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BONDS OF UNION BETWEEN TROPICAL MEDICINE AND GENERAL MEDICINE[1]

IT is my wish that my first greeting and that my parting words should express my appreciation of the honor and the pleasure of visiting you here in Porto Rico. It is distinctly profitable to me to have this opportunity of exchanging ideas and experiences with you in our chosen field of work. In arranging for our conference this evening Dr. Lambert wrote to me suggesting that my talk ought to have a title, and he even ventured to hope that this idea would not come as too much of a surprise to me. So I have selected a subject that will permit us to wander where fancy leads, perhaps, who can tell, to one or two unexpected developments.

Our crowded activities grant us but little time for reflection, and it is easy to overlook points of contact between the adjacent fields such as tropical and general medicine. Inevitably, these two fields have exerted a profound influence on each other.

Strangely enough, the conception of tropical medicine is rather foreign to many individuals in the profession at home. The very name sometimes tends to frighten people away. To a few, it represents merely a curiosity, interesting but unimportant. One of your obligations will be to arouse still further the interest and support in New York City of your work here. One summer I met an elderly physician who had spent his life travelling in the tropics. He said to me: "Mercy, no, I stay on the ship. I never look at those patients ashore. Why should I clutter up my head with all that nonsense? If I ever had to treat those diseases I'd go somewhere and take a month's course in the subject." A few of the more serious minded deceive themselves into thinking that tropical medicine represents a real opportunity to make distinctive discoveries of new etiologic agents and new and important clinical entities almost without effort. Sometimes I feel that my friends almost take it to heart that the generosity of nature in tropical lands should cheat industry and so rudely violate the stern principles of no reward without great labor.

[1] A popular address delivered on February 22, 1927, as visiting lecturer at the School of Tropical Medicine, University of Porto Rico, San Juan, Porto Rico.

Suppose, however, that we want to choose as a model a disease of extremely intricate nature, the details of which have been worked out with exactness. It is best to turn to the tropics for an example. In the etiology of malaria we have not one but three distinct parasites very closely related; however, to the eye of the protozoologist they are readily distinguishable. The transmission of the disease from patient to patient is a complicated process. The sexual forms of the malarial parasite come into prominence. They leave their intermediate host, man, to take up a cycle of development in their definitive host, certain species of mosquito. After the completion of this cycle the mosquito is ready to set up new infections in man. The pathology of the disease is well-nigh completely understood. The diagnosis in patients is reasonably exact. The treatment, though not perfect, is marvelous compared to that of many bacterial infections. Lastly, prophylactic measures, though difficult, are quite feasible. The antimosquito campaigns have been enormously facilitated by Marshall Barber's ingenious introduction of Paris green for combating larvae.

We look to Sir Patrick Manson and to the British nation as the founders of tropical medicine. They took advantage of their natural opportunities. Great Britain with her extensive possessions has many physicians employed in foreign service and they all come back to one spot. The conditions in the United States are very different. We have only a few men who go to the tropics and they come home to scatter across a broad continent. A corresponding difference applies in the two countries to patients who are invalided home. With us, this works to the disadvantage both of the patient and of the subject of tropical medicine. In the absence of the incentive of stern necessity, interest has been intermittent and the development of the subject has been slow and arduous in the United States. When I wanted to learn something of the scope of your new undertaking I turned not so much to the American medical journals for information but to the literature from Great Britain. There have been some exceptions to this general lack of interest at home. Occasionally some of our institutions have devoted a number of years of work and considerable sums of money to purely tropical problems, meanwhile carefully avoiding any permanent responsibility in this field. There seems to be a willingness to profit by special opportunities arising in the field of tropical medicine accompanied by an unwillingness to offer continuous support to the necessities of the daily routine in this subject.

Clearly we can not model our schools after the British pattern. We must find somewhere compensating advantages in our own circumstances. Here in Porto Rico one sees the fascinating opportunity of making a complete study of a disease. The hospitals provide facilities for thorough clinical observation. The laboratories stimulate the desire to experiment in many theoretical directions. But we are not satisfied until satisfactory results have been obtained in the prevention of disease under varying conditions and the unforeseen but interesting disadvantgaes of actual work in the field.

The finger of erudition is often pointed at the term "tropical medicine." Some medical men seem to feel that they have fathomed the mysteries of this term when they learn that workers in the tropics have frequently made the statement that there is no such subject as tropical medicine. In one sense, I agree with this. Admittedly the phrase is one of convenience rendered necessary by the geographical separation of diseases. But the distinction is justified on scientific grounds. Fascinating fundamental principles are found in tropical medicine for which no counterpart exists in the diseases prevalent in cold climates. Thus, for all intents and purposes, the insect hosts of disease and even the protozoan infections take no real part in the life of the student of medicine in the temperate zones. Let me assure you, it is a difficult matter to find a satisfactory phrase for differentiating the medicine of cold climates from that of the tropics. "Internal medicine" does not help greatly. Largely in a complimentary sense I have been using the expression "general medicine." In so far as general medicine permits itself to forego the subjects of protozoology and entomology, it becomes an important branch of the larger field of medical sciences.

Well, this ought to convince you of my warning that we might be tempted to wander from any fixed topic. I will not in any way attempt to sum up the more striking achievements of tropical medicine but will emphasize only those features in which this subject and general medicine have exerted an influence upon each other. Indeed, as already indicated, we must close some of the most brilliant pages of medicine.

In my judgment, the most fundamental influence that tropical medicine has exerted in the field of medical sciences is to be found in the discovery of vitamins and the group of diseases sometimes designated as avitaminoses. You remember when we studied medicine, not so very long ago, beriberi was classed among the specific infectious diseases, with the reservation that some unknown toxin might play a more or less decisive rôle. We now know that neither of those factors plays any part whatever. The clinical and epidemiological data seemed at times in the past to offer almost convincing evidence of

the infectious nature of this disease. Many striking incidents occurred in a manner almost suggestive of some conspiracy in nature to conceal the facts. For example, a ship with its crew in apparently good health would call at a port where beriberi prevailed. Then after some days the disease would, so to speak, "break out" among the crew. Furthermore, if a mother suffering from beriberi nurses her own child she becomes a serious menace to the health of that child. Such children are prone to develop beriberi in an acute form that terminates suddenly in death.

But the Dutch investigators knew and had known for many years that beriberi was due simply to lack of proper nourishment. This discovery was made by Eijkman in 1890. There is an incident of a very human nature connected with Eijkman's production in chickens of experimental beriberi or, more accurately, polyneuritis. At the time he was not working on beriberi at all and chickens are very likely the last animal that he would have selected for the study of beriberi. The incident, as it was described to me, occurred in this manner. The animal quarters for these experimental chickens lay at a little distance from the laboratory alongside the hospital building. Very unexpectedly, the chickens developed polyneuritis to such an extent that the investigations that were in progress were seriously threatened. So Eijkman courageously decided that it was important to find out the cause of this polyneuritis. Then, to his surprise, the chickens promptly recovered so he was obliged to go back to his original problem only to be interrupted again by the recurrence of polyneuritis. Many observers would very justly have felt discouraged at this point. But Eijkman with remarkable skill succeeded in unravelling a mysteriously intricate network and revealed a clear chain of events occurring in logical sequence. The denouement came in this manner. In the hospital the patients were served with polished rice. For the protection of the patients, a rule was in force that any rice left on the patient's plates must be thrown out. In the laboratory the animal boy was given a small sum of money for the purchase of the cheaper unpolished rice for the experimental chickens. Now the Malays are very kindly in their disposition and the laboratory boy had a good friend among the orderlies in the hospital. It does not require much imagination to see that it was easy to provide a handsome diet of polished rice from the hospital for the chickens in the animal house nearby. There was no need wantonly to squander these valuable funds supplied for the purchase of unpolished rice. Now by mere coincidence, it happened that after the first outbreak of polyneuritis appeared, the orderly in the hospital went on vacation and the laboratory boy went back to feeding unpolished rice. The symptoms disappeared and the animals recovered with mysterious rapidity. Then when everything was going nicely, the orderly returned to the hospital and the chickens resumed their diet of white rice and with it the symptoms of beriberi returned.

Looking back at this distance, everything is beautifully clear. There is some strange substance in rice bran, in various grains, and in many other foods which is necessary for normal nutrition; in the absence of this substance polyneuritis develops. Eijkman picked his way with remarkable accuracy through this complicated maze in the face of many conflicting theories about beriberi. His conclusions were promptly rejected. The rest of the world said, "Oh, no, it just can't be true!" Twenty years later several investigators had the very happy thought of trying it. They made a discovery. They found that it is true. There is no flaw in Eijkman's experimental data, though his interpretation has subsequently undergone some revision.

Beriberi is not uncommon in the Philippine Islands. The Filipinos offered information to the profession which was not utilized. Patients under treatment sometimes reported that they could cure themselves with a diet of a certain native bean. Not knowing the facts about beriberi at this time, we explained to them very kindly the error of their ways. They accepted our explanations with gratitude and continued in their superstitious practice. Their confidence was not misplaced, that is, their confidence in dietary measures was justified.

Barely a quarter of a century after Eijkman's first publication, the physiologists began an accurate investigation of the requisites of a balanced diet. The first steps were difficult. It was necessary to give up some old established comfortable views. Proteins, carbohydrates and fats with a little salt and water had long been regarded as an adequate bread of life. Now it was rather disturbing to have to admit that some utterly unknown substance, even in minute quantity, exerts a powerful influence and is an essential item in our daily diet. Funk used the term vitamin to designate this substance which prevents the development of beriberi. As the interest in this phase of nutrition increased, other vitamins were discovered. Recently one has been described by Evans in California which is concerned not with ordinary nutrition but with the process of reproduction. In the field of medicine investigators naturally sought to explain other diseases on the basis of a dietary deficiency. It had long been known that scurvy is relieved by lime juice. Work of outstanding importance was accomplished by Goldberger in pellagra. Now in beriberi the problem is relatively simple; in

pellagra it is more complicated. Undoubtedly much valuable progress has been achieved in pellagra, but it is by no means certain that we have the complete story in hand as yet. It is not clear whether a vitamin is lacking or whether the deficiency lies in some other factor. The process of disproving the theory of a specific infection proved to be a long task even in the simple conditions of beriberi. One frequently hears of pellagrins whose symptoms do not yield to dietary measures. In this type of patient, it is particularly desirable to obtain additional evidence before utterly dismissing the idea of an infectious disease. In brief we are forced into a somewhat unwelcome situation; it is clear that a dietary disorder can produce symptoms sufficiently like those of a specific infection to cause long-standing confusion.

In the subject of etiology, let me mention first of all an interesting association between two diseases, that, let us say, seldom meet. One of them, namely sprue, is truly a product of the tropics. The other, progressive pernicious anemia, is a serious mystifying disease of cold climates. So far as we can tell at present, it occurs at least very irregularly in the tropics. Here in Porto Rico sprue is endemic. I intend to take full advantage of my opportunity to learn a great deal from you about this disease. You all know of the important work of Dr. Ashford on the rôle of monilia. As regards the causation of pernicious anemia, so little is known that one is fancy free to hold almost any view. It may be a specific infection, or perhaps it is a disturbance of nutrition or it has even been regarded as a disorder of the blood-forming organs analogous to malignant disease. With all the intensive study that has been made there is still very little to guide one along the correct path. Any suggestion would be very welcome. You may be interested to know that students of pernicious anemia are beginning to take an intense interest in sprue because of certain similarities which sometimes occur in these two conditions. I refer more particularly to anemia, to achylia gastrica and to the changes in the spinal cord. Here we have clearly a clue to be followed up and it is being followed by several investigators in the United States. Considerable work is now in progress on the occurrence of monilia in pernicious anemia. It is much too early yet to say what course this work may take, what it may develop into, or where it may eventually lead us. You see that we are only at the very beginning of our knowledge concerning the cause of pernicious anemia. But in the treatment of anemia very gratifying progress has recently been accomplished through dietary measures by Minot and Murphy. The very name of this disease, progressive pernicious anemia, is sufficiently terrifying and there is already ground for hope that we may be justified in dropping the word "progressive" from this ominous phrase. By means of a diet of liver, patients suffering from pernicious anemia have experienced relief over periods of one to two years in a manner that has not been accomplished by any previous form of treatment.

Now you all know that the old-established treatment of sprue rests on dietary measures. This type of therapy, though very new in pernicious anemia, is an old procedure in sprue. We have the strawberry diet, the milk diet and the meat diet. It is already time to consider the inter-application of dietary measures in the case of refractory patients afflicted with either of these diseases. To illustrate, some sprue patients improve best of all on a meat diet but can not stand the monotony of this régime. One would not hesitate to transfer such a patient to the diet of liver as used in pernicious anemia.

We are now in a position to consider for a moment the question of etiology. We are confident that an infectious process is an important factor in the production of at least some of the symptoms of sprue. In pernicious anemia, the relief of symptoms by a change in diet might be interpreted as evidence of a purely metabolic disorder. But the analogy with sprue must be studied further. As yet it would be premature to close our minds completely to the possibility of a specific infection in pernicious anemia. It would be a distinct advance if one could establish even the general type of disease to which this form of anemia belongs. The geographical distribution of sprue and pernicious anemia renders it difficult for one individual to familiarize himself with the two diseases; yet a mutual understanding of the two is helpful.

I believe it was Pascal who gave us the important counsel that the apparently trivial exception sometimes opens up the way to unsuspected fields of important information. In this connection let us remember that scarlet fever is not endemic in the tropics, but streptococcal infections occur here though perhaps with less frequency than at home. This leads us, in a minor way, to something of a paradox. The weight of opinion in the United States at present tends toward the acceptance of the streptococcus as the cause of scarlet fever. It has always been a puzzling question as to why scarlet fever does not occur in the tropics; if it is caused by a streptococcus the solution of this question becomes even more important. You are all familiar with the happy results that are being accomplished in the treatment of scarlet fever with serum, this work having been initiated in your affiliated school in New York by Dochez and by the Dicks in Chicago. Clearly there is an opportunity here in Porto Rico which does not exist in New York and Chicago to furnish supplementary evidence regarding

the etiology of scarlet fever. I assume that no one here or elsewhere in the tropics has yet studied anew the streptococci found in the tropics and compared them serologically with those isolated from scarlet fever. If the streptococcus found in scarlet fever occurs here, then one has the interesting task of discovering why it does not produce the clinical symptoms seen in cold climates. Cases of scarlet fever are imported here occasionally, but perhaps the streptococcus is unable to persist in virulent form in this climate. This would imply a subtle difference between this streptococcus and its close relatives. Very likely one might be able to find an analogy for these circumstances, but the situation is sufficiently interesting to arouse the imagination.

It is an easy matter to explain why some diseases are limited to the tropics. We have in but very few instances arrived at a plausible explanation to tell us why some types of infection are found only in cold climates. The subject of scarlet fever brings to mind the question of measles. It is always helpful to see the same disease under varying conditions. The characteristics of life in the tropics offer certain minor advantages in the very difficult problem of the etiology of measles.

We are all familiar with the world-wide distribution of bacterial diseases as a group; many species utterly disregard climatic conditions. The protozoa are very discriminating and their home is distinctly in the warm climates. Pathogenic amoebae do get something of a foothold in the north; but there is room for doubt as to how long they would maintain themselves there if the supply from the tropics were suddenly and completely wiped out. The spirochaetes in their biological characteristics occupy a position which, in one sense, is intermediate between the bacteria and the protozoa. Likewise, the geographical position of the spirochaetes follows an intermediate course. There are a few pathogenic species which thrive entirely independently of climatic conditions, but the pathogenic group taken as a whole shows a predilection for the tropics.

Long ago Schaudinn suggested that the cause of yellow fever would prove to be a spirochaete and Stimson demonstrated a spirochaete in the kidney in one patient dying presumably of yellow fever. The demonstration of leptospira as the causative agent of infectious jaundice added a fresh impetus to the search for spirochaetes in yellow fever. Noguchi worked intensively in this field. His results raise questions of fascinating interest regarding the relationship between yellow fever and infectious jaundice, *i.e.*, Weil's disease. The subject is an intricate one, and it will be best to reserve it for detailed discussion at a later period.

Let us turn to an example where our information concerning the main features of etiology is complete. It is one in which tropical medicine received very direct assistance from general medicine. Clinical analogies had long been recognized between yaws and syphilis and even over-emphasized. Following Schaudinn's announcement of the discovery of *Treponema pallidum,* Castellani very promptly supplied convincing evidence establishing a similar treponema as the cause of yaws. Some of you may not recall that the treponema of yaws was actually seen by Castellani before *T. pallidum* was described, but under such circumstances that Castellani did not appreciate its etiologic rôle. The subject of tropical medicine just missed the opportunity of pointing the way to the etiology of syphilis. Yaws and syphilis illustrate well the firm bonds of union between tropical medicine and general medicine. It is impossible to appreciate either of these diseases thoroughly without a comprehensive knowledge of the other. Yaws is one of the comparatively few diseases that is truly tropical and this geographical limitation is not dependent on an insect vector. It seems to me by no means fanciful to regard syphilis as an evolutionary change in yaws for its adaptation to cold climates. While the treponema of yaws is restricted to the tropics, it is indeed regrettable that nature has succeeded so well in adapting *T. pallidum* to the rigors of cold climates.

Thus the development of our knowledge of yaws owes a real debt to the general medical sciences not only as regards its etiology but also in the application of the Wassermann reaction. This debt has been in a large measure repaid in a way that is not fully appreciated. Chemotherapy of the systemic infections received during its infancy almost its sole impetus from the field of protozoology and the spirochaetal diseases. The development of salvarsan was very closely associated with tropical medicine. As you know, trypanosomes and the disease trypanosomiasis, *i.e.*, sleeping sickness, furnish a practical method for the study of chemotherapy. Ehrlich, by systematic investigation, tested his long series of compounds on trypanosomes and on various spirochaetes. It so happened that salvarsan proved to be effective for many spirochaetal diseases, including syphilis. More recently the Rockefeller Institute devised a chemotherapeutic agent, tryparsamide, for the treatment of trypanosomiasis. Some clinicians with an inquiring turn of mind tried the effect of this drug in the late stages of syphilis with results of decided interest.

There is another achievement in chemistry that we must not pass over. Under the conditions of tropical life, several important parasites can be demonstrated in the blood stream. A good staining technique is in-

dispensable. The German workers in tropical medicine did their part in developing the Romanowsky stain to the point where it has become the routine method which is used in one form or another throughout the world in the study of blood conditions. The modification devised by Giemsa has been applied by Wolbach in the study of sections, particularly for the demonstration of Rickettsiae in tissues. This technique has proved itself to be very valuable in attacking this interesting and difficult group of microorganisms.

Biochemistry has also made its contributions. Some years ago there was a small outbreak of Asiatic cholera in Manila. At that time it fell to my lot to be on duty in the cholera wards. Some of the patients in the stage of reaction showed unmistakable clinical signs of air-hunger, an almost typical Kussmaul's coma. Obviously these cases were not associated with diabetes, and the urine, as a rule, was free from acetone. However, the clinical signs of acidosis were characteristic and it seemed advisable to look for some method other than the tests for acetone bodies for the recognition of acidosis. Accordingly these patients were injected with sodium bicarbonate. Enormous quantities—90 or 100 grams—were often required to render the urine alkaline, whereas if a healthy person takes a teaspoonful of soda the urine changes promptly from an acid to an alkaline reaction. Formerly, a large proportion of all cases of Asiatic cholera, roughly 15 per cent., died of uremia. Now it was found that early treatment of cholera patients with bicarbonate practically eliminated the complication of uremia. Therefore it seemed probable that a similar lack of alkali might occur in patients developing uremia in the terminal stages of Bright's disease as we see it in cold climates. Accordingly, in Baltimore I examined such patients. They showed an even more intense degree of acidosis than the cholera patients, but obviously they were in the end stages of a long-standing disease and no lasting benefit could be expected from treatment with alkali. For our understanding of nephritis it is important for us to know that acidosis is one of the factors which is responsible for the symptoms of uremia. This fact is now generally accepted, for it has been confirmed by many observers using chiefly the method of direct chemical analyses of the blood—a method that in my opinion is rather less delicate than the test of tolerance to alkali.

These studies in Asiatic cholera therefore have given us an improvement in its treatment and a demonstration of a type of acidosis that differs markedly in detail from that of diabetes. This acidosis occurs not only in the nephritis of cholera patients but also in the ordinary nephritis of cold climates.

Before leaving the general field of chemistry, let us look kindly and briefly at a small and now harmless skeleton behind the curtain. An error of a pharmacological nature delayed the development of emetine therapy in amoebic dysentery for two decades. From time immemorial the natives of India had known that ipecac often, though by no means always, gave relief in the treatment of dysentery. With the differentiation of the amoebic and bacillary forms of dysentery, medical men became more and more interested in ipecac treatment. Now if any of you have ever had occasion to undergo the old ipecac treatment, you will know what a heroic undertaking it is to manage sixty or ninety grains of this drug, on account of the fearful nausea that ensues. In an honest effort to advance the use of ipecac, the idea was put forth that the drug does not owe its action to its content in emetine. For a time this conception gained ground. In my day in medical school we were advised to use de-emetinized preparations of ipecac in treating amoebic dysentery. It seems strange that this error was not corrected by the experience gained in actual practice. One possible explanation is that some of the presumably de-emetinized preparations still contained appreciable quantities of emetine. The correction of this error came about twenty years later in this manner. Vedder working in Manila found that emetine was very toxic for the cultures of the harmless water amoebae and he advised that the pure alkaloid be tried in the treatment of infections with *E. histolytica*. Rogers acting on this suggestion developed the treatment of amoebic dysentery by the intramuscular injection of emetine.

We must at least mention the topic of climatology. We may look forward to interesting advances in this subject in the future. Under natural conditions, it is very difficult to judge the effect of climate on man. There are many variable factors which are not susceptible of control and the effects are not susceptible of exact analysis. Accordingly, some wonderful laboratories have been equipped at home in which artificial climates may be produced and regulated at will. The problems are numerous; some physiologists are interested in determining the effect of climate on the nervous system and on the mental processes of the individual.

In matters pertaining to climate, I would rather turn to the infectious diseases. Radical theories have been advanced suggesting that infectious diseases may change profoundly under the influence of climate. Diseases may be limited by climatic conditions. In the main, they refuse to change their symptoms and

their general behavior. To illustrate, it is now accepted that syphilis and yaws are distinct diseases. We no longer hold the view that syphilis under tropical conditions changes its manifestations and gives rise to yaws.

Permit me to point out one very interesting organism, namely, *B. pestis*, the causative agent of bubonic plague. This disease is transmitted by the rat flea. But at times, especially in northern climes, this same bacterium *B. pestis* has given rise to a disease characterized not by buboes but by pneumonia and we have pneumonic plague. Occasionally, cases of bubonic plague also develop pneumonia, but epidemics of the pneumonic form occur only in cold climates. Insects play no part whatever in transmission, but droplet infection is the mode of conveyance. Droplet infection explains itself. Some of you may remember having been in countries so cold that on stepping out of doors in the morning you could "see your breath" on the air. It is this "breath" which carries microorganisms with it. The organism *B. pestis* is easily recognized in each of these two types of disease. Otherwise a strange error might easily have occurred. It would have been only natural to suppose that these two types of infection were totally unrelated diseases; one a pneumonia, always fatal, and epidemic in cold climates, the other a disease of rats and man, carried by fleas and characterized by buboes.

Let us now take up briefly some phases of epidemiology and hygiene. Manson really shocked his best friends by suggesting that filaria, a parasite found in the blood, might be transmitted by mosquitoes. Undaunted, this pioneer demonstrated in 1878 that certain species of mosquitoes take up filaria from the blood of a patient. Moreover, he followed developmental stages of the filariae in the mosquito. The manner in which the mosquito carries the infection back to man was not demonstrated for a period of many years. In the meantime it fell to the lot of Theobald Smith to furnish the first complete demonstration of "insect" transmission. As you know, he showed clearly that the disease Texas fever, which affects southern cattle, is transmitted by the cattle tick. In spite of very clear experimental evidence, this conception proved almost too daring for scientific acceptance by the leaders in medicine of that period. In the face of skepticism, the demonstration of the insect transmission of malaria, of yellow fever and other diseases soon followed. An incident occurred in Cuba which expresses very well the attitude in that day toward the insect transmission. You remember that Dr. Carroll, of the yellow fever commission, submitted to an experimental infection with this disease. When his infection was fully developed he told his nurse one evening that his attack of yellow fever was produced by the bite of a mosquito. During his convalescence, Dr. Carroll was looking through his clinical chart and he found this entry by the nurse, "Patient is delirious to-night; says he got his fever from a mosquito bite." Eventually a long list of infections have been shown to be insect borne. Indeed it might seem that an easy way was opened up to determine the rôle of insects in transmitting an infectious disease. The problem however is not quite so simple. I would call your attention to the stubbornness with which kala-azar has steadfastly refused to yield the secrets of its way of life in the insect world. It seems now, under the attack of the three commissions engaged for some years in field work, that kala-azar is almost proven to be transmitted not by fleas nor the lowly bedbugs but by sand flies.

One of the very few absolute triumphs in hygiene in the United States was accomplished by virtue of the demonstration in Cuba concerning the rôle of mosquitoes in yellow fever. This disease has now vanished from general medicine of our northern cities, probably never to return. A relative triumph in hygiene in our southern states owes its origin to the demonstration by Ashford and his colleagues here in Porto Rico that uncinariasis can be controlled by appropriate field measures. It challenges the imagination to conceive the far-reaching ramifications of this work that had its beginning here only a few years ago. Recently this work has received a fresh impetus by virtue of the remarkable advances made in Washington by Hall and his associates in antihelminthic therapy. The splendid development of the hookworm campaign throughout the world reached such proportions that the practical difficulties multiplied themselves. The introduction of carbon tetrachloride by Hall furnished a reviving influence to this campaign.

One could easily multiply the instances in which remarkable results have been achieved through public health measures in cold climates for the purpose of driving back to their home the diseases originating in the tropics.

Recently I have had the pleasure of visiting the department of health and the privilege of seeing something of the work of your director of public health. The achievements in hygiene in Porto Rico are progressing to such an extent that the workers in this institute will be driven not merely to neighboring islands but farther and farther from these shores on expeditions for research in its many and varied phases. There are advantages and disadvantages about these expeditions. The one thing we can be sure of is that they are a necessity.

Obviously the opportunities for teaching here are splendid. I hope it may be your privilege to bring about a closer association in work, in thought and in

the exchange of ideas between the staffs of this school and that of your affiliated school in New York. Perhaps you may enjoy the distinction of seeing these two branches of medicine grow into one subject. Fortunately you do not have the responsibility of providing here a complete course in general medicine. Our schools at home are carrying a burdensome curriculum that is constantly growing. But think of the situation which pertains, for example, in the University of the Philippines. There the students must be given a thorough foundation not only in general medicine but they must also be prepared to meet the daily problems in protozoology, helminthology and entomology. After all, the ideal location for a school of general medicine is in the tropics. Such schools have not as yet attained the distinction of a genuinely international reputation. The opportunity for the time being is lying dormant.

We have sketched very lightly some of the more obvious ways in which the interests of tropical and general medicine are intermingled to form the growing structure of the medical sciences. We have illustrated this by a consideration of vitamins and their relationship to beriberi, scurvy, rickets, pellagra and to the physiology of nutrition and reproduction. You have before you now the important results which have just been achieved in the study of rickets by the commission from the Yale Medical School. The relationship of sprue and pernicious anemia commands special interest here in Porto Rico. The study of the streptococci in the tropics will aid in advancing our knowledge of scarlet fever. Among the spirochaetal diseases there is much opportunity for reflection. We have the treponema of syphilis and yaws and the leptospira of Weil's disease and yellow fever. In the field of biochemistry, progress has been made in the study of nephritis as it occurs in Bright's disease and in Asiatic cholera. Turning from medicine to hygiene, we find in many respects a common interest in principles, and the necessities of travel and commerce bring about a closer association in practice. In the experience of the individual these relationships can as a practical matter be but little more than points of contact between the medicine and hygiene of these distant zones. As we look more closely we find firm bonds of union between the medical problems of lands that lie always in the summer sunshine and those accustomed to perpetual fog.

Little did the physiologists dream that a fundamental discovery in nutrition would originate in the small island of Java. Nature has lavishly endowed this island of Porto Rico. Its stimulating freshness lends inspiration for work and for ideas. The richness of your chosen subject defends it against monotony. Here it will be easy and natural to follow the precept recommended by Professor Williams at the founding of the Sigma Xi. He said in part, "In kindling your torches we bid you light them at the brightest living altars of learning and not at the smouldering embers of dead issues." As the years slip by, many students will look back with satisfaction on the incentive received in this favored place.

It is a matter of importance to the scientific world that the people of Porto Rico have achieved a definite consciousness of their responsibilities in science. The leaders in the development of this island are not satisfied merely with commercial progress. The activities of your investigators have given Porto Rico a place of leadership in science in tropical America. This is an enviable position which in time will be challenged by your neighbors in friendly rivalry. But with the foundation of past achievement and with mature plans and preparations for the future, it is a leadership which Porto Rico is in a position to maintain.

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NOMINA CONSERVANDA

AN article in the November, 1926, issue of the Proceedings of the Entomological Society of Washington by Mr. W. L. McAtee, entitled "Nomina Conservanda from the Standpoint of the Taxonomist," shows such an astonishing failure to grasp the relations that exist between nomenclature and taxonomy that I can not permit it to pass without protest.

"Why do scientists," queries Mr. McAtee, "most of whom presumably are evolutionists, attempt to block development in taxonomy while constantly accepting change in other fields both within and without the domain of science?" The inference is that adherents of the idea of nomina conservanda must answer to the charge of being obstructionists. The answer is that they do not attempt to block development in taxonomy; no such charge could be made by one who understood the function of the rules of nomenclature.

Taxonomy concerns itself with the classification of organisms, and modern taxonomists accept the principle that classification must express, as nearly as may be, organic relationship. In a word, taxonomy must as closely approximate the phylogeny of organisms as the state of our knowledge makes possible. It is therefore a science, and, like all sciences, is dependent upon our knowledge of facts and our interpretation of the significance of the facts we know. It would be intolerable to have it codified or ruled upon by any group of individuals, however organized, for it is the bounden duty of every man of science to make known the facts of science as he perceives them,