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Reply to Ernhart, Scarr, and Geneson

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Drs. Ernhart and Scarr's gravamen, composed with the assistance of David Geneson, Esq., seems to be that having put on the robes of whistleblowers, they failed to receive the gratitude from the public and their colleagues that they felt was their due. Instead, they were abused and bullied by me, vilified by a learned society, rudely parodied in a newspaper article, caricatured in a cartoon, treated with suspicion by the press and colleagues, and accused of being employees of the lead industry. There is more. The *Harvard Mental Health Letter* unfairly rejected a manuscript as "too political," and they believe that their grant applications are receiving overly severe, possibly biased reviews. This stigma is said to have extended past them; Dr. Scarr reports that her daughter has been the recipient of anonymous professorial obloquy.

In the space allotted me, I correct some, but not all, of the distortions, elisions, and misrepresentations in their article. I refer readers to a published article in *Pediatrics* (Needleman, 1992) for further information.

In 1974, one of the better early articles investigating the relationship between lead and children's IQ was that of Claire Ernhart (Perino & Ernhart, 1974), who reported that Black preschoolers with blood leads over 40 μ/dl had lower IQ scores than children with blood lead levels below 30 μ/dl. At the time, it served as the standard for work in the field. In 1979, I published an article in the *New England Journal of Medicine* (Needleman et al., 1979) that responded successfully to some of the extant design problems in this area and, as a result, became the focus of considerable attention. The lead industry, in the form of the Lead Industry Association and the International Lead–Zinc Research Organization (ILZRO), was uncharacteristically silent. Then, they called for my original data. I declined. I was willing to share my data, and did, with other bona-fide investigators, but I excluded the industry from membership in this class. The lead industry had expended considerable energy to damage the reputation of earlier lead scientists, and I became their newest target (Baldwin, 1992).

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During the 1982 Environmental Protection Agency (EPA) Lead Criteria Document hearings, the industry raised questions about the fidelity of my data, and a committee of outside scientists was designated to look at my data and Ernhart's in depth. Sandra Scarr was a member of that committee. Scarr stated that "my cooperation was limited." This is demonstrably untrue. After the visit was concluded, the Chairman of the visiting committee and chief of the EPA Criteria Office, Dr. Lester Grant, wrote me (L. Grant, personal communication, April 8, 1983):

> Thank you for taking time out of a busy schedule to meet last week with myself and members of the expert committee on associations between neurobehavioral effects and low-level lead exposures in children. Your assistance in providing an introductory overview of your studies, in making computer printouts of data and statistical analysis results available for inspection by the committee, and in answering questions posed in regard to procedures employed in collecting the data and analyzing the results of your studies was quite helpful and appreciated by us, as was the assistance provided by your secretary during our visit.

The committee's draft report stated that no conclusions could be drawn from either Ernhart's or my study. I had discovered 11 errors in their draft report and contested their conclusion. EPA, through Hugh Pitcher, then at the EPA Office of Policy Assessment, asked me to reanalyze the data to meet the criticisms of the visiting committee. I did, and I presented them to the EPA Clean Air Science Advisory Committee (CASAC) meeting in North Carolina. Scarr's statement that CASAC decided that the study "was not so seriously flawed as to exclude its findings from consideration" is also demonstrably misleading. This is what EPA, following CASAC's advice, said:

> A pioneering general population study was reported by Needleman et al (1979). . . . Significant effects ($p < .05$) were reported for full scale WISC–R [Wechsler Intelligence Scale for Children–Revised] scores, WISC–R verbal IQ scores, for 9 of 11 classroom behavioral scale items, and several experimental measures of perceptual motor function.
>
> Reanalyses carried out in response to the Committee's recommendations have been reported by Needleman (1984), Needleman et al (1985) and U.S. EPA's Office of Policy Analysis as confirming the published findings on significant associations between elevated dentine lead levels and decrements in IQ. (United States EPA, 1986, pp. 12-86–12-87)

Dr. Morton Lippmann, who was the Chairman of CASAC at that time, later wrote about that meeting and commented on the thoroughness of the review (M. Lippmann, communication to Bernadine Healy, February 7, 1992):

> As Chairman of the EPA's Clean Air Science Advisory Committee (CASAC) between 1983 and 1987, I participated in a series of thorough reviews of the same charges, and our committee was unanimous in finding them groundless. No study is perfect, or fully and perfectly presented in scientific papers, and Dr. Needleman was cooperative and convincing in addressing all of the concerns raised by EPA's own investigative committee and the expert members on our CASAC Panel.
>
> I have continued to follow the emerging literature on the neurotoxic effects of lead on children, and there can no longer be any scientific doubt that the initial and disturbing findings by Needleman and his colleagues are thoroughly consistent with other related but different studies.

My reanalysis of the data was also published, after peer review, in *Science* (Needleman, Geiger, & Frank, 1985). There actually have been five reanalyses of my data by independent reviewers (Atkinson, Crocker, & Needleman, 1987; Needleman et al., 1985; Schwartz, in press; and the analyses of the members of the University of Pittsburgh Inquiry Panel and Hearing Board). Each of these investigators was given a set of raw data to use; each found a lead effect. The comment that the Atkinson et al. (1987) article was published in an obscure journal is gratuitous, as is the vague statement about appropriateness of the reanalysis. Scarr and Ernhart fail, as is their custom, to mention what precisely was inappropriate.

Sandra Scarr never conducted any studies of lead in development to my knowledge. After her brief term on the visiting EPA team, she was not visible to me until 1990, when she surfaced along with Ernhart as a member of a lead industry defense team. The EPA and Department of Justice were suing three owners of a lead mine in Utah, asking compensation to remove a mountain of lead tailings. I was an expert witness for the government. Again the industry requested my raw data; again I refused. Instead, I offered Scarr and Ernhart the opportunity to review my printouts in my lab. Because I did not extend the invitation to industry lawyers, when one showed up, I offered him a chair in the hall. The claim was settled, and the government received $63 million to make the area safe for residents.

In her lecture at Massachusetts Mental Health Center in 1991, Scarr vividly described her and Ernhart's departure from my lab:

> So we left with our notes, trembling, trembling. We went to the airport, set ourselves up with a martini at the United Airlines Red Carpet Club, and wrote an outline for our report. So the report results from the two of us having done notes on all of his analyses which is what I did, and notes on how he got rid of subjects that were contrary to his hypotheses.

It is not surprising that this fevered state of mind incubated a troubled report. The report was circulated widely. I received a copy from a lawyer in Philadelphia. He obtained it in a trial in which Ernhart participated as a witness for the defense. Then I began to receive copies of newspaper columns by Warren Brookes, a syndicated economic columnist of conservative bent. He stated that two psychologists had uncovered what may be the scientific fraud of the century, and he called on Congressman Dingell to look into the matter. The footprints of a public relations campaign were visible everywhere. After these columns appeared, I was notified that the Office of Scientific Integrity had instructed the University of Pittsburgh to conduct an inquiry into my work.

The inquiry panel reviewed my raw data and printouts. Scarr and Ernhart's quotation from the panel's comments is extraordinarily selective. This is what they omitted: " . . . the panel found no evidence of fabrication, falsification or plagiarism; (2) material failure to comply with federal requirements for the protection of researchers . . . ; or failure to meet other material legal requirements governing research" (Needleman Inquiry Panel, 1991, p. iii). I showed in my response to the Dean of the School of Medicine that the statement that I misclassified subjects was erroneous; many of the subjects that they said should have been included in the study were in fact not subjects at all. They were vacant case numbers in the chemistry lab book.

Nevertheless, the university moved to begin an investigation. I was told that I would have the opportunity to confront my accusers, who were willing to come to Pittsburgh. Because it has always been my firm belief that truth grows only in the sunlight, and that I had nothing to hide, I made an unprecedented request: I wanted the hearings to be open to scientists, the press, and the public. Amazingly, the administration denied my request. I went to the Faculty Senate, the Faculty Assembly, and the Faculty Counsel. My colleagues on the faculty unanimously instructed the university to grant me an open hearing. I filed a suit in federal court for that right.

The university reluctantly acquiesced, and the hearings were opened. I was told that my accusers now were not sure that they would come. Negotiations ensued, but I was not a party to them. It is clear that Scarr and Ernhart reached an agreement with the hearing board that they would be shielded from answering certain questions of mine. At frequent intervals during the hearing, William Cooley, the chair, instructed my accusers that they were not required to answer my questions, blunting my efforts to get at the truth. The nature of their refusals was often instructive.

Scarr and Ernhart claimed that there were three grounds for believing that I had committed misconduct. First, they said that I failed to control for important covariates. This is, of course, a commonly debated issue in psychological studies, but they attempted to elevate it to the level of cheating. I measured the relationship between lead and IQ (using the WISC–R) and other non-age-normed outcomes. WISC–R scores are, of course, age normalized

when calculated. For the tests that were not age normed, we entered an age adjustment into the model. We did not enter age into the model when outcomes were age normed, because that was a typical example of overcontrol. In the *Science* article, I published analyses of lead-IQ regressions, adjusting for age, but using *raw* IQ scores as dependent variables. The relationship was unchanged. This has been brought to my accusers' attention on multiple occasions.

It was clear to me that Scarr and Ernhart applied a different standard to my work than to their own. In the hearing, I asked Ernhart about her approach to age adjustment. The hearing transcript reads (University of Pittsburgh, 1992, p. 47):

Dr. Needleman: Isn't the Wechsler age adjusted?
Dr. Ernhart: The norming of the Wechsler is age adjusted . . . norming alone is not sufficient to handle age variation. . . .
Dr. Needleman: So it would be better to enter age into the model?
Dr. Ernhart: Yes. . . .
Dr. Needleman: In your 1981 paper did you put age into the model?
Dr. Ernhart: My study is irrelevant to the issues here today.

Ernhart had not controlled for age in her study, and Scarr had used age-normed IQ scores without adjusting for age on more than one occasion.

The second charge was that I excluded cases to optimize the relationships I reported. We had made an a priori decision to exclude cases of children who had head injury, did not speak English as the first language, or were thought to have had lead poisoning. In the hearing, I showed both Scarr and Ernhart a piece of computer code from my printouts that headed *every* data analysis. Translated, it said: "Select if lead level equal high or low, and head injury equal 'no,' and plumbism equal 'no' and English is the first and only language in the home." This proved conclusively that the subjects were excluded on criteria that were preestablished and that the exclusion was executed by computer without any human judgment. There could be no selection to optimize hypotheses. I asked Ernhart if she had seen this piece of code (University of Pittsburgh, 1992, p. 73):

Dr. Needleman: . . . does this look familiar to you at all?
Dr. Ernhart: I don't recall having seen this.

This piece of computer code appeared 24 times in the printouts, at the top of every analysis that I provided them. No one who was looking for the methods of exclusion could have missed it. I pursued the same question with Scarr (University of Pittsburgh, 1992, p. 143):

Dr. Needleman: Did you see it [the code]?
Dr. Scarr: I assume that I did if it was a piece of paper on the inside—
Dr. Needleman: Then how could you say that we excluded cases with knowledge?
Dr. Scarr: Because I saw the computer outputs in which children were marked in and out and they were changing samples along the way, the way I interpreted what I saw. Of course we couldn't ask you any questions at that time. It was difficult to interpret, so we reported what we saw.
Dr. Needleman: So you might have made a mistake?
Dr. Scarr: I'm waiting for the investigation board to look at just that sort of evidence.
Dr. Needleman: What's your perception, given the fact that code was there, and given the fact—
Dr. Scarr: I'm not prepared to say because I don't know.

The third charge, that we capitalized on chance, was simply silly. Everyone studying lead measures IQ as an outcome as we did. We added many other measures, and most showed a decrease in function with lead.

Scarr and Ernhart also stated that I had sued them before, but there is absolutely no truth to this.

Scarr and Ernhart bridle at the thought that they might be considered employees of the lead industry. Yet, within a year of the time that Ernhart reevaluated her conclusion that lead was toxic at low doses to children and stated that "if there are, in fact, behavioral and intellectual sequelae of low levels of lead burden . . . these effects are minimal" (Ernhart, Landa, & Schell, 1981), she became a grantee of the ILZRO. She began to speak against controlling lead in the environment. When there was a move to put lead back in gasoline, Ernhart appeared in testimony for the Lead Industry Association, declaring there was no valid health reason to ban its use. Over 7 years, she received about $375,000 from ILZRO. Scarr has appeared as a paid expert witness for the lead industry in at least two lawsuits. Geneson is a lawyer for Hunton and Williams, a firm that represented EI DuPont and Ethyl Corporation of America (the two largest manufacturers of tetraethyl lead) before the Federal Trade Commission and the EPA. When Ernhart was asked who was paying their legal fees, she refused to answer. Every expert witness knows that being questioned on interest or bias is routine, yet these witnesses refused to answer questions on this point. The newspapers then carried a story that a "trust fund" had been set up to cover Scarr and Ernhart's legal fees by unknown donors.

Heroism is often purchased at a steep price. Many whistleblowers in the past have been maltreated by their institutions. Two authentic whistleblowers come to mind: Robert Sprague, who, after 17 years of continuous funding by

the National Institutes of Health, felt compelled to expose a fraudulent psychologist and as a result lost his grant support; and Margot O'Toole, who was unable to find employment in science for years after she reported that Imanishi-Kari had manipulated data. Scarr and Ernhart dishonor the name of authentic whistleblowers by trying to join their ranks. I know Scarr and Ernhart. They are not Sprague and O'Toole.

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Of Whistleblowers, Investigators, and Judges

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Needleman's (1993) reply to our article in this journal (Ernhart, Scarr, & Geneson, 1993) is a good example of the tactics he uses to deflect attention from questions of his scientific misconduct. Rather than address the many doubts about his scientific conduct, he attempted to focus readers' attention on (a) the motives and character of colleagues who question his research, (b) legitimate debates in the research literature on low-level lead effects on children, and (c) testimonials by colleagues who cannot know about misconduct in his research. He did not address our major thesis—that procedures by which investigations of scientific misconduct are carried out require revision. Our experiences as whistleblowers were used to illustrate the need for change. Needleman's reply illustrates how he has dealt with the series of investigations of his scientific misconduct—with sarcasm and innuendoes about the honesty and character of investigators, portrayed as conspirators with the lead industry (and newspaper columnists?) to bring him down. For more details of his obfuscation, see Scarr (1993), Ernhart (1993), and Ernhart and Scarr (in press).

INVESTIGATION–BACKGROUND

It is remarkable that Herbert Needleman continues to attack us personally as whistleblowers in his case. The responsibility for evaluating the misconduct issue is not ours—we have never had full access to the records. Our role was to report suspicions of his misconduct, described in the report that we filed with the National Institutes of Health–Office of Scientific Integrity (NIH–

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OSI) in May 1991. His colleagues at the University of Pittsburgh, not we, found him guilty of deliberate misrepresentations in his publications (*Needleman v. Healy,* 1992; Cooley, Doreian, McCall, Reinmuth, & Rosenkranz, 1992).[1]

From 1983 to the present, Dr. Needleman's continuing troubles with scientific integrity have resulted from increasingly thorough inquiries and investigations conducted serially by (a) the Expert Committee on Pediatric Neurobehavioral Effects of Low-Level Lead Exposure of the Environmental Protection Agency, (b) Drs. Ernhart and Scarr, (c) the NIH–OSI, (d) the Inquiry Panel at the University of Pittsburgh, (e) the Hearing Board at the University of Pittsburgh, and (f) the Department of Health and Human Services–Office of Research Integrity (DHHS–ORI, successor to NIH–OSI). At each stage, it was found that there were enough unresolved indications of irregularity to warrant continuance to the next step. The problems found now rest with the DHHS–ORI, for their judgment, and with the U.S. District Court for the Western District of Pennsylvania, which will have to resolve Needleman's suit against the University of Pittsburgh and the NIH.

At each step, the investigative groups could have found him innocent of misconduct and ended the series of investigations; they did not. Rather, the investigation process has continued serially because increasingly thorough investigations have documented more securely the actual conduct of his research. Despite his focus on our role in bringing suspicions about his research to the attention of the NIH–OSI, we are at least four steps removed from Dr. Needleman's current plight. The two University of Pittsburgh panels of dedicated and dispassionate scientists were the only ones with sufficient access to his data to make a limited but conclusive evaluation.

INVESTIGATION–UPDATE

At the request of the University of Pittsburgh, we kept the report (Cooley et al., 1992) of the Needleman Hearing Board confidential while his appeals to his Dean and the Provost were reviewed. The Dean accepted the report, and Needleman subsequently withdrew the appeals to the Provost. The DHHS–ORI is now conducting its own investigation. If he is found to have committed scientific misconduct, Needleman will be offered a hearing. Because this hearing could be compromised by release of the report, we were asked to continue to maintain confidentiality.

Needleman v. Healy (1992), however, has made public an important finding—He was found to have engaged in deliberate misrepresentation. How deliberate misrepresentation escapes the definition of misconduct is a matter of speculation (Taylor, 1992). ORI recently reached a finding of scientific misconduct on the part of an investigator who was said to have "falsely reported" a critical fact and "intentionally mislead colleagues" (p. A1) in the Gallo case (Hilts, 1992). If ORI concurs with the Pittsburgh verdict of deliberate misrepresentation by Needleman, it would be difficult to avoid a decision of misconduct in this case.

[1]The University of Pittsburgh released the Needleman Hearing Board Report (Cooley et al., 1992) after the preparation of this article. This report, now publicly available, documents well both the substandard science and the deliberate misrepresentation involved in this study.

MORE EVIDENCE FOR THE ORI

ORI has access to more information than did the Pittsburgh Board. For example, a graph altered by Needleman may be included in the investigation. (The Hearing Board attributed the alterations to one of Needleman's associates. Needleman has not provided an explanation of these alterations and, even though he has used the altered graph in presentations, he is apparently allowing the attribution of responsibility to his colleague to stand.) The original graph, a plot of the cumulative frequency of Verbal IQ scores of low- and high-lead children, was deceptive in that no adjustment was made for confounding (Needleman, Leviton, & Bellinger, 1982). The graph has since been enhanced (Needleman, 1987) to bring the difference between medians to 6 points, similar to the difference between means that is central to Needleman's argument. Furthermore, additional high-lead, low-IQ points were added; this addition supports computation of high relative risk ratios for low IQ as a function of high lead level. This altered graph was used to support the recent redefinition of lead poisoning issued by the Centers for Disease Control (CDC, 1991).

Lingering questions about reanalyses are highlighted by Needleman's reply. One reanalysis he has repeatedly attributed to the Environmental Protection Agency's (EPA's) statistician, Hugh Pitcher, evaporated at the Open Hearing, when Pitcher admitted he had never conducted a reanalysis of Needleman's data. Insofar as we can determine, all reanalyses, including those he provided to the EPA and published (Needleman, Geiger, & Frank, 1985), were of various unspecified subsamples of less than the full sample of 270. Describing the most recent reanalysis, Schwartz (1993) stated that no subjects were excluded, but at Needleman's open hearing Schwartz said that he used the data of only 210 of the 270 children. Reports of this and the various other reanalyses provide no description of, or justification for, various subject inclusions and exclusions.

Needleman (1993) said, "each of these investigators was given *a* [italics added] set of raw data to use" (p. 93) for reanalysis—*a* set, not *the* set of raw data that formed the basis of his disputed reports. From testimony at the Open

Hearing and from reports from the Inquiry Panel and Hearing Board, it is not clear Needleman gave the investigators the same raw data set that he used in his earlier analyses. In addition, reanalyses done by Needleman and by others were based on the same misrepresentations of subject selection and exclusion that tainted the original reports.

During the University of Pittsburgh's two investigations, Needleman did not reveal that his data were analyzed at least three times during data collection, that is, before the set of analyses we saw and before those provided to the Inquiry Panel. The Inquiry Panel was unaware of the existence of these earlier analyses. The Hearing Board discovered the second, but not the first and third, during its investigation. These three reports shed light on what was happening during data collection.

First, consider Needleman's statement that a decision to exclude certain classes of children was made a priori and that these criteria were applied through all analyses. This is not so; if these criteria had been set before the conduct of the study, it would have been apparent at the first analysis that considerable data to be excluded were being collected. The report of the first analysis (Needleman, 1977) indicated that 157 children had been tested; 129 children were described, and the data of 93 were analyzed. This first report was quite specific regarding children not brought in for testing (bilingual home, moved, not interested, etc.) but did not report the exclusion of any already tested children. Of the 129 children described, 18 were of birth weight less than 88 oz (low birth weight).

In the second analysis (Needleman, Gunnoe, Leviton, & Peresie, 1978), the data of 116 children were analyzed; children with low birth weight, history of lead intoxication, or head injury were excluded from analysis. Bilingual home was not mentioned.

In the third report (Needleman, 1990), the data of 130 children were analyzed. Fifty-eight additional children were excluded on the basis of diagnosis as lead poisoned, bilingual home, or history of central nervous system injury. Bilingual home was now an exclusion criterion for already tested children, low birth weight was not. In this third report, the children were said to be excluded prior to scoring but their scores were entered into the computer. This report also noted that 47 children who had been tested and excluded for discrepancies in dentine lead level would be reconsidered for inclusion in the study if later dentine samples were concordant with earlier values.

Needleman (1993, p. 99) argued that a computer code used to exclude cases was based on preestablished criteria and not human judgment. The early analyses, with changing criteria for the exclusion of cases, indicate that he was well aware of the variables, and possibly the individual cases, that might affect findings. The exclusion of the "lead poisoned" cases (M IQ = 100, SD = 16) is deceptive. One of these children had an IQ of 134, yet Needleman has frequently stated that no high-lead child in the study had an IQ greater than

125. The reports of the Inquiry Panel and Hearing Board (Cooley, 1992) documented continuing changes in the dentine lead criteria for inclusion in the study. The reports of early analyses indicate that the criteria for the exclusion of already tested children also changed during data collection.

The three analyses conducted during data collection were remarkable in other ways. In the third analysis (N = 130), the difference between the groups in reaction time was 0.0; in the published version (N = 158), the difference reached statistical significance. From the first to the second analysis the sample gained 21 low-lead, but only 2 high-lead children. From the second to the final sample, the increment was 17 low- and 25 high-lead cases. Testing of high-lead children late in the data collection period contributed to significant differences among groups in age, although Needleman categorically denied this bias in testing procedures at the Open Hearing.

More important, the selection of cases for testing may have otherwise been biased. Teacher ratings were available; the proportion of high-lead children rated hyperactive, and impulsive was significantly greater among the final 58 cases than the proportions of high-lead children so rated in the pool from which the sample was drawn.

Reports of the second and third early analyses indicated control for child's age; it was only in the analyses preceding publication that age was deleted. The effect of lead became statistically significant after this deletion. The ORI is aware of these new revelations.

LEAD INDUSTRY CONSPIRACY?

We state unequivocally that we are not employees or representatives of the lead industry or otherwise under any obligation to industry. Needleman's accusations amount to libel.

Needleman stated that within a year of the time that Ernhart (Ernhart, Landa, & Schell, 1981) wrote "If there are in fact, behavioral and intellectual sequelae of low levels of lead burden *independent of other aspects of parental and social influences on development,* these effects are minimal," (p. 918)[2] she became a grantee of the International Lead Zinc Research Organization (ILZRO). This is false. The article was in press in 1980. Ernhart's ILZRO grant began in 1983. In the intervening time her lead-effects research was supported by the March of Dimes and the Perinatal Clinical Research Center (at Cleveland Metropolitan General Hospital), which, in turn, was supported by a U.S. Public Health Service grant.

[2]The italicized segment, important to the conclusion drawn, was omitted by Needleman. The inference drawn was determined by careful review of the results. This inference is not inconsistent with later research in which significant effects were found sporadically. When such effects are obtained, after control of confounders, they are small.

Needleman writes as if there is something inherently suspect about receipt of an ILZRO grant. Given that such grants have no strings, it is entirely proper and beneficial to society for industry to contribute to the research enterprise. The money is not pocketed; as with other grants, it is extended to the grantee's institution to pay the expenses of doing research. A number of prominent researchers have received ILZRO support. Needleman often refers with admiration to his mentor, Randolf Byers, although Dr. Byers received research support from the Lead Industries Association (Byers, 1959; Byers & Maloof, 1954; Buyers, Maloof, & Cushman, 1954).

We have acted as expert witnesses in lawsuits involving purported lead poisoning. Defendants (primarily property owners, not industry) have the right to expert opinion. The fees we have charged are about 10% of what Needleman has been known to ask to testify for plaintiffs in the same kinds of cases. Lawsuits claiming lead poisoning have increased remarkably since the CDC (1991) redefined lead poisoning; Needleman played a major role on that committee.

Our attorney, David Geneson, is a member of a large, multi-city firm that represents more than 10,000 clients, so it is not surprising that Needleman finds some clients offensive. Actually, we contacted the firm because one of Scarr's daughters worked there.

LITIGATION TO PROTECT HIS DATA

Although Needleman (1993) may be technically correct in saying that he has not sued us before, it is not true that he "offered Scarr and Ernhart the opportunity to review my printouts in my lab" (p. 97). In fact, Needleman filed an affidavit in the U.S. District Court for the District of Utah to prevent our review of his data! The judge in the Superfund case ordered him to open his files to our scrutiny. This is a matter of public record.

Following the failed attempt to prevent us from seeing his data, the U.S. Department of Justice acted on his behalf (at taxpayer expense) to try to prevent us from divulging what we had seen in his laboratory. Presumably, the aim was to preserve his reputation as an expert witness in Superfund cases tried for the EPA.[3] We retained counsel to prepare and argue our case in order to preserve our right to freedom of speech. Fortunately, the judge ruled eloquently against Needleman, as quoted in our article (Ernhart, Scarr, & Geneson, 1993).

Our reluctance to participate in the misconduct hearing was due to the suit to have us silenced, to Needleman's lawsuit against NIH and the University of

[3]Nothing about this matter was provided in materials obtained from the EPA under the Freedom of Information Act.

Pittsburgh (*Needleman v. Healy,* 1992), and, most important, to the fact that NIH and the University had accepted responsibility for the investigation.

MORE PARTICULARS

Needleman (1993, p. 98) complained that we were "shielded from answering certain questions" at his hearing. If the chairman was remiss, it was because he allowed Needleman's insulting and irrelevant questions to continue. The University's Research Integrity Policy states: "Reasonable rules of relevancy shall guide the chairperson in ruling on the admissibility of evidence."

Needleman contends that we apply a different standard to his work than to our own because we criticized him for dropping age as a confounder, although we did not include age in analyses in some of our own studies. This is specious. Age was considered but was not a confounder in our studies; it was in his. It was clear from his inclusion of child's age during early data analyses that he recognized the problem but, with child's age included, did not get the desired results.

At the hearing and in his response to us, Needleman has produced many personal testimonials by trusting colleagues and friends. With one exception, colleagues who worked with him on the study continue to be silent.

Needleman (1993) continued to contribute confusion to investigations of his work. One item in his reply to our article is new: The notion that the Inquiry Panel said that subjects who should have been included in the analyses were actually vacant case numbers and that he reported this to the Dean of the School of Medicine. Needleman's response (personal communication to G. M. Bernier, December 30, 1991) to the Dean says nothing about empty cases, and there is no mention of this notion in any other documents, confidential or not, available to us. Why would anyone enter nonexistent cases into computer data files?

CONCLUSION

Deliberate misrepresentation continues. Needleman attacks the whistleblowers, the NIH, and his own University to deflect attention from the indications of misconduct in his research. Federal agencies base policies on his research and on his assertions regarding the purported effects of low-level lead exposure on children. The policies divert resources and attention from the real needs of children. Misconduct issues will ultimately be resolved by the ORI and the courts. Public policy issues may require reconsideration in light of further evidence.

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BOOK REVIEW

If I Were a Rich Man, Could I Buy a Pancreas? And Other Essays on the Ethi of Health Care. Arthur L. Caplan, Bloomington and Indianapolis: Indiar University Press, 1992, 348 pages + xvii, $29.95 (hardcover).

Reviewed by Michael J. Sal
University of Iov
College of La

For the busy researcher, practitioner, scholar, or educator, this first paragrap is an abstract of this book review. The book consists of 20 essays on a varie of topics: applied ethics; animal and human experimentation; reproductic and genetics; transplantation; aging; competency; chronic illness; rehabilit tion; and health care cost, allocation, and policymaking. The author is well-regarded medical ethicist. The book is easy and fun to read. (Yes, yc read that correctly.) It is also highly informative and thoughtful.

This book is a collection of essays by Arthur Caplan, most of which ha been published previously elsewhere. Caplan is a well-known medical ethici director of the Center for Biomedical Ethics at the University of Minnesot and a member of the faculties of the Departments of Philosophy and Surger

The essays range over considerable territory, the contours of which a reflected in the six sections of the book into which its 20 essays are organize The book's introduction (the 20th essay) is on the predicament of the philos phy of ethics during this century, which is as interesting as any of the othe The six sections of the book are as follows:

I. The Nature of Applied Ethics
II. Ethical Issues in Animal and Human Experimentation
III. Advances in Reproduction and Ethics
IV. Transplants and Other Unnatural Acts
V. Aging, Chronic Illness, and Rehabilitation
VI. Money, Medicine, and Morality

Considering the gravity of the topics and the complexity of the issu involved, the book is a remarkable pleasure to read. Caplan writes with a sty