

REPRINTS AND REFLECTIONS

Anticontagionism between 1821 and 1867^{♦*}

The Fielding H. Garrison Lecture

Erwin H Ackerknecht

Accepted 30 October 2008

Nothing might perhaps orient us quicker in our subject matter than perusal of what Hippolyte Marie Bernheim (1840-1919) (who was a contagionist himself and an authority on epidemic diseases before he became famous as a psychotherapist) had to say in 1877 concerning Jacob Henle, the teacher of Robert Koch.¹ Henle lives in our minds and textbooks as the man who “produced the first clear statement of the idea of a *contagium animatum*,” who fought a bold vanguard action. To Bernheim the situation appeared as follows:

“The serious observers recognized the emptiness of these fantastic concepts. Towards the middle of the century the doctrine of the *contagium animatum* was generally abandoned as a product of the imagination, lacking scientific foundations. Among medical leaders Henle was perhaps the last who defended in 1853 with strong determination the doctrine of the *contagium vivum* which he had defended already in 1840 with great logical vigor. Yet the parasitary doctrine has during the last 10 years regained considerable credit in public opinion as the result of new research and more positive findings.”

It becomes thus obvious that what to us appears a vanguard action, impressed Henle’s contemporaries rather as a rearguard action, the last gallant defense of a dead hypothesis. That the theories of contagion and the *contagium animatum* appeared old and obsolete to many in the first half of the 19th century is easily seen from the following examples:

Trotter speaks contemptuously in 1804 of the “relicts of the old animalcular hypothesis of contagion.”² “Alpha” states in the *Lancet* in 1832 that certainly not within the last fifty years have any diseases been added to the list of the contagious ones.³ Ozanam writes in 1835: “Nous connaissons un

grand nombre d’auteurs qui ont écrit sur l’animalisation des contagés . . . nous ne perdrons pas de temps à confuter ces hypothèses absurdes.”⁴ [“We know a great number of authors who wrote about the transmission of contagion by microscopic organisms . . . we will not waste time in refuting these absurd hypotheses.”] Wunderlich speaks in 1843 of the “remnants of childish ideas.”⁵ J.K. Mitchell, one of the inventors of the fungus miasma, regrets in 1848 that “Morgan and Holland reverted to the exploded animalcular theory of Kircher and Linnaeus,”⁶ “which has hitherto been so feebly sustained by proofs, as to have at no time received general favor from the profession, although supported by some eminent men in almost every period of medical history.”⁷ E.A. Parkes states in 1849: “During the last sixty years, however, the study of several diseases imperfectly known to the older physicians has added so many new facts to our knowledge of the several specific epidemic diseases, that the strict contagion theory has been insensibly undergoing alteration, until in the present day it tends to become merged in a higher generalization.”⁸ C.F. Riecke states in 1859 that the contagious doctrine has made no progress in centuries, and recent research has reversed the whole old authoritarian building of the contagion doctrine.⁹ Even a modern author, Major Greenwood, feels that Henle’s essay “is worth reading, but not better worth reading than a book published in 1546 and written by a Veronese physician, Hieronymus Fracastorius.”¹⁰ And Charles Singer, who has been a most indulgent and sympathetic historian of animate contagionism, states that, except for a small school at the beginning of the 18th century, no real progress was achieved between Fracastorius and Pasteur.¹¹

As a matter of fact, contagion and the *contagium animatum* were rather old theories around 1800. The youthful appearance they enjoy in our mind today is

* 1821 is the date of the famous Barcelona yellow fever outbreak and 1867 of the last great European cholera epidemic. In preparation of this paper the Library of the College of Physicians in Philadelphia and Dr. W.B. McDaniel II have been most helpful through liberal book loans.

♦ Ackerknecht EH. Anticontagionism between 1821 and 1867. *Bulletin of the History of Medicine* 1948; 22, 562-593. Abridged and with portions translated. Reprinted with permission.

exclusively due to the very thorough rejuvenation they underwent in the 1870's and 80's. Once we realize the oldness of both theories we can hardly be surprised in finding that their development was by no means unilinear, but a continuous series of ups and downs, of acceptance and refutation. The notion of contagion, almost unknown to classic antiquity, had become firmly entrenched in Western culture after the acceptance of the (contagionist) Jewish Old Testament as a holy book in Christianity. After the introduction of quarantines in most Christian countries in the 15th and following centuries the notion of contagion had in addition the official backing of the state, the worldly authority.

The idea of the *contagium animatum* had been formulated first in the 16th century by Cardanus, Paracelsus, and above all, by Fracastorius (1546). It had been further developed by V. de Bonagens, Fallopius, Mattioli and many others. It had not fared too well under the hands of Montanus, Valeriola, Sanctorius, and particularly Facio. But it had victoriously returned in the 17th century with the microscopic "worms" of A. Hauptmann, Father Kircher (1659), Chr. Lange, etc. Around the turn of the 17th century it had reached perhaps its highest elaboration with Lancisi, Andry, Vallisnieri, and after the Marseilles plague of 1721 with Bradley, Goiffon, and Lebègue. The satirical *Système d'un Médecin Anglois* (by M.A.C.D. Paris, 1726) had almost ruined it through ridicule. But though now undergoing progressive degeneration, according to Singer,¹² it had still inspired a Linné (1757), Plenciz, Lorry, etc., and in the 19th century Rasori, H. Holland, and Henle. With Ag. Bassi (1838), Davaine (1850), Villemin it started on a new experimental basis; but how was one to differentiate at that moment solid acquisitions from the uncritical productions of Donné or Hallier? The day belonged to Woehler and Liebig's cruel anticontagionist jokes (1839), fashioned after the Parisian "M.A.C.D." of 1726.¹³

It was, curiously enough, in the first half of the 19th century, that is shortly before their final and overwhelming victory, that the theories of contagion and the *contagium vivum* experienced the deepest depression and devaluation in their long and stormy career, and it was shortly before its disappearance that "anticontagionism" reached its highest peak of elaboration, acceptance, and scientific respectability. It might contribute to our understanding of the phenomenon when we realize that what happened to medicine in the first half of the 19th century and what looks now to us only like normal birth pains or vigorous growth, might just as well be regarded as a deep crisis.¹⁴ René la Roche called it downright a revolution, an event which is known to produce new things in the midst of a maximum of confusion and disorder. It is by no means incidental that the medical revolution paralleled everywhere so many political revolutions, and that the "anticontagionist revolution" in France was preceded, just like her political sister, by an American anticontagionist revolution.

The men who brought about this last victory of anticontagionism worked with unprecedented energy. The fact that such outstanding leaders of modern "contagionism" and thoroughly unromantic characters as William H. Welch and C.-E. A. Winslow have found understanding words for anticontagionism, seems to justify closer occupation with this movement.

"The official opinion, as expressed by sanitary authorities at that time (1848) was definitely hostile to the germ theory of disease. I attach, however, no great importance to this circumstance, for it is not clear what practical use sanitarians would have made of this theory with the knowledge existing at that time. Hypotheses born before their time are often sterile. They must have some relation to the state of knowledge existing at the time, and history affords many instances of the useful purpose served for a time by inadequate and even erroneous theories. It is doubtful whether any more useful working hypothesis concerning the sources of epidemics would have been framed in 1848 than that which guided most of the sanitary activities at that period and for many subsequent years, erroneous as it was, and tenaciously as it was held after it had served its primary purpose. This doctrine, as is well known, was the so-called filth theory of the generation of epidemic diseases."¹⁵

"We cannot dismiss the resistance of the medical profession to the doctrine of contagion as merely an evidence of hidebound conservatism. There were sound reasons for this attitude. The layman perceived the broad truth of contagion as he watched the plague spread from country to country and from seaport to seaport; but the physician knowing the facts more intimately realized that no *existing* theory of contagion taken by itself could possibly explain those facts. Contagion, before the germ theory, was visualized as the direct passage of some chemical or physical influence from a sick person to a susceptible victim by contact or fomites or, for a relatively short distance, through the atmosphere. The physician knew that such a theory was clearly inadequate. Cases occurred without any possibility of such a direct influence. Cases failed to occur when such a direct influence was present. Epidemics broke out without the introduction into the locality of any recognizable cases from without; and within the city or country they raged in a particular section and failed completely to spread beyond the border of that area. Outbreaks began and outbreaks ceased without any causes that would be directly related to the presence or the absence of the sick. Until the theory of inanimate contagion was replaced by a theory of living germs, and until to that theory were added the concepts of long-distance transmission by water and food supplies and, above all, of human and animal carriers – the hypothesis of contagion simply would not work."¹⁶

The anticontagionists were motivated by the new critical scientific spirit of their time. (Griesinger calls it somewhat sourly "Zweifelsucht"¹⁷).

Contagionism was so old that it seemed never to have been submitted to rational examination. So they did submit it. It is no accident that so many leading anticontagionists were outstanding scientists. To them this was a fight for science, against outdated authorities and medieval mysticism; for observation and research against systems and speculation. Chervin's battle cry was "non verbis, sed factis." ["not words, but actions"] J.A. Rouchoux stated that experience of the typhus and yellow fever epidemics of the Napoleonic wars had more than anything else undermined the belief in contagion,¹⁸ and coldly declared in the Academy of Medicine in 1832 that the experience with cholera "va achever le discredit des mesures sanitaires (quarantines)".¹⁹ ["will discredit the use of quarantine as a sanitary measure".]

Still, the vigor of our movement would remain largely unexplained, did we not realize the powerful social and political factors that animated this seemingly scientific discussion. Contagionism was not a mere theoretical or even medical problem. Contagionism had found its material expression in the quarantines and their bureaucracy, and the whole discussion was thus never a discussion on contagion alone, but *always on contagion and quarantines*. Quarantines meant, to the rapidly growing class of merchants and industrialists, a source of losses, a limitation to expansion, a weapon of bureaucratic control that it was no longer willing to tolerate, and this class was quite naturally with its press and deputies, its material, moral, and political resources behind those who showed that the scientific foundations of quarantine were naught, and who anyhow were usually sons of this class. Contagionism would, through its associations with the old bureaucratic powers, be suspect to all liberals, trying to reduce state interference to a minimum. Anticontagionists were thus not simply scientists, they were reformers, fighting for the freedom of the individual and commerce against the shackles of despotism and reaction. This second aspect of anticontagionism contributed probably no less than its scientific aspects to its gaining over the majority of those parts of the medical profession that were independent of the state.

That the anticontagionists were usually honest men and in deadly earnest is shown, among other things, by the numerous self-experiments to which they submitted themselves to prove their contentions. Between the plague auto-inoculation of Desgenettes in 1798 and Pettenkofer's swallowing of a cholera culture in 1892, (both men, by the way, were rather "contingent contagionists" than pure anticontagionists), a veritable epidemic of self-experimentation seems to have shaken the medical profession. Yellow fever self-experiments are reported e.g. from Pfirth v. Salun, Lavallée, Musgrave, Potter, Chervin,²⁰ Prost, Dorsey, O'Connor, Govin,²¹ Ffirth, Cathrall,²² Guyon,²³ and Puhlschneider. Famous are the plague self-experiments of Clot-Bey, the offers for plague self-experiment by

Chervin, Lassis, Costa, Lapis, and Lasserre,²⁴ and the cholera self-experiments of Fay, Scipio Pinel, Wayrot,²⁵ and J.L. Guyon. The amazing thing is that almost all of these experiments failed to produce the disease. They therefore greatly increased the faith of and in the anticontagionists. We hear only of a Dr. White dying in a plague self-experiment, the suicide of a Paris student who had too well succeeded with a syphilis inoculation,²⁶ and the death of the contagionist E. Valli from yellow fever in Havana in 1816 (V. overdid a little; he had already survived a successful plague vaccination self-experiment in Istanbul in 1803).²⁷

In their positive theories the anticontagionists were anything but uniform and often blissfully unaware of the fact that their theories were, especially when they adhered to the classic Hippocratic epidemic constitution or "atmospheric influences," even older than contagionism. Many followed a more modern and localized "miasma" theory (poison arising from decaying animal or vegetable matter, "filth"). From the miasmatic or "filth" theory to a purely social concept was but a short step.

In our discussions there did exist, like in all such situations, besides the two extremist wings, a large center of "moderates" that tried to compromise, the so-called "contingent contagionists," counting in its ranks such highly respected men as Milroy, James Johnson, Parkes, Riecke, etc. And it is precisely the attitude of this center which decided on the practical applications, and which best illustrates the general orientation of a given period. It is extremely typical for our period that the center, though admitting theoretically contagion in certain limits and as *one* possible factor of many, *practically*, that is in the condemnation and abolition of quarantines, the supreme test of one's convictions, followed the *anti-contagionists*. The anticontagionists, though castigating the center cheerfully for its inconsistencies, were well aware of this fundamental closeness of both tendencies²⁸. The change of the orientation of the center since the times of Richard Mead (1721), when on the basis of a similar theoretical compromise it headed towards *contagionism*, is very significant.

In spite of the name, none of the anticontagionists was an absolute anticontagionist, denying the existence of any contagious diseases. Even such radical anticontagionists as Ch. Maclean or J.A. Rochoux admit the existence of such contagious diseases as syphilis, gonorrhoea, smallpox, measles, and the itch²⁹. But the actual and imaginary clinical and epidemiological difference between these diseases and those "big three" against which the quarantines were mainly directed: *plague, yellow fever, and cholera*, confirmed them in their anticontagionism. Around these three diseases, which together with typhus constituted the main health problem of the period, did the discussion primarily evolve, and it is through a more detailed discussion of the attitude of the medical profession towards yellow fever, cholera,

plague, and typhus that we intend to picture anticontagionism between 1821 and 1867. Epidemics of smallpox, influenza, meningitis, dysentery, etc. ravaged Europe in the same period. But none of them found a similar scientific and emotional response, partly probably because none did kill quite as dramatically and extensively as the "big three", partly because none of these "minor evils" resulted in so hated an institution as the quarantine.

Limited in time and space, I am obliged to omit from my discussion more or less the American prologue of the anticontagionist revolution (Rush-Webster) and to treat in a very summary way its later representatives, the English sanitarians (Chadwick, Southwood Smith, John Simon, etc) or Pettenkofer and his followers. I feel justified in this procedure as there exist for Rush-Webster³⁰ as well as Chadwick Simon³¹ very excellent and detailed recent discussions by Winslow and others. Because of the same limitations, I have also quoted only a few of the most significant contributions out of a practically inexhaustible contemporary literature on yellow fever, cholera, plague, and typhus.

* * *

ANTICONTAGIONISM AND YELLOW FEVER

It was of great consequence for the success and spreading of anticontagionism in the 19th century that yellow fever, a disease which through its mechanism of transmission is far less of a "contagious" disease than either cholera, or plague, or typhus, was the first subject of the great 19th-century discussions between contagionists and anticontagionists, and that in the yellow fever discussion the anticontagionists were represented by a man of unusual intelligence, perseverance, and poise: Nicolas Chervin. It is also typical that practically *all* "professional anticontagionists," that is those men who not only accepted anticontagionism, but made the fight against the theory of contagion and quarantines their life work, had had their first epidemiological experiences with yellow fever. The easy victory of anticontagionism in the yellow fever discussion set a fatal pattern for later discussions on cholera and plague.

It seems that modern anticontagionism in regard to yellow fever really started in the famous Philadelphia yellow fever epidemic of 1793. René La Roche gives to Jean Devèze (1753-1825),³² one of the many French refugee physicians from San Domingo (La Roche himself was the son of such a San Domingo physician), the credit of having brought an idea that had been apparently long prevalent in the West Indian and other tropical yellow fever centers,³³ to a theater from which it was to conquer the world (Devèze regarded also plague, typhus and dysentery

as non-contagious.) You all remember how through his 1793 experiences Benjamin Rush was converted from contagionism to anticontagionism, how in 1799 the anticontagionist Philadelphia Academy of Medicine was founded. When Chervin 20 years later came to Philadelphia, he found but 4 or 5 contagionist physicians left.³⁴ The writings of the North American anticontagionists made a deep impression upon European doctors, which was reinforced through the fact that all the leading physicians of the ill-fated Napoleonic expedition to San Domingo (1802-3), that was wiped out by the yellow fever, returned as confirmed anticontagionists. (Trabue, FV Bally (1775-1866), A François (1775-1840), L Valentin (1758-1829), etc.) and an international authority like Alexander von Humboldt sided with the anticontagionists (1802).

Through the Napoleonic Wars the French and English became sensitive to the fact that in Spain they had a center of yellow fever right in their back yard. Outstanding French army doctors like P. Assalini (1750-1840) and FP Blin (1756-1834) found in 1805 and 1801 the Cadiz yellow fever epidemics, that killed 20 per cent of Cadiz' inhabitants, to be of a non-contagious character. Robert Jackson (1750-1827), the famous English army physician, came to the same conclusion in 1821.³⁵ In the yellow fever epidemic of Gibraltar of 1814 the majority of the local physicians voted anticontagionist in a kind of referendum.³⁶

When in 1821 a very fatal epidemic of yellow fever broke out in Gibraltar, the (contagionist) French government and the Academy of Medicine, becoming more and more alarmed, despatched a study commission consisting of the Academy secretary Et. Pariset (1770-1847), Bally, and François³⁷ to Barcelona in order to clear up the fundamental problems of the disease in view of future protective legislation. The three musketeers of contagion returned with a very contagionist report, obtained a life pension of 3000 Frs. per year; and a very stringent quarantine law was passed through the chambers in 1822.

The anticontagionists too had flocked to Barcelona, to study the epidemic, and in 1822 a manifesto on the 1821 outbreak appeared, signed by such well-known anticontagionists as the Frenchmen, S. Lassis (1772-1835), JA. Rochoux (1787-1852), the British Th. O'Halloran and Charles Maclean (ca. 1766-1825), the American John Leymerie, and 11 local physicians, among them 4 recent converts to anticontagionism, F. Piguillem, M. Duran, J. Lopez, and S. Campmany.³⁸ The manifesto showed that there was no positive proof that the disease was imported from Cuba; that it could satisfactorily be explained through the miasms of local filth, particularly sewers; that no infection through fomes had occurred during the epidemic; that it was strictly seasonal; that attendants to the sick showed below average morbidity; and that numerous cases were struck for the second time.

The manifesto helped Maclean defeat a quarantine law in the Cortes in 1822, but produced little reaction abroad. Especially in France everything seemed under control. At this moment *Nicolas Chervin* (1783-1843) returned from America, where since 1814, first practicing in Guadeloupe, then travelling all along the East coast of the continent from Guiana to Maine, he had studied yellow fever and collected a truly amazing and well-authenticated documentation in favor of anticontagionism. In North America, e.g. he had found 568 yellow fever anticontagionist physicians as compared to 28 contagionists.³⁹ Chervin immediately went to Spain to check Pariset's report, and the documentation he brought back did not leave much of the permanent secretary's hasty, hysterical pudding. In 1825-26 Chervin bombarded the Chamber with petitions to reopen the case of yellow fever quarantines on the basis of his documentation. The Chamber passed the problem to the Academy of Medicine. The Academy appointed a committee of 18 that examined the documentation for 11 months. In 1827 Coutanceau (1775-1831) submitted a report to the Academy, (primarily the work of Villermé, the greatest living French sanitarian, Réveillé-Parise, Emery and Lambert) that in spite of its diplomatic terms, criticized by Chervin, adopted Chervin's point of view, completely demolished Pariset's report, and recommended stopping further enforcement of the 1822 quarantine law. The Coutanceau report was, after long discussions, where especially RND Desgenettes (1791-1858), the former head of Napoleon's Medical Corps, and JB Louyer-Villermé (1776-1837) championed Chervin's cause, and in spite of the violent resistance of Pariset, of heavy governmental pressure and maneuvers, adopted by a majority of the Academy in January, 1828, and quarantine credits accordingly cut down by the Chamber. The Academy of Science took a similar stand⁴⁰. Chervin received the Grand Prix de Médecine of the "Institut" for 1828.

It is almost impossible to exaggerate the importance of this step of the French Academy, the leading medical corporation in the leading medical country of the period. It set the pattern for the Western world. It set the pattern for the Academy's own attitude in later discussions on cholera and plague. Only deep conviction could bring the Academy to withstand official pressure and to expose its own permanent secretary. The Academy remained faithful to Chervin. Together with P. Louis and Trousseau it delegated him to Gibraltar to study there the yellow fever epidemic of 1828. In 1832, the cholera year, it elected him a member, not because of his political radicalism, as the *Lancet* had claimed, but in spite of it.⁴¹ Chervin made a deep and lasting impression on the profession like no other contagionist. Even most of his adversaries spoke with respect of him. J. Raige-Delorme (b.c.1795), editor of the *Archives Générales* from 1823 to 1854, remained full of an unchanging admiration

for Chervin throughout the years.⁴² The four friends Chervin mentioned in his own testament – Réveillé-Parise, Londe⁴³, Rochoux, and Civiale – were all outstanding medical men. In 1863, 20 years after Chervin's death, the contagionist Charcot regarded Chervin's work on yellow fever still as final.⁴⁴

Chervin's influence was not so much due to the arguments he used, which are similar to those of all anticontagionists, e.g. coincidence of outbreak and ship's arrival, not importation; non-transmission to nurslings of affected mothers or hospital attendants; the disease strikes only in certain localities, sick fugitives do not infect; fomes do not infect; the contagionists do not observe their own laws, etc. etc.⁴⁵. It was due to his great conscientiousness, precision, and honesty – he submitted material adverse to his theory with the same industry as favorable material – and his poise – he kept the lunatic fringe of anticontagionism at a safe distance; he limited himself to what he actually knew – and to his perseverance, even more than to his brilliant intelligence and enormous knowledge, which made him tower above most contemporary contagionists and anticontagionists. His personal integrity and disinterestedness were above any doubt. Although representing the interests of a very wealthy class, he died a poor man. His published work consists only of pamphlets. His magnum opus on yellow fever remained unfinished. It is one of the tragic jokes of fate that Chervin's many talents and virtues were spent on a lost cause.

In the above-mentioned Gibraltar epidemic of 1828 the majority of the garrison's doctors and the majority of the official British inspecting commission, although presided by the violent contagionist Sir William Pym, gave a verdict against contagion.⁴⁶ The same conclusion was reached by the Inspector General of Army Hospitals, JL Gillkrest (d. 1845). This, by the way, was the last large yellow fever epidemic in Europe (except for the outbreaks in Lisbon in 1857 and Madrid in 1878) which made it all the easier for yellow fever anticontagionism to stay.

In 1850 D. Blair and the local physicians of Guiana, under approval of J. Davy, Inspector General of the British Army, declared yellow fever to be non-contagious.⁴⁷ The same conclusion was reached by the British Yellow Fever Commission in 1852,⁴⁸ and the General Board of Health in 1853. Riecke defended this opinion in 1854. In 1855 the Philadelphian R. La Roche's great anticontagionist classic on yellow fever came out. "As a work of profound erudition, at once complete and exhaustive, written in a scholarly style, and evincing the most patient and extra-ordinary research, the monograph on yellow fever, by Dr. La Roche, is without a rival in any language."⁴⁹ In 1859 G. Milroy (1828-1886), the famous English sanitarian, asked for removal of the useless yellow fever quarantines for the sick,⁵⁰ just as JK Mitchell had done in 1846.⁵¹

By 1868 opinion of the majority, under the influence of men like F. Mélier (1789-1866) and W. Griesinger (1817-1868), and of new observations, seems to have swung back to the concepts of contagiousness and importation of yellow fever, except for the English, who according to Dutroulau, were prejudiced through their commercial interests.⁵² In the United States the reaction seems in general to have set in even earlier, just as the wave of anticontagionism had started earlier.

CHOLERA AND ANTICONTAGIONISM

Yellow fever, fortunately, remained but a potential danger for Europe and discussion of its contagiousness more or less an academic problem. Cholera overran Europe and the world in four major pandemics during the 19th century, spreading terror like the medieval plague, killing millions (in England and Wales alone ca. 15,000), and constituting everywhere a tremendous medical, political, economic, and human problem.

Cholera had been a native of India for centuries, and the Anglo-Indian physicians became thus the first authorities on cholera for their Western colleagues, like the West Indian physicians had once been on yellow fever. These Anglo-Indians, especially those reporting on the 1817 epidemic, were confirmed anticontagionists. The whole medical body of Bengal and the majority of the Bombay physicians decided this way.⁵³ The Anglo-Indians were among the last to abandon anticontagionism in cholera.

The 1817 Indian cholera had advanced to the Near East in 1821 and knocked so loudly at the door of Europe that farseeing observers, contagionists (Moreau de Jonnés, born 1778) and anticontagionists (Maclean) alike foresaw that the next cholera wave, starting in India in 1826, would probably enter Europe. Governments, especially those of France and Russia, took early precautions in the form of rigid military cordons and quarantines. To no avail; in 1831 cholera overran Russia, and in 1831-32 the rest of Europe. Leon Colin⁵⁴ has left an excellent summary of the effect of the first great victory of cholera on epidemiological thought:

Cette marche rapide, à travers l'Europe de la première épidémie de choléra au moment même où l'on avait le plus grand espoir dans l'influence prophylactique des cordons sanitaires, la préservation de tant de pays qui, au contraire, étaient demeurés en libre communication avec les régions atteintes, diminuèrent aussi de beaucoup la confiance publique dans les mesures quarantaines. On sait qu'alors on révoqua en doute d'une manière absolue la transmissibilité de cette maladie, de l'homme à l'homme, et qu'on en revint à la doctrine du génie épidémique; la contagion fut presque, d'une manière générale, traitée de croyance chimérique, le progrès semblait être du

côté de ceux qui ne l'admettaient plus; leurs idées étaient considérées comme des idées de nature libérale par excellence, d'autant plus en rapport avec la dignité humaine que leur conséquence pratique était la suppression de toute entrave portant atteinte à la liberté de l'homme; l'esprit du temps était, en un mot, l'inverse de celui d'aujourd'hui; et, au lieu d'être voué au culte de la spécificité étiologique, on traitait volontiers d'esprits rétrogrades ceux qui admettaient encore la contagion du choléra, du typhus, et de beaucoup d'autres maladies.

[This rapid progress across Europe of the first cholera epidemic at the same time as there was the greatest faith in the prophylactic effects of *cordons sanitaires*, the well-being of so many people who were, on the contrary, in open communication with affected areas, also diminished public confidence in quarantine measures. This brought doubt about the absolute manner of transmission of this disease, from man to man, and one came back to the notion of a 'genie' of epidemiology; contagion was nearly always dealt with as a fanciful belief and progress seemed to be on the side of those who accepted no more than this; their ideas were regarded as those of a progressive nature, all the more so since they seemed more in line with human dignity in that the consequent practice was to remove any obstacles to man's freedom of movement. The spirit of the time was, in a word, the opposite of today; and instead of being dedicated to the notion of a specific aetiology, readily treated as retrograde spirits those who still admitted the contagious nature of cholera, typhus and many other diseases.]

European doctors could not fail to be impressed by the fact that the majority of those Western physicians, who encountered cholera first, rendered a verdict of non-contagious (eg Astrachan⁵⁵, Moscou⁵⁶). The numerous commissions that the French and other European governments sent into the cholera regions claimed the same results and found quarantines useless: Gerardin and Gaynard, who with Jules Cloquet formed a Russian commission;⁵⁷ the Polish commission (Dalma, Sandras, Boudard, Dubled, Alibert, Ch. Londe).⁵⁸ The same holds good for the great sanitarians F. Foy (1793-1867) and RHJ Scoutetten (1799-1870) who were sent to Warsaw and Berlin⁵⁹. Hammett's report on Dantzic was "lost" by the contagionist English Health Council⁶⁰.

When cholera struck England in 1831 the authorities and the overwhelming majority of physicians were contagionist. Thomas Wakley stopped an anticontagionist series by "Alpha" in the *Lancet* and came out strongly for contagion⁶¹. The adoption of "contingent contagionism" by James Johnson (1777-1845), the international authority on tropical diseases and physician extraordinary of the king, and of a straight anticontagionism by the highly respected A. Bozzi Granville (1793-1871), who had so valiantly defended contagionism and quarantines in the plague discussions of the 1820's, marks the turning of the tide.⁶²

In 1832 the editor of the *Edinburgh Medical and Surgical Journal* turned anticontagionist.⁶³ The books of J. Lizars (1793-1860) and T. Molison reinforced the anticontagionist trend, the Westminster Medical Society, probably the most active medical society in London at the time, voted anticontagionist (24:22)⁶⁴. G. Sigmond (b.1790), himself a convert, *stated in May that probably the majority of physicians was now anticontagionist*,⁶⁵ and even Wakley *tuned down his contagionism considerably*.⁶⁶ JG Gillkrest came out in favour of the anticontagionists⁶⁷, and H. Gaultier's book on the Manchester epidemic illustrated well the current anticontagionist approach.⁶⁸

Perhaps the outstanding promoter of anticontagionism in France in the 1832 epidemic was the famous physiologist, pathologist, pharmacologist, and clinician, François Magendie (1783-1855). In December 1831, when France was still free of cholera, he went to Sunderland to study the disease. His conclusions, as presented in the Academy, were that the disease was not imported and contagious but due to incredible social conditions – humidity, lack of ventilation and light, “filth” – and that quarantines were therefore useless. He won an easy victory against Moreau de Jonnés.⁶⁹ In the following Paris epidemic he showed great courage and skill in fighting the disease. His opinions were but reinforced through this experience,⁷⁰ and the events of 1848. Magendie felt that four out of the five quarantine diseases – leprosy, typhus, yellow fever, and cholera – were certainly not contagious, the fifth, plague, was probably also non-contagious.⁷¹ It is probably under his impulsion that the doctors of the Hôtel-Dieu (Petit, Recamier, Dupuytren, Husson, Magendie, Breschet, Honoré, Guéneau de Mussy, Samson, Caillard, Gendrin, Bailly) published an anticontagionist declaration⁷². An Academy of Medicine report and instruction of the same year, written by de Mussy, Bielt, Husson, Chomel, Andral, Bouillaud, and Double, is clearly anticontagionist by implication.⁷³ JB Bouillaud (1796-1881) expanded his anticontagionist views in a lengthy treatise of the same year and defended anticontagionism vigorously as a member of the Chamber in 1846⁷⁴. To this long and brilliant list of French anticontagionist clinicians we have still to add the names of Broussais,⁷⁵ Piorry,⁷⁶ HMF Desruelles (1791-1858),⁷⁷ of the hygienists F. Foy (1793-1867),⁷⁸ Ch. ES Gaultier de Claubry (1785-1855),⁷⁹ Ch. F Tacheron (b. 1790), and of the naval medical officers JJ Souty and F Levicaire.

It is practically impossible to list all outstanding French physicians who became anticontagionists. A list of the contagionists is far more feasible. It consists more or less of the two great surgeons Velpeau and Delpech, and the two psychiatrists Foville and Parchappe. And, of course, Pariset and Moreau de Jonnés.

The trend towards anticontagionism in the 1831 epidemic was the same in Germany as elsewhere (see the writings of HW Buek, N Weigersheim, Koelpin, etc.⁸⁰). But due to the general low status of

German medicine at the time it was of small importance.

The *pandemic of 1848-49* confirmed the anticontagionist beliefs of 1831-32 in practically all observers, and won even new adherents to the cause. The reports of the Anglo-Indian surgeons, collected by the anticontagionist Rogers, continued to emphasize anticontagionism.⁸¹ Prof. EA Parkes (1819-1876), who made for the General Board of Health a much admired analysis of the earliest cases of the London 1848 epidemic, had been formed in India, and it is therefore not surprising that his “modified contagionism” practically excluded the contagiousity of cholera as well as yellow fever and plague.⁸² WF Chambers (1786-1853), physician of the Queen, and the respected JA Wilson (1795-1883) preached anticontagionism.⁸³

The significant change in 1848 as compared to 1831 was that now for the first time in centuries a governmental agency was defending the tenets of anticontagionism: *the General Board of Health* (Ashley, Edwin Chadwick, and its medical member Dr T Southwood Smith (1788-1861)). The 1849 *Report on Quarantines* of the Board is clearly opposed to quarantine. Its 1848 Instructions⁸⁴ and its 1850 *Report on the Epidemic Cholera of 1848 and 1849* (based mainly on the surveys of Drs. J Sutherland (1808-91), RD Grainger (1801-65), and J Milroy), were openly and pronouncedly anticontagionist. Grainger made the same point in his Hunterian Oration of 1841. The Board of Health was not the least surprised by the increased morbidity and mortality of 1848-49 as compared to 1831-32. Far from attributing it to a relaxation of quarantines, it attributed it to the increase since 1831 of those conditions which it regarded as the causes of the disease and sources of the “miasma”: overcrowding, filth, dampness, faulty drainage, vicinity of graveyards, unwholesome water, and unwholesome food.⁸⁵ The Board and its medical followers were strengthened in their beliefs through the fact that they had been able to predict such an increase in morbidity and to designate in advance the very houses where cholera would break out.⁸⁶ (Usually houses infected also with typhus.) Their own “model buildings” – buildings struck in 1831, but remodeled since – remained this time free after sanitation.⁸⁷ The anticontagionists of the General Board of Health were pragmatists insofar as their emphasis was no longer on a discussion of the scientific problem of contagion, but on exposing and removing those conditions whose elimination would prevent cholera, no matter what the rationale. It is in this spirit that they were enthusiastically followed, eg by the *British and Foreign Medico-Chirurgical Review*.⁸⁸ This group thus illustrates most clearly the positive side of anticontagionism, although this positive tendency existed in all outstanding anticontagionists (eg B Rush, Chervin, the Barcelona Manifesto of 1822, etc.). The sanitary activities of the General Board of

Health, at least as objectionable a “waste of money” to the merchant class as quarantines, also show that the anticontagionists were more than mere mouth-pieces of a ruthless and economy-minded bourgeoisie. The depth of anticontagionism in the England of 1848 is visible from the fact that the conservative London College of Physicians, Maclean’s old pet enemy, came out in October, 1848 with a document admitting the impotency of cordons and quarantines.⁸⁹ And yet 1849 sees also the first publications of J Snow on cholera which were probably among the most effective gravediggers of anticontagionism.

1848-49 brings a reconfirmation of anticontagionism also in France. AA Tardieu (1818-1879), the great French hygienist, states in his interesting 1849 book that “it is very evident that measures taken in view of the contagion are entirely without avail, and that consequently the contagion itself is very improbable”.⁹⁰ He was joined in this opinion eg by the hygienists P Jolly (1790-1879) and JL Collineau (1781-1860), and the clinicians Martin-Solon (1795-1856), E Emery (1780-1856)⁹¹ and JHE Monneret (1810-68).⁹²

In Germany the *Medizinische Zentral Zeitung* was anticontagionist in regard to cholera.⁹³ So was the bright young star of German science, Rudolf Virchow.⁹⁴ CF Riecke’s (born 1802) anticontagionist writings were successful. The famous and very influential chemist Justus Liebig came out for anticontagionism.⁹⁵ The majority of Russian physicians was anticontagionist.⁹⁶

A Cholera Quarantine Conference, organized in Paris by Mélier in 1851, was practically without result, mostly because of English anticontagionist opposition under Sutherland. This is understandable in view of the fact that the evidence on the effect of quarantines in the first two pandemics remains rather contradictory and bewildering. Copenhagen was free of cholera with quarantine in 1831, heavily struck in 1852 after abolition.⁹⁷ Belgium reported good experiences with quarantine in 1831 and 1848.⁹⁸ On the other hand, Tardieu and Lasègue (the latter had made a personal study of the problem in Russia) claimed that there was no difference between the 1830 epidemic (with cordons) and the 1848 epidemic (without cordons).⁹⁹ Copeland’s explanation of the higher mortality in 1848 because of relaxation of quarantines¹⁰⁰ is balanced by the above-mentioned argument of the Board of Health. The Austrian emperor, the Prussian king, the French government (1832), the Dutch government, the London College of Physicians – all admitted publicly the ineffectiveness of quarantines and cordons.¹⁰¹

It seems that during the third cholera epidemic (1852-55) and even more so during the fourth (1865-67) – these two connected with major wars while their two predecessors had been connected with revolutions – the majority of the profession returned to contagion. Winslow has traced recently John Simon’s gradual development from anticontagionism

in 1849 to contagionism in 1865.¹⁰² This seems to be fairly typical, though some, of course, changed their positions earlier, some later, some never.

It is significant that the *Archives Générales*, still under Raige-Delormé’s leadership, softened their attitude toward contagion considerably and progressively after 1850, being 100% contagionist by 1865. In 1853 the Royal College of Physicians made a strongly anticontagionist statement on cholera.¹⁰³ The rôles had been reversed within 60 years, the College and government being now anticontagionist, the profession increasingly contagionist. A Hirsch stated in a survey in 1854 that after a prevalence first of contagionists, then of anticontagionists, now some compromise theory was ruling.¹⁰⁴ He quoted Snow still disparagingly.¹⁰⁵ Only two years later the same author claimed that the majority were contagionist (though he still gave a long list of anticontagionists) and spoke with the greatest sympathy of Snow’s and Budd’s work.¹⁰⁶ Although CF Riecke adopted formally an eclectic position in his 1854 book (spontaneous generation plus contagion plus epidemic constitution) his violent opposition to quarantine revealed his fundamentally anticontagionist position which to a large extent he derived from Hecker and Haeser. On the other hand, he had an inkling of the rôle of the healthy typhoid carrier.¹⁰⁷ This most important and revolutionary *notion of the healthy carrier* was more fully developed by *Pettenkofer* for cholera in and after 1855.¹⁰⁸ In 1857 Griesinger felt that since about 1852 the contagiousity of cholera was generally recognized.¹⁰⁹ In 1859 Milroy held cholera to be “feebly contagious,” but continued to oppose quarantines.¹¹⁰ In his 1861 book on puerperal fever even I. Semmelweis called cholera still “non-contagious”. SA Fauvel’s (1813-84) report of 1866 seemed to many definitely to prove contagiousity of cholera. Mélier reintroduced cholera quarantines in 1866, having re-established those for yellow fever for 1853. *One of the most signal defeats of anticontagionism was the adoption of the importation and contagiousness of cholera by the Anglo-Indian medical men in 1866, after a resistance of more than 60 years.*¹¹¹ And Virchow, the anticontagionist of 1848, admitted in 1869 that “all the more recent research suggests an independent organism, especially a fungus” (as the cause of cholera).¹¹² That anticontagionism died hard is visible from the fact that the French Academy still received 36 anticontagionist (as against 109 contagionist) reports in an 1884 outbreak of cholera (Philippe Ricord (1800-89) defended in these discussions the non-contagiousness of cholera), and that the British government denied the imported character of a cholera epidemic in Egypt as late as 1885.

PLAGUE AND ANTICONTAGIONISM

When plague struck the English and French troops during the Napoleonic expedition in Egypt in 1798-99, the medical commanders of the French (Assalini,

Desgenettes) as well as of the English troops (Sir Robert Wilson) agreed that the disease was non-contagious.¹¹³ This fact can be found in the 1819 report of a committee of the House of Commons on plague and quarantine, representing "the first official inquiry ever made concerning this institution". Although the result of this inquiry was procontagionist, the mere fact of having obtained it at all was a considerable performance. It was due to the perseverance of Charles Maclean (born c. 1768) who deserves some more detailed attention because of the great rôle he played in the development of anticontagionism.

Maclean had entered the service of the East India Company in 1788, studied yellow fever in Jamaica in 1790, and worked in India since 1792. In 1796 he published his first anticontagionist book in Calcutta,¹¹⁴ which obviously found wide attention in and outside of India. An American edition appeared in 1797 and a German translation in 1805. Expulsed from India in 1798 because of a radical political pamphlet, he drifted around observing, writing, agitating, serving with the English army in 1804-5, and joining eventually the Levant Company in 1815. In the Eastern Mediterranean he did serious research on the contagiousity of plague, which he promptly contracted in an Istanbul plague hospital, and on which he published a two-volume book in 1817.¹¹⁵ This book induced the Government to consult the arch-contagionist London College of Physicians in 1818 on plague and quarantines, and resulted in the above mentioned Committee of the House of Commons in 1819. We have encountered Maclean already in the Barcelona yellow fever epidemic of 1821, and obtaining rejection of a quarantine law by the Cortes, Oct. 18, 1822. Maclean seems to have died shortly before his final triumph, that is the relaxation of the plague quarantine by the British government in 1825.

There is no lack of evidence that Maclean made a deep and lasting impression on his contemporaries. Séguier, the French consul general in London, reported in a letter to his government of May 20, 1825, "the opinion of Dr Maclean, who pretends that plague is not contagious, is prevalent in England".¹¹⁶ Tweedie, himself a contagionist, stated for the same period the "more general disbelief in the contagious nature of the plague".¹¹⁷ A collaborator of the *Lancet* in 1825 declared on the authority of Maclean that cholera was not contagious.¹¹⁸

Maclean gave a summary of his opinions in a memorandum to the Spanish Cortes in 1822 during his campaign against the pending quarantine laws.¹¹⁹ He differentiated "epidemic" from "contagious" diseases. While the true contagious diseases (syphilis, smallpox, etc) had a specific virus (Maclean was one of the few at the time to poke fun of the idea of spontaneous generation of disease virus¹²⁰); had a clear-cut clinical picture; occurred independently of season, the state of air, and other circumstances; and struck a person but once; the "epidemic" diseases

(primarily yellow fever, plague, typhus, and cholera) had protean forms, were clearly seasonal, and could attack the same person repeatedly. The burden of the proof that these diseases *were* contagious was, in Maclean's opinion, with the other party. But he drew attention to such facts as that attendants, doctors, the family of the sick, etc. experienced in his "epidemic" diseases no above average morbidity (quite different in this respect from "contagious" diseases like smallpox); these epidemic diseases remained localized and seasonal. They would not spread in time and space without limit, as they should, were they contagious. Maclean also claimed that plague fomes proved to be harmless in Turkey, recurrence of plague in the same person frequent, and quarantines completely ineffective to prevent epidemic diseases. In his positive theories Maclean was far less consistent: plague was to him entirely dependent on atmospheric influences;¹²¹ Irish typhus resulted from want of food, employment, and hope;¹²² yellow fever from filth;¹²³ and cholera from "local causes" and "winds".¹²⁴ Quarantines were really the cause of 19/20 of all cases in epidemics by enforcing confinement in pestilential air; producing concealment of the disease, desertion of the sick, and deadly terror. Quarantines were amoral, ineffective, and the source of enormous gratuitous expenses and vexation.

Maclean's ideas were too extreme, his character too unbalanced, his death too early to effect the general change of ideas concerning the contagiousity of plague. This deed is attributed by Griesinger to *Clot-Bey*. AB Clot (1793-1868), French-born and educated, had become the chief army surgeon of the Egyptian dictator Mehmet Ali in 1825. His abilities as a surgeon, his work as an organizer of sanitary improvements, of a medical school, a school of pharmacy, and a school for midwives in Egypt won him international fame. In a great plague epidemic in 1835 Clot's countryman, LR Aubert-Roche (1809-74), another remarkable character, convinced him of the non-contagiousity of plague and the uselessness of quarantines. From now on Clot-Bey spent a great deal of his considerable energies and prestige on fighting the theory of plague contagion and plague quarantines. His last book, written in 1866, two years before his death, at the age of 75, is called *Derniers mots sur la peste* [Last words on the plague]. In their anticontagionist attitude Clot-Bey and Aubert-Roche seem to have been in full harmony with the majority of the European physicians practicing in Egypt. Egypt was for centuries regarded as the home of plague, like the West Indies ranked as the home of yellow fever, and India as the home of cholera. And the Egyptian physicians were anticontagionists in regard to their main "native" disease, like the West Indians and Anglo-Indians had been in the corresponding case.¹²⁵

England and Austria had abolished plague quarantine in 1841. In France it was felt that clarity on the subject was needed before one could overcome

administrative opposition, especially in Marseilles, to similar changes. The Academy of Medicine charged a committee with a report which was submitted by CR Prus (1793-1850) in 1846, hotly debated for many months, and eventually adopted after the usual bickering with the contagionist government and smoothing of formulations. This report, which among other things served as a model of Virchow's famous Upper Silesia report of 1848, followed in its details more the brand of anticontagionism as represented by Aubert-Roche, a "localist", "miasmatic", and sanitary reformer, than the one of Clot-Bey, a "tellurist" and Hippocratic believer in "atmospheric conditions". Prus' main conclusions were that plague was not imported either in Egypt or Syria or Turkey, but arose spontaneously out of humid houses, putrifying animal matter and other filth. That neither contagion nor the contagiousness of fomes nor the inoculability of plague were proven; that the miasma was propagated through the air, not through patients; that ships were dangerous only in ports suffering themselves from an epidemic constitution; and that, therefore, progress of civilization was the only preventive against plague, and Egypt needed mostly a good administration. In the discussion the main champions of contagionism were Pariset and Bousquet, of anticontagionism F Dubois d'Amiens (born 1799; he became the successor of Pariset one year later), Londe, Piorry, and Rochoux. In adopting the Prus report on plague the Academy sided again with anticontagionism, as it had done in the case of yellow fever in 1828 and cholera in 1832. The discussion lost much of its practical value through the long absence of plague epidemics in Egypt after 1845.

TYPHUS AND ANTICONTAGIONISM

An analysis of the attitude towards typhus in our period is made somewhat difficult by the fact that explicit differentiation between typhus and typhoid was rare even in the 1850's, in spite of the classic work of Bretonneau (1826), Louis (1829), and particularly WW Gerhard and Pennock (1837). Still, the disease to which we now apply the term typhus was so obviously contagious that it was recognized as such even by an arch-anticontagionist like Rochoux,¹²⁶ (who, by the way, was one of the first to separate typhus and typhoid). Typhoid, on the other hand, seemed so little contagious that its "non-contagiousness" served long as one of the differentiating criteria. Especially the Paris School (eg Andral, Chomel in 1834) upheld its non-contagiousness.¹²⁷ Murchison found it only "feebly contagious" in 1862,¹²⁸ and in 1863 the contagionist Charcot had still to report on many typhoid anticontagionists.¹²⁹

The idea of the non-contagiousness of true typhus developed thus late, rather as a by-product of the

general anticontagionist wave, and was of relatively short duration. Among the first to deny the contagiousity of typhus were Elliotson,¹³⁰ and Corrigan,¹³¹ the great Irish clinician and statesman who saw in famine the sole cause of the terrifying Irish outbreaks of 1800, 1817-18, and 1826.

When Virchow, at the occasion of the Upper Silesia epidemic of 1848, declared that "no facts exist that prove contagion, and certain experiences speak against contagion",¹³² he was well aware of the novelty of the statement. He was, on the other hand, not a little proud of it, regarding it as truly scientific and modern. The anticontagionism of Grainger, Adams, and Armstrong in regard to typhus showed that the English sanitary movement was equally radical.¹³³

The contagionist Millies, in discussing the problem in 1857, stated that numerous anticontagionists did still exist, especially in Paris.¹³⁴ This might, of course, be a confusion with typhoid. But Murchison (also a contagionist) made the same statement in 1862.¹³⁵ And it is reiterated by Foerster in 1863.¹³⁶ Yet by 1868 Virchow, one of the authors of anticontagionism in typhus, declared: "There is much exaggeration, yet the fact remains that the typhus diseases and especially petechial fever may become contagious; that sometimes they may acquire even this quality to the highest degree". And by 1871 he admitted: "As much as I hesitated formerly to admit that contagion is the ordinary way in which typhus epidemics develop, I must confess today that I, like so many earlier observers, am more and more forced, through continuous experience, into the contagionist camp."¹³⁷

PRACTICAL AND THEORETICAL ASPECTS OF ANTICONTAGIONISM

After having studied the scientific triumphs of anticontagionism in the 1820's, 1830's, and 1840's, and its decline in the 1850's and 1860's, it might be indicated to summarize the practical results obtained by the anticontagionists in the relaxation and abolition of quarantines and cordons, which, after all, was their main goal.

The first to change their quarantine acts were the English and the Dutch in 1825.¹³⁸ Further relaxations occurred after 1831 and 1841.¹³⁹ From 1849 on up to the 1880's English official bodies came out with anti-quarantine declarations. The first changes in France occurred in 1828 and 1832. In 1835 even Marseilles, super-contagionist since the plague of 1721, softened her quarantines.¹⁴⁰ In 1849 quarantines were suppressed altogether, reinstated in 1850, abolished for cholera again in 1853, and reinstated in 1866.¹⁴¹ In the general disappointment with quarantines and cordons after 1831 large concessions were made to the anticontagionists in Austria (1841) and Russia (1847).

The practical successes of anticontagionists were not limited to their victories over quarantines.

Their operations against “filth” increased greatly their prestige. While it was difficult for the contagionists to prove that a respective epidemic would have been even worse without quarantines, health improvements after removal of “filth” seemed to be causally related to the latter action. Barcelona and Alicante did not experience further yellow fever epidemics after such campaigns in 1827, respectively 1804. The General Board of Health could point in the cholera of 1848-49 to the immunity of its cleaned “model houses”. (Those parts of Hamburg that had burned down after 1831 and had been rebuilt also remained healthy in 1848.)¹⁴² Typhus morbidity¹⁴³ and mortality¹⁴⁴ had considerably decreased from the 1840’s to the 1860’s, thanks to the sanitary measures of the anticontagionist Board of Health and its successors. The anticontagionist Aubert-Roche had an enviable health record during the construction of the Suez Canal in the 1850’s and 1860’s.

In examining critically the theoretical foundations of anticontagionism, we should be aware of the fact that certain basic weaknesses were common to *both* parties, anticontagionists and contagionists. Both parties occasionally used unreliable information. Both parties were still obsessed with the Hippocratic idea of the air as primary medium of transmitting the noxious element, whether miasma or contagium, at the expense of any other possibility (this idea prevails even with Henle and Holland). In both parties those were still numerous who believed in the fundamental unity of fevers¹⁴⁵ Both parties suffered from the “fallacy of a single cause”.¹⁴⁶ Both parties were reduced to reasoning by analogy, a procedure the dangers of which were clearly shown by Wunderlich.¹⁴⁷ Both parties used the animal experiment still very little, and what experimenting they did lacked method and inventiveness. Though the experiments of the next generation did not solve all problems either, they afforded an uncomparably higher degree of certainty than the mere observations on which both parties based their reasoning. Among these observations the contagionists would usually pick a set of more or less true facts that confirmed their theory, leaving out another set of equally true, but incompatible facts, which in their turn the anticontagionists would triumphantly present as proof of their theory. Or, “facts” and “observations” being highly complex and ambiguous, both parties would take up the same fact, but succeed in interpreting it in the contrary sense.¹⁴⁸ One of the truest statements in this direction is that of Wakley that the most disturbing facts (like low morbidity of attendants) *were fundamentally not explained by either theory*.¹⁴⁹ Some have not been explained up to this very day. “Observations” depended also to a large extent on the location of the observer. Brochard writes in 1851: “Les médecins conduits par leur observation à la croyance d’une forme contagieuse habitent tous de petites localités... rien ne manque à leurs enquêtes; mais l’autorité manque à leurs paroles.” [Their observations

led doctors to a belief in a contagious form in all small localities... nothing was lacking in their observations, but their words lack authority.]¹⁵⁰ Griesinger too explains anticontagionism through the rule of big city doctors. “But in Paris, as in all big cities, the facts of contagion of as frequent a disease (as typhoid) can rarely be observed conclusively.”¹⁵¹ The great triumphs of a rapidly growing chemistry, the influence of a Liebig, the recent victories over “vitalism” would particularly militate against a biological explanation of epidemics as given by the *contagium vivum*.

I am afraid that, forced to decide ourselves a hundred years ago on the basis of the existing materials, we would have had a very hard time. *Intellectually and rationally the two theories balanced each other too evenly. Under such conditions the accident of personal experience and temperament, and especially economic outlook and political loyalties will determine the decision. These, being liberal and bourgeois in the majority of the physicians of the time brought about the victory of anticontagionism.* It is typical that the ascendancy of anticontagionism coincides with the rise of liberalism, its decline with the victory of the reaction. Of course, the latter was not the only factor of the decline of anticontagionism. Was it new discoveries as Bernheim and others declared? Certainly, Snow and Budd made quite an impression. Yet, some doubts are at least allowed, in view of the fact that the change was prior to the decisive discoveries of Pasteur, Koch, etc. The discoveries of Davaine and Villemin were isolated and little known. Henle’s crown witnesses, the itchmite or the fungi in skin diseases, could most easily be dismissed as inconclusive and bad analogies by the anticontagionists,¹⁵² when even a contagionist like Trousseau reacted to these discoveries only by no longer counting scabies and favus among the contagious diseases, but transferring them to the parasitary group.¹⁵³ Roy states quite correctly in 1869:

“Mais la difficulté que les auteurs de cette doctrine (Henle and Holland) éprouvèrent à généraliser des faits de parasitisme externe aux maladies du sang la laissa tomber assez longtemps dans l’oubli et tous les pathologistes s’en tinrent à la théorie que M. Liebig commença à développer dès 1839.”

[But the difficulty for the instigators of this doctrine (Henle and Holland) proved to be to generalise the fact of external parasitism to diseases of the blood. It had lain forgotten for so long, and all the pathologists stuck to the theory which M. Liebig began to develop in 1839.]¹⁵⁴

A. Hirsch makes a somewhat revealing statement in 1856: “The more the belief in the contagious nature of cholera became widespread in the medical world, the smaller became the number of facts in which contagious genesis could not be proven.”¹⁵⁵ This suggests that to a certain extent emotional conversion, based on the accumulated despair of repeated cholera epidemics and on the normal “swing of the pendulum,” preceded

intellectual insight and discovery, rather provoking than being provoked by the latter. While contagionism had “pragmatically” hardly done better than anticontagionism, it could claim one major accomplishment at this turning point: it had kept alive interest in that part of experimental research that would bring the solutions of the future. Henle had produced Koch.

Far from underplaying the economic implications of their fight, the anticontagionists usually emphasized readily this popular aspect of the problem. They wrote long and detailed dissertations of exactly how many millions of pounds, francs, or dollars were yearly lost through the contagionist error.¹⁵⁶ Chervin, who characterized the whole as a political, administrative, moral, medical, and commercial problem,¹⁵⁷ was not afraid of such revealing word combinations as “question du plus haut intérêt pour l’humanité et le commerce” [question of the greatest interest for humanity and for trade] or “entraver le commerce et consacrer une erreur funeste à l’humanité”. [to block commerce and to create a disastrous error for humanity.]¹⁵⁸ Gaultier wrote in 1833: “Quarantine is useless, and the injury it inflicts on the commercial relations and maritime intercourse of the country is an absolute and uncompensated evil.”¹⁵⁹ Anticontagionist medical journals reprinted speeches of commerce-minded deputies.¹⁶⁰ Liberal and commercial newspapers like the *Journal de Commerce*, the *Constitutionnel*, and the *Courier* supported Chervin in 1827. French medical authors of the 60’s and 70’s saw very clearly the connections between anticontagionism and commercial interests – in England.¹⁶¹ Still, to call the anticontagionists simply “mouthpieces of commercial interest” would be a regrettable oversimplification of the situation. To many of them both slogans: freedom of commerce (no quarantines), and freedom of science (anticontagionism) were, together with others, like freedom of the individual (against any bureaucracy), the natural expression of the same fundamental attitude and social position. Some, as the following quotation from Maclean shows, were perhaps not so much interested in commerce at all, but tried, in appealing to commercial interests, to mobilize every possible ally:

“I am rather at a loss to conceive how their being injurious to commerce could, in a commercial country, be regarded as an argument against seeking the abolition of otherwise pernicious establishments . . . For my part, far from thinking it culpable to have availed myself of the support of commerce in combating the ridiculous but very pernicious dogmas of medical schools . . . I am very free to confess that I have, upon this occasion, diligently sought to range every interest, over which I could exercise the smallest influence, on the side of truth.”¹⁶²

It should also not be overlooked that into the contagionist attitude no less earthly factors entered, like the needs of real estate interests in denying “local causes” of epidemics.

The political background of the anticontagionist discussion was no less obvious. The leading anticontagionists – Maclean, Chervin, Magendie, Aubert-Roche, Virchow, etc. – were known radicals or liberals. The leading contagionists (with the lone exception of the liberal professor Henle) were high ranking royal military or navy officers like Moreau de Jonnés, Kerandren, Audouard, Sir William Pym, Sir Gilbert Blane, or bureaucrats like Pariset, with the corresponding convictions. The anticontagionists skillfully interspersed political arguments in their discussion. Maclean did not tire to trace contagionism back to the Pope’s political maneuver of transferring the Council of Trent in 1547, made possible by the ad hoc contagionist theories of his servant Fracastorius,¹⁶³ published, by the way, in 1546. This was also a fine occasion to mobilize anti-catholic and antireligious emotions. Appealing to the “irreverence of the age for mere authority,”¹⁶⁴ Maclean thundered against the “monopoly” of the College of Physicians.¹⁶⁵ The same spirit was kindled by reminders of the shooting of three anticontagionists in Thorea in 1815, the same act of violence in Noja in Apulia in 1815, the burning of Dr Armesto’s anticontagionist book in Spain in 1800, and the hanging of an anticontagionist doctor and tailor in Brandenburg in 1707.¹⁶⁶ To Maclean the very worst aspect of the quarantines was that they were ready “engines of despotism”.¹⁶⁷ Chervin expressed freely his disgust with the “servile spirit” that penetrated French contagionists, but satisfied himself that those in power would not be there tomorrow.¹⁶⁸ Writing this three years before 1830, he proved not too bad a prophet.

Economic factors did not only determine the stand of many in the anticontagionism discussion. *Economic factors* were consciously used by many to give a *causal explanation of epidemics* in our period (see Maclean above, Villermé, Magendie, etc.). This “sociological” (as contrasted to biological) theory of epidemics can be found already in the 18th century under the influence of enlightenment. (JP Frank, Chassanis, Targioni-Tozzetti). Yet its spread is a feature of the 19th century. I have already discussed in my doctor’s thesis in detail Virchow’s famous social epidemiological ideas, which were *one* of the reasons for his later reluctance to accept the results of bacteriology,¹⁶⁹ and more recently Virchow’s French predecessors.¹⁷⁰ In listing the epidemiological theories of the 19th century, I feel it would be justifiable to add to the two great divisions: the *physico-chemical or geographic* (including the “tellurists” and “miasmatisists”) and the *biological* (parasitists, bacteriologists), a third one, the *sociological*, represented by such men as Virey, Villermé, Mélier, Aubert-Roche, Alison, Davidson, and Virchow. The lines of division will not always be clear-cut and coincide with other lines of division. A “sociologist” might, for instance, be a contagionist (Davidson) or, more often, an anticontagionist.¹⁷¹ The essential characteristic of the sociologist is that all

these problems become of a very secondary importance when compared to social factors. Typical in this respect are: the message of Gerardin and Gaynard from Poland: "Les Commissaires pensent que le principal préservatif du cholera consiste dans l'amélioration de la condition sociale des populations." [The Commissioners think that the principal protection against cholera lies in the improvement of the social condition of the people.]¹⁷² Aubert-Roche's motto for his plague book, which is also the basis of the Prus report of 1846: "La civilisation seule a détruit la peste en Europe, seule elle l'anéantira en Orient," [Civilisation alone destroyed the plague in Europe, it is the only thing which will destroy it in the East]. or the typhus studies of Alison, Bateman, Corrigan,¹⁷³ and Franque.¹⁷⁴ I cannot take up the whole Virchow complex here again, but I would like to refer at least to such typical features as the conclusions of his 1848 report, his paper on the epidemics of 1848,¹⁷⁵ his thesis that social conditions determine whether typhus or typhoid develops,¹⁷⁶ his interpretation of cretinism as a social disease,¹⁷⁷ etc. The sociological theory, claiming a kind of "social epidemic constitution," suffers often from the same haziness that is so characteristic of the theories of telluric "epidemic constitution."

This strange story of anticontagionism between 1821 and 1867 – of a theory reaching its highest degree of scientific respectability just before its disappearance; of its opponent suffering its worst eclipse just before its triumph; of an eminently "progressive" and practically sometimes very effective movement based on a wrong scientific theory; of the "facts," including a dozen major epidemics, and the social influences shaping this theory – offers so many possible conclusions that I feel at a loss to select one or the other, and would rather leave it to you to make your choice. I am convinced that whatever your conclusions, whether you primarily enjoy the progress in scientific method and knowledge made during the last hundred years, or whether you prefer to ponder those epidemiological problems, unsolved by both parties at the time and unsolved in our own day, all your conclusions will be right and good except for the one, so common in man, but so foreign to the spirit of history: that our not committing the same errors today might be due to an intellectual or moral superiority of ours.

Notes

¹ *Dict. Encycl. Sc. Med.* (A. Dechambre ed) 1. Ser., vol. XX, p.7, 1877.

² Scott, H.H., *A History of Tropical Medicine*, Baltimore, 1942, p.29.

³ *Lancet* 1831-32, II, p.21; see also Holland, H., *Med. Notes and Reflections*, Phila. 1839, p.342.

⁴ Ozanam, J.A.F., *Histoire médicale des maladies épidémiques*, Paris, 1835, III, p.6. On Ozanam see Galdston, I.,

in *Essays presented to A. Castiglioni* (Supp. Bull. H. Med. No. 3), Baltimore, 1944, pp.89-102.

⁵ *Arch. Phys. Heilk.* 2:321, 1843.

⁶ Mitchell, J.K., *Five Essays*, Phila. 1859, p.111.

⁷ *Ibid.*, p.4.

⁸ *Brit. For. Med. Chir. Rev.* 4:252, 1849.

⁹ Riecke, C.F., *Die Reform der Lehre von der Contagion*, Quedlinburg, 1854, pp. XVII, 177. See also Virchow's diatribe against unscientific, antiquated contagionism (in his *Archiv*, 2:263; 1849)

¹⁰ Greenwood, *Epidemics and Crowd-Diseases*, N.Y., 1935, p.58.

¹¹ Singer, Ch. and D., *The Development of the Doctrine of Contagium vivum, 1500-1750.*, 17th *Int. Med. Congress* (1913), Sect. XXIII, p.188, London, 1914.

¹² Singer, *l.c.*, p. 205

¹³ Most of the above data are taken from Singer 1914, Singer, Ch. and D., *The Scientific Position of Girolamo Fracastoro*, *Ann. Med. Hist.* 1: 1 ff., 1917, and Loeffler, F. *Vorlesungen ueber die geschichtliche Entwicklung der Lehre von den Bakterien*, Leipzig, 1887.

¹⁴ The crisis aspect of this period has been emphasized by Shryock, e.g. in his *Development of Modern Medicine* (Phila. 1936). See also Ackerknecht, E.H., *Beitraege zur Geschichte der Medizinalreform von 1848*, *Sudhoff's Archiv* 25 (1932), chs. III and IV.

¹⁵ Welch, W.H., *Public Health in Theory and Practice*, New Haven, Yale University Press, 1925, p.27.

¹⁶ Winslow, C.-E., A., *The Conquest of Epidemic Disease*, Princeton, Princeton University Press, 1943, p.182.

¹⁷ Griesinger, W., *Infektionskrankheiten*, Erlangen, 1857, p. 120.

¹⁸ *Dictionnaire de médecine*, Paris, 1834, Vol. VIII, p.503.

¹⁹ *Archives générales de médecine*, 28:286, 1832

²⁰ Thomassen, E.H., *Untersuchung ob das gelbe Fieber ansteckend sey.*, Bremen, 1832, p.27.

²¹ Chervin, N., *Examen des principes de l'administration en matière sanitaire*, Paris, 1827, p.29.

²² La Roche, R., *Yellow Fever Considered in its Historical, Pathological, Etiological and Therapeutical Relations*, Philadelphia, 1855, Vol. II, p.527.

²³ *Ibid.*, p.262.

²⁴ *Lancet*, 1834-35, I, p.862; *Lancet*, IX, 1825-26, p.134.

²⁵ Tardieu, A., *Treatise on Epidemic Cholera*, Boston, 1849, p.239.

²⁶ *Lancet*, 1829-30, II, p.291

²⁷ Hirsch-Gurlt, *Biographisches Lexikon der hervorragenden Aerzte*, Wien-Leipzig, 1888, Vol. VI, p.62.

²⁸ La Roche, *l.c.*, p.569

²⁹ Maclean, Ch., *Evils of Quarantine Laws and Non-Existence of Pestilential Contagion Deduced from the Phenomena of the Plague of the Levant, the Yellow Fever of Spain, and the Cholera Morbus of Asia*, London 1824, p.136 Rochoux (*Dictionnaire de médecine* vol. 8, 1834, p. 504) even adds rabies, scarlet fever, anthrax, hospital gangrene.

³⁰ Winslow, *l.c.*, p. 193 ff.; Spector, B., *Noah Webster's Letters on Yellow Fever*, (Suppl. Bull. H. Med. No. 9),

- Baltimore, 1947, and especially that extraordinary product of American medical scholarship: La Roche's book on yellow fever, Phila., 1855.
- ³¹ Winslow, *l.c.*, 236 ff.; Newsholme, A., *Evolution of Preventive Medicine*, Baltimore, 1927, p. 121, ff.
- ³² La Roche, *l.c.*, p.239, Devèze regarded also plague, typhus, and dysentery as non-contagious.
- ³³ La Roche, *l.c.*, 247 ff. gives long lists of tropical anticontagionists.
- ³⁴ *Ibid.*, p.237
- ³⁵ La Roche, *l.c.*, 245.
- ³⁶ Maclean, *l.c.*, p.76.
- ³⁷ La Roche, *l.c.*, p.257
- ³⁸ Maclean, *l.c.*, p.141. Maclean reprints the whole manifesto, p. 122 ff.
- ³⁹ Chervin, 1827, p.15.
- ⁴⁰ *Archives Générales de Médecine*, 2 s., 1: 609, 1853.
- ⁴¹ *Lancet*, 1832-33, I, p.373
- ⁴² e.g. *Arch. Gén.* 16: 310, 1828; 22: 437, 1830.
- ⁴³ For Réveillé-Parise, Londe, Villermé, and many other anticontagionists as hygienists see Ackerknecht, E.H., *Hygiene in France 1815-49, Bul., Hist. Med.*, XXII, pp. 117-155, 1948.
- ⁴⁴ Charcot, J-M, *Oeuvres complètes*, Vol. VIII, p.144, 1889.
- ⁴⁵ Chervin, 1827:59.
- ⁴⁶ Chervin, *Lettre sur la fièvre jaune qui a régné à Gibraltar en 1828*, Paris, 1830, p.16. For a good analysis of the Gibraltar epidemic by the contagionist Peter Wilson, with emphasis on "filth", see *Lancet*, 1829-30., I., p.395 ff.
- ⁴⁷ *Brit. For. Med. Chir. Rev.* 6: 428, 1850.
- ⁴⁸ Scott, *l.c.*, p.352.
- ⁴⁹ Gross, S.D., *History of American Medical Literature*, Philadelphia, 1875, p.25
- ⁵⁰ Milroy, G., *Quarantine As It Is and As It Ought to Be*. London, 1859.
- ⁵¹ Mitchell, J.K., *l.c.*, p.115.
- ⁵² Dutrolau, AF., *Traité des maladies des Européens dans les pays chauds*, Paris, 1868
- ⁵³ Chervin, 1827, p.26. Maclean, *l.c.*, p.397. Maclean attributes this to his teachings and publications in India (Calcutta, 1796). See also Charles Searle, (Madras) *Cholera*, London, 1830. W Twining (1780-1835) *Practical Account of Epidemic Cholera*, London, 1833, etc.
- ⁵⁴ *Dict. Encycl. Sc. Méd.* (Dechambre ed.) 3. ser. vol I, p.51, Paris, 1874.
- ⁵⁵ *Arch. Gén.* 28: 131, 1832
- ⁵⁶ *Arch. Gén.* 27: 554; *Lancet*, 1831-32, I, p.17
- ⁵⁷ *Arch. Gén.* 30: 138
- ⁵⁸ *Arch. Gén.* 28: 134, 1832; *Lancet*, 1831-32, I., p.113
- ⁵⁹ *Arch. Gén.* 28: 452, 1832, *ib.* p.451
- ⁶⁰ *Lancet*, 1832-33, I., p.372
- ⁶¹ *Lancet*, 1831-32, I., p.261
- ⁶² *Lancet*, 1831-32, II., p.299, 301
- ⁶³ *Lancet*, 1831-32, II., p.23
- ⁶⁴ *Ibid.*, 150.
- ⁶⁵ *Ibid.*, 147.
- ⁶⁶ *Ibid.*, 156, 178.
- ⁶⁷ *Lancet*, 1832-33, I., p.113
- ⁶⁸ Gaultier H., *The Origin and Progress of the Malignant Cholera in Manchester*. London 1833. G. regards the relation between the arrival of sick persons and a new outbreak as purely coincidental in view of the many exceptions (p.14). He accuses effluvia of excrements as the source of the disease (p.112).
- ⁶⁹ *Arch. Gén.* 28: 430, 1832.
- ⁷⁰ See his 1832 book on cholera.
- ⁷¹ *Lancet*, 1843-35, I., p.224, 273.
- ⁷² Tardieu, *l.c.*, p.244.
- ⁷³ *Arch. Gén.* 29:122, 1832
- ⁷⁴ *Arch. Gén.* 4, ser. 11:370
- ⁷⁵ *Lancet*, 1831-32, II. P.148.
- ⁷⁶ *Arch. Gén.* 28: 608, 1832.
- ⁷⁷ *Arch. Gén.* 27: 140, 1831
- ⁷⁸ *Arch. Gén.* 28: 452, 1832.
- ⁷⁹ *Arch. Gén.* 2 s., 1:532, 1833.
- ⁸⁰ Haeser, H. *Lehrbuch d. Gesch.der Medizin*. Jena, 1882, Vol III, p.921 ff.
- ⁸¹ Rogers, S., *Report on the Asiatic Cholera in the Madras Army from 1828 to 1844.*, London, 1848.
- ⁸² *Brit. For. Med. Chir. Rev.*, 1849, vol.4: 251 ff.
- ⁸³ *Lancet*, 1849, I., p.166, *Lancet*, 1848, II., p.502
- ⁸⁴ Tardieu, *l.c.*, p.154.
- ⁸⁵ See *Report*, pp.37-63
- ⁸⁶ *Report*, p.18
- ⁸⁷ *Ibid.*, p.69
- ⁸⁸ *Brit. For. Med. Chir. Rev.*, Vol. V, p.216 ff., 1850, VII: 1 ff., 1851 These very eloquent articles are probably the work of John Forbes. See also the significant Southwood Smith quotation in Newsholme, *l.c.*, p. 126, for this pragmatic attitude.
- ⁸⁹ *Ibid.*, p.32.
- ⁹⁰ Tardieu, *l.c.*, p.111.
- ⁹¹ *Arch. Gén.* 4.s. 20:110 ff., 1849
- ⁹² *Arch. Gén.* 4.s. 16:521, 1848
- ⁹³ *Schmidt's Jahrb.* 62:142, 1849.
- ⁹⁴ Virchow, *Ges. Abhandlungen aus dem Gebiet der oeffentlichen Medizin*, Berlin, 1879, Vol.1, p.24.
- ⁹⁵ *Lancet*, 1848,II., p.454.
- ⁹⁶ *Brit. For. Med. Chir. Rev.*, 3:26,1849.
- ⁹⁷ Scott, *l.c.*, 699.
- ⁹⁸ *Lancet*, 1848,II., p.196.
- ⁹⁹ Tardieu, *l.c.*, p.111; Lasègue, *Arch. Gén.* 4.s. 18:114,1848
- ¹⁰⁰ *Lancet*, 1849, I, p.292
- ¹⁰¹ *Lancet*, 1831-32, I., p.182; *Arch. Gén.* 27:137,1832.; *Dict. Encycl. Sc. Méd.* 3. ser., Vol. I, p.51; *Lancet*, 1831-32, II., 572; GJ Mulder in *Arch. Physiol. Heilk.*, 1849, 8:489 ff.
- ¹⁰² Winslow, *l.c.*, p.255 ff.
- ¹⁰³ *Lancet*, 1853, II, p.399
- ¹⁰⁴ *Schmidt's Jahrb.* 84:90, 1854.
- ¹⁰⁵ *Ibid.*, 84:92. The same does even the contagionist *Lancet*, 1849, II, p.318.
- ¹⁰⁶ *Schmidt's Jahrb.* 92:252, 259
- ¹⁰⁷ Riecke, *l.c.*, p.165.

- ¹⁰⁸ Griesinger, *l.c.*, p.252, 254.
¹⁰⁹ *Ibid.*, p.255.
¹¹⁰ Milroy, *l.c.*, p.10.
¹¹¹ *Dict. Encycl. Sc. Méd.*, 1. ser. Vol. 16, p.755, Paris 1874.
¹¹² *Arch. f. path. An.* 1869, 45:279
¹¹³ Maclean, *l.c.*, p.90.
¹¹⁴ Maclean, Ch, and Yates, W., *A View of the Science of Life, Calcutta*, 1796.
¹¹⁵ Id. *Results of an Investigation Respecting Epidemic and Pestilential Diseases*. 2 vols. London 1817-18.
¹¹⁶ Chervin, 1827, p.124.
¹¹⁷ *Lancet*, 1826-27, 12:777
¹¹⁸ *Lancet*, 1825, 6:275
¹¹⁹ Reprinted in Maclean 1824, pp. 198-261.
¹²⁰ *Ibid.*, p.204.
¹²¹ *Ibid.*, pp. 94, 219.
¹²² *Ibid.*, p.6.
¹²³ *Ibid.*, p.129.
¹²⁴ *Ibid.*, pp. 410, 414.
¹²⁵ Riecke, *l.c.*, 96; *Lancet*, 1828-29, I. p.390.
¹²⁶ *Dict. de Méd.* XIX, p. 601 ff., 1839
¹²⁷ Murchison Ch, *A Treatise on the Continued Fevers of Great Britain*, London 1862, p.428; see also Griesinger, *l.c.*, p.120.
¹²⁸ *Ibid.*, p.2.
¹²⁹ Charcot, *l.c.*, p.19.
¹³⁰ *Lancet*, 1829-30, II, 543.
¹³¹ *Lancet*, 1829-30, II, 600.
¹³² *Archiv.* 2:263,1849
¹³³ *Lancet*, 1849, I., p. 106, 202.
¹³⁴ *Schmidt's Jahrb.* 1857, 96:250
¹³⁵ Murchison, *l.c.*, p.79. He alludes here to the "Sanitary Movement."
¹³⁶ *Schmidt's Jahrb.* 1863, 117:94
¹³⁷ Virchow, *Ges. Abh.* I, p.447,449
¹³⁸ Scott, *l.c.*, p.752; *Arch. Gén.* 2. ser. 3:147, 1833
¹³⁹ *Arch. Gén.* 4.s. Vol. X, 487, 1846.
¹⁴⁰ *Dict. de Méd.* Vol. 19,7:601 ff., 1839
¹⁴¹ *Dict. Encycl. Sc. Méd.*, 3. ser., Vol. 1:136, 1874
¹⁴² *Brit. For. Med. Chir. Rev.*, 7:38, 1851
¹⁴³ Murchison, *l.c.*, p.8.
¹⁴⁴ *Dict. Encycl. Sc. Méd.* 2 s., Vol. 7, p.526, 1873
¹⁴⁵ As a matter of fact no anticontagionist ever made a statement comparable in absurdity to the one of Pariset that the Cadiz yellow fever was transformed cholera and imported by a ship from *Calcutta* (La Roche, *l.c.*, p. 204).
¹⁴⁶ Winslow, *l.c.*, 250 (Another version of the same fallacy: S Smith, Henderson, and Florence Nightingale felt that all fevers had one cause. Murchison, *l.c.*, p.4.)
¹⁴⁷ Wunderlich, *l.c.* p. 323.
¹⁴⁸ Londe developed this point in *Arch. Gén* 4.s., 11:489, 1846
¹⁴⁹ *Lancet*, 1831-32, II, p.156
¹⁵⁰ *Arch. Gén.* 4.s. 23:123
¹⁵¹ Griesinger, *l.c.*, p. 120. This does, of course, only mean that the tendency towards anticontagionism was stronger in big cities. There were also plenty of anticontagionists in small places. See e.g. *Lancet*, 1831-32, I, 796.
¹⁵² Riecke, *l.c.*, p.30.
¹⁵³ *Dict. Encycl. Sc. Méd.* 1. ser. Vol.20, p.1-2,7,1877.
¹⁵⁴ Roy F. *Revue historique des différentes théories qui ont eu course pour expliquer l'origine des maladies infectieuses*. Strassbourg, 1869, p.15
¹⁵⁵ *Schmidt's Jahrb.* 92:255, 1856
¹⁵⁶ Maclean, *l.c.*, p. 30 ff.; Chervin in *Arch. Gén.*2.s. 3:147.1833.
¹⁵⁷ Chervin, 1827, p. 132.
¹⁵⁸ *Ibid.*, p. 107, 116.
¹⁵⁹ *L.c.*, p. 34.
¹⁶⁰ e.g. *Arch. Gén.* 27:134, 1831.
¹⁶¹ Dutrolau, *l.c.*, p. 444; *Dict. Encycl. Sc. Méd.* 3.s., Vol. I, p.97-98
¹⁶² Maclean, *l.c.*, p. XXIII.
¹⁶³ *Ibid.*, p.6, 213.
¹⁶⁴ *Ibid.*, p.69.
¹⁶⁵ *Ibid.*, p.21.
¹⁶⁶ Riecke, *l.c.*, p. 5.
¹⁶⁷ Maclean, *l.c.*, pp. 35, 214, 271
¹⁶⁸ Chervin, 1827, p. XVIII
¹⁶⁹ Ackerknecht, E.H. *Beitraege zur Geschichte der Medizinalreform von 1848*, *Sudhoffs Arch.* 25:62-183, 1932.
¹⁷⁰ Id. *Hygiene in France 1815-1848*. *Bull. Hist. Med.* 22:117-155, 1948. Especially for the post-1848 developments of these theories see the excellent study of G. Rosen: *What is Social Medicine?*, *Bul., Hist. Med.* 21: 674-733, 1947.
¹⁷¹ Like the editorial writer of the *Brit. For. Med Chir. Rev.* 5:217 ff., 1850.
¹⁷² *Arch. Gén.* 28: 427, 1832.
¹⁷³ Murchison, *l.c.*, p. 75.
¹⁷⁴ *Schmidt's Jahrb.* 96: 251, 1857.
¹⁷⁵ *Ges. Abh.* 1879, I., p.117 ff.
¹⁷⁶ *Ibid.*, p. 284-285.
¹⁷⁷ *Ges. Abh.*, 1856:927