The initiation of modern neurological surgery was made possible by three discoveries in the nineteenth century. The introduction of two of these, anesthesia and antisepsis, vastly increased the scope of surgery in general, and made brain surgery feasible technically. Such operations were performed rarely, however, because there was no way to locate lesions that did not involve the skull. The problem was solved when it was discovered that in the brain there is focal representation of bodily function. This third fundamental concept—cerebral localization—became an important part of the foundation on which modern neurosurgery was built.14

Since the dawn of recorded surgery it has been recognized that brain injuries are sometimes accompanied by localized losses of bodily function. In the fifth century B.C., Hippocrates noticed the association between brain damage and aphasia, ophthalmoplegia, and anisocoria.2 He also recognized that unilateral brain injury occasionally is followed by convulsions or paralysis of the opposite side of the body.2 However, for twenty-three centuries there were no scientific theories advanced to explain these associations. Instead, interest in cerebral physiology centered around philosophical discussions of the seat of the soul.9,12,17

In the early nineteenth century Franz Joseph Gall and Johann Caspar Spurzheim advanced a pseudoscientific theory of brain localization known as phrenology.7,9,15,17 These men believed that various subdivisions of the brain governed various intellectual activities, and that the relative development of the subdivisions could be assessed by examination of the overlying cranium. Although certain aspects of their theory were correct, most of their ideas were so absurd that the entire theory was rejected by most scientists. Especially opposed to the concepts of cerebral localization and phrenology was Pierre Flourens, whose studies on animals indicated to him that cerebral gray matter was homogeneous and equipotential. Flourens was an established physiologist, and his authoritative views were accepted for half a century.13,16,17

However, clinical evidence to the contrary was accumulated. For example, Jean Bouilllaud and Paul Broca found that injuries to the third left frontal convolution resulted in motor aphasia, and Hughlings Jackson defined the relationship between convulsions on one side of the body and disease of the opposite hemisphere.7,9,11,13,17 Related experimental studies did not support these clinical observations immediately, probably because of the crude laboratory apparatus then in use. In the early nineteenth century Luigi Rolando had observed muscular contractions during the electrical stimulation of the cerebral hemispheres of a pig.17 But it was not until 1870 that the doctrine of cerebral localization was placed on a firm basis by experimentation.

In that year, two young Berlin physicians, Gustav Fritsch and Eduard Hitzig, published the results of cerebral stimulation and ablation in dogs.2 Because there had been no suitable laboratories available to them, they did their first experiments in Hitzig’s home, operating on Frau Hitzig’s dressing table.8 Despite these modest facilities, Fritsch and Hitzig produced a classical work which opposed the prevailing concepts of cerebral function. Their experiments were confirmed by David Ferrier, and initiated a series of similar studies which made possible the first modern operations for localized cerebral lesions.17

Fritsch produced no scientific contributions of note after 1870, but Hitzig continued to contribute to the understanding of cerebral localization for 35 years.4-6,8
Physiology ascribes to all nerves, as a necessary condition of the concept, the characteristic of excitability, i.e., the capability of responding with their specific energy to all the influences by which their state is changed at a certain rate. But for the central part of the nervous system other ideas exist, though to be sure certain aspects of these are not accepted generally. It would be too long a process and it would also not serve the special purpose of the present work if we wanted to mention from the enormous relevant literature only the results that seem to us reliable and that were obtained by stimulative experiments on all the separate parts of the central nervous system. However, while with regard to the excitability of the organs composing the brain stem there exists a very great difference of opinion regarding stimulations other than organic ones, while very recently a violent dispute broke out about the excitability of the spinal cord, the conviction generally has been held since the beginning of the century that the hemispheres of the cerebrum are absolutely not excitable by any stimuli familiar to physiologists.

Haller and Zinn⁴ alleged that they saw convulsive movements when the medullary substance of the cerebrum was injured. However, at that time people were too little accustomed to a strict limitation of the stimuli used, which meet almost insurmountable obstacles in the brain, to pay much attention to these data later. As Longet remarked, it is probable that those experimenters had penetrated up to the medulla oblongata with their instruments.

But Longet² himself says the following on the subject:

"On dogs, rabbits, and on some kids, we have stimulated the white substance of the cerebral lobes with the scalp; we have cauterized it with potassium, nitric acid, etc., we have passed galvanic currents in every direction through it, without succeeding in initiating involuntary muscular contractions or developing convulsive twitchings; the same negative result was obtained by directing the same agents toward the gray or cortical substance."

Magendie's vivisections⁵ led to the same results. Later we shall deal with the rather similar conclusions of Flourens which are based upon results of bisections and denudations.

Matteucci⁶ also found the cerebrum and cerebellum of the rabbit to be entirely nonexcitable by electrical stimuli.

Van Deen⁷, with whose name the theory of non-

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References


The Electrical Excitability of the Cerebrum*

G. FRITSCH and E. HITZIG

excitability of the cerebrospinal centers was connected recently, went even considerably farther in his conclusions than all the experimenters before him and most of those after him. While previously, besides the spinal cord, at least some basal portions of the brain were still regarded as being excitable, he stated positively, on the basis of his experiments on the rabbit, which moreover were very poorly described, that the entire central nervous system was not excitable.

Likewise, Eduard Weber in experiments with the rotation apparatus in the cerebrum of frogs, saw no muscular spasms occur.

Budge, who also sacrificed an extremely large number of animals, expresses himself, apart from many other similar points, as follows:

"If, according to the present standpoint of science, we are permitted to conclude that no motor fibers exist in nervous tissue in which no spasms occur after a stimulation, then we can assert with the greatest certainty that not a single fiber of such nerves, which proceed into voluntary muscles, runs in the hemispheres of the cerebrum. Not a single observer has ever seen a movement of such muscles after stimulation of the above-mentioned central parts."

Finally, we also mention the opinion of Schiff, one of the most experienced vivisectors:

"In accordance with the declaration of many investigators, I can confirm that the stimulation of the cerebral lobes, of the corpora striata and of the cerebellum does not produce any trace of spasm in any of the free muscles of the body. Even the intestines remained motionless when these parts were stimulated while I had maintained the circulation, which is absolutely necessary in such experiments."

It can be seen that in any science other than physiology there was hardly a question about which the opinions agreed so well, and which appeared as completely settled, as the question of excitability of the cerebral hemispheres. Moreover, it would be easy to collect additional similar citations if this would be of some benefit.

So far as we know, only one author besides Haller and Zinn has seen something different, and his information aroused in Eckhard, who mentions the fact, so little belief that Eckhard conceals the name and the source. The statement in question reads: "It has been reported that, in disk-like ablation of the frontal lobes of the brain, lively movements were seen in the anterior legs."

To be sure, this in itself is not much, for one cannot see from it how the experiment was performed. However, if it was performed with the necessary precautions, it would prove an important principle, namely, the principle that by some kind of stimulus, either that of the scalpel, or of the oxygen or blood, movements of voluntary muscles can be produced from the frontal lobes. In any case, it seems that this isolated observation has not been followed up by anybody, for this report by Eckhard was the only trace left.

Before we turn to our own experiments, it is necessary to present the opinion concerning the motor processes in the central organs which was formed in consequence of the above-mentioned experiments and of the famous cerebral ablations by Flourens.

This ingenious and successful investigator was able, by using at least purer methods, to arrive at results that deserve to be regarded as a basis for almost all the knowledge obtained later on this subject.

After numerous ablations of the cerebrum, most of which were performed on birds but also on mammals, Flourens saw all signs of volition and consciousness of sensation disappear; however, at the same time, in all the muscles of the body, movements could be incited by outside stimuli. Such animals can stand very well on their feet, they run when they are hit; birds fly when they are tossed into the air, they defend themselves when they are tested, they swallow objects which are placed in their mouths, and even the iris contracts because of light stimulus. But never do such movements occur without the influence of an external stimulus. Animals deprived of their cerebrum always sit in a sunken position, as if sleeping, and this state of theirs cannot be changed even when—although starving—they are placed on a mountain of foodstuffs.

Flourens concluded therefrom that the cerebral hemispheres are not the site of the immediate principle of muscular movements, but are only the site of the volition and of the sensations.

Although this series of experiments and the conclusions drawn from it seem to be quite satisfactory, the additional results and conclusions of Flourens cannot be reconciled with results obtained by other means.

When Flourens ablated only one hemisphere from animals, they became blind on the opposite side, but they retained their full control of volition over all the voluntary muscles, and after overcoming a weakness of the opposite half of the body, which did not always occur, they did not

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7 Untersuchungen über das Nervensystem. Frankfurt a. M. 1848, no. II, p. 84.
11 In the place cited, p. 35.
differ in anything from nonmutilated animals. When he progressively ablated disks of the cerebrum of other animals, either from the front backward or from the back forward, or from above downward or from the outside inward, he noticed under all these conditions a regular gradual decrease of the sensory perceptions and of volition. But if he went beyond a certain limit, then all these characteristics attributed to the mind were suddenly extinguished, and the animal sank into the described dreaming state.

Furthermore, if he kept the ablation at that limit, the animal regained within a few days the previously lost faculties and was able to continue to exist for a long time with the same psychic characteristics as if nothing was taken away from its cerebral substance. Flourens concluded from that\(^{12}\) that the cerebral lobes exercise their functions with their entire mass, and that there is no separate site either for the various faculties or for the various perceptions. He also concluded—in contradiction to the first conclusion—that a remaining part of the hemispheres can recover the full use of all functions.

In any case, the most striking of all the reported experiments is the one described in the book mentioned, part II, page 101. In this experiment Flourens had ablated from a pigeon the entire attainable cortex of the cerebrum on both sides, therefore the ganglionic part, i.e., the part that still was regarded as the essential portion that shelters the first tools of the psyche. Nevertheless this pigeon began as early as the 3rd day to again exercise its psychic functions, and on the 6th day it had regained everything that seemed to have been entirely taken away from it through the operation. However, these experiments or their applicability were hardly or not yet tried on higher animals, and Schiff\(^{13}\) states the same thing about that, although this investigator calls attention to the evident differences in structure and function between animal and human brain.

Therefore, according to these and later expanded investigations, the following approximate opinion was formed about the central locations of muscular movement:

In most parts of the brain stem, also downward to the spinal cord, there are a number of preformed mechanisms, which are capable of a normal total excitation on two paths. One of them runs from the periphery—the path of the reflex; the other radiates from the center—the path of volition, of the psychic impulses. This center lies presumably in the ganglionic substance of the cerebral hemispheres, but the individual parts of the psychic center are not localized on the individual parts of the organic center. However, its discovery, the discovery of the probable site, or of the nearest tools of the psyche, remains closed to us, because the substrate does not respond to the stimuli familiar to us with any apparent reaction.\(^{14}\) Whatever objections could be raised with regard to clinical observation, were soon eliminated by the not unjustified reference to the imperfection and ambiguity of autopsy sections and the simplicity and clarity of these vivisections. Finally, cases of congenital or acquired defects of individual cerebral parts without corresponding disturbances of cerebral functions were mentioned to prove how unessential the brain is for life.

These views were gradually modified in limited areas by a series of well ascertained facts indicating other conditions. Through Bouillaud it had become known a long time ago (1825) that the symptom complex, which is now called aphasia, can be caused by destruction of a small eccentric part of the cerebrum. Recently, numerous authors have contributed to a more precise definition of this sentence. There also exist in the literature a certain number of cases which showed paralysis of an arm or leg during life, and small disorganizations of the cerebrum at autopsy. Unfortunately, from Andral's compilation\(^{15}\) it cannot be seen how many such cases refer to the cerebrum itself and how many to its large ganglia. However, we must completely agree with what he states at the end of this observation:

"How can one fail to conclude from these facts that in the present state of science a distinct site in the brain can be assumed for the movements of the upper and lower limbs? Undoubtedly this distinct site exists, because each of these limbs can be paralyzed separately, but we do not yet know it."

To this statement we can only add that the cases concerning the corpus striatum and the optic thalamus must be excluded when these statistics are used for the determination of the place of origin of movement, because in these two large ganglia conducting paths from the hemispheres to the periphery already exist. Such facts indicated that the origin of at least individual psychic functions is tied to circumscribed parts of the brain. Goltz also came to the same conclusion when he was able to demonstrate in frogs, whose cerebrum he had extirpated, a residue of intelligence still existing in the optic lobe.

The only investigator who, on the basis of anatomic examinations, the possibility of which was doubted by some, assumed a quite decisive viewpoint which deviated considerably from the prevailing opinion, was Meynert. According to him,

\(^{12}\) In the place cited, pages 99 and 101.

\(^{13}\) In the same book, page 336.

\(^{14}\) See the newest textbooks of physiology, for example, Ranke, Grundziige etc., page 750, ff.—L. Hermann, Grundriss, 3 ed., 1870, p. 436 and 436, ff.

the cerebral cortex, which must be regarded as
the center of concepts, is divided into many much
less circumscribed areas, whose significance for
the individual kinds of concepts is produced by
the nerve fibers of the so-called projection system
which insinuate in the ganglion cells.

Meanwhile the premises for many conclusions
to be drawn from the fundamental characteristics
of the cerebrum were changed by the results of
our own examinations.

The starting point for these studies arose from
observations which one of us had the opportunity
to make on man, and which concern the first
movements of voluntary muscles that were
brought about by direct stimulation of the cere-
bral organs and observed on man. He found out
that, by conduction of constant galvanic currents
through the posterior part of the head, move-
ments of the eyes could be easily obtained which,
according to their nature, could have been incited
only by direct stimulation of cerebral centers.
Since these movements occurred only by galva-
ization of that region of the head, it could be
assumed that they were caused by stimulation of
the corpora quadrigemina, as indicated by some
things, or of adjoining parts. However, since such
ocular movements also appeared when the tem-
poral region was galvanized and certain tech-
niques were used which increased the stimulation,
the question arose whether in the latter method,
loops of current, which penetrated up to the base,
caused the movements of the eyes or whether the
cerebrum, in contradiction to the general opinion,
possesses electrical excitability.

After a preliminary experiment by one of us
had given a generally positive result with regard
to the rabbit, we pursued the following course for
the definite solution of the latter question.

The skulls of dogs, which were not narcotized
in the first experiments but were later narcotized,
were opened by a crown trephine at a flat spot.
Then, with cutting, rounded bone pincers, either
the entire half of the vault of the cranium or only
the portion covering the anterior lobe was re-
moved. In most cases, after using one hemisphere,
we proceeded in exactly the same manner with
the other half of the vault of the cranium. In all
these cases, however, after a dog bled to death
because of a slight injury to the longitudinal
sinus, we left completely intact a median bone
bridge protecting this sinus. Then the hitherto
uninjured dura was slightly incised, seized with
the pincers, and completely removed up to the
edges of the bone. During this maneuver the dogs
expressed severe pain by crying out and by char-
acteristic reflex movements. But later, when the
stimulus of air had acted for a long time, the
remaining hard cerebral membranes became much
more sensitive, a fact which we were very careful
to take into consideration when we arranged the
stimulative experiment. We were able to insult
the pia to various degrees by mechanical or other
stimuli, without the animal showing any sign of
sensation.

The devices for the electrical stimulation were
arranged as follows: the poles of a chain of 10
daniells went over a commutator to two terminals
of a Pohl tumbler switch, from which the cross
was removed. The conducting wires supplying the
current of a secondary induction coil ended at the
two opposite terminals. From the middle pair of
terminals, two wires led to a rheostat of 0-1 Siemens-unit resistance, which was inserted as
shunt. The main circuit continued over a DuBois
switch to two small insulated cylindrical ter-
inals which, on the other side, carried the elec-
trodes in the form of very fine platinum wires,
which were each provided in the front with a very
small knob. These platinum wires ran through two
small pieces of cork, the front of which they per-
forated, not parallel, but in a small angle, so that
the little knobs could quickly change their dis-
tance from each other by a slight displacement.
As a rule, this distance was about 2-3 mm. It
was necessary to give to the platinum wires only
a small mechanical resistance and the little knobs,
because otherwise any uncertainty of the hand,
even the respiratory movements of the brain,
led immediately to injuries of the soft mass of the
central organs.

The chains we used consisted of Siemens-Halske
cardboard elements which, according to a previ-
ous test, did not possess the full electromotor
force of a daniell, and each had a resistance of
about 5 Siemens units. As a rule, the resistance
of the shunt circuit was low, namely, at 30-40
Siemens units. The strength of the current was so
small that metallic closing of the circuit just
produced a sensation on the tongue touched with
the little knob. Considerably higher strengths of
current, as well as the elimination of the shunt
circuit, were used only for control experiments.
In the stimulative experiments with the induction
current, which were instituted much more infre-
cently, the resistance of the shunt naturally de-
pended each time on the arrangement of the coils.
In most of the experiments we also used a current
which just produced a sensation on the tongue.
By using this method, we obtained the follow-
ing results which we present as the product of a
very large number of experiments that agree up
to the smallest details for the brain of the dog,
without describing all these experiments. Since
the method is precisely described and the factors
to be mentioned in the following discussion are

16 Hitzig: Ueber die galvanischen Schwindelempfin-
dungen und eine neue Methode galvanischer Reizung
der Augenmuskeln, Verhandl. der Berl. med. Gesellcch.
11. A comprehensive work will soon follow.
considered, the repetition of our experiments is so easy that one will not have to wait a long time for confirmations.

One part of the convexity of the cerebrum of the dog is motor (this expression is used in the sense of Schiff), another part is not motor.

In general, the motor part lies more to the front, the nonmotor part lies to the rear. By electrical stimulation of the motor part, combined contractions of muscles of the opposite half of the body are obtained.

When weak currents are used, these contractions of muscles can be localized on certain, limited groups of muscles. When stronger currents are used, other muscles and even muscles of the corresponding half of the body, also participate in the stimulation of the same or adjoining places. However, the possibility of isolated stimulation of a limited group of muscles is reduced when very weak currents are used on very small places which we want to call centers for short. As a rule, a very small shifting of the electrodes sets the same extremity into motion; however, if, for example, extension occurs, then the shifting causes flexion or rotation. We found the parts of the cerebral surface lying between the centers designated by us, to be nonexcitable by the above-described method of stimulation when the minimal strength of current was used. However, when we increased either the distance between the two electrodes or the strength of the current, then twitchings could be produced; but these contractions of muscles seized the entire body in such a manner that it was not possible to distinguish whether they were individual or bilateral.

In the dog the location of the centers, which soon will be designated further, is very constant. The precise ascertainment of this fact encountered, at first, some difficulties. However, we removed these difficulties by finding out first the place which gave the strongest spasm of the group concerned when the stimulating current was weakest. Then we introduced a pin between the two electrodes into the brain of the still-living animal and compared, after taking the brain out, the individually marked spots with those of the alcohol preparations of previous experiments. How constantly the same centers are localized is best indicated by the fact that we have repeatedly been able to find the desired center in the central point of a single crown-trephine opening without any other opening of the skull. After removal of the dura, the muscles controlled by it twitched with the same certainty as if the entire hemisphere had been exposed. To be sure, in the beginning we had greater difficulties even in a quite free field of operation. For, although the individual convolutions of the brain are quite constant, their development in their individual parts and location to one another shows considerable differences. It is even rather the rule than the exception that the corresponding gyri of the two hemispheres of the same animal are differently built in separate parts. Besides, one time the middle portion of the convexity is more developed, another time the portions situated in the front or in the rear are more developed. If one also takes into consideration the necessity of leaving intact a certain portion of the meninges of the brain, and the darkening of the picture by the varied vascular distribution which make the gyri very indistinct, one will not be surprised about the difficulties we found in the beginning.

In order to make the repetition of our experiments still easier, we give below more accurate data concerning the location of the individual motor centers and we follow the nomenclature of Owen.\(^\text{17}\)

The center of the cervical muscles (see $\Delta$ of the figure) lies in the middle of the prefrontal gyrus, where the surface of this convolution drops sharply. The extreme end of the postfrontal gyrus shelters in the region of the end of the frontal fissure (see + of the figure) the center for the extensors and adductors of the foreleg. Somewhat to the rear from that and nearer the coronal fissure (see + of the figure) lie the central regions controlling the flexion and rotation of the limb. The place for the hind leg (see $\delta$ of the figure) is also in the postfrontal gyrus but medial to that of the

\(^{17}\) See also Reichert: Der Bau des menschl. Gehirns. Leipzig 1861, part II, p. 77.

foreleg and somewhat more to the rear. The facial nerve (see 5 of the figure) is innervated by the middle part of the suprasylvian gyrus. The place in question often exceeds 0.5 cm. in expansion and extends from the principal flexion above the sylvian fissure forward and backward.

We must add that we did not succeed in all the cases to set the cervical muscles in motion from the first-mentioned place. We have been able often enough to cause contraction of the muscles of the back, tail and abdomen from the parts lying between the designated points, but a circumscribed place, from which they could be stimulated separately, could not be determined with certainty. We also found the entire part of the convexity, which lies posterior to the center of the facial nerve, to be absolutely nonexcitable by quite disproportionate intensities of current. Even when the shunt circuit was disconnected, therefore when a current of 10 daniels in action, no spasms of muscles occurred.

The character of the spasms produced by stimulation of these motor centers differs according to the kind of stimulation. The stimulation by simple metallic closing of the chain current gives only a simple, rather fast-passing spasm. If, instead of the chain being closed in its metallic part, this is done by putting up the electrodes, then greater strengths of current are needed in order to obtain the same effect. Therefore here too the law of DuBois-Reymond applies. Metallic reversal always produces a ceteris paribus greater effect of stimulation than mere closing, but without the occurrence of two spasms (the second for the opening). However, in this kind of stimulation sometimes tetanus of the group of muscles concerned also appears, especially when the digital flexors are involved, although additional stimulating factors do not occur. When one of the electrodes has first acted, even only a short time, then the other one produces immediately at the same place a greater stimulating effect than it can before or afterwards.

While this agrees completely with what is known about the characteristics of peripheral nerves, we cannot, for good reason, omit to call attention to a different, moreover physiologically highly interesting stimulating factor. This is the absolutely constant predominance of the anode. In fact it seems as if within the minimum strengths of current only the anode incites spasms. Because the determination of this point is absolutely necessary for making the examination easier, we performed the following experiments which we often repeated.

1) With the ordinary distance between the electrodes, the place was sought from which spasms could be incited with the minimal strength of current, and in order to be entirely sure of that, we first performed repeated metallic closings. Then we used the current in open chain, without changing the place of the electrodes and closing them again. Now the spasms failed to appear. When we again opened, turned and closed, the stimulating effect was somewhat greater than in the first closings. This could be repeated as often as desired. If one or the other electrode left its place under repeated closings of the chain, this could be the cathode, without impairing the effect of stimulation. But the anode could not be shifted far from the point of stimulation, without either cessation of spasms, or spasms occurring in other groups of muscles.

2) The anode rested on the extension center, the cathode on the flexion center for the anterior extremity. Closing gave extension, reversal (in closed chain)—flexion, reversal—extension, reversal—flexion, and so forth. Therefore, each time the center corresponding to the anode was stimulated.

With regard to newer physiologic studies, it is tempting to attach to this fact some considerations concerning chemical processes in nerve activity. However, we prefer to abstain from that for the time being. The new facts which appeared in this study are so manifold, and their consequences extend in so many directions that it would be of little advantage to wander through these paths that require close investigation.

We must add here that in a somewhat longer closing of the chain, the more strongly stimulating effect of the change of the electrodes also expresses itself in the following manner. When we had produced a spasm while the anode lay on a center and the cathode lay on a spot which was indifferent to the strength of current used, and we left the chain closed for a somewhat longer time, then the closing of the reversed current sometimes incited, after previous opening, a single spasm, very seldom a repeated spasm. This means that, after a somewhat longer action of the anode, the central nerve substance reacted for a short time, even in minimal currents, also to the cathode. For several reasons one must use for this experiment only very weak currents, especially because stronger currents immediately destroy the substance by electrolysis.

In stimulation with tetanizing induction currents the effects of stimulation, depending on the kind, are not so constant. Frequently, tonic contractions of the masses of muscles concerned occur, which slacken in intensity only after a long time. Often there is an initial maximum contraction which is followed, after seconds-long duration of the current, by such a considerable diminution that the contraction could be regarded as completely expired if at the moment of the opening a
small movement in the sense of slackening contraction did not occur. These differences, as well as some phenomena which will be mentioned below, seem to be in causative connection with the individuality of the experimental animal—its greater or lesser excitability.

In continued use of stronger currents, symptoms of exhaustion occur—the necessity of stronger currents for obtaining the same effect, also complete absence of spasms. Very often, blood suffusions of the cortical substance take place. But more often one observes, even after weak currents, a series of symptoms, to which the opposite meaning must be given.

Eduard Weber has already stated that after a current has been opened that tetanizes the spinal cord of a frog, secondary movements occur in all the muscles of the body. This fact seems to be entirely forgotten. At least we would assume that otherwise it would be used as an argument by the supporters of the excitability of the spinal cord.

Something very similar takes place after tetanization of the cerebral substance. After a stimulation of a few seconds, secondary movements occur in the dependent musculature, which show a clearly quivering character in the region of the facial nerve. The extremities show rather the picture of clonic spasmodic movements—differences which in any case depend on the different kind of muscular attachment. Even when the brain is at rest, these local spasmodic attacks can often repeat themselves. In isolated cases they also occur after insult of the cerebral substances with closings of the chain current. But as a rule, they are not observed after stimulation with these currents. In two of our experimental animals, well-characterized epileptic attacks were formed from these subsequent movements. The attack began on one side with spasms in the previously stimulated musculature, but later spread to all the muscles of the body, so that it developed to a complete extensor tetanus. The pupils widened to the maximum. One of the animals had two such attacks, the other had three. One could object and say that the dogs had been epileptic previously. But one of the dogs had been with the same master for 6 years, without having ever suffered from spasms. The background of the other dog remained unknown.

Now we shall mention the objections which could be raised against our experiments.

The first objection, which is always raised in electrical stimulation experiments by experts, is based on the current loops which can reach distant parts. If we disregard the question whether cortices or medullary substance of the cerebrum are excitable, this objection is easier to remove than any other. In the first place, the currents used by us for the demonstrative experiments were generally weak. And since the substance of the brain possesses a very great resistance, since other conducting parts were not in the vicinity, and finally since the distance between the electrodes was small, the density of the current, according to the laws of current distribution in nonprismatic conductors, could be only a minimal one at a very short distance from the inlets. This would sufficiently refute a priori the objection in question. We also have a long series of direct proofs for us. If the current loops reached the peripheral nerves first, then the nerves of the same side always lay closer, and they did not have the remotest reason to turn exclusively to the other side. Also the motor nerves of the eyes on the same side lay much closer to them than any other nerves that may come into question. The very mobile eye, which is so well balanced in labile equilibrium, forms, without requiring any preparation, the most excellent physiologic rheoscope; in minimal current loops it would also move much more easily than an anterior extremity, not to speak of the posterior extremities. But on the entire convexity, so far as it can be exposed, there is not a single place from which eye movement can be obtained even with stronger currents than the ones we ordinarily used. Hereby, also one part of the question would be disposed of, which caused one of us to institute these studies.

Finally, we wish to mention a fact of great physiologic and pathologic interest. It is the fact that, with fatal hemorrhage, the excitability of the brain decreases unusually fast and disappears almost entirely, even before death. Immediately after death it is completely extinguished even to the strongest currents, while muscles and nerves still react excellently. This seems to suggest that experiments about the excitability of the central organs should be instituted with undisturbed circulation.

In the second place, it could be assumed that not peripheral nerves or the spinal cord, about which the same thing can be said, but other cerebral regions than the large hemispheres could be hit by current loops. If this were the case, then the proof of electrical excitability of other cerebral regions would also be an important discovery. For at the present time it is generally asserted that most of them are inaccessible to direct stimulation. However, as it can be proved, even for electrical stimulation, that is not so. Those parts, which are said by a few investigators to possess direct excitability, are the posterior parts (cauda) of the corpus striatum, optic thalamus,
If we exclude the corpus striatum, then all the other morphologic constituents of the brain just mentioned lie so much in the rear that in frontal sections they are only found if one, working posteriorly, arrives at the parts of the cerebrum that no longer react. The only exception is the corpus striatum, the caudal of which lies, however, in the nonexcitable zone. Therefore, it could be possible that precisely the anterior or middle portion of this ganglion, the portion which is supposed to be nonexcitable, is excitable and is the place of origin for the effects of our stimulation. From the very beginning this appeared improbable because in equal strength of current the spasms ceased as soon as the electrodes changed their place by a few millimeters. For if straight lines are drawn through the two assumed admission points and a perpendicularly lying point drawn below their connecting line in the corpus striatum, an equalilateral triangle is obtained whose equal sides would give off current paths of least resistance. Since the resistance of the two must be approximately the same, then ceteris paribus, the effect of stimulation should also be the same, which is not the case.

Not satisfied with these conclusive aprioristic evidences, we also tried to obtain direct proof. For this purpose we gave Carlsbad insect needles a dense insulating cover by immersing them repeatedly in a gutta-percha solution in chloroform. Only the tip and the head were kept conductive. When we introduced these needles into the rear part of the cerebrum, we obtained no trace of a spasm even with very much stronger currents, until the rheophores, which had penetrated several cm. deep, touched the cerebral peduncle. But then the animal received, with a lively jump, general agitation of the muscles. That was different when the anterior half of the brain was stimulated in the same manner. If it was to be assumed that current loops, penetrating up to the corpus striatum, incited the spasms which occurred by superficial stimulation, then the spasms should gradually become stronger with the penetration of the electrodes. However, this did not take place, but rather the spasms spread to other muscles and showed a different behavior which we shall not describe here in detail. Consequently, it can be assumed with certainty that neither the above-mentioned ganglion nor the structures composing the brain stem participated in the spasms stimulated from the convexity.

Another objection which could be raised and has been raised against all the previous successful experiments on stimulation of the central organs (spinal cord, brain stem) would be based on reflex occurrence of the contractions. This objection too can be eliminated by conclusive proofs.

**Footnotes:**
22 We call nonexcitable, without prejudice, all those regions from which no spasms can be produced.

Reflexes could be incited by the nerves of the dura and those of the pia mater, but we were protected by abundant exposition of the cerebral surface against stimulations of neighboring nerves of the cranial coverings. However, at one edge of the wound lay the partly detached temporal masses of muscles. These structures, which most probably had preserved their excitability, would have immediately revealed to us weak current loops. But sensitive fibers in the cerebrum itself have not yet been demonstrated or even assumed. Also, the complete insensibility of its substance does not give the slightest evidence for such an assumption.

As regards the dura, we have already stated above that it already possesses a certain inherent sensitiveness in the physiologic state, but that this sensitiveness increases very rapidly after opening the skull. For this reason it is advisable to operate quickly, because otherwise the experimental animal, even if it is tied firmly, makes, by powerful jumps, the sparing of the cerebral substance very difficult during excision of this membrane. But if it is excised up to the edges of the bones, one is sufficiently protected against reflexes of its nerves. We assured ourselves of that in various manners. In the first place, in our stimulation experiments we incited crossed spasms, while reflexes always occur first on the same side (Pflüger). In the second place, the spasms ceased when the place was slightly changed but the distance from the remaining dura was the same. In the third place, they also ceased when we came closer to the dura, provided we did not find any motor centers. We did not even obtain any spasms, always under the last-mentioned provision, when the electrode lay close to the dura but still on the cerebral substance. However, if, in the fourth place, we touched the dura itself, there appeared in many cases, even if no current flowed through, but upon electrical stimulation, very lively reflex movements in an extremely characteristic form. But these movements looked quite different from the effects of our other stimulations. In the former, they always presented the aspect of fitness; extension of the head, contractions of the muscles of the spine, cries and moans even under morphine narcosis, seldom movements of the extremities. The picture of our experiments was quite different. Even non-narcotized animals lay motionless, indifferent, while we set in motion at one time an anterior extremity, at another time a posterior extremity by an electrical stimulus.

The pia cannot be reflected back in the same manner; on the contrary, it must be handled as carefully as possible. For the injury of a single one of its numerous swollen vessels overflows the field of operation with blood and can wreck the entire experiment, thus making the sacrifice of the animal useless. However, this does not hinder the
proof of its being unessential for the occurrence of effects of our stimulation. Apart from all the reasons which we have already mentioned when we discussed the dura, the following is more than sufficient. Like Longet and others, we found the pia to be insensitive. We cut around it over a motor center, sparing the larger vessels, without any change of the effect of stimulation. We excised it at such a place—the spasms never failed to appear. We stuck insulated needles into the cerebral substance, and the muscles still twitched when we did that in the region of the motor sphere; they did not twitch under any of these conditions when we went beyond the posterior limit of this sphere. Moreover, it should be of interest to state here that neither morphine- nor ether-narcosis had an essential influence on the success of the experiments.

Finally, one may ask how it is that so many earlier investigators—among them the most brilliant ones—arrived at opposite results. To this we have only one answer: "The method creates the results." It is impossible that our predecessors had exposed the entire convexity, for in that case the spasms would have been elicited. The posterolateral wall of the dog's cranial vault, under which there are no motor parts, is recommendable, because of its formation, for placing the first trephine openings. Probably they began the operation here and neglected to break open anteriorly, under the erroneous opinion that the individual areas of the surface were equivalent. This was based upon the still widespread assumption that all the psychic functions were omnipresent in all the parts of the cerebral cortex. If they had thought of a localization of the mental functions, they would have regarded the apparent nonexcitability of individual parts of the substrate as something self-evident and would have examined all its parts. Perhaps none of the previous investigators had assumed that we could arouse ideas with our stimuli or could demonstrate something aroused on the vivisected animal. This leads us to the discussion of a question which could be addressed to us, although without justification. Some people may ask us to explain the observations which exist in sufficient number about surgical injuries of the brain without disturbance of any function. In the first place it is not our task to solve this apparent contradiction; for, before this duty becomes incumbent upon us, they must prove to us that the parts in question were injured or lost—a somewhat difficult undertaking. But neither we nor others know anything more accurate about other parts of the convexity, except perhaps what is known of the third frontal convolution, and this speaks precisely for us. As we have said before, the contradiction is only an apparent one; the parts of the cerebrum are not equivalent.

It also seems to us very appropriate to remind the reader of Griesinger's following remark which fits this point very well:

"Some kinds of doubts were expressed about these observations. In almost all the cases only the intelligence was considered, while the disposition and the volition have remained entirely without consideration, and usually only the slightest demands were made on the intelligence, for example, the answering of simple medical questions, in order to declare it uninjured. In none of these observations was the intelligence tested in its entire scope, and in many of them, namely, in all hospital observations, a comparison of the mental condition after the illness or the loss of substance with the previous status was utterly impossible, etc."

As his matter requires, Griesinger looks only at the psychic condition. With regard to somatic functions we can ask, even with greater justification, exactly what he wants from the investigation of the psychic condition. Where are the studies on muscular characteristics or the qualities of the sense of touch, which would be more at the right place here than in other places where they—a scientific humbug—serve only to spray sand in the eyes of harmless readers! Some experiments, which will be mentioned in the following discussion, will show how well-founded this demand of ours is.

If we now look back at the results of our studies and ask ourselves what we have obtained through them in knowledge of the characteristics of the central organ, then it is our duty to distinguish between that which can rightly be concluded as certain, and that which has been made only probable.

In the first place, as a certain accomplishment we can mention the definitely proved fact—which can be reproduced at any moment—that structures of the central nervous system respond to one of our stimuli with a visible reaction. In principle, this alone would be of certain significance for physiology, since thereby the contradiction in the definition is removed, to which Fick rightly pointed recently and which refers to the beginning of this work.

Also confirmed is the fact that a considerable part of the nerve masses which compose the large
hemispheres—it can be said almost one-half of them—is in direct connection with muscular movement, while another part apparently has nothing to do directly with it. Although this seemed very simple and self-evident, it was hardly clarified up to now. We refer for this purpose to what was said in regard to the historical synopsis. Whenever one spoke of such centers in the brain, even in most recent times, only basal parts—pons, thalami, etc. were mentioned, and when those postmortem results were explained, the common expressions were carefully used. Only a few specialists in brain anatomy, among them especially Meynert, had expressed themselves, to be true in a different manner from Gall, in favor of a strict localization of the individual psychic faculties.

However, if we raise the question whether the effects of stimulation incited by us are produced by direct influence on those centers of the gray cortex in which the motor impulse of volition occurs, or whether one must think of stimulation of the medullary fibrillation, or whether a third thing is possible, then our answer must be much more reserved. Even if we assume that it has been proved that the motor phenomena in question are incited by the ganglionic substance—and this is not the case—it would still not be proved in those motor phenomena which are liberated by internal occurrence, that precisely this part of the cortex supplies the substrate for the first externally directed link in the chain which begins with the first occurrence of a sensual impression, and whose preliminary end meets the expression of volition which appears as muscular movement.

It is rather not inconceivable, and it cannot be excluded by what we know in anatomy about the anastomosing structure of these parts, that the cerebral portion, which includes the birth place of the volition of movement, is just another one or perhaps a more manifold one, and that the regions called centers by us act only as mediators, collecting places in which similar but more suitable arrangements of muscular movements occur than in the gray substance of the spinal cord and the base of the brain. We shall soon see to what extent we have even uncovered a certain physiologic justification to allow a place for this point of view.

After we have granted, in this reservation, the widest scope to the purely psychologic possibilities—and we emphasize this expressly—we turn to the discussion of the question concerning the value of the gray and of the white substance for the occurrence of the effects of stimulation described by us. If the question is put in this form, then it should be partly possible now to answer it satisfactorily. But if, instead of the more common concepts of gray and white substance, the words fibers and cells were compared, the possibility of a solution could not be perceived, for, since in the gray substance fibers and cells are inseparably mixed, an isolated examination of the individual morphologic constituents cannot be performed. Therefore, even if the direct proof of excitability were also produced for the gray substance, one could still object that, not the ganglion cells, but the interspersed nerve fibers of this substance provide the truly excitable part. At the present time the state of the question is that, by means of the above-mentioned experiments concerning the introduction of insulated needles, we have proved adequately the excitability of the medullary substance. Since the essential nervous constituents of the medullary substance—the nerve fibers—continue with the same anatomic characteristics into the cortical substance, there is no reason to assume an essential change of their physiologic characteristics before their anatomic continuity is interrupted by new structures. For this reason, the excitability of a part of the fibers, also of the cortex, can be rightly assumed. As said before, by the means available up to now it cannot be decided definitely whether the fibers alone or also the cells are excitable.

However, in an indirect manner a somewhat probable conclusion can be drawn concerning the function if not concerning the excitability of the cellular part of the cortex. In the description of our experiments we saw that, with a minimum strength of current, contractions of muscles occur only if the electrodes are in very specific places, and that they cease or they appear in other muscles when the electrodes deviate even only slightly from the aforementioned places. This behavior admits only two possibilities. Either the stimulus is picked up by the ganglion cells lying in the immediate vicinity of the electrodes and is converted by them into muscular movement, or precisely at these points excitable medullary fibers appear very near the surface, so that they are very favorably situated for stimulation. Since no other reason can be detected as to why the medullary fibers in question should mostly approach the ganglion cells precisely here, rather than go and meet their fate by penetrating into the others, it can be assumed that precisely those ganglion masses are intended for precisely those nerve fibers for the production of organic stimuli.

By the methods used up to now it cannot be decided whether a certain, ordinarily cooperating sum of these organic stimuli produces exactly the same motor manifestations as our electrical stimulus. For the simple theory of specific energies does not suffice here; we must develop a new viewpoint for the new facts found. We do not have here nerve fibers which run straight to the

26 See for example, Griesinger in the place cited, page 4, and many other authors, but the same on page 23.
end organ, but rather fibers which emanate from the most central place of the cerebrum and can arrive there after they have passed a number of more and more peripherally situated stations, in each of which their liberated tensions are converted in a certain manner that is not exactly known, so that the result is what we call an appropriate movement. Of course, by a stimulus produced at any point of this path we can at the most show only what usually occurs in the more peripherally situated stretch and the more peripherally situated stations, while the functions of the central stations are not observed. Even this can be expressed only with a certain restriction since the stimulation of a larger sum of fibers is necessary for producing a certain motor modality, which fibers, however, do not lie together as comfortably in the central organs as in the trunk of a peripheral nerve. However, there is another way of solving the question according to the significance of the individual parts of the cortex; it is the extirpation of circumscribed and well known parts of it. We have also begun to embark upon this protracted road in the following manner.

After the soft parts of two dogs were reflected back, the skulls were opened by means of a crown trephine at the place where we presumed the center for the right anterior extremity to be. We chose the center for an extremity because possible motor phenomena should appear most distinctly in an extremity, and we did not choose the center for the posterior extremity because its position would possibly lead to the opening of the longitudinal sinus. Then the dura of the exposed place was removed; by electrical stimulation we ascertained that we had hit the desired place; so far as necessary, the pia was cut around and somewhat lifted from the cortical substance by means of a fine scalpel. In one case the removed piece was about as large as a small lentil, in the other case a little larger. Then the skin wound was united by means of button sutures. In the first case the animal had lost only a few drops of blood during the entire operation; in the other case the bleeding was not inconsiderable. The first case healed by first intention, the other case did not. But the two experimental animals presented symptoms which differed only in degree. The aspects of their disorder regarding motor disturbances were as equal as possible. This complete agreement of the results of the two experiments and their importance for all the observations which resulted from our other experiments cause us to mention them here, although we would like to collect additional similar experiences before any kind of publication is done. The necessity of giving this work a preliminary conclusion has prevented us from doing this up to now; moreover, it will be seen that a single successful experiment is sufficient for conclusions to be drawn by us ad hoc.

Both experimental animals showed immediately after operation, which occurred under morphine-narcosis, some general weakness which soon disappeared. But later the following was observed:

I. When running, the animals set the right front paw inappropriately more inward or more outward than the other one, and slid with this paw, never with the other, slightly outward, so that they fell to the ground. No movement disappeared entirely; however, the right leg was drawn up somewhat more weakly.

II. When standing, very similar phenomena. Besides, the front paw was placed on the dorsum instead of on the sole, without the dog being aware of it.

III. When sitting on the hind part, when both front paws rested on the ground, the right front leg slid gradually outward, until the dog lay entirely on the right side.

But under any circumstances the dog could immediately get up again. On the right front paw, the skin sensibility and the sensitivity to deep pressure showed no demonstrable abnormalities.

Most striking was the following experiment in the first dog even after the wound had healed for a long time, after all the reactions had ceased, and it was on the 15th and even on the 28th day after the operation.

While the dog was standing, his right front paw was set on its anterior, upper edge so much inward and to the rear that it lay between the other three legs. If, by patting, the dog was prevented from moving away, the dog let the paw stay in this uncomfortable position for any length of time. But if he received any kind of motor impulse, he ran away, moving his sick leg almost as briskly as the other three. It was not possible to make the same experiment with the left leg, because the small animal always withdrew this limb and brought it into its previous comfortable position.

We also save here all the additional conclusions and observations, especially certain comparisons with human pathology, for another occasion, and regard the following only as essential for the present work. By extirpation of a part of what we call the center of the anterior extremity, the two experimental animals had lost only incompletely the ability to move the anterior extremity, and probably they lost no sensibility whatever. But they obviously had lost a partial consciousness of the conditions of this limb, the ability to form complete concepts about it; therefore, they suffered from a symptom which occurs in a very similar manner in a form of the disease group tabes, except that injury of a sensory conducting path surely did not exist here. In order to denote this condition better, one could perhaps express one-

22 The second one is not mentioned here because, for experimental reasons, it was only three-legged.
self as follows: there still existed some motor conduction from the psyche to the muscle, while there was an interruption somewhere in the conduction from the muscle to the psyche. This interruption possibly concerned the end station of the hypothetical path for the muscular sense; but in any case it had its seat at the place of the center which was injured by us.

No matter how this was, it is certain that an injury of this center only alters, but does not remove, the voluntary movement of a limb which surely depends on the center, and that therefore other places and paths are also open to a motor impulse, in which this impulse can be born and hasten to the muscles of that leg, so that our reservation . . . was well founded. But it is also equally sure that such an injury, although small in comparison to the ablations of Flourens, Hertwig and others, produces very distinctly perceptible symptoms when the right spot is found; and the symptoms are perceptible precisely on that limb whose muscles previously contracted upon electrical stimulation of the now destroyed masses.

From the above it becomes evident that in previous colossal mutilations of the brain either other parts were chosen, or the necessary attention was not paid to the finer functions of the motor mechanisms. It also results from the sum of all our experiments that by no means, as Flourens and most of the others after him believed, is the psyche a kind of total function of the entire cerebrum, whose expression can be removed as a whole but not in its individual parts by mechanical means, but that rather individual mental functions, probably all of them, contribute to the occurrence of the matter in circumscribed centers of the cerebral cortex.

Berlin, 28 April, 1870