

# JAPANESE CULTURE AND THE PROBLEM OF MODERN SCIENCE

JAMES BARTHOLOMEW  
*Ohio State University*

## I. SCIENCE, CULTURE AND SOCIAL STRUCTURE

### *Japanese Perspectives*

On November 22, 1901 professors and students of the Tokyo Imperial University Medical School held exercises honoring Dr. Erwin Baelz on the twenty-fifth anniversary of his appointment to the faculty. Seemingly ignoring the purely ceremonial nature of the occasion, the German physician chose to unburden some of his accumulated frustrations in replying to the greetings of his Japanese colleagues and commented as follows on the state of science in Japan at that time:

It seems to me that in Japan erroneous conceptions about the origin and nature of Western science are widely prevalent. It is regarded as a machine which can turn out so much work every year....which can without further ado be transported from the West to any other part of the world, there to continue its labors. This is a great mistake. The Western scientific world is not a piece of machinery, but an organism and like every other organism, if it is to thrive, it needs a particular climate, a particular atmosphere.<sup>1</sup>

Baelz especially criticized what he thought was a tendency of the Japanese to seek the "latest acquisitions of science, instead of studying the spirit which made the acquisitions possible."<sup>2</sup>

The retiring German professor was saying, in effect, that science in Japan despite thirty years of sustained growth had remained epiphenomenal and largely uncreative, continuing to rest lightly on the surface of a culture indifferent to its values and impervious to its influences. A historian taking note of these strictures several decades later might well be disposed to regard them as the angry words of a man displeased at being eased out of an agreeable position to make room for a Japanese. Indeed, they may have been just that. There is, however, one very important reason for treating the German scientist's remarks as something more than an expression of personal resentment. That is the remarkable extent to which succeeding generations of Japanese scientists have themselves bemoaned the supposed lack of creativity in prewar science and its "peculiar" relationship to society and culture.

Modern critics have leveled four principal accusations against prewar science, of which one involves the old issue of "copying." The exponents of this thesis, who stress the key role of the government in the promotion of science, have claimed that high-level government officials had little interest in scientific research and preferred to promote science by importing knowledge, techniques and skills from abroad. The government's awareness of science in the words of one scientist, was dominated by an "illusion of copying everything;"<sup>3</sup> and to that extent it failed to understand science correctly or support it adequately.<sup>4</sup>

Others assert that traditional Japanese ways of thinking were so irrational as seriously to inhibit the development of scientific thought. Dr. Nakamura Hajime, a leading exponent of this thesis, has said certain features of the Japanese language encouraged irrationality. He observes, for instance, that Japanese has no established method for creating abstract nouns, lacks the infinitive form of the verb and the relative pronoun "which," and tolerates frequent changes from one grammatical construction to another within the same sentence.<sup>5</sup> Others who emphasize the influence of "irrationalism" attribute its persistence to attitudes toward nature,<sup>6</sup> tradition-inclined ideological movements in the Meiji Period (1868-1912)<sup>7</sup> and an excessive concern with practical social and political needs by early Japanese scientists which obscured their awareness of the need for intellectual reforms.<sup>8</sup>

Yet a third group claims the "peculiar" nature of the institutionalization of Japanese science results from the historical fact of its having entered the country "separated from industry." One interpretation of this view, expressed by the Japanese Communist Party and Kobe University physics professor, Dr. Yuasa Mitsutomo, among others, states that science's lack of a well-developed, independent base in Japan's private industry made it excessively dependent on support by the government and thus vulnerable to various kinds of state "interference."<sup>9</sup> Yuasa suggests such interference was possible because Japanese scientists had no tradition of resisting authority as European scientists did. But other critics who stress the relationship of science to industry as an important factor in its later development say Japanese science in any event lacked a tradition of scientific thought sufficiently powerful to resist the government's "semi-feudal absolutism" successfully.<sup>10</sup> They attribute the weakness of scientific thought in Japan to the timing of the Meiji Restoration in world history on the assumption that science and technology are largely a function of economic development. In their view, consequently, because Japanese capitalism in 1868 was so backward compared to European capitalism, the state had to intervene on its behalf and in doing so necessarily caused technology and science to become

dependent on itself as well. Such dependence is then said to have "distorted" their development.

Finally, it is held that either the formal organizational arrangements<sup>11</sup> within which science existed or the behavior of scientists themselves were responsible for certain alleged shortcomings. In particular senior scientists are charged with perpetuating a "feudalistic apprentice system" of recruitment and socialization in dealing with students and younger colleagues, exercising such control over them that free exchange of views within the research group and cooperation with members of different groups were obstructed or even precluded.<sup>12</sup> Whether these practices occurred with greater frequency than could have been the case in Western countries is not an issue for these critics as all assume that to be true. Disagreement focuses rather on the question of whether the motivations of prewar scientists were "improper" to begin with, or alternatively, whether these motivations became deviant because certain formal organizational arrangements channeled them in certain ways. Considering the large number who believe the university chair system (*koza seido*) had adverse effects on research, it appears that most favor the latter possibility.<sup>13</sup> Yet even among those attributing principal blame to organization, ambivalent tendencies toward the other point of view persist. This is seen, for instance, in the writings of Dr. Sakata Shoichi, a leading physicist at Nagoya University who has been among the most influential critics.

In 1947 Sakata published an essay called "Research and Organization" in which he criticized senior Japanese scientists for allegedly obstructing cooperation and criticism but claimed the chair system was ultimately responsible for these problems. However, he went on to make the interesting claim that a new form of organization introduced at Nagoya University had substantially reduced such practices in his department.<sup>14</sup> The innovation to which he referred was the laboratory council system proposed originally in 1939 by the British physicist, John Desmond Bernal.<sup>15</sup> At Sakata's instigation a laboratory council was established in the Nagoya University Physics Department and every researcher given an equal voice in it, diminished but slightly by a democratically elected chairman. The stated objective was to encourage a "democratic spirit" among the researchers and this had largely been achieved after a one-year trial, according to Sakata, as even the chairman's ideas had received their share of criticism from the younger physicists.<sup>16</sup>

Despite the optimism of these early claims it should be noted that later participant-observers took a much less sanguine view of the new reform than Sakata had done. In 1963 two of his colleagues, Dr. Otsuki Shoichiro and Dr. Nojima Tokukichi described the laboratory council system as follows:

Whenever the [social and cultural] forces to support it were lacking, the new system even became a means for concealing the contradictions within the old system. One may question whether the [formal] signs of democratization actually brought into being conditions encouraging either greater freedom for the individual researcher to develop his abilities or more effective cooperation among researchers. The laboratory council system retains within itself the perpetual danger of degenerating into the chair system compared to which it represents only a change in form.<sup>17</sup>

Sakata himself had been aware of this possibility, however; for in the 1947 essay he wrote: "Democratization of research organizations will be extraordinarily difficult without democratization of the entire society." While not wishing to pursue this point just yet, it may be suggested that in this case at least, changes in organizational arrangements apparently did not have the impact on interpersonal relations which some sociological theory would presumably have predicted.

When each of the preceding arguments is carefully analyzed three basic conclusions emerge. Two are reasonably self-evident and are universally accepted among the Japanese critics mentioned here: that science in Japan was not very creative in the prewar period; and that science remained culturally epiphenomenal in the sense that certain social and cultural values associated with its presence elsewhere were not accepted in Japan within either the scientific community or the general society. The third conclusion, by contrast, is much less obvious and is not universally shared. It would hold that science, so far as it was epiphenomenal remained uncreative; and that to the extent it may have been uncreative, remained epiphenomenal. Such a conclusion necessarily follows from arguments advanced by the first and fourth groups of critics but is not implicit in those of the other two, though many of them do believe Japanese science was uncreative nevertheless. This is a point of some importance to which the discussion will return later on.

The question remains, however, whether any of these conclusions should be accepted. It cannot be conceded at the outset, for instance, that Japanese science was epiphenomenal in this sense. Such a proposition, if true, could only be established by certain comparisons between science in Japan and science elsewhere. Accordingly, the question of whether Japanese science in the prewar period was really epiphenomenal or not will be the first to be taken up in the following discussion. Later it will be suggested that science in prewar Japan was in fact epiphenomenal, a conclusion which leads to the second question: why? The third question, then, will focus briefly on creativity. No attempt will be made to evaluate prewar science either in general or in any of its specific manifestations. Rather, the question

considered will be of the form, to the extent Japanese science was uncreative in some demonstrable way, could any of the arguments described earlier contribute substantially to an acceptable explanation?

These questions will be considered within the framework of a specific case study, the institutionalization and development of the biomedical sciences in Japan, especially bacteriology, during the latter part of the nineteenth century and the first two decades of the twentieth. In order to establish certain points in the argument specific comparisons will be made with the situation in Germany during the same period. But before proceeding directly to the case study it is necessary first to discuss in greater detail several aspects of the charges leveled against prewar science by its critics and to point up their relationship to a more comprehensive body of sociological theory.

### *The Functionalist Perspective*

It is often argued, as indeed most of the Japanese critics cited here do, that scientific research requires the maintenance of certain *values*<sup>18</sup> in whose absence science cannot function. In accord with this view modern functionalist sociology has defined science as a social system integrated by these value orientations and even claimed their maintenance within the scientific community explains the persistence of science through time.<sup>19</sup> The best-known definition of these value-orientations is probably Professor Talcott Parsons' pattern-variable scheme in which science is said to require adherence to the values of universalism, achievement, functional specificity, affective neutrality and collectivity orientation.<sup>20</sup> Moreover, Parsons would say, the practice of science must by *definition* be characterized by adherence to these values at three levels of generality. In decreasing order these may be described as the level of philosophical or epistemological orientations, the level of institutional or organizational norms and, at the lowest level, personal value commitments.<sup>21</sup> Within this analytical framework, of course, the term "institutionalization" of science refers primarily to inculcation of the necessary values at all three levels.<sup>22</sup>

This approach to science has certain implications for the view functionalists adopt concerning the historical relationship of science to culture and social structure. If it is true, in other words, that science cannot flourish without certain values which, for the sake of this discussion are assumed to be at least similar to the above-mentioned pattern-variables, one can only conclude that in the culture or society "receiving" it, science in varying degrees will replace or destroy those values which conflict with its own. From a functionalist point of view science could be said to produce "standardized

contexts of experience”<sup>23</sup> in every society or culture where it becomes established, a hypothesis which Kenneth Downey has termed the Destruction Theory of the institutionalization of science.<sup>24</sup>

Arguments advanced by two groups of Japanese critics seem to define science precisely as functional analysis has done and thus subscribe necessarily to the Destruction Theory of its relationship to culture and social structure. Their contention in essence amounts to saying that science “ought” to have destroyed certain aspects of the traditional culture and social structure but failed to do so because certain basic values associated with science in other countries were not accepted in Japan. Consequently, the process by which science became established (not “institutionalized”) there was somehow improper, deviant or peculiar. The implication of the copying thesis is that this occurred because the government’s policies precluded the necessary socialization in research. The argument about formal organization, on the other hand, suggests the chair system was to blame because it gave senior professors too much power and restricted horizontal movement between research groups; while the argument about behavior merely says the requisite values were not institutionalized without providing any explanation.

In response, this paper will endeavor to show that these arguments are incorrect, that the assumptions on which they are based are dubious and that they are incompatible with the empirical evidence.

Critics emphasizing the importance of irrationalism, language and historical factors, on the other hand, seem to accept quite a different set of theoretical assumptions while reaching similar conclusions about the epiphenomenality and uncreativity of prewar science. Of strategic importance for all of their claims is a definition of science as *ideas* rather than one based on *values*. Moreover, there is an implicit assumption that ideas under certain conditions may be regarded as independent variables in socio-historical analysis. Thus some areas of Japanese science might have been uncreative in their view because of linguistic inadequacies while science in general could have remained epiphenomenal due to certain historical factors or failures of *ideology*.

The position taken in regard to these arguments will be that they are essentially valid in so far as they lead to a more accurate empirical description of Japanese science in its relations to culture and social structure and help substantially to answer the three questions posed earlier. While not necessarily accepting the conclusions they reach about creativity, their basic assumptions seem potentially compatible with a different conclusion and at the same time capable of providing valuable clues to any potential answer to the third question.

It may be immediately objected, of course, that functionalism also affords *ideas* a high degree of autonomy or independent variability in the creation, though not the application, of other scientific ideas. Indeed, Parsons said precisely that in a 1938 essay.<sup>25</sup> In practice, however, functionalists have invariably proceeded from institutional structures to cognitive ones in their analysis of science, explaining the creation of scientific ideas as a function of certain value orientations and formal organizational arrangements, a procedure which seemingly denies the very autonomy with which scientific ideas are said to be endowed.<sup>26</sup>

Functionalism's explicit assumption that values can be regarded as non-problematical and given appears to be the principal source of this contradiction. In Professor Parson's words: "The whole nature of the theory of action in general is...such that precisely with respect to variability of structure, patterns of value orientation as the focus of institutionalization must play a crucial role."<sup>27</sup> Moreover, functional analysis displays notable inconsistency in its use of the value-orientation concept. In sharp contrast to the levels of philosophical or epistemological orientation and institutions where values or norms are treated as the independent variable, at the level of actual human behavior values become a product of interpersonal relations which, in turn, are partially a product of formal organizational arrangements — being thus regarded as a dependent variable. (Although the concept of functional equivalents resolves this contradiction in certain cases, it is not relevant to the particular issue this paper will raise — whether certain allegedly fundamental values are needed at all below the highest level of generality). Nor is such confusion merely the result of using one term where another should have been introduced; since the pattern-variable scheme by *definition* covers all three levels. This assumption, it is argued, leads to conclusions about the relationship of science to culture and social structure which are unsound in general. Moreover, in the Japanese case particularly it will be asserted that functionalist assumptions produce conclusions about the relationship of science to culture which are demonstrably inconsistent with the empirical evidence.

How can the validity of these arguments be demonstrated? Because of their basic assumption that values are given and non-problematical at all levels of generality, functionalists have typically stressed the role of formal organizational arrangements in either facilitating or inhibiting the ability of individuals to act in accord with the values of science. While the possibility that more than one behavior pattern can uphold the necessary values is not only admitted but even insisted upon, there are clearly limits to the amount of possible variation. Ultimate failure to uphold the values of science, of

course, means by definition that creative science cannot be done.

Among the most provocative studies incorporating functionalist assumptions are those of Joseph Ben-David and Awraham Zloczower concerning the biomedical sciences in nineteenth-century Germany. They have argued in various papers<sup>28</sup> that the formal organization of German academic medicine exercised an inhibitory influence on the productivity of medical research there and did so by encouraging inappropriate patterns of behavior among scientists. It is interesting to note that the eminent Japanese bacteriologist, Kitasato Shibasaburo (1853-1931), trained in Germany at that time and well acquainted with conditions there, made precisely the same argument with reference to the Tokyo University Medical School (and the institutions modeled on it) in Japan, claiming that similar patterns of behavior among biomedical scientists in the two countries resulted from the same cause. The studies of Ben-David and Zloczower concerning Germany and Kitasato's arguments concerning Japan therefore permit a comparative analysis of formal organizations and of patterns of behavior among biomedical scientists in each of the two countries within the organizational context. Both because Kitasato's arguments focused on bacteriology and for reasons to be explained momentarily the analysis will be confined almost entirely to that discipline.

Based on this analysis two arguments will be advanced. First of all, formal organizational arrangements concerning science in Japan were, if anything, considerably more flexible than those in Germany and ought to have elicited very different patterns of behavior than those which actually existed if the basic assumptions of functional analysis were correct. Their failure to do so, of course, means that different value orientations existed in Japan than existed in Germany at both the institutional and personal levels. From a functionalist perspective this conclusion means that science in Japan was not only culturally epiphenomenal in the sense defined earlier but by definition was not science at all! However, and this will be the second argument, the research done by the Japanese scientists in question seems to have suffered not at all from the effects of whatever values the biomedical science community maintained. In fact, the claim will be made that the values existing in the Japanese case were at least as congenial to science as those which functional analysis has claimed are essential.

Procedurally, the following section will present the case for the "epiphenomenality" of prewar Japanese science, that is, that it emphasized certain strategic values markedly different from those of German science; while the reasons for this, together with the creativity problem mentioned earlier, will be discussed in the last section.



## II. THE SOCIAL RELATIONS OF BACTERIOLOGY IN GERMANY AND JAPAN

### *Government and the Biomedical Sciences*

Aside from Kitasato's arguments and the researches of Ben-David and Zloczower, bacteriology was made the focus of the case study for two other reasons. A principal aim of this paper is to demonstrate that creative scientific research can be done without adherence to certain values deemed indispensable by functional analysis at all but the highest level of generality. Thus, it was necessary to investigate an area of science in which important work was done by the Japanese; and bacteriology, together with the closely related field of pathology, meets this condition. Another reason has to do with the nature of bacteriology itself. Because it studies the relationship of specific microorganisms to specific diseases, this science has always required an intimate relation between the research and the clinical aspects of medicine. Robert Koch, who was largely responsible for its establishment as a full-fledged science, began his medical career not as a professor in a laboratory but as the District Physician at Wollstein in East Prussia; and Professor Ben-David rightly stresses the importance of the clinical experience for Koch's success as a bacteriologist.<sup>29</sup> Referring to this characteristic of bacteriology, Dr. Abraham Flexner once described it as a kind of "bridge" between the two branches of medical science which in nineteenth-century Germany were gradually driven further apart through the effects of specialization and professionalization.<sup>30</sup> One may reasonably assume, in consequence, that a science of this kind would be especially vulnerable to the effects of changing organizational arrangements on its overall effectiveness.

As bacteriology was the field in which Japanese scientists made the largest number of important contributions to knowledge during this early period it is not surprising to discover that it was among those most securely institutionalized<sup>31</sup> and among the first to become so. While other sciences had but a single institutional base in Japan before 1900, bacteriology had two. In Germany this field emerged as a distinct, theoretically-based discipline within medical science in the mid-1870's due to Koch's pioneering studies of anthrax and particularly his discovery of methods for attaining bacterial cultures in a pure form. Only six years later, in 1881, this new science was being taught regularly at the Tokyo University Medical School and by 1884 had been recognized there by appointment of a full-time lecturer.<sup>32</sup> Interestingly enough, this first faculty appointment was made in bacteriology at Tokyo University a full year before Koch himself became professor at Berlin.<sup>33</sup>

The pioneer bacteriologist in Japan, who held the first chair at Tokyo

University until his death in 1919, was Dr. Ogata Masanori, a graduate of the same institution who also studied four years in Germany, both at Max von Pettenkofer's Hygiene Institute in Munich and at the Imperial Health Office's Pathology Institute in Berlin. He returned to Japan in 1883 and began lecturing at Tokyo University while directing a small laboratory called the Hygiene Institute operated by the Japanese Home Ministry.<sup>34</sup> In 1908 a second chair was added at the Tokyo University Medical School to which a junior colleague of Ogata, Dr. Yokote Chiyanosuke, was named professor. By the time Ogata died, there were also three associate professors of hygiene and bacteriology at the Tokyo University Medical School.<sup>35</sup>

Much of the work in this field at the University was done in Ogata's Hygiene and Bacteriology Section but by no means all of it. The remarkable popularity of these studies in Japan is also attested to by contributions from professors in physiology, anatomy and even surgery.<sup>36</sup> More important were studies carried out in Dr. Aoyama Tanemichi's Internal Medicine Section and the Pathology Section of Dr. Miura Moriharu, Dr. Yamagiwa Katsusaburo and later Dr. Nagayo Mataro. As research in bacteriology was sustained only in these sections, together with the Ogata Section, our analysis of its social relations will be confined to these three groups.

There was also a second major center of bacteriological studies in Japan at this time, more important in some ways than the Tokyo University Medical School. This was the Institute of Infectious Diseases, established originally by Kitasato with private support in 1893. Two years later it became an official agency of the Home Ministry's Bureau of Public Health and in 1899 with Kitasato's approval passed wholly into the control of the Home Ministry. Though challenged on occasion by certain elements within the bureaucracy, he continued to exercise virtually complete authority over the Institute until October 1914 at which time there occurred certain changes in the relationship of the Institute to the bureaucracy which were not to his liking. He therefore resigned and established a private laboratory of his own to which the entire research staff of the Institute of Infectious Diseases emigrated with him.<sup>37</sup>

Bacteriology's remarkable development in Japan, which establishment of these two institutions symbolizes, was only possible for two principal reasons. One was that Japanese scholars had made strenuous efforts to acquire as much scientific and technical information from Europe as possible during the country's two and a half centuries of isolation. Annual trade missions from the Netherlands brought books and scientific instruments to Japan which by the end of the eighteenth century had permitted the stirrings of an incipient native research tradition in astronomy, medicine and occasionally in other

fields as well.<sup>38</sup> Still, the importance of these developments during the seclusion period for the development of modern science after the Meiji Restoration should not be overestimated. Such knowledge as Japanese scholars acquired of scientific subjects was usually incomplete and often erroneous. And because so much of the empirical knowledge entering the country in these years was partially or even wholly detached from theory, most fields of science after the Restoration had to be created out of nothing.<sup>39</sup>

The biomedical sciences were a partial exception because the Japanese had taken particular interest in this area for practical reasons and before 1868 were far closer to the frontiers of knowledge here than in any other field of science.<sup>40</sup> Such progress in medical science provided a basis for rapid intellectual development later on but was also important in two other ways. For one, even the most rudimentary education in medicine served to introduce relatively large numbers of Japanese youth to science before the Restoration. While only a small fraction of those who graduated from schools where Western medicine was taught later pursued careers in science, one may assume the number of scientists produced during the period before 1900 would have been smaller yet had earlier attempts to acquire knowledge of Western medicine not been made. But the fact that early progress in medicine established that discipline as the "official image" of science in Japan whereas physics had enjoyed that distinction in Europe is perhaps even more important. This, it is suggested, may account in part for the failure of modern science to influence other spheres of Japanese culture more than it actually did.

Modern science's development as an organized social activity in Japan cannot be understood apart from the activities and policies of the Japanese government. During the first few years after the 1868 Restoration Japan's new leaders adopted a wide range of basic reforms aimed at making their country the equal of any in the West. Expansion of military power was the keystone of these reform efforts; but tied to it were policies calling for abolition of the traditional class system, industrialization, expansion of education and the promotion of science and technology. A dual strategy was adopted in the latter case which brought foreign teachers to Japan while Japanese youth were sent by the government to Europe and occasionally to the United States. Except in the very early years, scientific and technical subjects had no monopoly on those which the Japanese *ryugakusei* were sent to study as law and public administration were studied considerably more often than science or engineering. However, a consistently high percentage of the *ryugakusei* went abroad to study medicine, usually to Germany but

sometimes to Britain, France or Austria. Indeed, of the total number sent abroad by the government between 1895 and 1912, some one-hundred and eight, or about seventeen percent, fell into this category.<sup>41</sup>

Establishing universities, special schools and technical institutes was still another way in which the government endeavored to foster scientific progress. Creation of Tokyo University in 1877 by merger of three existing schools was the most important single step of this kind. In 1886 a graduate school was added and in 1897 a second "imperial university" was established at Kyoto. By 1920 there were four imperial universities, each having schools of medicine, law, engineering, science and liberal arts. For all of these institutions Tokyo University served as an organizational model.

### *Kitasato's Criticism of the Tokyo University Medical School*

Among the hundreds of young Japanese who benefitted from these efforts of the government to promote science was Kitasato Shibasaburo. Born in the Kumamoto region of Kyushu island, Kitasato first encountered Western medicine in 1871 at a school in Kumamoto which the ruling daimyo of the area had established within the grounds of his castle. He decided to study medicine in Tokyo after completing his course there and in 1874 entered an academy which later became part of Tokyo University. Hoping for a career in medical research he decided to enter the service of the Bureau of Public Health after graduating from the Medical School in 1884. Fortunately, employment there gave him the opportunity to assist Dr. Ogata Masanori in his experiments and thus introduced him formally to bacteriology. Within a year the government awarded Kitasato a stipend for advanced work in this field at Koch's laboratory in Berlin University.<sup>42</sup>

During his six years in Berlin Kitasato made two contributions to science of great importance. In 1888 he published a paper describing the procedures he had used to achieve a pure culture of the tetanus bacillus. As this was an effort in which several other prominent bacteriologists had failed, his success attracted considerable attention.<sup>43</sup> However, subsequent research which he did in some sense overshadowed it. Continuing his work on tetanus, Kitasato was able to demonstrate in 1890, together with Emil von Behring, working on diphtheria, that there exist certain substances in the blood serum of the body capable of neutralizing foreign materials. The epoch-making paper in which they reported these researches not only explained the basic processes of immunization but also laid the theoretical foundations of the new science of serology.<sup>44</sup>

These years were also important to Kitasato in another way and it is this

aspect of his experience in Germany on which attention will be focused here. For Kitasato, while working with Koch, came to believe that bacteriology in Germany was in an "unfortunate position" generally and that the organization of the German medical schools, especially the one at Berlin, made an already undesirable situation worse. The experience in Germany convinced him also that these defects had been transmitted to Japan through its adoption of German academic medicine as an organizational model. In his estimation the worst feature of German medical science was the separation it made between clinical medicine and basic medicine. Using the University of Berlin Medical School as a frame of reference he argued that the formal organization of the Tokyo University Medical School was harmful to medical research because it differentiated sharply between clinical medicine and basic medicine just as the Berlin Medical School did.<sup>45</sup>

Although some of the force behind Kitasato's argument stems from the characteristics of bacteriology described earlier, a consideration which influenced his thinking as much or more was the fact that his teacher, Robert Koch, believed the Berlin Medical School's organization had made *his* research difficult. Two letters Kitasato wrote in 1892 describe an incident occurring in 1890 which affected Koch's interests so adversely as to prompt the German scientist's resignation from the faculty of the Berlin Medical School. The incident in question was the tuberculin controversy which resulted from a speech by Koch implying that a new substance he had discovered, called tuberculin, might help cure tuberculosis. When the new discovery failed to do so, Koch's reputation suffered a temporary disgrace which, as he saw it, might have been avoided had the Medical School not insisted on the usual division of labor between its basic and clinical sides. Because Koch held a professorship in hygiene and bacteriology on the basic side, he had to entrust the requisite clinical tests to two professors on the clinical side. Whether reasonably or not, he seems to have blamed the two colleagues for his own mistake when the tests which they made after his announcement showed tuberculin to be therapeutically inefficacious.<sup>46</sup> This experience of Koch seems to have been a very considerable influence in turn on Kitasato's thinking about the social relations of bacteriology in Japan.

In the writings and statements of Kitasato, then, are two interrelated criticisms of the social relations of bacteriological research at the Tokyo University Medical School based on the social relations of bacteriological research at the University of Berlin Medical School. The first was that the organization of the Medical School inhibited cooperation among each of the various sections relating to bacteriology. His second criticism was that the progress of that discipline was also impaired by the fact that the Medical

School had established a sharp division of labor between its basic and clinical sides. Both of these criticisms were directed at the formal organizational arrangements characteristic of the biomedical sciences in the two countries. For analytical purposes it is useful to view the first as essentially a critique of the so-called chair system and the latter as referring to certain influences of professionalization in medicine on bacteriology.

*Formal Organization in Two Medical Schools: Berlin and Tokyo*

What was the chair system and how did it come into being? Essentially, the term referred to the structural division of the spectrum of knowledge into a series of discrete units with a full professor presiding over each. To that extent it was an inheritance from medieval times in Germany. The principal concern of this paper, however, is with aspects of the chair system which influenced its response as a system of institutions to the expansion of the bio-medical sciences, especially bacteriology, during the nineteenth century. Close attention will therefore be paid to the relationship between the chairs and the clinics, laboratories and institutes which grew up at that time partially as a result of greater specialization and professionalization in medicine. As for the chairs themselves, they rarely if ever included more than a single professor and a handful of students before the nineteenth century. But the growth of science changed all that dramatically. By the time Kitasato arrived at the Berlin University Medical School in 1885 a chair in medicine commonly had one or two associate professors, several lecturers, teaching assistants and research assistants in addition to the full professor and the graduate students. Ben-David and Zloczower have argued that in Germany this complex of organizations affected patterns of interpersonal relations among medical scientists in ways which were detrimental to the progress of medicine, including bacteriology.<sup>47</sup> And because the behavior of Japanese medical scientists resembled that of medical scientists in Germany in certain ways, Kitasato argued that the chair system must have been responsible in each case. Given the similarities between his argument and the functionalist approach to science, the question of whether he was right or not is a matter of some interest.

Establishment of a chair system at Tokyo University was first suggested by the school's president in a letter to the Education Ministry in September 1890 although no action was taken on this request until the summer of 1893 when Inoue Kowashi became minister. The new minister seems to have viewed the chair system as a means of alleviating the financial burden of paying large salaries to the foreign professors still teaching at the univer-

sity.<sup>48</sup> The foreign teachers cost three times as much to employ as Japanese and he believed their number could be substantially reduced if more efficient use were made of the native faculty. Establishment of a chair system would further this aim, as he saw it, because the teaching staff would not be required as before to teach any course in their department but could now teach exclusively in their respective fields of specialization.<sup>49</sup> From this point of view the reform was clearly a success because it enabled the government to reduce spending on the university by twelve percent while actually increasing the number of professors.<sup>50</sup>

Financial considerations relating to the chair system had one other important effect, which was to suggest adoption of the French chair system in preference to the German one.<sup>51</sup> While the German system permitted only one chair per discipline, the French system allowed multiple chairs as needed; so Tokyo University also came to have a multiple chair system. That financial considerations were behind this decision is indicated by an interview which another Minister of Education gave to a medical journal called the *Ikai Jiho* in 1913. Its reporters asked Dr. Okuda Kijin why Tokyo University Medical School had so many professors in view of the fact that German medical schools managed to carry on with far fewer. In replying the Minister explained that Germany had considerably more medical schools than Japan and emphasized that no one of them had to accommodate as many students as those in Japan did.<sup>52</sup> The clear implication of the Minister's remarks was that it was cheaper to establish new chairs at existing universities than to establish new universities from the ground up.

There was probably another reason as well for adopting the French chair system rather than the German one. Unlike German higher education, Japanese higher education was highly centralized, reflecting differences of political organization in the two countries. Japan was a centralized empire in which Tokyo controlled education at all levels for the entire country. Germany after 1871, by contrast, was a federalized empire each of whose formerly independent states retained extensive autonomies in educational matters. A more appropriate model for Japan than Germany, in consequence, must have been France where centralization was typical of all areas of administration, education among them.<sup>53</sup> This influence of French higher education on Japanese higher education is important to note in connection with Kitasato's implicit assumption that German educational models were the only ones employed by the Japanese in the biomedical sciences.

The chair system itself had assumed the following pattern at the Tokyo University Medical School by 1908:<sup>54</sup>

<b>BASIC MEDICINE - Number of Chairs</b>		<b>CLINICAL MEDICINE – Number of Chairs</b>	
Anatomy	2	Internal Medicine	3
Physiology	1	Surgery	2
Pharmacology	1	Orthopedic Surgery	1
Pathology	2	Obstetrics and Gynecology	1
Biochemistry	1	Pediatrics	1
Hygiene and Bacteriology	2	Ophthalmology	1
Legal Medicine	1	Dermatology and Urology	1
		Psychiatry	1
		Oto-rhino-laryngology	1

By contrast, the Berlin University Medical School's chair system looked like this at that time:<sup>55</sup>

<b>BASIC MEDICINE – Number of Chairs</b>		<b>CLINICAL MEDICINE – Number of Chairs</b>	
Anatomy	2	Internal Medicine	1
Physiology	1	Surgery	1
Pharmacology	1	Orthopedic Surgery	1
Pathology	1	Obstetrics and Gynecology	1
Biochemistry	1	Pediatrics	1
Hygiene and Bacteriology	1	Ophthalmology	1
Legal Medicine	1	Dermatology	1
		Pathological Anatomy	1
		Psychiatry	1
		Oto-rhino-laryngology	1



Two interesting differences are immediately apparent from the charts. First, Berlin had only one chair for hygiene and bacteriology while Tokyo University by this time had two. Secondly, no discipline at Berlin except anatomy had more than one chair while five disciplines enjoyed that distinction at Tokyo University. With these differences in academic organization in mind, the larger social context in each of the two countries may be considered in order to show ultimately that the formal organization of the Tokyo University Medical School could not have had nearly so much influence as Kitasato claimed on the status of bacteriological research there.

Sociologically speaking, the hallmark of the biomedical sciences in Germany during the period of their greatest fruition was competitive interaction. Among the twenty-eight university medical schools there was a constant flow of professors and students with each university endeavoring to attract the most capable.<sup>56</sup> Berlin University in particular, because it occupied the preeminent position, made an effort to secure the best students and the most distinguished professors for its faculty. There especially it was required that anyone hoping for an appointment first make a reputation at another university.<sup>57</sup>

Beginning in the last quarter of the nineteenth century, however, the German medical schools, including Berlin, began to deviate from the system in certain ways. Professors and students still moved from one university to another and competition still took place for distinguished faculty members, but a process of "compartmentalization" began to inhibit these interaction patterns. Accompanying the rise of the medical research institutes most medical schools began to duplicate facilities, became reluctant to share them with members of other sections, and in the clinical disciplines, showed a frequent unwillingness to allot part of the patient case load to other sections or to let them perform activities claimed by a particular section as its own responsibility.<sup>58</sup> This movement toward "compartmentalization" in Germany is said to have resulted primarily from the inability of the academic system to expand in a manner appropriate to the needs of medical science. Establishment of the institutes in the German medical schools, therefore, resulted partly from a need to differentiate research roles and provide more positions for scientists than the academic system would have created otherwise.<sup>59</sup> While medical science expanded organizationally by creating new chairs during the first three quarters of the nineteenth century, it expanded scarcely at all in the latter decades. Establishment of specialized research institutes became a way of allowing science to expand while maintaining the chair system intact. Such a mode of expansion, however, created certain problems which seriously impaired the effectiveness of the system.

Whereas German academic medicine had displayed a high incidence of competition between the chair-holding professors (*ordinarii*) and the private lecturers (*privatdozenten*) in each discipline during the first three-quarters of the century, competition in the latter years tended to be replaced by various types of patron-client relationships between the two groups. Partly for that reason and partly because the number of attractive positions available to the younger men declined drastically, certain fields lost momentum and eventually experienced declines in research productivity.<sup>60</sup> These changes are said to have resulted from the rise of the institutes which, in turn, were partly a function of the chair system's failure to expand. Earlier in the century only the most rudimentary facilities had been needed for research and those who did research usually had a medical practice or some other source of income with which to support themselves as *privatdozenten* before receiving the call to a professorship. The relatively small number of working scientists, moreover, together with the relatively large number of universities, meant that innovations and reforms were accepted and diffused rapidly through competitive interaction among the universities.<sup>61</sup>

The internal growth of science juxtaposed to the academic system's inability to expand effectively stifled this process. Because the facilities required for research were far more expensive than most scientists could afford and because the academic system was expanding vertically through the establishment of institutes but scarcely at all horizontally through the establishment of chairs, younger men had to enter the institutes to acquire the credentials needed for an academic career. Research facilities at the institutes, however, were monopolized by the directors who usually held professorial chairs concurrently; and control of these facilities by the professors enabled them to curtail and finally eliminate the competition from the private lecturers which had existed earlier. The lecturers, therefore, apprenticed themselves to the professors as research assistants in order to rise in the rigidly structured system. Having eliminated the possibility of direct competition from the younger men, the professors frequently came to value in their assistants such qualities as an ability to take the professor's side in an academic dispute and to avoid disagreement with him. Movement of such "schools" of medical scientists from one university to another under these circumstances, moreover, often involved transferral of the entire group.<sup>62</sup>

Although conditions in Japanese medical science at that time had some similarities to conditions in Germany, the differences must be given far greater weight. Quite apart from what happened in practice, there was a certain ideological emphasis on competitive interaction in Japan just as there

was in Germany. Dr. Okuda's remarks in the *Ikai Jiho* interview mentioned earlier implied a favorable view of both the freedom of movement German professors and students enjoyed and the competitive interaction created by the *privatdozent* system. But in the same interview the Minister emphasized that these practices could not be permitted in Japan because the country lacked Germany's large number of university medical schools and professors.<sup>63</sup>

Numerous other persons in positions of influence also stressed the importance of institutionalized competition for the well-being of science. In 1890 Dr. Hasegawa Tai, a physician member of the Diet, requested that body to establish a second imperial university at Kyoto because, in his words: "Observation shows that because of the lack of competition, the professors at Tokyo University have ceased....to discover new scientific theories and the students....to pursue their objectives."<sup>64</sup> And in 1893 when Education Minister Inoue Kowashi introduced the chair system with its incentive payments for research, a desire to promote greater competition among the professors is known to have been among his principal concerns.<sup>65</sup> Similarly in 1918, the Education Ministry's Chief of the Bureau of Professional Education, Matsuura Shinjiro, told the Budget Committee of the Diet's Lower House he agreed with the committeeman who said that "competing chairs" (*kyoso koza*) improve the quality of a university; and in this same testimony he emphasized that only lack of money had prevented more of such chairs from being established.<sup>66</sup>

Matsuura's statements indicate that Japan's four major medical schools were supposed to compete with each other and there are good reasons to believe that they did so. To cite one example of such competition: the Dean of the Tokyo University Medical School in 1910 ordered one of his students to begin research on a disease which had attracted attention from medical research groups at the Kyoto University Medical School and the smaller Okayama Medical College, saying, "Tokyo University Medical School must not fall behind those institutions."<sup>67</sup> And if the predictions of functional analysis are correct, the existence of multiple chairs in several disciplines at each of the four imperial universities would have encouraged other forms of competition, as among professors for students.

However, it is exceedingly unlikely for reasons to be explained later, that the formal organization of the Medical School really encouraged this kind of competition at all. Indeed, the empirical evidence strongly suggests that Tokyo University Medical School had problems of disunity and compartmentalization more severe, if anything, than those at Berlin. An article in the July 18, 1914 issue of *Ikai Jiho* noted that every section at the Medical

School insisted on having its own library, specimen room, equipment room and other research facilities, demands for which in the journal's opinion, were not only wasteful but conducive to discord among the professors and students.<sup>68</sup> Later that year an anonymous physician who either spoke for Kitasato or may even have been Kitasato, told the *Tokyo Asahi Shimbun* that the Medical School had built four chemical laboratories though requiring only one. He denounced this as a superfluous form of "competition against oneself."<sup>69</sup> Certain prominent members of the medical profession who also served in the Diet were equally critical of the Medical School on this point. When the Education Ministry in 1918 requested money for a chair in serology, Dr. Tsuchiya Seizaburo, editor and publisher of the medical journal, *Nihon no Ikai*,<sup>70</sup> objected saying that an existing chair in that field at the Institute of Infectious Diseases (associated with Tokyo University Medical School after March 1916) made a second one unnecessary. He urged the Ministry of Education to follow the practice in European medical schools of teaching serology in connection with a chair of pharmacology or bacteriology and declared that the university had only requested the second chair "because of some enmity among its professors."<sup>71</sup> Dr. Yagi Itsuro was another member of the Diet who apparently believed this. Yagi had graduated from the Tokyo University Medical School and studied several years in Germany at the University of Rostock before entering private practice in Nara Prefecture.<sup>72</sup> Despite what one might assume to be his loyalty to Tokyo University, he opposed a government appropriation bill for his alma mater in 1914 and offered the following explanation for doing so: "The professors of the Tokyo University Medical School," he asserted, "do not even deserve to be called scientists [because they] confine themselves to small domains and will not cooperate with each other in research."<sup>73</sup>

Precisely why Tsuchiya and Yagi made these remarks is open to a variety of interpretations. Tsuchiya had vested professional interests opposed to those of the "University Medical School Faction" or *Daigaku Ha* as it was called. Moreover, he was a well-known admirer of Kitasato, whose attitude toward the Medical School has already been indicated.<sup>74</sup> Yagi, on the other hand, had not even met Kitasato at this time and was also attacking people with whom he had once been associated.<sup>75</sup> It is possible that his views and to some extent those of Tsuchiya, reflected the influence of the traditional Western ideology of science. Certainly Yagi's experience in Germany suggests this possibility. On the other hand, both physicians may have been describing the affairs of the Medical School professors more or less accurately.

If Ben-David and Zloczower are right, the German medical schools, including Berlin, exhibited certain characteristics which might permit

application of the same description to them: 1) There was a kind of “roping-off” process occurring in the German medical schools whereby a particular section or institute would claim exclusive rights in certain fields of research or assert that it alone had the right to perform autopsies or care for certain kinds of patients; 2) Many professors resisted the establishment of a second chair in their discipline because this might have required them to share the patient case load and certain research facilities or because it might have reduced their income from student fees;<sup>76</sup> 3) The tendency of many sections to lay claim to a particular field of research inhibited the career possibilities of younger men working in the same field who found themselves in the “wrong” section.<sup>77</sup> Collectively, these practices are said to have created serious tensions within German academic medicine, not only creating factions or “schools” but undermining its productivity in the process.

But the real problem lies not in determining whether such tendencies, termed “compartmentalization,” actually existed or not. Certainly there are very good reasons, including the evidence cited here, to believe that they did. Considerably more important is the question of why Ben-David and Zloczower have argued that in Germany these developments resulted from changes in the formal organization of the biomedical sciences, specifically from the creation of the institutes and clinics. If one accepts this as a working hypothesis, the question of whether it could also account for similar, even identical, kinds of behavior in Japan immediately arises. In other words, were factions there a product of formal organizational arrangements as Kitasato claimed? This question, of course, can only be answered by comparing formal organization in the two medical schools and the two university systems. Such a comparison, it is argued, indicates that while compartmentalization with its factions or “schools” did emerge in Germany as a product of organizational changes, in Japan compartmentalization existed all along because of the prior existence of factions! This conclusion seems to follow from the fact that Tokyo University and the Japanese university system were organizationally flexible at precisely the points where Berlin University and the German university system were rigid.

Evidence presented earlier indicates, first of all, that Tokyo University often had two or even three chairs in a single discipline whereas Berlin almost never did. This meant that chances of obtaining a chair were greater at Tokyo than at Berlin and, moreover, that potential for competition in the formal organization of the Medical School was also greater in the former. Secondly, it was much easier to create new chairs in the Japanese system than it was in the German. As Bureau Chief Matsuura said in 1918: “In the imperial universities the establishment of chairs depends on the development of

science itself."<sup>78</sup> At any particular German university, by contrast, the number of chairs could usually be increased only by dividing an existing field into smaller fields, not by adding a second or third chair in the same field.<sup>79</sup> And thirdly, it is important to note that Japanese higher education experienced considerably more horizontal expansion during this period than did German higher education, as the Japanese government between 1885 and 1920 built three new imperial universities while in Germany the last prewar foundation was made at Strassburg in 1874.<sup>80</sup>

### *Medical Scientists and Medical Practitioners*

Investigation of the effects of professionalization in the medical systems of the two countries tends, if anything, to reinforce the basic conclusion that compartmentalization in Japan was not the result of formal organizational arrangements as such. The object here will be to show that the movement toward professionalization in Japanese medicine had far less influence generally than it did in Germany; and consequently, that Kitasato's insistence on the influence of the basic-clinical medicine dichotomy at Tokyo University was largely misplaced.

So far as bacteriology in Germany is concerned the principal effect of professionalization was to establish a sharp differentiation of medical roles based on the classifications of researcher and clinician. First of all, it promoted a gradual differentiation of professors in medical schools from the physicians who practiced medicine. Before about 1850 large numbers of German scientists in all fields had earned their livings by practicing medicine. But when medical research emerged as a fulltime occupation, a distinction came to be made not only between physicians and professors but between those in basic medicine and those in the clinical disciplines.<sup>81</sup> Ordinary physicians, moreover, as a result of this change, were effectively deprived of the right they had once had to utilize the facilities for research in the medical faculties and the public hospitals. With professionalization both came to be monopolized by the professors.<sup>82</sup> Finally, opportunities for communication between professors and physicians became much less frequent when they occurred at all because the professors withdrew from the ordinary medical societies to form their own professional associations.<sup>83</sup> The principal effect of these changes on bacteriology would seemingly have been to obstruct the very unification of clinical practice and research which had led to its creation in the first place.

In Japan, by contrast, these problems were certainly much less acute if they existed at all. While a sharp division between basic medicine and the

clinical specialties was instituted at the Tokyo University Medical School under German influence, a considerable amount of evidence strongly suggests no sharp differentiation was made in Japanese medicine as a whole between the roles of medical scientist and physician. Assuming they had the proper clique affiliations, physicians were given access to university and hospital research facilities until at least 1917 and possibly even thereafter.<sup>84</sup> Moreover, all the professors of clinical medicine at Tokyo University themselves maintained substantial private practices and usually their own hospitals, largely because academic salaries were so low. In certain cases, professors were said to be earning between 40,000 and 50,000 yen (approximately 20,000 to 25,000 dollars) each year from treating private patients outside the university setting. Even the Dean of the Medical School, Dr. Aoyama Tanemichi, as one of the university's three leading internists — who also did bacteriological research — earned 20,000 yen annually despite his administrative functions.<sup>85</sup>

Tokyo University professors of clinical medicine, of course, were not atypical in maintaining private practices as such, for Berlin University professors in the clinical specialties also saw patients outside the academic framework. To that extent treatment of patients by Tokyo University professors outside the Medical School setting does not in itself demonstrate that Japan lacked a sharp role differentiation between scientists and physicians. However, it must be emphasized that Tokyo University professors seem to have devoted the *major* portion of their time and energies to seeing private patients whereas the Berlin professors generally remained loyal to the academic ideal.<sup>86</sup> The very small number of medical specialists in Japan at the time, presumably, was one reason for the inability of the Japanese professors to do this. While German patients had a relatively large number of such specialists available to them, the Japanese did not; so there was an important social need for the services of the well-trained Tokyo University professors. "Considering the present state of Japanese culture," the Vice Minister of Education declared in 1920, "it may not be such a bad thing for professors of clinical medicine to maintain large private practices."<sup>87</sup> And the inability of married professors with families to live on a professor's salary, acknowledged even by the Education Ministry, must have been a second.<sup>88</sup> At Berlin, by contrast, a professor in the Medical School could enjoy a comfortable standard of living, especially when income from student fees, nonexistent in Japan, was taken into account.<sup>89</sup>

Yet, it is perfectly true, as Kitasato might have argued, that most of the work in bacteriology at Tokyo University took place in two laboratories on the basic side. And here conceivably there might have been a problem; for

professors in the basic disciplines were not permitted to maintain private practices and had to supplement their meager incomes by additional, part-time lecturing or by writing textbooks.<sup>90</sup> Actually, however, it seems unlikely that bacteriology could have suffered on that account for denial of the right to practice medicine by no means implied denial of access to the necessary clinical facilities. Dr. Miura Moriharu and Dr. Yamagiwa Katsusaburo in the Pathology Section were permitted to carry on clinical tests and observations in the main Tokyo University Hospital<sup>91</sup> while Dr. Ogata Masanori and his associates in the Hygiene and Bacteriology Section used the clinical facilities of the Komagome Hospital, a university affiliate.<sup>92</sup> Dr. Nagayo Mataro in Pathology, moreover, had access both to the main university hospital and later to the clinical facilities of the Institute of Infectious Diseases.<sup>93</sup>

That Japanese bacteriology did not suffer from the formal organizational divisions between basic and clinical medicine which may well have plagued this science in Germany is also suggested by two other considerations. One is a campaign, partly ideological, partly political and partly economic in motivation, directed against the professors of clinical medicine which aimed to enforce just such a division. As early as 1893 the clinical professors were attacked by the Great Japan Medical Association (*Dai Nihon Ishi Kai*) for taking work away from general practitioners by treating private patients. The *Ikai Jiho*, which participated editorially in these attacks, declared that the Medical School professors were supposed to serve as "models" for the Japanese medical profession and demanded they "resign and take some other job" if they could not live on their salaries.<sup>94</sup> Certain other vehicles of public opinion and professional medical opinion also found these deviations from professional standards reprehensible, emphasizing that professors must not neglect the teaching and research responsibilities for which they had been hired. In 1900 a former Vice Minister of Education, concerned that too many of the scientific papers appearing in the *Daigaku Kiyo* (University Annals) were written by foreign scientists, exclaimed in a speech to the House of Peers: "What research have these professors done? What discoveries have they made? What have they written?"<sup>95</sup> From all indications this movement reached a crescendo after the First World War broke out since the Anglo-French naval blockade of Germany removed that country as a possible source of medical, scientific and technical information for the Japanese. Thus the *Tokyo Asahi Shimbun* published a series of articles in November 1914, vigorously attacking the professors for the attention they gave to treating private patients: "The professors of clinical medicine at the Tokyo University Medical School treat patients in their homes, operate hospitals and say they



are doing scientific research.”<sup>96</sup> One physician member of the Diet even declared in a speech delivered the following month that if the professors did not do more research, they should be forced into early retirement in order to make room for those who would!<sup>97</sup>

It may be suggested that the very vehemence of this campaign and the rather considerable period of time during which it persisted suggests that Japan could not have institutionalized a lasting role differentiation between the physician and the medical scientist much before 1920, if then.

Still another aspect of the Japanese medical profession pointing to this conclusion is the way membership in the leading medical societies was determined. It was noted that in Germany medical scientists and physicians by this time generally did not belong to the same professional organizations. In Japan, however, membership in professional medical societies seems to have been based primarily, though not exclusively, on clique affiliations. Thus, medical men joined either the Meiji Medical Association (*Meiji Ikai*), created in 1894, or the Japan Federation of Medical Societies (*Nihon Rengo Ishi Kai*), which existed under various names from 1893, according to whether they identified themselves with the so-called University Faction (*Daigaku Ha*) or the Anti-University Faction (*Min-i Ha*).<sup>98</sup> Each association included medical scientists as well as physicians among its members. Kitasato for many years refused to participate in the activities of either one because this would be “improper for a scientist.”<sup>99</sup> But even his German-style professionalism gave way to social reality when colleagues in the Federation’s predecessor persuaded him to accept the presidency of their organization in 1916. His attitude, however typical in Germany, was not typical in Japan; most other Japanese medical scientists were very active in these two physicians’ organizations and despite their numerical inferiority often dominated them.

#### *Informal Organization in the Tokyo University Medical School*

If the tendencies toward compartmentalization criticized by Kitasato and other Japanese scientists did not result from the Medical School’s formal organization, they could only have resulted from its informal organization. Interestingly enough, while Kitasato himself appears never to have charged the Medical School publicly with factionalism as such, there are good reasons for believing that he did recognize its existence and that he took an unfavorable view of it, at least so far as bacteriology was concerned. Certainly persons with whom he enjoyed close association spoke out against factionalism in the Medical School often enough.<sup>100</sup> And there is little doubt but that

Kitasato himself had more than sufficient motivation to confine his public statements to criticisms of formal organization and pass over informal organization, as it were, unnoticed.

Existence of the highly influential "Kitasato Faction" (*Kitasato Batsu*),<sup>101</sup> it is suggested, gave him reason enough to maintain silence on this point. It was noted earlier that Kitasato had built his original Institute in Tokyo with private support and that later on he managed to fend off several attempts by opponents within the bureaucracy to undermine his authority over it. These efforts succeeded only because he enjoyed the support of an extensive network of strategically placed friends, associates and former pupils in various parts of the government, particularly in the Home Ministry. Not only was the Bureau of Public Health completely under his influence but the overwhelming majority of Japan's prefectural and other local public health officials were graduates of the special course in health administration which he offered at the Institute of Infectious Diseases.<sup>102</sup> These persons were a ready-made pressure group on whom he relied with striking success to protect his interests as needed. So influential was the "Kitasato Faction" that *Munsey's Magazine* in 1907 suggested Kitasato might be among the eleven most powerful men in Japan, a judgment confirmed by Japanese sources as well.<sup>103</sup> Indeed, it was precisely this concentration of power and influence which prompted the Education Minister, Dr. Ichiki Kitokuro, and the Prime Minister, Count Okuma Shigenobu, to change the Institute's administrative relationship to the Cabinet in 1914.<sup>104</sup> Small wonder, then, that Kitasato avoided discussing factionalism by name in public.

Even when he did say or write things which implied an unfavorable view of factions, he confined himself to deploring the influence they supposedly had on inter-group cooperation. However, contemporaries of Kitasato and more recent critics have also accused the Medical School's factions of excessive particularism in recruitment of faculty and of suppressing free discussion among their members. It therefore seems appropriate here to consider these criticisms as well.

Among the accusations leveled against the Medical School was that its faculty was excessively "inbred" due to particularistic recruitment and promotion procedures. It was said that only by graduating from the Medical School and having a relative on its faculty could a talented young biomedical scientist become a Medical School professor at all.<sup>105</sup> In fact there is a good deal of evidence to support this assertion. Hardly ever was anyone from outside the University invited to join its faculty. So rare was the occurrence that the *Ikai Jiho* in 1905 claimed the appointment of Dr. Suto Kenzo, graduate of a private medical school, to an associate professorship in

biochemistry heralded a major change in recruitment policies.<sup>106</sup> But the prediction proved false.

The question, of course, is what one chooses to make of all this. Certainly the traditional ideology of science has always stressed the ill effects among scientists of particularism in any form. Thus Theodor Billroth, professor of surgery at the University of Vienna, warned in 1876 against "forming a faculty exclusively of natives and making professorships hereditary in certain families" on the grounds that such practices "always have baneful results."<sup>107</sup> But even if this and similar statements are justified in a general way, their applicability to the practices of the Tokyo University Medical School during this period is not at all clear; as certain evidence suggests that the recruitment system there was quite universalistic in content, however particularistic in form. The principal mechanism for recruitment of faculty at the Medical School was the comprehensive examination given students upon completion of the regular M. D. course. This examination accomplished three things. It determined a student's rank in class; it determined the sections of the graduate school he might enter and those from which he would be excluded; and it eliminated all but the select few from whom the professors would choose their future sons-in-law and successors. Whenever possible the professors made these selections from those placing first, second or third in the examination.<sup>108</sup> The talented young biomedical scientist, as defined by the examination, then married a professor's daughter and eventually acquired a chair, though usually not the one occupied by his father-in-law.

The examinations naturally stimulated keen competition among the students which presumably insured that any potential recruit to the faculty had attained a certain standard of excellence. One successful veteran of the examinations, Dr. Manabe Kaiichiro, recalled that when he graduated at the top of his class in 1904, the competition was "unbelievably severe" as the examination "determined a person's fate for the rest of his life."<sup>109</sup> Nor was competition confined to the students; the professors also are said to have competed for the most promising son-in-law. In general professors on the clinical side whose sections enjoyed greater prestige had the advantage.<sup>110</sup> Dean Aoyama, for instance, was able to get the number two man in the class of 1907, as his Internal Medicine Section was particularly well regarded.<sup>111</sup> On the other hand, Dr. Ogata, whose Hygiene and Bacteriology Section on the basic side ranked considerably lower in student estimation, tried but failed to marry his daughter to the top man in the class of 1902.<sup>112</sup>

Among the five senior men doing bacteriology during this period, only the youngest, Nagayo, had a relative closely associated with the Medical School. (His father had been its dean from 1874 to 1879).<sup>113</sup> The four older men

were among the first generation of professors so naturally did not enjoy that advantage. Two of the four senior professors, Miura and Yamagiwa, had graduated first in their respective classes of 1881 and 1889;<sup>114</sup> while Aoyama and Ogata, as *ryugakusei*, must of necessity have been in the upper ten percent of theirs. And Nagayo himself, apart from whatever career benefits his father's achievements gave him, was certainly as well qualified in a formal sense as the others, having graduated second in 1904.<sup>115</sup> Thus, if even a minimally universalistic character is attributed to the examination, it seems unlikely the quality of the faculty could have suffered greatly from such "inbreeding."

Tolerance of criticism and free discussion, by contrast, is a matter about which generalization is slightly more difficult for the Medical School as a whole. Among the three sections where bacteriology was done, free-ranging discussion and criticism seem to have been actively encouraged in two, the Pathology Section and Ogata's Hygiene and Bacteriology Section; but in Aoyama's Internal Medicine Section both were probably inhibited. Concerning the Pathology Section there exists a remarkable unanimity of opinion on this point. One of its members said that Dr. Miura, the senior professor by date of appointment, strongly encouraged his students to formulate opinions of their own;<sup>116</sup> while another said he carefully avoided use of status language in order not to discourage free expression of views.<sup>117</sup> In fact Miura seems to have developed a special technique (which was probably not unusual at all) for eliciting opinions from students and junior members of the academic staff. His procedure was to share an *o-bento* (box lunch) with them every Saturday afternoon, followed by a long walk and usually a visit to the Yukokuro Restaurant where all imbibed freely. "On these occasions," said Dr. Yamagiwa, who had himself been a student of Miura, "reserve between professor and students was cast aside."<sup>118</sup> Equally so was this the case when Nagayo took over active direction of the laboratory from the two older professors in 1906. Nagayo made a practice of levying fines on members of the laboratory group who used honorific forms of address when speaking to him and generally shared tea and cakes with his junior associates every afternoon.<sup>119</sup>

Much less is known about the interpersonal relations of the Hygiene and Bacteriology Section as it had considerably fewer members than the other two laboratories and remaining descriptions of its internal affairs are accordingly quite scarce. However, Ogata is said on at least one occasion to have accepted from two of his students criticism described as "direct and unreserved" about a matter of some scientific importance;<sup>120</sup> and surviving general descriptions of his personality are consistent with this assertion.

Tokyo University's senior bacteriologist is described as "modest," "taciturn" and "living in an ivory tower," hardly the sort of traits associated with an authoritarian personality.<sup>121</sup>

Quite the opposite traits apparently characterized Dr. Aoyama Tanemichi, the Medical School's powerful dean and most influential of the three internists. Most accounts agree that he was "arrogant," "haughty," overbearing at times and susceptible to flattery.<sup>122</sup> His students were afraid of him and whenever possible avoided expressing opinions contrary to his. One former student recalled that the surest way to pass one of the dean's oral examinations was to "expound eloquently to Dr. Aoyama nothing but his favorite opinions about the pathology of a disease."<sup>123</sup> Though referring in this case to Aoyama's manner of instruction in the hospital ward, his procedure in the laboratory was apparently much the same. While in 1959 several of his former students attempted to show that he was really very tolerant of dissenting opinions, their descriptions are congruent neither with specific details of his style of leadership nor with the description provided by his own biographer in 1930.<sup>124</sup> "Whenever a student wrote a paper and submitted it to Aoyama," wrote Dr. Uzaki Kumakichi, "he would scrutinize it with great care and criticize it sharply. He seldom accepted a new thesis at first reading. In the event that a student presented a particularly bold idea, Aoyama would scold him, saying, 'Are you certain you want to write something so audacious?' Moreover, in the event the student had contradicted a leading authority, Aoyama always warned him he must reconsider that part of the argument."<sup>125</sup>

Despite such relatively authoritarian behavior, or perhaps because of it, this and other factions in the Medical School generated a deep loyalty to the senior professor and strong solidarity among their respective members. So the question naturally arises as to what influence either may have had on the amount of free discussion. Most critics of factionalism in Japanese science have stressed the influence of the senior professors in inhibiting criticism. But the possibility that certain kinds of group solidarity were also detrimental should be considered as well. In fact it appears that solidarity was a negative factor in the Aoyama group. One source states that if any student appeared to question Aoyama's judgment of a patient's condition too openly during a bedside diagnostics session, other section members were certain to reprimand him for it later.<sup>126</sup> And on one particular occasion students of Aoyama's were responsible for disrupting what existing accounts suggest was a legitimate student protest movement against the Medical School administration so their professor, the Dean, would not "lose face."<sup>127</sup>

Still, none of this permits the generalization that solidarity inhibited

criticism and free discussion in and of itself. In the Pathology Section, for instance, there was quite as much solidarity and exclusivity toward other groups as in the Aoyama Section. But within the group Miura tried and largely succeeded in instilling a "spirit of harmonious cooperation and mutuality."<sup>128</sup> When we note that frequent drinking and socializing with the members of his section was one of the ways he used to accomplish this, it seems significant that Aoyama rarely did either of these things. Except for an occasional glass of wine in his home, the Dean was a teetotaler who even lectured his students and colleagues on the evils of excessive carousing.<sup>129</sup> Considering the remarkable extent to which Japanese society in general relies on informal socializing with alcohol to ease tensions between persons of different status, the absence of this socializing or its inhibition in the Aoyama Section could only have had a deleterious effect on its morale and effectiveness.<sup>130</sup> In short, this comparison suggests that the personality of the senior professor *did* largely determine whether a faction encouraged new ideas or resisted them. If this inference is justified, then the answer to the question of whether group solidarities in the Medical School obstructed free discussion or not probably depends on which pattern of social relations one thinks was more typical; and on that point, it is argued, most evidence favors the pattern of social relations in the Pathology Section.

In the analysis of the problems of free discussion and recruitment procedures at the Medical School one important assumption has been made — that neither changed significantly through time. Since both patterns were congruent with fundamental aspects of Japanese tradition, such an assumption is probably justified. But in reference to the problem of inter-group cooperation it probably is not since this pattern involved a major change from traditional behavior. Moreover, the empirical evidence suggests the problem of inter-group cooperation is unresolvable unless significant change through time is assumed from the beginning.

Consider the following pieces of evidence. In 1894, one year after the chair system was established and a mere eighteen months after several associates of Aoyama had attacked Kitasato in print on purely personal grounds,<sup>131</sup> the same two scientists from different *institutions* led a joint research expedition to Hong Kong seeking to determine the cause of plague.<sup>132</sup> As Kitasato's ensuing paper, which appeared in *The Lancet*, left certain scientific issues unresolved, a lively controversy arose in Japan over the validity of his claim to have isolated the offending bacillus. Accordingly, the Medical School dispatched several research expeditions to Taiwan and the Kobe-Osaka area in the late 1890's to resolve them and included in each were professors from different *sections*, usually Ogata and Yokote from Hygiene

and Bacteriology, and Yamagiwa from Pathology.<sup>133</sup> Yet by the second decade of the twentieth century one finds these same two sections working for an extended period of time on precisely the same disease with no indication of cooperation between them at all.<sup>134</sup> Moreover, secondary accounts of the history of bacteriology in prewar Japan never mention any cooperative research involving members of different groups after these particular expeditions.

This, of course, does not prove conclusively that cooperative research had ceased among Japanese bacteriologists by this time; but there are good reasons for believing that it had and that consequently Kitasato's general description of the situation at Tokyo University should be accepted even if his public explanation for it should not be. In all probability there was not very much willingness within any of the relevant sections on either side of the Medical School to cooperate with the others. Members of Aoyama's section on the clinical side probably took a very exclusivist attitude toward the members of the Pathology and Hygiene Sections on the basic side and *vice versa*. While the evidence in general supports this assertion, the more difficult problem as before is to formulate a convincing explanation for it.

Logically, there are only three agents to which the decline of cooperation could be attributed: the professors, the students, or both. Two of these, involving the professors, it is suggested, can be eliminated from consideration on the following grounds. First, the professors were all on amicable terms with one another and remained so throughout this period so far as can be determined. Secondly, there were no changes in personnel among them except for the promotions of Yokote in 1908 and Nagayo in 1911; and there are no reasons at all to suppose either event made any substantial difference. Thirdly, the professors did in fact cooperate with each other before about 1900 but not thereafter. In short, the problem is to explain not merely the decline of cooperation but its timing as well.

To that end it is argued, first of all, that cooperation among different sections was not inhibited to any extent by the professors but rather by the often intense feelings of solidarity which developed among the students and younger section members. Suppression of actions which their peers defined as disloyal to the group was one manifestation of this solidarity and cooperation with other groups, one suspects, was often defined in that way. Consider the following event which occurred at the Medical School during this period. In 1916 Dean Aoyama secured the prior agreement of his two internal medicine colleagues to establish an institute for hydro-therapeutics and X-ray treatment. As a cooperative venture the institute was supposed to serve the needs of all three sections but did not because members of two of the

sections refused to work there. The younger men apparently objected to the Dean's appointment of a former student as director and not even pressure from their own professors managed to change their attitude.<sup>135</sup> While one can never hope to know all relevant aspects of this situation, the account of it which survives does suggest not only the failure of formal organizational arrangements to dictate behavior but the limited ability of the professors to stimulate cooperation among their respective sections.

That being the case, the principal reason for a decline in cooperative research about 1900 would simply have been that membership in each of the three sections began to increase sharply about that time. Consider the following chart:

**Membership In Tokyo University Medical School  
Sections Relating To Bacteriology**

Section	1897	1908	1917
Internal Medicine (Aoyama)	5	10	21
Hygiene and Bacteriology	3	5	15
Pathology	8	24	46

In no case does it show anything less than a three-hundred percent increase in the membership of each section during the twenty years between 1897 and 1917.<sup>136</sup> Thus, even if a professor wished to stimulate certain kinds of cooperation with members of other groups, the effect of these increases would have been to limit his ability to do so because peer group influence on each member would have been much greater than before, assuming constancy of solidarity feelings between individual section members. Moreover, not even the opposing effects of growth in numbers beyond a certain point would necessarily have enhanced prospects for cooperation since loyalty to the group remained the standard by which social action was legitimated whatever the centrifugal effects of factions within the larger faction may have been.

But even if Kitasato and other critics were correct in recognizing a near absence of inter-group cooperation at the Medical School, one must still consider the more basic issue of whether that really mattered. Were the exclusivist behavior patterns of its research groups really detrimental to creative research or might they in some respect have promoted creativity? Stated in this form the question obviously admits of no definite answer as



factionalism undoubtedly had both effects on different occasions. However, the problem here is considerably more limited as it is only required to demonstrate that the Medical School's informal organization *could* benefit creativity and that it actually did so in important ways.

Two accomplishments of the Pathology Section provide empirical evidence for this argument. In 1915 Dr. Nagayo Mataro began research with several members of his section on the cause of a disease called scrub typhus or *Rickettsia tsutsugamushi*. This disease had first been reported by Japanese physicians around 1900 but within a few years was also found to exist in the Malayan peninsula, the Dutch East Indies, Australia and India. The large area over which scrub typhus was dispersed, together with its apparent links to a large number of other diseases, thus attracted wide attention among Japanese scientists even though the affected areas in Japan itself consisted only of a few sharply defined river valleys in three mountainous prefectures.<sup>137</sup> Accordingly, members of Ogata's Hygiene and Bacteriology Section along with investigators from several lesser institutions also began studying this disease, thereby creating a highly competitive research situation.

From what is known of the incident it seems likely that factionalism benefitted the investigation in two ways. Its competitive pressures, first of all, stimulated Nagayo to begin studying the disease himself. In 1915 the Medical School was the target of bitter criticism from the recently displaced Kitasato Faction which claimed the University's contributions to bacteriology had been few. Specifically to refute these accusations, Nagayo became the first scientist anywhere to study the highly contagious disease in the field and led the first of many expeditions to Yamagata Prefecture in July of that year.<sup>138</sup> One may also suppose the same competitive pressures from other groups kept him there. Within a year the Nagayo team managed to link the disease to a specific pathogenic agent. However, they were not able to explain its life cycle completely until 1924; and even then had to carry out many more expeditions and laboratory tests before the medical profession bestowed its unanimous approval on their findings in 1930.<sup>139</sup> In short, competition among factions encouraged replication and independent testing of claims based on research findings and in that way benefitted science. Secondly, when the unusual amount of work and the extraordinary investment of time required for resolution of the scrub typhus problem are taken account of, it seems reasonable to suppose a well integrated research group would have a natural advantage over one less integrated. Indeed, that was precisely the sort of group Nagayo had tried to create in the first place by socializing with his younger colleagues and by discouraging their use of status language when speaking to him.

In the work of Dr. Yamagiwa Katsusaburo and his associates on cancer still another potentially beneficial effect of factionalism on scientific research can be seen. In 1915 Yamagiwa achieved one of the most important advances in the entire history of cancer studies with a "classical" paper demonstrating that tumors could be produced in experimental animals by the application of coal tar to the skin over prolonged periods. His work was important both theoretically because it placed Rudolph Virchow's doctrine of chronic irritation as a cause of cancer on a sound experimental basis and methodologically as well since it enabled researchers to induce tumors in host animals far more easily than had been possible earlier.<sup>140</sup> In the words of *The Lancet*: "It is impossible to over-estimate the importance of Yamagiwa's discovery for the study of cancer."<sup>141</sup> Indeed, the 1915 paper made him a leading candidate for the 1926 Nobel Prize in Medicine, awarded, however, to Johannes Fibiger in an action now widely acknowledged to have been an error.<sup>142</sup>

For present concerns the point of greatest interest is the process by which Yamagiwa managed to achieve these results. Essentially they owed as much to the intense loyalty of his younger colleagues and students as they did to his own brilliance. Not only did Yamagiwa himself specifically say this,<sup>143</sup> the facts of the matter seem to admit of no other interpretation. The reason is simply that Yamagiwa was a semi-invalid who suffered from pulmonary tuberculosis for nearly all his professional life.<sup>144</sup> Because of his physical condition it is highly unlikely he could have accomplished anything unassisted; for even with help his 1915 paper represented ten years of work.

### III. THE VALUES OF JAPANESE SCIENCE IN HISTORICAL PERSPECTIVE

#### *Individualism and Science: Europe and Japan*

Both because of the frequency with which they are cited by Japanese critics and because of their theoretical significance in functional analysis, discussion in this paper has centered on the fourth category of arguments: formal organizational arrangements and their relationship to creativity. The basic claim of these critics is that Japanese science was epiphenomenal and uncreative first because of its "feudalistic" apprentice system of recruitment and socialization and secondly, because of its failure to permit free exchange of views within research groups and cooperation among them. For the most part it is alleged that the chair system was responsible for these deficiencies. But was it? Ben-David and Zloczower argued that similar shortcomings

appeared in German medical science because of the one-chair rule, the difficulty of establishing new chairs and the lack of horizontal expansion in higher education during the late nineteenth century. These factors, they suggested, diminished the ratio of academic positions to those seeking them, created obstacles to the horizontal movement of students and younger scientists, and therefore encouraged the formation of patron-client relationships between scientists of higher and lower status which eventually lowered research productivity. So far as bacteriology specifically was concerned, the differentiation of scientists from physicians implied by the basic-clinical separation is said to have been harmful because it denied research facilities to the latter and direct access to clinical facilities to the former while segregating each in different professional organizations.

But was the Japanese situation really similar? Clearly it was not. First, the Japanese chair system, partly because of its French antecedents, permitted multiple chairs per discipline; secondly, it was not particularly difficult to establish new chairs; and thirdly, Japan's higher educational system was expanding horizontally much more than Germany's was. Thus the ratio of positions available to the number seeking them was higher in Japan; the students were objectively freer to move horizontally within the university, and their objective motivation to become the client of a senior scientist was therefore less. In the particular case of bacteriology, the objective situation was also more favorable in Japan since the differentiation of scientists from physicians had not progressed nearly so far, research facilities were not denied to the latter (until about 1917) nor clinical facilities to the former, and both belonged to the same professional organizations. Formal organizational arrangements, therefore could not have been responsible for the "deficiencies" of Japanese science attributed to them.

The alternative claim is that the scientists themselves were responsible for the alleged deficiencies. Rather than dispute this assertion directly, it is argued that, for the most part, these practices were not quite the deficiencies they might seem. Whether they married professors' daughters or not, for instance, Tokyo University Medical School professors were selected from among the better qualified. Moreover, criticism and free discussion were not only tolerated but encouraged except in Aoyama's Internal Medicine Section. Even the decline of cooperation, it is suggested, did not necessarily mean competition and creativity were compromised. Indeed, the work of Nagayo and Yamagiwa suggest that solidarity, loyalty, integration and competition, the results of distinctly Japanese value orientations, were not only compatible with creative science but probably gave it a highly positive stimulus. Thus, while some of the practices described as "deficiencies" actually existed, they

were not what critics have made them out to be. Others, moreover, did not exist at all. Thus, the problem for functional analysis, it would seem, is that it could not have predicted this combination.

From this analysis two conclusions can be drawn and one of them answers the first question posed in this paper. Since Japanese science had certain fundamental value orientations markedly different from those of Western science at both the level of organizational or institutional norms and the level of personal value orientations, it *was* epiphenomenal. These different values in the Japanese case, according to functional analysis, ought to have been the result of inflexible organizational arrangements as they were in Germany. But the preceding discussion has shown this could not have been the case. Functionalism also maintains that because certain necessary values were absent, whatever science was done under the circumstances could not have been creative by definition. But the Pathology Section's contributions to knowledge indicate that was not the case either. In short Japanese science was not necessarily uncreative because it was epiphenomenal. When, therefore, the ability of the Japanese to do creative science in the absence of certain allegedly fundamental values is taken into account, a second conclusion follows: that any theoretical explanation concerning the relations of science to culture and social structure in which values are assumed to be non-problematical at all levels of generality is empirically false and theoretically unsound.

In order to answer the second question posed earlier, it is necessary at this point not only to state what values were lacking in Japan but to indicate their place in the series of assumptions underlying functional analysis and criticisms by Japanese who define science in terms of values. It has been pointed out that Japanese values appear to have stressed solidarity, loyalty, affectivity and integration in addition to inter-group competition. Affectivity and solidarity in particular seem to clash with the emphasis Parsons places on affective neutrality and specificity. Given his definition of those terms, this is tantamount to saying the Japanese were not sufficiently individualistic.

The term "individualism" or "individuation" merits closer examination as it is central to the discussion here. Definitions vary according to context and the philosophical predilections of the writer. Theodorsons' *A Modern Dictionary Of Sociology* applies "individuation" to "the breakdown of group ties and the emergence of individuals who lack strong feelings of group loyalty..."<sup>145</sup> while Bernard Barber describes "individualism" as "a moral preference for the dictates of individual conscience rather than for those of organized authority" and declares it "an attitude...most congruent with science."<sup>146</sup> It is scarcely accidental that connotations so divergent have

attached themselves to the same term, as the weakening of primary group ties and other intermediate-level associations has often been attributed in Western countries to the rise of science (among other things).<sup>147</sup> But is science really responsible? If it is, how can one explain the absence of such a process in Japan whose modern history has also been dominated by the rise of industry, technology and science?<sup>148</sup> More likely, individualization or individuation as an influence on social structure or individualism in science should be attributed to the impact of ideological forces and the specific historical conditions within which science first arose in Europe.

These considerations lead to the second of the three questions posed at the beginning of this paper: Why was Japanese science "epiphenomenal"? Why did it lack the individualistic values of science in the West? One potential explanation advanced by Japanese critics was the frequency of "copying" stemming from the government's policies toward research. This argument appears to have a certain superficial plausibility since it is true that the Japanese government generally did not encourage scientific research, at least before 1914 and in some ways not before 1940. (Bacteriology was the single exception, partly because of its relationship to the well-being of the military). Nevertheless, the copying thesis should ultimately be rejected because it assumes these values would have become institutionalized if the government's policies had been different. Yet there is no reason at all to suppose such values as affective neutrality or specificity are inculcated by the mere act of *doing* research. One supposes rather that they become accepted because scientists are exposed to a cultural environment in which great ideological stress is placed upon them.

Peculiarities of language and thought processes are a second factor which several Japanese critics have suggested. Certainly the formation and acceptance of values is affected by language and ways of thinking. But the precise mechanisms by which they make their influence felt are matters of great controversy lying far outside the scope of this discussion. It is sufficient for present purposes to say they must have been important in undetermined ways.

Japanese critics defining science in terms of ideas, it was noted, have often linked science's epiphenomenality to the specific historical circumstance of its dependence on government patronage. Though surely wrong in attributing epiphenomenality to lack of government support for research, their emphasis on the importance of the government's role seems entirely plausible in itself. Much of Western science's individualism has commonly been explained by the self-supported, amateur status of its early practitioners and their lack of sustained patronage from the state.<sup>149</sup> By contrast, modern science in Japan

was almost completely dependent on government support (such as it was) from the beginning. One might therefore suppose this unusual degree of dependence on government did obstruct in Japan those *ideological* forces which created the individualistic ethos of science in Europe. In this sense science's identification with the "collectivistic" aims of the state may also have impaired the ability of these same forces to undermine family loyalties and other primary group associations.

There are at least three other historical factors in Japan which probably inhibited the creation of a more individualistic ideology of science, if the European experience is any indication. One of these has already been mentioned: the fact that historically, it was the biomedical sciences rather than physics which in Japan formed the official, public image of science. Not only did medicine receive far greater attention than the other sciences in the Tokugawa Period, its extreme predominance continued well into the twentieth century. Science degrees conferred by Tokyo University provide one index of medicine's greater influence. Between 1876 and 1916, 2,613 degrees were conferred in medicine but only 814 in all other sciences. At the doctoral level the figures are similar in magnitude: 200 to 82 for the years 1888-1910.<sup>150</sup> This fact is important because physics projects a much more individualistic image than any of the biological sciences do. Roger Krohn found that physicists (and "academic" scientists generally) are noticeably more likely than any kind of biomedical scientist to stress the importance of "personality" and "the individual" over "situation" and "the team" for creativity,<sup>151</sup> providing empirical support for the more impressionistic conclusions of earlier investigators. Why these differences exist is a matter which cannot be explored here. However, it seems significant that physics (optics and mechanics) attained intellectual maturity in the seventeenth century when science was very much an amateur activity, while medicine's maturity was delayed until the nineteenth century when professionalization was already beginning to reshape the social bases of science in fundamental ways.<sup>152</sup>

That Japan therefore acquired modern science in the nineteenth century *after* its professionalization was already well along is the third historical factor meriting emphasis. Professionalization marked an important change for science in general because it involved greater emphasis on the functions and responsibilities of the professional peer group and less on those of the individual practitioner. Of course, this earlier tradition of individualism retained considerable influence in the West where science had existed for several hundred years. But Japan had no such ideological heritage; and a newly deindividualized professional science could hardly compensate for its absence.

Japan lacked still another important historical experience associated with the rise of an individualistic ethos for science in the West: the long tradition of often intense conflict with religion. Not that scientific explanations for natural phenomena went totally unchallenged in Japan. They did not. Some eighteenth-century Buddhists opposed the replacement of Sumero cosmology by the newly-acquired heliocentric view on religious grounds<sup>153</sup> and conflict was rife between Christians and the defenders of science in the Meiji period. But all of this pales by comparison with the history of such conflicts in Europe. It is scarcely novel to observe that a principal reason for the conflict between science and religion was the heritage of Aristotelean scholasticism which combined supernatural beliefs and empirical information in a synthesis so intricate that an attack on one part necessarily appeared to endanger the whole. Thus, scientists could hardly avoid controversy with religion; and one aspect of their response was the creation of appropriate values concerning the ways in which information was obtained. For ideological and political reasons scientific evidence had to be overwhelming against the theological opposition; and the values which emerged presumably contributed to that end. If Kenneth Downey is correct in assuming that values appropriate to an age of warfare between science and theory (e.g. "organized scepticism" or "individualism")<sup>154</sup> may no longer be needed in the West, what reason is there to assume they were *ever* needed in Japan, given its relative lack of experience with such warfare?

Having presented the case for rejecting theoretical explanations of science's relationship to culture and social structure in which values are thought non-problematical, it is now appropriate to consider briefly the problem of creativity in prewar Japanese science. This complex subject cannot be discussed in any depth here but one important point can be made about it in view of the general argument advanced in this paper. That is that creativity in science is far more a matter of ideas than it is of values.

Earlier it was observed that many critics who define science in terms of ideas nonetheless believe it was not very creative in Japan. If that was the case, one can only conclude that any acceptable explanation would have to base itself on factors of language, irrationalism and the absence of a tradition of scientific thought before modern times. Other arguments that have been advanced in connection with creativity seem to offer little. "Copying" is synonymous with the problem itself and is therefore not an explanation. Similarly, the arguments about organizational arrangements and behavior also have serious defects which the previous discussion has presumably made apparent.

*The Accommodation Theory of Science and Culture*

The analysis presented here thus would appear to require an alternative approach to the problem of science, culture and social structure, one in which for the Japanese case and presumably for others, questions of language, thought patterns and historical factors would have a central place. The approach suggested is in no sense original, having been proposed in a somewhat different form by the late John Peter Nettl and others.<sup>155</sup> Its principal recommendation is that science be investigated by proceeding from cognitive structures to institutions rather than the reverse. From this perspective science would be seen to seek social attachment, gathering the necessary force or power to influence, encourage or even dictate the conditions permitting it to flourish. It is suggested that science may do this by defining itself as an *ideology* and that it has, in fact, done precisely this in the past. With science defined as ideas, its principal impact on culture and social structure would be limited to producing changes in information. Thus, particular values held by a culture "receiving" science would be substantially affected only in so far as they were closely linked to some natural phenomenon concerning which a change in information was taking place. This description of what Downey has called the Accommodation Theory of science, culture and social structure<sup>156</sup> seems far more applicable to the Japanese case than that embodied in the Destruction Theory. Were it not for the corrosive effects of certain ideological influences it seems entirely likely the same would be true for Western countries as well.

## FOOTNOTES

1 Erwin Baelz, *Awakening Japan* (New York: The Viking Press, 1932), p. 149.

2 *Ibid.*, p. 150.

3 Sakurai Joji, *Omoide no Kazukazu* (Tokyo: Herald Sha, 1940), pp. 19-20.

4 Sawayanagi Masataro, "Gakumon Dokuritsu no Shin Undo to Sono Kompon Mondai", *Shin Nihon*, 5:2 (1915).

5 Nakamura Hajime, *The Ways Of Thinking Of Eastern Peoples* (Honolulu: The East-West Center Press, 1964), pp. 531-576.

6 Yukawa Hideki, "Modern Trend Of Western Civilization And Cultural Peculiarities In Japan", in *The Japanese Mind*, ed. Charles A. Moore (Honolulu: The East-West Center Press, 1967), pp. 54-55.

7 Kikuchi Toshihiko, "Introduction to Chapter XII", *Nihon Kagaku Gijutsu Shi Taikai*, (Tokyo: Nihon Kagaku Shi Gakkai, 1962), vol. I, Tsushi I, p. 489.

8 Yoshida Mitsukuni, "Meiji no Kagakushatachi", *Jimbun Gakuho* XXIV (March 1967), pp. 230-231.

9 Morito Tatsuo, *Kagaku Kenkyu Jo Ron* (Tokyo: Kurita Shobo, 1939), p. 15 and



Yuasa Mitsutomo, *Kagaku Shi* (Tokyo: Toyo Keizai Shimpō Sha, 1961), pp. 228, 282. Hereafter cited as Yuasa, *Kagaku Shi*.

10 Otsuki Shoichiro, Nojima Tokukichi and Maki Jiro, "Nihon ni okeru Kagaku, Gijutsu to Kagakusha", in *Kagaku, Gijutsu to Gendai*, ed. Sakata Shoichi (Tokyo: Iwanami Shoten, 1963), p. 283. Hereafter cited as Otsuki, Nojima and Maki, "Kagaku, Gijutsu". Judging from these statements there appears to be some disagreement within the Marxist camp over whether scientific ideas really belong to the realm of "superstructure" or not.

11 "Formal organization refers to the organizational pattern designed by management or some other agency. Informal organization refers....to the actual organizational relations as they evolved as a consequence of the interaction between the organizational design and the pressures of the interpersonal relations among the participants." Cf. Amitai Etzioni, *Modern Organization* (Englewood Cliffs, New Jersey: Prentice-Hall, 1964), p. 40.

12 *Nihon Igaku Hyakunen Shi*, ed. Tamura Masao (Tokyo: Rinsho Igaku Sha, 1957), p. 4.

13 Yuasa Mitsutomo, *Kagaku Goju Nen* (Tokyo: Jiji Tsushin Sha, 1952), p. 262 and Hiroshige Tetu, "Social Conditions For The Researches Of Nuclear Physics In Pre-War Japan", *Japanese Studies in the History of Science* No. 2 (1963), p. 84.

14 Sakata Shoichi, "Kenkyu to Soshiki", *Shizen* (September 1947), p. 10.

15 John Desmond Bernal, *The Social Function of Science* (London: Routledge and Kegan Paul, 1939), pp. 267-278.

16 Sakata, "Kenkyu to Soshiki", p. 13.

17 Otsuki, Nojima and Maki, "Kagaku, Gijutsu", p. 310.

18 "Values are modes of normative orientation of action in a social system which define the main directions of action without reference to specific goals or more detailed situations or structures." Cf. Talcott Parsons, *Structure and Process, In Modern Societies* (New York: The Free Press, 1960), p. 171. Hereafter cited as Parsons, *Structure And Process*.

19 Cf. Norman Storer, *The Social System of Science* (New York: Holt, Rinehart and Winston, 1966). Hereafter cited as Storer, *Science*.

20 Talcott Parsons, *The Social System* (Glencoe, Illinois: The Free Press, 1951), p. 343. Cf. pp. 60-67 for definitions. Hereafter cited as Parsons, *Social System*.

21 Cf. Parsons, *Structure and Process*, ch. IV for discussion of values and levels of generality, pp. 132-168.

22 Parsons, *Social System*, p. 342.

23 Alex Inkeles, "Industrial Man: The Relation Of Status To Experience, Perception and Value", *The American Journal of Sociology*, (July 1960), p. 29.

24 Kenneth Downey, "Sociology And The Modern Scientific Revolution", *The Sociological Quarterly*, 8:2 (Spring 1967), pp. 246-249. Hereafter cited as Downey, "Sociology".

25 Talcott Parsons, *Essays in Sociological Theory* (rev. ed.; New York: The Free Press, 1964), p. 23.

26 This contradiction appears in Professor Parsons' own writing. In contrast to the emphasis he placed on the "autonomy" of scientific ideas in 1938, in 1951 he wrote: "Science....requires quite definite conditions in the structure of the social system, as well as the cultural prerequisites in the form of the adequate state of existing knowledge. Knowledge....gets applied only through the mechanisms of institutionalization of roles within which the requisite (my emphasis) combinations of motivational and cultural

elements can develop. Only by becoming in this sense incorporated into the structure of the social system, thus coming to constitute more than a body of 'ideas,' does empirical knowledge [i.e. science] acquire the basis for a major influence on action." Cf. Parsons, *Social System*, p. 348.

27 *Ibid.*, pp. 152-153.

28 Among those which will be cited elsewhere see especially: Joseph Ben-David and Awhraham Zloczower, "Universities and Academic Systems in Modern Societies", *Archives Europeenes de Sociologie*, III:1 (1962), pp. 45-84. Hereafter cited as Ben-David and Zloczower, "Universities".

29 Joseph Ben-David, "Roles And Innovations In Medicine", *The American Journal of Sociology*, 65:6 (1960), p. 562. Hereafter cited as Ben-David, "Roles And Innovations".

30 Abraham Flexner, *Medical Education in Europe* (Boston: D. B. Updike, 1912), pp. 237-238. Hereafter cited as Flexner, *Medical Education in Europe*.

31 In this instance the term "institutionalized" refers to the fact that formalized occupational niches reserved for bacteriologists had been established at Tokyo University and elsewhere.

32 "Eiseigaku no Reimei o Kataru", *Nihon Iji Shimpō*, 1956 (October 21, 1961), p. 30. Hereafter cited as "Eiseigaku no Reimei".

33 Richard Bochalli, *Robert Koch* (Stuttgart: Wissenschaftliche Verlags-gesellschaft, 1954), p. 77.

34 "Ogata Masanori Sensei Tanjo Hyakunen Kinen Zadankai", *Nihon Iji Shimpō*, 1507 (March 14, 1955), pp. 1005-1028. Hereafter cited as "Ogata Masanori Sensei".

35 *Tokyo Daigaku Igaku Bu Hyakunen Shi*, ed. Tokyo Daigaku Igaku Bu Hyakunen Shi Henshu Iinkai (Tokyo: Tokyo Daigaku Shuppan Kai, 1967), p. 298. Hereafter cited as *Igaku Bu Hyakunen Shi*.

36 "Eiseigaku no Reimei", p. 31.

37 For a brief account of these matters in English see John B. Blake, "Scientific Institutions Since *the* Renaissance: Their Role in Medical Research," *Proceedings of The American Philosophical Society*, 101:1 (February 1957), pp. 57-58.

38 George B. Sansom, *The Western World and Japan* (New York: Alfred A. Knopf, 1958), pp. 199-205.

39 Albert M. Craig, "Science And Confucianism In Togukawa Japan", in *Changing Japanese Attitudes Toward Modernization*, ed. Marius B. Jansen (Princeton: Princeton University Press, 1965), pp. 149-151, and Shigeru Nakayama, *A History of Japanese Astronomy* (Cambridge: Harvard University Press, 1969), pp. 226-231. Hereafter cited as Nakayama, *Japanese Astronomy*.

40 Donald Keene, *The Japanese Discovery of Europe* (rev. ed.; Stanford: Stanford University Press, 1969), pp. 16-31.

41 The figure 108 is given by Watanabe Minoru, "Japanese Students Abroad And The Acquisition Of Scientific And Technical Knowledge", *Cahiers d'Histoire Mondiale*, IX:2 (1965), p. 291. Seventeen percent is an estimate based on the numbers of Tokyo University students graduating in various subjects during this period. Cf. Yuasa, *Kagaku Shi*, p. 161.

42 Miyajima Mikinosuke, *Kitasato Shibasaburo Den* (Tokyo: Iwanami Shoten, 1931), pp. 3-25. Hereafter cited as Miyajima, *Kitasato*.

43 Takano Rokuro, *Kitasato Shibasaburo* (Tokyo: Nihon Shobo, 1965), pp. 29-35. Hereafter cited as Takano, *Kitasato*.

44 *Milestones in Microbiology* ed. Thomas Brock, (Englewood Cliffs, New Jersey:

Prentice-Hall, 1961), pp. 138-140.

45 See the following sources for Kitasato's criticisms of the Tokyo University Medical School: Miyajima, *Kitasato*, p. 101; Kitasato Shibasaburo, "*Shorai no Igaku*", *Yomiuri Shimbun*. No. 11998 (October 11, 1910), p. 5; *Kitasato Kenkyu Jo Nijugo Nen Shi* ed. Kitasato Kenkyu Jo. (Tokyo: Kitasato Kenkyu Jo), pp. 5-6. *Kitasato Shibasaburo*, Statement to the *Yomiuri Shimbun*, No. 13468 (October 20, 1914) p. 4; and Kitasato Shibasaburo, "Chinjoshō", letter of October 30, 1914 to Count Hijikata Hisamoto, Chairman of the Daj Nihon Shiritsu Eisei Kaj, (Great Japan Hygiene Society), in *Dai Hygiene Society*), in *Dai Nihon Shiritsu Eisei Kai Zasshi*, 379, Supplement Sanjuni-ji 46 *Nihon Shiritsu Eisei Kai Zasshi*, 379, Supplement Sanjuni-ji Sokai, I (1914), pp. 2-3. *Nihon Teikoku Gikai Shi Kanko Kai*, 1926-30), vol. II. Cf. "Yosan Sainyu Saishutsu So Yosan An Mombusho Jokan, 1893 Fiscal Year", 4th Diet, Representatives, January 11, 1893, p. 762. Hereafter cited as *DNTGS* plus volume number.

47 Ben-David and Zloczower, "Universities", pp. 45-84.

48 Terasaki Masao, "Koto Kyoiku," in *Inoue Kowashi no Kyoiku Seisaleu*, ed. Kaigo Tokiomi. (Tokyo: Tokyo Daigaku Shuppan Kai, 1969), p. 378. Hereafter cited as Terasaki, "Koto Kyoiku."

49 Kimura Tadasu, *Inoue Kowashi Kun Kyoiku Jigyo Sho Shi* (Tokyo: 1894), pp. 73-74.

50 Terasaki, "Koto Kyoiku", p. 378.

51 *Ibid.*, pp. 358-361.

52 Okuda Kijin, "Okuda Bunsho to Kataru", *Ikai Jiho* 990 (June 14, 1913), pp. 1098-1099. Hereafter cited as "Okuda Bunsho to Kataru".

53 Herbert Passin, *Society And Education in Japan* (New York: Bureau of Publications, Teachers College, Columbia University, 1965), p. 69.

54 *Igaku Bu Hyakunen Shi*, pp. 293-314.

55 Flexner, *Medical Education in Europe*, pp. 331-332.

56 Twenty-eight was the number of German-speaking universities, not the number in the German Empire itself.

57 Theodor Billroth, *The Medical Sciences in the German Universities* (New York: The Macmillan Company, 1924), p. 179. Hereafter cited as Billroth, *Medical Sciences*.

58 Awraham Zloczower, *Career Opportunities and the Growth of Scientific Discovery In 19th Century Germany; With Special Reference To Physiology*. Unpublished M. A. Thesis at the Hebrew University of Jerusalem, 1960, p. 42. Hereafter cited as Zloczower, *Career Opportunities*.

59 Ben-David and Zloczower, "Universities", pp. 49-50.

60 Zloczower, *Career Opportunities*, pp. 6, 89.

61 Joseph Ben-David, "Scientific Productivity And Academic Organization In Nineteenth-Century Medicine", *American Sociological Review*, XXV:6 (December 1960), pp. 828-843. Hereafter cited as Ben-David, "Scientific Productivity".

62 Zloczower, *Career Opportunities*, p. 89.

63 "Okuda Bunsho to Kataru", pp. 1098-1099.

64 Quoted in Shigeru Nakayama, "The Role Played by Universities in Scientific and Technological Development in Japan", *Cahiers d'Histoire Mondiale*, 9:2 (1965), p. 348.

65 Terasaki, "Koto Kyoiku", p. 377.

66 *Teikoku Gikai Shugiin Iinkai Giroku*, ed. Shugiin Jimukyoku (Tokyo: Shugiin Jimukyoku, 1920), vol. 42. Cf. "Daigaku Tokubetsu Kaikai Hoan hoka Ikken", House of Representatives Budget Committee, Forty-second Diet, Second Session, February 13,

1029, p. 9. Hereafter cited as *TGSIG* plus volume number.

67 Miyagawa Yoneji, "Densembyo Kenkyu Jo", in *Omoide no Aoyama Tanemichi Sensei*, ed. Kumagaya Kenji (Tokyo: Aoyama Sensei Tanjo Hyakunen Sai Jumbi Inkai, 1959), p. 322. Hereafter cited as *Omoide*, ed. Kumagaya.

68 "Daigaku Igakka no Komponteki Kaisei", *Ikai Jiho*, 1047 (July 18, 1914), pp. 1248-1249.

69 "Bo Igaku Hakase Dan", *Tokyo Asahi Shimbun*, 10154 (October 24, 1914), p. 5.

70 *Gikai Seido Nanajunen Shi: Shugiin Giin Meikan*, ed. Shugiin Jimukyoku (Tokyo: Okurasho Insatsu Kyoku, 1962), p. 318. Hereafter cited as *Shugiin Giin Meikan*.

71 *TGSIG*, 40:1. Cf. "Tokyo Teikoku Daigaku oyobi Kyoto Teikoku Daigaku Rinji Seifu Shishutsu Kin Kuriiri ni kansuru Horitsuan hoka Ikken", House of Representatives Budget Committee, Fortieth Diet, Second Session, February 1, 1918, p. 5.

72 *Shugiin Giin Meikan*, p. 516.

73 *TGSIG*, 32-35. Cf. "Yosan Iinkai Giroku Sokki", House of Representatives Budget Committee, Thirty-fifth Diet, Seventh Session, December 16, 1914, p. 82.

74 "Ikai Dantai Undo Shi", Parts 15 and 16, *Ikai Jiho*, 1215-1216 (October 6-13, 1917), pp. 1757, 1801. Tsuchiya was a staunch proponent of a national federation of physicians and frequently attacked the "University Faction" for allegedly blocking its creation. He was associated with the loosely-knit *Min-i Ha* or "Private Physicians' Faction", referring to the fact that its adherents were neither members of the Tokyo University Medical School faculty nor government employees of any kind. In numbers *Min-i Ha* greatly exceeded the *Daigaku Ha*. Cf. "Ikai Dantai Undo Shi", Part 4, *Ikai Jiho*, 1200 (June 23, 1917), p. 1152.

75 Miyajima, *Kitasato*, pp. 304-305.

76 Zloczower, *Career Opportunities*, p. 20.

77 *Ibid.*, p. 43.

78 *TGSIG*, 40:1. Cf. "Tokyo Teikoku Daigaku oyobi Kyoto Teikoku Daigaku Rinji Seifu Shishutsu Kin ni kansuru Horitsuan hoka Ikken", House of Representatives Budget Committee, Fortieth Diet, Second Session, February 1, 1918, p. 8.

79 Ben-David and Zloczower, "Universities", pp. 49-50.

80 Sumeragi Shido, *Daigaku Seido no Kenkyu* (Tokyo: Yanagihara Shoten, 1955), p. 338 and Flexner, *Medical Education in Europe*, p. 387.

81 Ben-David, "Roles And Innovations", p. 558.

82 Billroth, *Medical Sciences*, p. 29 and Flexner, *Medical Education In Europe*, pp. 145-166.

83 *Medical Education*, ed. Commission on Medical Education (New York: Commission On Medical Education, 1932), p. 344. Hereafter cited as *Medical Education*, ed. Commission.

84 "Ikai Dantai Undo Shi", Part 12, *Ikai Jiho*, 1211 (September 8, 1917), p. 1608.

85 "Daigaku Kyoju no Naishoku", Part V, *Tokyo Asahi Shimbun*, 9747 (September 12, 1913), p. 5.

86 Flexner, *Medical Education in Europe*, p. 148 and Abraham Flexner, *Medical Education* (New York: The Macmillan Company, 1925), p. 40.

87 *TGSIG*, 42. Cf. "Daigaku Tokubetsu Kaikei Hoan hoka Ikken", House of Representatives Budget Committee, First Session, Forty-second Diet, February 12, 1920, p. 2.

88 *TGSIG*, 40:1. Cf. "Tokyo Teikoku Daigaku oyobi Kyoto Teikoku Daigaku Rinji Seifu Shishutsu Kin Kuriiri ni kansuru Horitsuan hoka Ikken", House of Representatives

Budget Committee, Fortieth Diet, Third Session, February 4, 1918, Testimony by Tadokoro Yoshiharu, Vice Minister of Education, pp. 19-20. Hereafter cited as *TGSIG*, 40:1, Tadokoro.

89 *Medical Education*, ed. Commission, p. 337.

90 *TGSIG*, 40:1, Tadokoro, p. 21.

91 *Tokyo Teikoku Daigaku Byorigaku Kyoshitsu Gojunen Shi*, ed. Nagayo Mataro (Tokyo: Teikoku Daigaku Igaku Bu Byorigaku Kyoshitsu Goju Shunen Kinen Kai, 1930), vol. I, pp. 97-98. Hereafter cited as *Byorigaku Kyoshitsu*.

92 Ogata Norio, "Kitasato, Ogata Ryo Sensei", *Nihon Iji Shimpo*, 1415 (June 9, 1951), p. 1566.

93 *Nagayo Mataro Den*, ed. Nagayo Hakushi Kinen Kai (Tokyo: Nagayo Hakushi Kinen Kai, 1954), pp. 257-260. Hereafter cited as *Nagayo Mataro Den*.

94 "Aki no Mizu", *Ikai Jiho*, 1003 (September 13, 1913), pp. 1712-1713.

95 *DNTGS*, IV. Cf. "Gakusei Chosa Kai Setchi ni kansuru Kengi An", Fourteenth Diet, Peers, January 31, 1900, p. 111.

96 "Daigaku Kyoju no Naishoku", Part IV, *Tokyo Asahi Shimbun*, 9745 (September 10, 1913), p. 5.

97 *DNTGS*, IX. Cf. "Tokyo Teikoku Daigaku Ika Daigaku Kyoju no Shokuseki no kansuru Shitsumon", Thirty-fifth Diet, Representatives, December 15, 1914, pp. 960-961.

98 See footnote 74.

99 Kitajima Ta-ichi, *Kitajima Ta-ichi Jiden* (Tokyo: Kitajima Sensei Kinen Jigyo Kai, 1955), p. 92. Hereafter cited as *Kitajima Ta-ichi Jiden*.

100 Kitajima Ta-ichi, Deputy Director of the Institute of Infectious Diseases under Kitasato, stresses the latter's close ties to the *Ikai Jiho* and claims this journal, which frequently attacked "factionalism" in the medical profession, was virtually his mouthpiece. Cf. *Ibid.*, p. 99.

101 Eric R. Wolf provides a description of the term "clique" or "faction" which is essentially identical with the Japanese term *batsu* or "faction" as used in this paper: "Compared to the...friendship relation which covers the entire role repertoire of the...participants, clique relations tend to involve primarily the set of roles associated with the particular job. Nevertheless, the clique still serves more purposes than are provided for in the formal table of organization of the institution. It is usually the carrier of an affective element, which may be used to counterbalance the formal demands of the organization, to render life within it more acceptable and more meaningful. Importantly, it may reduce the feeling of the individual that he is dominated by forces beyond himself, and serve to confirm the existence of his ego... But it also has important instrumental functions, in rendering an unpredictable situation more predictable, and in providing for mutual support against surprise upsets from within or without. This is especially true in situations characterized by a differential distribution of power. Power superiors and inferiors may enter into informal alliances to ensure the smooth prosecution of their relationship, to guard against unbidden inquiries from the outside or competition from the inside, to seek support for advancement and other demands." Cf. Eric R. Wolf, "Kinship, Friendship, and Patron-Client Relations in Complex Societies," in *The Social Anthropology Of Complex Societies*, ed. Michael Banton (London: Tavistock Publications, 1966), pp. 15-16.

102 *Kitajima Ta-ichi Jiden*, p. 58.

103 Henry George, Jr., "The Strong Men of Japan", *Munsey's Magazine*, (October

- 1907), pp. 103-111.
- 104 "Denken Ikan no Hishi", *Ikai Jiho*, (June 19, 1915), p. 9.
- 105 "Batsu no Ika Daigaku", Part II, *Tokyo Asahi Shimbun*, 10159 (October 29, 1914), p. 5. Hereafter cited as "Batsu no Ika Daigaku".
- 106 "Gakubatsu Daha no Yoi Ichirei", *Ikai Jiho*, 554 (January 28, 1905), p. 131.
- 107 Billroth, *Medical Sciences*, p. 174.
- 108 "Batsu no Ika Daigaku", Part II, p. 5.
- 109 *Manabe Kaiichiro*, ed. Manabe Sensei Denki Hensan Kai (Tokyo: Nihon Onsen Kiko Gakkai, 1950), p. 105. Hereafter cited as *Manabe Kaiichiro*.
- 110 Arima Eiji, "Tamashi ni Oeru", in *Omoide*, ed. Kumagaya, p. 367. Hereafter cited as Arima, "Tamashi".
- 111 Arai Tsuneo, "Mikake ni yoranu Shojikisha", in *Omoide*, ed. Kumagaya, p. 141.
- 112 "Ogata Masanori Sensei", pp. 1013-1014.
- 113 *Igaku Bu Hyakunen Shi*, p. 289.
- 114 *Byorigaku Kyoshitsu*, I, pp. 169, 228.
- 115 *Nagayo Mataro Den*, p. 96.
- 116 *Byorigaku Kyoshitsu*, I, p. 193.
- 117 Yamagiwa Katsusaburo, "Ko Nihon Byori Gakkai Meiyo Kaicho Miura Moriharu Sensei Tsuito no Ji", in *Byorigaku Kyoshitusu*, I, p. 211.
- 118 *Ibid.*, p. 208.
- 119 *Nagayo Mataro Den*, pp. 102, 121-122.
- 120 "Ogata Masanori Sensei", p. 1021. Statement by Dr. Ogata Norio. The matter in question concerned the disease *Rickettsia tsutsugamushi* for which Dr. Ogata Masanori incorrectly claimed to have discovered the pathogenic agent.
- 121 *Ibid.*, pp. 1010, 1013, 1021.
- 122 Mitamura Takujiro, "Tanemichi no Sekai Dotoku", in *Omoide*, ed. Kumagaya, p. 163. Hereafter cited as Mitamura, "Tanemichi".
- 123 Yamada Jiro, "Karusawa Sanso", in *Omoide*, ed. Kumagaya p. 348.
- 124 Imamura Yoshio, "Mangakyo", in *Omoide*, ed. Kumagaya p. 61 and Mitamura, "Tanemichi", p. 168. One possible explanation for this is that authoritarian behavior in a Japanese scientist was ideologically much less acceptable in 1959, due to post-war "democratic" reforms, than it was in 1930.
- 125 Uzaki Kumakichi, *Aoyama Tanemichi* (Tokyo: Aoyama Naika Doso Kai, 1930), p. 201.
- 126 Fujita Shuichi, "Aoyama Tanemichi", in *Kinsei Iretsu Den*, ed. Umezawa, Hikotaro (Tokyo: Chugai Igaku Sha, 1954), ch. 21, p. 274.
- 127 Arima, "Tamashi", p. 362.
- 128 *Byorigaku Kyoshitsu*, I, p. 193.
- 129 Yada Zennoshin, "Ketto Sokutei Kushin", in *Omoide*, ed. Kumagaya, p. 82; Sakai Tanibei, "Genkan Ban Nikki", in *Omoide*, ed. Kumagaya, p. 397; and Takahashi Akira, "Kanrei no Shukuji", in *Omoide*, ed. Kumagaya, p. 183.
- 130 Ezra Vogel, *Japan's New Middle Class* (Berkeley: University Of California Press, 1963), pp. 104-105.
- 131 *Kitajima Ta-ichi Jiden*, pp. 24-25.
- 132 Takano, *Kitasato*, pp. 72-80.
- 133 "Eiseigaku no Reimei", pp. 33-34.
- 134 This statement is an inference, not empirically demonstrable, based on the fact that both sections were working on the same problem (*Rickettsia tsutsugamushi*) at the same

time and in the same institution. Such a state of affairs in the United States or Europe usually indicates a complete lack of cooperation and there is every reason to believe it indicates the same thing in Japan as well!

135 *Manabe Kalichiro*, pp. 141-142.

136 *Igaku Bu Hyakunen Shi*, pp. 352, 356; 388-390. Unlike the other sections, the Aoyama Section was located at the Dai-ichi Clinic until 1902, not at the Medical School itself. Moreover, the figures given here for its membership in 1897 and 1908 are an estimate based on membership figures for the other two sections of Internal Medicine.

137 Joseph E. Smadel, "Scrub Typhus", in *Viral and Rickettsial Infections of Man*, ed. Thomas M. Rivers (2nd ed.; Philadelphia: J. B. Lippincott Company, 1952), p. 638.

138 *Nagayo Mataro Den*, pp. 160-161.

139 *Ibid.*, pp. 162-168.

140 W. Cramer, "The Late Professor Yamagiwa", *The Lancet*, CCXVIII (May 24, 1930), p. 1155 and Kanematsu Sugiura, "Katsusaburo Yamagiwa", *The Journal of Cancer Research*, XIV:4 (October 1930), pp. 568-569.

141 *Ibid.*, p. 1155.

142 *Nobel: The Man and His Prizes*, ed. H. Schuck et. al. (Amsterdam: Elsevier, 1962), p. 247.

143 *Byorigaku Kyoshitsu*, I, p. 261.

144 *Ibid.*, p. 261.

145 *A Modern Dictionary Of Sociology*, ed. George A. Theodorson and Achilles G. Theodorson (New York: Thomas Y. Crowell, 1969), p. 199.

146 Bernard Barber, *Science And The Social Order* (Glencoe, Illinois: The Free Press, 1952), p. 65. Hereafter cited as Barber, *Science*.

147 Cf. Don Martindale, *Social Life and Cultural Change* (Princeton: D. Van Nostrand Company, 1962), pp. 437-502.

148 William Caudill and Harry Scarr, "Japanese Value Orientations And Culture Change", *Ethnology*, I:1 (1962), pp. 53-91 and Ronald P. Dore, "Mobility, Equality And Individuation In Modern Japan", in *Aspects of Social Change in Modern Japan*, ed. R.P. Dore (Princeton: Princeton University Press, 1967), pp. 113-150.

149 Roger Krohn, "Science and The Practical Institutions", *Proceedings Of The Minnesota Academy Of Science*, 28 (1960), p. 165. Hereafter cited as Krohn, "Practical Institutions".

150 Yuasa, *Kagaku Shi*, p. 161 and *Who's Who: Hakushi in Great Japan*, ed. Iseki Kuro (Tokyo: Hattensha, 1922-30), vol. II.

151 Krohn, "Practical Institutions", pp. 165-168.

152 It may also be significant that the major advances in physics had occurred in Britain and France while the biomedical sciences reached maturity in Germany where the individualistic ethos was probably somewhat less potent.

153 Nakayama, *Japanese Astronomy*, pp. 204-205.

154 Downey, following Storer, uses the term "organized scepticism" which Storer himself states is essentially the same as Barber's term "individualism" Cf. Downey, "Sociology", p. 253; Storer, *Science*, pp. 79-80; and Barber, *Science*, p. 65.

155 John Peter Nettl, "Ideas, Intellectuals and Structures of Dissent", in *On Intellectuals*, ed. Philip Rieff (Garden City, New York: Doubleday Anchor, 1970), pp. 57-134.

156 Downey, "Sociology", p. 248.