Case 35.—E. S., aged 58, operated on in 1908, carbolic acid and a sling being employed. *After-History:* Writes, February, 1910, that she is "much better; can get about and do a little of her work. The pains are entirely gone, and everything is very satisfactory."

Case 36.—A. S., aged 44, seen and operated on for movable kidney in 1908, carbolic acid and a sling being employed. Appendicectomy also was performed. The patient had long been an invalid; Mr. Grieg Smith had removed her ovaries on account of pain. *After-History:* After the operation in 1908 she was well for a year, but since then she has had much pain in the appendix region, and is a chronic invalid. She was seen and examined in February, 1910, when the kidney could not be felt and appeared to be fixed. No general enteroptosis. I thought some such condition as mucous colitis probable, from the history and character of the pain.

Case 37.—A. B., aged 33, was operated on in 1902, carbolic acid and a gauze sling being employed; the kidney was stitched to the rib to rotate it. The patient had hydronephrosis and kinked ureter. Two months after operation she was relieved. No subsequent information.

Case 38.—E. W., aged 22; operation in 1905; carbolic acid and sling. No after-history.

It will be observed that I have been able to obtain some after-history in 30 out of 38 cases, not including one (A. B.) who was well two months after the operation. Of these 30 cases a classification may be made as follows:

Operation.	Cases.	Cured.	Considerable Relief.	Little or no Relief.	Death.
1. Transcortical suturing	13	9	1	4	0
2. Suturing of capsule	8	3	1	3	1
3. Carbolic and sling	8	5	1	2	0
Total, all methods	30	17	3	9	1

One patient had both kidneys fixed with complete relief, but I have only counted her once.

It may be simplest to use the information at our disposal to find an answer for certain important questions.

Is the Operation for Movable Kidney to be Urged on Patients?

The figures have been collected by writing to hospital cases, in several instances many years after the operation was performed. This is to put a severe strain on the reputation of any operation. Such highly esteemed procedures as appendicectomy and gastro-jejunostomy would be considerably criticized by such a testing. The patient may include all sorts of subsequent troubles in the reply, and call a success a failure in consequence. In many of the "failures" here recorded the fixation of the kidney was excellent. Again, these patients are hardworking women, unable to take care of themselves and rest when weary. Nevertheless, they were well pleased with the result in 20 out of 30 cases. As we shall see, the failures were often due to an erroneous diagnosis. It may be concluded that when other possible causes of the symptoms can be eliminated the operation for movable kidney is usually satisfactory.

What are the Usual Sources of Failure?

There are two. The first is slipped kidney. In the list before us we have clear evidence that in two cases (A. M., M. M.) following the transcortical operation, and in probably two cases (A. G. and probably H. T.) following the operation of suturing the capsule, the kidney has become loose again. In a case operated on before 1900 by the transcortical method relapse occurred (E. E.), and she had a second fixation in 1900. There is no proof of relapse following the carbolic and sling operation.

It does not necessarily follow that the symptoms will return in full force. Two of the cases of slipped kidney have been classed as having obtained "considerable relief."

Some care is necessary before deciding that a kidney has really become loose again. In many cases the kidney is fixed lower than its normal anatomical position, and it may move an inch or two with respiration, and even allow the hands to meet above it, without becoming abnormally mobile.

The other source of failure is an erroneous diagnosis. Evidence for this is fairly clear in some of the cases. One patient (A. M.) had her appendix removed and her kidney fixed twice without relief; she then had a gastro-jejunostomy for slight dilatation of the stomach from healed gastric ulcer, and has been perfectly well for more than two years. Another (E. W.) had general enteroptosis. Another (A. S.) had already had her ovaries removed, and her appendix was removed when the kidney was fixed. In one other patient (M. M.) relapse appeared to be due to dragging on the scar, the muscles not having been sutured in layers.

After how long an Interval may the Patient be regarded as in all probability Cured?

Various dates are given for relapse of the pain—five weeks, six weeks, four months (kidney slipped during accidental strain), nine months (scar became weak), and one year. This last was the patient (A. S.) referred to above, in whom the kidney was probably not at fault.

What Method of Operating Gives the Best Results?

While the aggregate numbers are large enough to judge of the success of the operations considered as one, I do not consider that they are adequate to decide with authority between the three operations practised. I was particularly unsuccessful in tracing the after-histories of the second operation, and so may have missed cases that would have put it in a more favourable light. When larger statistics are available to be added to these, we may be able to speak with more confidence. We must give the third method credit that it is not yet proved that the kidney can slip afterwards, but it would be unwise to build too much on the negative evidence of only 8 cases.

Is the Suture Material of any Importance?

Three kidneys were sutured with silk, and are known to have remained fixed. In 12 cases catgut was used, and 4 have failed, possibly on account of slipping. In 6 cases reindeer tendon was used; of these 2 were not relieved, possibly owing to slipping of the kidney. Cases in which the return of pain was certainly not due to the kidney are not included here.

SUMMARY.

1. Complete or considerable relief was afforded in 20 out of 30 cases, treated by several different methods of operation.
2. In at least 3, probably 4 cases, the kidney again worked loose.
3. Several of the failures were certainly due to a mistaken diagnosis as to the cause of the pain.
4. No cases of slipping were found in 8 cases treated by the carbolic paint and gauze sling method, and followed from two to ten years.

THE "CARDIO-SPLANCHNIC PHENOMENON" OF ABRAMS.

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In the *American Journal of Medical Sciences* of January, 1904, p. 125, will be found a paper by Dr. Albert Abrams, of San Francisco, on what he terms the "cardio-splanchnic phenomenon," and defines as follows:

"If the lower sternal region—that is, the sternum contiguous to the heart—is first percussed in the standing and then again in the recumbent posture, one may appreciate a decided alteration in the percussion tone; in the former attitude it is resonant or even hyper-resonant, in the latter it is dull or flat. This is the cardio-splanchnic phenomenon."

Now this description is absolutely contrary to fact, and, thus defined, there is no such thing as the "cardio-splanchnic phenomenon." I was unaware of Dr. Abrams's statement till my attention was called to it last year, when I published a paper¹ dealing with the rightward edge of the cardiac dullness and the changes which occur in it with change of position. This paper slightly amplified what I had already pointed out in 1905.² I showed that, alike in health and in disease, the rightward margin of the

heart's dullness is, almost always, and often much, further to the right in the erect than in the recumbent posture; also that this is a point of great clinical importance, since it necessitates comparisons in comparable positions; and finally that in most cases the transverse measurement of the dullness in the erect position yields a closer approximation to the true width of the heart than the measurement made in recumbency.

The question at issue is thus no academical one; the error involved is not of the pleasant sort which can be left to die a natural death. On the contrary, this error stands right across the path to a better appreciation of the effects of posture on cardiac physical signs, that *terra incognita* to the majority of cardiac case-recorders. It must therefore be summarily disposed of, and I would ask my readers to satisfy themselves, as they can very quickly, of the absolute incorrectness of the statement I have quoted.

In order to percuss similarly in the two positions, I would suggest—though such a precaution is by no means essential—that the margin of dullness be mapped out in the sitting-erect and recumbent postures, with the fingers of the hand on the chest pointing down the body, so that no error can be thought to be introduced by a different mode of application of this hand. It will be observed that Dr. Abrams's results and mine were both arrived at by the ordinary digital method of percussion.

As sufficient instances of my own percussion have been already published, and as it may be desirable to show to some that I am by no means alone in my conclusion, I subjoin the following observations, kindly made for me, on unselected normal chests, by my friends Dr. Ransom Pickard and Dr. R. V. Solly. "R." and "L." mean "to right" and "to left" of the mid-sternal line. It will be noticed that there is not a single exception in the series. For convenience of comparison with previously published measurements, the dullness has been taken as beginning at the right side of the finger laid on the chest where the note on it, moving leftwards, is first noticed to become dull.

Dr. Solly's Measurements.

Sex.	Age.	Sitting Erect.	Recumbent.
M.	30	R. 1½ in.	R. ¾ in.
F.	30	R. ¾ in.	Mid-sternum.
F.	16	R. 1¾ in.	Mid-sternum.
F.	21	R. 1½ in.	L. ⅞ in.
M.	20	R. 1½ in.	L. ⅞ in.
M.	23	R. 1½ in.	L. ⅞ in.

Dr. Pickard's Measurements.

Sex.	Age.	Sitting Erect.	Recumbent.
M.	31	R. 1 in.	L. ¾ in.
M.	19	R. 1 in.	L. ¾ in.
M.	22	R. 1½ in.	L. ¾ in.
*F.	28	R. 1½ in.	Mid-sternum.
F.	30	R. 1½ in.	Mid-sternum.
F.	20	R. 1½ in.	Mid-sternum.

* This patient was slightly anaemic.

I have explained these changes (in the way which naturally occurs to one) as being due to a dropping back of the heart from the anterior chest wall, when the recumbent posture is assumed. Dr. Abrams does not agree with this view, and states that skiagrams show that no such dropping back occurs. But Professor Rudolf of Toronto has published a skiagram showing plainly that it does occur,⁴ and my friend, Dr. J. D. Harris, has taken skiagrams of three chests which also show this. Indeed, it is difficult to see why the heart should be independent of gravity, unless we assume it to be, what it usually is

not, a rigid organ completely filling the interval between the spine and the sternum.

But, in any case, the appeal to skiagrams does not greatly help Dr. Abrams, for it is not difficult to introduce error in such lateral photographs by slightly differing the direction of the rays passing through the chest. And he has himself found it impossible to demonstrate a change in sagittal skiagrams of the heart corresponding to the change he describes in the percussion dullness.

The following facts can be easily enough explained, if it be assumed that the heart tends to fall back from the anterior chest wall on recumbency when free to do so, but are difficult to account for otherwise.

1. Whereas in most hearts, normal or diseased, the changes indicated above in the rightward margin of dullness can be readily identified, in cases of extreme cardiac enlargement it is not uncommon to find no change whatever, the heart being apparently firmly wedged, owing to its size, between the anterior chest wall and the spine.

2. In the great majority of cases of cancer in their later stages—and in some cases rather early—the heart is lax like the other muscles, and is imperfectly filled (as the patient's pale surface, feeble pulse, and heart sounds, and half-empty arteries indicate), besides being sometimes smaller in size than normal; in these cases the transverse measurement of the cardiac dullness may be almost normal in the erect position, yet when the patient lies on his back this measurement becomes strikingly reduced, and, indeed, the dullness sometimes actually vanishes altogether.*

How, then, can such an error have arisen? Apparently from two causes:

1. Dr. Abrams has evidently approached the subject with a biased mind. He begins his paper thus:

To my knowledge this is a heretofore undescribed phenomenon as far as its clinical manifestations are concerned. The facts, however, up to the point of its clinical identification have been fully established by the physiological investigation of others.

And writes further on:

In the erect posture the blood leaves the right heart, which topographically is beneath the lower sternum, and tends to accumulate in the splanchnic area, whereas recumbency opposes this influence of gravity.

2. He has confused percussion sounds with percussion sensations of resistance—two very different things, which must especially be kept apart in this investigation, seeing that in recumbency the anterior chest wall itself usually falls slightly towards the spine, as I have shown,⁶ a change which may possibly affect the sensation of resistance in percussion. He expressly states that "in the elicitation of the cardio-splanchnic phenomenon palpatory percussion must be our chief mentor," explaining this term thus:

The skilled clinician in his interpretation of percussional phenomena is guided not so much by the ear as by the *tactus eruditus*; thus, palpatory percussion will often yield results which would wholly escape the observer who is influenced only by what he hears and not by what he feels.

In his definition, however, of the "cardio-splanchnic phenomena" above quoted he uses terms which are well understood to describe sounds and sounds only. "Resonance," "hyper-resonance," "flatness," and "dullness" are not descriptive of resistances. As his definition stands, his description of the effect of change of position is, as I have said, absolutely contrary to fact.

* It should also be remembered that a sort of spurious emphysema often exists in cancer cases, which may help to produce the remarkable reduction of dullness. Dr. Soltan Fenwick, I think, first called attention to this.⁶

REFERENCES.

- ¹ Loc. cit., p. 128.
- ² BRITISH MEDICAL JOURNAL, June 4th, 1910.
- ³ *Med.-Chir. Transactions*, vol. lxxxix.
- ⁴ BRITISH MEDICAL JOURNAL, October 20th, 1906.
- ⁵ *Cancer and Tumours of the Stomach*, 1902, p. 74.
- ⁶ BRITISH MEDICAL JOURNAL, March 15th, 1902, "Posture and Heart Murmurs."

THE Isle of Man Hospital and the Children's Home each receive £1,000 under the will of the late Mr. James Mylchreest, late Lloyds agent in the island.

UNDER the will of the late Mrs. Margaret Eliza Clague, the widow of a Manx medical man, Guy's Hospital, London, and the Royal Infirmary, Liverpool, each benefit to the extent of £1,000. To Noble's Hospital in the Isle of Man is left a house in Douglas.