

**LETTRE ECRITE A MONSIEUR L'ABBE BOURDELLOT ... PAR C.G.  
[GADROYS] ... POUR SERVIR DE RESPONSE AU SR LAMY &  
CONFIRMER EN MESME TEMPS LA TRANSFUSION DU SANG PAR DE  
NOUVELLES EXPERIENCES.**

**TRANSLATION BY PHIL LEAROYD**

The full title of this letter, written by 'C.G.' (i.e. Claude Gadroys) to Monsieur l'Abbe Bourdelot is: 'Lettre écrite a Monsieur l'Abbé Bourdelot Docteur en Medecine de la Faculte de Paris, & Premier Medecin de la Reine De Suede, par C.G. pour servir de réponse au Sr Lamy, & confirmer en mesme temps la transfusion du sang par de nouvelles experiences.' [i.e. Letter written to Monsieur l'Abbé Bourdelot Doctor of Medicine of the Faculty of Paris, and First Physician of the Queen of Sweden, by C.G. to serve as an answer to Sr Lamy, and to confirm at the same time the transfusion of the blood by new experiences.]. This letter was written on the 8<sup>th</sup> August 1667. A copy of this letter is available to read or download from:

<https://gallica.bnf.fr/ark:/12148/bpt6k1268937x/f1.item>

The content of this letter is of interest regarding the history of blood transfusion from the viewpoint that it was written on the 8<sup>th</sup> August 1667, approximately seven weeks after Jean Denis performed his first human transfusion (on the 15<sup>th</sup> June 1667), which is commented on in this letter. The letter also provides details of the transfusion performed by Denis and Emmerez on Baron Bond on the 24<sup>th</sup> July 1667.

This letter is identified within the text to have been written in response to a letter published by Guillaume Lamy, who attempts to condemn the use of transfusion using 'metaphysical reasoning'. Gadroys attempts to bring a balanced argument into the debate as to the usefulness or otherwise of blood transfusion, given the restricted knowledge of the time regarding the actual role of blood, genetics and immunity. The letter written by Lamy that is frequently referred to in Gadroys' letter was written on the 8<sup>th</sup> July 1667 to M. Moreau, and is titled: Lettre écrite a Monsieur Moreau Docteur en Medecine de la Faculté de Paris, Conseiller, Medecin, Lecteur & Professeur ordinaire du Roi, par G. Lamy, Master aux Arts en l'Université de Paris; contre les pretendues utilités de la transfusion du sang pout la guerison des maladies, avec la réponse aux raisons & expériences de Monsieur Denys. A copy of this letter can be read or downloaded from:

<https://gallica.bnf.fr/ark:/12148/bpt6k1269952x.r=G.%20Lamy?rk=85837;2#>

In fact, Gadroys initially discusses the possibility that the content of Lamy's letter has actually been written by Moreau, given that the 20-year-old Lamy at the time was one of Moreau's students, but is at the same time very critical regarding the somewhat biased arguments put forward, which Gadroys criticises as being naive and ill-thought through. Gadroys argues for experimentation by independent researchers and believes that only experience and not simple reasoning can be used to establish if blood transfusion can be useful. Again arguments are put forward regarding in what situations transfusion could be used, which include an abundance as well as impurity of blood but not a lack of it!

Note: Lamy then wrote a second letter on the 26<sup>th</sup> August 1667 reaffirming some of his arguments against transfusion titled: Lettre écrite a Mr. Moreau, Docteur en Medecine de la Faculté de Paris, Conseiller, Medecin Lecteur & Professeur ordinaire

du Roy, par G. Lamy, dans laquelle il confirme les raisons qu'il avoit apportées dans sa premiere lettre, contre la transfusion du sang, en répondant aux objections qu'on luy a faites. A copy of this letter can be viewed or downloaded from:

<https://gallica.bnf.fr/ark:/12148/bpt6k1269787f.r=G.%20Lamy?rk=107296;4>

Given the date that Claude Gadroys letter was written as well as its content, it has been referred to by a large number of authors of published texts on the topic of the history of blood transfusion. As such, I have provided an English translation of this letter in the hope that it will allow its actual content to be read by a larger audience. Whilst I am obviously aware that instantaneous computer-generated translation is possible, this process struggles with specialist terminology and also produces a 'colloquial style' not always representative of the original text. I have tried to produce as accurate a translation as possible given that the printed text includes the variable trans-positional use of the letter u for the letter v, and the use of the long-form version of the lower case letter s. The paragraph structure and use of italics in the translation is reproduced from the original publication. Although I have taken great care not to knowingly misrepresent the author's original meaning I cannot guarantee that this work does not contain 'translational errors' and the reader is recommended to check specific details against the original French text.

#### NOTES:

Pierre Michon Bourdelot (1610-1685) was the family physician for the Condé family (from 1642 to 1651) and subsequently the physician to Queen Christina of Sweden (from 1651 to 1653). On returning to France he obtained ownership of the abbey of Massay, in Berry, which gave him the right to the title *abbé*. Early in 1664 he resumed the 'Académie Bourdelot' that he had created before leaving for Sweden, which were a series of meetings attended by future members of the Académie Royal, nobles, men of letters, philosophers, alchemists and scholars. It is for this reason why, as the organiser of these meetings, Claude Gadroys wrote his letter to Pierre Bourdelot.

Guillaume Lamy (exact birth and death dates unknown) graduated as a 'Maistre aux Arts' from the University of Paris, which meant that having completed a series of courses and defended a thesis in philosophy he had the right to teach humanities. He subsequently published works on philosophy that involved his concern to try to harmonise philosophy and science. These philosophical interests led Lamy to study medicine. It is known that he was admitted as a doctor in 1672 and since the medical course took 5-7 years to complete he must therefore have commenced his medical studies no later than 1667, the year that he published his two letters against the use of transfusion, beliefs that were in line with the Académie des Sciences, which recommended against the practice.

Claude Gadroys (1642-1678) was born in Paris and studied philosophy and theology. He published works on Cartesian physics and not medicine. Being one of the earliest and most enthusiastic followers of René Descartes, his primary publication 'Discours physique sur les influences des asters', published in 1674, defends a mechanical account of astrology.

Letter written to Monsieur l'Abbé Bourdelot Doctor of Medicine of the Faculty of Paris,  
& First Physician of the Queen of Sweden, by C.G. [Gadroys] to serve as an answer  
to Sr Lamy, and to confirm at the same time the transfusion of the blood by new  
experiences

Sir,

As you seemed very satisfied with the letter which Mr. Denis wrote concerning the transfusion of blood, and that you even had it read in your public meetings, I believe you will not find it wrong that I send you an answer, that a person who is still unknown to me has taken the liberty of making it to him, and that I myself make some reflections on it, that I would submit all the more willingly to your judgment, as I am persuaded that you are not interested in any party except that of reason, and that your kindness will not deny both of us the instructions which are necessary for us in this meeting to bring us to agreement, seeing that you lavish them on us so liberally by the learned remarks, which you share with us in all your conferences.

So please, sir, about this ordinary facility you have of hearing all those who speak only with the intention of defending the truth, and allow me to explain to you as briefly as possible, some thoughts that came into my mind, both concerning the author who made this response, and concerning the reasons he used to combat the experiments of Mr. Denis.

As for the real author of this response, I will tell you that the opinions are very different. Some spread the rumour that it is Mr. Moreau who is writing to himself, because they saw him solicit in person, and with great eagerness, permission to print it.

Others want it to be a more senior Doctor of Medicine, who has run through the shops of several booksellers, to engage them in the printing of this response; and they are based on the first fifteen lines, which expressly mark that Mr. Moreau has great deference for everything that comes from the author, that he always enters easily into his feelings, that his reasonings please him greatly, and that they never bring him any disgust, that his very imaginations are very pleasant to him, and finally that this response is addressed to him, only to satisfy the desire that he has expressed to know the opinions of this author on the subject of transfusion. Is it possible, they say, that Mr. Moreau, Professor Royal, was to consult a simple Master of Arts on matters relating to his profession? Is it true that an old doctor in the reputation where he is looks for lights in the head of a young man of 20, who is his student for the first year, and that he presses him to write on a matter of this importance? In truth, it would be detrimental to Mr. Moreau's capacity to believe him, and we would have done him more honour if we had changed this preface, or if the author had taken another quality than that of Master of Arts.

Others finally say that these two doctors have a large part in the body of this answer, and that they have provided all the reasons, they had to fight transfusion, to a common friend who put them in their day, and who was good enough to lend his name and his pen to give them to the public.

But for my part, I cannot have this thought of these gentlemen, and although I am assured that one of them often accompanies the alleged author to the homes of persons of quality, to go and make presentations to them of this answer, as if it were a master who wrote theses with his student, I cannot imagine that this writing was done with their participation, and I believe them to be too enlightening not to have seen in them all the faults which are found there against the first principles of philosophy and medicine, against experience and common sense. So allow me therefore that I justify these gentlemen, and let me show you that they have no part in it, by noticing some faults of the grossest, in which doubtless they would not have fallen, if they had put the hand there.

The first thing in which I find fault is that this Mr. Lamy undertakes to refute the experiments of Mr. Denis by simple reasoning. Do we not know that the quibbling of

the School can provide reasons to fight and to defend all sorts of feelings, and that there is only experience that is capable of giving a final decision, and of completely dispelling the sophisms of the most obstinate, mainly when it is a question of physics or medicine? There was no shortage of reasons a hundred years ago to prove that antimony or emetic wine was a venom and a poison, since the use of it was for the time being forbidden by a decree of the Faculty of Medicine; and there is still no shortage of it today, to prove quite the contrary that it is a purgative of very great importance, and which can have marvellous effects, since the same Faculty made a decree last year, by which it allows it, and even orders its use. But we can say that it was only the experience that decided everything in this meeting, and that the healing of several people, and among others our monarch, has done more to convince minds of its usefulness than all the arguments that have been used to reject it and to defend it.

It is the same with all remedies. There is not one which is not approved by some physician who believes he has the right reason on his side, and disapproved at the same time by others who imagine themselves to be better founded; and in truth one should not consider as more reasonable than he who gives his hands only to experience alone. Now the transfusion will not have this particular prerogative, of being received and approved by everyone; it is a new operation of which antiquity has never spoken; it is a new way of curing several illnesses, which is proposed to physicians to be their judges and arbiters; and as it concerns the health and the life of men, one cannot examine it too closely. But also we would be greatly mistaken if we wanted to rely on the judgment of a few, who, imagining that they know everything, find themselves extremely surprised when something is offered to them which had not occurred to them; the mere name of the thing is capable of astonishing them, and attracting their censure. Only those will be better off, who will not rush their judgment in this encounter, but who will wait several experiments to decide, and as it is a matter of the utmost importance, it would be desirable that those who are in power make several of them at the earliest, to examine them themselves, or to have them examined by prudent and disinterested doctors like you.

The second thing that surprises me in Mr. Lamy's response is that the content of his letter agrees so little with the promises he makes in the title, to respond to the reasons and experiences of Mr. Denis. Mr. Denis satisfied in his letter the objections that had been made to him in his public lectures against transfusion; then he gave four very solid reasons which could favour this operation; and finally, after proving that it would be more appropriate to use the blood of some animals for this operation than that of man himself, he confirmed everything he had in mind by admirable experiments. However, Mr. Lamy does not show that Mr. Denis answered the objections badly, he did not touch on any of his reasons; and if he speaks of his experiences, it is only to disguise them and add circumstances to them, which entirely change their nature. This is what you will see in the reflections that I will make on the main points of his answer.

In the second line, sixth page, *as the foundation*, he says, *of all that I will propose next, it should be noted that when the blood of an animal is passed through a man, a very great deal of it is involved, a small quantity with that of the man, etc.*

In truth, this foundation is very badly established; the building that will be erected in ruins will soon fall into ruin. Who told Mr. Lamy that only a small amount of animal blood mixes with that of man? Can we not evacuate as much blood as we want before introducing new blood by transfusion? And if so, who will prevent a large quantity of the blood of the animals that will have been chosen from me from then on, with the little that will remain in the veins of man?

And even if one had not made such a great evacuation, the speed with which the arterial blood of the animal enters a vein of the man, to go directly to the heart, is quite capable if not of stopping, at least of greatly delaying the movement of the blood of the other veins which bring blood from all sides, and thus this new blood

would always make more room for itself in the heart than Mr. Lamy imagines. Which would be easy to confirm by the laws of mechanics and by a few experiments; but I am willing to suppose that only a very small quantity enters. Let's see the lectures that Mr. Lamy claims to derive from it.

He says that it follows necessarily from his principle, that there is no disease where transfusion can be usefully applied; and it comes into view by a beautiful division that it makes. *All diseases*, he says, page 3, line 18, *the cause of which is internal, generally precedent, either from the abundance of the blood, or from its impurity*. And then he adds that to remedy the abundance of blood, it suffices to draw it out by bleeding, and that to correct its impurity, one must look for other ways than transfusion.

Hey what, is it a Master of Arts from the University of Paris speaking? Can one reach this degree without knowing logic, and without having learned the rules of a good division? Mr. Lamy says that all diseases are generally preceded by two causes, namely, either from the abundance of the blood, or from its impurity. Shouldn't one more member be needed for this division, to make it whole? Is not scarcity of blood such a secondary source of many diseases as the other two of which he mentions? This third cause must have remained at the tip of the pen, or out of bad faith, because the transfusion seemed to him perhaps too useful to all those who have lost a lot of blood, or out of ignorance of the first principles of medicine and logic; I leave the judge to you. But allow me to follow it step by step, and to examine the two other causes which seemed to him more favourable than the third, of which he did not speak.

As for the abundance of blood, he says line 20, *that everyone will agree that it would be ridiculous to suggest transfusion to cure it, but that it suffices to take away what is too much by bleeding*. This is speaking too generally, not everyone will agree. I know of several sc̄avans doctors, who maintain that blood never sins in quantity, but only in quality; that is to say, when a man is judged to have too much blood by the fullness of his vessels, by the colour of his face, by ailments in the head, by the bleeding from the nose, or otherwise; it does not follow that he indeed has more blood than he needs; but he is perhaps too fat, and the great agitation he has in his vessels is quite capable of producing all those effects which are attributed to abundance; just as water which boils on the fire sometimes rises over the edges of the cauldron and spreads itself in the ashes, although there is not more than enough to fill half the cauldron. If these doctors have some reason to be in this thought, and if they sometimes succeed in curing this apparent fullness by simple refreshments, without bleeding; it seems that on these occasions one could also use the transfusion of fresh blood after bleeding, just as to prevent the water of a cauldron from spilling over the edges while boiling, one can take away a certain quantity of the hot, and put back as much colder.

As for the impurity of the blood, Mr. Lamy says line 28, *that it produces an excessive heat which is met there, which cannot be extinguished by the blood which one will pass from a healthy animal in a sick one*; and he proves it, because these men on whom Mr. Denis had the transfusion done, felt a great heat in their arm because of where this new blood passed, and he proves it, because these men on whom Mr. Denis had the transfusion done, felt a great heat in their arm through which this new blood passed, and because the large quantity of clean blood, which is in the veins of a man, is more capable of communicating its heat to the small quantity of new blood, than of receiving any cooling from it, even if it were supposed to be cooler than that of man.

Mr. Lamy wants the intemperance of the blood to come only from its excessive heat: *I do not believe*, he says page 5, line 28, *that there are cold diseases*, and so since the new blood introduced by the transfusion, always warms, or is warmed by the mixture of that of the man, it must be concluded that the transfusion can have no usefulness.

But it is all very well for a young man to assure that there are no cold illnesses; it is to boldly determine what the entire Faculty of Medicine would not dare to do; it was certainly necessary that the passion he had for contradicting experience, and to make himself complacent to those who had inspired him with this generous design to oppose the transfusion, had so heated his blood and his brains that he believed that his illness was an illness of all nature, and that there was nothing in the world capable of being able to temper it. However, many people complain of catarrhs, colds and cold sores; others are prone to colic, rawness of stomach, paralysis, cold drops and several ill effects of the pituitary. To whom will they turn for some relief? If Mr. Lamy and those who protect him do not recognize cold illnesses, it would be in vain to ask them for the remedies. But perhaps they will also admit it in practice, and if they act in good faith, they will candidly admit that they only denied in this writing that there were cold illnesses, only because they would not otherwise have been able to cast doubt on the usefulness of transfusion, they who want the blood transmitted to warm coughs, or to be warmed.

But I am quite willing to tell Mr. Lamy that all illnesses are hot. Does it follow from this by a good consequence that the transfusion is useless? Is there not in the veins of any animal a fresher blood than that of a man who is in the ardour of fever? No, said Mr. Lamy, since those on whom the transfusion was made felt heat in the arm, while the blood from the artery of a lamb was introduced into them.

A fine consequence, it is as if I said that a veal broth will warm the patient more, because he feels it hot while swallowing it, and generally that all the beverages which one puts on the fire before taking them, will increase the fever, because they warm the throat in passing. Mr. Lamy, who wants to be taken for a man who perfectly possesses the old and the new philosophy, makes it clear by this way of reasoning that he is ignorant of both. The followers of the old philosophy will tell him that he has confused actual heat with virtual heat, and that he should know that there is a lot of difference between being currently hot, and having the truth to warm up. The followers of Gassendy or Descartes will complain that he has not sufficiently distinguished the different figures of the parts with their movement. And all together, both the ancients and the moderns, will agree to teach him that everything that feels hot does not heat up, but cools quite often; as, on the contrary, anything that feels cold does not refresh, but just as often heats up; for example, cold water poured on lime heats it more than hot water would; strong water that feels cold, heats the metals so much that it dissolves them; the Spirit of Nitre, or the Oil of Tartar insinuating coldness into the veins, does not fail to cause such heat and such fermentation, that one sees in a short time the blood come out of its vessels, and change into a foam which covers the whole skin; and the Spirit of Vitriol, on the contrary, being pushed all hot into the veins, does not fail to cool the blood so much, and to stifle its natural heat, that it congeals and coagulates it in a very short time throughout the body.

Mr. Lamy is not, it seems to me, a better philosopher, when he adds that if the new blood does not heat up that of man, at least by right it is heated to the same degree. *Is it not much more probable*, he says, page 4, line 15, *that this great quantity of own blood joined with the excessive heat which is found in the heart, will heat this foreign blood in the same degree?* Reason has always agreed with experience in persuading me that if a pint of hot water were capable of heating half a septier of cold water, that it would be poured into; this small quantity of cold was also capable of slightly cooling the large quantity of the hottest, and thus a more temperate and less hot compound resulted from it than before. But Mr. Lamy has many other lights on this subject, he wants a cold blood, such as that of a calf, to be heated by the own blood of man without cooling it, and that from these two bloods there results a mixture as hot and in the same degree as before. If his thought had any place, it would be necessary to abandon all those whose blood was heated by fever, it would never be necessary to order them any refreshing drink; for the little

chyle, which passes through the thoracic canal before mixing with the blood in the subclavian, being in very small quantity, and much less than that of new blood introduced by transfusion; this chyle, however cold, would soon be heated by the blood of man to the same degree, and thus would never cool it.

I know very well that Mr. Lamy makes a great difference between the chyle and the transmitted blood; *Transfusion*, he says, page 5, line 23, *can in no way cool, & the chyle as it is will always diminish somewhat from the heat*. But I do not see why the chyle such as it is can always decrease from the heat of the blood where it mixes, and not the transmitted blood which mixes there in greater quantity. On the contrary, experience shows us that there are several strong chyles, which heat and ferment the blood so much that they give fever on approaching the heart; which has not yet happened by the blood which has been introduced in the transfusion. Mr. Lamy is therefore very seriously mistaken when he undertakes to prove that transfusion will be quite useless in hot illnesses. Let us see if he will be happier in those which show some particular malignancy of the blood.

Mr. Lamy uses on page 6 the example of wine, and because Mr. Denis had asserted that too coarse blood could be softened and become more subtle, too subtle could be fixed and thickened, too hot could be tempered, and too cold could be warmed by the mixture of certain bloods which one would choose on purpose; just as a wine that is too harsh can be sweetened, a weak one can become more vigorous, the fat can be degreased, and that which is gassed can be corrected by mixing certain liqueurs that the innkeepers are not unaware of. *That is what I boldly deny*, says Mr. Lamy, *for wine that is too sweet is not sweetened by a little sweet wine, cloudiness is not clarified by clearness, weak wine does not become vigorous with a little strong wine, the fat does not lose this quality by the mixture of that which is opposed to it, in a word, that which is spoiled is not corrected by that which is good, but by the mixture of certain liquors*.

Can we see in the world a greater lightness than that? He boldly tells me what he does not know, and what anyone other than him would know how to ignore. For everyone knows well enough that it is usually only by mixing wines from different countries, that the innkeepers find the means of satisfying the diversity of tastes, and that they are even forbidden by the police to make other mixtures. In truth, Mr. Denis only speaks of it in general, and contented himself with saying that wine can only be corrected by mixing certain liqueurs, because he did not want to get involved in a detail that would have been out of place, such as determining whether it was Orleans or Burgundy wine that should be mixed with Brie, etc. He supposed that everyone knew it, or cared little about it; and yet Mr. Lamy claims to derive more benefit from it, as if Mr. Denis had wanted with him that these liquors were other than wine itself. For my part, I do not so much blaspheme Mr. Lamy's ignorance in this encounter as the boldness in which he boasts; because being Norman, as he says he is, one should not be surprised that he is better informed about how to mix pears with apples to make good cider, than the method of mixing several kinds of liqueurs to make the wine more delicious and more delicate.

Mr. Lamy passes on page 7 to several particular illnesses, such as pleurisy, rages, erysipelas, madness, etc., and he claims to show that Mr. Denis was very wrong to speak of it in his letter. But he is taking a lot of unnecessary trouble, he is having a great passion to contradict, I believe that for its complete confusion it is only necessary to copy Mr. Denis' own words. *We could foresee*, says Mr. Denis in page 10, line 10, *some usefulness and some advantages of transfusion, in pleurisy, pox, leprosy, cancers, ulcers, erysipelas, rabies, madness and other diseases resulting from the malignancy of the blood: but we must await its success in the experiments which may be made of it in a short time*. Could there be anything more modest? Mr. Denis does not assure anything, he wants to wait until the experiments have made him know what everyone is allowed to conjecture at present; and yet Mr. Lamy

attacks it as if he had spoken positively; he takes the opportunity to dwell on all these illnesses, and to share his beautiful imaginations with the public.

He says on page 7, that transfusion would not be safe for pleurisy, because the blood transmitted always heats up instead of cooling down; but it is a supposition which I have hereby convinced of falsity. He then adds a few remarks on leprosy, erysipelas and cancers, which are no better founded: but I want to wait with Mr. Denis for the success of the experiments, I do not want to dwell unnecessarily on the conjectures that can be formed on both sides. I only ask you, sir, to remark in passing that what idea Mr. Lamy wants us to have of his person when he speaks of madness. *I will not speak*, he says page 8, line 28, *of madness, not being able to recount here all the species. I will tell you only, sir, if my madness is never cured except by transfusion, there is a good chance that I will never be wise.* If he is mad, as he supposes, one need not worry too much about what he says, *Os fatuorum ebullit stultitiam.* (Proverb 15) As for me, I have no intention of undertaking it on this point; the Sage closes my mouth when he says, *Ne respondeas stulto juxta stultitiam suam, ne efficiaris ei similis.* (Proverb 25)

Let us therefore pass further, and see what turn Mr. Lamy gives to the experiments of transfusion, of which Mr. Denis spoke in his letter. He wants to prevent the transfusion from all those surprising effects that were noticed in a young man of fifteen, who, after having received the arterial blood of a lamb, was cured of a strange drowsiness, which numbs his body as much as his mind; and to come to the end of his design, he gives free rein to his imagination; and after throwing himself on some commonplaces of fear and drowsiness, he concludes that it is the only apprehension that has done everything in this encounter. *The apprehensive view*, he says on page 10, line 3, *which he had of an unused remedy, and the event of which could only appear to him to be very doubtful, set his spirits in motion, and freed them from the embarrassments which prevented them from distributing themselves, from which the release of spirits afterwards produced all the advantages which are attributed to the transfusion.* I wonder how Mr. Lamy took the liberty of making a supposition so contrary to the truth, and which destroys itself so easily. Because firstly if this young man had had to be cured by apprehension, he would undoubtedly have been cured 24 hours before the transfusion, then he had had a fairly large one, when he let himself fall the day before from the top of a 10-foot ladder, as is expressly noted in the letter of Mr. Denis.

Secondly, could Mr. Lamy doubt that Mr. Denis had not taken the necessary precautions to remove all fear from those whom he exposed to the transfusion? Wasn't that the only reason he didn't want to risk the operation on a criminal? *Several people pushed us*, says Mr. Denis in his letter page 11, line 32, *to ask for a criminal to make the first attempt on him. But having reflected that a man in this state, who is greatly altered by the apprehension of death, could be intimidated even more: and that by considering transfusion as a new kind of death, this mere thought could cause him some trouble and some syncope, which would doubtless be attributed to the transfusion. We did not think fit to expose ourselves to this danger, nor to make ourselves importunate to his Majesty without any necessity, and persuading ourselves that there would not be so much to fear from people who knew us perfectly, and who would have some confidence in our words, we preferred to wait for a favourable opportunity to discover some of them as we wanted them, than to put ourselves at risk of losing everything through too much haste.* After these words, is Mr. Lamy right to feign a strong fear of transfusion in those on whom it has been tested, since the sole aim of Mr. Denis was to banish it. If Mr. Lamy was not sufficiently informed of the circumstances of this operation by the letter of Mr. Denis, he ought to inquire more about it, and not imprudently suppose this circumstance of fear, which changes and entirely alters the fact. He should address himself to some of those who were present at the operation; and he would have learned from them that this young man was far from apprehending transfusion at all, since he did not



even know what transfusion was, and that he imagined that the lamb was fitted on his arm only to suck all the bad blood out of him, in a way that was made to pass for him as old and very common. It would have been said to Mr. Lamy, that to be sure of the greater effect of the transfusion, some time later about half a pallet of blood was drawn from this young man, and that having compared it with the one that had been made before, it was found to be a little more vermilion and more runny. He would have been told several other circumstances which would no doubt have prevented him from supposing a falsehood, of which he will never get over, and from falling into several other obvious contradictions, which I will show you are filled with at the end of his answer.

He says in page 10 that to achieve his purpose, he wants to show that the transfusion can have unfortunate consequences, and cause several unknown diseases; and to prove it he first uses a comparison which seems to him very convincing. *As it cannot be*, he says on page 11, line 16, *that an animal is generated from the seed of another of a different species, and also there is no appearance that an animal can be nourished by the blood of another of a different nature.*

It would be easy to show Mr. Lamy that he is hardly just in his conclusions, by teaching him that it quite often happens that females nourish in their wombs, by the transfusion of their own blood, fetuses of different species and which have been engendered by the seed of males, also of different species. It would be easy to answer him, that a man cannot be begotten by the seed of a sheep, nor of other animals; by the pips of an apple, or by the seed of several other plants, on which, however, it feeds quite commonly. It would still be easy to make him see the absurdity of these thoughts, when he says on page 12, that if one uses a lamb in the transfusion, it would be to be feared that those who exposed themselves to it would become covered with wool all over their body, and would not feel a pair of horns growing from their head, because there are in the blood of a lamb particles suitable for form all these parts. For if it were permitted to reason in this way, man would have to be forbidden to use all animals for his food, lest feathers come to him like birds, scales like fish, and wool like sheep. You should never haunt a graft of pome fruit on a trunk of stone fruit; because there being in the juice of this trunk, particles capable of producing stones and almonds, it would follow that the transfusion of this juice into the graft would always produce the same things there. It must be said, on the contrary, that as the grafts so filter between their fibres the juice of the trunk on which they are placed, that they convert it into their own nature. The flesh also and the blood of animals are so filtered, cooked and elaborated passing through the heart, the veins and the arteries of man, that all the smallest particles change shapes, and take that which is the cleanest to combine into its substance.

I know very well that Mr. Lamy wants there to be a great difference between the flesh that one eats for food, and the blood that one transmits immediately in the veins, *because*, he says, *the flesh suffers a lot of changes, which the blood does not suffer.*

But when he has studied medicine for some time, he will know that all authors have always distinguished three principal coctions in food, of which the first which is to make food in the stomach, is not considerable in comparison with the two others which make chyle and blood in the heart, the liver, the spleen, and generally in all the parts which are nourished. He will be taught that like the coction which is made from the juices of the earth in the roots and in the heart of the trunk, is not used so much for the production of certain fruits, as the last filtration which is done of these juices in the small fibres of the grafts; also it may be that all these coctions, which are admitted into the stomach and into the heart or the liver of animals, are not so much effective in giving the particles of food the shapes which are necessary for them to convert into the substance of man, than the diversity of the pores which sift them in the last place, and which find them different in the bones, the flesh, the cartilages; and the other parts, where the ancients admitted for this reason as many different

assimilating faculties. Now, although the new blood which is given in the transfusion does not pass through the first coction which is made in the stomach, it nevertheless passes through the other two, repeating several circulations with its own blood, and thus nothing prevents it from being able to nourish man, and to convert into its own substance.

But there is no need to answer Mr. Lamy to seek reasons, or examples which seem to take us away from our subject. It is only necessary to produce the experiences we have in this very matter in question. Mr. Lamy denies that an animal can live on the blood of another of a different species, and yet this dog which was given the blood of a calf about five months ago, in the presence of Mr de Montmor, and several other persons of quality, is not yet dead; on the contrary, he is doing very well, and has become fatter than before. Mr. de Sarte and Mr. Lamy know well that a person worthy of faith assured them of this, and that it was up to them to go and see him for 15 days. Mr. de Bourges, Doctor of Medicine, will be able to produce another, which he still keeps at home, although it was also about 5 months ago that he was transfused with the blood of a calf, several people of quality will testify that they have seen for a month a small spaniel female dog, very low, and quite languid with old age because she was about twelve years old, which after having received the blood of a kid, by the skill of Mr. Emmerez, became shortly afterwards more vigorous and thinner, and hot even in less than eight days. Those who have read Mr Denis' letter, and who know of the success that the doctors of England, Holland, and Italy have had in examining the usefulness of transfusion, will not trouble themselves about all the other reasons, which Mr. Lamy uses at the end of his answer to elude the experiments.

This is also why I do not intend to dwell on it much. I will only point out to you that the unfortunate consequences that he foresees from the transfusion are things common to all remedies, and to all foods themselves.

He therefore says on page 13 that if transfusion were in use, doctors would employ, according to their whim, the blood of different animals, etc. But if a remedy is to be rejected only because a physician's caprice can abuse it, medicine must defend them all without reserve. Moreover, the same accidents are to be feared in the diversity of the flesh and of the liquors with which we feed, as in the diversity of the blood that we would give in the transfusion.

He says again on the same page, that the blood of animals would be very harmful to us, because they do not live as long as we do. But for this reason Mr. Lamy would reduce us to take no other food than the flesh of deer, crows, and some other animals which live for a very long time.

He adds on page 14 that physicians will never be able to make considerable progress in transfusion, because it is almost impossible to discover the complexion and temperament of the animals whose blood must be taken. But doctors are not in such great ignorance as Mr. Lamy imagines, there is none who do not know that calf's blood, for example, is fresher and smoother than sheep's blood, that the blood of a kid is more subtle than that of a lamb, and so of the others. And even if they did not yet have a fairly exact and perfect knowledge of it, there would not be so much difficulty in obtaining it, as there was in knowing the nature and the qualities of several plants.

Finally, Mr. Lamy fears that by communicating to man the blood of a beast, he is at the same time being communicated to him some brutal inclinations. But I beg you, sir, to notice that there is a great difference between the time of childhood when the parts are weak and delicate, and the time of a later age when your same parts are strong and vigorous. It is quite true that in the first conformation the parts could contract some brutal inclinations, if they were maintained by the continual transfusion of the blood of some animal. Just as we have often seen that children have an inclination to jump like goats, because they have been fed with their milk. But also we must say, using the same comparison, that as people who have passed their

infancy, and whose parts are very strong, do not contract the inclinations of cows, donkeys, or goats, of which they take milk for all food for whole years: these same people would never take in this state the inclinations of animals, when the blood was given to them by transfusion. Moreover, I do not believe that when one would be obliged by some disease or otherwise, to repeat the transfusion 3 or 4 times on children, there will happen to them any unfortunate accident for the inclinations, just as we have never heard of any happening to them for having been suckled for 3 or 4 days on the milk of a goat or a sow.

After all these reasons, Mr. Lamy concludes his letter as he had begun it, that is to say that he receives his praise, as if he distrusted that his work was not enough to commend him tenderly. He is not satisfied with having first begun his panegyric to the dishonour even Mr. Moreau. He is not satisfied with having first begun his panegyric to the dishonour even Mr. Moreau. He continues it in all the pages of his letter, *You know*, he says to him, page 9, *that it is not my custom to believe miracles without examining them very seriously.* And in the last page he completes it in the same manner by addressing him again in these terms. *You know that I am not one of those whimsical minds who do not approve of an opinion unless its antiquity renders it venerable; neither of those evaporates a feeling only because it is new.*

In a word, if we want to go back to the beginning, the middle, and the end of his letter, it is an oracle that has Mr. Moreau throwing incense. I don't want to stop him, nor seem jealous of his good fortune. But if I knew him, I would advise him for his honour to publicly release his letter, and to avail himself from now of the privileges, which he says in the last page that he can enjoy by the law of his country; for a young man is always more laudable to retract his opinions as soon as he has stated them lightly, than to persist in a ridiculous obstinacy against reason and experience.

But in order not to omit anything that can help to convince him, I must add here the detail of a celebrated experiment which has recently been made on a patient, with such surprising success, that several sc̄avans doctors have been obliged to go there, and to admit that the transfusion could have very considerable effects later on.

About a fortnight ago a foreigner was abandoned by four doctors who had treated him for three weeks for a hepatic and biliary flow, mixed with bilious diarrhoea, with a very violent fever. But after ordering him bloodlettings of the arms and feet, purgations, and enemas, as much as their prudence saw fit, he became so weak that he could no longer move, he lost his speech with knowledge, and the continual vomiting of everything that was given to him joined his flow, they completely despaired of it, and said that there was no longer any remedy, because there was no longer any way to bleed it, nor to give it any hold, either from above or from below.

His relatives and his friends seeing him in this state were content to try all things, and to have the same recourse to transfusion; they ran to Messrs. Denis and Emmerez to implore this last aid from them. But when these gentlemen had seen the state in which this patient was, they absolutely refused to come to the execution, saying that the transfusion could not cure the corruption of the solid parts, to remedy the gangrene which was apparently in the intestines, and that if it had been intended to help him by transfusion, it would be necessary to correct it sooner, and at the same time that he had had large evacuations of blood, because the veins had no doubt since been filled with the serositis and humors which are intended to quench the parts, as it was easy to judge by the degree of dryness of his skin. Notwithstanding all these reasons and several others, which these gentlemen employed to apologize honestly; we came back to them 3 and 4 times to make them new solicitations, to give this satisfaction to the friends of the patient, not to see him die without having tried all possible remedies; and as they saw themselves extremely in a hurry, they took the liberty of putting their honour under cover, to say that they did not want to undertake anything on the doctors who had treated this patient, that they had to be sent for, and that if they wanted to declare that they were abandoning him, and even consent to the transfusion being attempted, it would be done at all

hazards by their prescription. The ordinary doctor, who passes for a very capable and very prudent man in the Faculty of Paris, also came quickly to give testimony in the presence of several persons of quality, that 4 of his colleagues had abandoned the patient with him, and that since there was no other remedy that could be brought to him than transfusion, he would willingly consent to it being performed in his presence, principally because this operation was not, in his opinion, capable of hastening the death of a man who apparently had not yet two hours to live.

On the oral and written assurances given by this knowledgeable doctor, they did not pretend to undertake the transfusion of the blood of a calf into the patient's veins, and although he was already in a lethargic slumber, with convulsions of the limbs, and a strong deep-set and tingling pulse; here is the unexpected change that happened to him after a slight transfusion of about two palettes that was given to him in the morning. His pulse rose instantly and grew more vigorous, the convulsions stopped, he stared fixedly at those who were near him, and after giving all possible signs of perfect knowledge, by answering very appropriately and in diverting tongues to those who spoke to him, he fell asleep, a rather sweet and tranquil sleep, and having taken his leave 3 quarters of an hour later, he ate very well the rest of the day with several broths, herbal tea, and jelly, without vomiting anything, letting nothing go down there, although there were 3 whole days that he could not have been able to take anything by mouth, and that his lienteric flux had not left him since his illness. After having remained about 24 hours in this state, his strength began to fail, his pulse sank, and his intestines drained him with the last weakness. His friends who had seen the day before a change so noticeable afterwards from the transfusion, wished it to be started again; and after several entreaties, they were granted to content them with making one as light as the previous day, because it was increasingly confirmed that there was a strange corruption in his bowels, which could not be repaired by transfusion, any more than by any other remedy. After this transfusion, which was performed at six o'clock in the morning, the patient regained some vigour, which was not of very long duration; for although he took his broth well enough without vomiting it, he always emptied himself downwards; and at midday he began to decline little by little until the last sigh he breathed at five o'clock in the evening, without showing any sympathetic movement. It was judged that it would be very appropriate to open his body in the presence of the doctors, and having done so, the ileum intestine was first found to be retracted into itself from top to bottom, and below this knot, to the bottom, the guts were all fluid, gangrenous, and with an unbearable stench. The pancreas was extraordinarily hard with obstructions that did not allow pancreatic juice to flow into the intestines. The spleen was square in shape and four fingers thick; the liver very large and liquid in some places; the heart very dry and all burnt; and having finally discovered the vein, through which the transfusion had been made, from the opening of the arm to the heart, there was scarcely any blood found there, any more than in the other veins, nor in the ventricles of the heart, because the little that had been given to it, was completely soaked in the flesh, because of their warmth and their degree of dryness. All this and several other circumstances can be confirmed by the testimony of twelve trustworthy people who attended this opening, and by the certificates that the doctors gave to send to the relatives of the deceased.

If after that Mr. Lamy, or others contradict the experiment, and they say, as we have already done, that all those who worked on this experiment did not find the same success, you know that each is responsible only for what he does, and not for the faults of others. If I were allowed to say more about it, I could add here some other experiences which would not displease you; but you will be able to learn them more exactly from the very mouth of those who have had the advantage of succeeding in it, they will be able to communicate them to you, until they have a considerable number to give them to the public.

For my particular, I beg your pardon, if I have bored you a little too much by the length of my letter, I had no intention of ever writing on these matters; but as I saw that Mr. Denis said loudly that he would not answer those who only attacked this experience by metaphysical reasoning, and that the protectors of Mr. Lamy took the opportunity to take advantage of this silence. I have taken the liberty of picking up some reflections, which I had made while reading their reply; it is true that these are only the attempts of a disciple, who undertakes what does not deserve a master's stroke; I address them to you as to one who is less capable of concern in this encounter, I submit them to your judgment, without expecting praise from you, if you believe that I deserve blame. And I protest to you that my greatest passion is none other than to take advantage of this opportunity to show you that I am,

Sir

Your very humble and obedient servant,

C. GADROYS.

From Paris, 8 August 1667