WILLIAM HARVEY'S De Motu Cordis, published in 1628, is widely regarded as the most important book in the history of medicine. It presented for the first time an accurate description of the circulation of the blood through the heart, arteries and veins. Probably even more important, it reintroduced into medicine the method of scientific experimentation (in limbo for 14 centuries), and so led to a revolution in biological science. De Motu Cordis made the all-important break with philosophical dissertation, finely spun theories, and traditional authority, and initiated a new era of scientific medicine.

William Osler wrote in 1906:

To the age of the hearer, in which men had heard, and heard only, had succeeded the age of the eye, in which men had seen and had been content only to see. But at last came the age of the hand — the thinking, devising, planning hand; the hand as an instrument of the mind, now reintroduced into the world in a modest little monograph of seventy-two pages from which we may date the beginning of experimental medicine.

Most accounts of Harvey's work on the circulation stop with his 1628 book; some also tell of the next 40 years of bitter resistance to his new idea. Why the resistance? Partly because Harvey had challenged and corrected dogma that had been accepted as truth for 14 centuries, and partly also because, although Harvey had presented precise observations, experiments and irreproachable logic on many points, he had not done so on two important ones. He did not prove experimentally that right ventricular blood did not seep (or "sweat") into the left ventricle through Galen's "invisible pores in the ventricular septum" and, although he had convinced himself that his "porosities" provided the missing link in the pulmonary circulation, he did not prove their existence experimentally.

For these reasons, most accounts of the discovery of the circulation of the blood end with Malpighi's 1661 direct observations of the pulmonary capillaries of a frog and credit Malpighi with closing Harvey's circle. Harvey had no microscope; Malpighi did, and not only did he prove that the pulmonary circulation was a closed circle, from artery to newly discovered capillaries to vein, but he also began an important revolution of his own: the beginning of cell biology.

I often wondered (in hindsight!) why Harvey, the experimentalist, did not do a simple perfusion experiment to prove the absence of ventricular pores and the presence and capacity of "porosities" in the lung. The fact is, he did; not only that, but he did it on the human heart and lung. The account of this experiment is not in De Motu Cordis, but in a 1651 letter, written in Latin to a friend; in it, 10 years before Malpighi discovered capillaries, Harvey described a "recent experiment...from the implications of which there is no escape." One paragraph, in Latin, first published in Harvey's Opera Omnia in 1766, republished in Robert Willis's The Works of William Harvey in 1847, and finally republished again by Kenneth Franklin*7 is reproduced on the next page.

Below is Franklin's English translation*8:

Harvey's first letter to Paul Marquart Schlegel, Hamburg

I have pleasure in describing here an experiment, tried out recently by myself in the presence of several colleagues, and from the implications of which there is no escape. The pulmonary artery and pulmonary vein and the aorta were ligated in the cadaver of a throttled human being, and the left ventricle of the heart was opened. I then introduced a small tube through the vena cava into the right ventricle and at the same time fastened on to the tube an ox's bladder as is usually done in the injection of clysters. This I filled almost full of warm water, and injected it with great force into the ventricle mentioned so that almost a pound of fluid passed over into it and its neighbouring auricle.

*For what we now call the pulmonary artery and pulmonary vein, Franklin used in his translation the terms "artery-like vein" and "vein-like artery."
What happened? The ventricle in question (together with its auricle) swelled up violently, but not even a small drop of water or of blood escaped through the gap in the left ventricle. When the aforementioned ligatures had been released, the same tube was introduced into the pulmonary artery, and after a tight ligature had been made to prevent the water from getting back into the right ventricle, I attempted to drive this water into the lungs. At once it shot forward, mixed with a large amount of blood, from the cut in the left ventricle in such a way that as much water came out from the cut in question as was pushed into the lungs at the individual compressions of the bladder. You can try it so often as you wish and discover that it is so.

March 26, 1651

Thus, in one magnificent experiment, easy for any skeptic to reproduce in a dead man or animal, Harvey killed two birds with one stone. He proved that not a single drop of water seeped through Galen’s invisible pores in the ventricular septum and that Harvey’s “porosities of the lungs” swiftly conducted large volumes of blood or water from the pulmonary artery to the left atrium and ventricle. Thus, it was Harvey who established the existence of the “missing link” and it was Harvey who won his own battle. Malpighi’s microscope, of course, was still necessary to identify the structure of Harvey’s porosities — that they were capillaries with continuous walls and connected with small arteries at one end and small veins at the other.

There have been at least 19 Latin editions of De Motu Cordis and more than 30 translations of it into eight living languages (at least two of which include translations into English of letters that Harvey wrote between 1628 and his death in 1657*), and a tremendous number of articles, books and sections of books deal solely with Harvey’s life and his studies. In addition, there are regular Harvey Lectures in New York and an annual Harveyan Oration in London, plus three Harvey celebrations every century in Britain: on his birth date, 1578; on the date of his death, 1657; and on the date of publication of De Motu Cordis, 1628.

So, you should ask, with this torrent of material on Harvey, why am I writing this note? Because I did not know of Harvey’s 1651 letter until a few weeks ago, and I learned of it by pure chance. I own a copy of Circulation of the Blood: Men and Ideas* that I refer to frequently. In consulting this book on a matter unrelated to Harvey, it fell open to pages 78–79, where in italics was an abridged English translation of the paragraph from Opera Omnia.

Naturally, I wondered whether I alone was unaware of its existence. A quick poll of some medical
colleagues showed that they too were unacquainted with Harvey's letter to Schlegel. I then wondered how often and how prominently the letter had been reproduced or mentioned in biomedical books and journals. The next step was a search of journals and books where the letter merited publication. I did not find it in hundreds of such publications, including cardiology texts and journals or in books on medical history (including Garrison's, Morton and Garrison's, Castiglioni's, Willius', Willius and Keys', the Benchmark Volumes, the American Physiological Society Handbooks), in books on the pulmonary circulation or even in the Proceedings of the 1957 Harvey Tercentenary Congress.*

Who did write about it? So far, my research associate has found references to it in 10 books*7-10-16 and one journal.7 All but one were printed in England and all are located in the historical section of medical libraries. No author gave the experiment more than a half page of text; none placed it in a prominent place in his book, gave it more than a one- or two-line comment, or mentioned it again in an article on Harvey. Typical of this pattern is that of Franklin, foremost Harveian historian of recent years. He delivered an essay on Harvey at the 1957 Harvey Tercentenary Congress (whose Proceedings were published in 1958), but he made no mention of the 1651 letter, although he knew that Willis had translated it in 1847 and Franklin himself was also translating it. Perhaps Franklin, like others, did not attach great significance to the direct experimental evidence; he commented only that

The finding on a throttled man which is mentioned in the second paragraph of Harvey's letter to Schlegel is an important piece in the total circulatory story told by the former.

I believe that the 1651 letter is far more than that; it describes Harvey's most exciting experiment. As such, it deserves publication with a proper title in an American medical journal with wide circulation so that others who, like me, missed it, can have the pleasure of learning how Harvey himself closed the circle and completed his own work on the circulation at the age of 73 — and so that those who knew about it all along can have the pleasure of writing to me and telling me how and when they learned about it.

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J H Comtor, Jr

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