

1. Weinstein E. Woodrow Wilson's neurological illness. *J Am Hist.* 1970; 57:324-51.
2. Bruenn HG. Clinical notes on the illness and death of President Franklin D. Roosevelt. *Ann Intern Med.* 1970; 72:579-91.

To the Editor: Marmor's view that Woodrow Wilson suffered from writer's cramp or joint or tendon inflammation due to excessive writing is not borne out by the record. By 1896 Wilson had been typing most of his work for years, and typing and golfing were also affected. Writer's cramp is further ruled out by sensory symptoms. In calling Wilson a hypochondriac whose condition was psychosomatic, Marmor portrays Wilson's personality inaccurately. Wilson was a man of great fortitude who rarely let his ailments keep him from his duties. As for "recurrent multifocal radiculopathy," the transient feeling of heat in Wilson's left shoulder that Marmor cites as a manifestation was a response to the strain of typing left handed. Wilson did not have involvement of his left upper extremity until 1913, and it did not recur. Nowhere in the Wilson papers is there any reference to involvement of the left lower limb.

The combination of sudden onset of enduring monocular blindness and episodes of weakness of the opposite upper extremity in a patient with hypertension and arteriosclerosis is clinical evidence of occlusive carotid-artery disease, rarely diagnosed until the 1950s. Both central retinal-artery occlusion and venous hemorrhage are features that have arterial ischemia as the primary event.¹ I use the term "stroke" to indicate any rapidly developing vascular lesion, occlusive or hemorrhagic. Marmor attacks my supposed belief that the retinal stroke of 1906 caused an organic mental syndrome with enduring cognitive loss, although my book² describes the effect of this catastrophic event on Wilson's emotional and social behavior.

Marmor disputes the evidence that Wilson had encephalitis in 1919, because he was not comatose, somnolent, or delirious. The cerebral manifestations of the epidemic were highly variable, and Wilson had marked changes in mood, had a delusional experience, and engaged in bizarre actions. Here Marmor seeks to discredit the observations of reliable witnesses.

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1. Hayreh SS. So-called "central retinal vein occlusion." *Ophthalmologica.* 1976; 172:1-37.
2. Weinstein EA. Woodrow Wilson: a medical and psychological biography. Princeton, N.J.: Princeton University Press, 1981.

The above letters were referred to the author of the article in question, who offers the following reply:

To the Editor: I am pleased that Cousin Rodman supports my view that Woodrow Wilson's symptoms were more likely a result of peripheral disease than of stroke. Carpal tunnel syndrome is a possible explanation, but Wilson reported paresthesias only rarely; he did not describe nocturnal pain, thenar wasting, or serious difficulty with tasks other than writing; and he also had "neuritis" in his leg. I suggested disk or nerve-root irritation — hardly a zebra — only as one possibility among many.

Shapiro criticizes me for omitting Gifford's remarks. They were not mentioned because they do not apply to Wilson's early career. Gifford reported the recollections of an associate of de Schweinitz, Dr. Fewell, who told him only that the observations were made while Wilson was president; Weinstein himself dates them after 1915,¹ and they may well have been made after Wilson's October 1919 stroke.

I have no quarrel with Shapiro's conclusion that Wilson had vascular disease for a long time — indeed, this was discussed in my article — but its severity and effect on Wilson's behavior, especially in the Princeton and early presidential years, are problematical. As Dr. Francis D. Boyd told Wilson in 1906, "If we should lay off from work every man whose blood tension is slightly off the normal, a great many very useful and important men would be idle."²

Weinstein is correct when he says that Wilson typed a great deal of work in 1896, but he also did a considerable amount of handwriting,

including first drafts of lectures and articles (one was "48 pp of my closely written manuscript") and extensive correspondence.³⁻⁵ I agree that Wilson had great fortitude, but he also had unusually frequent colds, digestive complaints, aches and pains, and the like; my article said only that some of his ills were psychosomatic — a characterization used by Weinstein as well.⁶ I am curious about how Weinstein determined that Wilson's left-hand symptoms were caused by fatigue but that his right-hand symptoms were sequelae of stroke. Regarding left-side neuritis before 1913, Wilson's physician, Dr. Grayson, described pre-presidential "neuritis in his left arm and shoulder,"⁷ and Wilson's biographer, R. S. Baker, in describing the events of 1906 wrote that Wilson "had been suffering for some time from neuritis of the left shoulder and leg."⁸ The alleged mood changes and delusions at the Paris Peace Conference are, to say the least, open to other interpretations.⁹ Weinstein's "reliable witness" was the White House usher, whose judgments are rather personal and whose reliability has been questioned.¹⁰

In his book, Weinstein carefully describes embolization from carotid occlusive disease, and he repeats in his letter the criterion of monocular blindness. However, monocular blindness does not signify carotid stroke when the blindness is caused by non-embolic pathology, such as a vein occlusion or macular hemorrhage. Weinstein now seeks to justify the finding of hemorrhage by citing Hayreh's argument that arterial ischemia underlies one form of vein occlusion. Hayreh's thesis is controversial within ophthalmology, but even granting its verity, the arterial ischemia may have a variety of causes, and vein occlusions (which often occur in mild hypertension and arteriosclerosis) are not diagnostic of carotid stroke.

Weinstein's letter is puzzling in some respects. If he wishes now to use the term "stroke" for any type of vascular lesion, in eye as well as brain, he contradicts the detailed description of carotid embolic disease in his book, and he reduces his thesis to a meaningless level. He misquotes me in saying that I attribute to him a belief that Wilson's alleged strokes caused cognitive loss; I merely pointed out that experts in neurology question whether strokes can cause marked personality changes (as Weinstein describes) without also causing cognitive damage. Shapiro notes correctly that Weinstein reported Wilson's ocular hemorrhage in his 1970 paper. This makes it all the more disturbing that Weinstein omitted the evidence of hemorrhage from his 1981 book. I am disappointed that his letter does not deal with the substantive questions of why such important data were omitted and why the hypothetical nature of his diagnoses was not made evident.

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1. Weinstein EA. Woodrow Wilson: a medical and psychological biography. Princeton, N.J.: Princeton University Press, 1981:206-7.
2. Link AS, ed. The papers of Woodrow Wilson. Vol. 16. Princeton, N.J.: Princeton University Press, 1973:445-6.
3. *Idem.* The papers of Woodrow Wilson. Vol. 2. Princeton, N.J.: Princeton University Press, 1967:366-8.
4. *Idem.* The papers of Woodrow Wilson. Vol. 9. Princeton, N.J.: Princeton University Press, 1970:338-47, 368, 420, 422, 430, 455.
5. *Idem.* The papers of Woodrow Wilson. Vol. 10. Princeton, N.J.: Princeton University Press, 1971:319.
6. Weinstein EA. pp. ix, 36, 49, 62, 69, 76, 142.
7. Memorandum of interviews with Dr. Cary T. Grayson, Washington, D.C., February 18, 19, 1926.
8. Baker RS. Woodrow Wilson: life and letters, Princeton 1890-1910. Garden City, N.Y.: Doubleday, Page, 1927:201.
9. George JL, George AL. Woodrow Wilson and Colonel House: a reply to Weinstein, Anderson, and Link. *Polit Sci Q.* 1981;82; 96:641-65.
10. Bailey TA. Woodrow Wilson and the great betrayal. New York: Macmillan, 1947:98.

FOREIGN-MEDICAL-SCHOOL GRADUATES OBJECT TO THE VISA QUALIFYING EXAMINATION

To the Editor: The Visa Qualifying Examination is a two-day test composed of approximately 950 multiple-choice questions concerning the basic and clinical sciences.¹ It is equivalent to the National

Board of Medical Examiners (Part I and Part II) Examinations. Since 1977, foreign-medical-school graduates (FMGs) wishing to go to the United States to engage in activities directly concerned with the care of patients have been required to pass this examination. It is not required for pure laboratory research. The older and simpler examination given by the Educational Council for Foreign Medical Graduates was not required for this purpose either.²

Most FMGs who fail the Visa Qualifying Examination fail Part I (basic sciences) and do well in Part II (clinical sciences). Undoubtedly, this is due to the fact that basic-science subjects are covered at the beginning of medical school, and many years have elapsed since FMGs have studied them. Also, many foreign physicians are involved in clinical work, and the use of the basic sciences is not essential in their everyday practice. Moreover, VQE scores are compared with the scores received on the NBME, Part I, which is taken by American students shortly after they complete the basic-science program in medical school, when their knowledge of it is fresh. If they were to take the same test many years later, their scores would be much lower. Thus, this scoring system is unfair to the FMGs.

Obviously, there must be certain measures to control the immigration of FMGs, but to curb it the way the VQE does, by means of a tough Part I, is unfair to qualified physicians who want to go to the United States to complete their education in clinical fields. Also, to give free entrance to FMGs who want to do nonclinical research, without testing their knowledge of the basic sciences or even their proficiency in the English language, is an astonishing paradox.

The administration of two different examinations — one on basic sciences for those interested in basic research and one on clinical sciences for those wishing to enroll in a residency training program — would be a fair and easy solution.

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1. Educational Commission for Foreign Medical Graduates. Visa qualifying examination information booklet. Philadelphia: Educational Commission for Foreign Medical Graduates, 1982.
2. *Idem*. Information for the January and July 1982 ECFMG examinations. Philadelphia: Educational Commission for Foreign Medical Graduates, 1982.

A COMPETITIVE APPROACH TO JOURNAL PUBLICATION

To the Editor: I would like to suggest an improvement in the current process for publishing journal articles. The current method requires that authors submit their articles to only one journal at a time. The decision to accept or reject an article may take as long as three months to make, and articles are rarely accepted outright; usually some amount of revision is necessary before final acceptance. If the article is rejected, the author may resubmit the article to another journal, starting the lengthy process again.

I suggest that authors be permitted to submit articles to two separate journals simultaneously. Of course, publication would be limited to only one journal. This approach would provide direct competition for the article and encourage high-quality reviews performed at a rapid pace. Furthermore, the current review process is sometimes uneven in quality, and this approach would increase the number of constructive reviews available for use in further revisions. Better papers and speedier publication of scientific information would result.

Dual submission has disadvantages. It would increase the number of manuscripts received by a journal and thereby increase the number of reviewers required. I think, however, that the advantages outweigh this increased burden.

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Editor's reply: Dr. Gilman's proposal would make any editor of a peer-reviewed journal shudder.

It would certainly increase the leverage of authors (who now are pretty much at the mercy of editors), it would probably provide authors with more reviewers' comments, and it might even slightly reduce the elapsed time between submission and initial editorial response. But Dr. Gilman's proposal would also greatly increase the work of overburdened editorial offices and would at least double the reviewer hours now being invested in the peer-review effort. In their capacities as editors, editorial-board members and referees, a small army of busy scientists, practitioners, and academics already devote an appreciable fraction of their working time to reviewing manuscripts. To double that work and to establish and maintain the elaborate communications system that would be required to operate Dr. Gilman's plan would seem to me intolerable and quite unjustified.

As new journals proliferate and the number of manuscripts submitted for review increases, the demands of the peer-review system and the costs incurred are already escalating rapidly. I cannot imagine many editors agreeing to a proposal that would greatly compound that problem. A more sensible approach would be to reexamine the peer-review system in an effort to determine exactly what it accomplishes and how it might be improved. Two recent commentaries on this subject in the *British Medical Journal* make interesting reading.^{1,2}

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1. Lock S. Peer review weighed in the balance. *Br J Med.* 1982; 285:1224-6.
2. Smith R. Steaming up windows and refereeing medical papers. *Br Med J.* 1982; 285:1259-61.

"HUMAN" CELL CULTURE LINES NOT ALWAYS AS ADVERTISED

To the Editor: During the past 19 months, our laboratory has examined the karyotypes from 10 different established cell lines, at the request of various colleagues. These cell lines were being used in the research effort of five separate laboratories, with the assumption that the cells were of human origin. Our analysis showed that 7 of the 10 lines were in fact not of human origin. Four were marmoset, two were rat, and one was mouse. Identification of the nonhuman origin of these cell lines has in some cases prevented investigators from wasting valuable research efforts and in others has resulted in the withdrawal or major modification of submitted papers and the rewriting of a doctoral thesis. The cell lines came from a variety of sources, including the Human Mutant Cell Repository in New Jersey.

As far as we can determine, the genotype established through chromosome analysis has not been related to mere erroneous labeling of tissue-culture stocks. Moreover, the frequency of the unexpected genotype seems higher than one would anticipate if spurious mislabeling in the investigator's laboratory were the explanation.

We are concerned about the possible frequency of mislabeling of tissue-culture genotypes as human. Karyotype analysis is a relatively simple and inexpensive procedure. We therefore suggest that investigators examine the karyotype of cell lines used in research.

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Massachusetts Medical Society Registry on Continuing Medical Education

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