disease, but he now seeks for a substance fitted to aid him in the sole aim of his life—prevention or cure. That knowledge is but a flimsy web that cannot direct us to the proper therapeutic agent to be selected for the treatment of disease. The student early discovers this truth; but sometimes too late for the safety or comfort of his patient. He should continually inquire of himself, what medicines he would give, and what line of management he would pursue, in a given case; if he know not, he should ask his teachers, and make them what they profess to be. By remaining ignorant he is acting wantonly and sinfully.

Botany forms a tributary stream to materia medica and therapeutics, and opens to the student's search a rich field for useful application. The course of botany forms a portion of the labour of the summer session, when nature displays her vegetative charms in the flowery meads, and e'en upon the window-sills of the murky edifices of town.

Medical jurisprudence, again, with its full list of portentous questions, questions on which hang truth and life, is one of the subjects for the summer's reflection. Forensic medicine is generally too little valued by students, and the want of the information which it conveys is sorely and deeply felt when the medical practitioner is made the subject of judiciary inquisition. In the court of justice there should be no doubts; we should have studied the subject on which we are to be examined in all its bearings; we should have made up our minds completely to the intent of the evidence which we are able to give; and we should then deliberately and calmly declare those opinions to the best of our knowledge, before God, which our conscience warrants, unhesitatingly and firmly. Let us beg of the student, while it is yet time, to bestow a fair proportion of mature thinking on forensic medicine.

The records of warfare, from the earliest times down to the present day, have shown the necessity of discovering the strength and position of the enemy. This is the duty of a valuable and distinguished corps, the aides-de-camp. The information which these officers bring is the mental perception of the General, from whose reasoning faculties flow the instructions which are intended to anticipate the probable intentions of the enemy. We say *probable* intentions, because

the General has no means of inferring intentions save from appearances. Medicine is a perpetual warfare against death and his ministering angels; the General of the defensive army is the mind of the practitioner of medicine; his aides-de-camp are his eyes, his ears, his hands; the enemy's arms and strength are disease; the business of the practitioner and his educated senses to reconnoitre and oppose disease. It may be said that every study of the medical student has this object; but there is one more than all the rest to which our figure points,—it is morbid anatomy. No opportunity should be permitted to pass by the student without investigating the cause of death, the disease, the morbid changes and elements; for by this knowledge we most effectually obtain a familiarity with the principles upon which our treatment should be founded.

To this brief sketch of your duties, Gentlemen, and of the expectations with which society regards the profession into which you are about to enter, we can only add our wishes for your success, and our hopes that God may speed you.

OBSERVATIONS ON THE STATIC LUNG TESTS.

By WILLIAM AUGUSTUS GUY, M.B., Cantab.,
Professor of Forensic Medicine, King's
College, London, and Physician to the
King's College Hospital.

I HAVE elsewhere* entered into a minute and laborious examination of the static lung tests, availing myself for that purpose of observations drawn both from foreign and domestic sources. As conclusions drawn from such mingled sources may, perhaps, be viewed with some distrust, I propose on the present occasion employing only such English observations as I can bring together; and, should they be found to confirm the unfavourable view which I have been led to take, I trust that these tests may be safely consigned to the oblivion which they have appeared to me to deserve.

The observations which I am about to employ are partly borrowed from the two essays of Mr. Taylor, published in the "Guy's Hospital Reports," and the paper of Dr. Brady in the "Dublin Journal of Medical Science," and partly furnished by myself. The facts amount to thirty-three in number, and all refer to children at full term, without distinction of sex, and in whom the lungs were ascertained to be healthy. I propose to adopt the same order of examination as in my essays already

* Edin. Med. and Surg. Journal.

referred to, beginning with the absolute weight of the lungs.

In the following table the observations are arranged in two columns, and in such a form as is best adapted to answer the purpose which I have in view. The numbers are arranged in the order of their magnitude, beginning with the smallest, and those which are equal, or nearly so, are placed opposite to each other. The reason of this arrangement will be explained presently.

TABLE I.

Before Respiration.—After Respiration.

B. R.	A. R.	B. R.	A. R.
510	510	687	—
520	—	694	—
550	546	703	—
—	562	713	726
586	590	744	746
630	—	—	774
630	—	—	861
632	—	—	920
640	—	1054	1000
647	—	—	1173
658	—	—	1189
666	—	—	1203
683	675	1480	—

On adding together the numbers in the two columns we obtain the following results :—

TABLE II.

	Before Resp.	After Resp.
Maximum	1480	1203
Minimum	510	510
Mean	707	820

If the results of so small a number of facts were entitled to any confidence, it would follow that the absolute weight of the lungs of mature children who have respired, exceeds that of mature still-born children by only one-seventh. This small addition to the weight of the lungs, however, by no means represents the full effect of respiration, inasmuch as still-born children are larger, and their lungs, consequently, proportionally heavier, than those of children born alive. Thus, in the present instance, the average weight of the body in the still-born is 41,547 grains; in those born alive, 41,492 grains; the former exceeding the latter by about one-eleventh. If we suppose the mean weight of the body of the still-born and those born alive to be equalised, and the mean weight of the lungs after respiration to be increased in the same proportion with the weight of the body, we shall have the weight of the lungs after respiration about 880 grains, instead of 820, or an increase of one-fourth, instead of one-seventh. Small as this difference may appear, it does not vary much from the average deduced from a larger number of facts; thus, in the essays already referred to,* the excess

of the weight of the lungs of children born alive over those of the still-born is one-fifth. On referring to Table XXXII., however, in which the weights of the lungs before and after respiration for equal weights of the body are compared, it will be seen that for bodies weighing between 40,000 and 50,000 grains, the process of respiration adds little less than one-half to the weight of the lungs. But the averages in these tables were deduced from observations on children who had survived their birth one month or less, whilst the tables in the present essay includes only one case in which the duration of respiration had exceeded three days. This difference sufficiently explains the different effect of respiration in the two sets of tables.

It results, then, from the above table, and from those now referred to, that the process of respiration when *recently* established increases the weight of the lungs by about one-fourth, and that when it has continued one month or less, the average increase for equal weights of body is less than one-half. These weights and this increase is very remote from the rough estimate which has made the weight of the lungs before respiration about one ounce, and after respiration about two ounces. The ascertained weight before respiration is nearer two ounces than one; and the increase, even when respiration has lasted a considerable time, is less than one-half, instead of being equal to the weight assumed for the still-born.

This trifling average difference between the weight of the lungs before and after respiration gives little encouragement to the employment of the mean numbers as standards of comparison in doubtful cases. Where the averages differ so slightly, it must necessarily happen that the numbers from which they are respectively derived must often coincide. A glance at Table I. will show that this inference is not less probable than true; for although the smaller numbers abound at the top of the left-hand column, and the larger ones at the bottom of the right, the same number or numbers differing but little from each other, are found no less than seven times in both columns. In these cases, of course, the weight alone could have given us no assistance in solving the question of respiration or no respiration.

But it is still more worthy of notice, that whereas the same number, 510, is the minimum before and after respiration; the greatest weight in the still-born *exceeds* the greatest weight in those born alive by no less than 277 grains, or rather more than one-fourth. As it is obvious that any number intervening between the highest and lowest weight might occur either before or after respiration, so it would be quite impossible to decide by the weight alone whether the child had or had not respired. The possibility of a very low weight of lung being met with in a child which has survived its birth some

* Edin. Med. and Surg. Journal, No. 150, conclusion 9, in which read 874 for 574.

time, is illustrated by the second number in the right-hand column (546), the weight of the lungs of a child which had survived its birth two days. As this is by no means the only instance of prolonged respiration to be found in the table (there are instances of twenty-four hours, two days and a half, and even one of two months), it follows that the table presents rather a favourable view of the effect of respiration in increasing the weight of the lungs. When we reflect that in the majority of doubtful instances of infanticide respiration is not likely to have continued many minutes, and that consequently but little addition could have been made to the weight of the lungs, we can have little hesitation in summarily rejecting the absolute weight of the lungs as a test of respiration.

The fact already alluded to, viz., that the bodies of still-born children are heavier than the bodies of those born alive, greatly impairs the use of the absolute weight of the lungs as a test of respiration; and very naturally suggested the modified test which bears the name of Ploucquet. This test consists, as is well known, in a comparison of the weight of the lungs with that of the body. How favourable the three facts on which Ploucquet so strongly based his recommendation of that test were to its application, is equally well known. According to these imperfect data, the proportion which the weight of the lungs bore to that of the body before and after respiration respectively, was 1 to 70, and 1 to 35; proportions much more favourable than those established by the observations of any subsequent observer. The following table exhibits at a glance the value of the test, as far as it can be determined by a small number of facts:—

TABLE III.

Before Respiration.—After Respiration.

B. R.	A. R.	B. R.	A. R.
1 : 91.....—		1 : 57.....	1 : 56
1 : 82.....—		1 : 54.....	1 : 55
1 : 74.....—		1 : 51.....	—
1 : 71.....		1 : 51.....	1 : 52
1 : 71.....		1 : 49.....	1 : 49
1 : 70.....		—.....	1 : 48
1 : 67.....	1 : 65	—.....	1 : 46
1 : 61.....	1 : 61	1 : 45.....	1 : 45
1 : 60.....		—.....	1 : 44
1 : 59.....	1 : 59	—.....	1 : 44
1 : 57.....		—.....	1 : 41
1 : 57.....		—.....	1 : 39

TABLE IV.

	Before Resp.	After Resp.
Maximum.....	1 : 45.....	1 : 39
Minimum.....	1 : 91.....	1 : 65
Mean.....	1 : 63.....	1 : 50

In the formation of Table III., the same facts* are employed as in Table I.; and

the result is obviously more favourable to Ploucquet's test than to the absolute weight of the lungs; for whereas there was not a single instance in which we could have been justified in inferring respiration or the reverse from the absolute weight of the lungs, there are no less than eleven cases in which this question might have been decided by Ploucquet's test, provided the number of observations were held sufficient for the establishment of a numerical test. The first seven lines of the left-hand column are instances in which the proportion which the weight of the lungs bore to that of the body was less than in any instance in which respiration had taken place; whilst the four last numbers in the right-hand column are instances in which the proportion of the lungs to the body was greater after respiration than in any case of still-birth. Hence, there are seven cases in which, assuming 1 : 65 to be the real minimum after respiration, we should have been justified in stating that the child was still-born; and four cases in which, assuming 1 : 45 to be the real maximum before respiration, we might have asserted with like confidence that the child had breathed.

But here the question arises, are we justified, on the authority of so small a number of facts, in asserting that 1 : 65 is the real minimum after respiration, or 1 : 45 the real maximum before respiration? Certainly not; for it might happen that the very next proportion added to either column would destroy the value of all the figures above or below the opposite one.

On referring to my former essays, I find that the proportions derived from a larger number of facts are widely different from those of Table IV. Thus, the maximum before respiration, instead of being 1 : 45, is 1 : 24; and the minimum after respiration, instead of being 1 : 65, is 1 : 132. The general conclusions in my former essay, however, fully bear out the results of the tables contained in the present, as far as regards the superiority of Ploucquet's test to the absolute weight of the lungs; for "whereas there were only eight instances in which the maximum weight of the lungs after respiration exceeded the maximum weight before respiration, there are no less than twenty-nine instances in which the proportion which the lungs bear to the body is greater after respiration than before; that is to say, there are twenty-nine instances in which, assuming the proportion 1 : 24 to be the true maximum, we could state with certainty that respiration had taken place. On the other hand, there is only one instance in which the proportion of the lungs to the body is less before respiration than the least proportion observed in children born alive; in other words, there is only one case recorded in which, assuming 1 : 132 to be the real minimum after respiration, we should have been justified in as-

* With the exception of one, in which the weight of the body was not given.

getting that respiration had not taken place."

One serious objection must always lie against numerical tests, employed for purposes demanding extreme accuracy, as is the case with all medico-legal inquiries, viz., that the highest and lowest values, which are the only ones that can be employed with any confidence, can be ascertained with certainty only by an accumulation of facts never likely to be made for any purpose. If one group of observations establish a given maximum before respiration, and a given minimum after respiration, it does not follow that the addition of another group of observations, or even of a single observation, may not establish a higher maximum or lower minimum. Hence we are never in a position to state with confidence on the strength of these tests that a child has or has not respired. Where our observations are very numerous, we may use the extreme values as standards of comparison, with a confidence proportioned to their number, and to confirm probabilities derived from other sources; but the occasions on which we can make use of these tests are of extremely rare occurrence.

Assuming, then, for the moment, that Ploucquet's test admits of occasional application, the question arises, is it necessary to make use of it in those instances where it would give a strong probability? The answer is obvious. Such a test can never be necessary, where other and more simple means are at hand of determining the same question beyond the reach of doubt or cavil. What those means are I shall take some early opportunity of showing; at present I am satisfied with proving the very limited use which can by any possibility be made of Ploucquet's test, and the still lower value of the absolute weight of the lungs as a test of respiration.

September 24, 1842.

DISCOVERIES IN EMBRYOLOGY.

(From Professor VALENTIN'S Report on the Progress of Embryology in the Year 1840.)*

SOME interesting discoveries rendered the past year a highly productive one for embryology. Two main problems which engaged the various physiologists here occupy the foreground, namely, the earliest development of the mammalia, and the metamorphoses of the germinal membrane in its transformation into the embryo. * * *

So long as the metamorphoses of the Purkinjean [germinal] vesicle, following fecundation, could be considered only hypothetically, it was assumed that this vesicle either burst and poured out its contents, or became flattened, and now contributed to the forma-

tion of the germinal membrane in one of these two ways. Both theories had been put forth before the discovery of the germinal spot. But when the existence of the latter became known, the discoverer said that probably the *macula germinativa* represented the first foundation of the germinal membrane. This conjecture obtained more probability from the obvious fact, that the number, size, and distribution of the germinal spots alternated according to the different stages. Research, however, first in the mammalia, and then in reptiles and fishes, showed that in consequence of fecundation the interior of the germinal vesicle presents new cells, or that (as was seen in the rabbit) within the germinal vesicle new cells are really built up upon the foundation of the germinal spots. (Introductory Remarks, p. 13.)

First stages in the development of the fecundated ovum, especially that of the mammalia.—As was already remarked in the introduction, the most important publications of the past year concerning embryology are concentrated in the subjects of this chapter. We will, therefore, before presenting some extracts of the details, state the most important results. With few exceptions, to be mentioned, all the observations have reference to the mammalia, and indeed to the rabbit.

(1.) *At the period of the rut certain changes have already taken place in the ovarium, the [Graafian] follicles, and the structures appertaining thereto.*—Through an increased congestion of the ovary single follicles become more strongly developed. The germinal spot, which gives the impulse to the formation of the new cells, probably undergoes changes of this kind. From the observations of Negrier, above-mentioned (p. 248), it may be conjectured that in the human female also the period of menstruation is attended by similar phenomena.

(2.) *Fecundation itself apparently comes to pass in the following manner.*—A portion of the semen that has been brought to the surface of the ovarium probably passes into the ovum, and gives the stimulus to the formation of cells within the germinal vesicle. * * *

(3.) *The number of ova prepared for fecundation by the rut does not correspond with the number of the subsequently fecundated ova, but generally exceeds the same.*—This fact, already known, has been confirmed by the latest researches on the rabbit.

(4.) *It often happens that more ova pass out of the ovary than are fecundated, or at least than become developed.*—Herein accord the observations of Barry with those of Pappenheim. The former found in the tubes and uterus unfecundated or aborted ova. In like manner parts of the [Graafian] follicle which usually remain in the ovary, for example, portions of Barry's ovisac, may be found in the oviducts.

* Repertorium für Anatomie und Physiologie. Jahrgang, 1841.