woman showed symptoms of a perforation of the stomach, and on being operated on she was found to have a large retrogastric collection of pus. The pancreas was gangrened, and a fistula, which persisted a long time, showed that the pancreas was the primary seat of the disorder. In the second case the patient, who had lost 50 kilos in weight, was supposed to be suffering from cancer of the stomach. It was found, however, that an enormous mass of adhesions had formed as a result of suppuration of the pancreas. Drainage was made and the patient is now quite cured.

Correspondence.

The Operative Treatment of Strabismus.

An answer to Dr. Geo. M. Gould's paper in THE JOURNAL of the American Medical Association, November 1, page 1106.

PITTSBURG, PA., Dec. 3, 1902.

To the Editor:—If in the following I shall attempt to refute some of Dr. Gould's arguments against the operative treatment of heterophoria and strabismus, I am fully aware of the difficulty of the task. With his overwhelming figures, the weight of his well-earned reputation, and the admirable spirit of sincerity which pervades every phase of his reasoning, Dr. Gould's position is formidable enough to intimidate any "deus minoris gentis."

However, I believe that in the seemingly impregnable wall with which the gallant defender of conservatism has surrounded himself, there are several weak spots; some of the fundamental stones are loosened already by the able efforts of others, and here and there I even find that the builder has altogether neglected to support his foundation with concrete material. In fact, he even furnishes us with the weapons to upset his structure. To speak less metaphorically, and to go right "in medias res," the anatomic relation of Bonnet's capsule to the tendons and its bearing on the conditions as created by tenotomy have been completely ignored in Dr. Gould's chain of evidence. To anyone familiar with the fact that Bonnet's capsule is reflected on the tendons where they pierce the capsule and becomes continuous with the fasciæ of the muscles (lateral invagination) the effect of severing the tendon must be self-evident: The muscle will exert a certain amount of traction on the capsule; if we now sever the connective tissue between the tendon and the capsule, a procedure which Dr. Gould sees fit to deride as "post-graduate," the muscle will glide back farther into the orbit. For illustration we assume that the internal rectus of the right eye has been severed. Abduction will immediately take place, caused chiefly by the action of the external rectus. The gap thus formed will be the result of two factors: tonicity of the rectus internus and abduction through the rectus externus. While I readily admit that this abduction is partly neutralized by the active effort of the patient to lessen the traction of the wound, I can not see how reinsertion at the former line of insertion can possibly take place, since there remains a gap which no amount of voluntary or involuntary compensation on the part of the other muscles will completely overcome.

This is one theoretical "scrap of evidence" according to which it is utterly impossible that the tendon should become reattached to its former line of insertion, provided, however, that we have severed it completely. All surgical analogy helps to strengthen this assertion. There is not one tendon in the human body which, severed and left alone, will reinsert at its former point. If restitution of the function of the muscle actually takes place it can not be due to any other agency than the one pointed out by Dr. Gould himself, namely, regrowth of tissue. But who will deny that in the latter case—filling of the gap through new tissue—an actual lengthening of the muscle has taken place?

The mere admittance of this possibility renders irrelevant the argument that a tendon will not grow on an unabraded, uninjured surface. However correct this assertion may be, it does not come into consideration in our case for the above reason that the gap formed by the operation will be bridged over by newly-formed connective tissue which finally will unite the two surfaces, and, by its interposition, will add to the total length

of the muscle. If it happens occasionally that this bridge does not develop, it does not necessarily imply that no insertion whatever will take place. What Dr. Gould pronounces the invariable rule, to-wit, that the surface behind the stump is unabraded and uninjured, seems to me, on the contrary, a very rare exception. The technic of the operation makes such abrasions more than probable, and if there were in my mind any doubt left, my practical experience of about two score of repeated tenotomies would remove even the slightest shadow of such a doubt. The rule is that in repeated tenotomy we shall find the line of insertion moved back. So typical have I found this condition that in a second tenotomy on the same muscle I invariably make the conjunctival incision from 2 to 4 mm. farther back than in a first tenotomy.

Another equally good *modus restitutionis* is that in which filling of the gap through newly-formed tissue is combined with insertion behind the former line of attachment. In such cases we find a broad surface instead of a line of attachment, a condition which clearly indicates that the surface behind the stump was abraded during the first tenotomy. But leaving aside even these practical "scraps of evidence," there is another factor which has not been considered by Dr. Gould. Assuming that no reattachment has taken place at all (for instance, immediately after the operation), has the severed muscle lost all of its power over the eyeball? By no means. The traction on Bonnet's capsule still remains and will prevent the antagonist from exerting his full force. If this were not the case, how could we explain the formidable amount of adduction that remains after complete severance of an internus? Merely by the action of the auxiliary adductors? If so, why will the severing of the belly of the muscle produce such a marked decrease in adduction, or abduction, as the case may be?

So far, all of our arguments have been directed against the theory of reinsertion at the old place, together with the establishment of a *status quo antea*, on which theory Dr. Gould bases his assertion: "Hence, in a few months or a year all the old troubles return after needless surgical intervention."

For obvious reasons we shall not condemn all of Dr. Gould's conclusions because one of his premises was incorrect. Thus we perfectly agree with him as regards the one feature that tenotomy can do little or nothing toward removal of the primary causes of the large majority of strabismus cases. We shall be able to correct slight degrees of astigmatism by relieving some of the lateral pressure on the globe; we can remove part of the strain on the antagonist and thus enable the latter to respond more promptly to impulses; we bring the optic axis in a more favorable position for macular vision, where macular vision is still possible, but, unless conditions are especially favorable, some of the old troubles (not all of them) will return, not because the tendon will reinsert at the old line of attachment, but because the primary factors can not be eliminated in all cases.

All of these objections can not have any bearing on the important rôle which tenotomy, eventually combined with advancement, plays as a preparatory measure for the treatment of some of the above troubles. Take, for instance, all those cases of heterophoria and strabismus in which the primary factor is hypermetropia, with its excess of accommodative impulses, which implies at the same time an excess of convergence. An immense expenditure of nervous energy, time and money may be saved to the patient if we remove the deficit in abduction by a suitable operation on the hypertrophied internus. The correction of the refractive errors will be simplified, the time of the treatment shortened and the appearance of the patient permanently improved; in other words, surgical intervention will smooth the path for intelligent non-surgical treatment.

Nevertheless, Dr. Gould assures us that with the exception of one (!) case he never saw the results of an operation in heterophoria or strabismus of which he should be proud to say he did it! Considering the sincerity, enthusiasm and trustworthiness of the distinguished Philadelphia author, there remains nothing for us but to express our regret that he should be the victim of what I am inclined to take for an extraordinary coincidence. With Dr. Risley I offer as an ex-

planation for this strange phenomenon that the scores of successful operations that he must needs have performed himself never returned to his view, simply because they were cured or perfectly satisfied with the results obtained.

Having thus established the fallacy of some of Dr. Gould's premises, I proceed to examine his arguments in favor of non-operative treatment. In this respect, it seems to me, Dr. Gould has committed the error of "reckoning without his host." While this would not necessarily imply the scientific untenability of his position, it will erect an unsurmountable barrier to the indiscriminate acceptance of his therapeutic suggestions. "Fiat principium, pereat mundus," is a very dangerous maxim. A therapeutic measure, be it ever so correct, must be feasible before it can be universally adopted. Applied to our case, we must always bear in mind that we have often to contend with factors which are absolutely beyond our control. Of these factors I mention here the intellectual standard of the vast majority of our patients, their ages, their individual wishes, the time required and inconvenience caused by prolonged treatment, and last, but not least, the financial circumstances of the average patient. Once more I repeat, that all these objections are not directed against the correctness of the assertion that the non-surgical treatment of strabismus is the more accurate, scientific and (occasionally) the more harmless mode of therapeutics, but against its practical feasibility. We must reckon with circumstances as they are and not as they might be, which does not, at the same time, exclude our duty to work toward a state of conditions, as they should, or will be, in the far, far-away future.

Until the arrival of that millenium state, when the public will realize the necessity of having their babies' eyes examined by the expert; until the latter, including those who had the misfortune of an European education, with all its "ophthalmologic error and therapeutic sin," will be able to use retinoscopy to such a degree of accuracy that they may determine the refractive errors of the fetus in utero; until the dawning of that Bellamic era of prosperity which will enable us to engage a couple of special nurses for our little tots—not until then will the conservative treatment of strabismus become uniformly adopted.

The sovereign indifference which Dr. Gould exhibits with reference to the cosmetic possibilities of tenotomy is surprising, to say the least. It has been my experience, which I believe will be verified by the majority of my esteemed confrères, that 95 out of every 100 patients who consulted me for correction of an existing squint, did so in the expectation of having their unsightly deformity removed, and not for the relief of asthenopia. From the standpoint of the average patient the importance of correction of his refractive errors dwindles into insignificance as compared with that of his deformity. As a matter of fact, can we hold him responsible for this indifference to his ametropia or astigmatism, if he suffers no inconvenience whatever from the latter defects, if time and again we meet with patients who had been unconscious of their ametropia up to the very day that they consulted us for the purpose of having their eyes "straightened"? Besides, Dr. Gould's allusion to the *laissez aller* policy is not very happily illustrated by the example of "the leg that we do not strap out of the way"; a man may see with one eye, and see pretty well, but I have yet to see the first man who walked on one leg.

In the introductory lines of this paper I have taken the liberty of pointing to the fact that Dr. Gould has furnished us with the tools for upsetting his structure. "Every good oculist," he says, "has hundreds of patients who have no deformity and who have binocular vision—so long as their spectacles are in place. Raise the lenses and instantly there is strabismus! That is a demonstration convincing to everyone." Indeed it is! A more striking illustration could not have been found in the whole material of evidence against the failures of conservatism. Just imagine the happiness naturally arising from the consciousness that the mere raising of the lenses instantly restores the strabismus! The happiness of such a patient can only be surpassed by that of another, to whom we have just brought the glad tidings that it will probably take ten years to cure his strabismus.

In the foregoing lines we believe to have amply demonstrated to what fallacies the rigid adherence to any one system may lead. It has led one of America's foremost oculists to denounce surgery as the "despair of medicine"! A statement like the above, coming from such an authoritative source, involves an imminent danger to both the profession and the public. It will be taken up by every quack in the country and serve as a ready reference in their pompous advertisements, to cure "crossed eyes, cataracts, films and scums" without the use of the knife.

It is not within the scope of this paper, nor within the ability of its writer, to point out all the tortuosities, grades and obstacles in the path which alone can, will and must lead to salvation. I will, therefore, restrict myself to the summary statement that the solution of the problem lies, in my opinion, in the happy blending of conservatism with radicalism. The promiscuous adoption of the one is as dangerous as the wholesale condemnation of the other.

In conclusion, I deem it my duty to express surprise at the low opinion that Dr. Gould seems to entertain with regard to European oculists. The statement that "there is probably not an 'ophthalmic surgeon' in all Europe who can use this method [retinoscopy] with accuracy"—not to dwell on the difficulty of furnishing the proof for such an assertion—bears every mark of improbability in consideration that this method is practiced by several of Europe's best oculists, almost in total preference to any other method of refracting children.

Another criticism of the same order is equally unjustified. Quoting a paragraph in Dr. Fuchs' text-book, Dr. Gould says: "In anisometropia we give the same glasses for both eyes, or correct only one eye and place a plane glass before the other." Commenting on this paragraph, Dr. Gould remarks: "I set this sentence out in capital letters as displaying as much ophthalmologic error and therapeutic sin as could easily be gathered into so many words."

In the first place, the quotation is not correct. The text before me (page 659, translation by Dr. Duane, 1896), reads: "We therefore prefer in anisometropia either to give the same glasses for both eyes, or to correct only one eye and place a plane glass before the other." In connection with the preceding and following sentences and duly considering the word "therefore," the sentence gains an altogether different aspect. After stating that the obvious course would seem to be to correct the anisometropia by ordering different glasses for the two eyes, Dr. Fuchs remarks that this measure proves impracticable in most cases; mentions the symptoms arising from anisometropia of a high degree, gives his explanation, which I believe to be theoretically and practically irreproachable, and finally, by inserting the above quoted sentence, "we therefore prefer," etc., calls our attention to the method to be adopted for those cases in which we have failed to give satisfaction by prescription of different glasses, referring chiefly to glasses with a different index (+ or —), as can be readily seen from the connection with another preceding sentence.

A. S. SIGMANN, M.D.

COMMENT BY DR. GOULD.

A more careful reading of my paper would have resulted in an avoidance of a number of errors in Dr. Sigmann's courteous criticism. I will not take up valuable space to detail these, except in one illustrative instance: In the body of my article it is expressly stated that the excerpt wherein occurred the quotation from Fuchs was from an article written ten years ago. I do not garble quotations, and, when written, the quotation was verbatim from the last edition. That four years later Fuchs had progressed to the position illustrated by Dr. Sigmann's quotation is encouraging. What will the progress be in the next four years?

As to the misuse of truth by quacks, that does not concern me. To refuse to accept and state truth because of such dangers is to accept the ethics of the quack. As to European oculists, they are the peers of any as regards pathology and inflammatory and surgical diseases. But proper refraction prevents both classes of diseases. Of proper refraction European oculists know almost absolutely nothing. In this their work is the most absurd. A patient of mine consulted the

most famous "ophthalmic surgeon" of Germany last summer, and for an enormous astigmatism (of which the surgeon was utterly incurious) he ordered massage of the eyes by the patient. For blepharitis, due to refractive error, another famous man told another patient to squeeze out the secretion of the Meibomian glands with her fingers. A great French oculist ordered simple isometric spherical lenses for astigmatic anisometropia, and expressed indignant surprise at the idea that he, the oculist, should have kept any record of the lenses he had ordered. I could enumerate many such laughable instances. But such things are common enough in America also, and to gibe at the refraction of the *fetus in utero* does not make them less common.

It is true that extremes are dangerous and that the middle-of-the-road is usually the safest and truest. Not always, however! A compromise, if for the sake of compromise, may be a worse error than either extreme. The time is not far past when one who should have proposed to cure strabismus (and surely heterophoria!) by any other method than operation would have been looked on as crazy.

GEORGE M. GOULD, M.D.

Midshipman Aikin and Vivisection.

PHILADELPHIA, Dec. 5, 1902.

HON. JACOB H. GALLINGER,

Chairman of the Senate Committee on the District of Columbia, Washington, D. C.

My Dear Sir: As you have repeatedly introduced bills into the Senate for the purpose nominally of regulating experiments on animals in the District of Columbia, which bills, however, if they had become laws, would in fact have prohibited many, if not all of them, I deem it my duty to call your attention to the case of Midshipman Aikin of the United States Naval Academy, who was recently injured in a football game. My reason for doing so is to show you by a single concrete example that knowledge gained by animal experimentation is an immense boon to humanity and that, therefore, such experiments should be heartily encouraged.

The facts of Mr. Aikin's case are as follows: When I first saw him, three days after the accident, I found that he had been unconscious for a half hour after the accident and ever since then had complained bitterly of headache, which he located always in the forehead. Mentally he was very dull, though not comatose. His pulse was slowed down to 52 instead of being 72, the normal. Soon after the accident he began to develop convulsions, first in the right leg, afterward in the right arm also, the right arm being finally the chief seat of the convulsions. When they were very severe they involved the left side also. The face was never involved. In six hours and a half after I first saw him he had 24 of these attacks, all limited to the right arm. They were not attended by any loss of consciousness. They exhausted him very greatly, especially when they were excessively severe. Several times it was necessary to give him chloroform.

There was no fracture of the skull. The only physical evidence of any injury was a very slight bruise at the outer end of the left eyebrow.

Had I seen this case before 1885 I would have been unable to explain why the spasms were chiefly manifested in the right arm, and from the evidence of the slow pulse, the headache, the stupor, the bruise in the left temple, etc., I would have been justified in inferring that probably the front part of the brain was injured at the site of the bruise. Had I opened his skull at that point I would have found a perfectly normal brain and have missed the clot. The young man, therefore, would have died, whether his skull had been opened or not.

In 1902 observe the difference. As a result of the knowledge derived from experiments on animals which have located precisely the center for motion of the right arm on the left side of the brain near the top and a little in front of a vertical line drawn through the ear, and disregarding entirely the site of the headache and the bruise, I reached the conclusion that there had been a rupture of a blood vessel within the head, which had poured out a quantity of blood, and that the

situation of the clot should correspond to the "arm center." The location of this arm center was far away (about three inches) from the location of the bruise, and its position was fixed absolutely as a result of experiments on animals, confirmed later by many operations on human beings and also by postmortems.

As soon as the skull was opened at this point the clot was found, its thickest point being exactly over the arm center and nine tablespoonfuls of blood were removed, with the result that the patient's life has been saved. The blood had first been poured out over the "leg center," which is located a little higher up than that for the arm. This explained the early spasms in the right leg. The clot did not extend, however, further down than the arm center. This explained why the face was never convulsed, for the "face center" lies just below that for the arm.

I think if you were to ask the parents of this young man how many animals they would be willing to have sacrificed in order that their son might have his life saved, there would be no doubt of the answer. Indeed, had it been your own son, I can not doubt the answer. But this is only one of hundreds of cases in which a similar exact localization has been made by many surgeons, both in Europe and America. Yet, by a curious coincidence, in the very same issue of a Baltimore newspaper containing a reporter's account of the successful operation on Midshipman Aikin there is a letter signed by an active antivivisectionist agitator, which, among other misstatements and misrepresentations usually found in such publications, asserts that "brain surgery is disregarded."

If the laws which you and your friends advocate were in force, the conditions for scientific investigation in medicine in this country would be quite as deplorable as those in England. For example, when Lord Lister, who has revolutionized modern surgery, largely as a result of such experiments, wished to discover possibly some still better way of operating by further experiments, he was obliged to go to Toulouse to carry them out, as the vexatious restrictions of the law in England practically made it impossible for him to continue there these pre-eminently humane experiments.

Again, when Sir T. Lauder Brunton in London started a series of experiments on animals to discover an antidote for the cobra and other snake poisons of India, where every year 20,000 human lives are sacrificed by snake bites, these beneficent researches were stopped by the stringent British laws to protect animals. Meanwhile half a million of human beings have hopelessly perished.

Who, I may ask, is the more humane; he who, doubtless with the best and sincerest motives of love for dumb beasts, would prohibit experiments on animals and thereby prevent the acquisition of such knowledge and so compel surgeons to stand with folded arms and see innumerable lives thus needlessly sacrificed; or he who, by properly instituted experiments, would discover such new truths and apply them to the service of humanity?

The antivivisectionists have frequently denied that surgeons have learned anything from such experiments. I presume that I may be considered a competent witness as to the source, at least, of my own knowledge, and I state with the greatest positiveness that without the knowledge derived from experiments on animals which have demonstrated the facts of cerebral localization, I should never have been able to locate the clot in Mr. Aikin's head and to remove it, nor would I have been able in the last fifteen years to locate numerous tumors and other brain troubles and relieve many of them. What is true of myself is equally true of other surgeons.

In view, therefore, of the evident and positive benefit of such experiments, I trust that you will be willing to desist from further efforts at such repressive and, as I regard it, most inhumane and cruel legislation.

As this matter is of vital importance to the well-being of the entire community, I shall take the liberty of giving this letter to the press as soon as you have received it.

Yours very respectfully,
W. W. KEEN,
Professor of Surgery, Jefferson Medical College.