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FORUM

Whither universities?

In "A New Model for the American Research University" (*Issues*, Spring 2015), Michael M. Crow and William B. Dabars argue that public and private research universities are stuck in a pattern of incremental change, when the times call for radical reform. Research universities, long the gold standard of higher education in the United States, must be scaled up and freed from current design constraints that hamper their ability to produce the kind and quantity of education and research the nation needs at this moment in its history. The new model they describe advocates a dramatic expansion of enrollment at research universities to encompass the top 25% of the nation's most academically talented students instead of the 5 to 6% they educate now. While noting research universities' contributions to the knowledge economy, Crow and Dabars criticize the research enterprise in general for being "carried out largely in isolation from the socioeconomic challenges faced by most Americans." Thus, their model organizes research—more of which they feel should be cross-disciplinary—around societal problems rather than the traditional disciplines. Perhaps the most serious design flaw they see in today's research universities is the academic department, which, they maintain, impedes the flow of interdisciplinary collaboration within and beyond the university's walls.

Their recent book from which this article is drawn, *Designing the New American University*, comes at a time when the nation's research universities are searching for new models adequate to the realities they face. This is one of its appealing aspects: The authors offer a bold prescription for change, buttressed with a historical perspective on the evolution of the research university; a strong defense of the role of the arts, humanities, and social sciences; and recent theorizing about knowledge and knowledge institutions. They also provide a valuable real-life example of their model, reflected in the changes that Crow has orchestrated as president of Arizona State University (ASU) over the past decade or so. Anyone interested in alternative futures for the research university will want to follow this ongoing experiment in institutional redesign.

It is clear that Crow and Dabars' model is tailored to what they regard as the nation's 100 or so principal research universities. What is not entirely clear is whether they intend their model to be for a few of those institutions or for all of them. Although they write that restructuring initiatives are "necessarily sui generis because at bottom there should be nothing generic about institutional design," their title and much of the book suggest that their model has wide applicability. But there are at least two reasons for caution.

First, the overwhelming majority of public research universities are not, as the authors argue, deliberately curtailing

enrollment as a strategy for ensuring their elite standing in national and international rankings. The University of California and public research universities like it are prepared to grow in order to meet student demand and national needs. Yet scaling up the proportion of students they enroll to 25%—an enormous increase—would serve neither students nor institutions. Students can choose from a wide mix of excellent colleges and universities, including ones that offer opportunities for undergraduate research; there is no reason to believe that research universities are the only avenue to a 21st-century education. The costs of expansion would be enormous, at a time when the moderating of the Great Recession has done little to ease the fiscal struggles of higher education nationwide. Per-student funding in the states is still 27% below what it was in 2008. (The University of California system now receives the same level of support from the state that it did in 1999, even though it educates 83,000 more students and 42% of its undergraduates are low-income Pell Grant recipients.) If current national budget trends continue, according to the Pell Institute for the Study of Opportunity in Higher Education, in 10 years there will be states in which higher education receives no funding at all. Innovations and adaptations—massive online open courses, or MOOCs, for instance—have a role in addressing this fundamental problem, but a real solution requires significant new investments of money. It is not just a question of organization and will.

Second, although the university research enterprise can always be improved, it does not need to be reinvented. Cutting-edge, cross-disciplinary work is thriving as never before at U.S. universities, and so are partnerships with governments, regions, and private

REBECCA KAMEN

Rebecca Kamen's artwork is inspired by the process of scientific discovery. Her investigations of scientific drawings and writings from rarely seen manuscripts form the basis of her artwork. Informed by wide-ranging research into cosmology, history, and philosophy, her work reflects how the ideas of science permeate all areas of human endeavor—including art.

Through residencies and research opportunities, she has investigated rare scientific books and manuscripts at the libraries of the American Philosophical Society, the Chemical Heritage Foundation, and the Cajal Institute in Madrid, using these significant scientific collections as a catalyst for the creation of her work. She has also conducted research at the Center for Astrophysics at Harvard University, the Kavli Institute at Massachusetts Institute of Technology, and the Neuroscience Program at the National Institutes of Health, where she was artist-in-residence in 2012.

Kamen's artwork has been exhibited nationally and internationally. She is the recipient of many awards and fellowships including a Chemical Heritage Foundation Travel Award and a Pollock-Krasner Foundation Grant. As professor emeritus at Northern Virginia Community College, her research and lectures explore how the arts and creativity can enhance innovation and the understanding of science. Her exhibition, "Fundamental Forces," is on view at the National Academy of Sciences, Washington, DC, through July 6, 2015.

—Alana Quinn

Images courtesy of the artist.

REBECCA KAMEN
From *Art/Science*
Butterflies of the Soul, 2013
Acrylic on mylar
60 x 32 x 7 inches

industry. Further, a reorientation away from basic research and toward more attention to broad societal challenges or specific local needs is an idea with profound implications that should be carefully considered. Since the federal government's decision at the end of World War II to make universities the center of the nation's research enterprise, the United States has come to rely almost exclusively on these institutions for the fundamental discoveries on which the flow of new knowledge and new applications depends. Moving toward a strongly problem-solving approach could diminish that role, which has yielded spectacular dividends for society.

Crow and Dabars offer many ideas for change that are stimulating and useful. But we should also keep in mind how inventive and resourceful research universities have been in overcoming the obstacles that strew the path to innovation. They still are.

Richard C. Atkinson
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Patricia A. Pelfrey
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The public research university in its post–World War II form needs reinventing, and no other university leader has set about this with the ambition and focus of Michael Crow, in the company of his colleague William Dabars. They have finished work on one of three parts of this reinvention. But there are two unfinished parts that are still causing problems.

The finished piece is a new version of open admissions. The New American University (NAU), modeled on the authors' home institution, ASU, offers a place to all "academically qualified students," where "qualified" is defined with democratic expansiveness. One expression of this policy is that ASU accepts 80.2% of its applicants and is proud of it. Another is ASU's extensive use of online education, signaled recently by a controversial partnership with edX to offer General Education courses for ASU credit to students who have not actually been admitted to ASU. Crow and Dabars' underlying principle is that "intelligence is distributed throughout the population" and will take forms to which standard admissions procedures are blind.

On the level of economic pragmatism, they argue that their type of neo–open admissions is the only way that the United States can reverse its descent into the world's richest "undereducated" society. On the level of social ethics, they break with the university's dysfunctional attachment to selectivity, in which a university's greatness is measured by the proportion of people it excludes from the start. Public university flagships have become as exclusive as the Ivy League a generation ago, while the Ivy League schools, now rejecting 90 to 95% of applicants, have become floating islands of educational wealth with little resemblance to the rest of the sector. Crow and Dabars associate this exclusion not with quality, but with scarcity: "scarcity is the brand that our elite universities are selling." The country desperately needs the end of scarcity—and of tokenistic diversity—in high-quality higher learning, and few people see that as clearly as the authors.

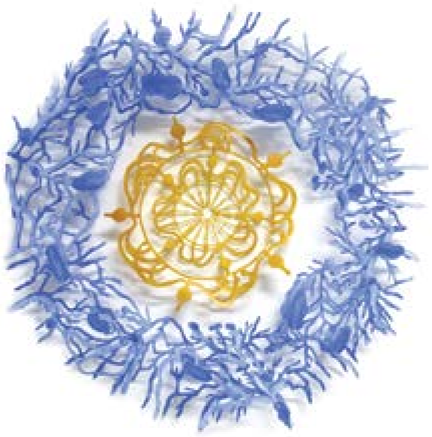
The second part of the reinvention is moving from factory-style throughput to mass "higher learning" (Thorstein Veblen's term, not theirs). The social value of commodity skills has dropped, but what in the NAU model offers intensive, individualized, creative learning to students who experienced weak learning opportunities during their earliest years, to say nothing of their mixed K-12 experiences, and who exactly are the underserved students that selective public universities reject? The NAU lets them in, and then what? Crow and Debars offer quantitative metrics, but these could merely reflect reduced program requirements and lowered cognitive demands in individual courses at the historical moment when these need to be raised. ASU deserves its reputation for the creative use of technology, and yet instructional technology's record is weakest with the entry-level and at-risk students that the NAU accepts.

Inclusive education is meaningful only if it offers high intellectual standards, and these cannot be achieved in the traditional manner of postwar expansions: on the cheap. The NAU must offer mass quality and not just mass credentialing. In reality, this will require generous public funding based on enrollments and aimed at intensive learning, and that is precisely what states are decreasingly willing to fund. Crow and Dabars dodge the issue of public funding, which means dodging the question of funding unglamorous undergraduate instructional quality that offers none of the private returns or bankable impacts that attract investors and donors. If Crow won't shout from the rooftops, "Top quality via full funding for all students," what educational leader will?

The third aspect of NAU reinvention must be a new freedom in the relation between teaching and research, and on this point Crow and Dabars don't admit that there is a problem. Entry-level students—most undergraduates, in fact—are not educationally equipped to share in or contribute to research, and this is certainly true at the wonderfully inclusive NAU, where many students will need long-term skill development. There is always a cognitive divide at research universities that 100 years ago induced the economist and sociologist Veblen to recommend a protective barrier between the "college," focused on acquiring and using existing knowledge, and the "university," supporting the unadministrable agonies of advanced research that necessarily engage the hardest possible problems with the most arcane expertise in the unwelcoming twilight of the knowledge frontier.

There is also a budgetary conflict of interest between the undergraduate teaching and research functions. The latter has long depended on using a share of enrollment revenues to cover unreimbursed costs. During the period when enrollments and per-student funding always increased, tuition and state funds could be used to cross-sub-

ART/SCIENCE The art/science work has been influenced by research in astrophysics and neuroscience.



REBECCA KAMEN

Flare, 2012 (top)
From *Art/Science: Astrophysics*
Acrylic on mylar, fiberglass rods
25 x 21 x 6 inches

Illumination, 2012 (below)
From *Art/Science*
Acrylic on mylar
36 x 36 x 10 inches

sidize shortfalls in cost recovery. This is why public universities with low tuition and small endowments have been able to compete with the Yales and MITs in research. But now, after years of state cuts and, increasingly, caps on tuition hikes, public research universities are struggling to pay for instruction, much less cover indirect cost shortfalls for cutting-edge research. Crow and Dabars boast of ASU's ever-growing research expenditures, but of the $405 million it spent on research in fiscal year 2013, almost $150 million came from its own institutional funds. How can the NAU maintain that level of research subsidy (37% of total funds at ASU, 19 to 20% nationally), protect its cutting edge from the "college," and yet serve the college's needs for ever-better education for ever-more comingled levels of undergraduate skill?

Public university research and teaching funding are both in trouble. The NAU model envisions fully democratic higher education. But getting there will be not only a matter of design, but of fighting for public resources in a way that universities have long preferred to avoid.

Christopher Newfield
Department of English
University of California, Santa Barbara

Crow and Dabars argue passionately for the need for an institutional university model that combines pedagogy and research, broad student access, and commitment to societal impact. These ideals in fact define the land grant university, as it evolved from the 1862 Morrill Act, which provided federal land to states to establish universities to provide liberal and practical education to what the bill called "the industrial classes." Many of the nation's great public universities took their origins from this act. But their three goals—broad access, research excellence tied to instruction, and public service—do not always sit easily with one another, and state reductions in the funding of public universities have exacerbated the tensions among them.

Crow and Dabars advocate most strongly for access. Indeed, under Crow's presidency, ASU has grown to be the largest public university in the United States, offering admission to all qualified students who wish it. Crow has also led ASU through an extraordinary period of growth in research; the two, the authors argue, are complementary, with the research enterprise providing the basis to educate students in ways that uniquely qualify them to meet the challenges of tomorrow's world.

This is a claim that many observers believe, although actual evidence—of the percent of students in large public research universities who engage in research, and of the greater effectiveness of education in such institutions—is harder to come by. It may be comforting to believe in this synergy, but it takes more than assertion to make it so. Many kinds of postsecondary institutions seek to engage students in research, and the sheer scale of many public universities limits the use of high-impact practices such as independent research. Relatively little is known, with any precision, about the value of a broad and deep research program for undergraduate education.

The authors direct much of their criticism to admissions practices that exclude the majority of applicants, but here they play somewhat fast and loose with figures. The numbers they quote for rejected applicants from Ivy League universities, for example, are not unduplicated individuals, nor are the numbers from University of California campuses.

The fact that the new model for the U.S. research university is not as new as Crow and Dabars claim does not diminish the accomplishments of ASU, nor does it undermine the argument they make for the urgent need of greater enrollment capacity in public educational institutions. The nation's democracy depends on social mobility—and public education, as the Morrill Act envisioned, is its most powerful engine.

Arthur T. Johnson
Professor Emeritus, Fischell Department of Bioengineering
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Climate model worries

"Climate Models as Economic Guides: Scientific Challenge or Quixotic Quest?" (*Issues*, Spring 2015) provides an interesting, entertaining, yet sobering critique of the use of climate models for policymaking. The authors—Andrea Saltelli, Philip B. Stark, William Becker, and Pawel Stano—go as far as to question the scientific value of climate model research. The basis of their argument is that by failing to account for all of the important uncertainties in the models, current climate change predictions based on climate models can be seriously misleading. These unrealistic predictions can then be used by policymakers to mislead the citizens in either direction: to promptly develop or to postpone climate policymaking. This ambiguity ends up polarizing the public, and the chosen course of action usually benefits wealthier countries and powerful corporations.

The uncertainties associated with climate models are themselves uncertain because researchers just do not have all of the information required to account for all sources of uncertainty; we are not even able to properly account for all of the uncertainties of which we are aware. As the authors state, "...estimates of uncertainty are themselves almost entirely uncertain." Does this mean that attempts to develop climate models and to quantify their uncertainties are futile? I do not believe so.

Trying to understand and explain physical phenomena is a fundamental part of human nature. Researchers will continue trying to model and predict climate, as important scientific insight may be gained in the attempts to explain or predict climate change. Also, uncertainties associated with climate models have to be studied even if it is just to realize that the uncertainties are too large, and model predictions may be useless. Unfortunately, because greed and delusion are also part of human nature, regardless of the looming uncertainties, some scientists invested in climate models may be too eager to put a high value on their predictions, and corporations and governments may support climate policies that promote their own self-interests. Society needs to assess the uncertainties even if it is for self-defense, to protect from unsubstantiated claims meant to advance unwise policies.

Many researchers agree, as I do, on the danger of using climate model predictions to guide policy decisions. However, the authors do not suggest any other course of action, and to be paralyzed into inaction is also unwise. Society should at least request from scientists that uncertainty quantification be done with full disclosure of what can and cannot be assessed. It will not be possible to focus on developing fast and efficient methods for uncertainty quantification without actually questioning what the uncertainties really mean and whether or not they are realistic. It is also necessary to focus on educating the public to think critically about science, so that no scientific predictions are taken at face value without questioning. As Crow has written in "None Dare Call It Hubris: The Limits of Knowledge" (*Issues,* Winter 2007), "We all operate out of self-interest, which is entirely rational." But in today's complex society, each entity's actions affect others. Flexible policies that encourage individual innovation and are mindful of large-scale consequences are required.

Luis Tenorio
Associate Professor of Applied Mathematics & Statistics
Colorado School of Mines

This quartet of authors argues convincingly that it is foolhardy to use climate models as detailed economic guides. But failed quests for detail do not cast doubt on the underlying science. Insights from basic science stand regardless of researchers' inability to produce high-fidelity simulations. Even while we cannot simulate future details of Earth's atmospheric circulation, we know that increasing concentrations of carbon

REBECCA KAMEN
Growth Cone #1, 2012
From *Art/Science*
Acrylic on mylar
38 x 16 x 13 inches

REBECCA KAMEN
Portal, 2014
Installation, close-up view
Mylar, sound, fossils
Soundscape: Susan Alexjander
Photo: Gary Freeburg

PORTAL

Inspired by gravitational wave physics and Einstein's notion of *Gedankenexperiment* (thought experiment), this installation interprets the tracery patterns of the orbits of black holes and the outgoing gravitational wave of this astronomical event. The inclusion of the fossils references similar patterns found within micro and macro scales. Kamen created it in celebration of the centennial of Einstein's discovery of general relativity.

dioxide risk exciting climate feedbacks, the effects of which we cannot foresee. Think of a train traveling at speed toward an obstacle known to lie somewhere on the track ahead. If the obstacle is sufficiently large, it is straightforward to conclude that the train is likely to be derailed. It is far more difficult to determine (probability distributions of) exactly where individual carriages will come to rest, the damage to the contents of each, and personal injuries suffered at a given seat. Incomplete knowledge need not stifle action in the face of such risks.

Science is never "unequivocal." Science never provides "facts" about the future. And given that it is impossible to obtain "tested physical theory" on planetary scales, under conditions that have never (yet) happened, requiring such tests ensures a policy of "no action." All that said, the science underpinning the insight that increasing greenhouse gases will lead to warming is "as-good-as-it-gets" science. And as-good-as-it-gets science is often found on the backs of envelopes. We can calculate the Moon's surface temperature with surprising ease and accuracy. Although estimating Earth's is significantly more complicated, we have had clear, quantitative insight into the effect of greenhouse gases since Syukuro Manabe of the Environmental Science Services Administration's Geophysical Fluid Dynamics Laboratory published computations in the 1960s. Those basic insights stand. The same cannot be said for simulations of Earth's general circulation. There are well-understood phenomena that current models do not simulate realistically due to purely technological constraints. For example, we know how to represent rock in a global climate model rather well, yet the Andes are two kilometers too short in workhorse models used in what is called the Coupled Model Intercomparison Project Phase 5, or CMIP5. Such shortcomings yield visible imperfections in simulations of current climate and ponderable obstructions to researchers' ability to realistically simulate feedbacks and the climate change they drive.

Model-based climate projections are incomplete without an estimate of the probability of a big surprise: the probability that structural model inadequacy, for instance, renders them scientifically misinformative. A model used by the Intergovernmental Panel on Climate Change (IPCC), called AR5 SPM, comes laudably close, saying that there is a 10 to 34% chance that the change in global mean temperature over the final 20 years of this century will fall outside "the ranges derived from the concentration-driven CMIP5 model simulations." Should this happen, the strategy derived from a tool called UK Climate Projections 2009 (UKCP09), developed here in the United Kingdom, will collapse, a point not always reflected clearly in its worked examples. Are UKCP09's probability distributions for rainfall in the quad of my Oxford College on the wettest day in 2095 informative? Although grand-sounding justifications applicable to the earliest global climate models (or easily adopted by the marketers of perpetual motion machines) abound, there is still little public scientific debate of the limits beyond which high-resolution simulations should not be used in guiding development and policy.

The combination of clarifying what we cannot simulate and refusing to showcase "best available" numbers that are neither robust nor adequate for the purpose is simply basic scientific good practice. One danger of overselling our insight into the details is that it may well cast doubt on the as-good-as-it-gets scientific evidence that there is a clear and present risk of significant negative impact.

Leonard A. Smith
Professor in Statistics (Research)
Director of the Centre for the Analysis of Time Series
London School of Economics

REBECCA KAMEN

Plato's Water, 2008 (right)
From *Divining Nature: Elemental Matters*
Mylar and fiberglass rods
60 x 36 x 12 inches
Photo: Angie Seckinger

Divining Nature: An Elemental Garden (above)
Installation at Greater Reston Art Center, Reston, VA, 2009
Photo: Angie Seckinger

Layout detail for *Divining Nature: An Elemental Garden Installation* (far right)
Drawing: Alick Dearie

DIVINING NATURE: AN ELEMENTAL GARDEN

This sculpture and sound installation translate chemistry's Periodic Table, a chart of letters and numbers, into a garden of sculptural elements based on geometry and atomic numbers. Laid out in a Fibonacci spiral, the sculptures symbolize the orbital patterns of the first eighty-three naturally occurring elements. It also includes a series of wall sculptures inspired by the Platonic solids which Plato associated with the four classical elements of earth, air, water, and fire.

REBECCA KAMEN
Fluid #3, 2010
From the *Fluid Series*
Acrylic on mylar, fiberglass rods
11 x 18 x 4½ inches

FLUID SERIES These layered wall reliefs explore nature as an energy mapping system. Informed and inspired by micro and macro views of the Universe, as well as other scientific visualization models such as fluid mechanics, these sculptures interpret and make visible the fluid energy of matter, creating a bridge between art and science.

I generally agree with the ideas expressed in this article, but I find that the authors' distinction between "policy simulation" and "policy justification" is not very convincing. For if one believes, as I do, that the results of economic models used to attempt to quantify the net costs and benefits of mitigating climate change over the long run are unscientific, in part because of the uncertainties inherent in the models and their assumptions, then these models can be used neither to simulate policies nor to justify policies to any reasonable level of accuracy. Thus, I more strongly support Saltelli *et al*. when they question whether many model-based "facts" are scientific at all.

That issue aside, the article has other rather profound implications, some of which I do not believe that the authors sufficiently stress. One implication is that the peer-review process for research papers published on physical climate modeling, as well as on economic modeling of climate change, is clearly broken. After all, given the article's claim that uncertainty is not usually accounted for properly, many if not most of the papers should have been rejected. This is certainly true when it comes to the economic modeling done by integrated assessment models, which is what underpins certain critical sections of the IPCC Working Group III report on mitigation.

The peer-review process should also have led to the rejection of many research papers in these fields, because the basic logic of many conclusions drawn from "inter-model comparison studies" is often totally flawed. For example, many inter-model comparison studies ask the valid question at the beginning, namely, to what extent are the differences in results from different models for the "same" scenario due to differences in model structure versus differences in input assumptions? The studies then claim to proceed to provide an answer. But they never do, because they never have each model run with the same values of key input assumptions, to the extent allowed by the different model structures.

Another problem noted by the authors is that the use of the term "probability" in the IPCC reports as applied to likely temperature increases due to climate change is really quite fallacious. They basically say this explicitly, but the point needs to be emphasized. Because it is not possible to know anything about the probability distributions of even a single key input parameter for the physical climate models, it certainly is not possible to know the likely probability of the resulting temperature increases for any given level of radiative forcing over time. Yet, the "Summaries for Policy Makers" of the IPCC reports are replete not only with the term "probability," as Saltelli *et* *al*. described, but with actual numerical values for the relevant probabilities.

Richard A. Rosen
Tellus Institute
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The authors provide a frank and incisive review of discussions and scientific analysis on climate change. They warn of the uncertainties in the predictions of global warming models. Climate models are designed to produce information. But I suggest reading the article from a perspective of information quality, or what I and a colleague, Galit Shmueli, have called InfoQ. InfoQ ties together the goal, data, analysis, and utility of an empirical study. It is deconstructed in eight dimensions: data resolution, data structure, data integration, temporal relevance, generalizability, chronology of data and goal, operationalization, and communication. To assess InfoQ, these eight dimensions must be evaluated in the context of specific goals and objectives.

Saltelli *et al*. focus on the lack of generalizability and limitations in several of the global warming publications, if one is interested in formulating policies, which affect the economic scene. They state that "ensembles" are not in any sense representative of the range of possible (and plausible) models that fit the data, which implies a lack of generalizability. They also state that the sensitivity analysis varies only a subset of the assumptions and only one at a time. But this precludes interactions among the uncertain inputs, which may be highly relevant to climate projections. It also indicates poor generalizability. In terms of operationalization, the authors distinguish policy simulation from policy justification. The operationalization of the climate model in terms of justification is the problematic part the authors want to emphasize. An InfoQ assessment of the various studies cited can help further elucidate the difference between scientific insight and evidence for policymaking.

The authors' underlying approach is scientific. The assumption is that the correct view of an issue such as climate change should be evidence-based. Unfortunately, many forces are now participating in this controversial field, with apparent collateral damage. See for example the blog on how the education system in the United Kingdom is affected by such discussions: https://tthomas061.wordpress.com/2014/04/09/climate-catastrophism-for-kiddies/.

If the aim is to be "evidence-based" and "scientific," then Saltelli *et al*. have provided an excellent perspective. To help focus the discussion, one might want to bring in the perspective of information quality that combines generalization and operationalization, two critical aspects of the global warming debate. Even without that, the authors should be gratefully thanked for insightful contributions.

Ron S. Kenett
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Visiting Professor, Institute for Drug Research, Hebrew University Faculty of Medicine, Israel
International Professor, New York University Center for Risk Engineering
Chairman and CEO, The KPA Group, Israel

The authors of this thoughtful and measured article deserve to be commended for their calm and reasonable tone in a subject area that they note from the outset is "polarized" and "fraught." The core of their well-argued case is at least partly captured by these three statements: One, "Given the economic and societal ramifications of climate change, it is not surprising that several disciplines claim to provide certainties and solutions. Among these, computer modeling is perhaps the most visible, pervasive, and opaque." Two, "…models share common errors whose magnitudes are simply not known." And three, "…[a danger] is that, with excessive confidence in our ability to model the future, we will commit to policies that reduce, rather than expand, available options and thus our ability to cope with the unknown unknowns of our future."

There seems little doubt that laypeople can be unduly impressed by computer outputs. The remarkable impact of the Club of Rome and its report *The Limits to Growth*, issued in 1972, is testimony to that. The foolishness of such trust has been revealed by the world unfolding in

REBECCA KAMEN
Fluid #8, 2010
From the *Fluid Series*
Acrylic on mylar, fiberglass rods
18 x 32 x 4½ inches

MANUSCRIPT AS MUSE

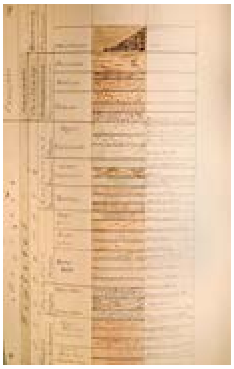
Many people think of old books as obsolete, especially in the digital age. As an artist, I have always perceived books as a source for creative inspiration.

The books I viewed during my residency at the American Philosophical Society Library took me on a remarkable intellectual journey. One of the most exciting observations was how drawing became a visual recording device for scientists before the invention of the camera. Looking at the sketchbooks of Lewis and Clark, the incredibly detailed bug drawings of John LeConte, and John Benbow's sketches in *The Bee Book*, to name a few, I found myself humbled by the authors' ability to record their observations, not only via the written word, but through beautifully rendered forms.

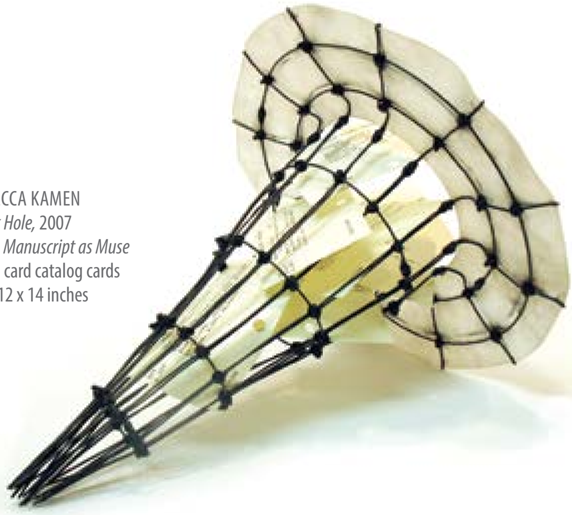
The works in the series *Manuscript as Muse* allude to the visual power of books. The process of layering graphite and acrylic on mylar—like pages from a book when viewed together—create a complex visual story.

—Rebecca Kamen

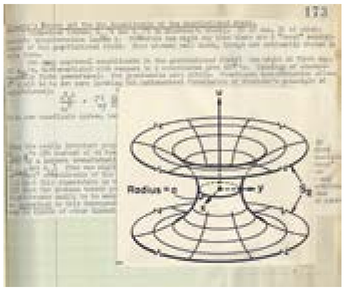
This page and opposite:
Manuscripts courtesy of the American Philosophical Society Library.

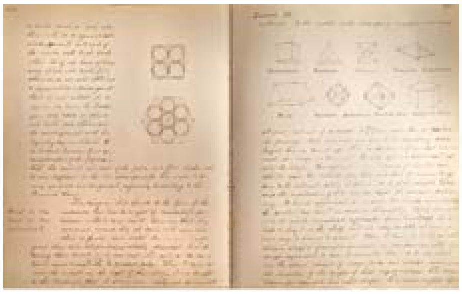


REBECCA KAMEN
Strata 2, 2008
From *Manuscript as Muse*
Acrylic, graphite on mylar
11 x 7½ x 3 inches

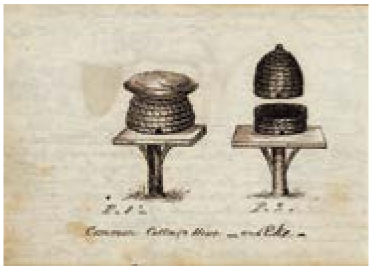


REBECCA KAMEN
Black Hole, 2007
From *Manuscript as Muse*
Wire, card catalog cards
12 x 12 x 14 inches



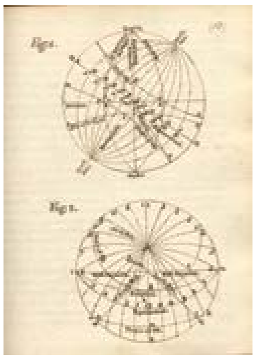


REBECCA KAMEN
Crystal, 2008
From *Manuscript as Muse*
Acrylic, graphite on mylar
10 x 10 x 3 inches



REBECCA KAMEN
Hive, 2008
From *Manuscript as Muse*
Acrylic, graphite on mylar
11 x 8 x 2¼ inches

REBECCA KAMEN
Shell, 2008
From *Manuscript as Muse*
Acrylic, graphite on mylar
14 x 7 x 2½ inches



REBECCA KAMEN
Matrix 1, 2008
From *Manuscript as Muse*
Acrylic, graphite on mylar
8 x 8 x 3 inches

the intervening decades in a dramatically different way from that report's vivid auguries of doom and disaster.

In our time, the computer models of climate have been elevated some way beyond their deserved status by campaigners agitated by the possible effects of humans' carbon dioxide emissions on climate. In a study published in 2013 by the Heartland Institute, *Global Climate Models and Their Limitations, Climate Change Reconsidered II: Physical Science*, Anthony Lupo and William Kininmonth have presented a detailed and more technical analysis of the many limitations of such models, not least in areas where model output can be compared with observations, and their work provides useful background and reinforcement for the present article.

John Shade
Inverness, Scotland

The policy debate with respect to anthropogenic climate change, addressed by Saltelli and colleagues, typically revolves around the accuracy of models. People who contend that models make accurate predictions argue for specific policies to stem the foreseen damaging effects; those who doubt their accuracy cite a lack of reliable evidence of harm to warrant policy action.

These two alternatives are not exhaustive. One can sidestep the "skepticism" of those who question existing climate models, by framing risk in the most straightforward possible terms, at the global scale. That is, we should ask, what would the correct policy be if we had no reliable models?

Humans have only one planet. This fact radically constrains the kinds of risks that are appropriate to take at a large scale. Even a risk with a very low probability becomes unacceptable when it affects all of us—there is no reversing mistakes of that magnitude.

Without any precise models, we can still reason that polluting or altering the environment significantly could put us in uncharted territory, with no statistical

META

This series explores ideas of alteration, transposition, and transcendence. Each sculpture incorporates concepts of mapping time and occurrence. It also reflects Kamen's longstanding fascination with the relationship between scientific and sacred motifs.

REBECCA KAMEN
Immortal, 2003
From *Meta*
Steel wire
25 x 6 x 4 inches

track record and potentially large consequences. It is at the core of both scientific decisionmaking and ancestral wisdom to take seriously the absence of evidence when the consequences of an action can be large. And it is standard textbook decision theory that a policy should depend at least as much on uncertainty concerning the adverse consequences as it does on the known effects.

Further, it has been shown that in any system fraught with opacity, harm is in the dose rather than in the nature of the offending substance: Harm increases nonlinearly to the quantities at stake. Everything fragile has such a property. Although some amount of pollution is inevitable, high quantities of any pollutant rapidly increase the risk of destabilizing the climate, a system that is integral to the biosphere. Ergo, we should reduce carbon dioxide emissions, even regardless of what climate models say.

This leads to the following asymmetry in climate policy. The scale of the effect must be demonstrated to be large enough to have impact. Once this is shown, and it has been, the burden of proof of absence of harm is on those who would deny it.

It is the degree of opacity and uncertainty in a system, as well as asymmetry in effect, rather than specific model predictions, that should drive precautionary measures. Push a complex system too far and it will not come back. The popular belief that uncertainty undermines the case for taking seriously the "climate crisis" that scientists say we face is the opposite of the truth. Properly understood, as driving the case for precaution, uncertainty radically underscores that case, and may even constitute it.

Joseph Norman
Yaneer Bar-Yam
New England Complex Systems Institute

Rupert Read
School of Philosophy, University of East Anglia

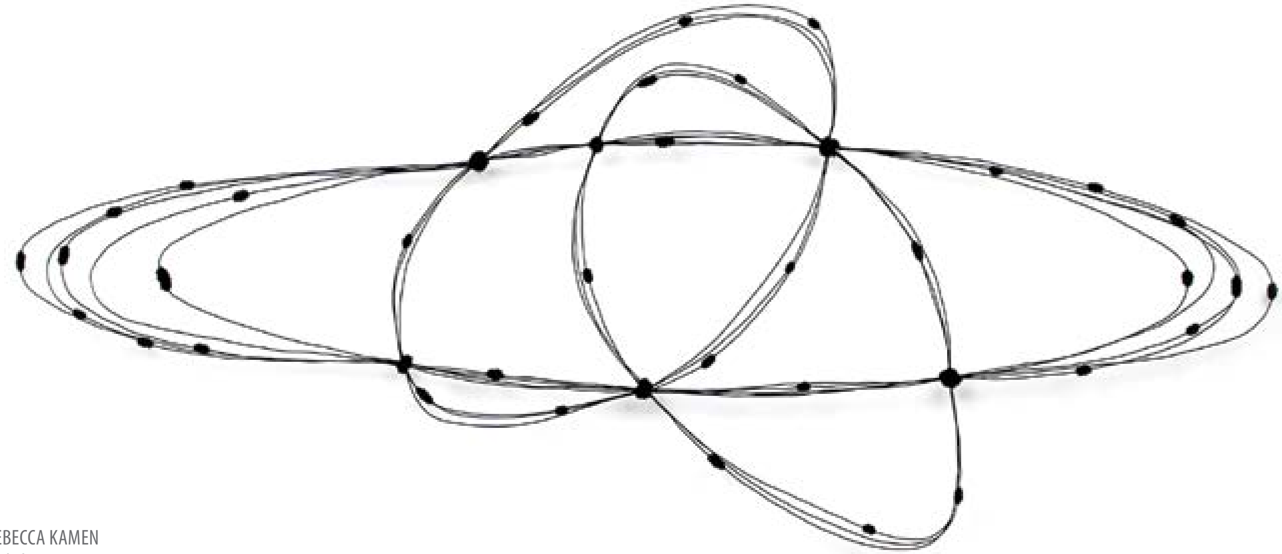
Nassim Nicholas Taleb
School of Engineering, New York University

Good behavior

In "Informing Public Policy with Social and Behavioral Science" (*Issues*, Spring 2015) Brian Baird lays out five recommendations to bridge the gap between academics—specifically in the social and behavioral sciences (SBS)—and policymakers. But there are three important observations he misses that have implications for the type of institutional development that should take place.

First, the strength of SBS is in its theoretical and methodological diversity. Baird recommends a "collaborative, consensus process to identify robust scientific methods and findings that are of potential interest to policymakers." This is not achievable in SBS, however, at least not in the sense laid out by Thomas Kuhn, an influential U.S. physicist, historian, and philosopher of science. Economists, sociologists, psychologists, and researchers in other SBS disciplines appropriately develop and test their own theories, at a variety of different levels of analysis, using a wide range of analytic methods, to address vastly different research questions. This is not because SBS researchers are unaware of one another's research, but rather because of the extraordinarily complex nature of the key units of observation for SBS: individual people and groups thereof (e.g., organizations, communities, jurisdictions), both with innumerable and intangible "moving parts" that are inordinately more difficult to observe (much less predict and explain) than, say, biological or engineering systems.

Second, there is no shortfall of institutional mechanisms for translating and communicating SBS research to policymakers. Most of Baird's recommendations are akin to similar calls for technology transfer from the "hard" academic science and engineering fields to industry. I agree with Baird that one should not presume trickle-down from SBS to policymakers, and that institutional development for translating what SBS academics know to policymakers in a language that the latter can understand and apply is a good



REBECCA KAMEN
Coded Sequence 3, 2003
From *Meta*
Steel wire
52 x 24 x 3 inches

idea. However, institutions of this kind have existed in SBS for some time. There are upwards of 300 schools of public affairs in the United States, and many of these are much more than professional schools focused on teaching basic skills such as policy analysis to graduate students. Within many of these schools are multidisciplinary policy research centers explicitly designed to translate SBS research findings for decisionmakers in particular areas of public policy, using many of the approaches that Baird recommends. For example, Georgia Tech, Arizona State, Harvard, and Ohio State have centers focused on science and technology policy.

Third, the real problem is that policymakers lack absorptive capacity. The institutional gap between academic research and policymakers as characterized by Baird is already being bridged in numerous policy areas and in the ways he suggests, at least for SBS. (In contrast, most bridging institutions for the sciences and engineering focus on industry, not government.) If new types of institutions connecting academics to policymaking are to be developed, they should not focus on the translation of research findings (from SBS and otherwise), but rather on developing the absorptive capacity of policy decisionmakers to distinguish scientifically derived information from other sorts of information. In other words: translation is not enough. Policymakers should possess the basic skills that any graduate student possesses after completing his or her first year in a public affairs program. And many of the modes of communication and teaching recommended by Baird would be very useful for accomplishing this task.

Craig Boardman
Associate Director, Battelle Center for Science & Technology Policy
Ohio State University

Biomedical overbuilding?

"Have Universities Overbuilt Biomedical Research Facilities?" (*Issues*, Spring 2015) admixes a narrow focus and questionable statistics with a broader, valid concern that care be taken in the consideration of proposals to eliminate government reimbursement for university construction of research facilities. The authors—Arthur Bienenstock, Ann M. Arvin, and David

REBECCA KAMEN
In the Beginning, 2003
From *Meta*
Acrylic on mylar
36 x 24 inches

Korn—set their sights on the expansive critique and set of recommendations that Bruce Alberts has made for rescuing U.S biomedical research from its current plight. (Alberts has made this case in several venues, but Bienenstock *et al.* focus in particular on an editorial published in 2010 in *Science* magazine.) The authors, however, home in on only one of the recommendations: "full reimbursement to amortize loans for new buildings." Moreover, Alberts offered that recommendation toward the close of a trenchant analysis of the systemic propensities toward mismatches between the supply and demand for biomedical researchers, a dynamic propelled in part by "perverse incentives" in research funding that "encourage grantee institutions to grow without making sufficient investments in their own faculty and facilities."

The authors' primary counterargument is that the quantity of academic biomedical research space per million dollars of support from the National Institutes of Health (NIH) fell from its 1987–1995 level through 2003, and only afterward began to rise, reaching a level in 2011 below the base period. They present these data as evidence that there was a "significant" shortage of academic research space in the late 1990s. They thereby contend that "Given the absence of evidence for systematic overbuilding, there is no apparent justification for altering federal reimbursement policies related to the construction of research facilities."

The data they present finesse rather straightforward considerations that adding researchers, staffs, and students attendant on increased research funding (from NIH and elsewhere) takes less time than constructing buildings, so that (short-term) space squeezes are predictable whenever expansion occurs. More importantly, their focus on this measure alone leads them to quickly pass over what in effect is the central thesis of Alberts's argument; namely, as they themselves note, "some institutions or classes of institutions may have overbuilt." Reflecting deep-rooted institutional imperatives and aspirations, fueled by congressional policies to foster geographic and institutional dispersion, U.S. universities have their own field of dreams, believing that if they build it, it (funding) will come. This is the behavioral syndrome that Alberts's proposals are designed to cure.

But Bienenstock *et al.* raise a deeper concern that I share. The closing recommendation in Alberts's essay comes across as an unbounded call for a reexamination of policies (e.g., payment of indirect costs or support of faculty salaries) that are foundational elements in the social contract binding together the federal government and universities. If indeed undertaken, any such reexamination must be driven and shaped by more than the admittedly troubled setting of the academic biomedical sciences. Thus, to attend only to the matter of research space, academic research space devoted to the biological and biomedical sciences constituted the largest share of all such space, but this share represented only 27% of the total, according to a report by Michael Gibbons, Research Space at Academic Institutions Increased 4.7% between FY2011 and FY2013, issued as an InfoBrief by the National Science Foundation's National Center for Science and Engineering Statics in March 2015. Enlarging upon the Bienenstock *et al.* argument, it would be a mistake to attempt to correct the systemic flaws in the biomedical sciences without considering the impacts of any proposed policy changes—including but not limited to federal reimbursement policies for construction of research facilities—on other fields of research or disciplines, or on research universities in general.

Irwin Feller
Visiting Scientist, American Association for the Advancement of Science
Professor Emeritus of Economics, Pennsylvania State University

MATTER
This series of complex wire sculptures was created for an exhibition at the American Center for Physics celebrating the centennial of Einstein's discovery of special relativity.

REBECCA KAMEN
From *Matter*
Doppler Effect, 2005
Steel and copper wire
10 x 12 x 10 inches