ful youth, tread upon as many corns east as west of the Mississippi River. Be that as it may, if the wild scientists of the woolly west desire to pass resolutions to protect themselves from outside aggression and internecine strife, we should expect such ambition for self-improvement to be lauded rather than condemned by the cultured exponents of an older civilization east of the Alleghany Mountains.

In defense of the southwest code, we note first that it is specifically stated to be "tentative." We take this to mean that any of the rules may be altered or stricken out to which there is sufficient objection at home or abroad. Furthermore, it appears that the code is intended by members of the Southwestern Division to apply only to themselves; as we understand it, they have no intention—and probably no hope—of applying their reforms to scientists at large. There is, therefore, no occasion for immediate alarm, unless it be on the part of individual eastern scientists who intend, for climatic or other reasons, to migrate to the Southwest.

Now it would be invidious to intimate that Dr. Kempton would personally violate, or condone the violation of, any of the rules in question. We interpret his attitude rather as a kind of Menckenese objection to the appearance of anything savoring of a Rotarian philosophy among scientific men. For this point of view there is much to be said. But as between the two extremes of super-sanctity and sub-Menckenism we plead for a carefully weighted mean.

We should like to believe that scientific men as a class are above the need of a code of ethics. But the enthusiasm with which we commonly refer to an admired colleague as a gentleman *and* a scholar seems to involve a tacit admission that the virtues connoted by these two terms may exist separately as well as in combination.

It may be urged that, if a scholar be not already a gentleman, he can not be made one by any array of rules or resolutions. Alas, too true! But it would appear advantageous at any rate to have a definite code, by which one might decide for himself whether or not he is a gentleman, instead of depending on his colleagues to tell him, which sometimes causes lasting embarrassment on both sides. Then, too, even if it be antecedently improbable that anything can be done about the ethics of the present generation, there is the coming generation to consider—the nascent Ph.D.'s and innocents yet unborn. Is it not our duty to provide that they may learn by precept what it is not wholly certain we can teach them by example?

If, in view of these weighty arguments, it should seem desirable to follow the lead of the physicians, southwestern scientists and other groups of professional men in semi-public service, and to adopt a code of ethics to apply to scientists at large, we propose that, somewhere near the bottom of the list of needed reforms, the following be included:

Rule 160z. Scientific men shall be restrained from flailing each other through the medium of the press. The following penalties shall be provided:

(a) For gentle sarcasm the offender shall be given n black marks, in a large book to be kept by the Secretary of the National Research Council.

(b) For open satire he shall be given black marks to the number of $2n+1$.

(c) For burlesquing or lampooning colleagues, he shall receive a number of black marks to be represented by the expression

$$-\log\frac{1}{(n+1)^{10}}$$

(d) For comparing colleagues, by direct statement or implication, with realtors, insurance brokers, chirotonsors or morticians, he shall be turned over to the ministrations of practitioners of said professions, successively, in the order named.

In conclusion, I hasten to specify, on behalf of both Dr. Kempton and myself, that such rule shall not be retroactive in its application. *Ex hoc malo proveniat aliquod bonum.*

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SCIENTIFIC BOOKS

Les Physiciens Hollandais et la Methode Experimental en France au XVIIIème Siécle. Par PIERRE BRUNET, Paris, 1926.

An Introduction to the Study of Experimental Medicine. BY CLAUDE BERNARD, translated by HENRY COPLEY GREENE and LAURENCE J. HENDERSON. New York, Macmillan Co. 1927.

THERE never was a time when man did not use the experimental method of investigating his environment. There never has been a time when man did not form hypotheses on observations made thus and in other ways. An editor of Bacon's *Novum Organum* more than a hundred years ago remarked that Sir Isaac Newton had a very extraordinary method of making discoveries. When he was engaged in his famed inquiries about light he seemed first to have imagined in his mind how things were and afterwards contrived his experiments. Newton boasted he made no hypotheses, but no mind will work without hypotheses. There can be no doubt Newton made hypotheses both before and after he contrived his experiments on light or gravitation either. It is remarkable there was once a majority of scientific men who, less than a hundred years ago, talked about the experimental method in such a manner the inference was justified that obtaining knowledge by the experimental method is the only way of obtaining knowledge. Claude Bernard was born in the year 1813; in that year Dr. Shaw wrote the above in the appendix he added to his translation of the *Novum Organum.* Magendie was Bernard's teacher and the inheritor of what the Dutch had been so instrumental in introducing to the French a hundred years before. Paul Bert, who succeeded to the chair of his master, says this book of Bernard's, published first in 1865, struck cultivated minds with admiration and astonishment, and it may be added threw many of the admirers off their base and into a foolish exaggeration which is not apt to be repeated now, sixty years later that it appears in the admirable form given to it in this translation.

It is seldom it is thought worth while to republish any work of medicine or general science after sixty years, but this work is in the same class with the *Novum Organum.* I fancy it is the lingering tradition of the mental obsession to which Bert refers we may look for the interest the publishers expect. It is opportune that we have Brunet's book to enlighten us as to how the experimental methods entered France so long before Bernard made it unreasonably dominant, an exaggeration against which he repeatedly protests. Such exaggerations are always to be expected to attend the advent of every manifest step forward in the progress of thought and the achievements of men, but we must realize that Bernard has no claim and made none to having first introduced modern experimental methods in France.

When the House of Hanover crossed the channel to govern England there was a large following from the Netherlands, though perhaps not so large as Holland had sent over with her Prince of Orange and long before that the English Puritans had fled to the Low Countries from the British Isles, so there had been large and frequent interchange between the two nations for many generations before. Even the war between the Dutch and the English on the water had done something. It was not broadsides alone of shot they had fired into one another. It was ideas too they had exchanged after Lord Bacon's death. The new learning and the models by which Bacon and Newton shaped it made a deep impression on the Dutch, but they had no thought of not making hypotheses. Much which Newton called mathematics, others called reasoning, they insisted. Although the mathematician, left to his own processes, "never deviates into sense," he must have a beginning that is to him rational if not sensible, while the result to the layman is neither; but sensible is a word that long since has begun to be slippery in usage. Physics formed the basis of Newton's mathematics and reckoning from it was more exactly done than by hypotheses, but in mathematics hypotheses also have to be used. The Dutch followers of Newton saw more clearly than some of his later English, but s'Gravesande declared that in order to arrive at the admirable wisdom of Newton every physicist should see to it that his reasoning was not founded on simple hypotheses alone. At Utrecht they instituted a large laboratory supplied with instruments of every kind then procurable for the study of physical phenomena, yet s'Gravesande laid down the maxim that the student should try to imagine in his own mind how the phenomena he was about to investigate could arise. Despite his warning that some method more reliable should form the basis of his projected work, six rules for the use of hypotheses were given by the Dutch savant and Brunet says he was apparently not far from thinking that "truths" themselves of physics are only high probabilities. Consequently the results of the experimental method are naught but hypotheses, however close to the truth they may come. We can not deny the experimental method is more often the origin of theories than of facts. Thus early in the Baconian and Newtonian creed did heresy arise when transported to the continent by their disciples. Nevertheless it was they who transmitted to France long before Claude Bernard a rational experimental method. It was not Bernard who was responsible for an irrational experimental method which caused much of the falling away of students in science in France in the latter part of the last century rather than the Franco-Prussian war.

Not only did primitive man experiment with the experimental method, but when science dawned in our civilization it was not entirely neglected by the old Nature Philosophers, though we have scant record of any. Hippocrates and Celsus, however, are recorded as having resorted to it in medicine and Galen was one of the greatest experimenters who ever lived. Neither Bacon nor Bernard were the earliest pioneers, nor the Italian School. Men believed Bernard had but followed in the path of Bacon, but he did far more than that. He was the most important of his critics. He was no bigot who believed that pure induction from observed facts can often lead directly to the truth. In this many, who have insistently quoted him for two generations of scientific men, have totally misapprehended him and English readers are put in the way by the present volume of avoiding this error. As to his inspiration by Bacon, nothing could be more erroneous. The chief criticism which Bernard aims against the methods of modern science is not against the neglect of experimental science which had long flourished in France before him but specifically against the mistaken ideas of Bacon, evidently motivated by the abuse of the precepts of the inductive methods of research. In the introductory chapter of his work he insists that "the experimental method is nothing but reasoning by whose help we methodically submit our ideas to experience—the experience of facts. . . . The experimenter must be at once theorist and practitioner. . . . It is impossible to devise an experiment without a preconceived idea."

The chances were forty years ago that a student on entering a laboratory for the first time was adjured by the director to approach his problems with an open mind almost so insistently that the student had reason to believe he was to approach them with an empty mind. He must have no preconceived ideas as to the solution of them. This was mere drivel. It put a premium on infancy and idiocy. Claude Bernard was supposed to be the protagonist of such doctrine, for the history of medicine, especially of science, was not wide spread among laboratory workers in those days. Bernard insists "facts are necessary materials, but their working up by experimental reasoning, *i.e.*, by theory, is what establishes and really builds up science." Bernard seems, by the frequency with which he sounds this note, to be protesting against certain pernicious tendencies in French methods of the pursuit of science. It was the revival of a new science in the Netherlands, which was the origin of its juvenescence in France. There were more solid cultivators of it than Voltaire, but when he returned from England he made the ideas of science he had imbibed there popular. Probably his friend, Mme. du Chatelet, understood them better, but he was the greatest propagandist of them all, so that through him a very much more extended stratum of the people became acquainted with the ideas, which several years before had arrived by the way of Holland and were thus given a wider circulation than otherwise would have been the case from the introduction Nollet was able to give them among learned men.

Descartes and his philosophy had had a great effect on the teachings in the universities of Holland, but most of the physicists there appreciated the inefficiency of his doctrine. Boerhaave in his doctor's thesis in 1693 had already published matter in which he dwelt on the importance of the experimental method in medical research. In less than ten years he had acquired a great reputation at the University of Leyden, which extended throughout Europe. Leeuwenhoek had already communicated his observations with the microscope to the Royal Society of London, but it was in 1715 that s'Gravesande was appointed secretary to the embassy sent to congratulate George I on his accession to the crown of England. By means of one of the students he had had in the Low Countries he gained the acquaintance and the esteem of Newton and became a member of the Royal Society. When he returned to Holland Boerhaave had pronounced a discourse *De Comparando in Physicis,* which acted as a sort of primer of science for future work in Holland. After that the names we know of those who became indoctrinated with the ideas of the experimental method in science there are numerous. Thither Voltaire repaired to consult Boerhaave about not his health alone, but about the new things in science. In France already Nollet's influence had become marked among men of science. When the instruments then in use in the neighboring country became known in France they were constructed with more precision there, but Brunet does not forget the indebtedness not alone of France but of England and Holland also to the Italians, Galileo, Torricelli, Redi, Borelli. Milton drew on Italian culture for his great works in literature. Harvey, Willis, owed a deep debt to them. Later, however, the current of advance in methodology in science flowed through England, and with the impulse of Bacon's crude revolt from scholasticism behind them the new departure found its way to France very largely through Holland.

It was Boerhaave and s'Gravesande and Muschenbroek and doubtess many others who introduced and developed an admirable experimental method in the early part of the eighteenth century in Holland. They yield in no way to their great protagonists in England in that acuity of intellect, which manifests itself by talent and common sense rather than by genius. They escaped many of the errors of Bacon and Newton. They got their cue from them, but when the mantle fell on their shoulders science made vast strides in the Netherlands. In its earliest years in France there had already been some instruction given and some exposition of the experimental method by Polinière, but already the fame of Boerhaave was spreading there as well as elsewhere in the continent and in England and students were going to him from the latter country, whence at the beginning of the century he had drawn his first inspiration. Vaillant, who had been fired by the lectures of Boerhaave, left a posthumous book in the care of the latter, a confidence which Brunet thinks exceptional in a Frenchman for a Dutchman at that time, but its title has a significant interest for us. Boerhaave, five years after Vaillant's death in 1722, published it at Leyden and Amsterdam in 1727 as the *Botanicon Parisiense*. In France Castel had been provoked to attack Boerhaave for too much worship of Newton as the greatest of physicists and especially because he wished to banish every sort of hypothesis, which exclusion Castel was quite right in declaring often stops access to the truth, claiming that even Boyle in England had given utterance to much of the same criticisms. But the work of Vaillant found its way into France and Noguez in 1725 translated a work of Niewentyt and in 1731 Boerhaave was elected to membership in the Academy of Sciences at Paris. Nollet, in 1738, was made the incumbent of a public chair of experimental physics in Paris founded by Cardinal Fleury. It was in Italy, in Florence, however, that there existed the oldest Academy of Experimental Physics in Europe.

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SPECIAL ARTICLES

LONG-LIVED CELLS OF THE REDWOOD

THE principal features of long-lived cells in the massive stems of cacti have recently been described by the senior author.[1] Medullary cells of the tree cactus (*Carnegiea*) were seen to remain alive for periods well over a century and the results of the examination of elements of all ages indicate active enlargement during the second half of the century.

Another cactus, the melon cactus or bisnaga (*Ferocactus*), which has an ovoid-cylindrical trunk was found to include similar medullary cells of great age which, however, ceased to grow after the first decade. The development of the outer cortex as a layer several inches in thickness is of such character as to demonstrate that the cortical cells have a similar period of enlargement and survival.

In all of these cells the carbohydrate components, pentosans and hexoses progressively decrease with age. The fatty substances, or lipins and nitrogenous substances, change least.

Transformation of sugars to wall-material with consequent thickening is apparent in *Carnegiea* and in the medulla of *Ferocactus*; crystals of salts accumulate in all cases. It was notable, however, that in the cortex of *Ferocactus* the disappearance of the carbohydrates extends even to the walls, which are thinner at a hundred yards than at ten, suggesting the liquefaction and removal of pentosans. It seems probable that the only consumption or use of the lipins from these cells is of that which may be in the walls and cytoplasm, and that these substances in the nuclei are but little affected.

That ray cells may attain an age of many years in the whitish sap wood of tree trunks is implied in the writings of many authors. We can not find any definite statement of living cells in heart wood, although Strasburger's account of starch in trunks of the red beech 125 layers from the surface suggests that these cells might be alive.[2] The negative assertion that "only the outer layer of the wood composed of the more recently-formed annual rings thus contain living cells and constitutes the splint-wood" made in the 14th German edition of Strasburger's text-book of botany and found on p. 158 of the 5th English revision as translated by Lang in 1921 may be taken as a correct presentation of present knowledge of this matter.

Certain features of behavior of the trunks of the California Redwood uncovered by our study of the hydrostatics of this tree led us to examine the parenchymatous cells of the trunks. There are two different types of living cells in newly-formed secondary xylem of *Sequoia sempervirens:* wood-parenchyma cells that stand in vertical files scattered among the tracheids, and ray-parenchyma cells. As in most other woody stems, the wood-parenchyma and ray-parenchyma cells of the alburnum or whitish sapwood are living and densely packed with starch.

The change from alburnum to duramen (heartwood), macroscopically recognizable in *Sequoia* by a brownish-red coloration of the duramen, is accompanied by a disappearance of starch and protoplasts from all wood-parenchyma cells and the formation of an orange-colored resin that completely or partially fills the lumen of the wood-parenchyma cells. For this reason wood-parenchyma cells are often called resin cells. The transition from alburnum to duramen is also accompanied by a disappearance of starch from the ray-parenchyma cells but this disappearance of the starch is not always followed by death and disintegration of the protoplasts. These living ray cells of the duramen have a thin layer of cytoplasm next the wall, a conspicuous nucleus, and a large central vacuole. The ray cells may remain unchanged for a long time and we have observed ray-parenchyma 70 layers deep in the heartwood with clearly defined protoplasts and apparently normal nuclei. As the sapwood in such trees included 21–23 layers these cells were about a century old. Cells of older annual increments in these stems also appeared to be living but this could not be determined with certainty since the granular nature of the cell contents obscured the nuclei. Some trees have ray-parenchyma cells that show a granular cytoplasm and small droplets of resin shortly after the transition from duramen to

[1] D. T. MacDougal. "Growth and Permeability of Century-old Cells," *Amer. Naturalist* 60, 393–415, 1926; and Frances L. Long, "Characters of Cells attaining Great Age," *Amer. Naturalist,* September–October, 61, 385–406, 1927.

[2] Strasburger, E. "Ueber den Bau und Verrichtungen d. Leitungbahnen in den Pflanzen." Pp. 274–275. 1891.