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ON MATHEMATICAL OWNERSHIP IN RESEARCH: A LOOK BACK AT WORDS OF FRANK F. BONSALL

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Abstract Words of Frank F. Bonsall (1920–2011) on what ownership of work should mean for mathematicians—taken from a thought provoking essay published over forty years ago—are still pertinent in 21st century academia where important aspects of intellectual probity lie at the heart of research endeavours.

"Mathematical rigor is like clothing: in its style it ought to suit the occasion, and it diminishes comfort and restricts freedom of movement if it is either too loose or too tight."

George F. Simmons

1 Context

In 1982, English mathematician Frank Featherstone Bonsall (best known for his research interests in functional analysis) was moved to compose an essay where he opined on a variety of matters mathematical ('A Down-To-Earth View of Mathematics', *American Mathematical Monthly*, Vol. 89, pp. 8–15). He expressed some stances which, in hindsight, have not stood the test of time so well (he saw no place in mathematics for the large scale teamworking on problems found in experimental sciences of the day, and took a dim view of computer-assisted/generated proofs—both of which have come to pass and are gaining momentum across the mathematical community), but he did have interesting things to say about the way mathematicians should go about deploying results in their research that are taken from external sources—something, of course, on which a large proportion of work relies. This topic is part of a wider discourse to be had—on the way we, as professional mathematicians, conduct ourselves and uphold the integrity of the discipline—and I thought it instructive to pick out those considerations of Bonsall that are germane to a discussion on the issue of 'ownership' as they retain a resonance today still and offer a valuable point of reference to anyone in the business of creating new mathematics.

2 Bonsall on the Practice of Research

Demanding a certain personal standard that he felt was required in directly appealing to, and utilising, the work of others for research purposes, he wrote

> "I am not concerned with logical refinements but only with the practical necessities of the science of mathematics as practiced in the real world. In using somebody else's theorem, I cannot rely on the distinction of the author or the prestige of the journal but must check its correctness for myself. I must myself understand how the theorem follows from the axioms with the aid of such other theorems as I have already checked. This act of understanding cannot be performed for me by anyone else, still less by a computer, and I have only one lifetime in which to work. Even if I am fortunate enough to be free from other work and distractions, the greatest enemy to accuracy is boredom. Thus to perform its health-giving function a proof must be understandable by a real

> live mathematician in a reasonably short time. What is reasonable will depend on the importance of the theorem; . . .
>
> How we check correctness is up to us; it is our own reputation that is at stake. Some have the patience to do a meticulous line-by-line verification, others will rely mainly on checking with examples and on their experience and intuition to direct them to check the crucial steps. The meticulous detailed check is not necessarily superior. Not only is boredom a deadly enemy, but some major gap may go undetected. Everything that is written down may be perfectly correct but it may still leave the theorem unproved. The best check involves both methods." (p. 10),

and, shortly afterwards (p. 11),

> "It would be splendid if all proofs could be intelligible to all professional mathematicians. In our real world, understanding a proof needs a great deal of prior knowledge, at least in the more highly developed branches, though we can fairly demand that this prior knowledge be available from published sources. Our concept of the science of mathematics requires that we should personally understand the proof of a theorem before using it. The need to master a great deal of difficult mathematics before we can use a theorem severely limits the speed at which we can progress."[1]

One clear hindrance to this are mistakes that show up in disseminated articles, and Bonsall noted that (p. 9)

> "All human beings are fallible, and so errors inevitably occur, ranging from serious errors of understanding to minor misprints, which are very difficult to eliminate because of the tedium of proofreading. In ordinary prose there is enough redundancy for a scattering of misprints to be no more than a minor nuisance; but there is little redundancy in a mathematical formula and a single misprint may well change the meaning completely."

He expanded on this accordingly:

> "It might be interesting to attempt to classify the errors of professional mathematicians. They range from trivial misprints to thumping great mathematical howlers. A not uncommon sort of error involves some obvious foolishness [in writing] . . . , in which case the reader can make the correction without difficulty. But . . . , the author may have gone on to base the subsequent argument on this [blunder]. Then the reader will have trouble. A more serious sort of error is one which invalidates some important conclusion but does not stem from some obvious foolishness but rather from some subtle misunderstanding. The fact that errors exist in almost all major publications would seem to make it impossible for mathematicians to use the work of others without the entire subject collapsing into a morass of uncertainty. And so it would, had not mathematicians devised a scientific method through which the important theorems become known with a degree of certainty that is lacking in all other human activities."

What he referred to here relates to something that has developed traction elsewhere in some academic circles in the context of scientific publications containing deceptive results—through genuine oversight or (in the hurry for release to serve personal recognition and repute) as a deliberate act of fakery—which, to make matters worse, might lack enough methodological details to allow independent 'repeatability' and in turn prevent an acceptable degree of verification; he emphasises the importance of procedures in place to mitigate such events:

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[1] Bonsall acknowledged that many applied mathematicians naturally feel less responsibility for the correctness of theorems from pure mathematics to which they turn to facilitate and progress work. That is a reasonable enough point, but he perhaps overplayed his hand a little in judging that (p. 11)

> ". . . Because of the brevity of human life [mathematicians] have to content themselves with reading enough of the proofs to be sure that they are interpreting the results correctly. If this were to become a widespread practice it would be fatal for mathematics. So long as it is more or less isolated it may not matter [too] much, [for] fallacious results will usually be dead branches or will eventually lead to obvious absurdities."

It is without question in our nature that we prefer the actively creative side of mathematics to toiling under some of the more mundane details precursing and/or stalling progress, and to suppress such urges in the name of a perceived optimal rectitude is a big ask, as it were.

> "It is well known that experimental and observational scientists have devised a scientific method whereby they can use the work of others. In this method, results and observations are published together with sufficient information about the experimental techniques to enable other experts in the field to verify the results for themselves. In this way, errors, and even fraudulent claims, are eliminated. It is sometimes argued that this picture of the scientific method is illusory in that the great majority of experiments are not repeated. It is no doubt true that many results are of a routine nature, appear entirely unsurprising to other experts in the field, and may not be tested by repetition of the experiment. But the important results, those that conflict with the expectations of experts or change the subject radically, will be tested."

On mathematics specifically, Bonsall noted that mathematicians, too, have

> ". . . devised an effective scientific method appropriate to their subject; but, perhaps because the word *science* is usually attached to the experimental and observational sciences, this is less widely understood. This scientific method for mathematics involves the publication of results in the axioms-theorem-proof form. Introduced by the classical Greeks more than 2,000 years ago, this form is now almost universally in use. The explicit statement of the axioms (or definitions, I make no distinction) enables another mathematician to decide whether the theorem is applicable to his own problem, and the proof enables him to check that the theorem is correct.[2] The proof also provides some very useful redundancy. Mathematics has to be written in ordinary language supplemented by formulae, and it is often written with insufficient care or with "abuse of notation." Every mathematician is familiar with the experience of needing to dip into the proof before being able to understand what is being claimed in a theorem."

He continued thus, making suggestions to ensure that the welfare of mathematics is robust (pp. 9–10):

> "By the *science of mathematics*, I mean the collaborative activity of mathematicians publishing their work in this form, each author accepting full responsibility for the correctness of the whole of his publication *including the results that he quotes from other authors*. Pursued in this way, mathematics can remain permanently healthy, significant error being eliminated automatically. Everything in a publication must be based on the individual understanding of the author, nothing being accepted on authority, no matter how distinguished. Since the author is staking his reputation on the work of other authors that he uses, that work is checked by somebody who has the strongest incentive to detect the errors. This is far more effective than relying on referees, reviewers, and other more or less passive readers. A second mathematician using the work of another may himself fall into error in following the proof, but at least he has a strong emotional drive to avoid such error. In fact, if human nature is what it is commonly supposed to be, he may well have a stronger motive to detect error in that work than the original author had. It is true that this scientific method will not detect the errors in theorems that are never used. But that is unimportant; such a theorem is a dead branch anyway."

[2] Bonsall clarifies this point, writing (p. 10)

> "The nature of the axiomatic method has frequently been misunderstood even by distinguished mathematicians. It has sometimes been interpreted as an attempt to start from some primitive axioms of set theory and then to build the whole edifice of mathematics on this foundation by rigorous logic. This is the very opposite of the axiomatic method as understood by the real live mathematician. Instead of tying him down to some dubious foundations the axiomatic method allows him to dance in the air by taking anything he pleases as his starting point.
>
> The words and symbols used in mathematics must be defined, but can only be defined in terms of other words and symbols, and so in the last resort cannot be defined at all. Thus when Jones uses Smith's theorem he may not know precisely what Smith meant. But this is of no importance; if Smith's theorem is correct for Jones's concepts, Jones can go ahead and use it. Likewise we need not concern ourselves with the logical language or rules of proof that are used. It may be that Jones, when he reads Smith's proof, will sometimes decide that the proof does not satisfy his own logical requirements. In that case his remedy is either to find a new proof or to reject Smith's theorem."

Fair enough, I think.

He set down his thesis that the discipline only has a self-correcting mechanism if individual mathematicians take full responsibility for what they publish and do so with absolute ethical veracity, raising questions (still valid) about the levels of conscientiousness that one mathematician holds over another when applying—either directly or indirectly—the results of somebody else (by inference, this spills over into the general approach to the construction of theorems, the formulation of results, and the production of papers—in other words, one's whole *modus operandi* from start to finish across the research cycle). It is quite telling that Bonsall recognised that thoroughness and diligence in this regard has a non-negligible time cost, and one wonders how much accuracy in analysis/computations is lost in the modern day rush to produce papers as almost sole evidence of an existence which is both credible and creditable. "Less is more, sometimes", as the adage goes, particularly so if the 'more' contains sloppy and/or error-ridden work that is misleading, embarrassing, unhelpful, and, ultimately, a disservice to mathematics (if a mistake is serious, and attached to a result seemingly important, it wastes the time of the community).

> "... it is unhealthy for mathematical science when we exploit some immensely difficult theorem ... long before we can verify its correctness. In the case of ... a spectacular theorem we can perhaps be reasonably confident that, if it were eventually found to be false, that falsity would become so well known that the resulting errors would eventually be eliminated. But a willingness to act in this way in the interest of speed weakens our resolve and may eventually be fatal—like too much riding in cars when we would be healthier on foot or at least on bicycles. Other more obvious kinds of bad mathematics, slipshod inaccurate work and too ready an appeal to "it is easy to see" are also due to too much haste [in publication]. ...
>
> ... If we find it difficult to understand some result that we want to use, we should not run away from it but should persevere until we really understand it. There is quite a good chance that in so doing we shall find some underlying simplicity that has been hidden under complications. If so, we shall have made a double contribution to our science; whereas if we go ahead and publish without such understanding we risk making a negative contribution. Again, our difficulty may stem from an error in the result; and if that error is nontrivial its discovery may lead to some very interesting new mathematics." (p. 14).[3]

I allow myself to digress a little. In 1950 Albert Einstein wrote a short commentary, 'On the Moral Obligation of the Scientist' (*Impact of Science on Society*, Vol. 1, pp. 104–105), outlining, *inter alia*, the parlous state of the planet and the gnawing tension in those scientists who seek understanding of the physical world while contributing to the creation of tools that undermine its security. He noted that the quest to derive insight into apparent complexity sits side by side with a search for simplicity and economy in underlying precepts, rules and assumptions. So, also, for mathematicians where we, like Einstein, balance such forces by possession of an exigent faith that permits their co-existence and without which he, for one, "... could not have a strong and unshakable conviction about the independent value [of] knowledge." He explained further:

> "This, in a sense, religious attitude of a man engaged in scientific work has some influence upon his whole personality. For apart from the knowledge which is offered by accumulated experience and from the rules of logical thinking, there exists in principle for the man in science no authority whose decisions and statements could have in themselves a claim to "Truth". This leads to the paradoxical situation that a person who devotes all his strength to objective matters will develop, from a social point of view, into an extreme individualist who at least in principle, has faith in nothing but his own judgment. It is quite possible to assert that intellectual individualism and the thirst for scientific knowledge emerged simultaneously in history and remained inseparable ever since.

[3]The notion of proof is not absolute, with ideas on what constitutes an acceptable proof having changed and reflected the state of mathematics as it has grown and matured over time. An excellent 1991 article, 'Rigor and Proof in Mathematics: A Historical Perspective', was published in *Mathematics Magazine* (Vol. 64, pp. 291–314) where Canadian mathematician and mathematical historian Israel Kleiner mapped out the evolution of the concept and prosecution of proof at those turning points during past periods that made the greatest contributions to its elucidation; the way in which he surveys central and core trends makes the paper both readable and enjoyable for any interested reader.

> Someone may suggest that the man of science as sketched in these sentences is no more than an abstraction which actually does not exist in this world, not unlike the *homo oeconomicus* of classical economics. However, it seems to me that science as we know it today could not have emerged and could not have remained alive if many individuals, during many centuries, had not come very close to the ideal." (p. 104).

The sentiments expressed are shared with others of a similar mind and countenance. In particular, these words from this intellectual giant remain true if, I contend, "science/scientist" are replaced with "mathematics/mathematicians"—this is why both the role of proof, and our relationship with it, lie at the kernel of those professional propensities driving much of what we do in research and how it is executed; proof, in whatever form it takes, reveals and shines a permanent light on our truths as a source of professional reassurance and gratification.

Papers offered first as a set of lectures at the University of Chicago during 1946 were published in-house the following year, in Vol. 1 of *Works of the Mind*, by the University of Chicago Press (eds. J. Adler and R.B. Heywood). Hungarian-American John L. von Neumann—whose genius was legendary even in his own lifetime—was a brilliant 20th century mathematician, physicist, computer scientist, engineer and polymath who contributed an essay titled 'The Mathematician' covering the nature of intellectual effort in mathematics. His opening words capture perfectly the assignment faced by anyone attempting to write about the topic in a coherent and informative manner, and perchance goes part of the way in explaining why some of us wrestle with things such as mathematical rigour, proof, detail, abstraction, aesthetic, application, ethical/moral concerns, error, (un)certainty, and so on, so much of the time. Placed in Appendix A, von Neumann's narrative backdrop gives a little more context to Bonsall's piece, existing as it does along with other works by those who have chosen over the years to ponder and register fundamental human issues in and around mathematical endeavour with measured acumen, shrewd finesse, and no small percipience born of experience and sophisticated thinking. The task has been, and continues to be, best taken on by those at the higher echelons of the discipline of mathematics who have gravitas and personality. One such person was the American mathematician and expositor Philip J. Davis who, in an interesting article 'Fidelity in Mathematical Discourse: Is One and One Really Two?' (*American Mathematical Monthly*, Vol. 79, pp. 252–263 (1972)), included a section (Section 5) on fidelity in mathematical proofs and echoed some of the points made by Bonsall (see Appendix B). To give some background to his paper—which had a broad aim of presenting some non-Platonistic aspects of mathematics—it was the outcome, he said, of computational experiments he had conducted in trying to prove and derive theorems in elementary analytic geometry (something Bonsall would have dismissed out of hand); they led him naturally and inevitably to speculate on the credibility level of a result proved/derived by computer as compared with one "hand crafted" in traditional fashion.[4]

3 Closing Remarks

Despite not having a Ph.D. himself, and in a very real sense being self-taught as a researcher, Bonsall supervised a number of doctoral candidates (who we learn knew him affectionately as "F.F.B."). It was not uncommon for those graduating around the time of World War II to forge a livelihood as university instructors and researchers in mathematics—as many had done before them—for opportunities often offered themselves to those who were good enough and had access to the kind of undergraduate education available to the elite few; a lifetime of pressure-free scholarhip awaited them, so it is little wonder some flourished and became commanding figures in their fields of specialism. Evidently, Frank F. Bonsall became a mathematician of some prominence (he was regarded as a leading expert in Banach algebras)—guiding young postgraduate students through their studies in caring and astute ways so that many of them went on to have successful careers,[5] undertaking (by outside request) various committee work over

[4]Interestingly, Davis—a prolific author of textbooks who moved from publishing technical material to take on a host of philosophical questions within mathematics that he felt needed further interrogation or fresh treatment—stated that arguments made in his essay brings one to conclude that, in some of its features, mathematics takes on the attributes of an experimental science.

[5]During the academic year of 1962–1963 Bonsall was said to have been directing no less than nine Ph.D. students while continuing to produce high quality research papers.

many years, receiving honours/awards, and continuing to publish until 2000—and we are the better for his deliberations on the chosen topic for this piece.

A few biographical notes are in order (drawn from the 2020 article by T.A. Gillespie in Vol. 69 (pp. 63–77) of the journal *Biographical Memoirs of Fellows of the Royal Society*). Bonsall, forced to interrupt his undergraduate studies at Merton College (Oxford) in 1940 after the outbreak of the war, served the remaining time of conflict and more in the Corps of Royal Engineers (including two years in India from 1944 to 1946, after which he was demobbed holding the rank of major). Upon completing the final academic year of his degree at Oxford, he accepted a temporary one-year lectureship at the University of Edinburgh from 1947 to 1948, before moving to a permanent post at King's College, Durham (later to be called Newcastle University), the following year (subsequently spending the academic year 1950-51 on study leave at the Stillwater campus of Oklahoma State University, where he began his work in functional analysis which sustained him for the rest of his life[6]). It was at Durham, under the influence of Polish born German W.W. Rogosinski, that Bonsall made up for his lack of research training and developed himself into an excellent researcher, eventually replacing Rogosinski as Chair of Mathematics in 1959 (the latter had moved ahead of him, from Aberdeen to Durham, in 1945, rising to Head of Department where he was in a position to recruit Bonsall having met him in the spring of 1948 on a visit to Edinburgh; a strident and talented mathematician, Werner Rogosinski fled Germany in 1937 and came to England at the invitation of G.H. Hardy (with whom he later collaborated) and J.E. Littlewood to avoid Nazi persecution). In 1965 Bonsall moved back to Edinburgh—to take up a recently instituted second mathematical chair (the Maclaurin Chair, named after Colin Maclaurin)—where he remained (bar the 1965–1966 academic year spent at Yale University in America) until retirement in 1984.

A man with grace and humour, he was not afraid to express views on issues close to his heart. He spoke publicly—at his inaugural address after appointment to the Edinburgh Maclaurin Chair—of the dangers of using the results of others without being sure of their truth, and his 1982 article contains a swipe at proofs that required the checking of special cases whose magnitude necessitated the assistance of a computer. He did not accept such so called proofs as authentic and, in a similar vein, at his 1990 speech of thanks for an honorary doctorate from the University of York he spoke on the pitfalls of over-dependence on computer models in science. Men such as Frank Bonsall are always worth listening to—even if on occasions one's own voice is a slightly dissenting one—as they are authoritative and proven academics.[7]

Having this in mind we finish with some words of a more general nature to round off the presentation of what is a fine commentary from a respected mathematical protagonist, and a champion of the proverbial cause for all of the right reasons (p. 11):

> "Live mathematics is that body of mathematical theorems that is currently understood by living mathematicians. A substantial trace of this mathematics is left behind in a fossilized form in publications, just as the coral reef is left by the polyps. Standards of rigour and the active interests of mathematicians change with time, and so in practice it is only quite recent publications that are actively used. The proportion of the human race that understands the notion of mathematical proof is quite small and the notion of proof does not seem to come naturally to children."

He added to this, and with a little more zeal,

> "In fact there is much greater readiness to accept authoritative statement than to undertake the effort needed for understanding. It is not at all hard to envisage the decline of our civilization into an authoritarian state in which mathematical understanding disappears altogether and mathematicians are replaced by priestly persons interpreting these

[6] For the record, he was granted a four month stay, over the winter of 1960–1961, as visiting professor at the Indian Tata Institute of Fundamental Research in the city of Bombay (now Mumbai).

[7] I am reminded of the view of the eminent German-American mathematician Richard Courant in his well known narrative 'Mathematics in the Modern World', *Scientific American*, Vol. 211, pp. 40–49 (1964):

> "The question "What is Mathematics?" cannot be answered meaningfully by philosophical generalities, semantic definitions or journalistic circumlocutions. Such characterizations also fail to do justice to music or painting. No one can form an appreciation of these arts without some experience with rhythm, harmony and structure, or with form, color and composition. For the appreciation of mathematics actual contact with its substance is even more necessary." (p. 42).

I feel it correct to say that these points are indisputable.

mysterious writings. Indeed, perhaps this happened in some ancient civilizations. It is not even necessary to require the suppression of mathematical understanding by authoritarian force. Perhaps the mathematical powers of the human race could be atrophied by soft and easy living and reliance on calculators at school."

This last sentence—given the problematic levels of mathematical literacy across many U.K. state schools—was certainly not out of place when Bonsall penned his work.

A Photograph of Frank F. Bonsall

Appendix A: On the Nature of Intellectual Effort in Mathematics (J.L. von Neumann)

Here we offer the opening paragraphs of the published 1947 essay by von Neumann as mentioned at the conclusion of Section 2.

"A discussion of the nature of intellectual work is a difficult task in any field, even in fields which are not so far removed from the central area of our common human intellectual effort as mathematics still is. A discussion of the nature of any intellectual effort is difficult *per se*—at any rate, more difficult than the mere exercise of that particular intellectual effort. It is harder to understand the mechanism of an airplane, and the theories of the forces which lift and which propel it, than merely to ride in it, to be elevated and transported by it or even to steer it. It is exceptional that one should be able to acquire the understanding of a process without having previously acquired a deep familiarity with running it, with using it, before one has assimilated it in an instinctive and empirical way.

Thus any discussion of the nature of intellectual effort in any field is difficult, unless it presupposes an easy, routine familiarity with that field. In mathematics this limitation becomes very severe, if the discussion is to be kept on a nonmathematical plane. The discussion will then necessarily show some very bad features; points which are made can never be properly documented, and a certain over-all superficiality of the discussion becomes unavoidable.

> I am very much aware of these shortcomings in what I am going to say, and I apologize in advance. Besides, the views which I am going to express are probably not wholly shared by many other mathematicians—you will get one man's not-too-well systematized impressions and interpretations—and I can give you only very little help in deciding how much they are to the point.
>
> In spite of all these hedges, however, I must admit that it is an interesting and challenging task to make the attempt and to talk to you about the nature of intellectual effort in mathematics. I only hope that I will not fail too badly."[8]

He didn't, unsurprisingly.

Appendix B: On Fidelity in Mathematical Proof (P.J. Davis)

In the aforementioned entertaining and erudite 1972 article, Davis wrote

> "The authenticity of a mathematical proof is established by verifying that a sequence of transformations of atomic symbol strings is legitimate. In point of fact, proofs are not written in terms of atomic strings. They are written in a mixture of common discourse and mathematical symbols. *Definitions* are made to serve as abbreviations for longer combinations of words and symbols. *Lemmas* are introduced as temporary platforms and scaffoldings from which one can argue with less fatigue and hence greater security. *Corollaries* are introduced for the psychological lift of obtaining deep theorems cheaply.
>
> *Splicing* two theorems is standard practice. In the course of a proof, one cites Euler's Theorem, say, by way of authority. The onus is now on the reader to supply the particular theorem of Euler that the author is talking about and to verify that all the conditions (in their most modern formulation) which are necessary for the applicability of the theorem are, in fact, present.
>
> If splicing is common to lend authority, then *skipping* is even more common. By skipping, I mean the failure to supply an important argument. Skipping occurs because it is necessary to keep down the length of a proof, because of boredom (you cannot really expect me to go through every single step, can you?), superiority (the fellows in my club all can follow me) or out of inadvertence. Thus, far from being an exercise in reason, a convincing certification of truth, or a device for enhancing the understanding, a proof in a textbook on advanced topics is often a stylized minuet which the author dances with his readers to achieve certain social ends. What begins as reason soon becomes aesthetics and winds up as anaesthetics.
>
> To go from the foundations of mathematics to any of the advanced topics on the frontier can be done in about 5 or 6 books. Perhaps 1500 pages of proof text of current style. This is humanely broken into smaller bits. The lengths of these smaller bits vary from discipline to discipline. . . .
>
> I do not know many people who would volunteer to check a fifty page proof. Value judgements would enter; it would depend on what is at stake. A purported proof of the Riemann Hypothesis might attract more checkers than the sum of two excessively long integers. But one doesn't have to deal with fifty page proofs: most proofs in research papers are unchecked other than by the author. But then, most theorems are without issue: the last of a line of noble thought. They remain unchecked in the light of usage. They are loaded with errors." (p. 259).

Noting the role of computers in checking hand work or developing new results, he stated that ". . . , the same remarks apply, but the probabilities may be altered. . . ." He then moved on to discuss fidelity in computing, and concluded

[8] I have been unable to source the original version of this essay, instead using a republication that appeared in the 2004 text *Musings of the Masters: An Anthology of Mathematical Reflections* (Mathematical Association of America, Washington (ed. R.G. Ayoub)); the quotation runs over pp. 172–173 (and the full piece over pp. 172–184).

> "The upshot . . . is that the authenticity of a mathematical proof is not absolute, but only probabilistic. Proofs have attached to themselves lists of discoverers, sponsors, users, checkers, authenticators, rearrangers, generalizers, simplifiers, rediscoverers, swamis, communicants, and historians. These lists are all incorporated into the scholarly apparatus of publication and in the constant exposure that goes on the blackboard." (p. 260).

As with von Neumann's writing, Davis', too, contains threads of ideas that were to be articulated later by Bonsall; informative, interesting, and supportive of them, they are worth including.

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