Truth or Trope?
Genetic Research=Eugenic Practices=
Fascism=Hateful; Murderous Behavior

Critics: Truth

Two critics, an historian/philosopher of science and a psychologist, asserted here, on "political" grounds: Human behavioral genetics studies, particularly of so-called criminal traits, lead invariably — if not inevitably — to racist, elitist, and repressive policies that are aimed at the poor, the weak, and other vulnerable segments of society.

This analysis by Garland E. Allen, Ph.D., of Washington University in St. Louis, and Andrew Futterman, Ph.D., of Holy Cross College in Worcester, Mass., is materialist, and Marxian. They told colleagues at the genetics and crime conference:

Even though individual researchers may be well-meaning in their genetic studies, scientists are poor social critics. So their findings end up serving "the interests of the middle and upper middle classes (not to mention the enormously wealthy.)"

"The important lesson," the two critics say, "is to see how easy it may be for academic scientists, in need of research funds . . . to end up serving class interest of which they may not be fully aware."

Allen and Futterman insisted that, despite their political motives in opposing behavioral genetic studies, their distrust is intellectually well-grounded: The fatally flawed work of past researchers and the illegitimate political ends to which it was

Historians: 'Trope'

This is "a powerful trope," an historian declared in a paper prepared for the University of Maryland's conference here on genetics, violence, and crime (PROBE, Oct.). "It is also a trope that many thoughtful scholars and researchers have examined and explicated, as a warning to colleagues. 'Beware,' these authors are saying:

"Pay attention to the history of genetic research and eugenic practice, because otherwise you may repeat it."

(A trope, says our O.E.D., is "a figure of speech which consists in the use of a word or phrase in a sense other than that which is proper to it . . . ."

The science historian who introduced this trope into the conference, Ruth Schwartz Cowan, Ph.D., of State University of New York at Stony Brook, then deconstructed it. It is, historically, "only a partial truth," she said.

A University of Maryland legal historian, Richard F. Wetzel, Ph.D., carried this analysis a major step further: He showed that German eugenicists and biocriminologists in the Hitlerian era by and large were not antisemites. Their efforts to identify a criminal gene, or genes, had failed. Therefore, their research and ideas contributed little to the Nazis' killing of mental patients and other "inferiors," such
Follow-up

Harvard Fumbles
Fisher Case Inquiry

We were surprised to learn in September, from Pittsburgh surgeon Bernard Fisher, M.D., that Harvard’s Center for Health Communication, a part of its School of Public Health, was planning a postmortem conference on press coverage of the Fisher Case. It would be an invitational event. We had not been invited.

PROBE, readers will recall, was the first to report the fraudulent record-keeping at St. Luc’s Hospital in Montreal (Sept. ’93) that was the nub of Fisher’s problem. PROBE also was the first — and only — publication to cover the Chicago Tribune’s overblown coverage of this case, the following year, as a bad-news story about the media, which it was, and not as a major medical scandal — which it wasn’t (April, May, June, Sept. ’94). PROBE continues to be the only publication that reports it this way (Feb., March, ’95).

With this in mind, we contacted the Harvard event’s organizer, Jay Winsten, and offered our services:

We pointed out that no media critic was scheduled for his media critique panel. What is more, among the journalists scheduled for the panel, only one, Steve Twedt of the Pittsburgh Post-Gazette, shares our view — based on his own extensive research — that surgeon Fisher has been scapegoated for opportunistic, ideological, and political purposes. (Twedt’s work has of course been disdained by some because it appeared in Fisher’s “hometown paper.”)

Meanwhile, at least four of Fisher’s nemesis, including ex-ORI chief Suzanne Hadley, Trib reporter John Crewdson, ex-NCI oncologist Bruce Chabner, M.D., and breast cancer activist Fran Visco had been invited to participate at Harvard (albeit only Hadley and Visco showed up).

Winsten initially was receptive to our offer to participate. But after huddling with colleagues, he phoned back to say he would not invite us, since, he said, we would be perceived to be part of the “Fisher network.”

We replied that he had not felt similarly constrained when he invited much of the anti-Fisher network, including journalists, to Harvard. We told him we certainly would have had a good word to say about Fisher, but would address the forum strictly for ourselves — as the only science media critic on the case.

Winsten again declined.

We don’t know whether Winsten reads PROBE. He is not a subscriber. But he and his associates can hardly be ignorant of our work, since they reviewed Twedt’s series on Fisher in the Pittsburgh P-G, in which our coverage is cited.

(We spoke in this period with Fisher and his daughter, Beth. He could not attend, [see main story, Page 3]. We urged Beth continued on following page

Gap is Being Bridged
Between AIDS Patients
And AIDS Researchers

“Black folks and women should be part of the picture” in AIDS clinical studies, says caseworker Theodore Kirkland, of Washington, D.C. His job is to recruit them.

Kirkland runs the Capital Research Network, a new element in the District of Columbia’s AIDS care consortium.

“What we’re trying to do,” he explained in a recent phone interview, “is to get people to volunteer for clinical trials.”

He visits gay bars, homeless shelters, street fairs, and AIDS service organizations to find potential recruits, and persuade them to participate. In the Network’s start-up year, Kirkland said, he has recruited over 125 volunteers; most have been referred to one or more clinical trials at Georgetown University, Johns Hopkins, NIH, or other nearby medical centers. About 60% of his recruits are male homosexuals; about 30% are past or present I.V. drug users. Most are black or hispanic.

The Network was founded by American University psychologist Scott Parker, Ph.D. and three like-minded Washingtonians who realized that there was a gap between sick people who need AIDS care and clinical researchers who need to fill their protocols. Researchers’ needs and frustrations — were increased recently when the fed stipulated that blacks and women must be proportionately represented in AIDS and other clinical trials. The service is free for the patients.

“Blacks are the hardest to get, and the ones the drug industry is most in need of,” Parker said last month by phone.

Recruiter Kirkland said he faces resistance:

• “Blacks are suspicious that there’s a ‘Tuskegee’ [experiment] out there,” he said.

• “In the Spanish community there is a distrust of government,” particularly the immigration service.

• Among women, he finds “a problem of distrust, since the research community is white and male.”

The recruiter said he tells prospects:

“There’s a cure out there, and the only way we’re going to find it is by people participating in the research.”

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MEMBER, NEWSLETTER PUBLISHERS ASSOCIATION

Probe
Pivotal Reporting on Fisher Case Is Harshly Hit at Harvard Forum

The Chicago Tribune editor who handled reporter John Crewdson’s investigative reports on surgeon Bernard Fisher, M.D., and the breast cancer research project that he headed, had doubts about the stories. Editor Judy Peres was particularly concerned by the “alarmist — probably unnecessarily alarmist” — implications of some of Crewdson’s writing.

A Trib science writer, Peter Gorner, criticizes Crewdson for leaving out — “mistakenly or out of ignorance” — the fact that other large, randomized studies reached the same conclusion that Fisher did, a conclusion that Crewdson said might be fraudulent and wrong. This information “absolutely” should have been included, Gorner says.

Crewdson’s inflammatory reporting in the Trib (March 13, ’93) is almost wholly responsible for the damaging national hysteria that unseated Fisher as head of the National Surgical Adjuvant Breast Project (NSABP).

Crewdson’s reporting has had continuing — and devastating — effects on breast cancer research, particularly the NSABP-run Tamoxifen Trial. Its purpose is to determine if this anti-estrogenic drug will prevent breast cancer in women who are at high risk of the disease.

The NCI shut the trial down for six months after Crewdson’s reports began to appear, a blow to enrollment “from which [it] is still trying to recover,” according to Oncology Times (November). The negative publicity, generated by Congress as well as by Crewdson, also led to a sharp increase in women going off of their assigned medication in the double-blind study: Only 8,300, or 72% of the women are still taking their assigned drug.

As of September 1, 11,531 women were enrolled of the 16,000 who are needed. Only 60 new participants are being recruited each month, an inadequate number, the oncology monthly reports.

New Views Heard

The new revelations about the still-smoldering case — Fisher is being investigated by the Federal Office of Research Integrity (ORI), and has sued ORI, the National Cancer Institute (NCI) and their officials under the Federal Privacy Act for blacklisting his work — were revealed to several dozen participants in a recent confidential, closed-door critique of the print media’s role in the case. The conference was convened late in September by the Harvard School of Public Health in Boston.

The secrecy, meeting planners indicated, was to facilitate the free flow of comments by individuals who might otherwise be reticent to speak. However, Fisher worried that the closed forum, which included more enemies than friends, would further tarnish his name.

Fisher was invited. But he did not attend, on the advice of his attorney in the Privacy Act suit, according to Fisher, in Pittsburgh, and Harvard public health specialist Jay A. Winsten, Ph.D., who organized the journalistic postmortem. Fisher asked that Winsten be replaced by the lawyer.

Winsten denied this request, both men said in phone interviews, because some panelists may yet be deposed in Fisher’s suit. They did not want his lawyer to hear their recollections of the case, which could lead to later questions.

Winsten then invited a member of Fisher’s family, his Harvard-trained daughter Beth Fisher, M.D., who is a physician in Charlottesville, Va. She declined. (See Follow-up, Page 2.)

Coverage Assessed

The Harvard critique was set up to explore how the press framed the issues in the Fisher case, and why. It looked, too, at the coverage’s impact on public policy, and on breast cancer patients and scientists. Lessons were sought to help participants “deal differently with the press next time.”

Participants included medical and academic administrators, journalism teachers and journalists, government officials and

continued on page 5
Relman Backs Trib

The only physician on the Harvard panel, ex-New England Journal editor Arnold Relman, M.D., defends Trib reporter John Crewdson’s coverage. He faults his medical colleagues and NCI for failing to fulfill their responsibilities to the public and the medical profession.

“It was not clear at the time that Crewdson broke the story that there were not widespread irregularities [in the NSABP studies] that had to be corrected,” Relman said recently by phone from Boston. Those who criticized Crewdson at the outset “were wrong,” Relman declared.

“I think John Crewdson and the press were right in being suspicious,” he said.

Relman focused his criticism on Montreal breast surgeon Roger Poisson, M.D., who falsified entry data to get ineligible women into the study; Fisher and his University of Pittsburgh and NSABP colleagues; and NCI officials. Relman said they put their own interests ahead of their foremost responsibility, of responding promptly and decisively to the discovery of Poisson’s fraud. He said:

“In scientific research, if you don’t tell the truthfully, and as soon as you know the full extent of the errors, you’re in trouble. NCI and Fisher didn’t act as decisively as they should have!”

As it turned out, Relman explained, the NSABP study conclusions, at least in the short term, turn out to be valid. Fisher, he said, was not being dishonest. Poisson was wrong, Relman added, but he had the best of intentions. The major fault thus was not going public quickly and decisively, with the discoveries of fraud, once they became apparent.

Relman’s Rx for researchers:

“Be honest, and totally responsive and responsible!”

Peto Blasts It

Oxford medical statistician Richard Peto, F.R.S., arguably the dean of his field, condemns the attack on surgeon Bernard Fisher as basely motivated, stupid, and absurd. These views appear in a paper he drafted last summer, and has submitted for publication, along with 19 European scientific co-signers.

Peto, who has been a harsh critic of the Crewdson-Dingell—ORI attack on Fisher, flew to Boston in September to present his views to the Harvard postmortem conference on the print media’s treatment of the Fisher case. He and his colleagues brand the episode, and particularly Rep. John B. Dingell’s intervention, “Kafkasque.”

From a “scientific viewpoint,” they charge, “the chief failures of responsibility and of scientific integrity lie with the accusers, not the accused.”

One long — and continuing — sticking point is whether the Montreal cases should have been deleted from the NSABP tallies, once a surgeon there admitted he had enrolled “a few slightly ineligible patients” (to use Peto’s phrase). This unresolved disagreement may be partly why the New England Journal of Medicine, which reliable sources say has had an NSABP reanalysis of the key lumpectomy study in hand for almost a year, has thus far not published it.

Peto and his colleagues insist it would be scientifically wrong to delete any of the Montreal patients. “One of the central principles of randomized trials is that all randomized patients that possibly can be included should be included in the main analysis,” they say. “On scientific grounds, therefore, the NSABP was, quite rightly, unwilling to delete 1,400 properly eligible and documented patients, and was properly reluctant to delete completely the remaining 100 [of whom were ineligible by a few weeks or so], even though removal of about 0.2% of 50,000 randomized patients would

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PROBE’s View

We agree more with Richard Peto than with Arnold Relman (above). Yes, Fisher should have been more forthright when asked about the problem.

But: Fisher had reported the problems in Montreal very promptly to NCI, which officially forbade him to say anything about them for more than two years. The agency reviewed the case in this time, and found the Montreal surgeon guilty of scientific misconduct, based on his own admission.

If there had been any discoveries during this period in Montreal or Pittsburgh that called the lumpectomy study into question, we believe NCI could and would have blown the whistle, loudly. The ORI—which is not under NCI’s thumb—almost certainly would have.

We believe frauds and conspiracies exist. But massive ones, involving many people, are highly improbable. This applies to Fisher’s NSABP, NCI, and the Department of HHS. (The CIA’s shocking cover-up of the Ames case is certainly an exception.) Investigative reporters walk a thin line between suspicion and paranoia. They need to keep this in mind — and stay on the suspicion side. Indulging paranoia in print when you have nothing more than suspicion to go on is an error in our view.

The fact that one or even a handful of researchers, out of 5,000, falsified entry data into a study — but not outcome data — is not grounds to impeach the whole study unless you have other strong suspicions. Crewdson, by all accounts, did not have sufficient grounds to discount the whole of NSABP when he published his first report — and some of the grounds that he published later turned out to be wrong.

Given the damage he did to women’s peace of mind, researchers’ reputations, and the progress of key studies — the NSABP’s patient enrollment fell almost to nil — the setbacks far outweigh the reforms Crewdson claims for his reporting. — D.R.Z.
Harvard ...

continued from page 3

researchers, according to one Bostonian who was present. A public report is now being written, this participant said.

Major players in the Fisher case were invited to attend as panelists or as discussants. They included ex-ORI chief and advisor to Rep. John Dingell (D-Mich.), psychologist Suzanne Hadley, Ph.D., Washington, D.C., and former NCI official Bruce Chabner, M.D., one of Fisher's administrative overseers, who now is at Harvard. Chabner was a late no-show, one conference participant told Probe last month by phone.

Crewdson was invited to attend, but sent his regrets... The Tribune was represented by Peres, who also is science writer Gomer's editor.

Case Report Prepared

Gomer's comments appear in a narrative "case report," prepared for the conference by Washington, D.C., freelance writer Lisa Sheikh. She based it on documentary and journalistic records of the case, and interviews and correspondence with key participants, several of whom later were panelists in Boston. Sheikh sent her initial draft to her informants for comment, and then re-edited it into a working draft to guide the discussion. Crewdson declined to participate in this exercise.

Gomer told Sheikh that Crewdson and many other Trib journalists and editors who handled the story seemed to be unaware of the lumpectomy vs. mastectomy history. Even when it was mentioned, it was discounted. He said:

"I would have made a paragraph up high, saying that as far as anybody knew, the operation is safe, and that the other studies were not in question. One part of this study is in question. . . . "Crewdson," Gomer said, is "an investigative reporter, and there was fraud in a study," according to the case report. "I'm a science writer," he added, "and I know that for 30 years doctors have been fighting over those two operations, and that they had reached a consensus. And I know that no incident of fraud in any one study or even a bunch of studies is going to change anyone's mind."

Crewdson argued, however, that the other studies weren't worth much statistically. This was why the NSABP fraud was so significant, Sheikh quotes his Trib editor, Judy Peres, as saying.

Tone It Down, Editor Said

Peres said she was confident that Crewdson's reporting was correct. But she worried about how the story would play:

"I actually argued to tone down some of Crewdson's stuff," she explained in the conference report, "because even though you know he's right, the conclusion that you would draw from it is alarmist . . . ."

Peres said that since her two reporters - both Pulitzer Prize Winners - disagreed on the story, she let each have his say:

Basicly, my way of reconciling it was to let each [reporter] take responsibility for what was published under his own byline. I raised the question. They were both able to defend their position. I trust them both. So, they can disagree. I mean, scientists disagree.

I talked to several scientists about the NSABP thing. . . . A lot of them weren't as well informed as my reporters.

Journalists were not Crewdson's only critics. Chicagoan Michelle Melin, who is director of patient services at Y-ME, a breast cancer hotline, was "harshly critical" of the Trib's coverage, Sheikh reports. Melin called it "absolutely, uncategorically" alarmist, Sheikh says.

Activists Upset, Too

"We were very angry at the Tribune for their coverage, and tried to explain to them how [it] could really harm women," Melin told Sheikh. "I'm not saying that scientific misconduct should be covered up, just reported fairly."

Crewdson had champions as well as critics. Notable among them was internist Arnold Relman, M.D., the former editor of the New England Journal of Medicine. Given what was known at the time, Relman said, the coverage was wholly appropriate. (See separate story, P. 4).

Medical writer Paul Goldberg, of the Cancer Letter in Washington, D.C., told Sheikh that Crewdson's stories "were not off the wall," but rather were "very much in the mainstream of journalism."

The press nevertheless came in for caustic treatment from other participants in the Harvard event. British medical statistician Richard Peto, F.R.S., of Oxford University — who confirmed the NASBP findings' validity after they were challenged by ORI, NCI, Crewdson and Dingell — was harshly critical of coverage in the Trib and the New York Times and others (as were we at the time [PROBE, April, '94]).

"I think that the newspaper reports have made clear that the type of errors involved [in the NSABP study] were the type that could not materially bias the results," Peto told Sheikh, a point that he reiterated in Boston. The science writers "should have

continued from following page
Harvard...

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known better," he told her.

"I think that people have been frightened into acquiescence in stuff that the [writers] know, absolutely explicitly, to be utter nonsense, and that [same criticism] would apply to lots of people at NCI as well," the British expert told freelance writer Sheikh.

Reporter Responds

Crewdson took strong exception to Sheikh's reporting and analysis. While he chose not to confront his critics directly, he sent, with Peres, a thick, detailed rebuttal to criticisms contained in Sheikh's report. Peres handed it out to panelists and audience participants.

(PROBE requested a copy from Peres. She initially agreed, and phoned Harvard to have a copy sent from the boxful she'd brought from Chicago and left behind in Boston. She was told she couldn't release it to PROBE, even though Crewdson had asked that it be distributed to conference participants, as well as "anyone else who has been furnished with Ms. Sheikh's manuscript." When Peres then asked him for a copy for PROBE, he declined to provide it, she said, citing the confidentiality agreement with Harvard. Neither would he discuss it. PROBE obtained Sheikh's report and Crewdson's manifesto elsewhere.)

One journalist who attended the conference jestingly compared Crewdson's massive 162-page document to the Unabomber's manifesto, published recently by the Washington Post (Sept. 19).

In this massive rebuttal, Crewdson concedes no errors. He attacks Sheikh's criticism that, in his words, he "failed to include various exculpatory statements by NSABP and National Cancer Institute (NCI) officials who were under fire for their handling of the Poisson affair."

Errors Are Seen

Crewdson retorts that Sheikh "has assembled an impressive collection of misstatements, misinterpretations and misrepresentations, many of them from individuals whose careers and reputations were affected by the [Roger] Poisson affair" in Montreal. (Poisson is the one NSABP doctor who has admitted to misconduct in the studies.)

Harvard might better have used its "distinguished offices" to explore "the institutional weaknesses, procedural dilemmas, and ethical conundrums" raised by surgeon Poisson's actions and NSABP and NCI's failure to vigorously respond, Crewdson said. He writes, proudly, that in the aftermath of his reporting NCI admitted a half dozen errors and oversights, and imposed over a dozen reforms, including the firing of surgeon Fisher.

"The failed institution," Crewdson asserted, "was not the press."

Despite this tardy rejoinder, Crewdson and the Trib were definitely not heroes at Harvard's postmortem on the Fisher coverage.

Truth...

continued from page 1

put, condemn this line of study. Particularly, they cite foolishly flawed eugenics research, principally in the U.S. and Europe; studies in the 1960's and '70s of alleged criminality in men with an extra male chromosome, the so-called XYY syndrome; and contemporary Swedish studies on criminal traits in adopted children. The two critics say:

• The data are persistently inconclusive.
• Historically, the theories that arise out of these studies are misused.
• People are "seriously harmed" as the result.

Eugenics, they explain, was adopted by proponents of social and economic efficiency programs who wanted to lower social costs and free society from the burdens of caring for the disabled and ill. They favored social regimentation as a way to raise industrial productivity and profits.

The two scientists nevertheless appear somewhat ambivalent about their anti-scientific position regarding behavioral genetics. They note, for example, that genetic and biological determinism have "always" been used to limit, rather than expand downtrodden peoples' access to social resources. But, they allow, "there is no logical necessity to this outcome." They cite Down's syndrome as a genetic condition for which growing biological understanding has been accompanied by deinstitutionalization and other humane social policies. These reforms have improved these children's lot in recent decades.

"Genetic limitations, even when they clearly do exist, do not in themselves dictate any specific social policy," Allen and Futterman allow.

Bad Work Denounced

In a similarly hedged statement, in a long and convoluted paragraph on behavioral genetics, they end up saying that "we strongly oppose pursuit of such work." But their meaning is unclear, and when their antecedent for "such work" is pursued, upward, through a dozen lines of turgid prose, it turns out that "what we do oppose are studies of human behavior genetics that are over-simplified and methodologically sloppy, [and] claim more for their results than the results can demonstrate." [emphasis added]

Who could argue? The camouflaged qualifier turns their much-feared target into a straw man. It also spares Allen and Futterman from the logic of their argument, which is to oppose a developing line of scientific investigation. And it spares them, too, the embarrassment of saying who would ban their colleagues' studies, and how it should be done.

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That, they presumably will leave to the politicians and moralists, who care little for thinking or for research — or its benefits — to start with. By Allen and Futterman's logic, we also should ban cancer research, since most "cures" thus far have failed, and many patients have, in fact, been injured by the side effects.

— D.R.Z
as Jews and Gypsies. (See below.)

The term *eugenics*, Cowan noted, was coined by Darwin’s cousin, Francis Galton, as “the science of improving human stock.” In this vein, *eugenics* and the various other types of genetics that have followed it, such as *medical genetics*, are all simultaneously science and technology; basic and applied science; and political as well as scientific programs.

The political program, she stressed, more often than not has been — and is — reform — not inhumanity and repression.

Many eugenicists were “progressives,” Cowan explained. For most — and historically the majority were women — the aim was to help mankind.

Cowan said that her own and others’ research on the history of genetics indicates that eugenicists were politically varied. Some were in fact conservative, even reactionary. Others, like John Maynard Keynes, the economist; Harold Laski, the British socialist; and sexologist Havelock Ellis were socialists or radicals. Margaret Sanger was a eugenicist, too. Cowan said:

> "The history of the eugenics movement teaches us that there is nothing politically determining about genetic or biological determinants. . . . Similarly, genetic and/or eugenic arguments can be, and have been made for policies as conservative as immigration restriction and as radical (at the time) as legalized abortion. In political terms the eugenics movement appears to be a chameleon, taking on different political colorations in different times, in different places, even in different minds.

The Eugenics’ programs for improving the human stock were similarly diverse. While some advocated sterilizing the “unfit” others supported tax incentives to encourage stable, “fit,” middle class families to procreate — a principle that is now embedded in the latest Republican tax bill in Congress.

“Today,” Cowan said, “few of us recognize that this almost unexceptional provision of income tax law originated in eugenic arguments.”

Some eugenicists were racists, as well as hereditarians, Cowan notes. They were advocates for the three darker expressions of eugenics: immigration restriction, miscegenation laws, and genocide. Prominent eugenicists supported the first two, if not the third.

**Hybrid Vigor Hailed**

However, Cowan declared, “the logic of eugenics was not necessarily in favor either of immigration restriction or of racial separation.” In fact, some eugenicists — particularly in Mexico — turned the racial purity argument on its head: Based on the genetic principle of hybrid vigor, they insisted, racial mixing was better for a nation than racial purity.

For many eugenicists, Cowan added, the notion of “racial hygiene” did not refer to Aryans or Germans, or any other particular group, but rather, as Galton originally intended, to the “human race.” In Hitler’s Germany, some eugenicists leaned toward the Nazi racist view, while a competing group insisted 

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**Anarchist Offers His View**

> “Few people will resist the introduction of a genetic technique that eliminates a hereditary disease. It does no apparent harm, and prevents much suffering. Yet a large number of genetic improvements, taken together, will make the human being into an engineered product rather than a free creation of chance (or God, or whatever, depending on your religious beliefs).”


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**Simple Links Unreliable**

In Cowan’s view, there is no necessary correspondence between a set of political beliefs and a set of scientific interests. Rather than try to relate science to politics, she added, all disciplined scholars, whether they are historians, psychologists, or geneticists, ought to be skeptical of everyone’s findings — including their own. This is a professional, not a political imperative, in her view.

“As citizens,” she added, “we ought to make the best judgments we can on the basis of whatever philosophies we hold, and whatever information we regard as least fallible.” But we must recognize “that neither history nor science gives us any guarantees of what connections will be made and what consequences will result.”

A major complaint about biocriminology in general, and genetic research in particular, is that, as with the Nazis, it will lead to racism, persecution of minorities, even “final solutions.”

**German Researchers Scrutinized**

This proposition was assessed by University of Maryland historian Richard F. Wetzell, Ph.D., an expert on German criminology. He said that modern German biocriminology sprang from the work of Cesare Lombroso and Franz von Litz, in the late 19th century.

They wanted to individualize criminal law and punishment. Their aim was to tailor punishment to the individual’s crime and rehabilitative prospects, on the one hand. They also wanted to predict and intervene in “born criminals” lives before they began their criminal careers. Lombroso, Litz and their followers thus were reformers — and some, as it happens, were Jewish.

Initially, Wetzell said, these researchers felt they could identify target families, based on observed traits such as imbecility, feeble-mindedness, and other “inferiorities.” But after a half-

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November 1, 1995 (delayed)
century's effort, they realized they had failed to delineate meaningful prognostic categories. Gustav Aschaffenburg, who was Jewish, wrote in 1932, as Hitler came to power, that crimi­
nal biology was "far from possessing reliable methods for exa­
mining and evaluating the personality of the criminal." So it was "currently impossible to separate corrigibles from incor­
rigibles with criminal-psychological methods."

Did criminal biology contribute to Nazism?

Not very much, said historian Wetzell. For one thing, Jewishness was not identified by criminal biologists as one of the "inferiorities" that needed to be cleansed. That was the Nazis' idea. (What did happen is that Jews like Aschaffenburg lost their jobs.)

Some later, Nazified, criminal biologists did link Jewishness to crime — but not to Jews' genetic predispositions.

"As far as the [Nazis'] central research agenda — the con­
nection between heredity and crime — was concerned," Wetzell said, "anti-semitism was of little importance."

American eugenicists were a more important influence on Nazi sterilization laws and practices than were German biocrinologists, he added. Nazi laws in 1933 legalized steril­
ization of people who were "hereditarily ill," but do not men­
tion criminals, Wetzell explained. The reason, as one biocrim­
inologist pointed out then, was that criminal behavior did not stem from a "unified hereditary disposition."

Wetzell concludes (in part):

While the finding that anti-semitic ideology did not fully penetrate criminal biology may seem surprising in the context of our image of the Nazi state, it makes good sense if seen in the context of the long-term development of German criminal biology. For although criminal biologists had been strongly attracted to biolog­
ical, hereditarism, and eugenic ways of think­
ing... their work had showed no signs of anti-

Legal historian Wetzell and science historian Cowan thus are in agreement: Genetic and biological research on crime, however flawed they may have been from the scientific viewpoint, were not precursors of the Final Solution.

Peto ... 

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make virtually no difference to anything."

In comments submitted to the Harvard conference, Trib reporter John Crewdson claims credit for the strict new audit­ing procedures introduced by NCI in the wake of his reporting.

Peto and his colleagues retort that these are futile, dangerous, unproductive changes. Clinical trials that are so fundamentally flawed that problems could be detected by audit are uncom­
mon. Even when they do occur, the statisticians and clinical trial specialists said, they are unlikely to have much effect on the growth of medical knowledge. But:

"Rules that waste a lot of time (and money) and lead scientists to work defensively could very well have a big negative influence."

Peto and his colleagues warn:

"If clinical trials are made more politically correct, then they may well be made less scientifically correct. That is not what future patients deserve from the current generation of medical research workers."

The psychiatrist in our family returned home from a recent conference with this succinct definition of her pro­

fession:

Psychiatry is the study of the id by the odd.