the red than is the center, and this is one additional cause of the difference. (See Fig. 5.)

More on this subject I have not time to say on this occasion, but it will be seen that the extinction of light for the center and the outside of the eye differs on account of this.

I must take you to a theory of color vision which, though it may not be explanatory of everything, at all events explains most phenomena—that is, the Young-Helmholtz theory. The idea embodied in it is that we have three sensations stimulated in the eye, and that these three sensations give an impression of a red, a green and a violet. These three colors I have said can be mixed to match any other color, or in other words, the three sensations are excited in different degrees, in order to produce the sensation of the intermediate spectrum colors, and those of nature as well.

The diagram, Fig. 4, shows the three sensations as derived from color equations made by Koenig. It will be seen that there are three complete color sensations, all of which are present in the normal eye. I would ask you to note that at each end of the spectrum only one sensation is present, viz., at the red end of the spectrum, the red sensation, and at the violet end the violet.

This is a matter of some importance, as we shall now see.

It will be recollected that in making the extinctions, the D light of the spectrum was made equal to one amyl-acetate lamp, and the other rays had the relative luminosity to it which they had in the spectrum before they were extinguished. The luminosity curve of the spectrum is shown in Fig. 5.

Suppose we make all the luminosities of the different rays equal to one, A L, we should not get the same extinction value, as shown in the continuous lines in Fig. 3. The violet would have to be much more reduced, but by multiplying the extinction by the luminosity we should get the curve of reduction for equal luminosities, and we get the dotted curves in Fig. 3.

It will be seen that it is the violet under such circumstances that would be the last to be extinguished, and that all the rays at the violet end of the spectrum would be extinguished simultaneously, as would also those at the extreme red. This looks a confirmation of the Young-Helmholtz theory which I have briefly explained, for we cannot imagine that it can be anything but a single sensation which fails to be excited.

The violet is extinguished when it is $\frac{15}{10,000,000}$ A L,

that is, a screen placed 817 feet away and illuminated by an A L violet lamp would be invisible. The blue-

green (F) light when it is $\frac{17}{10 \text{ millionths}}$ or 770 feet away.

The green (E) light $\frac{35}{10 \text{ millionths}}$ or 550 feet away.

The orange (D) light is extinguished as before at $\frac{350}{10 \text{ millionths}}$

or 180 feet away, while the red (C) light has only to be reduced to $\frac{2,220}{10 \text{ millionths}}$ or an A L

lamp radiating C light would have to be placed only 67 feet away, while the radiation for an A L of the color of the B light of the spectrum would have to be $\frac{2,600}{10 \text{ millionths}}$

diminished to but $\frac{10 \text{ millionths}}$ or the screen would have to be placed 60 feet away.

It is therefore apparent that with equal luminosities the violet requires about 175 times more reduction to extinguish it than does the red, and probably about 25 times more than the green.

This being so, I think it will be pretty apparent that, at all events from the extreme violet to the Fraunhofer line, D, of the spectrum, the extinction is really the extinction of the violet sensation, a varying amount of which is excited by the different colors. If then we take the reciprocals of the numbers which give extinction of the spectrum, we ought to get the curve of the violet sensation on the Young-Helmholtz theory. For if one violet sensation has to be reduced to a certain degree before it is unperceived, and another has to be reduced to half that amount, it is evident that the violet sensation must be double in one case to what it is in the other; that is, the degrees of stimulation are expressed by the reciprocal of the reduction.

Such a curve is shown in Fig. 5 (in which also are drawn the curves of luminosity of the spectrum when viewed with the center of the retina and outside the yellow spot). And it will be noticed that it is a mountain which reaches its maximum about E. Remember that the height of the curve signifies the amount of stimulation given to the violet sensory apparatus by the particular ray indicated in the scale beneath.

Turning once more to Fig. 3, it will be noticed that if any one or two of the three sensations are absent, the persons so affected are what is called color blind. Thus if the red sensation is absent, they are red-blind; if the green, then green-blind; if the violet, then violet-blind. If both red and green sensations are absent, then the person would see every color, including white, as violet. The results of the measurement of the luminosity of the spectrum by persons who have this last kind of monochromatic vision should be that they give a curve exactly or at all events very approximately of the same form as the curve given by the reciprocals of the extinction curve obtained by the normal eye, as the violet sensation is that which is last stimulated.

It has been my good fortune to examine two such persons, and I find that this reasoning is correct, the two coinciding when the curves for the center of the retina are employed.

Further, I examined a case of violet blindness, and measured the luminosity of the spectrum as apparent to him. Now if the Young-Helmholtz theory be correct, then in his case the violet sensation ought to be absent, and the difference between his luminosity and that of the normal eye ought to give the same

curve as that of the violet sensation. This was found to be the case.

Again, the reciprocal of the extinction curves of the red-blind and green-blind ought to be the same as those of the normal eye, for the violet sensation must be present with them also. This was found to be so. We have still one more proof that the last sensation to disappear is the violet.

If we reduce the intensity of the spectrum till the green and red disappear to a normal eye, and measure the luminosity of the spectrum in this condition, we shall find that it also coincides with the persistency curve. On the screen we have a brilliant spectrum, but by closing the slit admitting the light and placing the rotating sectors in the spectrum and nearly closing the apertures, we can reduce it in intensity to any de-

FIG. 4.—COLOR SENSATIONS.

gree we like. The whole spectrum is now of one color and indistinguishable in hue from a faint white patch thrown above it. If the luminosity of this colorless spectrum be measured, we shall get the result stated. The curve obtained in this way is in reality identical with the other curves. By these four methods then we arrive at the conclusion that the last color to be extinguished is the sensation which when strong gives the sensation of violet, but which when feeble gives a blue gray sensation.

One final experiment I may show you. It has been remarked that moonlight passing through painted glass windows is colorless on the gray stone floor of a cathedral or church.

We can imitate the painted glass and moonlight. Here is a diaper pattern of different colored glasses, and by means of the electric light lantern we throw its colored pattern on the screen. The strength of moonlight being known, we can reduce the intensity of the light of the lamp till it is of the same value. When this is done it will be seen that the pattern remains, but is now colorless, showing that the recorded observations are correct, and I think you are now in a position to account for the disappearance of the color.

I have now carried you through a series of experiments which are difficult to carry out perfectly before an audience, but at any rate I think you will have seen enough to show you that the first sensation of light is what answers to the violet sensation when it is strong enough to give the sensation of color. The other sensations seem to be engrafted on this one sensation, but in what manner it is somewhat difficult to imagine. Whether the primitive sensation of light was this and the others evolved, of course we cannot know. It ap-

FIG. 5.

pears probable that even in insect life this violet sensation is predominant, but at all events existent. Insects whose food is to be found in flowers seek it in the gloaming, when they are comparatively safe from attack. Prof. Huxley states that the greater number of wild flowers are certainly not red, but more or less of a blue color. This means that the insect eye has to distinguish these flowers at dusk from the surrounding leaves, which are then of a dismal gray; a blue flower would be visible to us while a red flower would be as black as night. That the insects single out these flowers seems to show that they participate in the same order of visual sensations. I venture to think, without

adopting it in its entirety, that these results at all events give an additional probability as to the general correctness of the Young-Helmholtz theory of color vision. Where the seat of color sensation may be is not the point, it is only the question as to what the color sensations make us feel which the physicist has to deal with. The simpler the theory, the more likely is it to be the true one, and certainly the Young-Helmholtz theory has the advantage over others of simplicity.

THE ORIGIN OF THE SIGMOID FLEXURE AND THE APPENDICULA VERMIFORMIS OF THE HUMAN COLON.

THE sigmoid flexure of the human colon is the necessary result of the change from quadruped to biped, from the horizontal to the vertical position.

The colon having formerly occupied the position of an easy curve, its greatest weight rather tending from the anus downward toward the umbilicus, thus rather straightening that part of the colon which now contains the sigmoid flexure. The gradual change from the horizontal to the vertical position changed all this radically, formed of the colon an arch, whose columns bent and kinked from sheer force of the weight they were compelled to support.

The appendicula vermiformis, or worm-like appendage, resulted likewise from the same cause, namely, the upright position. Through means of the weight of the feces or excreta causing the extension of the colon beyond and below the ileo-cæcal valve, from the difficulty of expelling the accumulations in the same, it finally became constipated, peristaltic action becoming more and more difficult, expansion and contraction of the muscles ceased partly, and finally altogether, with it also a diminishing of the circulation of blood must have taken place. What other result could there be than that this part of the colon became useless and consequently doomed to gradual reduction and final disappearance?

Lady Lake, Fla.

CHAS. F. SCHNEIDER.

WATER CURES FROM CLINICAL AND EXPERIMENTAL POINTS OF VIEW.*

By Prof. WILHELM WINTERNITZ.

IT is now thirty years since I began to investigate the action of water in its various temperatures and aggregate forms, and to observe how the thermic and mechanical influences manifest themselves in the healthy and in the sick. The first results aroused in me a lively desire to do away with hydrotherapy as a specialty and to make it the common property of society in so far as it merited general adoption. But after hydrotherapy had received an apparent impetus during the sixties, and especially after the question of the regulation of warmth had come to stand in the zenith of physiological interest, my hopes began to sink, and to-day, in common with other distinguished clinicians, I must declare that the proselytizing power of my own convictions has not proved equal to the achievement of this ideal. Nevertheless, the dissemination of theoretical knowledge has been greatly promoted; hundreds of my direct and indirect pupils are to-day conducting hydropathic institutes, and hundreds of thousands receive treatment for their maladies in accordance with my principles.

A few years ago I believed that the principles of my method were firmly established, and I shall endeavor briefly to sketch its main features, and to show that it is possible to speak in a rational manner of thermic and mechanical effects upon the organs and their functions. I cannot, of course, go into details here, but will simply assert that we are thus enabled to elevate, depress or modify the supply of nerve force. I merely remind you that we are in position to recall to normal perception anæsthetic portions of the body, and of again producing anæsthesia through the application of cold. We are able to exert an action not alone on the peripheral nerves, but also through the latter, by reflex action, on the central organ, and even on the gray cortex of the brain.

I have not hesitated to learn from laymen. In Grunberg, for example, I saw a young, robust patient suffering from severe circular insanity, characterized by great depression, with feelings of anxiety, notions of persecution, self-reproaches, abstinence, impurity, etc., which alternated with symptoms of most pronounced excitation. Patient had just experienced a violent attack of mania, during which four attendants placed him gently upon a couch prepared with moist padding (water bed). The first shock of the cold sheet resulted in giving the patient a few moments' rest, and the interval was utilized by the attendants in wrapping up the patient tightly, closing the moist ends over his head, and thus forming a kind of therapeutic strait jacket. After five minutes the patient began struggling to remove the sheet, whereupon he was placed on another couch similarly prepared, and the interval of rest produced reflexively proved of longer duration. Thus from morning until evening sixteen "wrappings" were performed, the attack of mania then vanishing completely. We physicians would perhaps have hesitated to subject to such a thermic process a subject suffering from accelerated circulation and violent congestions to the head. The result was an extraordinary surprise to me, and subsequently I succeeded by following this method in treating satisfactorily very severe circular forms, with the severest stages of excitation and koprolalia. This case proves that through the periphery we can exert reflex action upon the central organ in this manner. The nervous condition depends on the impulses which are conducted to the center from the periphery. When this stimulus gradually fades away, and the peripheral vessels, through repeated stimulation, are brought into a state, not of parietic, but rather of tonic enlargement (which, under the co-operation of the inhibitory nerves, may become active), the modified distribution of the blood will have a powerful influence. It goes without saying that, aside from the influence upon the nervous forces, that exerted upon the circulation must also be taken into account, and we hope that psychiatric specialists will make experiments in this very form of nervous excitement, on a greater scale than hitherto. It is an

* From the *Internat. Klin. Rundschau*.

established fact that all forms of excitation are influenced by this procedure, especially sleeplessness, which is benefited by application of cold packings.

Much more important is the action on respiration and circulation through thermic and mechanical stimuli. Throwing but a few drops of water into a person's face, deep inspiration results and simultaneously a modification in the rhythm of respiration. Experiments which I have made with the respiratory apparatus have taught me that we are able, through thermic influence applied to the surface of the body, to produce a very powerful action on the exchange of gases—not alone, indeed, on the volume of the expired and inspired air, but also on the absorption of oxygen and the formation of carbonic acid.

But the influence which we can exert upon the circulation by means of thermic and mechanical stimuli applied to the surface of the body is far more striking. We can act at pleasure, not only upon the heart, but upon the peripheral vessels as well. We know that by irritating the source of the vagus, the frequency of the heart's action is modified. I have been able to show, with the aid of sphygmographic and sphygmomanometric observations, that cold as well as heat effects a very transient acceleration of the heart's action, which then recedes, and which we can again through definite processes accentuate further and cause to reach a point of considerable elevation. The most palpable exhibition of this is in exophthalmic goiter. I have seen such a case in which, partly from thermic and partly from mechanical stimuli, the pulse, accelerated to 200, was depressed to 40 beats, in a few minutes. This I observed for the first time in the Sander Institute at Stockholm, where a similar result was obtained by means of strokes applied to the back. There are indeed manifold variations of methods with which to obtain the same effects—even through simple reflex irritation. Thus, for example, the heart's action may be retarded and the single contractions made more vigorous, by flowing foot baths, rubber tubes applied to the neck and back and by application of cold compresses to the heart region. By the application of thermic irritation, applied to the peripheral vessels and vaso-motors, we can effect contraction, relaxation with loss of tonic tension or with maintenance of the tone. It makes a great difference whether the vessels enlarge with loss or preservation of tone, since it is a fact established by various physiologists that the loss of vital power is much greater when the elasticity of the tissues and the tension of the vessels are relaxed; circulation is more favorably influenced when the vessels enlarge while preserving their tone. This we experience in fever, in which, with loss of tone, passive stases often supervene.

I would further mention the active hyperæmia which may be artificially produced by thermic irritation. Our efforts are often crowned with great success when we succeed in inducing the flow of blood to certain parts of the body. Let us imagine an acute arthritis rheumatica and neuralgia; some thermic disaster occurs in the form of a moist, continued draught of air, provoking reflexly in the nerve disturbances of circulation, consisting of reflexive contractions of the vessels of the organ in question. In such a diseased organ the degenerative products—in the muscle they consist chiefly of acids—accumulate, through which, if less alkaline blood flows by, the nerves of sensibility are irritated. If now, through massage, electricity or thermic stimulus, a fluxion is produced, the vessels are caused to enlarge actively, more alkaline blood is introduced, and we succeed in overcoming the muscular rheumatism soon after its supervention, or in lessening the pain of chronic conditions. In the nerve a similar process is exhibited. A tetanized nerve yields an acid reaction on cross section; if we now succeed in bringing alkaline blood to the inflamed nerve, thus neutralizing the acid, the entire process can be terminated. This truth holds good in countless cases.

Far more important is the *passive hyperæmia* which is at the root of many processes, and which may be affected by thermic and mechanical agents. I cite an example. A woman once came to me, who for two years had been suffering from continuous metrorrhagia, and in whom the gynecologists could diagnose nothing more than a hyperæmia and swelling of the uterus, and a cyanotically colored *os uteri*, obtaining no results from all manner of injections. I regarded the hemorrhage as *passive*, since the clinical symptoms seemed to suggest this view. I recalled a patient whom I had seen at the clinic of the famous Traube, complaining of hæmoptoeic symptoms. To our great astonishment, Traube advised him to make foot tours in the mountains and to take a great deal of general exercise. From the dark sputum and the very tense pulse, Traube assumed the existence of a passive hyperæmia, and when I met this man subsequently in a party of mountain tourists in the Tyrol, I was enabled to convince myself personally of the good results from this therapy. In the case before me I resorted to vigorous methods calculated to elevate general tone, strengthen the pulse and induce a flow of blood to the periphery. The patient was given shower baths and flowing foot baths; she underwent thermic and mechanical treatment applied to the abdominal and lumbar regions and producing shock, whereupon the bleeding ceased after a few weeks. Likewise, in similar cases of persistent nose-bleed due to passive hyperæmia, I could usually obtain benefit by stimulating the circulation.

A very important question, which permits us to include within the province of thermic and mechanical actions an entire series of nutritive disturbances, is the *influence upon secretion and excretion*. Resorting to thermic and mechanical methods, we can affect the discharge of water through the skin, the kidneys and the lungs. Weirich demonstrated that a few minutes' friction applied to the surface of the skin augmented the excretion 60 per cent. In a whole series of chronic diseases, it is very important to remove the customary burden from certain afflicted organs. Numerous experiments have led me to conclude that it is possible, acting upon the surface of the skin thermically and mechanically, to produce more favorable conditions of circulation and more copious diuresis. The fact that we are able to augment the frequency of the heart's action and its vigor, as well as to overcome disturbances of circulation in many cases with almost physical certainty, has prompted me, as well as those of my

colleagues who treat disease according to my methods, to obtain frequent relief even in organic troubles, especially in disturbances of cardiac compensation.

A few years ago I believed that the fundament of my system was permanent, and that we needed simply build farther. Then came suddenly—overturning and transforming everything—the new etiological discoveries and conclusions. How could I believe that my method was rational, unless I could show that I could act on all the various micro-organisms with water, and unless I could influence the etiological factor? The need of comprehending causal relations, which is common to all physicians and investigators, was sorely tried; the basis of our therapy was in a measure destroyed, and we were forced back upon purely empirical support. To be sure, we continued to cure the sick, but I was naturally unwilling to concede that my methods were irrational. But the bacteriological billows have been somewhat lulled, and further researches showed that there are ever in the morbid organism a number of forces and processes which are capable of resisting infection. And so I inquired, "Is it not possible that our therapy possesses a certain power to influence this natural resisting energy of the organism? In this respect we have learned not a little. At the outset it is generally known that the healthy and vigorous organism can combat infection far better than the diseased, and we have shown that our therapeutic methods really enable us to invigorate the organism.

We were accordingly constrained to find a basis for a strengthening method. But people are always somewhat skeptical where there are no numbers and figures. Then appeared an interesting work by two Italian investigators, who showed that thermic and mechanical stimuli materially increase the resistance of the muscles to fatigue, and that thus may be explained the tonic effects of thermic and mechanical measures. It was thought to test the muscular energy by the dynamometer, but the latter permits of no precise determinations. Mosso then constructed an apparatus which enables us to measure the muscular vigor quite objectively. Mosso showed that every man exhibits a curve peculiar to himself and individual in form, the latter remaining the same after the lapse of years. Voluntary contractions of the fingers lift weights, so long as this is possible, and the liftings are recorded on paper. Now it has been shown that, under normal conditions, an individual is capable of lifting with the right hand 45 times 2 kilos., at intervals of 2 minutes; with the left hand, 50 times; following a cold bath at 10°, the working power is almost doubled. Warm baths reduce the number of the contractions; massage causes the fatigue to vanish. Warm baths, however, may also augment the resistance of the muscles to exhaustion, although in such case vigorous mechanical measures must be combined with the high temperature.

From the investigations of Brunner we know that micro-organisms are excreted in the perspiration; a quantity of intoxication products is likewise excreted through the urine. In various infectious diseases, the alkalinity of the blood diminishes; an alkaline urine becomes acid under the action of cold upon the organism. Hence, under the influence of cold more acids are excreted, imparting to the blood an augmented alkalinity. The investigations of others, again, have shown that the urotoxic coefficient of the urine in febrile diseases is elevated, under the action of cold, to five and six times the normal. A large quantity of toxic substances are disposed of through the urine, and the organism is liberated from the poisons.

It is a very interesting fact that in many forms of febrile disease the proportion of red corpuscles in the blood sinks materially (often by one half million and more), and that they reappear immediately after the crisis. They certainly could not have been formed within the few hours. We here have before us something which primarily produces the intoxication and the fever, namely, the loss of vascular tone. In individual vessels the blood corpuscles coagulate; these are excluded from circulation, and, according to Flueter, globular stases ensue. In such cases it is rational to proceed to an immediate restoration of the tone. I have shown repeatedly that it is an easy matter, after a cold bath and with the aid of the sphygmographic curve, to recognize the renewed tone of the vessels. Is it not possible that restoration of the tone before the crisis would tend to produce better and more favorable conditions of circulation? Clinical observations prove that, under treatment with cold, stasis is more infrequent than ordinarily.

And if I add to the foregoing my latest observation (as I have learned subsequently, the same thing was reported previously by Rovigi), that through the action of cold an artificial leucocytosis is effected, which, it is assumed, bears a relation to the destruction of the micro-organisms, it must be stated that we have a good foundation for the view that at last our methods are to be brought into rational accord with our present knowledge. The prosecution of further researches in this direction I warmly recommend to my professional colleagues.

UNNECESSARY PAIN IN DENTAL OPERATIONS.*

By JAMES A. REILLY, D.M.D.

It is not my intention this evening to attack any established theories or to attempt to overthrow any cherished opinions or prejudices. I simply desire to call your attention to a few points in everyday practice, and would prefer to suggest a few things you ought not to do rather than those you should do. Indeed, I am to address you from the standpoint of a patient in the chair rather than as the operator at its side.

One forenoon during my senior year at the Harvard Dental School, and while in charge of the dental department of the Bennet Street Dispensary, among the numerous patients was a lad of twelve or thirteen years. He went through the usual preliminaries required in order to have an inferior bicuspid tooth extracted. The operator mechanically picked up his mirror and pliers to examine the tooth, or what remained of it, and almost simultaneously with their introduction into the boy's mouth there was a terrific

scream and a plunge that almost carried him through the window. An attempt at extraction by a street dentist had resulted in the removal of the crown, leaving the entire coronal portion of the pulp standing unprotected. The dentist simply plunged his pliers into the mass of living tissue. Was not that an abuse utterly reprehensible on his part? I think it was, and so would you, I believe, had you been the sufferer. Yet we are doing just such things every day in one form or another.

That "familiarity breeds contempt" is nowhere more noticeable than in the use of dental instruments and appliances. Not long since, a gentleman somewhat prominent in dental organizations told me he had not a dozen excavators in his possession; that he excavated all his cavities with the aid of the dental engine, and wished to wager me that I could not find a cavity in a tooth that he could not reach and prepare as well, if not better, with the engine than it could be accomplished with hand excavators. Upon being questioned if his patients did not complain of being hurt, he replied, "Confound the patients! my duty is to protect myself." If this gentleman could but be patient and operator at the same time, I have no doubt that he would be easily induced to trade some of his burs for hand excavators. Has he not, to say the least, become too "familiar" with his engine? This I consider an extreme case of abuse, for, allowing for a moment that all cavities may be reached (which I do not believe, unless he destroys a vast quantity of sound tooth substance), the time that is gained by its use is but a trifling compensation for the torture that is thus inflicted on children and excessively nervous adults, and I suppose he has such patients. He may run his engine slowly, use the sharpest burs and all the obtundents at his command, but does he diminish the loss of tooth structure thereby, or reduce the inherent antipathy to dental operations which the average patient has? Does he not absolutely destroy the last vestige of confidence the little one may possess who has been beguiled into the chair by its parent with unqualified assurance that "it will not hurt a bit"?

Another appurtenance, no less barbarous in some of the details than the untimely use of the dental engine, is the rubber dam. A prominent writer says, if it is at all difficult to apply, the rubber dam should not be used in the cases of the very young, very sensitive, or very nervous patients. How many of us draw the line at these classes? It is not my intention to point out the occasions for its use or to urge upon you its abandonment, for I consider it a *sine qua non* to good results in numberless cases. But I would like to call your attention to the contempt for your patients' feelings that a "familiarity" with its application breeds.

You are all aware how quickly you jerk your head away if by accident the floss slips too rapidly between your own teeth and burrows itself in your gums while you are cleansing them. How often the same thing is perpetrated on your patients, and nothing thought of it, by you at least, while you are laying coil after coil of cable on teeth that oftentimes do not require ligatures! Frequently, indeed, they serve only to obstruct access to the cavity. We all know, or should know, that with holes of proper size and shape in the dam the employment of ligatures is necessary only in a limited number of cases, provided the tartar has been removed from about the margins of the gum. But for pure, unalloyed torture, permit me to present to your consideration a clamp and an awkward or heavy-handed operator, and I think there are a few such in the profession.

I speak from experience, for it once fell to my lot to sit in the chair with a clamp on an inferior wisdom tooth, compelling me to keep open house during the space of three and one-half hours. My knowledge now teaches me that it was entirely unnecessary, and that the cavity might have been filled, with the aid of napkins, in less time than it took to get the rubber and clamp adjusted, and with infinitely less pain and discomfort.

Now, I do not maintain that the clamp should be relegated into "innocuous desuetude," but I do say that extreme care should be exercised in selecting the proper ones to be used in each particular case, so that they may be easily adjusted, and that the most delicate and extreme accuracy of manipulation possible be employed while placing them upon the teeth. I know of nothing more repellent to the average patient than the rubber dam and its accompaniments; therefore I think it behooves us to manifest a little compassion by dispensing with the use of the clamp, or the ligature, and even the dam itself, whenever it is practicable.

Another medium for pain-culture, and one which gives ample opportunity for the application of all the reserve abuse we may have stored away, is obtained during a course of regulating. A great deal of pain and soreness, of course, it is needless for me to say, is unavoidable while moving the teeth about, but there is also a large amount carelessly inflicted by over-anxious operators, too eager to accomplish in one day what should take a week, and again doing to-day what they must undo to-morrow.

I once saw a case of regulating that was worthy of the attention of the society for the suppression of cruelty to children. The teeth were very much displaced, and appliances were adjusted to almost all the teeth simultaneously. Too much force was applied, and too long an interval allowed to elapse before changing, so that when I saw the mouth there was scarcely a tooth in the superior maxilla that could not have been easily removed with the fingers. For articulation the patient could not bring the teeth together without suffering intense pain. And all this under the direction of a reputed skillful operator. The effect of such operations is most pernicious, for the impression they produce on the patient's mind is often more enduring than what they effect in the physiognomy, and frequently nothing short of an exposed pulp will permit further dental operations during those years when the closest scrutiny and care should be exercised.

This, then, is the point I wish to make regarding the lack of care to avoid pain during regulating; that oftentimes nothing is gained by the operation, because if you succeed in holding your young patient's interest to a successful termination of the work, you have also generated mentally such an intense dread and abhorrence of you and your benefactions that it is not until caries has obtained a firm foothold, and some-

* Read at a meeting of the American Academy of Dental Science, in Boston, Mass.