy and nature-cures, we can only say that homœopathy has no connection whatever either with magnetic therapeutics or with nature-cures. In the same way it is chiefly spiritists who believe in astrology. I have also heard it remarked in conversation that many occultists and spiritists believe that premature burial is anything but a rare occurrence. Some people have even assured me that there are fire-proof human beings—*i.e.*, persons who cannot be destroyed by fire. It is obvious that many are influenced to enter on these matters simply because of the implied revolt against orthodox opinion, nor can it be denied that the cause of occultism is furthered by the arbitrary refusal of many adherents of academic learning to give it a hearing. Hatred of science and the joy of opposing it, combined with an uncritical tendency to mysticism, explain how it is that occultists acknowledge a belief in such heterogeneous domains of the the phenomenal as those of the divining-rod, animal magnetism, homœopathy, and the action of the magnet on human beings. *It would be safe to bet 10 to 1 that, if science were to give official recognition to animal magnetism and transposition of the senses, a very large number of occultists would at once commence an attack on those beliefs.*

2. As further sources of error, I must mention sense-delusions and all kinds of errors of perception. Spiritists are particularly prone to sense-delusions. One can readily observe at spiritistic séances the way in which semi-darkness facilitates the appearance of all kinds of forms, and how everything is magnified by imagination. I remember a séance at which, on a lady declaring she could see an apparition, a gentleman present told her to close her eyes, because she was obviously not seeing with them, but with her stomach; and that, therefore, she could see

much better with her eyes closed. It is often very difficult to distinguish accurately from what direction the sounds and noises come. For this reason, spiritists who are in a dark room believe they hear the music of guitars that are floating about in the air, whereas the instruments really remain in the same place all the time. It is the same with the "raps" which spiritists declare proceed from the tablecloth or the table itself, although they are really produced by the medium's foot. These illusions of perception extend even farther. Any one who wishes to see inanimate objects move of their own accord, and who for a considerable time vividly expects the occurrence of the phenomena, is very likely to see such movements, although the objects never stir from their respective positions. It was precisely in the case of Harnack's subject (p. 505) that I saw how easily tension of the apparatus of attention leads to the illusory perception of movements which objective investigation proves never took place.

3. Illusions of memory constitute another source of error. I have already pointed out their importance when discussing cases of the supposed appearance of dying persons to friends or relatives at a distance, and will only add that Christian considers illusions of memory a special source of error in telepathic experiments. In clairvoyance, also, it may happen that when a phenomenon occurs many people may believe that they have seen it before. Mnemonic adaptation, which has already been mentioned, is just as important as illusions of memory, because details that do not favour an occultistic interpretation of the phenomena may be subsequently eliminated by a capricious use of the imagination.

There is one special form of illusion of memory that has to be considered in many cases. It has been described by Bernard Leroy, a French author, who calls it *Illusion de fausse reconnaissance*, but Sander, Kraepelin and others had previously reported cases of the kind. The phenomenon consists in a person having the impression that he sees or experiences something for a second time, which is really perfectly new to him. Although in most cases the error is at once corrected and the illusion recognized, there are others in which this does not occur. This is particularly the case in mental disease, but it may also happen when the subject is perfectly normal. The effect produced on some people by this phenomenon is peculiar. They neither consider the whole affair an illusion, nor do they

believe that the incident had ever really once happened to them; on the contrary, they are convinced that this is its first occurrence, but they believe that they have once had a mental vision of it. This leads them to the conviction that they possess the power of prevision, and some, indeed, declare they are clairvoyant in their dreams, others in the waking state. Undoubtedly this mnemonic error explains many cases of prophetic dreams and also of second sight, which latter, according to Perty, is but the vision of a present or coming event seen in a rapidly-passing delirium occurring in the waking state.

We have also to reckon with a further source of error that lies in the memory. It consists in a person being under a delusion as to what he recollected at a particular time. An example of this. X. went to a clairvoyante who told him of something that had happened to him years before. He admitted the accuracy of her statement, but subsequently explained that he had completely forgotten that episode of his life until the clairvoyante reminded him of it. From this he would have us believe that the clairvoyante could not have acquired any knowledge of his past either by telepathy or from any signs made by him. A careful investigation of several of these cases has provided me with almost irrefutable evidence that X. may quite well have recollected the episode during the séance but have afterwards believed that he had previously forgotten all about it until reminded of it. During the séances the clairvoyante by her remarks drew X.'s attention to things he had forgotten, and the associations thereby aroused refreshed his memory on various points. The excitement that prevailed and X.'s feeling of expectance favoured the stirring up of such recollections. As we saw when discussing telepathy, directly a subject recollects anything the fact is easily betrayed by his making some sign. The prevailing excitement smooths the way for subsequent mnemonic errors, so that X. is quite unable to decide later on whether he remembered the episode at a certain moment or whether the clairvoyante made him think of it at that moment. We know that this kind of confusion of memory often occurs, and I consider it far from improbable that the séances of the "great medium" Mrs. Piper may be partly explained in this way.

Mrs. Piper is an American medium. It is said of Mrs. Piper—or, rather, of the spirit that is supposed by spiritists to act through her—that, in the

course of some very carefully conducted experiments, she spoke of things of which she could previously have had no knowledge. The most frequently cited cases are those in which she held séances with Professor Hyslop, to whom she communicated facts concerning his dead relatives, which apparently she could not have learned in any ordinary way. Max Dessoir does not consider this a case of simple fraud, and we must admit that if it was, the fraud was perpetrated very cunningly. But Max Dessoir also thinks that the telepathic theory fails to afford an explanation, for the very reason that Hyslop may very well at one time have known of these matters, but that he was certainly unaware of them when the medium related them. Now, Max Dessoir thinks that there is certainly no instance of a medium having acquired by telepathic means a knowledge of what is unconsciously retained in another person's memory. Since Hyslop, moreover, was unconscious of these matters at the time the medium mentioned them, it seems that we may exclude the possibility of his having guided her by signs. Nevertheless, I do not think that it is necessary to appeal to occultism for an explanation of the case. In the first place, the theory of fraud was not actually disproved by the investigators. They say they took all kinds of precautions, but it has not been proved that Mrs. Piper might not have previously found out at least much of what she communicated. There is no necessity to assume that all successes can be explained in this way. Dessoir certainly further admits that some of the successes might be accounted for by the medium carefully putting together all that she heard during the séance. When, for example, the medium posed as the spirit of Hyslop's dead father and said to the son, "You are not the strongest man," and the son remarked, "He warned me hundreds of times that I was not as strong as other people," Max Dessoir thinks that this can be explained by the fact that any one could see that Hyslop was not very robust. At all events, some of the cases could quite well be explained in this way. When we further come to consider that in a whole series of cases the statements were wrong, there seems to be very little necessity indeed for our assuming that Mrs. Piper possesses unknown powers.

The fact that memory is not infallible also fills me with mistrust. In any case, some one was present at Hyslop's experiments who was acquainted with the episodes related by the medium, and of whom, at least, it was not proved that he was ignorant of their occurrence. We may add that letters subsequently received showed that some of the communications made by Mrs. Piper were correct. As we shall see later on, some of these cases can be very easily explained as mere coincidences.

4. Suggestive gestures and other signs, by means of which the subject learns what he or she is expected to do, play an important part in those sources of error that lie outside the person of the subject. When dealing with the question of the training of hypnotics I explained how often the latter perceive signals in the most unimportant signs. We can readily understand that such insignificant signs given by the experimenter or anybody else present constitute an important source of error. It is essentially to hypnotism that we owe the discovery of this

source of error, since the training of hypnotics clearly showed that even insignificant signs act suggestively on the subject experimented on. When discussing super-normal thought-transference, I gave a detailed explanation of the way in which the most diverse organs of sense may help the subject to make use of such signs. Consequently, whenever there is the possibility of the subject being influenced by signs, it is the first duty of the experimenter to impose such conditions that the subject will be unable to perceive such signs. As another of the chief sources of error I must, moreover, add the circumstance, that if the experimenter or any other person present believes, or begins to believe, in the reality of the phenomena, they very easily become unintentional accessories of the medium. It is just the same as with table-turning, in which the believers push harder than the unbelievers and so set the table in motion.

It must also be pointed out, that by means of signs things may be communicated which, at the same time, are non-existent in the primary consciousness of the agent who communicates them. We have already seen this in automatic writing (p. 246). But if, as Lehmann and Hansen have pointed out, unintentional whispering plays so great a part, it is quite conceivable that some people, perhaps those who are psychically predisposed to the influence of processes going on in the secondary consciousness, may be induced to whisper by such processes. I am led to this assumption by the reflection that involuntary whistling is a process analogous to automatic writing. If subconscious processes can be communicated by means of automatic writing, it is presumable that the same thing can be done by whispering. In any case, I have recently learned from some experiments I made—they did not produce anything new in this respect—that thought-reading is possible even when the subject is not conscious of what has to be done. I requested a hypnotized subject to write down the problem I was to solve on a piece of paper. In one case I was to take a duster from its place and put it in a cigar-box. The subject wrote this down on a piece of paper which she folded while I stood apart and therefore had no knowledge of the nature of the task. I then suggested to the subject, who was still in hypnosis, that on waking she was to fix her thoughts intently on the task but was not to remember its nature consciously. At the same time I suggested to her—I

had often made the ordinary thought-reading experiments with her—that she was not to notice whether she made any movement or was guiding me. I then woke her up. In spite of the fact that I did not know what the task was, I performed it correctly, guided by her. Since the task was written on the paper the experiment was easily controlled. The subject neither knew that she moved nor was she conscious of the processes that made her do so. This case is only intended to show that processes going on in the secondary consciousness can cause the subject to make signals. The experiment was quite a simple one. Finally, if this sort of thing does happen, the assumption that there are people who can make known the processes of their secondary consciousness unintentionally and without noticing it would not be contrary to our scientific ideas or the views we already hold. This is also a very important source of error in clairvoyance, and may have played a part in Hyslop's experiments, for, at all events, it was never proved that he had not previously heard of the things that were then told him about his relatives—*i.e.*, that the matters in question were not hidden in his secondary consciousness. Eberhard Wolff even attempts to utilize the case of Mrs. Piper as a proof that unintentional whispering does not explain the phenomena of telepathy. I do not consider that the conclusions hitherto arrived at finally decide whether subconscious processes may not be communicated by involuntary whispering.

4. Fictitious interpretation. It should not be forgotten that the vast majority of prophecies are as indefinite as the sayings of the Delphian Oracle, but that the subsequent interpretation of them leads the uncritical to believe that even clairvoyance is an established fact. We come across a similar phenomenon in cases of thought-transference. As Lehmann and Hansen have shown, the drawings said to be reproduced by telepathy are often so indefinite that they may be taken to represent anything you please. They have shown, for example, that the same drawing will do for a candlestick or a cat. Whoever wishes to prove the reality of telepathy gives any interpretation he likes to a drawing. A book by an anonymous writer, H., that appeared in 1848, contains much interesting information on prophesying. We are told that should the event not agree with the prophecy, we must remember that everything contained in a prophecy is not to be taken literally, so much is only metaphorical. As an example of the ambiguity of prophecies

Hennig cites the case of Malachi, who is said to have characterized each Pope by a short descriptive epitheton. Thus Leo XIII. is said to be referred to in the words Lumen de Cœlo, and his successor is characterized by Ignis ardens. Now, Hennig shows to how many cardinals this prophecy might apply if only a little ingenuity were exercised in interpreting it. It would describe that restless being Rampolla accurately. Cardinal Svampa might also have been meant, for *svampa* is Italian for torch; but it would also apply to Cardinal Gotti, whose coat-of-arms contains a torch; or to Cardinal Capecelatro, an idealist filled with modern ideals. But it would apply even better to Cardinal Hohenlohe, whose name is a literal translation of Ignis ardens. But when Cardinal Sarto, of whom nobody had ever thought, was made Pope, Malachi's prophecy was interpreted as pointing to him. But as there was nothing fiery about Cardinal Sarto, either intellectually or in his temperament, and, on the other hand, Malachi could not have prophesied falsely, it was discovered that the coat-of-arms of S. Dominic, on whose day (August 4th) Sarto was elected Pope, contains a dog from whose mouth a flame issues, and it was at once said that the prophecy Ignis ardens referred to Cardinal Sarto, and thus Malachi's prophecy was fulfilled. Hennig has done a great service in dissecting this case so thoroughly. It is only by quibbling that so many, many prophecies are made out to have come true.

Spiritists have lately been very fond of quoting the following case as a proof of clairvoyance. On the evening of June 10th, 1903, Charles Richet held a mediumistic séance. The people present wished to hear "raps," and they did. The following words were rapped out:—"Bancalamo" and "Rtguettefamille." Directly Richet read these words he is supposed to have been struck with the idea that they formed a complete sentence with a definite meaning, and he deciphered them as follows:—*Banca la mort guette famille.* This communication was rapped out between 10.45 P.M. and 11 P.M. On the following day the news arrived that Queen Draga of Servia, together with the king and her family had been murdered overnight, and the sentence was immediately made to refer to that crime. To accomplish this the following method of interpretation was necessary. Banca was converted into Panta. Richet thought it was so easy to mistake a B for a P. The Germans have a way of saying *ponne bouteille* instead of *bonne bouteille*, and the Servian pronunciation of T is best represented by the French C or Z. The name of Queen Draga's father was Panta, and the sentence consequently referred to him and a threat to murder his family. Richet certainly admitted that the words, "la mort guette famille," might apply to any number of families, and that it was consequently necessary to calculate the probability of Banca having been rapped out correctly. This

he found to be 1 : 10,000. Consequently the choice lay between coincidence and clairvoyance. Richet assumed that it was a case of clairvoyance, because a Press Agency in Paris received the news of the murder at 10.45 P.M. the same evening.

I have asked different people what they would read into such a sentence as "Banca la morte guette famille," and most of them replied that the word "Banca" would make them think of "a bank," and that they should never think of turning it into "Panta." At all events, I should have been much more inclined to find the case one of clairvoyance if any great bank had failed and driven one or two people to suicide. The theory of clairvoyance would have applied equally well to either supposition, mine or Richet's. Another man I questioned thought the sentence referred to death on a glacier. He considered "Banca" meant "Bianca," white death, or death on a glacier. I think that any one from the Sunda Islands would have referred it to Banca, an island east of Sumatra. A man, who was interested in ancient arms, said it must mean Bancal, a term for a curved sabre. A lady to whom I showed Richet's lettering, "bancalamo rtguettefamille," said it made her at once think of calamus, as if something had been done with calmus. Another man said it reminded him of a Ban of Croatia, because "ban" was the French for it. I looked this up in the dictionary, and can only say that "ban" has so many meanings that it might be made to supply a dozen—nay, hundreds and even thousands of interpretations of that prophecy. But Richet was determined to have it that the murder of the Servian Royal Family was conveyed to Paris by means of clairvoyance, and so he proved it.

Richet's method of turning Panta into Banca and Banca into Panta reminds one of those philological tricks against which we were warned in our school-days—the little dodges by means of which one word could be derived from another by changing a letter; always, of course, on apparently scientific grounds.

6. The probability of chance success. As many experiments fail, it should be considered whether the number of successful ones exceeds probability. Preyer, however, is inclined to think that for our objects statistics of probability have little significance. But if such calculations are to be taken into consideration they must be made correctly. Richet's case shows how little this is thought of. He calculated the probability of "Banca" having been correctly given at 1 to 10,000. But he quite overlooked the fact that "Banca" was not spelled as one word; it was wrapped out as "Bancalamo." He also overlooked the fact that if the sentence had not been made to fit in with the murder of the Servian king, it might equally well have been made to refer to a thousand other episodes, as I showed in my explanation of fictitious interpretations. Moreover, he did not take into account the frequency of false prophecy, or, more especially, how often things have been communicated at his own séances which were never substanti-

ated. It is only when all these matters are taken into account that we can ascribe any reasonable importance to probability. But to pick out arbitrarily some point that favours clairvoyance, and at the same time to pass over unsuitable elements, cannot give a correct picture of the probability that the clairvoyant was accurate in his prediction. It almost invariably happens that prophecies to which but one meaning can be ascribed are only published when the event prophesied has occurred, a circumstance that does not exactly show that much credence is to be placed in previsional clairvoyance. Nevertheless, a few cases have been published beforehand, though certainly with marvellously bad results. Only, spiritists are much too fond of suppressing their failures; for example, I have been unable to find that any further mention has been made of the prophecy published in 1899 that Dreyfus would die in 1904. There can be no sense in calculating the probability of success in cases of clairvoyance unless the failures are included in the calculation.

7. Coincidence. *E.g.*, a command given in thought may be obeyed, because by chance, or for some other reason, experimenter and subject think of the same thing. In telepathy the first order thought of is nearly always that the right arm should be raised. This source of error is both great and interesting. It has lately been carefully examined by a member of the American branch of the Society for Psychical Research, C. S. Minot. Thus, it has been discovered that every one prefers certain figures, etc., which recur strikingly often, even when the choice is left open. Now, when in a telepathic experiment A. thinks of a number which is to be divined by B. without the latter making use of any known method of perception, it would be necessary to discover if they prefer the same figures, if they have the same "number habit." This must also be weighed in experiments with cards, in which it appears to me the ace of hearts is very often chosen. It is evident that great care must be exercised in drawing conclusions, and that the study of mysterious phenomena leads to the recognition of important laws.

We have already come across coincidence as a source of error in the supposed appearance of dying persons to their friends who are far away. What Parish considered as coming under the fourth source of error was really a special case of coincidence. Coincidence and fictitious interpretation to-

gether very frequently lead investigators into error, because the greater the number of interpretations that can be given to a manifestation—*e.g.*, in clairvoyance—the more likely will the case be one of coincidence.

It sometimes happens that the probability of coincidence is so great that the manifestation is utterly valueless. In the case of the medium, Mrs. Piper, it is mentioned that Hyslop was asked by the ghost of his father at one of the séances: "Do you remember the penknife with which I used to clean my nails?" Hyslop said, "No"; and the spirit replied, "You must remember the little knife with a black handle, that I first of all carried in my waistcoat-pocket and then in my coat-pocket?" Hyslop then wrote to some of his relatives and learned that his father had possessed a black penknife with which he cleaned his nails. "Only it appeared that Hyslop senior did not carry the knife either in his waistcoat-pocket or in his coat-pocket, but in his trouser-pocket." In the preface to the German edition of the book in which Mrs. Piper's feats are described, Schrenck-Notzing remarks with regard to the above and other cases:—"These examples are quite satisfactory as far as minor details are concerned." I must acknowledge that this example does not satisfy me, and that I am as little satisfied with the other examples with regard to either major or minor details. In any case, numbers of people carry a black-handled knife with which they clean their nails. Hyslop's experiment failed in one important point; his father did not carry the knife in his waistcoat-pocket or coat-pocket, but in his trouser-pocket. This point is certainly mentioned, but only in a way that would make the error appear to be of no importance. I do not quite understand what things of this nature are intended to prove. At all events, as far as the knife was concerned, the probability of chance success was so great, that the whole episode cannot be said to have proved anything.

If any good is to come from the investigation of the phenomena of occultism, the way in which the minutes of occultistic séances are kept is of the greatest importance. I consider a systematically kept protocol a necessity, but I no longer think such documents as convincing as I used to do. A superficial glance at the published protocols is often convincing, and spiritists are very much astonished when doubts are expressed as to the accuracy of the reports.

Unfortunately, it is generally found to be quite right to doubt whether these reports are a faithful reproduction of what actually occurred. A few years ago, I was able, in an essay entitled *Physician and Judge*, to throw some light on the untrustworthiness of the official records kept of the proceedings in magistrates' courts, about which I gave the following explanation:—"These records are not calculated to give a correct view of how such cases are conducted. If a protocol is to have any claim to accuracy it ought not to be a mere *résumé* of the proceedings, but should be an accurate transcription of the shorthand notes of every question and answer. It ought to be possible to gather from the report whether the witness hesitated or vacillated, whether a question was first of all answered in the negative and whether an affirmative answer was only given after the question had been frequently repeated. In such a case the affirmative answer ought not to be quoted by itself. It is only when the protocol records that the witness hesitated or was at first silent, that any weight can be laid on such a document." The above concerned judicial records. But I hold the opinion that we must demand as much of the records of occultistic investigations. When much conversation goes on at such séances, only an extremely expert and persevering stenographer can follow the proceedings. But even such a one is quite unable to reproduce mimetic signs in writing. In addition to being an extremely expert shorthand-writer, he must be able not only to decide when he is to insert the word "stop" in his notes, but also the tone in which it was said, and that is hardly possible. In short, even stenographic notes are extremely untrustworthy. In addition to this, most of the minutes are not even taken down in shorthand; or, at least, not in the same way that the proceedings in Parliament are. As a rule, some one, who may possibly be prejudiced, dictates the notes at a séance. Of course, if the person who dictates is prejudiced, and anything occurs that is not in accord with occultistic views, he will involuntarily fail to record it; or, at least, give it no prominence in the report. He will consider such events mere trivialities, whereas they are of the utmost importance to a really objective investigator.

As we have already seen, Crookes has written an account of his experiments with Home, which obviously contains many important additions. But how little value he attaches to these important "trivialities" is best shown by the fact that he

omitted them in the first edition; as a natural consequence, this edition has long been the mainstay of spiritists. A second edition appeared later on, which, as Lehmann has shown, entirely contradicts the first, because the "trivialities," which showed how unreliable the whole séance was, were left out in the earlier publication. Spiritists are strikingly reticent about this criticism of Crookes' experiments; at least, they have never attempted to controvert it. This is all the more important because Crookes' experiments were formerly held up as the *non plus ultra* of research, and were said to be unimpeachable. If an author leaves out of his account of an event the most important details, even when the latter are mentioned in the official records, it is easy to guess what may happen even in the way in which the records of other cases were originally drawn up. I have no doubt that the same sort of thing occurs in many cases of thought-transference. Schrenck-Notzing admitted, in reply to some critical remarks which Richet and I had made about his telepathic experiments, that he "did not consider it altogether impossible that some of the published reports were so coloured—unintentionally and involuntarily, of course—as to give a favourable impression of the results obtained." It is particularly important when investigating cases of telepathy and clairvoyance, to notice the different things the mediums have a shot at. Any one can easily observe at such séances that the correct result is only obtained after three or four failures, but that once obtained, the spiritists present are jubilant and suppress any reference to the failures. It is anything but easy to decide to what extent these failures that are so quickly forgotten are considered in the published reports. I certainly surmise that they are left out in numerous cases, and that only the successes are reported, most of which can be explained by the laws of probability, or by a correct combination of the circumstances of the case having been made.

In short, it must not be imagined that the official report of a séance gives an accurate account of what really occurred. For example, a clairvoyante as a rule never gives a consecutive account of anything she is supposed to be describing; she hesitates and pauses until some one present, either by nodding or shaking his head—perhaps unintentionally—gives her the cue that she is on the right track; or some one sighs, which is also a sufficient hint. Grützner has already very properly emphasized the fact that certain subsidiary details which are

of great importance may easily escape the observer. Here we have just the same thing happening as in juggling, in which those of the onlookers who think themselves smartest believe they see everything, whereas they really miss every essential detail. I should like to refer once more to the case of "Clever Hans," because it shows the exact value of these protocols. It is true that Stumpf did not publish the detailed report upon which his fallacious statement was based. But Stumpf's own statement clearly proves that he did not see the most important thing that happened—viz., the signs given to the horse. If we adhere to the fact that such a thing occurred in the case of a committee over which Stumpf presided, we can make a very shrewd guess as to what happens where other experimenters are concerned. The gentlemen who are supposed to investigate clairvoyance and other mediumistic phenomena are taken in, just as Stumpf was in the case of the horse. When we are assured that the clairvoyantes were acquainted with things that no one else knew, I am compelled to add that the same was said of "Clever Hans"; for example, Herr Grabow, a member of the Council of Education, and one of Stumpf's committee-men, persistently maintained that "Clever Hans" made correct arithmetical calculations, even when no one present was aware of the problem set.

We have also to reckon with the fact that most of the people who make such experiments are lovers of the mysterious and earnestly desire successful results. In such cases everybody sees what he wants to see. I do not intend to blame spiritists, etc., specially in this respect, for it is a matter of daily observation that preconceived notion obscures the view of scientists as well. A few examples will show how easily this occurs in other respects. There are districts in which prayers are offered up for rain when there is a long-continued drought. Any one who believes in the efficacy of prayer is easily able to prove to his own satisfaction that such petitions are answered. A second example: I remember a lawyer in Silesia who firmly believed in a preparation for making the hair grow; although nobody could perceive any diminution in his baldness, he was thoroughly convinced that he was putting on a fairly strong fresh growth of hair. He looked in the glass every day, and always thought he could detect fresh hairs. A third example: Children are convinced that the song, "Fly away, lady-bird!" really makes the insects fly. They keep on singing until the

lady-birds fly off of themselves, and then they believe that the song was the cause. In short, we observe how in each of these cases the wish to see a thing happen misguided the judgment, and the same thing occurs in spiritistic séances, but more especially with respect to the way in which the minutes are kept. Even in the case of doubters the experiments cause such a tension of the apparatus of attention that their mental condition cannot be said to favour the exercise of criticism. We have also to consider that the whole environment rarely or never favours the formation of an objective decision. With some people it provokes laughter, but on others the emotional effect is such that they cannot judge the proceedings from a cold-blooded point of view.

As I have already mentioned, we need not attach much importance to the fact that a few savants uphold the reality of occultistic phenomena. I myself formerly attached a certain amount of importance to this fact. But since I have observed the utter helplessness of savants directly they enter on methods of investigation with which they are not thoroughly acquainted, I have become convinced that mediums easily lead great savants by the nose. After one or two séances, mediums easily recognize what will satisfy one person but not another. I remember once meeting a savant of my acquaintance, who was a believer in occultistic phenomena and especially in the miracles of Eusapia Palladino, and how instructive that *rencontre* proved to me. When he explained to me that he had seen objects move and tables which were not attached to the medium jump up in the air, I told him that I had witnessed similar phenomena, but did not consider them convincing, as the whole proceeding could very well be worked by means of a string. He thereupon assured me that he had examined the subject, and was therefore convinced that she had no string about her. I then asked him how long the examination lasted, and his answer showed that it only took up a few minutes. I then told him how astonished I was that he should think himself able to discover in a minute or two whether a woman had a string concealed about her person, and I further assured him that all professional jugglers would agree with me that a thorough examination was extremely difficult. But he remained obdurate; it was enough for him that he had examined the woman. This only shows how careful we should be to avoid being imposed on by authorities!

When I come to look through the vast literature of occultism, I find that *I am totally unable to discover even one single series of experiments that carries with it a convincing proof of the reality of occultistic phenomena;* nothing but casual observations or unchecked experiments. There was a time when some of the telepathic experiments carried out in England—more especially those made by Guthrie and Birchall—appeared to me, relatively speaking, free from error. Nevertheless, when I take into consideration the way in which the reports are drawn up, I am compelled to admit that those experiments are not convincing. As Max Dessoir has pointed out, it is only when a single experiment is selected and then performed under varying conditions that we can speak of a scientific investigation (*cf.* p. 533).

There is nothing to be said against the present investigation of inexplicable things. Almost all great steps in natural science have been made by some one who had the courage to contest existing views, in spite of the danger of looking ridiculous. Harvey was obliged to struggle with the prejudices of his colleagues for years before the circulation of the blood was accepted. The fall of meteors projected by other celestial bodies was long denied. Modern anatomy was founded by Andreas Vesalius, who had to fight hard against the prejudices of his day. Helmholtz relates that a very distinguished surgeon told him he would never use the ophthalmoscope, because it was dangerous to throw a glaring light into a patient's eye. Another declared that the instrument might do for doctors afflicted with weak sight, but his eyesight was good and did not require such assistance. As we see, the fact that a thing is attacked ought not to prevent its being investigated. The assertion that a thing is contrary to the laws of nature and therefore wrong should not be considered conclusive. The contradiction is often merely apparent, and the laws of nature, as we call them, are only derived from facts we have observed. When new facts are observed which do not agree with laws of nature we have been accustomed to accept, it is our duty not to deny the facts, but to modify those laws. Theories never precede facts. Observation and experience come first, then theory (Spencer). The electric current does not cause muscles to contract because the book says so; the book says so because the current causes the contraction.

In spite of scientific doubt, truly great men always endeavour

to avoid dogma and *à priori* conclusions. If they cannot examine everything themselves, they yet consider a scientific examination even of the improbable necessary. An example which Delbœuf brings forward may be mentioned. Darwin once wished, it is said, to examine the influence of music on the growth of plants, because such an influence had been spoken about before him, and .he therefore made some one play the bassoon for several days, close to some planted beans. If this anecdote is not true, it is well invented. *The non-recognition of dogma distinguishes science from blind faith, but to say a fact is impossible because it is opposed to the laws of nature is to dogmatize.*

Our knowledge of nature is still very defective. No one has ever explained even the simplest mental process. No one has explained how an ovum, fertilized but soulless, develops into a being with a soul. We have not the remotest notion what goes on in the brain when the will acting through it and the nerves causes the muscles to contract. Indeed, we do not even know why an apple falls to the ground. The most elementary processes are inexplicable wherever we look, and most people do not think them inexplicable because they see them every day. Some one has justly said that dreams, as well as hypnotism, might be called swindles if they did not happen every day. The world still puts so many riddles before us that it is quite inadmissible to attempt to shirk the investigation of certain processes on the ground that they are contrary to the laws of nature. Science is still very far from having achieved a position that would justify such an attitude.

We do, indeed, flatter ourselves that knowledge has reached an extraordinarily high degree of perfection. But if we look back, we find that the same opinion was held in the past. "No one acquainted with universal history and the ever-increasing field of scientific research can doubt that our century which is now drawing to a close, and more especially the last ten years of it, will be placed by an impartial posterity at the head of the most brilliant and marvellous epochs in the annals of mankind. . . . No matter whether we direct our attention to the domain of the sciences or the realms of nature, we are filled with astonishment and wonder at the extraordinary progress made in our days." That was the opinion expressed by Fikenscher in 1799. And Wieland wrote in his essay on magnetism, published in 1787:—"At a period in which

knowledge is so much more general than it used to be, and science stands on a higher pedestal than it ever did before, etc." Belief in the unparalleled development of science is common to all ages, and for this very reason it is our duty to discover whether it is justified. Our knowledge is so incomplete, and whichever way we look so many riddles meet our gaze, that we have no right to flatly refuse to recognize any domain of research.

In spite of the progress which the exact sciences have made, we must nevertheless admit that the inner connection between the body and the mental processes is utterly unknown to us. Under these circumstances we should not refuse to examine the apparently inexplicable. *Let us, however, impose severe conditions, and not accept any facts on authority without proof.*

BIBLIOGRAPHY.

As I have read nearly all the authors I have quoted, in the original, it would take too much space to mention them in detail. There are catalogues for certain periods—those of Möbius in Schmidt's *Jahrbücher* for the movement of 1880, and Max Dessoir's Bibliography of 1888, with an appendix containing a list of the works that led to the foundation of the schools of Charcot and Nancy, and also a list of the earlier works produced by the followers of those schools. Bibliographies are also occasionally published in the *Revue de l'Hypnotisme.* In response to numerous requests, I append a list of some of the more important works.

I. Books giving a General Account of Hypnotism.

1. Forel, *Der Hypnotismus.* Fourth edition, Stuttgart, 1902. (This is an interesting book and contains many aphorisms. It explains the general importance of hypnotism.)

2. Bramwell, J. Milne, M.B., C.M., *Hypnotism: its History, Practice, and Theory.* London, 1903. (Bramwell's book is based on an accurate knowledge of the subject. He gives the results of his own psychological experiments; for example, the fulfilment of suggestion after a long interval during which the time is calculated subconsciously.)

3. Crocq, *L'Hypnotisme scientifique.* Second edition, Paris, 1900. (A monograph dealing with the views of the Charcot and Nancy schools separately.)

4. Grasset, *L'Hypnotisme et la Suggestion.* Paris, 1903.

5. Preyer, *Der Hypnotismus.* Vienna and Leipzig, 1890. (Lectures delivered by the late Professor Preyer at the University of Berlin, containing many historical facts.)

6. Lœwenfeld, Dr. L., *Der Hynotismus. Handbuch von der Lehre der Hypnose und der Suggestion mit besonderer Berücksichtigung ihrer Bedeutung für Medizin und Rechtspflege.* Wiesbaden, 1901. (A good work and to the point. Suggestion is treated as a special phenomenon.)

7. MINDE, *Ueber Hypnotismus.* Munich, 1891. (An objective historical study, dealing especially with the older mesmeric literature.)

8. BINET AND FÉRÉ, *Le Magnétisme animal.* Paris, 1887. (Contains the views of the Salpêtrière school.)

II. HISTORICAL WORKS.

1. ENNEMOSER, *Der Magnetismus.* Leipzig, 1819. (Contains much historical information about animal magnetism.)

2. REGNAULT, *Hypnotisme, Religion.* Paris, 1897.

3. STOLL, *Suggestion und Hypnotismus in der Völkerpsychologie.* Second edition, Leipzig, 1904. (This book deals far more with the part played by suggestion than with that played by hypnotism in ethnological psychology. At the same time, it contains much important information on the two questions with regard to both civilized and uncivilized races.)

4. BECHTEREW, *Die Bedeutung der Suggestion im sozialien Leben.* Wiesbaden, 1905. (This book treats of the general significance of suggestion in social life. It contains accounts of epidemics of convulsions, the suicidal tendencies of sectarians, epidemic sorcery, demoniacal obsession, the epidemic spread of mythical doctrines, panics among men and animals, the effect of suggestion on crowds, etc..)

III. MEDICAL WORKS.

1. LIÉBEAULT, *Du Sommeil et des États analogues considérés surtout au point de vue de l'action du moral sur le physique.* Paris, 1866. A German translation appeared in 1892. (Psychological analysis of hypnotic and ordinary sleep. Full of information gained from case-books.)

2. BERNHEIM, *Hypnotisme, Suggestion, Psychothérapie.* Second edition, Paris, 1903. (Shows the universal importance of suggestion with and without hypnosis. Written for doctors. Many cases cited.)

3. GROSSMANN, *Die Bedeutung der hypnotischen Suggestion als Heilmittel.* Berlin, 1894. (A collection of reports drawn up by doctors in different countries on the importance of suggestion as a therapeutic agent.)

4. HIRSCHLAFF, *Hypnotismus und Suggestiv-therapie.* Leipzig, 1905. (This is a thoroughly revised edition of Hirsch's book. Strongly recommends the use of the author's method of superficial hypnosis. Deep hypnosis is superfluous in medicine, since it possesses no therapeutic value and is only useful for experimental purposes.)

IV. LAW TREATISES ON HYPNOTISM.

1. LIÉGEOIS, *De la Suggestion et du Somnambulisme dans leurs rapports avec la Jurisprudence et la Médicine legale.* Paris, 1899. (A rather diffuse book, containing much of deep interest.)

2. BENTIVEGNI, *Die Hypnose und ihre zivilrechtliche Bedeutung.* Leipzig, 1890. (A clever and thoughtful work.)

3. DELBŒUF, *L'Hypnotisme devant les Chambres législatives belges.* Paris, 1892. (The author advocates, among other things, public exhibitions of hypnotism.)

V. PSYCHOLOGICAL WORKS.

1. JANET, PIERRE, *L'Automatisme psychologique.* Paris, 1889. (Detailed psychological experiments on human consciousness, its analysis by means of hypnosis, etc.)

2. WUNDT, *Hypnotismus und Suggestion.* Leipzig, 1892. (Deals with the psychological importance of hypnotism and suggestion. An attempt to give a psychological explanation of the phenomena.)

3. DESSOIR, MAX, *Das Doppel-Ich.* Second edition, Leipzig, 1896. (Short psychological studies, partly connected with hypnotic experiments.)

VI. PSYCHO-THERAPEUTICS.

1. LŒWENFELD, *Lehrbuch der gesamten Psychotherapie.* Wiesbaden, 1897. (Contains much interesting information on medical psychology. Lœwenfeld draws a sharp theoretical distinction between suggestion and other psycho-therapeutic remedies, but he does not deal so thoroughly with the latter in the practical part of his treatise.)

2. ROSENBACH, *Nervöse Zustände und ihre psychische Behandlung.* Second edition, Berlin, 1902. (Treatment by means of a kind of educational therapeutics, to the exclusion of suggestion.)

3. ESCHLE, *Die Krankhafte Willenschwäche und die Aufgaben der erziehlichen Therapie.* Berlin, 1904. (Somewhat on the same lines as the preceding work.)

4. DUBOIS, *Les Psychonévroses et leur traitement moral.* Second edition, Paris, 1905. (Instructional treatment.)

5. CAMUS ET PAGNIEZ, *Isolement et Psychotherapie: Traitement de l'Hystérie et de la Neurasthenie, Pratique de la rééducation morale et physique.* Paris, 1904. (The title of the book indicates the contents. The book also contains evidences of patient historical research in psychology on the part of the authors.)

VII. BOOKS ON SUGGESTION WITHOUT HYPNOSIS.

1. SIDIS, *The Psychology of Suggestion.* New York, 1898.

2. BINET, *La Suggestibilité.* Paris, 1900. (Experiments on children.)

3. LEFÈVRE, *Les Phenomènes de suggestion et d'autosuggestion, précédés d'un essai sur la Psychologie physiologique.* Brussels, 1903. (Treats of both the psychological and medical importance of suggestion. Special reference is made to the part played by suggestion in pathogenesis.)

VIII. OCCULTISM.

1. OCHOROWICZ, *De la Suggestion mentale.* Paris, 1887. (Though the book does not prove telepathy convincingly, it is written with scientific earnestness, and is clever and interesting.)

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CORRECTIONS.

Page 32, line 19, *before* Epheyre *insert* Maupassant.
,, 130, ,, 19, *after* mamma *insert* "Kurella mentions an analogous phenomenon observed in certain pathological states—the photographic similarity of certain recurrent attacks in epileptics and persons suffering from periodic mania."
Page 235, line 3, *for* perceptive *read* perspective.
,, 284, ,, 23, ,, Munster ,, Munter.
,, 290, ,, 21, ,, Finkelberg ,, Finkelnburg.
,, 293, ,, 22, ,, Finkelnberg *read* ,,
,, 294, ,, 7, *after* auto-suggestibility *insert* (Hirschlafl).
,, 306, ,, 9, *for* Forel *read* Jolly.
,, 308, ,, 17, ,, Ortizky *read* Orlitzky.
,, 309, ,, 8, ,, Hirst ,, Hirt.
,, 312, ,, 4, ,, Pewnizki *read* Pewnitzki.
,, 312, ,, 15 from bottom, *for* Fränkel *read* Frenkel.
,, 312, ,, 12 ,, ,, ,, ,, ,,
,, 316, ,, 12, *after* peripheral nerves *insert* (Hilger, van der Briele).
,, 331, ,, 7, *for* Forel *read* Faber.
,, 350, ,, 1, ,, only ,, more than.
,, 417, ,, 3 from bottom, *for* Eyraud *read* Gouffé.
,, 447, ,, 7, *for* disassociations *read* dissociations.
,, 462, ,, 6 from bottom, *for* Vignolle *read* Vergnolle.
,, 465, ,, 8 ,, ,, stigmatist ,, stigmatic (Rieks).
,, 466, ,, 3 ,, ,, makes a speech *read* speaks a language.
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,, 473, ,, 10, *for* Henning *read* Hennig.
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,, 505, ,, 3 from bottom, *for* Reich *read* Reichel.

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p 176. Hypnosis is not sleep.

p 156 Influence of training (rel[illegible] general.)

p 158. Habituation very clear &

p 487. Drowsiness follows waking from tran[illegible]

p 87 Catalepsy of arm

230. Suggestibility increased in hypnosi[illegible]

235. Response to one suggestion favors t[illegible] response to later ones — (doubtful rationalization,

235. not necessary to begin with eyes - tch. bg.

264. States clearly suggestion main phenomenon of hypnosis &

65-6 Probably a statement of his view but rather indirect - that hypnosis has a heightened suggestibility

67. Suggestion connected with association

Note: Probably combine p230 + 264

BF114
913M

Sleeden arm p 208, 211

p 185 holds sleep + deep hypnosis are not different - valuable for book.

& p 124 Hypermnesia.

valuable material on P. H. A.

p 58 &, 61, 123,

p 163 If amnesias associated with P. H. S.

p 243. Plausible but inadequate theory of P. H. A.