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AN INQUIRY INTO THE REASONS WHY THE HORSE RARELY VOMITS.

BY

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THIS subject has, within the last two centuries, engaged the attention of numerous physiologists, the object of whose inquiries, however, has differed somewhat from ours; for the majority of them have been imbued with the prevalent opinion that the horse NEVER vomits. Thus Lamorier¹ addressed to the French Academy of Sciences Une mémoire où l'on donne les raisons pourquoi les chevaux ne vomissent point; and M. Flourens² recently published a paper on non-vomiting in the horse, heading it with the following dogma, "Le cheval ne vomit point; c'est là ce que chacun sait."

Were it not that M. Flourens is one of the most recent, as well as most renowned, writers on this subject, we should not deem it necessary to contradict this statement, because it has long been known that horses occasionally, though rarely, do vomit. True it is that, since as a general proposition we cannot assert that the horse does vomit, it might be supposed that the converse statement would hold good; and so it does; but only in a sense that is too vague to warrant its being adopted as the title of a vexed scientific question.

In the pursuit of our inquiry, under present circumstances, it being indispensable that we should clear the path from error before we can hope successfully to attain the truth, we shall, firstly, quote cases of horses having vomited; secondly, analyse the opinions which, at various periods, have been propounded on this subject; and, thirdly, adduce reasons to prove why vomiting in the horse is of rare occurrence.

I. CASES OF VOMITING IN THE HORSE. In relating the history of a horse affected with spasmodic colic, M. Charlot³ makes the following

¹ Histoire de l'Académie Royale des Sciences. Année 1733.

² Gazette Médicale de Paris. Année 1849, No. 5, 3 Février, p. 79-81.

³ Recueil de Médecine Vétérinaire, tome iv, p. 128.

statement: "Speedily, and before me, the animal gathered together its limbs, made an effort, by contracting the abdominal muscles, opened its mouth, and ejected, by the nostrils, a thick, bloody, fœtid liquid, of acid taste, and mingled with fragments of forage. The act of vomiting was repeated in less than half an hour. It gave rise to the evacuation of about two pints of liquid, which had been previously swallowed, and in which two strongyli were contained." M. Tombs¹ states that a bay mare, while on a journey, vomited a gallon and a half of indigested food. Though we do not agree with the explanation which he gives of the phenomenon, his statement of fact respecting it is so plain as to be irrefutable.

These two cases appear to be conclusive evidence to prove that M. Flourens, and many of his predecessors, have sought explanations for a supposed, and not real, fact; and, therefore, have at least incurred the danger which attends those who study nature with imaginations erroneously preoccupied.²

11. HISTORY OF THE SUBJECT. Were we to arrange the historical part of the subject chronologically, it might prove tedious; to avoid which, we shall class opinions according to the analogies they present.

1. While acknowledging that the stomach of the horse is situated at a distance from the muscular walls of the abdomen, M. Dupuy³ attributed, in a great measure, the difficulty of vomiting in that animal to powerful compression of the œsophagus by the muscular fasciculi of the right crus of the diaphragm, in the substance of which it passes. A similar reason was, indeed, hinted at long since by Peyer;⁴ but its fallacy is too apparent to need much comment, since, in the same manner as food passes freely down the œsophagus to the cardiac orifice, it may, without obstacle, retrace the same course.

2. A second class of writers have imagined the existence of a valvular apparatus at the cardiac orifice, destined to prevent the return of food into the œsophagus. Among them was Lamorier,⁶ who attributed the greater part of the horse's difficulty in vomiting to the impossibility of compression of the stomach by the abdominal walls and diaphragm, alleged that a crescentic valve was so arranged as partially to close the cardiac orifice, and prevent the return of coarse food through it. Dr. Gurlt⁶ has figured a spiral valve at the cardia; and Mr. Spooner,⁷

³ Journal de Physiologie, Experiment. et Path., par F. Magendie, tom. i., p. 338.

⁴ Merycologia sive de Ruminantibus et Ruminatione, p. 178.

5 Loc. cit.

⁶ Die Anatomie des Pferdes in 70 lithographirten Tafeln, von D. E. F. Gurlt, tabula 14, fig. 3, text p. 66.

⁷ Compendium of the Vet. Art. By James White, edited by Mr. W. C. Spooner, 1842, p. 66.

¹ Veterinarian, vol. xii, p. 797.

² In a report read by M. Mignon to the Société Centrale de Médecine Vétérinaire, (Bulletin de la Société, année 1847, tome deuxième, p. 191, et seq.), on a memoir by M. Valtat, on Vomiting in the Horse, many cases of its occurrence in that animal are related. We refer to this report as embodying much interesting information. For other cases of the same kind, see a paper by Professor Gellé, translated in the Veterinarian, vol. xii, p. 486, from La Zooiatre du Midi. See also Professor Rey, Veterinarian, vol. ii, p. 413. It is almost superfluous to observe, that we only cite cases which bear evidence of correctness. We take no notice of many of the cases in which horses are said to have vomited, because these cases are indefinitely related.

after observing that the cardiac orifice "has a sort of valve formed by the duplication of its membrane," adds, that "the œsophagus, just previous to its entering the stomach, makes an acute angle, by which means, in great measure, vomiting in the horse is almost entirely forbidden." It has not, however, been satisfactorily shewn that either of the forms of valve above alluded to, exists in the horse's stomach.

3. M. Colin,¹ after repeating Lamorier's observation, that the relative position of the stomach and large intestines protects the former from the direct pressure of the abdominal walls, correctly remarks that, at its terminal orifice, the walls of the œsophagus are very thick, and that the cavity in its interior is closed, the mucous membrane being folded like a radiated flower. To this arrangement, M. Colin attributes the impossibility, or extreme difficulty, of vomiting in horses; and he endeavours to justify his belief by applying to the stomach the theory of the hydraulic press. But such a line of argument is not justly applicable in the case of the stomach and œsophagus, which, as living and active organs, are not regulated solely by hydraulic laws.

4. A fourth class of observers have attributed the difficulty of vomiting in the horse, to the oblique insertion of the œsophagus into the stomach, and to a sphincter, which they allege to be formed at the cardia by the union of the muscular bands belonging to the stomach and œsophagus. This doctrine appears to have been first promulgated by Bertin,² who stated in proof of the existence of the cardiac sphincter, that the weight of a man did not suffice to expel water or air from the cardiac orifice of various stomachs, the duodenum being tied.³ Lafosse⁴ only took into account the sphincter at the cardia, whereas, of late, Berard⁵ and Rymer Jones⁶ have reiterated the conclusion of Bertin: and the former writer, moreover, remarks⁷ that the rhythmic movement which M. Magendie discovered in the lower third of the œsophagus, does not occur in the horse, whose œsophagus, he adds, is composed, in its last eight or ten inches, of a very elastic but inirritable tissue. Since, however, the microscope, no less than the naked eye, abundantly attests the true muscular nature of that tissue, M. Berard's denial of its irritability does not seem to be sufficiently warranted.

Bertin's theory has lately found a zealous advocate in M. Flourens,⁸ whose opinion on this question merits more careful study than do those of his predecessors, because he has based it on experiments which he affirms to be univocal and demonstrative of the obstacle to vomiting in the horse.

Having been interested by a perusal of M. Flourens' publication at an early period of my studentship, I at once repeated some of his

8 Loc. cit.

¹ Recueil de Médecine Vétérinaire pratique, III serie, to ne vi, p. 478.

² Histoire de l'Académie Royale des Sciences. Année 1746, p. 33-4.

³ Had Bertin reflected that sphincters perform their function by virtue of the vital power of contraction with which their constituent muscular fibres are endowed, and that therefore they are inactive after death, he would have sought other explanations for the fact he observed.

⁴ Dictionnaire Raisonné d'Hippiatrique, par M. Lafosse, vol. ii, p. 139.

⁵ Cours de Physiologie fait par P. Berard, vol. ii, p. 139.

⁶ Cyclopædia of Anatomy and Physiology : article Solipeda, p. 735.

⁷ Op. cit., vol. ii, p. 28.

experiments, and was surprised at the very different results which I obtained from them. A just appreciation of my position in relation to M. Flourens made me diffident, and caused me to repeat my observations and the study of his paper, at different times over a period of nearly three years. The result of this plan has been more clearly to establish the fallacy of M. Flourens' conclusions, and the mode by which he arrived at them. In justice to the originator, I prefix a translation of the account he gives of them to the results I have obtained, in order to afford full opportunity to every one to arrive at his own conclusions.

EXPERIMENT I. "The stomach being filled with water and the pylorus tied, the stomach was placed on a table, and on the stomach a board; two men then mounted on the board; they pressed it by jerks, and not a single drop of water escaped through the æsophagus.¹

"It was evident from this first experiment, that all the obstacle to the escape of water by the œsophagus was in the superior orifice of the stomach. This was rendered more evident by a second experiment.

EXPERIMENT II. "The stomach being filled with water and the pylorus tied, I introduced a metallic tube, about an inch long, through the œsophagus into the superior orifice. No sooner was the tube placed in the opening than water flowed through the œsophagus.

"The obstacle of the cardiac orifice having been surmounted, none other presented itself in the œsophagus.

"Hence the œsophagus, notwithstanding its strong circular muscle, takes no part in the phenomenon, as is even more clearly demonstrated by the following experiment.

EXPERIMENT III. "The stomach being filled with water and the pylorus tied, I cut, piece by piece, from the pharynx to the stomach, all, absolutely all, the œsophagus; the stomach was pressed, and not a drop of water escaped from it.

"The œsophagus having been removed, I was able to introduce my finger into the cardiac orifice, and I recognised two things; firstly, that the more the stomach was compressed, the more closely was my finger grasped; and secondly, that the oblique direction of the superior orifice greatly aided the phenomenon, for as soon as I rendered this orifice straight, water flowed.²

"Bourgelat believed that the principal cause of the not vomiting might be in the accumulated folds of the mucous membrane of the cardiac orifice. The following experiment proves that Bourgelat erred.

EXPERIMENT IV. "I made a large incision on the side of the stomach; I subsequently removed all the mucous membrane of the

¹ M. Flourens states that he repeated this experiment more than twenty times, and the result was always the same, except with one stomach, which he states was diseased, and describes as "percé de trous profonds tous remplis de vers." There can be no doubt it was a stomach containing bots, a condition perfectly consistent with health, inasmuch as in a considerable portion of healthy horses, bots inhabit the stomach.

² It is remarkable that this observation did not shake M. F.'s faith in all his results; had he connected that observation with the known fact that, as *muscles*, the stomach and œsophagus could alter their relative position, he must have seen that their vital endowments were an insurmountable barrier to the success of his purely mechanical experiments and arguments.

cardiac orifice, and even that of the œsophagus. I sewed up the opening made with the knife; I filled the stomach with water; I tied the pylorus; I recommenced compression of the stomach, and not a drop of water escaped.

"The third experiment limits the extent of the obstacle to vomit on the part of the œsophagus; here is one which limits it on the part of the stomach.

EXPERIMENT v. "I made a large incision on the side of the stomach, and I cut the two *lateral fasciculi* of the internal muscles, without however touching the internal fibres, which really are the sphincter.

"The stomach was then sewn up and filled with water, the pylorus was tied, and pressure recommenced. Not a drop of water escaped by the œsophagus.

"All these experiments are univocal; they all demonstrate: firstly, that the obstacle to the horse's vomiting is in the superior orifice of the stomach; secondly, that it is in that orifice alone; and thirdly, that it is there due, firstly, to that orifice having a sphincter, and secondly, to the direction of that orifice being *oblique*.

"Bertin had therefore suspected rightly, that the sphincter of the superior orifice of the stomach, and the oblique direction of that orifice, are the two causes why the horse does not vomit."

Such is M. Flourens' statement. My own experiments on twentytwo horses' stomachs¹ have given the following results.

A. Twelve stomachs (with from four to six inches of œsophagus attached to each) having been filled with water through the pylorus, and this aperture closed by ligatures, allowed that liquid to escape through the cardia when little or no pressure was exerted on them. In several, the water at once gravitated through the cardia; in others, a very slight manual pressure sufficed to force it through that passage. In all cases I have provided for the freedom of the cardiac orifice while pressure was applied to the surfaces of the stomach; this I have done by placing the viscus near the edge of the table, and allowing its cardiac end to project a little. M. Flourens states that he caused the stomach to be placed on a table, without giving any reason to suppose that he took the necessary precaution with reference to the cardia. By citing one of the above twelve experiments, it will be seen how likely the experimenter in such a case is to succeed in discovering the object of his wish, if he be not cautious in avoiding fallacies.

A horse's stomach, with about five inches of œsophagus, filled with water through the pylorus, and this secured, when placed near the edge of the table with the cardia free, allowed water to flow through it by mere gravitation; slight compression exerted on the organ sufficed to expel the water to a considerable distance. When I placed my open hand on the surface of the stomach and pressed forwards, the flow gradually diminished and eventually ceased, even though considerable pressure was applied. By directing the pressure forwards I directed the cardia downwards, and mechanically closed it: how likely, nay almost inevitable, must be a similar result for a similar

¹ M. Flourens states that he repeated his first experiment more than twenty times.

reason, when two men stand on a board placed over the stomach and press with jerks.

B. A stomach, which did not allow water to flow through the cardia when a weight of fifty-two pounds was placed on it, and another, which required the pressure of twenty pounds before the water escaped, allowed the fluid to flow when I applied very slight pressure to the cul-de-sac with my hands.

The result of this observation suggests another source of fallacy in M. Flourens' experiments. The force generated by the contraction of the stomach's muscular coat is distributed all over its superfices, whereas M. Flourens only applied vertical pressure to one aspect. It is easy to understand how the result obtained by his doing so, might have been readily altered by simultaneously compressing the extremities of the organ.

c. After filling a stomach with water through the pylorus, this orifice was closed by ligature, and the viscus placed on a table. By grasping the stomach with my two hands, and exerting very considerable pressure in different directions, no water escaped. I then placed the stomach near the edge of the table, and on it a board with fiftyfour pounds of iron; a little less weight than this sufficed to make the water dribble through the cardia; with this weight it flowed in a small but continuous stream. When I inclined the board backwards, so as to exert the greatest pressure on the great curvature of the stomach, the flow of water was freer than when I held the board horizontally; as I inclined it forwards the stream gradually diminished, and eventually stopped. Through an opening made at the great curvature I removed the mucous lining of the œsophagus, and that in the immediate neighbourhood of the cardia. On pouring water through the artificial opening, it escaped through the cardiac orifice in a large stream by mere gravity.

D. Four stomachs, in the same circumstances as the above, did not give exit to the contained fluid, though grasped and firmly pressed by the hands of two persons; but the water escaped freely from all, when the lining of the œsophagus and around the cardia was removed through an incised aperture made at the great curvature.

E. A stomach, treated and placed as usual, did not allow water to escape through the cardia, when a boy mounted on a board was placed over it; but the water flowed when a man, weighing one hundred and forty-four pounds, took the boy's place.

F. One stomach, in the same condition as the preceding, did not allow the escape of any water through the cardia when a man, weighing one hundred and fifty-seven pounds, mounted on a board which had been placed horizontally over it. When a boy, weighing sixty and a half pounds was added, water flowed in a continuous stream, and the viscus burst.

G. One stomach bore the weight of two men, without any water flowing through the cardiac orifice; on making an opening into the stomach, I found the mucous membrane at the cardia very much folded.

The experiments A, B, C, D, warrant us in denying the existence of a sphincter at the horse's cardia.¹ It would have been more satis-

¹ I performed careful experiments on three asses' stomachs, the results of which precisely agreed with these.

factory had the experiments E, F, G, been completed. Nevertheless the fact that the results most closely agreed with M. Flourens when, through inexperience, I conducted them with the least caution, may not be without weight in the establishment of the truth. When once it has been proved that, owing to its laxity, the thick mucous membrane forms folds at the dead horse's cardia, which more or less completely occlude it, it is easy to understand how—according to whether the stomach be contracted or dilated at the time of death—the degree of obstruction should vary. No application can fairly be made of the possibility of this obstruction in the dead horse's stomach to the physiology of that viscus during life; because the very fact of the mucus lining of a strong muscular tube being much folded, proves that its bore is susceptible of being much dilated, provided the stimulus to dilatation be communicated to the muscular structure.

Though we believe that the citation of further evidence on this point is not essential to the cause of truth, it may not be quite useless to oppose to M. Flourens the opinion published in 1847 by one of his distinguished countrymen. In M. Mignon's report, already quoted, we find the following passage. "The reporter of your commission, when in charge of the anatomical works at the Alfort school, has many times proved, while passing a current of water through the first portions of the alimentary canal, that it was extremely easy to make the water introduced into the stomach through the duodenum escape through the œsophagus; it was sufficient for this purpose to fix a stop-cock firmly and hermetically in the duodenum. The water on entering the stomach, first filled it, and visibly distended it; then dilated the cardia into a kind of funnel; finally forced its way through that orifice, and flowed outwards."¹

A somewhat less erroneous, because less exclusive, theory than any of the preceding has been advocated by Girard.² After having described the arrangement of the muscular fibres at the horse's cardia, he states, that he regards that as the principal cause of the inaptitude of monodactyles to vomit; as accessories to it, the peculiar mode of insertion of the œsophagus, the shape and position of the stomach.³ We have already confuted the evidence which has been adduced by Bertin and others, to prove that the obstacle to vomiting resides in the muscular fibres of the cardia. As to the influence of the peculiar mode of insertion of the œsophagus, and the shape of the stomach, we need only remark, that since both these conditions are susceptible of alteration by the muscular activity of those organs, no arguments, based upon the assumption of their being invariable, can be accepted as conclusive. To the position of the stomach, (near the spine, and

¹ Loc. cit.

² Traité d'Anatomie Vétérinaire, par J. Girard. Quatrième edition, 1841. Tome deuxième, p. 569.

³ HALLER, (*Elem. Physiol.*, edit. Lausanne, tom. vi, p. 291), states, that the narrowness and oblique insertion of the œsophagus, and the debility of the horse's abdominal muscles and diaphragm, are the causes of its inability to vomit. Haller's error is greater than it appears; for his words, "et ob musculorum abdominis atque diaphragmatis debilitatem", refer to the *physical weakness* of those structures, as proved by his adducing in proof of his statement the authority of Lamorier, (*loc. cit.*), who, having observed that rupture of the diaphragm is frequent in the horse, refers it to the *weakness* of that muscle,—an explanation which is certainly not borne out by observation.

separated from the floor of the abdomen by the enormous large intestines), even I, at one time, attributed a great part of the horse's difficulty to vomit. It occurred to me, however, that the act of parturition in the mare is one of great rapidity; that abdominal respiration during disease or severe exercise is, in the horse, very easily effected; in the performance of these functions, and in the voidance of urine and fæces, the abdominal muscles take a very active part; and yet the uterus, bladder, and rectum are as disadvantageously placed, with reference to the floor of the abdomen, as is the stomach; the fact is, that the abdomen being completely full, pressure is transmitted very effectively from its muscular walls to the contained organs.¹

Different as are the opinions we have hitherto commented upon, they yet present one remarkable point of analogy, inasmuch as all their authors attributed the horse's difficulty to vomit to a mechanical obstacle. It affords me pleasure gratefully to acknowledge, that for not falling into the same error I am indebted to Dr. Sharpey, who gave me an allimportant hint by suggesting an inquiry into the action of emetics on the horse. It at once occurred to me, that as the mechanical part of the act of vomiting is excited by a reflex stimulus from the nervous centre, it behoved those who undertook to demonstrate why the horse rarely vomits, to study two classes of phenomena-the nervous and the mechanical:---for it is quite obvious that if the stimulus to the expulsive effort be wanting, it is useless to attribute the impossibility of the evacuation of the stomach by the cosophagus to mechanical obstacles, for they have no opportunity of coming into operation. Moreover, since I have excluded the existence of any mechanical impediments to vomiting in the horse, it is evident that the question which forms the subject of my inquiry, can alone be solved by determining what is the action of emetics on the nervous system of the horse.

It is since I received and worked upon Dr. Sharpey's hint, that I have carefully studied M. Mignon's report, and have found that it occurred to him and his collaborators, that the efforts of those who had studied the subject as pure mechanists could but prove abortive. "Is not the stomach of the horse that vomits," asks the reporter, "in conditions which no experiment would reproduce? And do you give no consideration to the nervous element, which you forget to regard as one of the data of the problem?" As a sketch of their theory of vomiting in the horse, MM. Mignon, etc., state "that the physical and vital conditions of vomiting, in their orders of succession and importance, are the following :—

"1. Extreme distension of the stomach.

"2. Disappearance of the folds in the œsophagean mucous membrane, accompanied by dilatation of the cardia into a kind of funnel.

"3. Paralysis of the muscular coat.

"4. Energetic concurrence of the nervous force, and of the action of the expiratory muscles of the abdominal walls."

As to the first of these propositions, on which the reporters lay the

¹ The above paragraph had been written several weeks before I read the following passage. "Il nous suffira de dire que dans l'abdomen ou aucun vide n'existe, l'action des muscles du bas ventre doit se transmettre sans beaucoup de perte à tous les organes contenus dans la grande cavité splanchenique." (Mignon, loc. cit., p. 251.)

greatest stress,¹ it may suffice to say that it is an unfounded assumption.² We grant that frequently undue repletion of the stomach is the stimulus which, through the medium of the nervous centre, brings about the reflex movements, that end in evacuation of its contents by the œsophagus; but such repletion of the viscus is not essential to the perfect manifestation of the reflex acts in question. This is abundantly testified by the sufferers on a sea voyage, in whom the act of vomiting, frequently commencing when the stomach is empty, and continuing at other times after its evacuation, must primarily be due to a nervous cause alone.

The purport of the second proposition is best given in the terms of the report : "The disappearance of the folds in the mucous membrane at the cardia, and the dilatation of this part into a kind of funnel, are a necessary consequence of the eccentric and excessive distension of the stomach. The power that separates the sides of the viscus from each other is as great at the cardia as elsewhere ; and if in the first moments of repletion of the gastric pouch the œsophagean sphincter resists, it no longer does so when the repletion is excessive. The walls of the stomach, by the mere fact of their distension, carry with them the two bands of the sphincter, the rectilinear direction of which they interrupt ; and then these fleshy bands, far from enclosing the cardia, as between the branches of a compass, merely surround it as a section of a funnel. Thus it is that the œsophagean orifice is transformed into a wide, open, and round orifice.

The extreme dilatation of the stomach, by destroying at the cardia, and that in a purely physical manner, the obstacle which is opposed to the escape of matters through the œsophagus, must therefore be regarded as the principal condition of vomiting. This is accordingly confirmed by experiment. The experiment alluded to, is the one quoted at p. 555, from which M. Mignon discovered that if water were poured through the duodenum into the stomach, this first became distended, then the cardia opened and allowed the fluid to escape.

We confess that we were not a little surprised, when we first reflected that those who thus mechanically and positively argued from experiments on dead horses' stomachs, for the predetermination of vital phenomena, have in the same paper loudly inveighed against the

¹ "Nous ne prétendons pas avoir tout expliqué, tant s'en faut ; nous n'avons voulu établir qu'une seule chose, à savoir : que la dilatation excessive de l'estomac est la condition par excellence du vomissement dans le cheval, en raison du changement tout physique qu'elle produit dans la forme du cardia et de la conséquence physiologique, c'est-à-dire de la paralysie qu'elle amène dans la tunique contractile de l'estomac."

² That the extreme distension alluded to in the report, is not of that sudden kind which Magendie, (loc. cit., p. 13), noticed in the stomach of dogs about to vomit, as being produced by the deglutition of air, but its gradual distension by food, may be inferred from the terms of the report. "On se demande comment dans un aussi court instant que l'espace de quelques heures, par exemple, l'estomac peut, sans se rupturer, se distendre jusqu'à tripler et au delà sa capacité normale. La masse intérieure qui la produit n'est point tout à coup volumineuse ; c'est d'une manière insensible qu'elle se gonfie en quelque sort; l'action dilatante qu'elle exerce sur l'estomac, est donc lente et graduée. C'est ainsi que nous concevons que l'estomac soit parvenu à se dilater au point de contenir 35 livres de liquide ainsi que le rapporte M. Renault."

system of determining the functions of living parts, from the results of observations on dead ones.¹

Admitting the possibility of extreme distension of the horse's stomach, we deny that it in any degree has a mechanical tendency to open the cardia during life. Let any one look at a horse's small stomach, placed beside the enormous quantity of food that animal is capable of devouring in a short time, and he will be forced to believe that, in perfect health, the organ is susceptible of great dilatation, without either involving opening of the cardia, or impairment of its vital power of contraction. To argue that because one part of a hollow viscus is dilated, the other must be so likewise, is in opposition to known facts. Moreover, my own experiments on dead horses' stomachs, have shown me that not always is extreme distension of the viscus with water followed by escape of the fluid through the cardia; it is or is not, according to the presence or absence of mechanical obstructions. The opening of the cardia, no less than its closure, is, in the living animal, doubtlessly owing to vital contraction of muscular fibres.

The reporters found their third proposition on the general truth that extreme dilatation of a hollow muscle is attended with impairment of its contractile property; thus they make the third a necessary sequence of the first proposition. Since we have impugned the latter, the former, in our opinion, falls to the ground. In asserting that before a horse can vomit, the muscular coat of the stomach must be paralysed, Mignon, in main part, re-echoed the theory of Renault, who several years previously had announced, that not only was the muscular action of the stomach useless, but opposed to vomiting, which only occurred when that viscus became excessively distended.² My own experiments, conducted for the purpose of ascertaining the effect of injecting a solution of tartar emetic into the veins of the horse, force me to look on those of M. Renault with great doubt; for I have yet to learn that an experimenter has the power of exciting at will efforts to vomit in the horse; and what I have seen leads me strongly to suspect, that when after injecting tartar emetic into the veins of a horse, and cutting open the abdomen, observers have indulged in the belief that they were witnessing efforts to vomit, they have only witnessed the violent efforts made by tortured brutes in self-defence.

The theory advocated by Renault and Mignon, is, as the latter admits, founded on the doctrine first promulgated by Bayle and Chirac, and of late rendered notorious by the singular experiments with which M. Magendie defended it. The French hippiatrists, in fact, having exclusively studied the mechanical part of the act of vomiting, were forced to decide between the opposite doctrines, defended by Haller and Magendie, as to the participation of the stomach in the performance of that act. The result has been, the declaration of their adhesion

² Bulletin Soc. Centr. de Méd. Vétér., tome ii, p. 218, 219.

¹ On a selon nous grand tort de conclure du resultat d'expériences d'amphithéâtres trop souvent imparfaites et sur des organes morts, à ce qui doit se passer au milieu des parties que la vie anime. Ne serait ce pas une pretention trop étrange, que vouloir surprendre, à l'aide de moyens si grossiers, si limités, si incomplets, si matériels, le secret de ces phénoménes si insaisissables, si changeants et surtout si merveilleux de simplicité, qui se produisent au sein de toute organisation vivante! (Mignon's Report, loc. cit., p. 197.)

to their illustrious countryman; a result which would in all probability have been different, had they rigidly tested the validity of the claims of his doctrine,¹ and have taken due notice of the positive observations of Wepfer, Haller, and Rudolphi, and of the recorded cases in which vomiting occurred when extrinsic pressure was removed from the stomach, either by palsy or destruction of the abdominal muscles.

That the energetic concurrence of the nervous power, and of the action of the expiratory muscles of the abdominal wall, takes part in the act of vomiting, as stated in M. Mignon's fourth proposition, all will grant; but that it is fourth in the order of succession we deny, for it is well known that purely nervous phenomena are the first signs of a disposition to vomit; and that it is fourth in the order of importance we deny, on the ground of the experimental truth, that, whereas the condition of the stomach is unimportant, a participation of the nervous system is indispensable, before the movements necessary to vomiting can be effected. So unimportant, indeed, is the condition of the stomach, that vomiting may occur without it, as proved by M. Magendie's experiment of substituting a pig's bladder for a dog's stomach; and it is not a little surprising that the reporter, who seems to have laid so much stress on it, should not have discovered that his idea of "excessive dilatation of the stomach being the condition, par excellence, of vomiting in the horse", was opposed to the result of an experiment which he invoked in defence of his views.

Now that we have subverted three out of the four propositions on which Mignon's theory is founded, we should be at a loss to guess what might be his idea as to the reasons why the horse rarely vomits, if, in a discussion which ensued on his report,² he had not unequivocally avowed assent to Girard's *mechanical* doctrine, the fallacy of which we have already demonstrated.

Thus we have completed a critical and historical account of the opinions emitted on the subject; and, having proved that all observers have attributed the rarity of vomiting in the horse to mechanical causes which do not exist, it remains for us to determine whether there are any impediments to it in the nervous system of the horse. With this object in view, let us inquire into the action of emetics in the horse.

III. WHAT IS THE ACTION OF EMETICS ON THE HORSE? It is so well known among veterinary pathologists, that the horse is not acted on by emetics as is the dog, that, while they frequently prescribe them in the diseases of the latter, they never do so in those of the former. The most celebrated writers on veterinary therapeutics, generally exclude emetics from the list of medicines available for the relief of the diseases of the horse, and refuse to the emetic, *par excel*-

¹ Sound critics have long since decided that Magendie's conclusions on vomiting are not warranted by his experiments, which, at utmost, prove that vomiting may occur by the extrinsic pressure of the diaphragm and abdominal muscles, without the active co-operation of the stomach. They certainly do not disprove, that under ordinary circumstances, the stomach aids in the ejection of its contents by the esophagus.

² Les vraies raisons, les seules bonnes et importantes raisons de l'impossibilité du vomissement, indiqués et décrites d'abord par Bertin, ont été parfaitement développés par M. Girard, dans son mémoire de 1810, et surtout dans sa notice de 1841.

lence,—the potassio-tartrate of antimony,—any emetic virtue in the equine species, when administered internally.¹ Were this the sum total of our information on the point at issue, our inquiry would be at an end; but since it has been alleged by several French experimentalists, that the horse makes efforts to vomit when potassio-tartrate of antimony is injected into its veins, it is imperative that we should test the empirical grounds of such allegation.

We find it stated by M. Dupuy that he has succeeded in producing attempts to vomit in the horse, by injecting tartar emetic into the crural or jugular veins, in doses varying from six grains² to two drachms.³ The director and professor of the Veterinary School at Lyons, in their account⁴ of the action of tartarized antimony, thus express themselves : "Injected into the veins of the large herbivora, in doses of from nine to eighteen grains, tartar emetic occasions vomiting, or the phenomena which accompany that act of evacuation." They give no evidence to prove this statement.

Finally, we have to quote an experiment which was performed by MM. Leblanc and Mignon:⁵ "Thirty-six grains of tartar emetic injected into one of the jugular veins of a horse,—a longitudinal opening about seven inches long, made on the side of the linea alba,—exploration of the stomach with the finger,—contractions of the viscus insensible,—contraction of the abdominal muscles alternating with that of the diaphragm, the latter taking place during inspiration; at this moment the right crus of the diaphragm is forcibly extended, but the œsophagus is only moderately compressed between its fleshy lips, the intestines forcibly escape through the wound in the abdomen,—

1 "In the horse vomition cannot be excited." Introductory Lecture on Chemistry, by Mr. Morton, Veterinarian, vol. xiv, p. 767. Le cheval et nos herbivores ruminants sont des animaux chez lesquels nos émétiques sont sans puissance. Bouchardat, Formulaire Vétérinaire, p. 258. Of the action of potassio-tartrate of antimony in the horse, Mr. Percival says, "I give it in doses of a drachm and two drachms...Of little palpable efficacy given internally." Percival, On Form and Action in the Horse and the Effects of Medicine in Horses, p. 32. Nimrod (Veterin. vol. x, p. 415) states that the potassio-tartrate of antimony has always been considered among sportsmen as a cleanser of a foul habit in horses, in doses of from one to three drachms. In a discussion which took place at the Royal Academy of Medicine of Paris on the subject of vomiting, M. Renault thus expressed himself (Bulletin Soc. Centr. Vétér. tom. ii, p. 219). "I made a crucial incision into the abdomen of horses to which I had previously given tartar emetic. On introducing the hand into the wound, I felt the stomach forcibly contracting, and its contractions became visible on its being made to protrude through the abdominal walls. These contractions did not cease until the stomach was ex-cessively dilated, and then only did vomiting occur. I concluded from these experiments that the muscular coat is an obstacle to vomiting." After the undeniable proof we possess of the potassio-tartrate of antimony not acting as an emetic when administered to the horse, the result of M. Renault's experiments affords much surprise, which is raised into incredulity on reflecting that the stomach could not be pressed upon by the abdominal walls, because crucially divided. That they were not reunited by suture, is evident from his statement that he continued to observe the state of the stomach. It would appear fair to argue, that if a stomach were itself inactive, and not compressed by extrinsic force, its contents could not be evacuated.

² Veterinarian, vol. iii, p. 529.

³ Ibid. viii, p. 575.

4 Dictionnaire Général de Médecine et de Chirurgie Vétérinaires, et des sciences qui s'y rattachent, par MM. Lecoy, Rey, Tisserant, Tabourin.

⁵ Bulletin cit. p. 242.

eight minutes after this injection of tartar emetic, appearance of some nausea, or violent and simultaneous contraction of lower abdominal muscles and diaphragm. The whole intestine is removed in order to see the stomach well; the viscus follows, like a pendulum, the alternate movements of contraction and relaxation of the diaphragm; the stomach, which is tolerably distended, is the seat of slow, continuous contractions, which always commence at the same point, the pylorus. The contraction and relaxation of the lower abdominal muscles and diaphragm take place gradually and slowly. This partition yields little by little, but uninterruptedly, to the abdominal muscles; these act similarly with regard to the diaphragm, so that if we form a material idea of these respiratory powers, we can compare them to two machines always equidistant, and directed in the same course, each describing the half of a double cone, whose limited base of reciprocal excursion would be in the middle. When one of them is at the summit, the other is at the base, and vice versa; consequently, the action of the diaphragm increases in proportion as that of the abdominal muscles diminishes. These forces are therefore essentially respiratory. The one-the diaphragm-is active during inspiration ; the other-the force generated by the abdominal muscles-is expiratory. Renewed, sudden, violent, and simultaneous contractions of the diaphragm and abdominal muscles occur; the latter even appear to augment their action in proportion as the diaphragm relaxes; it is the nausea, or the manifestation of the effort. No vomiting has occurred; finally, the animal expires."

From this account it appears that, on seeing violent efforts of the abdominal muscles and diaphragm of a horse, into whose veins tartar emetic had been injected, and from whose abdomen all the intestines had been removed through a large opening, MM. Leblanc and Mignon inferred that the emetic was producing its specific action by exciting efforts to evacuate the stomach through the œsophagus. No vomiting occurred, and therefore the experiment, primá facie, establishes,firstly, that the horse makes efforts to vomit when potassio-tartrate of antimony is injected into the veins; secondly, that as those efforts are not followed by vomiting, there must be some mechanical impediment to the escape of matters through the cardia. Both these conclusions are erroneous; and they are so for the following reasons. The description given of the movements of the abdominal muscles and diaphragm, is not such as to prove beyond doubt that they were efforts to vomit. Confessing adhesion to the doctrine that the diaphragm takes an active part in vomiting, we deny that it does so by virtue of an inspiratory movement, and that, as it relaxes, the abdominal muscles make a sudden effort, as in forcible expiration. The act of vomiting is one sui generis, and not a respiratory act.¹ Let any one try to simu-

¹ Since the terms in which M. Mignon has related his experiment may leave some doubt as to his idea of the nature of the movements in vomiting, it may be cleared up by his ulterior exposition of that idea. "Ce phenomène (le vomissement) commence aux derniers termes de *l'inspiration* pour finir dans *l'expiration*. C'est dans ce dernier temps ou pendant le relâchement du diaphragme qu'a lieu le rejet des matières sans aucun doute; mais c'est dans l'inspiration, ou par le fait d'une violente et brusque contraction du diaphragm, aidé de l'action des muscles abdominaux, que l'estomac verse son trop-plein dans l'œsophage."—Op. cit., p. 244.

late the effort to vomit, or study the real effort in his own person, and he will feel that the glottis having been closed after a deep inspiration, the diaphragm and abdominal muscles act simultaneously, so as to press the stomach between them. To this it may be objected that, for the diaphragm to descend while the glottis is closed, the air in the chest should be rarefied, which is an unlikely occurrence. The fallacy of this objection is demonstrable by experiment, as Dr. Sharpey has long since shown in his lectures on anatomy and physiology in University College. A bandage being closely passed round the lower part of the chest, it is easy to press down the diaphragm at will; on doing so, the bandage becomes loose, evidently owing to drawing in of the cartilages of the lower ribs by the descending diaphragm. Thus the augmentation of the thoracic cavity in its long axis, is compensated by its decrease in the transverse; and it becomes obvious that the diaphragm may descend when the glottis is closed, without enlarging the chest, and therefore without rarifying the contained air. That, however, the witnessed efforts were in reality the pangs of death, is rendered more than probable by the terms in which the narrative of the experiment ends, -" enfin, l'animal expire." 1

But conceding, for the sake of argument, that the movements observed were efforts to vomit, we maintain that it is not just to conclude that, because they were not followed by vomiting, there must have been some mechanical obstacle to the escape of matters through the cardia. As the experiment was performed, it was impossible for the stomach to be pressed upon by either the abdominal muscles or diaphragm, and therefore there was no chance of its contents being evacuated through the œsophagus. Protected, as the horse's stomach is, by the far-back projecting ribs, it can only receive pressure from the abdominal muscles, indirectly, through the intestines. When these are removed, the viscus cannot be pressed upon by those muscles, and the pressure of the diaphragm on it is very slight, because the stomach undulates like a pendulum, backwards and forwards, in the empty cavity, without meeting with the least resistance.

The evidence which we have adduced, and which is all that we have been able to collect, in favour of the doctrine that efforts to vomit can be excited in the horse by injecting tartar emetic into its veins, may be thus briefly summed up :—

1. M. Lecoq and his colleagues (the veterinary professors of Lyons) have expressed an opinion in the affirmative, without publishing the grounds of their belief.

2. The experiment of MM. Leblanc and Mignon, which professes to prove the doctrine, appears to have been conducted with so little regard to the exclusion of sources of error, that it would be unwarrantable to draw any positive inferences from it.

3. As we are not acquainted with the details of the experiments which formed the basis of M. Dupuy's statement in support of the above doctrine, they need confirmation; a need which becomes im-

¹ As the performance of such experiments in horses renders it indispensable that they should be thrown on the ground and secured, great caution should be observed in interpreting the muscular efforts; for when a horse is cast he struggles violently for liberty, even before a bold stroke of the knife has made a gash seven inches long into its belly, and all the bowels have been removed.

perative, on reflection that the horse is not susceptible of emesis by the introduction of any medicinal substance into the stomach, and that as our knowledge of the action of emetics in the dog proves, that when introduced into the veins or into the stomach their effects differ in degree and not in kind, there is strong ground for the belief that the horse is unsusceptible of the specific action of emetics, even when directly injected into its circulating system.

In order to settle the question, I determined to appeal to experiment, and procured for the purpose a horse and a mule, both of sound constitution. I have injected into their jugular veins solutions of tartar emetic, in 5, 30, and 50 grain doses, but have never seen efforts to vomit; to avoid misunderstanding I may say, that I have never seen any such thing as the animals thus experimented on take a deep inspiration, fix the chest, and make sudden and forcible exertion with the abdominal muscles. Inasmuch, however, as I noticed some preternatural phenomena of muscular action in two of the experiments, a detailed account of them is rendered necessary. Twenty minutes after injecting into the left jugular vein of a horse thirty grains of tartar emetic, dissolved in three ounces of water, the muscles generally became rigid; but there was no movement caused by any of them. It was static, not dynamic contraction. After this, I noticed a good deal of twitching of the muscles of the fore and hind limbs, and of those of the lower part of the neck ; but the abdominal muscles acted evenly, though with more frequency than usual, as the respiration became a good deal accelerated. At the end of three hours the twitching had almost ceased, and had done so completely; and the animal had resumed eating in a little less than four hours.¹ Lest it should be feared that I misconstrued the phenomena of muscular contraction, I may say that almost every five minutes, and sometimes oftener, I took notes of the state of the animal; and did not note such words as spasm or effort, but avoided the danger of erring in judgment by describing in periphrases all I saw. It is important to notice that in the same animal, on a subsequent day, I injected 30 grains of tartar emetic in $1\frac{1}{2}$ oz. of water, without producing any sensible effect whatever. I injected 50 grains of tartar emetic, dissolved in 1¹/₃ oz. of tepid water, into the jugular of a mule, with the following result. Fourteen minutes after the injection, the animal did not present any change; but, in a few minutes more, left off eating and stood still. In the ensuing thirty-seven minutes, the muscular system was several times subject to momentary rigidity, which, with the exception of slight elevation and extension of the head, was unaccompanied by any movement. This muscular rigidity simulated the tetanic state, and contrasted strongly with the state of the muscles of the same animal while voiding fæces and urine; in an hour and fourteen minutes the mule re-

¹ A few minutes after I had injected the tartar emetic, the animal commenced purging, and continued doing so for an hour and a half. To determine whether this phenomenon was due to the tartar emetic or to the water, I injected three ounces of tepid water into the jugular vein of a mule—it had no effect; but purging was occasioned in fourteen minutes by injecting into the same vein, after the lapse of an hour, six ounces of tepid water. In none of my subsequent experiments was this purging produced,—a fact which needs further experience for its explanation.

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sumed eating, and presented no other symptoms. If I had been a little less cautious in the avoidance of fallacy, it is very likely I should, in observing these muscular phenomena in two of the experiments, have succeeded in seeing attempts to vomit; but they assuredly were not such, for they did not in any degree resemble the efforts made by the same animals to evacuate the bladder and rectum, or the efforts made by the dog and man to empty the stomach through the œsophagus.

Considering the vague manner in which M. Dupuy alludes to his experiments, and, on the other hand, confident of the fairness with which my own have been conducted and recounted, I feel myself justified in opposing my conclusions to his, and in stating that all the attempts hitherto made to excite efforts to vomit in the horse by emetics have failed. This unsusceptibility to emetic action, and the very rare manifestation of the phenomena of vomiting by the horse, must obviously be regarded as cause and effect, and, consequently, as the answer to the question, Why does the horse rarely vomit?

7th February, 1852.



