24 hours, whilst in an equal quantity of any other substance there are only four or five grains of carbon that could be safely exhibited, and which would be *inefficient* in quantity to counteract by its preventive powers the introduction of infectious miasma into the constitution.

I am, Sir, your obedient servant,
MEDICUS.

18th July, 1832.

REPLY OF MR. COSTELLO TO THE
LETTERS PUBLISHED IN DEFENCE
OF BARON HEURTELOUP'S NEW
MODE OF BREAKING STONE IN
THE BLADDER.

To the Editor of THE LANCET.

SIR,—In transmitting to you for publication in your valuable journal, a translation of my two letters to the President of the Academy of Sciences, I thought I had placed the object I had in view, namely, scientific and useful discussion, in such a clear light, that my motives could neither be misconstrued nor misrepresented. It is not my fault that it is so, and I sincerely regret it. In those letters I declared, as I do now, that I only sought for truth. I have no feeling, no object, in common with those who are interested "in making the worse appear the better reason." I have said then, as I repeat now, "Prove to me the value of your proposal, and I will freely acknowledge it;—show me the justness of your pretensions, and I will recognise them." There was surely nothing in this which could have caused it to be looked on as an improper demand. The truth must have been arrived at by the discussion into which it naturally led. There was nothing to call for a display of petulance or bad temper.

I am sorry that it was deemed advisable to assume so acrimonious a tone in replying to what are called my calumnies. No respectable member of our profession can applaud such a course. Respectful, is quite as effective as intemperate, language, to maintain the truth of our opinions. Both from feeling and habit I am averse to the use of harsh words, and I will not, therefore, allow myself to be betrayed into an imitation of the tone or spirit which breathes in some of the justificatory and reprobatory letters addressed to me in this week's LANCET, in behalf of M. Heurteloup. Angry controversy I will have nothing to do with, if I can avoid it. The discussion which sinks into vulgar abuse, and which employs no better arguments than offensive personali-

ties, I cannot condescend to prolong. It is at once foreign to and beneath my object. With these observations, it would, perhaps, be right to discuss these letters altogether, and I am sure every respectable member of the profession would approve of this course. Still, the fear that some well-meaning persons might form an erroneous opinion of me from these misrepresentations, induces me to take the trouble of replying to them.

Before I come to the letters, I shall take leave to introduce a few remarks. Since the first establishment of lithotritry as a surgical operation by my colleague M. CIVIALE, many persons (and I say it without disrespect) have laboured for an easily-acquired celebrity in connexion with it, by alterations or additions made in the construction of the instruments. No person has toiled more sedulously for this purpose than M. Heurteloup. In the infancy of lithotritry, *brise-pierres* or stone-breakers, with two branches, were proposed, and sometimes used. M. Heurteloup produced one of these. Four-branched pincers were also used; of these M. Heurteloup produced a modification. He also proposed moveable or complex drills, the parent idea of which, with various models, was already in existence; and, lastly, the curved percussor, copied from Weiss's, or even an older instrument. Now be it remembered, these instruments followed each other in regular succession, and as regularly were they laid aside. The appearance of any one of them was always deemed a legitimate opportunity for some oblique reflections on Civiale's instruments. Despite of the good Civiale's did, and are still doing, they were always on those occasions sure to be reviled. Each of these instruments in its turn was lauded as the "*ne plus ultra*" of surgical power in lithotritry, and in its turn was supplanted by another specimen, borrowed, or indigenous of this author's fecundity. It has long been my intention to offer to the profession a reasoned critique on the mechanism and action of this system of instruments. I shall now do so with as little delay as possible. With regard to the curved percussor, I have some remarks to offer, which shall be communicated in a separate paper.

The first of those letters is from Mr. Edwards. It may be disposed of in a few words. It commences by an allusion to the postscript of a paper published in THE LANCET a little time before. This postscript was merely a propitiatory version of the mishap which occurred through the use of the percussor. In a note, he says, "It was a first essay, and therefore little better than a model" (strange admission). Since then, he adds, "Sixty-two have been made, each more perfect than its predecessor; and the weight of the hammer being determined,

and the probable resistance of the stone calculated, the whole strength of a man may be applied in striking, without any fear of the beak giving way." Mr. Edwards, it will be perceived, has no mean opinion of man's credulity.

A certificate from Messrs. Brodie and Hume is given with this letter. It is unnecessary to say anything more respecting this certificate, than that it should be placed at the head of my second letter to the Academy. It will be found that they say precisely the same thing.

Next is a letter from Mr. Goodwin, in which this gentleman very quietly accuses me of that of which he is himself guilty—namely, of making statements not founded in fact, and to which, in his regard for truth, he says he feels bound to offer a positive contradiction. Let us put this gentleman's love of truth to the test: he states, "that having heard that there was a safer mode of cure than the use of the knife, practised by two gentlemen in the profession, both strangers to me, I inquired about it, and was recommended by a friend* to Mr. Costello. Upon this circumstance being known to Mr. Brodie and Mr. Morrat, my medical attendants, they both, for reasons best known to themselves, withdrew, and I was left alone waiting the return of Mr. Costello to town."

There is an obliquity about this sentence evidently designed to favour the inference that Mr. Brodie would not meet me in consultation, from which an honourable mind recoils. What then must we think of Mr. Goodwin's statements, when this first of them is unjustifiable either in spirit or fact? I felt it my duty, on reading this paragraph, to call on Mr. Brodie. I pointed out the phrase, he read the whole letter. He said "I am at a loss to conceive how such an unwarrantable use could be made of my name. I saw Mr. Goodwin sometime before he sent for you. I told him what I had heard on former occasions, both from yourself and M. Heurteiou, that where there was paralysis of the bladder, it was not a favourable case for your operation. This I mentioned to him, and I heard nothing more about it till long after you had seen him. I saw him afterwards with M. Heurteiou, upon the invitation of Mr. Goodwin, which I should as readily have done with you, had I been invited. I will not be the partisan of any person." He added, "This seems to distress your mind, and you are to blame for it. I should take no notice whatever of these attacks, and you will find out, by-and-by, that they cannot in the slightest degree affect your

success in your profession." When I experience such kindness and receive such advice from a gentleman so deservedly distinguished in science, I can well abate the anger which I may have first felt at Mr. Goodwin's ungenerous insinuation.

Mr. Goodwin states, that neither he nor Mr. Hodges ever saw any portions of stone voided—in fact, they believe that I never even felt the stone. Then, let me ask, what was it that was detached from within the bladder, and voided by the sound? What was it we caused to be dried on paper? And what was it that induced me to give him a hope of being relieved without the intervention of any serious operation, if it was not this sabulous or chalky matter which was detached and voided? It would have been singular, indeed, that I should have proposed to use Hunter's forceps to dislodge the stone, which, by the way, would have been as effective for that purpose as the percussor, if I had never felt the stone, and ascertained its position.

Mr. Goodwin further states that three sittings of a quarter of an hour each or more, and not two of five minutes, did take place. Out of thy own mouth shalt thou be convicted. The following is copied from a letter from Mr. Goodwin to me, after I had ceased to see him.

"Dear Sir,—Having twice borne the operation of the lithotrite, I trust I have given you a fair opportunity of exercising your skill with success, if mine was a case which came within the range of your instrument; but as I cannot bring myself to think so, or to undergo another trial, I take leave to say that I must decline doing so. I remain, dear Sir, &c.,

"GEORGE GOODWIN.

"Aug. 31."

Now perhaps Mr. Goodwin will be able to recollect which of us is right as to the number of attempts. What would Mr. Goodwin think of four unsuccessful attempts in one case, and five in another, which M. Heurteiou acknowledges that he made? With respect to the quarter of an hour's duration, I have only to observe that both M. Civiale and myself make it a rule never to prolong the sounding or lithotrising beyond from three to seven or eight minutes; we advise postponing the operation, rather than the bladder should be too much fatigued by sounding; for the state of the organ should always be respected. Now I have performed lithotrity in private practice as well as in public hospitals in England, before some thousand surgeons and others, and I defy any one to say that I have acted against this maxim in a single instance. I cannot admit, even for Mr. Goodwin's love of truth, that I departed from it in his case.

Mr. Goodwin is quite deceived, when he says I proposed to call in M. Civiale only.

* Mr. Bowdery, of Oxford Street, on whom I operated two years ago.—W. B. C.

When Mr. Goodwin declined this, I mentioned M. Heurteloup, and begged him to understand that I had no objection to his consulting him, that I wished not to be considered as standing in the way of his doing so.

At the second attempt, I proposed by means of Hunter's forceps to dislodge the stone, which I distinctly felt in the situation described, beneath the neck of the bladder, and from which it could not be removed, although the bladder contained a pint of injected water, and the position of the pelvis was raised, and varied from side to side. I left him to make up his mind on this matter; some days after, I received from him the above-mentioned letter.

Mr. Goodwin's gratitude to M. Heurteloup is a very proper feeling, I leave him to indulge it; his love of truth is quite another thing, and as it concerns me, I have taken the liberty of setting him right.

The next letter is from Mr. Briggs. It would be a waste of time to consider any communication on a subject of surgical science, emanating from a young gentleman, who has but just entered the profession. To animadvert seriously on anything which he pretends to utter of his own knowledge, would be breaking a fly upon the wheel. But this young gentleman is merely the cat's-paw in this matter; of this, there is sufficient internal evidence in the letter bearing his signature.

Before I come to the statements contained in this letter, I shall say a few words on his English. He begins, "The letter inserted by Mr. Costello in last week's LANCET, being evidently not written in a scientific point of view." As well might this unfledged controversialist talk of a letter written in a sun-beam; and again with risible solemnity, he says, "As Baron Heurteloup's pupil, which great advantage has enabled me to become acquainted with all that relates to his operations, &c." Make out who can the antecedent to which? it may be *Baron*, or *pupil*, or *As*, aspirated of course. But as I prefer an antagonist more worthy of me, I shall put you aside, Mr. Briggs, with a recommendation to go to school. Indeed you seem to want it; and notwithstanding you say you consider it your duty to take this upon yourself against your patron's wish, I shall persist in regarding your interference as both his work and his wish too.

After alluding to my letters to the Academy, with the flippancy congenial to ignorance, he asserts that those letters were never read in that learned assembly. With you, Master Briggs, I must deal decisively. I received a letter from the Secretary of the

Academy, of which the following is a copy:—

"Institut de France, Académie Royale des Sciences, Paris le 23 Avril, 1832.

"Le Secrétaire perpétuel de l'Académie à M. Costello, à Londres.

"Monsieur, l'Académie a reçu la lettre que vous avez bien voulu lui adresser sur les nouveaux instrumens de Lithotritie de M. Heurteloup. J'ai l'honneur de vous prévenir, *qu'après en avoir entendu la lecture*, l'Académie l'a renvoyée à l'examen de MM. Boyer, Dupuytren et Serres, commissaires déjà nommés pour prononcer sur les instrumens du chirurgien Français.

"Agréez, Monsieur, l'assurance de ma considération distinguée.

"(Signed)

ARAGO."

My second letter was also read in the Academy at the sitting of the 18th June, of this he will find a proof in the *Messenger* of the 1st instant. What think you now, Mr. Briggs, of the accuracy of *your* statements? If you understand French as well as you write English, and that will not be saying much for you, you will see that you have been misinformed with regard to the reading of my letters in the Academy. You indulge in a sneer at the character of those letters. With lips consequentially pouting and swelling with your important dictum, you exclaim, "O they were not read in the Academy, they were not scientific, they were mere *verbiage*." It certainly is consistent in you to say they were not scientific, for you find it convenient not to take the least notice of the scientific details contained in those two papers, which were listened to with profound attention by the Academy. Of yourself, you are unequal to the task, though you assume the capacity of giving an opinion on the subject. You eschew the scientific parts of those papers, and carefully dwell on those which form a good ground-work for a *puff*. You are too clever by half, Mr. Briggs. With respect to the case of Captain W——, this young gentleman intrudes on me with his advice. I should forsooth have taken my information from Mr. Money or Mr. Green! Does he suppose that I have nothing else to occupy my time, than going about amongst my medical brethren, to ferret out the particulars of the cases treated by his patron? If he had been imbued with that candour for which I was willing to give him and his patron credit, he would have published in the article I am now occupied in answering, the letter which I addressed to Mr. Money on this subject. Such a course would have been manly and professional; but it would not have suited the left-handed policy of my antagonist. Here is the letter in question:—

"To — Money, Esq., 3, Hanover-street,

"My dear Sir,—A journey into the country yesterday prevented me from sending you the explanation which you required of me the day before. I could not under any circumstances refuse it, and less so when I consider the conciliatory and handsome manner in which you asked it from me. I therefore do not hesitate to avow, that I am opposed to the interpretation which M. Heurteloup supposes might be made of my statement of the case of Captain W. The only object I had in view, in my letter to the President of the French Academy, was to correct a mistatement made to that body, of the results of my practice in England. It was asserted that M. Heurteloup had succeeded with the curved percussion instrument, in two cases, in which I had previously failed. In order to prove the incorrectness of this statement, it became necessary to enumerate the cases in which M. Heurteloup had operated, after I had been previously consulted; this I did. I gave credit for one case, but I withheld it in the other. My defence before the Academy was called for, and I think I was bound in justice to myself to make it; and although this was the second trespass upon my reputation as an operator (I allude to the misrepresentation of the results of my practice, made in a letter of Mr. Alcock nearly three years ago), the tone of my letter was by no means acrimonious. It was even respectful towards the individual, although condemnatory of the instrument. I trust, whenever I am forced to figure in scientific controversy, that I shall always know how to maintain my opinions by fair argument, without degrading myself or my profession by vulgar or low abuse of persons.

"I do not think that we are in a condition to dogmatize upon the results of surgical operations, or that we are justified in saying, that because such a surgeon has operated, or such a mode of operation has been resorted to, or even that such an accident has arisen in the progress of an operation, that therefore the patient has lost his life; this being my opinion, how should I favour the interpretation, that because M. Heurteloup operated, therefore the patient died? I should be base, indeed, if I could act in this manner. This would be the logic of a furious partisan, and not of a sincere investigator of truth. This is my conscientious opinion. Now, I go farther, and state that if I were even convinced that the operation of M. Heurteloup, or any other man, was the cause of a patient's death, I should not say it. I will not, therefore, suffer such an interpretation to be put on my words, not only because it

would be unjustly injurious to M. Heurteloup, but because it would be injurious to myself. Thanking you once more sincerely for your kindness on this occasion, believe me to be, dear Sir, yours, &c.

"W. B. COSTELLO."

"7, Parliament-street, June 29, 1832.

With respect to the case of Mr. Stockbridge, he says, "he need make no comments on it; it speaks for itself." If he had said no more about it, I should have deemed my letter sufficiently long, and dismissed it also. As he has been so imprudent as to speak of it, I shall soon convince him that the less he said about it the better it would have been for the cause he attempts to advocate. "A straight instrument," says he, "could not be introduced into the bladder without violence, which, in this operation, above all others, should be avoided." Although Mr. Briggs is but a meagre authority on this matter, I agree with him, as to the necessity of avoiding all violence. *Sed cecidit in foveam quam fecit*; here he has dug the argument from under his feet. Let us compare what occurred to this patient under our respective treatments. M. Heurteloup introduced a straight instrument once, and attacked a stone, part of which was voided. During nearly six months following this sitting, many others were attempted, but they all failed. These attempts at introducing the instruments were productive of great distress, and were followed by swelling of the parts, by sanguineous infiltration, and leeches were frequently applied to combat the irritation. These attempts were at length abandoned, the impossibility of using a straight instrument was announced, founded on a presumed enlargement of the prostate, and a curved instrument was proposed.

I was at this time invited to see the patient, and although it is insinuated that undue means were had recourse to to bring this about, I declare that this insinuation is utterly unfounded. As I stated on a former occasion, I introduced the straight instrument twice, without violence, and without difficulty: upon the first occasion, to ascertain if it was possible; upon the second, to attack the stone. Upon neither of these occasions was there a drop of blood shed; upon neither was the injected water tinged; upon neither was pain given, upon neither did pain, swelling, or difficulty of making water, supervene. After the sitting, the fragments were voided without difficulty, and the patient resumed his chair as before. Now will Mr. Briggs answer, if any violence were used at all, against which of us will the evidence of it be the strongest? I did not say that M. Heurteloup used violence; I said he found it im-

possible to introduce the straight instrument. Mr. Briggs retorts and says, "if I succeeded where his patron failed, it could only have been by prevailing by the force of my hand and a metallic instrument over the soft and yielding organs; for the Baron Heurteloup's acknowledged skill and talent could not have failed in the usual application of this instrument." Harsh words are not necessary, neither shall I make use of them to wither the man, who could have written the preceding sentence. It comes to this pitch of arrogance,—that because M. Heurteloup failed to do this, no one else could!!!

Happily for me, I have in my possession the means of protecting myself against this malevolent insinuation. In the account of the autopsy, I stated that I preserved the parts; a minute's inspection of them will suffice to show that *the urethra was healthy and the prostate but very little enlarged*. I did not on the former occasion give the document sent to me from the medical men at full length, I shall now add a few lines which are appended to their account:—

"We the undersigned were present at the lithotritic operation performed by Mr. Costello on the late Mr. J. Stockbridge, and certify that it was performed with the greatest gentleness and skill, and with little apparent suffering to the patient. The time occupied was about three minutes.

"Signed, "WM. C. CARVER.

"WM. NUNN, Surgeon.

"Royston, May 4, 1831."

This document was sent to me inclosed in the following letter:

"My dear Sir,—Inclosed is the document required. I should say to you, as a friend and well-wisher, publish the whole case, and I am quite sure it will tell to your advantage, and the sooner the better. You have nothing to suppress; everything was done that could be done agreeably to the present state of knowledge of your art. With best wishes for your success, believe me, my dear Sir, yours truly,

"WM. NUNN."

With the account of the autopsy, and a few reflections which I shall now add, I think we shall have this case fairly before us.

It is an object to remove the stone from the bladder as quickly as circumstances will safely permit, not only because when it is in our power we should restore a sufferer to health with the least possible delay, but because, in the case we are considering, the evils arising from reflected irritation, are the great source of danger in this complaint. As it is admirably, and in accordance with physiological principles, laid

down by Sir C. Bell, the urinary organs have throughout their different parts elective sympathies with each other; by elective sympathies we mean, that one portion of this apparatus will call another portion into a participation of its excitement or irritation, in preference to all other parts; thus the neck of the bladder, and the external orifice of the urethra, are in close sympathy, and whatever annoys the one is sure to involve the other in community of suffering. This is shown in common blenorhaghy. The orifice of the urethra is alone the seat of disease, yet micturition is frequent, and the neck of the bladder is the seat of different sensations of fulness, of tingling, or pain. Again, if the neck of the bladder be irritated, the orifice of the urethra becomes a large and faithful sharer of its distress. If the bladder be the seat of suffering, the sympathies of the kidneys are awakened; these organs are strained to overaction, excitement proceeds to sub-acute inflammation, enlargement, and alteration of their tissues, is the consequence, and thus the irritation of the bladder, which was the only disease primitively existing, by exciting the sympathies of another portion of the urinary apparatus, hurries it likewise into disease, and life is destroyed, not by the alteration which disease effects in the first affected organ, but by that which it induces in another organ of greater importance to the animal economy. An acute and sudden attack of nephritis is productive of the keenest suffering, while the alteration which is silently going on in the kidneys, the effect of sympathy with another portion of the urinary apparatus can scarce be suspected, much less diagnosticated. Pathological investigations fully authorise the foregoing observations. Now, from the autopsy of this case we find that death was caused by the disorganization of the kidneys. Was my operation, which was not painful, and which, locally, was followed by a soothing effect, the cause of this disorganization? No candid, no well-informed surgeon, will think so. How then does this case prove the utility of a curved instrument? The stone was broken by a straight one, and the cure would have been perfect if the kidneys had not been diseased!!!

Mr. Briggs says, in the statement made in the French Academy as to the cases in which his Patron was successful with the percussor, I was not the operator alluded to. I am fully aware that attempts have been made by other surgeons with the lithotrite, but is M. Heurteloup so uncandid as to conclude that because surgeons without adequate experience give lithotrity a trial, that their failures are to be ascribed to the inefficiency of the lithotrite? Dupuytren, Roux, and many others, have failed with

this instrument in cases where Civiale was afterwards successful. If he did not allude to me in the Academy, he ought to have said so distinctly.

After this comes a summing up under separate heads. 1st. He says it is impossible that I can have treated successfully with the instrument which he improperly calls *pierce-pierre* (for it crushes the stone as well as perforates it), the number of patients I name, because, adds he, it is natural to suppose, if I had effected these cures, I should have published them in detail.

This is tender ground for M. Heurteloup's advocate to tread upon.

A short time before M. Heurteloup came to this country, he stated to the Academy, without further details, that he had operated in *twenty-three cases*. On the cure of his first case in London, he stated that it made his *forty-third case*. Of his French cases thirteen only are published, some, or most, of them under simple initials. Am I therefore to conclude he has not had forty-two cases in France? If I adopt the reasoning of his advocate, I ought to do so. I should not make this observation, if I were not forced to repel the doubt which it is so ignobly attempted to affix to my veracity. Persons most liable to deviate from truth, are often too apt to suspect the existence of their own failing in others. *Sed fas est ab hoste doceri*. He shall have my cases in full and honest detail.

2dly. According to him, I have never seen the curved instrument, &c. &c.

He will doubtless be surprised to learn that I have *seen it*, that I *possess it*, that I have *operated with it*, and what may grieve him still more to hear, that I have so modified, improved, and strengthened it, as to render it a safe and valuable instrument.

3dly. He says "It is difficult to conceive how Mr. Costello, in no way distinguished by scientific labours, and *comparatively* young as to experience, should, in so positive and unrestrained a manner, give his opinion on this operation," &c.

I fully concur with Mr. Briggs as to my being undistinguished in science, and of slender repute. However fortunate I may have been hitherto, or may be in future, in relieving the sufferings of my fellow-creatures, I shall always be ready to acknowledge the humbleness of my talents. It would be a curse to be endowed with but a moderate capacity, if vanity were allowed to exaggerate it into something stupendous. Still I must take leave, undistinguished though I be, to remark on the indecency of my being told so by a young man, who is still in the rudiments of the science. What he can mean by the words *comparatively young in experience*, I am not able to say.

If the comparison be meant between his patron and myself, I must inform him that I have been longer at the profession than his patron, that I taught anatomy, ere yet his patron gave up his situation at the *Bureau de la Guerre* to study medicine, and that I have seen and done more of this operation than any other man living, Civiale alone excepted.

Lastly, he reflects on my delicacy for endeavouring to mar the rewards due to a professional brother; but he adds, when the facts are untrue (Are the facts untrue?) such conduct requires an appellation, which, out of respect, he refrains from applying. This is urged by the advocate of a man, who signalized himself by the acerbity of his *diatribes* against M. Civiale addressed to that same Academy, the effects of which were ten-fold more injurious to the author than the man against whom their shafts were aimed. Delicacy, indeed! When instruments long in existence are presented as inventions, accompanied by claims for reward; when such claims are substantiated at the expense of another's reputation, when theoretic perfections are urged forward, and practical defects concealed, it would be criminal in a man who humbly laboured in the cause of science and humanity, to withhold what he knew on the subject. Adopting the language of Mr. Briggs, I can say with reason, such conduct would indeed justify the appellation which he refrains from bestowing.

I have now done with this heap of letters. In replying to them, my greatest humiliation has been in finding myself compelled to descend to the tone in which they are written. I repeat it, a controversy which can only be carried on in terms of low personal invective, can have no bearing whatever on the objects I have in view. My profession of feelings on this point has been declared long since. The man who devotes himself to succouring his fellow-man, or to the improvement of his art, is respectable in my eyes. I feel towards him that interest, which community of pursuit and object inspires, and I wish him success as sincerely as I wish to be successful myself. From such a man I shall be ever ready to receive any useful suggestion, or real improvement. With such a man I can have no difference. But he who seeks fame to render it subservient to his cupidity, he who denies all merit in others to aggrandize his own, will ever be dispicable in my eyes, and whatever he asserts, I must scrutinize before I can believe it.

I have the honour to be, Sir, yours, &c.

WM. B. COSTELLO.

7, Parliament-street,
Monday, July 16, 1832.