



DR. MESMER

MESMERISM

by

DOCTOR MESMER

(1779)

*Being the first translation of Mesmer's
historic Mémoire sur la découverte
du Magnétisme Animal to appear in
English*

With an Introductory Monograph

by

GILBERT FRANKAU

(1948)

MACDONALD : LONDON

began to formulate his concept of the conscious and unconscious elements in the human mind.

The date of that book is 1893 and its author Thomas Jay Hudson. Its title page reads: *The Law of Psychic Phenomena. A Working Hypothesis.*

Hudson's hypothesis—briefly that we possess two minds, the one open to, the other completely closed against and immune from the process of inductive reasoning—is, I claim, an unbreakable link in my chain of evidence; as it were the central arch of a bridge spanning three centuries, the long road over which conducts the medical historian straight back from narcoanalysis (for which the hypnotic state is induced by drugs) and electric convulsion therapy (to a form of which, as you will read, Mademoiselle Paradis was also subjected) as practised by psychotherapists in 1948, through Freud's obsolescent psychoanalysis and Bernheim's equally obsolescent suggestion, to Elliotson, Esdaile and Braid. And from Braid, I claim, the road leads straight back to the author of this Dissertation whose hand rekindled in eighteenth-century Europe the torches of a science partly known to its so-called pagan inhabitants and consistently practised by the oriental world through countless centuries. . . .

Once again vanity, both the layman's and the professional's, tempts a conditioned pen. The pen would fain run on. But it has never been conditioned to the writing of medical history. And I must leave the fuller elaboration of my thesis to some writer of more knowledge, and maybe of greater objectivity.

In the meantime, let Mesmer speak for himself.

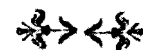
London.

July 1948.

G.F.

M É M O I R E S U R L A D É C O U V E R T E D U M A G N É T I S M E A N I M A L ;

*Par M. MESMER, Docteur en Médecine
de la Faculté de Vienne.*



A G E N E V E ;

Et se trouve

A P A R I S ,

Chez P. FR. DIDOT le jeune, Libraire-
Imprimeur de MONSIEUR, quai
des Augustins.

M. DCC, LXXIX.

FOREWORD TO THE PUBLIC

THE DISCOVERY, which has so long been sought, of a principle acting on the nerves should be of interest to all. It has the twofold aim of adding to their knowledge and of making them happier, by affording a means of curing the maladies which have hitherto been treated with but scant success. The advantages and the singular nature of this system were responsible, some years ago, for the eagerness of the public to grasp the first hopes which I held out ; and it is by perverting them that envy, presumption and incredulity have in a very short space of time succeeded in relegating them to the status of illusions, causing them to fall into oblivion.

I have vainly endeavoured to resuscitate them by the enormous number of facts ; nevertheless, prejudices won the day and truth has been sacrificed. But, it will be asked today, of what does this discovery consist ? how have you come by it ? what idea may be formed of its advantages ? and why have you not enriched your fellow-citizens therewith ? Such are the questions that have been put to me since my stay in Paris by persons who are highly qualified for taking up a new question.

It is with the object of giving a satisfactory reply and in order to provide a general idea of the system I propose, to free it from the errors with which it has been surrounded and to make known the vicissitudes which have formed an obstacle to its being made known, that I am publishing this Dissertation ; which is merely the forerunner of a theory I shall impart as soon as circumstances enable me to indicate the practical rules of the method I am announcing.

From this standpoint, therefore, I entreat the reader to consider this little work. I am well aware that it will raise

many difficulties, but it must be borne in mind that they are of such a nature as not to be solved by any amount of reasoning without the assistance of experience.

Experience alone will scatter the clouds and shed light on this important truth: that NATURE AFFORDS A UNIVERSAL MEANS OF HEALING AND PRESERVING MEN.

DISSERTATION ON THE DISCOVERY OF ANIMAL MAGNETISM

MAN is by nature an Observer. From his birth, his sole occupation is to observe in order to learn how to use his organs. The eye, for instance, would be useless to him if Nature did not cause him to pay attention to the slightest variations of which his observation is capable. It is by the alternating effect of enjoyment and deprivation that he learns of the existence of light and its different degrees, but he would remain in ignorance of the distance, size and shape of objects if he did not learn, by comparing and combining the impressions of other organs, how to correct one by the other. Most sensations are therefore the result of his reflections on the impressions assembled in his organs.

Thus Man spends his early years in acquiring the prompt and correct use of his senses. His gift of observation, which he has from Nature, enables him to form himself, and the perfection of his faculties depends on its more or less constant application.

Among the infinite number of objects which come successively before him, his attention is chiefly attracted by those which interest him for more particular reasons.

Observation of the effects which Nature is universally and constantly producing on each individual is not the exclusive domain of Philosophers; universal interest makes an observer of almost every individual. These observations, multiplied in every age and every place, leave nothing to be desired as regards their reality.

The activity of the human mind, together with its ambition for knowledge, which is never satisfied, in seeking to perfect knowledge previously acquired, abandons observation, replacing it by vague and often frivolous speculation. It forms and accumulates systems which have only the merit of their mysterious abstraction. It departs imperceptibly from truth, to such an extent as to lose sight thereof, setting up ignorance and superstition in its stead. Human knowledge, thus perverted, ceases to possess any of the reality which it had to begin with.

Philosophy has occasionally made efforts to free itself of errors and prejudices, but by overturning those edifices with too much vigour it has covered the ruins with disdain, without fixing the attention on the precious things contained there.

We see among the different peoples the same opinions preserved in a form so disadvantageous and dishonourable for the human mind that it seems improbable that they could have been set up in that form.

Imposture and aberration of reason would have attempted in vain to win over nations and cause them generally to adopt such obviously absurd and ridiculous systems as we see today; truth alone and the general interest should have conferred their universal nature on these opinions.

It may therefore, nevertheless, be asserted that among the vulgar opinions of all ages, whose principles are not rooted in the human heart, there are but few which, however ridiculous and even extravagant they may appear, cannot be regarded as the remains of an originally recognised truth.

Such are my reflections on knowledge in general, and more particularly on the fate of the doctrine of the influence of celestial bodies on the planet we inhabit. These reflections have induced me to seek, among the ruins of that science, brought so low by ignorance, what it might have contained that was useful and true.

In accordance with my ideas on this subject, I published

at Vienna in 1766 a thesis on the influence of planets on the human body. According to the familiar principles of universal attraction, ascertained by observations which teach us how the planets mutually affect one another in their orbits, how the sun and moon cause and control the ocean tides on our globe and in the atmosphere, I asserted that those spheres also exert a direct action on all the parts that go to make up animate bodies, in particular on the nervous system, by an all-penetrating fluid. I denoted this action by the INTENSIFICATION AND THE REMISSION of the properties of matter and organic bodies, such as gravity, cohesion, elasticity, irritability, electricity.

I maintained that just as the alternate effects, in respect of gravity, produce in the sea the appreciable phenomenon which we term ebb and flow, so the INTENSIFICATION AND REMISSION of the said properties, being subject to the action of the same principle, cause in animate bodies alternate effects similar to those sustained by the sea. By these considerations I established that the animal body, being subjected to the same action, likewise underwent a kind of ebb and flow. I supported this theory with different examples of periodic revolutions. I named the property of the animal body that renders it liable to the action of heavenly bodies and of the earth ANIMAL MAGNETISM. I explained by this magnetism the periodical changes which we observe in sex, and in a general way those which physicians of all ages and in all countries have observed during illnesses.

My object then was only to arouse the interest of physicians; but, far from succeeding, I soon became aware that I was being taxed with eccentricity, that I was being treated like a man with a system and that my tendency to quit the normal path of Medicine was being construed as a crime.

I have never concealed my manner of thinking in this respect, being unable to convince myself that we have made the progress of which we boast in the art of healing.

Indeed, I have held that the further we advanced in our knowledge of the mechanism and the economy of the animal body, the more we were compelled to admit our insufficiency. The knowledge that we have gained today about the nature and action of the nerves, imperfect though it be, leaves us in no doubt in this respect. We know that they are the principal agents of sensation and movement, but we are unable to restore them to their natural order when this has been interfered with. We confess this to our shame. The ignorance of bygone centuries on this point has sheltered physicians. The superstitious confidence which they had and which they inspired in their specifics and formulæ made them despotic and presumptuous.

I have too much respect for Nature to be able to convince myself that the individual preservation of Man has been left to the mere chance of discovery and to the vague observations that have been made in the course of a number of centuries, finally becoming the domain of the few.

Nature has provided everything for the existence of the individual. Propagation takes place without "system" and without trickery. Why should preservation be deprived of the same advantage? The preservation of animals affords proof that the contrary is the case.

A non-magnetised needle, when set in motion, will only take a determined direction by chance, whereas a magnetised needle, having been given the same impulse, after various oscillations proportional to the impulse and magnetism received, will regain its initial position and stay there. Thus the harmony of organic bodies, when once interfered with, goes through the uncertainties of my first hypothesis, unless it is brought back and determined by the GENERAL AGENT, whose existence I recognise: it alone can restore harmony in the natural state.

Thus we have seen, in all ages, maladies which become

worse or are cured with and without the help of Medicine, in accordance with different systems and by the most conflicting methods. These considerations have removed all doubt from my mind that there exists in Nature a universally acting principle which, independently of ourselves, operates what we vaguely attribute to Art *and* Nature.

These reflections have caused me to stray imperceptibly from the beaten track. I have subjected my ideas to experience for over twelve years, which I have devoted to the most accurate observations of all types of disease, and I have had the satisfaction of seeing the maxims I had forecast being borne out time and time again.

It was chiefly in the years 1773 and 1774 that I undertook in my house the treatment of a young lady aged twenty-nine named Oesterline, who for several years had been subject to a convulsive malady, the most troublesome symptoms of which were that the blood rushed to her head and there set up the most cruel toothache and earache, followed by delirium, rage, vomiting and swooning. For me it was a highly favourable occasion for observing accurately that type of ebb and flow to which ANIMAL MAGNETISM subjects the human body. The patient often had beneficial crises, followed by a remarkable degree of alleviation; however, the enjoyment was always momentary and imperfect.

The desire to ascertain the cause of this imperfection and my own uninterrupted observations brought me time and time again to the point of recognising Nature's handiwork and of penetrating it sufficiently to forecast and assert, without hesitation, the different stages of the illness. Encouraged by this first success, I no longer had any doubts as to the possibility of bringing it to perfection, if I were able to discover the existence, among the substances of which our globe is made, of an action that is also reciprocal and similar to that of the heavenly bodies, by means of which I could

imitate artificially the periodic revolutions of the ebb and flow just referred to.

I possessed the usual knowledge about the magnet: its action on iron, the ability of our body fluids to receive that mineral. The various tests carried out in France, Germany and Britain for stomach ache and toothache were known to me. These reasons, together with the analogy between the properties of this substance and the general system, induced me to regard it as being the most suitable for this type of test. To ensure the success of this test, in the interval of the attacks, I prepared the patient by the continuous use of chalybeates.¹

My social relations with Father Hell, Jesuit and Professor of Astronomy at Vienna, then provided me with an opportunity of asking him to have made for me by his craftsmen a number of magnetised pieces, of convenient shape for application. He was kind enough to do this for me and let me have them.

On 28th July 1774, after the patient had had a renewal of her usual attacks, I applied three magnetised pieces to the stomach and both legs. Not long afterwards, this was followed by extraordinary sensations; she felt inside her some painful currents of a subtle material which, after different attempts at taking a direction, made their way towards the lower part and caused all the symptoms of the attack to cease for six hours. Next day, as the patient's condition made it necessary for me to carry out the same test again, I obtained the same success with it.

My observation of these effects, coupled with my ideas on the general system, provided me with fresh information. While confirming my previous ideas about the influence of the GENERAL AGENT, it taught me that another principle was causing the magnet to act, the magnet itself being incapable of such action on the nerves, and I saw that I only had a short

¹ Presumably "iron" tonics. G.F.

way to go in order to arrive at the IMITATIVE THEORY, which formed the subject of my research.

A few days afterwards, having met Father Hell, I mentioned to him in the course of conversation that the patient was in a better state of health, also the good effects of my process and the hopes that I had, on the strength of this operation, of soon finding a means of curing nerve sufferers.

I found out not long afterwards, from the public and from the newspapers, that this man of religion, abusing his fame in astronomy and wishing to appropriate for himself a discovery of whose nature and benefits he was entirely ignorant, had taken upon himself to publish the fact that by means of some magnetised pieces, to which he attributed a specific virtue depending on their shape, he had obtained the means of curing the gravest nerve disorders. To lend support to this opinion, he had sent to a number of Academies some sets consisting of magnetised pieces of all shapes, mentioning according to their outline their analogy with the various maladies.

This is how he expressed himself: "I have discovered in these shapes, which agree with the magnetic vortex, a perfection on which depends their specific virtue in cases of illness; it is owing to the lack of this perfection that the tests carried out in England and France have met with no success". And by affecting to confuse the manufacture of the magnetised shapes with the discovery I had mentioned to him, he finished by saying "that he had communicated everything to the physicians, and particularly to myself, and would continue to avail himself of them for his tests".

The repeated writings of Father Hell on this subject inspired the public, which is always eager for a specific against nervous disorders, with the illfounded opinion that the discovery in question consisted in the mere use of the magnet. I in my turn wrote to refute this error, by publishing the existence of ANIMAL MAGNETISM, essentially distinct

from the Magnet; however, as the public had received its information from a man of high repute, it remained in its error.

I continued my experiments with different disorders so as to generalise my knowledge and perfect the application thereof.

I knew particularly well Baron de Stoerck, President of the Faculty of Medicine at Vienna and Chief Physician to Her Majesty. It was moreover seemly for him to be acquainted with the nature of my discovery and its purpose. I consequently placed before him the circumstantial details of my operations, particularly as regards the communication and currents of animal magnetic matter, and I invited him to verify them for himself, stating that it was my intention to report to him in future all progress that I might make in this new science. To give him certain proof of my good faith, I made known my methods to him without reserve.

The natural timidity of this physician, no doubt based on motives which it is not my intention to penetrate, induced him to reply that he wished to have nothing to do with what I was telling him about, and he begged me not to compromise the Faculty by giving publicity to an innovation of this kind.

Public prejudice and uncertainty as to the nature of my methods decided me to publish, on 5th January 1775, a Letter to a Foreign Physician, in which I gave an exact idea of my theory, the success I had hitherto obtained and the success I had reason to hope for. I set forth the nature and action of ANIMAL MAGNETISM and the analogy between its properties and those of the magnet and electricity. I added "that all bodies were, like the magnet, capable of communicating this magnetic principle; that this fluid penetrated everything and could be stored up and concentrated, like the electric fluid; that it acted at a distance; that animate

bodies were divided into two classes, one being susceptible to this magnetism and the other to an opposite quality that suppresses its action". Finally, I accounted for the various sensations and based these assertions on experiments which enabled me to put them forward.

A few days prior to the publication of this Letter, I heard that Mr Ingenhousze, member of the Royal Academy of London and Inoculator at Vienna, by entertaining the nobility and distinguished personages with experiments in electricity, and by the skill with which he varied the effects of the magnet, had acquired the reputation of being a physician. I heard, as I said, that when this gentleman learned of my operations, he treated them as vain imaginings, going so far as to say that only the English genius was capable of such a discovery, if it could be done. He came to see me, not to gain information, but with the sole intention of persuading me that I was laying myself open to error and should suppress all publicity with a view to avoiding inevitable ridicule.

I replied that he was not sufficiently talented himself to give me this advice, and that I should moreover have pleasure in convincing him at the first opportunity. This presented itself two days afterwards.

Miss Oesterline took fright and contracted a chill, causing a sudden stoppage, and she relapsed into her former convulsions. I invited Mr Ingenhousze to call. He came, accompanied by a young physician. The patient was then in a fainting fit with convulsions. I told him that it was the most favourable opportunity for convincing himself of the existence of the principle I announced, and the property which it has to communicate. I told him to approach the patient, while I withdrew from her, instructing him to touch her. She made no movement. I recalled him to me, and communicated animal magnetism to him by taking him by the hands; I then bade him approach the patient once more, while I kept at a distance, telling him to touch her a

second time. This resulted in convulsive movements. I made him repeat this touching process several times, which he did with the top of his finger, changing the direction each time. Always, to his great astonishment, he brought about a convulsive effect in the part touched.

When this operation was over, he told me he was convinced, and I suggested a second visit. We withdrew from the patient, so as not to be perceived even had she been conscious. I offered Mr Ingenhousze six china cups and asked him to tell me to which one he wished me to communicate the magnetic quality. I touched the one of his choice and then applied the six cups to the patient's hand in succession; on reaching the cup that I had touched, the hand made a movement and gave signs of pain. Mr Ingenhousze obtained the same result when he applied the six cups.

I then had these cups taken back to the place whence they had come, and after a certain interval, holding him by one hand, I asked him to touch with the other any cup he wished; he did so, the cups were brought to the patient, as before, with the same result.

The communicability of the principle was now well-established in Mr Ingenhousze's eyes, and I suggested a third experiment to show its action at a distance and its penetrating quality. I pointed my finger at the patient at a distance of eight paces; the next instant, her body was in convulsion to the point of raising her on her bed with every appearance of pain. I continued, in the same position, to point my finger at the patient, placing Mr Ingenhousze between herself and me. She underwent the same sensations.

Having repeated these tests to Mr Ingenhousze's satisfaction, I asked him if he was convinced of the marvellous properties about which I had told him, offering to repeat our proceedings if he were not. His reply was to the effect that he wished for nothing further and was convinced; but owing to his friendship with me, he entreated me not to

make any public statement on this subject, so as not to lay myself open to public incredulity. We parted, and I went back to the patient to continue the treatment, which was most successful. That same day I managed to restore the normal course of nature, thereby putting an end to all the trouble brought about by stoppage.

Two days later I was astonished to hear that Mr Ingenhousze was making statements in public that were quite the reverse of his utterances in my house, and was denying the success of the different experiments he had witnessed. He effected to confuse ANIMAL MAGNETISM with the magnet and was endeavouring to damage my reputation by spreading the report that with the aid of a number of magnetised pieces which he had brought with him, he had succeeded in unmasking me, proving that it was nothing but a ridiculous, prearranged fraud.

I must confess that such words at first seemed to me to be unbelievable, and I had some difficulty in bringing myself to regard Mr Ingenhousze as their author. However, his association with the Jesuit Hell, and the latter's irresponsible writings in support of such odious insinuations, aimed at ruining the effect of my Letter of 5th January 1775, removed all doubt from my mind that Mr Ingenhousze was the guilty party. I refuted Father Hell and was about to draw up an indictment when Miss Oesterline, who had been informed of Mr Ingenhousze's procedure, was so affronted at finding herself thus compromised that she relapsed into her former state, which was aggravated by a nervous fever.

Miss Oesterline's condition claimed the whole of my attention for a fortnight. In these circumstances, by continuing my research, I was fortunate in overcoming the difficulties which stood in the way of my progress and of giving my theory the desired perfection. The cure of this young lady represented the first fruits of my success, and I

had the satisfaction of seeing her henceforth in excellent health. She married and had some children.

It was during this fortnight that, being determined to justify my conduct and to give the public a correct idea of my abilities by unmasking Mr Ingenhousze's behaviour, I informed Mr de Stoerck, requesting him to obtain orders from the Court for a Commission of the Faculty to be acquainted with the facts, so that it might verify and make them known to the public. This step appeared to be agreeable to the senior physician; he seemed to share my viewpoint and promised to act accordingly, remarking, however, that he could not be on the Commission.

I suggested several times that he should come and see Miss Oesterline and satisfy himself as to the success of my treatment. His replies in this matter were always vague and uncertain. I explained to him how it would benefit humanity to have my method adopted by the hospitals, and asked him to demonstrate its utility forthwith at the Spanish Hospital. He agreed to this and gave the necessary instructions to Mr Reinlein, physician at that hospital.

The latter was a witness of the effects and usefulness of my visits over a period of eight days. On several occasions he expressed surprise and reported to Mr de Stoerck. However, I soon became aware that different impressions had been given to this leading physician. I met him almost every day and insisted on my request for a Commission, reminding him of the interesting matters about which I had told him, but saw nothing but indifference, coldness and reserve in his attitude whenever the topic was broached.

Being unable to obtain any satisfaction, and as Mr Reinlein had ceased reporting to me (I moreover found out that this change of front was the result of steps taken by Mr Ingenhousze), I realised my inability to stem the course of the intrigue, and sought consolation in silence.

Emboldened by the success of his plans, Mr Ingenhousze acquired fresh vigour; he vaunted his incredulity and it was not long before he succeeded in having those who suspended judgment or who did not share his opinion classed as feeble-minded. It will readily be understood that all this was quite enough to alienate the masses and have me looked upon at least as a visionary, especially as the indifference of the Faculty appeared to support that opinion.

What I felt to be most strange was that the same opinion should be shared the following year by Mr Klinkosch, Professor of Medicine at Prague, who, without knowing me and without the slightest idea of the true state of the matter, was sufficiently foolish (to use no stronger term), as to publish in the public journals the curious details of the impostures attributed to me by Mr Ingenhousze.¹

Whatever public opinion might be, I felt that truth could not find better support than in facts. I undertook the treatment of various disorders, including a hemiplegia, the result of apoplexy; stoppages, spitting of blood, frequent colics and convulsive sleep from childhood, with spitting of blood and normal ophthalmia. Mr Bauer, Professor of Mathematics at Vienna and a man of outstanding merit, was attacked by this latter malady. My work was crowned with the best possible success, and Mr Bauer himself was honest enough to make public a detailed report on his cure. However, prejudice had the upper hand. Nevertheless, I had the satisfaction of being quite well known to a great Minister, a Privy Councillor and an Aulic Councillor, friends of humanity, who had often recognised truth for themselves, seeing that they upheld and protected it. They even made several attempts to lighten the shadows in which it was

¹ Letter on Animal Magnetism and the Electrophorus, addressed to Count de Kinsky. It was included in the Proceedings of the Scientists of Bohemia, Vol. II, 1776. It was also printed separately and published at Vienna in the following year.—F.A.M.

being wrapped. They met with little success, however, it being objected that only the opinion of physicians was capable of deciding, and their good will was thus confined to their offers to give my writings the necessary publicity in foreign lands.

It was through this channel that my explanatory Letter of 5th January 1775 was transmitted to the majority of the scientific institutions, and to a few scientists. Only the Berlin Academy, on the 24th March that year, made a written reply in which by confusing the properties of ANIMAL MAGNETISM which I had expounded with those of the magnet, which I only spoke of as a conductor, it incurred a number of errors and its opinion was that I was the victim of illusion.

This Academy was not the only one to make the mistake of confusing ANIMAL MAGNETISM with mineral magnetism, although I have always stressed in my writings that the use of the magnet, however convenient, was always imperfect without the assistance of the theory of ANIMAL MAGNETISM. The physicians and doctors with whom I have been in correspondence, or who have endeavoured to find out my methods in order to usurp this discovery, have taken upon themselves to spread about either that the magnet was the only means I employed, or else that I used electricity as well, because it was well known that I had availed myself of both. Most of them have been undeceived by their own experience, but instead of realising the truth I was expounding, they have concluded from the fact that they obtained no success from the use of these two agents that the cures announced by myself were imaginary and that my theory was nothing but an illusion.

The desire to refute such errors once and for all, and to do justice to truth, determined me to make no further use of electricity or of the magnet from 1776 onwards.

The poor reception given to my discovery and the slight hopes it held out to me for the future made me resolve to undertake nothing of a public nature at Vienna, but instead to travel to Swabia and Switzerland and add to my experience, thus arriving at the truth through facts. Indeed, I had the satisfaction of making some striking cures in Swabia, and of operating in the hospitals, before the eyes of doctors from Berne and Zürich. They were left in no doubt as to the existence of ANIMAL MAGNETISM and the usefulness of my theory, which corrected the error into which they had been led by my opponents.

Between the years 1774 and 1775 an ecclesiastic, a man of good faith but of excessive zeal, was operating in the diocese of Ratisbon on various disorders of a nervous nature, using means that appeared to be supernatural to the less well informed in that district. His reputation extended to Vienna, where society was divided into two halves: one regarded his methods as imposture and fraud, while the other looked upon them as miracles performed by Divine power. Both, however, were wrong, and my experience at once told me that the man in question was nothing but a tool of Nature. This was because his profession, assisted by fate, had furnished him with certain natural talents enabling him to find out the periodical symptoms of maladies without knowing their cause. The end of such paroxysms was held to be a complete cure, and time alone could undeceive the public.

On returning to Vienna towards the end of 1775, I passed through Munich, where His Highness the Elector of Bavaria was kind enough to consult me on this subject, asking me whether I could account for these pretended miracles. I carried out before his eyes some experiments that removed any prejudices he may have had and left him in no doubt as to the truth I announced. Shortly afterwards,

the Scientific Institution of that city paid me the honour of admitting me as a member.

In 1776 I again visited Bavaria and secured similar success there in illnesses of different kinds. In particular, I effected the cure of an imperfect amaurosis, accompanied by paralysis of the limbs, which was afflicting Mr d'Osterwald (factually Oesterwald, G.F.) director of the Scientific Institution of Munich; he was kind enough to make public mention of this and of the other results he had witnessed.¹

After returning to Vienna, I persisted until the end of that year in undertaking no further work; neither would I have altered my mind if my friends had not been unanimous in opposing my decision. Their insinuations, together with my desire to see the truth prevail, aroused in me the hope of accomplishing this by means of fresh successes, particularly through some striking cure. With this end in view, among other patients I undertook the treatment of Miss Paradis, aged eighteen, whose parents were well known; she herself was known to Her Majesty the Queen-Empress, through whose bounty she received a pension, being quite blind since the age of four. It was a perfect amaurosis, with convulsions in the eyes. She was moreover a prey to melancholia, accompanied by stoppages in the spleen and liver, which often brought on accesses of delirium and rage so that she was convinced she was out of her mind.

I also undertook the treatment of one Zwelferine, a girl nineteen years of age who had been blind since the age of two owing to amaurosis accompanied by a very thick, wrinkled albugo with atrophy of the ball; she was also afflicted with periodic spitting of blood. I found this girl

¹ There was published at the beginning of 1778 a Collection of Cures effected by Magnetism, printed at Leipzig. This bulky Collection, whose author is unknown to me, has the sole merit of assembling faithfully, without partiality, the reports and writings for and against my system.—F.A.M.

in the Vienna orphanage and her blindness was attested by the Governors.

At the same time I also treated Miss Ossine, aged eighteen, who was in receipt of a pension from Her Majesty, as being the daughter of an officer in her armies. Her malady consisted of purulent phthisis and irritable melancholia, accompanied by fits, rage, vomiting, spitting of blood and fainting. These three patients and others besides were accommodated in my house so that I might continue my treatment without interruption. I was fortunate in being able to cure all three.

The father and mother of Miss Paradis, who witnessed her cure and the progress she was making in the use of her eyesight, hastened to make this occurrence known and how pleased they were. Crowds flocked to my house to make sure for themselves, and each one, after putting the patient to some kind of test, withdrew greatly astonished, with the most flattering remarks to myself.

The two Presidents of the Faculty, at the head of a deputation of their corps, came to see me at the repeated instances of Mr Paradis; and, after examining the young lady, added their tribute to that of the public. Mr de Stoerck, one of these gentlemen who knew this young person particularly well, having treated her for ten years without the slightest success, expressed to me his satisfaction at so interesting a cure and his regret at having so far deferred his acknowledgment of the importance of my discovery. A number of physicians, each for himself, followed the example set by our leaders and paid the same tribute to truth.

After such authentic recognition, Mr Paradis was kind enough to express his gratitude in his writings, which went all over Europe. It was he who afterwards published the interesting details of his daughter's recovery in the newspapers.¹

¹ See Appendix.

Among the physicians who came to see me to satisfy their curiosity was Mr Barth, professor of anatomy of diseases of the eye, and cataract specialist; he had even admitted on two occasions that Miss Paradis was able to use her eyes. Nevertheless, this man's envy caused him to state publicly that the young lady could not see, and that he had satisfied himself that she could not. He founded this assertion on the fact that she did not know or confused the names of objects shown to her. He was answered from all quarters that he was therein confusing the necessary inability of those blind from birth or at a very tender age with the knowledge acquired by blind persons operated on for cataract. How, he was asked, is it that a man of your profession can be guilty of so obvious an error? His impudence, however, found an answer to everything by asserting the contrary. It was in vain that the public told him again and again that a thousand witnesses had given evidence of the cure; he alone held the opposite view, in which he was joined by Mr Ingenhousze, the Inoculator of whom I have spoken.

These two individuals, who were at first regarded as fanatics by sensible, honest folk, succeeded in weaving a plot to withdraw Miss Paradis from my care, her eyes still being in an imperfect state, and made it impossible for her to be presented to Her Majesty, as was to have been the case. This inevitably lent credence to their assertion of imposture. To this effect they worked on Mr Paradis, who began to be afraid that his daughter's pension and several other advantages held out to him might be stopped. He consequently asked for his daughter back.

The latter, supported by her mother, showed her unwillingness and fear lest the cure might be imperfect. The father insisted, and this dispute brought on her fits again and led to an unfortunate relapse. However, this had no effect on her eyes, and she continued to improve the use of them.

When her father saw she was better, being still egged on by the conspirators, he returned to the charge. He demanded his daughter with some heat and compelled his wife to do likewise.

The girl resisted for the same reasons as before. Her mother, who had hitherto supported her, and had apologised for the lengths to which her husband had gone, came to tell me on 29th April 1777 that she intended to remove her daughter instantly. I replied that she was free to do so, but if fresh accidents were the result, she could not count on my help.

These words were overheard by the girl, who was so overcome that she went into a fit. She was assisted by Count de Pellegrini, one of my patients. Her mother, who heard her cries, left me abruptly and seized her daughter angrily from the hands of the person who was assisting her, saying: "Wretched girl, you too are hand in glove with the people of this house!" as she flung her in a fury head-first against the wall.

Immediately all the troubles of that unfortunate girl recommenced. I hastened towards her to give her assistance, but her mother, still livid with rage, hurled herself upon me to prevent me from doing so, while she heaped insults on me. I had the mother removed by certain members of my household and went up to the girl to assist her. While I was so engaged, I heard more angry shouts and repeated attempts to open and shut the door of the room where I was.

It was Mr Paradis who, having been warned by one of his wife's servants, now invaded my house sword in hand with the intention of entering the room where I was, while my servant was trying to remove him by guarding the door. The madman was at last disarmed, and he left my house breathing imprecations on myself and my household.

Meanwhile, his wife had swooned away. I had her given

the necessary attention, and she left some hours afterwards, but the unhappy girl was suffering from attacks of vomiting, fits and rages, which the slightest noise, especially the sound of bells, accentuated. She had even relapsed into her previous blind state through the violence of the blow given her by her mother, and I had some fears for the state of her brain.

Such were, for my patient and for me, the sinister effects of that painful scene. It would have been easy for me to take the matter to court, on the evidence of Count de Pellegrini and eight persons who were with me, to say nothing of other neighbours who could have acted as witnesses too, but, as I was solely concerned with saving Miss Paradis, if possible, I refrained from availing myself of legal redress. My friends remonstrated in vain, pointing out the ingratitude exhibited by her family and the wasted expenditure of my labours. I adhered to my first decision and would have been content to overcome the enemies of truth and of my peace of mind by good deeds.

Next day I heard that Mr Paradis, in an endeavour to cover up his excesses, was spreading about the most wicked insinuations regarding myself, always with a view to removing his daughter and proving, by her condition, the dangerous nature of my methods. I did indeed receive through Mr Ost, Court physician, a written order from Mr de Stoerck, in his capacity as head physician, dated Schoenbrunn, 2nd May 1777, who called upon me "to put an end to the imposture" (his own expression) "and restore Miss Paradis to her family, if I thought this could be done without risk".

Who would have believed that Mr de Stoerck, being so well informed by the same physician of all that had taken place in my house and, after his first visit, having come twice to convince himself of the patient's progress and the

success of my methods, could have taken upon himself to use such offensive and contemptuous language to me? I had indeed reason to expect the contrary, because being well placed for recognising a truth of this kind, he should have been its defender. I would even go so far as to say that as the repository of Her Majesty's confidence, one of his first duties under these circumstances should have been to protect a member of the Faculty whom he knew to be blameless, and to whom he had time and time again given assurances of his affection and esteem. I made answer to this irresponsible order that the patient was not in a position to be moved without running the risk of death.

Miss Paradis's critical condition no doubt made an impression on her father, and caused him to reflect. Through the intermediary of two reputable persons, he begged me to continue attending his daughter. I told him that I would do so on condition that neither he nor his wife ever again appeared in my house. My treatment indeed exceeded my hopes, and nine days sufficed to calm down the fits entirely and put an end to the disorders. But her blindness remained.

Fifteen days' treatment cured the blindness and restored the eye to its condition prior to the incident. To this period I added a further fortnight's attention to improve and restore her health. The public then came to obtain proof of her recovery, and everybody gave me, even in writing, fresh evidence of satisfaction. Mr Paradis, being assured of the good health enjoyed by his daughter through Mr Ost, who, at his request and by my permission, followed the progress of the treatment, wrote a letter to my wife in which he thanked her for her motherly care.

He also wrote thanking me and apologising for the past; he finished by asking me to send back his daughter so that she might enjoy the benefit of country air. He said that he would send her back to me whenever I might think

necessary, so as to continue the treatment, and he hoped that I would attend her. I believed him in all good faith, and returned his daughter to him on the 8th of June.

Next day I heard that her family asserted that she was still blind and subject to fits. They showed her thus and compelled her to imitate fits and blindness. This news evoked some contradictions by persons who were convinced of the contrary, but it was upheld and accredited by the obscure intriguers who used Mr Paradis as their tool, and I was unable to check its spread by the highest testimony, such as that of Mr de Spielmann, Aulic Councillor of Their Majesties and Director of the State Chancellery; Their Majesties' Councillors, Messrs de Molitor and de Umlauer, physician to Their Majesties; de Boulanger, de Heufeld and Baron de Colnbach and Mr de Weber, who, independently of several other persons, had almost every day followed for themselves my processes and results.

Thus in spite of my perseverance and my work, I have little by little seen relegated to the position of a conjecture, or at least of something uncertain, a truth that has been authentically proven.

It is easy to imagine how I might have been affected by the relentlessness of my enemies to do me harm and by the ingratitude of a family on which I had showered kindnesses. Nevertheless, during the last half of 1777 I continued with the cure of Miss Ossine and the aforementioned Zwelferine whose eyes, it will be remembered, were in an even more serious condition than Miss Paradis's. I also persevered successfully with the treatment of my remaining patients, in particular Miss Wipior, aged nine, who had in one eye a growth on the cornea, known by the name of staphyloma; this cartilaginous excrescence, of 3 to 4 lines,¹ prevented her from seeing with that eye.

¹ There is nothing which can clear up definitely to what Mesmer refers when mentioning "3 à 4 lignes". This has been translated by "3 to 4 lines"

I succeeded in removing the excrescence to the extent that she was able to read sideways. There only remained a slight albugo in the centre of the cornea, and I have no doubt that I would have caused it to disappear entirely, had circumstances permitted me to continue the treatment. However, being wearied by my labours extending over twelve consecutive years and still more so by the continued animosity of my adversaries, without having reaped from my research and efforts any satisfaction other than that of which adversity could not deprive me, I felt that I had done my duty by my fellow-citizens.

With the conviction that justice would one day be done me, I decided to travel for the sole purpose of securing the relaxation I so much needed. However, to guard against prejudice and insinuations as far as possible, I arranged matters so as to leave at home in my absence Miss Ossine and the girl Zwelferine. I next took the precaution of telling the public of the reason for this arrangement, stating that these persons were in my home so that their condition could be ascertained at any moment and thereby lend support to truth. They remained there eight months after my departure from Vienna and only left on orders from a higher authority.

On arriving at Paris in February 1778,¹ I began to enjoy the delights of repose there, in the interesting company of

but could equally well mean "3 to 4 lignes" taken in the sense of an old French measure, in which case the correct translation would be "7 to 9 millimetres" in present day equivalent measure. V. R. MYERS.

¹ My adversaries, who were ever on the watch to harm me, lost no time in spreading warnings about me on my arrival in France. They went to the length of compromising the Faculty of London by causing an Anonymous Letter to be inserted in the *Journal Encyclopédique* for March 1778, page 506; Mr Hell, bailiff of Hirsingen and Lundzer, did not scruple to lend his name to this libellous document. Nevertheless, I was not known to him, and I only saw him afterwards, at Paris, to receive his apologies. The untrustworthiness, inconsistency and maliciousness of this Letter are merely deserving of contempt, as will be found on perusing it.—F.A.M.

the scientists and physicians of that capital. However, acceding to their requests and to repay the kindness shown to me, I decided to satisfy their curiosity by speaking of my system. They were astonished at its nature and results and asked me for an explanation. I gave them my concise assertions in nineteen articles.¹ They seemed to them to bear no relation to established knowledge. I felt indeed how difficult it was, by reason alone, to prove the existence of a principle of which people had not the slightest conception. With this in mind, I therefore yielded to the request made to me to show the reality and the usefulness of my theory by the treatment of a few serious maladies.

A number of patients placed their trust in me. Most were in so desperate a plight that it required all my desire to be of use to make me decide on attending them. Nevertheless, I cured a vaporous melancholia with spasmodic vomiting, a number of longstanding stoppages in the spleen, liver and mesentery, an imperfect amaurosis, to the extent of preventing the patient from moving about alone, and a general paralysis with trembling which gave the patient (aged forty) every appearance of old age and drunkenness. This malady was the result of frost-bite; it had been aggravated by the effects of a putrid and malignant fever which the patient had contracted six years before in America.

I also obtained the same success in a case of total paralysis of the legs, with atrophy; one of chronic vomiting, which reduced the patient to a state of progressive emaciation; one of general scrofulous debility, and finally in a case of general decay of the organs of perspiration.

These patients, whose condition was known and verified by the physicians of the Paris Faculty, were all subject to considerable crises and evacuation, on a par with the nature

¹ These same Assertions had been forwarded in 1776 to the Royal Society of London by Mr Elliot, English Envoy to the Diet of Ratisbon; I communicated them to that Minister at his own request, after carrying out before him various experiments at Munich and Ratisbon.—F.A.M.

of their maladies, without making use of any medicaments. After completing their treatment, they gave me detailed declarations.

That should have been more than enough to prove beyond all doubt the advantages of my method. I had reason to flatter myself that recognition would follow. However, the persons who had induced me to undertake the foregoing treatments were not enabled to see their effects, owing to considerations and reasons which it would be out of place to enumerate in this dissertation.

The result is that the cures which, contrary to my expectation, were not communicated to bodies whose duty it might have been to call the attention of the public to them, only imperfectly fulfilled the task I had set myself, and for which I had been praised.

This induces me to make a fresh effort today in the cause of truth, by giving more scope and the publicity which they have hitherto lacked to my original Assertions.