It would appear from these tables that, after attaining a certain elevation, the frequency of the pulse is invariably increased. The level, however, at which this acceleration commences, evidently varies in different individuals; while in some, there is reason to believe that the influence of diminished atmospheric pressure may, within certain limits, be counterbalanced by concomitant circumstances capable of reducing the frequency of the pulse even while under such influences.

The pulse of M. Le Pileur was much less frequent at Chamounix than at Paris, although in the former locality there was everything in the approaching expedition calculated to produce nervous excitement. At Chamounix, it beat 60 per minute, and at Paris 67·25, notwithstanding the superior elevation of the valley.

In the case of M. Martin, on the contrary, the average frequency of the pulse was greater at Chamounix than at Paris, being 62·04 in the latter, and 69·00 in the former locality.

These same observers, having likewise made experiments upon the pulse of three guides who accompanied them to the summit, found the following differences in point of frequency between the level of the Valley of Chamounix and the highest point of the mountain:—

	Average of the pulse at Chamounix.	Do. at the summit.
Mugnier, aged 36	63·12	94·40
Coutet, " 34	77·75	117·83
Simon, " 27	69·22	111·66

It is worthy of note, that in these men, born among the Alps, the acceleration of the pulse was greater than in the case of MM. Le Pileur and Martin, who were habitually residing on a comparatively trifling elevation above the sea level, and but little accustomed to mountain air and exercise.

A *resumé* of the foregoing remarks may, I conceive, lead to the establishment of a few propositions relative to the affection which I have here designated by the term "mountain sickness".

1st. That in mountainous districts, and upon attaining a certain elevation, a series of physiological phenomena manifest themselves, which differ widely from the standard of health, and exist as long only as the exciting causes are in activity; disappearing upon a return to the ordinary level of human habitation.

2nd. That the discrepancy existing among travellers relative to these phenomena, is to be accounted for by their variability both in nature and degree; this variability being itself dependent upon conditions, referrible on the one hand to the individual, and on the other to the locality.

3rd. That, on the part of the individual, the following circumstances may modify the nature and intensity of the "mountain sickness": idiosyncrasy, previous condition of health, the habit of fatigue (especially that produced by ascents), and previous residence in a rarefied atmosphere.

4th. That, as regards the locality, the phenomena in question are most strongly marked where a considerable elevation above the previous residence of the individual is attained in a short space of time; as on the Pass of the St. Bernard, the Col of the Mont Moro, and the Col de St. Theodule, in the Alps.

5th. That the mountain sickness is characterised by the following symptoms, the entire category of which, however, is seldom, if ever, united in one individual: vertigo, headache, somnolence, dyspnoea, constriction of the chest, palpitation, syncopal tendency, occasional oozing of blood from mucous surfaces, increased rapidity of pulse, anorexia, nausea and vomiting, thirst, febrile tongue, muscular pains, sense of extreme debility in the lower limbs, with general prostration of strength.

6th. That these symptoms may be referred to a threefold source, viz., a gradually increasing congestion of the deeper portions of the circulatory apparatus, increased venosity of the blood, and loss of equilibrium between the pressure of

the external air and that of the gases existing within the intestines.

7th. That these exciting causes of mountain sickness are themselves the result of a change from a given atmospheric pressure and temperature, for one in which both are greatly and suddenly diminished.

Cheltenham, Nov. 24th, 1852.

ON INTERNAL UTERINE HÆMORRHAGE.

By GEORGE KING, Esq., Surgeon.

It may be remembered that I have drawn the attention of the readers of the former series of the *Provincial Medical and Surgical Journal* to the subject of *internal uterine hæmorrhage*, which seems to have escaped the notice of practitioners, as well as teachers and writers on obstetric practice; at least I have, like many others, searched in vain for information on this important subject, and for the records of such cases.*

The case reported in the number of the *ASSOCIATION MEDICAL JOURNAL*, for January 7th, as a "*peculiar case of accidental uterine hæmorrhage*," by Isaac Harrison, Esq., of Reading, was, I consider, one of *internal uterine hæmorrhage*. Being, I believe, the first who has directed the attention to this peculiar kind of uterine hæmorrhage, it is natural that such a case as Mr. Harrison's should attract my notice, and that I should make some remarks on it.

In directing attention to an occurrence which I consider has often till too late been neglected, *internal uterine hæmorrhage* during the period of gestation, I wish it particularly to be understood, that I do not mean to say that there is no external appearance or sanguineous discharge from the vagina: there often is in such cases; but it is so slight, and frequently at such lengthened intervals, that it passes unnoticed by both patient and doctor. This state of things goes on, until the patient herself begins to feel its debilitating effects on her constitution; and the practitioner is unconscious of the mischief going on, until his suspicion is excited by the constitutional symptoms. There is a universal paleness, which is first discovered in the face; the pulse becomes quicker and weaker; the abdomen is discovered to be much distended; the patient becomes restless and faint; her friends are anxious; and the *accoucheur* begins to see the danger, and feels his responsibility. All this may take place at almost any period of gestation, and without there being any *external* discharge. The danger does not so much depend on the quantity of blood lost, as it does on the velocity with which it is lost. In these cases, we cannot, by any means that I know, judge for a certainty either as to the quantity or velocity. All we know is, that frightful *external hæmorrhage* may occur; floodings may last for days; and even gushes of blood may be poured out from the vagina, without exciting much alarm for the safety of the patient; yet with even the slightest *internal hæmorrhage*, this insidious dripping within the cavity of the uterus may continue some time, and produce the most serious constitutional derangement, demanding the most prompt and energetic treatment, without our having any visible sign, or the slightest indication or knowledge of the cause, or the point from which it is emitted. We have yet to learn why the powers of nature should give way and often sink from such apparently trifling sanguineous discharges, and linger and survive a much more profuse one; and also why the uterus, a muscular substance, an organ only designed for the purpose of generation and the perfection of the fœtus (though it may be very vascular during gestation), should become a sponge to sop up the vital fluid of the mother as well as of the child, so that the life of both frequently is destroyed. As practitioners, we have to do with the symptoms, conse-

* I regret to find on referring to the index of the last volume of the *Provincial Medical and Surgical Journal*, that the word "*internal*" is left out in reference to the cases and treatment published by me.

quences, and treatment; and this is a very painful position to be placed in, as our resources are very limited, and the remedial measures are very few; and all those medicines which are called specifics in stopping hæmorrhage, are as futile as incantations. There is no medicine in the *Materia Medica* that can have a styptic effect in these cases; but we must do something, and all that can be done must be done mechanically, by removing the contents of the womb, or by external applications, employed as I have stated in another paper on this subject. The only means to be depended on as a remedy in these desperate cases, is cold water, a small hand, and the blunt hook. I have given directions for the application of cold water over the abdomen in such cases. If we can make the surface of the body contract, the internal parts will by sympathy contract likewise. Three out of four patients are lost in uterine hæmorrhage, mainly for want of being delivered when there was a possibility of delivering them, and a chance of saving their lives. The difficulty in such cases is not in what we ought to do, but when we should do it. I am not a disciple of the mystical German school, nor do I believe in the hydropathic method of treating diseases; but I do believe that in *internal* uterine hæmorrhage, the *wet sheet* may be very beneficially applied.

I have been led to these remarks by reading the case of accidental uterine hæmorrhage by Mr. Harrinson, as the case published by me at p. 38 of No. 11, New Series, which I sent previously to the date of Mr. Harrinson's paper, is so similar.

Mr. Harrinson asks, "Could a more accurate diagnosis have been arrived at? and if so, could the treatment have been materially modified?" This would imply that he wished to have the opinion of others, and that he sought information on the subject, as to the diagnosis.

With such symptoms, it was natural to suppose that there was some great mischief going on within the walls of the abdomen; and, in the patient's condition, the uterus would be suspected to be the seat of the disease or disorder, and the existing cause ought to be removed; but how and when was this to be done? Without having seen the case, it is very difficult to say what ought to have been the treatment; and it may be considered presumptuous to say, that anything that was done ought not to have been done, or that anything was left undone that ought to have been done, as there might have been some peculiar idiosyncrasy of constitution, or many special circumstances, that could be only judged of by those who were present. Still, a remark or two on the general treatment of such cases may be useful to those who may be so unfortunate as to meet with similar cases in their practice.

After the rupture of the membranes, a full dose of the secale cornutum might, I think, have excited the contracting power of the uterus, and, by its stimulating quality, have kept up the failing powers. The mode of delivery I do not consider to have been the most safe and proper. The blunt hook would have been a much safer instrument, and more applicable than the perforator or forceps. As there was no propelling action of the uterus, and the head presented, there would have been no difficulty in introducing it, and of getting a good hold. The child, I suppose, was dead, and it would not have been of much consequence to what part we had fixed it. The chin, occiput, and mouth, were to be easily got at, and there appears to have been no rigidity of the os uteri, resistance, or want of repose. There was only a lack of physical power: this the blunt hook would have safely and easily supplied. The operation of craniotomy can never, in my opinion, be justifiable, unless there be a hydrocephalic head presenting, or an unusual degree of ossification of the entire bones of the cranium, or a very great degree of deformity and contraction of the pelvic orifice, or a dying patient. I believe it would not be considered orthodox to let a woman die undelivered. The powers of action in this case did not seem to have been exhausted, as she survived some hours after the loss of some quarts of *fluid* blood. Its being fluid is, I consider,

a clear proof that that was not the cause of the previous symptoms; but that the clots resulted from the internal hæmorrhage going on before delivery was attempted. We thus possess two means or ways of restraining uterine hæmorrhage; firstly, by exciting the power of contraction; and, secondly, by promoting the formation of coagula. Mrs. G. must have sunk from the loss of the quarts of fluid blood. I never lost a patient from hæmorrhage after delivery, who survived it an hour.

Bath, January 14th, 1853.

SUDDEN DEATH FROM COLLAPSE IN A CASE OF RUPTURE OF THE BLADDER.

BY ALEXANDER HENRY PATERSON, M.D.

CASE. January 15th, 1853, M. Corr, æt. 45, a labourer, was, at half-past eleven, P.M., slightly intoxicated, and engaged in a quarrel with another man. On receiving a blow on the chest, he fell, but appeared not much hurt. He was separated from his antagonist, and walked a short distance. The two men then met, and renewed their dispute; and collaring each other, rolled down together, Corr falling on his back, and his antagonist upon him. As Corr was rising, his antagonist, who had regained his feet, ran towards him, and kicked him in the lower part of the abdomen. The blow was the more violent as the man had wooden clogs on. Corr fell back, and died immediately.

On making a *post mortem* examination, on the 17th, I found the brain healthy, though congested; the heart was free from disease, but much distended with black, feebly coagulated blood. The abdomen appeared healthy, and free from all injury. On scratching aside the peritoneum from the pelvis, my finger passed into the bladder, through a rent almost two inches in length. There was some bloody urine, perhaps an ounce, effused into the cellular tissue. The injury occurred on the left side of the body of the bladder. That organ and the urethra were healthy, and the peritoneum was uninjured. The body did not exhibit any internal marks of violence.

REMARKS. I have not met, either in Beck's or Taylor's Jurisprudence, with a similar case; and though I have seen several fatal instances of laceration of the bladder, death has not occurred for some hours or days.

It appears to me clear, that in this case death resulted from the shock to the sympathetic system; and that the rupture of the bladder had nothing to do with it. The darkness and fluidity of the blood, and the congested state of the brain, lungs, and especially of the heart, all confirm this view.

Altrincham, Cheshire, January 1853.

BIBLIOGRAPHICAL NOTICES.

A TREATISE ON TUBERCULOSIS, THE CONSTITUTIONAL ORIGIN OF CONSUMPTION AND SCROFULA. By HENRY ANCELL, late Surgeon to the Western General Dispensary, etc. pp. 778. London: 1852.

THE object of this very able and elaborate work is to draw attention to that state of the constitution which is characterised by the term *tuberculosis*, and on which Mr. ANCELL is satisfied that scrofula and consumption, in all their varied manifestations, depend.

"Tuberculosis is herein regarded, strictly, as a blood disease. I have endeavoured to collect all the most important facts relating to the physical and chemical properties of the blood, the aberrations observed in the phenomena of cell-growth and nutrition, the histology of tubercular deposits, and the special pathological anatomy of the tissues and organs, and have referred them to the primary affection as sequences, or results. Those maladies which are but local manifestations of the mor-