Safe and Sound Judgment

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Many points raised by Dr. Evan J. Ringquist's response in this issue already are addressed in my article, so readers should simply review its relevant portions. Just as he indicated that space constraints limited his response, I am limited to focusing on his comments that are less fully addressed in my article.

Role of Judges

Starting where Dr. Ringquist's response ended, I too am concerned about "the professional reputation[s] of" (2001:697) authors, not only his and mine, but also of other researchers in this field. They need to be aware of concerns about studies' methodologies that they might emulate, so they can make informed decisions about how to proceed. Furthermore, I am concerned about the professional reputations of the people who are the primary topics of Dr. Ringquist's research—judges. Dr. Ringquist (1998) hypothesized that judges' decisions were biased due to race, class, politics, and ideology, but, in fact, as my article demonstrates, judges do not even make those decisions.

Obviously the key theoretical difference between his views and mine is whether judges decide penalties in U.S. Environmental Protection Agency (EPA) civil judicial cases. Dr. Ringquist now acknowledges that judges have a limited role, because almost all such cases are settled rather than litigated and decided by judges. While he claimed that this is obvious in his articles (2001:695), only four times therein did he mention that the EPA might have a role in determining penalties (Ringquist 1998:1157; Ringquist & Emmert 1999:12, 24). In contrast, he stated several dozens of times that judges levied, imposed, assessed, or otherwise determined penalties. Dr. Ringquist appears to defend using judicial characteristics as causal variables by somewhat agreeing with my statement that "the court must not rubberstamp the agreement, but also must not substitute its own judgment for that

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of the parties to the decree" (Atlas 2001:646; Ringquist 2001:695). However, this quotation should not be taken out of context to suggest that I believe judges have a significant role in settled cases.

It is a legal fact that judges can only accept or reject proposed consent decrees in settled cases, and cannot impose penalties themselves. It is an empirical fact that judges virtually never reject proposed consent decrees. Thus, the penalties in settled cases, which compose more than 90% of all EPA civil judicial cases, are those agreed to by the EPA and by the defendants. If Dr. Ringquist wants to hypothesize that the characteristics of judges affected the penalties that the litigants agreed to in settled cases, he must provide a theory explaining how judges successfully pressure litigants to propose penalties to which they would not otherwise agree. Thus, his theory fails primarily because it provides no link between judicial characteristics and litigants' actions—a daunting task because a judge's power is severely limited to influence consent decrees. A judge must defer to the litigants' desire to settle; can only reject a consent decree if it is unfair, unreasonable, or inadequate; and can have his or her rejection overturned on appeal. It is difficult to imagine why litigants would feel susceptible to judicial pressure; thus, the characteristics of the judge would be irrelevant.

Consequently, references to studies examining the effects of judges' characteristics and social environment on such decisions are irrelevant, because judges do not make these decisions. Dr. Ringquist cited two other works, McSpadden (1997) and Kubasek and Silverman (2000), as specifically supporting his claim that judges affect penalties in consent decrees (Ringquist 2001:685). However, the McSpadden text, from a section titled "Court Oversight of Administrative Discretion," pertains explicitly to judicial involvement in legal challenges by others against EPA regulations and administrative decisions, not to enforcement actions against others by the EPA. Similarly, the Kubasek and Silverman text, from an elementary environmental law primer, says nothing about EPA enforcement actions. Thus, these works do not support Dr. Ringquist's assertion that judges affect penalties in consent decrees.

Despite the lack of evidence to support his theory, Dr. Ringquist claimed the relationships he hypothesized are correct because such relationships are sometimes statistically significant in his regression models (Ringquist 2001:688). I remain unconvinced, however, that relationships in models in which 64% to 84% of the variation in the dependent variable remains unexplained confirm the theoretical interpretation Dr. Ringquist offered. Sometimes relationships that initially are statistically significant vanish once the independent variables that explain most of

the variation in the dependent variable are included in analyses.[1] In addition, while Dr. Ringquist claimed that "in every case the empirical evidence supports the inclusion of these control variables" (2001:689), many of these variables' coefficients differ in direction and/or statistical significance among his regression models. Furthermore, our disagreement over several variables is not whether these coefficients should be statistically significant or in what direction, but rather what those variables actually measure and, thus, what conclusions should be drawn from the results. The mere fact that a variable is sometimes statistically significant does not prove that one's interpretation of it is correct.

Similarity of Our Research

Dr. Ringquist essentially claimed that I modeled my research after his, without giving him appropriate credit. In fact, by November 1996—more than two years before his article—I already had an initial version of my article. Aside from presenting it before faculty members at my university, I submitted it as a public comment to the December 1996 National Environmental Justice Advisory Council public hearing. I also presented this research in November 1997 at the conferences of the Association for Public Policy Analysis and Management and the Northeastern Political Science Association, respectively. Both the structure of my paper and the types of variables used have remained essentially the same since 1996; therefore, the similarities that Dr. Ringquist saw between his article and mine do not reflect my unacknowledged intellectual debt to him.

Dr. Ringquist also stated that his 1998 article "highlight[s] almost all of the problems with the [*National Law Journal*] piece that [I] also highlight in" "Rush to Judgment" (2001:693). This is incorrect. The first problem that he raised was that it was "curious" that the *National Law Journal* (*NLJ*) only examined cases from 1985 to 1991 (Ringquist 1998:1151). I did not identify this as a problem; rather, as note 4 of my article indicated, Dr. Ringquist did not correct an *NLJ* error, he created one that *NLJ*'s methodology avoided (Atlas 2001:641).

Second, Dr. Ringquist stated that, unlike *NLJ*, he excluded Comprehensive Environmental Response, Compensation, and Liability Act (CERCLA) cost recovery cases from his analyses (1998:1151). Yet, the *NLJ* study expressly stated that it excluded cases that "were designed to recover the costs of Superfund cleanup rather than penalize violators" (Lavelle & Coyle

[1] Ironically, while Dr. Ringquist criticized me for not including variables in my regression models that were statistically significant in his (Ringquist 2001:688), two highly statistically significant variables—minority percentage and multiple location cases—in his Ringquist (1998) regression models were not in the regression models in the article that he simultaneously coauthored (Ringquist & Emmert 1999).

1992:S4). Therefore, his criticism of *NLJ* is unwarranted. In "Rush to Judgment" I said that I found and excluded some CERCLA cases that *NLJ* might have overlooked, but I did not criticize *NLJ*'s handling of these cases.

Third, Dr. Ringquist criticized *NLJ* for not including control variables in its analyses (1998:1151–52)—a point I also made (Atlas 2001:640). As part of that criticism, however, he argued that one necessary control is using the average penalty per violation in a case as the dependent variable, rather than using the total penalty for all violations in the case. As I explained in my article, the information he used for the number of violations in a case was incorrect. Thus, figuratively speaking, his cure is presumably worse than the disease, and, needless to say, I did not follow his procedure.

Fourth, Dr. Ringquist criticized *NLJ* for not using tests of statistical significance in its analyses. I made no such criticism, because *NLJ* used essentially the entire population of cases, not a sample thereof for which statistical significance tests are relevant (Lavelle & Coyle 1992:S4). As I stated in note 22 of my article, I agree with *NLJ*'s approach (2001:663–64). Therefore, my article shares only one of the five criticisms that Dr. Ringquist made of the *NLJ* study, and I essentially side with the *NLJ* study on the other four concerns.

With respect to the problems in the *NLJ* study that my article identified, I noted that *NLJ* apparently treated each multilocation case as a group of individual cases. While Dr. Ringquist realized that multilocation cases existed, he did not criticize how *NLJ* used such cases (1998:1158), presumably because he did not perceive *NLJ*'s error. Second, I stated that *NLJ* categorized Hispanic-origin whites as whites, not minorities as I did (Atlas 2001:638). Dr. Ringquist also apparently categorized this group as minorities, but did not find fault with *NLJ*'s categorization (1998:1153). Third, I described several reasons why a case's penalty might not reflect the severity of the defendant's punishment (Atlas 2001:639–40). Dr. Ringquist mentioned only one such reason—the penalty's inclusion of the economic benefit to the defendant of noncompliance (1998:1157–58)—and I disagree with his method of trying to control for it. Fourth, I criticized *NLJ*'s use of zip codes to define the community around violators (Atlas 2001:640). In contrast, not only did Dr. Ringquist use zip codes, he, unlike *NLJ*, apparently did not identify and correct errors associated with using this method. Fifth, Dr. Ringquist and I agree that the *NLJ* study lacked control variables (Ringquist 2001:693; Atlas 2001:640); therefore, Dr. Ringquist's article shared only one of the five criticisms I made of the *NLJ* study.

Consequently, it is incorrect to claim that "[t]he similarities between the two pieces, however, are striking" (Ringquist 2001:693). Dr. Ringquist also noted that our respective depen-

dent variables were similar, as my penalty amount "excludes all cost recovery awards, uses constant dollars, and takes the natural log of penalties, none of which were done by *NLJ*, but all of which were done in Ringquist (1998)" (2001:693–94). However, I included all such variables beginning with the 1996 version of my paper. Furthermore, Dr. Ringquist never mentioned in his 1998 article that he transformed his penalty amounts into constant dollars. Finally, taking the natural log of the dependent variable is a standard technique when that variable is highly skewed. Therefore, Dr. Ringquist's suspicions about these similarities, once again, are baseless.

Large Company Variable

My article stated that Dr. Ringquist (1998) inappropriately operationalized the dummy variable indicating whether a defendant was a Fortune 500 company, because other defendants included both smaller companies and government entities (Atlas 2001:652). Dr. Ringquist said my criticism is invalid because he differentiated between these other defendants by including a dummy variable for the latter (2001:692). As I said in my article, I was aware that he used the latter dummy variable, but he never said the government defendant and the Fortune 500 dummy variables were linked, such that small businesses were the reference group.[2] When he used a dummy variable reference group elsewhere, he always stated this explicitly (Ringquist 1998:1157, 1158, 1159; Ringquist & Emmert 1999:17, 22). He did not do so for small businesses, and, in fact, he always separately discussed both the creation and results of the Fortune 500 and the government entity dummy variables (Ringquist 1998:1158, 1160; Ringquist & Emmert 1999:18, 19, 24, 26, 30). Thus, only examining his data could resolve this issue.

Number of Violations

Dr. Ringquist said that when I criticized his research for using "the number of counts brought against the defendant" to reflect the number of violations in the case, I mischaracterized his intentions (Ringquist 2001:695). He agreed that DOCKET's list of statutory sections violated in a case does not reflect the number of individual violations involved. However, he stated that when he used the word "counts," he meant the number of individual violations involved, and when he used the word "violations," he meant the number of statutory sections violated. First, his articles did not make this distinction, and, in fact, he appears to have used

[2] Dr. Ringquist claimed this "was pointed out repeatedly in prepublication reviews" of my article (2001:693). One reviewer mentioned that my criticism was incorrect, but did not explain why.

the terms "counts" and "violations" synonymously (Ringquist 1998:1151, 1157; Ringquist & Emmert 1999:18). Second, I quoted the text that I did simply because it was how he stated his hypothesis; I am not responsible for his use of what he considers an incorrect word. Third, I am unaware that there is a distinction between the terms "counts" and "violations," though Dr. Ringquist is entitled to his own terminology.

Even leaving aside his intent, his use of the supposed number of statutory sections violated was inappropriate for reasons described in my article. One of those reasons, which Dr. Ringquist apparently rejects (2001:695–96), is that "sometimes the statutory section cited was simply the section generally authorizing penalties for violations, rather than a section embodying a substantive requirement" (Atlas 2001:651). I further examined the statutory sections cited in the cases gathered for my research. For cases in which more than one statutory section was listed in DOCKET, 33.9% of the sections listed were the Clean Water Act (CWA) §309. This section simply authorizes the EPA to impose penalties for CWA violations and is not itself a substantive requirement (i.e., one cannot violate CWA §309). Thus, this statutory citation merely indicates the authority under which the EPA sought penalties for violations of another CWA statutory section also listed in DOCKET for that case. Another 12.7% of all multiple statutory sections listed in DOCKET were for similar provisions in the Clean Air Act (CAA) or the Resource Conservation and Recovery Act (RCRA). Also, some of the statutory sections listed in DOCKET were for sections that did not exist or that could not have been the basis for a violation (e.g., laws imposing obligations only on the EPA). Thus, most statutory sections that Dr. Ringquist would have counted as multiple violations were not violations at all.

Comparisons of Regression Models

Dr. Ringquist frequently tried to rebut my concerns about the quality of his data and the appropriateness of his variables by comparing our regression models. For example, he stated that my "models explain only half as much of the variation in civil penalties as does the comparable model in Ringquist (1998)" (2001:689). His model that he considers comparable—the results of which are in the first column of his Table 2 (Ringquist 1998:1161)—is, however, very different from mine. Most obviously, all of his dependent variables are different from mine. I used the logged total penalty in a case, whereas he used the unlogged total penalty in a case or the logged penalty per violation in a case. Although he considered the latter comparable to mine, the former may actually be more comparable, especially because, as I have described, the latter was based on an erroneous mea-

sure of violations. Thus, his most comparable model, in terms of a dependent variable, outperforms mine in R^2 by only 0.07, despite his use of three additional independent variables (or seven additional variables, if one does not include my four quadratic term variables).

Aside from the different dependent variables and other concerns raised in my article, our data are not comparable. Dr. Ringquist responded to all but two concerns that I raised about his research, one of which is how he analyzed multilocation cases. As my article stated, these can substantially affect analyses. Of particular concern is the differing number of multilocation cases that we identified. According to Dr. Ringquist, he found "about two dozen" cases from 1974 to 1991 (1998:1158), while I found 68 cases just from 1985 to 1991 (Atlas 2001:660). I correctly identified these cases by reviewing the cases' consent decrees. My concern is that Dr. Ringquist might have included in his analyses many more multilocation cases, but unknowingly treated each location therein as a single-location case. I found that treating each location in a multilocation case as a separate case, rather than as one case with a cluster of locations, approximately doubles the R^2. This is not surprising, because treating each location as a separate case essentially creates many duplicate records with identical dependent and independent variable values.

Even ignoring the lack of comparability of our regression models, Dr. Ringquist claimed that a "comparison shows identical conclusions for each hypothesis that the studies have in common" (2001:690). This is incorrect. A comparison of my Table 7 models to his model that he claimed was most comparable to mine shows various discrepancies. The minority percentage in his model was statistically significant only at the 0.10 level, with a coefficient of essentially zero; in contrast, this variable's coefficient was statistically significant and substantial in all of my models. Being a government entity had no statistically significant effect in his model, but it did in mine. Published cases (all of which were litigated cases) were associated with higher penalties in his model, but litigated cases produced lower penalties in mine. CAA cases produced lower penalties than RCRA cases in his model, but only approximately half the time in mine; thus, our model results agreed for approximately half of the variables.

Published Cases

Dr. Ringquist responded to my statement that "whether a case was published actually measured whether a case ended through litigation rather than settlement"[3] (Atlas 2001:648) by first arguing that his 1999 article "clearly show[s] that the num-

[3] Dr. Ringquist claimed that he raised this issue in his reviews of my article, but that I ignored it (2001:693). Peer reviewers' comments were anonymous, and thus I had no

ber of published cases is far smaller than the number of cases litigated to a conclusion" (Ringquist 2001:693). Although that article stated that there were 56 published litigated cases, it did not reveal the total number of litigated cases. However, his argument did raise new concerns. We both agree that litigated cases are a small portion of EPA civil judicial cases. Dr. Ringquist stated that 10% of such cases are not settled (Ringquist & Emmert 1999:10), and my review of DOCKET data found records for approximately 140 litigated cases of the kind that he included (about 50 of which were default judgments, rather than judge-decided penalties). However, half of these cases produced no penalties. Like *NLJ*, I excluded cases in which no penalties were imposed. In litigated cases, no penalty would presumably reflect a judge's finding that the defendant was not guilty, which should not be compared to cases in which defendants were found guilty and penalized. Thus, a zero penalty reflects a judge's decision about the defendant's guilt, not the severity of the violation. Consequently, Dr. Ringquist could have found only approximately 70 litigated cases with penalties, even assuming all such cases were in his sample. This number is consistent with his statement that 10% of all cases are litigated, which indicates that approximately 72 cases in his sample of 720 were litigated. Thus, he either inappropriately included zero penalty cases in his analyses or incorrectly claimed that his 56 published litigated cases composed only a small portion of all litigated cases.

Prior Offenses

Dr. Ringquist claimed he never intended to suggest that his variable of the number of prior offenses by a defendant measured anything more than what my article stated that it measured (Ringquist 2001:696)—the number of prior EPA civil judicial actions against that defendant (Atlas 2001:650). However, his articles described this variable as "representing the number of times a particular defendant has been penalized for regulatory violations in a particular district" (Ringquist 1998:1158), and as "representing the number of times a particular defendant has been penalized for violating an environmental statute in a particular court district" (Ringquist & Emmert 1999:18). I believe most people would not perceive the distinction made by Dr. Ringquist and would interpret it as I did. Even leaving this aside, his use of this variable was inappropriate for the reasons described in my article.

way of knowing if Dr. Ringquist was among the reviewers. Regardless, no review that I received mentioned this issue.

Violation Dummy Variables

Dr. Ringquist questioned the reference group that I used for the dummy variables indicating the type of violation involved in a case (Ringquist 2001:694). Unlike Dr. Ringquist, my analyses were not limited to CWA, CAA, and RCRA cases. Thus, the types of violations that I reported in Table 1, and which I used as dummy variables, were "the most common types of violations used in my analyses" (Atlas 2001:656). The reference group was composed of other types of violations.

Data Quality

Dr. Ringquist described his efforts to better ascertain the extent of errors in establishing locations of facilities used in his analyses. These efforts did not address my primary concerns about using zip codes to represent communities around violation locations. Regardless of his certainty that the DOCKET zip codes are correct for the facilities, I disagree that zip codes, which, on average, cover 40 square miles, reflect what environmental enforcement staff would think of when—and if—they took community characteristics into account in setting penalties. Also, based on my review of consent decrees, the facility listed in DOCKET might not be the location of the violation. Thus, Dr. Ringquist might have simply confirmed the correct zip code for an incorrect location.

Dr. Ringquist also noted potential inaccuracies in the latitude and longitude coordinates for violation locations that I might have used in my analyses (Ringquist 2001:691). First, he stated that 9 of 100 facilities' EPA coordinates he sampled had values to only two decimal places, making them less accurate (Ringquist 2001:691). However, only 2% of my violation location coordinates had such a concern, which would produce an error in latitude or longitude of no more than 0.7 miles. Second, Dr. Ringquist said that 10 of his sampled facilities had no accuracy value in the EPA's database for their coordinates. He then assumed that the absence of this information demonstrated that the coordinates were in error by at least a mile. Obviously, it instead indicated that this information was unavailable.

References

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