The Role of the Untrue in Mathematics

Chandler Davis

We obtain perspective on any human activity by standing outside it.[*] If mathematics were really concerned mostly with truth, or entirely with truth, then we may imagine that in order to appreciate it fully we might be obliged to position ourselves squarely in a world of fallacy: get a little distance on it. I am not proposing anything quite that quirky. I will speak just as unparadoxically as my subjects permit, but even so, there will still be slippery borderlines. There are plenty of ways in which untrue assertions demand our respect.

Mathematical reasoning can be applied to untrue assertions in the same way as to true ones. We may say to an 18th-century geometer, "Let us assume that through any point not on line *l* there is more than one line which fails to intersect *l*," and our interlocutor, no matter how absurd this assumption seems, will be able to scrutinize our deductions in the same way as if we had made less preposterous assumptions. It is necessary for us to be able to reserve judgment in this way—for consider this example: we may say, "Suppose if possible that p/q is a fraction in lowest terms equal to the ratio of a square's diagonal to its side," and we may want to establish that that supposition is not admissible. Then it is important that our interlocutor agree as to what reasoning is valid. If the rules changed, if there were one way accepted for reasoning about true assertions and a different way for all others, then there would be no way to prove anything by contradiction.

Let me nail this observation down a little more snugly. It is not an observation about *tertium non datur*. Maybe our interlocutor is skeptical about the notion that every meaningful proposition must be either true or false; that's all right: even if we allow that truth status need not be a binary alternative, still when we want to argue that a proposition fails to have some truth

status, we may have to use methods of argument that do not depend on that truth status.

Lawyers are clearer about this, perhaps. They frequently say, "Supposing, *arguendo*, that . . ." and proceed, arguing temporarily as though they were conceding a premise that they are not at all willing to concede. I like that notion of *arguendo*. Mathematicians used to use it more than they do today. In particular, the whole magnificent edifice of classical continuum mechanics seems to me to be a case of supposing *arguendo* that continuous media obey laws of particle mechanics which, however, the Bernoullis and Euler did not really expect them to obey: a dollop of matter has mass as though it were localized at a point, and Newton's laws are invoked even in problems where the idealization to point masses would be nonsensical. Maybe I'm on safer ground if I cite a different example: the development of topology of manifolds in the 20th century. It was plain that certain aspects of manifolds deserved study, but it was not clear what they applied to—whether to certain chain complexes, or to certain abstract topological spaces, or what. The study proceeded *arguendo* by deductions as reliable as they could be made under the circumstances, and discrepancies between different entries into the subject were tidied up as well as might be. (The forging ahead and the tidying up are seen together in a book such as Raymond Wilder's.) A chain of reasoning belonging to such an intellectual domain may turn out in the future to relate two chapters of truth, or it may turn out to be part of a great *reductio ad absurdum*; we deal with the deductive chain *arguendo*, independent of its ultimate fate; the tests of its soundness are the same either way.

Even more persuasive for my purposes today is another centuries-old habit of mathematicians: to find what value of the variable makes a function zero, one pulls a guess out of the air and substitutes it into the function, finding of course that the function fails to be zero there; then one extracts information about the problem from the failed guess. Such calculations by *regula falsi* were used off and on over the centuries to become systematized and exceedingly fecund from the 16th century on. Their naïve motivation must have been, back then, like reasoning *arguendo*, and this doesn't seem far off to me even in retrospect. One would be justified in 1100 (or in 1600) in trying 1.5 to see whether its square was about 2, even if one did not have an algorithm of root-finding and therefore did not know how taking this stab at $\sqrt{2}$ would lead one to a better guess. One would anticipate that working with the blind guess would teach something. And I observe in this context too, of course, that in order to hope to be taught anything, one would surely commit to reasoning the same for a wrong guess as for a correct one.

The freakish notion that mathematics deals always with statements that are perfectly true would disallow any validity for this example, or for most

discussion of approximation, for that matter. It would insist that "$\pi = 22/7$" be banned from mathematics as utterly as "$\pi = 59$"; half of our subject would be ruled out. There is no danger that mathematicians of this or any other age would really try to live by this freakish doctrine, but it persists in everyday discourse about mathematics. I began with discussion unrelated to approximate answers in order to emphasize that restriction to true statements would be crippling to even the most finite and discrete branches.

As we begin to examine the useful roles of less-than-true statements in our field, we have at once these two:

- Falsehood is something to avoid. We find it useful to reason by contradiction.
- Statements teach us something by their behaviour in reasoning arguendo, regardless of their truth value.

Then to continue the examination of the subject, we must recognize and defy the tradition that mathematics is truth, the whole truth, and nothing but the truth. Permit me to call it "truth-fetishism," though I accord it more respect than the playful label suggests. This tradition has taken many forms, and you may not agree with me in lumping them together.

By the 19th century, it had become clear that some true statements are contingent whereas others are essential to the cogency of human reasoning. (Thus it is merely a matter of observation that the South Pole is not in an ocean, and we can talk about it being in an ocean, even ask whether it once was; but we can not talk in any cogent way about the South Pole being on the Equator.) Truth-fetishism applied mostly to truths which were not contingent. In the decades after George Boole's "Investigation of the Laws of Thought," it became conventional to hold that, at least in philosophical and mathematical discourse, all true statements were equivalent, so that any true statement implied every true statement, and a false statement implied every statement whatever. If you have ever tried to get a freshman class to swallow this, you have probably appreciated the trouble Bertrand Russell had in his day.

Yes, I find it irresistible to retell the Russell anecdote: replying to the lay listener who objected that surely "$2 = 1$" does not imply that you are the Pope, Russell's put-down was, "You will agree that the Pope and I are two; then if $2 = 1$ it follows that the Pope and I are one." Now his verbal cleverness is charming, but it is off the point of the listener's objection, as the listener surely saw and we may hope Russell did as well. He was insisting that the only way to deal with truth and validity was the truth-fetishistic way, and that the only way to understand implication was material implication: that saying "*A* implies *B*" must be understood as saying "either *B* or not-*A*".

Boole's ambitious project of finding the laws of thought deserves the admiration it got. What kind of law should we hope for? We don't really want a prescriptive law ("thou shalt think in thy father's way") or a normative law ("here is the better way to think"), we want an empirical law, one that refers to thinking that is actually done. On the other hand, we can't insist that the laws of thought encompass our occasional pathology and our frequent simple blundering (to do that would be a formidable, never-ending task); so there is some normative selection; let us ask, however, for laws that apply to thinking as well as may be done. That doesn't mean surrendering to the truth-fetishists. Both before Boole's time and since, when given propositions A and B that have nothing to do with each other, a thinker does not set about inferring B from A.

It was natural, then, that even while truth-fetishism was extending its dominion, various resistance movements sprang up. Strict implication was distinguished from material implication, in the following sense. To say that A implies B could be regarded as a contingent statement even within logic, and there was a stronger statement that sometimes might hold: one distinguished the statement that A implies B from the statement that A must imply B, then one tried to elaborate rules of symbolic manipulation appropriate to thinking where both kinds of implication came into consideration. This "modal logic" of Langford and Lewis seemed to be a realistic strengthening of the vocabulary. Indeed, because there may be various bases for regarding an implication as necessitated, I even thought it worthwhile to allow for various strict implications within the same system. But in retrospect this program does not look like much of a success. None of the various algebraizations of strict implication seem to deepen one's understanding of thought.

At the same time there was some attention paid to allowing truth values intermediate between true and false, as by Jan Łukasiewicz. This is at least a start on embodying the notions expressed in everyday language by "sort of true" or "yes and no." It is a limitation to insist that the intermediate truth values be totally ordered—a limitation that could be overcome, and by the way, the corresponding limitation is not suffered by modal logic with multiple modal operators. In the later invention of "fuzzy logic" by Lotfi Zadeh, it is claimed that still greater flexibility is obtained.

In short, the 20th century brought us to an acknowledgement that truth may be of various strengths. The Gödel incompleteness theorems suggested that this was even unavoidable, that no matter how faithfully one hewed to the line that truth was the goal, there could never be a notion of truth that would sort all possible mathematical statements into an army of true ones and an army of others (their negations). If every axiom system leaves undecided propositions, then it seemed that every mathematician on the corridor might make a different choice of what arithmetic facts were facts.

Yet my deep discomfort with truth-fetishism is not addressed by making truth relative to a choice of axioms for set theory and arithmetic.

We can agree that asserting "*A*" is distinct from asserting "*A* is provable (in some specified axiom system)" and distinct again from asserting "*A* is provable (in such-and-such other system)"; certainly all are distinct from asserting "*A* is nine-tenths true" or "*A* is sort of true"; and the list of options can be extended, as I will presently maintain. None of the options takes care of the big issue: logic based only on truth values is an impoverished logic, in that it sets aside intrinsic relations between concepts. I stipulate, in case it is not already plain, that by "relations" here I do not mean subsets of some direct product, as in many elementary developments of mathematics. I mean substantive relations.

Let me turn to some other quarrels I have with truth-fetishism.

Many spokesmen may say, since Boole, that all true theorems are equivalent and every true theorem is a tautology. As hyperbole, I understand this and endorse it; but oh, what it leaves out! First, it renounces any distinction between hard and easy theorems; second, it renounces any distinction based on what the theorems are about.

Similarly, many spokesmen may say that our aim in mathematics is to simplify every proof to self-evidence—that the ultimately desired proof of a statement's truth not only is necessarily tautological but also is plainly so. (I recently saw this thesis attributed to Gian-Carlo Rota, but many before him subscribed to it, and he, to my reading, did not.) Again, this ignores too much. Granted, we try to clear away the extraneous, and the Book proof never goes off on an unnecessary detour; but sometimes a theorem is valued for bringing together pieces from different conceptual sources, and this payoff may be reduced if we simplify to allow quick attainment of a conclusion.

A third, grotesque example: some metamathematicians would have it that a mathematical theory is the set of propositions that are true in it. I have ranted against this interpretation elsewhere, maybe I don't need to belabour it here, but please indulge me while I do. A theory that is any good says something, opens some door. It has high points and central points; it has beginnings (I don't mean only axioms) and endings. Most likely it has avenues to other theories. The collection of all propositions that are true in it, on the other hand (if we could ever apprehend such a monster, which I doubt), consists mostly of banalities, so there can be no order, no revelation or insight. Such is not our science.

We must try to get realistic about deduction, the deduction we actually do at our best. What may one say in practice about a statement? On just the dimension of truth value—one may assert it; or going farther, one may claim to be able to prove it; or going less far, one may say one tends to believe it. If this is within mathematics, however, one probably says something

with more structure. Let's take a realistic possibility. One may say, typically, "I think I can prove it with an additional hypothesis." But stop right there! The truth-fetishist calls this vacuous; for of course the additional hypothesis could be the conclusion, and then of course it can be proved. We know this is irrelevant; we want to protest, "This is beside the point," just as Bertrand Russell's listener did; we feel the need for a sense to ascribe to the property "provable with an additional hypothesis." But needs such as this have not been described by modal logic or many-valued logic or fuzzy logic, and I suggest that enlarging the lexicon of truth-values is not the way to go to describe them. Truth—even truth understood in some new sophisticated way—is not the point.

The point is pertinence. The point is relevance.

In the last half-century, serious efforts have gone into analyzing relevance, but they commonly rely on deliberately ignoring the content. This approach is admittedly a sidetrack from the direction of my quest, but I can't brush it off. Today one may marshal computer power to discover which of a large population (of people; of factors in a plan; of propositions) are most related, but one often is looking just for the existence of some strong relation rather than for its nature. Only connect, as E. M. Forster said! This may be done in a search engine by looking for the singular values of a very sparse matrix of very large order. It is the few non-zero entries that guide us. At the intersection of Bacon and Shakespeare in the matrix appears a rather large number; people who deny that Bacon wrote the Shakespeare plays are there, right alongside those who affirm it, indiscriminately; and the weighting is upped by any discussion of the issue, including the present one, whether or not any new insight is achieved. In short, the relations are in the form of a weighted graph with positive integer weights. I can even give an example much closer to home: suppose two words are connected if they are often used in the same utterance; then "knife" and "bandage" will both have connections to "wound" although one causes the wound and the other is a response to it. This is an approach that suppresses syntax and even the distinction between yes and no.

As psychologists speak of the impact of *mere exposure* to a stimulus regardless of positive or negative reinforcement, this way of boiling down intricate data uses *mere association* regardless of the nature of the association.

The approach seemed wrongheaded to me at first, I confess. Architects said they broke down their design task by drawing a graph of which considerations were related and then analyzing the graph computationally to find subgraphs—sub-tasks—which could be carried out by separate teams. This was said to lead to efficient sharing of design effort; I was skeptical. But mere association can be a precious bit of knowledge these days, and I will pause to acknowledge it.

What leads people to apply such blunt tools is the extremely large number of variables one may be trying to handle in many an application today. Take again minimization of an objective function. If there are fifty thousand independent variables, inevitably most of them will be without effect on the function's value, and in such a fix, the finding of one that does have an effect is a big part of the solution, even if one doesn't find out at once what value one should best assign to it. What's more, it may be that not one of the variables affects the value of the objective function enough to rise above roundoff error: only by a better choice of coordinate system, perhaps, can directions having noticeable effect be chosen. There are many contexts that impose hugely many interacting variables, but I want to mention one where this so-called "combinatorial explosion" sneaks up on us. In behavioural evolutionary theory, the traits whose selection one seeks to reconstruct are not life histories but *strategies*, that is, complete repertories of responses to life's predicaments; thus any serious attempt must run up against the game-theoretic feature that the number of strategies grows with the size of the game faster than polynomially.

Perceiving pertinence may be undertaken, then, by means having a family resemblance to the extensional characterization of properties, by methods in which the nature of relations between two things is banished from consideration.

I do not cease to feel that pertinence should be respected as having structure, that what we employ, whether in reasoning or in observational science, should be not mere association but the structure of the association. Even if we call on high technology to explore a graph of connections between items, it will be natural to refine it to be a directed graph, a colored graph, and surely much more. Only connect!—but there is such a wealth of ways that two nodes may be connected.

Between two propositions there may subsist (aside from their truth or falsehood, as those may be in doubt) the relation that one entails the other, or the converse, or both, or neither. Though this may be all the truth-fetishist recognizes, we see every day that the relations possible between propositions are much more diverse. Simple illustrations will make my point. Let me begin with a mantra of 20th-century math education: "'but' means 'and.'" We all know that this makes partial sense: namely, if one says "John is poor but happy" one is asserting both "John is poor" and "John is happy". Nevertheless "but" is a major component in the structure of thought (like "nevertheless"), and the version having "but" as the connective is not the same as the conjunction of the two simple assertions. Many English speakers would find "John is poor but happy" cogent but not "John is rich but happy." You will easily find more and subtler everyday examples. Examples

within mathematics are subtler, inexhaustible, but more elusive; I will content myself with the one I already gave.

Of course I do not maintain that natural languages contain all the precision we seek for our logic. On the contrary, their ambiguities are sometimes just concessions to imprecision. If one says "John is rich so he is happy," it is not clear whether one means to assert that every rich person is sure to be happy; it is clear only that something is being said beyond the conjunction of "John is rich" with "John is happy." Similarly for the connective "aussi" in French. I do maintain that syntax of natural languages and our experience with reasoning can yield a great enrichment of our logical conceptual resources. The reason proofs are expressed in natural language is not only our deplorable lack of facility in reading formulas (however large a factor that may be), it is also the great power of nuance in natural language. The proper continuation of Boole's program is to do as much for relations as he did for truth-versus-falsehood. There is gold in those hills. My prospector's hunch is that the most promising underexploited lode is prepositions.

By now I have surely advanced enough dubious doctrines for one afternoon, but if I stopped now you would feel the lack of any mention of probability theory. It must fit into my talk somehow, right? Just so: it is a part of mathematics, it deals throughout with propositions which may turn out untrue, and I do have some dubious things to say about it. I was just saving it for last.

I have been discussing mostly the 19th and 20th centuries, but we must glance back now to the 16th. At its inception, was probability regarded as a competing notion of truth? There is no doubt that the idea of probability was close to the idea of truth at that early stage—etymologically, "probable" is "provable," and even today, "probity" means utter reliability—and the emerging notion of something having positive probability had to be disentangled from the different notion of appearing credible. This fascinating story has been closely studied in recent years, especially by Ian Hacking and Lorraine Daston, and I have nothing to add to their work. I pick up the story with the incorporation of probability into physics in the 19th century and its reconciliation with mathematics in the 20th.

The first development, the creation of statistical mechanics by Ludwig Boltzmann and others, and its success as a part of physics, has a consequence for the idea of truth in mathematics. Some statements about physical systems are definitively shown untenable if they are shown to hold with probability 0—or just with probability extremely close to 0. An applied mathematician has an obligation to accept the conclusion that the ice cube melting in your glass of water is not going to separate out again into ice, because the molecular theory that assigns to that outcome a prohibitively low probability is successful. The theory also says that the sequence followed by the

molecules during melting is reversible; the reverse process is nevertheless ruled out, and the argument uses probability. If G. H. Hardy or some other deplorer of applying mathematics wants to retain a notion of possibility for the ice cube to be reborn, fine; all I am saying is that a different notion of possibility and impossibility has emerged in statistical physics. That it happens to involve probability extremely close to 0 instead of probability 0 is just an aggravation of the antithesis. We have a hierarchy: something may be known untrue; or more generally it may be known to have probability 0; or still more generally it may be known to have so low a probability as to be ruled out. Let me emphasize that the consequences for relating predicted behavior to observed behavior are the same for all three. A physical theory which disproves a phenomenon we observe is refuted; but a physical theory which assigns a prohibitively low probability to a phenomenon we observe is refuted just as thoroughly. Statistical physics has extended its sway in the last hundred years and we must live with it. Its criterion of truth deserves our respect.

Finally, a look at probability as a mathematical theory. With A. N. Kolmogorov's wonderful little book (1933), probability seemed to have earned a place at the table of mathematics. Its special notions had been put in correspondence with notions of analysis and measure theory which were as clear as the rest. Aside from its application to gambling, insurance, and statistical physics, probability was now welcomed as a tool within mathematics. To speak of an event having probability 0 was exactly to speak of a subset having measure 0. A striking string of theorems came forth over the years. One did not conclude that a phenomenon was certain to happen by proving its probability was 1; but if one could prove its probability was 1, or merely that it had probability greater than 0, one could conclude that it was *capable* of happening: a set of measure greater than 0 had to have some elements in it. The first striking achievement of this sort long predated Kolmogorov, actually (and it was expressed in the probabilistic terms): Emile Borel proved that a sequence of decimal digits chosen at random represents a normal number with probability 1, and this provided the first proof that it is even possible for a number to be normal. (Many of you know the definition: a decimal expansion is normal provided that all sequences of k digits occur as subsequences of it with the same asymptotic frequency, and that for every k. Correspondingly, other bases than ten can be brought in.) Now I comment first on how sharply the relation of probability to mathematical truth here contrasts with what we saw in physics. The number-theorist contentedly deals with sets of measure 0, and moreover values the positive measure of a set primarily for its guarantee that the set is not empty.

While we were talking physics, there was perhaps a temptation to live on the intermediate level of the hierarchy I mentioned: to disbelieve in

events of probability 0. That doesn't work, of course. Yet there is a serious
catch in the number-theorist's usage too: to say that measure greater than 0
ensures that a set has members is to defy intuition. The intuitionist re-
sponds, "The set has members? Really? Show me one." Today, after a cen-
tury of debate, this catch is clarified, but, far from going away, it appears
insuperable. The constructible numbers (by any appropriate definition) are
a set of measure 0; yet they are the only numbers that might be shown. In
the conventional terminology of 20th-century analysis, almost all real num-
bers are not constructible; in our experience, every real number that can be
specified is constructible.

Now Kolmogorov knew all of this, he understood it better than the rest
of us do, yet it seems not to have bothered him. Shall I assume that he had
confidence that we would be able to straighten things out after his death?
That's kind of him. By all means let us try.