
Letters to the Editor



The Activism of Norbert Wiener

The celebrations of the centennial of Norbert Wiener's birth are over. His position as one of the great mathematicians of the twentieth century has been affirmed. The best living analysts have paid tribute to him and explained how much of their current research had its origins in the seminal ideas of this genius. Appropriate respect has been paid to his founding of cybernetics and to his influence in engineering, biology, and economics. Nevertheless, there is another aspect of this brilliant man that is not appreciated. Wiener the radical, a man of conscience and of great courage, who was opposed to institutions and encouraged independence. He would have even a stronger reputation today if he had courted powerful men and institutions rather than turned his back upon them.

We are all familiar with Wiener jokes. They are funny, and it doesn't even matter that many of them are also told about other famous men. They are told affectionately, but their continued repetition creates the impression that

Wiener was a foolish man, eccentric and egotistical. These jokes allow one to dismiss him and simultaneously praise his scientific work while belittling his social pronouncements. Even Pesi Masani's adulatory, and generally excellent, biography of Wiener apologizes for his hero's political acts.

Here are some of them. He resigned from the National Academy of Sciences. He declared, after World War II ended, that mathematicians should sever their ties to the military. He repeatedly tried to warn labor leaders about the effects of computers on jobs, recognizing the adversarial relationship between labor on the one hand and business and government on the other. He continued to criticize Harvard for the anti-Semitism he experienced there.

Certainly there is not enough room in a letter like this to explain why all these acts are political or how brave Wiener was to take up these issues

and how his reputation has been damaged by his refusal to play ball. But perhaps this quote, the preface to his book *Cybernetics*, will give the reader an idea of the kind of man Wiener was:

"Those of us who have contributed to the new science of cybernetics thus stand in a moral position which is, to say the least, not very comfortable. We have contributed to the initiation of a new science which, as I have said, embraces technical developments with great possibilities for good and evil. We can only hand it over to the world that exists about us, and this is the world of Belsen and Hiroshima. We do not even have the choice of suppressing these new technical developments. They belong to the age, and the most any of us can do by suppression is to put the development of the subject into the hands of the most irresponsible and most venal of our engineers. The best we can do is to see that a large public understands the trend and the bearings of the present work and to confine our personal efforts to those fields, such as physiology and psychology, most remote from war and exploitation. As we have seen, there are those who hope that the good of a better understanding of man and society which is offered by this new field of work may anticipate and outweigh the in-

cidental contributions we are making to the concentration of power (which is always concentrated, by its very conditions of existence, in the hands of the most unscrupulous). I write in 1947, and I am compelled to say that it is a very slight hope."

Right after the war Wiener refused a request by a missile engineer for one of his papers. He published his letter to the engineer in the January 1947 issue of the *Atlantic Monthly*, asserting that a scientist was responsible for the uses of his work: "I do not expect to publish any future work of mine which may do damage in the hands of irresponsible militarists." The idea of responsibility is central in both of these quotes, and he was right. The frenzied acquisition of tens of thousands of nuclear weapons has wasted our resources and has left us deeply in debt and plagued by mountains of dangerous bombs and nuclear waste. And the computer revolution has led to immense wealth for a very thin stratum of our society and to a general downgrading and unemployment for the rest. (After all, with better leadership and planning, the efficiencies brought by computers could have led to shorter work weeks, better education, and full employment).

As for the National Academy of Sciences, one may respect the institution and still admire Wiener, who chose to remain outside. Perhaps he remembered his pain at being an outsider for so long and chose to be in solidarity with all those who were excluded. Perhaps it was the same vein of moral discomfort which kept him from forgiving Harvard for the anti-Semitism experienced both by his father, a professor of Slavic, and by him as a graduate student and lecturer. It would have been beneficial for him to put this behind him and to play the "great man", but one could argue that it was better for Harvard and students of succeeding generations for him to remain critical and thus hasten reform.

I first met Wiener in 1959. Fortunately, I was made his assistant in a summer course he taught at UCLA on nonlinear problems in random theory. I was attracted to him by what little I

understood then of his mathematics and even more by his writings, particularly the introduction to *Cybernetics* quoted above, which helped me eventually sever my ties to the RAND Corporation. I returned to MIT to continue my graduate studies in 1961 and for three years was his graduate assistant. My duties were mostly papergrading and proofreading. (When I was finally ready to begin my thesis, he was already semiretired and spending a semester in Sweden, where he died.) I got to know him well and was very fond of him. He was always warm and accessible, and he was always lucid. Once I was walking in the hall at MIT with him when someone came up to him and started speaking to him. He stared up in the air, as though he didn't even see the person, and we walked on. Since I had met this person with Wiener a week earlier, I said to Wiener, "You know, this is...." He replied, in a firm voice, "Of course I know." I came to view Wiener's "lapses" as a defense mechanism. Almost everyone who saw him tried to speak to him, just, I assume, to be able to say that they had spoken with Norbert Wiener. I think that often his helpless, absorbed attitude was an affectation to gain some privacy.

Norbert Wiener's biographer, Masani, considers Wiener America's "second Leibniz", a profound philosopher and humanist. I think that he was also a man of great integrity and courage who used and risked his prestige to oppose militarism and the concentration of power in multinational corporations. Wiener wasn't the only great scientist of his time who spoke against the misuse of power. One thinks immediately of Robert Oppenheimer and Linus Pauling. It has only been thirty years since Wiener died. Where are the scientists today, aside from Noam Chomsky, who speak out for social responsibility?

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Reliability of Electronic Literature?

After reading Andrew M. Odlyzko's "Forum" article in the January *Notices*, I had a nightmare: electronic journals were the primary means of transmission and recording of scholarly progress in mathematics but sank to the quality of the newsgroup *sci.math*. The "publication continuum" was a river of submissions of dubious content, heaped with arguments and counterarguments by conflicting experts and crackpots.

I gave up trying to wade through the daily accumulation in *sci.math* years ago. To be sure, there were items of high quality, perhaps once or twice out of every hundred postings. I don't doubt that contributors find it enjoyable or useful to exchange comments (*ad nauseum* by my low tolerance), but I dread the possibility that this might become the standard format for mathematical literature. In the medium-to-long-term, the most important purpose served by mathematical literature is to provide reliable information on what is known. Odlyzko's prepublication continuum would make extracting information about past and current research the intellectual equivalent of hunting for antiques among the junk at an exponentially growing flea market.

Moreover, I do not believe that the reliability of the literature would be increased by further comments (particularly those of dubious and unmoderated correctness), and I am certain it would reduce the usefulness of the literature to near zero. Even if all supplementary remarks appended to a paper were correct and well considered, it would be quite difficult—particularly for a nonexpert—to determine which of possibly several apparently conflicting specialists is to be believed or to tease apart subtleties of statements based on conflicting terminology (various uses of "stability" and "hyperbolicity" in the context of algebraic and analytic geometry come immediately to mind, to say nothing of the brief correspondence I once had via *sci.math* with a computer scientist who called

the cyclic group of order n an “ n -dimensional torus”).

At “lower levels” of the continuum, these problems would completely swamp whatever useful information might be present. The lack of publication delay would only exacerbate the problem, since fear of posting something inane or plain wrong seems not to be a deterrent to the legions of crackpots who are currently excluded from publishing in respectable journals but who freely post to `sci.math` from commercial Internet servers. Essentially I agree with Quinn that the distinction between preprints and refereed articles should be absolutely clear. The convenience of having preprints made electronically accessible is obvious, but to make this the standard for mathematical literature in the future would be disastrous.

The security issues raised by having mathematical literature stored and transmitted entirely in electronic form are paramount, but I am not the person to address them. As an example, it seems that malicious computer hacking could make establishment of priority extremely difficult for electronic publications or could be used to alter their content. Libraries might, as policy, make hard copies for the sake of safety. This seems only prudent.

Drastic change in the future of mathematical publication is certain, but it is crucial to keep the present standards for acceptance and publication in journals. Technology now makes it possible to remove paper from the process; what should remain is the intellectual integrity of a refereed journal with the availability of a newsgroup.

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Forgive, But Don't Forget

Nobody demurred at holding an International Congress in Japan in 1990, and nobody should be reluctant to

hold one in Germany in 1998. Nazism, World War II, and all that—just water under the bridge, right? Forgive and forget, right? Maybe.

The wake of the Nazi nightmare for Germans is partly the process of recovering self-respect as a nation, and a major power at that. They have my support in this process, which is undoubtedly difficult.

We should be sympathetic to German mathematicians who survived the Nazi times. To collaborate with Nazism then, one did not have to advocate terror and racism. If one had the “right” ancestry, collaboration was made automatic. It was not advocacy of any politics. It was the path of least resistance, taken by the apolitical; it was the default option. And it was the path taken by most German mathematicians. We who were not there must understand this. Understanding it does not mean condoning Nazi philosophy and institutions.

I am uncomfortable about the Germans as hosts of our Congress, not because they claim respect as a people and as scientists, but because I see some of them equating this with a whitewash of the Nazi past. Nazism was and is wrong, not because its state lost a war, but because it was and is racist and anti-human. Germans should have full status in the international community as human beings, but that does not mean we should exonerate the many who denied the humanity of others or should pretend that they did not. Germans have every right to their glorious mathematical tradition, but one hopes they are proud to be heritors, not only of those like Ludwig Bieberbach who pled for Nazism and those like Helmut Hasse who went along with the tide but also of those like Emmy Noether who were forced to emigrate and those like Carl Ludwig Siegel who chose exile. (One who has this pride is Reinhold Remmert, *Mathematical Intelligencer* 17, no. 2.)

Reestablishing international amity means embracing the former enemy, but it does not mean embracing Nazism—rather, the reverse.

So let's attend the 1998 ICM in Berlin, but maybe it would be good to wear a T-shirt reading,

Forgive and forget?
I can forgive,
but I can't forgive you if you forget.

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Are Non-Ph.D.s Taking Jobs from Ph.D.s?

I am writing this letter anonymously because I am an untenured Ph.D. who might jeopardize employment prospects if it were published with my name on it.

Last year the *Notices* ran an article about the problem of too many Ph.D.s + too few jobs. A major source of the problem seems to get no attention: Many people with no Ph.D. occupy tenured positions at four-year institutions while Ph.D.s go without any job at all. Also, non-Ph.D. adjuncts fill enough courses to keep a large number of Ph.D.s unemployed. The purpose of this letter is to point out that this is a major source of the Ph.D. employment problem. Furthermore, I urge Ph.D.s, especially those with tenure, to stop cooperating with or granting acquiescence via silence to the practice of hiring non-Ph.D.s.

It is perennially claimed by the institution involved in the practice that the presence of non-Ph.D.s is only a legacy of the past, only a temporary measure, only related to remedial courses, etc. Then more non-Ph.D.s are hired, stay on forever, and end up teaching classes for college credit. It is fair to conjecture that the real motivation for hiring non-Ph.D.s is that they are cheap and disposable (until they get entrenched via longevity in the institution's political structure). Obviously, this motivation will always be strong. Therefore, we need to be vigilant.

There is a big difference between a high school teacher and a college professor—it is Ph.D. training that makes the difference. We know this,

but we tend to get hamstrung by the argument that it is hardly beyond reason that a non-Ph.D. could do a good job teaching, say, calculus. That argument could be taken a step further: It also is not beyond reason that some undergraduates could teach calculus well. But this misses the point: College students at every level deserve more than a good teacher—one who can clearly spell out the basic facts and skills. They deserve a high-level practitioner of the subject—one who has journeyed to and beyond the edge of knowledge. Hence the title Doctor of Philosophy, the intent of the doctoral thesis, and the rigors of its defense.

If we believe in the Ph.D. degree, then we should also believe that it is worth protecting. Note how the medical and legal professions protect their doctoral degrees. While it is certainly not beyond reason that an experienced registered nurse could diagnose and treat a variety of routine, minor injuries and decide which cases should have an M.D.'s attention, it is illegal for a registered nurse to diagnose or decide upon treatment for something as minor as a hangnail. While it is certainly not beyond reason that an experienced paralegal or legal secretary could handle minor, routine criminal and traffic violation problems and decide which cases should have a J.D.'s attention, no one but a J.D. can represent another in court. Why? Because the medical doctors and the lawyers don't want it. Why not? Let's not be naive: Doctors and lawyers know where their bread is buttered. We should take the isomorphic position in regard to our own doctoral degree as the J.D.s and M.D.s: namely, that a course taught by a Ph.D. in the respective discipline is a necessary condition for college credit to be given for passing that course.

Worked Hard for My Ph.D.

(Received March 20, 1995)

Editor's Note: It is the policy of the *Notices* Editorial Board not to publish letters written anonymously unless

the author can provide a compelling reason for the anonymity.

Shepp Replies to Vitulli et al.

This is in response to a letter of Professor Vitulli et al. titled "Some Misconceptions in Shepp Letter" in the *Notices*, March 1995.

Let me begin by saying that I agree with most of the policies of the AMS with respect to women. Specifically,

1. I agree that women are underrepresented in math and sciences as a result of (mostly subtle) pressure by society. This pressure is a remnant of the past, and it should not take place. Its elimination will benefit both women and mathematics as well as society as a whole.

2. I agree that sexual discrimination and harassment are unacceptable, including their subtle forms.

Now, here is where we disagree. Even though discrimination and harassment are shameful, you cannot eliminate them by redistributing rewards. Let me describe an analogous situation. I have spent many years fighting discrimination against the Jews in Russian mathematics. Suppose a Jewish mathematician, X, did not have the same opportunities and, as a result, did not achieve as much as his non-Jewish counterpart, Y. Let us further assume that if all conditions had been equal, X would have achieved more than Y.

Now, suppose both X and Y compete for the same reward, such as a faculty position or a grant, or both want to publish a book and there is only one slot. May we use the fact of the past discrimination against X to justify giving the reward to X rather than to Y, who is stronger than X due to his unfair advantage? Assume Y in particular is not guilty of any past discrimination.

My answer is no. It's two wrongs and no rights. I am sorry for X, but mathematics is mathematics. The same goes for hour speakers. By saying that the organizers should take extra steps to ensure that a potential woman speaker is not overlooked, the authors of the letter above imply that

such extra steps should not be taken with respect to any potential hour speaker. Any other interpretation is a form of double talk.

JCW documents advocate such things as incentive funding for meetings in which there are women speakers as a "mechanism and incentive for 'them' [men] to also invite junior women" rather than the "obvious big names". This is getting ridiculous.

As an editor of a journal, as a member of a panel considering grant applications, or in any other capacity, I make decisions based on the quality of the manuscripts, proposals, work, or achievements.

Women do not need the patronizing attitude, whether we call it extra steps or incentive. They need the same opportunities as men have. AMS should work on making this a reality. But those who distribute the rewards should be gender-blind. If that means that I am against a JCW resolution or AMS policy, so be it.

Shepp Replies to Lorch

Let me respond to Professor Lorch's letter by first quoting this part of his letter:

"He [Shepp] makes a mountain out of the molehill on which the 1972 AMS resolution promises to 'include more women on (a) Society programs and panels, including invited speakers and section chairmen, (b) committees and governing boards.'"

Unlike Professor Lorch, I do not view the 1972 AMS resolution as a molehill. (See my criticism of that resolution in my response to the letter by Professor Vitulli et al. published in this issue [above].)

Professor Lorch brings up another issue that was not part of the 1972 resolution, namely, the issue of the underrepresentation of minorities in mathematics (here we apparently agree—why indeed does the 1972 resolution refer only to women?). My attitude towards this issue is basically the same as that for women: namely, mathematicians and the entire society should make more efforts to attract minorities to our profession. Right

now, many talented young blacks do not have a chance to get a quality education at high school. Depriving them of that chance hurts them, hurts mathematics, and hurts the whole society.

However, what Professor Lorch (and also Vitulli, et al.) advocates is tokenism as a way of solving real and serious problems. We should distinguish between creating equal opportunities and deciding who deserves to be considered best. Tokenism may relieve our guilty conscience but it will do nothing to solve the real problem, and it surely will corrupt our values. Lee Lorch and I see the world differently, although we are old friends: to him, there are many eminent black mathematicians who, due to discrimination against blacks, do not get elected to the AMS Council. To me, the issue is that there are not enough eminent black mathematicians due to the lack of opportunities for black children.

On my suggestion to suppress given names to make it harder to determine gender, I did mean it seriously even though one could argue that it would not be very effective except for a "casual" discriminator. But it couldn't hurt, and if a person was systematically discriminating, maybe he would be caught persistently asking for irrelevant information. Maybe thirty years ago mathematics was small enough that all the members of the AMS knew each other, but things have changed. I will admit that as the erstwhile chair of the Committee to Choose Eastern Hour Speakers, I did not know the skin color of most of the people that appeared on the list of nominees; and if first names had been suppressed, I would not have been able to guess their gender either. I don't work in all fields of mathematics, and I don't know everyone. Nor do I care to know gender, skin color, income level, membership or nonmembership in the proletariat, religion, ethnicity, or any other irrelevant data in judging a mathematician. I am happy and eager to have Lee Lorch or anyone else nominate women and minority candidates for invited speakers, and committees ought to have people like Lee Lorch on them to ensure that

all classes are properly considered; but I believe the actual selection should be done on the basis of mathematical achievement alone. To help ensure this, I would propose something like the system I used: each member of a committee to choose speakers or other prizewinners would nominate candidates, suppressing both first names and items listed above as simply being irrelevant data. Each nominator instead should write a paragraph accurately describing the achievements of his/her nominee(s). The committee would then vote based on the descriptions. Some discriminators might know that some of the candidates are women and blacks and discriminate against them, but maybe others might discriminate against men and whites. The proposed method would avoid as much discrimination as possible, maybe(?).

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Communication, Not Just Publication

Readers of the April 1995 *Notices* may be interested to know that the "MSRI Workshop" discussed in A. Jackson's article of that title was known to its organizers (John Gage (SUN), Stu Loken (LBL), Andrew Odlyzko (Bell Labs), Dick Palais (Brandeis), and us), the participants, and those who helped fund it (including the NSF and DOE) as the conference on "The Future of Mathematical Communication". The emphasis was on communication and not just publication. Lots more information, links, and leads can be found on the Web pages for the conference: <http://www.msri.org/fmc/fmc.html>.

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Editor's Note on Notices Submissions

The editors of the *Notices* encourage readers to submit material for possible publication.

Articles should have content of significant interest to mathematicians and be from 2,500 to 6,500 words long. Articles on current mathematical research are particularly encouraged. Submission of articles (in ASCII or a TeX format) should be made by mail or e-mail (preferably both). Articles of shorter length can be submitted for consideration for the "Communications" section. Reviews, letters to the editor (specifically intended for publication), announcements, and materials for the departments should be sent to the Providence addresses below.

Submissions can be made to the Providence office (The *Notices*, American Mathematical Society, P. O. Box 6248, Providence, RI 02940; e-mail: notices@math.ams.org), the editor's office (Hugo Rossi, Department of Mathematics, JWB 210, University of Utah, Salt Lake City, UT 84112; e-mail: rossi@math.utah.edu), or through any of the associate editors.