

Are We Making a Difference?
An Essay in Honor of Howard Kunreuther

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Howard Kunreuther's career has been dedicated to producing research and remedies, aimed at helping people to make better decisions. That commitment has led him to uncommon research and collaborations. He has conducted field surveys, qualitative interviews, and laboratory experiments, in addition to more usual economic modeling and theorizing – in order to triangulate on complex problems. He has spent time with insurance salespeople, regulators, and executives – in order to understand problems more fully. He has written papers satisfying the editorial standards of economics, psychology, disaster research, and risk analysis – in order to get the work before the most relevant audiences. He has convened unusual working groups. He has provided missing pieces to similarly spirited work that others have undertaken, myself included.

How can we tell how well our great experiment is working? We've had a good time intellectually and made some great friends. Universities have thought enough of the exercise to indulge us with positions and resources. People attend our workshops, contribute to our journals, and study with us. We get to opine, here and there – in the news media, before government, on advisory panels, and in boardrooms. These experiences, in themselves, should constitute satisfying

careers. However, unlike Marx's philosophers, we want to change the world, not just explain it.

Yet, we are not consultants, entrepreneurs, or legislators, positioned to see our grand ideas through to practical realization. As a result, unless we leave, neglect, or redefine our day jobs, others must apply our insights. Understanding what they are doing is essential to evaluating how we are doing.

Unreasonable friends and enemies

Being recognized is no guarantee of success. There are honest brokers in the world of deeds, eager for scientifically sound proposals that they can carry forward. But there are also practitioners with foregone conclusions, looking for experts whose work they can invoke, in order to justify positions that they have already adopted. They may care little about the quality of our work, as long as it points in their direction. If our fame advances their cause, then they may help us to find better speaking engagements, better luck with our op-eds, and better consulting opportunities. But we are just means to their predetermined ends.

How can we tell whether we are being "kept" by the powerful, rather than getting well-deserved audiences? One positive sign is finding that our supporters have followed a discovery process paralleling our own, independently discovering a behavioral regularity that we have documented and explained. A second positive sign is finding that our supporters care about the details of our work and the science underlying it – and not just about our convenient truths. A

third positive sign is finding that our supporters are committed to empirical evaluation, meaning that they care about how well a program works and not just that it exists, bringing them wealth or power.

Just as acclaim can be embarrassing, if it means that we are being used, so can scorn be an honor, if it means that our ideas are so powerful that they must be attacked. Social scientists study basic human functions, which others are already addressing. Unless we fashion roles for those incumbents, we threaten them. A community activist once told me that she knew that her views mattered, when her life was threatened. Without worthy enemies, perhaps we're not saying much.

Our program?

If we get the chance to implement our work, we then need to ask whether the programs that emerge are faithful to our ideas. Implementation always requires some adaptation. However interdisciplinary our research groups might be, they are unlikely to include all the relevant expertise. For example, Howard's early work on the National Flood Insurance Program found that, despite having many attractive features, the program had not adequately addressed insurance agents' compensation. Without appropriate commissions, agents had less incentive to sell a policy, however attractive it might be for their clients.

As mentioned, evaluation research is essential, if one cares about a program's impacts (and not just its existence). Evaluation research begins by

assessing how faithful a program's implementation is to its underlying concept. That assessment keeps programs from being judged unfairly, based on experience with flawed imitations bearing their names. For example, the failure of poorly marketed flood insurance does not prove that insurance can never work, as a strategy for internalizing costs, reducing moral hazard, and sending price signals. Similarly, the failure of poorly executed risk communications does not prove that "information doesn't work." On the other hand, if a program cannot be implemented faithfully, then it may just be an ivory-tower idea.

As a program becomes more complicated, its implementation requires more and more kinds of expertise – to the point where the big idea may be just a banner under which multiple specialties ply their crafts. If so, then we should be proud of having allowed those experts in, even if the final product is less distinctively our own. Complex programs provide less clear tests for any contributing discipline or theory. If they don't succeed, then there are many places to assign the blame. Making such excuses can give us a second chance. If others make them, they can keep us from getting a first chance. If we don't get to try, it is hard to know whether our ideas lack merit or we lack the clout needed to get them a trial.

Reinventing our programs

Ensuring that our ideas are implemented faithfully requires collaboration with practitioners – observing and addressing the issues that arise, as they try to

make our ideas work. The greater our insight, the more it may lose in the translation, as practitioners struggle to understand and implement our novel concepts. Such a *curse of insight* can mean that the more we impress our colleagues, the more time we will need to spend, helping practitioners.

In addition to distracting us from our day jobs, such collaborations could cause discomfort, as practitioners assume ownership of our programs. Those practitioners may be enthusiasts, wanting to add their private stamp to our work, or they may be skeptics, trying to make the best out of programs that have been forced on them. They may need to make changes for change's sake, in order to demonstrate their value. They may misunderstand our program and inadvertently distort it. They may add vital elements that never occurred to us. They may just let our program die, convinced that their organization wasn't committed to it anyway. As these processes evolve, we will need to decide whether to assert or disclaim ownership, for programs that aren't quite what we had in mind.

Reinventing our institutions

In conversation, I once heard the president of a Pittsburgh foundation say, "When historians look back at our era, they will wonder why our universities were so little engaged with the problems surrounding them." Howard has been a leader, in creating exceptions to that rule. For such exceptions to be sustainable, however, they must be more than just indulgences for tenured faculty.

Sustainability should be more likely when

(a) We demonstrate that applications spawn interesting basic research, revealing new phenomena that complement those arising endogenously from normal science. For that to happen, we must domesticate these phenomena, so that they can be studied by disciplinary research.

(b) We change the currency of our disciplines, so that they accept the unconventional methods, collaborations, and products that applications require. For that to happen, we need to make the case that such work is unusually difficult – and not a refuge for those without the taste or talent for basic research.

(c) We get our best students to pursue applications, for the intellectual challenge that they bring (and not consulting fees or political influence). For that to happen, we need to get students with these tastes admitted to our programs, then effectively employed.

Sustainable change is likely to be maddeningly slow. People don't change their work patterns very quickly. Arguably, they shouldn't try, lest they lose the internal checks and balances that their disciplines have taught them.

Changing the Talk – and Perhaps the Walk

Whereas changing how the world works might be our aspiration, our success depends on forces beyond our control, including organizational and electoral politics. Changing how the world talks, however, might be within our grasp, if we can make our research accessible to people who might use its results.

Communicating with non-specialists requires listening, so that we can provide relevant work in comprehensible terms. It may also require reining in our own natural enthusiasm. Outsiders have little way of knowing the controversies within our disciplines nor the assumptions that their members all make. Unless we reveal our internal discourse, we may instill undue confidence – or paralyze those who observe the confident, conflicting claims of different scientists.

Social scientists are often asked for sweeping generalizations about human behavior. Sometimes, they can provide them (e.g., “people rarely panic, unless in confined spaces with limited egress and strong time pressure”). Often, though, behavior depends on the interactions of multiple processes (e.g., “whether people prepare for disasters depends on their resources, their access to relevant information, their trust in information providers, their ability to act, their dependence on others, their need to support others, ...”). In such cases, simple answers are misleading ones, as are policies based on them.

As a result, our communications succeed when they encourage more thoughtful, respectful, informed discourse about the challenges to effective decision making. It means fostering a realization that people have both strengths and weaknesses; that intuitive choices are sometimes good enough, while informed ones are sometimes inadequate; that we need evidence and not just assertion; that people differ in their intellectual and material resources.

The Science and Practice of Decision Making

A distinctive feature of our shared science is that it considers both the decisions that people face and the resources that they bring to them. It also considers people and their decisions in terms that reveal how sensitive choices are to human frailties and where help is most needed. Those comparisons embody the tension between economics, whose view of human performance can be unduly bright, and psychology, whose view can be unduly dour. Because disciplinary writing contains so many implicit assumptions, realizing the value of that tension requires more than just reading one another's work. Rather, it requires communicating directly and creating joint projects.

Much of our joint success would not have happened without Howard's ability to convene people in ways that allowed them to learn from one another. One secret to his success has been his recognition that poor decisions are a sign that life is complicated, not that people are hopeless. Among the many meanings of "irrational," Howard's embodies a faith that people can make better decisions, given proper support, and a deep commitment to providing it. Expecting early, easy returns on this investment would be "irrational."

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