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## The Need for Sound Judgment in Analyzing U.S. Environmental Protection Agency Enforcement Actions

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I agree completely with most of the issues raised in Mark Atlas's article "Rush to Judgment" in this issue (Atlas 2001). First, environmental justice is an important research topic that is too often addressed by writers with preconceived notions about the presence or absence of discrimination in environmental protection. Second, I agree with Mark's critique of the *National Law Journal* (NLJ) study, the first to investigate possible racial or class inequities in civil environmental penalties. Third, I agree that researchers need to use appropriate data, controls, and statistical techniques and that there is no evidence that civil penalties for violating environmental regulations are systematically lower in poor and/or minority communities. Finally, I agree that in "Rush to Judgment" Atlas makes a unique and valuable contribution to the environmental justice literature—which is why I recommended that the manuscript be published in *Law & Society Review*. Nevertheless, I believe that "Rush to Judgment" has substantial weaknesses that detract from its stature as a piece of social science research. Not surprisingly, I disagree with Mark's assessment of the validity of the conclusions from my previous work in this area—conclusions that "Rush to Judgment" reinforces in every respect. More important, however, I believe that Mark's article displays fundamental flaws regarding how we use evidence to draw generalizable conclusions about the world around us, and how one makes use of extant research to carve out a niche for one's own work.

### Clarifying the Evaluative Standard

The claim Mark makes in "Rush to Judgment" is that substantive misunderstandings and incorrect methodological choices render my previous work regarding environmental civil penalties

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“unreliable” or “per se unreliable” (Atlas 2001:643, 651). Reliability is an unusual platform from which to critique the work of others, since it is a property of measures and measurement rather than a property of research designs, models, or conclusions. Reliability is generally thought of as the ability of a measure or test to produce the same value in repeated trials under various circumstances. Using this standard, the conclusions in Ringquist (1998) are consistent across sampling frames, three measures of the dependent variable, and dozens of different models. Thus, the results in my previous research are nothing if not “reliable” in the traditional sense of the term.

The issue at the heart of Mark’s critique is not reliability, but validity—in particular, internal validity. In all prepublication versions of “Rush to Judgment,” the term validity was used. After I pointed out in my review of the manuscript that conclusions regarding the internal validity of my research need not follow logically from the critique offered, Mark replaced “invalid” with “unreliable,” but the critiques and their associated conclusions remained unchanged.<sup>1</sup> But let’s be clear. At issue is the assertion that the results of my previous research cannot be trusted—a classic definition of internal validity. I am confident that both the critiques of the work, and the conclusions following from these critiques, are in error, and I make my case to this effect in the following pages by addressing (1) Mark’s criticism of the models representing civil penalty assessment in environmental protection, (2) his criticisms of data accuracy, and (3) errors of omission and commission found in “Rush to Judgment.”

## **Modeling Civil Penalties in Environmental Protection**

### **Theoretical Refutation of the Modeling Critique**

The lion’s share of the criticism in “Rush to Judgment” centers on the inclusion of a group of control variables in Ringquist 1998. In a critique longer than the text of my original research note, each control used in Ringquist 1988 but not in “Rush to Judgment” is argued to be inappropriate. While space constraints limit my response to the five controls that attract the most criticism, these refutations are generalizable to the other control variables.

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<sup>1</sup> Atlas’s critique of my previous work that is found in “Rush to Judgment” is a classic example of what Fisher (1970) calls “the fallacist’s fallacy.” This fallacy is composed of several logically false statements, the most relevant of which are (1) an argument that is structurally fallacious in some respect is therefore structurally false in all respects; and (2) an argument that is structurally false in some respects, or even in every respect, is therefore substantively false in its conclusions. I hope to show, however, that even the structurally false premise at the heart of the fallacist’s fallacy is an inappropriate characterization of my previous work.

According to Mark Atlas, the most serious problem with the models in Ringquist (1998) is the assumption that judges might affect civil penalties in environmental protection. If his summarization of my previous work were accurate (later I show it is not), the single largest problem in Ringquist 1998 is the inclusion of a nonsignificant control variable—hardly a major threat to the validity of the conclusions reached in any statistical model. “Rush to Judgment” claims that there is no basis for considering the effects of judges on penalties. I disagree. First, the importance of judicial preferences, most commonly articulated through a measure of judicial partisanship, is a standard predictor in models of judicial outcomes. Judicial preferences are important predictors of outcomes in the U.S. Supreme Court (Segal & Spaeth 1993), U.S. Courts of Appeals (Songer et al. 2000), U.S. District Courts (Rowland & Carp 1996), and State Supreme Courts (Brace & Hall 1993). Also, meta-analysis demonstrates that the single most important predictor across scores of models of judicial outcomes is the ideology of the judge (Pinello 1999).

Second, environmental protection is a salient policy issue in which the two main political parties have clearly divergent positions (Kamieniecki 1995); and where the partisan affiliation of judges is an important predictor of case outcomes (Kovacic 1991). Finally, there is ample reason to expect that judges may affect civil penalties even in cases settled by consent decrees. When discussing the role of judges in environmental law, Kubasek and Silverman state that “the careful litigant will always try to know something about the judge’s ideology before bringing a case to trial. Certain courts earn reputations as being more pro- or anti-environment, and these reputations influence the types of cases brought to them” (2000:54). Furthermore, “if the judge finds that questions of fact do exist, he or she usually holds a pre-trial conference [where] the judge [and] the lawyers representing the parties . . . try to narrow the legal and factual issues and to work out a settlement if possible” (Kubasek & Silverman 2000:66). In addition, Lettie McSpadden—one of the leading scholars of environmental law, with firsthand experience at EPA—writes

For the period preceding the court’s verdict as well, the judge’s role has greatly expanded. Judges today often act as intermediaries, bringing opposing parties into their chambers to work out a compromise before the case reaches trial. In so doing, judges’ discretion and influence over policy are broadened greatly. The judges’ informal role of mediator has meant that the number of cases going to trial has declined. Many cases are settled by the parties and their attorneys, often with judicial encouragement (1997:170).

This evidence makes it difficult to argue that judges cannot affect penalties in environmental cases.

The inclusion of control variables for published cases, the history of the defendant, Department of Justice (DOJ) headquarters attorneys, and the seriousness of the offense is similarly supported by previous research in this area. First, Atkins (1992), Olson (1992), Siegelman and Donohue (1990), and Songer (1988), to name only a few, suggest that there may be significant differences between published and unpublished cases. Second, EPA's own guidelines recommend that repeat offenders be assessed larger penalties for violating environmental regulations—a position perfectly consistent with penalty determination in other areas of civil and criminal law. Third, although Environmental Protection Agency (EPA) and DOJ attorneys work as a team in environmental civil cases, statute and memoranda of understanding clearly state that the DOJ attorney is the lead attorney in these cases—a situation that has caused some friction between the two agencies (Cushman 1992). Regardless of whether one characterizes the role of EPA attorneys as members of a team or “assistants” (I use the former term, contrary to what is argued in “Rush to Judgment”), the fact remains that the influence of EPA in these cases is a constant. The most important area of variation with respect to attorneys, then, is the distinction between specialists at DOJ headquarters that self-select into environmental protection cases and generalist district attorneys. Both the repeat player hypothesis from judicial behavior and the agency mission hypothesis from public administration suggest that, all other things being equal, penalties should be larger under the former.

Finally, arguing that the number of sections of the law violated must be unrelated to penalty severity relies upon the remarkable claim that the number of laws you break is unrelated to the severity of the offense.

Mark Atlas claims that the inclusion of these control variables reflects fundamental misunderstandings regarding civil procedure in environmental protection and the nature of the data found in DOCKET. On the contrary, given the support for the inclusion of these control variables provided here and in my earlier research, it seems that the greater error would be not including these control variables in models of penalty severity. Of course, one can posit hypothetical scenarios in which each of the criticisms offered in “Rush to Judgment” is accurate. Moreover, I expect that one could find cases in which civil penalties were unrelated to judges' ideology, published opinions, defendant histories, the presence of a DOJ attorney, or the seriousness of the offense. However, we do not reject hypotheses on the basis of hypothetical scenarios or individual cases. In fact, hypotheses in social science are not required to be true for all cases. Our hypotheses, and the statistical techniques we use to test them, are based upon the assumption that *the hypothesis is true more often than not, ceteris paribus*. Furthermore, my argument is not that these

factors *must* affect the size of civil penalties in environmental protection, only that, based upon previous research and evidence, they *may* affect these penalties, and that these influences must be controlled for if we are to get a clear indication of the relationship among factors of race, class, and the size of civil penalties in environmental protection.

### **Statistical Refutation of the Modeling Critique**

Reasonable people can disagree over which control variables to include in statistical models. I assume that the factors deemed important by previous research might be important in this area, though in “Rush to Judgment” Mark assumes these factors are irrelevant. Modeling decisions reflecting these two positions are essentially choices between (A) including irrelevant predictors, and (B) excluding relevant predictors. Thus, it is useful to examine the statistical implications of both types of specification error. Under A, parameter estimates will be unbiased and consistent, but inefficient, and this inefficiency will be directly related to the ratio of  $K/N$  and the covariance of the irrelevant and relevant predictors (Berry & Feldman 1985; Gujarati 1995). In Ringquist (1998),  $K$  is at most 27 and  $N$  is between 531 and 751, so this potential inefficiency poses little threat. Moreover, if the included predictors are really irrelevant, the expected covariance with other included variables and the dependent variable will be close to zero. In short, even if the control variables I include are irrelevant, it is highly unlikely that this specification error compromises the validity of my conclusions. Under specification error B, however, the situation is much worse. Unless the excluded relevant predictors are independent of the included relevant predictors (a heroic assumption that is demonstrably false in this case), the error term from these models will be correlated with the included predictors, which renders the parameter estimates on these variables both biased and inconsistent. Also, parameter standard errors will be biased in favor of rejecting the null hypothesis, and explained variation will be quite low (Berry & Feldman 1985; Gujarati 1995). *Hypothesis tests under these conditions are statistically invalid* (i.e., internal validity is compromised). Since excluding relevant predictors is a far more serious problem than is including irrelevant predictors, it would seem prudent to include these controls.

### **Empirical Refutation of the Modeling Critique**

One could easily forget that, with respect to the control variables in question, Mark and I offer identical null hypotheses that these variables will have no effect on the size of civil penalties in environmental protection. The critical difference between our

positions is that I test these null hypotheses, while Mark accepts them as axioms. We can settle most of the disagreements regarding the inclusion of these control variables by taking a look at the statistical results.

Without exception, *every control variable previously discussed has a significant parameter estimate in the hypothesized direction* (as do most of the other control variables), and these variables remain significant across dozens of different model specifications. In short, the claim that these variables are irrelevant in predicting the size of civil penalties in environmental protection flies in the face of overwhelming statistical evidence to the contrary. Though the tables in Ringquist (1998) employ nonsignificant presidential dummies rather than a party dummy for the judge of record, I used presidential dummies (ironically) because a reviewer insisted upon this operationalization, even though models using the party dummy are superior by all measures. I include a party dummy in auxiliary analyses, however, and clearly state that the coefficient for judges appointed by Republican presidents is negative and significant (p. 1162), a fact that Mark fails to mention. Moreover, in Ringquist and Emmert (1999) we clearly show that, after controlling for other factors, penalties in cases overseen by judges appointed by Democratic presidents are more than twice the size of penalties in cases heard by judges appointed by Republican presidents!<sup>2</sup>

To further assess the relative importance of the control variables included in “Rush to Judgment” and Ringquist (1998), I estimated a new model of civil penalties and examined the standardized regression coefficients for each control variable. For the variables operationalizing the controls (though not the specific statutory variables) in Mark’s article (i.e., the CWA, CAA, litigation, government defendant, and time), the absolute betas are 0.20, 0.17, 0.01, 0.07, and 0.13, respectively, with an average absolute beta of 0.116. For the controls excluded from “Rush to Judgment” but included in Ringquist 1998 (i.e., Republican presidential appointees, *Fortune* 500 companies, number of statutory violations, DOJ attorneys, published cases, and prior civil penalties), these betas are 0.25, 0.24, 0.15, 0.10, 0.17, and 0.15, respectively, with an average beta of 0.177. Thus, not only are the control variables that Mark excluded in “Rush to Judgment” statistically significant, on average they are better predictors of penalty severity than are the controls he included in his analysis. One cannot invalidate these results by ignoring them or through offering vague protestations that they are somehow “erroneous,” “spurious,” or “misinterpreted.” Such protestations are, frankly, unbelievable and unscientific. A more likely conclusion is that

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<sup>2</sup> This effect is especially noticeable when controlling for the level of environmental commitment by a state’s political elites through the use of an interaction term (see Ringquist & Emmert 1999).



the theories of judicial outcomes that generate these hypotheses apply in this setting.

Finally, the consequences of excluding these variables becomes apparent when we look at diagnostic statistics for the models in “Rush to Judgment” and Ringquist (1998). To begin with, Mark’s models fail Ramsey’s RESET test for model excluded variable bias in model specification (see footnote 27). In addition, although comparing  $R^2$  values across models is always hazardous, these figures are instructive. The models in “Rush to Judgment” that include multiple site cases all have average adjusted  $R^2$  values (which control for the number of predictors in the model) of 0.18. The adjusted  $R^2$  from the comparable model in Ringquist (1998; i.e., using cases from 1985 to 1991 and a logged dependent variable) is 0.36.

### **Summarizing Refutations of the Modeling Critique**

First, each of the control variables I include in Ringquist (1998) has substantial support from previous research, making the theoretical foundation of Mark’s criticisms suspect. Mark may wish for longer discussions regarding these hypotheses, but such discussions simply are not possible in a brief research note like Ringquist (1998). Second, the statistical implications of excluding these control variables from models of civil penalties in environmental protection are far more serious than the implications of including these controls. Third, in every case the empirical evidence supports the inclusion of these control variables. Finally, the models in “Rush to Judgment” suffer from significant excluded variable bias as a consequence of omitting these controls, and Mark’s models explain only half as much of the variation in civil penalties as does the comparable model in Ringquist (1998). The fact that, even under these conditions, the analyses in “Rush to Judgment” reach the same conclusions as I did in Ringquist (1998) regarding racial and class inequities in the size of judicial penalties is a testament to the robustness of this empirical finding.

### **The Accuracy of Data from DOCKET**

The inclusion of various control variables poses no threat to the conclusions in Ringquist (1998), but the validity of these conclusions may still fall prey to data errors. “Rush to Judgment” raises important questions regarding the accuracy of the data in DOCKET. First, information regarding penalties, case outcomes, specific statutory violations, and the like may be in error. Second, the facility location information in DOCKET may be incorrect (e.g., the record may indicate the location of the corporate headquarters rather than the violating facility). Finally, the facility lo-

cation information in DOCKET may be correct, but the zip code of the facility may have changed between the time the case was completed and the 1990 Census.

At one level, the notion of error is absolute—an indicator is either accurate, or it is not. For questions of internal validity, however, this dichotomous definition of error is inadequate. Threats to internal validity stem not from the existence of error but from the frequency and magnitude of the errors.<sup>3</sup> In no way am I advocating ignoring the issue of data error. More accurate data are *always* preferable to less accurate data. The relevant issue is not whether errors exist, but whether the errors are sufficiently numerous to compromise the internal validity of any research that relies upon DOCKET. This is an empirical question.

I have worked with EPA data for well over a decade and have worked with various environmental justice data sets for six years. In my experience, EPA data sets do contain errors, but the extent of the errors is often exaggerated. Moreover, we can get some indication of the threat that data errors pose for the study of the equity consequences of civil environmental penalties. Consider the first type of error noted previously. After a thorough and laudable effort to validate the information in DOCKET, Mark claims that this database contains “almost every conceivable type of error, omission, and inconsistency.” I think the effort to improve the accuracy of the data in DOCKET is one of the most valuable aspects of “Rush to Judgment.” But again, the issue is not the variety of errors in DOCKET, but their prevalence. I am confident that a large number of scholars would be interested in a comparison of the original and corrected DOCKET data that highlighted the extent of the errors. Since “Rush to Judgment” does not provide a comparison of the original and corrected data, the only way we have to assess the extent of the errors in DOCKET is to compare the results from analyses that used the original data (i.e., my results) with results obtained from the corrected data (i.e., Mark’s results). This comparison shows identical conclusions for each hypothesis that the studies have in common. In short, although DOCKET may contain a variety of the first sort of error, the frequency of errors is not sufficient to invalidate research using the uncorrected data.

What about errors regarding facility location stemming from the use of DOCKET? To assess the seriousness of this threat to inference, I used the SAS computer package to draw a random sample of 100 from the 751 DOCKET cases used in Ringquist

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<sup>3</sup> As an illustration, consider the measure of state government ideology created by Berry, Ringquist, Fording, and Hanson (1998). In calculating this measure, we used the ICPSR data set on the partisan distribution of state legislatures created by Walter Dean Burnham. We later found that this data set had dozens of errors. After correcting these errors, the measures calculated from the original and corrected data sets were correlated at 0.9953. Data set errors, though troubling, do not always pose threats to inference.



(1998). Following Mark's methodology, I obtained facility identification (ID) numbers from DOCKET and used the Envirofacts database to find the zip code for each facility. I also obtained the latitude and longitude coordinates for each facility, then used the MapLinx computer package to determine whether the latitude and longitude of the facility fell within the zip code identified in DOCKET.

Consistent with Mark's experience, the Envirofacts database contained no location information for nearly 30% of the 100 facilities in my sample. Subsequent conversations with EPA officials confirmed that most of these facility ID numbers belonged to facilities that were no longer in operation. Of the remaining 71 facilities, eight had incorrect zip codes in DOCKET. The zip codes for four of these facilities had changed between the time the case was concluded and 2001, though it is likely that for several of these facilities the zip code change occurred after the 1990 Census, and thus this alteration poses no threat to inference. Four other facilities simply had the wrong zip code listed, with the facilities occurring 0.2 miles to 4 miles outside of the zip code listed in DOCKET. Finally, only two facilities with incorrect zip codes were contained in the subset of cases concluded between 1985 and 1991 that serve as the basis for the analyses completed by *NLJ*, Ringquist (1998), and Atlas in "Rush to Judgment."

In addition to providing latitude and longitude for regulated facilities, the Envirofacts database includes a field indicating the accuracy of these coordinates. In my sample of 100 facilities, the values in this accuracy field range from  $\pm 2$  meters to  $\pm 11,000$  meters. One can get a second indication of the accuracy of the latitude and longitude coordinates in Envirofacts by examining the number of significant digits of these coordinates. Typically, coordinates in Envirofacts are carried to six decimal places. Many facilities, however, have their locations recorded far less accurately, and moving from six to two decimal places degrades the accuracy of the coordinates by roughly a mile. Using these two indicators, I found nine facilities for which the error associated with the latitude and longitude in Envirofacts was greater than the one-mile radius Mark employed. I also found ten facilities with no value in the accuracy field. Thus, my sample produced at least nine, and as many as 19, cases where the one-mile ring Atlas (2001) used most likely does not contain the facility in question because of the inaccuracy in the latitude and longitude coordinates—though only three of these cases were concluded between 1985 and 1991.

Almost all data sets contain error, and the data used in Ringquist 1998 and "Rush to Judgment" are no exception. While we must do all we can to root out these inaccuracies—and make no mistake, Mark does far more to root out inaccuracies in

DOCKET than I did—inaccuracies will remain. One must be careful, therefore, to distinguish *potential* threats to validity from *actual* threats, and to not assume that data errors automatically generate the latter. Both DOCKET and Envirofacts contain inaccurate location information for a small number of facilities, but the data for the post-1984 cases that serve as the basis for the analysis in Ringquist 1998 and “Rush to Judgment” appear to be of better quality than the earlier data, as Mark suggests. Moreover, it is highly unlikely that the small number of location errors in DOCKET and Envirofacts invalidate the conclusions from either study. This position is supported by a re-examination of influence diagnostics, first reported in Ringquist 1998, which shows that cases with incorrect location information do not substantially affect parameter estimates.

### **Errors of Commission and Omission in “Rush to Judgment”**

Some of the most troubling elements in Atlas (2001) are the errors of commission, omission, and misrepresentation found in it. Space constraints prevent me from addressing all of these errors in detail; however, I will highlight some of the most egregious examples to illustrate my point.

#### **Errors of Commission**

Some of Mark Atlas’s criticisms of the control variables that I used in Ringquist (1998) are theoretically and statistically inadequate (see previous remarks), while others rely upon misrepresentations of my argument (see later). Mark’s critiques of at least two control variables, however, are simply wrong. First, Atlas argues that the positive and significant coefficient for the *Fortune* 500 variable is uninterpretable because “the remaining defendants [i.e., the reference category] are composed not only of smaller businesses but also of government entities . . . . As a result, Ringquist inappropriately operationalized this variable” (p. 652). Mark and I agree that defendants in environmental civil cases can be grouped into three categories: large businesses, small businesses, and government entities. In order to estimate the effect of membership in these categories, one must include dummy variables equal to the number of categories minus one (the reference category). In this case, I used two dummy variables representing government entities and large businesses, with the reference category being small businesses. The positive coefficient for *Fortune* 500 companies shows that they receive larger penalties than do small businesses, and the negative coefficient for government entities represents the opposite, all other things equal. The notion that the *Fortune* 500 coefficient is uninterpretable

able because of a composite reference category made up of small businesses and government entities is simply incorrect and reflects a misunderstanding of how to use and interpret dummy variables—a fact that was pointed out repeatedly in prepublication reviews of “Rush to Judgment.”

Second, Mark takes issue with the published case control variable, claiming that “whether a case was published actually measured whether a case ended through litigation rather than settlement” (p. 648). This criticism is also factually incorrect. Ringquist and Emmert (1999) clearly show that the number of published cases is far smaller than the number of cases litigated to conclusion. Moreover, we test for the effects of litigation versus publication by including variables representing both concepts in the same model of penalty severity (Ringquist & Emmert 1999:25). The coefficient for the litigation variable is 0.052 and is not significant, but the coefficient for the publication variable is 0.791 and is significant at  $p < 0.01$  (a result that would be impossible if the variables were highly collinear). The coefficients are directly comparable because both are dummy variables. This difference is what motivated me to use the published case variable rather than the litigation variable in Ringquist (1998)—a manuscript that, although published prior to Ringquist and Emmert (1999), was submitted for review far later. Thus it is demonstrably untrue that an overlap between litigated and published cases “explain[s] why Ringquist found ‘published’ cases to be . . . statistically significant” (p. 648). I pointed out this error in my reviews of “Rush to Judgment,” and again my advice was ignored.

### Errors of Omission

I find two types of omission errors in “Rush to Judgment,” neglecting to recognize intellectual debts and neglecting to discuss implications of modeling choices. First, Mark devotes a large amount of space in his article to distinguishing his analyses from those completed in Ringquist (1998). The similarities between the two pieces, however, are striking. First, in Ringquist (1998) I highlight almost all of the problems with the *NLJ* piece that Mark also highlights in “Rush to Judgment.” Moreover, in Ringquist (1998) I actually correct as many of the same criticisms as Mark does concerning the *NLJ* piece, and I control for one critique of the *NLJ* piece (neglecting to control for firm size and profitability) that Mark raises but does not address. Still, Mark claims that “no thorough review of the [*NLJ*’s] methods and results has been published” (p. 637).

Second, many of the measurement and modeling choices in “Rush to Judgment” are identical to those in Ringquist (1998). When Mark measures the dependent variable he excludes all cost recovery awards, uses constant dollar figures, and takes the

natural log of penalties; none of which were done by *NLJ*, but all of which were done in Ringquist (1998). In addition, all of the variables in “Rush to Judgment” except for total population (which is not significant) are some function of control variables that I first used in my 1998 article. Finally, his presentation of results (quartile means, followed by correlations, followed by regression analyses) and their meaning is identical to mine (Ringquist 1998). There is nothing wrong with these similarities—indeed, they give us more confidence in the comparability of the results. My point is that researchers need to identify the similarities between their own work and the work of others, in addition to the differences.

Far more serious are the errors of omission in the analyses in “Rush to Judgment.” Mark Atlas takes the innovative approach of including dummy variables representing violations of various elements of environmental statutes, rather than simply controlling for violations of the statutes themselves. This effort at greater specificity in modeling is laudable, but it poses some risk. In many cases in DOCKET one finds violations of more than one of the statutory elements controlled for in Mark’s models (e.g., a company can be charged with violating both State Implementation Plans and new source performance standards under the CAA). Mark recognized this in Table 1 and note 26, but did not discuss the prevalence of this problem, nor its full implications. Examining DOCKET, I found 176 cases in which the defendant was charged with violating more than one of the statutory provisions that Mark used in his article, though it is unlikely that he included all of these cases in “Rush to Judgment.” With no discussion of how these cases were handled, we have to assume that the relevant statutory dummy variables take on a value of one in each of these cases (i.e., the categorization scheme is not mutually exclusive and thus cases load on more than one statutory variable), which means that the cases contribute no independent information to the statutory parameter estimates (Hardy 1993).<sup>4</sup> Statistically speaking, it is as if these multiple violation cases did not exist for the purposes of calculating the coefficients on the statutory variables. Moreover, since all of the statutory violations listed in Table 1 are also included as dummy variables, we have no idea what the reference category is to assess the meaning of the statutory coefficients. Twice in my prepublication review of “Rush to Judgment” I pointed out this potential problem, but the error remains uncorrected.

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<sup>4</sup> To be fair, my own analysis suffers from the same problem. However, since I include dummy variables only for statutes, and since (as Mark correctly points out) there are few multiple-statute cases, the problem is far less serious.

### Misrepresentation of the Arguments in Previous Research

My response to Atlas (2001) would not be complete without discussing the somewhat nebulous concept of misrepresentation. When I read his characterization of the arguments regarding the control variables I used in 1998, I do not recognize my own article. Three examples will have to suffice as indicators of these misrepresentations in “Rush to Judgment.” For additional examples, I refer readers to Ringquist (1998).

First, the characterization of my argument regarding the role of judges in penalty assessment bears little relationship to the argument laid out in the original article. In criticizing the rationale for including a variable representing the judge of record’s partisan affiliation, Atlas (2001) says that judges cannot play a “deciding” role in settled cases, that it is “impossible for judges to replace settling parties’ negotiated penalties with their own preferred penalties” (p. 645); that “there is no basis for claiming that settled EPA civil judicial cases are decided by judges” (p. 647); and that “judges [cannot] impose their preferences on penalties” (p. 646). The implication is that I argue that these things are possible, when in fact I argue nothing of the sort. He also claims I argue that “judges were heavily involved in EPA settled cases” (p. 645). A close reading of Ringquist (1998) and Ringquist and Emmert (1999) will find nothing close to any of the statements I just identified. My claim is far less grandiose, and much closer to a position cited approvingly in “Rush to Judgment”; that “the court must not rubberstamp the agreement, but also must not substitute its own judgment for that of the parties to the decree” (p. 646).

Second, Atlas (2001) takes issue with my use of the number of statutory violations as an indicator of the seriousness of the case against the defendant. He claims that this variable is operationalized as “the number of counts brought against the defendant” (p. 650). I use this phrase in my 1998 article when discussing the seriousness of the offense, and he is absolutely correct in stating that the number of counts brought against the defendant is not found in DOCKET (which, as anyone with even minimal familiarity with environmental policy knows, can run into the hundreds). Atlas’s use of this sentence to describe the nature of the discussion regarding statutory violations, however, is seriously misleading. The concept operationalized here is the number of sections of environmental statutes violated by the defendant, and the assumption is that defendants who break more laws may receive larger penalties—a position strongly supported by the evidence (see previous comments). The term “number of counts,” an unfortunate choice of words on my part, is used only twice in Ringquist (1998)—both times after the repeated use of “violations” compelled me to inject a little variety into the prose. How-

ever, I used “number of violations,” or related terms truer to the concept (which is found in DOCKET), more than twenty times in my article, and I used this statutory reference in all tables and in all discussions of empirical results. The meaning in the discussion of this concept is clear, yet Mark chose to take one part of one sentence to characterize incorrectly a lengthy discussion of this concept.

Finally, Atlas (2001) takes issue with my use of the number of prior civil penalties to represent the past history of the defendant. This measure does not include administrative or criminal penalties that might have been experienced by the defendant. This does not mean, as he claims, that the variable is “operationalized incorrectly.” The concept I seek to measure here is the number of times a civil judgment was previously brought against a defendant. I never suggest that the indicator captures all possible previous violations by the defendant—contrary to his characterization of my argument. Finally, while my models might be improved by including additional indicators of defendant histories, it is less clear how omitting altogether this significant predictor advances research in this area.

In these and other instances, Mark Atlas seriously mischaracterizes the nature of my argument regarding various control variables. I highlighted these misrepresentations in prepublication reviews of “Rush to Judgment,” but they remain. Misrepresentations such as these are fine examples of combining a reduction to absurdity with a straw man, both of which are universally regarded as inappropriate techniques when constructing an argument. Researchers have an obligation to honestly and fairly represent the work of others when attempting to carve out a niche for their own work in the world of ideas, especially when it is unlikely that the audience has read the work being summarized. “Rush to Judgment” does not fulfill this obligation, and this is disappointing.

## **Conclusion**

On balance, “Rush to Judgment” is a quality piece of research that reinforces earlier findings regarding potential inequities in civil environmental penalties, enhances the generalizability of conclusions regarding these inequities by examining the question at various levels of aggregation, and significantly advances the state of the art in assuring data quality and dealing with the possible confounding effects of multiple location cases. The body of knowledge regarding environmental equity is better for the publication of this research. Ironically, these contributions of “Rush to Judgment” are almost wholly independent of the criticisms of my previous work. Rather than enhancing the article, inaccurate claims regarding the validity of the conclusions in



Ringquist 1998 detract from the scholarly contribution of “Rush to Judgment,” and the professional reputation of its author.

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