

## COMMENTS AND RESPONSES

**Will Evidence Be Enough to Promote Interactive Communication for the Benefit of Patients?**

**TO THE EDITOR:** Foy and colleagues (1) provided evidence to support common sense, showing that patients with depression or diabetes have better outcomes when family physicians and specialists work together as a team, with different kinds of collaboration (for example, e-mail or face-to-face conversations).

We recently conducted a study to improve quality of care, specifically evaluating the follow-up after discharge in a series of 176 persons aged 15 to 25 years who attempted suicide (mean age, 20 years) and were hospitalized (mean duration, 1.1 day) in the 9 main hospitals in Picardie, France. Among the 90% of persons who were referred for follow-up (2% declined, 5% did not require follow-up, and 3% were missing data), 42% had to make the appointment themselves, despite the importance of follow-up in the days after an attempt, especially for adolescents. As one would expect, significantly more patients ( $P < 0.001$ ) completed follow-up when the appointments were planned during the hospitalization (60%) than when the patient had to make the appointment (40%).

Interactive communication for improving the effectiveness of primary care may remain a real challenge, despite evidence-based medicine. Interactivity means that a message is related to a number of previous messages and to the relationship between them. This is much more complex than when there are too many paths of care and the patient is reduced to an input (diagnosis-related group) in a streamlined, 1-way sequence of processes.

*Alain Braillon, MD, PhD*  
*Aurore Bernardy-Prud'homme, MD*  
*Gerard Dubois, MD, PhD*  
 Public Health, Northern Hospital  
 80000 Amiens, France

**Potential Conflicts of Interest:** None disclosed.

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**IN RESPONSE:** We thank Dr. Braillon and colleagues for their comments. We agree that implementing effective systems that can deal with the complexity of patient needs in an increasingly complex health care environment is an important challenge. Of note, our work suggests that the systems required for an effective line of interactive communication between collaborating primary care and specialist physicians need not in themselves be complex (for example, planned telephone or face-to-face contact). We would encourage further work to delineate the core "active ingredients" of collaborative interventions to inform policy initiatives to improve the continuity and outcomes of primary care.

*Robbie Foy, MBChB, PhD*  
 University of Leeds  
 Leeds LS2 9LJ, United Kingdom

**Potential Conflicts of Interest:** None disclosed.

**Electrocardiography and Preparticipation Screening of Competitive High School Athletes**

**TO THE EDITOR:** I read with interest Maron's recent editorial (1) but take issue with several of his assertions. My arguments are based on my experience with screenings in a multicounty region in suburban Chicago. Since 2006, our program, Young Hearts for Life, has screened more than 45 000 high school students.

First, I believe that Maron's efforts to relate the Italian system to the U.S. system are flawed. He contends that the disparity in physician resources between Italy and the United States is dramatic. The absolute number of athletes in the United States is larger than that of Italy, but as a percentage of the population, U.S. athletes number about half that of Italy (5% in the United States vs. 10% in Italy) (1). The number of primary care physicians per capita is similar at 1 per 1000 persons in the United States versus 0.9 per 1000 persons in Italy (2). Besides, the American Heart Association (AHA) already recommends that all young adult athletes be seen by a physician (3).

Second, I believe that Maron exaggerates the magnitude of a potential national electrocardiography (ECG) screening program in the United States when he says that 75 million U.S. citizens are younger than 18 years of age. Electrocardiography is recommended only for persons aged 12 to 25 years, of whom there are closer to 40 million according to U.S. census data from 2005. Furthermore, Maron's estimate of the incidence of sudden cardiac death is debatable. The statistic of fewer than 100 athlete deaths per year is based not on a scientific registry, like the Italian data, but rather on a compilation of media reports (4).

Third, the concern about excessively high rates of abnormal results does not reflect more contemporary standards of ECG interpretation, with rates reported at 5% or less (5). In our screening program, we have refined testing by using "stratified screening." Our abnormal result rate of 2.3% can be managed by the medical community (6). Moreover, I do not understand the preoccupation with false-positive results when similar issues have not impeded testing in other areas of medicine (for example, mammography and phenylketonuria screening in newborns). The medical community's approach has always been to pursue improvements that reduce the false-positive rate through refinement in techniques and experience. It should be no different with ECG screening.

In addition, I believe that the focus on screening only athletes, as defined in the AHA guidelines, is too restrictive. Shouldn't we be concerned about sudden cardiac death in all young adults and not just in a restrictively defined subgroup called "athletes"? Atkins and colleagues' prospective study (7) on the incidence of sudden cardiac death in children reported a much higher death rate than that cited by Maron (specifically, more than 2500 sudden cardiac deaths in U.S. young adults annually). We should not assume that our only choices are either mandated screening, as in Italy, or no screening at all. The United States is a nation with a tradition of ingenuity. Solutions to problems are not found by saying how we cannot achieve a worthy

goal, but rather by asking how we can. Until we change this mindset, we will continue to unnecessarily lose too many of our precious youth to potentially preventable causes of sudden death.

*Joseph C. Marek, MD*  
Midwest Heart Specialists  
Oak Brook Terrace, IL 60181

**Potential Conflicts of Interest:** Dr. Marek is affiliated with the Young Hearts for Life screening program for high school students. Young Hearts for Life is a 501(c)(3) public charity from which Dr. Marek receives no financial compensation.

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**TO THE EDITOR:** The editorial by Maron (1), as well as the articles by Baggish and colleagues (2) and Wheeler and colleagues (3), have interesting implications regarding the scientific debate about preparticipation cardiovascular screening for athletes. Baggish and colleagues (2) compared a screening protocol with or without 12-lead ECG in a cohort of U.S. collegiate athletes and showed that inclusion of the ECG improved sensitivity for detecting important cardiac abnormalities from 45.5% to 90.9% and altered the negative predictive value of screening from 98.7% to 99.8%. Wheeler and colleagues (3) assessed the costs and survival rates in U.S. athletes who were screened with or without 12-lead ECG and estimated that ECG resulted in 2.1 life-years saved per 1000 athletes screened. The incremental cost-effectiveness ratio of the screening with ECG was \$42 000 per life-year saved.

Because of these new investigations and the Italian experience with preparticipation screening (4, 5), scientific evidence supporting the efficacy and cost-effectiveness of a customary screening ECG for athletes is growing. Therefore, we believe that the current position of the AHA (6), which recommends medical history and physical ex-

amination, is inadequate and inconsistent with evolving scientific knowledge. We acknowledge that implementing a national screening program for athletes that includes a customary ECG is a difficult task in a large and multiethnic society, such as that in the United States, and that such a screening program may not be a top health care priority. We also acknowledge that this type of screening program should not be federally supported (it is not federally supported in Italy either). However, we believe that high schools and colleges share an ethical and legal obligation to ensure that their young students avoid the cardiovascular risk related to sports participation. Young athletes and their families should be fully informed regarding the additional value of ECG and should be offered screening with an ECG. Denying athletes such opportunities may be viewed by some as denial of a potentially life-saving diagnostic test because it detects silent cardiomyopathy, a potentially lethal condition. Moreover, it is perhaps only a matter of time before a malpractice lawsuit is brought in the case of a young athlete dying suddenly who had not been screened with an ECG.

*Antonio Pelliccia, MD*  
Institute of Sport Medicine and Science  
I-00197 Rome, Italy

*Domenico Corrado, MD, PhD*  
University of Padua Medical School  
35121 Padua, Italy

**Potential Conflicts of Interest:** None disclosed.

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**IN RESPONSE:** I read with interest Dr. Marek's reaction to my editorial. Unfortunately, my comments seem to have been largely misunderstood. First, the disparity cited between U.S. and Italian physicians performing screening examinations relates specifically to the unique cadre of sports medicine physicians solely dedicated to this

program in Italy—a crucial resource that simply does not exist in the United States, making national mandatory ECG screening exceedingly difficult, if not impractical. Second, the sudden death of any young athlete is tragic, but these events are uncommon (1–3). Third, Dr. Marek’s characterization of the Sudden Death in Athletes Registry (1) as “a compilation of media reports” is inaccurate. Of note, tabulation of deaths of athletes in Italy is based on events limited to the Veneto region, which comprises only 9% of the Italian population. Fourth, no evidence suggests that mortality rates due to cardiovascular disease in athletes differ between the United States and Italy (3), despite dissimilar screening strategies (3, 4). Fifth, Dr. Marek suggests that I cited obsolete data for false-positive test results (that is, 10% to 20%) and claims a rate of only 2% from his own program. I encourage Dr. Marek to publish his stratified screening data so that we may all learn from his substantial experience screening 45 000 high school students over the past 4 years. I believe that we should be “preoccupied” with false-positive ECG test results because of the potential for overburdening the system, excessive subspecialty testing, overdiagnosis, unnecessary anxiety and confusion for patients and families, and unwarranted disqualification from sports. Sixth, suggesting that the debate is about a choice between mandated ECG screening (the Italian model) and “no screening at all” is a mischaracterization of the athlete screening process in the United States. Screening of U.S. high school and college athletes (with history and physical examination) is practiced almost universally (5) and has value (6). Seventh, it is an exaggeration to suggest that our national character is somehow dependent on adopting mandatory ECG screening for athletes. Preparticipation screening in large general populations is deceptively complex and requires measured consideration (7, 8) because it is affected by practical, societal, cultural, and legal considerations relevant to the U.S. health care system (8). Perhaps most important, limiting mass screening to sports participants would undoubtedly be regarded as exclusionary, if not discriminatory, with respect to young nonathletes. Finally, although the 2007 AHA screening recommendations (9) do not endorse mandated national ECG screening for all competitive athletes, in no way do the recommendations discourage individual initiatives, such as Young Hearts for Life. Moreover, it is incorrect to characterize the many clinicians and experts who do not endorse ECG screening on a required national basis as complicit in past and future sudden deaths of young athletes because of their misguided “mindset.”

I appreciate the comments on strategies for cardiovascular screening in competitive athletes from Drs. Pelliccia and Corrado. Some of the remarks, however, reflect a naive zeal to export ECG screening to the United States. The comments about ethical and legal obligations and the language about “denial of a potentially life-saving diagnostic test” and it being “only a matter of time before a malpractice lawsuit is brought” were unsettling and could be interpreted by some as thinly veiled accusations of unethical behavior and dereliction of duty if ECG screening is not done. In the United States, medical practice (for example, preparticipation screening) must conform to accepted, customary, and reasonable standards as formulated by the medical establishment—not from governmental legal fiat, as in Italy (4). Consensus guidelines, such as the 2 AHA scientific statements on athlete preparticipation screening (9), make important contributions to this customary and accepted framework of practice. Therefore, I would emphasize that no legal obligation

exists on the part of U.S. practitioners, high schools, or colleges to routinely incorporate ECGs into their screening practices.

*Barry J. Maron, MD*

Minneapolis Heart Institute Foundation

Minneapolis, MN 55407

**Potential Conflicts of Interest:** Dr. Maron is the chairperson of the AHA position paper on guidelines for athlete screening (9).

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#### Complexities of Defibrillator Deactivation

**TO THE EDITOR:** We strongly agree with Goldstein and colleagues’ (1) conclusion that hospice may improve quality of care if patients with implantable cardioverter-defibrillators (ICDs) are identified and ICD deactivation is addressed. In our own hospital-based heart failure cardiology and palliative care practices, we always address ICD deactivation before discharging patients to hospice care. However, given the complexity and difficulty of decision making for families and caregivers during transitions in care, some patients are not ready to deactivate their ICD at the time of hospital discharge. We appreciate that hospices do accept patients who have functioning ICDs with the presumed goal of addressing deactivation after enrollment. As the authors noted, their study does not directly address patient-level preferences about ICD inactivation. In a small study by Goldstein and colleagues (2), persons with ICDs had not discussed ICD

deactivation and were unaware that it was an option. We have also observed that most patients are not made aware of the potential future burden at the time of ICD implantation. We have occasionally noticed a paradox in which patients, despite preferring palliative care or hospice care, remain unwilling to have their ICDs deactivated. Furthermore, it is possible that hospice nurses are appropriately querying hospice patients about their preferences for ICD deactivation, regardless of whether the hospice has an ICD deactivation policy. Of note, the median length of stay in hospice in a large sample of Medicare patients with heart failure was only 17 days, with nearly one third of patients staying fewer than 8 days (3). Thus, a short enrollment in hospice at the end of life may be an additional barrier to establishing highest-quality conversations between cardiac patients and hospice care providers. Finally, clinicians need to be aware that, unlike patients with cancer, 19% of patients with heart failure are discharged from hospice, and nearly 1 in 4 patients survive more than 180 days after hospice enrollment. Thus, admission to hospice care is not a certain indication of terminal heart failure, which adds to the complexity of prognostication and decision making surrounding this disease.

Sandesh Dev, MD  
Anthony N. Galanos, MA, MD  
for the Duke Supportive Cardiology Group  
Duke University Medical Center  
Durham, NC 27710

**Potential Conflicts of Interest:** None disclosed.

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**IN RESPONSE:** We appreciate Drs. Dev and Galanos's thoughtful response to our article. The goal of our work has always been to encourage patient-clinician communication about ICD deactivation to ensure that patients and families can make informed choices that are consistent with their values and desires for care. As such, we do not suggest requiring deactivation of devices in all patients and encourage physicians to respect patient autonomy when it comes to ICD management—even if, in the clinician's mind, the patient's decision seems to be counterintuitive. Drs. Dev and Galanos's comments about patients not always deciding to deactivate an ICD are in line with our clinical experiences and other published data. Specifically, in our article in *Annals* several years ago (1), we found that only 21 of the 27 patients (78%) with an ICD who were reported to have had a previous conversation about device deactivation chose to deactivate the shocking function. Patients often have had their defibrillators for several years and may develop complex psychological relationships with respect to the device (2, 3). As 1 person with an ICD stated in our focus group: "All I know is that it is there to help me and as long as I don't do wrong by it, it won't do wrong by shocking me" (4). When describing the percentage of patients with heart fail-

ure who are discharged from hospice alive, Drs. Dev and Galanos's response shows that the course of patients with end-stage heart disease can be particularly difficult to predict (5). The experience of patients with heart failure underscores the importance of our recommendation that conversations should occur earlier in the course of a patient's illness, so that the patient and their caregivers can begin discussions and determine whether and when in the future the patient might want the device deactivated. These conversations will make decisions easier for the family if they find themselves in an emergency situation in which the patient is being shocked repeatedly at the end of life (1). Only by enabling early and comprehensive communication about ICD management in the context of other discussions about the patient's overall goals of care can we ensure the best quality of life and care for patients with advanced heart disease and for their families.

Nathan E. Goldstein, MD  
Melissa Carlson, MBA, PhD  
Mount Sinai School of Medicine  
New York, NY 10029

Jean S. Kutner, MD, MSPH  
University of Colorado Denver School of Medicine  
Aurora, CO 80045

**Potential Conflicts of Interest:** None disclosed.

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#### Possible Stereotyping in a Personal Essay

**TO THE EDITOR:** I am writing in response to Smythe's recent essay (1) about the interaction between himself, the chair of a department of surgery, and a black cleaning lady who was grieving the loss of her husband. I have no doubt that his efforts in writing this story were well intended. He probably wanted to increase awareness that we, as doctors, must show compassion both inside and outside the office.

I am an internist and a writer; my current focus is writing for children and young adults. I was uncomfortable with what I believe to be stereotyping in Smythe's essay, both of the woman, named Mattie, and of himself. I believe this is all too common.

Smythe begins, "In my role as chair of department of surgery, I occasionally work fairly late in the office, especially on days when Hercules shoots the administrative eagle, releasing me like Prometheus from the bondage of paperwork and meetings." I think his placing himself in the realm of the gods is not helpful. During

one of these late evenings, he encounters Mattie, the black cleaning lady.

We aren't given Mattie's last name. What does she look like? She is overweight with "ample arms." Her form is a "backlit silhouette filling the lower two thirds" of his office doorway. He imagines this hospital scrub-attired woman, "for a moment . . . wearing a Sunday dress and hat," which is a common stereotype of older black women. Mattie also speaks in a heavily overdone and, yes, stereotypical, black dialect. (Twain gets away with writing dialect, but it is appropriate and well done in his work.)

Here's an example: One evening, weeks after the author first met Mattie, she takes a break from her work and comes into his office and sits, telling him about the vision she has had of her deceased husband: "[My husband] look so handsome to me. I gets up out of bed and stand up, my eyes still closed and we start to dancin'. I dancin' with him right there, in my bedroom!" As she tells him this, the doctor notes that "[Mattie] shuts her eyes and starts to move rhythmically in her seat, open hands lifted up in the air, moving as if dancing to some slow jazz beat."

Is there anything about the portrayal of this black woman that is not a stereotype?

Did Mattie look, dance, and talk as he presents her? I'm sure there was some truth in the description—there always is. But I am just as sure there is probably much about the real Mattie that is not described; details that are specific to her. And Smythe is not a god, even if he uses godly images to describe himself. Sometimes, however, doctors think they have those powers.

The essay is about a doctor imparting wisdom to help an older, relatively poor black woman deal with her grief. But I found it paternalistic. In reality, this woman would have, at her age and station, just as much, if not more, to teach him about loss and suffering. That might have been an interesting topic. Instead, he made her a character who is innocent about grief and in need of a "god's" help.

We all have to be careful to avoid stereotypes in our thinking and in our writing, especially in a distinguished journal, such as *Annals*.

Janice M. Scully, MD  
Fayetteville, NY 13066

**Potential Conflicts of Interest:** None disclosed.

#### Reference

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**IN RESPONSE:** I thank Dr. Scully for her comments and her expression of concern regarding stereotyping in "Mattie's Visit."

The supposed reference to the surgeon as a god in this piece, and its relation to the interaction with the grieving woman, requires a great deal of imagination—perhaps Dr. Scully is promoting a stereotypical depiction of a surgeon?

The opening paragraph was simply an effort to describe the conflict that many physicians have when administrative life takes them away from patients. We miss our patients not for what we can pass down from Mount Olympus, but for what they teach us (and reward us with in the process). I am not speaking of praise, money, or "godly" fame, but rather intimacy and trust, exemplified by the gift of this woman's touching story and her coming to terms with her loss. If nothing else, as a hubristic surgeon playing God—one in-

tending to make the correlation that she describes—I would have chosen a more powerful deity. Poseidon, the god of the sea and earthquakes (literally, "Earth Shaker") comes to mind.

My interaction with this woman, and her language and appearance, were as depicted—not a stereotypical reconstruction. Her first name was changed slightly and her last name omitted to protect her privacy.

I write essays about interactions with patients and family members of all races, religious backgrounds, and ethnicities. If this had been a rural white farmer from the United States, or a migrant worker from another country, I would have depicted these dialects and appearances as accurately as possible as well.

I wonder if the authors of *The Color Purple* or perhaps *Push*, the novel that the acclaimed movie *Precious* was based on, would take Dr. Scully's advice? If they did, would these works be as meaningful to us? Like the characters in these books, Mattie is not ashamed of her language or appearance.

If we would take the time to listen to our patients and families more closely, regardless of how they look or sound, or where any of us feel we exist in the artificial continuums of society, socioeconomic status, and race, we might be enriched.

Thanks to this reader's critique, I have revisited the essay and have found that rather than the husband "visiting" Mattie, from my perspective, Mattie "visited" me and reminded me of the importance of engagement and listening.

W. Roy Smythe, MD  
Scott & White Healthcare Temple Clinic  
Temple, TX 76508

**Potential Conflicts of Interest:** None disclosed.

## CLINICAL OBSERVATION

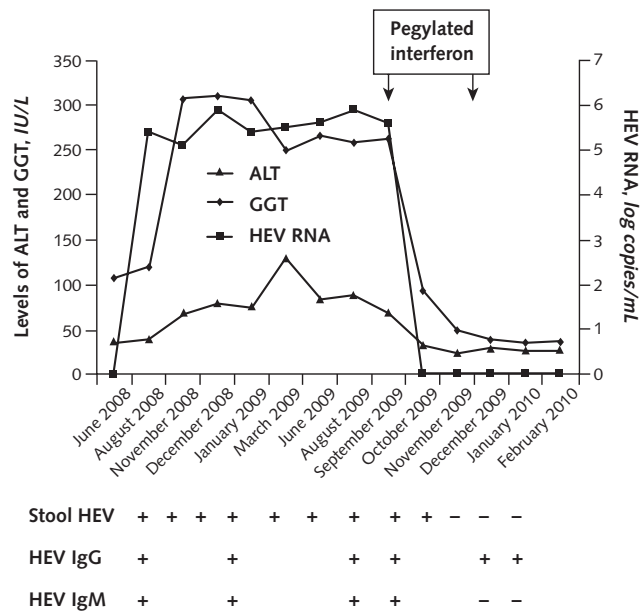
### Chronic Hepatitis E Virus Infection: Successful Virologic Response to Pegylated Interferon- $\alpha$ Therapy

**Background:** Acute hepatitis E virus (HEV) infection is usually followed by complete recovery, but some cases of chronic HEV infection have been reported.

**Objective:** To report a case of chronic HEV infection in an immunocompromised patient with no history of immunosuppressive therapy or HIV infection.

**Case Report:** Clinicians diagnosed hairy cell leukemia in a man aged 57 years in 2007. He was never treated because the disease was indolent with only moderate thrombocytopenia (platelet count,  $85 \times 10^9$  cells/L). We saw him in November 2008 for elevated liver enzyme levels (alanine aminotransferase, 135 IU/L; aspartate aminotransferase, 80 IU/L; and  $\gamma$ -glutamyltransferase, 311 IU/L). We ruled out all other causes of acute and chronic liver disease and detected HEV RNA in the patient's serum and stools. Test results for anti-HEV IgG and IgM were positive (Adaltis, Ingen, France), and the HEV genotype was 3c. We performed a liver biopsy in December 2008 and found mild lobular hepatitis without fibrosis. The leukocyte count was  $4.3 \times 10^9$  cells/L, and the hairy cell leukocyte count was  $0.45 \times 10^9$  cells/L. The CD4<sup>+</sup> cell count was normal ( $0.582 \times 10^9$  cells/L). We detected no monocytes in the peripheral blood. After 1-year follow-up with-

**Figure.** Changes in liver enzyme levels and HEV viral load before and during therapy with pegylated interferon- $\alpha$ 2b.



ALT = alanine aminotransferase; GGT =  $\gamma$ -glutamyltransferase; HEV = hepatitis E virus.

out therapy, we started the patient on a 3-month course of pegylated interferon- $\alpha$ 2b, 1  $\mu$ g/kg of body weight per week (Figure). The serum HEV RNA concentration decreased from 5.6  $\log_{10}$  copies/mL at baseline to 2.4  $\log_{10}$  copies/mL by week 2, and the patient achieved a complete virologic response by week 4 (Figure). At week 7, liver enzyme levels remained within the normal range, and we could not detect HEV RNA in stools (lower limit of detection, 200 copies/g). We stopped antiviral treatment in November 2009 and could not detect serum HEV RNA after 5 months (lower limit of detection, 200 copies/mL) (Figure).

**Discussion:** We have reported elsewhere (1) that HEV infection can evolve into chronic hepatitis E in organ transplant recipients receiving immunosuppressive drugs. Two other cases (2, 3) of prolonged HEV infection have been reported: 1 in a patient with lymphoma being treated with rituximab (2), and another in an immunocompromised patient with HIV infection (3). The lymphocyte and CD4<sup>+</sup> cell counts were lower than normal in our transplant recipients (1) and in the patient with HIV (3), and they were not reported for the patient with lymphoma (2). Hairy cell leukemia is an indolent chronic B-cell lymphoproliferative disease with a good long-term prognosis, and its main complication is infection (4). We chose to treat this patient with pegylated interferon- $\alpha$ 2b for several reasons. We have reported (1) that immunosuppressive drugs are

associated with progression to severe liver fibrosis in chronic HEV infection, so we treated this patient's chronic HEV infection with pegylated interferon- $\alpha$ 2b before using immunosuppressive drugs for leukemia. Also, interferon- $\alpha$  was the first effective treatment for chronic hepatitis B and C infections, and we have reported elsewhere (5) that pegylated interferon- $\alpha$  has antiviral effects in liver transplant recipients with chronic HEV infection. In addition, pegylated interferon- $\alpha$  is used to treat hairy cell leukemia and has no substantial adverse effects (4). In our patient, a short, 3-month course of pegylated interferon- $\alpha$ 2b was associated with clearance of HEV without relapse. Although pegylated interferon- $\alpha$ 2b might have affected the leukemia, which in turn might have led to improved immune function, we believe that pegylated interferon- $\alpha$ 2b had a direct antiviral effect against the HEV infection because of the rapid decrease in serum viral RNA after only 2 weeks of treatment without a change in monocyte or lymphocyte levels.

**Conclusion:** To our knowledge, this is the first report that chronic HEV infection may occur in an immunocompromised patient without a history of immunosuppressive therapy or HIV infection. In addition, we suggest that pegylated interferon- $\alpha$  is an effective treatment for chronic HEV infection.

Laurent Alric, MD, PhD

Delphine Bonnet, MD

Guy Laurent, MD, PhD

Nassim Kamar, MD, PhD

Jacques Izopet, PharmD, PhD

Toulouse University Hospital

31059 Toulouse, France

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