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A

CONTRIBUTION

TO

COMPARATIVE PATHOLOGY,

BEING

A FURTHER INQUIRY INTO THE REASONS WHY THE HORSE RARELY VOMITS.

BY

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A CONTRIBUTION TO COMPARATIVE PATHOLOGY, &c.

The disputed question amongst physiologists concerning the act of vomiting in the horse, having been one of the objects of my earlier experimental inquiries, I published the results in the *London Monthly Journal of Medicine* in February, 1852. The late Mr. Percivall honoured me by transferring my memoir to the pages of this periodical; but through some curious coincidence, the printer altogether omitted reproduction of the foot notes, several of which contained facts and reflections directly bearing upon, and materially affecting the reading of the text. I thus laboured under the disadvantage of being misinterpreted by the readers of this Journal, and by the members of several learned societies on the Continent, who chanced to refer to the maimed edition of my 'Inquiry,' and on it to found observations, the fallacy of which would, I presume to think, have been apparent and hence avoided; if the physiological considerations I had developed, had not been in part accidentally suppressed.

Under these circumstances, I propose making an abstract of my first 'Inquiry,' commenting upon it according to the results of subsequent reflection, and appending an analysis of two elaborate memoirs which have recently been published on this subject by my friends Ercolani and Vella, of Turin, and by M. Colin, of Alfort. I shall conclude with such suggestions for further physiological and clinical inquiry as seem calculated to develop interesting and useful truths; and with a view to aid those who may feel disposed to go deeply into the question, a bibliographical notice shall be appended.

Previously to 1852, the question, why does the horse *not* vomit, had been the theme of discussion, and thus the certainty of erring was involved in seeking a solution to a question wrongly stated; for there can be no doubt, as a respectable number of recorded cases proves, that the horse does occasionally, though rarely, vomit. The cause of this rarity was the object of my research, which I commenced by thus analysing the doctrines previously advanced.

1st. Dupuy in great measure attributed the difficulty of vomiting in the horse to powerful compression of the œsophagus by the muscular fasciculi of the right diaphragmatic crus; but in the same manner as food passes freely down the œsophagus, no obstacle exists to its return so far as the aperture in the diaphragm is concerned.

2dly. Lamorier and Gurlt referred to cardiac valves the impossibility of food passing back into the œsophagus from the stomach; but neither the crescentic valve of the former, nor the spiral valve of the latter, exists in nature.

3dly. I referred to M. Colin's attempt to solve the question by applying to the horse's stomach the theory of the hydraulic press; and I remarked that "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." I shall presently have again to refer to M. Colin's analogical explanation, since he has repeated it in his treatise on 'Comparative Physiology,' and sought to prove it by arguments and experiments equally founded, in my opinion, on insufficient basis, and employed in the solution of a physiological question with inadequate appreciation of vital forces and conditions.

4thly. Bertin's doctrine of a cardiac sphincter came under my consideration, together with the experiments and arguments with which it was sought to be definitively established by the perpetual secretary of the French Academy. Bertin stated, that when a horse's stomach was removed from the body, filled with air or water, and tied at the duodenum, the weight of a man did not suffice to expel the contents from the cardiac orifice. M. Fluorens repeated the same experiment twenty times, and found that even the jerking pressure of two men did not make a single drop of water escape through the œsophagus. M. Fluorens devised and executed other experiments, by which he was led to the conclusion that Bertin had with perfect correctness referred the reason why the horse does not vomit, to the sphincter formed by the muscular fibres at the horse's cardia, and to the oblique direction of that orifice. However remarkable it may appear, that such observers as Bertin and Fluorens should at the distance of a century fall into the same error on matters of very simple experiment, there can be no question that they did so with the dead horses' stomachs referred to. The notes of a lengthened series of experiments published in my first 'Inquiry,' disprove the notion that the horse's cardia is, even after death, guarded by a barrier insurmountable to regurgitant fluids. But assuming, for the sake of argument, that when a dead horse's stomach is filled with fluid, and closed by ligature at the duodenum, the cardia remains hermetically closed, however forcibly the viscus be compressed, it is matter of surprise that MM. Bertin and Fluorens should not have perceived the fallacy of referring the phenomenon to a cardiac sphincter, and of regarding the oblique insertion of the œsophagus as an accessory impediment during life to the act of vomiting. When, in his third experiment, M. Fluorens found that by mechanically altering the direction of the cardiac orifice, he influenced the outflow of the stomach's contents, he should have lost all faith in his arguments. Had he connected that observation with the fact that, as *muscles*, the stomach and œsophagus have during life the power of altering their relative direction, he must have seen that their vital endowments were an insurmountable barrier to the success of his purely mechanical experiments and arguments. Assuming that water did not flow out of the cardia of the distended and compressed dead stomach, Bertin and Fluorens would have avoided the error of referring the phenomenon to sphincteric action, had they reflected on the post-mortem condition of the lips, the anus, the vulva, and the urinary bladder, which, tight and closed in life, are flaccid and open after death; because sphincters are essentially instruments of life, owing their functional activity to the vital endowment of their constituent muscular fibre, powerless after death.

The fifth doctrine confuted in my first 'Inquiry' was Girard's, a compound of Bertin's and Lamorier's. Girard also attached great weight to the arrangement of the muscular fibres at the horse's cardia, and to the peculiar mode of insertion of the œsophagus—conditions discussed in the foregoing paragraph. Furthermore, he regarded the position of the stomach near the spine, and separated from the floor of the abdomen by the intestines, as an obstacle to its being sufficiently compressed to reject its contents. But even this objection is frail; the act of parturition in the mare is one of great rapidity; 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 relatively as disadvantageously placed as the stomach with reference to the floor of the abdomen; the fact is, that the abdomen being completely full, pressure is transmitted very effectively from its muscular walls to the contained organs.

Having thus examined and disproved the existence of the anatomical conditions which were said mechanically to impede regurgitant evacuation of the horse's stomach, I submitted 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 œsophagus to mechanical obstacles, for they have no opportunity of coming into operation. Accordingly, I directed my inquiries to the question, What is the action of emetics in the horse? and after noting the fact that in general practice they are never employed, because of the general impression that they are wholly inoperative, I proceeded to analyse the experiments instituted for the purpose of determining the effect of injecting tartar emetic into the horse's veins by Dupuy, Renault, Leblanc, and Mignon. The conclusion to which this inquiry led me, was thus expressed: "there is strong ground for the belief that the horse is unsusceptible of the specific action of emetics, even when directly injected into the circulatory system." In order to settle the question I determined to appeal to experiment, and injected into the jugular veins of a horse and mule of sound constitution, various watery solutions containing from five to fifty grains of the potassio-tartrate of antimony, but without ever witnessing efforts to vomit; whereupon I thus concluded the memoir: "I feel myself justified 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?"

While discussing this question some months afterwards at Stuttgart, with the learned Hering, he made me acquainted with the results of experiments by Viborg, of Copenhagen, who was stated to have succeeded in producing efforts to vomit in horses, by the injection of tincture of white hellebore into the jugular vein. Moreover, Professor Hering courteously offered to repeat the experiments in my presence in the veterinary school under his direction, an offer which I embraced with delight. In less than two minutes after injection of a drachm of the said tincture into the jugular, the horse became restless and covered with profuse sweat; viscid saliva flowed in large quantity, the pulse became small, the muscles of the neck spasmodically contracted, and those of the abdomen rigid; the latter were, however, much less affected than the former. These symptoms gradually disappeared without the manifestation of any others, and in about an hour the horse had regained his pristine condition. The experiment was repeated with a similar result.

The impression produced on my mind by the just-quoted experiments was, that the injection of white hellebore produced greater and more speedy nausea, and more action of the cervical and abdominal muscles than I previously believed any agent could produce; and, while the phenomena of muscular contraction certainly did not amount to the violent muscular phenomena of the act of vomiting as witnessed in the dog and man, yet it was proved that the horse was not wholly unsusceptible of the nervous influence which is known to precede the act of vomiting; and that once that influence had been produced, some very remarkable phenomena of muscular contraction ensued. Subsequent observation and reflection has confirmed that impression, the result of which is an admission that when I stated in 1852 that "all the attempts previously made to excite efforts to vomit in the horse by emetics had failed," I should have been nearer the truth had I been less general, and substituted the name *tartarized antimony* for the generic expression *emetics*. I at once communicated this criticism of my own opinion to Professors Ercolani and Vella, the secretaries to the Biological Society of Turin, before which learned body my first 'Inquiry' had been discussed. After commenting in most generous and encouraging terms on my anxiety to discover the truth, those gentlemen made my communication of the experiments with hellebore, the basis of an inquiry, the results of which I shall narrate as succinctly as is consistent with a clear statement of fact.

Ercolani and Vella invited Dr. Waller to perform on the horse, before the Piedmontese Biological Society, the experiments which he had successfully performed at Bonn, on dogs and frogs, of inducing vomiting by reflex movements, excited by galvanizing the superior extremity of a divided vagus nerve. The result was not considered conclusive, and a new and modified trial was about to be made, when the receipt of my communication from Vienna led to the adoption of a new plan of experiment, with the combined injection of hellebore, and galvanization of the distal end of the cut *par vagum*. Unfortunately, my learned friends at Turin proceeded to the new inquiry with an extreme, and in my opinion unwarranted, appreciation of Viborg's and Hering's experiments, which they held to prove that the horse is really susceptible to emesis without vomiting taking place, wherefore the obstacles to the performance of that act must be essentially mechanical. I shall subsequently develop the reasons why I consider these conclusions based on insufficient evidence.

All the phenomena which I had witnessed after the hellebore injection, were manifested when the experiment was repeated by Ercolani and Vella, who additionally report having observed violent contractions of the abdominal muscles, synchronous with spasms of the pharynx and convulsive opening of the mouth. In another experiment, the injection of three drachms of tincture of hellebore did not suffice to produce the violent abdominal action, but this became manifest on galvanizing the distal extremity of a previously divided vagus nerve. Comparing these symptoms with those reported to have been present in the rare cases in which horses have vomited, the experimenters concluded that the emetic action of the *Veratrum album* on the horse's nervous system was proved beyond doubt. In pursuance of their inquiry those gentlemen injected the tincture of the white hellebore in the jugular vein of a dog, producing extremely violent vomiting; and they performed a similar experiment on the rabbit (an animal which, like the horse, is held not to vomit), and observed violent diaphragmatic and abdominal movements, and opening of the mouth as if to vomit, but not doing so; facts which were admitted as proof that in animals habituated to vomit, such as the dog, injection of white hellabore into the veins produces the reverted evacuation of the stomach's contents through the mouth; but that no such evacuation occurred, though the necessary muscular efforts were excited, by injecting the same substance into the venous system of animals such as the horse and rabbit, commonly regarded as unsusceptible of emesis.

Without giving any weight to the great practical fact that veterinary therapeutics as specially applied to the horse, altogether exclude emetics, because inoperative; without taking into consideration the futile experimental attempts to excite efforts to vomit by injecting into the veins, the emetic, *par excellence*, tartarized antimony; without reflecting on the extraordinary means to which they had to resort to excite efforts to vomit in horses subjected to experiment; Ercolani and Vella concluded that the horse's nervous system is susceptible of emesis, and that in the domain of mechanical impediment was to be sought the reason why the act of vomiting is in that animal so rare, a conclusion which, with the greatest deference for my learned friends, I cannot but characterise as exclusive, inasmuch as it is a statement of a general proposition on the basis of an extraordinary fact, manifested under peculiar circumstances, and opposed to the results of larger experience obtained in conditions much more natural, and more closely according with comparative physiological and therapeutic observation.

In quest of the *mechanical impediment*, the physiologists of Turin more especially addressed themselves to inquire into the circumstances which led so distinguished an anatomist as Gurlt to affirm the existence of a cardiac valve, a statement to which subsequent observers have almost unanimously denied the real attributes of fact. On compressing a distended horse's stomach, to which about four inches of œsophagus had been left attached, Ercolani and Vella observed that the thick and rugous mucous membrane protruded from the œsophagean orifice, wherefore they entertained suspicion that Gurlt's valve might really originate in the distended stomach, in consequence of the fissure of a fold in the thick and loose cuticular portion of the gastric lining. Their suspicion acquired the strength of demonstrated truth after the following experiment. An aperture having been made in the large curvature of a horse's stomach, a glass plate was fixed to the margins so as to allow an observer to see what occurred at the cardia when the distended stomach was compressed. On perceiving that the folds of the mucous membrane at the cardia became so numerous and close as completely to close the orifice, they, after a few other experimental observations of secondary import concluded that the obstacle to vomiting in the horse is purely mechanical, and principally due to the insuperable impediment offered to regurgitant gastric evacuation by the large and numerous folds into which the lining of the stomach is thrown, whenever the distended viscus is compressed. The thick muscular coat was held to act as an auxiliary impediment, by preventing the expansion of the larger and thicker internal lining.

For the purpose of brevity and clearness I have not gone into some of the details reported by the experimenters, particularly as I see no reason for entering into a minute analysis of the steps of their inquiry, fallacious as it is in its spirit and foundation. Many as are the reflections suggested by the numerous controversies to which the theme of our inquiry has led during the last 120 years, none is so curious as that which inspires surprise at seeing so many men, habituated to observation and experiment, directly contradicting each other in the simplest matters of fact, and making use of arguments so partial and exclusive, as to be destitute even of *a priori* value in the explanation of vital phenomena, and to admit of complete refutation by comprehensive observation and logical interpretation of physical laws and vital conditions.

The fact that therapeutics, as applied to the horse, exclude emetics, that the great majority of experimenters have agreed upon the inoperativeness of emetic substances even when injected into that animal's veins, did not for an instant suggest to Ercolani and Vella that the horse is really very much less susceptible to the nervous impression of emetics than the animals which vomit, and that whatever its physical inability to the performance of that act, that was as a rule secondary to the vital insusceptibility. On the contrary, so soon as they believed that they had finally excited in the horse efforts to vomit, under the most extraordinary circumstances, and by the most powerful means, they thus argued: the horse is susceptible of the nervous impression of emetics, the horse does not vomit, therefore the obstacle must be mechanical. How certainly and completely does an assumption contrary to fact lead to error. To state as a general proposition that the horse is susceptible of the nervous impression of emetics, is to enunciate an assumption contrary to fact. To argue on that fallacious assumption as if it were a demonstrated truth, must lead to error. It is remarkable that it did not occur to the Turinese physiologists that the very fact of the cuticular coat at the cardiac end of the stomach being very loose and easily thrown into numerous folds, appears a provision to allow of the rapid and extensive dilatation of the thick muscular investment; and that, consequently, supposing the stimulus to cardiac dilatation to be conveyed to the muscular coat, the very condition which they regard as an obstruction would permit the opening of a capacious channel. When they pressed the dead stomach, and saw the dead, thick, and loose cardiac lining, fold and plug the aperture, they did not take into consideration the fact that in the act of vomiting in the dog, compression by the abdominal muscle and diaphragm is but one of the reflex muscular movements conducive to the rejection of the contents of the stomach; another is the anti-peristaltic movement of the viscus and of the œsophagus, whereby the cardia is opened and the stomach's contents collected near it, so as to be suddenly jerked out by the violent extrinsic compression. The observations of Wepfer, Haller, Béclard, and Legallois, prove these propositions, and it can no longer be matter of question, that the nervous impression of emetics is reflected to the stomach, œsophagus, and pharynx, no less than to the diaphragm and abdominal walls; and that, consequently, any attempt to explain inability to vomit, which, like Ercolani's and Vella's altogether ignores the great indisposition to emetic action, and assumes the stomach and œsophagus to be dead and inert, is inconsistent with the true solution of the problem.

We have finally to examine the teaching of M. Colin, as propounded in his very elaborate physiological treatise. After avowing that "Bertin was in the truth when he regarded the sphincter at the cardiac orifice as the essential obstacle to vomiting in the horse," M. Colin admits that my observations as to the insensibility of that animal to emetics is full of justice, and that it had not been made by the experimentalists who preceded me. But this admission did not prevent him reverting, in 1854, to his old explanation according to hydraulic laws, which I had combated in 1852. The assumption necessary to the application of this theory, that the cardiac sphincter is permanently constricted, is a gratuitous one; and moreover, to the stomach and œsophagus, as living and active organs, cannot be applied the explanation of purely physical phenomena enunciated by hydraulic law. Nevertheless, our author believes the doctrine which I contest, so rational, and so perfectly in accordance with the conditions of the stomach, as not to require experimental demonstration; yet he searched for it, and found it. As to the alleged rationality of the doctrine, I repeat it is not reasonable to assume, before the fact is proved, that the cardiac muscular fibres are *permanently* constricted. All analogy and physiological knowledge suggest as reasonable that these muscular fibres arranged in alternate circular, spiral, and longitudinal layers, can close or enlarge the aperture they surround, according to the nature of the stimulus they receive; it is certain the aperture is often largely opened for natural purposes, and it remains to be determined by experience whether the opening of the cardia, which is one of the movements concurrent to the act of vomiting, takes place in the horse. This question must be solved by experience, to which M. Colin has appealed, conclusively so in his opinion, without result in mine; and that because he has performed his experiments under such conditions, that their results are not applicable to the question under consideration. Thus he argued that the muscular closure of the cardia is the reason why the stomach cannot evacuate its contents through the œsophagus, from the three following experiments. Firstly, in a horse just fed, he cut through the linea alba and duodenum; through the pylorus he introduced his finger into the stomach, and found that orifice partially open, its border at intervals gently compressing the finger. He then opened the stomach at the large curvature, and found the cardia quite closed; on pushing his finger into the orifice it was forcibly constricted. In a second experiment, performed on a horse that had been feeding for several hours and had just drunk half a pail of water, the anterior flexure of the colon was displaced so as to lay bare the greatly distended stomach which was forcibly compressed with the hands in various directions, without anything escaping through the cardia. The viscus was only partially evacuated, by its contents flowing slowly towards the intestine. "I repeated," to translate M. Colin's words, "this experiment in other horses placed in the same conditions. In one instance, the compression was so powerful as to tear through the muscular coat at the right end of the great curvature. The results were always the same. If the contents of the stomach were fluid, they partially passed into the duodenum; if they were comparatively dry, they became clogged, and only passed into the gut in small proportions. The obstacle is, therefore, at the cardia." I can simply regard these experiments as furthermore substantiating the very well-known fact, that during the early part of the process of digestion the cardiac is *quite*, and the pyloric orifice *almost*, closed. I cannot see how these results, obtained by such violent experiment during the process of healthy digestion, can in any way apply to solve the question of vomiting. The fact that very violent compression only sufficed partially to evacuate the stomach through the pylorus, even when the contents were fluid, might have suggested to M. Colin that it was precisely as logical to say that the most forcible pressure did not suffice thoroughly to evacuate the fluid contents of a living horse's stomach through the duodenum, as it was to say that such pressure altogether failed to press the contents of it through the cardia. M. Colin was arguing on the functional activity of a healthy stomach and its orifices, after having exposed and handled that viscus in such a manner as certainly to impair, possibly utterly to neutralize the operation of that function. The normal peristaltic movement of the stomach, under the healthy stimulus, suffices thoroughly to empty the contents of the horse's stomach into the gut; and absolute and comparative observation discloses no condition in the structure of the stomach which can oppose reverted evacuation of its contents through the cardia, provided the abnormal stimulus come into operation, which in other animals is known to excite anti-peristaltic movement of the stomach and œsophagus, and aperture of the intermediate passage, in addition to violent efforts of the diaphragm and abdominal muscles.

Our author performed many other experiments, with a view to defend the position he had taken; but they are, for the purpose required, amenable to the same criticism as, with much deference, I have ventured to pronounce on the preceding; and when M. Colin states, *if he be not deceived*, those experiments demonstrate that the obstable to vomiting in solipedes resides in the cardiac sphincter, I believe *he was deceived* in the reality of his demonstration.

But exclusive as M. Colin represents himself in devising, indicating, and arguing on the experiments above referred to, he is essentially eclective when exercising the functions of a didactic writer; and accordingly we find him sum up by teaching, that the constriction of the cardiac sphincter, and of the thick muscular lower end of the œsophages are the chief impediments to vomiting in the horse; but that we must regard as auxiliary impediments, the smallness of the stomach, its distance from the abdominal wall, its not being subject to great distension under ordinary circumstances, the short period during which alimentary matters remain in it, and finally, the slight degree in which the viscus is susceptible to the exciting causes of vomition; in exposition of the last statement, M. Colin grants that the introduction of tartar emetic into the digestive organs neither provokes efforts to vomit, nor nausea; that those phenomena are but rarely and


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