

THE CURIOSITIES OF CREDULITY.

IN the last number of *Fraser's Magazine*, Mr. A. R. Wallace holds me up as "an example of what prepossession and blind scepticism can do for a man"; "how it makes a scientific man unscientific, a wise man foolish, and an honest man unjust."

The following historical narrative will serve, I think, as "an example of what prepossession and blind credulity can do for a man," and will further afford a very useful lesson as to the "fallacies of testimony" in regard to the class of subjects at present under discussion between Mr. Wallace and myself.

Every one who has attended to the history of animal magnetism knows full well that a belief in its higher pretensions not only prevailed extensively in France during the decade 1820-30, but took a very strong hold of the medical profession in that country, many of its most distinguished members giving their public attestation to the reality of those claims. Thus M. Rostan, one of the ablest medical psychologists of his day, contributed to the first edition (1825) of the 'Dictionnaire des Sciences Médicales' (of which he was one of the conductors) an article on "Magnétisme Animal," in which he detailed experiments carried on by himself and other eminent physicians, which had entirely satisfied them of the truth of clairvoyance. Another very able advocate of mesmerism during this epoch was M. Georget, a young physician of high reputation, and the author of a much esteemed treatise on the 'Physiology of the Nervous System.*' And a Commission appointed by the French Academy of Medicine in 1826 to inquire into the subject (of which Commission M. Husson, Physician to the Hôtel Dieu, was the reporter), reported in the same sense in 1831, its members bearing their personal testimony to the genuineness of phenomena which they had themselves witnessed and tested, and of which they considered that no reasonable doubt could be entertained.

The state of mind of these eminent men, therefore, in regard to mesmerism was thus exactly parallel, on the one hand, to that of the authorities of Salem (New England) in 1692 in regard to witchcraft, and, on the other, to the present attitude of Mr. Wallace and his associates in regard to spiritualism. On evidence which "hundreds of the most solemn people knew to be true," the Salemites hung scores of innocent people. And so, on evidence which Mr. Wallace and his friends know to be true, they brand as "arrogant" sceptics not only myself, but the great body of medical and scientific men of whose opinions on this subject I am the exponent, because, warned by the experience I am now relating, we decline to accept their testimony as binding on our own belief.

Our mental attitude, on the other hand, is that of the courageous sceptics of 1692, who, possessed by "the froward spirit of Sadduceeism," caused the release of 150 reputed witches, and the stoppage of proceedings against 200 more, in spite of the indignant protests of Dr. Cotton Mather and the "hundreds of most solemn people" who backed it up. And it is also that of the obstinate sceptics in the French Academy of Medicine forty-six years ago, who dared to question the authority of MM. Rostan and Georget, as well as of the eminent reporter and other members of its Commission; and who succeeded in preventing the Academic adoption of their Report, which was simply *enterré* in the archives of the Academy, as the expression of the opinion of the individuals composing that Commission.

Early in 1837, however, the Academic discussion was renewed; and this renewal elicited the following remarkable statement from M. Bousquet:—

* It so happened that my father, having broken down in health from over-work, was, during some months of 1826-27, under the medical care of MM. Rostan and Georget, the latter of whom told him that the evidence of the reality of spiritual existence afforded by clairvoyance had brought him back from a state of materialistic atheism,—exactly what a lady of high culture told me some twenty years ago in regard to spiritualism.

"Messieurs, tout le monde a la prétention de bien voir; tout le monde croit avoir bien vu; et vous savez combien un homme est fort, lorsqu'il peut dire, — 'J'ai vu.' C'est sans doute un grand avantage; toutefois l'illusion est à côté de la réalité. Georget croyait donc avoir bien vu; il y paraît assez à la manière dont il parle du magnétisme dans son ouvrage sur le système nerveux. Cependant, on sait aujourd'hui qu'il a été trompé par des misérables qui s'en vantent. Je tiens cela de M. Londe, le collaborateur de Georget, et le témoin de toutes ses expériences. Ainsi, Messieurs, Georget est mort plein de foi dans le magnétisme; son ouvrage reste, et l'auteur n'est pas là pour effacer les erreurs qu'il contient."—The circumstance referred to by Dr. Bousquet was a death-bed confession made by a female hospital patient, one of the principal subjects of MM. Rostan and Georget's experiments on clairvoyance; who declared that she and a confederate (who occupied the next bed) used to spend many delicious hours of their nights in chuckling over the deceits they had put on the doctors, and in contriving new ones for the next day. The effect of this disclosure upon the mind of M. Rostan (which I learned at the time through the private channel already referred to) is shown by the fact that when a second edition of the 'Dictionnaire de Médecine' came out in 1838, he withdrew the article he had contributed to the first, this being replaced by one from the pen of M. Calmeil (a physician of the highest repute in the same line), which went as strongly against the pretensions of animal magnetism, as Rostan's article of 1825 had gone in their favour.

At a subsequent sitting of the Academy, an earnest appeal was made to it by a young magnetizer, M. Berna, to enter anew upon a systematic investigation of the whole subject. "Ma croyance au magnétisme," he urged, "n'est point le fruit de l'enthousiasme ou d'un examen superficiel, mais de plusieurs années d'expériences et de méditation. . . . Je propose de faire voir, sur des personnes que j'ai actuellement à ma disposition, des faits concluants en faveur du magnétisme." Moved by the obvious sincerity of this appeal, and unwilling to hold back from inquiring into the facts which M. Berna professed himself fully prepared to substantiate, the Academy appointed a second Commission; which included MM. Roux, Bouillaud, Hippolyte Cloquet, Pelletier, and other distinguished members of its body, with M. Dubois (d'Amiens) as its reporter. This Commission reported, six months afterwards, that M. Berna had utterly failed to prove his case; the only *fait concluant* demonstrated being that he had been victimized by cunning cheats. Against this conclusion a protest was made by M. Husson, the reporter of the first Commission; but the report of M. Dubois was nevertheless almost unanimously adopted by the Academy. It was to meet the argument of M. Husson—that, although M. Berna's *clairvoyantes* had failed, other magnetizers might bring forward more "lucid" subjects—that M. Burdin offered his prize; and a third Commission was then appointed, for the special purpose of investigating the claims of clairvoyance. This third Commission included, with M. Husson, the reporter of the first, and M. Dubois, the reporter of the second, such acknowledged leaders of the medical profession as MM. Chomel, Louis, Double, and Moreau. It continued open to the investigation of all claims to the Burdin prize for a period of three years. It detected and exposed the trickery of the claimants who ventured to present themselves. And when, in 1840, it presented its Report, the Academy was so completely satisfied that the members of its first Commission had been (like the Salemites of 1692) "sadly deluded and mistaken," that it arrived at the determination thenceforth to regard all communications on the subject of animal magnetism as *non avenues*, having no more claims on its attention than claims to the discovery of "perpetual motion" or the "quadrature of the circle" would have upon that of the Academy of Sciences.

Now I ask what would be thought of the fair-

ness of a stanch Scripturalist, who should now quote, as valid testimony to the universality of the Noachian Deluge, the 'Reliquiæ Diluvianæ' of Dr. Buckland, whose fundamental doctrine was subsequently retracted by its author in his Bridgewater Treatise; or should accuse a scientific opponent either of culpable ignorance, or of intentional *suppressio veri*, in making no mention of a Report presented in favour of the same doctrine to a Scientific Society, which not only never adopted it, but, in the course of a few years, passed upon it the strongest possible sentence of condemnation? Yet this is exactly what Mr. Wallace has done in reviewing my 'Lectures' in Mr. Crookes's Journal, accusing me of "ignoring every particle of evidence which is too powerful to be explained away"; and citing, as conspicuous examples of one-sidedness, my silence as to M. Rostan's article and M. Husson's report. If time had permitted, I should have most gladly adduced in my 'Lectures' these very testimonies, as conspicuous examples of the extent to which the most able, but "prepossessed" men may be led away by cunning cheats,—M. Rostan, by his own confession; and the members of the first Commission, on the almost unanimous verdict of the French Academy of Medicine.

That animal magnetism is now, as in 1840, regarded by the highly-trained medical intelligence of France as a "dead letter," only worthy of attention as "a curiosity of history," which "points a moral" in regard to other like demands on human credulity, may be judged from the manner in which it is treated in one of the great medical dictionaries now in course of publication. The second section of the 'Dictionnaire Encyclopédique' contains a long and elaborate historical article on "Magnétisme Animal," from the pen of M. Dechambre, who has the reputation of being one of the ablest of French medical critics. After bringing down his history to 1840, M. Dechambre thus continues:—"Ici pourrait se terminer l'histoire analytique du Magnétisme Animal; car il ne se produira plus désormais, en France du moins, que des faits isolés, dépourvus de toute authenticité, et le plus souvent pour les besoins d'une misérable industrie." Further on, he says:—"Quant à toutes les propriétés et facultés extraordinaires dont on a doté les somnambules, et qu'il est inutile de rappeler, nous attendons sans impatience ni préoccupation qu'on en démontre mieux l'existence; et nous les considérons, jusqu'à nouvel ordre, comme un double produit de l'illusion et de la supercherie." And he sums up as follows:—"Comme ceux des effets que nous regardons comme possibles résultent d'une autre cause que l'influence d'un agent spécial dit Magnétisme, nous terminons par cette conclusion radicale: *le Magnétisme Animal n'existe pas.*"

In this condemnation M. Dechambre does not hesitate to include the Odyllism of Von Reichenbach, which Mr. Wallace (in his review of my 'Lectures') blames me for repudiating,—the reality of Reichenbach's experimental results having been attested by about sixty persons of repute in Vienna, including "a number of literary, official, and scientific men and their families"; and having been verified in this country by Prof. Gregory of Edinburgh, and by Dr. Ashburner in London. Now it so happened that I was assured at the time by the late Prof. Daubeny, of Oxford, who himself witnessed Von Reichenbach's experiments at Vienna, that nothing could be more loose and unscientific than the manner in which they were conducted; and the verdict of that very clear-sighted and trustworthy observer has been subsequently confirmed by the general *consensus* of the scientific and medical public of Germany, which (as I have been recently assured by my distinguished friend, Prof. Hofmann, of Berlin) would treat any attempt to rehabilitate Odyle (as it appears from M. Dechambre's testimony that it would be treated by the scientific and medical public of France) as simply *non avvenu*. And any one who is acquainted with the state of scientific and medical opinion in this country must be well aware, that any attempt to rehabilitate Odyle,

except on the basis of a new set of experiments, in which the old sources of fallacy should be carefully guarded against, would be utterly futile; neither the authority of Prof. Gregory in Edinburgh, nor that of Dr. Ashburner in London, having been considered by the scientific and medical contemporaries among whom they respectively lived, and to whom their qualifications for such an inquiry were well known, as of more account than that of Von Reichenbach himself.

I do not for a moment call in question the right of any one either to hold or to express his belief in clairvoyance and Odyllism. But I do protest against the right of such a one, either to call in question the candour and honesty of any other, who entertains an opinion as to the probative value of the evidence on these subjects that differs from his own; or to charge him with perverting the facts of history because his conclusions as to the untrustworthiness of that evidence are drawn from a survey of the *whole* of the history, and not from selected parts of it. WILLIAM B. CARPENTER.