1979

Effect of Representation on a Claimant's Success Rate -- Three Study Designs

William D. Popkin
Indiana University Maurer School of Law, popkin@indiana.edu

EFFECT OF REPRESENTATION ON A CLAIMANT'S SUCCESS RATE–THREE STUDY DESIGNS

William D. Popkin*

Lawyers familiar with the adversary process are likely to assume that a representative will help a claimant win.[1] However, that assumption may be incorrect in a nonadversary proceeding involving claims against the government, where a hearing officer sympathetically develops the claimant's case and the government is not represented. In that setting, a representative might help a hearing officer gather and clarify evidence, but the absence of the representative might not dampen the hearing officer's zeal for developing the claimant's case.[2]

The effect of representation on a claimant's chances of success in nonadversary proceedings is an important issue because it raises questions about the public's responsibility to encourage representation, especially in programs involving the delivery of cash, goods or services to people in economic distress. Other criteria exist for determining the effect of representation besides a claimant's success rate, such as whether representation produces accurate results and whether a claimant's satisfaction with the decision-making process is affected by representation.[3] But there are good reasons for focusing on success rates. Given

*Indiana University School of Law (Bloomington). I am grateful to my colleague, Professor Ilene Bernstein, for reviewing an earlier draft of this paper.

1In the adversary context, they are probably right. H. KLAVEN & H. ZEISEL, THE AMERICAN JURY 371–72 (1966).

2Yourman, *Report on a Study of Social Security Beneficiary Hearings, Appeals, and Judicial Review*, RECENT STUDIES RELEVANT TO THE DISABILITY HEARINGS AND APPEALS CRISIS, 168–69, SUBCOMM. ON SOCIAL SECURITY, HOUSE COMM. ON WAYS AND MEANS, 94TH CONG., 1ST SESS. (Comm. Print 1975).

3J. MASHAW, W. SCHWARTZ, C. GOETZ, P. VERKUIL, F. GOODMAN, SOCIAL SECURITY HEARINGS AND APPEALS, 28–29 (1978) [hereinafter cited as MASHAW].

that accuracy is particularly difficult to determine,[4] equal access to the procedures which produce success becomes a major criterion for fairness. Although claimant satisfaction is an important value, its meaning is somewhat unclear,[5] and there are indications that the results in a case may dwarf the effect of procedures on claimant satisfaction.[6]

Any decision to encourage representation in nonadversary proceedings has serious financial implications,[7] however, and should not be taken unless we are reasonably certain that representation helps claimants win. Several studies have reported a positive correlation between the presence of representation and the claimant's success rate,[8] but they do not control for the other factors that could contribute to success. Dixon's speculation illustrates the problem:

> For example, claimants that are confident they are disabled may approach attorneys more often; but the contrary could also be true, that claimants with strong cases may avoid the expense of an attorney. Arguably, lawyers tend to reject those cases with low likelihood of success. . . . More plausible explanations for the higher [claimant win] rates in attorney cases may be that the attorneys present the cases better, that a hearing examiner may anticipate that attorney-represented cases are more apt to be appealed and possibly reversed above, and that some hearing examiners may be favorably disposed to a fellow professional. . . .[9]

One study undertook a discriminant factor analysis of the hearing process, including the representation variable, but could rely on the results only for conclusions arrived at by other means.[10]

---

[4]A technique for identifying "potentially erroneous" decisions is described in MASHAW, *supra* note 3, at 14–15. It utilizes discriminant factor analysis to predict outcome. A "relatively short list of characteristics of a case" had high predictive capacity. The authors suggest that departure from the predicted result, which is the result an average ALJ would reach, is a good candidate for review by higher agency officials to see if there are errors. The authors admit to the tentativeness of this approach, however. Their book does not contain an extensive discussion of "the methodology, its problems, and its limitations." *Id.* at 7.

[5]*See, e.g.*, J. THIBAUT & L. WALKER, PROCEDURAL JUSTICE, 84–85, 90–91 (1975) (distinguishing the litigant's view of fairness from the litigant's sense of control and involvement in the process).

[6]D. HORWITZ, THE COURTS AND SOCIAL POLICY, 181–82 (1977).

[7]*See* Popkin, *The Effect of Representation in Nonadversary Proceedings—A Study of Three Disability Programs*, 62 CORNELL L. REV. 989, 1040–48 (1977).

[8]*Id.* at 1024–25; Boyd & Johnson, *Report of the Disability Claims Process Task Force*, RECENT STUDIES RELEVANT TO THE DISABILITY HEARINGS AND APPEALS CRISIS, 101–102, SUBCOMM. ON SOCIAL SECURITY, HOUSE COMM. ON WAYS AND MEANS, 94TH CONG., 1ST SESS. (Comm. Print 1975); R. DIXON, SOCIAL SECURITY DISABILITY AND MASS JUSTICE—A PROBLEM IN WELFARE ADJUDICATION, 81–84 (1973). No effect of representation on success rates was found at the Social Security reconsideration stage in Popkin, *supra* note 7, at 1024, 1026–27.

[9]R. DIXON, *supra* note 8, at 82.

[10]MASHAW, *supra* note 3, at 7.

Understandably, hearing officers whose responsibility in nonadversary proceedings is to protect the unrepresented claimant are skeptical about a representative's ability to help claimants win.[11] But that may be a self-fulfilling prophesy. Where the hearing officer thinks an attorney is helpful to the judge, there is evidence that claimants are more likely to win.[12] More surprising is the impressionistic evidence indicating that representatives are not very good at performing some of the tasks that might help claimants win. Thus, one study began with a view that claimants should be informed about the positive correlation between representation and success rates,[13] but doubts about the representative's effectiveness at hearings[14] and a concern that ability varied widely among sub-classes of representatives, led the authors to withhold that recommendation.[15]

The difficulty in obtaining hard data about the effect of representation on success rates arises primarily from problems in isolating the effect of representation from the inherent winning potential of the claimant's case in the absence of representation. The failure to solve these problems renders indeterminate any conclusions about the effect of representation. Such indeterminacy will seem especially serious to lawyers. The lawyer's tendency to focus on the individual case breeds skepticism of the generalities of social science research[16] and guarantees that weaknesses in the research will be sought out. For a social scientist addressing an audience of lawyers, it is therefore especially important

---

[11]Yourman, *supra* note 2, at 137–38, 169; MASHAW, *supra* note 3, at 92.

[12]MASHAW, *supra* note 3, at 23.

[13]*Id.* at 95.

[14]*Id.* at 73, 86, 92-93. *See also*, Yourman, *supra* note 1, at 168–69. Representation quality at Social Security hearing stage varies; perhaps one-half to three-quarters of the representatives are sufficiently knowledgeable.

[15]*Id.* at 95–97.

A study of AFDC hearings found no effect of representation on a claimant's chances of success. Handler, *Justice for the Welfare Recipient: Fair Hearings in AFDC—The Wisconsin Experience*, 43 SOC. SERV. REV. 12, 30 n. 24 (1969). Attorneys appeared in 12 percent of the AFDC fair hearings and the success rates were "about the same" for those who appeared with and without attorneys. Handler also cites lawyer inadequacies in juvenile court where they are co-opted into dropping their advocacy role to minimize conflict when there is an on-going agency-client relationship. *Id.* at 32-33. Perhaps the problem of co-option when there is a continuing agency-client relationships affects the role of representatives in both AFDC and juvenile court proceedings, but not other benefit programs, such as Social Security, where the claim is not granted as part of an on-going service relationship. *See*, D. HORWITZ, *supra* note 6, at 188–194.

[16]*See* L. FRIEDMAN AND S. MACAULAY, LAW AND BEHAVIORAL SCIENCES 1 (1969) ("The sociologists are more eager to mark out boundaries, to seek models for testing; the law professor far more willing to look at all the data he can find and see what he can make of it.") *See also* D. HOROWITZ, *supra* note 6, at 274.

to be sensitive to weaknesses in experimental design that prevent answering questions crucial to the formulation of policy. In what follows, I will discuss three study designs that attempt to solve the problem of isolating the effect of representation on a claimant's chances of success and will consider the strengths and weaknesses of each design. I will use as my example of a nonadversary program the Social Security disability program. This program has the advantage for research purposes of applying a statutory definition that is very rich in both technical and nontechnical issues, of both a simple and highly complex nature.[17] The technical issues concern the existence of medical impairments and range in difficulty from the simplest impairment to those involving mental illness and pain. The nontechnical issues range in difficulty from a determination of age and years of schooling to the effect of both physical impairments and vocational factors on ability to work. Social Security disability disputes are therefore a good proxy for complex nonadversary litigation in general. In one respect, however, Social Security disability litigation is easier to study than other types of litigation because the results are dichotomous: the claimant is either disabled or not disabled and there is no authority for the agency to compromise on the basis of hazards of litigation. We do not, therefore, confront the problem of rank ordering the results of litigation, as in criminal cases where it might be unclear whether a fine or a five day jail sentence is worse.

DESIGN NUMBER ONE
MULTIPLE REGRESSION, USING DATA FROM ACTUAL CASES

If a relationship between two variables, such as representation (the independent variable) and the determination of disability (the dependent variable), can be explained by alternative hypotheses, such as the

[17] 42 U.S.C. § 423(d)

(1) The term "disability" means—(A) inability to engage in any substantial gainful activity by reason of any medically determinable physical or mental impairment which can be expected to result in death or which has lasted or can be expected to last for a continuous period of not less than 12 months. . . .

(2) For purposes of paragraph (1)(A)—(A) an individual. . . . shall be determined to be under a disability only if his physical or mental impairment or impairments are of such severity that he is not only unable to do his previous work but cannot, considering his age, education and work experience, engage in any other kind of substantial gainful work which exists in the immediate area in which he lives, or whether a specific job vacancy exists for him, or whether he would be hired if he applied for work. For purposes of the preceding sentence (with respect to any individual), "work which exists in the national

potential of a case for winning without regard to representation, a common statistical procedure is to identify the other variables that might affect the decision and, through multiple regression analysis,[18] isolate the effect of representation that survives consideration of these other variables. The first study design discusses the strengths and weaknesses of this procedure.

The strengths are obvious. If we are suspicious that representatives are taking "winning" cases, we gather the data necessary to determine whether winning characteristics in the absence of representation are present in cases with representation, thereby explaining the success enjoyed by represented claimants. There are, however, difficult problems with this procedure.

A. Identifying and Measuring Variables

1. *Identifying Variables*

The agency more or less routinely generates a large amount of data, such as the following:

I. Demographic data
   a. age
   b. sex
   c. race

II. Vocational background data
   a. years of education
   b. age
   c. region in which decision occurs

III. Medical information
   a. primary part(s) of body affected

IV. Procedural characteristics of the case
   a. region in which decision occurs
   b. judge
   c. presence of expert advice or witness
   d. presence of representative

economy" means work which exists in significant numbers either in the region where such individual lives or in several regions of the country. . . .

(3) For purposes of this subsection, a "physical and mental impairment" is an impairment that results from anatomical, physiological, or psychological abnormalities which are demonstrable by medically acceptable clinical and laboratory diagnostic techniques.

[18]*See* F. KERLINGER & E. PEDHAZUR, MULTIPLE REGRESSION IN BEHAVIORAL RESEARCH (1973); G. WESOLOWSKY, MULTIPLE REGRESSION AND ANALYSIS OF VARIANCE (1976); R. HARRIS, A PRIMER OF MULTIVARIATE STATISTICS (1975).

It is possible but unlikely that these variables will explain a great deal of the variance in case outcome and it is therefore necessary to develop a more inclusive list by developing a better understanding of the decision-making process. Two ways to develop the necessary understanding of the Social Security disability program are (1) direct observation, and (2) interviews with knowledgeable observers of the litigation process, such as former and present judges, representatives from the private sector and legal services, and agency personnel. There follows a tentative list of outcome-determining variables that might result from these efforts. The list is only suggestive of the richness of variables that might affect case outcome:

I. Type of evidence variables
   a. number of medical reports supplied by claimant
   b. quality of medical evidence supplied by claimant
   c. reputation of doctor(s) supporting claimant
   d. claimant's demeanor (if there is a face-to-face meeting with the hearing officer)

II. Vocational background
   a. level of unemployment in the community[19]

III. Medical information
   a. severity of particular impairment
   b. number of parts of body impaired
   c. particular combinations of medical impairments

IV. Procedural characteristics of case
   a. type of representative (lawyer, nonlawyer, etc.)
   b. cross-examination
   c. length of hearing

In the Social Security disability program, we seem to be fortunate in having a recent agency regulation listing variables which the agency considers important in finding disability when information about the claimant's medical impairment does not dispose of the case or the claimant is not obviously disabled.[20] These variables are:

I. Age
II. Education level
III. Work experience (skilled, semi-skilled, unskilled)

[19]For research purposes, it does not matter that this variable should not be considered by the hearing officer because the program is a disability rather than an unemployment program. 20 C.F.R. § 404.1509(b) (1978). If hearing officers use this variable to determine outcome, it belongs in the study.

[20]20 C.F.R. § 404.1503—§ 404.1513; 20 C.F.R. app. 2 § 404(P) (1978).

IV. Transferability of skills
V. Residual functional capacity based on exertional factors (the ability to do sedentary, light, medium, heavy, or very heavy work)

However, there is evidence that the predictive capacity of these variables is slight, perhaps because they are relevant in close cases decided on other grounds.[21]

2. *Categories of Variables*

Once the outcome-determining variables are identified, we must develop the categories of those variables that explain outcome. For example, the agency has concluded that certain age categories are useful in deciding cases[22] and, if these age categories predict results and are not merely normative rules disregarded by the hearing officers, they should be used by the researcher. Categorizing the age variable is easy, however, compared to other variables. Agreeing on a meaningful set of categories for ranking the severity of the medical impairment and the reputation of expert witnesses will be extremely difficult.

3. *Measurement*

The next step is to measure each case with respect to the variables. Where findings are not controversial (for example, race, sex, or the presence of an expert), this is not difficult. Even apparently subtle judgments, such as the individual's residual functional capacity for work, might be the product of a series of mechanical and uncontroversial tests (e.g., the ability to lift weight).[23] In many cases, however, there will be two fundamental problems with measuring variables. First, accurate measurement of variables, such as the quality of medical evidence, is extremely difficult. Knowledgeable observers may not solve the problem. These issues are, after all, the very ones that cause judges the most difficulty and on which there is likely to be a wide variety of opinion. Doubts about the measurement process would be reduced if several observers were consistent in their measurements but the use of more than one observer per case raises research costs.

Second, the measurement of variables that are not mechanical and uncontroversial is likely to be influenced by the observer's view of the overall outcome of the case. Evaluation of the evidence and findings of intermediate facts on which the ultimate finding of disability is based

[21] MASHAW, *supra* note 3, at 19.
[22] 20 C.F.R. § 404.1506; 20 C.F.R. app. 2 § 404(P) (1978).
[23] 20 C.F.R. §§ 404.1505, 404.1510; 20 C.F.R. app. 2 § 404(P) (1978).

(such as transferability of skills)[24] are not easily isolated from the ultimate decision. For this reason, the agency's findings on intermediate facts, such as the transferability of skills, may not be reliable for the researcher's purpose and, for the same reason, we may suspect the observer's measurements. The problem cannot be dismissed as mere bias. Every decision-making process is a creative process in which "facts" take shape in the light of the broader outcome for which they are relevant.[25] The distortion in research findings that this can create is seen from the following example. If the claimant is considered disabled, the observer (or hearing officer) may find that the claimant does not have transferable skills, because that finding makes the case appear more favorable to the claimant on the ultimate issue of disability. If the claimant in this case had a representative, the representative will appear less useful than he or she might actually have been, because the case will appear easier to win than it actually was, as a result of the distorted finding on skill transferability.

This distortion could be reduced by obtaining measurements from those who are unlikely to be concerned with case outcome. For example, doctors could judge physical impairment and vocational experts could judge transferability of skills. It is in the expert's nature to lose sight of the overall case outcome and, for that very reason, their judgments on intermediate facts might not be distorted, at least by outcome considerations.

B. Representative Interaction with Variables

Even accurate measurement will not isolate the raw facts of the case as they would exist without representation. In this study design, case observations are made after the representative has used his persuasive and investigative skills to mold the raw material presented to him by his client. The case may look easy to win (skills are nontransferable, etc.), but only because the representative made it look easy. For example, suppose we find a positive correlation between (1) representation and nontransferable skills, and between (2) nontransferable skills and a finding of disability, but no correlation between (3) representation and a finding of disability, controlling for skill transferability. Representatives might still be helpful by shaping the facts on the issue of skill transferability so that the hearing officer would be able to find for the claimant. But this research result is equally compatible with

[24]20 C.F.R. § 404.1511(e); 20 C.F.R. app. 2 § 404(P) (1978).

[25]Fuller, *An Afterword: Science and the Judicial Process*, 79 HARV. L. REV. 1604, 1615–16 (1966). Facts carry with them a heavy but unmeasurable contamination of law.

the conclusion that representatives are taking cases to a tribunal that would have been there anyway and that would have resulted in a finding of nontransferable skills without a representative. Nothing in multiple regression analysis can unravel this interaction. At most, such a finding could trigger a close examination of cases in which representation is associated with the variable that makes the case easier to win (e.g., nontransferable skills), to see whether we can find out if the presence of the variable resulted from the representative's efforts.

In summary, design number one has two major problems: measuring difficult-to-measure variables and isolating case potential when case observation occurs after a representative has entered the case.

DESIGN NUMBER TWO
PREDICT OUTCOME WITHOUT REPRESENTATION; ASSIGN REPRESENTATIVES TO CASES AFTER OUTCOME IS PREDICTED; COMPARE ACTUAL OUTCOME AND PREDICTED OUTCOME

Another approach to determining case potential without regard to representation is to develop a reliable prediction of outcome without representation and then assign representatives to these cases to see if they make a difference. The basic outline of this study design is as follows:

STEP 1 Knowledgeable observers would observe a series of actual cases in which there was no representation and would attempt to predict case outcome. They would be asked to predict outcome at various stages of the decision-making process, such as after intake, after development of the evidence by agency personnel, and after a hearing (if any). Their efforts to predict outcome would be compared with actual outcome. Observers who were good predictors of actual outcome would then be selected.

STEP 2 The selected observers would predict the outcome of another group of cases in which there was no representation. Representatives would then be assigned to this group of cases after the outcome prediction had been made. The actual results with representation would be compared to the predicted results to obtain a measure of the effect of representation. By predicting outcome and assigning representation at various stages of the decision-making process, we can also determine whether it matters at what stage a representative enters the case.

The theory behind this study design is to avoid the problems of measuring outcome-determining variables and of isolating case potential after representatives are present, both of which existed in the first study design. As we will see, however, study design number two presents problems of its own.

A. Measurement

The primary question asked of knowledgeable observers in this study design is a prediction of outcome. These observers should, therefore, have a different perspective from the observers in study design number one, who were asked to focus on medical or vocational questions, not overall outcome. Study design number two would use experts in predicting outcome, such as representatives and hearing officers, rather than doctors or vocational experts. Such observers might, however, have their own biases; representatives, for example, might doubt that an unrepresented claimant could win.

B. Omission of Cases with Representation

In study design number one, observations were distorted by the inclusion of cases with representation, after interactions between the basic facts of the case and representatives had occurred. In this study design, we correct that problem by observing only cases without representation. This allows us to predict outcome when the claimant is unrepresented so that the effect of representation can be determined after assigning a representative to the case. However, the omission of cases in which claimants have selected their own representative could distort the findings if the omitted cases are different in important respects from cases without representation. Thus, if representatives take cases which are borderline cases, in which they can help the claimant win, but unrepresented claimants have such weak cases that no representative can help, study design number two will not observe an effect of representation, even though representatives are helpful. It goes without saying that we cannot forbid representation so that all cases will be included in our research population. Unless there are only a few represented claimants, their omission from the study will be a serious problem.

One way to deal with this problem is to gather the same type of data gathered in study design number one for all cases, whether or not there is a representative, to see whether the cases in which claimants select their own representatives and the cases with unrepresented claimants differ in any significant respect. But this procedure has the shortcomings of study design number one, in that the cases with representation are not observed prior to any interaction between the "facts" and the representatives.

Another solution is to use the information gathered from the observers and the agency about cases without representation to categorize the cases in terms of the ease or difficulty of winning.[26] Unless the winning potential of cases with representation was so different from the winning potential of cases without representation that cases of the former type did not appear in the unrepresented claimant group, we could use that information to obtain an accurate picture of how representation interacts with every type of case, categorized by its potential for winning. However, we would still not know the winning potential for cases in which claimants selected their own representatives.

C. Assignment of Representation

The process of assigning representatives to unrepresented claimants would also present problems if the assignment process altered the way representation usually occurred. Unlike the criminal bar, there is no well established bar of disability practitioners and it therefore might be difficult to replicate in a study the typical pattern of representation. Moreover, the very act of assignment might create incentives for atypical behavior by representatives who know their work is being studied.

DESIGN NUMBER THREE MODIFIED TRUE EXPERIMENT

The purpose of the first two study designs was to overcome the impossibility of designing a true experiment. In a true experiment, representation would be randomly assigned and the results for the represented claimants would be compared to the results for the remaining control group of unrepresented claimants. Because the randomization process would produce an experimental and control group that were identical in all respects except representation, the effect of representation could be inferred by comparing the two groups. This procedure is impossible, however, because claimants cannot be forbidden representation. Claimants who choose their own representative will therefore self-select themselves out of the study, and we cannot be sure that their omission is inconsequential because it is the very possibility of a systematic relationship between the winning potential of a case and the presence of a representative that gave rise to skepticism about the two-

[26]*See* MASHAW, *supra* note 3, at 14, 18-19, explaining how statistical analysis can be used to devolop an index of factors predicting outcome.

way correlation of representation and success rates in the first place.[27] However, the problem of omitting represented claimants from the study already exists in study design number two. Step 2 of study design number two assigned representatives to unrepresented claimants, omitting represented claimants from the study, and compared the actual results with predicted results. Since represented claimants are omitted in any event, why wouldn't we prefer to randomly assign representation to a group of otherwise unrepresented claimants and compare the results in those cases with the results in a control group of unrepresented claimants? We would at least avoid the problem of having to predict likely case outcomes in the absence of representation, or so it would seem. In other words, if study design number two is feasible, why isn't a true experiment possible, modified by omission of cases in which claimants select their own representatives?

The problem with the modified true experiment is that the group of unrepresented claimants, from among which we select some claimants for random assignment of representation, is itself unlikely to be the entire population of unrepresented Social Security claimants or a random sample of that population. Social Security cases arise all over the country. A random sample from this population will produce claimants in widely scattered urban and rural areas. Random assignment of representation to these claimants would be impossible because there would not be enough of the representatives typically available to Social Security claimants in close enough geographical proximity to the claimants. The "modified true experiment" therefore would be restricted regionally in ways that could defeat the advantages of the experiment by systematically biasing the cases with respect to the ease or difficulty with which they could be won. We could, of course, compare the population in the region studied with the omitted populations. We would make the comparisons on the basis of those facts which make a claimant's chances of winning more or less likely. In that way, we can see whether the regional limitations of the study create a systematic bias. But it was the difficulty of making determinations about likely case outcomes that made the modified true experiment an attractive alternative to study design number two in the first place. It turns out that this difficulty cannot be easily avoided.

CONCLUSION

An empirical study is like any argument. It results from assumptions and makes certain points. If the assumptions are wrong or the points

---

[27] *See* text *supra* note 8.

tangential, the study is not likely to be very useful. Thus, if representation is important primarily because it improves claimant satisfaction, the results of a study about success rates are beside the point. If success rates are relevant, but the study improperly assumes that the population studied is the same as the population omitted from the study, the results can be fairly challenged if they are presented as evidence about the entire population.

But lawyers are always making tangential arguments and questionable assumptions and are accustomed, through argument, to bringing these weaknesses out into the open. What then is special about social science research? There are two problems. First, the hard data on success rates may dwarf the softer concerns with claimant satisfaction and deflect attention from the unproven assumptions underlying the research.[28] Second, arguments about the assumptions are sometimes relatively inaccessible, except to those trained in social science research.[29] Teaching the lawyers and even judges about social science research is not a sufficient solution to this problem if they do not also learn how to communicate decisions to the public in terms that make the decisions understandable and, therefore, legitimate.

The limitations of social science find their mirror image, however, in the weaknesses of legal argument. The lawyer often retreats to unprovable assertions as a technique of debate and picks at the assumptions of social science research with a tenacity reserved only for social science. Good social science, by making the assumptions of a research design both explicit and understandable and by explaining how the conclusions relate to policy formulation, is the best antidote for the skeptical lawyer.

---

[28]For a debate over the effect of mixing hard and soft information, *see* Tribe, *Trial by Mathematics: Precision and Ritual in the Legal Process*, 84 HARV. L. REV. 1329, 1359-65 (1971); Finkelstein & Fairley, *A Comment on "Trial by Mathematics,"* 84 HARV. L. REV. 1801, 1806–07 (1971); Tribe, *A Further Critique of Mathematical Proof*, 84 HARV. L. REV. 1810, 1819–20 (1971).

[29]*But see* Finkelstein, *Regression Models in Administrative Proceedings*, 86 HARV. L. REV. 1442, 1462–67 (1973), for a discussion of how assumptions concerning heteroscedasticity were debated in an administrative setting.