COMMENTS AND RESPONSES

Determining the Benefits of the New York City Trans Fat Ban

TO THE EDITOR: Angell and colleagues (1) want to be congratulated for finding that their own regulatory approach has succeeded—according to them. The New York City Department of Health and Mental Hygiene passed a regulation restricting the use of trans fatty acids (TFAs) in chain restaurants, and the authors have discovered that—lo and behold!—this law is being obeyed. But what have they accomplished in terms of public health? That is a difficult question to answer.

The amount of TFAs in one's diet is repeatedly referred to as a "recognized risk factor" for heart disease. However, this assessment is based entirely on observational dietary studies—and diet is not listed among the main coronary risk factors. Furthermore, on the basis of my 20-plus years of practicing internal medicine, I can attest to how hard it is to significantly reduce lipid levels through diet, despite all the stringent diet programs we so assiduously distributed to our at-risk patients. Now, at last, the authors assert that they have found the culprit: TFAs. They say that by eliminating these heart-attack generators, we can relax and throw away those statins.

Not so fast. Is there a shred of evidence that decreasing TFA intake actually lowers lipid levels—much less reduces the toll of cardiovascular disease? No. So why are the authors trumpeting how their new regulations have led to restaurant food with "healthier fatty acid profiles"?

Health issues aside, where will such measures lead us? In the accompanying editorial (2), Gerberding says, "Unfortunately, relying on consumers alone to make healthy choices about food intake is a strategy that has not worked, as our growing obesity epidemic demonstrates." Since when is "allowing" Americans to choose their own food a public health strategy? And what does TFA intake have to do with the obesity epidemic? Not a thing, as I'm sure Gerberding knows.

If the authors are allowed to merely declare that their government regulatory program on TFAs is a success, there are in all likelihood more such strategies for "guiding" consumer health choices coming down the pike, to your local pharmacies and restaurants, and your kitchens. The authors apparently believe that we cannot be trusted to figure out our own diet choices regarding our health, and they nominate the New York Department of Health and Mental Hygiene to be the food arbiter of first resort. This would be a bad idea for health and for personal responsibility.

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Potential Conflicts of Interest: The American Council on Science and Health, a nonprofit consumer education organization, accepts no-strings-attached donations from corporations, individuals, and foundations. Food-related companies have contributed well under 2% of the Council's budget.

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1. Angell SY, Silver LD, Goldstein GP, Johnson CM, Deitcher DR, Frieden TR, et al. Cholesterol control beyond the clinic: New York City's trans fat restriction. Ann Intern Med. 2009;151:129-34. [PMID: 19620165]
2. Gerberding JL. Safer fats for healthier hearts: the case for eliminating dietary artificial trans fat intake [Editorial]. Ann Intern Med. 2009;151:137-8. [PMID: 19620167]

TO THE EDITOR: What Angell and colleagues (1) could not say was how this initiative has actually helped the citizens of New York. The presumption is that ridding the food supply of TFAs will reduce the cholesterol levels of millions of people. Improving the cholesterol levels should result in reduced cardiovascular disease, but has it? Because most New Yorkers eat in restaurants, often several times a week, there should be a significant decrease in the number of heart attacks and heart attack deaths, right? And certainly, with all the modern statistical tools we have available and with a precise knowledge of the exact date that the TFA ban went into effect, we must be able to show the health benefits that accrued from the New York ban. Surely we can be spared the lame excuse that "cardiovascular disease is multifactorial; therefore, we can't really tell whether the ban worked." No metrics are in place to measure what should have been the primary goal of the ban. The persons behind the ban felt that they could dispense with the trivialities of actually determining whether their initiative really accomplished anything. The journey is more important than the destination, as they say. The fact is, there is nothing in place to "show me the money."

For 1500 years, the world believed in "spontaneous generation" simply because the Greek philosophers said it was so. Francis Bacon, often referred to as the father of the scientific method, didn't buy it and effectively said, "Show me the money!" Credibility must be based on experimentally derived evidence.

The scientific integrity of the United States and its great institutions is declining because we are associating credibility with institutions rather than with evidence. We accept notions without demanding proof. It may well be that spontaneous generation is real and that New York's citizens have benefited from the TFA ban, but until objectively obtained evidence confirms this, the only ones who have really benefited are the persons and institutions who have garnered unwarranted praise and free publicity. Consumers should be served a lot better than that.

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Potential Conflicts of Interest: None disclosed.

Reference

1. Angell SY, Silver LD, Goldstein GP, Johnson CM, Deitcher DR, Frieden TR, et al. Cholesterol control beyond the clinic: New York City's trans fat restriction. Ann Intern Med. 2009;151:129-34. [PMID: 19620165]

IN RESPONSE: Regarding Dr. Ross's comment, evidence supporting restriction of TFA use is not limited to observational studies. A multitude of controlled feeding trials (1) have assessed the effect of TFAs on serum lipid profiles. Meta-analyses of up to 60 of these trials (2, 3) show a relative increase in low-density lipoprotein cholesterol levels and a decrease in high-density lipoprotein cholesterol levels when TFAs are consumed, compared with saturated or *cis* unsaturated fats. As Gerberding says in her accompanying editorial (4), the science for eliminating exposure is "rock solid."

Mr. Satin expresses frustration that measuring the isolated effect of TFA restrictions may not be possible, even with modern statistical tools. Unlike biomedical research, evaluation of population-level health interventions does not always allow for randomized, controlled trials. Once there is scientific consensus that the exposure of interest is dangerous, such as for lead-based paint, pesticides, or TFAs, such trials may also be unethical. That said, evaluation is integral to good public health practice, and the best possible methods should be used to assess interventions.

Evaluations under way include assessment of replacement products, changes in fatty acid composition of foods, and investigation of coronary heart disease mortality. We are assessing changes in coronary heart disease risk and in markers of TFA intake coronary by repeating NYCHANES (New York City's 2004 Health and Nutrition Examination Survey). The NYCHANES included a representative sample of sera in repository, which can provide a preintervention baseline.

In her commentary (4), Gerberding affirms the substantial public health risk associated with artificial TFAs but raises concern that healthier oil supplies are not sufficient for replacement at the national level. Similar arguments were raised in 2005 at the time of our proposal. All failed to materialize as practical obstacles. No shortages of safer replacement products have been reported, and saturated fat use has declined in at least some areas. Indeed, the food industry proved efficient when faced with a clear imperative and an appropriate timeline.

Finally, Dr. Ross says that restricting TFA use reduces consumer choice. We beg to differ. Artificial TFAs were added to restaurant meals unbeknownst to consumers; menus never offered a choice between french fries with or without artificial TFA. Industry chose to use artificial TFAs because of practical industrial advantages. As its dangers are now apparent, there is no reason not to remove it from our food.

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Potential Conflicts of Interest: None disclosed.

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Reclassification Calculations for Persons With Incomplete Follow-up

TO THE EDITOR: We applaud Cook and Ridker (1) for their clear discussion of reclassification measures and recent developments in judging the incremental value of a biomarker for prediction of outcome. A key area of application is in cardiovascular disease, in which the time horizon is typically 10 years. One important problem recognized by Cook and Ridker is that not all persons will have follow-up completed until 10 years. Kaplan–Meier curves and Cox regression analysis were introduced long ago to deal with such censored observations. Reclassification measures, such as the net reclassification index (NRI) (2), have been proposed for binary data and currently have no way of incorporating incomplete follow-up. As with other model performance measures in survival analysis, reclassification statistics can be estimated at different times during follow-up.

To address the issue of censored data, Cook and Ridker propose selecting only persons with follow-up complete at a certain time point (8 years in their example). They included most control participants, because 23 611 of 23 792 women had follow-up of at least 8 years, excluding only 181 (1%). But only 560 of 766 women had a cardiovascular event before 8 years of follow-up, leading to exclusion of 206 (27%).

We suggest a simple alternative based on the expected number of case patients and control participants calculated by using the Kaplan–Meier estimator. This approach was recently found optimum in assessing calibration of survival models (3). It appropriately handles censored data and does not throw away useful information. We provide a revision to Figure 1 of the original article (**Table**) created with our proposal, with cell entries for case patients and control participants obtained by multiplying the 10-year Kaplan–Meier rates by the total persons in each cell at 10 years given in the original table. We then expect 697 case patients and 23 861 control participants at 10 years of follow-up.

The reclassification numbers change to some extent. Although the conclusions remain largely the same in this example (NRI, 9.9% vs. 9.8% in the original), we recommend our simple estimation procedure of the NRI for future application with censored observations. Especially when more censoring occurs early during follow-up, our approach is attractive. In this case, choosing 1 time point for analysis can lead to exclusion of many control participants, or relatively many case patients, making the NRI estimate unstable. Some specific issues, such as bias and precision, require further research. We note that the asymptotic CI for NRI calculated with the approach outlined by Pencina and colleagues (2) is no longer valid for the current extension. A practical solution would use bootstrap estimation (4), which is also useful for bias correction (as also suggested by Cook and Ridker).

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Table. **Reclassification Table Comparing 10-Year Risk Strata for Models That Include Risk Factors for Cardiovascular Disease in the Women's Health Study With and Without SBP***

Model Without SBP	Model With SBP, *n* (%)				Total, *n* (%)	Reclassified Into New Risk Category, %		
0% to <5%	0% to 5%	5% to 10%	10% to 20%	≥20%		Lower	Higher	Total
Persons included	20 372 (96.6)	696 (3.3)	23 (0.1)	0.0 (0.0)	21 091 (85.9)	0.0	3.4	3.4
Case patients†	264.8 (84.8)	47.3 (15.2)	0.0 (0.0)	0.0 (0.0)	312.2 (44.8)	0.0	15.2	15.2
Control participants†	20 107.2 (96.8)	648.7 (3.1)	23.0 (0.1)	0.0 (0.0)	20 778.8 (87.1)	0.0	3.2	3.2
Observed risk, %‡	1.3	6.8	0.0					
5% to <10%								
Persons included	635 (26.6)	1441 (60.3)	307 (12.8)	7 (0.3)	2390 (9.7)	26.6	13.1	39.7
Case patients†	27.9 (14.3)	121.0 (61.8)	44.8 (22.9)	1.2 (0.6)	196.0 (28.1)	14.3	23.5	37.8
Control participants†	607.1 (27.7)	1320.0 (60.2)	262.2 (11.9)	5.8 (0.3)	2194.0 (9.2)	27.7	12.2	39.9
Observed risk, %‡	4.4	8.4	14.6	17.5				
10% to <20%								
Persons included	4.0 (0.5)	204 (25.0)	519 (63.5)	90 (11.0)	817 (3.3)	25.5	11.0	36.5
Case patients†	0.0 (0.0)	8.8 (7.7)	74.2 (65.2)	30.8 (27.1)	113.8 (16.3)	7.7	27.1	34.8
Control participants†	4.0 (0.6)	195.2 (27.8)	444.8 (63.2)	59.2 (8.4)	703.2 (2.9)	28.3	8.4	36.8
Observed risk, %‡	0.0	4.3	14.3	34.2				
≥20%								
Persons included	0.0 (0.0)	2.0 (0.8)	54 (20.8)	204 (78.5)	260 (1.1)	21.5	0.0	21.5
Case patients†	0.0 (0.0)	0.0 (0.0)	14.0 (18.9)	60.0 (81.1)	74.0 (10.6)	18.9	0.0	18.9
Control participants†	0.0 (0.0)	2.0 (1.1)	40.0 (21.5)	144.0 (77.4)	186.0 (0.8)	22.6	0.0	22.6
Observed risk, %‡		0.0	25.9	29.4				
Total								
Persons included	21 011 (85.6)	2343 (9.5)	903 (3.7)	301 (1.2)	24 558 (100.0)			
Case patients†	294.2 (42.3)	178.1 (25.6)	132.7 (19.1)	91.8 (13.2)	695.9 (100.0)			
Control participants,†	20 716.8 (86.8)	2164.9 (9.1)	770.3 (3.2)	209.2 (0.9)	23 862.1 (100.0)			
Observed risk, %‡	1.4	7.6	14.7	30.5				

SBP = systolic blood pressure.
* Using the 10-year Kaplan–Meier estimates to estimate the number of case patients and control participants. Reclassification improved by 10.5% in case patients (124 to 51 of 696), whereas classification worsened in control participants by 0.6% (848 to 999 of 23 861), leading to a net reclassification improvement of 9.9%.
† Originally, case patients and control participants were counted at 8 years of follow-up, ignoring censored observations. Here, we use the 10-year Kaplan–Meier estimates to estimate the number of case patients and control participants (e.g., 1.3% × 20 372 = 264.8 cases expected at 10-year follow-up).
‡ Observed risk at 10 years is estimated from the Kaplan–Meier curve by using observations in each cell.

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IN RESPONSE: We thank Drs. Steyerberg and Pencina for bringing up an important point about evaluating reclassification measures in the presence of survival data. When the outcome is time to an event, such as a cardiovascular event, care must be taken to accommodate censoring. The reclassification calibration statistic can easily be calculated by using survival data, as indicated in our article. The Kaplan–Meier estimate of the event rate as of 10 years, for example, can be used to obtain the expected number of events within each cell of the reclassification table. D'Agostino and Nam (1) suggest that with survival data, the degrees of freedom should be $k - 1$ rather than $k - 2$, where k in the setting of reclassification is the number of cells containing at least 20 persons.

The use of survival data is more problematic for the NRI and integrated discrimination improvement, which both rely on case–control status. A similar problem occurs for the *c*-statistic, but methods to accommodate survival data have been established (2). For the NRI, Drs. Steyerberg and Pencina propose using the expected number of cases based on the Kaplan–Meier estimate in each cell—the same calculation needed for the reclassification calibration statistic. Although an estimated SE is not currently available for this measure, a CI and an SE can be determined by using bootstrap samples.

We suggest that both the reclassification calibration statistic and the NRI be computed for reclassification tables, even in the presence of survival data.

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Potential Conflicts of Interest: None disclosed.

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Stent Placement in Patients With Atherosclerotic Renal Artery Stenosis and Impaired Renal Function

TO THE EDITOR: When the results of a randomized trial disagree with clinical experience and previous publications, their validity merits its critical examination. In the STAR (STent placement and blood pressure and lipid-lowering for the prevention of progression of renal dysfunction caused by Atherosclerotic ostial stenosis of the Renal artery) trial assessing the efficacy of renal artery stenting, Bax and colleagues reported no benefit and recommended avoiding stenting in patients with renal artery stenosis (1). This could lead to denial of the procedure, by physicians and by managed care organizations, to patients who truly need it.

We have personally observed many patients who, after renal artery angioplasty with or without stenting, experienced rapid amelioration of such problems as refractory hypertension, congestive heart failure, and renal insufficiency. Thus, it is inconceivable to us, and to many others, that stenting is of no benefit. Unfortunately, there is also no doubt that stenting is overused, often done in cases where it is unlikely to benefit.

Why did this randomized trial show no benefit? First, among the patients randomly assigned to the stenting group and included in the intention-to-treat analysis, 40 of 64 (62.5%) were unlikely to benefit because 12 had stenoses less than 50% and were not even stented; 22 had stenoses of 50% to 70%, which usually is not hemodynamically significant (even some stenoses of 70% to 90% are not hemodynamically significant [2]); and stenting was not performed for various reasons in 6 others. Unfortunately, hemodynamic significance of the stenoses was not assessed. Second, all patients were required to have a treated blood pressure less than 140/90 mm Hg on entry, thus excluding patients with resistant hypertension, who are more likely to have true renovascular hypertension and ischemic nephropathy. Third, there is a major unmentioned bias: Patients strongly believed to have true renovascular hypertension, who would be the most likely to benefit, are generally referred for stenting rather than being entered into a randomized study that could deny them the procedure. There is no easy answer for this problem, and in such situations, a randomized trial might be the wrong type of study.

Extrapolation of the results of this study to patients with unequivocal renovascular hypertension and ischemic nephropathy is unwarranted and wrong. Clearly, there are cases where the wisdom of stenting is unclear, but this study did not address that important question.

The overuse of renal artery angioplasty and stenting clearly merits condemnation. However, the benefit of the procedure in appropriate patients should not be withheld on the basis of this trial. Instead, clarification of the indications for stenting is needed.

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Potential Conflicts of Interest: None disclosed.

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TO THE EDITOR: We read with interest the randomized trial on renal artery stenting by Bax and colleagues (1). We agree that randomized trials are ultimately the way to prove or disprove the usefulness of renal artery angioplasty and stenting in the treatment of renal artery stenosis. However, we are concerned that, because of several issues, this particular study will do neither.

First, the abstract is inconsistent. The Results section, which should essentially reflect a nonsignificant reduction in the primary end point and no significant difference in the secondary end point, is not adequately summarized, and the Conclusion section does not reflect the fact that, despite the unusually high number and severity of procedure-related complications in the stenting group, mortality did not differ between the 2 groups. Because the abstract is the most widely disseminated part of any article, these shortcomings are very unfortunate.

Second, as acknowledged by the authors and in the editors' notes, the study was underpowered because the rate of events in the control group was lower than anticipated. This impairs the study's ability to detect a real difference between renal artery stenting and medical therapy (2), although it is noteworthy that the point estimate of the hazard ratio for the primary end point (0.73) favors stenting. Moreover, the evidence for renal revascularization therapy to halt progression of renal insufficiency is not solid and, as such, choosing an increase in creatinine as the primary end point was not a good choice.

Finally, the significance of renal artery stenosis was not physiologically assessed—a widespread problem in current randomized trials of renal stenting (3). Indeed, patients with renal stenosis severity as low as 50% were included, when the evidence suggests that stenoses less than 70% are not hemodynamically significant (4). Inclusion criteria contained no stringent requirement of therapy with at least 3 antihypertensives, as required by the American College of Cardiology/American Heart Association guidelines (5). These factors may also have biased the study results toward showing no benefit with renal stenting.

In summary, every piece of evidence that contributes to our better understanding of the optimal therapy of renal artery stenosis is welcome, but only if the evidence is clear. Unfortunately, the study by Bax and colleagues raises more questions than provides answers and will make it harder for other investigators to obtain support for future trials addressing the issue.

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IN RESPONSE: Our study showed that stent placement incurs costs and complications for an at best uncertain benefit in patients with impaired renal function and atherosclerotic renal artery stenosis (ARAS). For these patients, stenting remains a subject of research. We want to emphasize that we focused our study on impaired renal function and ARAS and therefore do not make statements about other categories of patients, such as those with renovascular hypertension, therapy-refractory hypertension, or congestive heart failure.

As discussed in our article, the lower-than-anticipated event rate reduced the power of our study, and we might therefore be dealing with a chance finding. In the meantime, the ASTRAL (Angioplasty and Stenting for Renal Artery Lesions) study (1) from the United Kingdom, which includes more than 800 patients, has found similar results and did not show any effect in predefined subgroup analysis, such as severe renal dysfunction or severe or bilateral renal artery stenosis.

The statement by Drs. Mann and Sos that patients are unlikely, a priori, to benefit from stent placement is simplistic and is not correct, because the different categories stated largely overlap.

Both Drs. Mann and Sos and Drs. Jovin and Topaz suggest that poor patient selection (for not using functional stenosis tests) and including patients with stenoses of 50% to 70% explains the negative results. The hypothesis that patients with more severe ARAS would benefit more from stent placement has, however, not been demonstrated in terms of renal function. In fact, the severity of the stenosis is not correlated with renal function in patients with ARAS and impaired renal function (2), and it is neither a predictor of progression of renal failure nor a predictor of outcome after revascularization (3–5). Yet, most of our patients (67%) had stenosis greater than 70% to the most affected kidney. When we designed our study back in 1999, a reduction in luminal diameter of 50% or more (corresponding to a surface reduction >70%) was widely considered to be clinically significant (6). Although our insight on the relationship between stenosis severity and degree of renin release by the poststenotic kidney and renovascular hypertension may have improved in the last decade, functional tests have not been proven to predict favorable outcome after stent placement as far as renal function is concerned. This underscores the fact that the pathophysiology of renal failure in this group of patients is extremely complex. In addition to reduced blood flow, renal function in these patients is also the dependent on presence of small-vessel disease, glomerulosclerosis, and renal fibrosis.

Although there may be some criticism, our study shows that indiscriminate widespread introduction of stent placement without proper scientific evaluation is unjustified, costly, and dangerous for patients.

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Clinical Observation

Colored Sweat Caused by Pseudochromhidrosis

Background: Pseudochromhidrosis and chromhidrosis are conditions associated with colored sweat. Pseudochromhidrosis is production of colorless sweat that becomes colored when it reaches the skin and combines with other agents, whereas chromhidrosis is a rare condition associated with production of colored sweat by apocrine sweat glands.

Objective: To report a case of a patient with pseudochromhidrosis.

Case Report: A 36-year-old white man presented with a 2-year history of brown-black sweat on his face, neck, and thorax. Colored sweat was intermittent and was not related to a specific time of day, body position, or food consumption but was pronounced after participation in sports. It was not accompanied by odor, fever, local or systemic symptoms, or changes in the patient's skin appearance. The sweat dyed his clothes (**Figure**), pillows, and sheets. He did not use deodorants. His medical history was unremarkable, and he reported no trauma, known allergy, or drug use. The colored sweat was not associated with clothing or exposure to chemicals or dyes. Factitious disorders were ruled out after psychiatric evaluation.

Complete blood count, prothrombin time, partial thromboplastin time, international normalized ratio, biochemistry, and urinanalysis were normal. The toxicology examination was negative. Determination of 5-hydroxyindoleacetic acid to rule out the carcinoid syndrome and of homogentisic acid to rule out alkaptonuria and ochronosis in a 24-hour urine collection were normal. Gram stains of smears obtained from the affected sites for pathogen isolation were negative. Computed tomography of the abdomen and indium-111 pentetreotide scintigraphy (Octreoscan, Covidien, Mansfield, Massachusetts), which we performed to further examine the possibility of the carcinoid syndrome, were normal.

The patient underwent skin biopsy from the frontal thorax. Histologic examination after periodic acid–Schiff, Giemsa, and Gram staining revealed the presence of bacteria with a fibrillary and diphtheroid shape type "Y," which is characteristic of *Corynebacterium*. Small gatherings of atrophic, cystically dilated hidropoietic glands surrounded by infiltration with inflammatory cells were found; no lipofuscins were found in the sweat glands, which ruled out chromhidrosis. Thus, the diagnosis of pseudochromhidrosis due to skin infection with *Corynebacterium* was confirmed. The patient was treated with oral erythromycin, 250 mg 3 times daily for 10 days, combined with topical application of erythromycin gel. Complete resolution was obtained at 7 days, and the disorder did not recur in the next year.

Figure. **Brown sweat in a patient with pseudochromhidrosis.**

The patient's shirt is dyed by brown sweat due to infection with *Corynebacterium*. He was successfully treated with oral erythromycin, 250 mg 3 times daily for 10 days, combined with topical erythromycin gel.

Discussion: Pseudochromhidrosis is production of colorless sweat that becomes colored when it reaches the skin and combines with other agents, usually chromogenic bacteria (*Corynebacterium* and *Pieridae* species) or chromogen material, such as extrinsic dyes and paints (bromophenol blue or copper) (1).

Chromhidrosis is a rare condition associated with production of colored sweat by apocrine sweat glands. The disease is more common in black persons. The colored sweat is frequently confined to the face and axillae and occasionally to the abdomen, chest, thighs, groin, and genitalia. Cases of red, blue, green, yellow, and black sweat have been reported (2, 3). Chromhidrotic apocrine glands are characterized by deposition of lipofuscins in a higher-than-normal concentration or a higher-than-normal state of oxidation of unknown cause. The diagnosis is made by examination of skin biopsies obtained from the affected sites by ultraviolet light at 360 nm (Wood's lamp) for the presence of lipofuscins, which have a yellowish to green appearance. An even rarer condition is chromhidrosis due to production of colored sweat by the eccrine sweat glands after ingestion of certain dyes or drugs.

The differential diagnosis includes bleeding disorders, hyperbilirubinemia, alkaptonuria, ochronosis, the carcinoid syndrome, dermatitis simulate, colonization or infection with *Pseudomonas* species, chromhidrosis, pseudochromhidrosis, poisoning, and copper exposure (4, 5).

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Potential Conflicts of Interest: None disclosed.

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Corrections

Correction: Screening for Breast Cancer

There is an error in the recent U.S. Preventive Services Task Force recommendation statement on breast cancer screening (1).

Lines 7 to 8, page 719, right column, should say " . . . increases steeply with age, starting in the 40s." This has been corrected in the online version.

Reference

1. US Preventive Services Task Force. Screening for breast cancer: U.S. Preventive Services Task Force recommendation statement. Ann Intern Med. 2009;151:716-26, W-236. [PMID: 19920272]

Correction: Probiotics for Ulcerative Colitis

In the letter by Conen and colleagues (1) on probiotics for ulcerative colitis, the authors should be listed as follows:

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This has been corrected in the online version.

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1. Conen A, Zimmerer S, Trampuz A, Frei R, Battegay M, Elzi L. A pain in the neck: probiotics for ulcerative colitis [Letter]. Ann Intern Med. 2009;151:895-7. [PMID: 20008769]