Expert Evidence in Flatland: The Geometry of a World Without Scientific Culture

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I. INTRODUCTION

Although I was invited primarily to comment on Professor Dale Nance's contribution to this Symposium, *Reliability and the Admissibility of Experts*,[1] I hope my comments will have relevance beyond the four corners of his article. Without question, his article is a laudable piece of evidence scholarship. Indeed, that is its main defect. But Professor Nance is far from alone, for this failing is shared by much of the legal scholarship and judicial authority in this area. Briefly put, Professor Nance deals with the subject of expert evidence mainly the way traditional evidence scholars have approached this subject from time immemorial. But expert evidence is not like other kinds of evidence. While it shares many superficial features with ordinary evidence, it is nothing like such mainstays of evidence doctrine as hearsay or character. This bare resemblance has led many astray. Scientific evidence—and after *Kumho Tire*[2] all expert testimony—requires courts to make science policy.[3] This is not

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[1] Dale A. Nance, *Reliability and the Admissibility of Experts*, 34 SETON HALL L. REV. 191 (2003).

[2] Kumho Tire Co. v. Carmichael, 526 U.S. 137 (1999)

[3] In this Essay, I use the terms "expert evidence" and "scientific evidence" interchangeably. In evidentiary terms, this is entirely appropriate since Rule 702 applies to all expert testimony, not just scientific evidence. Moreover, in *Kumho Tire*, Justice Breyer made clear that courts should expect no bright line dividing the scientific from the nonscientific. Yet many courts and commentators continue to speak as if some line exists separating the two. Even Professor Nance is partly guilty of seeing the world in this way. *See* Nance, *supra* note 1, at 202. But, as I have argued elsewhere,

> Science does not "exist" categorically or in some concrete encyclopedia of knowledge that passes muster by, say, some committee of the National Academies of Science. Science is a process or method by

optional; it is part of the intrinsic nature of the subject. The admissibility decision necessarily requires a policy judgment, and any treatment of the subject that fails to appreciate and incorporate that fact is missing an essential dimension of the problem.

In *Flatland: A Romance of Many Dimensions*,[4] a classic mathematical and satirical novel first published in 1884, Edwin A. Abbott explored what the world would look like to creatures that live in two-dimensional space, having only length and width. In Abbott's flatland, geometric creatures inhabit a world that consists of an infinite plane. According to Euclid, a point has zero dimensions, a line one, a plane two, and a solid three. Abbott was actually interested in the provocative notion that we live in a universe with four (or more) dimensions—a notion with much modern currency given contemporary theories in physics. But imagining four dimensions is quite difficult, so Abbott constructed a world with only two and described how that world would perceive creatures or objects from a three-dimensional world, a world he called "spaceland." Appropriately, for present purposes, the story is narrated by a conservative lawyer named A. Square. Other flatlanders appear as straight lines, and only by feeling their fellow creatures can they determine how many sides other flatlanders have. In the rigidly hierarchical society of flatland, in which the number of sides one has designates social class, the upper classes consider this feeling about rather gauche.

When a three-dimensional creature enters flatland, it appears to

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> which factual statements or predictions about the world are devised, tested, evaluated, revised, replaced, rejected, or accepted . . . Courts make a fundamental error when they try to divide the world into science and specialty categories. In truth, every expert who appears in court has "specialized" knowledge of one sort or another. At best, it is specialized knowledge based upon good applied science; at worst, it is specialized knowledge based upon "years of personal experience."

David L. Faigman, *Is Science Different for Lawyers*, 297 SCIENCE 339, 340 (2002) [hereinafter *Is Science Different*]. Therefore, both as a practical matter and under the Rules of Evidence, it is inappropriate to distinguish between scientific and specialized testimony. Rule 702 thus would be clearer if it simply applied to all specialized knowledge that "will assist the trier of fact to understand the evidence or to determine a fact in issue." FED. R. EVID. 702. The issue presented by this Symposium—what standards should be applied post-*Kumho Tire*—also would be more plainly presented, since courts would be obligated to consider explicitly how well supported proffered, specialized knowledge must be before gaining admission, whether based on sound scientific methods, personal observation, or some level in between.

[4] EDWIN A. ABBOT, FLATLAND: A ROMANCE OF MANY DIMENSIONS (1884); *see* EDWIN A. ABBOT, THE ANNOTATED FLATLAND: A ROMANCE OF MANY DIMENSIONS (Introduction and notes by Ian Stewart) (2002).

hover in space, expanding and retracting in dimension as it moves across the flatlanders plane of vision. It would be as if the flatlander could perceive the world only from the surface of a pond. A flatlander would not be able to see below the surface of the water or above it. If a beach ball broke the surface, it would first appear as a dot, progressively expanding to its full diameter—though seen as a straight line—and then decreasing in size to a dot before disappearing entirely.

Professor Nance's evidentiary world operates similarly in two dimensions. His flatland cannot account for the three dimensional space in which science operates. Professor Nance's world also lacks depth. It is the depth of the scientific enterprise. Nance's flatlanders cannot see science before it emerges on the horizon of the courtroom or after it moves beyond that horizon. An evidentiary flatlander sees scientific research programs as straight lines, and the standard for assessing admissibility amounts to little more than comparing their sizes. Flatlanders have no perception of past scientific processes and no conception of future potential. Their world lacks scientific culture. Science seems to float abstractly along the flatlanders' horizon, unconnected to space or time.

Abbott's flatlanders live in the same universe as spacelanders (i.e., us), so many of the descriptions of reality naturally resonate for Abbott's three dimensional readers. The same is true for Professor Nance's discussion of expert testimony in evidence flatland. In particular, Professor Nance's two main theses have value in evidence spaceland. The first is his belief that reliability is not a dichotomous variable. Despite the fact that he believes that my coauthors and I (hereinafter referred to as Faigman et al., with my apologies to my coauthors, David Kaye, Michael Saks, and Joseph Sanders) erected just this sort of dichotomy, through our recognition that *Daubert*[5] requires a "threshold of reliability,"[6] no one truly believes that reliability is a dichotomous variable. The admissibility decision is, of course, categorical, but reliability and validity provide no such simple dichotomy. The first section of this Essay briefly addresses Professor Nance's imputation that Faigman et al. sought to make reliability dichotomous by mandating a minimum threshold requirement for scientific evidence.

The second major theme of Professor Nance's article is his embrace of what Faigman et al. called the "better evidence principle."

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[5] Daubert v. Merrell Dow Pharm., Inc., 509 U.S. 579 (1993).

[6] David L. Faigman et al., *How Good Is Good Enough?: Expert Evidence Under* Daubert *and* Kumho, 50 CASE W. RES. L. REV. 645 (2000).

Here, he finds much to like in our approach, although his is both rather more limited and somewhat broader than what we contemplated. It is this thesis that loses depth when described from Professor Nance's flatlander perspective. The Nance better evidence rule, unlike Faigman et al.'s, is driven almost entirely—albeit not completely—by evidentiary concerns. Faigman et al. were driven by different considerations. It was our contention that the evidentiary determination regarding expert evidence must take into account—it must integrate into its foundational premises—the culture of the scientific method. Whereas Professor Nance sees expert evidence as another species of evidence law, not unlike hearsay or character evidence, Faigman et al. saw it as a different animal altogether.

II. RELIABILITY AS A CONTINUOUS VARIABLE

Professor Nance laments, "Unfortunately, prominent scholars . . . often agree, explicitly or implicitly, on the idea that the post-*Daubert* regime rightly requires expertise to exceed . . . a threshold of reliability."[7] He cites Professor Imwinkelried and Faigman et al. as examples. Professor Imwinkelried, in this Symposium, unambiguously states that he does not see evidentiary reliability in dichotomous terms.[8] Faigman et al. also hereby disclaim any intention to transform reliability into a dichotomous variable by our statement that *Daubert* requires "a minimal threshold of reliability." All we meant was that, unlike *Frye*, in which expert evidence might be admitted after it has gained the confidence of a well-meaning guild—with no empirical testing at all—*Daubert* requires some minimal amount of testing. Although not all courts have implemented *Daubert* in this way, we argued that *Daubert* requires the production of *some* data that are the product of *some* reasonably reliable methods that have produced *some* results that can be generalized to the case in which they are offered. This is the minimum and this is what the *Daubert* regime demands. Our point, then, was similar to Chief Justice Rehnquist's admonition in *Joiner*[9] that courts cannot rely merely on the *ipse dixit* of testifying experts that their conclusions are reliable.

Professor Nance is also incorrect in supposing that by advocating a minimal threshold we sought to "transform continuous variation

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[7] Nance, *supra* note 1, at 220-21.

[8] Edward J. Imwinkelried, *The Relativity of Reliability*, 34 SETON HALL L. REV. 269, 270 (2003) ("I agree with Professor Nance's thesis that reliability should be conceived in relative terms.").

[9] Gen. Elec. Co. v. Joiner, 522 U.S. 136 (1997).

into dichotomous choice." It is commonplace for continuous scales to have thresholds that create "dichotomies." Thirty-two degrees Fahrenheit is the threshold for water turning to ice on the continuous scale of temperature. I would imagine that even Professor Nance would grant that expert evidence regarding a prediction of future behavior (say, future violence) that relies on "scientific" measuring devices such as tarot cards, tea leaves, and crystal balls, falls below "a minimal threshold of reliability" under *Daubert*.

I do agree that in regards to methods more sophisticated than crystal balls or casual observation, the reliability criterion should be relative. Indeed, this is the whole point. Unlike Professor Nance, however, I would not approach this issue as merely an evidentiary matter. This needs to be understood as a matter of science policy.

III. EXPERT EVIDENCE IS DIFFERENT

A. The Nature of Expert Evidence

The principal difference between expert testimony and other sorts of testimony to which the Federal Rules apply is that in virtually all cases a large aspect of the evidence transcends the particular dispute in which it is offered. In the terminology of Kenneth Culp Davis, hearsay and character evidence are paradigmatic examples of adjudicative facts.[10] They are particular to the dispute and their verity and value can largely be assessed through the lens of the particular case. The same is not true for expert evidence. Whether implicit or explicit, almost all expert opinion depends on considerations that transcend particular cases—or, at least, it ought to.

The essence of expert opinion is that it is the process of both inductive and deductive reasoning. Whether a defendant suffers from the battered woman syndrome or a tire failed because of "overdeflection," the essence of the expert's testimony is that this case is like other similarly situated cases in the world beyond the particular courtroom. Similarly, accountants and realtors believe that their conclusions follow from experience with the world beyond the courtroom. The specific case is in some way representative of some general phenomenon.

Obviously, ordinary testimony also concerns experiences beyond the courtroom. A witness who states that the light was red when the

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[10] *See* Kenneth Culp Davis, *An Approach to Problems of Evidence in the Administrative Process*, 55 HARV. L. REV. 364, 402-03 (1942).

plaintiff's car went through the intersection or that the bloody knife was found in the defendant's garage, is testifying to facts that occurred outside of the courtroom. But the testimony pertains to single incidents, and, beyond the circumstances under which they were observed, neither the witness nor the fact finder needs to infer anything about the nature of the world more generally. Consider, for example, the difference between a police officer's testimony that he discovered drugs in the *possession* of the defendant and his proposed testimony that the defendant *intended to sell* those drugs. As an ordinary fact witness, the officer might say that he found two pounds of marijuana wrapped in plastic and stashed in the defendant's toilet-tank. In addition, the officer might testify that he found a scale for weighing small quantities in the defendant's bedroom and baggies in the kitchen. The officer, however, might seek to testify further—now as an expert—that in his experience defendants possessing two or more pounds of marijuana, together with scales to weigh the drugs and baggies to package them, usually possess the drugs with the intent to sell them. Although courts routinely allow this sort of so-called experiential testimony, the officer is no longer a simple fact witness. The witness, like all experts, is saying that this defendant shares certain general characteristics that are associated with drug dealing, as opposed to simple possession for personal use.[11]

Professor Nance offers the example of realtors who propose to testify on the value of certain property. One party offers an expert who has taken an ostensibly scientific approach to the subject by surveying comparable property in the vicinity. The opponent challenges this evidence and offers its own expert, one who used a "gestalt assessment," which is more ordinarily described as "years of experience." Professor Nance's answer to the question whether one, both, or neither of the experts gets to testify is instructive. His focus is exclusively on the courtroom use of the evidence, and he admirably crunches the various considerations that might be presented in that limited forum. He never mentions, or suggests as relevant, what sorts of empirical methods should be expected to be brought to bear in answering the empirical question regarding the value of property.

Professor Nance analyzes such issues as the burden of proof, the judge's role as "representative" of the jury, jury credulity, and so forth. The key component and conclusion of his analysis seems to be

[11] As discussed below, the officer's hypothesis that possessors of two pounds of marijuana intend to sell rather than simply use the drug is testable by the scientific method. Whether it should have to be tested is a policy judgment. So far, the courts have assiduously avoided confronting this question or even recognizing that it must be answered.

that the judge should admit "the best evidence that is or should have been reasonably available, with due regard to the adversarial structure of the trial."[12] This means that evidence will be excluded under this theory only if the proponent of the evidence could have secured better evidence and that the evidence was not reasonably available to the opponent.[13] Thus, Professor Nance constructs a scheme whereby the law of evidence would demand the best evidence that could be brought into the courtroom, and the assessment would be almost entirely a comparative one between what the proponent proffered and what other proof the proponent could have introduced. In effect, then, the admissibility determination is made from a snapshot, and the court sees only the evidence before it, with no consideration of what has occurred before or might occur after the decision. Professor Nance, however, carves an exception to this static picture for certain kinds of expertise, such as forensic evidence, since "repeat players, . . . may plausibly be considered in regard to the long run of cases, rather than based on what is reasonably available in the short enough run to address a particular case."[14]

The myopic snapshot by which Professor Nance would have trial courts judge scientific evidence is fundamentally flawed. It fails to take into account the dynamic nature of empirical inquiry. More ominously, it also fails to take into account the law's effect on many of the disciplines that hawk their wares at the courthouse door. If the gatekeeper metaphor is to have any meaning, it cannot be limited to having judges choose between poor alternatives. Where Professor Nance would have judges ask whether better evidence is available, I would have them ask whether better evidence *should be* available. Judges should operate in three-dimensional space, not two.

Indeed, as noted, Professor Nance advocates this more dynamic approach for "repeat players, such as the state in regard to forensic science techniques."[15] But the exception itself well illustrates the

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[12] Nance, *supra* note 1, at 241.

[13] *Id.*.

[14] *Id.* at 249. This strategy seems to serve a possible normative agenda in Professor Nance's proposed approach. As he states, his "test will place greater demands on the prosecution than on the accused in criminal cases, and it will place greater demands on powerful civil defendants than on impecunious civil plaintiffs." *Id.* at 247.

[15] *Id.* at 249. He does not say whether this exception applies to other "repeat players," such as large corporations. If one objective is to put "greater demands on powerful civil defendants," then he might impose such a burden. Unlike prosecutors, however, it is somewhat less clear that powerful civil defendants have the burden under *Daubert* to produce such evidence. Prosecutors are not only repeat players, they bear the burden of proof as the proponent of the expert evidence.

cramped evidentiary perspective he brings to the subject of expert evidence. Although litigation may be about "repeat players," science is not. Indeed, the whole point of science, as exemplified in the "peer review" standard of *Daubert*, is that many unrelated players will be part of the knowledge production business.

Instead of "repeat players," courts should attend to repeat expertise. Consider, for example, the psychological concept of post-traumatic stress disorder (PTSD). Plaintiffs and defendants in civil cases, and prosecutors and defendants in criminal cases, have offered evidence of this syndrome. Professor Nance would seemingly admit this evidence so long as it is as good as any other evidence that might be offered on the relevant issue—except, perhaps, for prosecutors who might be obligated to demonstrate that better evidence could not be forthcoming. But this approach completely ignores the law's influence on what work gets done. Simply put, if courts demand better evidence than what has so far been done, then, and often only then, will that work be done. In the case of PTSD, psychologists are not conventional parties to the litigation involving their research, yet they are very interested participants in the process and extremely sensitive to what courts demand as adequate proof. What Professor Nance is missing is the fact that science is not a product of any one litigant or set of litigants. For instance, the fact that a prosecutor in federal court in Newark has no data to validate handwriting identification has very little to do with that prosecutor. Indeed, the last thing the evidence rules should do is encourage the United States Attorney in Newark to start collecting data. The question is not whether the Newark prosecutor should be estopped from offering expert testimony because of a lack of data. The issue is whether the courts should expect the scientific community (broadly defined) to have produced better data on handwriting.

Professor Nance correctly states that the admissibility determination for expert evidence is a relative one. His error is in limiting the dimensions on which this comparison is done. Professor Nance appears to fear that an expansion of the inquiry to take into account the dimensions outside the courtroom will leave courts with no bearings. Limiting the comparison to the courtroom permits judges the relatively simple task of measuring length and width. Perhaps expert evidence that is longest, or perhaps that with the most number of sides,, can be considered "best." Measuring depth complicates the judge's gatekeeping task enormously. It requires a reliability assessment of not only what is, but what might be.

Professor Nance complains that this sort of approach is "vacuous," for "[i]t merely restates the 'sufficient reliability' idea

without telling us anything affirmative about how to fill it out."[16] Professor Nance continues, complaining,

> Without giving attention to the parameters in the balance that determine whether a given degree of reliability is reasonable for admission, this also effectively requires the judge to answer the question, "Does the degree of reliability of the expertise outweigh?" Unless something follows the word "outweigh," the question is practically incoherent. Outweigh *what*?[17]

The question is incoherent only because Professor Nance asks it in the flatland of the individual courtroom. Nance's concern about incoherence lies in the limitations of his flatlanders' spatial disabilities, not in any inherent incoherence in the three-dimensional world of scientific evidence. Consider Professor Nance's real estate appraiser example. Suppose that a realtor offers to testify on the basis primarily of his "years of experience," perhaps buttressed by a listing of a dozen comparable properties in the area. In Professor Nance's world, the judge is reduced to comparing this expert testimony to what other evidence might be available. To my knowledge, there is unlikely to be much better evidence available to the proponent, and thus the evidence will be admitted. In Nance's flatland, a realtor with twenty-five years of experience and a list of a dozen "comp-properties" would be the longest line around.

Adding a third dimension to this analysis changes it completely. Now the question is, given the nature of the subject and the legal consequences that follow making a mistake (either of the false-positive or false-negative variety), how much and what kind of testing should be necessary before it is admitted?[18] This is hardly an incoherent question. There are a multitude of ways that a scientifically-minded person might validate real estate appraisals. Appraisers, for instance, routinely assess the value of property before it goes on sale. Yet, to my knowledge, appraisers do not keep track of the actual sale price of those houses, much less subject those data to statistical analysis. Confounds like the amount of time between the appraisal and sale or changing market conditions could be measured and analyzed. It does not take great scientific imagination to come up with many other research paradigms that might be brought to bear on this issue.

I am not suggesting, however, that real estate appraisers (or researchers interested in this subject) should have to conduct

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16 Nance, *supra* note 1, at 222.

17 *Id.*

18 *See Is Science Different*, *supra* note 3, at 340.

original research testing their proficiency at the given task. Whether such research should be required is a policy question on which I will remain agnostic. But courts should have to make this policy judgment, for realtors, for psychologists, for forensic scientists, for medical doctors, and for any and all expert witnesses.

Undoubtedly, many will complain about the difficulty of the task. It is true that measuring depth, as well as length and width, complicates the judge's job. But so be it. Science is complicated. In fact, however, most of these determinations are not rocket science and are well within the grasp of most attorneys. The task of evaluating realtors in three dimensions, for instance, is hardly formidable. Much of forensic science and psychology similarly does not challenge the legal imagination too strenuously. Indeed, the key stumbling block seems not to be whether the depth of the scientific enterprise can be understood, but a belief among many that this dimension is outside the purview of evidence law. The flatlander's perspective, according to this view, is mandated by the nature of the evidence world in which we live, rather than any lack of desire to escape to spaceland.

B. Scientific Evidence as Science Policy

Two-dimensional perspectives such as Professor Nance's flatland tend to concentrate on the adversarial nature of the trial and pay particular fidelity to the role of juror as trier of fact. Given that the evidentiary plane is all that there is, this focus is hardly surprising. But once a third dimension is added, pedestrian concerns about jury credulity and attorney-control over evidence ought to recede into the background.

Professor Nance is duly concerned about the juror's role in the litigation process. As others have struggled, he seeks to retain a prominent role for jurors in the evaluation of scientific evidence. This concern, however, is primarily associated with the belief that scientific evidence is somehow like ordinary evidence. This leads Professor Nance into the thicket concerning jury credulity and the debate whether jurors are any less sophisticated than judges as consumers of expert testimony. Not surprisingly, he rejects any notion that they are less sophisticated. Such a view is heresy to evidence doctrine. He bases his judgment of juror capacities on a combination of presumption, intuition, and empirical research that itself would never pass muster under *Daubert*.[19] His alternative

[19] Nance, *supra* note 1, at 232-33. Evidence scholars and judges are keen to extol the virtues of jurors. And it would be impolitic for me to suggest otherwise. The

explanation for the judicial gatekeeping role, again not too surprisingly a product of the adversarial process, is his belief that expert evidence is more manipulable than ordinary evidence, and thus requires greater "judicial management."[20]

The issue of judge versus jury, when considered in three dimensions rather than two, is largely irrelevant. It is the judge's task to evaluate the validity of proffered expert testimony, and that is all there is to it. There are an assortment of reasons why it is the judge's job, some of which will appeal to Nance's flatlanders and others to spacelanders. Therefore, the question is not whether judge or jury should decide preliminary questions of fact (and policy) regarding the validity of expert evidence. The only question is what is the nature of the judge's job in this regard—the rest will be done by the jury.

The relative nature of the reliability determination is perhaps the best reason for why the admissibility decision regarding expert evidence is a policy judgment. Professor Nance and others see the relativity of science only as manifested in the courtroom. Professor Imwinkelried, for example, in his response to Professor Nance, finds "three respects in which the concept [of reliability] is certainly relative: (1) the specificity of the claim the expert makes; (2) the use to which the expert's proponent wants to put the claim; and (3) the definiteness with which the expert proposes couching his or her ultimate opinion."[21] But there is an additional dimension inherent in the scientific enterprise itself. Most importantly, this is the question of error rates.

Consider the two empirical questions presented in *Daubert*: Does Bendectin cause birth defects? And, if so, did Bendectin cause the plaintiff's birth defects? The ultimate legal issue is the latter question, on which the plaintiff bears the burden of proof. Here, I want to focus on the first question, what courts commonly refer to as "general causation." In considering proffered research on the

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research on the question of judge versus juror, however, remains too scant to be of much use. Also, logic would suggest that judges' repeated dealings with at least certain kinds of expert evidence would give them certain advantages. The hypothesis, then, is not that judges are smarter or more scientifically literate than jurors, but that they have more experience with it. Indeed, where judges don't have more experience with science, there is reason to believe juries might have an advantage simply due to having multiple jurors to contemplate the complexities of the subject. But these are empirical hypotheses that have yet to be studied closely. As the text makes clear, for me this is largely an academic question, since I believe it is the judge's duty to make the admissibility decision as a matter of law.

[20] *Id.* at 232.

[21] Imwinkelried, *supra* note 8, at 269.

empirical relationship between Bendectin and birth defects, one of four possibilities are possible: the court could conclude that the basis for the asserted connection (1) is valid when it is valid (true-positive); (2) is valid when it is not valid (false-positive or Type 1 Error); (3) is not valid when it is not valid (true-negative); or (4) is not valid when it is valid (false-negative or Type 2 Error). The following table illustrates these four possibilities:

		GROUND TRUTH	
		Valid	*Not Valid*
COURT'S ADMISSIBILITY DETERMINATION	*Valid*	True-Positive	False-Positive (Type 1 Error)
	Not Valid	False-Negative (Type 2 Error)	True-Negative

Each of these possibilities ordinarily has a probability figure attached to it, and these rates are known with more or less confidence. Here is not the place to explore the details of this point, for evaluating the nature of these error rates would take more space than this Comment permits. Suffice it to emphasize that whatever decision a court makes regarding the admissibility of expert testimony, it faces the prospect of making one of two kinds of error. Given a particular amount of research, it is possible to minimize one kind of error while increasing the likelihood of making the other kind of error. Thus, in legal terms, it is possible to increase conviction rates by lowering the standard inherent in the traditional view that it is better to let ten guilty go free than to convict one innocent person. But making it easier to convict guilty people means that more innocent people will be convicted.

Returning to the Bendectin example, a judge's decision regarding the admissibility of expert evidence that the drug causes birth defects might be wrong for one of two reasons. The court could conclude that Bendectin is, more likely than not, a teratogen when it is not (false-positive) or that Bendectin is, more likely than not, not a teratogen when it is (false-negative). The consequences of these two kinds of error are very different. A false positive means that the litigation goes forward, possibly resulting in, among other things, "erroneous" verdicts for plaintiffs, the removal of a beneficial drug from the market, and bankruptcy for the defendant. A false negative means that the litigation ends, and, among other things, the possibilities that plaintiffs injured by the defendant's drug do not

receive compensation and a dangerous drug remains available in the marketplace.

Which type of error, false-positives or false-negatives, is better avoided is a policy judgment that is an inherent component of expert evidence. Whether judges want this responsibility or not, it is theirs. Ignoring it does not make it go away. Professor Nance's flatland ignores this dimension of the scientific method.[22] In flatland, the societal consequences of empirical error are not part of the admissibility decision because Nance's vision lies only along the evidentiary plane. It is not that this error somehow disappears or becomes inconsequential as a result. It is still there, but it lies in a third dimension that is outside the courtroom. The science policy choices that are presented by this third dimension are never consciously made, though they surely have consequences in the flatland courtroom and in flatland society beyond.

IV. CONCLUSION

In the world of flatland, inhabitants cannot perceive anything that occurs outside the two dimensions in which they live. From their vantage point, the third dimension might as well not exist. Certainly, it cannot be accounted for, and can only be dimly understood. Professor Nance approaches the subject of expert evidence much as a flatlander might—in two-dimensional space. In Nance's flatland, judges would pay no heed to what occurred before the expertise reached the courtroom and would ignore what might happen to the expertise subsequently. More troubling, Nance's flatland-judges fail to consider the ramifications of their admissibility decisions both in regard to the development of the respective expertise and in terms of the costs of errors—both false positive and false negative—for society at-large. In the two dimensions of Nance's flatland, his proposals admittedly appear sound enough. When his proposals are viewed in three dimensions, however, they lack depth and substance. Since courts exist in three dimensional space, where their admissibility rulings have consequences for the world beyond, standards for expert testimony should be rendered in three dimensions. Anything less is flatly inadequate.

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[22] Professor Nance's flatland perspective ignores most aspects of the scientific method outside of the courtroom. In addition to the error rate problem, Professor Nance ignores the effect the law can have on the development of science, the guild issues that infect some areas of science, and the evolutionary and dynamic character of the scientific enterprise.