Rush to Judgment: An Empirical Analysis of Environmental Equity in U.S. Environmental Protection Agency Enforcement Actions

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In 1992, the *National Law Journal* (*NLJ*) published a study claiming that the U.S. Environmental Protection Agency discriminated against minorities in its civil judicial enforcement actions by penalizing environmental law violators in minority areas far less than in white areas. *NLJ* claimed that this differential showed a lack of commitment to protecting the environment and public health in minority areas. This article augments, reexamines, and analyzes in a more sophisticated manner the available data. My empirical analyses demonstrate the unreliability of *NLJ*'s methods and conclusions and indicate that minorities have not been discriminated against in these enforcement actions.

Introduction

Over the past decade, concern has grown about the impact of pollution on particular population groups. Some people have concluded that minority and/or low-income people bear disproportionately adverse health and environmental effects from pollution (Austin & Schill 1991; Bullard 1994a). This conclusion led to the environmental equity movement, which seeks the equitable treatment of people of all races, incomes, and cultures in the development, implementation, and enforcement of environmental laws and policies.

The environmental equity movement emerged in the early 1980s (Hamilton 1995; Szasz & Meuser 1997). Subsequent studies and public attention raised concerns about environmental programs' fairness and protection, concerns that now receive increased attention at all levels of government, as well as in the private sector. The Clinton administration demonstrated its concern in an Executive Order requiring that federal agencies make

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environmental equity part of their missions (42 U.S.C. § 4321 [2000]). Similarly, the U.S. Environmental Protection Agency (EPA) lists environmental equity among its priorities. In 1992 EPA created the Office of Environmental Equity (later renamed the Office of Environmental Justice) and commissioned a task force to address environmental equity issues, and it now oversees the National Environmental Justice Advisory Council, a federal advisory committee composed of citizens with relevant experience who offer guidance to EPA (U.S. EPA 1995a).

Environmental equity research has been done on various topics—location and cleanup of contaminated sites (Hird 1993; Zimmerman 1993), siting of hazardous waste management facilities (Anderton et al. 1994; Boer et al. 1997), location of facilities using large amounts of toxic chemicals (Centner et al. 1996; Ringquist 1997), and distribution of air pollutants (Perlin et al. 1995; Brooks & Sethi 1997). One aspect of environmental equity that created particular controversy in recent years is alleged discrimination by EPA in its civil judicial enforcement actions—cases in which EPA seeks civil penalties in court against environmental law violators. In 1992 this allegation appeared in articles in the *National Law Journal* (hereinafter the *NLJ* articles), a weekly legal periodical (Lavelle & Coyle 1992). The *NLJ* articles described *NLJ*'s investigation into the environmental equity implications of various EPA actions.

NLJ compared the civil judicial penalties against environmental law violators in disproportionately white, minority, high-income, and low-income areas, respectively. Based on this comparison, *NLJ* concluded that minority areas were discriminated against because violations there had substantially lower penalties than in white areas. *NLJ* believes that when EPA imposes higher penalties in one area compared to others, it demonstrates EPA's greater commitment to protecting the environment and people's health in that area.

NLJ's theory is consistent with much of the reasoning behind environmental equity concerns. The basic assumption is that because of intentional discrimination or a lack of political power to effectively protest nearby environmental risks, those risks become disproportionately concentrated in minority and low-income areas. Presumably, those in power—in this situation, government enforcement staff and judges—believe that minority and low-income people are less deserving of protection from the consequences of environmental violations. As a result, those who endanger them receive less-severe punishment.

In the criminal law context, numerous studies concluded that prosecutors are less likely to seek severe punishment of criminal defendants if minorities are the victims, and that judges and juries are less likely to impose such punishment in these situations (Paternoster 1984; Ekland-Olson 1988; Sorenson & Wallace

1995; but see Klein & Rolph 1991). Consequently, *NLJ*'s approach to this issue simply extended these environmental equity and criminal justice theories to the EPA civil enforcement context.

In the several years after their publication, the *NLJ* articles' data and conclusions have been cited approvingly in dozens of articles and books concerning environmental equity (Been 1993:1009, n. 39; World Resources Institute 1993:185–86, 190; Bullard 1994b:452; Head 1995:52; Mank 1995:338). "The *National Law Journal* report includes perhaps the most comprehensive empirical investigation to date regarding the existence of a disparity, based on race or income, of EPA's allocation of enforcement resources and leverage" (Lazarus 1993:818, n.125). The *NLJ* articles prompted EPA to convene an internal committee to examine for itself whether race-based disparities in penalties existed and to adopt enforcement policies promoting environmental equity (U.S. EPA 1993b). A more recent article, however, reanalyzed some of the same data and concluded that there was no discrimination against minorities (Ringquist 1998).

My purpose in this article is to augment and analyze in a more sophisticated manner the available data to assess what factors, including the race and class of the people in areas around environmental law violations, influence EPA civil judicial penalties. Because *NLJ* published its articles itself, and thus bypassed the peer review process for most published studies, and because the articles' findings have been cited so frequently with so little critical analysis, in this article I evaluate *NLJ*'s study in detail. In addition, I closely examine the Ringquist article. The hope is that better understanding the concerns of both studies will end the confusion and stalemate created by the disparate conclusions of the different studies and also will provide guidance for future research in this area.

As this article will demonstrate, serious substantive and methodological problems undermine the usefulness of prior studies in this area. When more accurate data are used in more sophisticated analyses, there is no indication of discrimination against minorities or low-income people. Instead, the more important determinants of penalties are the facts of the cases, as it should be.

Literature Review

NLJ Articles

In September 1992, the issue of environmental equity began receiving greater attention because of the *NLJ* articles, printed in a special 12-page section entitled "Unequal Protection: The Racial Divide in Environmental Law." The *NLJ* articles provided the

results of an eight-month-long special investigation by *NLJ* staff. *NLJ* analyzed the environmental equity implications of various EPA activities, including its civil judicial enforcement actions, and concluded that "[t]here is a racial divide in the way the U.S. government . . . punishes polluters. White communities see . . . stiffer penalties than communities where blacks, Hispanics and other minorities live. This unequal protection often occurs whether the community is wealthy or poor" (Lavelle & Coyle 1992:S1).

NLJ's methodology was fairly straightforward. It used EPA's Civil Enforcement DOCKET database (hereinafter DOCKET), which is EPA's system for tracking civil judicial cases filed on EPA's behalf by the U.S. Department of Justice (DOJ). *NLJ* extracted from DOCKET the name and address of each facility involved in cases that ended between 1985 and March 1991, the penalties imposed, and the environmental statutes that were the basis for the cases. Of 1,214 cases in this time period, *NLJ* deleted those in Puerto Rico or the Virgin Islands, those in which no penalty was imposed, those in which no specific facility was cited as the source of the violation, and those where—based upon *NLJ*'s method for collecting demographic data—no people resided near the facility.

To measure the race and income characteristics of the people around these facilities, *NLJ* used 1989 U.S. Census Bureau estimated data for the zip codes in which these facilities were located. After determining the median household income and the percentage of the population that was white for each zip code containing a facility, these zip codes were ranked by these two variables, respectively. *NLJ* then designated the zip codes that ranked in the highest and lowest 25% of all zip codes with respect to white population percentage as the "white" and "minority" areas, respectively. Zip codes ranked in the highest and lowest 25% of all zip codes with respect to median household income were designated as the "high-" and "low-" income areas, respectively.

After comparing the penalties against facilities in these white, minority, high-income, and low-income areas, *NLJ* concluded the following:

- The mean penalties in white and minority areas for all types of cases were $153,607 and $105,028, respectively. The mean penalties in high- and low-income areas for all types of cases were $146,993 and $95,564, respectively (all figures in U.S. dollars).
- The mean penalties in white areas were higher than in minority areas for cases involving violations of the 1976 Resource Conservation and Recovery Act (RCRA), the statute governing hazardous waste; the 1970 Clean Air Act (CAA), the statute governing air pollution; the 1974 Safe Drinking Water Act

(SDWA), the statute governing public drinking water supplies; the 1972 Clean Water Act (CWA), the statute governing water pollution; and more than one environmental statute (hereinafter "multistatute" cases). The mean penalties in white areas were less than in minority areas for cases involving the 1980 Comprehensive Environmental Response, Compensation, and Liability Act (CERCLA), the statute governing cleanups of some contaminated sites.

- The mean penalties in high-income areas were more than those in low-income areas for the CWA and multistatute cases, but less than those in low-income areas for the RCRA, CERCLA, CAA, and SDWA cases.

In *NLJ*'s interpretation of these findings, it assumed that when EPA imposes higher penalties on facilities in one area as compared to others it demonstrates EPA's greater commitment to protecting the environment and people's health in that area. Consequently, because almost all of its results indicated that facilities in white areas were penalized more severely than facilities in minority areas, *NLJ* concluded that the latter areas bore the burden of environmental discrimination. Because the results were mixed with respect to high- and low-income areas, *NLJ* concluded that the income level of an area did not determine how much it was protected from environmental harm.

Evaluation of *NLJ* Articles

Although the *NLJ* articles have been cited frequently and there have been a few brief critical comments concerning them (Bryant 1993), no thorough review of the *National Law Journal*'s methods and results has been published. To some extent, this is because *NLJ* has declined to cooperate in efforts to replicate its findings.[1] Thus, without access to *NLJ*'s data or analyses, one can only evaluate the study by using *NLJ*'s description of its methods and findings, as well as information from DOCKET. My examination demonstrates that the *NLJ* study is unreliable for various reasons.

First, as noted earlier, *NLJ* claimed that it had identified 1,214 "cases" in DOCKET. A tabulation of DOCKET, however, demonstrates that this figure includes hundreds more cases than are actually there. *NLJ* apparently did not recognize that DOCKET contains many multilocation cases—individual cases in which violations occurred at more than one location—such as asbestos abatement contractors acting illegally at numerous buildings. EPA typically created a separate duplicative record in

[1] "The authors requested the data set upon which these findings were based but were told that such data would not be released because the findings were 'too controversial'" (Boerner & Lambert 1995:65, note).

DOCKET for each violation location. Furthermore, for each location, EPA recorded in DOCKET the *total* penalty for *all* violations in the case as the penalty for the violation at *each* location. Thus, for example, a case involving violations at 30 locations in which a $100,000 penalty was imposed was represented in DOCKET by 30 separate records, each of which indicated a $100,000 penalty. Approximately 25% of all DOCKET records are part of multilocation cases. Therefore, if multilocation cases are not properly handled, they are a potentially important source of error in analyses. Exactly how each case's total penalty should be apportioned across the violation locations is uncertain, as is how *NLJ*'s inclusion of them in its analyses affected its results. All that is certain is that *NLJ*'s analyses contained probably hundreds of observations that had erroneously duplicative penalty amounts.

Second, it has been stated elsewhere that *NLJ*'s use of quartiles to separate its cases into white and minority areas resulted in a minority area quartile that was not necessarily predominantly minority (Bryant 1993; Kuehn 1994). As *NLJ* itself noted, its minority area quartile included areas that were as little as 20.8% minority (Lavelle & Coyle 1992:S4). Thus, the penalties against defendants in those areas do not necessarily reflect what happens to defendants in predominantly minority areas, making any comparison to white areas suspect. What has gone unnoticed, however, is how *NLJ* apparently defined "minority." Even though *NLJ* never stated it explicitly, it is clear that it categorized Hispanic-origin whites as whites, rather than as minorities.[2] Most other environmental equity studies have defined them as minorities.

Whether one classifies Hispanics as minorities or whites can make a substantial difference in the minority composition of an area. For example, 24.7% of Texans are minorities if one excludes Hispanic-origin whites, but 39.2% are minorities if they are included. In California the percentages are 30.8% and 42.6%, respectively, and in New Mexico 24.2% and 49.5%, respectively. These differences should be even greater in smaller areas, where racial and ethnic groups tend to concentrate, such as in the zip codes that *NLJ* used. Therefore, a zip code that did not fall in *NLJ*'s minority area quartile—as *NLJ* had defined minority—could be a minority area by other people's definition. Most important is whether *NLJ*'s definition of minorities reflects what judges and enforcement staff thought, but this is unknown,

[2] *NLJ* noted that 83.1% of the U.S. population is white, according to 1989 Census estimates (Lavelle & Coyle 1992:S4). Although this figure is close to the final 1990 Census figure for whites (80.3%), it is far above the 75.8% that is classified as *non-Hispanic* white. In addition, *NLJ* noted that people in areas with poor air quality were "78.7 percent white, 14.2 percent black and 8.2 percent Hispanic" (Lavelle & Coyle 1992:S6). These numbers equal 101.1%, without even including people of other races. Presumably, these numbers double-counted Hispanic-origin whites, once as whites and again as Hispanics. Thus, *NLJ* clearly did not separate Hispanic-origin whites from other whites and include them as minorities.

and thus one cannot conclude that *NLJ* was incorrect. Its definition, however, is certainly questionable and inconsistent with other research.

Third, for several reasons, the monetary penalties recorded in DOCKET do not necessarily reflect the severity of the punishment imposed, which is key to *NLJ*'s theory of discrimination. EPA's policy since 1984 has been to recover any economic benefit of noncompliance (U.S. EPA 1984). Thus, in addition to penalizing defendants for violating the law, the penalty also should include an amount equal to what they saved from not complying, such as by not obtaining a permit, not installing required pollution control equipment, or not filing a required report. Consequently, in comparing penalties of $50,000 and $100,000 against two defendants, for example, one might assume that the latter defendant was punished more severely. In reality, the first defendant may have gained no economic benefit from noncompliance, while the second defendant may have saved $80,000 from noncompliance, with that amount being included in its penalty. Thus, the true punishments imposed on the defendants were $50,000 and $20,000, respectively. Therefore, without knowing how much, if any, economic benefit recovery was included in a penalty, one cannot judge its true severity, and the amount of that recovery is almost always not revealed.

Also, defendants can negotiate with the U.S. EPA (1998b) to reduce their penalties in exchange for conducting Supplemental Environmental Projects (SEPs). Activities that benefit the environment, but that the defendants are not otherwise legally required to implement, such as pollution prevention, recycling, or environmental audits, are considered to be SEPs. Ordinarily, the penalty can be reduced by no more than 80% of the cost of the SEP and any economic benefit of noncompliance plus some additional penalty must still be recovered. These SEPs are not included in the DOCKET penalty amounts. Thus, a relatively low penalty could be obscuring the fact that a defendant spent a substantial amount on a SEP to reduce its penalty to that level (for example, in fiscal year 1998, the average SEP cost $411,000, far more than the average civil judicial penalty [U.S. EPA 1999]).

In addition, the penalty imposed by EPA can be based on a defendant's ability to pay (U.S. EPA 1986b). If, for example, the penalty that EPA would normally impose in a situation would bankrupt the defendant, EPA can lower the penalty. Thus, a relatively low penalty actually could be a severe penalty because that may be all the defendant could afford without going bankrupt. Therefore, in cases where any of these EPA policies were applied, the reported penalties would not necessarily reflect the actual costs to the defendants of resolving their cases or the severity of their offenses. Also, the true severity of penalties is measured not by their absolute amounts but instead by their amounts relative

to the maximum penalties that could have been imposed. Realistically, there is no way to determine the maximum possible penalty, because it is a function of the maximum statutory penalty for the violation, the specific facts and law of the case, and their probable judicial interpretation if the case were to go to trial. For all of these reasons, using the penalty amount as the measure of EPA enforcement stringency is unreliable.

Fourth, *NLJ* used zip codes to define the relevant population around each violation location. As noted elsewhere, the zip code around a facility does not necessarily describe the people that are relevant for environmental equity purposes (Zimmerman 1994; Fahsbender 1996). Zip code boundaries are designed for the convenience of the U.S. Postal Service, not for demographic analyses. Consequently, a zip code should not be expected to represent a neighborhood in the way that most people think of it. Naturally, this does not necessarily mean that the demographic characteristics that *NLJ* used were unrepresentative of the residents of the area around a violation location that judges and enforcement staff hypothetically considered. It is possible that an applicable zip code's characteristics were very close to those of the area in judges' and enforcement staff's minds. Thus, though the area that *NLJ* used may be inappropriate, it may have no actual effect on the results. Because the area that judges and enforcement staff hypothetically considered is unknown, there is no way to validate or invalidate *NLJ*'s choice.

Finally, and most importantly, even assuming that the penalty against a defendant reflects the severity of the punishment and thus the extent to which the nearby population is protected and that the demographics of the zip code containing the violation location accurately represent the nearby neighborhood, an analysis such as *NLJ*'s has little reliability. *NLJ*'s analysis consisted completely of comparing the mean penalties for the highest and lowest quartiles for the race and income variables, respectively. Although this method could be helpful when used with other analyses, it is of little value alone, particularly when no other variables are controlled for in these analyses. The most serious problem in *NLJ*'s methodology was its failure to control for the effects on penalties of variables other than an area's racial and income characteristics.

For example, white area defendants may be more likely to commit environmental violations that traditionally elicit higher penalties; cases in minority areas may be disproportionately concentrated in earlier years, when penalties were lower due to less-aggressive enforcement efforts; or high-income area defendants may be more likely to have the financial resources to litigate and cases concluded through litigation, rather than settlement, could result in higher penalties. Essentially, *NLJ*'s approach was, first, to demonstrate that a disparity existed in penalties when penal-

ties were analyzed solely from the perspective of one variable; then, to assume that only that variable caused the disparity; and, finally, to provide the most unsettling explanation for that variable's effect. For example, another possible explanation for lower penalties in minority areas is that, instead of EPA discriminating against people living nearby, more defendants in those areas were minority-run facilities, and EPA thus penalized them less because of EPA's lenient treatment of them. Consequently, for all of these reasons the *NLJ* study should never have been considered reliable.

Ringquist Article

A more recent article attempted to reanalyze DOCKET data in a more sophisticated manner and to test other theories of the determinants of penalties (Ringquist 1998).[3] Rather than analyzing only the 1985 to 1991 period in the *NLJ* analyses, Ringquist also analyzed DOCKET cases from 1974 to 1991.[4] Ringquist used the zip codes of the facilities recorded in DOCKET to establish their locations. After some preliminary analyses, he described regression analyses that used numerous variables to operationalize various hypotheses.

To test the environmental equity hypothesis for how penalties are set, Ringquist used the minority percentage and per capita income of the zip code that a facility was in. He also tested various hypotheses about the characteristics of the cases. To control for the severity of the violation, Ringquist (1998:1157) included "the total number of counts brought against the defendant in each case," which he stated was in DOCKET. Ringquist also hypothesized that larger companies would be penalized more because their economic benefits from violations—which should be recovered in their penalties—would be larger. Thus, he included a dummy variable indicating whether the defendant was a *Fortune* 500 company. He also included a dummy variable indicating whether the defendant was a government entity, hypothesizing that they were more successful in litigation. Ringquist (1998:1158) also included "a variable representing the

[3] Ringquist coauthored a subsequent article (Ringquist & Emmert 1999) using almost all of the same cases but somewhat different independent variables to explain differences in penalties. The hypotheses he tested in both articles were largely identical, but because race and income variables were not included in the later article's analyses, I focus only on the earlier article here.

[4] Ringquist noted that *NLJ*'s use of the 1985 to 1991 period was "a curious one," because DOCKET cases were available from the mid-1970s. Though *NLJ* did not state it explicitly, it likely began with 1985 data because in 1984 EPA's formal policy began to require that penalties recover any economic benefit of noncompliance, in addition to punishing violators. All else held constant, this policy should thereafter have increased penalties. Consequently, by excluding cases prior to 1985, this policy change did not affect *NLJ*'s analyses. Ringquist's analyses of all DOCKET cases did not control for this policy change.

number of times a particular defendant has been penalized for regulatory violations in a particular district," on the assumption that more prior violations would lead to more severe penalties. Hypothesizing that penalties were affected by whether the decision in the case was published, he added another dummy variable indicating this factor (operationalized as appearing in the *Federal Supplement*, the official source of Federal District Court judicial opinions, or the *Environmental Law Reporter*, a monthly periodical and reference service of environmental law articles and information). Finally, Ringquist recognized what *NLJ* had not—the presence of multilocation cases. He incorporated a dummy variable showing whether the case involved violations at multiple locations, hypothesizing that such cases might result in higher penalties.

Ringquist also believed that judicial characteristics and behavior could affect penalties. To operationalize these hypotheses, he used dummy variables indicating the president who appointed the judge who presided over a case, because judges' characteristics reflected the presidents who appointed them. He obtained this information from surveys that he sent to Federal District Courts in which the cases were filed. Ringquist also proposed that support for environmental protection in a state might influence penalties, either because judges responded to external political pressure or because they embodied the prevailing attitudes of political elites in their areas. He reflected this support for environmental protection through variables measuring the pro-environment voting record of members of Congress from the state. Ringquist also hypothesized that the political environment might affect penalties in another way, suggesting that penalties might be lower in states with politically influential polluting industries or that penalties might be higher in states where industries posed a greater pollution threat. He operationalized industry's political power/pollution threat for each case through a variable reflecting the economic contribution to the state by those industries that he identified as most responsible for pollution in the state where the violation occurred.

Finally, Ringquist proposed that institutional factors might influence penalties. He first hypothesized that who was president when a case ended might affect penalties, and thus he added dummy variables showing the tenure of each Republican president during the period in question. Second, he hypothesized that penalties would be higher in cases in which the prosecution was directed by an attorney from DOJ's Washington headquarters, as opposed to a DOJ attorney from the U.S. Attorney's Office where the case arose. He assumed that DOJ headquarters attorneys were more sympathetic to environmental concerns because they were specialists in that area and worked in an organization dedicated to that mission. Thus, he included a dummy va-

riable indicating whether the case was directed by a DOJ headquarters attorney.

Ringquist used two dependent variables, the total penalty in a case and the average penalty per violation in a case. For the latter, he divided the total penalty in the case by the number of violations. Thus, Ringquist created four regression models, one using each of these two dependent variables in each of the two periods in question. All of the models explained modest, but statistically significant, amounts of the variation in penalties. Almost all of the variables representing characteristics of the cases were statistically significant in all of the models, most with coefficients in the expected direction. Virtually none of the variables representing characteristics of the judges were statistically significant in any models. Most of the variables representing the political environment surrounding judges were statistically significant, almost all with coefficients in the expected direction. Most of the variables representing institutional factors were statistically significant. Finally, the percentage of minorities in an area was statistically significant only in the average penalty models, but the relationship was positive—the more minorities in an area, the higher were the penalties, contradicting the *NLJ* findings. The per capita income of an area was statistically significant in only one model. Thus, with respect to environmental equity issues, Ringquist found no support for discrimination against minorities in these EPA enforcement actions.

Evaluation of Ringquist Article

Substantive Issues.

Compared to *NLJ*'s study, Ringquist's research began in the right direction, particularly his inclusion of variables other than race and income that might affect penalties. Despite this promising start, however, some apparent misunderstandings of the substantive aspects of EPA's civil judicial enforcement process and of what DOCKET data represented, as well as some of his methodology, render his research unreliable. First, and most severe from a theoretical and methodological basis, was his assumption that judicial characteristics affect penalties. Ringquist correctly stated that approximately 90% of EPA civil judicial cases are settled by EPA and the defendants, rather than litigated to a conclusion before judges. According to DOCKET data, 91.8% of all cases were settled, and 2.4% ended in default judgments against the defendant (i.e., the defendant did not contest the action and thus EPA obtained the penalties that it sought, rather than what

the judge decided).[5] Though he never stated it explicitly, Ringquist obviously assumed that judges still exercise substantial control over penalties in settled cases, and thus judges' characteristics are relevant.

Despite the clear importance of this assumption for his research, no support was actually provided for it.[6] The claim that judges play a meaningful, much less deciding, role in settled EPA cases is entirely inconsistent with my personal experience and knowledge. Among the environmental attorneys with whom I dealt—as an environmental attorney since 1984 who assisted both EPA and private parties in enforcement actions—it was common knowledge that it would be extraordinary if judges played any role in settled EPA cases. Ringquist did not cite, nor could I find, any documentation for his claim. Perhaps the most extensive discussion of EPA enforcement efforts over the past 30 years (written by a former EPA attorney) hardly mentions judges at all, much less attributes to them any significant role (Mintz 1995).

The only implicit substantiation offered by Ringquist for his notion of judges' roles in EPA civil judicial cases was citing Kritzer (1993) that "settlement negotiations involve numerous opportunities for judicial involvement" (Ringquist & Emmert 1999:15). However, Kritzer is neither relevant to nor supportive of this statement. Kritzer's study was based on a sample of approximately 1,500 civil cases settled in 1978, drawn from five Federal District Courts and from state courts in those jurisdictions. Even if all of the federal cases that he sampled included all of the EPA civil judicial cases concluded in those five District Courts in 1978, those cases would compose only 7 of the 1,500 cases. Because Kritzer sampled only a small percentage of all District Court cases, however, it is almost certain that no EPA cases were in his analyses.

Therefore, Kritzer's research is relevant only if we believe that his findings can be generalized to EPA cases. This is implausible and, based on Kritzer's repeated statements, even he would undoubtedly agree. Kritzer subtitled his book "Understanding the Negotiation Process in Ordinary Litigation," and he repeatedly emphasized that his research only concerned "ordinary," "day-to-day," and "everyday" cases. Nowhere does Kritzer even mention environmental enforcement actions. The overwhelming majority were simple tort or contract actions, and only a small

[5] These and other numbers in this section are based on the corrected and augmented version of DOCKET that I prepared during this research, which is described later.

[6] Ringquist incorrectly claimed that *NLJ* also concluded that judges' decisions contributed to inequities in penalties. In fact, *NLJ* stated in its articles that judges played little role in imposing penalties—one of its articles' headlines was "Negotiations Are Key to Most Fines" (Lavelle & Coyle 1992:S6).

percentage involved government actions. The notion that such cases are similar to EPA civil judicial cases is untenable.

Even if we ignore this inconsistency, Kritzer's research not only does not support the claim that "settlement negotiations involve numerous opportunities for judicial involvement," it demonstrates the opposite. Kritzer (1993:37) found that 75% of the surveyed attorneys had two or fewer exchanges of settlement offers with their opposing attorneys, and 73% spent no more than a handful of hours on settlement negotiations (32). Such a brief process obviously would not leave many opportunities for judicial involvement.[7] In fact, Kritzer hardly even mentions judges in his book, only stating in his next-to-last footnote that the role of judges in settlement negotiations was discussed by other authors. Therefore, nothing in Kritzer supports the claim that judges are involved in settlement negotiations in EPA cases.

In addition to claiming that judges were heavily involved in EPA settled cases, Ringquist claimed that they actually might have more discretion in setting penalties in those cases than in cases that go to trial (Ringquist & Emmert 1999:15). This idea is incorrect. In cases that are litigated to a conclusion, judges can impose penalties up to the statutory maximum per violation. However, most settled cases are ended by a consent decree between the government and the defendants.[8] A consent decree is somewhat of a contract establishing their rights and responsibilities that must be approved by the judge overseeing the lawsuit (this is why Ringquist found that presiding judges existed even for settled cases). In many cases, the government will officially file the lawsuit against the defendant at the same time that it submits to the court the proposed consent decree that they had already negotiated. In settled cases the law is unequivocal that a court "does not have the power to modify a consent decree; it may only approve or reject it" (*U.S. v. Cannons Engineering Corp.* [1989]).[9] Thus, it is legally impossible for judges to replace settling parties' negotiated penalties with their own preferred penalties.

The only discretion that judges have is whether to approve the consent decree, and even that discretion is limited.

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[7] In fact, Kritzer only specifically discussed government cases when he noted that "relatively little time is spent on negotiations when one of the litigants is a government actor" (1993:33). One of his explanations for this finding is consistent with the EPA civil judicial case scenario—"cases involving the government are not appropriate for negotiation or are past the negotiation stage by the time a lawsuit is filed" (Kritzer 1993:151, n.6).

[8] "The settlement of a potential civil judicial action should almost always result in a negotiated consent decree" (Price 1983a). It also has been DOJ's policy since 1973 that any settlement to enjoin discharges of pollutants into the environment be embodied in a judicially approved consent decree (28 C.F.R. § 50.7 [2000]).

[9] See also *U.S. v. Telluride Co.* (1994); *U.S. v. Shaffer Equip. Co.* (1994); *U.S. v. Crown Enameling, Inc.* (1987); *U.S. v. Jones & Laughlin Steel Corp.* (1986); *Walsh v. A&P* (1983); *Officers for Justice v. Civil Service Com.* (1982).

> Because a consent judgment represents parties' determination to resolve a dispute without litigating the merits, the court's role is . . . only to determine whether the settlement negotiated by the parties is in fact a fair, reasonable and adequate resolution of the disputed claims. . . . In determining whether a consent judgment is fair, reasonable and adequate, the court must not rubberstamp the agreement, but also must not substitute its own judgment for that of the parties to the decree. (*U.S. v. County of Muskegon* [1998], citations omitted).[10]

However, judges must give deference to the decision by DOJ and EPA to settle a case.[11] Furthermore, if judges decide to reject a consent decree, they have the burden of explaining why it is unfair, unreasonable, or inadequate. Finally, judges' exercise of such discretion can be appealed and overturned, just as other trial court decisions.

Therefore, the only way that judges could impose their preferences on penalties is by continually rejecting proposed consent decrees until they contained the penalties that the judges desired (assuming that their rejections were not overturned on appeal). Thus, a critical question is how often judges reject proposed consent decrees. Before a judge decides whether to approve a proposed consent decree, a notice of its existence must be published in the *Federal Register* so that the public may submit comments on it (28 C.F.R. § 50.7 [2000]). The notice briefly describes the defendants, the alleged violations, and the terms of the proposed settlement, typically including the penalty.

I compared the penalty proposed in the *Federal Register* notices for 544 of the settled cases that I analyzed as part of this research against the final penalty recorded in the case. In only eight cases was there a difference between the proposed and final penalties. In six of these cases the differences appear to be due simply to a typographical error in the *Federal Register* notice. Thus, it is an indisputable fact that judges rarely, if ever, reject consent decrees because they disagree with the penalties imposed. Therefore, Ringquist correctly stated that judicial decisions in settled cases are rarely appealed (Ringquist & Emmert 1999:15), but it is because judges virtually always approve proposed settlements.

Consequently, the only way that judges could affect penalties is by influencing the settling parties before they submit a proposed consent decree. Neither Ringquist nor any other source provides any support for this scenario. Obviously, the only leverage that judges have is threatening to reject any consent decree that does not contain the penalties that they want. Of course,

[10] See also *U.S. v. Telluride Co.* (1994); *U.S. v. Monterey Inv.* (1990); *U.S. v. Tyson* (1988); *U.S. v. Conservation Chemical Co.* (1988).

[11] *U.S. v. City of New York* (1998); *U.S. v. Shaffer Equip. Co.* (1994); *U.S. v. Monterey Inv.* (1990); *U.S. v. Tyson* (1988).

because the legal burden is on judges to explain why they oppose a proposed consent decree and their decisions can be overturned, such a threat is limited. Also, if judges do act in such a manner, apparently they are extraordinarily successful at imposing their will on the settling parties because—as previously described—they rarely have to reject proposed consent decrees. It also is difficult to imagine that judges involve themselves in cases where the penalties are typically so small. As is described later, most penalties in EPA civil judicial cases are $40,000 or less, and only a small percentage even exceed $100,000. Such small stakes hardly seem worth a busy judge's attention.

Judges also might affect penalties without being directly involved, through their reputations. If certain judges had reputations for severely or lightly penalizing environmental law violators in cases that went to trial, this factor might influence the penalties that the settling parties agree upon and propose in their consent decrees. The problem with this scenario is the absence of any past record that the settling parties could examine. From 1985 to March 1991, there were only 53 EPA civil judicial cases decided by the 860 district court judges active during that period.[12] Thus, only a small percentage of judges tried even a single EPA civil judicial case during this time. Even before 1985, judges tried about 100 or so such cases. Once again, therefore, only a small percentage of the judges active from 1985 to March 1991 could have previously tried even a single case. Consequently, only rarely would there be an applicable judicial track record that could affect settling parties' decisions.

Therefore, there is no basis for claiming that settled EPA civil judicial cases are decided by judges. The facts demonstrate quite the opposite. Such a claim simply represents a basic misunderstanding of the law, the enforcement process, and the data. The penalties in consent decrees are not judicial decisions, and any relationship between judicial characteristics and penalties can be assumed to be spurious.

Although there is a substantial body of research examining the impact of judges' characteristics on judicial decisions in legal proceedings, that research obviously is inapplicable when judges are not the ones making the decisions. Only a few percent of all civil lawsuits are actually litigated to a conclusion and decided by judges. Under the legal rules of civil procedure, when the plaintiff and defendant agree to a settlement, they can simply have the case dismissed. They are not even required to inform the judge in the case of their settlement terms, much less does the judge have any power to change them. Thus, prior research on judicial

[12] The number of active judges was obtained from the Federal Judicial Center Internet site, http://air.fjc.gov/history/judges_frm.html.

decisions obviously is limited to that very small portion of cases that judges actually decide.

Settled EPA civil judicial cases would be handled in the same manner as other civil cases, if it were not for special policies applied to them. Without these policies, settled cases could be simply dismissed by the agreement of EPA and the defendants, and judges would not necessarily even know, much less decide, the penalties imposed. As previously described, however, unlike that for almost any other type of civil action, government policy ordinarily requires that environmental enforcement settlements be embodied in consent decrees that are subject to public comment and judicial approval.[13] It is only because of this unusual government policy that judges have any role in environmental enforcement settlements and, as described earlier, that role is severely circumscribed. Consequently, prior research examining the impact of judges' characteristics on the small portion of typical civil lawsuit decisions made by judges is inapplicable to the EPA enforcement settlement scenario. That research could be relevant to EPA civil judicial cases that are litigated to a conclusion, but these compose less than 200 cases over 25 years.

The lack of a judicial role in setting penalties also invalidates the theory behind Ringquist's distinction between defendants who were government entities and those who were not. The fact that government entities may be more successful in litigation is irrelevant if cases are settled. In any event, it should be noted that the basis for this hypothesis was the advantage that government entities had in U.S. Supreme Court cases, hardly a situation similar to being defendants in EPA enforcement actions.

The positive relationship that Ringquist found between penalties and whether a case was "published" was either spurious or misinterpreted. No more than a few dozen cases that Ringquist analyzed—the ones litigated to a conclusion—could have had published judicial opinions. Thus, the use of the *Federal Supplement* and the *Environmental Law Reporter* to determine whether a case was published actually measured whether a case ended through litigation rather than settlement. As I describe later, there are good reasons to hypothesize that penalties would differ between litigated and settled cases. These may explain why Ringquist found "published" cases to be a statistically significant determinant of penalties. It also should be noted that most settled cases are "published," in a sense. As previously mentioned, set-

[13] Even absent this requirement, there would be some situations in which the government would prefer that a settlement be contained in a consent decree. If the settlement terms include, as is often the case with environmental violations, some future actions by the defendants (e.g., installation of pollution control equipment by specified deadlines), the government could immediately seek judicial enforcement, including contempt of court fines, against defendants who did not complete those actions. If the agreement was not in a judicially supervised consent decree, the government would have to file a new lawsuit to enforce the terms of the settlement agreement.

tled cases ended by a consent decree between EPA and the defendants have notices concerning them published in the *Federal Register* so that the public can submit comments on them. Full-text copies of consent decrees are available from DOJ and the LEXIS on-line legal research service. Thus, there may be more opportunity for the public to become aware of the terms of settled cases than of judicial decisions.

Another apparent misunderstanding led Ringquist to include another suspect variable, that of prosecutions led by DOJ headquarters attorneys. This is a distinction without a likely difference. It is important to understand that almost all environmental cases are referred by EPA to DOJ, because only DOJ ordinarily is authorized to officially represent the government in court in such cases.[14] Ringquist noted that "the DOJ or U.S. attorney and EPA attorneys work as a team, with the EPA attorney providing technical support, policy advice, and assistance with regulatory procedure" (Ringquist & Emmert 1999:12). This characterization of EPA's role is erroneous. The memorandum of understanding between DOJ and EPA that has governed their conduct during litigation since 1977 states

> that participation by [EPA] attorneys under this memorandum includes appearances in Court, participation in trials and oral arguments, participation in the preparation of briefs, memoranda and pleadings, participation in discussions with opposing counsel, including settlement negotiations, and all other aspects of case preparation normally associated with the responsibilities of an attorney in the conduct of litigation. (Bell et al. 1977:3)

Consequently, EPA is far from simply being DOJ's assistant. Most important, it is EPA's penalty policies, not any from DOJ, that are used to calculate the government's claim for penalties.[15] Also, EPA's administrator must agree to any settlement (Bell et al. 1977). There are numerous governmental guidance policies that document not only EPA's active participation in litigated cases but also the expectation that it—not DOJ—is primarily responsible for determining the appropriate penalties and negotiating with defendants (Strock 1990; Adams 1988; U.S. EPA 1984; Jacobs 1983; Price 1983b; Perry 1982a, 1982b). Therefore, the assumption that the government's approach to a case critically depends on whether a DOJ headquarters attorney is involved is

[14] Under the CAA, CWA, and SDWA, EPA can prosecute a case itself if DOJ refuses to do so (33 U.S.C. § 1366 [2000] and 42 U.S.C. §§7605[a] and 300j-9[f] [2000]).

[15] EPA's initial negotiating position is supposed to be the penalty determined from its internal penalty policy applicable to the environmental law violated (U.S. EPA 1986a, 1987, 1990a, 1990b, 1990c, 1990d). These policies typically provide a matrix displaying the appropriate range for a penalty depending upon the seriousness of the violation and its importance to the regulatory program in question. The policies then give EPA staff the discretion to modify these penalties within specified ranges due to certain factors, such as the defendant's willingness to cooperate, good faith efforts to comply, and prior record of noncompliance.

unfounded. EPA does not necessarily need the involvement of a DOJ attorney specializing in environmental cases to advocate stiffer penalties. If specialists in environmental law were presumed to perform this function, then there would be no distinction among almost all cases because EPA attorneys are almost always involved. Thus, there is no substantiation for distinguishing cases on this basis. Furthermore, the positive relationship that Ringquist found between penalties and DOJ headquarters-led cases may simply reflect the types of cases that are referred to those prosecutors (e.g., cases of national significance or violations in multiple states), rather than the impact of those prosecutors on cases.

Methodological Issues.

Other concerns with Ringquist's research revolve around its methodology. First, he apparently used the number of prior EPA civil judicial cases in which a defendant was involved to represent how many times that defendant had been penalized for violations, and he used this number to show the extent of that party's undesirable past record that might affect penalty decisions. These several hundred prior judicial cases, however, include neither EPA administrative cases nor criminal actions. During the period covered by *NLJ*'s analysis, EPA referred more than 300 criminal cases to DOJ for prosecution and initiated more than 20,000 administrative actions itself (U.S. EPA 1993a). Also, DOCKET does not include the over 67,000 environmental enforcement actions initiated by states during this time (U.S. EPA 1993a). The number of prior violations measured by Ringquist, then, falls far short of the actual number of prior alleged violations that might be taken into account by EPA. According to DOCKET data, less than 15% of defendants had a prior civil judicial action initiated against them, and most of these had only one such prior action. Therefore, aside from being operationalized incorrectly, this variable had an extremely limited range.

Second, Ringquist used the "number of counts brought against the defendant" (Ringquist 1998:1157) as a measure of the severity of the case. DOCKET, however, does not contain this information; therefore, what Ringquist used was erroneous. In fact, because more than 90% of all cases are settled, any such variable is moot. In the consent decrees embodying the settlements in these cases, a clause usually is included stating that the settlement is not a finding that the defendants committed any violations. Consequently, using information from these settlements as statements that violations occurred is inappropriate, as neither EPA nor the defendants chose to test their cases.

The violation-related data that DOCKET does provide are the section(s) of the environmental statute(s) under which the ac-

tion was brought and a word or phrase indicating the type of alleged violation(s) in the case (e.g., spill, permit violation, etc.). Using these data leads to errors in both directions. Oftentimes a single violation can be illegal under multiple statutory sections. Also, sometimes the statutory section cited was simply the section generally authorizing penalties for violations, rather than a section embodying a substantive requirement. Thus, using the number of statutory sections cited could overstate the number of actual violations.

More important, however, there can be multiple violations of the same statutory section. For example, every day that a facility is not in compliance with its permit is a separate violation, subject to the maximum penalty.[16] Using the number of statutory sections cited in DOCKET understates the number of actual alleged violations, drastically so in many cases. According to DOCKET data, approximately 60% of cases cited only one statutory section, approximately 25% cited only two, and approximately 10% cited only three. Therefore, this variable has a very limited range that, undoubtedly, does not reflect the true number of violations. These data also conclusively demonstrate that the number of statutory sections cited in the case do not represent the number of violations. Even if the maximum statutory penalty of $25,000 was imposed for each statutory section cited (which, pursuant to EPA's penalty policies, would be rare), about 60% and 90% of the cases should have penalties of no more than $25,000 and $50,000, respectively (i.e., one or two violations times the $25,000 maximum penalty, respectively). Yet, according to the DOCKET data, only 42% and 61% of the cases' penalties fell below these maximum amounts. Using the statutory sections cited to represent the number of violations, then, clearly understates the latter. The same understatement results from using the word or phrase recorded in DOCKET indicating the type of alleged violation(s). Thus, not only is the variable that Ringquist included in his regression models to represent the number of violations invalid, the entire models that used the average penalty as the dependent variable are per se unreliable, because this variable was computed by dividing the penalty in a case by what Ringquist assumed was the number of violations.

Third, to represent larger companies' greater economic benefits from noncompliance, Ringquist determined whether a defendant was a *Fortune* 500 company. Because the economic benefits from noncompliance are determined by examining the facility that violated the law, rather than the company of which it

[16] For example, some of the DOCKET cases litigated to a conclusion and whose judicial opinions were published indicate that the judges counted hundreds or thousands of days of noncompliance as separate violations. See, e.g., *U.S. v. Chevron U.S.A., Inc.* (1985) (1,524 violations); *U.S. v. Environmental Waste Control, Inc.* (1989) (2,514 violations); *U.S. v. Vineland Chem. Co.* (1990) (1,223 violations).

is a part, it is uncertain, at best, whether there would be any relationship between such benefits and the size of the entire company. In addition, the EPA policy requiring that penalties include the economic benefit from noncompliance began in 1984. Thus, this variable should not be applicable to the first ten years of cases that Ringquist used. Most important, however, this variable does not measure precisely what it was intended to measure. Approximately 25% of all defendants in EPA civil judicial cases are government entities. When a dummy variable is used to represent *Fortune* 500 defendants, the remaining defendants are composed not only of smaller businesses but also of government entities, which Ringquist hypothesized for other reasons would receive lower penalties. It is unclear, therefore, whether the positive relationship that Ringquist found between penalties and *Fortune* 500 defendants is due to their greater economic benefits from noncompliance or to their disadvantage of not being a government entity. As a result, Ringquist inappropriately operationalized this variable.

Fourth, Ringquist included a dummy variable indicating whether the case involved violations at multiple locations, theorizing that such cases might result in higher penalties. Although the basis for this variable is plausible, Ringquist did not address the methodological implications of violations at multiple locations. If a defendant created environmental dangers at multiple locations, then the difficult analytical question arises about how to examine the relationship among the demographic characteristics at those locations and the penalty for the violations. One could use the demographic characteristics at each location and somehow divide the total penalty in the case among the locations. Alternatively, one could combine the demographic characteristics across all of the locations and then relate the total penalty in the case to those composite characteristics. Neither method is necessarily correct, but a method must be used to incorporate multilocation cases in any analyses. Ringquist did not explain how he resolved this question. Consequently, we do not know how reliable is his multilocation approach.

Fifth, Ringquist used the economic contribution to the state by industries that he identified as most responsible for pollution as a measure of the environmental threat they posed and their political power. The latter theory, however, was inapplicable to many of the cases that he analyzed. Of the CWA cases (which account for 41.8% of all DOCKET cases), 47.0% had government entity defendants, and dozens more involved illegal filling of wetlands or mining waste discharges. Thus, most water pollution cases either involved no industry defendants or industries that Ringquist did not include among those most responsible for pollution. Of the CAA cases, 30.3% involved violations of asbestos abatement laws by construction firms and dozens more involved

government entity defendants or automobile repair shops. Many air pollution cases, therefore, involved no industries or industries that Ringquist did not include among those most responsible for pollution. Ringquist used an industrial measure of political power that is irrelevant to many of the cases he analyzed. If this variable measures anything, it would be the environmental threat that some industries posed in a state, even though those industries are not likely to even have been involved in the case in question.

Finally, Ringquist's use of the zip codes listed in DOCKET to establish the locations of violations, and thus the area from which to extract demographic characteristics, was unreliable. As noted before with respect to the *NLJ* articles, there is no reason to believe that zip codes represent the neighborhoods around these violation locations. Furthermore, according to EPA, the addresses for many facilities in DOCKET may be their mailing, rather than physical, addresses (Reilly & Lavelle 1993). Thus, the listed zip codes may be for defendants' post office boxes or main office locations, rather than for the areas around the violation for which demographic characteristics are needed. Though *NLJ* tried to verify the DOCKET zip codes, Ringquist did not. "The NLJ took precautions to assure that the zip codes we used were based on the actual site location . . . , establishing accurate address information through calls to corporate headquarters, post offices, city halls and EPA regional offices" (Lavelle & Coyle 1992:S4). *NLJ* stated that it "checked and corrected 121 incorrect zip codes from the EPA's records"[17] (Lavelle & Coyle 1992:S4).

These problems hamper any use of DOCKET's zip codes, but they were exacerbated by the use of older DOCKET cases than those in *NLJ*'s study. Although *NLJ* began with 1985 cases, Ringquist started with 1974. Because zip code boundaries can change over time, even if the zip code in DOCKET was correct when the case ended, it might not be the violation location zip code in 1991, when the 1990 Census data were reported by zip code. It is quite likely that many of these locations had different zip codes in 1991 than they did in the 1970s and 1980s; thus, the demographic characteristics used for a location might be for the wrong zip code. Furthermore, even if the zip code remained the same, the 1990 Census characteristics could be very different from those in the zip code when the case ended. Ringquist's analyses assumed that all of the DOCKET zip codes are correct and that locations' zip codes and demographic characteristics did not change between 1974 and the early 1990s. These assumptions are obviously unlikely, so there is substantial uncertainty about the

[17] Unless these calls requested the zip codes applicable to facilities at the time the cases ended, however, these "corrected" zip codes presumably were those that applied at the time of *NLJ*'s call. Consequently, these zip codes would not necessarily be correct.

reliability of the demographic data that Ringquist used to test his environmental equity hypotheses.

For all of the above reasons, the Ringquist article cannot be relied upon to address the question of what factors, including environmental equity characteristics, affect penalties. Therefore, this article's research was intended to address the substantive, data, and methodological problems encountered in the prior research.

Research Hypotheses

The primary research hypothesis for my analyses was that the race and income characteristics of the population in the areas around environmental law violators have no effect upon the penalties imposed. This hypothesis embodies how enforcement efforts should be pursued—color- and class-blind.

The other research hypotheses are other explanations for variations in penalties. First, I hypothesized that the major determinants of penalties are the facts of the individual cases—e.g., the statute violated and the specific type of violation. This hypothesis reflects how EPA enforcement efforts should proceed—focused on the particulars of the violation. I had no specific research hypotheses concerning the importance and direction of each of their effects on penalties. Instead, my overarching hypothesis is that the effects of at least some of these case characteristics are more important than the race and income characteristics of the people around the violation location.

Second, I hypothesized that the year that a case ended affects the penalty, with a positive relationship between penalties and the recency of the case. EPA has asserted, as supported by outside observers (Bryant 1993) and by its overall enforcement results, that it has become more aggressive in its enforcement activities since the mid-1980s. These results indicate that between 1985 and 1991, EPA's enforcement budget more than tripled (U.S. EPA 1991) and the amount of civil judicial penalties rose nearly tenfold (U.S. EPA 1993a). Apparently, no analysis has been done, however, concerning whether the increased penalties simply reflect the influence of other factors, such as a changing mix of cases, rather than a more-severe EPA attitude toward violations. Although Ringquist hypothesized that penalties might differ only by presidential administration, the civil penalty data reflect a sharp and fairly linear increase beginning in 1984 (U.S. EPA 1993a). Thus, I hypothesized that penalties, measured in constant U.S. dollars, should have consistently increased over this time, controlling for other factors.

Third, I theorized that the penalties against "public" defendants are lower than those against business defendants. This view is identical to Ringquist's hypothesis, but the justification is en-

tirely different. In my experience with EPA's CERCLA enforcement actions, there is a pronounced EPA favoritism toward public entities, such as municipalities, which results in their being penalized less than businesses (U.S. EPA 1998a). No analysis, apparently, has been done as to whether this bias has extended to other EPA enforcement efforts. However, according to the then-director of EPA's Office of Waste Water Compliance, in cases where municipalities significantly violated water pollution laws, "EPA has 'almost never' recovered the economic benefit of such violations. 'Generally, we have felt that the money is better spent on building treatment works, than on fines'" (Bureau of National Affairs 1995:1908). In addition, EPA's most recent CWA penalty policy expressly allows for reductions in penalties for municipalities as part of a "national municipal litigation consideration" (U.S. EPA 1995b:17). These CWA practices are especially relevant because 79.6% of the DOCKET cases involving public defendants are CWA cases. Therefore, it is reasonable to assume that EPA would derive less internal gratification, fewer public relations kudos, and more political pressure from severely penalizing fellow government entities (or even quasi-public defendants such as private hospitals and schools). I thus hypothesized that penalties against public defendants should be lower, other factors held constant.

Fourth, I hypothesized that cases ended through litigation result in higher penalties than those that are settled. This view assumes that if EPA is forced to expend resources to litigate a case to a conclusion, it will seek higher penalties as compensation for its efforts. In addition, by refusing to settle the case, a defendant automatically loses some potential reductions in the penalty pursuant to EPA's penalty policies. "In those cases which proceed to trial, the government should seek a penalty higher than that for which the government was willing to settle, reflecting considerations such as continuing noncompliance and the extra burden placed upon the government by protracted litigation" (U.S. EPA 1986a:2). Because, consistent with *NLJ*'s case selection procedure, my analyses excluded litigated cases in which no penalty was imposed (i.e., the defendants won completely), the litigated cases would reflect only those in which EPA successfully exacted some punishment. There thus should be a positive relationship between penalties and litigated cases.

Finally, I hypothesized that penalties would be higher where more people resided near the location of the violation. Pursuant to its penalty policies, EPA is supposed to consider the health threat from a violation in setting its penalty (U.S. EPA 1987, 1990d, 1995b). All other factors being equal, then, the more people that reside around a violation location the more likely the violation could pose a risk, and the more serious that risk may be.

Therefore, I hypothesized that penalties will be higher when more people might be endangered.

Data

Initial Selection of Cases

Like the *NLJ* study, I began my analyses by identifying the locations of violations from DOCKET. To be comparable to the *NLJ* study, I initially selected all civil judicial cases from 1985 to March 1991 (other than in Puerto Rico or the Virgin Islands) in which a penalty was imposed.[18] Unlike *NLJ*, I did not initially exclude cases if no specific violation locations were identified or if my subsequent check of the Census data applicable to the locations (described later in this article) indicated that no people lived nearby. Such cases were, however, ultimately excluded from some of my analyses. For purposes of these analyses, a few cases that were listed separately in DOCKET but which were actually combined with other cases by the time they ended were combined into one case for each defendant involved. In addition to these exclusion criteria used by *NLJ*, a careful examination of the CERCLA cases revealed that almost all involved the government's recovery of contamination cleanup costs, rather than actual penalties against defendants. Thus, I excluded these cost-recovery cases from my analyses. Omitting these cases also eliminated almost all multistatute cases; thus, unlike NLJ's analysis, I did not include this characteristic in my analyses.

Initial Case and Defendant Information

For each selected case, I obtained from DOCKET the penalty imposed and the environmental statute(s) violated, just as did *NLJ*.[19] In addition to this information, I extracted data relating to whether the case ended through litigation (including a few default judgments) or through a settlement, the year a case ended, and the type of violation. With respect to the latter, EPA has assigned codes to a few dozen types of violations. For each case, EPA can then use up to five of these codes to indicate the type(s) of violation(s) involved. Five codes are more than adequate, as less than one-half percent of all cases analyzed had even five types of violations coded for them. Table 1 displays the most common types of violations used in my analyses. (The total is

[18] There were only 21 cases not involving contamination cleanup costs in which no penalties were imposed. Of these, 12 were litigated to a conclusion, 3 were dismissed by the judges, 3 were voluntarily dismissed by the parties, 2 were default judgments, and the nature of 1 was unknown.

[19] Even though almost all cases ended in settlements in which violations were neither admitted nor proved, for purposes of brevity in this article I use "violations," rather than "alleged violations."

Table 1. Percentages of U.S. EPA Civil Judicial Cases Involving Particular Types of Environmental Violations (N = 915 cases)

Cases with Violation (%)	Type of Violation
28.6	CWA permit violation
17.4	CAA State Implementation Plan (SIP) violation
12.1	CAA asbestos abatement violation
11.5	Other CWA violation
10.5	Other CAA violation
10.4	Violation of an administrative order
8.9	CWA pretreatment violation
8.6	Failure to obtain a required permit
7.9	RCRA violation
5.6	CAA New Source Performance Standard (NSPS) violation

more than 100% because some cases had multiple types of violations.)

Instead of including a variable simply representing a CAA or CWA violation in general, as did *NLJ* and Ringquist, my analyses distinguish among types of violations within these statutory programs. Also, I specifically identified some types of violations that cut across statutory programs, e.g., lacking required permits or violating administrative orders. Judges and enforcement staff may react to such violations quite differently, and they may reflect offenses that have varying severity.

In addition, I recorded whether at least one of the defendants in a case was a public/quasi-public entity. Public defendants included state and municipal agencies, medical facilities, and schools and universities. To be consistent with the hypothesized relationship between public defendants and penalties, all medical and educational institutions were designated as public defendants, even if they may have been private institutions, though only a few such cases arose. Of all the cases, 24.7% involved public defendants.

Verification of DOCKET Information

Except for *NLJ*'s efforts to verify DOCKET zip codes, neither it nor Ringquist attempted to confirm DOCKET data. In my research, I took extensive efforts to do so. DOCKET was not created with the expectation that it would be used in data analyses, but only to maintain a simple record of EPA's civil judicial actions; thus, it would not be surprising if its more than 25 years of data contained errors, omissions, or inconsistencies. In addition, if other information existed that could augment DOCKET data and could provide more precise measures of the locations of violations it would be especially useful in this type of research. Therefore, I attempted to obtain the consent decree for each DOCKET case that was ended through settlement from 1985 to March 1991.

As I noted before, full-text versions of consent decrees are provided by the LEXIS on-line legal research service. These consent decrees—typically dozens of pages long—ordinarily contain various information about the case, including the identities of the defendants, a brief description of the types of violations, the locations of the violations, and the penalties. Of the 841 cases that ended by settlement that I ultimately identified during this research, I found 757 (90%) of their consent decrees or related court documents. If I found no consent decree, I searched LEXIS for the *Federal Register* notice that announced the proposed consent decree for the case. These notices ordinarily contained a brief description of key facts from the consent decree (i.e., the defendants, types of violations, and penalties in the case). Information on another 43 (5.1%) settled cases was found in these notices. Finally, if I did not find a consent decree or *Federal Register* notice, I searched the *Environmental Law Reporter* for information on the case, as that periodical typically contains brief reports of proposed consent decrees. I found information on another eight (1.0%) settled cases from this source. Thus, a total of 96.1% of all settled cases had at least the key parts of their information confirmed through this process.

For information on the 74 cases that I ultimately identified during this research as having ended by litigation, I searched LEXIS for the judges' opinions. For 39 (52.7%) of these cases, I found these opinions or other related court documents. If I found no judicial opinion for a case, I searched the *Environmental Law Reporter* and the Bureau of National Affairs' *Environment Reporter* for information, as those periodicals often contain brief reports of litigation. From these sources I found information on another four (5.4%) of the litigated cases. Thus, a total of 58.1% of all litigated cases had at least the key parts of their information confirmed through this process.

Finally, to better ensure that DOCKET omitted no cases, I performed LEXIS searches to identify any settled or litigated cases during the period in question and to check whether they appeared in DOCKET. This process led to a number of cases that DOCKET completely omitted or that it erroneously recorded as having no penalty or as falling outside the period of interest. In all, of the 915 settled and litigated cases that I ultimately identified during this research, I confirmed at least the key information from 93.0% of these cases through these processes.

The comparisons of DOCKET data against the other sources of information uncovered almost every conceivable type of error, omission, and inconsistency in DOCKET. Some penalty amounts were incorrect, and sometimes DOCKET included in penalties the amounts paid not only to the federal government but also to state or local governments. (Because this research focused on what influenced federal penalties, I deducted these payments to

other entities from the penalties for my analyses.) Sometimes it was possible to separate the penalties paid by different defendants for separate violations that EPA had combined into one case. In such cases, these distinct defendant/violation/penalty occurrences were treated as separate cases, because, presumably, EPA sought a penalty for each based on its particular set of facts. Often, EPA had not recorded in DOCKET all of the applicable types of violations for a case or had done so incorrectly. Sometimes DOCKET misclassified cases that ended through settlement as having ended through litigation. Typically, the additional information sources also contained very specific indications of the locations of the violations, which proved to be critical in precisely locating them, as I will discuss in the following section. All of this information was used to produce the corrected and augmented version of DOCKET used in this research; thus, using the existing DOCKET data in any analysis is inherently suspect.

Identifying Violation Locations

The *NLJ* analyses used 1989 estimated census data for the race and income characteristics of the 1989 zip codes matching DOCKET addresses. Ringquist used the 1990 Census data of the zip codes of these addresses in 1991. As noted earlier, an inherent problem in determining the demographic characteristics of areas around violation locations is that some DOCKET addresses are incorrect. Because of the obvious importance in environmental equity research of obtaining correct locations, I made extensive efforts during this research to do so. I attempted to identify as precisely as possible the locations of these violations and to do so in a way that would not be subject to variations in the future (e.g., using zip codes or Census units whose boundaries may change over time).

First, rather than using the DOCKET address for each violation location, this research began with its unique EPA identification number, as assigned to the location by EPA and listed in DOCKET. These identification numbers stay with a specific location, regardless of changes over time in street addresses, zip codes, or Census unit boundaries. I then matched the EPA identification numbers to EPA Envirofacts databases, which are accessible from EPA's website and which contain many EPA identification numbers and EPA's best estimate of the corresponding locations' latitude and longitude.

Further research was done on locations that were not matched to a set of EPA coordinates through this process. In many cases, the consent decrees, *Federal Register* notices, and judicial opinions gathered during this research contained the specific address or unique permit identification number for the lo-

cation at which the violation occurred, or otherwise provided useful information. I then used this information in further searches of EPA's Envirofacts databases to determine the locations' coordinates. Of the 847 single-location cases (multilocation cases are discussed in the following section), 659 (77.8%) were matched to their coordinates through this process. Another 87 cases (10.3%) were matched to their coordinates through a website that provides coordinates for any street address. Finally, the coordinates rather than street addresses of two cases were already listed in DOCKET. Thus, I obtained a total of 748 (88.3%) of the DOCKET violation location coordinates through these processes.

Of the 99 single-location cases for which coordinates were not found, 23 had no violation location relevant for this research. These were cases in which a violation occurred, but any possible resulting harm occurred at no definable location (presumably the same cases that *NLJ* excluded because no specific facility was cited as the source of the violation). For example, these locations included those in which automobile mechanics illegally tampered with customers' catalytic converters, thus causing harm wherever the automobiles went, not where the mechanics were. Many other cases for which coordinates were not found were in isolated, rural locations, such as wetlands, wells, and mines. (Twenty were in Alaska—typically, isolated mining facilities—and 14 were injection wells or small public drinking water systems.) Because I excluded violation locations with no people nearby from my analyses involving demographic characteristics, I would have likely eliminated most of the locations with missing coordinates eventually for that reason. Therefore, almost all DOCKET single-location violations were precisely located, and those not located likely would not have significantly affected my analyses.

Identifying Multilocation Cases

As described earlier, some DOCKET cases involve violations at multiple locations. *NLJ* apparently did not realize this and erroneously treated each violation location as a separate case. Ringquist realized this, but did not indicate how he either apportioned each multilocation case's penalty among the locations or how he combined the demographic characteristics around the locations to create a composite demographic profile for the entire case. According to my review of DOCKET and other information, there were 68 multilocation cases, composing more than 320 violation locations. Of these violation locations, approximately 120 involved asbestos abatement infractions by contractors, around 100 involved cities' multiple wastewater treatment plants or sewage discharge points, and about 30 involved offshore oil-drilling platforms.

After reviewing the information for 19 of these multilocation cases, I made no attempt to obtain the coordinates of their locations. Almost all of these cases involved locations that clearly had no people nearby (e.g., off-shore oil platforms); multiple wells in rural areas with probably few, if any, people living around them and with no usable location information provided; or multiple wastewater treatment facilities or sewage outfalls spread around a city or region.[20] Aside from the low probability of obtaining coordinates for these locations, their number and geographical dispersion made it very unlikely that judges or enforcement staff would know, or even make an effort to know, what were the demographic characteristics of the people around them. Thus, I obtained no demographic characteristics for these cases. I made efforts to obtain the coordinates of the violation locations for the remaining 49 multilocation cases, just as described previously for the single-location cases. I obtained the coordinates for all of the violation locations for 28 of these cases, and thus gathered their demographic characteristics.

Demographic Data

All of the demographic data used in my analyses were from 1990 U.S. Census files. Because the period for these analyses is from 1985 to 1991, the 1990 data are a close match, and significant changes in areas' characteristics over that time are unlikely. The relevant population around an environmentally related location for which such Census data should be sought has been defined in various ways in different environmental equity studies. Counties, zip codes, Census tracts, Census block groups, judgmentally defined neighborhoods, and geographic concentric rings have all been used as units of analysis. Different units of analysis may be more appropriate for different types of studies (Zimmerman 1994; Mohai 1995; Fahsbender 1996).

Many environmental equity studies focus on whether environmentally regulated facilities are discriminatorily located in certain communities. In those situations, as well as in this article, what is the relevant "community" is an uncertain historical, sociological, and psychological question that depends largely on what the hypothetical discriminators thought of as the community (Mohai 1995; Fahsbender 1996). Given the impossibility of anyone knowing what the enforcement staff thought of as the community around each violation location, I used geographic concentric rings around the locations as the units of analysis. If, in

[20] Also, even though the coordinates of two multilocation and one single-location cases were available, they were not used because at least one of the locations was a public attraction (e.g., Ellis Island, a football stadium, and an amusement park). Thus, it is more likely that enforcement staff considered the potentially exposed population to be visitors to the location, rather than people who might live nearby. Using the demographic characteristics of the latter, consequently, would be inappropriate.

fact, the enforcement staff is aware of the demographics of the people who live around a violation location, I believe that most likely they gained that knowledge by driving around the location, and thus a moderately sized concentric ring would be relevant. The width chosen for the concentric ring should, again, reflect the community; past environmental equity studies have used distances ranging from one-half mile to a few miles. The results I present in this article are primarily for a ring with a one-mile radius because that three-square-mile area seemed to reasonably reflect the area that someone might consider as consisting of the community around a location, and, as I describe later, using smaller rings did not meaningfully change the results.

Naturally, no one can ascertain whether the enforcement staff thought of the relevant population as being contained in a ring around each location. It does seem far more plausible than the zip code boundaries used in the *NLJ* and Ringquist studies, however. It is difficult to imagine that enforcement staff would even know what the zip code boundaries are around a location, much less that they would use these boundaries to define the surrounding community. In particular, though a one-mile ring encompasses an area of 3.1 square miles and the median size of a Census tract is 2.2 miles, the median size of a zip code is 40 square miles[21]—a large area for anyone to consider as a community. Furthermore, even though Census tracts, block groups, or other administratively set boundaries might sometimes better reflect what residents consider as their community, relevant to my research is only what the prospective discriminators—not their targets—thought of as the community. I considered it unlikely that enforcement staff, particularly judges, would know or even attempt to identify the Census units containing, and perhaps adjacent to, violation locations and then research their demographic characteristics.

I processed the coordinates of 818 violation locations through geographic information system software. At least some people resided in the rings around 640 (78.2%) of these locations; 586 (78.3%) of the 748 single-location violation cases were in populated areas. The Census data extracted for each ring were the total number of people within the ring around each location, their median household income, and the proportion of them that were minorities (other than non-Hispanic white), African American, Hispanic-origin, poor (i.e., below the official poverty line, based on their household size), and low income (which I defined as anyone below one and one-half times the official poverty line). I included the latter two measures of income, instead of only *NLJ*'s median household income or Ringquist's per capita income, because they might better reflect the presence of

[21] Land areas calculated from U.S. Census data files.

people of lower income. Per capita income is a mean that may be skewed upward by the presence of a relatively small number of high-income residents. Median household income data may obscure the presence of many people with incomes far below the median.

Analyses

Bivariate Relationships Between Penalties and Income and Between Penalties and Race

A first step in evaluating the relationship between penalties and the race and income variables was to produce scatterplots. This method is particularly important because one may hypothesize different functional forms for such a relationship that might affect the efficacy of other analytic methods—it might be linear, curvilinear, or a step function (e.g., penalties become substantially lower in areas only when the proportion of minorities exceeds a certain level). The scatterplots that I produced (which are available upon request) plotted the minority proportion, median household income, and poor and low-income proportions in the one-mile ring around each violation location against the penalty imposed in the case. These figures revealed no relationship whatsoever between penalties and the race and income variables.

The next step in analyzing whether a relationship existed between penalties and the race and income variables was calculating correlation coefficients. I computed the correlations using both Pearson's *r* and Spearman's *rho,* the latter in case of concerns over the normality or homogeneity of the variance of the data. These correlations are displayed in Table 2. As the results demonstrate, there are no substantial or statistically significant relationships[22] between the race and income variables and penalties.

Table 2. Correlations Between Penalty Amount in U.S. EPA Civil Judicial Cases and Race and Income of People Around Violation Locations (N = 586 single-location cases in populated areas)

Correlations	Pearson's *r*	Spearman's *rho*
Minority proportion	0.014	0.066
African American proportion	–0.019	0.054
Hispanic proportion	0.073	0.057
Median household income	–0.077	–0.018
Poor people proportion	0.022	0.011
Low-income people proportion	0.033	0.019

[22] Statistical significance actually is irrelevant in my analyses because they are based on essentially the entire population, rather than a sample, of civil judicial cases during the time in question. Consequently, any numbers or differences among them are real, rather than due to sampling error. This problem is a recurring issue in environmental equity

Comparison of Quartile Means

Next, I used *NLJ*'s analytic method—comparing the mean penalties in single-location cases that fell into the quartiles with the highest- and lowest-minority proportions and median household incomes, respectively. Table 3 displays the results.

Table 3. Comparison of Mean Penalties in U.S. EPA Civil Judicial Cases Within Quartiles Distinguished by Race and by Income (N = 147 single-location cases in populated areas in each quartile)

Quartile	Mean Penalty (U.S. $)
Race	
White quartile	113,791
Minority quartile	133,808
Hispanic quartile	113,632
African American quartile	100,797
Income	
High-income quartile	82,816
Low-income quartile	162,804

Unlike *NLJ*'s findings, these results indicate that there is no substantial difference in mean penalties between the white and minority quartiles (and even less when the white quartile is compared to the quartiles that I added for Hispanics and for African Americans). Furthermore, the difference that exists is in *favor* of minority areas. Like *NLJ*'s findings, there is a substantial difference in mean penalties between the high- and low-income quartiles. The difference, however, is once again the opposite of what *NLJ* found. Low-income areas had substantially *higher* penalties than high-income areas. Thus, according to *NLJ*'s theory, these results demonstrate discrimination against wealthy white people.

Median Penalties

Any comparison of mean penalties, however, can be a misleading indication of the distribution of penalties, because there is a relatively small number of very large penalties that dramatically affects the overall mean penalty and the mean penalties within quartiles. This comparison is deceptive because 39.9% of the combined penalties imposed in all single-location cases in populated areas was accounted for by just 15 cases (3%), those in which the penalties were at least $1,000,000. If these obviously unusual cases were excluded from the analysis, the mean penalty across all of these cases would decline from $121,713 to $73,180.

research because many studies obtained all or almost all of the items they were seeking, yet they still used tests of statistical significance to determine which findings were important. For the purposes of this article, measures of statistical significance are provided (for readers who believe that they are useful), but they do not determine the importance of results.

If another ten cases with penalties between $500,000 and $1,000,000 were omitted from the analysis, the mean penalty would decrease to $62,285. Ignoring only 4% of the cases, then, nearly halves the mean penalty.

With such a skewed distribution of penalties, the median may better reflect the true "typical" penalty. Therefore, instead of a mean penalty of $121,713, the median penalty is $40,000. This extreme difference not only underscores the importance of using the median as the preferred measure of central tendency in this situation but also indicates how relatively modest are the typical penalties. Unless there is an enormous degree of discrimination against particular areas or groups, the amount of any discrimination penalty premium would likely, for practical purposes, be rather small in dollar terms. Probably a key reason that the typical penalties are relatively small also shows the difficulty in using penalties as a measure of the severity of the enforcement action. Most of the settled cases not only imposed a penalty but also required actions by the defendants, e.g., installing new pollution control equipment, beginning environmental training programs for employees, or keeping certain heavily polluting production lines closed. Not complying with these commitments subjected defendants to contempt of court fines and, often, stipulated penalties. Thus, in most settled cases, the burden imposed on defendants came not from the penalty, but primarily from the activities they were required to undertake, often entailing expenditures that were orders of magnitude more than the penalties. However, these expenditures were not necessarily truly penalties; typically, they were simply what the defendants needed to do to comply with the law.

The differing conclusions that may be drawn from evaluating median versus mean penalties are demonstrated by using *NLJ*'s analytic method. In single-location cases in populated areas, the mean penalty in the high-income quartile was $82,816 versus $162,804 in the low-income quartile (Table 3). Using medians, however, the typical penalty in the high-income quartile was $37,500 versus $50,000 in the low-income quartile. The difference in medians is far less, and far less meaningful, than the difference in means. The primary reason for this variation in results is that the low-income quartile contains a few more cases with unusually large penalties than does the high-income quartile. Of the 15 cases with penalties exceeding $1,000,000, six fell in the low-income quartile, while only one fell in the high-income quartile. Eliminating these few unusually high penalty cases from the analysis reduces the difference between these quartile means from approximately $80,000 to less than $9,000. Thus, when analyzed more thoroughly, the large difference between high- and low-income areas' penalties essentially disappears.

Naturally, we do not know whether the differences that *NLJ* found between its quartile means were skewed in this same way. If *NLJ* were to disclose the data that it used, this result could be checked. It is clear, however, that the analysis that *NLJ* did was especially susceptible to such misinterpretations, particularly given the severe skew in the overall distribution of penalties. By *NLJ*'s reporting only the quartiles' means, it was more likely that a few outliers could create a misleading impression of the typical penalty within a quartile. Furthermore, focusing only on the extreme quartiles made *NLJ*'s analysis very sensitive to whether one or more of these rare large-penalty cases happened to fall in a particular quartile.

This result would be a particular concern in *NLJ*'s analyses of the quartiles' mean penalties for specific types of violations. For example, the most publicized result of *NLJ*'s analysis was the much higher RCRA penalties in white areas versus minority areas. According to *NLJ*, however, only 16 cases fell into each of these two quartiles. By *NLJ*'s own data, half of the difference between these white and minority quartile means was due to just one large penalty RCRA case that fell in the white quartile. Any conclusions based on such small numbers of observations are especially unreliable when the presence of only a few observations in a quartile can raise the quartile mean enormously.[23] Aside from the other methodological problems with the *NLJ* study that I previously described, the uncertainty about whether the quartile means are representative of the quartiles' penalties consequently makes the study unreliable.

Regression Models

Single-Location Cases Regression Model

My final set of analyses used multiple regression models. The dependent variable in each model was the penalty in a case. To account for the possibility that judges and enforcement staff may have increased the penalties they imposed over time, consciously or unconsciously, simply to reflect inflation, I transformed the penalties into constant 1985 U.S. dollars by adjusting them, consistent with changes in the Consumer Price Index (U.S. Bureau

[23] In fact, *NLJ* claimed to be concerned about the dangers of relying upon small numbers of observations. *NLJ* stated that it included in its minority quartiles some violation locations whose populations were far from predominantly minority because including only predominantly minority locations would have produced too few cases to analyze. "Without enough sites in each analysis category, it would be difficult to delineate trends over and above random differences that may occur among sites" (Lavelle & Coyle 1992:S4). Yet *NLJ* then proceeded to claim as reliable its results based on comparisons of two groups of only 16 RCRA cases each.

of the Census 1993).[24] Because, as demonstrated previously, the distribution of penalties was significantly skewed, they were transformed into natural logarithms.

I initially examined six different regression models, but they differed only in how they included the race and income variables. There were two sets of three models each. One set used the minority proportion around a violation location as an independent variable, while the other set used the proportions of African Americans and Hispanics, respectively. This was done to determine if it was the proportions of specific types of minorities near a violation location that affected penalties, rather than only the combined proportion of all minorities. For example, other researchers have claimed that the presence of Hispanics, but not African Americans, is related to environmental equity concerns (Been 1997).

The three models within each of the two sets differed only in how they included the income variable. One model used median household income, another used the proportion of poor people, and another used the proportion of low-income people. The results I describe in this article only focus on the model in which I used median household income and the proportion of all minorities. This is because the African American and Hispanic proportion variables were never close to statistical significance in the models they were in, but the total minority proportion was. Consequently, this finding indicates that it may be the total proportion of minorities in an area, rather than their specific types, that affects penalties. In addition, leaving aside statistical significance, according to these models, the penalties imposed in areas that were 100% African American would, controlling for other factors, be only moderately higher than in 0% African American areas (in effect, discrimination in favor of this group). The difference between areas that were 0% and 100% Hispanic, respectively, also showed discrimination in favor of this group. Because the coefficients for both of these minority groups were positive, I decided to combine them and use the total minority proportion as the preferred measure of race.

Also, whether income was measured by median household income or by the proportions of poor or low-income people made no difference in the models; thus, examining only one of these income variables also reflects the impacts of the others. Because the minority proportion was substantially more correlated with the poor and low-income proportions than it was with median household income, the latter was the preferred measure of income I used. Finally, the same variables (other than those reflecting race) were statistically significant in all of the models,

[24] All federal agencies, including EPA, have been required by law since 1990 to periodically adjust their maximum statutory civil penalties to reflect inflation (28 U.S.C. § 2461 note [2000]). EPA first did so in 1996 (U.S. EPA 1996).

their coefficients varied by less than 10%, and there was virtually no difference across the models in their overall explanatory power. Consequently, the results presented here are representative of all the models.

The independent variables in this model thus included the median household income, number of people, and minority proportion in the one-mile ring around each violation location in single-location cases. (Analyses using multilocation cases are discussed later.) Because a curvilinear relationship between these variables and penalties was possible (e.g., penalties might increase as the number of people around a violation location increased, but at a decreasing rate), I also included the quadratic forms of these variables in the model. The other independent variables were the year a case ended and—coded as dummy variables—the types of violations in a case, whether a case did or did not involve a public defendant, and whether a case ended in settlement versus litigation (with public defendant and settled cases coded as ones, respectively). A correlation matrix of these independent variables is provided in Table 4.

Single-Location Cases Regression Model Results

Table 5 displays the coefficients of the independent variables, and their statistical significance, that resulted from the analysis. With respect to my research hypotheses, these results first support the hypothesis that the income level of an area has no meaningful effect on penalties, as this variable was not statistically significant. In addition, aside from statistical significance, median household income had a minuscule impact on penalties; even areas in which median household incomes differed enormously would have only slightly different penalties, all other factors held constant. Furthermore, in affluent areas (median household income of approximately $45,000), penalties actually decreased as median household income increased.

Contrary to what I hypothesized, the results show that the race of an area does affect penalties, as this variable (though not the quadratic term) was found to be statistically significant. Contrary to *NLJ*'s claims, though, penalties *increased* as the proportion of minorities in an area *increased*. Thus, if one believes that lower penalties in an area reflect less concern about that area's residents, this finding indicates discrimination *against* whites. For example, a $40,000 penalty in an area that is 100% white would be $59,490 in a 50% minority area and $88,475 in a 100% minority area, all other factors held constant. Thus, in percentage terms, the difference in penalties in favor of overwhelmingly minority areas is far higher than the difference found by *NLJ* in favor of white areas.

Table 4. Correlation Matrix of Variables in U.S. EPA Civil Judicial Case Regression Analyses

	X_1	X_2	X_3	X_4	X_5	X_6	X_7	X_8	X_9	X_{10}	X_{11}	X_{12}	X_{13}	X_{14}	X_{15}
X_1: Minority proportion															
X_2: Median household income	−0.30														
X_3: Number of people in area	0.35	0.04													
X_4: Year case ended	−0.05	0.02	−0.02												
X_5: Settled case	−0.03	0.03	−0.01	−0.08											
X_6: Public defendant	−0.17	0.02	−0.13	0.03	0.14										
X_7: CAA SIP violation	0.06	0.04	0.06	−0.14	0.12	−0.19									
X_8: CAA NSPS violation	0.06	−0.04	−0.06	0.03	0.04	−0.09	−0.10								
X_9: Asbestos abatement violation	0.03	0.04	0.16	0.07	0.06	−0.09	−0.22	−0.10							
X_{10}: CAA other violation	0.08	−0.04	−0.08	−0.06	0.03	−0.14	0.06	0.04	−0.11						
X_{11}: CWA permit violation	−0.18	−0.08	−0.22	0.03	0.16	0.61	−0.32	−0.15	−0.24	−0.17					
X_{12}: CWA pretreatment violation	0.06	0.02	0.25	−0.09	0.06	−0.01	−0.19	−0.09	−0.14	−0.10	−0.04				
X_{13}: CWA other violation	−0.03	−0.05	−0.11	0.07	0.04	0.07	−0.16	−0.08	−0.12	−0.09	0.13	−0.01			
X_{14}: RCRA violation	−0.03	−0.01	−0.07	0.15	−0.21	−0.19	−0.17	−0.08	−0.13	−0.09	−0.18	−0.11	−0.05		
X_{15}: Administrative order violation	−0.10	−0.04	−0.11	0.05	−0.05	0.24	−0.15	−0.06	−0.13	−0.07	0.26	−0.04	0.02	0.07	
X_{16}: Failure to obtain permit	−0.05	−0.06	−0.10	0.10	0.06	−0.03	−0.14	−0.08	−0.11	−0.06	−0.01	−0.06	0.50	0.33	0.01

The results, however, do support my hypothesis that the more important determinants of penalties are the facts of the individual cases. The only types of violations that were not statistically significant were NSPS, failure to obtain permit, and asbestos abatement violations. Also, the statistically significant variables were, typically, found to have large impacts on penalties, as is demonstrated by the data in Table 6. To indicate the importance of the independent variables, aside from their statistical significance, I calculated the changes in penalties that would result from changes in these variables. For each variable, these changes were from a penalty of $40,000 (the median penalty in all single-location cases), with all other variables held constant.

As the figures in Table 6 demonstrate, the variables with the largest absolute effects on penalties are the types of violations (and those types of violations that were not statistically significant generally had the smallest impacts on penalties). The differences in penalties caused by race (a maximum difference of $48,475 in penalties between a 100% white area and a 100% minority area) are typically much less than the differences caused by the type of violation. Thus, even though violations in different locations would be penalized differently depending on the racial composition of these locations, the differences are much smaller than those caused by other variables.

Table 5. Single-Location U.S. EPA Civil Judicial Cases Regression Model Results

Dependent Variable: Natural Logarithm of Penalty Amount		
Independent Variable	Coefficient	(standard error)
Minority proportion	0.794	(0.328)*
Minority proportion^2	−0.898	(0.737)
Median household income	8.14e-06	(6.97e-06)
Median household income^2	−3.91e-10	(3.09e-10)
Number of people in area	−4.40e-06	(4.91e-06)
Number of people in area^2	−2.68e-12	(3.18e-11)
Year case ended	0.077	(0.032)*
Year case ended^2	−0.008	(0.019)
Settled case	0.501	(0.229)*
Public defendant	−0.391	(0.160)*
CAA SIP violation	0.914	(0.197)**
CAA NSPS violation	0.429	(0.272)
Asbestos abatement violation	0.110	(0.229)
CAA other violation	1.183	(0.230)**
CWA permit violation	1.285	(0.209)**
CWA pretreatment violation	0.851	(0.219)**
CWA other violation	0.657	(0.246)**
RCRA violation	1.496	(0.250)**
Administrative order violation	−0.509	(0.193)**
Failure to obtain permit	−0.360	(0.264)
Constant	9.401	(0.246)**
Adjusted R^2	0.164**	

N = 586 single-location cases in populated areas
** Statistically significant at the 0.01 level.
* Statistically significant at the 0.05 level.

Table 6. Change from $40,000 Penalty Caused by Specific Independent Variables

Independent Variable	Change from $40,000 Penalty
RCRA violation	+$138,545
CWA permit violation	+ 104,658
CAA other violation	+ 90,602
CAA SIP violation	+ 59,756
CWA pretreatment violation	+ 53,697
CWA other violation	+ 37,129
Settled case	+ 25,990
CAA NSPS violation	+ 21,436
Asbestos abatement violation	+ 4,670
Recency of case	+ 3,000 to 4,000 per year
Failure to obtain permit	− 12,090
Public defendant	− 12,953
Administrative order violation	− 15,956

Also consistent with what I hypothesized, the recency of the year in which a case ended had a statistically significant, positive impact upon penalties (though not the quadratic term). Adjusted for inflation and with other factors controlled for, penalties tended to rise in the years from 1985 to 1991. A median penalty of $40,000 in 1985 thus would increase by $3,000 to $4,000 in each succeeding year. In addition, my hypothesis was supported that penalties against public defendants were lower than those assessed against business defendants, as this variable was negative and statistically significant. A median penalty of $40,000 against a business defendant would thus be nearly 50% higher than that imposed on a public defendant.[25]

Cases ended through litigation did produce different penalties than those that were settled, but the relationship was the opposite of my hypothesis. As reflected by the positive coefficients for settled cases, penalties were smaller in litigated cases, perhaps indicating that recalcitrant defendants were more successful in limiting their liability to EPA than I had assumed. A median penalty of $40,000 resulting from a litigated case would be nearly $26,000 less than one that was settled.

Finally, the number of people residing around the violation location had no statistically significant impact on penalties. Aside from not being statistically significant, only if the population density around the violation location was several hundred times more than the national average would there be any substantial impact on penalties.

I also conducted the single-case regression analyses using geographic concentric rings of one-half and one-quarter mile radii,

[25] Because of the substantial correlations, as indicated in Table 4, between public defendants and CWA permit violations and between CWA other violations and failure to obtain permit violations, these variables' coefficients may be unstable and thus the amount of the penalty changes attributable to each of them should be regarded as less certain.

respectively. The observations in these analyses were the 434 and 250 violation locations with people in the quarter-mile and half-mile rings, respectively. The results were very similar to those for the one-mile rings, thus indicating that the relationships are robust. The coefficients for 17 of the 20 independent variables had the same sign across all three models, and none of the three variables (median household income, number of people, and the square of the year that the case ended) with inconsistent signs were statistically significant in any of the models. All of the variables that were statistically significant in the models for the two smaller rings also were statistically significant in the one-mile ring model, though the opposite was not always true. There was very little difference in the R^2s of the models, but the size—though not the direction—of any particular coefficient varied among the models. It therefore appears that the basic relationships among the variables are not sensitive to the size of the geographic area used in the analyses.

All Cases Regression Models

It is impossible to know how judges and enforcement staff approach multilocation cases—how, if at all, in their own minds they apportion the total penalty in a case among the violation locations and how they take into account the demographic characteristics of those locations. Including these multilocation cases in analyses may be important, because at least some of their characteristics are quite different from single-location cases. Multilocation cases are one and one-half times more likely than single-location cases to involve public defendants, CWA permit violations, and asbestos abatement violations, and far less likely to involve NSPS, SIP, and RCRA offenses. Also, the mean penalty in multilocation cases was $423,821, nearly four times higher than in single-location cases (though the median penalty was only about 40% higher). Furthermore, though only 31% of the areas around single-location cases had minority proportions above 0.37 (one and one-half times the national minority proportion), 67% of the areas around multilocation cases had such high minority proportions. Although *NLJ* mistakenly assumed that each location in multilocation cases was a separate case, Ringquist correctly identified these cases, but did not indicate how he incorporated them into his analyses. In this research, I tried multiple ways of incorporating them, with the intent of, in effect, using them in sensitivity analyses to determine whether a particular method affects the results.

Per location model.

In this model, I assigned each location in a multilocation case a pro rata share of the case's total penalty. For example, if a case

involved a $100,000 penalty for violations at five locations, each one was apportioned one-fifth—$20,000—of that penalty. This model is the easiest, and perhaps most intuitive, way of apportioning penalties. Thus, in effect, each location in a multilocation case was treated as a separate case.

Per capita model.

In this model, I allotted each location in a multilocation case a share of that case's total penalty, equal to its share of the combined population in the one-mile rings that the locations were in—basically, a per capita share. This model essentially assumes that judges and enforcement staff generally know the number of people potentially exposed to each violation location and that they perceive that more of the total penalty in a multilocation case is imposed on locations in the more populous areas. Thus, each location in a multi-location case was treated essentially as a separate case.

Combined area model.

In this model, I combined the demographic characteristics of the population in the one-mile rings around the violation locations to produce the minority proportion, number of people, and median household income for the people around any of the case's violation locations. For the latter variable, I calculated the weighted average of the locations' median household incomes (weighted by the locations' proportions of their combined populations). I then assigned the total penalty for the multilocation case to the applicable combined area. This model essentially assumes that judges and enforcement staff generally know the demographic characteristics of the areas around the violation locations and think of the penalty as being imposed indivisibly upon a single area that represents a composite of the demographic groups. Therefore, I treated each multilocation case as only one case.

Thus, I modified the regression model that I had developed for single-location cases to include the multilocation cases. As with the single-location case model, I included multilocation cases only if all of the violation locations' latitude and longitude coordinates were known and if the areas were populated. In the Per Location Model and in the Per Capita Model for incorporating multilocation cases, I used each case's violation locations as a separate observation. Although the demographic characteristics around the locations varied, the other independent variables—the characteristics of the case—obviously did not. Therefore, the values of these variables across the locations in a multilocation case would not be independent, violating a fundamental regression assumption. To address this concern, the Per Location and

Per Capita Models were estimated using Huber's robust standard errors (Huber 1967), treating each case as a cluster and thus each location in a multilocation case as part of a cluster of observations. Also, because I combined all violation locations into one observation in the Combined Area Model, I added a new independent variable to the model that represented the number of locations in the case at which violations occurred. I hypothesized that the penalty in a case would be positively associated with the number of violation locations, because judges and enforcement staff would regard violations at more locations as separate violations, each of which can be penalized, and possibly as endangering more of the environment and people, which could increase the gravity of the offense.

All Cases Regression Models Results

Table 7 displays the coefficients of the independent variables, and their statistical significance, that resulted from these analyses. These models varied little from each other or from the single-location model. The only noticeable difference is that both the year the case ended and whether it ended by settlement or litigation are not statistically significant (though close to it) in

Table 7. Regression Model Results for All U.S. EPA Civil Judicial Cases

	Dependent Variable: Natural Logarithm of Penalty Amount Coefficient (standard error)					
Independent Variable	Per Location Model		Per Capita Model		Combined Area Model	
Minority proportion	0.833	(0.301)**	0.940	(0.313)**	0.802	(0.317)*
Minority proportion^2	−1.065	(0.695)	−1.184	(0.719)	−1.021	(0.730)
Median household income	8.64e-06	(6.85e-06)	8.78e-06	(6.90e-06)	7.21e-06	(6.89e-06)
Median household income^2	−4.09e-10	(2.96e-10)	−3.76e-10	(2.97e-10)	−3.60e-10	(3.08e-10)
Number of people in area	−6.97e-06	(4.54e-06)	−5.62e-06	(4.63e-06)	−3.27e-06	(4.61e-06)
Number of people in area^2	−5.60e-12	(2.27e-11)	−4.06e-13	(2.36e-11)	−1.10e-11	(2.71e-11)
Year case ended	0.055	(0.035)	0.057	(0.035)	0.070	(0.032)*
Year case ended^2	−0.004	(0.021)	0.002	(0.021)	−0.006	(0.018)
Settled case	0.513	(0.301)	0.518	(0.299)	0.512	(0.226)*
Public defendant	−0.495	(0.169)**	−0.450	(0.170)**	−0.410	(0.157)**
CAA SIP violation	0.846	(0.239)**	0.862	(0.238)**	0.879	(0.193)**
CAA NSPS violation	0.367	(0.252)	0.399	(0.251)	0.393	(0.271)
Asbestos abatement violation	0.042	(0.261)	0.065	(0.260)	0.044	(0.226)
CAA other violation	0.916	(0.288)**	0.931	(0.290)**	1.134	(0.228)**
CWA permit violation	1.266	(0.250)**	1.250	(0.249)**	1.271	(0.207)**
CWA pretreatment violation	1.066	(0.279)**	1.036	(0.268)**	0.865	(0.215)**
CWA other violation	0.576	(0.296)	0.608	(0.296)*	0.632	(0.247)*
RCRA violation	1.414	(0.279)**	1.451	(0.278)**	1.464	(0.250)**
Administrative order violation	−0.536	(0.182)**	−0.499	(0.184)**	−0.532	(0.191)**
Failure to obtain permit	−0.359	(0.296)	−0.349	(0.297)	−0.363	(0.265)
Number of locations in case					0.606	(0.148)**
Constant	9.494	(0.296)**	9.442	(0.296)**	8.844	(0.289)**
Adjusted R^2	0.181**		0.172**		0.183**	
N = cases in populated areas	640		640		610	

** Statistically significant at the 0.01 level.
* Statistically significant at the 0.05 level.

the Per Location and Per Capita Models; however, they are in the Combined Area and Single-Location Models. In addition, the number of locations involved in the case is statistically significant in the Combined Area Model and has a substantial impact upon penalties. For example, a $40,000 penalty in a single-location case would be $73,356 if there were two locations, $134,528 for three locations, and $246,711 for four locations. In virtually all other respects, the models performed comparably, and the independent variables had very similar coefficients. Consequently, whether and how one incorporates multilocation cases into a regression model appears to make little difference in assessing the impacts of most variables on penalties.

Although all of the regression models developed during this research performed similarly, explaining highly statistically significant amounts of the variation in penalties, it is important to note that all of them explained only a modest amount of that variation. The R^2s ranged only from 0.16 to 0.18, which is from 0.02 higher to 0.20 lower than the models used by Ringquist, though his used several more independent variables. Thus, most of the factors that influence penalties are not captured in these models.[26] This result should not be surprising because many of the factors that would be expected to influence penalties were unavailable, e.g., the severity of the offense, SEPs, the number of violations, the days of noncompliance, the extent to which the defendants exhibited good faith in their efforts to comply, prior violations by the defendants, whether the desired penalty was reduced because of the defendants' inability to pay, and recalcitrance by the defendants. According to EPA's penalty policies and environmental statutes, some or all of these factors are to be taken into account in setting penalties. Yet none of these are in DOCKET, nor are they otherwise realistically available. Even a review of the small number of published judicial opinions in which the judges explained how they set the penalties they imposed in litigated cases reveals the mix of factors that are considered and how their impacts vary.

Consequently, it is highly unlikely that, in the absence of more complete information, any analysis can substantially specify what determines penalties. None of the model results, however, provide any basis for concluding that penalties are lower in disproportionately minority or low-income areas. These model results do appear to demonstrate that it is the characteristics of the case, rather than of the area in which a violation occurs, that are more important in determining the penalty imposed. Those characteristics are the model variables that had the greatest im-

[26] In addition, all of the models failed the Ramsey (1969) test for omitted variables. Naturally, while this may have affected the coefficients of the variables, it simply reinforces the finding that the overall explanatory power of the models was low, which renders the particular values of the coefficients less important anyway.

pacts on penalties. The variables that are not incorporated in the models would primarily be other characteristics of the cases. The type of violation, how and when the case was concluded, whether the case involved multiple locations, and whether the defendant was a public entity are the key factors in the models affecting penalties. With the arguable exception of the latter variable, these are factors that should be determining penalties. (If environmental equity is supposed to promote equity in penalties, one could argue that business and public defendants should be penalized equally.) There is no indication from these analyses that inappropriate factors, such as the demographic characteristics of violation locations, substantially affected penalties.

Conclusions

The development and implementation of environmental policy has proven to be a sensitive and potentially inflammatory process, partly because it inherently addresses activities posing possibly serious and irreparable harm to the environment and public health. People are rightly concerned about threats from contaminants they may not even be able to sense, and about what their government is doing to protect them. Although environmental equity, in theory, may be an unassailable objective, it inherently introduces into environmental policy the issues of racism and class discrimination. Consequently, if analyzed and used inappropriately, environmental equity runs the risk of simply adding fuel to the fire consuming rational environmental policymaking.

As the analyses in this article have demonstrated, there is no evidence that violations of environmental laws in areas that are disproportionately minority or low-income tend to be penalized less than violations elsewhere. Although I have used a full range of analytic methods to verify the results of *NLJ*'s research, none of the results I obtained demonstrated any meaningful relationship between income characteristics and civil judicial penalties. With respect to race characteristics, my analyses consistently revealed a modest relationship with penalties, but it was in the opposite direction than that claimed by *NLJ*. Penalties tended to be higher as the presence of minorities increased. The factors that influenced penalties the most, though, were the characteristics of the cases. Nevertheless, given how much of the variation in penalties was left unexplained by the regression models presented here, it would not be surprising if the finding of discrimination against whites was spurious and would disappear if data on other relevant variables were available. Because most of these missing variables are other characteristics of the cases, however, this simply underscores the influence of the facts of the case in setting penalties. Though the inclusion of other variables might affect the coefficients of the existing variables, they cannot improve the

amount of variation in penalties explained only by the latter. Thus, on the basis of these analyses, variations in EPA civil judicial penalties are primarily due not to discrimination, but to other factors.

Aside from my presenting the findings of these analyses in this article, I have attempted to contribute to an improved understanding of the existing research in this area and to better research in the future by my thorough evaluation of the previous studies on this topic. All too often, especially concerning environmental equity subjects, policymakers are confronted with dueling studies that reached conflicting conclusions, making good policy decisions far more difficult. The problems I have identified with these earlier studies should make the following three different, but interdependent, constituencies more aware of their obligations in the policy analysis process.

First, EPA needs to be more aggressive in its efforts to adequately inform users of its data regarding how these data were gathered and what they measure. EPA has performed admirably in making its data easily available to the public. However, that public service can be counterproductive if the users are unclear about what the data represent. If EPA can make the data available, it should require little additional effort, especially in regard to the small amount of information in DOCKET, for EPA to adequately explain where they come from, what they represent, and what data-quality issues exist. In the case of DOCKET, EPA has never, at least publicly, pointed out the serious misunderstandings of the data that may occur and that are evident in the earlier research that uses them. Assuming that EPA was aware of these misunderstandings in previous research (and it would pose even more troubling questions if it were not), quickly issuing public statements about these errors, especially those in the *NLJ* studies, could have prevented years of others' unwarranted reliance upon them and an unfair maligning of EPA's enforcement efforts. Also, given the many errors that my research found in DOCKET, EPA should make greater efforts to ensure data quality. Though these and other EPA data may originally have been gathered purely for internal purposes—and thus EPA staff may have understood the data and their limitations—their public availability now has made it too easy for misinterpretations to occur.

Second, regardless of how well EPA fulfills its responsibilities, researchers must ensure that they understand the data they are using and the substantive context that give those data meaning. Given the often complex nature of the legal reporting requirements that produce most EPA data, it is not difficult for researchers to define data erroneously for their studies. The earlier studies seriously misinterpreted DOCKET data and evidenced researchers' unfamiliarity with the context of civil judicial actions, leading to errors that made their findings unreliable. This

squandered considerable efforts by those researchers, and could have been avoided. Particularly when one is researching environmental equity issues, when the results may determine whether certain people or organizations are branded as discriminatory, it is important to avoid errors that can permanently taint others' reputations. Unfortunately, the inflammatory conclusions of *NLJ*'s articles have survived long after most people's willingness to thoroughly examine them.

Finally, the reviewers and consumers of research must be more vigilant in deciding what studies to accept. Naturally, because *NLJ* published its own results, there were few safeguards to prevent its authors from making errors. There were, however, far too many people who appeared too willing to accept *NLJ*'s conclusions without seriously examining or questioning *NLJ*'s methodology. Obviously, EPA—who presumably should have had a vested interest in rebutting *NLJ*'s conclusions—also did not make aggressive efforts to do so. Thus, there was a general failure of review in this area, though with *NLJ*'s study it understandably could only have come after the fact. If all of these constituencies—EPA, researchers, and reviewers and consumers—increase their efforts in the future, more accurate and useful research can be done.

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