LETTERS

LAND USE HISTORY AND PUBLIC HEALTH

Eckel, Rabinowitz, and Foster have done well in revealing the presence of heretofore unrecognized lead-smelting sites.[1] They demonstrate that land use history can be an important tool in revealing public health hazards. Perhaps if other studies are performed in the future, additional important historical resources will be utilized. Such resources could include city street directories, real estate atlases, building permits and plans, title records, archived business records, and aerial photographs, among others.

It is likely that other unrecognized lead-smelting sites exist in the United States. It is also likely that there are numerous unrecognized historical industrial sites that generated and disposed of hazardous substances and materials in urban, suburban, and rural environments. The skills of many disciplines will be required as additional unrecognized sites of concern are discovered, mapped, and remediated to protect human health and the environment. ■

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BIOPREPAREDNESS AND PUBLIC HEALTH

I was astonished to learn from Fee and Brown's article on preemptive biopreparedness[1] that beginning in the 1950s, Dr Alex Langmuir's stress on biopreparedness efforts fed the Cold War climate, narrowed the scope of public health activities, and failed to achieve sustained benefits for public health programs across the country. They point out that as "funding for biological warfare research was increasing . . . funds for local health departments were cut sharply. Jobs in public health departments went unfilled for long periods" and "enrollments in schools of public health declined."[1(p725)] To suggest that this sorry state of affairs was, in some manner, attributable to Langmuir's biopreparedness efforts taxes credulity. I would remind Fee and Brown that during these same years, the number of telephone poles increased sharply, but I wouldn't suggest that this, in some manner, served to precipitate the demise of public health.

It is fact that during the 1940s and 1950s, there was little Public Health Service money spent on biological weapons programs or research and little activity devoted to it. I joined Langmuir's Epidemic Intelligence Service (EIS) in 1955. Of the 50 or so EIS officers at that time, there was exactly 1 who worked part-time in the evaluation of a new anthrax vaccine among workers in goat-hair processing plants. Virtually all others were working either at the Centers for Disease Control or in state health departments endeavoring to bolster infectious disease reporting and control or participating in studies of the new polio vaccine. During the 4-week orientation training, there was only a 1-hour lecture on biological weapons, and that was dropped a year later. Note that the annual cadre of EIS officers was substantially smaller than the number of new enrollees in just 1 school of public health. They were, in fact, an inconsequential fragment of the total available public health manpower.

Certainly, the status and strength of public health waned after World War II as a result of many different factors. Of significant importance were developments in biomedicine and society that dramatically changed the health environment. New vaccines and antibiotics, along with improved housing and sanitation, altered the infectious disease burden and this, along with improved drugs and surgery, shifted the focus toward curative medicine. It is also fact that faculties of schools of public health became increasingly preoccupied with academia and less with the practice of public health, a factor that undoubtedly contributed to a less vocal and articulate public health presence.

Amidst all of this, there was a notable beacon, and that was Alex Langmuir, one of a diminishing number of passionate advocates and public health practitioners. As Fee and Brown note, Langmuir defined and stressed the principle of surveillance—a concept and approach whose importance is now widely recognized internationally but was unknown as such in the 1950s.[2] He believed that it was important to establish effective international, national, and local reporting systems to monitor and analyze, on a continuing basis, the epidemiologic patterns of infectious diseases so that strategies and resources could best be applied in a timely manner to prevent and control disease. Today, we accept this as an obvious precept of good public health management. Then, it was a new and different concept. By 1968, it was considered to be of sufficient importance to warrant asking Langmuir to lead special technical discussions on the subject at the World Health Assembly.[3]

By 1961, Alex had started a surveillance program for leukemia (the first of its kind) and, a few years later, a program to monitor family planning data.[4] It was noted then that Alex's EIS officer was the *only* public health service officer engaged in family planning. So much for allegations of his narrow focus. However, I am pleased that Fee and Brown believe that "[n]either Langmuir nor the biological warfare establishment can be held responsible for *all* that was lost to public health in the late 1940s and early 1950s" [emphasis added].[1(p725)]

Should we be concerned about bioterrorism today? From Fee and Brown's superficial explication of the threat, it might well appear that the concerns are only déjà vu. Regrettably, however, the world has changed, making the prospects for use of such weapons far more likely than ever before. Many of these reasons have been reviewed elsewhere.[5] Curiously, however, Fee and Brown's article makes no mention of the most serious development of all. With the Biological Weapons Convention, which came into force in 1972, essentially all countries agreed to cease research on biological weapons and to destroy existing stocks of material. One notable exception was the former Soviet Union, which undertook to develop a greatly expanded research and production capacity. That enterprise eventually involved some 60 000 people and 50 laboratories.[6]

The science of biological weapons was advanced significantly in the former Soviet Union, but at a price. Included in that price were some 100 fatalities in Sverdlovsk when anthrax spores were accidentally released into the environment from a bioweapons production center. Many of the Russian scientists have now dispersed to other countries, bearing with them expertise and, undoubtedly, some of the special strains they developed. At least 10 countries are now engaged in developing and producing biological weapons. What with the growing power of biotechnology, one has to anticipate that this technology, like all others before it, will eventually be misused.

Meanwhile, as we review the nation's capability to respond to a medical or public health catastrophe, whatever its source, we discover a tattered, seriously underfunded public health infrastructure and a medical care system that is taxed today by even a small outbreak of influenza, that is suffering severe shortages of health care personnel, and that regularly experiences shortages of the most common antibiotics. Were a new strain of influenza to explode in pandemic form—were 100 acutely ill persons to appear over a 1- or 2-day period in any emergency room—present systems would be overwhelmed. This, unfortunately, is fact. With the ability of organisms to move readily and rapidly around the world, we can no longer count on living in a fortress home protected from naturally occurring or manmade threats by the Atlantic and Pacific oceans.

Drs Fee and Brown, we are living now in the 21st century, and it is a different world than in 1950. Worries over biological preparedness may appear to be of little relevance to you, but to those of us concerned with the health of our own and the world's population, they are anything but. ■

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FEE AND BROWN RESPOND

Clearly, our perspective is considerably different from Dr Henderson's. We do not see eye to eye on many things, and as a result, we have divergent views not only of the past but of our current circumstances as well. Since we don't occupy common ground, it is not surprising that we do not share the same eerie feeling of déjà vu. What you see depends, to a large degree, on where you stand.

On one point, we feel the need to respond specifically to Dr Henderson and, interestingly, it is a point on which we seem to agree. Dr Henderson acknowledges that "the status and strength of public health waned after the [second world] war, a result of many different factors." He cites developments in biomedicine, new vaccines, and antibiotics that, with other factors, "shifted the focus toward curative medicine." He also acknowledges that faculties of schools of public health "became increasingly preoccupied with academia and less with the practice of public health." Absent from Dr Henderson's account of the postwar period is mention of the political climate, which we take to be crucial. As many scholars have noted, the climate of postwar hysteria drove prominent political figures to glorify science as a conflict-free alternative to health and social policy, just as it drove liberal academics into more esoteric, apolitical research areas. It was also the political and cultural climate that allowed certain fields—behavioral science and strategic studies—to grow and flourish while others withered or remained moribund. Public health efforts oriented to biopreparedness and formulated in Cold War terms could obviously flourish, whereas social equity concerns did not. Langmuir's campaign was well suited for its time and circumstances, and thus his efforts succeeded and his programs grew. These were not accidental connections, but more the result of an adaptive process akin to Darwinian selection.

We leave to others the debate over the necessity, wisdom, and political ramifications of current biopreparedness efforts. We insist that history has lessons that can be usefully drawn when its complexity and subtlety are fully appreciated, not rushed over, distilled in some selective fashion, or inappropriately removed from its political context. ■

[October 26, 2001—The threat of biological weapons is evidently more serious and more immediate than many of us thought in the weeks and months before September 11. Nevertheless, as the United States races to put in place new security forces and barriers, and as the people turn to their newspapers and television sets in daily anxiety, it is still imperative that we think carefully about the long-term implications and consequences of our current choices. Once—as we hope—the immediate threat subsides and we begin to reestablish a sense of security, we may discover that it is much more difficult than we expected to return to priorities that have been temporarily set aside: the plight of the disenfranchised and the uninsured; social disparities in health; human and civil rights; and social justice. Historical experience shows us that it can be difficult to rebalance our priorities and social progress in a context of anxious attention to national security. If history teaches us anything, it is that we should consider very carefully the steps that we take in the present, as these will create the path to the future.]

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INDIVIDUALIZED OR POPULATION RISKS: WHAT IS THE ARGUMENT?

With regard to the "Risky Concepts" focus in the March issue of the Journal,[1–3] are we to believe that there is a polarization of population vs individual approaches to prevention, when the two are intuitively complementary?[1–3] Or is the allegation a pretext to extol command-performance population interventions while obscuring the obvious perception that any form of intervention eventually touches individuals?

Colditz believes we know all there is to know about risk factors and asserts that "readily modifiable risks can be readily translated into ways to reduce the population burden of cancer."[1] Yet he should pause to consider the current prevalence of smoking and obesity in America. Talk of coercive measures that promise "little individual gain"[1] uncovers the Achilles heel of mass interventions. Mainly, they engender little individual motivation because, as Rockhill notes, most risk factors "are neither necessarily nor sufficiently causal at the individual level."[3] Colditz may lament a current ethos that favors individual freedom to choose, but he dares not offer alternatives.[1]

Begg, on the other hand, asserts that "the primary purpose of epidemiology is to determine individual risks,"[2] but he spends most of his article documenting the seemingly hopeless complexity of exogenous and endogenous interactions that determine individual risks. New proteogenomic devices may unearth ever more of the possible factors involved, but—probably with few exceptions—more factors are apt to further complicate the assessment of adverse and beneficial combinations that make up personal and population risks.

Pace Colditz, epidemiology is not at the end of its road, but it could not avoid facing what appears to be an increasingly complex biological reality. Reductionism may still come in handy for theoretical disputes or to advocate mass interventions, but truly defensible public health injunctions may not be possible without unraveling generic and individual complexity in some meaningful way. In this sense, the epidemiology of multifactorial diseases is still in its infancy and in need of much patience and humility. ■

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THE INTERPRETATION OF STATISTICALLY SIGNIFICANT RESULTS: THE IMPACT OF THE TAIWAN HELMET USE LAW

Chiu et al. reported the dramatic impact of the Taiwan motorcycle helmet use law on the mortality and morbidity of head injuries 1 year before and after the law's implementation.[1] The authors appear, however, in several places to be confused about the interpretation of statistical test results and inappropriately use these results to support their conclusion.

In Table 2,[1(p795)] they compare Glasgow Coma Scale [GCS] scores for the severity of motorcycle-related head injuries before and after implementation of the Taiwan helmet law. They report a 34.3% reduction in injury frequency for the severe injury group, a 35.5% reduction for the moderate injury group, and a 32.3% reduction for the mild injury group; they also report that a highly significant likelihood ratio χ^2 ($P<.001$) was associated with the results. But the likelihood ratio test performed here could not be used to support the claim that the number of severe head injuries decreased. It tested the proportional difference of various severity levels as a fraction of the total number of head injuries before and after the law was passed. More precisely, the test evaluated whether the proportions for the 3 severity levels before implementation of the law (484/5260=9.2% for the severe group, 521/5260=9.9% for the moderate group, and 4255/5260=80.9% for the mild group) were similar to those after implementation (318/3535=9% for the severe group, 336/3535=9.5% for the moderate group, and 2881/3535=81.5% for the mild group).

Reanalyzing the GCS data, I could not find any of the differences ($P=.77$) that Chiu et al. mentioned in their Results section ($P<.001$). This is not surprising, given the very similar proportions for the 3 severity levels before and after implementation of the law that are

shown in Table 2. Even though the overall number of injuries decreased by 33%, from 5260 to 3535, a quite different picture was shown by the likelihood ratio test. There is no statistical difference in the proportions of head injuries of various GCS severity levels during the 2 periods.

Second, regarding the Glasgow Outcome Scale (GOS) scores results also reported in Table 2,[1(p795)] although a significant likelihood ratio χ^2 ($P<.001$) was found for the changes in severe outcomes, it only indicates that the proportions of some of the outcomes, as a fraction of the total number of cases, were not statistically the same before and after the helmet law. Even though the proportions presented in the table appear quite similar, this significance is not surprising given the large sample size of the study. But the significant test result does not necessarily suggest that "outcomes were better (lower [GOS] score) after implementation of the law," as the authors claimed.[1(p794)] In order to show the decrease of the GOS scores, the authors should have applied an ordered categorical data analysis approach. The application of a mean score test using integer scores before and after the law showed no statistical significance ($P=.12$)—that is, the average GOS scores are quite similar.

Third, reanalyzing Chiu and colleagues' Table 3,[1(p795)] I found that all significant likelihood ratio tests for associated injuries, except for "chest," resulted in an increased odds of associated injuries, such as "total associated injuries" ($P<.001$), "cervical spine" ($P<.001$; this contradicts the authors' conclusion of nonsignificance for "cervical spine"), and "upper extremity" ($P<.001$). These results suggest a significant increase in the proportion of various associated injuries. Again, the authors inappropriately interpreted the significant χ^2 results and misused the statistical test results to support their conclusion of a reduction of associated injuries.

In order to arrive at meaningful and convincing conclusions, it is crucial that appropriate statistical analysis methodologies and proper interpretation of the results be applied. Without a proper denominator for the total number of motorcycle-related injuries (or accidents) before and after implementation of the law, it is very difficult to convince the reader of the law's effectiveness on the basis of a percentage change in head injuries alone. To determine the true impact of the helmet use law on the mortality and morbidity of motorcycle head injuries in Taiwan will require further study. ■

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ERRATUM

In: Heloma A, Jaakkola MS, Kähkönen E, Reijula K. The short-term impact of national smoke-free workplace legislation on passive smoking and tobacco use. *Am J Public Health.* 2001;91:1416–1418.

In Table 2 (p 1417), the first 2 columns under "Total" had the wrong headings: "1994–1995" should have appeared over the first column, and "1995–1996" should have appeared over the second column. The table is presented below with the correct headings.

TABLE 2—Prevalence of Daily Smoking By Education Level: 1994–1995 and 1995–1996

	Elementary or Comprehensive School			Senior High or Vocational School			College or University			Total		
	1994–1995 (n = 324), %	1995–1996 (n = 333), %	P^a	1994–1995 (n = 355), %	1995–1996 (n = 361), %	P^a	1994–1995 (n = 274), %	1995–1996 (n = 312), %	P^a	1994–1995 (n = 967), %	1995–1996 (n = 1035), %	P^a
Men	39.7	34.3	.228	33.9	26.9	.068	19.8	19.6	.967	32.3	27.4	.045
Women	32.0	29.5	.723	26.4	19.2	.295	11.8	9.7	.631	22.9	18.6	.220
Total	37.2	33.4	.307	32.9	25.3	.034	16.8	16.0	.790	29.6	25.0	.021

[a] χ^2 test for comparison of prevalences in 1994–1995 and 1995–1996.