Bearing the simple title ‘Anticontagionism between 1821–67’, Erwin Ackerknecht’s 1948 Fielding Garrison lecture to the American Association for the History of Medicine is one of the best known articles in the modern history of medicine. It not only opened the door to studies relating medical theory to the politics of epidemic response, but helped us appreciate that medicine could be ideological. Ackerknecht’s best known claim in the paper is in fact three: an empirical claim, an historical interpretation, and an evaluation. The empirical claim was that in the period covered by the paper (the chronology was looser than the title suggests; Ackerknecht took examples from the 1790s), the view that ambiguous epidemic diseases—i.e. plague, yellow fever, and cholera—were transmitted by contagion was dominant in the anti-liberal states of central and eastern Europe, while the anticontagionist view (or better, views), that these diseases derived from some transitory complex of environmental and even social conditions was dominant in the liberalizing states of Britain, France, and the US, and that, in such places, it was the doctrine held by revolutionaries or radical reformers. ‘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’ (Ackerknecht, p. 9).

Why should this have been so? Ackerknecht interpreted: ‘Contagionism would, through its association with the old bureaucratic powers, be suspect to all liberals, trying to reduce state interference to a minimum’ (Ackerknecht, p. 9). These liberals and revolutionaries were seeking many freedoms, but most important in respect to epidemic response were those of movement and trade. Their opposition to ‘state interference’ was part of a campaign to transform administration from sinecures propped up by hollow dogma to meritocracy founded in rational accountability. They held out great hope of progress: the human condition was far from what it might be; reform—environmental, social, and more broadly institutional—was unambiguously good; it would bring freer, happier, and more prosperous citizens—and stop epidemics!

By contrast, the states of the old order operated on increasingly outdated mercantilist principles of Kameralwissenschaft. According to these principles, much studied by Ackerknecht’s friend and fellow medical historian George Rosen, home industries were to be protected in hopes of increasing the tax base. Travel and trade were much less opportunities than dangers needing to be carefully regulated, and security was much more important than some nebulous ‘progress’. Epidemics were but another form of threat to the state, and warranted the same response as any foreign enemy: in Austria, Hungary, and the Balkans, the Hapsburg Empire deployed a huge garrison to cordon off its southeastern frontier, the same frontier along which it had fought the Turks in earlier centuries. The quarantines by which these states protected themselves were predicated on the truths of contagion, but their administration was embedded in moribund bureaucracies. The bureaucrats, cynical and complacent, were happy with things as they were. To reformers, quarantines, enforced and defended however poorly they worked, were but one more dogma that escaped accountability.

And, finally, Ackerknecht assessed. He held that, given the existing state of biomedical knowledge, it was understandable, even perhaps right and good, that one—even a medical professional—would have taken a stance on this central question of etiology on the basis of one’s politics: ‘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 outlook and political loyalties will determine the decision’ (Ackerknecht, p. 17). This provocative statement continues to resonate; I shall address below what I think Ackerknecht meant by it.

Of the three claims, the first, and, to some degree the second, insofar as it is predicated upon it, have been
subjected to exhaustive test by Peter Baldwin, an historian of comparative state formation. Baldwin, focusing on Britain, France, Germany (primarily Prussia), and Sweden finds some evidence to support Ackerknecht, but he finds a more complicated picture, especially when practice is considered in addition to assertions of principle. Of the three diseases that Ackerknecht considered, Baldwin treats only cholera (he also discusses syphilis and smallpox), and he takes Ackerknecht’s issues beyond Ackerknecht’s period. With regard to cholera, Baldwin interprets the response of European states in terms of a learning curve, though what was being learned, changed. In 1830–31, cholera hit the anti-liberal states of eastern Europe first; their plague-based quarantine and cordon defenses failed to halt the disease and/or evoked dangerously high levels of potentially revolutionary social disorder. They rapidly switched to milder and gentler tactics of social and environmental reform. But by the fourth and fifth cholera pandemics—roughly the 1860s and the 1880s—they, and the more liberal states, were beginning to re-impose contagion-based controls on the movement persons, though much less so on goods, even before Robert Koch isolated his comma vibrio in 1883.

And yet, as Baldwin makes clear, the approach in both sorts of state was, at least considered in terms of theory, eclectic. For Ackerknecht had made a plausible but unwarranted assumption of a close and necessary relation of theory to practice. Here theory, presumably the composite of experience, dictates practice: if you are a contagionist you will support cordon and quarantine; if you are an anticontagionist you will oppose these and support environmental reform. And yet, interdicting ill persons could be done in addition to, rather than instead of, ridding towns of bad smells, and it often was. And one can see why. An accretive logic operated. The front-line responder was more likely to be judged on what hadn’t been done than on what had. There was more here than the need to humour the prejudices of every town councillor or minor princieling: it was far easier to add a plausible new way to fight epidemics than to demonstrate beyond doubt the worthlessness of an old one. The pragmatist, unlike the purist, might see merit in meeting uncertainty with multiple layers of protection. Baldwin labeled this approach ‘Pascallian’: the philosopher Blaise Pascal had famously argued that even if one did not know there was a God it made sense to play it safe and act as if there was one (Baldwin, pp. 99, 142). (Cipolla, pp. 4–5, 45–48, 60). But it can also be seen as embodying the prudential principle, recently much talked of as a proper framework for assessing technical and environmental change.

Baldwin observed also that commerce and contagionism were hardly as antithetical as Ackerknecht had suggested. So long as they were evenly assessed, the costs of quarantining cargoes or passengers were unobjectionable. They became part of the costs of doing business: a well run system of quarantines, no less than a sound banking or insurance system, secured international confidence (Baldwin, p. 204). Anticontagion-based campaigns for massive retrofitting of urban infrastructure to remove wastes and prevent miasms were usually costlier, even if the costs might be shared more widely and over a longer time. And, whatever the reigning theory, an epidemic usually brought trade and industry to a standstill. Finally, if there was a time when commercial interests dictated medical theory, it was not Ackerknecht’s period, but the generation after 1867, when one finds accusations in the cholera literature that senior British public health authorities in India have been bought off: only this, it is held, can explain their continued anticontagionism. These criticisms—Robert Koch is among the most vehement—were recognizing the same empirical relation that Ackerknecht would (at least for the anticontagionist side of the correlation) and interpreting it in the same way (Baldwin, pp. 192–5).

One can quibble with Ackerknecht’s broad claims, by finding radicals who were contagionists (like Thomas Wakley), or reformers on health and social matters who were politically conservative, like William Pulteney Alison. More finely grained studies often reveal that the relation of etiological sensibilities to politics varied from place to place. Within the greater British Isles there were different configurations in England, Scotland, and Ireland. London differed from Bristol and from the north; Edinburgh from Glasgow; Dublin from Cork and both from much of rural Ireland. And the period was one of change, in different ways and rates in different places. For Britain, Ackerknecht’s claims probably fit best during the 1820s. And yet, with all the qualifications, there is still signal among the noise.

But what kind of signal is it? Ackerknecht’s article was founded not only on the dichotomy between contagion and anticontagion, but on a second, between the realm of theory choice in biomedical science and that of politics and ideology. The rub of the article was the correlation between elements of these disparate domains. Yet, like most other historians of medicine and science of his generation, Ackerknecht lacked conceptual and analytical tools that might have suggested that the domains were not so disparate in the first place. The debates he studied were no mere exercise in abstract induction and deduction. Rather, the conflation of medicine and politics was so quick and easy because Ackerknecht was addressing multi-layered questions, bearing on everything from political rights and governance to epidemic-response policies, which had central medical components. This is not a world in which the enterprise of etiological hypothesis-testing is suddenly and surprisingly found to be rife with political implications and complications, which, in the absence of adequate data and analytical methodologies come to dominate. On the contrary, the medical was inherent to the political.
Medical writers engaged with issues of rights, governance, and epidemic response in registers other than those familiar to philosophers of science. I have noted already Baldwin’s observation that practitioners of epidemic response were often oblivious to the dictates of theory, and cheerfully combined techniques in ways which violated theorists’ principles. No less important is the plainly polemical, and overtly rhetorical character of much of the literature Ackerknecht digested.

For Ackerknecht, wanting to take seriously issues of the fit of theory to evidence, rhetoric and polemic constituted distraction and disappointment: the intrusion of the political into the medical. Mostly he overlooked. He did admit however, that his partisan subjects were characteristically selecting their evidence: ‘the contagionists would usually pick a set of more or less true facts [bearing in this case on the results of experiments on animals] 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’ (Ackerknecht, p. 17). However unacceptable such tactics might be to the resolution of theoretical controversies in a transcendent medical science, they were understandable in the mixed political-medical realm. There rhetoric was not a dirty word, but an art essential to the creation of authority and of action. The timetable of the epidemic, not the leisurely pace of biomedical science, dictated the schedule of response.

The Ackerknechtian antinomies of contagion-anticontagion are themselves central constituents of such rhetoric. The dichotomous framework was more useful for dismantling an opponent’s naïve oversimplifications than for presenting one’s own nuanced views. Their utility came at the cost of clarity in representing either the state of etiological knowledge or the positions of practitioners. One is, or should be, struck by the appalling asymmetry as well as the imprecision. Anticontagionism was defined as a negation; it was, Ackerknecht admitted, not one position but many. Logically, it must include all positions other than contagionism; rhetorically, it could be a useful umbrella for one’s own position or for one’s opponent’s, depending on who controlled the burden of proof. Many of the positivist French clinicians, among Ackerknecht’s favorite reformers, were pressing that burden on traditional contagionism. An epidemic presented an array of facts—social, environmental, pathological. But the so-called ‘contagia’ were not facts at all but rather constructs that some persons in established positions used for their own ends. To expose those ends and uses did not require positing an alternative. And yet not all in that broad church of anticontagionism were adept at or saw advantage in holding firm to positivism. Always there were temptations to generalize and then theorize, and sometimes good political reasons for simplistic but powerful slogans, like Edwin Chadwick’s famous and false dictum: ‘all smell is disease’ (which did not imply that ‘all disease is smell’, though one can imagine Chadwick subscribing to that too). But contagionists too contributed to an image of unity and simplicity. To the degree that anticontagionism could be rendered as a particular position, it might be ridiculed as vague and untestable, hence unscientific. Or, only reduce it to simple tenets from which observational implications can be derived, show these not to obtain, assert that anticontagionism has been falsified, and therefore, the question being dichotomous, assert that one has proved contagion.

Unable to appreciate how far the literature was one of rhetorical excesses, Ackerknecht also had difficulty knowing what to make of the many who defied the dichotomy. Some of these were the eclectic practitioners Baldwin has recognized. These cannot be seen solely as rude empiricks trying to satisfy some local worthy’s crotchets on burning tar or swabbing vinegar, or, alternatively, as rigorously applying a calculus of potential error costs to an integrated epidemic response policy. Rather, many were, as good empiricists, moving back and forth between observation, theory, and practice, employing flexible and rudimentary working hypotheses. There was also that large middle ground of theorists, the ‘contingent contagionists’. Indeed, outside the hubbub of controversy, many, even most, warrant this description. It fits the contagionist heroes Robert Koch and John Snow, who see fecally contaminated water as the most important of the environmental media of cholera communication, and the often vilified Max von Pettenkofer, for whom soil and air played a not quite analogous role. Contagion and environment were both important; indeed, David Barnes describes a ‘sanitary-bacteriological synthesis’ in the late 19th century.

Ackerknecht acknowledged the importance of the middle position—it is precisely the attitude of this centre which decided on the practical applications, and which best illustrates the general orientation of a given period (Ackerknecht, p. 9)—but did not suggest an adequate way to handle it. To see the contingent contagionists as compromisers, he hints, might credit their pragmatism and political skills, but at the expense of their epistemic integrity, for no philosophy of science calls on us to split the difference in the face of irreconcilable theories purporting to account for the same phenomena. On the other hand, if contingent contagionism was a third class of explanations, or even a loose framework that fostered empiricism without falling into dogma, how then was it that etiological debate had been (and still was in Ackerknecht’s day), framed dichotomously, neglecting, for example, important questions of pathogen ecology?

Ackerknecht’s assessment poses a different problem, normative rather than interpretive. When rival
hypotheses are equally plausible, can (do, will, should) political values decide the matter? Koch and company certainly thought not: when they charged Anglo-Indians with letting commercial values guide cholera science, it was plainly an accusation. Many modern scholars, by contrast, are apt to see Ackerknecht’s assessment in light of changes in the history, philosophy, and sociology of science that began in the 1970s and which had taken hold by around 1980: it was appreciated that under-determination of theory by observation is not simply an incidental feature of some scientific controversies, but inevitable. Always it will be necessary to apply one’s chosen epistemic virtues to the process of theorizing as well as to that of observing and experimenting (for a philosopher’s introductory account see Kosso18). To scholars habituated to this approach, Ackerknecht was a precursor. Between 1821–67 it had been impossible, he insisted, to select one of these two rival positions without that selection being political. ‘Yes’, they (we) chorused, ‘that is true, but it is always and necessarily true’. And some pushed further, to suggest that since controversies invariably required the intrusion of extrascientific factors, they were fundamentally extrascientific.

Perhaps the contagion-anticontagion debates were not about medicine at all but ultimately about despotism and liberalism, tariffs vs free trade.

But the latter position certainly overstates, and neither version helps us make sense of Ackerknecht. Despite the occasional strong statement—e.g. a reference to the ‘powerful social and political factors that animated this seemingly scientific discussion’ (Ackerknecht, p. 9)—he is no relativist. At the end of the article, Ackerknecht (p. 19) would apply the social explanation only to anticontagionism. He was not (explicitly at least) positing a general principle, would have been uneasy with any claim of under-determination of scientific knowledge. But what then, precisely, does he want to suggest about the place of politics in theory choice, particularly in the medical realm?

Ackerknecht too was an adroit rhetorician. The equilibrium he described was admittedly a passing stage in the evolution of etiology, but it threatened to become much more. Recognizing, if perhaps only vaguely, the empirical inadequacy of the contagion-anticontagion dichotomy, he chose to deploy that framework rather to explode it. But what was at stake in 1948 that made it so important for this Garrison lecturer to make these points about medical history?

We get inklings of an answer when we realize that the representation of equal plausibility—the famous ‘Intellectually and rationally the two theories balanced each other too evenly’—is misleading. It reflects a transitory state, occurring as the trend lines of the two positions cross. Though it will soon fall, the stock of the anticontagionists was rising; they are the voices of rigour and progress, while that of the contagionists, who are presented as remote and despicable ancestors of any modern position, was rightly falling, though it would soon rise. But it is plain that for Ackerknecht, the anticontagionists wore the white hats. They are the revolutionaries (American revolutionaries, significantly, even if their revolution will only come to fruition in France). They, presumably unlike the complacent contagionist dogmatists, are persons of ‘unprecedented energy’. How poorly they were regarded in 1948 is made clear by the pains Ackerknecht took to defend them. True, there was a ‘lunatic fringe of anticontagionism’ (Ackerknecht, p. 11),1 yet ‘the anticontagionists were usually honest men’ (Ackerknecht, p. 9), whose ‘personal integrity and disinterestedness were above any doubt’ (Ackerknecht, p. 11).1

They also had the courage of conviction: Ackerknecht admired the self-experiments (the best known occurring after his period) in which researchers put their lives and beliefs on the line by ingesting or otherwise introducing into their bodies presumably contagious materials, most famously cholera discharges or cultures (and, remarkably, almost always confirmed their hypotheses in not coming down with the disease) (Ackerknecht, pp. 9).1 Also critical was the respect they had garnered from such unimpeachable authorities as William Welch and C.-E.A. Winslow. These ‘outstanding leaders of modern “contagionism” and thoroughly unromantic characters’ had ‘understanding words’ for the poor anticontagionist (Ackerknecht pp. 8–9).1 Ackerknecht endorsed both Welch’s argument that, prior to development of bacteriological methodologies, a contagionist position had no practical payoff, and Winslow’s, that a contingent contagionism better fit the facts.

While their revolutionary anticontagionism might not be new—as old as Hippocrates—the contagionism of the day, and particularly the doctrine of contagium animatum was even more tainted by its Old Testament origins, and by the easy medieval move from possession by devils to infestation by pathogens. Jacob Henle, the early germ theorist with whom Ackerknecht opens his article, might exemplify modern science, but his contemporaries would have been unable to extricate Henle’s contagionist proposals from this decidedly unenlightened heritage. ‘It was no accident that so many (of the) leading anticontagionists were outstanding scientists’, Ackerknecht observed: ‘To them this was a fight for science, against outdated authorities and medieval mysticisms’ (Ackerknecht, p. 9).1 In this vein, Ackerknecht could disguise his story as simple irony: ‘a strange story...a theory reaching its highest degree of scientific respectability just before its disappearance; of its opponent suffering its worst eclipse just before its triumph...’ (Ackerknecht, p. 19).1 Only by appreciating all these factors could we understand the flourishing of anticontagionism just before the triumph of its opposite. Surely it was ‘one of
the tragic jokes of fate that [the anticontagionist Nicholas] Chervin’s many talents and virtues were spent on a lost cause’ (Ackerknecht, p. 11).¹

But much more is at stake than Chervin’s tragedy, or the comedy of theoretical error, or even an explanation of the puzzling anomaly of latter-day anticontagionism. Matters of social medicine and contemporary medical politics are not far from the surface. Ackerknecht wants to say, but cannot quite, that the anticontagionists’ descendents still wear the white hats. He wrote in an era of germ-theoretic triumphalism. To this, he, like fellow émigré medical historian Henry Sigerist and their American protégé George Rosen, had a mixed reaction. Remarkable scientific achievements and public enthusiasm for them might come at the expense of a more broadly reformist public health.¹⁹ In 1948 the voices of reform were evident in the U.S., in Harry Truman’s postwar flirtation with a national health insurance program, in the emergence of the NHS in Britain, and in the emerging WHO. (Ackerknecht did not, as would a later group of medical historians, make explicit reference to the centennial of the radical ideals of 1848; given his own scholarly interests, he might well have done so. This had been the subject of his 1932 thesis) (Rosenberg, pp. 516–17).¹⁹ It was important to rebalance, reclaim, and, perhaps, reorient. The stock of the anticontagionists needed to rise; we needed to redefine a history of medical science that included them as much more than straw figures to be quickly and thankfully gotten past. The contagionists’ stock did not need to fall for this to happen, though Ackerknecht does remind readers that early 19th century contagionism (like mid-20th century contagionism?) remained conspicuously uncritical.

Ackerknecht, who had his own reasons for a broad church concept of anticontagionism, took the bold step of including in his anticontagionist camp a social and economic as well as an environmental version of the determinants of health and disease: ‘From the miasmatic or “filth” theory to a purely social concept was but a short step’. Fever in Ireland was, in the view of the anticontagionist ideologue Charles Maclean, (but also in the view of William Bateman, William Pulteney Alison, and nearly every Irish physician), due in part to despair (Ackerknecht 15, 18). Remarkably, he recognizes that this concern with social and economic factors can be (and was) as much a part of the anticontagionist as an anti-contagionist outlook, even if it arose more often in the latter (or so he thought). He goes on further to state near the end of the lecture, and after the opportunity to criticize has passed, that, to the sociologist, ‘all these problems (of deciding between contagia or anticontagia) become of a very secondary importance when compared to social factors’ (Ackerknecht, p. 19; ital. mine).¹ It was a quiet allusion to his fascination with social medicine, evident in a long standing interest in the doctor-scientist-legislator-reformer Rudolph Virchow, who as young physician had in 1848 anatomized the political roots of typhus in Silesia. For Ackerknecht and for other social historians of medicine, Virchow in Silesia had the status of ‘Ur-narrative’, similar to that of John Snow and the Broad Street pump for epidemiologists, notes Charles Rosenbug (Rosenberg, p. 531).¹⁹

All this sets up a conclusion whose disingenuous gentleness masks an existential punch. Ackerknecht, following the principles of Bertolt Brecht’s epic theatre, moves from the conducting of a performance to press the moral issues embedded in etiological theory directly on his hearers. He first does this explicitly in his invitation to assess. At issue was not what position medical scientists had taken in the mid-19th century, or why, but what would we have done? ‘We’—one notes, not the anonymous ‘one’, but Ackerknecht’s fellow historians of medicine, many of whom were practitioner-historians—’would have had a very hard time … [O]utlook and political loyalties will determine the decision.’ (Ackerknecht, p. 18).¹

Each may reach a different conclusion; Ackerknecht will ‘leave it to you to make your choice’ (Ackerknecht, p. 19).¹ So, in fact, he must do from the way he has posed the invitation, for ‘the accident of personal experience and temperament’ cannot be overcome and need not be apologized for. But he does load the deck. He describes anticontagionism as an ‘eminently “progressive” and sometimes very effective movement based on a wrong scientific theory … ’ (Ackerknecht p.19).¹ He then offers the reader/hearer only two conclusions, and smuggles in a seemingly innocuous, but potentially explosive caveat. Some will ‘primarily enjoy the progress of scientific method and knowledge made during the last hundred years’. They will highlight the ‘triumph’ of contagionism. Others will (as has Ackerknecht in the lecture), ‘prefer to ponder those epidemiological problems, unresolved by both parties at the time and unsolved in our own day’, [ital mine] (Ackerknecht p.19).¹ Nothing prepares the reader for this last phrase, and Ackerknecht does not explain it, though it arguably builds on his earlier argument. If analogies hold, we too—as members of Ackerknecht’s audience—must recognize that public health practice and the course and conduct of research embody political commitments.

To reinforce that point, Ackerknecht asserts that only one conclusion is not acceptable: that we fall victim to that error ‘so common in man, but so foreign in the spirit of history: that our not committing the same errors today might be due to an intellectual or moral superiority of ours’ (Ackerknecht p. 19).¹ History teaches us that choices were and are hard; and they are at once epistemic and ethical. Plainly the attack is on hubris; focused on ‘triumph’, some historians of medicine, Ackerknecht suggests,
may forsake a critical open-mindedness in science and in politics; something Ackerknecht clearly fears for in 1948. For celebratory medical history is inherently dangerous; the medical historian must, like the ‘light ‘em up boys’, at the end of the *Threepenny Opera*, illuminate the present, equally problematizing it and facilitating our work in it (at the end of *The Threepenny Opera*, the lights are turned on the audience, transferring to it the moral issues of the play).

And in 2008? We enjoy an abundant richness of analytical techniques for making sense of etiological complexity—from geophysical and demographic scales down to molecular. At issue is not so much which of two mutually exclusive etiological hypotheses better represents reality, but at what levels complexity can best be made sense of, and by what modalities we can most effectively improve health. The competition over which path of research should dominate reflects an interpenetration of the social, political, economic, and cultural with the medical, analogous to that which intrigued Ackerknecht. The modern investment in subdisciplinary specialization—in skills and equipment—is the most marked difference. The biomedical scientist in the period Ackerknecht studied often had a joint facility in demography; microscopy; physiology, chemistry, and pathology; and diagnostics and therapeutics to a degree that is difficult to imagine in our own age of specialization. But to understand those interpenetrations remains important, for our research policies remain equally epistemic and ethical, if in more complicated ways. In dichotomizing for the period 1821–67, Ackerknecht, oversimplified, whether intentionally or not. Already by 1948, matters were much more complicated; the lesson he could apply was simply to be on one’s guard against hubris. This is always good advice, but as Ackerknecht’s legatees, we are challenged to push the analyses, and the debates on the issues entangled with them, further as well.

References


