

The cases recorded by Dr. Braxton Hicks are the very antipodes of this description. "A constant discharge, profuse, irritating, watery, frequently mixed with blood, and very fetid; the pain above the pubes very distressing, preventing sleep, like labour-pains; very anæmic and flabby aspect; extremely weak. (In the course of a few months) the last time I heard of her, she was sinking fast. A growth the size of a Seville orange was attached to the whole of the os uteri; the consistence so soft that the finger passed into it upon the slightest pressure; the whole organ flexed from infiltration of solid deposit into the surrounding parts, hard and enlarged; the large encephaloid mass was removed, but the solid scirrhus infiltration in and about the uterus increased, giving rise to severe pain; it extended to the vaginal wall, and in four months had arrived at the meatus urinarius."

Sir Charles Clarke clearly points out the distinction between these two distinct diseases:—"A carcinomatous thickening bears no resemblance to the cauliflower excrescence, and the size of the former remains unaltered by pressure and undiminished by death." And from the remarks of Dr. Hicks it is evident he has never seen a case of cauliflower excrescence, and does not know the disease.

Dr. Hicks, however, observes:—"The growths were exactly like a 'cauliflower,' much more than to 'brain.' Hence I call them 'cauliflower,' not 'encephaloma.'" But the singular fact is he does not call them "cauliflower" except in the title, and does call them "encephalomatous mass," "encephaloid mass," "solid scirrhus infiltration," "infiltrated with scirrhus," "malignant disease," &c. And I must consider it a most unwarrantable proceeding to describe an "encephalomatous mass" a "malignant disease" involving the substance of the uterus, and extending nearly the whole length of the vagina, as a "cauliflower excrescence," which grows from the mucous membrane near the os uteri, never extending into the structure of the uterus, and of so simple a nature that it can be cured by the use of astringents.

But this proceeding of Dr. Hicks, as well as his remarks upon the subject, are explained by the romantic views he enunciates with regard to "malignant disease," which he defines "depends upon an over-excitement and misdirection of the cell-formation rather than upon the deposits of any particular form of cell"; as "dependent on the relative intensity of the malignant force in the elementary tissue most affected by it." Here I must admit he has the advantage of me, for I cannot form the slightest idea of what an "over-excited cell" or a "misdirected cell" can be like, nor how either of these singular anomalies of nature, supposing them to have any existence, can be influenced by the relative intensity of a malignant force which appears to exist only in Dr. Hicks's imagination. But he appears so certain it does exist there that he adds: "This seems to be the more philosophical explanation of the change, making the various forms of the disease rather a question of degree than of any specific difference beyond that derived from the nature of the structure involved." Again I must confess my inability to comprehend what is meant by this transcendental philosophy. But this I do gather: Dr. Hicks finds cells in the "simple villous disease"; finds them also in "ordinary epithelioma"; finds them also in an "encephalomatous mass"; and he imagines the possibility that these cells in the "simple villous disease" may become "over-excited" or "misdirected" by "the relative intensity of the malignant force,"—that these cells in the simple villous disease may thus undergo different transformations, and finally become "malignant disease." Having arrived at this conception, the next step is the following conclusion:—"From what has been here advanced, I consider that there is no reason to separate cauliflower excrescence from the other forms of malignant disease affecting the os uteri, but that by insensible gradations they pass into each other, not only in a series of cases, but also in the same person."

After this "more philosophical" conclusion, arrived at by such "subtle argumentations," as his friend Dr. Barnes would call them, I fear it may be useless to make the commonplace observation that these transformations of these naughty, over-excited, and misdirected cells are never seen in nature; that a simple villous disease has never been known to become transformed into malignant disease; that malignant disease begins as such, continues as such,

and ends as such, with the existence of the individual. But if I am unable to follow Dr. Hicks in these "transformation scenes," I may perhaps be of service to him in a difficulty which he points out. If, he says, the term "cauliflower" be limited to the disease Sir Charles Clarke applied it to, "what are we to call the more canceroid forms which have a greater resemblance to cauliflower than the simple villous disease?" I would suggest the term "broccoli" might be appropriate. We then would have "Sir Charles Clarke's cauliflower excrescence," and "Dr. Braxton Hicks's broccoli excrescences"; and the early doings of those "over-excited," "misdirected cells" might be termed "Braxton Hicks's broccoli sprouts."

I am, Sir, yours, &c.,

December, 1873.

T. SNOW BECK, M.D.

## FRENCH TESTIMONY TO THE BENEFICIAL WORKING OF THE CONTAGIOUS DISEASES ACTS IN ENGLAND.

To the Editor of THE LANCET.

SIR,—I have just received from Paris the last number of "Les Annales d'Hygiène," containing an article entitled "Study of Prostitution and the Prevention of Venereal Diseases in England," by Dr. J. Jeaunel, Member of the Council of Health of the French Army. The frank comments of the author on recent legislation present several points of interest which I think should be brought under the notice of those who have investigated the subject and introduced the Contagious Diseases Acts in England. After reminding us of the condition of the streets and the public exhibition of prostitutes in our thoroughfares previously to 1864, Dr. Jeaunel goes on to say, "At the present day, thanks to the reclamation of writers on social science, and to the efforts of advocates of public hygiene, the English nation has resolutely set itself to work in repressing the public exhibition of prostitution, as well as considering the dangers of syphilis, and we Frenchmen should follow the example of England, if we are desirous of rendering perfect the administration of our sanitary institutions in relation to these evils." Dr. Jeaunel truly remarks "that Englishmen have not confined themselves to imitating French institutions, but have organised a uniform, centralised, and well-directed service, in vain asked for by French surgeons many years ago. England," continues Dr. Jeaunel, "far surpasses us in institutions destined to diminish the misery and repress the scandalous conduct caused by prostitution as well as preventing the propagation of venereal diseases. It is in England that we now have to seek for a model system. In France the bureaux of public morals, dispensaries, venereal hospitals—the institution of which we initiated,—after remaining without control, and in a state of the most complete anarchy, are still refractory to every kind of progress, and appear condemned to the most contemptuous indifference by the public authorities. In France military statistics furnish but an uncertain light to public hygiene. In England, on the contrary, prostitution is studied as a social disease with judicious perseverance, and under a Parliamentary Commission. We see these evils attacked with a well-balanced regard for public rights and liberty: we hear venereal diseases loudly denounced as a national plague spot and combated on a uniform system after full consideration; we notice the efficacy of these measures as evinced by special statistics, proposed, introduced, and collected by a Director-General and thoroughly sifted by a Parliamentary Commission; lastly, we find these statistics printed for the House of Commons in order to aid in the discussion of the proposed Acts. It is true that these Contagious Diseases Acts—the object of which is to promote morality and public health—are not put in force all over England; but even in this reserve we have reason to admire the prudence and practical spirit of the British nation. In England nothing relative to the national interest is decided upon in a hurry, no rash steps are hazarded, no one is taken by surprise. The unprotected districts are made a means of comparison with those towns and cities where the Contagious Diseases Acts have been in force, and furnish the best means of convincing the public of the necessity of all districts being subjected to general supervision."

This candid and unsolicited testimony of a competent

French author on the working of the Contagious Diseases Acts in England is one of the most satisfactory evidences yet offered of the benefit of our recent legislation on prostitution. Let us hope that the French Government will in future be guided by our experience. I am not without hope that Prussia as well as other Continental Governments, seeing the beneficial influence of our social regulations, will introduce such sanitary reforms as may tend ultimately to stamp out one of the greatest evils that can affect the health of the military and naval as well as that of the civil population in all large cities.

I am, Sir, yours truly,

Queen Anne-street, Jan. 1874.

W. ACTON.

## THE VALUE OF EUROPEAN LIFE IN INDIA.

To the Editor of THE LANCET.

SIR,—While acknowledging, with thanks, the courtesy and general fairness of your remarks on what I have written regarding the value of European life in India, I trust you will pardon me for failing to perceive that I have been defeated by those who are opposed to my views, or that any of the positions which I took up have yet been turned either by my Indian critics or by yourself.

I quite concur with you that so important a question cannot be settled by controversial contentions, inspired or otherwise. At the same time I am not prepared to concede that the mere figures collected by actuaries for purely commercial purposes can be accepted as a true and just estimate of the real risk to life which those who serve or sojourn in the East must accept as the condition of such service or dwelling.

There is probably no department of the service to which the application of the numerical method needs greater care and discrimination than that which treats of life and death, for in none are unexplained and undigested numerical results more likely to lead to erroneous conclusions, for reasons on which it is unnecessary to dwell, as they are well known to all medical writers who have paid attention to the subject.

A mere glance at the works of Gavarnet, Radické, and other authorities of our own time will show the danger of a misuse or misunderstanding of figures as exponents of facts in medicine and therapeutics. Morgagni's maxim, "*Perferendæ sunt observationes*," is nowhere more strictly applicable than to the question which is now the subject of a controversy, to which an acerbity of tone and temper has been imparted which is difficult to understand, except it be in a sordid sense.

If my views were really as dangerous and erroneous as they are represented to be, why do not those who are opposed to them let those who believe in them work them out to their logical results with the disastrous failure predicted?

The plan of treating with violence and vituperation new views which interfere with a more ancient order of things, particularly where the pocket is concerned, is an old old story.

I hope you will not deem me irreverent, for it is in no spirit of irreverence that I invite the attention of my critics to the example of Demetrius the silversmith, "who made silver shrines for Diana," that "brought no small gain to the craftsmen, whom he called together, with the workmen of like occupation, and said, 'Sirs, ye know that by this craft we have our wealth.'"

Equally great seems to be the Diana of our modern Ephesians, and equally full of wrath of the same unselfish sort are, manifestly, her present worshippers.

My object in addressing you is not, however, to carry into your columns, even were you disposed to permit it, a controversy which, as you rightly remark, cannot determine a scientific question. To ascertain the real value of European life in India has higher ends and bearings, far above and beyond its commercial relations, important as they are.

My immediate purpose is to explain that the number of European and East Indian *employés* engaged on the Indian railways in 1866, noted by me as 8001, is taken from the Parliamentary Return for 1866-67, page 12, paragraph 41. It refers, however, I find on re-examination, to the half-year ending on the 30th June, 1866. I have not been able to find the figures for the whole of that year.

The figures for Sindh, 2400, the East India Railway, 1943, and the Punjab and Delhi Railway, 221, in all 4572, make no distinction between Europeans *pur sang*, and Eurasians, or those of mixed birth. The deaths during the half-year were, in Sindh 1; on the East Indian line 29, of whom 1 was killed by an accident; and on the Punjab and Delhi line 1. From a note contained in the statement, it appears probable that the Punjab and Delhi servants were included in the Sindh statement, in which case they must have been counted twice over. The above-mentioned are only a portion of the Indian railways.

The figures throughout the Parliamentary Returns are evidently not quite exact, and there are some lacunæ which are nowhere supplied. I therefore attach no further value to them in the way of argument than as showing how much the climatic evils of residence and service in India must have diminished, when those who lead the most exposed of all lives in times of peace die in such diminished numbers, as compared with the figures given in the returns of the most eminent actuaries of either military or civil lives.

Some of the remarks in your last article satisfy me that you somewhat misapprehended my contention; and several of my statements, to which you object as not germane to the question, are in reality replies to specific assertions of some of my critics, and were not imported into the discussion by me.

For example, I adduced the testimony afforded by the Indian members of the Athenæum in no statistical sense, but to rebut the assertion that long-continued service in India very rarely caused decay of body and mind—*et sic de cæteris*.

I am now engaged in collecting new figures with a view of throwing light upon the question in its purely scientific aspect, apart from all commercial considerations. These I shall submit, when the opportunity is afforded to me, to the Statistical Society, and they will, I have every reason to believe, afford proof that my views on the subject are in the main correct and well founded.

I have nowhere said, and I do not believe, that the risks to life in tropical are not greater than they are in temperate climates; but I do contend that life, to the strong, healthy, and temperate, is in ordinary circumstances as safe in the one as in the other.

I am, Sir, your obedient servant,

Jan. 13th, 1874.

F. J. MOVAT, M.D.

## THE TEACHING AND PRACTICE OF PSYCHOLOGICAL MEDICINE.

To the Editor of THE LANCET.

SIR,—I have read with astonishment and regret Prof. Laycock's article on "The Teaching and Practice of Psychological Medicine as Influenced by Classifications of Insanity," published in THE LANCET of the 10th inst. I would never have lifted pen in answer to the remarks Prof. Laycock makes about me, my work, and acquirements; but when I find my expressions of respect as a student for the work and teaching of my master used as a medium for an attack upon his memory, it behoves me to speak. Anyone who has read the article must have noticed the antithetical silence which follows Professor Laycock's remarks on the qualities of Cullen. After quoting my opinion that Skae must be regarded as the Cullen of psychiatric medicine, he dissents, and proceeds to compare the systems of classification of mental diseases of the two authors in this wise:—"Cullen was a man with a rare combination of mental powers. A learned man, a keen observer, an acute thinker"; evidently implying that Skae was deficient in these qualities. It is not for me to speak of Skae's genius; all who knew him acknowledged it, and there is not one of his many friends who will not resent the implied insult on his memory. Let me remind Professor Laycock and the medical public that the classification of insanity on which I based the opinion was published in 1863, and that David Skae died in 1873. Why did not the professor attack the theory in the lifetime of its author? Why did he barely wait till the grass had grown over his grave ere he ventured to undervalue and depreciate the work? It must be admitted,