

THE FIRST HUMAN BLOOD TRANSFUSION

by

A. D. FARR*

INTRODUCTION

THE OLD concept of the movement of blood, based upon the teaching of Galen, was that it was constantly formed in the liver, passed to the heart, and from thence by means of the arteries and veins to the tissues where it was burned up, as wood is consumed by fire.¹ In such a belief there was no logical reason to propose a transfusion of blood for purposes either of restoring "diseased" blood or of replacing blood lost by haemorrhage, and it was not until Harvey's discovery of the continuous circulation of blood around the body (first announced in 1616² and formally published in 1628³) that such a procedure suggested itself with any force. Then, as Sir Geoffrey Keynes has pointed out, "There were several phases to be gone through: (1) The formulation in men's minds of the *idea* of introducing other fluids and drugs in solution into the circulation of an animal; (2) The extension of the idea to include the introduction of blood; (3) The preliminary experiments on the circulating blood of animals; (4) The application of the results to the therapeutic use of blood transfusion in man."⁴

The first authenticated successful intravenous infusions into living bodies were carried out by the astronomer, and subsequently architect, Dr. (later Sir Christopher) Wren who in a series of experiments⁵ dating from 1657 injected a number of fluids into the veins of animals so that "By this *Operation* divers Creatures were immediately purg'd, vomited, intoxicated, kill'd, or reviv'd, according to the quality of the Liquor injected: Hence arose many new *Experiments*, and chiefly that of *Transfusing Blood*".⁶ The transfusion of blood from one animal to another was first undertaken for the Royal Society⁷ during 1665, when dogs were the principal experimental animals,⁸ and

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¹Cf. C. Singer and E. A. Underwood, *A short history of medicine*, 2nd ed., Oxford, Clarendon Press, 1962, p. 65.

²*Praelectiones anatomiae universalis*, London, Churchill, 1886. (Facsimile reprint of Harvey's manuscript notes for his 1616 Lumleian Lecture. English translation with annotations, Berkeley, University of California Press, 1961. Edited, with an introduction, translation, and notes by G. Whitteridge, Edinburgh, Livingstone, 1964.)

³W. Harvey, *Exercitatio anatomica de motu cordis et sanguinis in animalibus*, Frankfurt, 1628. The text in English, translated by K. J. Franklin, is published in *William Harvey. The circulation of the blood, and other writings*, London, Dent (Everyman), 1963.

⁴G. Keynes, *Blood transfusion*, Bristol, Wright, 1949, p. 4.

⁵Described in detail in R. Boyle, *Some considerations touching the usefulness of experimental naturall philosophy*, Oxford, H. Hall for R. Davis, 1663, part 2, pp. 62-65.

⁶T. Sprat, *History of the Royal Society of London*, London, T. R. for J. Martyn & J. Allestry, 1667, p. 317.

⁷Wren was one of the most prominent founder members of the Royal Society, which received its royal charter in 1662.

⁸Thomas Coxe reported to the Royal Society on 24 May 1665 "that he had made an experiment of injecting blood of one pigeon into the vein of another", but this was neither attested nor (apparently) repeated. 'Journal Book of the Royal Society', in T. Birch, *History of the Royal Society*, London, A. Millar, 1756, vol. 2, p. 50.

the first successful blood transfusion was performed in February 1665 by Richard Lower,⁹ the details being subsequently given in his book on the heart.¹⁰

Although the most widely reported “first” transfusion of a man is that of Arthur Coga, “an indigent Bachelor of Divinity, of Cambridge,”¹¹ on 23 November 1667, this was only the first human transfusion in England,¹² pride of place for the operation having gone five months previously to a Frenchman. The report of this latter operation had been sent promptly to England and published, but it was then suppressed, and few copies of the account have survived.

PUBLICATION OF THE REPORT

The original report was made in the form of a letter to Henri de Montmort¹³ by Jean Denis,¹⁴ Professor of Philosophy and Mathematics in Paris, and was dated 25 June 1667. This letter was published in Paris¹⁵ and a copy sent to the Royal Society in London.

As a group of scientists and philosophers had come together in London and founded the circle which subsequently became the Royal Society,¹⁶ so in Paris did similar assemblies gather. In particular a private “Academy” – interested more in science than the arts – met each week at the Quai des Augustins, in the home of de Montmort, who was Counsellor of State and first Master of Requests to Louis XIV; and it was in this assembly, which in 1666 became the Académie des Sciences under the king’s patronage, that much of the early work on transfusion in France was carried out.

Prominent among the members of de Montmort’s Academy was Jean-Baptiste Denis (c. 1635-1704).¹⁷ Aged about thirty-two, Denis was a brilliant young man who had first obtained the degree of Bachelor of Theology and then gone on to study medicine at Montpellier, gaining his doctorate in 1667 and in the same year being appointed Professor of Philosophy and Mathematics in his native Paris. De Montmort had apparently taken up Denis as his protégé, and encouraged him to experiment with blood transfusion.

⁹ R. Lower, ‘The method observed in transfusing blood out of one live animal into another’, *Phil. Trans. R. Soc. Lond.*, 1666, 1: 353-358.

¹⁰ R. Lower, *Tractatus de corde*, London, J. Allestry, 1669.

¹¹ Keynes, op. cit., note 4 above, p. 16. This description is strictly inaccurate. Coga actually graduated as a Bachelor of Arts at Pembroke College, Cambridge, in 1659-60. (J. Venn and J. A. Venn, *Alumni Cantabrigienses*, Cambridge University Press, 1922, vol. 1, p. 365).

¹² The transfusion was performed by Richard Lower and Edmund King in London, at Arundel House “in the presence of many considerable and intelligent persons”, and reported by King in ‘An account of the experiment of transfusion practised upon a man in London’, *Phil. Trans. R. Soc. Lond.*, 1667, 2: 557-559.

¹³ Henri Louis Habert de Montmort (died 1679). The name is spelled Montmor in *Phil. Trans.* on more than one occasion, but this is apparently a vagary of either the translator or the printer, as it appears consistently as Montmort in French reports. The only biographical note of de Montmort which has been traced is a very brief entry in *Grand Larousse encyclopédique*, Paris, 1962, vol. 5, p. 737.

¹⁴ Some modern commentators have spelled the name Denys (e.g., see note 4 above, pp. 12-15), but the contemporary usage was generally Denis in both French and English sources.

¹⁵ J.-B. Denis, *Lettre écrite à Monsieur de Montmort. . . . touchant une nouvelle manière de guérir plusieurs maladies par la transfusion du sang, confirmée par deux expériences faites sur des hommes*, Paris, Jean Cusson, 25 June 1667.

¹⁶ See note 7 above.

¹⁷ For biographical details of Denis, see *Dictionnaire de biographie français*, Paris, Letouzey, 1965, vol. 10, p. 1034; also the study in J.-J. Peumery, *Les origines de la transfusion sanguine*, Amsterdam, Israël, 1974, pp. 27-30.

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Assisted by a surgeon, Paul Emmerez,¹⁸ Denis performed a number of experiments transfusing dogs during the first half of 1667,¹⁹ and these were reported in the form of letters²⁰ printed in the *Journal des Sçavans* in March²¹ and April²² of that year. The first transfusion of a man took place shortly after 15 June 1667, followed by a second a few days later, and these cases were also reported in the form of a letter²³ – this time to de Montmort. The letter format was probably little more than a device, as that of 25 June was immediately printed and a copy sent to London, to the Royal Society, where it was produced at a meeting on 4 July²⁴ by Dr. Wilkins, one of the secretaries.²⁵

On 22 April 1663 the Royal Society had appointed Wilkins and Henry Oldenburg (c. 1615-1677) to be joint secretaries.²⁶ Oldenburg, who had been one of the first members of the society²⁷ and a member of the first council,²⁸ was a German-born “philosopher and man of letters”²⁹ who had settled in England. As secretary Oldenburg at first received no salary³⁰ and, being perpetually hard-pressed for money, was authorized in 1664 to publish the “Transaction of the Society”³¹ – although the net profit of this seldom amounted to £40 a year.³² As editor of the *Philosophical Transactions* Oldenburg was instructed that each issue “be licenced by the council of the society, being first reviewed by some of the members of the same”³³ and, apart from the periods of the plague and the aftermath of the fire of London, this was generally done:³⁴

¹⁸ Paul Emmerez (or Emerrez) (d. 1690) was born in Saint Quentin and was known as a skilful surgeon and first-class teacher of anatomy and surgery. He was at one time Provost of the Society of Surgeons of Paris. For biographical notes, see N. F. J. Eloy, *Dictionnaire historique de la médecine*, Frankfurt and Liège, Bassompierre, 1778, vol. 2, p. 138.

¹⁹ These were not the first animal transfusions in France, however. The first was performed for the Académie Royale des Sciences on 22 January 1667. The report of this was first published in full in English in H. E. Hoff and R. Guillemin, ‘The first experiments on transfusions in France’, *J. Hist. Med.*, 1963, 18: 103-124.

²⁰ The letters are addressed simply “à M. . .”, but it is most probable that they were to Henry Oldenburg, secretary of the Royal Society. See Peumery, *op. cit.*, note 17 above, p. 19.

²¹ J.-B. Denis, ‘Extrait d’une lettre de M. Denis, professeur de philosophie et de mathématique, à M. . . . touchant la transfusion du sang. De Paris, ce 9 Mars 1667’, *J. Sçavans*, 14 March 1667, pp. 69-72.

²² J.-B. Denis, ‘Extrait d’une lettre de M. Denis, professeur de philosophie et de mathématique, à M. . . . touchant la transfusion du sang. Du 2 Avril 1667’, *ibid.*, 25 April 1667, p. 96.

²³ See note 15 above.

²⁴ ‘Journal Book of the Royal Society’, in Birch, *op. cit.*, note 8 above, p. 186.

²⁵ Dr. John Wilkins (1614-1672), a founder member of the Royal Society. He resigned his secretaryship in December 1668 on his appointment as Lord Bishop of Chester. *Ibid.*, p. 331.

²⁶ *Ibid.*, vol. 1, p. 236.

²⁷ He was proposed on 26 December 1660. *Ibid.*, p. 8.

²⁸ *Ibid.*, p. 236.

²⁹ *Dictionary of national biography*. This very full article by a some-time assistant secretary of the Royal Society cites all other known biographical sources for Oldenburg.

³⁰ The council did, however, make occasional *ex gratia* payments to Oldenburg “for the great pains, which he had taken in behalf of the society”. Birch, *op. cit.*, note 8 above, vol. 1, p. 135 (£40 on 21 December 1666); p. 273 (£50 on 27 April 1668). On 3 June 1669 the council “ordered that a salary of forty pounds a year be allowed to Mr. Oldenburg, one of the secretaries of the society, from the time, that the last present was ordered to him.” *Ibid.*, p. 376.

³¹ “It seems probable that there was a tacit agreement between Oldenburg and the Council that he should reimburse himself in some measure for his time and trouble as Secretary by the publication of the *Philosophical Transactions*.” R. K. Bluhm, in H. Hartley (editor), *The Royal Society: its origin and founders*, London, Royal Society, 1960, p. 192.

³² *Ibid.*, pp. 192-193.

³³ Birch, *op. cit.*, note 8 above, vol. 2, p. 18.

³⁴ *Ibid.*, *passim*. Up to 1668 there is no record of the “licensing” of issues nos. 6-8 (period of the plague); 12; 14; 18-20 (period of the fire of London); 22; 27-28.

nevertheless, Oldenburg retained firm editorial control over the selection of manuscripts.

Oldenburg maintained a vast foreign correspondence, and, although most of this was scientific, some of it was political. As a result of an injudicious criticism of the conduct of the war then being fought with Holland, which he had apparently made in one of his letters, Oldenburg incurred the displeasure of King Charles II and was arrested under warrants dated 20 June 1667, and imprisoned in the Tower of London³⁵ where he remained for more than two months before being released on 26 August.³⁶ This imprisonment was important for the history of blood transfusion as it was during this period that the letter from Denis, describing the first blood transfusion, was received by the Royal Society.

The twenty-seventh issue of the *Philosophical Transactions* was due to be published in July 1667 and, with its editor in the Tower, another (unknown) hand prepared Denis' letter for press as No. 27, dated 22 July. There was clearly some irregularity in the production of this issue, for while it carried the correct date, sequential page numbers, and issue number, it did not carry the heading *Philosophical Transactions* (Figure 1). It is highly probable that Dr. Wilkins, as joint secretary, was responsible for this issue but no record of it being "licensed" by the council of the society appears in its Journal Book.³⁷ There was no issue of the *Philosophical Transactions* published in August, and Oldenburg was released late in that month before a September issue was called for.

On his return Oldenburg seems to have disapproved of the publication of Denis' letter. A new issue of No. 27 was prepared and published on Monday 23 September 1667 (Figure 2), being dated "For the months of *July, August, and September*". In this issue pages 489-504, which in the original No. 27 had constituted Denis' letter, were replaced by a selection of other items.³⁸ Oldenburg wrote on the new opening page (489): "The Author of these Papers, returning now to his former Exercises, which by an extraordinary Accident he was necessitated to interrupt for some months last past, thought fit to comprise the *Transactions* of all the Months omitted in one *Tract*:
...'³⁹

The reason for Oldenburg's disapproval of Denis' letter apparently lay in an assertion in its opening sentence referring to "The project of causing the Blood of a healthy animal to passe into the veins of one diseased, having been conceived *about ten years agoe*, in the illustrious Society of *Virtuosi* which assembles at your house". Oldenburg noted:

³⁵ For an account of this incident, see D. McKie, 'The arrest and imprisonment of Henry Oldenburg', *Notes and Records*, 1949, 6: 28-47.

³⁶ In his diary (entry for 25 June 1667) Pepys, commenting upon Oldenburg's imprisonment and the reason for it, dryly noted that this "makes it very unsafe at this time to write, or almost do anything". R. Latham and W. Matthews (editors), *The diary of Samuel Pepys*, London, Bell, 1972, vol. 8, p. 292.

³⁷ Birch, op. cit., note 8 above, vol 1, *passim*.

³⁸ Because of the confusion of two different issues of no. 27 of *Phil. Trans.*, each carrying the page numbers 489-504, they will be separately referred to as: (a) J.-B. Denis, 'A letter concerning a new way of curing sundry diseases by transfusion of blood. Written to Monsieur de Montmor, Counsellor to the French King, and Master of Requests', *Phil. Trans. R. Soc. Lond.*, 1667, 2 (27A): 489-504. (b) H. Oldenburg, 'An advertisement concerning the invention of the transfusion of blood', *ibid.*, 1667, 2 (27B): 489-504.

³⁹ At least two different versions of the new page 489 (note 38b above) appeared. The texts of these were the same, but the printer's layouts were different.

A LETTER

Concerning a new way of curing sundry diseases by Transfusion of Blood, Written to Monsieur de MONTMOR, Counsellor to the French King, and Master of Requests.

By J: DENIS Professor of Philosophy,
and the Mathematicks.

Munday July 22. 1667.

SIR,



THE project of causing the Blood of a healthy animal to passe into the veins of one diseased, having been conceived about ten years ago, in the illustrious Society of *Virtuosi* which assembles at your house; and your goodness having received M. Emmeriz, & my self, very favorably at such times as we have presum'd to entertain you either with discourse concerning it, or the sight of some not inconsiderable effects of it: You will not think it strange that I now take the liberty of troubling you with this Letter, and design to inform you fully of what pursuances and successes we have made in this Operation; wherein you are justly intitled to a greater share than any other, considering that it was first spoken of in your *Academy*, & that the Publick is beholding to you for this as well as for many other discoveries, for the benefits & advantages it shall reap from the same.

But that I may give you the reasons of our procedure & convince

C c c

vince

Figure 1

PHILOSOPHICAL TRANSACTIONS.

For the Months of *July, August, and September.*
Monday, Septem. 23. 1667.

The Contents.

An Advertisement concerning the Invention of the Transfusion of blood. An Account of some Experiments of Infusing Liquors into the Veins of Animals; As also, of some new discoveries pretended to be made in the ~~Structure~~ of the Brain and the Tongue. An Experiment upon blood grown cold. Some Observations of Quicksilver found at the roots of Plants; and of Shells found upon in-land mountains; Other Observations made by a curious person in his Voyage from England to the Caribes, concerning the rasting of Iron by the Sea-air; the Changes of Thames-water carried by sea; The Variety of the Colours of the Sea; The burning of the same; the Night-winds in the Indies; The Relations of the Seasons of the year rectified; Observables about Tortoises; The condition of English bodies first coming to Jamaica; A way of preserving Ale, as far as to the same Island. An Extract of a Letter concerning some Magnetical Experiments; and an Excellent Liquor made of Cyder Apples and Mulberries. An Account of two or three Books; One, THE HISTORY of the ROYAL SOCIETY: The other, DISQUISITIO DE FRTU FORMATO, The third, MUSCULI DESCRIPTIO GEOMETRICA.

*An Advertisement concerning the Invention of the
Transfusion of Blood:*

THE Author of these Papers, returning now to his former Exercises, which by an extraordinary Accident he was necessitated to interrupt for some months last past,
Ccc thought

Figure 2

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that if himself had published that *Letter*, which came abroad in July last, *Concerning a new way of curing sundry diseases by Transfusion of blood*, written to Monsieur de Montmor, &c. by J. Denis Prof. of *Philosophy*, &c. he should then have taken notice, as he doth now, of what is affirmed in that *Letter* about the time and place of the Conception of that Transfusing design; and intimated to the Curious, that how long soever that Experiment may have been conceived in other parts (which is needless to contest) it is notorious, that it had its birth first of all in England; some Ingenious persons of the *Royal Society* having first started it there, several years ago, (as appears by their *Journal*) and that dextrous Anatomist, D. Lower, reduced it into practice, both by contriving a method for the Operation, and by successfully executing the same: wherein he was soon overtaken by several happy Trials of the skilful hand of Doctor Edmund King, and others, encouraged thereunto by the said Society; which being notified to the world *Numb.* 19 and 20. of these *Transactions*, Printed *Novem.* 19. and *Decemb.* 17. 1666; the Experiment was, soon after that time, heard off to have been tried in forrain parts, without hearing anything of its having been conceived ten years ago.⁴⁰

Whether the German-born and -educated Oldenburg had become so attached to his adopted country that he was behaving in a manner more English than the English, or whether he was merely jealous of the Royal Society's undoubted priority in the practice of animal-to-animal blood transfusion is not clear, but he continued to react strongly to any suggestion of French priority in this new "Invention". In the following month's issue of the *Philosophical Transactions*, concluding a general "Account of more Tryals of Transfusion"⁴¹ he felt "obliged to remove a mistake found in one of the late French Journals, affirming with confidence, that 'tis certain, the French have given the English the first thought or notion of this Experiment". He then gave a detailed résumé of the English experiments recorded – and published – by the Royal Society during 1665-1666,⁴² adding the comment that "it seems strange, that so surprising an Invention should have been conceived in France, as they will have it, ten years ago, and lain there so long in the womb, till the way of Midwiving it into the world was sent thither from London".⁴³

Whatever his motives, Oldenburg managed to suppress the letter of Denis and replace it with his own July-September issue of the *Philosophical Transactions*, also marked No. 27.

ACCESSIBILITY OF THE REPORT

It has been alleged that copies of the suppressed issue of the *Philosophical Transactions* consisting of Denis' letter to de Montmort are "now of great rarity",⁴⁴ and in an attempt to discover the extent of its present distribution a survey has been made of thirty-three locations in the United Kingdom where there was reason to believe that copies of the *Philosophical Transactions* for 1667 were held. These locations consisted of the twenty-three listed in the *British Union Catalogue of Periodicals*

⁴⁰ Op. cit., note 38b above, p. 490. The English reports referred to appeared in *Phil. Trans. R. Soc. Lond.*, 1666, 1 (19): 352; and 1 (20): 353-358. On 21 March 1667, "Mr. Oldenburg produced an account, which he had received from Paris, of the success, which the curious had met with there in the same experiments" [i.e., transfusion of dogs]. Birch, op. cit., note 8 above, vol. 2, p. 161.

⁴¹ 'An account of more tryals of transfusion, accompanied with some considerations thereon, chiefly in referrence to its circumspect practice on man; together with a farther vindication of this invention from usurpers', *Phil. Trans. R. Soc. Lond.*, 1667, 2 (28): 517-525.

⁴² *Ibid.*, p. 524.

⁴³ The dispute 'The first blood transfusion: French or English?' has recently been studied in depth by M. T. Walton in a paper of that title in *Med. Hist.*, 1974, 18: 360-364. He concluded that: "With neither side able to claim the conception of transfusion, the English had to be content with having performed the first recorded procedure and the French with the pioneering of human transfusion." (p. 362).

⁴⁴ Keynes, op. cit., note 4 above, p. 12.

(BUCOP)⁴⁵ and ten others known to the present author. This survey revealed twenty-three sets of the original publication.

Of the thirty-three libraries investigated, no details were available from one of those listed in BUCOP (the Central Library at Brighton), and one reported having no holding of the issues for 1667 (University of Bristol). Two sets of abridged versions of the *Philosophical Transactions* for this period have been published, one by John Lowthorp in 1705,⁴⁶ and one by Hutton, Shaw, and Pearson in 1809.⁴⁷ Both abridgements refer only to the paper by Oldenburg in the July-September 1667 issue and neither makes any reference to Denis' letter. Six libraries hold only one or other of these abridged versions: University College, Cardiff; the Central Library, Dundee; the Royal College of Physicians and Surgeons, Glasgow; the Royal Botanic Gardens, Kew; the Central Library, Newcastle upon Tyne; and the District Central Library, Preston.

Two facsimile reprints of the early volumes of the *Philosophical Transactions* have been made. In 1963 the Johnson Reprint Corporation of New York issued a set in which the 1667 issue contained only the Oldenburg paper, and not Denis' letter, and this reprint only is held by four libraries: University of Birmingham; British Library Lending Division, Boston Spa; John Rylands Library, University of Manchester; and the Wellcome Institute for the History of Medicine, London.

In 1963-64 a facsimile reprint was published jointly by De Graaf of Nieuwkoop and Israël of Amsterdam. This copy was made from that in Amsterdam University Library and contains both Denis' letter and Oldenburg's paper. Only 300 copies were printed and the edition is now out of print.⁴⁸ Only one British library reported holding a copy of this reprint: the Central Library at Leeds.

Of nineteen libraries holding bound volumes of the original issues of the *Philosophical Transactions* for 1667 twelve (with holdings of fourteen copies) possess only the July-September issue with Oldenburg's paper. These are the University of Aberdeen; the City Reference Library, Birmingham; St. John's College, Cambridge; University of Edinburgh; University of Glasgow (two copies); British Library Reference Division, London (two copies); the Royal College of Surgeons, London; the Royal Institution, London; University College, London; the University of London Library; the Radcliffe Science Library, Oxford; and the private library of Sir Geoffrey Keynes. Also, one of two copies in the Bodleian Library, Oxford, contains only the Oldenburg paper. In the British Library (Reference Division), London, there is additionally a seventeenth-century translation into Latin of the *Philosophical Transactions*,⁴⁹ which contains only the paper by Oldenburg.

Only eight copies of Denis' letter appear to have survived in original copies of the *Philosophical Transactions* which have been located in Britain: these are as follows:

(i) University Library, St. Andrews. This is the only No. 27 in the set.

⁴⁵ *British Union Catalogue of Periodicals*, London, Butterworth, reprinted 1970.

⁴⁶ J. Lowthorp, *The Philosophical Transactions and Collections to the end of the year 1700, abridg'd and disposed under general heads*, London, T. Bennet, 1705.

⁴⁷ C. Hutton, G. Shaw, and R. Pearson, *The Philosophical Transactions of the Royal Society of London . . . abridged, with notes and biographic illustrations*, London, Baldwin, 1809, vol. 1.

⁴⁸ De Graaf, personal communication, 1979.

⁴⁹ *Acta Philosophica Societatis Regiae in Anglia. Anno 1667*, Amsterdam, 1672.

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- (ii) Trinity College, Cambridge. This set has both issues of No. 27.
- (iii) Royal College of Physicians, London. This set has both issues of No. 27.
- (iv) The Royal Society, London. This set has both issues of No. 27.
- (v) The Royal Society of Medicine, London. This is the only No. 27 in the set.
- (vi) The main Bodleian Library, Oxford. Bound into the back of one of two odd copies of this volume.⁵⁰ Oldenburg's paper is also bound in sequence in this copy.
- (vii) The Central Library, Wigan. This set has both issues of No. 27.
- (viii) A loose copy formerly in the private library of Sir Geoffrey Keynes, but donated by him to the Library of the Royal College of Surgeons, London.⁵¹

A copy of the original French pamphlet by Denis, published in Paris in 1667,⁵² is to be found in the British Library (Reference Division) bound into a volume of seventeenth-century tracts on blood transfusion.

A number of indices to the *Philosophical Transactions* have also been published during the last 300 years and it is interesting to note that all but one of these refer only to Oldenburg's paper and make no mention of Denis' letter. The exception, *An index to the anatomical, medical, chirurgical and physiological papers contained in the Transactions of the Royal Society* by James Briggs, published in 1814,⁵³ refers only to the letter of Denis and ignores Oldenburg's No. 27. It is interesting to conjecture which set of the *Philosophical Transactions* Briggs used in drawing up his selective index.

From this survey it appears that only one facsimile and eight original copies of the translation of Denis' letter, published as a July 1667 issue of the *Philosophical Transactions*, are accessible in Britain. A number of other copies must exist elsewhere, however.⁵⁴ Apart from the 300 copies of the 1963-64 De Graaf-Israël reprint, in 1667 there were almost 200 Fellows of the Royal Society,⁵⁵ each presumably in receipt of the *Transactions*, and it is probably that of these more than the ten copies referred to above⁵⁶ have survived. Although Denis' letter is not as rare as suggested by Keynes, it is certainly not readily available to students in Britain.

CONTENTS OF THE REPORT

Denis began his report⁵⁷ with the assertion (already referred to) that the concept of blood transfusion had originated ten years previously in de Montmort's "Society of *Virtuosi*", and went on to summarize the experiments on animal-to-animal transfusion which Emmerez and he had conducted earlier in 1667,⁵⁸ including transfusions

⁵⁰ E. Heaton, personal communication, 1979. The shelf-mark of this volume in the Bodleian Library is AA 63-98 Med.

⁵¹ G. Keynes, personal communication, 1979.

⁵² See note 15 above. The B.L. press-mark is 783.g.23(12).

⁵³ London, Stace.

⁵⁴ Apart from the copy in Amsterdam used for the 1963-64 De Graaf-Israël reprint, a copy is also known to exist in the library of Dr. John Fulton at Yale University. Keynes, op. cit., note 4 above, p. 12n.

⁵⁵ Sprat, op. cit., note 6 above, p. 431.

⁵⁶ Eight in Britain, one in Holland, and one in the U.S.A.

⁵⁷ See note 38a above.

⁵⁸ It is interesting to note that Denis made no reference either to the previous English experiments (of which he may or may not have been aware) or the earlier French experiments conducted by the Académie Royale des Sciences: for the latter see 'Procès-verbaux manuscrits de l'Académie Royale des Sciences - Observations sur la transfusion du sang d'un animal dans un autre. Expériences faites du 22 Janvier 1667 au 21 Mars 1667', (Archives de l'Académie des Sciences de Paris). Cited by Peumery, op. cit., note 17 above, pp. 16-19.

between animals of different species.

Three reasons why it would prove impossible to extend transfusion to men were then quoted and answered. One of these, referring to the aphorism of Hippocrates that “If blood be poured out preternaturally into a cavity, it must necessarily become corrupted”,⁵⁹ required little more than an exercise in semantics to refute, with Denis taking the line that the criticism was due solely to a misunderstanding of Hippocrates’ meaning. It is interesting, however, that at such a late date appeal could still apparently be made to Hippocratic writings rather than to experimental evidence.

The other two criticisms are of greater significance – even if that significance could not at the time be appreciated. In meeting the point that blood of different animals “must be Poyson and Venome in respect of [one] another” Denis acknowledged “that there are as many different complexions and various qualities in the blood, as there are Individuals in every Species” but did not believe that this mattered any more than differences in the meats which men eat. He did not know the almost literal truth of his concept of the individual nature of the identity of human bloods,⁶⁰ nor the difficulties which these differences would still be causing to the practical introduction of blood transfusion even 250 years later.⁶¹ The third point – the problem of coagulation – was to take even longer to resolve,⁶² but it is surprising that Denis could dismiss it on the grounds that it was “an accident which never hapned to us”. Denis was notably reticent about his techniques of transfusion but if (as is probable) his methods at all resembled those of other contemporaries⁶³ then it would be easy to be unaware either of coagulation within the apparatus during the operation or of the small volume of blood which would, in consequence, pass between the two animals.

Apart from doubts about “the possibility of the thing”, other objections had apparently been raised concerning its applicability to human recipients. One critic doubted whether “sound” and “diseased” bloods would mix, and argued that even if they did, “should the pure blood mingle with the impure, yet it would not long preserve its Purity and temperament”. Other critics went further and, using the analogy of a barrel once used for vinegar souring everything else put in it subsequently, claimed that diseased bloods would “corrupt the laudable blood, wherewith they are newly irrigated”. Denis’ consideration of this again returned to the analogy of food, noting that “the strength and goodness of the meats and drinks we take, is able to correct the ill-temperament of the blood and render it better” – and that the “ill-temperament of the blood” did not harm the food.

⁵⁹ Hippocrates, *Aphorisms*, Sect. 6, No. 20. In *The genuine works of Hippocrates*, trans. F. Adams, London; Sydenham Society, 1849, p. 755.

⁶⁰ In January 1975 there were 397 known antigens that can be detected on red cells (P. D. Issitt and C. H. Issitt, *Applied blood group serology*, 2nd ed., Oxnard, Calif., Spectra Biologicals, 1975, p. 385). These are “capable of defining several million phenotypes; though many of the phenotypes would be so rare that they might not appear in a hundred years of steady work. Some of the possible genotypes must be so rare that they may never have formed the blood of an Englishman” (R. R. Race and R. Sanger, *Blood groups in man*, 6th ed., Oxford, Blackwell, 1975, p. 507).

⁶¹ For a study of the problems of blood groups and transfusion in the early twentieth century, see A. D. Farr, ‘Blood group serology: the first four decades’, *Med. Hist.*, 1979, 23: 215-226.

⁶² The first successful transfusion of anticoagulated blood (using sodium citrate) was given on 14 November 1914 in Buenos Aires. L. Agote, ‘Nueva procedimiento para la transfusión del sangre’, *An. Inst. modelo clín. méd.*, 1915, 1: 24-31 (text in Spanish and French).

⁶³ See, for example, notes 9 and 19 above.

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In this latter context a case of failure to transmit mange between dogs by transfusion was adduced by Denis to “prove” his case, and the very valid point made that the disease was not necessarily due to the blood of the mangy dog being “putrified, and corrupted in his Veins”. Such separation of cause and effect was not common in seventeenth-century thinking on blood transfusion.⁶⁴

For his own part Denis produced four arguments in favour of human transfusion, and here yet again he returned to his favourite theme – the analogy of transfusion with feeding.⁶⁵ He compared transfusion between animals with that “of the Maternal blood into the Umbilical Vein of the Infant, therewith to nourish, vivifie and encrease all the parts of the same”,⁶⁶ and regarded it as a more efficient means than eating for introducing nourishment into the system. He also saw transfusion as a way “to draw out the Corruption” (as in the common practice of blood-letting), without weakening the patient, “since the new blood conduceth to strengthen them more”. Finally, with creditable perception, Denis saw transfusion as a treatment both for haemorrhage and diseases, although in the latter case he referred to disorders (such as “Madness” and “Dotage”) none of which is today seen to be “arising from the Malignity of the blood”.

Before coming to the account of the first human transfusion Denis commented upon views that such transfusions “ought to be done with blood of the same species” – which was seen as posing a high risk to the donor, and therefore acting as a contra-indication for the procedure. In explicitly rejecting this thesis, and opting for animal blood, Denis unwittingly laid foundations of disaster for the practice which he was trying to promote.

The doctrine of incompatibility between blood of different species was not established until 1825,⁶⁷ the first use of human donor blood having been made only ten years before that.⁶⁸ The 148 years following Denis’ first human transfusions saw an almost total neglect of the practice following the severe reactions reported by him, the death of one of his patients following two large transfusions of calf’s blood,⁶⁹ and a

⁶⁴ Even religious attitudes were at that time related to blood. Eighteen months after Denis was writing, Pepys (14 November 1666) recorded a discussion which “did give occasion to many pretty wishes, as of the blood of a Quaker to be let into an Archbishop”. Latham and Matthews (editors), *op. cit.*, note 36 above, vol. 6, p. 371.

⁶⁵ Such an impression did this analogy make upon patterns of thought about transfusion that as recently as 1961 Denis was still being quoted by Jehovah’s Witnesses to support their refusal of blood transfusions on the grounds that God had forbidden the eating of blood (e.g., Genesis 9:4; Leviticus 3:17; Deuteronomy 12: 23-25; etc.). *Blood, medicine and the law of God*, New York, Watch Tower, 1961, p. 14.

⁶⁶ In this section Denis made one of the earliest recorded statements by a medical practitioner that mother and child were distinct entities, and were not “to be consider’d, but as one and the same Body and Substance. For it happens frequently enough, that the Paternal Seed predominating above that of the Mother, the Constitution of the Foetus is very different from her’s that bears it” (pp. 497-498); This attitude, some 200 years before Mendel’s foundation of the science of genetics – was at variance with much medical thought of the time. (For a study of medical attitudes to foetal and maternal life, see A. D. Farr, ‘Medical developments and religious beliefs’, Ph.D. thesis, Open University, 1977, pp. 310-319.)

⁶⁷ J. Blundell, *Researches physiological and pathological*, London, Cox, 1825, pp. 81-92. The critical sentence is on p. 92: “As it is clear, from the preceding experiments, that the blood of one sort of animal cannot, with impunity, be substituted indifferently, and in large quantities, for that of another sort of animal; it follows, of course, that in performing the operation of transfusion on the human body, the human blood should alone be employed.”

⁶⁸ J. Blundell, ‘Some account of a case of obstinate vomiting’, *Med.-chir. Trans.*, 1819, 10: 296-311.

⁶⁹ A third transfusion was to have been undertaken but was not started, and the patient died the following night (*Phil. Trans. R. Soc. Lond.*, 1668, 3 (36): 710-715). The patient’s wife subsequently admitted having poisoned him with arsenic (*ibid.*, p. 714).

subsequent sentence by the Lieutenant in Criminal Causes “that for the future no Transfusion should be made upon any Human Body but by the Approbation of the Physicians of the Parisian Faculty”,⁷⁰ who were bitterly opposed to the procedure.

Denis’ reasons for preferring animal blood included again his analogy with feeding – men consume animal meat and milk, so why should they not receive animal blood? – but were also a reflection upon life in seventeenth-century society. Because animals were not subject to “debauchedness and irregularity in eating and drinking”, and could be fed beforehand “with more care and exquisiteness than ordinary”, their blood would be “less full of impurities, than that of men”. Besides, unlike men, they were expendable – and possibly this was the factor which carried most weight. Denis’ scruples about human experimentation were not entirely disinterested, however, for he rejected the idea that he should “beg some condemned Criminal, on whom to make the first Essay” solely because, through fear, such a man might be “cast . . . into faintings and other accidents, which would undoubtedly be ascribed to the Experiment by such as decry it”.

The first human recipient of a blood transfusion was a youth of fifteen who had been bled twenty times in the preceding two months by his physicians as treatment for “a contumacious and violent fever”.⁷¹ It is not surprising that, as a result, “his wit seem’d wholly sunk, his memory perfectly lost, and his body so heavy and drowsie that he was not fit for anything”: he was clearly severely anaemic as a result of his treatment, to which Denis correctly attributed his current condition.

One basic precept of transfusion in the seventeenth century was that in order to make room for the transfused blood it was necessary first to remove some of the patient’s own blood, and Denis took “about three ounces” (about 90 cm³) and replaced it with some three times that volume of blood from the carotid artery of a lamb.⁷² During the operation the patient “felt a very great heat along his arm” – a classical symptom of a haemolytic transfusion reaction⁷³ – but otherwise recovered relatively uneventfully and exhibited “no longer that slowness of spirit nor heaviness of body” which had previously troubled him.⁷⁴ This result is a little surprising, for if the youth received enough blood to cure his anaemia it is a wonder that he did not suffer a much more severe reaction.⁷⁵ Denis, however, had no doubts that his patient’s

⁷⁰ *Ibid.*, p. 714. See Peumery, *op. cit.*, note 17 above, pp. 70-75, for an assessment of this case.

⁷¹ The amount of blood removed must be a matter of conjecture, but as late as 1903 one very eminent physician was recommending “the abstraction of from 20 to 30 ounces of blood” (600-900 cm³) as a single treatment in cases of cardiac valvular lesions. W. Osler, *The principles and practice of medicine*, 5th ed., New York, Appleton, 1903, p. 731.

⁷² The exact date of the first transfusion is uncertain. Denis met the youth on 15 June 1667 and claimed to have observed him “as he sate at dinner, [and] as he was eating his breakfast”. As the operation was subsequently performed “at five of the Clock in the morning”, it was possibly on 16 June, but more probably the following day, 17 June.

⁷³ “The commonest symptoms associated with a transfusion of incompatible blood when a haemolytic antibody is concerned are as follows: feeling of heat along the vein into which the blood is being transfused . . .” P. L. Mollison, *Blood transfusion in clinical medicine*, 5th ed., Oxford, Blackwell, 1972, p. 549.

⁷⁴ The youth’s recovery was apparently complete, for Denis subsequently took him into his home as a valet. P. M. de La Martinière, *Remontrances charitables à Monsieur Denis*, Paris, 11 May 1668.

⁷⁵ As did another of Denis’ patients a year later. After receiving a (second) transfusion of the blood of a calf, estimated as “more than one whole pound” in amount, there was evidence of rigor, lumbar pain, and haemoglobinuria – as well as a localized reaction at the site of infusion. J. Denis, ‘An extract of a letter . . .’, *Phil. Trans. R. Soc. Lond.*, 1667, 2 (32): 617-624.

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improvement was attributable to “that little Arterial Blood of the *Lamb*, which having been mixt with the mass of his thick blood, was like a ferment to it, to rarifie and attenuate it more than ordinary, whence follow’d the production of greater plenty of spirits, and a more nimble performance of the actions of the body”.

Some time within the following week Denis performed a second transfusion, this time upon a healthy paid volunteer forty-five years of age, who was relieved of ten ounces (300 cm³) of blood and then allegedly given twice that volume from the “crural” (femoral) artery of a lamb. Like the first patient, this man also felt “a very great heat from the Orifice of his Vein up to his arm-pit”, but once again it must be doubted if the patient actually received as much as 600 cm³ of blood in view of the relative mildness of the reaction reported. So little incommoded was the recipient, indeed, that so far from resting himself as he was requested, Denis found that “’twas impossible to prevail with him, and we could not keep him from falling to work with the poor *Lamb*, cutting his throat, and fleaing him, in which he is very dextrous, having exercis’d the same profession from his youth”.⁷⁶ The man then went “to the Tavern to drink part of the money given him for his daies business”,⁷⁷ and the following day he was apparently his usual boisterous self and anxious to volunteer for any future transfusion that might be planned.

Denis gave no indication of the technique which he adopted other than to comment that it was “very different from that which we use upon beasts.” Whether his excuse – that de Montmort (to whom he was ostensibly writing) could more readily understand it by seeing for himself – was the whole story may be doubted. Rivalry between French and English scientists was such that while the former would readily publicize the fact of their priority they may well have preferred not to provide information to enable their rivals to copy them. Indeed, four months later Oldenburg published a proposed technique for human transfusion contrived by Dr. Edmund King, and acidly referred to “Monsieur *Denys* not having thought fit to describe the *manner* they used in *France* for *Men*; nor any body else, come to our knowledge”.⁷⁸

Denis concluded by admitting that his experiments “indeed have not yet proceeded very far”. It was to be the further extension of these experiments, terminating with the sentence of the Lieutenant in Criminal Causes,⁷⁹ that would effectively stop the development of blood transfusion for another 150 years.

SUMMARY

The first transfusion of blood to a human subject, which took place in Paris in 1667, was attended by publicity on the part of its author but, due to the claims for priority in transfusion generally contained in the report, the secretary of the Royal Society of

⁷⁶ However, this same man was described as a porter when Denis later employed him as an attendant for a later transfusion patient. *Ibid.*, p. 620.

⁷⁷ The same behaviour by Arthur Coga, the first man to be transfused in England, on the occasion of a second transfusion in December 1667 may well have masked signs of a haemolytic reaction which went otherwise unrecorded. Coga said that “he had been at first somewhat feverish upon it; which was imputed to his excess in drinking too much wine soon after the operation.” Birch, *op. cit.*, note 8 above, p. 227.

⁷⁸ *Phil. Trans. R. Soc. Lond.*, 1667, 2 (28): 522. Although this issue is dated Monday 21 October, according to the Royal Society’s Journal Book, King’s method was not produced before the Society until 24 October. Birch, *op. cit.*, note 8 above, p. 202.

⁷⁹ See note 70 above.

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London suppressed the English translation and it is now of some rarity. The report – the text of which is given below – is of particular interest not only for its own sake but also because it contains arguments both for and against the extension of transfusion to men in the light of mid-sixteenth-century understanding of physiology, and also contains the first report of the symptoms of intravascular haemolysis following a transfusion of incompatible blood.

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A LETTER

Concerning a new way of curing sundry diseases by Transfusion of Blood, Written to Monsieur de MONTMOR, Counsellor to the French King, and Master of Requests.

By J. DENIS *Professor of Philosophy, and the Mathematicks.*

SIR,

The project of causing the Blood of a healthy animal to passe into the veins of one diseased, having been conceived *about ten years agoe*, in the illustrious Society of *Virtuosi* which assembles at your house; and your goodness having received M. *Emmerz*, & myself, very favourably at such times as we have presum'd to entertain you either with discourse concerning it, or the sight of some not inconsiderable effects of it: You will not think it strange that I now take the liberty of troubling you with this Letter, and design to inform you fully of what pursuances and successes we have made in this Operation; wherein you are justly intitled to a greater share than any other, considering that it was first spoken of in your *Academy*, & that the Publick is beholding to you for this as well as for many other discoveries, for the benefits & advantages it shall reap from the same. But that I may give you the reasons of our procedure & convince you that we are not culpable of the rashness wherewith we may possibly be charged: be pleased to permit me to relate to you in few words the reasons alledg'd by some persons, to oppose our design, and to compare the same with those which engag'd us to execute, and by certain degrees carry it on to that perfection wherein it is at present.

You have heard of the tryal we made about four moneths agoe upon Dogs, to transmit the Blood of the *Crural Artery* of one into the *Jugular Vein* of another. And being this operation even at the first attempt succeeded as happily as we could wish, according as it is more at large set forth in the *Journal des Scavans*, of *March* last; we were encouraged to repeat the same several times both in publick and private, and we added so many circumstances to the manner of performing it, that its easiness seemed to invite us not to neglect it, but to make abundance of Observations which might be of some benefit in Practice.⁸⁰

Accordingly, We afterwards made the Transfusion several waies, sometimes from an *Artery into a Vein*, sometimes from *one Vein into another*, both in those of the Neck, and those of the Legs; in Dogs both weak and strong, great and small; as also in such as had already either received or communicated blood in former transfusions. And not finding one of nineteen to die,

⁸⁰ See notes 21 and 22 above.

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but on the contrary alwaies observing some surprising effect in all such as had received new blood, we were strongly perswaded that the transfusion would have no such dangerous consequences, as some people endeavor'd to presage.

Wherefore we resolv'd to drive the business yet a little further, and not contented to have seen it happen well in subjects of one and the same species; we thought good to try it in some of a different species, and accordingly we took a Calfe and a Dog; because we conceiv'd the blood of these Creatures altogether dissimilar. In your presence we transmitted a *Calfs blood, into a Dogs Veins*, on the 28. of *March*; ⁸¹ since which time we have also done the same upon other occasions, alwaies adding somewhat new towards the facility of the Operation. Yet in all these Experiments we never could observe any indisposition in the Dogs which receiv'd the Blood; whence we became confirm'd in the Opinion, that there was more ground to hope effects rather advantageous than hurtful to mankind, from this discovery of Transfusion of Blood.

Nevertheless, that we might vent nothing unadvisedly, we publisht these Experiments, and were well pleased to understand the judgment of the ablest Philosophers and Physitians, upon the matter we examin'd, whether any of them had reasons weighty enough to dissuade us from carrying on our tryals even in Man; and I beseech you permit me to sum up the principal here which are come to my knowledge, and judge yourself whether they ought to have stopt us, and whether we deserve praise or blame for having proceeded further. Some have attack'd this design by decrying it as Chymical & impossible. 1. Because the diversity of Complexions (which is founded in the Blood) supposeth so great a diversity in the several Bloods of different Animals, that 'tis impossible but one must be Poyson and Venome in respect of another. 2. Because Blood extravasated or remov'd out of its natural place, must necessarily corrupt, according to the dictate of *Hippocrates*. ⁸² 3. Because the Blood issuing out of its proper Vessels, and being to pass through inanimated conveyances, such as are the Pipes or Tubes employ'd in the Transfusion; it must infallibly coagulate, and so coagulated descending to the heart must cause there a Palpitation, whereof death will be the speedy consequent.

These Reasons have not appear'd to us of great importance. For as to the first, we acknowledge that there are as many different complexions and various qualities in the blood, as there are Individuals in every Species; But we believe it not conclusible from thence, that the Blood of one must be Poyson in respect of others; no more than it follows, that all the Meats wherewith we are nourisht, and whereof our Blood is only an Elixir or Quintessence, must corrupt and poison the same, because they are endu'd with qualities differing from those of the Blood. On the contrary, as 'tis certain that cooling Meats or Medicaments serve to temper the heat of the Blood when it boiles in the Veines, and hot food or Physick excite new vigour in such blood, as old age or a Disease hath render'd torpid and coagulated in the Vessels; so 'tis as reasonable to infer, that great advantages will follow upon the mixture of different bloods, provided the same be directed and manag'd by the ordinary prudence of able Physitians; and that if a hot blood can reinfuse new strength into that which languisheth with coldness, such blood as hath colder qualities, may also check the Ebullitions and Tumults of that which is overmuch chased.

As for the authority of *Hippocrates*, who saith that extravasated blood corrupteth, It is no wise repugnant to our pretensions. For to salve the honor of that great Person, his meaning must be rightly expounded, which is no more but this; that the Blood necessarily corrupteth when it is out of the Vessels, which preserve its heat, and which permit it a natural motion and free circulation, whereby it purifieth itself continually. For example, being extracted into a dish, wherein all parts are stopt and coagulated, it must in a little time alter its constitution, as experience witnesseth. Nor doth this hinder but that it is also corrupted sometimes in its own

⁸¹ Reported in *Phil. Trans. R. Soc. Lond.*, 1667, 2 (25): 453.

⁸² See note 59 above.

place, contrary to that Aphorisme, *Quicquid corrumpitur, in loco alieno corrumpitur*.⁸³ For if its motion be interrupted in the Veines either by some obstruction form'd therein, or some division occasion'd by a Wound or Contusion, it corrupeth in a short space and degenerateth into purulent matter. But in this Experiment of Transfusion, the Blood is communicated in such manner, and passeth out of one animal into another, that 'tis impossible its natural motion should be interrupted. 'Tis true it passeth through unnatural Conduits, but these being once warmed and admitting no air into their cavities, they cause no more alteration in the Blood than the Arteries and Veines themselves do.

Moreover, such as conceive that the blood Transfused, must needs coagulate by the way and afterwards cause some mortal palpitation; they foresee an accident which never hapned to us, and against which I desire no other security than the experience we have had of all the animals hitherto employ'd by us, which are still living.

Others who have either been witnesses of some of our Transfusions, or have understood the same from credible relators, dispute not the possibility of the thing: but yet not to authorize appears new, they say, That whatsoever care and caution be used in the Transfusion, it can never be practis'd upon Man with successe; and these are their principal Reasons.

First, The blood of a sound, and the blood of a diseased body having qualities very different, the one being pure, the other impure, a perfect mixture thereof cannot be effected; they are two contraries, which will be at perpetual feud, the issue whereof can be no other But the ruine and destruction of the subject on whom the experiment is attempted: I wish those that discourse thus, first understood but what they endeavour to perswade others of; and that they would explaine to us what artifice they fancy in the Veins and Arteries, to give passage to one sort of blood & exclude another at the same time. For my part I confesse I cannot comprehend why the continual circulation and rarefaction made in the heart by the heat of its Ventracles, are not more than sufficient to make a perfect mixture there of these two sorts of blood, & the difficulty seems the greater in regard experience appears to flatter me into a contrary opinion. For having a few dayes agoe syring'd about a quarter of a pinte of Milk into the veins of an Animal, and having opened the same some time after, we found the Milk so perfectly mixt with the whole substance of the blood, that there was not any place wherein appear'd the least footstep of the whiteness of Milk, and all the Blood was generally more liquid and less apt to coagulate.

The second Objection of the same Author is, That should the pure Blood mingle with the impure, yet it would not long preserve its Purity and temperament; in regard that upon its Arrival at the Liver (which is the grand Organ of Sanguification wherein the blood receives all its qualities) it would infallibly lose all its goodness, and in an instant become like the whole Masse, which we suppose corrupted; and consequently can produce no benefit therein by its mixture.

But this Objection is ill enough grounded, since it supposeth the blood to be made in the Liver, and is contrary to experience which demonstrateth most evidently, both the existence of blood in Animals before the formation of the Liver, and also the Arrival of the Chyle, and at the heart, and conversion of the same into blood there, before it entreth into the Veines which carry it to the Liver: wherefore I dismiss this reason and proceed to another, which seemeth stronger and is offer'd by persons more judicious.

'Tis not contested in this Objection in what place sanguification is perform'd, because 'tis held to be done in several parts of the body, namely by reiterated circulations in the heart, Veines and Arteries: But 'tis likewise maintain'd that all these parts come either by sickness or age, to a certain degree of intemperature and malignity, from which there is no possibility to recover them; and that in this state they have power to communicate their evil qualities to whatsoever approacheth them; whence consequently in a little time they corrupt the laudable blood,

⁸³ "Whatever will be corrupted, will be corrupted when transferred to another place." This aphorism is not Hippocratic.

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wherewith they are newly irrigated. Proof of this is afforded both by example and experience. The example is taken from a *Hogshead of Vinegar*, which being once thoroughly imbued with that *Liquor*, is no longer fit to contain any other, but every thing pour'd thereinto is immediately infected with sowness by contagion of the *Caske*. The experience consists in that which was lately publisht by an English Doctor, who transfusing the blood of a Mangie Dog into a sound one, to try whether the Mange would be communicated with the blood, found the Mangie Dog cur'd, and the other who had receiv'd his blood not to become Mangie.⁸⁴

Now to answer to all this in order, I say in the first place that such a great intemperature, from whence 'tis said the blood can never recover, is either rare or very common. If very rare, it requires not much consideration, and if the mixture of laudable blood suffice not to restore it to its due temperament, I would gladly learn other means to do it. But if such intemperature be ordinary in all diseases and in all declining old Ages, I demand to what end serve the aliments which *Physick* allows, or the *Medicaments* it prescribes in these occasions? Can these things enter into the body without mingling with the blood within an hour? And if by so mingling they contract the ill temperament of the blood, and impart not to the same any of their good qualities, is it not losse of time to study *Physick*, and to reduce into practice what we are taught touching the choice of all those things? For my part, I am so far from having such a thought, that I doubt not but the strength & goodness of the meats & drinks we take, is able to correct the ill temperament of the blood and render it better: If there be any impediment of this effect, I should not so much look for it in the blood itself, or in the parts which elaborate the same, as in those through which the aliments passe before they arrive at the heart. For 'tis sufficiently manifest that if there be any irregularity in the stomach, if the digestion be not well made, if the juyces of the Gall and Pancreas (which serve for Dissolvents in the Intestines) be not well temper'd, if there be any Obstruction or Abscesse in the Glandules of the Mesentery which strain the purer portion of the Chyle; the best and most excellent things become so transform'd before their mixture with the blood, that no good nourishment or refreshment to the sick can with reason be expected from them.

Now in this way of *Transfusion* all those inconveniences are avoided; the blood communicated is not made to passe through those many turnings and windings, where the least corruption of a part destroys in an instant whatever good the soundness of the rest can produce. The good blood enters immediately into the Veins of the receiving Animal, and there mixing and circulating with the rest, carries good nourishment to the principal parts, which are not ordinarily indisposed in sickness and old age, but by the vitiosity and faultiness of the blood which humecteth [moistens] them.

This is an Opinion which I see many Physitians embrace, and were anything capable to make me renounce it, the *Barrel of Vinegar*, above mention'd must not be it. For if *sowness* be the last quality which Wine assumes, and (as I may say) its Death, since it is never recoverable from the same; I do not believe that a like quality is to be found in the blood; that is to say, which corrupts the blood in such manner that it can never be purifi'd, unlesse perhaps in some Diseases accounted incurable, which are not pertinent to the Question. But if comparisons be currant, let us rather consider Wine with all sorts of other qualities but sowness, and then our comparisons will in my opinion, fall more just. For as the roughest Wine may be sweetened, the fowlest clarified, the weakest become stronger, the Oylie rid it self of its fatness; in a word, that which is decay'd may be amended by mixture of certain liquors, known to those that have the secrets of them, and practise the same every day: In like manner, 'tis reasonable to conceive that blood too thick may be refined and subtilised, that too subtle be fixed and incrassated [thickened in consistency], that too hot be temper'd, that too cold be heated; and all this by the mixture of

⁸⁴ T. Coxe, 'An account of another experiment of transfusion, viz. of bleeding a mangy into a sound dog', *Phil. Trans. R. Soc. Lond.*, 1667, 2 (25): 451-452.

other sorts of blood, the particular qualities whereof are known to the Physician who prescribes the *Transfusion*.

As for the Experiment of the *Dog*, which is said to have received the corrupted blood of another Mangie one without contracting his disease; many things of sufficient importance may be said of it. 1. Is there any assurance that the blood of the Mangie Dog was putrefied, and corrupted in his Veins? On the contrary might it not have been purified before, by discharging its impurities through the pores of the flesh, where the same was converted into Mange upon the Skin. This which confirms me the more in this particular is, that very frequently Scabs divert a greater malady; since if those impurities which cause the same, issued not out at the surface of the body, they would remain mixt with the substance of the blood, and falling upon some noble Part, produce very dangerous effects therein. 2. Supposing the blood of the Mangie Dog wholly corrupted in his Veins, is it necessary it should produce the Mange in him that receives it? Is it not possible that vitious [defective] blood may be purified when it becomes mixt with better, and that the great heat which caused the extravasation in the one in order to production of the Mange, may be allayed by the coolness of the other wherewith it is mixt, and consequently not produce the same effects. Lastly, is there any assurance that the Mangy Dogs blood hath not produc'd some evil effects in the body of him who receiv'd it? He should have been open'd some days after, and perhaps the corruption which render'd not it self apparent outwards, might have been visible within.

Hitherto therefore I find not either Reasons or Experiments sufficient from the opinion I have conceived of the benefit of *Transfusion*; and in the Discord I observe of Physitians, whereof some approve the Invention, and maintain that it may with prudence be prescrib'd for the care of divers Diseases, others respect it as a very useless Novelty: and Lastly, others keep themselves undetermin'd, expecting what successes the Experiment will produce; I willingly joyn with the first, and resign myself to the ensuing reasons, until some body shall produce others to the contrary of greater weight.

1. The *Transfusion* of the blood of one animal into another is sufficiently taught us by Nature it self, and it must be granted that if we ever practice the same, we shall do no more but imitate her; since whil'st she cannot yet administer nourishment to the *Foetus* by the mouth, and his stomach is not fit for digestion, she makes a continual Transfusion of the Maternal blood into the Umbilical Vein of the Infant, therewith to nourish, vivifie and encrease all the parts of the same. Nor is it to be answered here, that the Mother and Child are in this state to be consider'd, but as one and the same Body and Substance. For it happens frequently enough, that the Paternal Seed predominating above that of the Mother, the Constitution of the Foetus is very different from her's that bears it, although nourisht by the transfusion of her blood.

2. The *Transfusion* of blood is but a very compendious way of the continual transfusion of our Aliments, which is also taught us by Nature after our birth. For, whereas the Meats which we eat for the reparation of our strength, and refreshment of the heated parts, have very many impurities mixt with their good juices, which must be concocted by the heat, and digested in the acid Liquor of the Stomach; which done, the purer portion of the same digested juices is converted into Chyle, that Chyle impell'd into the Veins to be mingled with the Blood, convey'd therewith to the heart, and there receive its utmost perfection; The *Transfusion* of blood is made more speedily, and with much more effect, since thereby, in a very small time, a most elaborated Liquor is immitted immediately into the Veins, where it instantly redresseth such defects as it findeth in the internal parts, when they are distemper'd.

3. Physicians cannot deny, That the greatest part of our Diseases are but Results of the Distemper and Corruption of the blood, since the speediest and commonest remedy thay have in Practice, is, to evacuate the same by Phlebotomy, or else refresh and cool it by Juleps. But they must also confess, That whilst they endeavour to draw out the Corruption, they at the same time

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diminish the strength and vigor; and that great Fevers by this means are oftentimes follow'd by Faintnesses and Dropsies: On which consideration, some Physitians spare bleeding as much as possibly they can. Moreover it must be acknowledged, that Drinks passing through the Stomach and Intestines, before it arrive at the Veins and Arteries, to be mingled there with the blood, they may be alter'd a thousand waies by the Ferments occurring in the passage, or else they may cause some alteration in those parts by Ferments accompanying them, and so leave weaknesses and dangerous Crudities behind them. But in *Transfusion* all these things are avoided, and all Physitians brought to agreement within a short time. They who are for blood-letting, find the same practis'd in this Operation, the old and corrupt being first evacuated, to make room for new and pure. And those also that decry bleeding, as that which too much weakens the sick, will have no reason to complain, since the new blood conduceth to strengthen them more.

Lastly, Every one knows that many persons dye through loss of blood, and Haemorrhagies not to be stopt, many are emacrated by them, and others precipitated into untimely old Age, by defect of blood and vital heat. Now who doubts but that the *Transfusion* of a mild and laudable blood may preserve the one, and prolong the life of the others. A man may foresee some benefits and advantages from this Operation in *Pleurisies, the small Pox, Leprosies, Cancers, Ulcers, St. Antonies fire, Madness, Dotage*, and other *Maladies* arising from the Malignity of the blood: But the success is rather to be expected in Experiments, which will be made within a little time. In the interim, give me leave to acquaint you with my Judgment concerning the Election of the Animal that is to communicate its blood.

Many have conceiv'd, That if ever the *Transfusion* of blood should come to be practis'd upon men, it ought to be done with blood of the same Species; and consequently, That it would be a very barbarous Operation, to prolong the life of some, by abridging that of others. But for my part, I am far from that Opinion, and I am perswaded that it will be much more expedient to make use of the blood of other Animals, than of that of men, for many reasons, the chief whereof are these.

1. 'Tis easie enough to judge, That the blood of Animals is less full of impurities, than that of men, because debauchedness and irregularity in eating and drinking, are so ordinary⁸⁵ to them, as to us. Sadness, Envy, Anger, Melancholy, Disquiet, and generally all the Passions, are as so many causes which trouble the life of man, and corrupt the whole substance of the blood: Whereas the life of Brutes is much more regular, and less subject to all those miseries, which we ought to consider as sad consequences of the prevarication of our first Parents. And indeed Experience sufficiently shews us, That if 'tis a rare chance to find ill blood in the Veins of Beasts; 'tis almost impossible not to find some corruption in that of Men, how healthful soever they seem to be. Yea, even in Children yet sucking, it is not wholly unblamable; because, having been nourisht with the blood and milk of their Mothers, they have suckt corruption together with their nourishment.

2. If Physicians so successfully employ the milk of certain Animals, to which they oblige certain Patients for some whole years together: If we are very well nourisht with the flesh of some, during all our lives; and if their juices be so advantageous to us for repairing our natural strength; Why may we not hope greater advantages from the mixture of their blood with ours.

3. The Operation may be made more boldly, and with greater success by employing Brutes. For, being we design to take the best blood that is to be had, and least subject to coagulation, 'tis certain that the Arterial blood upon account of its heat and tenuity, possesseth both these qualities in much more perfection than the Venal. And the dangers and inconveniences which would occur in opening the Arteries of a man are not considerable, when 'tis a beast on whom the Operation is made.

⁸⁵ This is an incorrect translation, and should read "are not so ordinary . . ." (see note 15 above).

4. Beasts may be better fitted and prepar'd for this use than men. For I should advise those, that would employ this Experiment to the best advantage, to feed their Beasts for some daies before-hand with more care and exquisiteness than ordinary: since, if by the taste we can discern the flesh of Calves fed for some time, with Milk, and Yolks of Eggs to be much more pleasant than that of others; reason ought to perswade us that their blood is also meliorated according to their feeding.

All these Reasons joyn'd to the Experiments we have made, could not but induce us to give assurance to the publick of the little danger to be fear'd, and the considerable effects to be expected from this Operation. Yea, we imagin'd 'twere no rashness to make tryal of it upon men, and indeed divers persons of much gravity and prudence solicited us to beg some condemned Criminal, on whom to make the first Essay. But having consider'd that a man in that condition, being already much disorder'd by the apprehension of death, might be further intimidated, by looking upon this transfusion as a new kind of death; and thus, conceit might possibly cast him into faintings and other accidents, which would undoubtedly be ascribed to the Experiment by such as decry it: We thought not fit to expose our selves to that danger, nor to importune his Majesty without any necessity; being perswaded that there would not be so much reason to fear the like events in persons whom we knew perfectly well, and who had some confidence in our words, we chose rather to wait till a favourable occasion offer'd us such a person as we wisht, than to hazard the loss of all by too much precipitation. This Resolution being taken we neglected nothing that prudence obliged us unto; and at length after some attendance we lighted upon a subject suitable to our wishes. The particularities of our proceedings I here subjoyn in few words, they being so many authentick confirmations of all that I have hitherto written.

On [Saturday] the 15 of this Moneth, we hapned upon a Youth aged between 15 and 16 years, who had for above two moneths bin tormented with a contumacious and violent fever, which obliged his Physitians to bleed him 20 *times*, in order to asswage the excessive heat.

Before this disease, he was not observed to be of a lumpish dull spirit, his memory was happy enough, and he seem'd chearful and nimble enough in body; but since the violence of this fever, his wit seem'd wholly sunk, his memory perfectly lost, and his body so heavy and drowsie that he was not fit for any thing. I beheld him fall asleep as he sate at dinner, as he was eating his Breakfast, and in all occurrences where men seem most unlikely to sleep. If he went to bed at nine of the clock in the Evening, he needed to be wakened several times before he could be got to rise by nine the next morning, and he pass'd the rest of the day in an incredible stupidity.

I attributed all these changes to the great evacuations of blood, the Physitians had been oblig'd to make for saving his life, and I perswaded my self that the little they had left him was extremely incrustated by the ardour of the fever (which usually dissipates only the more tenuious part) and so stagnating in his vessels, he wanted the motion and heat necessary to volatilise the same, and to diffuse a sufficient activity into the Nerves and Muscles. Accordingly my conjecture was confirmed by our opening one of his Veins, for we beheld a blood so black and thick issue forth, that it could hardly form it self into a thread to fall into the porringer. We took about three ounces at five of the Clock in the morning, and at the same time we brought a *Lamb*, whose *Carotis Artery* we had prepar'd, out of which we immitted into the young mans Vein, about three times as much of its Arterial blood as he had emitted into the Dish, and then having stopt the orifice of the Vein with a little bolster, as is usual in other phlebotomies, we caus'd him to lie down on his Bed, expecting the event; and as I askt him now and then, how he found himself, he told me that during the operation he had felt a very great heat along his Arm, and since perceiv'd himself much eased of a pain in his side, which he had gotten the evening before by falling down a pair of staires of ten steps. About ten of the clock he minded to rise, and being I observed him chearful enough, I did not oppose it; and for the rest of the day, he spent it with much more liveliness than ordinary; eat his Meals very well, and shewed a clear and smiling countenance. He

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bled only 3 or 4 drops at the nose, about 4 a clock of the evening, and after he had supt very well, I caused him to go to bed about 9, and falling asleep at 10, he awakned at 2 after mid-night, and finding that he could not fall asleep again, he arose at 4 in the morning. All this day we observ'd his humour much more lively, and the agility of his body much greater than ordinary. The next day he slept a little more, and from that time he easily got the victory over his drowsiness, which before he had often attempted without successe; for now he never fails to rise very early without needing to be wakened. He executes nimbly whatever is appointed him, and he hath no longer that slowness of spirit nor heaviness of body, which before render'd him unfit for any thing. He grows fat visibly, and in brief, is a subject of amazement to all those that know him, and dwell with him.

Now who sees not that all these admirable effects undoubtedly proceed from that little Arterial blood of the *Lamb*, which having been mixt with the mass of his thick blood, was like a ferment to it, to rarifie and attenuate it more than ordinary, whence follow'd the production of greater plenty of spirits, and a more nimble performance of the actions of the body.

This first Tryal thus succeeding engag'd us to make a second upon a stronger *Man*, aged 45 years. Now this *Man* having no considerable indisposition, we intended to make a larger Transfusion upon him, than on the former. But finding his vessels very low & not well fill'd with blood, we took from him only about 10 ounces, and afterwards imitted into him twice as much from the *Crural Artery* of a *Lamb* purposely provided for it, as well because the same is bigger and easier to come at than the *Carotis*, as for that we would put some difference between this second Experiment, and the first. The man abated nothing of his Jovial humor during all the time of the Operation, and amongst other reflections which he made concerning the placing of the *Lamb* near his *Arme*, he said merrily that there were strange waies in Physick to preserve life, that he knew not who had invented this of bleeding, but that he felt a very great heat from the Orifice of his Vein up to his arm-pit; which proceeded from the course of the new *Arterial* blood, passing up that way towards the heart.

When the Operation was ended, we advised him to lie down to rest; but being he found no indisposition in himself, 'twas impossible to prevail with him, and we could not keep him from falling to work with the poor *Lamb*, cutting his throat, and fleaing him, in which he is very dextrous, having exercis'd the same profession from his youth. Afterwards he declared his intention to return home, and promised us that he would take a mess of some comforting broth there, and lay himself to rest for the remainder of the day; but as soon as he went forth; he betook himself to find out his Comerades, and carryed them to the Tavern to drink part of the money given him for his daies business: at noon finding himself more hearty, (whether by the new blood he had received six hours before, or by the quantity of wine he had drank) he fell upon a sort of work so laborious to his whole body that it might almost tire a horse; thus he spent all the afternoon, and so kept us from taking such observations upon him as we had intended. I met him the next day in the streets, and understanding from himself this behaviour, I was surprised at it, and blam'd him of imprudence. But he told me in excuse of himself, that he could not be at rest when he was in health, that he had felt no pain either during or after the operation, that he had eaten, drank and slept very well, that he had more strength than before, that if we were minded to repeat the same experiment at any time, he desired we would choose no other person for it but him, & that another time he would lie down to rest, and punctually behave himself as we should command him.

This is an account of our Experiments, which indeed have not yet proceeded very far; nevertheless I could not longer conceale them from your curiosity, knowing well that from these few observations you will forsee consequences and advantages enough. I have not described the manner of our making this Experiment upon man, which is very different from that which we use upon beasts: but the particular relation would be but tedious and useless to you, since you will

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behold it more plainly in an example when ever you shall find a convenience to command us to make the same before you. And I assure my self, you will therein admire the dexterity and sagacity of Mr. Emmerez, and confess that his industry renders him as happy in this operation as in many others of Chirurgery which are much more difficult; for indeed 'tis done with as much speed as an ordinary phlebotomy, and he that receives new blood complains not of any pain that he feels.

But I perceive that I abuse your patience, and that the length of my letter hath passed the bounds wherein the respect I have for the person ought to confine me, I beseech you pardon me this liberty, and be not displeas'd that I take this occasion to consecrate to you my most humble service, and to assure you that I am,

Sir, *Your most humble and obedient servant,*

J. DENIS

Paris, June 25, 1667.