would be different from those observed in guinea-pigs and rabbits.

As already stated, rabbits and guinea-pigs passively immunized suffer from cerebral tetanus when the toxin is injected into the brain. The question arises: Is this also the case in animals whose immunity is active? This question is very pertinent, because many observers have advanced the opinion that antitoxin is elaborated by the cells which are susceptible to the toxin; that each symptom which is influenced by the toxin responds by producing antitoxin. Two rabbits immunized against tetanus, and possessing an antitoxic serum, were found to be insusceptible to subcutaneous injection of 12 c.c. of tetanus toxin, which was fatal in a dose of 2 c.c. in the case of a susceptible rabbit. One-tenth c.c. of toxin was introduced into the brain of the immune rabbits at the same time as into a normal control animal. On the following day all the rabbits presented signs of cerebral tetanus. The control animal died during the course of the day; the immunized animals died with typic symptoms, one after seventeen days, the other after twenty days. These experiments demonstrate that the nerve-cells of an actively immune rabbit are sensitive to the toxin and that it becomes very difficult to understand how these nerve-cells could prepare an antitoxin. It would appear, on the other hand, as if during the course of the immunization the nerve-cells had never come into contact with the toxin.

Certain animals are naturally immune to various microbic toxins. The rat, for instance, does not suffer from a dose of diphtheria poison which is sufficient to kill several rabbits. One-tenth c.c. of diphtheria toxin was injected into the brain substance of a rat, and death followed on the third day, hence the brain of the rat is also sensitive to the poison, and as it does not die when large quantities of toxin are injected subcutaneously, it follows that the poison does not then reach the brain but is arrested in some part of the body. The natural immunity of the rat does not depend upon the resistance of the nerve-cells against diphtheria, but upon some other property of the body. ROUX and BORREL showed that similar facts obtained with regard to the rabbit as to morphin, toward which this animal is looked upon as being insusceptible. In other words, the nerve-cells of the rabbit are not insensitive to morphin.

All the facts go to show that in acquired as well as in natural immunity against certain poisons, the immunity does not depend upon insusceptibility of the nerve-cells, at least not of those of the brain. The poisons are retained by other cells, which exercise a protective rôle and probably manufacture antitoxins. ROUX and BORREL ask: Where are those cells? and answer that perhaps phagocytic cells are capable of also destroying the poisons produced by microbes. They are not ready to insist upon this, but it would seem to them that the questions of immunity against the microbes and of immunity against the toxins will ultimately receive similar solutions.

CREOSOTE NOT A SPECIFIC.

The drug, creosote, has become so inseparably connected in history, in clinical experience, and in present day teachings, with the disease of pulmonary tuberculosis, that its acceptance as a specific becomes a natural consequence. Such empiricism, however, is fatal to the scientific treatment of tuberculosis even though creosote does seem to be indicated in every case. Creosote probably exercises its curative action in the lung because of its physiologic property of stimulating the bronchial mucosa, where it is eliminated, and its action as an expectorant; not as a specific for the disease of tuberculosis.

The healthy human body is not susceptible to infection by the germs of tuberculosis because a vital resistance of the tissues prevents the development of any bacteria that may gain entrance, and those bacteria that develop in a lung to cause a miliary tuberculosis, do so because—from malnutrition, lack of exercise, the inhalation of sewer gas or dust particles, an ordinary cold or a previous attack of pneumonia—the lung tissues possess a diminished resistance and afford a suitable soil for their development. A diminished resistance is probably present in the bronchial mucosa, of which there is a vast expanse in the lung, and when an infection is once established at a given point it is clear that the adjacent mucosa will afford slight hindrance to an extension of the tubercular process. The object of our treatment should be to raise the vital resistance of these adjacent cells of the bronchial mucosa, in order that they may resist an extension of the disease and thus limit the tubercular inflammation to a single spot, which should mean an eradication of the disease. Creosote *does* improve the resisting power of each cell, because it stimulates it to a more perfect metabolism, and if to this favorable condition there are added an abundance of good food to supply these cells with nourishment, and plenty of fresh air to supply the blood with good red oxyhemoglobin that may further assist their metabolic processes, the patient is placed under the best conditions for recovery. The creosote, it will be observed, does not exercise an antibacillary action, nor does it even diminish the virulence of the tubercle bacillus, but as a treatment for tuberculosis it is the most satisfactory agent that we know, since it succeeds in establishing a condition of health in the mucosa that will help nature to overcome the disease.

CORRESPONDENCE.

The Jugglery of Statistics. A Reply to an Absurd Editorial.

WASHINGTON, D. C., Oct. 22, 1898.

To the Editor:—I desire to invite attention to the editorial comment in the *Philadelphia Medical Journal* of Oct. 15, 1898, under the caption: "The One Fact the Whitewashing Committee Must Explain." I would not have troubled you with this communication but that I have regarded the said

journal as a paper of value and of increasing importance whose editorial utterances should have weight. The article to which I take exception as unworthy of the columns in which it appeared, presents to its readers the large ratio of deaths from disease in the Spanish War, as the one fact to be explained, "the crux of the entire discussion," and manifestly elated with his discovery, the editorial writer exclaims in a burst of triumph: "Explain us that right thoroughly and without equivocation, Honorable Sirs," etc. This exuberance of diction from the editorial pen manifests that the writer sent his manuscript to the printer while dazzled by the brilliance of his discovery and without giving it that careful consideration which is needful for the reputation of the editorial columns. I am constrained to look upon this as the explanation of the article. Doctors are generally credited with being weak on figures, and my experience shows that they are not strong in questions of ratios, making honorable exception in this regard of the health officers who deal systematically with medical statistics. I must charitably suppose that the writer in question participates in the general professional failing because I am unwilling to believe that he knowingly presented figures which did not sustain his conclusions, trusting that the latter would be accepted by readers who were unfamiliar with his arithmetical premises. These premises are that: There were killed in the War of the Rebellion 46,874, died of wounds 39,715, died of disease 172,907; and in the recent war: killed 289, died of wounds 65 and died of disease 2565. These figures are official, and I have no exception to take to them. But he continues: "If we reduce these figures to percentages based on the best estimates of the numbers of soldiers participating, and place figures side by side we find:

"Causes of Deaths.	Civil War.	Spanish War.
Killed	18.1%	9.6%
Died of Wounds	15.3%	2.3%
Died of Disease	66.6%	88.1%."

Now in the calculation of these ratios no consideration is given, as claimed by the editorial, to "the best estimates of soldiers participating" nor, I may add, to the relative duration of the two wars. They express merely the relative proportion of deaths from battle casualties and disease in the constitution of the total mortality. Yet from these data the editorial writer, knowingly or unknowingly, concludes that the 88.1 per cent. of deaths from disease in the Spanish War, as compared with the 66.6 per cent. in the Civil War is a point of the highest importance. "There can be no possible question" he says "that the general mortality from disease has been greatly and indubitably lessened in the last thirty-three years. If so, of course the percentage mortality from disease in the Spanish War should be decidedly lower than in the war of the Rebellion. Instead of this it has been decidedly higher," and the members of the Presidential Investigating Commission are called upon to take note that the medical profession regards the cause of this as the main thing that is before them for explanation.

The absurdity of this may be well illustrated by adding to the statistics and ratios given above those derived from the reports of the engagement of Admiral Dewey's fleet on May 1, 1898, at Manila. Dewey's fight lasted only one day. The figures given for the Spanish War covered a period of five months and those for the Civil War a period of five years and two months; but as the element of time does not enter into the calculations of the ratios criticised this element need not be considered in the engagement at Manila. Here we find that no man was killed, no man died of wounds received, but that one man, an engineer officer, succumbed to heat exhaustion or insolation. Stating these figures as percentages of the total mortality we find: killed 0 per cent. died of wounds 0 per cent, died of disease 100 per cent, as compared with 88.1 per cent. in the Spanish War and with 66.6 per cent. in the War of the

Rebellion. If it is needful for the Honorable Sirs to give such high consideration to the greater death-rate from disease as compared with the deaths from wounds and deaths on the battle-field in the Spanish War considered as a whole, how serious is the consideration they ought to give to this frightful rate of mortality from disease as compared with that from battle casualties in the Bay of Manila.

It was my intention in beginning this letter to state the conclusions that are really admissible from the figures and ratios presented by the *Philadelphia Medical Journal*, but the pressure of current work at this time is very great, and I conceive that I have effected my purpose by directing attention to the unsubstantial basis on which the argument of the editorial writer was constructed.

Respectfully yours,

CHAS. SMART,

Deputy Surgeon-General, U. S. Army.

Twins with Complications.

LAUREL, IOWA, Oct. 18, 1898.

To the Editor:—October 2 I was called to attend Mrs. R. in confinement and found myself confronted with a number of unpleasant conditions, not the least of which was a morbid fear on my patient's part that she would not be able to give birth to her child, or children as proved to be the case, she having been told at her previous confinement, about a year ago, when she gave birth to triplets, all still-born, that she would not live through another confinement. She was a German woman, large and strongly built, about 40 years of age and had given birth to five children, the oldest 14 years of age. She said she had suffered a great deal at all her previous confinements and that they had been slow, lasting from twenty-four to forty-eight hours. She complained of being "deathly sick" and felt as if she was going to vomit.

From the size and shape of the abdomen, I concluded I had a case of multiple pregnancy, but the sound of but one fetal heart could be heard on auscultation. Digital examination revealed an irregularly contracted vagina, due, no doubt, to the healing of her previous lacerations. The os, however, was fully dilated, and I found a foot protruding, which as a strong pain now came on, I grasped, making slight traction and with some manipulation succeeded in bringing down the other foot. During the next pain the body was born and the head, with chin to the front, was engaged in the superior strait. Now, when good pains were most needed, the patient "gave up" and wished me to leave her to die, as she had been told that she must. Finally, after a delay (which proved fatal to the child) she was delivered of a large girl weighing eight pounds. Another child could be outlined high up in the fundus uteri, and digital examination showed a "cone-shaped" bag of water through the os. The fetal parts could not be made out. The patient gave up entirely, and I let her rest an hour and a half, when, as the pains did not come on, I gave her 15 m. fl. ext. ergot and in a short time they again became quite strong and regular. Another digital examination showed that the child was not presenting, hindered, I think by the low attachment of the placenta. But by making slight downward pressure from above during a pain, I finally succeeded in getting the parts to engage. The sac, which was very dense, had to be ruptured, when an unusually large amount of the liquor amnii came away and I found a foot protruding almost exactly as did the first one. The child, a boy, also weighed eight pounds and was alive, but minus the penis. Where the penis should have been there was a slight elevation about one-eighth of an inch above the surface, with an opening at the apex, through which the child urinates. The placenta, which was delivered in about thirty minutes, also seemed to share in the general anomalous condition. It consisted of two distinct placentæ, seemingly enveloped in a common sac. They were connected at their sides by a strong fibrous band and each had