and adults, conversion of ingested nitrate to nitrite is enhanced. Premature or newborn infants may also be more susceptible because of their higher levels of fetal hemoglobin (HbF).[12] Normal HbF fractions at birth are approximately 65% to 80% of total hemoglobin. During the next few months after birth, hemoglobin F production declines and production of hemoglobin A predominates. By the age of 4 months, normal HbF fractions are approximately 10% of total hemoglobin.[13] Infant concentrations of nicotinamide–adenine dinucleotide-dependent methemoglobin reductase (an enzyme responsible for reduction of methemoglobin back to normal hemoglobin) provides only half the reductase activity present in adults,[14] thus resulting in increased methemoglobin that place newborns and young infants fed formula diluted with nitrate-contaminated well water at higher risk for toxicity.[15]

It seems certain that continued advancement will occur in the measurement of dyshemoglobinemias such as methemoglobinemia that can pose life-threatening toxicities. At this time, Masimo is awaiting U.S. Food and Drug Administration (FDA) 510(k) clearance for its new bedside Radical-7 pulse oximeter, which will allow continuous noninvasive measurements of COHb, MetHb, O_2 saturation, pulse rate, and perfusion index. The Rad-57 cm pulse oximeter is the newest FDA-approved advancement in screening devices offering several advantages over older pulse oximetry technology. Additional data in the higher ranges of MetHb and COHb in higher ranges would be helpful in assessing the accuracy of the device as referenced by Drs Reddy.[15,16] This device is likely to be clinically useful in several settings, including occupational and environmental sites where rapid, simple, noninvasive assessments of MetHb, COHb, and oxyhemoglobin are warranted.

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Lung Cancer Mortality in the German Chromate Industry, 1958 to 1998

To the Editor: To put in accurate context the article by Birk et al that concludes that the study's findings suggest "a possible threshold effect of occupational hexavalent chromium exposure on lung cancer,"[1] three points should be considered: 1) these data were withheld from a federal rulemaking proceeding, 2) these data were actually part of a larger study that refutes the authors' conclusion, and 3) after examining the study, the U.S. Occupational Safety and Health Administration (OSHA) rejected the authors' conclusion.

The issue of the carcinogenicity of hexavalent chromium (Cr[VI]) is one of great public health and regulatory interest. In October 2004, OSHA issued a proposed standard that would have greatly reduced workplace exposures to Cr(VI).[2] The proposed rule included a risk assessment that was based on data from workers with generally higher exposure levels than those seen in the Birk et al article. Throughout the rulemaking proceeding, OSHA asked for epidemiologic

data on workers with lower exposure to Cr(VI), but neither the chromium industry nor the epidemiologists who conducted this study provided it to the agency at the public hearings or during the public comment period.[3]

In fact, the data presented by Birk et al were part of a larger multiplant study done for the chromium industry, completed more than 2 years before OSHA issued its proposed standard, but never published or given to OSHA by its authors.[3] That study, in which workers from four low-exposure facilities (two German facilities and two U.S. ones) were combined into one cohort reported significantly elevated lung cancer risk at both high and intermediate exposure levels, along with a clear dose–effect relationship[4] (see Table 1). Despite repeated emphasis in the study protocol and the final report on the need to maximize statistical power by combining data from the four facilities,[4,5] the investigators subsequently divided this study into two components and published two statistically underpowered studies.[1,6] In their logistic regression in the second of these papers, Birk et al combined the low and intermediate exposure categories from the final report into a single referent category. The result was the disappearance of the finding of greatest regulatory importance: the increased risk of lung cancer among those in the intermediate exposure group whose exposures were well below the OSHA standard in effect at the time and close to the exposure limit OSHA was contemplating. The dose–effect relationship vanished as well, because only a single nonreferent exposure category remained (see Table 2).[3] In comments to OSHA, two industry groups cited the supposed lack of a positive finding at low exposures in the German data as evidence the proposed OSHA standard was unnecessarily stringent.[7,8]

The authors justify their straying from their own protocol and dividing the study into two smaller, underpowered studies by noting that exposure history was estimated using airborne Cr(VI) levels in the U.S. facilities and urinary chromium levels in the German plants. In the final report, provided to the chromium manufacturing companies that paid for the study, these data were combined, using, ironically, a conversion factor[9] that is cited in the present paper when comparing the measured urinary Cr(VI) levels to OSHA's permissible exposure limit.

It is important to note that OSHA has rejected Birk et al's conclusion that "these data suggest a possible threshold effect" of Cr(VI) exposure on lung cancer. The agency's scientists explained in the final standard that the Birk et al study's "small cohort size, few lung cancer cases (eg, 10 deaths in the three lowest exposure groups combined) and limited follow up (average 17 years) severely limit the power to detect small increases in risk that may be present with low cumulative exposures."[10]

The Occupational Safety and Health Act instructs the agency to use the "best available evidence."[11] We believe that failure to provide the original study to OSHA, as well as the decision to bifurcate the study into two separate, underpowered publications, made after the final report was complete and the results were known, are inconsistent with the obligation of scientists to fully and promptly report findings of public health importance, even those that may trouble the sponsors of their study.

TABLE 1
Elevated Lung Cancer Mortality Risk in Intermediate and High Exposure Groups in Original Unpublished Study*

Cumulative Exposure to Cr(VI)	OR†	95% CI
Low (<40 μg/L)	Reference	—
Intermediate (40 μg/L to <200/L)	4.9	1.5–16.0
High (≥200 μg/L)	20.2	6.2–65.4

*Adapted from Table 17 in Final report: collaborative cohort mortality study of four chromate production facilities, 1958–1998.[4]
†Odds ratio from logistic regression analysis.
CI indicates confidence interval.

TABLE 2
Increased Risk of Lung Cancer Mortality Risk Among Workers With Intermediate Exposure Disappears in *JOEM* Publication of German Component of Study*

Cumulative Exposure to Cr(VI)	OR†	95% CI
Low and intermediate (<200 g/L)	Reference	—
High (≥200 μg/L)	6.9	2.6–18.2

*Adapted from Birk et al.[1]
†Odds ratio from logistic regression analysis.
CI indicates confidence interval.

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Authors' Response

To the Editor: Michaels et al attempt to "put in accurate context" our recent paper on an epidemiological study of lung cancer mortality among a cohort of German chromate industry workers.[1] We address each of the three specific points relevant to our research, as well as an overarching policy issue, raised by Michaels et al.

First, Michaels et al claim that "these data were withheld from a federal rulemaking proceeding." We conducted the original study under a contract between Applied Epidemiology, Inc. and the Industrial Health Foundation (IHF) and submitted our 139-page technical report to IHF (dated April 7, 2003) completing our contractual obligation. It is standard in the consulting industry to undertake client assignments on a confidential basis. Although the IHF study was undertaken on such a basis, we had the right to publish the results after considering (without obligation) client comments. Without any further contract with IHF, we began preparing manuscripts long before the Occupational Safety and Health Administration (OSHA) review. At the urging of the U.S. Office of Management and Budget, however, we accelerated this process so that at least the basic results from the U.S. and the German cohorts might be published and available to the OSHA process. The U.S. paper[2] was accepted for publication immediately, whereas the German paper was held up in review by a different journal for many months—ironically until the end of the OSHA comment period—and rejected. We then submitted it to *JOEM* and, after revision (results and interpretations unchanged), it was accepted and provided to OSHA.

Michaels et al's second point is that "these data were actually part of a larger study that refutes the authors' conclusion." That the German paper was part of a larger study effort is of course correct, as described in both the U.S. and the German papers, as well as in a response to Michaels et al's earlier criticisms of the U.S. paper.[3] That the results refute our conclusion that the data suggest a threshold is inaccurate and based on information taken out of context.

Originally, the German cohort was part of a study that combined this group with a cohort of U.S. chromate industry workers to maximize statistical power. However, as explained in both papers and in response to Michaels et al's earlier criticisms, several substantial differences between the cohorts complicated the analyses. Specifically, differences were seen by gender (only the U.S. plants included women), age (U.S. employees were substantially younger at hire and at end of follow up), ethnicity (U.S. plants included black and Hispanic workers, German plants had nearly no minorities), smoking prevalence (U.S. plants had lower rates), and—perhaps most importantly—exposure data (U.S. plants only had air monitoring data, German plants relied mainly on urinalysis). Because of these potentially important differences by country, and the fact that 88% of the lung cancer deaths were observed among the German subcohort, colleagues attending the Epidemiology in Occupational Health Symposium held in Barcelona in 2002, where the study was publicly presented,[4,5] recommended that the results be stratified by country and reported separately. This suggestion had also been made after review of the study protocol by a senior academic epidemiologist who served as an external advisor.

While stratifying the results by country resulted in a U.S. report with low statistical power (fully acknowledged in the paper), it was felt that those results should be made available to the scientific community through a peer-reviewed publication. Consequently, a more straightforward statistical analysis of the German data was possible with statistical power only slightly lower than that of the combined cohorts. Because the risk for the U.S. plants was not elevated, and all U.S. deaths occurred in the lowest exposure category, the resulting lung

cancer standardized mortality ratio (SMR) for the German plants increased relative to the combined cohorts. SMR analysis by level of cumulative exposure (see Table 4 in the German paper) generated similar results to the combined cohort (also reflected in two additional tables in the client report) leading to our suggestion of a threshold.

We performed logistic regression analyses in the original study and, as described in our article, to investigate simultaneously the influences on the risk estimates of cumulative exposure, peak exposure, and smoking. We do not consider the results presented in Michaels et al's Table 1 to be reliable largely because of the substantial differences between the U.S. and German cohorts. Also, we note the instability of the models as reflected in the wide confidence intervals, the uncertain adequacy of the fit of the model, and the fact that the logistic regression did not take into account the time dependency of the cumulative exposure metric. The results of the logistic regression analysis for the German plants, described in our article and partially included in Michaels et al Table 2, were taken directly from Table 19 of the client report and were not, as suggested by Michaels et al, produced after the decision to report results separately by country. We have explained in both the publication and the client report that a "low" exposure category estimate was technically impossible, because there were no cases with all necessary data available in the "low" category in the German cohort.

Michaels et al's third point largely reiterates (by quoting OSHA) our own words: "Demonstrating a clear (and statistically significant) threshold response in epidemiological studies is difficult especially, as in this study, the number of available cases is relatively small, and the precise estimation of small risks requires large numbers."[1] We hope, however, that by presenting such results—even if only suggestive—we will stimulate additional useful epidemiologic research and expand scientific inquiry.

Finally, although offering no solution, Michaels et al do raise a legitimate regulatory/policy point: how can the regulatory community obtain all scientifically sound and relevant evidence for their deliberations? Policymakers and regulators readily rely on material published in the peer-reviewed scientific journals. It is understood—correctly or incorrectly—that the peer review process imparts some degree of consistency with current standards and therefore quality. Michaels et al previously asserted that "Parties involved in the rulemaking process should also be required to certify that they have submitted all relevant data to the public record, whether or not those data have undergone peer review."[6] They apparently include material that is not published and not subject to rigorous peer review to be among the "best available evidence" that OSHA is to use. We believe that this is not so straightforward and that fuller discussion is warranted.

We are disappointed that Michaels et al have chosen to speculate about (or at least not read carefully and represent accurately) our work for IHF and the publications derived from that work (especially because we did attempt to discuss these very points directly with Michaels during several phone conversations around May 2005). Nevertheless, we are grateful to the editor of *JOEM* for the opportunity (not offered by the journal in which Michaels et al first published their story[6]) to respond and to offer a clearer perspective on our research.

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The Beryllium Occupational Exposure Limit: Historical Origin and Current Inadequacy

To the Editor: We are writing in response to "The Beryllium Occupational Exposure Limit: Historical Origin and Current Inadequacy"[1] by Jonathon Borak. As Assistant Secretary of Energy for ESH, one of us (D.M.) was the federal official responsible for overseeing the Department of Energy's (DOE's) Chronic Beryllium Disease Prevention Program final regulation, which lowered the level triggering protection for beryllium-exposed

workers in the U.S. nuclear weapons complex from 2.0 $\mu g/m^3$ to 0.2 $\mu g/m^3$.

We share Dr Borak's conclusion that the current Occupational Safety and Health Administration (OSHA) occupational exposure standard (OEL) of 2.0 $\mu g/m^3$ is inadequate to protect workers from chronic beryllium disease (CBD). The scientific literature summarized by Dr Borak, particularly the cases among workers and community residents exposed to levels well below the current standard,[2,3] strongly suggests that there is no safe level of exposure to beryllium.

One clear policy implication of this literature is that the OSHA OEL needs to be dramatically strengthened and that beryllium exposure must be eliminated whenever possible to prevent CBD. Beryllium plays an important role in national security but given the metal's severe and well-established health effects, it is hard to justify using beryllium in golf clubs, bicycle frames, and other consumer products. Even if the workers manufacturing these products could be protected from all beryllium exposure, recent reports of CBD cases among metal recycling workers[4] underscore the need to remove beryllium from commerce.

The primary limitation of Dr Borak's editorial is his failure to recognize that OELs are shaped not only by science, but also by the actions of individuals and corporations, who wield political and economic power. In our view, one of the primary reasons the inadequate OEL for beryllium has remained unchanged is that Brush Wellman, the sole North American beryllium producer, has relentlessly opposed more protective standards for exposed workers.

In 1975, OSHA proposed a comprehensive standard to protect beryllium-exposed workers, including a plan to lower its workplace exposure limit to 1 $\mu g/m^3$, on the basis of beryllium's carcinogenicity.[5] OSHA's effort was foiled, however, by a collaboration of the DOE, the Department of Defense, and the beryllium industry.[6] (Secretary of Energy, Bill Richardson, acknowledged the industry–government intervention, explaining "Priority one was production of our nuclear weapons . . . [the] last priority was the safety and health of the workers that build these weapons."[7])

By the late 1980s, the existence of CBD cases among workers exposed to beryllium at levels below the existing standard led Dr Merril Eisenbud, the author of the Atomic Energy Commission's original (1949) standard, to end his support of the 2 $\mu g/m^3$ OEL.[8] This evidence also prompted the DOE to begin, in 1991, the administrative process of lowering the OEL applied in nuclear weapons facilities. Brush Wellman opposed the change, contending that DOE had no evidence "that the existing standard is unsafe or that the new proposed standard affords any greater degree or [sic] safety."[9]

Despite the compelling and growing evidence, Brush Wellman defended the 2 $\mu g/m^3$ OEL with a tautologic argument, essentially: "We will examine the work history of all workers with CBD; even if we do not find evidence they were exposed to levels above the standard, we will assume they have been, since CBD is only associated with excessive exposure levels." This can be seen in Brush's 1991 talking points, which advised its executives to defend the 2 $\mu g/m^3$ as follows: "(1) Experience over several decades has, in our view, demonstrated that levels of airborne beryllium within the OSHA threshold limit value afford a safe workplace. (2) In most cases involving our employees, we can point to circumstances of exposure (usually accidental), higher than the standard allows. In some cases, we have been unable (for lack of clear history) to identify such circumstances. However, in these cases we also cannot say that there was *not* excessive exposure"[10] (emphasis in original).

Around the same time, Brush Wellman also opposed the American Conference of Governmental Industrial Hygienists's (ACGIH's) efforts to lower the beryllium threshold limit value (TLV), writing "We feel the evidence for retaining the presently adopted TLV is compelling" [and] ". . . there is still no evidence of any diagnosed cases of CBD where the exposure level can be reasonable demonstrated to have been at or below the 2 μg level."[11]

Dr Borak states that in 1996 Brush Wellman (in a notice for customers) "expressed uncertainty that the OSHA PEL was adequately protective."[1] In its communications with regulatory officials, however, the manufacturer retained its traditional opposition. For example, in a meeting on the proposed DOE standard, an executive asserted that the company "is unaware of any scientific evidence that the standard is not protective. However, we do recognize that there have been sporadic reports of disease at less than 2 $\mu g/m^3$. Brush Wellman has studied each of these reports and found them to be scientifically unsound."[12]

By 1999, the ever increasing number of CBD cases rendered the claim that the old standard was safe less and less plausible. Yet, this did not compel Brush to endorse a more protective OEL. Rather, the manufacturer asked DOE to delay issuing a new standard, claiming that "important research is underway which may provide a scientific basis for a revision to the occupational standard for beryllium" and pointing to studies on particle size, particle number, and particle surface area.[13]

In retrospect, it is clear that Brush's interpretation of the evidence supporting the adequacy of the OEL was incorrect; independent experts recognized this inadequacy more than 15 years ago. The beryllium industry had a strong financial incentive to challenge the mounting evidence and to oppose regulatory action that would result in a lower exposure limit. It appears this incentive shaped the interpretation given to scientific evidence by scientists employed by the beryllium industry. A policy lesson here is that regulatory agencies should discount the interpretation of data by parties with financial conflict of interest.

Although the past cannot be changed, much can be done to better protect cur-