cepted. I do admit that Kilgar bates me—but then Kilgar bates the divil himself; the divil himself when he was a dhragoon couldn't shoot with Misther Kilgar iv Athlone. That's admitted, an' can't be denied.'

"'Indeed? Misther Kilgar invited ye help him shoot the hill, I suppose?'

"'Ay. Ye see it's this way: There's a scoundhril iv a fella goin' about here —an' unhung too, I'm sorry to say—that they call the Red Poocher. He was startin' in with his thricks upon sthrangers when I come here, an' I put about the size iv a naggin iv shot into him wan evenin' an' passed him on from me moor —Meenavalla. He come this way, an' Kilgar, noways loth to help the lame dog over the stile, give him another fistful or two iv the same medicine, an' sent him further. On the sthren'th iv this we sthruck up an acquaintance, an' shot our lan's day about, formin' an alliance that has sthruck terror to the hearts of all poochers, an' kep' them as mute mice in a male-bag.'

"'Railly?' siz the sthranger, in a very sleekit, quiet way. 'Then I'm mortial glad to l'arn it. I am mighty intherested meself in the suppression iv poochers an' poochin'.'

"'Right ye are, oul' chap. Give us yer han' on it,' siz Hedger, reachin' for the fella's fist.

"'Aisy, aisy,' siz the fella, dhrawin' back. 'Mortial much intherested, I say, in the suppression iv poochers and poochin', an' that's why it'll give me shupreme pleasure to—with all the expedition I can —present you, Misther Hedger iv Oxfoord, England, with a writ for a very han'some figure i' damages, be raison iv yerself an' yer sarvint, in conjunction with another pair iv notorious poochers—wan iv them popularly known as the Red Poocher— shootin' my Binbane *take* for four days, an' killin', slayin', and otherwise desthroyin' the grouse, snipe, an' hares thereon, an' other game. *My* name is Misther Kilgar — Misther Augustus Kilgar, iv Athlone, solicitor. An', furthermore, Misther Hedger iv Oxfoord, Englan',' siz he, still in the politest manner imaginable, 'I may mention for yer gratification that if yer English frien's don't die till they taste some iv the many hampers iv game you've been thrinnlin' off to them from Meenavalla, they're likely to live to a very ripe oul' age. It's a sort of consolation to me to know that if the Red Poocher got yer help to pooch me, he likeways took the loan iv ye to help pooch yerself.

"'As to the criminal action ye've left yerself open to, Misther Hedger iv Oxfoord, England, I'll lay that entirely atween yerself an' the police.'

"Faith, poor Grip's 'wakened again, an' as fresh as a May flower. Ellen, *a theagair*, Grip would die in the dumps if I didn't let him toss a *broc* now an' again for sport."

THE CENTURY'S PROGRESS IN EXPERIMENTAL PSYCHOLOGY.

BY HENRY SMITH WILLIAMS, M.D.

I.

A LITTLE over a hundred years ago a reform movement was afoot in the world in the interests of the insane. As was fitting, the movement showed itself first in America, where these unfortunates were humanely cared for at a time when their treatment elsewhere was worse than brutal, but England and France quickly fell into line. The leader on this side the water was the famous Philadelphian Dr. Benjamin Rush, "the Sydenham of America"; in England, Dr. William Tuke inaugurated the movement; and in France, Dr. Philippe Pinel, single-handed, led the way. Moved by a common spirit, though acting quite independently, these men raised a revolt against the traditional custom which, spurning the insane as demon-haunted outcasts, had condemned these unfortunates to dungeons, chains, and the lash. Hitherto few people had thought it other than the natural course of events that the "maniac" should be thrust into a dungeon, and perhaps chained to the wall with the aid of an iron band riveted permanently about his neck or waist. Many

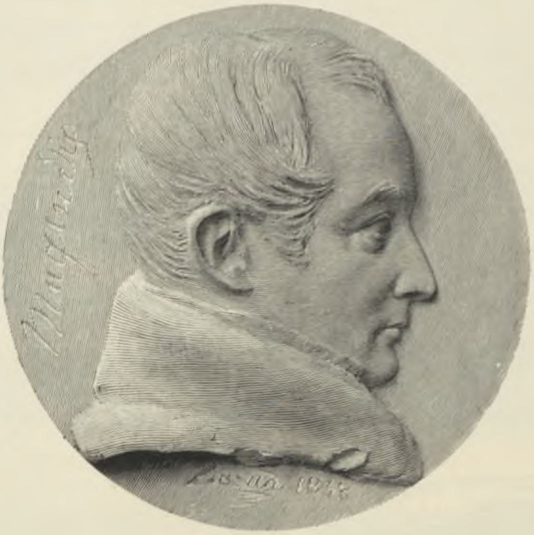
Reproduced by permission of Goupil et Cie, Paris.

PINEL AT LA SALPÊTRIÈRE, IN 1795, RELEASING THE INSANE FROM THEIR MANACLES.

From the painting by Tony Robert-Fleury at the Salon of 1876.

an unfortunate, thus manacled, was held to the narrow limits of his chain for years together in a cell to which full daylight never penetrated ; sometimes—iron being expensive—the chain was so short that the wretched victim could not rise to the upright posture, or even shift his position upon his squalid pallet of straw.

In America, indeed, there being no Middle Age precedents to crystallize into established customs, the treatment accorded the insane had seldom or never sunk to this level. Partly for this reason, perhaps, the work of Doctor Rush, at the Philadelphia Hospital, in 1784, by means of which the insane came to be humanely treated, even to the extent of banishing the lash, has been but little noted, while the work of the European leaders, though belonging to later decades, has been made famous. And perhaps this is not as unjust as it seems, for the step which Rush took, from relatively bad to good, was a far easier one to take than the leap from atrocities to good treatment which the European reformers were obliged to compass. In Paris, for example, Pinel was obliged to ask permission of the authorities even to make the attempt at liberating the insane from their chains, and notwithstanding his recognized position as a leader of science, he gained but grudging assent, and was regarded as being himself little better than a lunatic for making so manifestly unwise and hopeless an attempt. Once the attempt had been made, however, and carried to a successful issue, the amelioration wrought in the condition of the insane was so patent that the fame of Pinel's work at the Bicêtre and the Salpêtrière went abroad apace. It required, indeed, many years to complete it in Paris, and a lifetime of effort on the part of Pinel's pupil Esquirol and others to extend the reform to the provinces; but the epochal turning-point had been reached with Pinel's labors of the closing years of the eighteenth century.



FRANÇOIS MAGENDIE.
Engraved by E. Schladitz after the medallion by David d'Angers.

The significance of this wise and humane reform, in the present connection, is the fact that these studies of the insane gave emphasis to the novel idea, which by-and-by became accepted as beyond question, that "demoniacal possession" is in reality no more than the outward expression of a diseased condition of the brain. This realization made it clear, as never before, how intimately the mind and the body are linked one to the other. And so it chanced that in striking the shackles from the insane, Pinel and his confrères struck a blow also, unwittingly, at time-honored philosophical traditions. The liberation of the insane from their dungeons was an augury of the liberation of psychology from the musty recesses of metaphysics. Hitherto psychology, in so far as it existed at all, was but the subjective study of individual minds; in future it must become objective as well, taking into account also the relations which the mind bears to the body, and in particular to the brain and nervous system.

The necessity for this collocation was advocated quite as earnestly, and even more directly, by another worker of this period, whose studies were allied to those of alienists, and who, even more actively than they, focalized his attention upon the brain and its functions. This earliest of specialists in brain studies was a German by birth, but Parisian by adoption, Dr. Franz Joseph Gall, originator of the since notorious system of phrenology. The merited disrepute into which this system has fallen through the expositions of peripatetic charlatans should not make us forget that Dr. Gall himself was apparently a highly educated physician, a careful student of the brain and mind, according to the best light of his time, and, withal, an earnest and honest believer in the validity of the system he had originated. The system itself, taken as a whole, was hopelessly faulty, yet it was not without its latent germ of truth, as later studies were to show. How firmly its author himself believed in it is evidenced by the paper which he contributed to the French Academy of Science in 1808. The paper itself was referred to a committee of which Pinel and Cuvier were members. The verdict of this committee was adverse, and justly so; yet the system condemned had at least one merit which its detractors failed to realize. It popularized the conception that the brain is the organ of mind. Moreover, by its insistence it rallied about it a band of scientific supporters, chief of whom was Dr. Kaspar Spurzheim, a man of no mean abilities, who became the propagandist of phrenology in England and in America. Of course such advocacy and popularity stimulated opposition as well, and out of the disputations thus arising there grew presently a general interest in the brain as the organ of mind, quite aside from any preconceptions whatever as to the doctrines of Gall and Spurzheim.

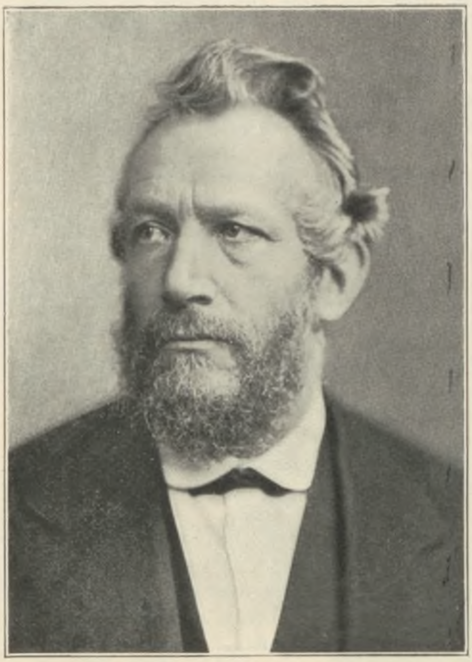
SIR CHARLES BELL.
By permission of G. Bell and Sons, London.

Prominent among the unprejudiced class of workers who now appeared was the brilliant young Frenchman Louis Antoine Desmoulins, who studied first under the tutorage of the famous Magendie, and published jointly with him a classical work on the nervous system of vertebrates in 1825. Desmoulins made at least one discovery of epochal importance. He observed that the brains of persons dying in old age were lighter than the average, and gave visible evidence of atrophy, and he reasoned that such decay is a normal accompaniment of senility. No one nowadays would question the accuracy of this observation, but the scientific world was not quite ready for it in 1825; for when Desmoulins announced his discovery to the French Academy, that august and somewhat patriarchal body was moved to quite unscientific wrath, and forbade the young iconoclast the privilege of further hearings. From which it is evident that the partially liberated spirit of the new psychology had by no means freed itself altogether, at the close of the first quarter of our century, from the metaphysical cobwebs of its long incarceration.

II.

While studies of the brain were thus being inaugurated, the nervous system, which is the channel of communication between the brain and the outside world, was being interrogated with even more tangible results. The inaugural discovery was made in 1811 by Dr. (afterwards Sir Charles) Bell, the famous English surgeon and experimental physiologist. It consisted of the observation that the anterior roots of the spinal nerves are given over to the function of conveying motor impulses from the brain outward, whereas the posterior roots convey solely sensory impulses to the brain from without. Hitherto it had been supposed that all nerves have a similar function, and the peculiar distribution of the spinal nerves had been an unsolved puzzle.

Bell's discovery was epochal; but its full significance was not appreciated for a decade, nor, indeed, was its validity at first admitted. In Paris, in particular, then the court of final appeal in all matters scientific, the alleged discovery was looked at askance, or quite ignored. But in 1823 the subject was taken up by the recognized leader of French physiology—



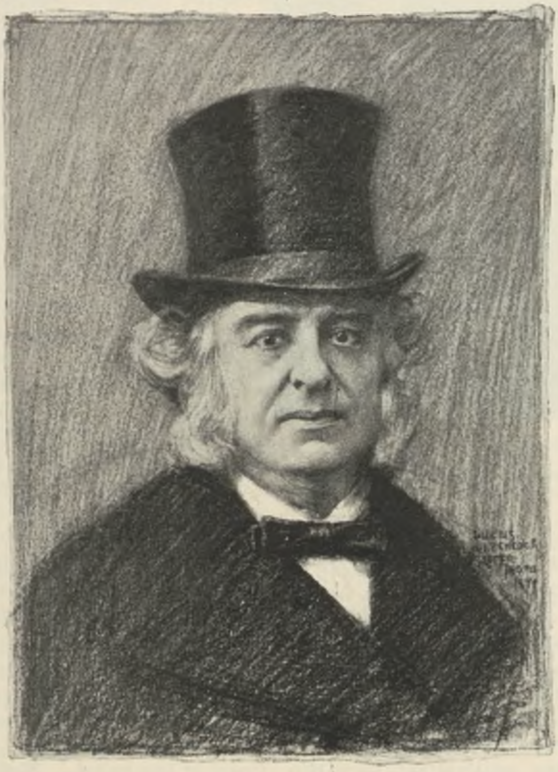
EMIL DU BOIS-REYMOND.
After a photograph by Loescher and Petsch, Berlin.

JEAN MARTIN CHARCOT.
After a photograph by Eug. Pirou, Paris.

François Magendie—in the course of his comprehensive experimental studies of the nervous system, and Bell's conclusions were subjected to the most rigid experimental tests, and found altogether valid. Bell himself, meanwhile, had turned his attention to the cranial nerves, and had proved that these also are divisible into two sets—sensory and motor. Sometimes, indeed, the two sets of filaments are combined into one nerve cord, but, if traced to their origin, these are found to arise from different brain centres. Thus it was clear that a hitherto unrecognized duality of function pertains to the entire extra-cranial nervous system. Any impulse sent from the periphery to the brain must be conveyed along a perfectly definite channel; the response from the brain, sent out to the peripheral muscles, must traverse an equally definite and altogether different course. If either channel is interrupted—as by the section of its particular nerve tract—the corresponding message is denied transmission as effectually as an electric current is stopped by the section of the transmitting wire.

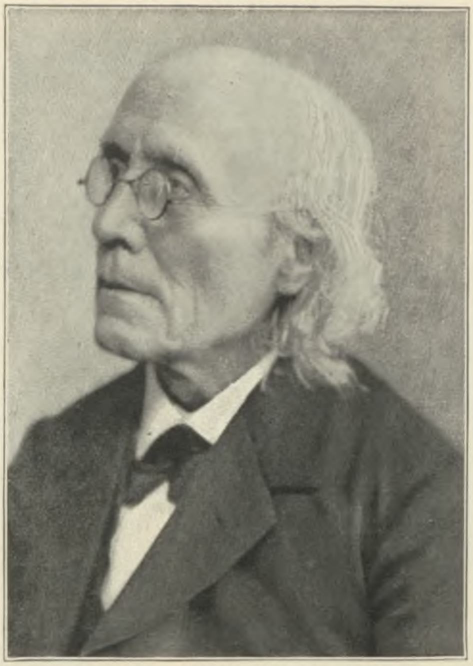
Experimenters everywhere soon confirmed the observations of Bell and Magendie; and, as always happens after a great discovery, a fresh impulse was given to investigations in allied fields. Nevertheless, a full decade elapsed before another discovery of comparable importance was made. Then Marshall Hall, the most famous of English physicians of his day, made his classical observations on the phenomena that henceforth were to be known as reflex action. In 1832, while experimenting one day with a decapitated newt, he observed that the headless creature's limbs would contract in direct response to certain stimuli. Such a response could no longer be secured if the spinal nerves supplying a part were severed. Hence it was clear that responsive centres exist in the spinal cord capable of receiving a sensory message, and of transmitting a motor impulse in reply—a function hitherto supposed to be reserved for the brain. Further studies went to show that such phenomena of reflex action on the part of centres lying outside the range of consciousness, both in the spinal cord and in the brain itself, are extremely common; that, in short, they enter constantly into the activities of every living organism, and have a most important share in the sum total of vital movements. Hence, Hall's discovery must always stand as one of the great mile-stones of the advance of neurological science.

All these considerations as to nerve currents and nerve tracts becoming stock knowledge of science, it was natural that interest should become stimulated as to the exact character of these nerve tracts in themselves; and all the more natural in that the perfected microscope was just now claiming all fields for its own. A troop of observers soon entered upon the study of the nerves; and the leader here, as in so many other lines of microscopical research, was no other than Theodor Schwann. Through his efforts, and with the invaluable aid of such other workers as Remak, Purkinje, Henle, Müller, and the rest, all the mystery as to the general characteristics of nerve tracts was cleared away. It came to be known that in its essentials a nerve tract is a tenuous fibre or thread of protoplasm, stretching between two terminal points in the organism — one of such termini being usually a cell of the brain or spinal cord; the other, a distribution point at or near the periphery—for example, in a muscle or in the skin. Such a fibril may have about it a protective covering, which is known as the sheath of Schwann; but the fibril itself is the essential nerve tract; and in many cases, as Remak presently discovered, the sheath is dispensed with, particularly in case of the nerves of the so-called sympathetic system.



PAUL BROCA.
After a photograph by Pierre Petit, Paris.

This sympathetic system of ganglia and nerves, by-the-bye, had long been a puzzle to the physiologists. Its ganglia, the seeming centres of the system, usually minute in size, and never very large, are found everywhere through the organism, but in particular are gathered into a long double chain which lies within the body cavity, outside the spinal column, and represents the sole nervous system of the non-vertebrated organisms. Fibrils from these ganglia were seen to join the cranial and spinal nerve fibrils, and to accompany them everywhere, but what special function they subserved was long a mere matter of conjecture, and led to many absurd speculations. Fact was not substituted for conjecture until about the year 1851, when the great Frenchman Claude Bernard conclusively proved that at least one chief function of the sympathetic fibrils is to cause contraction of the walls of the arterioles of the system, thus regulating the blood-supply of any given part. Ten years earlier Henle had demonstrated the existence of annular bands of muscle fibres in the arterioles, hitherto a much mooted question, and several tentative explanations of the action of these fibres had been made, particularly by the brothers Weber, by Stilling, who, as early as 1840, had ventured



GUSTAV THEODOR FECHNER.
After a photograph by Georg Brokesh, Leipzig.

to speak of "vaso-motor" nerves, and by Schiff, who was hard upon the same track at the time of Bernard's discovery. But a clear light was not thrown on the subject until Bernard's experiments were made in 1851. The experiments were soon after confirmed and extended by Brown-Séquard, Waller, Budge, and numerous others, and henceforth physiologists felt that they understood how the blood-supply of any given part is regulated by the nervous system.

In reality, however, they had learned only half the story, as Bernard himself proved only a few years later by opening up a new and quite unsuspected chapter. While experimenting in 1858 he discovered that there are certain nerves supplying the heart which, if stimulated, cause that organ to relax and cease beating. As the heart is essentially nothing more than an aggregation of muscles, this phenomenon was utterly puzzling and without precedent in the experience of physiologists. An impulse travelling along a motor nerve had been supposed to be able to cause a muscular contraction and to do nothing else; yet here such an impulse had exactly the opposite effect. The only tenable explanation seemed to be that this particular impulse must arrest or inhibit the action of the impulses that ordinarily cause the heart muscles to contract. But the idea of such inhibition of one impulse by another was utterly novel, and at first difficult to comprehend. Gradually, however, the idea took its place in the current knowledge of nerve physiology, and in time it came to be understood that what happens in the case of the heart nerve-supply is only a particular case under a very general, indeed universal, form of nervous action. Growing out of Bernard's initial discovery came the final understanding that the entire nervous system is a mechanism of centres subordinate and centres superior, the action of the one of which may be counteracted and annulled in effect by the action of the other. This applies not merely to such physical processes as heart-beats and arterial contraction and relaxing, but to the most intricate functionings which have their counterpart in psychical processes as well. Thus the observation of the inhibition of the heart's action by a nervous impulse furnished the point of departure for studies that led to a better understanding of the modus operandi of the mind's activities than had ever previously been attained by the most subtle of psychologists.

III.

The work of the nerve physiologists had thus an important bearing on questions of the mind. But there was another company of workers of this period who made an even more direct assault upon the "citadel of thought." A remarkable school of workers had developed in Germany, the leaders being men who, having more or less of innate metaphysical bias as a national birthright, had also the instincts of the empirical scientist, and whose educational equipment included a profound knowledge not alone of physiology and psychology, but of physics and mathematics as well. These men undertook the novel task of interrogating the relations of body and mind from the stand-point of physics. They sought to apply the vernier and the balance, as far as might be, to the intangible processes of mind.

The movement had its precursory

stages in the early part of the century, notably in the mathematical psychology of Herbart, but its first definitive output to attract general attention came from the master-hand of Hermann Helmholtz in 1851. It consisted of the accurate measurement of the speed of transit of a nervous impulse along a nerve tract. To make such measurement had been regarded as impossible, it being supposed that the flight of the nervous impulse was practically instantaneous. But Helmholtz readily demonstrated the contrary, showing that the nerve cord is a relatively sluggish message-bearer. According to his experiments, first performed upon the frog, the nervous "current" travels less than one hundred feet per second. Other experiments performed soon afterward by Helmholtz himself, and by various followers, chief among whom was Du Bois-Reymond, modified somewhat the exact figures at first obtained, but did not change the general bearings of the early results. Thus the nervous impulse was shown to be something far different, as regards speed of transit, at any rate, from the electric current to which it had been so often likened. An electric current would flash half-way round the globe while a nervous impulse could travel the length of the human body—from a man's foot to his brain.

The tendency to bridge the gulf that hitherto had separated the physical from the psychical world was further evidenced in the following decade by Helmholtz's remarkable but highly technical study of the sensations of sound and of color in connection with their physical causes, in the course of which he revived the doctrine of color vision which that other great physiologist and physicist, Thomas Young, had advanced half a century before. The same tendency was further evidenced by the appearance, in 1852, of Dr. Hermann Lotze's famous *Medizinische Psychologie, oder Physiologie der Seele*, with its challenge of the old myth of a "vital force." But the most definitive expression of the new movement was signalized in 1860, when Gustav Fechner published his classical work called *Psychophysik*. That title introduced a new word into the vocabulary of science. Fechner explained it by saying, "I mean by psycho-physics an exact theory of the relation between spirit and body, and, in a general way, between the physical and the psychic worlds." The title became famous, and the brunt of many a controversy. So also did another phrase which Fechner introduced in the course of his book — the phrase "physiological psychology." In making that happy collocation of words Fechner virtually christened a new science.

The chief purport of this classical book of the German psycho-physiologist was the elaboration and explication of experiments based on a method introduced more than twenty years earlier by his countryman E. H. Weber, but which hitherto had failed to attract the attention it deserved. The method consisted of the measurement and analysis of the definite relation existing between external stimuli of varying degrees of intensity (various sounds, for example) and the mental states they induce. Weber's experiments grew out of the familiar observation that the nicety of our discriminations of various sounds, weights, or visual images depends upon the magnitude of each particular cause of a sensation in its relation with other similar causes. Thus, for example, we cannot see the stars in the daytime, though they shine as brightly then as at night. Again, we seldom notice the ticking of a clock in the daytime, though it may become almost painfully audible in the silence of the night. Yet again, the difference between an ounce weight and a two-ounce weight is clearly enough appreciable when we lift the two, but one cannot discriminate in the same way between a five-pound weight and a weight of one ounce over five pounds.

This last example, and similar ones for the other senses, gave Weber the clew to his novel experiments. Reflection upon every-day experiences made it clear to him that whenever we consider two visual sensations, or two auditory sensations, or two sensations of weight, in comparison one with another, there is always a limit to the keenness of our discrimination, and that this degree of keenness varies, as in the case of the weights just cited, with the magnitude of the exciting cause.

Weber determined to see whether these common experiences could be brought within the pale of a general law. His method consisted of making long series of experiments aimed at the determination, in each case, of what came to be spoken of as the least observable difference between the stimuli. Thus if one holds an ounce weight in each hand, and has tiny weights added to one of them, grain by grain, one does not at first perceive a difference; but presently, on the addition of a certain grain, he does become aware of the difference. Noting now how many grains have been added to produce this effect, we have the weight which represents the least appreciable difference when the standard is one ounce.

Now repeat the experiment, but let the weights be each of five pounds. Clearly in this case we shall be obliged to add not grains, but drachms, before a difference between the two heavy weights is perceived. But whatever the exact amount added, that amount represents the stimulus producing a just perceivable sensation of difference when the standard is five pounds. And so on for indefinite series of weights of varying magnitudes. Now came Weber's curious discovery. Not only did he find that in repeated experiments with the same pair of weights the measure of "just perceivable difference" remained approximately fixed, but he found, further, that a remarkable fixed relation exists between the stimuli of different magnitude. If, for example, he had found it necessary, in the case of the ounce weights, to add one-fiftieth of an ounce to the one before a difference was detected, he found also, in the case of the five-pound weights, that one-fiftieth of five pounds must be added before producing the same result. And so of all other weights; the amount added to produce the stimulus of "least appreciable difference" always bore the same mathematical relation to the magnitude of the weight used, be that magnitude great or small.

Weber found that the same thing holds good for the stimuli of the sensations of sight and of hearing, the differential stimulus bearing always a fixed ratio to the total magnitude of the stimuli. Here, then, was the law he had sought.

Weber's results were definite enough, and striking enough, yet they failed to attract any considerable measure of attention until they were revived and extended by Fechner, and brought before the world in the famous work on psychophysics. Then they precipitated a veritable mêlée. Fechner had not alone verified the earlier results (with certain limitations not essential to the present consideration), but had invented new methods of making similar tests, and had reduced the whole question to mathematical treatment. He pronounced Weber's discovery the fundamental law of psychophysics. In honor of the discoverer, he christened it Weber's Law. He clothed the law in words and in mathematical formulæ, and, so to say, launched it full tilt at the heads of the psychological world. It made a fine commotion, be assured, for it was the first widely heralded bulletin of the new psychology in its march upon the strongholds of the time-honored metaphysics. The accomplishments of the microscopists and the nerve physiologists had been but preliminary—mere border skirmishes of uncertain import. But here was proof that the iconoclastic movement meant to invade the very heart of the sacred territory of mind—a territory from which tangible objective fact had been supposed to be forever barred.

Hardly had the alarm been sounded, however, before a new movement was made. While Fechner's book was fresh from the press, steps were being taken to extend the methods of the physicist in yet another way to the intimate processes of the mind. As Helmholtz had shown the rate of nervous impulsion along the nerve tract to be measurable, it was now sought to measure also the time required for the central nervous mechanism to perform its work of receiving a message and sending out a response. This was coming down to the very threshold of mind. The attempt was first made by Professor Donders, in 1861, but definitive results were only obtained after many years of experiment on the part of a host of observers. The chief of these, and the man who has stood in the forefront of the new movement, and has been its recognized leader throughout the remainder of the century, is Dr. Wilhelm Wundt, of Leipzig.

The task was not easy, but, in the long-run, it was accomplished. Not alone was it shown that the nerve centre requires a measurable time for its operations, but much was learned as to conditions that modify this time. Thus it was found that different persons vary in the rate of their central nervous activity—which explained the "personal equation" that the astronomer Bessel had noted a half-century before. It was found, too, that the rate of activity varies also for the same person under different conditions, becoming retarded, for example, under influence of fatigue, or in case of certain diseases of the brain. All details aside, the essential fact emerges, as an experimental demonstration, that the intellectual processes—sensation, apperception, volition—are linked irrevocably with the activities of the central nervous tissues, and that these activities, like all other physical processes, have a time element. To that old school of psychologists, who scarcely cared more for the human head than for the heels—being interested only in the mind—such a linking of mind and body as was thus demonstrated was naturally disquieting. But whatever the inferences, there was no escaping the facts.

Of course this new movement has not been confined to Germany. Indeed, it had long had exponents elsewhere. Thus in England, a full century earlier, Dr. Hartley had championed the theory of the close and indissoluble dependence of mind upon the brain, and formulated a famous vibration theory of association that still merits careful consideration. Then, too, in France, at the beginning of the century, there was Dr. Cabanis with his tangible, if crudely phrased, doctrine that the brain digests impressions and secretes thought as the stomach digests food and the liver secretes bile. Moreover, Herbert Spencer's *Principles of Psychology*, with its avowed co-ordination of mind and body and its vitalizing theory of evolution, appeared in 1855, half a decade before the work of Fechner. But these influences, though of vast educational value, were theoretical rather than demonstrative, and the fact remains that the experimental work which first attempted to gauge mental operations by physical principles was mainly done in Germany. Wundt's *Physiological Psychology*, with its full preliminary descriptions of the anatomy of the nervous system, gave tangible expression to the growth of the new movement in 1874; and four years later, with the opening of his laboratory of Physiological Psychology at the University of Leipzig, the new psychology may be said to have gained a permanent foothold, and to have forced itself into official recognition. From then on its conquest of the world was but a matter of time.

It should be noted, however, that there is one other method of strictly experimental examination of the mental field, latterly much in vogue, which had a different origin. This is the scientific investigation of the phenomena of hypnotism. This subject was rescued from the hands of charlatans, rechristened, and subjected to accurate investigation by Dr. James Braid, of Manchester, as early as 1841. But his results, after attracting momentary attention, fell from view, and, despite desultory efforts, the subject was not again accorded a general hearing from the scientific world until 1878, when Dr. Charcot took it up at the Salpêtrière in Paris, followed soon afterward by Dr. Rudolf Heidenhain, of Breslau, and a host of other experimenters. The value of the method in the study of mental states was soon apparent. Most of Braid's experiments were repeated, and in the main his results were confirmed. His explanation of hypnotism, or artificial somnambulism, as a self-induced state, independent of any occult or supersensible influence, soon gained general credence. His belief that the initial stages are due to fatigue of nervous centres, usually from excessive stimulation, has not been supplanted, though supplemented by notions growing out of the new knowledge as to subconscious mentality in general, and the inhibitory influence of one centre over another in the central nervous mechanism.

These studies of the psychologists and pathologists bring the relations of mind and body into sharp relief. But even more definite in this regard was the work of the brain physiologists. Chief of these, during the middle period of the century, was the man who is sometimes spoken of as the "father of brain physiology," Marie Jean Pierre Flourens, of the Jardin des Plantes of Paris, the pupil and worthy successor of Magendie. His experiments in nerve physiology were begun in the first quarter of the century, but his local experiments upon the brain itself were not culminated until about 1842. At this time the old dispute over phrenology had broken out afresh, and the studies of Flourens were aimed, in part at least, at the strictly scientific investigation of this troublesome topic.

In the course of these studies Flourens discovered that in the medulla oblongata, the part of the brain which connects that organ with the spinal cord, there is a centre of minute size which cannot be injured in the least without causing the instant death of the animal operated upon. It may be added that it is this spot which is reached by the needle of the garroter in Spanish executions, and that the same centre also is destroyed when a criminal is "successfully" hanged, this time by the forced intrusion of a process of the second cervical vertebra. Flourens named this spot the "vital knot." Its extreme importance, as is now understood, is due to the fact that it is the centre of nerves that supply the heart; but this simple explanation, annulling the conception of a specific "life centre," was not at once apparent.

Other experiments of Flourens seemed to show that the cerebellum is the seat of the centres that co-ordinate muscular activities, and that the higher intellectual faculties are relegated to the cerebrum. But beyond this, as regards localization, experiment faltered. Negative results, as regards specific faculties, were obtained from all localized irritations of the cerebrum, and Flourens was forced to conclude that the cerebral lobe, while being undoubtedly the seat of higher intellection, performs its functions with its entire structure. This conclusion, which incidentally gave a quietus to phrenology, was accepted generally, and became the stock doctrine of cerebral physiology for a generation.

It will be seen, however, that these studies of Flourens had a double bearing. They denied localization of cerebral functions, but they demonstrated the localization of certain nervous processes in other portions of the brain. On the whole, then, they spoke positively for the principle of localization of function in the brain, for which a certain number of students contended; while their evidence against cerebral localization was only negative. There was here and there an observer who felt that this negative testimony was not conclusive. In particular, the German anatomist Meynert, who had studied the disposition of nerve tracts in the cerebrum, was led to believe that the anterior portions of the cerebrum must have motor functions in preponderance; the posterior portions, sensory functions. Somewhat similar conclusions were reached also by Dr. Hughlings-Jackson, in England, from his studies of epilepsy. But no positive evidence was forth-coming until 1861, when Dr. Paul Broca brought before the Academy of Medicine in Paris a case of brain lesion which he regarded as having most important bearings on the question of cerebral localization.

The case was that of a patient at the Bicêtre, who for twenty years had been deprived of the power of speech, seemingly through loss of memory of words. In 1861 this patient died, and an autopsy revealed that a certain convolution of the left frontal lobe of his cerebrum had been totally destroyed by disease, the remainder of his brain being intact. Broca felt that this observation pointed strongly to a localization of the memory of words in a definite area of the brain. Moreover, it transpired that the case was not without precedent. As long ago as 1825 Dr. Boillard had been led, through pathological studies, to locate definitely a centre for the articulation of words in the frontal lobe, and here and there other observers had made tentatives in the same direction. Boillard had even followed the matter up with pertinacity, but the world was not ready to listen to him. Now, however, in the half-decade that followed Broca's announcements, interest rose to fever-heat, and through the efforts of Broca, Boillard, and numerous others it was proved that a veritable centre having a strange domination over the memory of articulate words has its seat in the third convolution of the frontal lobe of the cerebrum, usually in the left hemisphere. That part of the brain has since been known to the English-speaking world as the convolution of Broca, a name which, strangely enough, the discoverer's compatriots have been slow to accept.

This discovery very naturally reopened the entire subject of brain localization. It was but a short step to the inference that there must be other definite centres worth the seeking, and various observers set about searching for them. In 1867 a clew was gained by Eckhard, who, repeating a forgotten experiment of Haller and Zinn of the previous century, removed portions of the brain cortex of animals, with the result of producing convulsions. But the really vital departure was made in 1870 by the German investigators Fritsch and Hitzig, who, by stimulating definite areas of the cortex of animals with a galvanic current, produced contraction of definite sets of muscles of the opposite side of the body. These most important experiments, received at first with incredulity, were repeated and extended in 1873 by Dr. David Ferrier, of London, and soon afterward by a small army of independent workers everywhere, prominent among whom were Franck and Pitres in France, Munck and Goltz in Germany, and Horsley and Schafer in England. The detailed results, naturally enough, were not at first all in harmony. Some observers, as Goltz, even denied the validity of the conclusions *in toto*. But a consensus of opinion, based on multitudes of experiments, soon placed the broad general facts for which Fritsch and Hitzig contended beyond controversy. It was found, indeed, that the cerebral centres of motor activities have not quite the finality at first ascribed to them by some observers, since it may often happen that after the destruction of a centre, with attending loss of function, there may be a gradual restoration of the lost function, proving that other centres have acquired the capacity to take the place of the one destroyed. There are limits to this capacity for substitution, however, and with this qualification the definiteness of the localization of motor functions in the cerebral cortex has become an accepted part of brain physiology.

Nor is such localization confined to motor centres. Later experiments, particularly of Ferrier and of Munck, proved that the centres of vision are equally restricted in their location, this time in the posterior lobes of the brain, and that hearing has likewise its local habitation. Indeed, there is every reason to believe that each form of primary sensation is based on impressions which mainly come to a definitely localized goal in the brain. But all this, be it understood, has no reference to the higher forms of intellection. All experiment has proved futile to localize these functions, except indeed to the extent of corroborating the familiar fact of their dependence upon the brain, and, somewhat problematically, upon the anterior lobes of the cerebrum in particular. But this is precisely what should be expected, for the clearer insight into the nature of mental processes makes it plain that in the main these alleged "faculties" are not in themselves localized. Thus, for example, the "faculty" of language is associated irrevocably with centres of vision, of hearing, and of muscular activity, to go no further, and only becomes possible through the association of these widely separated centres. The destruction of Broca's centre, as was early discovered, does not altogether deprive a patient of his knowledge of language. He may be totally unable to speak (though as to this there are all degrees of variation), and yet may comprehend what is said to him, and be able to read, think, and even write correctly. Thus it appears that Broca's centre is peculiarly bound up with the capacity for articulate speech, but is far enough from being the seat of the faculty of language in its entirety.

In a similar way, most of the supposed isolated "faculties" of higher intellection appear, upon clearer analysis, as complex aggregations of primary sensations, and hence necessarily dependent upon numerous and scattered centres. Some "faculties," as memory and volition, may be said in a sense to be primordial endowments of every nerve cell—even of every body cell. Indeed, an ultimate analysis relegates all intellection, in its primordial adumbrations, to every particle of living matter. But such refinements of analysis, after all, cannot hide the fact that certain forms of higher intellection involve a pretty definite collocation and elaboration of special sensations. Such specialization, indeed, seems a necessary accompaniment of mental evolution. That every such specialized function has its localized centres of co-ordination, of some such significance as the demonstrated centres of articulate speech, can hardly be in doubt—though this, be it understood, is an induction, not as yet a demonstration. In other words, there is every reason to believe that numerous "centres," in this restricted sense, exist in the brain that have as yet eluded the investigator. Indeed, the current conception regards the entire cerebral cortex as chiefly composed of centres of ultimate co-ordination of impressions, which in their cruder form are received by more primitive nervous tissues—the basal ganglia, the cerebellum, and medulla, and the spinal cord. This of course is equivalent to postulating the cerebral cortex as the exclusive seat of higher intellection. This proposition, however, to which a safe induction seems to lead, is far afield from the substantiation of the old conception of brain localization, which was based on faulty psychology, and equally faulty inductions from few premises. The details of Gall's system, as propounded by generations of his mostly unworthy followers, lie quite beyond the pale of scientific discussion. Yet, as I have said, a germ of truth was there—the idea of specialization of cerebral functions—and modern investigators have rescued that central conception from the phrenological rubbish heap in which its discoverer unfortunately left it buried.

IV.

The common ground of all these various lines of investigations of pathologist, anatomist, physiologist, physicist, and psychologist is, clearly, the central nervous system—the spinal cord and the brain. The importance of these structures as the foci of nervous and mental activities has been recognized more and more with each new accretion of knowledge, and the effort to fathom the secrets of their intimate structure has been unceasing. For the earlier students, only the crude methods of gross dissections and microscopical inspection were available. These could reveal something, but of course the inner secrets were for the keener insight of the microscopist alone. And even for him the task of investigation was far from facile, for the central nervous tissues are the most delicate and fragile, and on many accounts the most difficult of manipulation of any in the body.

Special methods, therefore, were needed for this essay, and brain histology has progressed by fitful impulses, each forward jet marking the introduction of some ingenious improvement of mechanical technique, which placed a new weapon in the hands of the investigators.

The very beginning was made in 1824 by Rolando, who first thought of cutting chemically hardened pieces of brain tissues into thin sections for microscopical examination — the basal structure upon which almost all the later advances have been conducted. Müller presently discovered that bichromate of potassium in solution makes the best of fluids for the preliminary preservation and hardening of the tissues. Stilling, in 1842, perfected the method by introducing the custom of cutting a series of consecutive sections of the same tissue, in order to trace nerve tracts and establish spacial relations. Then from time to time mechanical ingenuity added fresh details of improvement. It was found that pieces of hardened tissue of extreme delicacy can be made better subject to manipulation by being impregnated with collodion or celloidine, and embedded in paraffine. Latterly it has become usual to cut sections also from fresh tissues, unchanged by chemicals, by freezing them suddenly with vaporized ether, or, better, carbonic acid. By these methods, and with the aid of perfected microtomes, the worker of recent periods avails himself of sections of brain tissues of a tenuousness which the early investigators could not approach.

But more important even than the cutting of thin sections is the process of making the different parts of the section visible, one differentiated from another. The thin section, as the early workers examined it, was practically colorless, and even the crudest details of its structure were made out with extreme difficulty. Remak did, indeed, manage to discover that the brain tissue is cellular, as early as 1833, and Ehrenberg in the same year saw that it is also fibrillar, but beyond this no great advance was made until 1858, when a sudden impulse was received from a new process introduced by Gerlach. The process itself was most simple, consisting essentially of nothing more than the treatment of a microscopical section with a solution of carmine. But the result was wonderful, for when such a section was placed under the lens, it no longer appeared homogeneous. Sprinkled through its substance were seen irregular bodies that had taken on a beautiful color, while the matrix in which they were embedded remained unstained. In a word, the central nerve cell had sprung suddenly into clear view.

A most interesting body it proved, this nerve cell, or ganglion cell, as it came to be called. It was seen to be exceedingly minute in size, requiring high powers of the microscope to make it visible. It exists in almost infinite numbers, not, however, scattered at random through the brain and spinal cord. On the contrary, it is confined to those portions of the central nervous masses which to the naked eye appear gray in color, being altogether wanting in the white substance which makes up the chief mass of the brain. Even in the gray matter, though sometimes thickly distributed, the ganglion cells are never in actual contact one with another; they always lie embedded in intercellular tissues, which came to be known, following Virchow, as the neuroglia.

Each ganglion cell was seen to be irregular in contour, and to have jutting out from it two sets of minute fibres, one set relatively short, indefinitely numerous, and branching in every direction; the other set limited in number, sometimes even single, and starting out directly from the cell as if bent on a longer journey. The numerous filaments came to be known as protoplasmic processes; the other fibre was named, after its discoverer, the axis cylinder of Deiters. It was a natural inference, though not clearly demonstrable in the sections, that these filamentous processes are the connecting links between the different nerve cells, and also the channels of communication between nerve cells and the periphery of the body. The white substance of brain and cord, apparently, is made up of such connecting fibres, thus bringing the different ganglion cells everywhere into communication one with another.

In the attempt to trace the connecting nerve tracts through this white substance by either macroscopical or microscopical methods, most important aid is given by a method originated by Waller in 1852. Earlier than that, in 1839, Nasse had discovered that a severed nerve cord degenerates in its peripheral portions. Waller discovered that every nerve fibre, sensory or motor, has a nerve cell to or from which it leads, which dominates its nutrition, so that it can only retain its vitality while its connection with that cell is intact. Such cells he named trophic centres. Certain cells of the anterior part of the spinal cord, for example, are the trophic centres of the spinal motor nerves. Other trophic centres, governing nerve tracts in the spinal cord itself, are in the various regions of the brain. It occurred to Waller that by destroying such centres, or by severing the connection at various regions between a nervous tract and its trophic centre, sharply defined tracts could be made to degenerate, and their location could subsequently be accurately defined, as the degenerated tissues take on a changed aspect, both to macroscopical and microscopical observation. Recognition of this principle thus gave the experimenter a new weapon of great efficiency in tracing nervous connections. Moreover, the same principle has wide application in case of the human subject in disease, such as the lesion of nerve tracts or the destruction of centres by localized tumors, by embolisms, or by traumatisms.

All these various methods of anatomical examination combine to make the conclusion almost unavoidable that the central ganglion cells are the veritable "centres" of nervous activity to which so many other lines of research have pointed. The conclusion was strengthened by experiments of the students of motor localization, which showed that the veritable centres of their discovery lie, demonstrably, in the gray cortex of the brain, not in the white matter. But the full proof came from pathology. At the hands of a multitude of observers it was shown that in certain well-known diseases of the spinal cord, with resulting paralysis, it is the ganglion cells themselves that are found to be destroyed. Similarly, in the case of sufferers from chronic insanities, with marked dementia, the ganglion cells of the cortex of the brain are found to have undergone degeneration. The brains of paretics in particular show such degeneration, in striking correspondence with their mental decadence. The position of the ganglion cell as the ultimate centre of nervous activities was thus placed beyond dispute.

Meantime, general acceptance being given the histological scheme of Gerlach, according to which the mass of the white substance of the brain is a mesh-work of intercellular fibrils, a proximal idea seemed attainable of the way in which the ganglionic activities are correlated, and, through association, built up, so to speak, into the higher mental processes. Such a conception accorded beautifully with the ideas of the associationists, who had now become dominant in psychology. But one standing puzzle attended this otherwise satisfactory correlation of anatomical observations and psychic analyses. It was this: Since, according to the histologist, the intercellular fibres, along which impulses are conveyed, connect each brain cell, directly or indirectly, with every other brain cell in an endless mesh-work, how is it possible that various sets of cells may at times be shut off from one another? Such isolation must take place, for all normal ideation depends for its integrity quite as much upon the shutting out of the great mass of associations as upon the inclusion of certain other associations. For example, a student in solving a mathematical problem must for the moment become quite oblivious to the special associations that have to do with geography, natural history, and the like. But does histology give any clew to the way in which such isolation may be effected?

Attempts were made to find an answer through consideration of the very peculiar character of the blood-supply in the brain. Here, as nowhere else, the terminal twigs of the arteries are arranged in closed systems, not anastomosing freely with neighboring systems. Clearly, then, a restricted area of the brain may, through the controlling influence of the vaso-motor nerves, be flushed with arterial blood, while neighboring parts remain relatively anæmic. And since vital activities unquestionably depend in part upon the supply of arterial blood, this peculiar arrangement of the vascular mechanism may very properly be supposed to aid in the localized activities of the central nervous ganglia. But this explanation left much to be desired—in particular when it is recalled that all higher intellection must in all probability involve multitudes of widely scattered centres.

No better explanation was forth-coming, however, until the year 1889, when of a sudden the mystery was cleared away by a fresh discovery. Not long before this the Italian histologist, Dr. Camille Golgi, had discovered a method of impregnating hardened brain tissues with a solution of nitrate of silver, with the result of staining the nerve cells and their processes almost infinitely better than was possible by the method of Gerlach, or by any of the multiform methods that other workers had introduced. Now for the first time it became possible to trace the cellular prolongations definitely to their termini, for the finer fibrils had not been rendered visible by any previous method of treatment. Golgi himself proved that the set of fibrils known as protoplasmic prolongations terminate by free extremities, and have no direct connection with any cell save the one from which they spring. He showed also that the axis cylinders give off multitudes of lateral branches not hitherto suspected. But here he paused, missing the real import of the discovery of which he was hard on the track. It remained for the Spanish histologist, Dr. S. Ramon y Cajal, to follow up the investigation by means of an improved application of Golgi's method of staining, and to demonstrate that the axis cylinders, together with all their collateral branches, though sometimes extending to a great distance, yet finally terminate, like the other cell prolongations, in arborescent fibrils having free extremities. In a word, it was shown that each central nerve cell, with its fibrillar offshoots, is an isolated entity. Instead of being in physical connection with a multitude of other nerve cells, it has no direct physical connection with any other nerve cell whatever.

When Dr. Cajal announced his discovery, in 1889, his revolutionary claims not unnaturally amazed the mass of histologists. There were some few of them, however, who were not quite unprepared for the revelation; in particular His, who had half suspected the independence of the cells, because they seemed to develop from dissociated centres; and Forel, who based a similar suspicion on the fact that he had never been able actually to trace a fibre from one cell to another. These observers then came readily to repeat Cajal's experiments. So also did the veteran histologist Kölliker, and soon afterward all the leaders everywhere. The result was a practically unanimous confirmation of the Spanish histologist's claims, and within a few months after his announcements the old theory of union of nerve cells into an endless meshwork was completely discarded, and the theory of isolated nerve elements—the theory of neurons, as it came to be called —was fully established in its place.

As to how these isolated nerve cells functionate, Dr. Cajal gave the clew from the very first, and his explanation has met with universal approval.

In the modified view, the nerve cell retains its old position as the storehouse of nervous energy. Each of the filaments jutting out from the cell is held, as before, to be indeed a transmitter of impulses, but a transmitter that operates intermittently, like a telephone wire that is not always "connected," and, like that wire, the nerve fibril operates by contact and not by continuity. Under proper stimulation the ends of the fibrils reach out, come in contact with other end fibrils of other cells, and conduct their destined impulse. Again they retract, and communication ceases for the time between those particular cells. Meantime, by a different arrangement of the various conductors, different sets of cells are placed in communication, different associations of nervous impulses induced, different trains of thought engendered. Each fibril when retracted becomes a non-conductor, but when extended and in contact with another fibril, or with the body of another cell, it conducts its message as readily as a continuous filament could do—precisely as in the case of an electric wire.

This conception, founded on a most tangible anatomical basis, enables us to answer the question as to how ideas are isolated, and also, as Dr. Cajal points out, throws new light on many other mental processes. One can imagine, for example, by keeping in mind the flexible nerve prolongations, how new trains of thought may be engendered through novel associations of cells; how facility of thought or of action in certain directions is acquired through the habitual making of certain nerve-cell connections; how certain bits of knowledge may escape our memory, and refuse to be found for a time, because of a temporary incapacity of the nerve cells to make the proper connections; and so on indefinitely. If one likens each nerve cell to a central telephone-office, each of its filamentous prolongations to a telephone wire, he can imagine a striking analogy between the *modus operandi* of nervous processes and of the telephone system. The utility of new connections at the central office, the uselessness of the mechanism when the connections cannot be made, the "wires in use" that retard your message, perhaps even the crossing of wires, bringing you a jangle of sounds far different from what you desire—all these and a multiplicity of other things that will suggest themselves to every user of the telephone may be imagined as being almost ludicrously paralleled in the operations of the nervous mechanism. And that parallel, startling as it may seem, is not a mere futile imagining. It is sustained and rendered plausible by a sound substratum of knowledge of the anatomical conditions under which the central nervous mechanism exists, and in default of which, as pathology demontrates with no less certitude, its functionings are futile to produce the normal manifestations of higher intellection.

CONCERNING THE JEWS.

BY MARK TWAIN.

SOME months ago I published a magazine article* descriptive of a remarkable scene in the Imperial Parliament in Vienna. Since then I have received from Jews in America several letters of inquiry. They were difficult letters to answer, for they were not very definite. But at last I have received a definite one. It is from a lawyer, and he really asks the questions which the other writers probably believed they were asking. By help of this text I will do the best I can to publicly answer this correspondent, and also the others—at the same time apologizing for having failed to reply privately. The lawyer's letter reads as follows:

I have read "Stirring Times in Austria." One point in particular is of vital import to not a few thousand people, including myself, being a point about which I have often wanted to address a question to some disinterested person. The show of military force in the Austrian Parliament, which precipitated the riots, was not introduced by any Jew. No Jew was a member of that body. No Jewish question was involved in the Ausgleich or in the language proposition. No Jew was insulting anybody. In short, no Jew was doing any mischief toward anybody whatsoever. In fact, the Jews were the only ones of the nineteen different races in Austria which did not have a party—they are absolutely non-participants. Yet in your article you say that in the rioting which followed, all classes of people were unanimous only on one thing, viz., in being against the Jews. Now will you kindly tell me why, in your judgment, the Jews have thus ever been, and are even now, in these days of supposed intelligence, the butt of baseless, vicious animosities? I dare say that for centuries there has been no more quiet, undisturbing, and well-behaving citizens, as a class, than

* See *Harper's Magazine* for March, 1898.