sied because, by serologic theory, immunity is simple. The idea has an anthropocentric or teleologic appeal. It satisfies our curiosities. We like to think that everything is designed for our own good. Investigators work on serologic problems, report serologic observations, and then draw immunologic conclusions. Serology is laboratory stuff, but immunology is Big Time. Experts, on whom we must rely for authoritative information, have an obligation. Bacteriologists have rested their conclusions on a misleading argument for so long that they fool even themselves and each other.

The simple thought that antigens and antibodies explain immunity leads to serious errors with vaccines and therapeutic antisera. The persistence of searches for good vaccines is a tribute to human optimism. The persistence of the sophistry that anything which stimulates the production of antibodies is a vaccine is remarkable, but it is no tribute. The introduction of an antigen into an animal necessarily, by definition, stimulates the production of antibodies. The literature is full of writers who, after introducing evident antigens, express surprise over the discovery of antibodies. To argue that a vaccine produces a resistance to infection because it has stimulated the production of antibodies confounds theory, speculation, and fact. It would be equally logical to account for the immunity by the sore arm produced and to vaccinate with a club. Thousands of purported vaccines have failed immunologically; all stimulated the production of antibodies. No vaccine could fail if we accept the fallacious confusion of immunology and serology. Vaccines do fail, often, immunologically.

Although the serum of any animal which has been inoculated with antigen must contain antibodies, the antiserum may have no immunologic value. There is often no possible relation to immunity. Are we to suppose that the inoculation of a person with the organisms which cause fire blight of pear trees would be an immunizing process? There would be antibodies.

Here is a specific example. Typhoid vaccine, one of the few successful vaccines, confers a significant resistance. Those who are vaccinated rarely have typhoid fever after ordinary exposure. This is demonstrated by the rates of infection in vaccinated and in unvaccinated persons in regions in which the disease is common. An attack of typhoid fever also confers an adequate immunity in those who recover. There is exposure to the antigenic components of the bacillus of typhoid fever with either infection or vaccination. Therefore, antibodies are formed. Should we yield to the temptation to argue that the immunity is *caused* by the antibodies? The factual observation is *only* that in this case antibodies and immunity usually coincide. But apparently serum of high antibody content is not of immunizing value, and quite surely persons with very little antibody after vaccination are as immune as those with much. We may also argue that in any infection permitting absorption of antigen there are antibodies, but in only a few do we have immunity or vaccines. That immunity and antibodies coincide proves nothing about causal relationship. Other explanations of the immunity have a stronger claim.

We are reporting an experiment. These ideas have been tried consistently on many academic and professional students. It is no trouble to lecture without mixing serology and immunology. It antagonizes no one, and it disturbs no one's right to decide when he wishes to relate immunity and serology. None of us working with our classes uses the inappropriate terms, such as "immunizing" a rabbit; each catches these phrases when others use them. There is no dogma in this. The dogma is in the enforced relationship between immunity and serology, not in the freedom which we should like to promote.

The origin of the confusion does not excuse it. The reactions between toxins, which are also antigens, and their antibodies happen to result in the neutralization of the poisonous properties of the toxins. This early observation, antedating other serologic observations, combines an immunologic concept and a serologic observation. Serologic principles arose from this observation and phenomena observed later. Introduce the white of an egg into a rabbit and there is stimulated the formation of antibodies which will precipitate the white of an egg. There is no immunologic thought whatever in this. Serology has expanded to a useful and moderately exact branch of science. It is incumbent upon us to keep it separate from immunity except when the connection is clear and irrefutable. Only after immunologic thoughts and serologic facts are each separately understood can possible relationships be examined. *First,* there is the immunologic concept concerned. *Second,* there are the observed serologic phenomena. *Third,* there may be considered combinations. Occasionally, and not often, immunologic phenomena appear to have serologic mechanisms.

We plead for separation of the concept of immunity and the phenomena of serology. Separation leads to a more precise expression of facts and arguments and to fewer fallacious deductions from our observations. Separation can be done with language, but so deeply is the error planted that bacteriologists can get out of it only by prolonged effort. We owe this effort to students and to others who wish to grasp something of scientific thought. We owe it to science, built upon reliance in the observations and logic of specialists in each component of science.

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A Comparison of the Total Leucocyte Count in the Heart Blood and Peripheral Blood of the Rat

In a recent issue of *Science* (April 30, p. 447) Quimby, Saxon, and Goff reported that the leucocyte count of the heart blood in the rat is only about one-fourth that of the peripheral blood (heart blood = 6,425 leucocytes/mm^3; tail blood = 23,810). In view of the large number of experiments which have been based on leucocyte counts of tail blood, this report seemed worth checking.

Twenty-seven male albino rats (Wistar strain) weighing 200 gm each and 8 male rats of mixed strains weighing 200–400 gm each were used. The Wistar rats were lightly anesthetized with ether and 0.25 ml of blood obtained by cardiac puncture, using a 27-gage needle. Immediately afterward, peripheral blood was obtained by nicking one of the dorsal tail veins with a sharp razor blade and collecting the free-flowing blood.

47–49) have demonstrated that restraint induces a lymphocytopenia in mice. The animals were kept at all times in an air-conditioned room at a temperature of 78 ± 3° F.

Total leucocyte counts were made by standard methods, using Bureau of Standards certified equipment. Monocytes were not distinguished from lymphocytes, nor were eosinophils and basophils from the neutrophils.

TABLE 1

LEUCOCYTE COUNTS OF HEART BLOOD AND PERIPHERAL BLOOD OF THE RAT

Animals	Source of blood	Total leucocytes/mm³	Neutrophils/mm³	Lymphocytes/mm³	
				Normal	After adrenal-cortical extract
18 Wistar rats	Heart	28,055 ± 782*	6,892 ± 569	21,163 ± 568	12,693 ± 671
	Tail	28,166 ± 773	5,432 ± 364	22,732 ± 550	13,582 ± 731
9 Wistar rats				Normal	After saline
	Heart	25,044 ± 624	4,488 ± 347	20,556 ± 872	19,467 ± 612
	Tail	25,122 ± 830	4,272 ± 383	20,851 ± 959	20,489 ± 347
4 mongrel rats		Killed by blow on head			
	Heart	21,800 ± 374	3,706 ± 299	18,094 ± 397	
	Tail	24,400 ± 867	3,416 ± 597	20,984 ± 540	
4 mongrel rats		Cardiac puncture on exposed heart			
	Heart	21,276 ± 619	3,627 ± 774	17,659 ± 246	
	Tail	22,270 ± 772	3,194 ± 611	19,576 ± 906	

* Standard error $= \pm\sqrt{\frac{\Sigma d^2}{n(n-1)}}$.

In order to determine whether heart blood and peripheral blood lymphocyte counts are affected equally by agents known to alter the lymphocyte count, 18 of the Wistar rats were given 2-ml subcutaneous injections of aqueous adrenal-cortical extract (Wilson), and the other 9 were given 2-ml subcutaneous injections of saline.

Four of the mongrel rats were killed by a blow on the head after the manner of Quimby, Saxon, and Goff and samples of heart and tail blood obtained. The other 4 mongrel rats were deeply anesthetized with ether, the heart was exposed, and blood samples were obtained directly from the beating ventricle.

Extreme care was observed in handling the animals, since Elmadjian and Pincus (*Endocrinology*, 1945, 37,

All of the data are presented in Table 1. It is apparent that there was not a statistically significant difference between the leucocyte counts of the heart blood and tail blood. The lymphocytopenia following injection of adrenal-cortical extract was of the same order of magnitude as that reported by Dougherty and White (*Endocrinology*, 1944, 35, 1–44) and was of comparable magnitude in heart blood and tail blood.

We are unable to explain the difference between our results and those of Quimby, Saxon, and Goff.

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Appeal for Scientific Literature for Austria

During a recent visit to Vienna to attend the 50th Anniversary Meeting of the Austrian Chemical Society, I learned at first hand something about the terrific problems being faced by the U. S. Information Center there. . . . Many of the libraries in Austria have either been destroyed or dispersed. Technical literature was, of course, not available during the war, and it is only returning very slowly now because of the usual difficulties of foreign exchange and world shortages. The U. S. Information Center has accumulated a small technical library, and it receives quite a good selection of current technical journals, but usually only one copy of each. In many instances this is the only copy of the journal in Austria. It takes little imagination to visualize what this means to research workers and teachers. . . . All of us know the past contributions of the Austrian scientists. They can and will contribute much more in the future. Right now they need technical literature of all kinds, standard works for reference and teaching, and current literature for research background.