LETTRE ESCRITE A M. MOREAU … 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 TRANSLATION BY PHIL LEAROYD

The full title of this letter, written by 'G. Lamy' (i.e. Guillaume Lamy) to a Mr. Moreau is: 'Lettre escrite a M. Moreau, Docteur en Medicine de la Faculté de Paris, Conseiller, Medecin Lećteur & 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.' [i.e. Letter written to Mr. Moreau, Doctor of Medicine of the Faculty of Paris, Counsellor, Physician, Reader and Ordinary Professor of the King, by G. Lamy, in which he confirms the reasons he had given in his first letter, against transfusion blood, answering the objections made to him.]. This letter was written on the 26th August 1667. A copy of the letter is available to read or download from the following sites:

https://books.google.co.uk/books?id=UcRjAAAAcAAJ&pg=PA4&lpg=PA4&dq=G+Lamy+%2B+transfusion&source=bl&ots=gIzWLtYFW9&sig=ACfU3U2BKmR5nHp-jArRY8aw5vE_c56p9g&hl=en&sa=X&ved=2ahUKEwjBr-fRk6ToAhWGShUIHc0iDE8Q6AEwDnoECAQQAQ

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

Lamy initially identifies that he had read this letter that has been written in response to Lamy's first letter to Mr. Moreau two days before (i.e. on the 24th August 1667) and that 'last Monday it was publicly read in the Assembly of Father Bourdelot'. Lamy initially identifies that the letter was written by 'one of Denis's students', though also states that the name of the author is unknown. He is critical of but eloquently responds to the apparent personal abuse that this un-authored letter is said to contain, that was apparently written in response to Lamy's first letter of the 8th July 1667, a copy of which is available to read or download from:

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

Lamy starts by stating that he is not 'against transfusion' but is against its stated usefulness and the exaggerated claims made by its supporters. Although this letter does not add any additional information regarding the practicalities of blood transfusion performed at that time in Paris, and only minor comments regarding the two transfusions that Denis had performed up to that time on patients, it provides valuable additional information regarding the arguments that ensued around the introduction and use of blood transfusion. Many of these arguments were obviously misguided and restricted by the medical knowledge of the time. For example, Lamy provides an explanation as to why transfusion was not thought useful for the treatment of haemorrhage, which he identifies to have been because the blood loss was commonly followed by a 'fever', a condition which he states was at that time treated by phlebotomy! He also discusses within this letter the belief that blood had a nourishing role within the body. Lamy also uses the Cartesian philosophical belief that foreign blood is not able to directly nourish the body (due to 'particular differences') and that only a person's own blood is capable of providing this. Lamy

uses this same information to counteract a claim made by Jean Denis in support of transfusion, that it is essentially no different to the natural process of maternal blood 'nourishing the fetus via the placenta'. Lamy's comments used to counteract this statement are however somewhat confused in that whilst he discusses the differences between mother and fetus due to the involvement of paternal 'seed', he confuses his argument by also discussing species differences, using the mule as an example of this.

At the time that he wrote these two letters, Guillaume Lamy (c.1644 – c.1682) was known to have been a young Master of Arts graduate from the University of Paris (a fact confirmed by a comment made by Claude Gadroys in his letter of the 8th August 1667 to l'Abbé Bourdelot*) and therefore was qualified in (and subsequently published material on) philosophy and humanities. These interests led Lamy to also study medicine and he is known to have acquired his doctorate in medicine in 1672. Since the medical course normally took between 5 to 7 years to complete, he must have commenced his medical studies no later that 1667, the year that these letters were written. At that time he would have been a student of Mr. Moreau, Doctor of Medicine at the Faculty of Paris. This statement is to some degree confirmed by the comment made to Lamy that 'they advise me to study medicine for a while longer'.

* A copy of the letter written by Claude Gadroys on the 8th August 1667 can be viewed or downloaded from: https://gallica.bnf.fr/ark:/12148/bpt6k1268937x/f1.item

This letter by Lamy is one of a number that were published regarding the potential usefulness or otherwise of the then 'new treatment of blood transfusion', performed on humans for the first time in Paris in June 1667, and presents important information regarding the attitudes, arguments and medical knowledge prevalent at the time. 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 a 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.

LETTER WRITTEN TO Mr. MOREAU,
Doctor of Medicine from the Faculty of Paris, Advisor, Physician, Reader and Ordinary Professor of the King;
BY G. LAMY
In which he confirms the reasons he gave in his first letter against the transfusion of blood, while answering the objections made to him.

Sir,
As I had acted with Mr. Denis in the most obliging manner in the world, and as my intention had been only to examine honestly the reasons which he proposed in favour of the transfusion of blood, and the experiences he had made of it. I was

extremely surprised when two days ago I read a letter written under the name of one of his students, in which I was rewarded for my courtesies by insults, and of my moderation by the most impassioned terms that anger and spite can supply. And as if these gentlemen had had no other intention than to treat me in all the most offensive manners, they did not propose a reasoning therein, that they did not fill it with a hundred insulting words; perhaps by a false policy, so that all those who would like to fight, as well as me, their new opinion were diverted from it by a way of acting so little in conformity with mine. But in truth, sir, my astonishment grew infinitely greater when I realized that they had had so little discretion to envelop you in their gossip, because you had the kindness to allow my letter to be dedicated to you, and because all the reputation that your merit gives you among intelligent people, could not prevent them from having feelings so far removed from verisimilitude. I would have an extreme regret that this letter which I addressed to you, had given them the opportunity to offend you, if I did not know that your virtue puts you above these feeble attacks, and if I were not sure that the calumnies of people, whose names we do not know, can in no way hurt the honour of a person who is esteemed by everyone, and who has other more important jobs than those which the examination of this novelty could furnish him. What seems to me more strange in their process, is that they pretend to be jealous of your glory, and that they try to persuade the less clairvoyant, by mischievously diverting the meaning of my words, that I have rather endeavoured to procure myself esteem, when I say that my reasonings do not displease you, than to do you honour by submitting them to your judgment. They do not recognize that I wanted to show the public, by this way of speaking, that you are not one of those strange minds who cannot approve of anything if it does not conform to their sentiments, but rather an honest man who has an esteem for everything that seems reasonably well imagined to you, although perhaps it does not seem real to you. As you know the sincerity of my thoughts, I am sure that you will not believe their impostures; so I only mention it in this letter with the intention of preventing others from being mistaken. I only beg you, Sir, to examine, with your usual kindness, the reply I am about to make to their letter, and to give me your judgment of it at your earliest leisure. It seems to me that one can very justly divide their letter into insults and objections. As for insults, as I have always considered them as sure marks of the weakness of mind of those who say them, they give me much more compassion than anger. And in truth I pity the misfortune that the authors of this insulting letter had to displease, by their way of acting, to all honest people, and to see that everyone condemned their letter to be corrected, when last Monday it was publicly read in the Assembly of Father Bourdelot, whoever it was dedicated to him, was the first to disapprove of it. I will therefore endeavour only to reply to the objections they have made to me; and I will even answer them as honestly as if they had proposed them with the politeness with which honest people, who are not preoccupied, are accustomed to examine the difficulties that are brought into conflict.

In the first objection they make to me, they find fault with the fact that I undertake to refute the experiments of Mr. Denis by simple reasoning, and in this they accuse me of a fault that I did not commit. Those who take the trouble to read my letter will easily know that I do not refute them; but on the contrary, let me assume them very obligingly in the manner in which he described them; and my intention was only to show that they are not sufficient to make admit the transfusion. I conjecture, however, that they mean that I must support my reasons by experiments; that is to say, to speak clearly, that they wanted me to put five or six people to death by the transfusion, in order to prove clearly that it is pernicious. For to experiment it on other animals, they would not have wanted to believe their death, even if it had happened; or at least they would have attributed it to the lack of skill of the surgeon, who would have performed this operation, as they insinuate at the end of their letter; although one of the most skilful surgeons of Paris assured in one of the conferences of Father Bourdelot, that a dog, on which he made the transfusion in the presence of

some famous doctors of the Faculty of Paris, fell into syncope, from which he barely brought him back, and died five or six days later.

In the second objection, the authors testify to being surprised that the content of my letter does not agree with the promises I make in the title. First of all, I do not show, from what they say that Mr. Denis gave a bad response to the objections he made in his letter. I beg you, Sir, to consider that they are of two kinds; some fight the possibility of transfusion, and thus I do not touch them, since experience shows me that it can be done; the others fight against its usefulness, among which I found only one which was considerable, and which deserved to be examined. But as I curiously had to give Mr. Denis the slightest opportunity to complain about me, and as it seemed impossible to me to be able to say my opinion on this objection, without his being offended, I thought it more appropriate to spare him by my silence. Nevertheless like his scholar urges me to discover my feelings, I will point out to all those who will want to take the trouble, that Mr. Denis, after having made a very strong objection to the transfusion, responds to an entirely different one; in such a way that he seems to have written the solution of a difficulty which he had only in thought, and not of that which he had written down on his paper.

It's on page 5, line 38, where he speaks in these terms: *It is claimed that all these parts*, he hear talks of those through which the blood passes while making its circulations, *come little by little, either by illness or by old age, to a certain degree of weathering and malignity, that it is finally impossible to withdraw them, and that in this state they have the strength to communicate their bad quality to all that approaches them, and thus they corrupt in a short time a laudable blood, with which they pretend to water them.* Then he wants to confirm this reason by the experience of a dog which had received the blood of another scabby dog, without the scab having been communicated to it. Judge, Sir, if this experience can support the objection he is making, and if there is any connection between the one and the other. The answer he brings is as far removed from the objection as the experience with which he claims to confirm it. Here are these own terms: *To answer all this in order, I say in the first place that this great intemperance, from which one wants the blood to be unable to return, is either rare or very common*. Then he continues to always talk about intemperance, and the malignity of the blood, and endeavours to prove that it can be corrected more easily by transfusion than by any other kind of remedy, without explaining how he can prevent this foreign blood from becoming corrupted by passing through spoiled and corrupted parts, which is the difficulty he set himself, and to which he forgot to respond. It is true that he could not, since the authors of the letter which I am refuting, assert in the story which they report of a Swedish Lord, on whom the transfusion was made some time ago, that Mr. Denis says she can't heal corruption from solid parts. This ingenuous confession, made in a few words, must stifle from birth the great hopes that have been conceived of the transfusion, and make even its approvers acknowledge that it can never bring very considerable utility. For all the diseases which occur without the internal parts being notably offended are very easily cured by ordinary remedies, and it is only the vice of these same parts which pains the physicians, and which resists their remedies with obstinacy.

For the reasons that Mr. Denis proposes in favour of the transfusion, to which I am reproached for not having given an answer, I did not bother, not being the same kind as it is made, of those by which he claims to show that it would be better to use the blood of animals, although it was very easy to prove the contrary. And I think I have sufficiently satisfied with the others, of which he makes use, to persuade his advantages in general, when I have proved in my letter that the blood of a beast cannot feed a man. I will say, however, in passing, that even though nature, as Mr. Denis says, seems to teach us about transfusion by the way in which she uses it to nourish the fetus in the womb of its mother, it does not follow for that that it is useful for curing diseases; and that there is a very large proportion between the mother's

blood and the fetus, which must feed on it, since it was partly formed from the seed of the mother, which was composed of particles of this same blood, which proportion is not found between the blood of an animal and the man, to whom it must serve as food.

You will also allow me, Sir, to tell you that I cannot agree with Mr. Denis, when he says that transfusion can remedy the loss of blood, and the haemorrhages, which cannot be stopped, are the cause of the death of sick people. For I do not think that foreign blood remains in the pierced vessels any sooner than the patient's own blood which has entered through their openings.

These gentlemen have attempted to destroy for two reasons, which I established at the beginning of my letter, when I asserted that foreign blood was mixed in small quantity with the own blood in the heart of the man. The first is that you can drain as much blood as you like, as long as you introduce new blood by transfusion; which does not bother me, since if this excessive evacuation took place suddenly, the patient would perhaps die at their hands; and if they do it at different times, it will be cured by bloodletting, without the need for transfusion; to which I add that if we reflect on the multitude of veins, and on the circular movement of the blood, we will easily recognize that we cannot prevent my supposition from always being true. I answered in my letter out of preconception of the second reason they gave, and so it would be useless to answer it again here.

They then attack with anger the general division that I have made of the internal causes of diseases; and claim that I was grossly mistaken when I said that all diseases, the cause of which is internal, generally proceed either from the abundance of the blood, or from its impurity, because the scarcity of blood that I had forgotten, is as fruitful a source of many diseases as the other two I have mentioned. But I cannot recognize the alleged error of which they accuse me, and I maintain that my division is very legitimate and very exact. For lack of blood cannot reasonably be counted among the first internal causes of disease, since it is usually only a sequel, and almost always presupposes some previous intemperance. It is easy to show this truth, especially since any decrease in blood coming from an internal cause, which is in question here, necessarily supposes either a defect in the parts which must convert food into praiseworthy blood, or defect in the food, which cannot be changed into blood well conditioned for the nourishment of the body, or an excessive heat which dissipates it and consumes it. And finally, so as not to make a detail that would be boring, I maintain that the scarcity of blood is always preceded by another disease, as if by a break in continuity, whether this break in continuity comes from an external cause, or whether it arises from the abundance or impurity of the blood; and thus it can always be related to the general division that I have made. But one should not be surprised that these gentlemen, who are not doctors, and who go beyond the limits of their profession, take me up on matters of medicine which are always very inappropriate.

But from whatever cause the scarcity of blood comes, they will be able to say that transfusion will be an excellent remedy to free the sick from the weaknesses which follow it, and to restore their initial vigour. I answer them, on condition that they will confess that my division was just, and that they were wrong to criticize it, that I doubt that it can be used in this meeting, although there is none more favourable for it; especially since I have shown that it is not advantageous to practice it in blood losses, and haemorrhages whose cause is internal, and that transfusion does not seem to be able to be useful to those who have lost excessive blood from large wounds, since physicians are usually obliged to cause them to bleed because of the fever which almost always accompanies them, or for some other known reason known to them. There is much more evidence that the transfusion would be harmful to them, then that there is an indication to draw blood from them. That if by misfortune an arm becoming untied after a bleeding, in a patient who is sleeping, caused a considerable loss of blood, the transfusion would perhaps be beneficial to

him, if the blood given to him could restore it; but as the blood of an animal of a different species, which these gentlemen claim to use, would not be suitable for nourishing it, the transfusion would be useless to it, and perhaps inconvenient.

When these gentlemen want to contradict me, in that I said that it would be ridiculous to propose transfusion to cure diseases which arise from the abundance of blood, and that it suffices to reduce the excess by bloodletting, they give an assured mark that the finest minds are liable to fall into very heavy faults. For I think that as nothing can ever be said more verily than what I have said; therefore one can never give an answer further removed from common sense than that which they have given. They say they know doctors, who assure that the blood never sins in quantity, but only in quality; if this paradox, manifestly contrary to experience and to everything that there has been of good doctors up to now, would not be obviously false, my proposition would nevertheless always be true, and one could only succeed in combating the general division that I hereby proposed of the internal causes of diseases. But in what school were they taught that plenitude never arrives except in appearance by warming the blood, and the great agitation it has in its vessels; if that were true, it would never meet without an extraordinary heat, and without fever: which nevertheless happens every day; and this plenitude of pure and praiseworthy blood is the good constitution, and so to speak the abundance of health, which the genius of medicine has commanded to diminish by the sake of foresight, so that the body without danger again resume good nourishment.

They will forgive me if I say that they have not read well what is written in my letter, when they hear that I said that the impurity of the blood results from an excessive heat which is found there. They are risking their reputation too much to propose something that anyone who can read will easily know is not true. Those who have the curiosity, will take the trouble to read in page 3, line 25, of my letter, and in page 6, line 4, of their answer.

These gentlemen then accuse me of having boldly determined what the entire Faculty of Medicine would not dare to do, when I proposed that I did not think that there were any cold illnesses. To recognize, Sir, that they impose on me a little too freely, and that they do not act with sufficient good faith, take the trouble to read on page 6, line 14, of their letter, and you will notice that they hear that I say that *the intemperance of the blood comes only from its excessive heat, and that I do not believe that there are cold diseases*. And after a while cast your eyes on page 3, line 27, of mine, and you will see that I claim that: *the diseases caused by the intemperance of the blood, draw their origin for the most part from an excessive heat which is met there*; and in page 5, line 29, *I do not believe in cold illnesses, or at least they are very rare*. After having conferred their quotation, with what I wrote there, you will know that they suppressed, on purpose in the first, *for the most part*, and in the second, *or at least they are very rare*; it was to take the opportunity to say to me all the insulting words which are found in the sequel; for if they had faithfully reported all that I wrote there, they would have had no pretext to get carried away, and to say that I boldly determine everything that the Faculty of Medicine would not dare to do, since to speak as I have done, and with the restriction that I bring, is not boldly determined.

But as these gentlemen try to bring me some illnesses, which they say are cold, it is appropriate to answer them, and to explain to them briefly my thoughts on this matter. When I say that I do not believe in cold illnesses, this must be understood from their antecedent cause, that is to say that I claim that there are no illnesses which are born of a cold bad intemperance of the blood, and not that there cannot be found illnesses which make one feel coldness, and which even have something cold for their joint cause, although heat is almost always the antecedent cause. It is as explained by it, so that I think, several famous doctors of your Faculty, and even a very famous of the number of the Royal Professors who teaches it publicly, and it is in this sense that I denied that there were cold illnesses, at least in such great

number, as these gentlemen imagine. This supposed, I maintain with much more skilful people than me, that the catarrhs, colds, flows and cold drops that these gentlemen propose to me, do not come from a cold intemperance which is in the blood, but rather from heat, like of the first cause which sets the mood in motion. For as we see that the rain, however cold it may be, owes its origin to the heat of the sun, which raises in the middle region of the air the vapours of which it is formed. Also it is likely that the heat of the entrails pushes vapours towards the brain, which are condensed there, flow to the lower parts, and produce there the diseases of which I have just spoken. For colic, which they number among the cold diseases, there are three kinds: known as, windy, bilious and nephritic. They have too much wit to think that the last two species proceed from coldness; and thus I believe that they meant to speak of windy conditions, the immediate cause of which is an air locked up in the colon, which, being unable to get out dilates it beyond measure, and excites unbearable pain. Now this air does not result from the coldness of the blood, but rather from flatulent foods, or from a bad fermentation of chyle. Finally, the paralyzes, which these gentlemen object to me, proceed from the obstruction of the nerves, which prevents the distribution of the spirits; either that in the nerve there is a body which obstructs it, or that outside there is something which compresses it: and as it matters little whether these bodies which clog or compress are cold or hot, one cannot say that the paralysis is necessarily born of the coldness of the blood; and thus if I had increased the proportion without excluding anything, as they impose on me, I would not be convinced of its falsity by the examples of the diseases which they proposed to me, the cause of which I explain quite well without their pretended coldness. But in truth it is going a little too far to want to oblige me to support a general proposition, when I have given it the restrictions that everyone can notice.

As I had shown in my letter that the arterial blood of an animal, of which Mr. Denis claims that it is better to be safe, having much more heat than the venal blood of a man could not cool it, they make many vain efforts to show the contrary. They do not, however, respond to the reason that I have given, but they repeat with great exaggeration the experience with which I tried to confirm it. All the same, they say, that whatever a broth makes the tongue and throat of him who tastes it feel warm, it does not follow that it should warm it up; also the foreign blood will not heat the own blood, although it makes the veins feel the heat through which it passes. All the same, they say, that whatever a broth makes the tongue and throat of him who tastes it feel warm, it does not follow that it should warm it up; also the foreign blood will not heat the own blood, although it makes the veins feel the heat through which it passes. If I had said that foreign blood should heat up, because it made its heat felt in passing, their comparison would be less imperfect. But I concluded that foreign blood, which caused the veins through which it passed to feel heat, was warmer than clean blood, which did not give them such a feeling; and from what I had shown that it was warmer, I believed that it followed quite reasonably, that it was more capable of warming the own blood than of cooling it. We cannot say the same thing about the broths we drink, which are always much less hot than the blood in the veins, although they produce a feeling of heat on the tongue. For it must be remarked that the tongue may feel less heat than the veins will not withdraw; which we will easily recognize, if we consider that the blood coming out of the vein, applied to the tongue, will appear very hot to it, although it does not make the vein feel any heat.

They pursue their reasons by proudly reproaching me for my ignorance. They accuse me of confusing actual heat with virtual one, and claim, so far as I can conjecture from the terms of their letter, that what is actually hot can have the virtue of cooling; and so that although the blood of an animal was actually as hot, or perhaps more, than that of man, it could nevertheless cool it by a virtual coldness. And I maintain that this is entirely impossible. I admit that experience shows me that what is actually cold can heat up, still isn't that too good to speak: but I won't be made to see what I think, that what is currently hot, can cool something about as hot.

The example of the spirit of vitriol, which grows hot in the veins, coagulates the blood and cools it, so they say, proves nothing to my dismay. Because coldness is not an effect which proceeds immediately from the spirit of vitriol, but is a result of the blood clotting which suffocates the spirits and therefore extinguishes their heat. I do not think that these gentlemen want to refresh the blood in such a way, otherwise it would only be necessary to use anything that can make us die, because all the causes of death extinguishing the heat are in this sense refreshing. These gentlemen, who want to pass for being very experienced, have nevertheless reported here experiences that are not true. Very knowledgeable chemists have assured me that they do not believe that the spirit of nitre, and the oil of tartar, can heat the blood, as they propose; and in fact there is much more likelihood that the spirit of nitre being acidic, like that of vitriol, would have the same effect; nevertheless not having had the leisure to experiment with it, I will not be sure; but I will certify in good faith that experience has shown me the opposite of what they say about lime, because hot water heats it up much more and more quickly than cold.

They also claim that I spoke against reason and against experience, when I said that the great quantity of own blood, mixed with the excessive heat which is found in the heart, will heat foreign blood in the same degree, rather than being refreshed; because, they say, if a pinch of hot water is capable of heating half a septier of cold, this small quantity of cold is also capable of cooling a little the great quantity of the hottest. But, Sir, I cannot help telling you that I am surprised how enlightened people, like these gentlemen, can make such a flawed comparison. They put half a septier of cold water on a full one of hot; can it ever be that they cause foreign blood to enter the heart in such proportion? Did I not demonstrate at the beginning of my letter that there was not found in the heart a quantity which was considerable? They compare cold water with hot water; but where can they find a blood which is in the same proportion of coldness with that of man, what is cold water with hot? Finally, they compare hot water, which does not have in itself a natural principle of heat, with blood, which is naturally hot, and whose heat is constantly maintained by the fire, which is always kindled in the heart, like in his fireplace. Let us make the comparison more just, and let us say that like a half septier of water which would have five or six degrees of heat, thrown into five or six pints of boiling water, the heat of which would always be preserved by the fire which boiled, could not cool, but on the contrary, would heat up like it; also the blood of an animal which is always very hot, meeting in small quantity in the heart of man, with his own blood, which is hotter than him, and whose heat is always nourished by the fire which makes us live, will receive a similar degree of heat, rather than temper it.

I have it seems to me very well demonstrated in my letter, that a chyle purposely composes refreshing juices, can, by continually resting in the heart, refresh it conveniently. But to add something to what I said; I beg you, Sir, to reflect on the nature of the fever, and to consider that I think with several expert doctors, that it is only an extraordinary fermentation of the blood, which can be stopped by alterative drugs, as we see that the fermentation of wine is arrested by milk, cheese, and other similar choices. That of beer, by vinegar and alum; and how in a word all fermentations can be impeded by several causes, as is very well proved by Kergerus, in the book he wrote on fermentation. Now, I do not see that a strange blood can arrest the fermentation of the blood of a man, as a little good wine cannot arrest the fermentation of another which is ready to corrupt, although the chyle which has been purposely made of suitable foods and remedies for this subject may very usefully and very advantageously do so.

These gentlemen affirm that there are certain chyles which give fever when approaching the heart; which transmitted blood does not: but it seems to me that they have not yet transfused men to whom chyle gave fever, to determine that transmitted blood does not.

After the authors of the letter which I refute, believed that they had sufficiently demonstrated that I had not succeeded well, in wanting to prove that transfusion could not cure the diseases which are born from the intemperance of the blood, they want to see, from what they say, if I will be happier in those which proceed from a particular malignity. But as they are a little too hostile to my happiness, they were afraid of meeting it on this occasion. This is why they did not respond to the reason I gave, to prove that it is not possible for the transfusion to cure these diseases, and that they only try to justify a comparison, which Mr. Denis makes on this subject, which I fought against, and which is not legitimate.

To achieve their purpose, they try to show that it is imprudent to say that the wine, which corrupts it, cannot be corrected by a little good wine: and to show my pretended ignorance, they say that everyone knows that the innkeepers strive to satisfy the diversity of tastes, by mixing wines from different regions. Judge, Sir, if the passion they had for ill-treating me by their words did not trouble their judgment? If satisfying the diversity of tastes was to correct the illnesses of wine, it should be said that water would also correct them, since we quite often see people to whom pure wine does not please, and who cannot drink it if they do not mix it with water? Is there anyone in the world so unenlightened, who, reading the terms of Mr. Denis, does not easily recognize that he is talking about correcting the bad qualities which can destroy wine, or at least that the comparison would be impertinent. We also want that by certain liqueurs, which Mr. Denis did not determine, he understood other wines which he did not want to name, because he supposed that everyone knew them; nevertheless he says in the first letter that they are secrets? Can we see a more manifest contradiction; those who want to disguise the truth must have a little better memory.

Now, to conceive that the bad qualities which tend to the corruption of wine cannot be corrected by the mixture of another better wine, it is necessary to know that in the wine which spoils, there is a fermentation which, if it was not prevented, would be followed by the entire corruption of the wine. This is why we try to stop this fermentation to prevent the principles of wine, which are in motion to separate, from becoming unborn; and this cannot be done by mixing a wine that can be enjoyed and well conditioned, but by other means that the curious will be able to read about in Kergerus and Vvillis. I am not unaware, however, that sweet wine which is not yet fermented, and which is not the one I spoke of in my letter, cannot clarify a cloudy wine, or give some strength to a weak wine, whose spirits will be puzzled by its phlegm and its earthly parts, by exciting a fermentation which can exalt the spirits, or precipitate the faeces; but I deny that a good wine ready to drink can arrest the fermentation of that which is going to be corrupted; and thus the comparison that I made of wine with blood, is not only much more accurate than that of Mr. Denis, but also very useful, to show that a pure and well conditioned blood cannot prevent the corruption of the one who spoils himself.

I will pass here several objections, the answer to which is easy, in order to examine whether the explanation which I have given to the experiments of Mr. Denis is as unreasonable as these gentlemen imagine. They say first that if fear could have set the mind of this young man in motion, he would have been freed from the annoying accidents which left him with fever, twenty-four hours before the transfusion, by the apprehension of the fall which happened to him at that time; but these gentlemen confuse, it seems to me, two very different passions, and whose movements are not alike; fright must be carefully distinguished from fear. The first is a passion which is born on the occasion of a great evil ready to overwhelm us, and which surprises us by its unexpected arrival, and the other is a passion which proceeds from an evil which we truly foresee, likewise we might be able to arrive, the outcome of which nevertheless appears a little doubtful. One pushes the spirits back in an instant, and arrests their movement in such a way that it sometimes causes sudden death, and the other disturbs our rest, agitates the spirits extraordinarily, and

prevents sleep; which causes some to confuse it with anxiety. The latter may well clear the spirits; but the first does not. Now the apprehension that this young man had in falling was a fright and not a fear, if they want to add to this fear the pain they caused him during the operation, I will not be sorry: for it seems to me that it could have contributed a great deal, just as we see that the pains caused to lethargics bring them back from their drowsiness.

They say in the second place, that this young man had no fear; but Mr. Descarte, very knowledgeable doctor of the Faculty of Paris, whose faith cannot be suspected by those who know his virtue and his sincerity, will assure you that an interested person in Mr. Denis's party told me in his presence that this young man had been blindfolded, as one does to those to those whose head is about to be cut off. Judge, sir, if this single circumstance was not capable of moving him and of making him believe that the success of this remedy could be unfortunate. Besides, it cannot be that he was stupid enough not to notice that this remedy was unusual, seeing the circumspections and the ceremonies they brought to make it. In vain do they sing the contrary: for as I am sure, Sir, that they have put in their letter, against you against me, five or six considerable impostures, I also think that they are quite capable of imposing on everybody. What I find pleasing is that they said in their second letter that they drew some blood from this young man after the transfusion, and compared him to the one that had been drawn from him before; because I had aduerty [sic] in mine that it would have been necessary to make sure of the experience.

As it is one of the main points of dispute between these gentlemen and me, to know if a foreign blood which has been passed through the veins of a man will be fit to nourish him, and that it is a question which deserves serious consideration. I beg you, Sir, to allow me to expand a little on this matter. I proposed in my first letter, that as it is not possible for an animal to be generated from the seed of another of a different species: likewise, it is not likely that he can feed on his blood; which I have tried to prove by reasons which I find quite strong, and to which it seems to me that they did not give a good answer. Because everything they bring serves much more to confirm what I have advanced than to destroy it. They say that females nourish in their womb, by the transfusion of their blood, fetuses of different species, and which have been understood by the seed also of males of different species. Whereupon I beg you to notice, that when an animal begets from two others different in species, it participates in the nature of one and the other, and resembles them outwardly. As one can easily see in the jumars and the mules: which confirms my opinion admirably well, and proves quite clearly, that there are, as I said, in the seed of each animal particles formed in such a way, that if they are put into a constant movement, they will arrange themselves to form an animal similar to that from which they emerged, and will never be able to produce another of different nature, if not by the mixture that they can have with the particles of a seed of different species, and in this meeting the animal produced is like a compound of the two natures. This is easily noticed in the examples that I have reported, and there is nothing wonderful in a mule being fed on the blood of a horse, since it was partly formed from its seed. But it is necessary to make here a rather curious remark, which very powerfully confirms my thought, knowing that the animals engendered by a male and a female of different species, participate much more in the nature of the mother than in that of the father; because besides being partly formed from his seed, as well as that of the father, they still have the advantage of having been nourished with her blood in the womb, and of having sucked the milk from her during their youth. This is why those who have described the means of obtaining good mules have remarked with much reason that it was much better to couple donkeys with horses than horses with asses.

From all that I have said, one can, it seems to me, fairly conclude, that as the seed is characterized to produce an animal similar to that from which it sprang, without the mixture of another of diverse nature may prevent him from giving evident

marks of it; in the same way the blood is composed, so that it can very well restore the parts of the animal in which it is formed, without another of a different species being able to rob it of the impression and the characteristics which make it proper there.

These gentlemen are almost always deceived by the comparison they make of food taken by mouth with transmitted blood, and conclude from one to the other what is not tolerable, since the foods we take lose their nature entirely by all the various changes they suffer in the mouth, in the stomach, and in all the other parts through which they pass before arriving at the heart, and by the separation which is made in the intestines of the parts proper to nourish man, and to serve his functions with those which are not. Now the blood transmitted cannot have all these changes, but such as it enters the veins, and disgorges it in the heart, or according to the opinion of the Cartesians, being only rarefied, it is afterwards pushed into the arteries, for in this state supply of nourishment to all parts of the body. Nevertheless, to give colour to their comparison, they advise me to study medicine for a while longer, to learn that there are three coctions made in our bodies, the first of which is made in the stomach, and is not, so they assure us, significant, who unanimously assure that the first coction is of such importance that the faults sometimes encountered in it can never be corrected by subsequent ones.

It now remains to examine whether the transmitted blood can be rendered fit to feed man, by circulating several times through the heart. For me I think this is not possible; because it would be necessary that the particles of this foreign blood, which are characterized and figured, in order to repair the daily loss that the solid parts of the animal, whose blood has been taken, could change shape, and receive a clean one, to restore the ruin that is done in the parts of the man, by the mixture which they would have with his own blood; which does not seem to me to be able to happen, as I have proved by the comparison which I have made before with the seed. And indeed if the blood of man could make foreign blood fit to nourish it, would it not also have the strength to corrupt it: or on the contrary, if foreign blood can correct the corruption of the blood of man, will it not have the virtue of making it similar to itself, as to the shapes of the particles of which it is composed.

They reply that the diversity of the figures perhaps comes only from the diversity of the pores which screen them, and not from the coctions. But I don't know what all these different mixtures would do, if what they say were true; and it is difficult to imagine that the sieves give the figures, since it seems that they are made only to separate things variously represented. The example they are looking for among the trees confirms what I am saying, for there are many that die when they are grafted onto each other, and a plant does not grow on all kinds of soil, for which no other reason can be given, except that there are not in the juices of trees proper corpuscles to pass into the fibres of all the others, nor in every part of the earth particles well enough formed to enter the pores of the roots of all strong plants; and when a tree grows haunted on another, it must be concluded that there are in the juices of this tree different atoms for the different pores of one and the other. The experiments which they have made, when they are true, do not convince; because not being able to do that they have drawn all the blood from an animal to give it new blood, the stranger always finds himself mixed up with his own. So that we must believe that if some animals escape from this operation without being inconvenienced, they have very vigorous blood, which has been purged like excrement of the foreign blood that has been given to it by the common way that purges the blood, either naturally or by means of remedies.

I want, Sir, before finishing this letter, to ask you to observe with me that these gentlemen are always unfair in their comparisons. As there are old people, they say, who feed on the milk of animals, without however contracting brutal inclinations; so it may be that they receive their blood without contracting these same inclinations. As

if the same thing could be concluded from the blood transmitted, as from food taken by mouth.

Let us therefore conclude rather, that as milk can give brutal inclinations to children, according to the report of the histories, even though this milk suffers many changes, and is purged of its coarse parts, before mixing in the blood; in the same way the foreign blood put without any alteration in the veins of a man will communicate to him inclinations in conformity with the nature of the beast from which he was drawn.

In truth, Sir, I must tell you that it is a pleasant thing to see the trouble that these gentlemen take to tell the story of the Swedish Lord at length, as if they had raised him to life by their transfusion. They accommodate everything to their advantage; they guess the causes of his illness and his death, after having recognized them by the opening they made of his body: but it seems to me that if they wanted to avoid the reproach of not having acted like prudent physicians, and to exempt themselves from the blame of having tried their remedy inappropriately, they should not boast of having been such skilful people in the diagnosis; and the last reflection that I beg you to make on this matter, is that if the dead succeed as well in confirming the usefulness of their remedy as those who by good luck will survive, there is no appearance of being able to show that the transfusion is pernicious.

I hope, Sir, you will judge by my answers that their objections do not have as much force as they imagine, and that if they had been a little less preoccupied, they would not have fallen into such furious outbursts. You will perhaps be surprised that I have always spoken in plural of the authors of this letter, as if I believed that they had been several to compose it. I confess, Sir, that it was my first thought, and that I was persuaded that Mr. Denis had furnished the reasonings, and his scholar the insults: for I did not think that a man who professes to be a philosopher could have enough baseness to say a thousand outrageous words to a person who has treated him, like me, with all sorts of civility. But as I finished this letter, I was obliged to change my feelings, having learned, as you know, from one of the friends of the alleged author, that he had committed no other fault than to swear his name to Mr Denis, whose behaviour he even disapproves of, and against whom he is as if scandalized to have put his name at the end of a letter, which has no greater proof than ridiculous suppositions against you, and insults in every way against me.

As for you, Sir, I hope that you will receive my reply with as much kindness as you have received my letter, although I am sure that you do not entirely approve of Descartes' principles, of which I have been obliged to help me to fight his cultists with more strength, and I also think that you know enough about my intention, to judge that all that I propose is only in order to better examine the transfusion, as well as to reduce it in practice, and to testify to you, Sir, in you making the judge of my reasonings that I have for your deserved, all the esteem that a young man like me can have who am without disguise,

Sir,

Your most humble servant,

Lamy

In Paris, 26 August 1667