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Office of Management and Budget
(Submitted Electronically to oira_submission@omb.eop.gov)

Dear Sirs:

I am grateful for the opportunity to offer a rather different perspective on how Executive Order 12866 might be rewritten, and how OIRA's regulatory review and coordinative processes might be improved, than that of most of the comments responding to the February 26 *Federal Register* notice and of most of the reform ideas proffered over the past 25 years. As you know, many current and past critics of OIRA have objected to its perennial embrace of cost-benefit analysis (CBA) as a tool to review agency regulations, while yet recommending that OIRA continue to review agency risk assessments (albeit in a less intrusive, rule-by-rule fashion). The other major current of advice contends that OIRA has been insufficiently intrusive, and should redouble and expand its efforts to counteract a "vicious circle" of exaggeration of risk and public demand for burdensome regulations.

I firmly believe, based in large part on several decades' experience as a risk assessment expert and as the agency official who led OSHA's interactions with OIRA during the second half of the Clinton Administration, that both of these broad prescriptions are deeply misguided. I believe OIRA *should* continue to employ CBA to review individual rules and should take a more active role in coordinating regulatory and non-regulatory initiatives across government. But at the same time, we need to admit that OIRA has never provided the "dispassionate second opinion on agency actions" that the February 26 request posits as given; in fact, it has facilitated its own "vicious circle" of *underestimation* of risk, exaggeration of cost, and insufficiently

ambitious and creative solutions to environmental, health, and safety (EHS) problems across government.

I emphasize that these views are my own, and not necessarily those of UMDNJ or the University of Pennsylvania. I also emphasize that I have worked with many of the current and former staff at OIRA over a long period of time and respect their considerable skills as regulatory analysts. To conclude, as I have, that the rule-by-rule reviews I experienced at OIRA as an agency official were unsatisfying is not to say that any of the individual objections OIRA raised were not meritorious or well-intended.

These comments will generally support the conclusions that:

- OIRA should strive to employ a more sophisticated, probing, and humane brand of CBA to review rules, not to abandon CBA in favor of some less replicable and transparent method;
- The Executive Branch has never had adequate leadership and staff capable of reviewing agency risk science; it should establish such a capacity either within OIRA or within OSTP, CEQ, or some new entity[1]; failing that, it should instruct OIRA to review agency CBAs *without* second-guessing scientific conclusions outside its expertise;
- OIRA has consistently failed to review agency rules from both complementary vantage points of "how can we chip away at what we've been given, in order to make it less protective and (perhaps) less costly?" and "how can we improve what we've been given in order to make it more protective and (perhaps) more costly?" My own interactions with OIRA, exclusively during a period when the White House was ostensibly favorable to substantive regulation, suggest that in more than 100 "suggestions" about individual regulatory provisions, OIRA required OSHA to scale back the stringency or coverage of our proposed or final rules, but rarely if ever prodded us to consider making them more protective.

[1] I am intrigued by Professor Rose-Ackerman's suggestion (p. 3 of her comments) that the NAS or the GAO could be tasked with reviewing (among other things) agency risk science, but I think that as long as there is going to be White House review of regulatory provisions and regulatory economics, there should be some Executive Branch capacity in regulatory science as well, and that duplicating this externally could add further delay without significant benefit.

- Even more significantly, OIRA has failed to correct egregious examples of agency inaction; the "prompt letters" John Graham introduced occasionally had the desired effect, but more often engendered only lip service on the part of the agencies prompted. The Executive Order should set forth a public process for suggesting targets for OIRA prompt letters, and should require agencies to make steady progress towards final action on matters prompted by OIRA (unless they can explain their reasoned objections publicly); it should also encourage agencies to backstop these concerns by issuing bulletins or guidance while regulatory action is pending.

- OIRA has consistently acted as if risk assessment (the raw material for benefits assessment) is the phase of CBA that needs the most oversight and rigor—to the contrary, regulatory cost accounting is clearly the weak link in CBA, and needs much more transparency, sophistication, and even-handedness. This is the half of CBA that OIRA staff *should* be adept at.

- OIRA has almost always construed "interagency coordination" as a means to convince one agency to forego its objections to the action of another agency that might hinder its mission; instead, OIRA should proactively seek to spur multiple agencies to collaborate and to solve problems that cross institutional or physical boundaries.

- OIRA should pursue non-traditional approaches to EHS hazards, but should realize that guidance documents, market mechanisms, and the like are only part of the portfolio of tools—there are various unexplored ways to achieve more protection at less cost.

- OIRA should rethink its biased approach to risk-risk tradeoffs, which exaggerates potential (or wholly made-up) downsides of regulation and rarely considers ancillary benefits or properly construes the tradeoffs as an impetus to solve multiple risks.

- OIRA could serve as the focal point for an entirely new approach to risk management—a "solution-focused" paradigm that uses risk assessment and economic analysis to point the way to optimal technologies and other control measures, rather than to dissect problems and set numerical standards that may be merely aspirational (see Appendix A to these comments for a brief description of this paradigm).

The remainder of these comments will follow in two sections: (1) suggestions organized via the eight topic areas listed in the February 26 *Federal Register* notice; and (2) additional specific suggestions for text changes in E.O. 12866 itself.

Comments in Each of the Eight Topic Areas

1. *OIRA-Agency Relationship:*

I came to government in 1995 as a critic of the OIRA-Agency relationship, based primarily on my concerns with unqualified OIRA analysts pursuing unscientific crusades against what they (mis)perceived as problems with quantitative risk assessment. I left DC in 2000 (to become an OSHA regional administrator) pleasantly surprised at the quality of the OIRA regulatory reviews; a very large number of the changes OIRA asked for made our proposed and final rules more cost-effective and clear. However, the relentless nature of the reviews—questioning every provision or explanation thereof that might result in EHS protections that could be scaled back without clearly jeopardizing the entire purpose of the rulemaking—was disappointing and ultimately could lead to unfortunate strategic behavior on the Agency's part.

There are unjaundiced ways to look at a regulation, that flow from a willingness to believe that some provisions could be expanded, implemented more quickly, or otherwise made more stringent (or more reliably-enforced) *without* unduly raising the burden on the regulated. OIRA should try much harder to add value in this way when it conducts reviews, rather than gaining satisfaction merely by chipping away at what the proposing Agency wishes to promulgate.

In addition, I remain concerned about the propriety of OIRA, with the current skills and backgrounds of its staff and leadership, second-guessing agency risk science. This is a perennial problem, as this excerpt from the scientific literature in 1993 suggests:

> OMB's comments are full of errors and misconceptions that demonstrate a fundamental lack of understanding by OMB of the scientific methods relied upon by OSHA. OMB's comments are also remarkably lacking in scientific objectivity... In some instances, OMB's errors are so blatant that they can only be understood as attempts by OMB to discredit OSHA's analysis by any means possible...Appropriate peer review of the risk assessments of government agencies is highly desirable, as is also economic evaluation of regulations affected by risk assessments. However, to avoid subverting the decision-making process, it is important that risk assessments, as well as reviews of those risk

assessments, be conducted by qualified individuals and in an unbiased manner. Kenny S. Crump and Robin Gentry, *Risk Analysis,* **13:** 487-489.

The remedy of establishing a risk-science review function conducted largely by scientists is also not a new idea: "Over time, OSTP could act as a force for consistency and reasonableness of practices, *while counteracting the tendency of OMB's economists to jump into scientific matters that are outside their expertise."* John D. Graham, "Edging Toward Sanity on Regulatory Risk Reform," *Issues in Science and Technology,* Summer 1995, pp. 61-65 (emphasis added). I am not unaware that since making this observation, Dr. Graham himself hired several scientists at OIRA, but I think that adding three or four analysts (unless I am out-of-date here) can only hope to paper over a more fundamental structural problem here.

2. ***Disclosure and Transparency:***

My only comment here, other than to applaud the various improvements in OIRA disclosure made under John Graham's tenure at OIRA, is to encourage OIRA to involve the agency staff more in discussions with regulated parties. Perhaps this has since changed, but in my experience circa 1995-2000, we were required to attend such meetings but were cautioned not to engage directly with the aggrieved stakeholders.

3. ***Encouraging Public Participation:***

I think the public has ample access to agency regulatory processes, and that the agencies have made strides to improve that access in recent years (bringing rulemaking hearings to the field, for example). The more important issue is "access to what?" One of my enthusiasms for a "solution-focused" approach to risk assessment and management stems from the fact that existing processes have tended to channel community participation in regulation towards controversial issues regarding the size

and nature of the problem, rather than towards the real benefits and costs of technological and other solutions to the problem.

4. The Role of CBA:

A. Problems on the "Cost Side":

If this statement sounds counter-intuitive, I've made my point already: OIRA should be more concerned about errors and lack of rigor with respect to the costs of each regulation than it is about the benefits (risks reduced) of each regulation. The poor track record of regulatory economics in estimating *ex post* costs (with a bias towards overestimation) is well-known[2], but there are much larger issues at work here. With colleagues at Princeton University, Resources for the Future, and elsewhere, I am conducting a multi-year study on the different ways risk scientists and regulatory economists handle uncertainty and interindividual variability in their respective domains. It is disquieting that much of the OIRA guidance, and essentially all of the Congressional regulatory "reform" proposals of the 1990s, aims its sights at improving risk science rather than regulatory economics, and even leaves the latter out of many key recommendations regarding rigor, transparency, peer review and the like that apply in spades to the former. Just to give one example: we accept that small individual risks can be "de minimus," which implies directly that a regulation that reduced a de minimus environmental fatality risk to the entire U.S. population could save hundreds

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[2] See, for example, (1) Harrington, Winston, Richard D. Morgenstern and Peter Nelson (2000). "On the Accuracy of Regulatory Cost Estimates." *Journal of Policy Analysis and Management*, **19(2)**, pp. 297-322. (2) Hazilla, Michael and R.J. Kopp. 1990. "Social Cost of Environmental Quality Regulations: a General Equilibrium Analysis." *Journal of Political Economy*, **98**, pp. 853-873 (August). (3) Goodstein, Eban, and Hart Hodges (1997). "Polluted Data: Overestimating Environmental Costs." *The American Prospect*, **35** (Nov./Dec.), pp. 64-69. (4) Office of Technology Assessment, U.S. (1995). *Gauging Control Technology andRegulatory Impacts in Occupational Safety and Health: An Appraisal of OSHA's Analytic Approach*, report #OTA-ENV-635, September 1995, 102 pp. (5) Porter, Michael E. and Claas van der Linde (1995). "Toward a New Conception of the Environment-Competitiveness Relationship." *Journal of Economic Perspectives*, **9(4)**, pp. 97-118.

of lives and yet be assessed as having exactly zero benefit (300 million increments of zero cumulate to zero). Yet we have no analogous concept of the "de minimus cost." Similarly, we wring our hands over the possibility that a rule may "overprotect" the majority for the sake of providing adequate protection to a highly-exposed minority, and yet we rarely consider the rules that are *not* promulgated because the costs would fall on a highly-influential minority. Perhaps the major analytic reform in CBA, and at OIRA, in my opinion, involves the need to harmonize the treatment of like phenomena on both sides of the cost/benefit divide.

B. Problems with "Risk-risk" analyses:

OIRA should encourage the agencies to explore much more fully the indirect effects of their regulations, but only if it is willing to step back and take a *much* more even-handed and logical approach to "risk-risk tradeoffs" as well as to consider other important secondary effects. On the latter point, I agree with Revesz and Livermore that the Executive Order should require even-handed treatment of the secondary benefits of regulation as well as secondary harms -- and I would supplement their comments by emphasizing the secondary *economic* benefits of regulation. Agencies should be strongly encouraged to consider general-equilibrium measures of regulatory cost, so that OIRA will not continue to compare a tally of benefits (that includes offsetting ones) to a partial tally of costs (that ignores offsetting ones). But within the realm of ancillary risks alone, OIRA has consistently failed to realize that *not all purported trade-offs are real trade-offs,* for two fundamental reasons that the pioneers of the risk-risk literature have inexplicably not shown much interest in. First, many secondary risks are completely within the control of the regulated parties who may *claim* that they are inevitable. Whenever a regulation may make substitutes necessary or more attractive, there are always more or less perverse substitutes available. In one of the rules that OIRA reviewed when I was at OSHA, we were required on three separate occasions to attend a meeting with industry representatives and OIRA at which dire predictions were offered of the carnage our rule would cause "when" the industries

were forced to use riskier inputs and practices. With over 10 years' hindsight now available, it is clear that in none of these cases were these predictions in any way borne out, probably because the need to substitute away under the rule was exaggerated, but especially because there were (even at the time) more sensible adaptations available than the ones we were warned about. OIRA needs to be on guard for the "sham tradeoffs" analysis that depends on a fanciful behavioral assumption. The second major problem with penalizing one regulation by the size of the secondary risks it could engender is that this treats the two risks as zero-sum combatants rather than as a "wake-up call" to consider controlling *both* risks - and no one is in a better position that OIRA to respond by being more active rather than cringing in the face of such tradeoffs. If the industrial process at issue can only make use of one of two harmful substances, the option of controlling both in cost-beneficial ways must be considered, and OIRA must consider it if the Agency has not (or has been blindsided by the tradeoff late in the game).

Finally, I hope it goes without saying by now that the most tenuous indirect effect of all—the purported "richer is safer" effect by which regulatory costs can supposedly lead to increased fatalities—is not worthy of agency or OIRA attention at this time. IF the agencies and OIRA tried to identify *whose* wealth would decrease **and whose would increase** as a result of a particular regulation, there might be some value in tallying up *all* the indirect effects, positive and negative, of changes in wealth on changes in health. However, it has never been possible to estimate what effect, if any, changing an individual's wealth might have on her health—all the studies to date have attempted to contrast the health of populations that differ in income or wealth—and even at the population level, more recent studies have suggested that the sign of the possible wealth-health effect may have been wrongly estimated.[3]

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[3] See, for example, a series of recent empirical analyses by Christopher Ruhm suggesting that mortality may *decrease* and physical health may improve when the economy temporarily weakens.

5. Distributional Equity:

Perhaps no other issue in CBA and regulatory review has recently been the subject of more angst, but less specific progress, than the need to account for the equity consequences of agency action and inaction. At the outset, I take some issue with the comments of Professor Rose-Ackerman (p. 2 of her comments) that we should not add distributive weights to CBA. We *already* add distributive weights to CBA, in the form of an exactly equal weight of (1/N) to every member of the population affected. This may be a sensible default position, but is in no sense a value-neutral one.

I agree with the comments of Professor Adler that there are well-established ways to account for distributional equity in public policies, and that CBA should make much more use of them. However, both his comments and those of many other scholars in the field[4] seem to conceive of "equity" as a function of the distribution of benefits and costs to subgroups with *other* salient characteristics (particularly income) that distinguish them. As important as this issue is, the more fundamental phenomenon is the concentration of risk or cost *irrespective* of these other characteristics. *We need a way to get past the implicit insensitivity of benefits valuation to the concentration of risk (and of cost).* Most experts give lip service to the observation that the VSL (value of a statistical life) concept is not intended for use when individual risks are so high that they are *per se* unfair, or at least that these risks ought not to be valued only as proportionately greater than "small" risks. And yet, OIRA recently approved an OSHA regulation (hexavalent chromium) where the individual excess lifetime cancer risk *at the new exposure limit* was estimated to be in the range of one to four **percent** (that is, 10,000 to 40,000 times the one-in-one-million benchmark EPA often strives for). I assert that regardless of the income, race, gender, etc., of the workers covered by this regulation, the number of statistical fatalities OIRA accepted should not have been valued as if they were the consequence of a diffuse pattern of "small" risks.

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[4] See, e.g., a recent article by former OIRA Administrator John Graham ("Saving Lives through Administrative Law and Economics") in the University of Pennsylvania Law Review (**157**:395-540).

I urge OIRA to convene a series of expert and public discussions to explore how agencies could use methods such as those described by Professor Adler to account for the premium society should place on reducing intolerably high individual risks, irrespective of the other characteristics of those facing them (and preferably even if we cannot specifically identify those at highest risk, but know that they exist).

6. *Avoiding Undue Delay:*

As long as more senior officials can and will enforce the requirement, allowing 90 days for OIRA review of NPRMs and final rules seems to me reasonable in light of the overall complexity of the public process. However, other aspects of this process, particularly the recent (over)emphasis on academic-style peer review exercises over and above the more egalitarian and transparent (and rigorous, if participants choose to make use of them) opportunities for public comment, do add undue delay. This is especially so in the case of an agency such as OSHA that already conducts trial-type rulemaking hearings.

7. *The Role of the Behavioral Sciences:*

As a separate file, I have transmitted a recent journal article (Finkel, A.M. (2008), "Perceiving Others' Perceptions of Risk: Still a Task for Sisyphus," *Annals of the New York Academy of Sciences*: **1128**: 121-137) that endorses a greater role for behavioral economics and neuroscience in risk management, but offers various cautionary remarks about the past misinterpretations of findings from these fields.

8. *New Tools for Achieving Public Goals:*

First, I encourage OIRA to promote non-traditional forms of EHS protection that go beyond information dissemination and the largely-meaningless "alliances" and other voluntary programs that occupied so many Agency resources during the past eight years. In various circumstances that can often be identified in advance, traditional rulemaking *is* needlessly adversarial, dilatory, and inefficient—but superior forms of control need to set measurable goals and means to evaluate and enforce them. OSHA experimented in the late 1990s with "enforceable partnerships"—product stewardship and similar programs that depended on collaboratory drafting by industry and labor of codes of conduct, goals, and timetables, and emphasized the willingness of manufacturers to help improve the knowledge and compliance behavior of their industrial customers. In OSHA's unique circumstances, these programs were enforceable via its "general duty" authority, but other agencies could package these sorts of ideas into contractual agreements.

Secondly, OIRA should get out of the "league table" business. These purported rank-orderings of the "bang for the buck" (usually conceptualized as the cost per life saved, or CPLS) of a diverse array of federal regulations are misleading on nearly every level and contribute almost nothing to sensible priority-setting. Other scholars, particularly Lisa Heinzerling[5], have shown how bizarre the choices of entries in these tables have been since OIRA analyst John Morrall first began constructing them in the 1980s; many of the least "efficient" interventions were never codified by federal agencies (perhaps because of their high cost/benefit ratios), while many of the most "efficient" ones are not regulatory at all, but are free-market transactions (often with no further opportunities, as with many medical technologies, to find additional producers and consumers and "save more lives"). But even comparing the CPLS of actual agency rules is an easy calculation to botch. Even the ratio of only two CPLS estimates involves four highly uncertain inputs (the cost of each risk and its benefit), and the uncertainty in that ratio, never even hinted at in the OIRA "league tables," is generally so large that

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[5] Heinzerling, L. (1998). "Regulatory Costs of Mythic Proportions." Yale Law Journal 107(7): 1981-2070.

there is a significant probability that the rule or program touted as "better" may in fact be "worse" - even by the narrow and partial measure of "cost per some of the benefits" that OIRA seems to regard as definitive.[6] To the extent that the least "efficient" actual rules seem to have low total benefits because they protect a small and/or otherwise disadvantaged group, or because they provide benefits beyond reductions in premature mortality, these tables also pit programs against each other for no logical or productive reason.

Additional Suggested Text Changes for E.O. 12866

Section 1:

- In (a), the "maximize net benefits" criterion is too restrictive and could lead to unwise results. For example, maximizing the absolute (as opposed to the relative) difference between benefits and costs biases the outcome towards more expansive, but not necessarily more efficient options. In other cases, important benefits (perhaps, but not necessarily, accruing to a specific subpopulation) might be extracted by going beyond the point at which absolute net benefit is maximized, but to a point where total benefits still exceed total costs. Agencies should be encouraged to "choose the regulatory (or other) alternative that most effectively meets the social goals of the regulatory program, considering benefits, costs, equity, and other factors."
- In this same sentence of §1(a), the parenthetical phrase is ungrammatical: it appears that agencies are instructed to "maximize distributive impacts". If the parenthetical is supposed to expand upon the concept of "net benefits," it should mention costs and other factors that are "net" of benefits; if it is supposed to connote that there are factors that cannot necessarily be quantified as part of the net, it should instruct agencies to consider them, not "maximize" them.
- Section (b)(1) would be the logical place to supplement the "problem-focused" approach with a "solution-focused" one (see above). Agencies should be encouraged to identify the *opportunit(ies)* for transformative

[6] See pp. 104-6 of the new National Research Council report **Science and Decisions: Advancing Risk Assessment** for a brief discussion of the special pitfalls of comparing two (let alone four or more) uncertain inputs to each other.

technological or other change that come(s) from confronting one or more problems and envisioning solutions to them.

- Similarly, in (b)(4), agencies should consider the nature of the costs and benefits of solutions to problems; setting purely risk-based priorities without thinking about the means and costs of control leads at best to a list of "what to worry about," not a list of what to consider doing[7].

- In (b)(5), there is no definition of "most cost-effective." I urge OMB not to develop a definition that puts agencies in a straight-jacket with respect to one-dimensional measures of relative cost-effectiveness (i.e., one that denigrates approaches that may have slightly less "bang for the buck" than wholly different ones). Instead, the agencies should be encouraged to reject options that are *absolutely* less cost-effective (that is, provide fewer benefits at greater cost[8]), but should be free to choose options that are both marginally more beneficial and marginally more costly.

- In (b)(6), there are no definitions of either "cost" or "benefit" (and none in the definitional Section 3 either). Of the many logical ways to parse these terms, I encourage OMB to define "benefits" as changes in things that are not traded in markets (e.g., environmental quality, health, longevity) and "costs" as changes in quantities that are traded in markets. In this way, ancillary risks can be kept on the benefits side of the ledger (as offsetting the primary risk-reduction benefits of the regulation being considered), and all social costs of complying with and adapting to the regulation (including "negative costs" in the form of employment or price effects in secondary markets) can be kept on the net-cost side.

- In (b)(8), I urge OMB to rethink this fetish about performance standards over design/technology standards. To the enforcing agency, the latter can be much easier to document and can provide more assurance that regulatory goals will be met. Perhaps more importantly, regulated industries often prefer more specific regulatory guidance (or at least the option to select a specified "safe harbor"), their rulemaking comments notwithstanding. Surely the recent example of the OSHA ergonomics rule, where a Congressional veto was largely driven by industry opposition to the extreme performance-oriented nature of the regulatory text, should make this point obvious.

- The recommendations in (b)(11) constitute a more sensible (and probably a mutually inconsistent) decisional criterion than the "maximize net benefit" instructions above.

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[7] See, e.g., **Worst Things First? The Debate over Risk-Based National Environmental Priorities,** A.M. Finkel and D. Golding, eds., RFF Press, 1996, 345 pp.

[8] Or that differ from the preferred option only by being either more costly (for the same benefit) or less beneficial (for the same cost).

Section 4:

- The Regulatory Working Group would be a natural forum for regular discussion of pending agency actions that stand to transfer risks to another agency's constituents, of agency inaction that impedes the mission of another agency, and of opportunities for coordinated action to solve problems with less uncertainty and self-contradiction for the regulated community. In my experience during 1995-2000, the RWG did very little of the above, although we were understandably preoccupied during most of this time with the numerous regulatory "reform" proposals in Congress.

Thank you again for the opportunity to participate in this reevaluation of EO 12866.

Sincerely,

Adam M. Finkel

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APPENDIX A

[abstract of a December 2008 presentation at the annual meeting of the Society for Risk Analysis]

"Solution-Focused Risk Assessment": Quickening the pace, accomplishing missions, expanding horizons. *Finkel AM*; UMDNJ School of Public Health AND Penn Law School*

Abstract: Re-conceptualizing risk assessment as a method for helping to solve environmental problems, rather than (merely) understanding environmental hazards, may provide three major classes of benefits over the status quo. First, it can help break the endless cycle of analysis: when the goal is to know enough to decide, rather than to know everything, natural stopping points emerge. Secondly, it can lead to more true decisions about risk reduction, rather than pronouncements about them. As much as agencies rightly value performance-oriented interventions, it is unfortunately the case that setting a permissible exposure limit or a national ambient air quality standard is often more a conclusion about what level of risk would be acceptable than any kind of guarantee that such a level will be achieved, let alone a decision about which actual behaviors will change and how. Third, it can promote expansive thought about optimal decisions, ones that resolve multiple risks simultaneously, avoid needless risk-risk tradeoffs, and involve affected stakeholders in debating what should be done. Arguably, the longer the disembodied analysis of risk information is allowed to proceed before solutions are proposed and evaluated, the more likely it is that the "problem" will be defined in a way that constrains the free-wheeling discussion of solutions (in other words, a new mirror-image adage that "if everything around you looks like a nail, the only question is what kind of hammer to pick out"). This presentation will explain these benefits with reference to several case studies of "what might have been," and then proceed to anticipate some of the significant concerns with the notion of eliminating the organizational part of the "firewall" between risk assessors and risk managers.

Perceiving Others' Perceptions of Risk

Still a Task for Sisyphus

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The recent literature providing insights from neuroscience and evolutionary biology into how individuals perceive risky choice situations represents a "second wave" of findings that recapitulates as well as challenges the risk perception research begun in the 1980s, which relied on psychometric survey research. Gleaning insights from the first wave of research that could improve the communication and control of environmental risks has yielded disappointing results. This is a result, in part, of the eagerness of scholars and pundits to posit a chasm between the "rational" and "objective" perceptions of experts, on the one hand, and a lay public that is seen as lurching between "paranoia and neglect" and as insensitive to the magnitude of risks. Interpretations of the psychometric research have suffered from inattention to uncertainty and interindividual variability in risk, to expert biases, and to important aspects of risky choice that were not explored in the first wave of research. Initial signs indicate that neuroscience and evolutionary biology research may fall prey to similar misinterpretations. This article summarizes some of the most intriguing findings of the "second wave" of risk perception research and advances four themes that may help make the new findings less divisive and more useful for improving risk communication and risk management. Continued research into risk perception should perhaps be embedded in a more general theory of public choice in the face of uncertain and variable costs and benefits and with a respect for distributive justice as an important goal in risk management.

Key words: **biases; heuristics; evolution; interindividual variability; neuroscience; risk assessment; risk perception; uncertainty**

Introduction

Understanding—or convincing ourselves that we understand—how people perceive risks can, ironically, lead to changes in the risks that people actually face. Risks from exposures to environmental hazards and unsafe products are controlled (or not) in large part by a system of regulatory requirements and economic (dis)incentives. The "risk managers" atop this system often see their mission as that of reducing "real" risks, as opposed to risks existing in the irrational perceptions of the public. The insights that neuroscientists, evolutionary biologists, and others offer about how humans perceive risk are, therefore, important in their own right but arguably are most important *after* they have been translated (or mistranslated) by practitioners responsible for identifying, estimating, communicating, and managing those risks. As a risk assessment practitioner and former regulator in the U.S. federal government, I welcomed the opportunity to listen to risk perception experts at the Montauk workshop and encourage other risk managers to immerse themselves in the growing literature on the biological basis of risk perception, to which this volume is an important contribution. I offer this article as a complement to the research findings in this volume from the vantage point of someone interested in the applications of risk perception research.

Until recently, the toolbox of risk perception research was limited to various survey techniques and observations from structured tasks and games—insights about "how people perceive" were gleaned primarily by what people said about their own preferences, aversions, and beliefs and by the choices they made in hypothetical situations. The papers in this volume attest to a groundswell of interest in biophysical and other quantitative evidence regarding perception in both humans and other animals and in insights from evolutionary biology as to how these perceptions may have arisen, been conserved, or been modified over time. The crucial question that will determine the success or failure

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of future dialogue and collaboration between neuroscientists and risk assessors/managers is this: *Will the latter group do any better at understanding the strengths, limitations, and practical applications of these new theories and experimental findings than it did with the psychometric insights of previous decades?*

If the lessons the risk assessment/management community gleaned from the previous generation of research is any guide to the future, there is reason for pessimism and concern. The temptation to overinterpret or to project what one wishes to see onto the data may be heightened as the new generation of findings arrive with the aura of objective, reproducible results.

In this article, I will first summarize the track record of risk assessment/management experts in translating insights from risk perception research. Next, I will briefly recapitulate some of what I see as the most intriguing insights presented elsewhere in this volume. Finally, I will discuss several themes I see as important at the boundary between risk perception and risk management, with an emphasis on sounding some cautionary notes about interpreting and translating the primary data.

Risk Perception Before Neuroscience

Findings

For much of the second half of the 20th century, risk perception research concentrated on identifying and explaining systematic differences between the expressed or measured perceptions of laypeople and one or more presumed "gold standards." These studies generally compared public responses either to some objective measure of the risks being evaluated or to the responses of "experts." One class of studies tried to decouple the quantitative aspects of risks from their larger contexts by assessing lay and expert performance on strictly empirical tasks, leading to the catalog of biases and heuristics most prominently explored by Tversky and Kahneman.[1] Both laypeople and experts, it seems, fall prey to errors from anchoring, overconfidence, the gambler's fallacy, and a host of other lapses. Both groups also exhibit arguably poor performance on basic tests of logical and probabilistic reasoning, believing, for example, that the famous "Linda" is more likely to be both a bank teller and a feminist than she is to be a bank teller with views anywhere on the spectrum from misogynist to feminist. The paper by Kurz-Milcke *et al.*[2] in this volume provides an excellent summary of tests of basic Bayesian reasoning, documenting the difficulty even experts have in estimating the predictive value of an imperfect test for a rare condition. Even when a person can estimate with reasonable accuracy the probability of a single event, we humans seem to be fairly inept at making law-of-large-number inferences; for example, the (high) probability that one's next breath will contain at least one of the molecules that Julius Caesar exhaled with his dying breath is almost universally underestimated.[3] This deficiency is similarly evident in the human tendency to view disease clusters as causal rather than coincidental (with a large enough sample, individual rare events have a high probability of occurring at least once by chance).

A related set of findings, summarized superbly in a 1985 magazine article,[4] has continued to demonstrate that when perception of risk becomes the raw material for choice, additional biases come into play. Both in hypothetical monetary gambles and in the classic "General's Dilemma," people tend to make different choices when the identical prospects are framed as losses versus framed as gains (a chance of "saving 200 [of 600] soldiers" versus the chance that "400 [of 600] soldiers will die").

Other groups of researchers, notably led by Paul Slovic, have explored risk perception via direct surveys of how various populations rank various risks, either by subjective concepts, such as severity, or by ordinal estimates of the magnitude of each risk (e.g., the number of attributable fatalities or cases). Slovic's well-known "psychometric paradigm" was developed to explain the common finding that laypeople tended to rank certain risks as more severe than did experts and to overestimate the magnitude of the same risks (while underweighting and underestimating other risks). The table of 30 activities and technologies first developed by Slovic *et al.* in 1980[5] (and reproduced in his influential 1987 paper in *Science*,[6] which in turn has been cited in at least 2000 other sources) shows, for example, that members of the League of Women Voters ranked nuclear power as the riskiest of the 30 hazards, while experts ranked it 20th; conversely, experts ranked medical X-rays 15 places higher than the laypeople did. The exercise of ranking risks by perceived and "actual" severity has since found its way into numerous federal and state comparative risk projects,[7] which have often recommended to legislatures that agency budgets be reallocated so that they can focus on the "worst risks" rather than the ones the public most fears.

Interpretations of Findings

The researchers initially responsible for pointing out the complexity of risk perception generally sought to describe it from within the domains of psychology and anthropology. Slovic and colleagues, for example,

have gradually made more ornate their original factor–analytic representation of the key determinants of risk perception, which at first included only two axes: the degree to which the risk was "dreaded" (itself a combination of having a catastrophic potential, being out of the control of the individual, and potentially concentrated among a small subpopulation) and the degree to which it was "unknown" (itself a combination of novel, unobservable, and insidious, with a long latency period). Nuclear power clearly can score high on each of these spectra, and this was especially so in the early 1980s when the surveys were first conducted. Over the years, these and other researchers have added various other factors to better explain the aspects of certain risks that seem to heighten public concerns disproportionately to their magnitude. Chief among these are the degree of trust that an individual or the public places in those responsible for creating or managing the risk, and the "affect" toward the source of the risk, which may cause people to regard an activity as more risky (and thus can rationalize it as more worthy of avoidance or opposition) if they first perceive it as undesirable, stigmatized, or otherwise associated with negative emotions.

Other researchers, most notably Douglas and Wildavsky,[8] have emphasized cultural factors that could influence risk perceptions. To the extent that perceptions are shaped by family, friends, and interest groups one feels affinity or antipathy toward, people may fixate on certain risks and ignore others more out of shared beliefs and biases than out of individual ones. These cultural anthropological insights into risk perception have been coupled with ideas from communication research about cultural institutions with the power to magnify or dampen perceptions, leading to the theory of the social amplification of risk.[9]

Almost as soon as the psychological and anthropological findings began to emerge, a variety of scholars not involved in studying how different people react to risks began to develop a cottage industry dedicated to putting these conclusions in a risk management and policy context. Epidemiologist Elizabeth Whelan, for example, began writing books and articles in the early 1980s with titles such as "Toxic Terror: the Truth Behind the Cancer Scares," in which the public is seen as a vast reservoir of unfounded fears. Several years before he became chief administrator of the Environmental Protection Agency's (EPA) research programs, George Gray echoed this belief[10] when he explained that "the field of risk perception tells us that there's lots of things that. . . make people's concern meters go up: and it's quite clear that chemicals, for a variety of reasons, do that. The fact that people can't see them. . . and that they have kind of scary names seems to make people react more strongly."

Perhaps more constructive have been the efforts of a small group of scholars who have tempered their observations about risks the public arguably fears too an eye toward other risks that we may not be concerned enough about. The progenitor of this group is probably John Graham, who as early as 1992 was writing that both public consciousness and societal interventions about a panoply of risks reflected a "syndrome of paranoia and neglect."[11] (In an article in press [p. 151],[12] Graham continues to describe the situation with this phrase, which may be significant given that he had spent the preceding 5 years as administrator of the White House office responsible for reviewing federal agency regulations). Although one might view the addition of "neglect" to the complaint about public perception as a second insult (laypeople are blasé about the wrong risks, too), Graham and others seem instead to be emphasizing that public fears can be channeled into a healthier direction, with the goal of "smarter" regulation of hazards worthy of concern rather than of less regulation across the board (for example, Graham has proposed that the United States can save 60,000 more lives each year by spending less to control on environmental chemicals and more on immunizations, bicycle helmets, and other more efficient lifesaving programs[13]). A more recent, albeit less sweeping, example of misplaced public priorities (an inverse relationship between the public fear of a risky situation and the "true" risk) comes from Gigerenzer,[14] who presented a compelling analysis of the additional trips taken by car rather than by plane—and approximately 1595 additional fatalities as a result—during the first year after the attacks of September 11, 2001.

The diagnosis of collective misperceptions has also spawned several diagnoses of a broken political system. Shortly before his appointment to the Supreme Court, Justice Stephen Breyer[15] wrote of a "vicious circle" wherein public fears about certain kinds of risk influence risk assessors and bureaucrats to generate more and more "conservative" (exaggerated) estimates of the magnitude of these risks, which in turn purportedly fuels more public concern and clamor for regulation. Cass Sunstein posited a similar dynamic when he and Timur Kuran[16] coined the term "availability cascade" to refer to the "self-reinforcing process of collective belief formation by which an expressed perception triggers a chain reaction that gives the perception increasing plausibility through its rising availability in public discourse." In other words, because we tend to fear what is more readily recalled, friends, the media, or other "availability entrepreneurs" can make us believe

TABLE 1. The "size" of 11 hazards and the public's concern about them (adapted from Ropeik, 2003)[17]

Hazard	Annual odds of dying (1 chance in . . .)[a]	Number of news articles[b]	Number of annual deaths[c]	Articles per death[d]
Skin cancer	29,500	102	9559	0.01
Food poisoning	55,000	257	5127	0.05
Bicycling	578,000	233	488	0.48
Heat exposure	950,000	229	297	0.77
Children (under 15) falling from windows	2,400,000	89	25	3.53
Fireworks	71,200,000	59	4	14.90
Amusement parks	72,300,000	101	4	25.89
Snake bites	19,300,000	109	15	7.46
Drowning while boating	400,900	1688	703	2.40
West Nile Virus	1,000,000	2240	282	7.94
Shark attacks	578,000,000	276	0.5	552.00

[a]Note that there were two other "dangers of summer" in the original *New York Times* column—lawn mowers and Lyme disease—for which there was no record of fatalities, only of injury or morbidity. The original column contained the odds of injury/morbidity for a different subset of the 13 dangers; this Table only extracts fatality information.

[b]Ropeik[17] tallied the number of articles in major U.S. newspapers and wire services during a 3-month period between June and September 2002.

[c]To derive the numbers in this column (not part of the original article), I multiplied the "1 chance in . . ." figures given in the article by the population of the United States in 2002, with the exception of the entry for "children under 15 falling from windows," where I used the number of U.S. residents under age 15.

[d]To derive the numbers in this column (not part of the original article), I divided the number of articles by the number of annual deaths.

certain hazards are more dire than they truly are. David Ropeik, who opened the Montauk conference with an excellent example of how many risk assessors tend to view the public in light of risk perception research, has documented a disconnect[17] between the efforts of the print media to call attention to various risks and the actual seriousness of each risk (as measured by the total number of injuries and fatalities attributable to it).

During the summer of 2002, Ropeik found, for example, that "major American newspapers and wire services ran 2240 articles on West Nile virus, which kills fewer than 300 Americans a year, while there were 257 articles on food poisoning, which will kill more than 5000 of us." TABLE 1 shows the 11 "dangers of summer" Ropeik and Holmes had depicted in roughly an ascending order of "more fear, less risk." I am at a loss to explain why the order is not strictly ascending in the implied units of "fear per unit of risk ("articles per death")," but the main point of the graphic that ran in the *New York Times* serves its intended purpose reasonably well—to show that some hazards generate much more media attention per unit of "actual risk" than others. For example, using 282 million as an estimate of the U.S. population in 2002, the media featured roughly one article on skin cancer and sunlight for every 100 annual deaths it causes, whereas it ran more than 500 articles on shark attacks for every attributable annual death, a disparity of more than 50,000: 1.

In practice, these various diagnoses exhibit a curious inconsistency, or perhaps an opportunism. Depending on the situation and the critic, the prescription offered in light of the observation that "the public fears the wrong risks" generally involves educating citizens to show them the error of their ways, or perhaps ignoring the clamor in the hope it will run its course. For example, when public outcry snowballed in 1989 following a report that the growth regulator Alar might pose an excess lifetime cancer risk as high as 1 in 4000 to children who frequently drank apple juice, prominent biochemist Bruce Ames[18,19] argued that reducing or eliminating Alar exposure should be of very low priority because the public was simultaneously exposed to an 18-fold greater cancer risk from the naturally occurring mold aflatoxin B_1 found in peanut butter. In other words, public misperception should not be allowed to interfere with the goal of addressing the worst risks first. During a series of congressional hearings in 1994–1997 on proposed legislation to set ground rules for how federal agencies should conduct quantitative risk assessment, John Graham testified on at least six occasions that Congress and the agencies should consider

revising upward the one-in-one-million benchmark that they generally used as the goal of risk-reducing regulation. He argued that lifetime excess environmental risks with probability less than about 5 per million should be regarded as *de minimis* and not worthy of regulation, on the grounds that the public already ignores at least one involuntary risk of this magnitude—the danger that an airplane crash will kill someone who is on the ground rather than on the plane (see section PERSONAL OBSERVATIONS (I) below). In other words, in this case public policies should be realigned in deference to fixed public perceptions.

Whether the aim is descriptive or prescriptive, the center of gravity of the risk assessment literature clearly sees many of the risk perceptions of laypeople as arising from deficiencies and errors in judgment, rather than from ways of processing empirical *and* other information that are worthy of some deference, if not respect. Even commenters regarded as politically progressive manifest what can charitably be described as disdain for those whose risk perceptions do not match those of (a subset of—see section PERSONAL OBSERVATIONS (I) below) "experts" in risk assessment. Sunstein's 2002 book *Risk and Reason*[20] minces no words in this regard: "when ordinary people disagree with experts, it is often because ordinary people are confused." He apparently sees no reason to question that there is a right and a wrong answer (as opposed, perhaps, to more than one legitimate view) to how dire any given risk is, or to question where the truth resides when perceptions duel: "precisely because they are experts, they are more likely to be right than ordinary people . . . brain surgeons make mistakes, but they know more than the rest of us about brain surgery."

Although it is my impression that many of the pioneers of the psychometric paradigm of risk perception have not tended to strenuously resist interpretations such as those Sunstein offers, some of them have provided some compelling counterweights. Most notably, Baruch Fischhoff has written for more than 25 years about the need to find a balance between the conclusion that "people are so poorly informed (and uneducable) that they require paternalistic institutions to defend them and, furthermore, that they might be better off surrendering some political rights to technical experts" and the conclusion that "people are so well informed (and offered such freedom of choice) that one needn't ask them anything at all about their desires."[21] Fischhoff has consistently warned about the former of these two lapses, concluding that some "errors" in individual decision making result from expert information proffered in a form that is "irrelevant, cluttered, incomprehensible, etc." and observing that in any event, the track record of laypeople (in this case, adolescents) shows that they "do surprisingly well, given the difficulty of the decisions facing them (e.g., intimacy, friendship, drugs, careers, identity, money, appearance). These decisions often pose hard tradeoffs, have unpredictable effects, require mastery of unfamiliar facts, and lack trustworthy information sources."[22] (An interesting speculation on possible shades of difference within the group of risk perception pioneers was provided by Bradbury,[23] who observed that in papers where Fischhoff was the lead or sole author, the prevailing attitude was that "all that anyone does know about risks can be classified as perceptions"; whereas when Slovic was the lead or sole author, public perceptions could be described as "faulty," and emphasis was placed on the notion of miscommunication from the experts to the target audience—which, no matter where the blame is placed, presupposes that information properly understood is the antidote and perception is the malady.)

As we begin to incorporate insights from evolutionary biology and neuroscience to supplement the psychological and cultural perspectives on risk perception, therefore, we should be aware that prominent observers seized upon the early contributions from these new fields as well, almost as soon as they were first made. For example, Justice Breyer claimed in 1993 that what "impede(s) rational understanding" of risk in the modern world was rational an evolutionary eyeblink ago, when wariness "helped us survive as we lived throughout much of prehistory, in small groups of hunter–gatherers, depending upon grain, honey, and animals for sustenance."[15] At roughly the same time, Terry Davies (who had recently finished a stint as EPA's top policy official) told the *Los Angeles Times*, "there are people who, for whatever reasons—early. . . toilet training or genetics or whatever—are very risk averse."[24] More recently, Ropeik (interviewed in Ref. 25) opined that "Evolution has taught us to protect ourselves immediately, before we have all the facts. . . We are biologically hardwired to fear first and think second."

It therefore seems that being able to posit evolutionary roots for "irrational" risk perception may change the complexion of the criticism leveled at the public from a diagnosis of willful disregard for "the facts" to one of a hardwired inability to adapt to risks that have changed faster than social institutions, and much faster than has evolution. Although it seems unavoidably pejorative to assert that contemporary public risk perception has changed little since the time of the Cro-Magnons, those interpreting the emerging research may well believe that it softens the blow to attribute

the "irrationality" to mental processes that have only recently become maladaptive. One cannot be accused of driving a wedge between "experts" and the masses if "irrationality" is found at the species level—unless, of course, the concern comes in the form of crocodile tears, and the implicit prescription is that risk policy must be closely held by those few who recognize and can somehow transcend the hardwiring that afflicts their fellows.

Personal Observations (I)

It would be foolhardy to deny that individuals can cling to opinions about risks that are at total variance with empirical observations and basic logic. I have no doubt that somewhere at this moment, someone is marching in opposition to a "risky" facility proposed for his/her neighborhood, all the while puffing on a cigarette—that this standard caricature of public illogic is more like a portrait in many cases. I have also seen first-hand how the "neglect factor" can frustrate attempts to provide efficient and equitable regulation of hazards, and I place some of the blame for this on public misperception. I have documented[26] that U.S. workers often face exposures to toxic and carcinogenic substances between 10,000 and one million times higher than the levels of the same substances in the general environment, and yet public interest in occupational health has remained low for decades while public concern over environmental health has continued to mount. For example, there were several prominent media accounts between 2000 and 2006 of a newly discovered relationship between occupational exposure to artificial butter flavoring in the manufacture of microwave popcorn and the grave lung disease *bronchiolitis obliterans*, which by 2002 had affected several dozen U.S. workers. In July 2007, however, when the first (and as of this writing, the only) reported case of *bronchiolitis obliterans* in a (monomaniacal) *consumer* of microwave popcorn was published, the interest increased markedly, and several manufacturers quickly announced they would remove the suspect chemical (diacetyl) from their products. As a former U.S. Occupational Safety and Health Administration official, I know we could have saved thousands of lives with a small relative transfer of public attention and funding from controls on toxic chemicals in the ambient environment to controls on the same chemicals at the point of production.

But a diagnosis of pervasive and systematic public "irrationality" requires more than anecdotes at the individual or population level. Even before attributing any mass deficiencies in risk perception to any particular cause (whether cultural, psychological, genetic, or cognitive), the deficiencies themselves need to be specified and documented. *The conclusion that the public exhibits "paranoia" (generally about environmental risks) and neglect (generally about familiar and self-imposed risks) falls short if any of the following predicates turns out to be incorrect:*

(1) *That laypeople and "experts" rank risks in very different ways.* One problem with holding up the risk rankings of experts as a gold standard against which to compare public rankings is that the two groups turn out *not* to differ so markedly in their priorities. There are indeed six hazards in Slovic's original list of 30 where the League of Women Voters' ranking and the scientific experts' ranking differed by more than 10 places; in four of those cases (nuclear power, police work, spray cans, and mountain climbing), the laypeople ranked the risk much higher than the experts did, and in two cases (X-rays and food preservatives), the converse was true. But overall, the rankings are much more concordant than discordant; eight of the 10 hazards the League of Women Voters' group ranked as most dire were also in the top 12 rankings of the experts, and eight of the 10 hazards the former group ranked as least dire were in the bottom 12 places of the experts' rankings. Statistically, I calculated that the two rankings were highly unlikely to reflect a significant intergroup difference (Spearman's rho $= 0.59$, $P < 0.002$). The same pattern was evident in a comparison of 38 hazards ranked by laypeople and toxicologists in Canada[27]—all of the toxicologists' "top 10" were also among the top 12 of the public list, and the overall correlation between rankings was even higher (Spearman's rho $= 0.77$, $P < 0.0001$). While the occasional marked discrepancies may reflect different levels of knowledge and "availability" (health professionals may rank medical X-rays higher because they have had first-hand experience with their adverse effects; laypeople may have been bombarded with news reports about the dangers of nuclear power), it certainly appears that neither laypeople nor experts systematically rank "important" risks below more lurid but less important ones. Even to the extent that the rankings differ, from the very beginning Fischhoff *et al.*[21] were emphasizing that laypeople and experts may diverge in one conceptually slight but mathematically important way in which they define risk, with the latter group focusing on long-term averages of the number of fatalities attributable to each hazard and the former group considering both the average and the "number of deaths that would arise from the most disastrous year one imagines happening in one's lifetime"—in other words, that the magnitude of a risk is a function of both its "normal" death toll and its catastrophic potential.

(2) *That the "experts" have the requisite knowledge and objectivity to serve as a gold standard of risk perception.* The field of risk analysis has struggled for several decades over the pros and cons of defining expertise and developing objective measures to accredit risk assessors, and has so far resisted moving in this direction. As a result, I believe the public has little basis for distinguishing individuals with expertise in one of the disciplines that provide important raw materials for risk assessment from those individuals (a much smaller set) with actual training and expertise in risk analysis itself. Estimating—or critically evaluating an existing estimate of—the probability and magnitude of a risk posed by a toxic substance requires expertise in how emissions become exposures (environmental transport, fate, and uptake), in how exposures become risks (toxicology and/or epidemiology, depending on the source of the dose–response information), as well as a nuanced understanding of probability and statistics. Lawyers, political scientists, economists, and (to some extent) physicians tend not to have expertise in these areas, and yet they are among the most prominent "experts" in risk analysis as described by themselves and each other. I pointed out in a review of *Risk and Reason*, for example,[28] that Sunstein (himself a law professor) relies almost exclusively on experts in fields *other* than risk analysis for his insights into which risks are trivial. I presented a brief catalog of scientific errors and misleading conclusions in his discussion (which he derived primarily from information provided by Aaron Wildavsky, a political scientist) of how "people were much more frightened than they should have been" about Alar. This is not to say that risk assessors are innocent of unwarranted forays into the technical domains of economics, law, and the like, or that relying only on experts in risk analysis would provide a clear and monolithic view about which hazards are dire and which are trivial (far from it, on both counts!)—only that when scholars, such as Sunstein, tell us that, compared to laypeople, certain observers are "brain surgeons" worthy of deference, one should ask whether the observers have actually ever studied, let alone practiced, "brain surgery."

Beyond parochial issues of specific expertise lies the unfortunate fact that "experts" are, of course, presumably subject to the same biases and foibles "hardwired" into all members of our species and may exhibit some special biases of their own. An elegant example of both of these truisms can be found in the work of Gigerenzer *et al.*[29] on "innumeracy" among physicians and other public health workers. These researchers found that all 18 of 18 experts who provided advice at AIDS clinics said that a low-risk person who tested positive for HIV had more than a 90% probability of truly having an HIV infection, despite the fact that correct application of Bayes' Theorem results in a positive predictive value of only about 50%. More than half of the counselors went out of their way to introduce their (incorrect) answer with a phrase such as "as I have already told you repeatedly, ..." I find it interesting that the literature on the biases that can cloud public perception of risk does not include (to my knowledge) any papers on the "vicious circle" of condescension and deference/intimidation that may fuel this particular kind of overconfidence bias in medical risk management. There does exist, however, a growing literature on the potential for bias inherent in the fact that, almost by definition, an expert is more likely than a layperson to have a financial stake in perceiving (or trying to convince others to perceive) a particular risk as small or worthy of neglect,[30] as well as empirical work documenting that the subpopulation from which experts tend to be preferentially drawn (white males) tends to systematically judge risks as less dire than the rest of the population.[31]

(3) *That average—or aggregate—measures of the "size" of risks are the logical ways to view them.* Because each person has a unique proximity, exposure intensity, exposure duration, and susceptibility to any given risk, the variability in risk from person to person is perhaps the single dominant risk attribute. Most introductory courses in risk assessment, I venture to say, now emphasize that single-point estimates of risk are incomplete and misleading, in part because of uncertainty but at least as much so because of interindividual variability. *And yet, the history of commentary on risk perception has single-mindedly insisted on comparing "perceived risk" to single-point estimates of "real risk."* Some of these comparisons use the "body count" (*viz.* this quote in Hampton[32] from George Gray about "overreaction": "one of the big challenges is to keep things in perspective; more people die drowning in their bathtubs each year than from mad cow disease"), while others use an estimate of average individual probability (see TABLE 1). But the latter is no more generalizable than the former; the two measures are scalar multiples of each other (average individual risk times number of exposed persons equals the expected fatality or injury count). *Although "irrationality" or insensitivity to the size of the population risk may explain public neglect of some "large" risks or "paranoia" about some small ones, the simple fact remains that anyone may sensibly fear a "small" risk (or ignore a "large" one) because it is not small (or not large) to him.*

For example, among the "dangers of summer" in TABLE 1 are shark attacks (infrequent) and bacterial contamination of meat (commonplace). Putting aside the grisly nature of the former outcome, which might

reasonably elevate one's fear, surely it is clear that while a landlocked carnivore would rationally fear the two risks in the same order as their population prevalence, a vegan surfer would equally rationally defy the population ranking and fear the larger of the two risks that *she* faces. Similarly, because the substantial majority of accidental deaths in bathtubs occur among persons younger than age 5 or older than age 65, it is rational for the rest of us to fear mad cow disease more than this particular hazard (the bathtub fatality risk is roughly 10^{-5} per year among persons age 0–4 and >65, but roughly an order of magnitude lower between ages 5 and 64).[33]

Average risk estimates can be particularly misleading when the average masks a bimodal distribution of risk, and especially when it may not be obvious what factors determine the disparity across subpopulations. Graham, who based his argument about the risk of being hit by a crashing airplane on the work of Goldstein *et al.*,[34] was correct that on average this risk was about five times higher than the one per million benchmark Congress has set for some environmental risks. Thompson *et al.*[35] showed, however, that the risk to the roughly 3% of Americans who live within two miles of an airport runway is roughly 1000 times higher than the risk to the other 97% of the U.S. population. So this example does not by any means prove (in this case, at least) that Americans manage to ignore risks on the order of 5 per million, as in fact almost all of the individuals who neglect this hazard are actually ignoring what is a *de minimus* risk to them, one well below the one per million benchmark.

It is often hard to obtain "individualized" estimates of risk, but that is in some measure a function of the difficulty experts have appreciating the breath of interindividual variation and the sensitivity of people's choices to the reality of their own situations, as opposed to an aggregate diagnosis of little relevance to them.[36] In a forthcoming paper,[37] I describe the additional difficulty my wife and I faced during various *in vitro* fertilization cycles as we tried to balance the risk of multiple births versus the risk of failing to conceive (two opposing functions of the number of embryos to transfer, a choice we had considerable discretion to make). If the population-based estimate we were consistently given (the probability of success per embryo) was higher than our individual probability, we would have been led to transfer an insufficient number of embryos, and, conversely, if our probability was higher than the population average. We "hit the jackpot" (a singleton birth) in 2000, in part because we made the informed guess (with essentially no help from otherwise excellent clinicians) that our risk of multiples was somewhat lower than the population's, and we transferred one more embryo than probably would have been optimal on average. Again, assuming that individuals care primarily about their own prospects (however much they might have altruistic interests in social optima), it would be a mistake to label an individualized risk perception as "idiosyncratic"—on the contrary, it can be irrational to perceive or to act as one would if one *was* the hypothetical average person.

(4) *That the "size" of a risk is its most important attribute.* Even in cases where the "body count" or the average individual risk accurately reflects the perceiver's own odds of dying from a specific hazard (or when the perceiver can estimate her own individual risk), why should we presuppose that fear or concern is solely a function of the odds? The pioneers in risk perception research were generally careful not to imply that dread, unfamiliarity, and other perceptual attributes of risk were "irrational," but many of the translators of that research to risk policy have advanced the notion that it is possible to fear of the "wrong" risk, which inherently assumes that magnitude should trump all other factors. I do not question, for example, Gigerenzer's 2006 finding[14] that roughly 1600 Americans died in traffic accidents during the year after September 11, 2001 who would have been traveling by plane had demand for air travel remained constant. However, to conclude further that these victims actively sought a riskier mode of transportation out of unwarranted fear of a less risky one presupposes that lower odds must evoke lower concern. If, however, *how you die* matters along with *that you die*, two probabilities may modify two perceptually different outcomes and cannot be compared the way an automaton would do so. We will never know how many of those 1600 victims of highway crashes chose to drive to their destinations because they chose to incur a larger probability of a death that would likely be instantaneous over a smaller probability of a death preceded by agonizing minutes of chaos and the awful opportunity of being able to contact loved ones before the grisly culmination of another's suicide mission. Analysts are used to the necessity of assuming *ceteris paribus* and hoping, in fact, that they are, but in the study of fatality risks, some analysts may not even appreciate that there are other things at stake.

The many different ways that a risk can culminate in a fatality may even help explain some of the most otherwise puzzling and depressing findings of risk perception research. Why on earth, Slovic *et al.*[38] assert, would subjects prefer a hypothetical investment that would "save 98%" of the 150 lives that would be in jeopardy from an airplane crash, as opposed to an investment in an alternative technology that would save

all 150 of them? I accept as entirely reasonable the explanation Slovic *et al.* proffer—that "saving 150 lives is diffusely good, hence only weakly evaluable, whereas saving 98% of something is clearly very good." (As usual, the secondary accounts of this research, notably the *TIME* cover story,[39] took a more condescending tack: "this reaction makes no sense... but there was something about the specificity of the [percentage] number that the respondents found appealing.") But if some or all of the respondents thought they were comparing two different ways to die, there may be no "illogic" at work. When I imagined "saving an entire planeload of people," I envisioned some kind of preventive technology that stops a crash from occurring, whereas "saving 98% of the planeload" seems inevitably to connote some kind of control that reduces the severity of a crash that puts everyone in peril, but allows a situation where a small minority of passengers are injured severely enough to die. Viewed this way, the second investment may seem to increase aggregate utility more than the first one does because with it, 150 people face death and 147 of them survive, compared to the preventive investment that saves a few more lives but in a much less perceptible manner. I am not suggesting that this sort of reasoning is socially optimal, but it at least may be as descriptive an explanation as innumeracy or illogic.

As discussed above with respect to individualized assessments of risk, environmental and medical risk assessment share some deficiencies in acknowledging the spectrum of individual preferences. A series of articles (see, e.g., Ref. 40) has exposed the implicit simplification in common medical guidelines, using as an example the well-known recommendation that women should only undergo amniocentesis to detect fetal Down syndrome when they reach 35 years of age (or when the odds of the fetus having the syndrome, as computed by noninvasive means, equal the risk to an average 35-year-old). This algorithm (screen when the odds of detecting the problem exceed the odds of the procedure causing a miscarriage) makes perfect sense *only* if the patient views the two grave consequences (failing to detect Down syndrome; undergoing a miscarriage of an unaffected fetus) as precisely equivalent. The two fields, it seems (see Ref. 41, especially pp. 335–339), share the conceit that it is noble to "assign no weights" to different consequences but fail to appreciate that such a stance assigns the exact relative weight of 1.000, in fact a highly restrictive assumption.

In the laboratory, a risk can be put forward as simply a probability and an outcome. In the real world of choice, not only may the outcome (even if it is a fatality) be more complicated than the researcher appreciates, but there are often multiple and subsidiary outcomes that the subjects may find impossible or illogical not to consider. For example, how do we know that the 1600 victims in Gigerenzer's drive-versus-fly analysis chose to drive on the basis of fatality risk, as opposed to some other distinction between the two modes of transport? How many people experienced (or heard about) the unprecedented delays and the new indignities of the security screening process at airports after September 11, 2001 and decided that driving was preferable regardless of any increment in risk of death? What comes off as masses "afraid" of flying may have been, to some extent, individuals who found flying unpleasant—or, as the statistics[42] on gasoline prices might have led Gigerenzer to mention, individuals taking advantage of a roughly 20% drop in the cost of automobile travel during the year after the attacks. So perhaps the temporary rise in driving was not entirely the work of "terrorists [who] strike a second time through the minds" of the public, as Gigerenzer concluded. As the more recent insights from neuroscience have begun to suggest (see next section), *risky choice is a great deal more complicated than comparing odds of death,* no matter how hard researchers may try to isolate this latter factor in psychometric studies of perception.

And so, 250 years after the St. Petersburg Paradox shattered the myth that expected monetary value (EMV) was the only rational way to assess risky choice, 50 years after the Allais Paradox showed that substituting utility for monetary value still left expectation as an unsatisfactory guide to choice, and 25 years after prospect theory, regret theory, and other refinements to utility theory were developed—many risk "experts" are still telling people there's something wrong with them if they don't perceive and prioritize based on expected population mortality (the life-and-limb analog to EMV) (see, for example, the title of Ropeik and Gray's 2002 book,[43] which refers to the "consequences" of each hazard they rank as the total number of victims, or a very recent addition to the Internet's resources on "real risks,"[44] which uses expected mortality exclusively). A reasonable goal for those aspiring to drive a wedge between public perceptions of risk and societal responses to them would be to bring the interpretation of those perceptions into the 19th century, at least.

Risk Perception Insights from Neuroscience and Evolutionary Biology

Findings and Interpretations

The articles in this volume are a valuable resource to introduce generalists and specialists in fields other than

neuroscience and evolutionary biology to the most important findings from the very recent spotlight these disciplines have shone onto human risk perception. As an attempt to recapitulate some of the themes that seem most important to this author (an environmental health scientist), I offer this brief catalog:

- Some of the factors included in the original psychometric paradigm of risk perception have an adaptive evolutionary basis and can be identified neurochemically. There is certainly a similarity, for example, between Slovic's original axis denoting unfamiliarity and the aversion to ambiguity detected in experimental studies before and since the development of fMRI techniques. If the somatic marker hypothesis (see the paper by Vorhold[45] in this volume) can explain the "comfort level" and lack thereof caused by associations with previous experience, the aversion to wholly unfamiliar risks may reflect much more than a bias against the "scary-sounding chemical name." It is even possible that people become blasé about repeated risks that they incur voluntarily because they never become associated with (or cease to be over time) negative somatic states, despite the mathematical truism that the longer you go without succumbing to a large risk, the smaller it may seem to you, even though what may really be happening is that you continue to luckily "dodge bullets."
- The "fast" and "slow" modes of cognition are not undesirable and desirable, respectively, but co-exist, have evolutionary bases for persisting, and complement each other. Tucker and Ferson's paper[46] admirably summarizes the evidence that both modes have ancient roots and serve different purposes.
- The duality of cognition and emotion is, similarly, a complementary one; both modes are necessary for optimal decision making. Vorhold[45] reviews neuro-anatomical and experimental findings and discusses the provocative assertion that a critical portion of the brain (the ventromedial prefrontal cortex) serves to link memory and other cognitive processes with emotional ones. A great challenge in risk regulation remains that of maintaining a conceptual distinction between "objective" and "subjective" measures of risk, without devaluing the latter (or worse, imposing emotion-laden constraints upon the assessment in the guise of "sound science"). If, as Vorhold reports, it is not only the existence but the integration of these two processes that allows one to cope with the panoply of decisions we all face, then a social decision-making system that imposes an institutional firewall between the two[47] will forever remain confused by a public that naturally interweaves them.
- Humans have a deep-seated drive for fairness and a finely-tuned system for distinguishing outcomes and processes on this basis. In my view, the convergence of the evolutionary insights into reciprocal altruism and moralistic aggression (Tucker and Ferson[46]), the experimental results from the "Ultimatum Game" (Sanfey and Chang[48]), and the fMRI findings about the role and function of von Economo neurons (Watson[49]) represents the most well-developed template for future multidisciplinary research into risk perception. The elegant research into the outputs from, and underlying neural bases for, the Ultimatum Game, when subjects believed they were facing a human versus a computer opponent, points to the paramount role of social justice in the evaluation of risky choice. The obvious hindsight question in light of this emerging research is: "how many of the 'insights' of the psychometric approach to understanding risk perception were compromised by not controlling for fairness?" Put another way, have we concluded that people are *afraid* of various "small" risks when in fact they are *disgusted* by the conduct that produced the risks? To ask one particular question out of a myriad, what if I say I am more concerned about mad cow disease than about drowning in a bathtub, not because I have the rank order "wrong" but because only the former risk evokes any moralistic aggression (right or wrong, the sense that someone is not being careful enough about safeguarding the food supply) and because I am expressing social concern rather than individual fear?
- Hormonal influences on risk perception add yet another complicating dimension—that of *intraindividual* variability—to the statistical analysis of expressed preferences. Watson[49] reviews recent findings that, among others, levels of serotonin and oxytocin can influence departures from risk neutrality, in either direction. Although this area of research raises many intriguing questions, it suggests to me especially that any individual's perception of risk can literally change from one day to the next—which raises questions about the representativeness of risk perception and ranking exercises that (by design) tended overwhelmingly to assess each subject's perceptions at only one point in time.

- We have evolved various systems for evaluating risky choice, not for evaluating "risks in a vacuum." The recent neuroscience experiments and evolutionary insights tend to explore how we form judgments about uncertain outcomes, not how we assign a level of fear or worry to an isolated situation. This contrasts somewhat with the earlier psychometric research, much of which appeared to find merit in querying subjects about risks and assumed that the responses were informative about "pure" attitudes toward these risks. I am admittedly extrapolating more here than in the five previous paragraphs, but it seems to me that the newer investigations realize more clearly that humans do not *merely* fear or not, tolerate or not, neglect or not, but we weigh the response to a risk inexorably by considering the choices it foists upon us. Thus, the new direction risk perception research has taken may be most valuable not only in helping understand how we choose but also that we choose—even though that insight may call into question (see subsection on "Choice Perception" below) the conclusions gleaned previously from responses to questions about risks, which may in fact have been responses about choice.

Personal Observations (II)

The "second wave" of empirical investigations into risk perception has enriched and complicated the picture in a way that calls for more creativity and less hubris as these insights are transferred to risk assessment and management policy. We are simultaneously beginning to learn both that humans have a "collective rationality" in our responses to risky choice situations and that we vary among each other and over the course of each individual life span in the absolute and comparative magnitude of our perceptions of each risk we encounter. The challenge, in light of the revealed complexity of risk perception and the continuing examples of individual and social responses to risks that are hard to defend, is to temper the impulse to condemn the public as well as the impulse to ascribe some hidden rationality to any seemingly bizarre reaction to risk. The heart may have reasons that reason knows not of, as Pascal famously observed, but the heart can still be misinformed. My own attempts to find the balance Fischhoff *et al.* pointed toward[21] focus on four themes that may help link the two waves of risk perception research and policy together:

(1) *Variability.* In order to discern whether individuals are responding to a risk "out of proportion" (in either direction) to its social magnitude, or to their individual probability of harm (or both), we need to appreciate and amass information on both the interindividual variation in risk itself and the variation in preferences across individuals. The former task is more one of education of laypeople and experts than one of methods development or data collection—the raw material to estimate the degree to which any respondent's risk of a shark attack (for example) diverges from the (small) population-average risk is generally there for the use. Although the information on individual susceptibility is not often readily available, the revolution in individual genetic information is changing this situation rapidly, and, in some cases (see the *in vitro* fertilization example above), personal medical history may suffice.

The breadth of the population distribution of individual utility functions, of minimum offers accepted in the Ultimatum Game, or of the nature and extent of brain activity stimulated in the ventromedial prefrontal cortex, however, remains to be estimated. This, in part, may be a result of the basic unit of analysis of evolutionary science (the population or species) and of anthropology (the cultural group), which naturally tend to view interindividual differences as "noise" or as less important than the central tendency. But we will never know how much fear or confusion truly gets in the way of responding to risk in proportion to our individual circumstances and preferences until we design risk perception studies that elicit those preferences and estimate those circumstances on an individual basis.

(2) *Speed and Accuracy.* The neuroscientists conducting the research have not, to my knowledge, equated the speed of a particular response to its accuracy or usefulness, but that hasn't stopped the "experts" from popularizing the notion that rapid risk perception is the villain here. As the *TIME* article[39] puts it, "a primitive, almond-shaped clump of tissue... [the amygdala] ... reacts the most dramatically... It's not until a fraction of a second later that the higher regions of the brain get the signal and begin to sort out whether the danger is real... Setting off your internal alarm is quite easy, but shutting it down takes some doing." I work on risk analyses that take months or years to write and hours to explain, so I am quite familiar with the pitfalls of snap judgments about risk and the virtues of slow incremental discussion. Nevertheless, something seems too facile about assuming that the slower "System 2" serves to save us from the mistakes that the "primitive" system makes more quickly. Tucker and Ferson[46] explain how each cognitive system, regardless of its speed, contributes to an overall rich picture of risk and response. A mundane analogy may help shed light on the variety of "intelligences," distributed unevenly across members of the population, and how

rapid cognition may be every bit as accurate as more deliberate modes. On tests of mental arithmetic and recall of memorized facts, I probably perform at the top percent or two of the population, but on the basketball court, I frequently find myself wishing I was not performing at "33 rpm" compared with teammates and opponents. The speed with which better players can correctly process information about what empty spot to pass the ball to, or what spot to move to in order to snag the rebound of a shot that has just left someone's hand, should evoke amazement, not scorn. By the time my otherwise-speedy brain can see what to do next (assuming the rest of me was fast enough or skilled enough to actually put my perceptions into practice), the opportunity to make the right decision is already over. As Watson[49] says so well, "fast, automatic, effortless decision-making" can be found in our species and can supplement and guide more deliberate cognition—that is, when and if time permits both systems to contribute. It would be a mistake, I contend, to let the conclusions of the "slow" system trump the initial judgments merely because they take longer to formulate.

(3) *Choice Perception.* The psychometric paradigm was supposed to teach us that risks are more than probabilities and consequences; that they evoke judgments about individual control, dread, and other aspects that can elevate "small" risks above "large" ones without violating any axioms of rationality. But as soon as we admit that people have difficulty disentangling perceptions of risk from perceptions of the social contracts that risky situations can violate—as the literature on the Ultimatum Game and other work reviewed in this volume makes clear—it seems to me that we simply must broaden our sights beyond attributes pertaining only to risk and include in our theory of perception a set of attributes pertaining to *choice* itself.

Faced with a risk, we can consider not only choices we can make to accept it, avoid it, or alter it, *but the choices others have made to bring us to this point.* Choice, especially when correctly viewed (i.e., realizing that "we haven't made a choice yet" is an oxymoron or a trap—in fact, the choice was made to accept the *status quo* for the while), is a much broader domain than risk alone. In particular, it involves consideration of *costs* and *alternatives.* Is it possible (I ask with no pejorative tone) to ask laypeople or experts to rank hazards according to how risky they appear, without receiving a jumble of perceptions about probabilities and affect, and also about costs of control and the opportunities to reduce the risk? Were the League of Women Voters' respondents in the original Slovic *et al.* survey revealing a *fear* of nuclear power or (and) a *concern* impelled by their views about alternative sources of energy or conservation as a preferred strategy? That risks of equal magnitude and consequence can evoke vastly different reactions is hardly a novel insight—Fischhoff[50] reminds us that 20 years ago, a very prominent guide to risk communication warned about the dangers of comparing disparate risks generated by disparate choices of others—but the practical risk communication literature has emphasized the affect created by the source of the risk, rather than the salience of the ways to control it.

In my own case, I would probably rank "driving while using a cellphone" high on any list of risks I was asked to rate, unless the investigators were extremely careful to instruct me to consider only the odds of harm being done. I have no idea whether it is my amygdala or my insula (see next paragraph) that actually "lights up" when I see a driver in my rear-view mirror talking on a cellphone, but upon some reflection, I am reasonably sure it is not fear that dominates my perception of this risk. I am thinking primarily not of any possible danger to myself but about the behavioral impulses that would cause someone to "multitask" in a way that seems almost polymorphously perverse to my scolding psyche. I admit that I wonder what inane chatter someone deems more important than concentrating on the road, am annoyed that I may have to be extravigilant or drive faster in order to compensate for his decreased reaction time, and engage in other ruminations I'm reluctant to commit to print. *If this complicated, perhaps pathological, mixture of anger, concern, and ideology is evoked by a common interpersonal situation also involving risk, how much are we oversimplifying to attribute all attitudes about a risky choice to fear or to innumeracy?*

Recent publications employing the Ultimatum Game show how one of the attributes of choice—reaction to the unfairness of some situations when human agency is involved—can override "rational" weighing of risk and return. Although accepting an unfair offer is efficient by both the Kaldor-Hicks (gains to beneficiaries outweigh losses to victims) and the Pareto (no one is made worse off) criteria, majorities of subjects from each of several dozen cultures[51,52] consistently reject them, and the more recent papers in the neuroscience literature suggest strongly that a particular area of the brain (the insula) is involved in reacting both to physical stimuli (e.g., odors) and to behaviors that we articulate as "disgusting." This example of "moralistic aggression"[46] is at least something risk perception experts have to reckon with (even if they privately view it as "irrational"), and it probably evinces various important efficiencies when the long view is taken (the society's welfare can eventually increase if those who

forego reward help to bring about more fair play by those who hold the initial resources). Simply being deep rooted is not necessarily an indication of a behavior we should respect, and it must be said that most of us have moved beyond disgust at behaviors (e.g., intermarriage, conception via *in vitro* fertilization) once regarded by many as repugnant. But the aversion to things disgusting has such apparent physiological benefits[53] and is so integral to our core moral values (it is surely no coincidence that we describe cheaters as bad apples, rotten eggs, and stinkers) that we should be especially careful not to try to assuage (or trample over) people's "fears" about risk when they are in fact concerns about the socially suboptimal behavior that creates the risk.

The lack of information about the costs of controlling many risks, especially environmental ones, further complicates matters. If the true (or perceived) cost of reducing a risk is low, an otherwise tolerable risk may sensibly be viewed as worthy of concern (which can then be misconstrued by others as something evoking fear). Conversely, a risk that is (or perceived as, or marketed as) prohibitively expensive to control may be an object of neglect, regardless of its magnitude. Because risks and costs are mirror images of each other, one would hope that analysts would provide estimates of comparable accuracy, precision, and transparency on both sides of the ledger. This ideal is far from the case.

Risk scientists ever more frequently provide quantitative depictions of uncertainty rather than ranges or multiple point estimates and, although with more success in terms of exposure than dose–response, also increasingly provide information about interindividual variability in risk. Whether thanks to these refinements or thanks to common sense (as in the self-assessment of risks, such as shark attacks and food poisoning, based on one's own known exposures and behaviors), individuals are increasingly well armed to appreciate that their own risks may diverge from those of the population or average member thereof. But on the "cost side," the public as well as decision makers are generally at sea; regulatory economists infrequently provide estimates of uncertainty when they assert how much it will cost to control a hazard[54] and virtually never provide information on how those costs might be distributed across different subsets of consumers, producers, and customers in the economy.[55]

The literature documenting large errors of overestimation (and occasionally of underestimation) when *ex ante* estimates of regulatory cost are compared to *ex post* realizations attests to how uncertain the published estimates really are[56,57] and thus how dramatically people may misperceive those costs. It seems reasonable to expect that such errors commonly result in people believing that a risk can be neglected because society is doing all it reasonably can to control it, when in fact inexpensive controls are not being implemented, or in people exhibiting extra concern over a risk because it is "needless" when in fact the costs of control would be overwhelming.

And however large or small the *total* costs may be, people (with one caveat; see next page) have no basis for "individualizing" the costs they themselves will bear if a risk is reduced or eliminated through regulation, other than by making the first-order (and quite precarious) assumption that every citizen will share equally in the economic burden. People may not want to pay more for safer chicken and may content themselves that the risk of bacterial contamination is tolerable because the risks under the status quo do not justify the costs of control, but they would be misperceiving (and "neglectful") if it turned out that all of the costs of increased controls on poultry production were borne by some producers rather than all consumers, as might occur if more efficient producers were to drive out the less efficient ones, with no impact on consumer price. The costs may still outweigh the total benefits in this case, but for consumers, control would be a win/win situation and against self-interest to oppose.

So to build a complete theory of risk perception (at least for the environmental, health, and safety risks controlled by government intervention), we need good information on cost but also a theory of "cost perception." The literature on the perception of *individual* expenditures (see, e.g., Ref. 58) provides a foundation for this, but needs to be supplemented with studies of how individuals perceive social expenditures, particularly regulatory ones. Do, for example, laypeople understand that many "costs to producers" are costs to themselves as consumers (the coal plant buys the scrubber but pays for it by increasing the price of electricity)? On the other hand, do they understand that some gains in health and safety can come at no cost to consumers, despite the large social costs and dislocations accompanying some regulatory programs? Do laypeople think that all estimates of the cost of government programs are part of "bait and switch" schemes that underestimate costs (as might be their intuition about the costs of military interventions, which seem to be more expensive than advertised), or realize that regulatory cost estimates tend to come with the opposite bias?

The asymmetry between how we think (and how much we think) about the benefits of risks reduced, as opposed to the costs of risk reduction, even extends to how we treat fear on both sides of the ledger.

Advocates for increased controls on environmental hazards may be motivated by (or may be fomenting) aversion to potential harms, and they may misperceive those harms as more dire than they actually are. Or, they may be missing the "bigger picture" that the risks are distributed and do affect them disproportionately but would harm more people if taken "out of their backyard." Experts, as we have seen, do not hesitate to call this paranoia, or the engine driving a vicious circle of self-interested behavior. But there is no analogous campaign, or even analogous terminology, to describe advocates *against* controls on risk, who are averse to potential *economic* harms to themselves, as irrational or paranoid or as belonging in a museum diorama of Neanderthal life—and no helpful books, articles, and Web sites offering to "explain costs as they really are" (i.e., trivialize them). *Fear of cost can derange the mind as powerfully as fear of death or disease can,* but for some reason it does not carry the same baggage, and this asymmetry can skew the entire regulatory system against some beneficial interventions. It is true that on the cost side, the persons most afraid may well be the ones perceiving the costs *to them* most clearly, as the potentially regulated parties often know very well that they will be disproportionately affected. Still, the fact that some individuals who most fear particular "small" population risks are the ones most affected has not seemed to deter experts in risk perception and communication from using population risk (analogy: total cost, not the cost to the most affected) as the gold standard.

So, given that risks are inextricably bound up in choices that involve changes in physical harms and economic harms, we need a broader theory of "choice perception," and paying attention to all the economic analogies to the more well-studied physical harms would be a good place to start. The core of a theory of choice perception would be the observation that "acceptable risk" determinations are not really about whether the risk is acceptable but whether it is *acceptable to accept the risk*—in other words, whether the risky situation is or is not the product of a stable and fair equilibrium among informed parties.

(4) *Perception versus Belief.* As we continue to add to the list of factors that people take into account when confronted with a risk—probability and magnitude, but also various qualities of the outcome, costs of control, alternatives for providing the benefits at lower risk, timing of costs and benefits, fairness, moralistic aggression, and on and on—it appears that people are doing much more than calculating odds; they are constructing an "edifice of belief."[59] The very existence of the term (and the field) of "risk perception" implies that these beliefs have pedigrees—that it is the right and the responsibility of experts to explore which beliefs conform more or less well to some objective reality, which people or groups are seeing more clearly, and which are seeing "through a glass darkly".

I find it curious that we do not treat all beliefs this way, to say the least. Any example chosen for contrast would alienate some readers, but I find instructive the analogy between environmental risks and a social issue such as gay marriage. Obviously, there are many in our society who regard the prospect of homosexual couples enjoying the legal and financial benefits of marriage as anathema. Individual opponents may say it is wrong and have no (need no) reference point other than some Biblical verses to undergird their conclusion. But if pressed for additional rationale, many, perhaps including the most religious, would refer to untoward consequences if this right were liberalized—that is, they would invoke a *risk* of some adverse outcome. Presumably, they would be no more interested in the countervailing effect of the tens of billions of dollars foregone in voluntary economic activity between couples and the catering and honeymoon industries[60] than the founders of the Environmental Defense Fund were interested in the transactions between producers and users of DDT that were curtailed by the ban on that substance. Although the informal public discourse doubtless includes proponents of gay marriage trying to convince decision makers and opponents that their dire risk estimates are exaggerated ("misperceived"), I know of no scholarly literature dissecting the "innumeracy" or the misplaced priorities of opponents. These arguments play out within the crucible of politics, law, and ethics, not of mathematics and science, even though scholars could certainly estimate (with uncertainty) the harms that could occur were policies to change.

Both kinds of issues involve both quantifiable risks/benefits and ideology. It is possible that the success my risk assessment colleagues have had in making body counts and "10 to the minus X" numbers so prominent has contributed to a relative over-reliance on quantification in environmental policy (or, depending on how you look at it, to insufficient reliance on cost–benefit quantification in many other areas of social policy).

Conclusions

How can we strike a better balance between undue sarcasm about, and undue deference toward, the risk perceptions of the public? How can the quantitative estimates of risk continue to play a central role so that

experts will not be shy about pointing out discrepancies between facts and fears based on true misreading of those facts, but not run roughshod over all the other aspects of risky choice that a rich theory of perception would also honor? One prescription would be simply to regard the "everything else" aspects of risky choice (all the factors other than the body counts) as items to consider *before* moving on to make claims of paranoia or innumeracy. If conclusions about primitive, reflexive reactions to risk or "early toilet training" were more of a last resort than a reflexive judgment of their own, the public and experts might be better able to reason together. There should be something quite humbling, in my opinion, about the findings[61–63] that (respectively) chimpanzees, persons with brain lesions, and persons whose brains had been disrupted temporarily by magnetic fields respond to the Ultimatum Game according to the economists' model of rational behavior, whereas uncompromised humans do not.

I also have a more ambitious prescription, aimed at improving risk assessment and management, which might also improve risk perception and communication. In a forthcoming paper, I will propose that for much of their work, environmental and health regulatory agencies should turn the 1983 National Academy of Sciences *Red Book* paradigm[47] on its head, and direct risk assessors to synthesize scientific and economic information in order to evaluate specific risk management options. This would contrast starkly with the status quo, in which assessors see their role as dissecting risks without regard to the alternatives for reducing them and who hand off their findings to a risk management process in which others look for solutions. Unfortunately, those others often convince themselves that setting a single-substance exposure limit *is* a decision (in my view, this pronounces what exposure would yield an acceptable risk but does not necessarily do anything to see if or how that goal is met). This new "solution-focused risk assessment" paradigm would emphasize optimal (and iterative) action to reduce risks over the current tendency to delay action until the risk is thoroughly understood—in essence, while the most complete risk assessment can at best tell us what to fear, a solution-focused risk assessment can help tell us what to do.

Uncertainties in risk (and/or cost) are in no way fatal to this paradigm; rather, they reveal the possible costs and benefits of taking no action, compared to alternative futures in which a specific action is taken. When decision makers and the public finally begin to understand that the answer to almost any question "is this the optimal risk management decision?" is a probabilistic one ("probably" being a more honest answer than "yes"), they will begin to demand uncertainty analyses rich enough to identify the optimum and the consequences of choosing incorrectly.

A solution-focused paradigm would also jump-start the transition from studying risk perception to choice perception. When the gold standard becomes "what does our best analysis say about the costs and benefits of the following 30 potential actions?" (rather than about the body counts associated with a list of 30 hazards), then experts will be able to study how laypeople perceive risky choice in a way that allows affect, fairness, cost, and many other factors to play the central role in theory that they already play in practice. As the raw materials for gauging perception improve, we need a risk assessment and management process worthy of the discoveries being made; discouraging the aimless dissection of problems in favor of analysis of choice may reduce the corrosive influence of disagreements over "what to fear."

While this article was in press, psychologist Steven Pinker wrote a powerful essay[64] on how the neuroscience research and evolutionary study into the origins of morality might change our views of moral dilemmas. He concludes that even though some aspects of the moral sense are "products of our biological makeup and have no objective counterpart in the world," morality is no "collective hallucination," but a sense that can impel us to "focus on goals we can share and defend." How apt his observations remain if we substitute "perception of risk" for "the moral sense." Studying why we perceive should not jeopardize our prospects for managing risks more efficiently and fairly, if Chekov's optimistic view (quoted at the end of Pinker's essay) is valid: "Man will become better when you show him what he is like."

During the first wave of risk perception work, we have gained insights but also created rifts between the public and decision makers. Perhaps in contrast to Sisyphus, the next rock we push up the mountain will reach a ledge that affords us a chance to stop fighting with nature and expend our energies more productively.

Conflicts of Interest

The author declares no conflicts of interest.

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