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Is Government Regulation Irrational?: A Reply to Morrall and Hahn by Richard W. Parker*

The modern debate over regulation has been shaped in very significant part by three widely-cited studies which purport to show that federal health, safety and environmental regulation is pervasively over-zealous and irrational.¹

One study by John Morrall, an OMB economist, claims that government regulations vary enormously in their life-saving cost-effectiveness and that many seem extraordinarily expensive, costing up to \$72 *billion* per life saved.² A second set of scorecards – compiled by Robert Hahn of the AEI-Brookings Joint Center for Regulatory Studies – claims that over half of all major regulations issued since 1981 fail a “neutral economist’s cost-benefit test” even using the government’s own numbers.³ The third study, co-authored by Bush’s regulatory “czar”, John Graham, claims that over 60,000 people lose their lives each year due to irrational government regulation – a situation Graham has called “statistical murder.”⁴

Only relatively recently have the studies themselves come under the glass, beginning with Lisa Heinzerling’s 1998 article in *Yale Law Journal* (critiquing Morrall) followed by my 2003 article in *Chicago Law Review* which examined all three studies.⁵ My critique concluded that:

“all three studies rely on undisclosed data and non-replicable calculations; use biased regulatory samples; misrepresent ex ante guesses about costs and benefits as actual measurements; and grossly under-estimate the value of lives saved, or the number of lives saved, or both. They also exclude all unquantified costs and benefits, disregard all questions about the fairness of the distribution of cost and risk, and conceal the large uncertainties that are present in virtually every regulatory analysis.”⁶

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1. For a thorough documentation of the extraordinary impact of these studies see Richard Parker, *Grading the Government*, 70 *U. Chi. L. Rev.* 1345, 1349-1354 (2003) [hereinafter *Grading Government*].

2. John F. Morrall III, *A Review of the Record, Regulation*, Nov./Dec. 1986 [hereinafter *Morrall 1986 study*].

3. Robert W. Hahn, *Regulatory Reform: Assessing the Government’s Numbers*, in Robert W. Hahn, *Reviving Regulatory Reform: A Global Perspective* 32 (2000) [hereinafter *Government’s Numbers (2000)*], updating Robert W. Hahn, *Regulatory Reform: What Do the Government’s Numbers Tell Us?*, in *Risks, Costs and Lives Saved* 208 (Robert W. Hahn ed., 1996) [hereinafter *Government’s Numbers (1996)*].

4. Tammy O. Tengs & John D. Graham, *The Opportunity Costs of Haphazard Social Investments in Life-Saving*, in *Risks, Costs and Lives Saved* (Robert W. Hahn ed., 1996).

5. Lisa Heinzerling, *Regulatory Costs of Mythic Proportions*, 107 *Yale L. J.* 1981, 1983 n. 2 (1998) [hereinafter *Mythic Proportions*];.

6. *Grading Government* at 1355 (cited in note 1).

In short, I found that these studies are so fundamentally flawed that they prove nothing at all about the rationality of regulation. Moreover, I showed that the worst of these flaws are endemic to the enterprise of compiling strictly numerical scorecards, thus rendering scorecards a defunct mode of analysis.

Tengs and Graham have not, to my knowledge, responded to their critics in print. Morrall and Hahn have issued replies – to which this essay responds in turn, beginning with Morrall.

I should emphasize at the outset, however, that on one thing we all agree. In Morrall's words, "Because of the significant contribution that good empirical analysis can make to saving lives through smarter regulation, it is important to correct erroneous charges."⁷ With the country and Congress evenly divided and regulatory policy at a crossroads, this has never been more true.

I. Morrall

Morrall's 1986 study has been enormously influential in breeding skepticism of government, particularly in the realm of health, safety and environmental regulation. But is his study reliable? The most important methodological concerns raised by his table are that his regulatory sample is biased against regulation; that he lowered agency estimates of regulatory life-saving benefits without documentation or clear justification; that he then further discounted these life-saving benefits at excessively high rates over arbitrary (and probably excessive) time periods; and that, in any case, his numbers ignore all unquantified benefits as well as some potentially important categories of quantified benefits.⁸

Morrall insists, in reply, that his regulatory sample is fair and that his estimates of regulatory costs and benefits are "defensible, replicable and, in all likelihood, more accurate than the corresponding agency estimates."⁹ As we will see, his response offers no substantiation for that claim. In fact, it raises further doubts.

A. Sampling bias

Morrall's scorecard analyzes a tiny sample of rules drawn from a much larger population. His broad inferences about the population are valid only if his sample is broadly representative of that population.

As I noted in *Grading the Government*, both Morrall's and Hahn's samples are drawn from rules for which Regulatory Impact Analyses (RIAs) are required.

7. See John Morrall, Saving Lives: A Review of the Record, 27 J. Risk & Uncertainty 221, 221 (2003)[hereinafter Morrall 2003 Reply].

8. *Grading the Government* at 1359-1360, 1363-1364, 1367-1370, 1370-1375, 1387-1389 (cited in note 1).

9. Morrall 2003 Reply at 228 (cited in note 7).

Because RIAs are required only of the costliest rules, not the most beneficial ones, Morrall's and Hahn's sampling criteria harbor a built-in bias towards a finding of over-zealousness.

Hahn at least tries to include all the rules for which RIAs were prepared. Morrall's sample of rules – particularly those pertaining to toxin control – is neither comprehensive nor randomly chosen. In fact, fourteen of the sixteen EPA regulations on Morrall's list have to do with four pollutants – asbestos, benzene, arsenic or radionuclides – which generated some of the most heated and heavily litigated controversies in all of environmental law.¹⁰

Morrall never explains his selection criterion except to note that he included regulations for which “reasonably complete information” on cost and benefit was available.¹¹ Yet, as Heinzerling observed, Morrall's original table omitted clearly beneficial final rules for which cost-benefit information was readily available,¹² while including eight proposed rules that were never enacted.¹³

It is significant that Morrall does not attempt to demonstrate – either in his original article or his reply – that his choice of rules actually represents a fair sample of the regulatory universe.¹⁴ Instead, his reply to criticism focuses on *one rule* which Heinzerling offered as an example of the *kind* of rule which Morrall might have included had he wished to offer a balanced picture of regulation: EPA's highly beneficial rule phasing down lead in gasoline. The lead rule clearly meets his data-availability criterion, but Morrall claims he excluded it because it fails another, never-before-stated criterion: “that life saving benefits must provide the majority of benefits and that non-health benefits must not exceed compliance costs.”¹⁵

10. See, e.g., cases and controversies collected in *Grading the Government* at 1365 n. 71 (cited in note 1).

11. Morrall 1986 Study at 27(cited in note 2).

12. She cites EPA's phase-down of lead in gasoline and its regulations controlling common air pollutants as examples. *Mythic Proportions* at 2016 (cited in note 5).

13. *Id.*

14. As Heinzerling notes, the cost-ineffective “bottom” of the Morrall table is heavily stocked with regulations that he and others at OMB had heavily criticized. *Mythic Proportions* at 2015(cited in note 5).

15. Morrall 2003 Reply at 225 (cited in note 7). In response to Heinzerling's complaint that his table omitted regulations that had high benefits relative to costs, Morrall points out that OMB's annual report to Congress regularly reports “very high benefits relative to cost produced by EPA's Clean Air program.” *Id.* at 6. The relevance of this observation to a defense of his earlier table is unclear. Morrall also notes that Heinzerling “does not cite agency regulations or RIAs available in 1986 that would allow cost per life saved estimates.” *Id.* That is true, but surely coming up with a fair sample – and demonstrating its representativeness – is Morrall's job, not Heinzerling's.

Does Morrall's very choice of cost-effectiveness metric (cost-per-life-saved) – and the exclusion of rules not analyzed in cost-per-life-saved terms – *intrinsically* tend to skew the sample against regulation? A comparison of Morrall's and Hahn's lists is instructive. In 1985 EPA issued a final rule setting forth NOx emissions standards for light duty trucks and heavy duty engines. See *Grading the Government* at 1743, Annex C, row 64 (cited in note 1). EPA estimates the cost of the rule and the tons of NOx and diesel particulates eliminated thereby, and pronounces the rule cost-effective, end of story. 54 Fed. Reg. 10,606 (1985). EPA did

This response is hardly a defense and mostly raises further questions. If the goal is a balanced appraisal of regulatory efficiency overall, why categorically exclude rules that offer cost-savings in excess of compliance costs? Why not include these clear-cut success stories along with the alleged failures? In any case, why was this criterion of inclusion (or exclusion) never mentioned before? One is left wondering, what other selection criteria has Morrall applied without telling us? Rather than vindicate the fairness of his sample, Morrall's response merely underscores the fundamental lack of transparency of his methods, a problem that surfaces repeatedly in both his primary analysis and his reply to his critics.

B. Reducing agency estimates of life-saving benefits

Biased sampling is, unfortunately, only the beginning of the problems with Morrall's study. In *A Review of the Record*, Morrall reports that in constructing his cost-effectiveness table he altered agencies' estimates of regulatory benefits. In some cases he relied on "published studies that appear to reflect prevailing scientific views more accurately than the agency estimate" while in other cases – for example, where an agency presented a range of estimates – he chose the point estimate that seemed to him "most reliable."¹⁶

Morrall says he lowered agency benefit numbers because he believes that agencies "tend to overstate the effectiveness of their actions."¹⁷ In my critique, however, I pointed out that Morrall nowhere establishes that agencies do, indeed, tend to over-state the effectiveness of their actions. In fact, I showed that while agencies may, as a policy matter, adopt conservative assumptions that produce high-end estimates of cancer risks, they often lack data to quantify non-cancer or

not bother to estimate lives saved or illnesses averted by the rule, and Morrall therefore excludes the rule from his table. Hahn, however, using a value-per-ton cost-effectiveness figure developed by EPA, would later estimate the net benefits of this same rule in excess of \$20 billion.

The implication: a highly cost-effective rule was omitted from Morrall's table simply because the agency failed to calibrate its cost-effectiveness in the metric that Morrall imposed as a precondition for inclusion in this table. This may not matter much if efficient and inefficient rules were equally likely to be excluded from Morrall's table by his choice of a metric of cost-effectiveness. But it is also possible – if not probable – that the cost-per-life-saved calculation was one that tended to get imposed on agencies by OMB (or by agencies on themselves in anticipation of OMB review) for rules that were controversial or were expected to be controversial. If so, Morrall's sampling criterion would create a built-in bias towards cost-ineffectiveness. While a rigorous test of this hypothesis is beyond my present scope, what is not hypothetical is the frequently cavalier approach of EPA to enumerating benefits in medical/biological terms in the analysis of many of the rules in Hahn's database. See *Grading the Government* at 1387-1400 (cited in note 1).

16. Id.

17. Id. at 28-29. Significantly, Morrall heavily emphasizes his conviction that agencies over-state regulatory benefits while completely ignoring a considerable body of empirical evidence which suggests that agencies often over-state regulatory compliance costs. See, e.g., Morgenstern, Richard D. et al, *The Cost of Environmental Protection*, RFF Discussion Paper 98-36 (May 1998); McGarity, Thomas O. & Ruth Ruttenberg, *Counting the Cost of Health, Safety, and Environmental Regulation*, 80 *Tex. L. Rev.* 1997 (2002) (describing procedural and structural factors that account for tendency to over-state regulatory costs).

ecological risks in any reliable way, with the result that these risks (and the associated benefits of reducing them) often end up being *under-estimated*. In *Grading the Government*, I offer numerous examples of actual rules where this clearly occurred.¹⁸ In his reply, Morrall does not contest any of these examples or the general point they illustrate. Instead, he simply repeats his earlier claim.

Beyond disputing the basic premise of Morrall's adjustments to agency risk estimates, Heinzerling and I also noted that Morrall and his colleagues at OMB at that time are not toxicologists or safety experts, and therefore are not qualified to substitute their judgment on risk and benefit for that of agency scientists.

Morrall's reply to this point is rather startling. It is also wrong. And it is revealing of the simple-minded intellectual hubris that critics have long complained of in connection with OIRA staff.

Morrall claims that it is perfectly fine for OMB economists to substitute their risk and benefit numbers for agency numbers because "OMB's calculations [about the risks of formaldehyde exposure and the benefit of reducing it] do not depend upon toxicology; they depend upon statistical estimation procedures. OIRA had several Ph.D. statisticians on its staff who worked on the analyses while OSHA did not. . . ."¹⁹

What this observation overlooks is that statistics, epidemiology and toxicology are not separable in chemical risk estimates. The interdependence of biological science and statistical inference is well-illustrated by the "Bradford Hill" criteria, which are widely accepted among scientists as encompassing the relevant tests used to determine the strength of a causal link between an environmental toxin and a disease – in other words, the magnitude of a toxic risk.²⁰

Of the nine criteria articulated by Sir Bradford Hill, at least five – biological gradient, plausibility, coherence, experiment and analogy – require explicit biological judgments.²¹ The remaining four criteria involve the application of

18. *Grading the Government* at 1391-1400 (cited in note 1).

19. Morrall 2003 Reply at 228 (cited in note 7).

20. See Sir Austin Bradford Hill, *The Environment and Disease: Association or Causation?*, 58 *Proc. of the Royal Soc. of Med.* 295 (1965), cited in Daniel J. Schneck, *The Hill Criteria*, *American Laboratory* (April 2004), available online at <http://www.isepubs.com/articles/al/a0404sch.pdf> (last visited on Aug. 2, 2004). I am grateful to Lynn Goldman, Professor of Environmental Health at the Bloomberg School of Public Health at Johns Hopkins University, for pointing me to the Bradford Hill criteria.

21. The nine Bradford Hill criteria are: (1) statistical strength of association between exposure and disease; (2) consistency of association across studies; (3) correct temporal order (exposure must precede disease); (4) specificity of association between risk factor and consequence; (5) biological gradient in the form of an ascertainable dose-response curve; (6) experimental verification through controlled experiments; (7) biological plausibility; (8) coherence of statistical evidence with established understanding of natural history and biology of disease mechanisms; and (9) plausibility by analogy to more generic and already proven theory of biological activity. *Id.*

The first four of these criteria lie arguably within the primary provenance of the statistician. The last five lie clearly within the domain of biologists, epidemiologists, physicians and toxicologists. When different studies yield conflicting results and when, as often happens, it turns that these studies meet some but not all of the

biological or medical judgment in the construction of regression models used for statistical inference.²² Statistics is a tool of toxicology and epidemiology, which risk assessors use along with many other tools. The fact that OMB has Ph.D statisticians on its staff does not render OMB economists more expert in risk assessment than the entire scientific staff of EPA and OSHA. It is remarkable that Morrall would assert otherwise.²³

This is not to say that OMB's risk judgments are always wrong, and the agencies' always right. Morrall points to cases where agencies disagreed on the appropriate risk model and the OMB adopted one agency's model over another's.²⁴ The problem is that Morrall offers no persuasive reason – either in his 1986 analysis or his 2003 reply to critics – for the neutral reader to trust Morrall's numbers more than those supplied by the agencies. In fact, the more Morrall tries to explain his numbers, the more doubts he raises.

Consider, for example, OSHA's ethylene oxide rule, a case where Morrall claims he used EPA's risk model over OSHA's because the former "was based on epidemiological evidence rather than extrapolation from an animal experiment."²⁵ Yet, as any qualified risk assessor knows, studies of epidemiological data are not automatically better than animal studies. It all depends on the quality of the data and the quality of the respective studies.²⁶

Bradford Hill criteria, scientific and policy judgment is once again required in order to determine whether and how strong the attribution of causality should be.

22. Indeed, any trained statistician knows that a mis-specified regression model – e.g. one that omits relevant variables, includes irrelevant ones, or mis-specifies their inter-relationship – can cause a risk to be either under- or over-estimated. In a very real sense, a statistic is only as good as the model used to estimate it, and the latter requires, in the field of toxicology, biological and medical judgment. For a good discussion of these issues in the context of econometric analysis, see William E. Griffiths et al, *Learning and Practicing Econometrics* 341-348 (1993); and William H. Greene, *Econometric Analysis* 402-404 (3d. ed. 1997).

23. In fact, his own boss, John Graham, appears to disagree with him. Graham has made much of the fact that he is bringing into OMB's Office of Information and Regulatory Affairs, for the first time, scientists and engineers with training in the specific disciplines involved in agency regulation. John D. Graham, *Presidential Oversight of the Regulatory State: Can it Work?*, Speech at Heinz School, Carnegie Mellon University, October 4, 2002 (available online at http://www.whitehouse.gov/omb/inforeg/graham_cmu_100402.html) (last visited Aug. 19, 2004).

24. Morrall 2003 Reply at 235, n. 24 (cited in note 7).

25. Morrall 1986 study at 29 (cited in note 2).

26. Cohrssen, John J. and Vincent T. Covello, *Risk Analysis: A Guide to Principles and Methods for Analyzing Health and Environmental Risks* 29 (1989) observes that "epidemiology is relatively well-suited to situations where exposure to the risk agent is high (such as cigarette smoking), where adverse health effects are unusual (such as rare forms of cancer), where the symptoms of exposure to the risk agent are known, where the link between the causal risk agent and adverse effects in the affected population is direct and clear, where the risk agent is present in the bodies of the affected population, and where high levels of the risk agent are present in the environment (e.g. in the soil or drinking water). However many of the environmental health risk agents currently subject to regulatory and societal concern do not fall into these categories. As a result, epidemiological studies used in risk analysis have important limitations that constrain their usefulness." Note that these factors

Morrall claims that he relied on the work of “well-respected risk assessors” such as Crump and Allen, two scientists who frequently consult for industry and who furnished a risk estimate for occupational exposure to benzene that came in at 30 percent of the OSHA number.²⁷ Morrall notes that OSHA subsequently reduced its estimate for risk of leukemia from benzene exposure to a number more in line with Morrall’s estimate.²⁸ Was this because OSHA became convinced of the error of its ways, or because OMB twisted arms?

Morrall claims that in assessing OSHA’s formaldehyde regulation, OMB used a “weight of the evidence” approach to include data from animal studies which included mice and hamsters as well as rats. Because the mice and hamsters seem to respond much more weakly to formaldehyde exposure than rats, the effect of including mice and hamster experiments was to significantly reduce the estimate of risk to humans.²⁹ Morrall justifies including the mice and hamster studies on grounds that “there is no evidence that rats are better predictors of human risks than mice or hamsters.”³⁰ This may be so, but it does not follow from this that mice or hamsters are necessarily better predictors than rats.

A standard practice in risk assessment is to base human risk estimates on the experience of the most responsive test animal, unless there is some reason to believe that a particular species is more indicative of human dose-response. Relying on the most sensitive species in cases of uncertainty is one of the safety factors that toxicologists routinely build into their risk assessments in the same way that engineers build safety factors into the design of bridges.³¹ Morrall calls his inclusion of mice and hamster studies in the risk estimate “innovative meta-analysis.”³² He is right. His analysis *is* innovative: it jettisons a standard source of precaution in risk assessment.

Morrall notes that he “often deflated agency assumptions concerning accident reduction from 100 percent effectiveness to a more reasonable figure of 50

are inherent limitations on the utility of epidemiological studies, over and above the limitations introduced by simple human error or oversight in the design and implementation of a particular study.

27. Morrall 2003 Reply at 228 (cited in note 7).

28. *Id.* at 12.

29. *Id.* at 10.

30. *Id.*

31. Stelljes, Mark E., *Toxicology for Non-Toxicologists* 92-93 (2000)(pointing out that that “we often cannot predict with any confidence whether humans will be more or less sensitive to a chemical than the animal species on which information is available”; that “uncertainty factors” are therefore used to build in an element of precaution to account for this uncertainty; and that one of those uncertainty factors involves using the results of the most sensitive animal tested as the basis for extrapolation to humans, unless there is good reason to believe that a particular species is the best indicator of human response).

32. Morrall 2003 Reply at 228 (cited in note 7).

percent.”³³ Assuming 100 percent effectiveness does indeed seem optimistic. But where does Morrall’s 50 percent substitute number come from? Why not 75 percent? Or 90 percent? Or 30 percent? Why assume a constant number across the board? Doesn’t the most likely rate of effectiveness depend in large measure on the rule involved? This practice of snatching numbers out of thin air and applying them as universal rules of thumb does little to bolster our faith in the reliability of his overall analysis.

In his 2003 reply, Morrall claims that some of his downward revisions of agency risk estimates have been vindicated in general terms by subsequent research or agency practice. For example, he claims that Health Canada recently adopted a risk assessment for formaldehyde exposure that is lower even than OMB’s 1986 risk estimate;³⁴ and that OSHA has since abandoned the methodology used in its 1976 coke oven rule, which represented the agency’s first foray into risk assessment.³⁵

This may be so. Agency risk estimates are fallible. New data and subsequent research will often cause agencies to revise their initial risk estimates either upward or downward. Even if agencies’ risk estimates are unbiased (i.e. as likely to go up with further research as to go down), one would expect a downward second-guesser of agency predictions to be vindicated roughly 50 percent of the time. This means that there will always be anecdotal evidence to support claims that agencies “generally” over-estimate *ex ante* risk and regulatory benefit, even if they do not. The formaldehyde rule does not prove that Morrall’s risk estimates are generally more reliable than those supplied by agencies.

C. Discounting benefits at an excessive rate over arbitrary, undisclosed and probably excessive time periods

Morrall and Hahn both follow the practice of many economists in discounting the future costs and benefits of regulation at a constant exponential rate. Hahn’s analyses explore the impact of setting the discount rate at three, five, and seven percent, respectively.³⁶ Morrall simply adopted a ten percent rate in his original 1986 study, which he then reduced to seven percent in the calculations used to produce the updated table that accompanies his reply to his critics.³⁷

The impact of these adjustments on calculated benefits was considerable. Discounting a constant stream of benefits over 25 years will reduce its present value by 30 percent at a three percent discount rate, or 50 percent at a seven percent rate.

33. Id.

34. Id. at 10.

35. Id. at 11.

36. Government’s Numbers (1996) and (2000) (cited in note 3).

37. Morrall 2003 Reply at 229, 230-2231, tbl 2 (cited in note 7).

If one further assumes, as both Morrall and Hahn do, that the stream of cancer risk reduction benefits (which dominate many of the health and environmental rule benefit numbers) only begins to accrue after a latency period of some 15-35 years, the impact of discounting can become truly enormous. Discounting a constant dollar annual benefit accruing over 25 years – beginning 35 years out – will effectively shrink benefits by a *factor of four* at a three percent discount rate, and a *factor of twenty* at Morrall’s and Hahn’s seven percent rate. Multiply benefits by a factor of twenty and *all* of the final rules in Morrall’s original table cost less than his reasonableness threshold of \$7 million per life.³⁸

Scholars disagree vigorously over the choice of discount rate, and even more fundamentally over the propriety of discounting at all.³⁹ While a full airing of this issue is beyond our present scope, suffice it to say here that there is no consensus among analysts (even among those that approve of discounting in principle) that the discount rate for regulatory benefits should be higher than three percent.⁴⁰

Just as important as the choice of discount rate – though much less often discussed – is the assumption adopted by various analysts as to the “latency period” over which benefits (especially cancer risk reduction benefits) are to be discounted. Simple arithmetic reveals that for benefits assumed to accrue after a latency period, the assumption about the length of that period exerts just as significant an impact on the benefits calculation as the choice of a discount rate.⁴¹ Moreover, the impact of one’s choice of discount rate is obviously much higher if a longer latency period is assumed than if a short one is chosen. For example, reducing the discount rate from seven to three percent will increase the present value of benefits by 75 percent if a five-year latency period is chosen. Stretch the latency period to thirty-five years and the same choice of discount rate makes a 450 percent difference.

Choice of latency period is clearly of fundamental importance to the calculation of regulatory benefits. Unfortunately, it turns out that the selection of a medically appropriate time lag for the onset of cancer risk reduction benefits is anything but

38. *Id.* at 22, tbl. 1. It appears that Morrall assumed a 35-year latency period for some but not all cancer-benefits addressed by regulations. *Id.* at 7, n. 16 and 9, n. 22 (noting that he chose a 20-year latency period for one rule, a 15-year latency period for another, and a 35-year period for a third). Unfortunately, he did not even mention his latency period assumptions in his original article. Nor does he offer any systematic disclosure of them in his 2003 reply. In view of these omissions, it is rather unfair for him to chastise his critics for making educated, but incorrect, guesses as to assumptions he should have disclosed, but did not. *Id.* at n. 16, 22.

39. For a good discussion of the propriety of discounting, see *Mythic Proportions 2043-2056* (cited in note 5); John J. Donohue III, *Correspondence: Why We Should Discount the Views of Those Who Discount Discounting*, 108 *Yale L. J.* 1901 (1999); Richard L. Revesz, *Environmental Regulation, Cost-Benefit Analysis, and the Discounting of Human Lives*, 99 *Colum. L. Rev.* 941, 950-962 (1999). For a presentation and discussion of the wide variety of discount rates furnished by economists, see Weitzman, Murray, *Gamma Discounting*, 91 *Am. Econ Rev.* 260 (Mar. 2001).

40. See Revesz at 980-981 and Weitzman (cited in note 37).

41. Simple application of the discounting formula reveals, for example, that for a single, lump-sum benefit accruing after a time lag, doubling the lag period from 10 to 20 years has almost exactly the same effect on calculated net present value as doubling the discount rate from 5 percent to 10 percent.

straightforward. An accumulating body of evidence suggests that, at least for rules which address prior exposures to certain “late stage” carcinogens, the proper time period for discounting is not the full latency period of the cancer after exposure *ab initio*, but the typically smaller time lag between (a) reduced exposure to a carcinogen and (b) the reduced probability of cancer.⁴² In other words, stopping or reducing a pre-rule exposure to a carcinogen can yield health benefits that accrue in less time than it takes for the cancer to develop from the very first exposure. EPA’s Science Advisory Board has observed that the “cessation lag” for exposure to some late-stage carcinogens can be as short as five years.⁴³

Clearly, the choice of latency period/cessation lag is an important variable that merits much more attention than it has received from scholars and analysts to date. *If* discounting is to be practiced, latency period assumptions need to be disclosed and defended on medical grounds, in agency analyses as well as secondary scorecards. Unfortunately, Hahn and Morrall join agencies and a host of scholars who have generally failed to appreciate the fundamental importance of the time lag variable, using undisclosed, undefended, arbitrary (and probably excessive) latency period assumptions when a medically-based estimate of the cessation lag would be more appropriate. The time has come to lay this error to rest.

Meanwhile the consequence of adopting arbitrary and (probably) excessive discount rates and latency periods is yet another downward bias to benefits estimates, albeit one of variable and uncertain size.

D. Ignoring unquantified and non-life-saving benefits

One of more egregious of Morrall’s errors is that he ignores all benefits that agencies failed to quantify, as well as quantified (but not monetized) benefits other than reduction in fatalities, hospitalizations or permanent disabilities. In so doing, he violates established canons of cost-benefit analysis.⁴⁴

The impact of this error is well illustrated in his treatment of OSHA’s formaldehyde rule – the most cost-ineffective rule on his list. At \$72 billion per life saved, OSHA’s formaldehyde rule does indeed appear unreasonably expensive. What Morrall’s table fails to disclose is that OSHA’s rule – beyond preventing about

42. EPA. Arsenic Rule Benefits Analysis: An SAB Review, EPA-SAB-EC-01-008 at 4-7(August 2001). Available online at www.epa.gov/sab/pdf/ec01008.pdf (accessed 3/5/2004).

43. *Id.*

44. See Morrall 2003 Reply at 234, n13 (cited in note 7) and Morrall 1986 study at 28 (cited in note 2)(noting that *monetized* non-health benefits were subtracted from costs, while non-fatality health benefits involving avoided hospitalizations or disabilities were monetized using “willingness-to-pay” estimates). Compare Kenneth J. Arrow, Maureen L. Cropper, George C. Eads, Robert W. Hahn, Lester B. Lave, Roger G. Noll, Paul R. Portney, Milton Russell, Richard Schmalensee, V. Kerry Smith, and Robert N. Stavins, Benefit-Cost Analysis in Environmental, Health and Safety Regulation: A Statement of Principles at 10 (1996)[hereinafter *Annapolis Principles*] (“not all impacts of a decision can be quantified or expressed in dollar terms. Care should be taken to assure that quantitative factors do not dominate important qualitative factors in decision-making”).

one cancer fatality per year (which, in Morrall's hands, becomes one-hundredth of a fatality per year, after adjustment and discounting) – was also expected to yield a host of unquantified but clearly substantial benefits.

These benefits are delineated at length in the preamble to OSHA's proposed rule: reduced or avoided burning eyes or noses, sore or burning throats, asthma attacks, chronic bronchitis, allergic reactions, dermatitis and skin sensitization. OSHA notes that over 500,000 American workers are regularly exposed to formaldehyde at concentrations that have been found to cause one or more of these illnesses or discomforts.⁴⁵

Is avoiding such discomforts and health hazards for 500,000 American workers "worth" the expenditure of \$36 million a year by a \$30 billion dollar group of industries? Will installing ventilators in the workplace also reduce employee exposure to other irritating and possibly hazardous chemical vapors?

These are the central questions of the formaldehyde rulemaking. They are quite unlike (and are far more complex than) the question implicitly posed by the Morrall table: how could OSHA be so stupid as to propose a rule that will cost \$72 billion for every life saved?⁴⁶

Although a full delineation of the regulatory benefits excluded from Morrall's table is beyond my present scope, there is no reason to assume that the formaldehyde rule is atypical. Most toxins that cause cancer may be expected to cause a rash of other health problems, avoidance of which (through use of simple devices such as ventilators) might well justify a moderately priced rule. If you focus only on quantified benefits to the exclusion of all benefits the agency narratively describes, you may miss the main point of the rule.

In Morrall's case, this selective focus on quantified lives saved (or life-equivalents saved) sets up a mathematical sleight of hand. Nine of the forty-four rules that appear in his original table are listed as saving less than *one-tenth* of a statistical life annually.⁴⁷ For these rules the calculated cost-per-life-saved is more than ten times higher than the actual cost of the rule. Such rules may be *bona fide* examples of regulatory folly. Or they may simply be rules aimed primarily at capturing benefits which Morrall does not count, and which are made to *look* absurdly expensive by the magic of dividing by zero – or near zero.

We have seen that Morrall and his fellow OMB economists are not professionally qualified to serve as as final arbiters of the "weight of the evidence"

45. Department of Labor, Occupational Exposure to Formaldehyde, 50 Fed Reg 50412 (1985).

46. In his 2003 reply, Morrall takes me to task for erroneously claiming, in my preliminary draft, that this rule was proposed and never finalized. Morrall 2003 Reply at 235 n. 23 (cited in note 7). Had he bothered to read my published article, he would have discovered that this mistake, inconsequential in any case, was found and corrected prior to publication. Grading the Government at 1387 (cited in note 1).

47. Morrall 1986 study at 30, tbl 4 (cited in note 2).

(including toxicological evidence) which undergirds various risk estimates. We have examined the overt bias revealed by Morrall's stated (and unsubstantiated) suspicion that agencies routinely over-state the effectiveness of their actions and by his acknowledgment that he adjusted agency benefit estimates only downward, never upward.

Most of all, we should be skeptical of Morrall's substitute numbers because they are largely unexplained and undocumented. His 1986 analysis does not report the agency estimates he adjusted, does not indicate the studies relied on for his own numbers, and does not even attempt to demonstrate the superiority of his own numbers over those of the agencies. In fact, his original analysis did not even clearly identify the rules he was examining.⁴⁸ We have seen that his belated effort in 2003 to rationalize his numbers for a handful of disputed rules is so partial, lame and error-prone that its main effect is merely to reinforce doubts.⁴⁹

Under these circumstances, do Morrall's numbers offer more reliable and complete measures of regulatory rationality than the quantitative estimates *and qualitative predictions and judgments* put forward by the regulatory agencies themselves? Given what we have seen, the only sensible answer to this question is no, and nothing in Morrall's response to his critics would warrant changing that answer.

48. This omission has led to an understandable confusion on the part of outside critics attempting to replicate his findings. For example, in his 2003 reply, Morrall takes me to task for claiming that he altered agency *cost* estimates. I based this charge in part on a comparison between the \$97 million cost estimate I discovered in EPA's 1986 rule on land disposal of hazardous waste, and the \$1.4 billion cost figure implied in Morrall's 1986 table (calculated by dividing the listed \$3.5 billion cost-per-life saved by the 2.52 figure for annual lives saved). In fact, as Morrall's 2003 reply points out, there were *two* land-disposal rules enacted by EPA in 1986. One contains the \$97 million cost figure; another the \$1.4 billion estimate. Morrall 2003 Reply at 227 (cited in note 7). It appears that Morrall is right that he did not alter EPA's cost estimate for its 1986 land disposal rules and I hereby offer that correction. But I did not, as Morrall claims, "substitute" a different rule in order to make him look bad. I was simply misled by an incomplete and ambiguous reference to "Land Disposal - 1986" appearing in his table – a confusion which would not have occurred had Morrall documented his rules properly in the first place. Morrall 2003 Reply at 223-224 (cited in note 7) offers a convenient re-printing of that notoriously incomplete and ambiguous list of rules.

49. Morrall insists that his findings are "replicable" and that the documentation supporting those findings is "on file." But he has declined to tell me where those files are, and repeated requests to see this documentation have been either ignored or rejected. Morrall seems to be of the view that "replicability" requires nothing more than a declaration by the original researcher that he has gone over the calculations and come up with the same result. See Morrall 2003 Reply at 228-229 (cited in note 7) ("I have examined each of the cost-effectiveness estimates questioned by Heinzerling and Parker, and reviewed sources and reasons for the estimates that were published.") To be "replicable" in the sense that scientists and other scholars use that word, the findings must be derivable by *outside* scholars drawing on the assumptions, sources, data and methods of the original study. When those sources and data are withheld, replicability is impossible and the only fair conclusion is that the findings are *not* replicable.

II. Hahn

Hahn apparently shares my unease with Morrall's tendency to alter agency benefit estimates, because Hahn goes to great lengths to insist (in his title and throughout his analysis) that his scorecard does not alter agency estimates: his scorecard uses "the government's numbers."⁵⁰

In *Government's Numbers (2000)*, Hahn re-affirms the conclusion of *Government's Numbers (1996)*, that less than half of the major regulations promulgated since 1981 fail a "neutral economist's" cost-benefit test, and that not promulgating these regulations would have saved regulated entities and/or consumers hundreds of billions of dollars.⁵¹ For EPA, which accounts for two-thirds of the rules in the database, the numbers are even bleaker. Less than a third of the major rules issued by EPA over the period 1981-1996 pass Hahn's cost-benefit test.⁵²

From these statistics, Hahn concludes that "agencies do not seriously consider the relationship between costs and benefits when making regulatory decisions."⁵³ He recommends that Congress "limit the scope of federal regulation to activities that agencies can justify on economic grounds" (thereby mandating that cost-benefit analysis trump all other modes of regulatory decision-making) and "consider establishing a congressional or independent agency responsible for replicating key findings used to support regulation before agencies finalize the regulations."⁵⁴

These are sweeping reforms. How much confidence can we repose in the findings that call for such draconian remedies?

Unfortunately, as I pointed out in my initial critique, Hahn *does* alter the government's numbers, principally by supplying zeroes in the benefits ledger— zeroes which violate recognized principles of responsible cost-benefit analysis and which have no basis in any government study.⁵⁵ I also pointed out that his regulatory sample is biased against regulation; that he confuses ex ante predictions with actual costs and benefits; that he suppresses and conceals huge uncertainties; and that he adopts a rigidly and narrowly utilitarian focus which ignores all ethical concerns with justice or fairness.⁵⁶

Hahn's reply consists of an essay entitled *Reviewing the Government's*

50. *Government's Numbers (1996)* *passim* (cited in note 3). See also *Id.* at 211 ("Unlike Morrall, however, this study attempts to avoid introducing adjustments to individual [agency] studies . . .").

51. *Government's Numbers (2000)* at 138 (cited in note 2).

52. *Id.*

53. *Id.* at 60.

54. *Id.* at 64.

55. *Grading the Government* at 1382-1405 (cited in note 1).

56. *Id.*

Numbers, which he has disseminated through the Social Science Research Network and posted

on the website of the AEI-Brookings Joint Center for Regulatory Studies.⁵⁷ Remarkably, Hahn's reply to his critics does not take issue with the logic or validity of *any* of the criticisms raised against his study. He simply avows that two of the criticisms – his exclusion of major categories of benefit and his choice of life value and discount rate – do not matter much to the end result.⁵⁸

Hahn bases this argument on two sensitivity analyses. First, he examines the impact of allowing life values and discount rate to vary from \$1-9 million and 1-9 percent, respectively, instead of his original range was \$3-7 million and 3-7 percent. Then he looks at the impact of adding “non-standard benefits” and excluding “zero-benefit rules” from the data base (cryptic terms that he fails to explain). From this analyses, he concludes that the overall findings of the earlier studies “appear to be robust” – meaning presumably that their indictment of regulation stands.⁵⁹

The discussion that follows will show that while Hahn's conclusions may (or may not) be robust against variations in life values and discount rates, the other errors that plague his studies go far beyond these assumptions and are far from harmless. In fact, they leave his main conclusions unsupported by his own analysis.

A. *The problem (again) of replicability.*

Hahn's original studies do not so much as *list* the rules in his database.⁶⁰ Moreover, although the titles of Hahn's studies might lead one to assume that he is simply reporting the government's numbers, he explicitly acknowledges in the text

57. Robert W. Hahn, Rohit Malik, and Patrick M. Dudley, Reviewing the Government's Numbers on Regulation, Related Publication 04-03 (Jan. 2004) available online at <http://www.aei-brookings.org/admin/authorpdfs/page.php?id=321> (last visited Aug. 1, 2004) [hereinafter Hahn 2004 reply]. Hahn's “reply” is hard to characterize. Although Hahn acknowledges that *Government's Numbers* was “one of the most controversial pieces of economic analysis published by the Joint Center”, Id., nowhere does he mention any critic of the original study by name. Nor does he provide any detail on the substance of the criticisms which have created the controversy to which he alludes. Instead, he merely offers a “sensitivity analysis” which examines the impact of (a) varying the implicit value of life and discount rate, and (b) disregarding “zero benefit” rules, an analysis which is incomprehensible unless one is aware that he has been criticized for undervaluing life saving benefits and zeroing the benefits of entire rules. Whatever the underlying intent, the effect of this approach is to set forth Hahn's response for the benefit of those who have encountered critiques of Hahn's work on their own, without enabling anyone else actually to find and read a full critique of his work.

58. Id.

59. Id. at 16.

60. Hahn's reply to his critics lists the rules in his database, along with their Regulatory Identification Numbers and associated Federal Register notices. See Hahn 2004 Reply at 17-22, App. 1 (cited in note 57). This is very helpful. But he still has not published his tabulation of regulatory costs and benefits. That may be found in Annex C of *Grading the Government* (cited in note 1).

of these studies that he adjusted agency numbers in significant ways: discounting benefits at a standard rate over a standard latency period, taking the mean of benefits ranges where agencies offer a range, applying a standard value to lives saved and illnesses avoided by regulation, etc.

His published studies offer no documentation of the calculations that yielded his extraordinary findings, yet for almost eight years his claim was accepted and widely reported by journalists and scholars around the country, without any sustained inquiry into his underlying data, assumptions and methods.

In 2001 I asked Hahn to supply the raw data and calculations supporting his conclusions so that I might try to replicate his findings. I did not anticipate resistance to such a request, inasmuch as requests for data to replicate findings are a routine feature of scholarly life and Hahn himself had previously written in the influential scientific journal, *Science*, that:

“Government should be allowed to use those research findings in developing regulations only after the agency has replicated the results or has certified that the results have been independently replicated. Replication is a key to ensuring the quality of results.”⁶¹

As it turned out, my effort to enlist Hahn’s cooperation with my attempt to replicate *his* results was met with considerable resistance and a delay of almost six months.

When I finally did receive his spreadsheet tabulation of the calculated costs and benefits for each rule, I discovered that his Excel spreadsheet does not record the calculations supporting his numbers; it simply reports the results of those calculations in various cells. One is left guessing as to the thought process and arithmetic that produced the numbers appearing in those cells, making it difficult if not impossible for outsiders (at least this outsider) to replicate Hahn’s numbers by applying the assumptions and adjustments he describes to the numbers that appear in agency regulatory impact assessments.

B. The problem of zeroed-out benefits

Though his calculations are opaque, perusing Hahn’s unpublished spreadsheet of regulatory costs and benefits quickly yields a remarkable discovery.⁶² Thirty-two of the 106 final rules in his database (nearly a third of all rules) are assigned a zero benefit. These rules, it should be emphasized, are not rules for which it is claimed that costs equal benefits, yielding a zero net benefit. These are rules alleged to offer *no benefit whatsoever*.

The list of zero-benefit rules includes:

61. Cohen, Linda R. & Robert W. Hahn, A Solution to Concerns Over Public Access to Scientific Data; suggestions to improve process under Freedom of Information Act, 285 *Science* 535 (1999)(reporting conclusions of *Government’s Numbers* (1996)).

62. Reprinted with permission in *Grading the Government* at 1463-1484, App. C (cited in note 1).

- a rule requiring that owners and operators of oil tankers develop plans to respond to large oil spills;
- a rule requiring double hull construction for oil tankers like the Exxon Valdez;
- a rule to implement 1990 Clean Air Act Amendments which require that certain sources of air pollutants hold permits and comply with permit conditions;
- a rule requiring the public reporting of releases of certain toxic chemicals from manufacturing facilities;
- a Clean Water Act rule aimed at protecting sensitive coastal areas from non-point-source water pollution;
- a Clean Water Act rule establishing technology-based water pollution discharge standards for electroplating and metal finishing point sources;
- a rule to protect agricultural workers from exposure to harmful pesticides;
- a rule establishing financial responsibility requirements for owners and operators of underground storage tanks;
- three rules establishing national primary drinking water standards to limit public exposure to toxic pollutants in drinking water;
- a regulation banning the manufacture and sale of products containing PCBs, a highly toxic and bioaccumulative substance;
- an HHS rule requiring improvements in clinical laboratory practices;
- an FDA rule establishing requirements for the safe handling of seafood in commercial processing operations;
- a proposed rule to prevent or reduce oil spills from non-transportation-related on-shore oil handling facilities;⁶³

It turns out that Hahn, with a few narrow and limited exceptions, assigned a zero value to any benefit which the government's regulatory impact assessment did

63. Grading the Government at 1382-1383 (cited in note 1).

not quantify and monetize.⁶⁴ With few exceptions, he also zero-valued benefits which had been quantified and monetized in an agency RIA but which failed to fit within his Procrustean categories of recognized benefit – reduction of cancer, heart disease, lead poisoning and accidents, and benefits of reducing a handful of air pollutants – even as he insisted that he was using the government’s numbers.⁶⁵

In *Grading the Government* I offered a taxonomy of the kinds of rules for which Hahn zeroes out benefits simply because they do not conform to his categories. The list includes:

- (a) procedural benefits (such as permitting requirements, disclosure rules, and financial responsibility requirements for potentially major polluters);
- (b) environmental benefits (such as preventing oil spills and protecting coastal waters and wetlands);
- (c) health benefits of preventing acute illness and/or death from causes such as poisoning (e.g. protecting 3.9 million agricultural workers from acute pesticide poisoning or protecting the safety of seafood);
- (d) benefits of reducing chronic, non-cancer illnesses such as neurological impairments, reproductive disorders, etc.
- (e) benefits of statutorily mandated rules for which the agency did not attempt to quantify benefits; and
- (e) benefits derived from inter-locking rules (that is, rules that operate jointly with another rule, thereby preventing a clear attribution of benefits to any one rule).⁶⁶

It is significant that Hahn, in *Reviewing the Government’s Numbers*, does not attempt to defend these exclusions on principled grounds. It is hard to see how he could do so, given that he is co-author and signatory of the well-known “Annapolis principles” for benefit-cost analysis, which explicitly hold:

64. All unquantified benefits are assigned a zero value. Hahn monetizes the value of benefits which the agency has quantified but *not* monetized in the case of benefits involving (1) avoidance of cancer, heart disease or lead poisoning, (2) avoiding of accidental death or injury; and (3) pollution from any of four named air pollutants.

65. The exceptions are health benefits of reducing emissions of a small group of air pollutants -- carbon monoxide, hydrocarbons, nitrogen oxides, particulate matter and sulfur dioxide – which Hahn monetizes regardless of whether the agency does so, on a per-ton-of-reduction basis. Even these benefits are counted only if the agency has quantified the lives saved, illnesses or injuries averted, or tons of pollutant removed. All other benefits are zero-valued. *Government’s Numbers* (2000) at 40 (cited in note 3).

66. *Id.* at 1384-1404.

“not all impacts of a decision can be quantified or expressed in dollar terms. Care should be taken to assure that quantitative factors do not dominate important qualitative factors in decision-making.”⁶⁷

Hahn’s defense, it would appear, is not that his methods are sound, but that his errors in this case serendipitously turned out to be harmless. Table 6 of *Reviewing the Government’s Numbers* reports that adding “non-standard benefits” and excluding “regulations with zero benefits” from the database only increases the pass rate from 43% to 50- 63%, depending upon the choice of life value and discount rate, leaving his main conclusions intact.

There are at least four things wrong with this response:

First, increasing the regulatory pass rate from 43% to 63% is not a trivial impact. It would seem that even the two errors that Hahn implicitly acknowledges exert a significant impact on results.

Second, Hahn’s terminology is misleading. The term “non-standard” seems calculated to make the benefits subsumed under this label appear exotic or peripheral. In fact, there is nothing “non-standard” about the benefit of preventing catastrophic oil spills, or avoiding acute pesticide poisoning, or preventing neurological or reproductive disorders. These are perfectly standard benefits in everyone’s nomenclature but Hahn’s. There is no basis for assigning such benefits an invented zero value, and certainly no basis for attributing the invented zero to the government.

This leads us to Hahn’s second misnomer, “regulations with zero benefits.” Hahn’s “regulations with zero benefits” are not, in fact, regulations with zero benefits. They are simply regulations for which Hahn has inscribed a zero in the benefit column of his spreadsheet, notwithstanding the fact that the agency in question may have described substantial benefits narratively and may even have quantified and monetized them in some cases.

Third, one cannot, as Hahn tries to do, correct an erroneous zeroing out of potentially substantial regulatory benefits by simply excluding the regulations thus mishandled from the database. One has to acknowledge the possibility that some or all of the regulations that were wrongly scored may have been success stories. Based on Hahn’s own calculations in *Reviewing the Government’s Numbers*, counting these zeroed-out regulations as successes (combined with adding back the agency-monetized benefits that Hahn erroneously zeroed out) would raise the overall pass rate to 74-75 percent.⁶⁸

67. Kenneth J. Arrow, Maureen L. Cropper, George C. Eads, Robert W. Hahn, Lester B. Lave, Roger G. Noll, Paul R. Portney, Milton Russell, Richard Schmalensee, V. Kerry Smith, and Robert N. Stavins, *Benefit-Cost Analysis in Environmental, Health and Safety Regulation: A Statement of Principles* at 13 (1996)[hereinafter *Annapolis Principles*] at 10.

68. The 75 percent figure is derived as follows: According to Table 8 of Hahn’s 2004 Reply, 61% of the 76 regulations left after excluding regulations for which Hahn zeroed out benefits – 50 regulations total – pass strictly numerical cost-benefit tests if we assume a life value of \$5 million and a discount rate of 5% (the mid-

Fourth, and this is by far the most important point, the impact of Hahn's zeroing out of benefits is not confined to the 32 final rules that were erroneously assigned a zero value in his original study. As I indicated in my former critique and illustrate in more detail below, Hahn also excludes large categories of benefit from his valuation of rules that display a *positive* number in his benefit column. For example:

EPA's Great Lakes Water Quality Guidance was issued to reduce – by 6-8 million pounds a year – the discharge of persistent, toxic and bio-accumulative pollutants such as mercury, cadmium, lead, PCBs, DDT, dioxin, chlordane, heptachlor, dieldrin, pentachlorobenzene, and mirex into the Great Lakes.⁶⁹ These are compounds of undeniable toxicity whose risks, as EPA pointed out, include neurotoxicity, fetotoxicity, endocrine disruption, hematological impairment, reproductive dysfunction, sensory and equilibrium disturbances, hyperactivity, aggressiveness, impairment of peripheral vision, impairments of hearing and speech. These compounds pose special risks to fetuses and infants because they bio-accumulate in the mother's fatty tissues and pass through to the fetus and the breast-fed infant. They also threaten significant ecological harm to the Great Lakes system and the wildlife inhabiting it: wildlife obviously do not have the options that humans have for choosing the food they eat and treating and filtering the water they drink.⁷⁰

Despite their evident hazard, the non-cancer risks of bio-accumulative toxins are very hard to quantify on a mass scale, even for humans. Unlike cancer, which is widely assumed to have a linear dose-response down to a zero exposure level (making the calculation of population risk from aggregate exposure data relatively simple), non-cancer endpoints generally have non-linear risk thresholds – which means that, to calculate a population risk from any given discharge, you have to know not only the exposure of the population to the pollutants issuing from the sources targeted by the particular regulation. You also have to know the cumulative

points of Hahn's ranges for these parameters). There are 32 regulations that are excluded from Table 8 on grounds that their benefits were erroneously zeroed out in the original study. Counting these 32 regulations as successful and adding them to the 50 numerical successes yields a total of 82 successful regulations, which is 74 percent of the 106 regulations in Hahn's original database. Even if one takes the *lowest* VSL and the highest discount rate in use today, the pass rate would drop to only 70 percent.

Of course, there is also the possibility that at least some of the thrown out rules are, in fact, failures. The question, however, is what has Hahn proved? Hahn has come forward with a positive assertion and thus bears the burden of proof. If Hahn believes that one or more of the zeroed-out regulations are actually failures even when correctly analyzed, it is incumbent on him to indicate which rules those are and to explain why he deems them failures from a cost-benefit perspective. If he deems them successes, he likewise should explain why. Either way, simply throwing out rules which he mis-characterized previously is not a rational way to correct the mistake.

69. Final Water Quality Guidance for the Great Lakes System, 60 Fed. Reg. 15,336 (1995).

70. Proposal to Amend the Final Water Quality Guidance for the Great Lakes System to Prohibit Mixing Zones for Bioaccumulative Chemicals of Concern, 64 Fed. Reg. 53,632, 53,638-53,639 (1999).

exposure of individuals in the population to these and other interacting pollutants from other sources.⁷¹

Data at this level of detail is not available for most compounds and most people most of the time. As a result, non-cancer risks are exceedingly difficult, if not impossible, to quantify at the population level in most cases.⁷² Moreover, the persistent and bio-accumulative character of toxins addressed in many of these rules means that past experience – even if it could be accurately characterized – may not be a valid guide to future risk.

In this case, EPA concluded that available data permitted quantification only of the benefits of reducing incidence of fatal cancer to *sports anglers* and *Native American subsistence fishermen* who eat fish they themselves have caught in the Great Lakes. That number, after extensive manipulation, became Hahn's number for the *total* benefit of the rule.

To believe that Hahn's numbers tell the whole story of EPA's Great Water Quality guidance you have to believe that the *sole benefit* of reducing toxic, bio-accumulative discharges into the Great Lakes by 6-8 million pounds a year is a single cancer prevented every three years.⁷³ That is not the case, and it is not the picture that emerges from a fair reading of EPA's explanation of the basis and purpose of the rule.

EPA's 1995 municipal waste combustor rule required the installation of filters to reduce particulate matter and sulfur dioxide emissions from waste combustors, a benefit which Hahn monetizes. This benefit, however, misses the primary purpose of the rule, which was to reduce, by 70-99 percent, combustor emissions of the toxic and bio-accumulative pollutants – dioxin, cadmium, mercury, and lead – which issued in substantial quantities from combustor smokestacks and which were not yet subject to *any* federal controls. These compounds are known to be hazardous to human health in very small doses. The rulemaking record makes clear that EPA considered reducing emissions of these substances to be a major benefit of the rule. Yet the entire benefit of reducing these toxic and bio-accumulative discharges is zeroed out in Hahn's accounting system, leaving the rule quantitatively defended only by its least important benefits.⁷⁴

71. Goldman, Lynn R. & Koduru, S., Chemicals in the environment and developmental toxicity to children: A public health and safety perspective, 108 Environ. Health Perspect. 443, 443-448 (2000).

72. EPA, Unfinished Business: A Comparative Assessment of Environmental Problems, App. 2, Report of the Non-cancer Risk Work Group, 1-1 to 1-2 (1987).

73. Ironically, Hahn's own spreadsheet narratively records some of the benefits that his tabulation excludes. See Hahn spreadsheet, cell CA-14 (noting that pollutant loadings under the rule would be reduced by 5.8-7.6 million lbs-eq/yr.), cell AV-14 (calculating, after discounting that the rule will avert only .3 cancer deaths per year), and cell BG-14 (erroneously remarking "no non-fatal benefits listed") (unpublished manu. on file with the author).

74. Emissions Guidelines: Municipal Waste Combustors: Proposed guidelines and notice of public hearing, 59 Fed. Reg. 48,228, 48,238 (1994) (existing combustors); and Standards of Performance for New Stationary Sources: Municipal Waste Combustors: Proposed rule and notice of public hearing, 59 Fed. Reg.

EPA's emissions standards for new and existing solid waste landfills require the largest five percent of landfills to collect and control emissions of methane, along with more than one hundred non-methane organic compounds (NMOCs), including volatile organic compounds (VOCs) and certain hazardous air pollutants. EPA estimated the rule would cost \$330 million a year, which would mean adding \$10-15 per year to household tipping fees, assuming full pass-through of costs.

On the benefit side, EPA monetized only the benefit of controlling VOCs, causing the rule to fail Hahn's test. Ignored by Hahn are all the benefits which EPA described narratively but lacked data to monetize: the benefit of substantially reducing odors emanating from large landfills, and of reducing emissions of a host of hazardous air pollutants such as vinyl chloride, carbon tetrachloride, chloroform, benzene and ethylene dioxide. Any fair reading of the record makes clear that EPA did not consider reducing VOC emissions to be the only – or even the primary – purpose of this rule. Hahn's accounting method does not zero out the benefits of this rule, but it grossly understates them.

EPA's on-board diagnostic rule required that newly manufactured cars and light-trucks have sensors and on-board diagnostic computers capable of detecting abnormal increases in air emissions and alerting the driver through a red light on the dash. The rule was expected to cost about \$65 per vehicle.⁷⁵ EPA's formula for allocating diagnostic costs to various benefits yielded a cost-effectiveness ratio of \$1974/ton of hydrocarbons, \$124/ton of carbon monoxide, and \$1974/ton of nitrogen oxide reduced, which is higher than Hahn's mid-range threshold of cost-effectiveness for these pollutants, thus yielding another "failed" rule.

What Hahn overlooks (though EPA does not) is the likelihood that the on-board diagnostic rule will also yield substantial cost savings to consumers – providing early detection of malfunctions that could lead to costly repairs later on, promoting swift and accurate diagnosis of malfunctions, and avoiding needless repairs.⁷⁶ Anyone who has had his car repaired can attest that averting even one major repair, or one mis-diagnosed and unnecessary repair, could easily save \$65. Nonetheless, these savings are zeroed out in Hahn's analysis, and the rule is rated a failure.

The list of examples could go on and on. Glancing down the line of negative numbers in the net benefits column of Hahn's unpublished spreadsheet seems to reveal a simple and damning picture: agencies repeatedly indicted by their own numbers. Reading through the agencies' own analysis, however, reveals a much different picture. It reveals that many benefits cannot be quantified and monetized using existing data and methods. Agencies are forced, in the last analysis, to make a decision based partly on numbers and partly on their professional judgment and

48,198, 48,207 (1994) (new combustors). See also *Grading the Government* at 1392-1394 (cited in note 1).

75. Final Rule: Control of Air Pollution from New Motor Vehicles and New Motor Vehicle Engines; Regulations Requiring On-Board Diagnostic Systems on 1994 and Later Model Year Light-Duty Vehicles and Light-Duty Trucks, 58 Fed Reg 9468, 9484 (1993).

76. Id.

common sense. When they do so, their explanation of the rule generally offers a narrative that makes a plausible, and often persuasive, case for its enactment, even though the numbers alone may not.

Of course, the agency *could* come up with a comprehensive number that attempts to encompass all benefits. But in many cases such a number would not have a sound scientific foundation and critics like Hahn would complain that it is groundless. Herein lies the Catch-22 which Hahn's methods pose for agencies: if they try to assign numbers to a benefit which cannot, in fact, be scientifically quantified and monetized based on available data and methods they will be accused of fudging the numbers and practicing "unsound science." But if they do *not* assign a number and simply describe the benefit narratively, Hahn and like-minded critics of regulation will zero it out.

In footnote 18 of their reply to critics, Hahn and his co-authors offer an observation that goes to the heart of the problem:

"This [claim that net benefits would increase substantially if the government did not implement regulations which fail Hahn's test] assumes that unquantified benefits are of second order importance to such regulations."⁷⁷

This essay has shown that such an assumption is not necessarily valid, and in numerous cases is demonstrably invalid.

What are the consequences of relaxing this assumption? Do the unquantified and un-monetized contributions – when combined with the monetized benefits – justify the expense of the rule in each case? That is a harder question which this study cannot answer categorically in the affirmative. Answering it rigorously would require a detailed study of the facts of each case. It would require expert judgments about which people might disagree. In short, it would require the kind of in-depth, three-dimensional investigation which I called for in my earlier critique, and which is the antithesis of Hahn's and Morrall's simplistic scorecard approach.

Hahn has famously called for the creation of an "independent agency responsible for replicating agency studies" before agencies rely on them to finalize regulations.⁷⁸ Ironically, it turns out that Hahn's and Morrall's own findings cannot be replicated using valid empirical methods, and their findings must therefore be discounted.

77. Hahn, Hahn 2004 Reply at 12 (cited in note 57).

78. Government's Numbers (2000) at 64 (cited in note 3).