Diagnosis of Obstructive Sleep Apnea in Adults

TO THE EDITOR: The American College of Physicians’ (ACP’s) clinical practice guideline about the diagnosis of obstructive sleep apnea (OSA) in adults (1) addresses a prevalent and serious medical illness that deserves the attention of internists. Although ACP’s clinical guideline and that of the American Academy of Sleep Medicine (AASM) share some similarities, substantial differences are of concern to the AASM.

The ACP’s recommendation to limit home sleep apnea testing to situations “when polysomnography [PSG] is not available for diagnostic testing” is overly restrictive and inconsistent with the AASM clinical guideline. Home sleep apnea testing interpreted by a sleep specialist in conjunction with a comprehensive sleep evaluation may be an equally viable diagnostic option in patients with a high pretest probability for at least moderate to severe OSA who do not have comorbid cardiopulmonary or neuromuscular disorders and in whom other sleep disorders are not a consideration (2). Under these conditions, a home sleep apnea test may be the most reasonable choice even when PSG is available. When these conditions are not met but sleep-disordered breathing is a consideration, we agree that PSG is the test of choice.

Furthermore, the ACP guideline places an inordinate emphasis on sleepiness as the main reason for evaluation with sleep testing. In the WSC (Wisconsin Sleep Cohort) study, only 37% of patients with severe OSA (apnea–hypopnea index ≥30 events per hour) reported daytime sleepiness, which is 1 of many symptoms that may suggest that OSA should be included in the differential diagnosis (3). Other symptoms include witnessed apnea, snoring, nocturnal gasping or choking, nonrefreshing or disturbed sleep, nocturia, morning headaches, impaired concentration, memory loss, and decreased libido (4). Concurrent risk factors, such as obesity, retrognathia on examination, hypertension, or type 2 diabetes, should prompt consideration for sleep apnea testing; however, some of the other causes of sleepiness do not require sleep apnea testing and respond to specific interventions. A report of excessive sleepiness should prompt a comprehensive review of the patient’s sleep schedule, questioning for auxiliary symptoms of narcolepsy, and consideration for sleep specialist referral if the cause is not apparent (5).

It is critical to advance high-value care of patients with a sleep illness, such as OSA. Physicians should inquire for symptoms of sleep disturbances and specifically look for sleep apnea in patients belonging to high-risk populations, including those who do not report sleepiness. The AASM recognizes that internists play an important role in the management of patients with OSA, and we believe that collaborative relationships between sleep specialists and internists will undergird our efforts to improve public health by promoting healthy sleep.

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References

My candle burns at both ends;
It will not last the night;
But ah, my foes, and oh, my friends—
It gives a lovely light.

—Edna St. Vincent Millay

TO THE EDITOR: We read with concern the ACP clinical guideline about the diagnosis of OSA (1). This guideline offers PSG as a solitary tool for sleep-related symptoms; the guideline’s broad application could result in unnecessary testing and treatment.

Polysomnography performed with current technologies and scored using the criteria recommended by the AASM in 2012 will yield an average apnea–hypopnea index that is 3-fold higher than that obtained with the equipment and scoring criteria available at the time of most of the studies cited to support this guideline. Flow changes are currently graded using a pressure-transduced air flow monitor—which is far more sensitive than the thermistor used in prior studies—and the new AASM guideline does not require oxygen desaturation to be present for a breathing event to be scored (2). In fact, a recent trial showed that the prevalence of an apnea–hypopnea index of 5 or more events per hour using the current criteria was 94.6% in a population with a “mild-moderate” pretest probability of OSA (3). This condition exists on a spectrum, and apnea–hypopnea index cutoffs are largely arbitrary. Given the changes in diagnosis, are we measuring clinically meaningful disease? What are the costs of overdiagnosis?

Furthermore, the term “unexplained sleepiness” (which is pivotal in the guideline’s first recommendation) is meaningful only when clinicians thoroughly understand the causes of sleepiness. The average physician receives approximately 2 hours of formal medical education on the evaluation of sleep disorders (4). This guideline fails to acknowledge that behaviorally induced insufficient sleep, insomnia, mood disorders,

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the restless legs syndrome, and many other problems cannot be measured by PSG or treated with continuous positive airway pressure.

We know from survey data that the average person in the United States effectively “burns the candle at both ends,” obtaining 6 hours and 40 minutes of sleep on the average workday (5). Recommending PSG for every patient whose sleepiness is “unexplained”—without also recommending qualitative and quantitative assessment of sleep duration—is ill-advised. We suggest that the ACP develop a comprehensive sleep-symptom guideline that encourages a more holistic evaluation of the patient who presents with sleepiness and focuses more attention on the limitations of PSG.

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Disclaimer: The views represented in this letter are those of the authors and do not reflect the policies of the U.S. Department of Defense or the U.S. Army.

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References

IN RESPONSE: Dr. Morgenthaler objects to the focus on unexplained daytime sleepiness as the symptom prompting investigation and suggests that other symptoms, including snoring, nonrefreshing sleep, morning headaches, impaired concentration, memory loss, and decreased libido, should prompt an evaluation for OSA. We disagree; many patients who present to a primary care practice have at least 1 of these symptoms, and they all do not need sleep studies. Unexplained daytime sleepiness is the only symptom of OSA that evidence from randomized, controlled trials shows is responsive to treatment. Population studies suggest that high rates of subjective sleepiness in the general population are not associated with sleep apnea and are likely attributable to factors other than primary sleep disorders (1). Another study of the general population has shown a high prevalence of sleep symptoms or disturbance among adults in the United States, with 75% reporting at least 1 sleep-related symptom (2). Furthermore, no evidence is available that shows that treatment specifically improves the particular outcomes previously mentioned; thus, knowledge of a diagnosis neither provides useful information to patients about their prognosis nor improves clinical outcomes.

Although home monitors may be appropriate for some patients, current evidence shows that PSG is still the gold standard diagnostic test and should be the diagnostic test of choice when available. Hence, we disagree with Dr. Morgenthaler’s comment that ACP’s recommendation to use PSG as a first option for sleep testing is too restrictive. Although the AASM recommends using portable sleep monitors for patients with a high pretest probability for moderate to severe OSA, there is currently no accurate way to predict who is at high risk for OSA before doing a sleep study. The Agency for Healthcare Research and Quality evidence report (3) and the evidence review update showed poor diagnostic accuracy for screening questionnaires. Also, no current evidence shows that untreated mild OSA has any effect on mortality or morbidity.

We agree with Dr. Hostler and colleagues that medical students generally receive inadequate training on sleep disorders. We suggest that physicians take a history to understand more about the patient’s sleepiness rather than recommend sleep studies for all tired patients. We also suggest that patients who report inadequate hours of sleep be instructed to get more sleep before further investigation. Dr. Hostler and colleagues also point out that newer sensors are more sensitive than older forms of PSG, and we agree that, as technology changes, so do the implications for disease diagnoses. However, this statement is true for many fields of medicine. The update of the Agency for Healthcare Research and Quality evidence report included studies published since 2010 that evaluate the newer technologies. Nevertheless, similar to the data before 2010, the definitions and measurements of apnea or hypopnea were substantially heterogeneous among studies. In addition, the study that Dr. Hostler and colleagues cite (4)—which suggested a greater prevalence of OSA and potential overdiagnosis—was done in a patient population with mild to moderate clinical suspicion of OSA. Finally, their definition of hypopnea was liberal, which possibly accounted for a greater prevalence of OSA.

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References

IN RESPONSE: We stand by our original statement and are unclear with regard to the relevance of citing data from a largely unscreened population because the denominator will not include many men with biopsy-detectable cancer (that is, "men with prostate cancer"). The purpose of our statement is to reorient patients away from any perception that prostate cancer is generally fatal. Empirical data show that this perception is widespread (1).

Dr. O’Callaghan and colleagues suggest that our statement will, of itself, “lead to a decrease in screening rates.” We find this concept questionable, especially because the state-
TO THE EDITOR: Kottilil and colleagues (1) raise issues about the future of HCV therapy. As they cite, more than 230 million persons are likely infected with HCV, and the number of persons found to be infected will increase with the updated recommendations from the U.S. Preventive Services Task Force (2). Treatment regimens have traditionally been wrought with adverse effects and dose adjustments. In the age of interferon-free regimens, patients tolerate therapy without many complications.

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Disclosures: Authors have disclosed no conflicts of interest.

Reference

TO THE EDITOR: Kottilil and colleagues (1) state, “For the first time since the identification of hepatitis C virus (HCV) . . . communitywide eradication of HCV infection seems possible.” However, direct-acting antivirals have never been shown to prevent cirrhosis or hepatocellular carcinoma. Even if these agents effectively prevented these crucial complications of chronic HCV infection, the prevalence of cirrhosis and liver cancer is already increasing and is likely to do so continuously for decades. Internists may assume some responsibility for antiviral administration and monitoring adverse effects of drugs, but the potentially lethal complications of chronic HCV infection will likely need specialty input for the remainder of our professional lives.

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Disclosures: Authors have disclosed no conflicts of interest.

Reference

We agree that internists, subspecialists, and public health authorities will need to work in cooperation to treat persons with HCV infection. Although some patients may benefit from a hepatologist referral, we also agree that identification on the front line to properly triage these persons will be essential to success (3). Hepatologists have been the primary providers in treating HCV infection up to this time. The American Association for the Study of Liver Diseases has partnered with the Infectious Diseases Society of America to develop a Web site for up-to-date guidelines to direct treatment of HCV infection (www.hcvguidelines.org) because therapy is rapidly evolving. Further, specialty societies are creating educational materials geared toward first-line providers to support efforts to eradicate HCV infection.

Internists are an integral part of the identification of patients with HCV infection, and coordinated efforts with the specialists who have a long history of treating this disease will be essential. Although now might be the time for internists to take the reins, we believe that it is important that they have the right specialist alongside to help guide these treatment plans. Of note, clinicians experienced in surmounting the hurdles of caring for and managing patients with mild chronic, advanced, and potentially fatal liver disease should provide guidance on expanding the pool of care providers.

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Disclosures: Authors have disclosed no conflicts of interest.

References

TO THE EDITOR: I read Kottilil and colleagues’ article (1) with great interest. This comment is to generate thought, not simply to be contrary. I have my clinical niche and certainly encourage other internists to aggressively pursue their interests, including HCV infection. I enjoy the opportunities to quarter-back for complex cases. My concern, to use 1 example, is that private insurers, Medicare, accountable care organizations, and patient-centered medical home initiatives expect internists to act as data clerks for every aspect of care for patients with a diabetes code in the electronic health record. I am faulted when the patient has an endocrinologist who already addresses the many diabetes mellitus checkboxes but does...
not use my electronic health record and am penalized by the National Committee for Quality Assurance certification program when I do not allocate staff to call the ophthalmologist who did not bother to send me a letter documenting the eye examination. Such an initiative as “the time . . . is now” for us to treat HCV infection must be accompanied by a plan for a comprehensive, system-based infrastructure supported by the companies claiming to be responsible for patient well-being, not just a way for us to get more patients into our office slots and increase the preapproval paperwork burden for our staff. If this issue will overwhelm subspecialists, then practicing internists, should be supported as we are expected to handle many more issues during an office visit.

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Disclosures: Authors have disclosed no conflicts of interest. Forms can be viewed at www.acponline.org/authors/icmje/ConflictOfInterestForms.do?msNum=L14-0549.

Reference

IN RESPONSE: Dr. Venes correctly states that “direct-acting antivirals have never been shown to prevent cirrhosis or hepatocellular carcinoma.” Indeed, the long-term effects of new drugs on the complications of HCV infection merit careful study to be certain that their apparent benefits are fully realized. The experience with virologic cure after interferon-based therapies suggests that cure of HCV infection using direct-acting antivirals will plausibly prevent such sequelae (1, 2). Dr. Venes also observes that, even if direct-acting antivirals prevent complications, we will have to manage many patients in the current cohort of HCV-infected persons who do not access care or who already have advanced liver disease. This observation is correct but underscores the need to treat as many patients with HCV infection as possible now so that we decrease progression of HCV infection-related liver disease to the greatest extent possible.

Dr. Shah and colleagues emphasize the importance of internists gaining knowledge and experience in the management of HCV infection and collaborating with subspecialists. We wholeheartedly agree. Professional organizations are accelerating their efforts to have continuing medical education for providers with different experiential backgrounds and to offer guidance documents and tutorials that are up to date, focused, and practical. The guidance Web site (www.hcvguidelines.org) that we and Dr. Shah and colleagues cited (and that 2 of us coauthored) has had more than 150,000 unique visitors and almost 1 million page views since its inauguration on 29 January 2014. Internists are highly skilled at working with subspecialists to provide excellent management for patients whose problems are beyond their expertise or scope of practice. Partnerships between primary care providers and subspecialists are vital for all aspects of medicine, including management of HCV infection.

Dr. Carson points out with passion the plight of internists in the current era. We are all required by payers and regulatory agencies to do more. The ability to support electronic health records and afford to pay staff who have the time to respond to these diverse demands is challenged by declining reimbursement. More important, such demands can rob us of the deep satisfaction of caring for the whole patient. We do not mean to suggest that it is practical for every internist to become an expert in HCV infection, but internists can certainly identify infected patients, enhance their linkage to care, and cure a large fraction of these patients.

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References

Low-Dose Computed Tomography Screening for Lung Cancer

TO THE EDITOR: As Wiener described, a Medicare advisory panel recommended against coverage of low-dose computed tomography (LDCT) for Medicare beneficiaries. Many professional organizations recommend LDCT provided that it is undertaken as a structured program in centers with considerable expertise in lung cancer care (1). Wiener suggests that the optimal way for the Centers for Medicare & Medicaid Services to proceed may be to offer coverage for LDCT screening only when performed by facilities that are certified as comprehensive, patient-centered programs.

The Building Trades National Medical Screening Program uses a regionalized approach to lung cancer screening, linking local providers with a regional cancer center of excellence. Under authorization from Congress, this program has been screening workers employed at U.S. Department of Energy atomic weapons facilities for occupational diseases since 1999. These persons are at significantly increased risk for chronic obstructive pulmonary disease and lung cancer mortality (3, 4) and are likely to live in rural areas that are not

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served by medical providers with the kinds of expertise recommended for LDCT screening.

The Building Trades National Medical Screening Program partnered with the Seattle Cancer Care Alliance (SCCA)—the comprehensive cancer center for the Pacific Northwest—to provide early lung cancer detection for persons from the Hanford Nuclear Reservation, located 200 miles from Seattle, while adhering to the National Comprehensive Care Network guidelines for eligibility and diagnostic evaluation (2). A local SCCA affiliate, Kadlec Regional Medical Center, administers the LDCT, and a thoracic radiologist at the University of Washington interprets the results. A multidisciplinary nodule board reviews suspicious nodules. Patients who require evaluation beyond repeated LDCT are invited to come to the SCCA; any surgical resection is done by the University of Washington cardiothoracic surgeons. The SCCA provides a smoking cessation program. To encourage care at the SCCA, the program facilitates travel.

As of September 2014, we have enrolled 184 participants from the Hanford Nuclear Reservation; 37 had indeterminate nodules (2), 17 had suspicious nodules, 5 have been diagnosed with lung cancer, and 94 have been referred for evaluation of medical findings other than lung cancer. Those with lung cancer were generally agreeable to travel for specialty care; 4 stage IA adenocarcinomas were removed with curative intent at the University of Washington. The fifth person opted not to come to the SCCA and was eventually diagnosed with stage IV squamous cell carcinoma.

We believe that this model can be expanded to offer LDCT screening at comprehensive, high-quality regional cancer centers to all persons at high risk for lung cancer.

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References

IN RESPONSE: We congratulate Dr. Welch and colleagues on their work to develop a regional lung cancer screening center serving high-risk persons living in rural communities. In particular, we applaud the incorporation of a smoking cessation program, the use of predetermined criteria for LDCT screening eligibility and nodule evaluation, and the involvement of multiple disciplines in deciding how to evaluate screen-detected nodules. However, these elements are not in themselves sufficient for a comprehensive LDCT screening program. Guidelines for LDCT screening also recommend shared decision making about the benefits and harms of screening and the maintenance of a registry of persons who have had screening (1, 2). Indeed, these additional elements are required for Medicare coverage of LDCT screening (3). Careful design and implementation of comprehensive LDCT screening programs, including these elements, should help optimize the balance of benefits and harms for persons being screened for lung cancer (4). However, further study of these programs as they are implemented in the real world is essential to ensure that lung cancer screening is safe, effective, and affordable.

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Disclosures: Disclosures can be viewed at www.acponline.org /authors/icmje/ConflictOfInterestForms.do?msNum=M14-1352.

References

Three Nonnucleoside Reverse Transcriptase Inhibitor-Sparing Antiretroviral Regimens for Treatment-Naive Volunteers Infected With HIV-1

TO THE EDITOR: A design characteristic in Lennox and colleagues’ trial (1) allowed patients to switch the third antiretroviral compound within the ACTG (AIDS Clinical Trials Group) A5257 open-label trial; this method is distinct from that of historical randomized clinical comparisons. The aim of this
protocol specification may have been to enable an antiretroviral therapy switch for patients facing nonserious adverse events, thus approaching a real-life clinical setting. A prerequisite for an unbiased observation is the free dispensing of all administered study drugs. However, a booster of ritonavir, 100 mg/d, was not provided for participants randomly assigned to a protease inhibitor regimen within the study, and whether affected patients were reimbursed for these costs remains unclear.

Second, atazanavir maintenance was most likely disadvantaged by the distinct design characteristic, because cosmetically adverse hyperbilirubinemia has previously not been a typical reason for atazanavir cessation (2–4). Physicians and affected patients had an interest to carry out the per protocol–allowed switch toward raltegravir, “killing 2 birds with 1 stone” and allowing patients to save money from a ritonavir prescription and overcome atazanavir-related hyperbilirubinemia. Preferred recruitment of impoverished, uninsured women may have further aggravated this circumstance, as previously described (5). Because of these limitations, any consequences of the trial on treatment guideline modifications should be evaluated with caution.

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Disclosures: Disclosures can be viewed at www.acponline.org /authors/icmje/ConflictOfInterestForms.do?msNum=L14-0591.

References

TO THE EDITOR: In their randomized, controlled trial, Lennox and colleagues (1) reported that discontinuations of atazanavir and ritonavir due to lack of tolerability were significantly more common than discontinuations of darunavir and ritonavir or raltegravir. This effect was mainly driven by atazanavir-induced jaundice and hyperbilirubinemia. This study is characterized by an innovative design in the field of HIV therapy because patients were allowed to change treatment groups if they were intolerant to randomly assigned treatment and because the time to discontinuation due to toxicity (as assessed and reported by the treating physician) was used as a predefined tolerability end point. Although we understand the logic behind this attempt to reproduce “real-life” conditions, we question whether this reproduction is within the possibilities and scope of a randomized, controlled trial.

In clinical practice, changing an antiretroviral regimen is a free and mutual decision by physicians and patients and is determined by many factors that are not present or accounted for during a randomized, controlled trial, such as socioeconomic characteristics, previous knowledge and experience with the drugs, peer influences, the physician-patient relationship, and the availability of all licensed alternative drugs. Last but not least, important determinants of physician prescriptions are patients’ expectations and, even more, physicians’ opinions on patients’ expectations (2). Therefore, the “threshold” of the tolerability of drug-related adverse effects varies not only among patients but also among physicians and may change over time within the same patient or physician. Also, ascribing a treatment switch to a single cause (“toxicity”) can be difficult and, ultimately, subjective.

For these reasons, we believe that assessing “discontinuation due to toxicity” as a component of a randomized clinical trial end point in the absence of a predefined definition of tolerability and toxicity can be challenging. In this respect, the data on treatment discontinuation rates obtained from observational cohorts of unselected patients are probably closer to reality.

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Disclosures: Disclosures can be viewed at www.acponline.org /authors/icmje/ConflictOfInterestForms.do?msNum=L14-0590.

References

IN RESPONSE: Dr. Stephan proposes a potential bias against ritonavir-boosted atazanavir due to the necessity to reimburse ritonavir copayments. He also suggests that efforts to ensure adequate representation of women may have enriched the study for impoverished participants predisposed to discontinue ritonavir-containing regimens to avoid costs. We did not target any particular socioeconomic subgroup of women for enrollment nor have we suggested that we did so. The potential biases associated with ritonavir use during the study were anticipated and addressed during trial design. As noted in the article, all participants not receiving ritonavir from a federal
insurance program were reimbursed in a timely manner. Furthermore, the protocol encouraged sites to make within-class regimen switches; as a result, 72% of those who discontinued ritonavir-boosted atazanavir switched to ritonavir-boosted darunavir.

Dr. Stephan also cites 3 studies in which hyperbilirubinemia was not a common reason for atazanavir cessation. One of these studies (ACTG A5142) did not include atazanavir. Another study (ACTG A5202) placed stringent limitations on regimen switching because it compared not only ritonavir-boosted atazanavir with efavirenz but also separate nucleoside reverse transcriptase inhibitor combinations. The third study (CASTLE [Comparing the Antiviral Efficacy and Safety of Atazanavir/Ritonavir With Lopinavir/Ritonavir, Each in Combination With Fixed-Dose Tenofovir-Emtricitabine in HIV-1-Infected Treatment-Naive Subjects]) compared a 3-pill, once-daily boosted atazanavir regimen with a 6-pill, twice-daily boosted lopinavir regimen, a design that may have discouraged regimen changes. Our study shows that some participants who develop cosmetically intolerable hyperbilirubinemia will choose to change therapy if equally effective and convenient options are available.

We agree with Drs. Lapadula and Gori that careful planning is essential for studies that include discontinuation of treatment due to toxicity as an end point. For this reason, toxicity end points in our study were predefined and strictly categorized according to the Division of AIDS toxicity table. Tolerability was also strictly defined as the intent of the participant to discontinue the treatment because of toxicity. We believe that participant-driven tolerability discontinuations for toxicity are a necessary and useful measure for comparing commercially available medications. However, we agree with Drs. Lapadula and Gori that study of observational cohorts can further inform results from randomized studies. In a recent analysis, 11.7% of Korean patients discontinued atazanavir because of jaundice (1) compared with 7.8% of participants in our study. This rate of discontinuation took place despite Koreans’ having a lower frequency of a polymorphism in uridine 5’-diphosphate glucuronosyltransferase 1A1 associated with atazanavir-induced hyperbilirubinemia than the HIV-1-infected population in the United States (2, 3). Our study population may therefore have facilitated our ability to detect this important tolerability difference between ritonavir-boosted protease inhibitors recommended by the U.S. Department of Health and Human Services.

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Disclosures: Disclosures can be viewed at www.acponline.org/authors/icmje/ConflictOfInterestForms.do?msNum=M14-1084.

References

OBSERVATION

Mercury Poisoning Presenting as Sporadic Creutzfeldt-Jakob Disease: A Case Report

Background: Mercury poisoning is a rare and highly disabling condition (1). Because early chelation therapy can decrease serum mercury concentrations, identifying mercury poisoning in its earliest stage is important.

Objective: To alert clinicians that mercury poisoning may be confused with sporadic Creutzfeldt-Jakob disease (CJD).

Case Report: A 42-year-old male office clerk who lived alone and was in good health except for well-controlled hypertension began having progressive memory disturbance that continued for 5 months. He was irritable and had difficulty recalling recent conversations. He also reported numbness and tingling in the limbs, slow movement, and sleepiness. A magnetic resonance image (MRI) of the brain at another hospital 3 months after symptoms began showed striking, relatively symmetric, high-intensity, diffusion-weighted imaging signals in the cortex without gadolinium enhancement (Figures A and B). An electroencephalogram showed slow overall activity. The clinical course and MRI suggested CJD despite unremarkable results on cerebrospinal fluid studies with normal 14-3-3 protein levels, and the sporadic form of the disease was diagnosed because there was no mutation in the PRNP gene. He was treated with intravenous methylprednisolone, 500 mg/d for 7 days, followed by oral prednisone tapering.

His symptoms did not improve, and his disability accelerated 1 week before admission to our hospital in March 2012. Neurologic examination found prominent cognitive impairment and extrapyramidal features. His Folstein Mini-Mental State Examination score was 20 out of 30. He had bilateral symmetric bradykinesia, cogwheel rigidity in the upper limbs, and impaired finger-nose coordination. An MRI of the brain revealed hyperintense diffusion-weighted imaging signals and hypointense apparent diffusion coefficient signals in the caudate nucleus and the frontal, temporal, and occipital cortices (Figures C and D). Results of screening tests for connective tissue disease and blood titers for rapid plasma reagin and voltage-gated potassium-channel and paraneoplastic antibodies were normal. An electromyogram revealed remarkably decreased sensory amplitudes in the limbs. Screening tests for heavy metals found a blood mercury level of 76.3 nmol/L (levels fluctuate with dietary intake and other...
We diagnosed elemental mercury poisoning and administered the chelating agent sodium 2,3-dimercapto-1-propanesulfonic acid. Numbness and tingling decreased substantially, memory and movement improved slightly, but MRI attenuation recovery images 3 mo after symptom onset. C and D. Hyperintense diffusion-weighted imaging signals and hypointense apparent diffusion coefficient signals in subcortical gray matter (caudate nucleus) and cortical gray matter 5 mo after symptom onset. E. No significant improvement on diffusion-weighted imaging after chelation therapy. F. Brain atrophy at 2-y follow-up.

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Discussion: Elemental mercury poisoning is rare, and inhalation of mercury vapor is the major route (1, 2). The only previous report with neuroimaging involved a 10-year-old child with isolated hyperintense lesions (3). We report what we believe to be the first case with neuroimaging studies of elemental mercury poisoning in an adult. These images show widely symmetric cortical and subcortical gray matter involvement. We are not surprised that sporadic CJD was the initial diagnosis given the combination of subacute cognitive impairment, Parkinsonian symptoms, and linear lesions in the bilateral cerebral cortex and the caudate nucleus (4), although other conditions have been reported to mimic sporadic CJD, including neurodegenerative diseases and antibody-mediated encephalitis (5). Clinicians should consider elemental mercury poisoning in patients suspected of having sporadic CJD.

Correction: Combination Antifungal Therapy for Invasive Aspergillosis

A recent article (1) had an error in Appendix Table 2. The percentage of GM antigen positivity only in patients receiving combination therapy should be 80%, not 15% as reported.

This has been corrected in the online version.

Reference

Correction: The Benefits of Detecting and Treating Mild Hypertension

Sundström and colleagues’ analysis included patients at a mean age of 63.5 years, not 18 to 60 years as originally stated in the accompanying editorial (1).

This has been corrected in the online version.

Reference