A LIFE OF EXPERIMENTAL ECONOMICS, VOLUME II

THE NEXT FIFTY YEARS

VERNON L. SMITH

A Life of Experimental Economics, Volume II

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The Next Fifty Years

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One cannot take advantage of his fellowman and come out ahead.
It just cannot be done.
—Quintin Lomax

Preface

This narrative, though printed in a second volume, is written without separation in the laminar flow of my experience: A seamless re-visitation of my life and its meaning; a dialog with past events and interconnections that began with the Nobel Foundation's request that I write a short biography for their Web site in 2002. That request parted the wardrobe for only a moment. I have now worn its sill thin with reentry.

Hayek's text best captures the personal and intellectual themes of this volume: "[C]ivilization rests on the fact that we all benefit from knowledge that we do *not* possess." (Hayek, *Rules and Order*, 1973, p. 14) Through our social and economic institutions, each individual benefits enormously, if not predominantly, from knowledge and its fruits that are provided by others; in particular, "knowledge-how" as contrasted with "knowledge-that." Although our social network system is the first and oldest of the two institutions, I begin in Chapters 19 and 20 with personal new learning in economics, both micro and macro. I introduce the word "humanomics" to refer to the study of human social and economic behavior in Chapter 21. There I write on the discovery of Adam Smith's model of human social behavior as providing a foundation for understanding the small group predictive failures of game theory in the 1980s and 1990s. Humanomics provides an integrated new synthesis of social and economic behavior long believed to live in unresolved anomalous conflict.

We begin in 1967 when Joyce and I return to Massachusetts, as citizens in the community, with a sense of permanence much different from when I had been a graduate student at Harvard. The twins, Deborah and Eric, will be in high school, with Torrie soon to follow. Joyce will be entering

the Unitarian Ministry and a new world of experience for us all, one that we entered without hesitation, and for which we had long prepared—at Purdue, all my friends knew I would move with Joyce and family. Those expectations of great promise are not to be disappointed.

I write of the five years we are once again in the East; years especially formative for our children who will be in transition to their subsequent careers and lives of their own. Joyce will fulfill with distinction and honor her exceptional promise as a student of the Chicago Divinity School. Events will return us to the West, where I will remain, while Joyce returns east to become a force in both the Ministry and the American Unitarian Association.

I will return full time to research and teaching in experimental economics and be recognized as "father" of its development in 2002, though, as you will learn in detail, I had an abundance of innovative help from students, co-authors, and many scholars along the way. That event will cap 26 years at the University of Arizona, but by then, I will have returned east to new opportunities at the George Mason University campus in Arlington, Virginia.

There will be changes in my married and family life that I must tell you about. And when I once again go West, it will be to new opportunities, new learning, and in so many ways a new career, including a second Camelot.

We begin the next fifty years.

Orange, USA Vernon L. Smith

Acknowledgements

My personal debts run deep, beginning with John Hughes (*The Vital Few*), for 36 years my trusted friend and confidant until his death in 1992. His mark upon me pervades these volumes.

To Silvia Naser (*A Beautiful Mind*) and my dear friend of 38 years, Deirdre McCloskey (*The Bourgeois Virtues*), who read the earliest drafts of the manuscript and corrected, nudged and encouraged me in directions that shaped it to the end.

Tom Hazlett (*The Political Spectrum*) friend and cherished co-author. I knew Tom before he knew me because I was an avid reader of his columns that appeared in *Reason* magazine beginning in 1989.

Andreas Ortmann, economic theorist, experimentalist, and intellectual historian *par excellence* in all, who's comments, reviews—both published and private—have never failed to be rewarding to me.

Steve Hanke (http://sites.krieger.jhu.edu/iae/), wise counselor on monetary and fiscal policy, whose private and published reviews have encouraged and supported my dedication to these volumes.

Charles Plott (*Collected Papers on the Experimental Foundations of Economics and Political Science*. In three volumes), who, because I could outfish him, suspected that there might be something to experimental economics and became a co-conspirator in its development in the 1970s. Charlie invented experimental political economy.

Shyam Sunder (*Theory of Accounting and Control*), methodologist, experimentalist, whose passion in the search for foundations has long been an inspiration.

E. Roy Weintraub (*How Economics Became a Mathematical Science*), reviewer, whose wide-ranging interests included me.

My Amazon reviewers, each in his own tongue:

Paul Johnson, friend and colleague, University of Alaska, Anchorage;

Herb Gintis (*Individuality and Entanglement: The Moral and Material Bases of Social Life*) with whom I share gloriously radical roots and a passion for moral wisdom.

Roger Farley, investor and portfolio manager, whom I do not know. But we resonate well.

Pete and Jackie Steele, a breed of the many ordinary people that have made America. May we never lose their unwavering integrity, love of life, and of the good land.

Stephen Semos for his careful editing, fact checking, and many suggestions for improving style and content down to the final crescendo.

Candace Smith, devoted companion in our explorations of love, understanding, and faith developing.

And to co-authors and students galore—Steve Gjerstad, Dave Porter, Stephen Rassenti, Arlington Williams, and more whose imprint is in these pages.

And a very special debt to the Liberty Fund for inviting me to many of their Socratic colloquia, over the last forty years, on topics and figures in the philosophy and history of the struggle for liberty. I want to acknowledge my frequent use of quotations from Adam Smith (Dugald Stewart edition, 1853) and David Hume, published by Liberty Fund and that are available for quotation and free electronic download access.

You will encounter many more in these pages. "This is remembrance—revisitation; and names are keys that open corridors no longer fresh in the mind, but nonetheless familiar in the heart" (Beryl Markham, *West with the Night*).

Praise for *A Life of Experimental Economics, Volume II*

"We learn from giants on whose shoulders we stand. There can be no doubt that 2002 Nobel Prize winner Vernon L. Smith is one of them. *A Life of Experimental Economics, Vol I: Forty Years of Discovery; Vol II: The Next Fifty Years*, is a much expanded version of his 2008 memoir *Discovery* in which he recounted his journey from birth until 2005. As many other reviewers did then, I called that earlier version a must-read and have recommended it to many of my colleagues and students as well as folks from other walks of life since. The new work reviews, and in places revises, that memoir and then adds several chapters that have been inspired by Vernon's more recent interests in the nature and causes of housing bubbles on the one hand and his attempt to draw out the insights to be had for modern economics from Adam Smith's *Theory of Moral Sentiments* on the other. The new volumes trace matter-of-factly the amazing journey from five-year old farm boy in the Great Depression to the towering, very public intellectual that Vernon is today. It does so—mostly—in the same conversational tone that made *Discovery* such a joy to read. (Yes, of course, the best pie this side of heaven is made from freshly cut rhubarb. And, yes, one should not mix strawberries with the rhubarb. Ever.) Be prepared to not agree with Vernon's opinions on all of the numerous issues discussed as we progress through the decades—many of his opinions are informed by a very libertarian streak indeed—but as provocative as they might be, they were formed in a lifetime of extraordinary achievements and extraordinary insights into human nature and institutions, as well as a deeply humanistic attitude."

—Andreas Ortmann, *Professor of Experimental and Behavioural Economics, School of Economics, UNSW Business School, Australia*

"This personal narrative guides the reader along a 90-year journey from a one-room school in Kansas through a career of scientific discovery, with a fairy-tail ending and a richly rewarding postscript. In contrast with other Nobel Prize winners in economics who often immerse themselves in their own "higher-level models," Vernon follows Adam Smith's methodology of detailed observation, with a special focus on human behavior in the lab and regulatory misbehavior in the wild. His intense love for life and research is conveyed in a sequence of colorful stories, presented against a landscape that switches back and forth from the American West to academic culture. The reader is treated to insights about how economics experiments and policy proposals are designed, interspersed with advice that ranges from relationships to making a good batch of chili from scratch."

—Charles Holt, *A. Willis Robertson Professor of Political Economy, University of Virginia, USA*

"Only Priests and Engineers populated Econo-Land. Priests spin theories without facts; Engineers collect data, give policy advice, and generally embrace only the most basic economic theory. Microeconomics textbooks the world over were filled with axioms and theorems, bereft of facts.

Over three score years ago, Vernon Smith, Engineer par excellence, set out actually to test economic theory! The Priests were horrified. Vernon put together a working laboratory, got amazing results having people play games of economic exchange, and started a movement that has radically altered the relationship of fact to theory in economics. Not only has experimental economics expanded a thousand-fold over the years, but leading journals now present models that attempt to account for the observed behavior of actual human subjects in field and laboratory.

Vernon changed my life in 1992, when I read an article he wrote in *Scientific American* surveying his work. I had thought that experimental economics was just a bunch of dimwits trying to show that Adam Smith's invisible hand really worked. I was wrong. Inspired by Vernon, I can honestly say that everything I assert with confidence about economics comes from either the result of experiments or observing the comparative performance of different real-life economic institutions.

Vernon Smith is larger than life. I recall vividly, as a young Associate Professor, meeting with Vernon at the American Economic Association meetings in New Orleans to try to convince him not to leave Massachusetts for Arizona. Vernon was dressed in a beautiful white Southern-style suit with a string tie and a stunning gold-embroidered vest. He was slim and gorgeous, with a big handlebar mustache. I had never met anyone like him in

my life, and the experience was amplified by the fact that I, and my friends, veterans of civil rights and anti-Vietnam War struggles, dress uniformly in jeans and torn polo shirts with pictures of Ché on the back and the peace sign on the front. Vernon was a veteran of the same struggles, a conscientious objector, but absent Ché. He declined to return to UMass, and the rest is history. This two-volume set is a memoir that every young, creative economist should read, and the deadwood in academia should shun at all costs."

—Herbert Gintis, *Santa Fe Institute, USA*

"Vernon Smith's ingenuity in developing experimental methods to study "that which is not" has deepened economic analysis and enabled us to examine questions of institutional change and market design that would otherwise remain hypothetical. Combining personal and professional reflections with the arc of U.S. economic history in the 20th century, this heartfelt and engaging story uses economics and philosophy to analyze a life of intellect, curiosity, enthusiasm, and purpose. Professor Smith's life experiences, his creativity in developing new economic ideas and new fields of inquiry, and his dogged commitment to inquisitiveness are inspiring examples of a well-lived life of the mind."

—Lynne Kiesling, *Purdue University, USA*

"A Life of Experimental Economics, 2 volumes, provides a vivid picture of one of the most vibrant minds in modern social science, Vernon Smith—the 2002 Nobel Prize winner in Economic Science. That Smith is an outstanding theorist and innovator of experimental methods in economic science is well known. But, Vernon Smith is much more than a first-class economic scientist. His life story, as told throughout these volumes, provides an outstanding example of life-long learning revealed through his explorations into natural history, economic history, and all human endeavors, ancient as well as modern, to unearth deep scientific explanations. Smith's insatiable desire to discover the mechanics and meanings embedded in human sociability are displayed beautifully in the pages of this autobiography. Vernon Smith has indeed lived a wonderful life, and continues to live a life full of intellectual curiosity and creativity in his quest to understand the human condition philosophically as well as scientifically. What a fascinating and amazing journey of discovery we are privileged to witness in reading A Life of Experimental Economics. Read it, absorb its lessons, and most importantly, strive to follow its example and be a life-long learner."

—Peter Boettke, *University Professor of Economics and Philosophy, George Mason University, USA*

"While the Marxist critique of political economy that constitutes the wheelhouse of my work in the philosophy and sociology of education could not be further from the pro-market libertarian views of Nobel Laureate Vernon Smith, *A Life of Experimental Economics* is a book that I would highly recommend to all. It is a fascinating work that illustrates the life of a man blessed with a singular curiosity and creative mind, a man gifted with the grace of humility and endowed with a formidable intellect and yet, most impressively, a man who refuses to sacrifice wonder at the expense of classical logic. If you scratch any theory, you will find an autobiography underneath. With *A Life of Experimental Economics*, you don't have to scratch that hard to understand the myriad ways that the formative experiences of the young Vernon Smith have been carried forth throughout his life, ever steeling his desire to make the world a better place. Born into a family who worked on the locomotives of the Wichita and Southwestern and Santa Fe railroad companies that used to carry cattle to the stockyards near Kellogg Avenue, who labored in the rolling wheat fields and the oil fields of Kansas, and who were educated in the one-room schoolhouses that served the cattle ranches and farming communities, Vernon Smith recounts his pathfinding journey from his farmhouse in America's heartland to his trailblazing work in the classrooms of the University of Kansas, Harvard, Caltech, Purdue, George Mason University and Chapman University. A lack of dialogue across differences on university campuses has enabled superficial characterizations of intellectuals and activists on both the right and the left. *A Life of Experimental Economics* reveals the folly of such stereotyping. The self-portrait that emerges from the pages of Smith's autobiography is filled with reflexive self-questioning, and a commitment to activism on behalf of racial, social and political equality. This is not a man ensepulchured in a brainpan filled with numerical abstractions and powered by a cold calculus of reasoning, but a man whose is motivated by a reverence for life and a betterment of the public good. His life is a journey of discovery, minted by curiosity and wonder, and one that eventually took him through the gates of the Judeo-Christian tradition, where his Christian faith has challenged him to rethink the very foundations of science. It is a journey that will enrich us all."

—Peter McLaren, *Distinguished Professor in Critical Studies, Chapman University, USA; Chair Professor, Northeast Normal University, China*

"Vernon Smith is one of our greatest living economists and at 91 years he is still active and strong. Along with others, he built an entire experimental science designed to reveal economic dynamics at the level of individuals that has global consequences. Vernon is as likable as he is deep. Who would have

guessed he began employment as a drugstore delivery boy at age 12—and worked with the Congress of Racial Equality at 15? There is no better introduction to Vernon than himself, an organism who remembers his life as he lived it—simple, humble, unpretentious and with a bias toward honesty and justice."

—Robert Trivers, *Evolutionary theorist, sociobiologist, 2007 Crafoord Prize winner and author*, Wild Life

"Vernon Smith's autobiography is an incredible life story of the whole person telling the reader so much more than his development as an economist. His single-room schoolhouse, two difficult years on the farm during the Depression Era, transformation from a C+ high schooler to a straight A Caltech and Harvard graduate, and deep engagement with his family, friends, and faith, all played essential roles in sculpting his curiosity-driven way of life and discoveries. Smith's fascinating tale will resonate with all who are willing to let observation and experience change their minds. It is an essential reading for aspiring scholars."

—Shyam Sunder, *Yale University, USA*

"Nobelist Vernon Smith presents a riveting intellectual history of his life and his life's work. The creator of the field of experimental economics has crafted a superbly written two volume treatise that is loaded with provactive details. It reads like a novel. And like all great novels, it contrains one great character: the classical liberal Smith, himself."

—Steve H. Hanke, *The Johns Hopkins University, USA*

"The brilliance of Vernon Smith will not surprise the reader. What might, however, is the playfulness of his child-like curiosity, the richness of his experience, the easy flow of his thought, and his passion to grasp the next problem, tiny or vast. That this character led to intellectual discoveries that changed the world is a meaty tale, but it is very nearly a side show. This is a compelling human drama, filled with warmth, pain and love, honest and unretouched. It forces the reader to think about the greatest challenges of the human condition, and yet details the delicious secret of the perfect hamburger. I felt privileged to consume this beautiful tome, and be charmed by its author on every page."

—Thomas Hazlett, *Hugh H. Macaulay Endowed Professor of Economics, Clemson University, USA*

Contents

Volume II

Part III East, Southwest, East Again

13 Yankee Land 3

14 West with the Night 17

15 Arizona and E-Commerce in the Laboratory 45

16 My Friends Were Finally Right 87

17 Wives, Daughters, and Sons 117

Part IV Rethinking Recessions, Markets, Adam Smith and Religion

18 Home Again: Chapman University 145

19 Economic Collapse 2007–2008: Would 1929 Be Reborn in Anemic Growth? 151

20 Markets: The Good, the Volatile and the Sometimes Ugly 179

21 Reconnecting Modern Economics with Its Classical Origins: Discovering Adam Smith 189

22 Faith and the Compatibility of Science and Religion 217

Author Index 235

Subject Index 241

List of Figures

Fig. 13.1	Monument Valley where the west begins, 1981	13
Fig. 13.2	House Trailer Gulch on Road to Lake Powell	13
Fig. 13.3	Campsite invasion by flotilla	14
Fig. 13.4	Ken Slight CO River triple rig	14
Fig. 13.5	Havasu Canyon meets CO River	15
Fig. 13.6	Vernon rainbow 1 of 4 CO River	16
Fig. 14.1	Pete unloading horses Salt Creek excursion	36
Fig. 14.2	Arthritic Man young, middle aged and old, Grand Gulch	37
Fig. 14.3	Breach birth Grand Gulch	37
Fig. 14.4	Anasazi porn man-wolf copulating while another waits his turn	38
Fig. 14.5	Great figures	38
Fig. 14.6	Where seldom seen Smith kneels, prays for precision earthquake	39
Fig. 14.7	Headdress	39
Fig. 14.8	d'ja ever hear the one about the five legged deer	40
Fig. 14.9	Giants of the Southwest	40
Fig. 14.10	Salt Creek all American man	41
Fig. 14.11	Five faces east	42
Fig. 14.12	Salt Creek shower	43
Fig. 14.13	Kokapellis galore	44
Fig. 15.1	Vernon and Paul Martin visit Lehner Ranch Mammoth Kill Site 1975	81
Fig. 15.2	In Situ Mammoth Bone at Ed Lehner Used Elephant Lot 1975	81
Fig. 15.3	2nd Generation PLATO Lab	82
Fig. 15.4	Discovery Arizona style	82
Fig. 15.5	Richard Kiser, Vernon, Praveen Kujal, Stephen, Spain Workshop	83

Fig. 15.6	ESA Westward Look Find Yourself	83
Fig. 15.7	Vernon and Jo Kwong, Liberty Fund, Lone Mtn	84
Fig. 15.8	Lone Mtn Ranch C&W, Find Yourself	85
Fig. 15.9	Candace, Cate, Vernon, Charlie, Maverick C&W	85
Fig. 16.1	2002 American Nobelists in Oval Office	111
Fig. 16.2	Nobel Prize Art and Gold Medal	111
Fig. 16.3	Vernon and Candace at Nobel	112
Fig. 16.4	Vernon and Ethelmae Humphreys, his favorite Missourian	112
Fig. 16.5	Candace and Sandy	113
Fig. 16.6	Carol and Josh	114
Fig. 16.7	Mary Louise Rasmuson, Vernon, Anchorage	115
Fig. 17.1	Joyce, Eric, Torrie, Deborah, and Yukon Hot Springs 1965	137
Fig. 17.2	Josh and family, the way we were 1981	138
Fig. 17.3	Dad with Josh feeling what it is like to be mom, 1981–1982	138
Fig. 17.4	Our loving Corporal Oscar in memorium	139
Fig. 17.5	Candace and Vernon, the way we are	139
Fig. 17.6	Deborah, Dad and Tal	140
Fig. 17.7	Torrie and Jim	140
Fig. 17.8	Dad, Marfa Texas, trip with Josh 2015	141
Fig. 17.9	Eric's IRS Lifetime Achievement Award from Commissioner May 2017	142
Fig. 18.1	Friends long standing ESA meetings 2016	150
Fig. 18.2	Innsbruck Bohm-Bawerk lecture with Jurgen Huber Left and Michael Kirchler June 22, 2015	150
Fig. 19.1	KU Honors Great Opera Star Joyce Didonata; lucky me on the left 2014	175
Fig. 19.2	KU Honors Shelia Bair, heroine of action in Great Recession 2012	176
Fig. 19.3	Neighbors Candace, Vernon, Steve, and Stephanie	177
Fig. 20.1	Sydney friends Andreas left Marcos right July 30, 2015	186
Fig. 20.2	Vernon and Ken Arrow Jerusalem July 11, 2016	186
Fig. 20.3	Sydney interview July 29, 2015	187
Fig. 21.1	Bagpipes and Scottish color celebrate Adam Smith	211
Fig. 21.2	Pullig the string on Adam Simth; Queen declined in favor of an American July 4, 2008	212
Fig. 21.3	Me and Bobby burns	213
Fig. 21.4	Candace with Adam; now there is a pair	214
Fig. 21.5	Towering into the twenty-first century	215
Fig. 22.1	Vernon and Candace, Christian Marriage Anchorage	233
Fig. 22.2	Sam Gregg and Vernon Acton Institute	233

Part III

East, Southwest, East Again

…magnificent…! I just got off the plane where I read the last 100 pages of "Discovery: A Memoir." What a stupendous book. It will – perhaps long after all of us reach 106 – become a cult classic. It is a stunning combination of biography, history, economics, and philosophy. With a travelogue and a heavy dose of warm-hearted Americana tossed in for good measure. I learned some wonderful things in the book, had a joy reading it, and marked it up beyond measure...to read to my 11 and 13 year-old daughters (although, I had to slam the volume just prior to my Marilyn getting to the Utah rock painting feature: the Anasazi porn.)…Even if you did eat your pet chicken…I am delighted to be a reader of your Memoir I, and eagerly anticipate Volume II.

—Tom Hazlett (June 3, 2011)

13

Yankee Land

In 1967, Joyce, Eric, Deborah, Torrie, and I moved to Sherborn, Massachusetts, where we would live until 1972, with Joyce serving in her first full-time position as a Unitarian minister. Joyce finished her degree at Meadville Theological School, University of Chicago, in 1967. Although women in the ministry were not new for Unitarians, shall we say that they were not exactly mainstream. Joyce was essentially a trailblazer in the emergence of a much greater recognition for women. That process, however, got off to a discouragingly slow start.

Joyce was at the top of her class with excellent credentials, but she received expressions of interest from only two congregations—one in Washington State and another in Massachusetts. Early on, we had agreed that she would locate a position best for her and her career. Wherever that was I would find a position there as best I could. I was resigning a chaired full professorship at Purdue, and anticipated that surely I would have no difficulties in relocating somewhere. However, I had strong hopes for the congregation in Washington State. I knew and greatly respected Doug North at the University of Washington. I had been there to give seminars, and I planned to contact Doug if Joyce worked out something in that region. Wherever we went, I knew that I might have to accept something temporary at the beginning because we might not know Joyce's decision until summer. Since I had no sympathy for the tenure system and had long favored its abolishment, I was comfortable with resigning from Purdue before knowing anything about my future. Freedom is ever available in this wonderful land; all you have to do is to exercise it without fear. There will be losses, of

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course, and I have had my fair share, but new opportunities and gains theretofore not available. "Breaking loose" was indeed our experience.

Joyce gave a sermon and visited the congregation in the Seattle area. She received high compliments on her sermon—Joyce was a poet and a deep, thoughtful speaker. The congregation considered her candidacy very carefully, but decided that they did not want a "lady minister." Wow, was that amazing—not necessarily the decision or the reason for not hiring her, which didn't startle us, but the fact that it was put in writing and signed officially! It was honesty write large, without shame. There was no way that I could imagine at that time a Unitarian congregation considering a black candidate, then turning him or her down because they did not want a "black minister." So much for the facts and the times—they were a—changing at a snail's pace for professional women.

It was much different in Sherborn. They liked her, gave her an offer, and she accepted. Sherborn was one of many historical New England towns—George Washington passed through Sherborn in his stagecoach on the way from Cambridge to rejoin his "army," and to hear the locals tell you about it, you would have sworn that it had just happened last week. As soon as we learned of our destination, I was in touch with my contacts at the Harvard Business School, the MIT Sloan School, and Brown University. I told them I was moving with my wife to Sherborn, and I was looking for a job. They all responded favorably to the idea of some kind of position, but Brown seemed best for me in terms of interesting teaching opportunities, and I was bringing an NSF research grant with me. It probably helped somewhat that Brown was cutting my salary less than I would have had to accept from the others (the HBS Dean complained of my Purdue salary, which meant that I was worth less to them, as indeed I was). However, much more important, Brown was an easier commute on the back roads from Sherborn to Providence, Rhode Island, than from Sherborn to Cambridge. The East Coast was in vigorous competition to cut my salary! I ended up at Brown, but with leave as a visiting professor at MIT in the first semester. I wrote up some research, thought about the design of some new experiments, and developed some new ideas, but mostly I just sat there in my office thinking, writing, and picking my nose.

The Sherborn congregation was wonderful. I was very excited about this new venture and a return to my Unitarian roots with Joyce and the family. The twins had turned seventeen in May, and Torrie had turned twelve in April. These would be interesting years living in Sherborn, where for the first time I absorbed a sense of Yankee New England culture, a sense not eas-

ily acquired when living as a graduate student in Cambridge from 1952 to 1955.

I soon discovered why we were in Sherborn. New England has a tradition of strong women with outstanding leadership and intellectual qualities. For Sherborn, Joyce was a slam dunk.

Sherborn and nearby Dover, where the high school was located, were Boston Brahmin country towns. They housed the upper, upper-class that I soon learned represents old wealth: the Cabots, Lodges, Saltenstalls, and so on. Wealth has to be in the family for more than a few generations to count in New England, and the amount of wealth is much less significant than how far back it goes. The Kennedy's were nouveau riche and counted for nothing with this crowd. A few local families owned islands off the coast of Maine, and one of the parlor room stories was about the Kennedy's (it never made any difference which ones), who were exploring the purchase of a Maine island that was up for sale. The scuttlebutt was that when that information got around, one of the Brahmins bought it to keep the Kennedy's out of that Maine island community.

Of course, Chappaquiddick soon came in for much juicy gossip by people claiming to be in the know about what really happened with Mary Jo Kopechne.

The East Coast cockfighting tradition is strong in the cultures of New York Puerto Ricans and New England Yankees. We discovered the Yankee expression of this tradition shortly after moving into Sherborn. Our two dogs soon became locally famous: King, who was of uncertain ancestry; and Tanya, who was a ninety-pound AKC-registered Alaskan malamute. King was a healthy old guy who had adopted us in West Lafayette in 1956. I had purchased Tanya from a malamute kennel in South Bend, Indiana, in 1960 when she was four weeks old.

King was an accomplished hunter of rabbits, squirrels, and raccoons. He taught Tanya the ropes, but she never quite developed the finesse with which he captured squirrels. If he flushed a squirrel, instead of running directly at it, he ran sideways to position himself so that the squirrel's target escape tree was between him and the squirrel, putting him out of the squirrel's field of vision. Seeing that King was not bearing down on Mr. Squirrel, the latter bounded in a more relaxed manner toward the tree. Suddenly, King appeared from behind the tree and a surprised Mr. Squirrel would take to the air to leap over King, often with success. Tanya was far less effective, however, and, without a hint of sophistication, would barrel directly at the moving squirrel—dirt, brush, rocks, and leaves flying in all directions. For this reason, it seems, King sometimes slipped off without Tanya when he had the urge to go for squirrels.

Dog owners can tell you all kinds of amazing mental feats their pets can perform, like mine about King. Scientific studies increasingly document them. In June of 2004, for example, *Science* reported experiments with "Rico," a nine-year-old border collie whose owners claim he knows the names of some two hundred objects in his huge collection of toys.[1] Carol Breckner sent me the article in an email that she entitled "Rico is smarter than the average university administrator." Although we have no controlled experimental data on her hypothesis, I've seen some interesting comparative clinical observations. The experimenters tested Rico for his naming skills by putting ten of his toys in a room isolated from his owners. Then they instructed Rico to fetch two randomly selected items at a time identified by name. In forty tests, Rico got thirty-seven correct—this particular noble dog had a vocabulary comparable to those of dolphins, apes, sea lions, and parrots that have undergone extensive professional training! Moreover, the researchers then repeated the test, now on each trial putting seven of Rico's familiar toys in the other room along with one he had never seen before. His owner then called out the unfamiliar name of the new toy, and Rico correctly retrieved the new item in seven out of ten tries. Carol was right!

Humans are supposed to be the only animals to use language in novel new ways, but that is probably just academic horse manure. It does not explain a number of animal accomplishments, as well as some controlled experiments noted by Temple Grandin (and C. Johnson) in *Animals in Translation*. For instance, Alex, a gray parrot, was taught to identify colors verbally by sounding them out (such abstract concepts were once thought to be impossible for birds): One day Alex, seeing his own image in a mirror, asked, "What color"? Upon being told, "You're a gray parrot," and after asking six different times, Alex was able to identify this new color in other objects.

Nor do animal (bird) brains lack the capacities necessary to initiate other novel adaptations. Blue jays, famous for hiding food, do so regardless of whether other jays are watching; but when other jays are watching and are then removed from view, the jays dig up all the mealworms they had hidden while being watched and relocate them in other hiding spots. In another experiment, two crows, Betty and Able, must learn to choose a hooked wire rather than a straight wire to access food. Abel snatched the hooked wire from Betty one day, leaving her only the non-functional straight wire, so she bent the straight wire into a hook! Moreover, she did it nine times using different techniques, even changing the angle to improve the fit of the tool. Nothing in nature can be bent to hold its shape like wire. Betty was not resurrecting some primitive birdbrained knowledge.

Scientists, as is their wont and duty, are skeptical of all this, but to this one it is completely natural that such a "fast-mapping" brain module (the name given by researchers for Rico's learning in the above exercise) would have emerged early in mammalian forms dating back to the dinosaur extinctions. Such a brain would have excellent adaptive value in foraging. Our King knew that if, on seeing a squirrel, he moved sideways to get the tree between him and the tree the squirrel was aiming at, and then raced to the tree to surprise Mr. Squirrel

[1]The original article plus new references and additional documentation can be accessed at, ftp://ftp.soest.hawaii.edu/engels/Stanley/Textbook_update/Science_304/Kaminski-04.pdf.

on the other side, he increased his kill rate. Tanya just barreled at the squirrel—dirt, gravel, twigs, and leaves flying—and always failed, but she had not spent any of her early life living off the countryside, whereas King had been a stray. These modules appear to need input from the environment to be initialized and developed for particular contexts. There is much validity in the saying "You can't teach an old dog new tricks."

But specialization is important, even for dogs: Tanya brought brute force to the hunt, and that was handy for King on other challenging missions, particularly coon hunting. Coons are extremely dangerous to dogs—half cat and half dog, they can tear open a dog's underbelly—but against the two of them, even the biggest coon had not a prayer. I watched Tanya catch and dispatch one on a moonlit night next to one of Indiana's bass lakes. King was not with us because he tended to roam too far, and I wanted to fish, not hunt for dogs. Asleep, I was awakened by Tanya. Tied to a nearby table, she was greatly agitated by something. I pulled out of the sack, donned pants, and looked around—there was nothing anywhere and you could read newsprint in that moonlight. I untied the leash end of her tether and walked her around the Scout to show her that all was well, but I was wrong. As we rounded toward the front, she suddenly bolted to the front wheel and pushed her head up into the wheel nacelle to the top of the tire. When she came back there was a coon in her jaws. She held it firmly against the ground, not turning it loose, clearly aware that she'd be in deep shit if she did. She kept her grip with long teeth rummaging around in head, neck, and flesh. After the coon was still, she very cautiously and slowly began releasing her grip. There was a slight wriggle, and again she sank those one-inch-plus malamute canine fangs into flesh. Finally, all was still, and she backed off, looked up to me, and wagged her bushy tail. She knew what she was doing. I did not.

One Sunday in Sherborn, I awoke at 6:00 a.m., and as I was getting dressed, I looked out the front bedroom window. In the front yard below, King and Tanya had stretched themselves out contentedly surrounded by a sea of chicken feathers. Damn, whose chicken coop had the two of them raided? I went downstairs to survey the damage. I was new in the neighborhood, and I walked around to see if I could find anyone with a chicken yard. I found nothing. I returned and cleaned up the yard and watered but did not feed the dogs. They did not need a reward, and it was evident that they would have had no interest in eating the usual fare. I decided to wait and see what I could learn about the source of their comfort.

The next day was Monday, and Eric, Debra, and Torrie were off to school. Torrie came home on the afternoon school bus and was in tears by the time she was at our house. She said that the other kids on the bus were saying that our dogs had gotten into the Greys' chicken yard and really created havoc. Torrie was convinced that they had it wrong, that King and Tanya had done no such thing. I pointed out to her that it was no doubt true, and that we would just have to make amends.

Some distance up the road from our first home in Sherborn was some acreage and an old farmhouse owned by Bob Grey, whom I now was able to identify. I went to his house, introduced myself, told him what I had learned, and told him that I was there to pay damages. He took me into his backyard, to show me his chicken yard. It was a fenced area. Inside the fenced area were four wood-framed cages, each enclosed with heavy-gauge fence wire. The dogs had entered the larger enclosed area by digging under the wire fence, as I knew them to be completely capable of doing. To keep them in my fenced backyard in West Lafayette, I had wired the fence with a charger suitable for confining bulls—about 10,000 volts—and they lost interest in their previous habit of tunneling under that chain-link fence.

The hens in Bob Grey's chicken yard were in the enclosed area, but outside the cages. Bob said that all but a couple of the hens escaped by going up onto the lower branches of the trees. I thought that involved more female chicken agility and smarts than I had ever witnessed among my mother's chickens, but I accepted it nonetheless. The four cages, however, had been ripped open. Bob Grey was amazed that all of them could have been ripped open, and the contents—a rooster in each—taken. I was not in the least amazed. Tanya had feet as big as my fist, with long heavy claws, and she could easily negotiate the wire with little help from King, who was more likely to end up with a torn nail. I would pay for the damages.

He said that the hens were of "little value," but the roosters cost him $150 each. Wow, some roosters, I thought to myself! In response to my puzzlement, he explained that they were really pets and were a valuable special breed. I had never heard of any special breed of chickens that were pets, and that needed separate confinement in wire cages! And each cost $150 at 1967 prices? None of my business, so I thought nothing of it, wrote him a check for the roosters and some change for the damage, and went home. I soon learned that it was all over Sherborn that, days earlier, the new Unitarian minister's dogs had broken into Bob Grey's fighting cock cages and eaten them all! That little New England town buzzed with the novel news and loved the humor of it all. I learned about an ancient Yankee tradition; and why those "pet roosters" were so valuable compared with the hens.

Someone pointed out to me that my homeowners' insurance covered property damage by an owner's dogs. I called the insurance company, and confirmed that that was true. An insurance adjuster came to the house to investigate, since the bill was $600 just for the birds. I told him the story. Naturally, he wondered why those roosters were so pricey, but accepted the pet rooster explanation, no doubt confirming with Grey as well. The insurance company reimbursed me and the matter was settled.

I forgot to mention that the sport of cockfighting is strictly prohibited in New England and throughout the USA. This prohibition accounts for the circumspection about pet roosters in Yankee land, where the culture thrives in spite of laws against it. Generally, it is called "order without law," the title of Bob Ellickson's book, but in this case, it was order contrary to law. Cockfighting is also a cultural attribute of Southwestern Hispanics as well as New York Puerto Ricans, but my dogs never tangled with any of theirs, or they might not have survived as my pets.

Dogs were domesticated from wolves (not jackals or coyotes, to which they are also genetically related). The archeological record shows domesticated dog bones buried with human bones as far back as 14–15,000 years ago. But recent DNA evidence shows that domesticated dogs diverged from wolves perhaps 35,000 years ago.[2] Also fossil evidence finds wolf bones in the vicinity of human bones. Wolves and more recent humans, 11–12,000 years age, share many social attributes: team hunting; non-kin, and same-sex associations; territoriality; and, of course, they collaborate to implement big game hunting strategies. These observations lead to the hypothesis that humans co-evolved with wolves, then dogs evolved from wolves: Human fitness was facilitated by human-wolf cooperation, implying that wolves affected our evolution, and vice versa.

The Sherborn oral history tradition was something to behold. I once heard a stalwart member of the church, Mrs. Douse, speak. Her family owned a large apple orchard in the town, and she was president of the Sherborn Historical Society. She talked about Sherborn people having received word of the battle at Lexington and Concord. The Sherbornites immediately dispatched a contingent of volunteer fighters, but they arrived too late. The battle was over, and the colonists had already beaten the Redcoats. Hearing Mrs. Douse speak of this early history, I had to remind myself that she could not have been there at the time. It was such a living reality for her, however, that she sounded as if she were giving an eyewitness account.

Soon after we arrived in town we were invited to dinner by one of the members of Joyce's congregation. Their house originally had been built in the eighteenth century, prior to the Revolution. The owner proudly took us upstairs to see the "'colonists' sweet revenge," an outsized floor plank under the rug. Apparently, all trees in the colonies that exceeded one and a half feet in diameter had been reserved to serve as masts in the Queen's Navy.

[2] https://www.sciencedirect.com/science/article/pii/S0960982215004327.

So these colonists had gone to the trouble of harvesting just such a tree and cutting a floor plank to be installed upstairs, covered by a rug. I loved it. The revolutionary spirit was still alive in the pride of the New England Yankees.

Joyce and I soon attended our first New England town meeting. What an experience. We all voted on every detail of municipal action, such as whether or not to buy a new police cruiser. No doubt about it, transaction cost increases rapidly with group size, but it is not perceived as a cost by those who attend the meeting—it's a big social occasion.

The American Economic Association meetings were held in San Francisco sometime before I was expecting to leave Purdue. These meetings, fully capable of boring you to tears, were memorable that year for several reasons. One was George Dalton from Northwestern, standing in the lobby talking with John Hughes. The Shriners were also meeting in San Francisco. George looked up and saw six or eight Shriners walking down the hall, each wearing their truncated cone hats with the tassels hanging down to the side. George said to John, "Look, there goes the board of editors of the Econometric Society." (Someone since has called the society's journal *Ecclesiastica.*)

While I was there, I got a call from an academic chemist, Moyer Hunsberger, Dean of Arts and Sciences at the University of Massachusetts. He wanted to talk with me. As dean, he had put together an outside committee to advise him on how to recruit a good economics department for Amherst. The committee was composed of Bob Solow, Larry Klein, and George Borts. Knowing of the Purdue years, they suggested that Moyer should try to recruit me to enlist in helping to go after some others. The committee was aware of our department-building efforts and success at Purdue. I was not then in a position to consider Moyer's proposal and I declined the offer.

For some time, I had not thought about my conversation with the dean, but he contacted me again after our move to Sherborn. The short version is that he made me an offer that I couldn't refuse: He restored my Purdue salary—I think it was about $30,000 in 1967—and I would teach one course per semester, as I had been doing at Purdue as a Krannert Outstanding Professor, and continue my research. I accepted, and ultimately at U. Mass. we recruited Rich Kihlstrom, Lennie Merman, Hugo Sonnenschein, Ron Oaxaca, Jim Cox, John Roberts as a pre-doc visitor, and assorted other outstanding people, but it was short lived.

Moyer ended up in one of those garden-variety university political hot corners—in this case, his mail was being intercepted and opened, and it was discovered that he was working on a recruiting coup that would bring in yet another group of well-known scholars—I think in history—from out-

side. Moyer had been caught in technical violation of faculty union rules, and being of high moral character, he felt that he had to resign. His many supporters, the chancellor, and our group could not turn him around. As they say, the reason that academic politics is the worst there is anywhere is because the stakes are so low. Faculty like to divide the existing budget among themselves, rather than use it for development, which tells you why any sort of academic entrepreneurship is all but impossible. I would find that characteristic in spades, years later in Arizona, and seven of us would leave for better opportunities elsewhere in 2001. At Arizona, I and my associates would be charged (off the official record of course) with fraud and criminal behavior, and a witch hunt would be set loose that would end with the tacit admission that the original charges were without merit—"no admission of wrongdoing" by all parties was the official face-saving language.

In due time, we got a new dean at U. Mass. who felt the department was badly unbalanced: We needed a Marxist theorist, a Marxist labor economist, a Marxist development economist, a Marxist monetary theorist, and so forth. He moved to do it. The criteria were now political worthiness, not scholarship. We insisted, however, on one basic condition. The new hires had to be top-notch economists, and I am damned if he didn't end up finding and recruiting the best: Herb Gintis, Steve Resnik, Sam Bowles, and assorted others. Furthermore, because their opportunity cost was low, they were all hirable. I had grown up with left-wing types through at least age twenty-one, and for me it was just a bunch of intrinsically smart people whose natural maturation and education had been delayed by several years. I think I was about 65% right in this evaluation. Most of them matured enough to go on to higher and far more interesting accomplishments.

The crew we had put together at U. Mass. was in big market demand, regularly getting offers all over the place, and the new dean's heavy-handed intervention—not new, but in a new direction—raised the local transaction costs too much in trying to defend the ramparts. So, we were all off to new challenges. It's remarkable how much damage can be done by an administrator who has no deeper vision than one driven by ideology, whether of the left or the right. Most people cannot make a judgment of anyone until they first determine what they think is your politics. They are baffled listening to your talks about economic issues if they get a mixed message and can't locate the box to fit you into. Then they get you tagged as a "conservative," but find that you opposed the Vietnam War, or are astounded to learn that the "conservative" Cato Institute was dead set against Desert Storm and the latest Iraqi war, whatever it was called. The great thing about America is that

you are free to be independent, and there are lots and lots of us out there, but independent in dozens of different ways.

In the end, the new department scattered to better jobs and more freedom. We gave up on the department-building business at U. Mass. Steve and Herb tried to convince me to return after going to California in 1972–1973, but I was worried that they wanted me to be the showcase "house black"; i.e., be the minority member ("non-radical") who kept them honest. I think I may have been wrong in that concern, but it was real to me at the time. I would have been OK there, but I had lost my colleagues. U. Mass. had an interesting blue-collar community, with lots of upwardly mobile kids who could not have gotten into prep school—the sons of Boston cops and firemen and tradesmen, to whom I could relate—but the politics of that one-party state were scary.

The legislature constantly and dependably tried to micromanage the university. This is a problem generally with state universities. It is why a little privatization is in order, but in Massachusetts it was carried to very clumsy heights; for example, clipping the chancellor's (Oz Tippo's) budget freedom upon learning that surplus funds in the coal budget were being used to buy library books. Shame on him, exercising local discretion when the legislature knew best!

We all stayed in contact with each other. I often took fishing trips west to Utah, Colorado, Telluride, and Lake Powell, and one summer three of my former U. Mass. colleagues and wives joined us for a week. I had bought a boat, stored it at Lake Powell, and bought getaway property in Monticello, Utah (Figs. 13.1, 13.2, 13.3, 13.4, 13.5, and 13.6).

Fig. 13.1 Monument Valley where the west begins, 1981

Fig. 13.2 House Trailer Gulch on Road to Lake Powell

Fig. 13.3 Campsite invasion by flotilla

Fig. 13.4 Ken Slight CO River triple rig

Fig. 13.5 Havasu Canyon meets CO River

Fig. 13.6 Vernon rainbow 1 of 4 CO River

14

West with the Night

Out here we call it (BLM)
the bureau of bad management.

—Pete Steele

Basically, I was a fish out of water in Massachusetts, and I was ready to return to my Western roots. Those roots had already taken me throughout the Southwest: Colorado, Utah, New Mexico, and Arizona in the early 1960s, and more seriously 1964–1967 after I bought my Scout at Purdue. While living in Sherborn, I would fly nonstop Boston to Denver, meet Charlie Plott and occasionally, our Merrill Lynch Indianapolis stockbroker, Langdon Kumler. From Denver, we would take Frontier Airlines—the one today is apparently a much later revived form of the first one that went bankrupt—to Grand Junction Colorado, then hop over to Moab, the airplane returning to Grand Junction the next day. At Moab "International," we would have a bush pilot from Hub Airlines fly us into Hall's Crossing, where we would rent a boat. The Hub flight would be in a Cessna, and I remember the pilot took one look at Charlie, then scanned Langdon, a six-foot, six-inch mountain of a man—Langdon had lost weight and was down to about 280 pounds—then he looked at me, relieved slightly, and said, "I sure hope you guys ain't got much baggage." We piled into his Cessna, and he hopped in with a small cloth bag. I said, "What's that for," as I knew that he was flying right back to Moab. He said, "That's my survivor kit," and hit the starter button. By early the next day after leaving Boston, I would be on Lake Powell with these bass hunters, but only after a long night sleeping in a mobile home, rattling from one end to the other with the snoring of

V. L. Smith, *A Life of Experimental Economics, Volume II*,
https://doi.org/10.1007/978-3-319-98425-4_14

Langdon, whom we always banished to the other end of the house, closing all the doors in between.

My family really learned to love the Four Corners area—Delores, Cortez, Telluride (before it got ski-slope fever), Monticello, Moab, Blanding, Mexican Hat, Kayenta. I have already written of our introduction to the area back when we bought our Scout 80. By 1969, we had bought an eighteen-foot outboard cabin cruiser and we rented dry-dock space for it at Hall's Crossing. Then we looked for a piece of land in the general area, and after considering two large acreages over in Colorado, we ended up buying a four-bedroom house on a half-acre inside the city limits of Monticello, Utah. It had a couple of dozen Mormon-planted fruit trees, and we were as happy as the proverbial pig. We lived summers in Monticello. I wrote, enjoyed boating and fishing outings. Joyce and I—sometimes all of us—four-wheeled from Canyonlands to the Maze; from Telluride over the spine to Ouray or up Black Bear jeep trail and over its 13,800-foot pass to the Million Dollar Highway, across the highway on a trail to Engineer Pass and down the back way into Silverton. Joyce, Torrie, Eric, and Deborah sure got big doses of mountain living. No wonder Torrie ended up with her husband Jim on a remote acreage in Colorado's San Luis Valley with a well-water pump, two goats for backpacking and solar panels for powering lights and the well-water pump in their Earth Ship home. Years later when I read Abbey's, *The Monkey Wrench Gang*, I could recognize all of Hayduke's jeep chases in southeast Utah.

One summer Joyce and I drove from Monticello to Blanding, where we could attend the nearest movie theater in that area. We went to see *A Man Called Horse*. Well, friend, this was Native American (known then as Indian) country, and if you want to know what it is like to feel like an outsider, see that movie in Blanding, Utah, surrounded by Native Americans cheering and yelling as the protagonist is dragged behind a horse and otherwise tortured with hooks in his skin. They relished the spectacle of that white man getting his just desserts. Joyce and I sort of slumped down in our theater seats hoping not to be noticed. Afterward, we high tailed it out of there back to Monticello.

During the 1960s and 1970s, we would do four Grand Canyon white-water trips, embarking in rubber rafts from Lee's Ferry, just above the Navajo Bridge across the Colorado River, and disembarking at Diamond Creek, some 240 miles downstream. These are the only two points of easy road access to the river's edge from the eastern side of the Colorado across to the western side. Elsewhere, access is blocked by high cliffs and ragged peaks. Today, you get access to Lee's Ferry by crossing the Navajo Bridge

and driving downstream a short distance from it—the only bridge over the Colorado between Glen Canyon and Hoover (or is it Roosevelt?) Dam. Disembarking on rubber rafts from the west bank of Lee's Ferry, you first run through Marble Canyon. That ends, fifty miles south to the river's confluence with the Little Colorado draining in from northern Arizona, and bringing with it a load of mud; next, through the various geological phases of the Grand proper until the Canyon begins petering out at Diamond Creek, just above Lake Mead.

At journey's end, when you disembark at Diamond Creek you are on foreign (Native American) soil, and you have to contract with the locals to pick you up and transport you to domestic soil. On one of these transitions, Ken Slight, our guide and the outfitter for Ken Slight Expeditions, sat behind the driver. The driver and Ken knew each other; as they talked, I listened in. The Native driver brought Ken up to date with new developments, celebrations, and plans in the community. A new tribal recreation center is now nearing completion, and come August it will be dedicated in a huge celebration. People will be coming from all over; there will be contests; games; and other great social events. Without the least politically correct hesitation, Ken asked, "When will the fights start?" The driver was offended not in the least, and excitedly began reciting other recent events in this category, indicating that they would no doubt begin most any time after the launch of the celebration.

We also did one river trip embarking at Green River, Utah, on the slow meandering Green River down to its confluence with the Colorado River in Canyon Lands, well upstream from Lake Powell. Picture Lake Powell nearly 200 miles long behind the Glen Canyon dam impoundment. Below the dam about 30 miles is where you come to Lee's Ferry. Floating the Green, some three-hundred-odd miles upstream from Lee's Ferry, nothing has to be tied down on the rafts. We would enter the river in bathing suits, float along with the boats in the warm river water, stopping now and then for hikes up beautiful side canyons, loaded with rock art, and climb up to each canyon's flat-topped overlooks. That is where early descendants of the First Americans would have camped, and manufactured arrow and spear points, and other cutting and scraping tools. We found discarded artifacts of these early hunter-gatherers and observed where they watched for game, friend, and foe. Rain exposes new artifacts, so after a rain it always pays to explore again terrain that has been examined before. The physics of exposure is simple on these cliff-tops: the arrow points and flint cutting tools are less dense than the fine-packed soil. Hence, the hard rains pack down the soil and the tools "float" ever so slowly to the surface.

On the cliff-sides are the dwelling places of these Native Americans with mano and metate grinding stones still to be found. A long hike up Barrier Canyon will bring you to the cliff-wall displays of art, and to the truly astonishing, awe-inspiring panoply of life-size figures floating with majestic, ghostly beauty and wearing studded crowns like the chieftains and shamans of a prehistoric fairy tale.

Joyce and I have also four-wheeled into this same region from landside, rich in the "Barrier Canyon Art form." Here, it is impossible to ascend the steep sandy slopes without letting much of the air out of your vehicle's tires. In deep sand you can easily bog down, running on hard tires, with all four wheels just mine sand with minimal forward motion. My Scout 80 always rolled on tube-type tires, and we carried an old hand-operated tire pump to re-inflate the tires when we returned to packed-earth trails. When I bought a second Scout in 1972, I had tubes installed in its tubeless tires. At around 10 pounds per square inch pressure, the seal around the rim side of a tubeless tire will break open and let all the air out. The dealer who sells you the tires, which he also guarantees, winces when you explain to him why you need the tubes installed in tubeless tires.

Where the Green River joins the roaring Colorado, you pull the rafts over to the bank, rope everything securely to the boat frame, and get ready for Cataract Canyon. We'd rope three separate rubber rafts side by side into a triple rig with the outboard motor at the back of the center raft. This arrangement provides dynamic stability in riding waves that would engulf a single boat. There are no rapids in the Grand like those in Cataract. Cataract has only wild undammed river upstream from it and therefore nothing comparable to Lake Powell upstream from Lee's Ferry, releasing water that generates electricity according to the diurnal cycle.

Hydro is a valuable source of peaking energy, wasted if used for the delivery of low-value base-load energy. Hence, you generate power in the peak-load daytime hours, and let the lake refill at night. This gives you alternating pulses of water flow determined by power deliveries in Los Angeles and Phoenix and reminds you that the long wire tentacles of distant cities reach out to you and project their undulating rhythm into that beautiful remote wilderness. Some find that reach disagreeable, but the tentacles also transmit a beauty connecting commerce with remote wilderness. At Lee's Ferry—only thirty miles through the narrow Glen Canyon downstream from the dam—the flow-level pulses are large, and ever so gradually, their amplitude dampens to nothing perceptible as you move down-river to Diamond Creek.

Where the Green joins the Colorado you are basically negotiating a wild river. When we left the banks of the confluence, the springtime flow was 60,000 cubic feet per second, nearly four times the typical off-season flow. When we pushed off, our hearts were pounding with anticipation as we approached the big drop—three back-to-back descents by the riverbed with only short spaces between. Our knuckles were white, our sphincters tight, as we held onto ropes wrapped around the raft tubes. The backwash of the first great wave swamped the Mercury outboard, we lost power, and as Ken Slight yanked the cord, attempting a restart, the rig slowly and steadily drifted from a proper diagonal orientation with the back-rushing waves, giving us maximum protection from a rollover, to a straight left endwise orientation, giving us the absolute least rollover protection.

I was on the outside leading rear edge of the first raft. As we started from the bottom of a twelve-foot plus wave, I looked up at its edge in the sky, and as all such waves are wont to do, it curled back toward us, and I knew that there was no way we could ride through that backwash without a curling rollover. The only question that momentarily crossed my mind was whether it would roll all three rafts into a big bloody *C* and dump us bottom side up into the current. It did not. It was an upside down *J* roll: The first rubber raft on our triple-rig pancaked over on top of the middle raft, but the other two held against another tumble. I ended up on the right rear edge of the middle raft to the right of Ken's motor position.

No one (and none of our gear) was dumped into the river; such was the skill with which Ken Slight had lashed the rafts to each other and all the gear to the rafts. Only the people are loose, raft-top, as no one wants to be lashed to a raft that goes over. It is much better to break free and float with a buoyant life preserver, hanging on to the side of the boat. We were badly shaken, to say the least; there was one bleeding nail but we were otherwise whole. (The injured hand was that of the poetic Ann Zwinger, one of our rafters; she was photographing and writing for her book *Downcanyon: A Naturalist Explores the Colorado River through the Grand Canyon*, 1995). The motor restarted and we pulled quickly to the bank to inspect everything, including ourselves, before the next drop.

The next two drops and all later rapids were negotiated without mishap, and we didn't lose power again. Ken hit them diagonally, and it takes more than a 60,000-feet-per-second throughput in Cataract to roll that first raft when it is leading diagonally with a front edge.

Georgia White, the Colorado River running legend, went through Cataract Canyon at a scary, precedent-setting flow rate of 100,000 feet

per second. How was that possible? She did it by flat-out cheating nature of her fury: Georgia lashed three of the giant pontoon boats—not our little Green River rafts—into a triple rig that might even have gone through the Perfect Storm without capsizing.

All our whitewater experience was with Ken Slight of Slight Expeditions in Green River Utah. Ken's idea of an off-season vacation was to go to South America and run its rivers, or to Alaska and run the Yukon, or to Africa and run God knows which forsaken river. I could write more of the life, adventures, and loves of Ken Slight, but there are better sources. Read *The Monkey Wrench Gang*. Seldom Seen Smith, the Jack Mormon river runner, is modeled on the Ken Slight of real life. I recall in the opening scenes he is driving across Glen Canyon Dam when he stops, dismounts his four-wheeler and kneels to pray for a "precision earthquake" here and now. I will say only that you needed to be tolerant, loving, and forgiving of a wife, girlfriend, or female boat mate who accompanied you, because Ken excelled as a womanizer. I am reminded of the song "Sheik of Araby": "At night when you're asleep, into your tent I'll creep." Though in his case, the Sheik stayed comfortably in his tent after a heavy day of river running, and the ladies did the creeping.

We also did horse-pack trips with Pete and Jackie Steele's Horse Head Pack Trips in Monticello, Utah. These were up to nine-day horseback excursions through Grand Gulch, Salt Creek Canyon, or Dark Canyon. Actually, Pete had run packs up to thirty days—a defining feature of thirty-day packs is that toward the end the only way you can fry eggs is scrambled. I think I was either a customer or a working packer on seven or eight of Pete's trips altogether, but it could have been more. Pete and Jackie would occasionally have only two or three paying customers, not enough for a normal pack trip, but they wanted to build the business. Pete would ask Torrie and me, who were his Monticello neighbors, to fill out the group at a discount rate. Eventually, after I learned the ropes, and he could trust me to roll sourdough biscuits in the flour sack and bake them in a Dutch oven, Pete would take me along as his packer and cook.

You may not be able to comprehend it, but that was my finest honor, exceeding honorary degrees, promotions, chairs, and the Nobel Award!

Pete was phenomenal. He once spotted a perfect arrowhead from horseback, riding in a pebble-rich creek bed. He unaccountably stopped, dismounted, bent over, looked at a sea of jumbled pebbles, picked something up, and showed us a fine piece of craftsmanship in flint, jasper, or onyx. For Pete's trained eye, the craftsmanship and symmetry in that artifact allowed it to stand out boldly in a pebble-strewn landscape. He knew where the

remote ruins were located and where you could see all the finest rock art. He is a can-do master in the spirit of McClure Stilly, the Kansas quarryman.

The BLM had no idea at the time where all these artistic treasures were to be found. Pete was a Jack Mormon who was the immediate descendent of one of the original eight families in one of Brigham Young's Mormon Missions that settled in the Monticello area. One of the US Park Service brochures touted Angel Arch in Salt Creek as not discovered until the late 1940s. Pete laughed and said, "It's the Park Service that had not discovered it. The Arch was well-known to the cowboys who had run cattle in the region for decades, and who searched for mavericks up Salt Creek." A maverick is a wild calf or cow born of a loose cow in the open range. It's when cowboys are catching mavericks that they wear chaps, spurs, and saddle lassos, not when herding domesticated cattle. Mostly these items are now worn for show or for the rodeo (*ro-dee-o,* not *ro-day-o*), and not for maverick piecework. Open-range ranchers give cowboys a share of what is fetched in the market for all mavericks they bring home. But it's hard work and requires a good price for beef to make it worth donning those chaps and spurs.

As we rode our horses and led the three pack mules down Grand Gulch—a canyon that eventually empties into the San Juan River,—Pete pulled over to the side. We would dismount, tie the horses to trees, and scale the side of the Gulch or hike up a side canyon. Occasionally we would climb to an area with large swimming pool-size potholes, filled with clear rainwater, and go for a swim. Or, as in Salt Creek, we would hike up to a waterfall and take a shower. But more likely we would climb to a panel of rock art—the Green Mask, the Breech Birth scene, the Arthritic Man, hunting and planting scenes, with ducks, Bison, deer, corn, men, women, Kokopelli figures with flutes serenading the women, hand prints, strings of dots, atlatls—spear throwers—and so on. It seems there is no end except what is imposed by civilization in the form of time limits on these trips. Returning again and again even to the same place, always yields new adventures.

The Arthritic Man, as Pete has named him, is a panel with three drawings, left to right in sequence of what is apparently the same man. In the first figure, he is large, robust with broad shoulders. In the next scene, he is shorter, slightly stooped, with slightly swollen knees, and has a cane. In the last, he is much stooped with bulging enlarged knees, hobbling on his cane. Appearing above the figures are skirted women. Pete's imaginative representation is that the old man remembered his youth and the dancing girls that he entertained. Then he started to get arthritis, and finally, he became very crippled with the disease, still recalling the dancing girls.

The Breech Birth is high on the left as you go down the Canyon, maybe three or four days into Grand Gulch, where it is wider and deeper. It's a life-size family scene showing a man, woman, and child. A baby, sitting up, is shown inside the woman, low in her abdominal region. Pete sees the man as the father and the woman as the mother of the child. The woman is pregnant with a baby that has dropped down, ready for birth, whose position suggests that it will be a breech birth.

Pete once took me up a side canyon and pointed up to a faint petroglyph (a rock-etched representation, not a painted "pictograph," or picture). The object was pecked out in the dark desert-varnished rock. The sun was flooding out our vision. He suggested that I, "Climb up to get a closer look, and tell me what you think it is." So I inched my way up to a ledge, where I was able to examine it with less glare. I studied it awhile and finally said, "Pete, it looks to me like a man is copulating with a wolf! They are standing face to face. And here is someone standing next to them who must be waiting for his turn, because he has an erection." Pete says, "Yeah, that's what it has always looked like to me." I have a fairly good color picture of the scene. Pete calls it Anasazi porn (see figures at the end of the chapter).

On another occasion, we dismounted and Pete took me up a narrow side canyon that he said he had never explored, having been up it only once. The occasion had been a puma (or cougar) that he sighted ahead of him in the Gulch, which darted into this canyon. He said that he followed it and learned something about pumas' stealth and skill in avoiding humans. The canyon had steep sides and was not even as wide as a regular two-lane country road, but it was very brushy. He picked his way up a city block or so and found that it was a dead end with a high, unassailable wall at the end. What happened to the puma? He found its trail, back down and out of the side canyon into Grand Gulch. That cat had slipped by within a few feet of Pete as he was hiking, and he never knew it. Yes, pumas avoid people like the plague. Moreover, hikers are safe unless they are stupid enough to do that which is probably impossible—corner one. This time we explored that side canyon without the distraction and anxiety of following a big cat, but we found nothing of interest.

In late summer Grand Gulch was usually pretty dry, but you could always dig for water. You take your folding portable (latrine) spade and dig a hole. Digging down a foot or two the sand is moist; dig a little more, and water seeps into the bottom of the hole. Take one of the drinking cups out of the "kitchen" and dip water out of the hole until you get a couple of the large pots filled. Let it settle and decant (pour off) the clear water on top, leaving a half-inch of dirt and sand in the bottom of the pot to be discarded. Boil the water vigorously on the open fire and let it cool. It's delicious, and you prepare enough for cooking and for cowboy coffee in the evening and again in the morning. Sometimes you can just dip it out of puddles in rock depressions, along the canyon floor. It's great so long as you decant it as you would a fine wine and boil it thoroughly.

In the spring, Grand Gulch has plenty of water. The horses and mules often wade in it, and you rinse the dishes in it and use the sand to scrub the pots and pans before they go into the hot soapy dishwater. Sometimes there is quicksand. We never happened to encounter any in Grand Gulch, but we did once in Salt Creek.

The side washes that empty into Salt Creek had water in them intermittently on that trip, so the sand bottoms were often still wet. We were approaching a cross wash where it was dry. Pete was first in our usual single-file string of horses and mules, and I was second, Joyce third, Torrie fourth, etc. We had left the mules behind to do some side-canyon exploration. Otherwise, Pete would lead the mules, and occasionally I would relieve Pete by leading them. Pete started across the sand wash—no more than ten feet wide—which appeared dry, like many others that we had crossed. His horse reached the center of the sand wash before breaking through into quicksand under a dry crust. Pete's horse sank deep enough to get sand and water into his saddlebag, high across the horse's rump, and ruin a camera. The horse's rump had sand up to a foot in front of its tail in back, and to the top of its chest in front. Pete immediately stepped onto the top of the saddle and jumped to the other shore side of the wash, which the horse had almost reached. The rest of us reined up our mounts and did not enter the wash.

I dismounted and crossed the wash only a few feet above the quicksand, where it was solid. I looked for a long branch. At some point, Pete had already instructed me that a stick pushed up and down in the quicksand around an animal would settle the sand and enable an escape. This time, we did not need it. The struggling horse settled the quicksand, regained a secure footing, and walked out.

Here is the physics of it: This quicksand is an emulsion of water, sand, and dirt that has dried superficially on top and formed a crust that looks solid, just like any other dry or moist sand pile. (I can't speak for the quicksand in *Indiana Jones*, but I suspect that it is either very different physics, or just Hollywood baloney like the quicksand in the old Tarzan movies.) If you break through it and slosh around, or push a stick up and down in it, the sand starts to settle to the bottom and the water starts to flow out if it's on a downhill slope. When we rode away from the anomaly in the middle of that wash, water was flowing out of it as if it was coming from a small underground creek. That country is full of underground creeks flowing out of ledges in the ground. The most spectacular is Thunder River that flows into Grand Canyon, which does indeed thunder out of a hole in the ground—an easy hike up from the Colorado riverbank.

Pete said that in Grand Gulch he was once leading the mules and was well across a large quicksand area before he broke through. He and the horse got out, but the mules were bogged down with their heavy packs and could hardy move. He had to remove each of their packs from the top—quite a chore. Mule packs are always lashed on, and taken off, from the bottom of the mule's belly.

Mules are the best of the animal side of a pack trip. They are reliable, dedicated, unflappable on steep-sided narrow trails, very smart, and comedians to boot. This last characteristic shades over into being playful after a long

day under a pack, and their unique personalities are to be enjoyed, like those of Torrie's and Jim's goats in Colorado. Pete had several mules and a packhorse, but my three favorites were the mules: Enid, Jane, and Enos. Jane was a beautiful red, gentle, and lovable. Enid was a kitchen specialist. Enos was a comedian par excellence.

The "kitchen" consisted of two rectangular boxes no more than about three feet high and two and half wide with a sloping top panel hinged at the bottom that Pete fabricated out of strong, but fairly light-weight plywood. The boxes contained spices, pans, utensils, knives, peanut butter, jelly, canned tuna and chicken, pickles, and bread (for lunch, no fire or biscuits, but I always tried to make enough for breakfast to have some left over for lunch so that I did not have to eat any of Pete's store-bought Wonder Bread). When we unloaded the kitchen in the evening, we set each box down next to where the campfire would be, opened the diagonal top of each, and rested them on stakes. That gave us two small tables—very handy in the wilds of Utah. Enid would carry the kitchen boxes—one on each side. When he came to a tree on his right and a boulder just beyond on his left, he knew to dip his right shoulder, get the right kitchen around the tree, then zig to the right and dip the left shoulder to get that box around the boulder. All those mules were cool on a high narrow trail, with a deep canyon on one side. The trail narrowed further and went around a corner with snags for the packs on the cliff side—no sweat, as the mules sashayed around the obstacles, un-spook-able servants of their adventurous packers. After a long day, packs removed, they rolled over on their backs, all four hooves in the air, and wiggle-scratched their backs. Then they got up, shook, and ran around like dogs. Pete would lie crossways on Enos's bare back, reach both hands down on the other side, and scratch his belly. That mule would stretch his neck way out, close his eyes, and just soak up the pleasure of it all.

Once, Hugo Sonnenschein, Rich Kihlstrom, and Lennie Merman, with wives and assorted children, visited us in Monticello. "Are you up for a horseback ride?" "Yes!" So I called Pete and set up a time for us all to meet him and a truckload of horses on the slope of the Abajo Mountains up the road just out of Monticello well below buck-board flat campground. It would be the next day at 1 p.m. We all piled into our cars and drove the two miles up to the meeting point on the slope. We waited, but there was no Pete. Finally, at about 1:45 p.m., I drove back down the mountain to the first phone booth and called. His wife Jackie answered. I said, "Where's Pete? I thought he was coming at 1 p.m." Jackie says, "When Pete says 1 p.m., that means he will go to look for the horses at 1 p.m. When he finds them and loads them he will be up." So Pete showed up in the truck

about 2:15 p.m. Pete, like many in the Southwest, is on IPT—that's Indian People's Time. Those of us on WPT—White People's Time—have trouble getting used to IPT, but it is actually wonderful once you do. I think of IPT as *Time and the River Flowing*. You must be on IPT on all whitewater trips; if you are not, you have no business being there, but in spite of yourself you will soon get into IPT. I'm on IPT on all pack trips, as well. The only times I do not take "homework" with me on outings are whitewater and horseback trips. These are full-time attention escapades. In both cases, the schedule is controlled largely by nature. It is a different and very satisfying world, particularly for people like me whose brains are always busy offline, but who learn to adjust to external reality on a long pack trip or river run where my brain is still busy offline. After returning from one of these great escapes, I typically write up a storm as the buildup in my brain inventory dumps its contents into my eagerly waiting mind and pen.

For the first time in 55 years, the legislature in China has amended the constitution to provide more secure property rights for China's emerging entrepreneurs. They have long suffered at the hands of corrupt local police and authorities who seize property. The alleged reason was that the entrepreneurs broke the law in accumulating their wealth. See Joseph Kahn, "China Moves to Protect Property, But the Fine Print Has a Caveat," in the *New York Times* (December 23, 2003, p. 1):

This problem in China is an issue everywhere, even where we have the benefits of "the rule of law" and the protections of private property: In the best of governments control by government agents of the access rights to resources of various kinds endows those agents with gatekeeper power over the rights holders. That control can easily be, and often is, exercised capriciously and arbitrarily by local agents far below the radar screens of upper managers in Washington. Pete Steele had a run-in with the Bureau of Land Management that provides a real live down-to-earth example. Here are the circumstances.

Pete owned a small ranch in Utah and held BLM grazing permits to a thousand odd acres of BLM land adjoining his ranch—the standard Western grazing set up. (The old Green Water ranch on the road to Hall's Crossing was a private 160-acre home ranch adjoined to 750,000 acres of BLM grazing permits!) In order to try to make ends meet as a pack guide, he conducted pack trips for hunting groups. He always explained that these were not his favorite outdoors groups to serve, but he needed to spread the cost of his liability insurance over as many trips as he could muster. As a hunter who needed the elk meat in his freezer for food, and also used bow and arrow for sport, he had nothing against hunting. He just did not like the macho hunter types that came his way in the pack business. In the events leading up to his run-in with the BLM, he had contracted with some hunters to go up Bridger Mesa not far from Salt Creek.

It is late October. The riders are ascending the trail on horseback with three pack mules, and a light snow starts to fall. "What do you think, Pete?"

Pete: "It could stop anytime; we almost never have snow this early." It continues to snow. They get to the top, and it's 6 p.m. and still snowing. "What's the prospect now?" Pete: "It will stop by morning and likely melt by noon." Well, as luck would have it, in the morning it is still snowing. They have to abandon the whole excursion, but it is now too dangerous for the horses to try to bring them down in the snow. Pete stashes all his packing supplies and saddles under tarps, releases the horses, and loads the three mules with the customers' personal belongings. Pete has complete confidence that the mules can negotiate the trail and make it down through the snow. The horses are survivors in that country and will winter well on the mesa. He will take the mules up in the spring and bring the horses and all the pack gear down when it is safe. They get down the mesa uneventfully, although as Pete put it, "You ought to have seen Jane skidding stiff-legged down the slope on that snow with ears waggling and tail flying."

All is fine except that later, in the dead of winter, the BLM is doing a routine helicopter flight over Bridger, and the pilot spots the horses. "What's this; wild horses on Bridger Mesa? Wonder how they got there? How could we have not seen them before? Wait, I think I recognize some of Pete Steel's horses." To shorten the story, there comes a knock on Pete's front door. Are those your horses on the mesa? Yes, and Pete tells what happened. "Well you gotta get them down." "I can't do it safely for my horses until spring." Pete refuses, and the local bureaucrats threatened to revoke his grazing permits. There is a standoff, but the situation is saved unintentionally by the famous Wild Horse Annie (Velma B. Johnson), who with good timing makes the headlines over a horse injury event somewhere in the West—Nevada I think. It seems that some horses were injured when some authorities were trying to bring them down a mountain in winter conditions. The local BLM backed off, and after the melt, Pete recovered his horses without mishap.

Sadly, Pete was not able to sustain Horse Head Pack Trips. Liability insurance got too expensive for his small, customized operation. Since he could not beat 'em, he joined 'em and became a BLM ranger, stationed first in a trailer at Kane Creek, which is a key entry-route into Grand Gulch. Jackie was grateful for family medical and retirement benefits for the first time in her life. We are talking here of an endangered species of independent operators who contributed tremendously to what America is all about. Later, Pete was reassigned to Roswell, New Mexico (the UFO capital of the world), and I have not seen him and Jackie for years. He sold some of his horses and mules to Ken Slight, who had long led hiking trips into Comb Wash, Escalante Canyon, and other choice Utah sites.

In November 2013, I sent a note and copy of *Discovery* to Pete and Jackie Steele, who were then living on a rural route near Monticello, Utah. Here is the reply and our warm exchanges:

11/12/2013

It was great to get your book. We opened it at the Doctors office and Pete was very impressed: That a Nobel winner could hold him in such high regard!

He wrote a note right after he got the book and I've misplaced it. When I find it I will send it along.

Yes, we've made it back to Monticello. Pete has been retired 12 years or so. He's had health issues. He caught Tularemia from skinning a deer without gloves. He almost lost his thumb cause the germ (?) entered through a paper cut. Then he was up on the mountain gathering wood and had a tree fall on his arm. He had a heck of a gash and couldn't drive…the grandson, about 10 at the time, got him down to the hospital. I hope you are grinning about these antics. [As indeed I am—ear to ear—it's another live and vibrant Pete Steele story. Who needs fiction? I am so delighted he read my tribute to him before he passed]

But the bad stuff was yet to come. He was diagnosed with Multiple Myeloma about 10 years ago, and then a few months after that he went to a clinic and was diagnosed with Pulmonary Fibrosis and Fibrosis of the Lungs (from his uranium mining).

His idea seems to be that he is going to live with it rather than die from it.

Diagnosed as a roaring stage three when he first went in, he is his cancer Dr's poster boy. He's been through most all of the chemo and has had two courses of radiation. He's had some issues, we go to Grand Junction Friday and we suspect that he'll have some radiation and be started on some new chemo which has worked well for MM.

But not to worry, every day he gets out of bed is another day of paradise.

The girls are doing great. Robin has two daughters and is living in Montrose. Stacey has a house in Alaska, but lives down here because of Pete's condition. She has Reed (now 22) and a 13 year old girl. Kristina was a kindergarten teacher, had a baby girl and is staying home to raise her. They live in Cahone, over by Dove Creek. Robin is newly divorced (again) and we couldn't have picked two better sons in law. They are great guys.

I really apologize for not mailing sooner. Your book is great, thanks a bunch for it.

PS [hand written] This wouldn't go so I am going to put it in the "other" mail today.

Good news from our trip to G. Jct.—He doesn't need radiation and the Dr. isn't going to give radiation either.

Take care,

Pete and Jackie

12/1/2013

Pete and Jackie:

I am just back from Thanksgiving in Tucson with my wife, Candace and opened your snail-mail. We live in Orange, CA most of the year, leasing from Chapman U, but still own a home in Tucson–one home is enough to own!

Joyce is living in Kansas City with her sister in a retirement community. Candace and I both went back to join her and the twins (Deborah and Eric) to celebrate the twins' 60th. Torrie drove over with her husband from Colorado, where they live above the San Luis valley high in a solar home they built themselves. Torrie has worked 2–3 days a week as a pharmacist in Alamosa, and is still

heavy into back-packing; Torrie follows the Ken Slight and Pete Steele outdoor life.

I still travel, give lectures, teach some, but mostly they pay me to do my research. No need to retire, as I would do the same thing anyway. Candace accompanies me; if she does not go, I almost never go.

Thank you so much for filling me in on Pete's health issues; I am not surprised by Pete's commitment to live with it. We all live on borrowed time and each day is paradise, indeed. I have been blessed with good health considering that I will soon be 87. My hearing is badly assisted by what they call hearing "aids" but mostly they just cost a lot of money. Eyesight is not good for night driving so I avoid that, and almost never drive anytime in CA.

Best wishes to you both; would love to see you, but it's a pretty long hike,…. Vernon…

9/7/2014

Pete and Jackie:

Here it is 9 months since we exchanged letters.

I am revising Discovery, and adding a lot of new sections. In the parts I am revising, and updating, I bring in letters and reports related to earlier descriptions. So, I am including your letter sent to me last December—hope that is ok with you.

I am still active, pretty much as ever, but at a little slower speed, feeling my years more than I did up to the early 80s when I felt no different inside than when 20.

I am so glad that we made contact again. Wishing you good health…Warm Regards,…Vernon..

9/10/2014

Vernon,

Like I mentioned before, I was truly amazed to have received your book and even more for the wonderful thing you wrote concerning me. As I remember the biscuits were very tasty. Please forgive my attempt to answer the note you wrote. You are welcome to use any of my letters as long as you edit them to keep me from looking too much like and old cowboy that has spent (much time) in the red rocks. [Pete is referring to southern Utah, known as the Red Rock Province.]

The trips I guided you on were some of the most enjoyable for me, Mary Wall and husband you will remember…

Pete got tired, so i [Jackie] *will try to finish this note. He is no longer taking chemo, his Dr. states the multiple myeloma won't get him, it will be the miners lung diseases he has. He does pretty well most days and is on oxygen 24 hours, he has portable units that can go with him when he is out of the house, he has been gold panning and hunting for buried treasure this summer…we are both dreading Monticello's cold winter.*

Take care, try to stay healthy! Keep us in your thoughts and write when you can…..Your friends, Pete and Jackie

After Pete sold his horses to Ken Slight, Ken conducted hiking trips supplied by pack animals. In this way, hikers needed only a light daypack

and lunch. For Ken, with his rafting legs, rafts and horses were a complete contradiction in terms. If you ask Ken Slight what his mount's name is, he replies, "Horse." For Pete, the horses and mules were an extension of his hands, brain, and personality. Pete and Ken: two jack Mormons who could not have been more different, though both were integrity writ large. In Ken's combined hike-pack operation, a packer and the horses would break camp after the hikers left each morning and move to the next overnight rendezvous. We did one horse-pack assisted hike with Ken down Comb Wash to its confluence with the San Juan River. A short hike upriver from the confluence is the location of one of the largest tapestries of rock art that you will find anywhere in the Southwest.

Our children had finished high school by 1972, and I accepted a fellowship at Stanford's Center for Advanced Study in the Behavioral Sciences in 1972–1973. During this period, my research turned to the economics of uncertainty, financial theory of the firm, and natural resource economics, but I continued to think and reflect about experimental economics and to use it in teaching. For me, all of it has to do with how things work, which is also what this book is about. This was a significant developmental period in that my brain was continuing to work on experiments, and was developing a fresh perspective though I was not yet consciously aware of that process.

Charles Plott and I talked experiment (e.g., the idea of induced valuation) on many bass fishing trips to Lake Powell and to Indiana Lakes in the 1960s. This series of conversations sparked both of our continuing interests in experiment and was the link to public choice and the whole field of experimental political economy. Charlie created that field, alongside his ingenious colleagues, Mo Fiorina, Mike Levine, Roger Noll, and others. Strangely, I knew nothing of this development until the work was well advanced, and ready to be reported in papers and publications.

At the Center, thanks to the encouragement of the anthropologist Bob Heiser, I wrote my "Pleistocene Extinctions" paper and submitted it to the AER. After a year's delay, I received a letter of rejection along with three favorable referee reports. That combination was a first for me! The editor explained that he had been unconvinced by the first two and had sought a third report, but was still not convinced. He said that in a way it was ingenious to interpret the prehistory of humans in terms of a common property resource model of hunting, but that once you had said it, there was nothing left to say! Of course, the same comment applies to all published papers, but most professionals are interested in how the argument and theory are developed, and how they relate to evidence. A good title and abstract are enough for browsers. The bottom line was that the basis for his rejection

was not assailable since he was the editor and could reject it for any reason he wished. I sent the article to George Stigler at the JPE (1975), along with the AER rejection letter and the three referee reports; George sent it to one referee, and it was accepted and published in a few weeks.

I also finished some generalizations of work in corporate finance, a field that I had come to think of as consisting of the non-theorem of Modigliani and Miller, on which I have already reported my encounter with "jolting Joe Stiglitz." The literature on M-M is an incredible story of an idea that had some merit and was probably an academic exercise worth exploring. The idea, however, led to a false result, whose assumptions were not supportable within the framework used to get the result, but nonetheless became enthroned in the literature. Widely influential for many years, it died a natural death, displaced by the new wave of excitement over rational expectations theory, also widely influential and dying a not so very slow death as I write. M-M was another one of those episodes in economics about which you could ask: "What is it good for?" Recall Leontief's answer: "It's good for teaching."

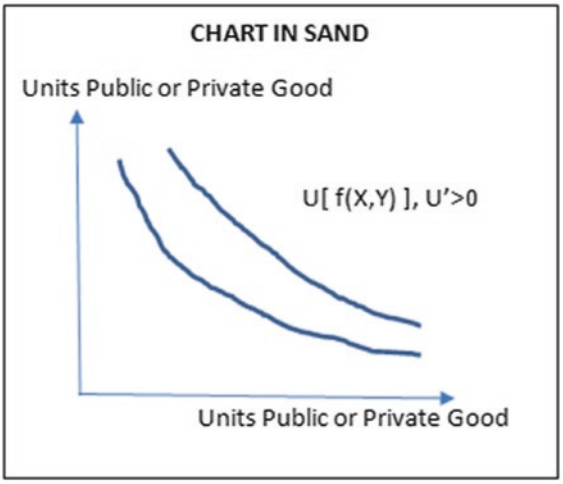
While at the Center, I continued to be in touch with Charlie Plott, who wanted me to join him in a joint Caltech effort in experimental economics. He engineered a Social Science Division offer for me to come in the capacity of a Sherman Fairchild Distinguished Scholar position for one year. That visiting scholar position provided the vehicle for us to offer a seminar for student credit in the spring quarter of 1974. I updated my old outline and notes from teaching experimental economics at Purdue from 1963 to 1967, and we had regular meetings attended by three paying student customers (including an undergraduate, Ross Miller who became a co-author) and several faculty members, including Mo Fiorina, John Ferejohn, Roger Noll, Jim Quirk, Lance Davis, and Bill Riker. Bill was also a visiting Caltech Fairchild Scholar on leave from the University of Rochester that year. He had done some political science game theory experiments, and we were off and running. Later, Bill reported that he had agreed to write a paper for an editor on experimental methods in political science, but after the course was finished, Bill reversed his decision. The seminar, as be described it, had completely changed his thinking about experiments and he wanted more time to reflect on the subject. I think that story summarizes well the intellectual ferment produced that semester. Afterward, at Caltech, experiments, including the new experimental political economy that I would learn about, would be central to the entire teaching and research program. My understanding is that that tradition has eroded some in recent years after good service for thirty-odd years.

I talked from my old Purdue lecture notes, including the theory of induced valuation, and developed many new notes in our Caltech seminar in 1974. Earlier, in 1972–1973 when I was still at the Center, Charlie had pointed out to me that these induced valuation ideas were catching hold and he and others needed something to cite. I remember at the time thinking that it should be no problem, as Charlie could always just cite correspondence or personal communication with me. (At that time, I had no idea that poker-Charlie had a whole research program underway on voting and public goods, using induced valuation concepts. So yes, he needed something to cite.) I responded by including those old notes in a methods write-up in the Caltech working paper series. I wrote a draft of that paper while still at the Center in 1972. Subsequently, at Caltech, I used that working paper to draft a short summary paper on induced valuation, included some data on the effect of incentives, and of excess supply on convergence. It was published as "Experimental Economics: Induced Value Theory," *American Economic Review*, 66(2), Papers and Proceedings of the 88th Annual Meeting of the American Economic Association (May 1976). In those days, invited papers presented at the annual meetings of the AEA were published in a special proceedings volume without refereeing. That is how the theory of induced valuation was published a dozen years after its original articulation and development. The Nobel citation included this paper.[1]

I might easily have not published it. In fact, its publication was a serendipitous afterthought. It was old hat for me, long after the synergies with students that gave rise to it. I cannot imagine any referees of the day seeing its significance, even in 1976. To any theorist it was trivial, just an observation pointing out an application of the theorem that utilitarian preferences were invariant under a monotone strictly increasing transformation of the utility scale. Hence, I would not likely have had the commitment to fight it through a resistant editor and refereeing process. No referee could appreciate that this topic was the elementary stuff that was changing the way we thought about economics in the laboratory. You cannot convey that in a paper, only in an entire research program. Charlie and the people in that seminar understood it. Bill Riker had a refreshed new perspective.

[1]See http://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2002/advanced.html.

Charlie's advice was good in urging me to write up these ideas in a paper. I was not reliable in such matters. It was the work and the shift in thinking underway that had absorbed me more than when and where to publish a piece that would develop the importance of using money to induce value on abstract items as a means of controlling supply and demand, incentives in general, and for motivating choice in game theoretic studies in the laboratory. The basic methodology was already transparent in Siegel's work the 1950s, but psychology was going in a different direction. Besides, it was a note really, not a paper, and even today economists generally would not likely understand its essential significance. I never really acquired the habit of keeping my ear to the rail. For me, self-absorption in its strategic manifestation is too costly and distracting. If I don't keep my eye on the ball I will fumble. I think of ideas as being a dime a dozen, and there always seem to be so many more. What is much more important is the development and implementation of the idea in a way that enables you to learn from observation, and know what should come next. This is particularly true once you have a concept-generating engine and technology like experimental economics, but more generally, it flows naturally out of a curiosity about how things work. Eventually, priority on any one idea is not the central issue that it may seem to be at the time, or in retrospect. I believe that what is important, and will come to be seen and understood, is what accumulates. McClure Stilley, cited at the beginning of the first volume of this autobiography, stated well the ultimate motivation for doing anything.



I was back into experimental research full time, except for finishing and publishing a few loose ends in resource economics. I also discovered the significance of the earlier discussions between Charlie and me on fishing trips during the 1960s, particularly his intense interest in the concept of induced valuation illustrated in the pictures I drew in the sand (see drawing). On a trip to Lake Powell in 1972, we were going over induced valuation, and he asked if the objects on the *x*-and *y*-axes could be public goods, and of course I replied that they could be anything—public or private goods—on which one wanted to induce preferences. To his everlasting credit, Charlie saw its depth and breadth immediately. I had been talking about it in seminars and teaching it in my Purdue graduate course during the years 1963–1968, but it had not yet had the impact on other economists thinking that it had had on mine. In his collected papers, published in 2001, Charlie wrote that he was so excited about it all that he had the design for the first public economics experiments mostly worked out by the time he returned to Caltech, where he would be working with Mo Fiorina. Charlie was an exporter, but I would share his gains from trade in a host of indirect ways.

I stayed on in 1974–1975 with a joint appointment at Caltech and USC, and we wrote up our experiments for Miller, Plott, and Smith (*Quarterly Journal of Economics*, 1977). This was surely one of the first experimental papers in economics with an undergraduate co-author, but Ross was a meaningful contributor and in fact wrote a preliminary draft for course credit. I put his name on the first draft I wrote for our joint paper, and Charlie went along with it. Subsequently, I wrote a paper with two undergraduates at Arizona (Coppinger and Titus) that won a Best Paper Award—it was good policy.

Ross M. Miller (1954–2013) went on from Caltech to a great career spanning both finance and academia. He wrote *Paving Wall Street: Experimental Economics and the Quest for the Perfect Market* (2001) and asked me to write the Foreword which I was happy to do. He also wrote: *What Went Wrong at Enron* (2002), which persisted for months on the *New York Times* business paperback best-seller list.

In 2013, Ross died in his sleep, very unexpectedly. Mary O'Keeffe, his wife of nearly 34 years, wrote: "Ross was the love of my life. We met in September 1975, at the beginning of graduate school, both of us age 21. We married in July 1979. We were not just husband and wife, but partners in virtually everything we did together. I miss him more than words can say."[2] No one can state a better tribute than did Mary for this gentle love-able soul.

[2]See http://rossmillermemorial.blogspot.com/2013/06/obituary.html.

Charlie and I wrote our paper on comparing institutions, later published in the *Review of Economic Studies* (1978). In 1974, I started the experiments that would lead to a series of papers testing the incentive properties of various public good mechanisms (1977–1984). I published the unanimity auction mechanism for public goods in 1977 (*Journal of Political Economy*) and compared the Lindahl free rider mechanism with Groves-Ledyard in 1979 (*Experimental Research in Economics*). Using Lindahl for the control was methodologically essential. You need first to establish that there is a free rider behavioral problem; then provide its solution. At the University of Arizona, I finished the work I had started at Caltech on alternative public good mechanisms.

At Caltech and Arizona, experimental economics blossomed, and it was good for something more than teaching (Figs. 14.1, 14.2, 14.3, 14.4, 14.5, 14.6, 14.7, 14.8, 14.9, 14.10, 14.11, 14.12, and 14.13).

Fig. 14.1 Pete unloading horses Salt Creek excursion

Fig. 14.2 Arthritic Man young, middle aged and old, Grand Gulch

Fig. 14.3 Breach birth Grand Gulch

Fig. 14.4 Anasazi porn man-wolf copulating while another waits his turn

Fig. 14.5 Great figures

Fig. 14.6 Where seldom seen Smith kneels, prays for precision earthquake

Fig. 14.7 Headdress

Fig. 14.8 d'ja ever hear the one about the five legged deer

Fig. 14.9 Giants of the Southwest

Fig. 14.10 Salt Creek all American man

Fig. 14.11 Five faces east

Fig. 14.12 Salt Creek shower

Fig. 14.13 Kokapellis galore

15

Arizona and E-Commerce in the Laboratory

Holmes and Watson go on a camping trip. After dinner and a good bottle of wine they retire for the night and go to sleep. Some hours later Holmes awakens, startled, and nudges his faithful friend. "Watson, look up and tell me what you see." "I see billions of stars, Holmes," replies Watson.

And Watson ponders for a moment, and says, "Well, astronomically it tells me that there are millions of galaxies and potentially billions of planets. Astrologically, I observe that Saturn is in Leo. Horologically, I deduce that the time is approximately a quarter past three. Meteorologically, I suspect that we will have a beautiful day tomorrow. Theologically, I can see that God is all-powerful, and that we are a small and insignificant part of the universe. What does it tell you, Holmes?"

Holmes is silent for a moment.

"Watson, you idiot! It tells me that someone has stolen our tent."

By this time in my professional life, I had realized the importance of tempering all my technical and analytical learning in economics with everyday common sense based on observation. I had learned to take seriously the actions of subjects in experiments. I should note that our "subjects" were not *only* (a word that skeptics seem always to use here) students; early on I had replicated our standard experiments using business persons. They—student or business people—all startled me with their accomplishments in the experiments despite having no sophisticated sense of what those accomplishments might be because they had not the information or the eyes for contemplating the whole. (Gradually I came to see that this was equally true of the profession, with an early exception of Charlie, who got it loud and clear from

V. L. Smith, *A Life of Experimental Economics, Volume II*,
https://doi.org/10.1007/978-3-319-98425-4_15

the beginning, followed by a very small handful of scattered souls whose numbers slowly increased.) When the subjects seemed to produce a result that was "wrong," in terms of standard theoretical predictions and expectations, experience had taught me to withhold judgment. When puzzled by replicable data, I had started naturally to think outside the conventional box and try to keep my brainwashed and conventionally educated cortex and memory banks from carrying the day. Understand, I still had (and have) a great distance to go, but I was starting to ask, "What are the subjects trying to tell us about the world as they see it?" Herb Simon shared this perspective, but with the cognitive psychologists and behavioral economists there was a tendency to have the same perspective as the economics profession: When subjects got it "wrong," they were being "irrational," and we should not question the theory or our interpretation of it. So, I tended to be at odds with both the profession and the behavioral economic psychologists, who believed in rational theory, but delighted in its prediction failures. What was important and significant was that the subjects did not have the worldview that the economics profession had, and I had already learned from my subjects that our professional understanding of market dynamics and microstructure was severely deficient: The subjects had it right; consequently, when they appeared to be getting it "wrong," some interpretive caution was in order. Clearly, we knew little of how economic actors functioned in the world and that if our understanding was to develop, we had to take seriously the actions of the participants who drove it, myopic as they might be, and, however, seemingly right or wrong they might appear.

Some famous and very accomplished economists have the commonsense abilities to think outside the box and to observe carefully and thoughtfully the world around them. (They would not likely have given Watson's answer.) Some of them are even Nobel Prize winners. Tom Schelling, who finally received the award in 2005, gets as much mileage out of everyday common sense as anyone. Ronald Coase, a careful commonsense observer, achieved the Nobel with essentially a few blockbuster papers out of a couple dozen. Herb Simon never allowed his technical interests to override his common sense. There are more, such as Adam Smith in the eighteenth and Frederick Bastiat—who is usually dismissed as "only a journalist"—in the nineteenth century, and another of the best, F. A. Hayek in the twentieth.

I considered staying on at Caltech in 1975, or going to Northwestern, where I had many close and longstanding friends like John Hughes and Stan Reiter, and former students like Mort Kamien and Nancy Schwartz. However, I feared the tug of a silver umbilical cord at Northwestern and the thought that maybe you can't go home again to a Camelot that had a time

and place combination that might not be repeated—U. Mass. had been a real live demonstration of the challenges to replication. Caltech was a pressure cooker—no change since my undergraduate years, 1945–1949. When I cook, I like to marinate all the ingredients of the product, and I don't like strategizing environments; lacking that was the essence of what had made Purdue work. I don't want to have to think about covering my rear flank, or with whom I can talk about what; it strains your memory. One story, the same for all, makes life easier.

Carol Breckner, who would become my wife, was not in favor of going to Northwestern, where I would be returning to an earlier fold of familiar friends and colleagues and where she was a stranger. Arizona would be a good place to start over together. More than anything, I was looking for new opportunity; I could not articulate it, but I thought I would know it when I saw it. In 1975, I did not see it at Caltech or at Northwestern, any more than I had seen it at the Harvard Business School, Carnegie Tech, or Princeton in 1955.

Then, in 1974, I gave a seminar at the University of Arizona. Rene Manes, a graduate student from my Purdue days, was dean of the College of Business and was interested in bringing me to Arizona. Intrigued, I returned in 1975 to give another seminar, visited with the administration, and sensed that this was what I was looking for. They had had some recruiting successes, but had much work ahead in building the faculty. Most impressive, however, was a committed top administration: John Schaefer, president; Gary Munsinger, vice president, and Al Weaver, a tough-minded no-nonsense provost whom I really liked. Actually, I liked the very things that many people hated about him. These guys had good values and were entrepreneurial, which were not exactly commonplace characteristics of university administrators, so my interest was sparked. I could see the dim outline of Em Weiler in this collage.

Em, as I have noted, once said that his first year as dean of the new Industrial Management School was the most difficult year of his life, but that he had one supporter in President Hovde, and if you get to have only one supporter, you want it to be the president. As it turned out, at Arizona, our program would have many other supporters who would sustain us for nearly two decades before the delicate, and rarely, blooming flower of university entrepreneurship would succumb to the inevitable. At Purdue, we had built castles in the cornfields; maybe in Arizona, we could build them among the saguaro.

There was another fact to take into account: I was already falling in love with Arizona and the university. I stayed at the Westward Look Resort and

could hear the coyotes at night. I sat in the bar and could look out to the south (this was before renovation ruined the view from the bar), the city lights stretched below me, and it just felt good. It led to about twenty great years of innovation and growth in experimental economics before administrative mediocrity and a typical faculty skirmish over imagined demons flooded the more recently planted crops. But nothing is forever, and if a program is successful, it will be a prominent target. Only he who does nothing will never be shot at.

Some Early Intellectual History

Carol and I decided to move to Tucson, arriving in the August summer heat, the monsoon lightning, sudden bursts of wind, and the gully washers of 1975. It was a good decision—good beyond my fondest hopes. I was there for twenty-six years before leaving for George Mason University in July 2001. That is a long and exciting story that continued the basic work begun at Purdue, which is where it all started for me, and which formed the primary citation by the Nobel Foundation. The story of our departure from Arizona begs to be told; though now (2007) is the time to do it, this is not the place to tell it. The full story is not only about the heights and depths that the human spirit can reach, but also about the long-term value as well as lessons that can be created out of that volatility. My focus here will be on the heights. The depths might depress you, but such are reported, and documented, in records deposited along with all my correspondence and papers in the Vernon L. Smith archives in the Duke University library.

The foundation-building years were 1975–1985. In the first several of those years, I had a number of key undergraduate and graduate students in experimental economics classes who were instrumental in developing and implementing a vision of computerizing the protocols for running human subjects in a great range of experiments that we soon had underway. These students deserve full and undiluted credit for creating Arizona's methodological revolution in experimental economics: Arlington Williams (economics), Mike Vannoni (engineering), Stephen Rassenti (systems engineering), Vickie Coppinger (nee Sandler, economics), and Jon Titus (economics). Then, the list expanded to include Jonathan Ketcham, Bruce Roberson, Don Coursey (all in economics), then David Porter, and others who followed. Without them, their dedication, and incredible on-the-ground smarts and dedication—each in his own way—nothing, or at least far less, would have come out of Arizona.

The curriculum mechanism for developing this program within the university bureaucracy was to offer an undergraduate and graduate course in experimental economics. At first, the two courses were combined into one, for duel-level credit. As in most universities, there was a complicated committee process for getting approval for new courses—a process bound to fail. Questions naturally arise when you want to teach any course in a subject that is not recognizable as part of most of the faculty's training when they were in graduate school twenty-five years earlier. I bypassed that process by commandeering a course, vague in title, listed but not taught. I used that duel-level course to introduce students to the literature of experimental economics, but I gave them no examinations on their comprehension of the readings.

I did not administer a course examination for the next twenty-five years at Arizona, a record that has now stretched into 40 odd years. We used the examination period scheduled at the end of each semester for completing our discussion and presentations. I think this may have been in technical violation of a university rule requiring all courses to have a final examination; university rules are not routinely enforced, and are correspondingly ineffective. More significantly, administrative decrees like this are based on a false premise. Education is not about knowing things. It's about discovering and implementing what you can do with what you know. It is about learning to learn. As an alternative to exams, we all made presentations, and each student was to propose an experimental study. We concentrated heavily on what was to be done and then doing it; on learning by doing; on learning new skills and tools, but as part of solving a problem that required one to learn or utilize whatever skills were needed. Just as competition—a poor word for free choice among alternatives—in the economy is a discovery procedure, so is education.

Albert Einstein was famously skeptical of the conventional approaches common in university education at the end of the nineteenth century. Thus, early in his university education, he found that in the study of physics he had learned to *"scent out that which was able to lead to fundamentals and to turn aside from everything else…that clutter up the mind and divert it from the essential."*

Einstein had studied in Switzerland, where apparently those wishing to learn suffered less than in other more coercive places that tended to smother all scientific curiosity. He spoke approvingly of only being required to pass two examinations. Otherwise, he was free to pursue his own studies in whatever way he desired. Moreover, he was especially blessed to have a good friend who faithfully attended and prepared careful notes on all the lectures. He turned to those lectures only when finally it was necessary to prepare for the

exams, gladly taking "*into the bargain the bad conscience as by far the lesser evil.*" It was, he felt, something of a miracle that contemporary instructional methods had not yet "*entirely strangled the holy curiosity of enquiry*" from *Albert Einstein: Philosopher-Scientist*, ed. P. A. Schilpp. La Salle, IL: Open Court Publishing, 1949.

At Arizona, several students over these years poured themselves into the exercise and defined projects that required more than a three-credit course investment. I enrolled them in one or two additional special studies courses to enable them to complete their projects. Some learned computer programming in order to complete their research program.

Arlington Williams was the pioneer. In 1975, Arlie undertook to write the first electronic version of the continuous double auction (double auction, or DA, is a real-time bid/ask/contracting procedure similar to, if not exactly like, the oral version I had first used in January 1956).

The program was functionally tested in the summer of 1976. We ran twelve experiments using designs that were identical to those we had also used in earlier oral DAs. Arlie wrote up the comparisons showing that the oral DA produced equilibrium more rapidly than the electronic version with inexperienced subjects, but that there was no discernible difference for once-experienced subjects.

The brain learns more rapidly to function in this market task by processing oral input and responding orally than by utilizing visual input and written trading responses. We learned that the cost of transacting is higher in the latter than in the former, and this exercise stood as an early demonstration showing how transaction costs can impact performance. But once those computer-assisted communication channels are practiced and become autonomic, the behavior is the same. Today, with students everywhere electronically literate that difference may not exist, or be reversed. By this time, Arlie—who was learning by doing—knew a tremendous amount of programming (the Tutor language) for the PLATO system. His DA software program was not efficient, though it creaked along, and he did exactly what the slovenly would never do: Scrapping all he had done, he just started over and rapidly produced a more streamlined piece of software.

The new program developed four versions of the bid/ask trading process, allowing us to learn much about the anatomy of the DA rules. This was not the first time for computerized experiments. In the 1960s, Austin "Augie" Hoggatt had run oligopoly experiments using posted price-quantity mechanisms. But this time it was based on real-time trading, and experimental

economics had become a sustained effort motivated by trading methods in practice, not only toy price mechanisms motivated by academic theory.

We were doing e-commerce in the laboratory, but we did not know that was what we were supposed to call it until it was reinvented much later on the Internet.

Mike Vannoni was also a front-runner and, at about the same time as Arlie, was developing PLATO for sealed-bid, two-sided trading mechanisms. Vicki Coppinger (nee Sandler), Jon Titus, and I ran manual experiments comparing the English, Dutch, sealed-bid first-price and second-price auctions, and Vicki followed up with a PLATO version of the sealed-bid auctions. This was the first of several papers that I would write with undergraduates at Arizona. They deserved more than a footnote at the bottom of the title page of the article that resulted from our joint discoveries.

I submitted the Coppinger, Smith, and Titus piece to the *Journal of Political Economy*, where it was accepted, subject to the condition that it be drastically shortened. Since I did not want to shorten it for the editor—Sam Peltzman at the time—we sent it to Bob Clower, editor of *Economic Inquiry,* where it appeared in full. It was a good decision: We won a best article award from the journal for that paper.

I worked for several months with Jonathan Ketcham to develop a smoothly functioning, rich, multifaceted version of the posted offer (PO) market mechanism, leading to a comparison between DA and PO by Jonathan, Arlie, and me. We published it in the *Review of Economic Studies* (1984).

Mike also developed Plato versions of various public good mechanisms that I had begun studying earlier at Caltech. We published the principal study using his software in the *American Economic Review* (1980). Don Coursey and I followed with Don's more comprehensive program for studying and comparing private, public, and externality-good decision mechanisms.

At the time, how were we thinking about our project? When we "went electronic," doing the first computer-assisted experiments in economics, 1975–1978, we thought we were making it easier to run the kind of experiments that we had been running for years. We could increase sample size and horizon length, and record the observations more easily and accurately. This was a very myopic belief, as we soon found that computerization changed our experience and changed the way we thought about experiments. Our designs and experiments gradually changed, without consciously planning it, in phases. That is a fundamental truth about how norms, practices, and institutions emerge, and why they are so far beneath our conscious awareness. What we learned experientially when we became computerized

was that we could conduct much more complex experiments and process data within much larger message spaces. We were soon running experiments that we would never have imagined we could consider theretofore. In particular, a central processor could apply optimization, coordination, and scheduling algorithms to the willingness to pay for and willingness to accept from messages (bids and asks) of decentralized agents with dispersed information.

With Stephen Rassenti in systems engineering, and skilled in developing optimization algorithms, PLATO enabled us to develop an entirely new approach to using the laboratory to test-bed new market designs and person-machine decision systems. From the beginning, it had the potential to replace ponderous, inefficient, command-and-control regulatory systems with self-ordering, self-regulating (under property right rules governing action) systems. Complex markets could be coordinated with support system designs that simplified individual decision operations. Individuals supplied willingness to pay and willingness to accept judgments based on local knowledge, valuations, and conditions; algorithms, applied to the messages from dispersed human agents, assured that each could do no better for himself against the constraints expressed by all others.

The test-bed idea emerged from the confluence of the work of many, and claims about who was first and how it all developed require deep scholarship in the history of economic ideas. Test-bedding emerged naturally, out of what many of us had been doing, and the challenge from the very beginning was provided by incentive issues in ongoing real-world markets.

Looking back, with the proverbial 20/20 hindsight, my experiments from 1965 to 1967, motivated by the Treasury bill auctions, comparing competitive and discriminative auctions (*Journal of Business*, 1967), were a part of what we later called test-bedding, borrowed from the long-established engineering term. Of course, no one at that time had the understanding we now have from hindsight.

The Treasury auction problem, identified by Milton Friedman in the 1950s, kicked off the discussion. He observed that when all winning bidders must pay the amount of his bid price, each bidder has the incentive to estimate, and bid only barely above the lowest of the accepted winning bids. He recommended uniform competitive price clearing, whereby the bidders would place bids specifying the price they were willing to pay and corresponding quantity they would offer to take at each price. Given the announced quantity to be offered, the auction process would then determine the cut-off price above which all bids are accepted, and the offered quantity sold, with each bidder paying only the lowest cut-off price. Friedman was

correct in supposing that there was an enhanced incentive to collude when Treasury auctions were organized so that a winning bidder paid the amount of his bid price. But his evidence—the narrow spread between lowest and highest accepted bid—was shown by my experiments to be a property of the incentives inherent in the rule that each accepted bid paid the amount of the bid. The rules gave the bidders a common incentive to estimate the lowest accepted bid, which was a common focal point, and then to bid ever so slightly above it. Hence, the rules induced the behavior that emerged endogenously. With everyone behaving in this manner, you observed naturally and spontaneously a narrow spread between lowest and highest accepted bid.

Much later, aided and abetted by Treasury's tie-breaking rules, Friedman's collusion hypothesis was confirmed by the Salomon Brothers scandal, discussed below.

In 1961, a precise general theory was published in the—obscure at the time—*Journal of Finance* by Bill Vickrey. He showed that in general each bidder should pay the opportunity cost his bid imposed on others. In the Treasury bill example, that was the highest ejected bid. Unfortunately, I did not know of Vickrey's great theoretical paper, nor had I or many others read and benefited from his work until years later. Vickrey's paper was far ahead of its time. People were not ready for it, as the full implications of the concept of incentive compatibility, and its application to auctions, had yet to be absorbed into professional thinking.

In the winter of 1968–1969, Henry Wallich (a Treasury consultant at that time, who was later to be appointed to the Federal Reserve Board) called me one night at home in Massachusetts. He was interested in my Treasury bill experimental paper and in the course of the discussion, requested reprints of that paper to help him persuade the Treasury to get off its duff and run field experiments comparing the current auctions with the alternative proposed by Friedman.

Henry had long "agitated," as he described it, at Treasury for a field test of the Friedman proposal, but he wanted to make a new attempt, armed with more than Milton Friedman's previous arguments. This involved discussions with Secretary of the Treasury George Schultz who brought a refreshingly independent attitude to government. A few arduous years later, this new effort led to sixteen bond auctions. Six were uniform price competitive auctions, inappropriately called "Dutch" auctions at the time because it was widely believed that Dutch descending price offer auctions were equivalent in their incentive properties to uniform (or second) price auctions. Ten of Treasury's 16 auctions were discriminative auctions in which each successful bidder paid the amount bid. Treasury was essentially conducting 16 field

experiments in institutional design in the early to mid-1970s. Henry Wallich later confirmed in correspondence with me that the laboratory experiments had played a helpful role in effecting this change in attitude. However, none of these efforts led to a change in policy for another 25 years; the change, rather, was precipitated by an investment banking scandal, not by the learning discovered in the field and laboratory experiments.

Why did the change take so long? Here is the short answer: The proposed change in Treasury auction rules was vigorously opposed by the primary auction dealers. These dealers are also prominent investment banking underwriters of new security issues. (There were 22 on the list in 2014, including the largest investment banks: Merrill Lynch a division of Bank of America; Citigroup; Goldman Sachs; J. P. Morgan; Morgan Stanley.) Their opposition ruled the roost until 1991, when "irregularities" were observed in the auction market for new Treasury issues, and Salomon Brothers, a longtime participant in these auctions, was found to be the source. From experiments, as I have noted, under Treasury discriminative auction rules, a bidder's incentive is to estimate and bid at or just above the lowest accepted bid. Consequently, there is a large spike of bids at the lowest accepted bid price, far more than is offered for sale after subtracting out all the strictly higher accepted bids. Unlike the procedure I used in the experiments, the Treasury's tie-breaking rule for rationing the excess available was to accept all quantities at the low bid in proportion to the amounts requested. If your bid was for half the total bid quantity, your pro-rata share was half the available securities at the low bid. A tied bidder was assured of receiving some of the offerings, and this was an incentive invitation for manipulation. In my experiments, a bidder was selected at random for the award; others received nothing, giving all an incentive to bid higher initially. Salomon Brothers was obtaining the lion's share of the entire amount available by placing bids for the account of various private clients in addition to bids for their own account. This was normally legitimate, except that the clients had no knowledge that Salomon was bidding in their name! It was fraudulent. Salomon normally bid by agreement for the account of any clients who wanted to participate in this exclusive dealer-dominated market.

Since that is a book-length story, I will excerpt some passages from an article by Milton Freidman on the topic and provide subsequent comment. For Milton, it was an "I told you so" decisive moment, in which Treasury was held accountable for failing to be more knowledgeable in structuring their bidding rules.

"The Salomon Brothers scandal arose because the U.S. Treasury uses a defective method to auction securities. An alternative method, suggested long ago and used briefly by the Treasury [their field experiments in the 1970s], would both yield more revenue to the Treasury and render such a scandal impossible….The present method involves payment of different prices by different purchasers, which tends to limit the market to specialists and to establish a strong incentive for collusion among bidders. A preferable alternative is to ask purchasers to specify the amounts they are willing to buy at a sched-

ule of prices, determine a single price so as to clear the market, and charge all purchasers that single price….the present system not only tends to limit the market to specialists, but to establish a strong incentive for collusion among bidders…If you pay the price that you bid, then it really makes a great deal of difference that you should bid very close to the final price at which the auction is going to be settled. The only way to assure that you do so is to get together with other people and arrange your bids…"

"A Dutch [uniform price] auction proceeds precisely as at present with one crucial exception: All successful bidders pay the same price, the cut-off price. An apparently minor change, yet it has the major consequence that no one is deterred from bidding by fear of being stuck with an excessively high price. You do not have to be a specialist. You need only know the maximum amount you are willing to pay for different quantities…Dealers no longer have a sheltered market in which they can conveniently gain by such tactics as Salomon followed…While my initial proposal produced a brief but lively discussion in professional journals, it had no effect on Treasury policy. However, in 1972, when George Shultz was Secretary of the Treasury and Jack Bennett was undersecretary for monetary affairs, I was able to persuade them to experiment with Dutch auctions for some issues of long-term securities. The experiments ended in 1974, when William Simon became secretary. For all his admirable qualities, Bill Simon was too recently a bond trader on Wall Street to welcome an experiment vigorously opposed by professional government bond dealers."

"Two economists at the Treasury subsequently evaluated the performance of comparable bond issues sold by Dutch auction and by the standard procedure. The results were unambiguous. The Treasury got a better deal under the Dutch auction. The economists estimated that the Treasury would have saved some $60 million by using a Dutch auction for the 10 issues sold by the standard method that they used as a basis of comparison. The estimated saving would have amounted to about 0.75% of the total revenue realized. If anything like the same results were realized today, the savings would clearly be substantial given the far larger amounts involved."

"The authors submitted their article to a professional journal, which accepted it for publication. However, for more than two years the Treasury refused to authorize its publication. That veto was withdrawn in late 1979. By that time, only one of the two authors was still at the Treasury, and he left shortly thereafter. Despite his intention to revise and publish the paper, as conveyed to me in a 1981 letter, the paper has never been published, so the evidence is still not a matter of public record."

"Perhaps the Salomon scandal will wake the Treasury out of its torpor, and lead it to adopt a Dutch auction for all of the securities it issues. If that mini-miracle were to happen, Wall Street would not like it. Authorized primary dealers (of which there currently are 40) would lose any excuse for existence…"

"When he left the Treasury, Jack Bennett returned to Exxon, where he introduced Dutch auctions for at least some Exxon debt issues. Since then, the practice has been spreading, so that it is no longer simply a theoretical possibility". (Milton Freidman, "How to Sell Government Securities," *Wall Street Journal*, August 28, 1991)

This was a good example of a substantive change in policy that was assisted by laboratory and field experiments applied to bidding theory. Many people, and many years, were required: Friedman, Schultz, Wallich; my experiments in 1967 motivated by Milton's original proposal; a Purdue thesis by Meyer Belovicz that greatly extended my *Journal of Business* (1967) results; but especially Che Tsao and Tony Vignola at Treasury and the Federal Reserve who analyzed the field data and the laboratory data, concluding that both pointed to the need for a change in policy. Milton refers to their paper "Price Discrimination and the demand for Treasury's Long Term Securities" which I sent him on July 3, 1979, because he had long been interested in the experiments that I and others were doing. I solicited the Tsao-Vignola paper (letter to Che Tsao dated June 7, 1977) for a refereed series I was editing, *Research in Experimental Economics* in the late 1970s, shortly after Che sent the paper to me (letter June 2, 1977). However, Treasury refused to release it on the excuse that "Senior policy officials at the Treasury feel that the paper deals with a highly sensitive subject and they have not had adequate time to review the paper and its findings" (Letter from Tony Vignola on Treasury letterhead, June 20, 1977). The issue was sensitive only to the investment bankers who did not want to live with rules that are more competitive and substantaily manipulation-proof. After much subsequent prodding by me (assisted by my local congressional representative who was also stonewalled by Treasury), Treasury finally released the paper in 1980. I had it refereed, but by then neither author was at Treasury—they needed access to the data again, now no longer available. Why it was no longer available is one of those questions a politically inspired process could not answer—so much for science, policy, and a glistening case of government-business cronyism.

Meantime, a backstory was playing out with Jack Bennett, who, as indicated by Milton, left Treasury to become an Exxon CFO where he pioneered the sale of Exxon bonds via the "Dutch auction" (uniform price) rules. Whatever other high Treasury officials thought of their bond experiment results, Jack Bennett was convinced that it supported the superiority of uniform clearing price auction rules. Milton is correct that the uniform-price procedure was and continues slowly to gain acceptance, but even he greatly underestimated the ability of the investment banking community to influence, and in fact manipulate, private markets and not only the Treasury bond market. Generations of Treasury officials have had instilled in them the fear that they might hold an auction to sell debt and no one would come—at least not enough bidders; that assigning privileged access to a small number of large primary auction dealers is the only way to assure an "orderly

market" for new issues. Since Treasury officials tend strongly to be investment bankers—through the revolving door connecting them—and primary auction dealers, this stance is self-fulfilling. Even more remarkable, in the Exxon experience, was their influence on private market attempts to bypass the traditional investment banking industry.

Here is a sketch of what Exxon encountered.

Exxon's first Dutch bond auction, for Gulf-Coast Waste disposal facilities, was a spectacular success for Exxon. Bennett had reasoned that Exxon, being one of the largest, best-known, and oldest companies in the world, had no need of investment banking underwriting services. Why not just offer securities directly to investors at auction, using the Treasury auction experiment results to define the rules? There was an immense outcry of complaint about Exxon's tactics of bypassing the traditional underwriting institutions and risking a disorderly resale market since many of the street bidders would be reselling the issue to retail investors.

The warnings of disaster were without merit, as bond dealers privately had no intention of missing out on what might become a trend: The $54.9 million bond offering was covered by nearly four times that volume of bids. Moreover, only 5% of the issue was taken by individual and institutional bidders, and 95% by broker-dealers. Nevertheless, in response to the hue-and-cry complaints, Exxon decided to make concessions. In their second bond issue for the financing of port facilities in Valdez, the bidding would be limited to members of the National Association of Security Dealers, removing all uncertainty that institutional and individuals would "undercut" the bids of the dealers. In effect, bidding for access rights to the chicken coop would be limited to the foxes! (See, Samuel Hayes, "The Exxon Corporation—Dutch Auction," Harvard Business School Case 9-278-070.)

In spite of these two highly successful Exxon bond auctions in 1977, the Exxon "experiment" ended. I do not know the backstory that might explain why, although I can guess. Years later, the experience was repeated by Google that offered shares directly to the market bypassing the underwriters.[1]

Returning to the nexus of the US Treasury and investment banking resistance to changing government bond auction procedures, nothing happened until Salomon Brothers committed outright fraud, abetted by the Treasury's tie-breaking rules in their discriminative auction. Interestingly, the analysis

[1]Here is an article expressing the aspirations of many that street bypass auctions would grow with the Google experience, but it was not yet to be: http://www.bloomberg.com/apps/news?pid=newsarchive&refer=&sid=aF53xCVMXIng.

never changed: Laboratory experiments, followed by two field experiments, tended to support a change in policy. What changed was that Salomon Brothers shot itself, and its investment banking buddies, in the feet.

Treasury auctions are buyer-bid markets, but the same principles apply to ("reverse") seller bid auctions. For decades, people have continued to make the mistake of believing that buyers are better off when sellers' bids to supply a product are accepted at their respect bid price offers, rather than at a uniform highest accepted bid clearing price. It is the other way around. This error reared its head in electricity markets in California and as far away as Britain. Some officials thought this alleged seller "advantage" is what accounts for the high wholesale prices in California and elsewhere when supplies are tight. It does not. Experiments—laboratory and field—with generation companies bidding to supply a wholesale market demonstrate that this is false. In these markets, generator bids are accepted up to the highest priced generator bid, and all generators are paid that highest price. Intuitively, it appears that buyers are leaving money on the table, because most of the generator bids are below the highest and you are "giving" the sellers the difference, and it looks like it can be fixed by paying each accepted generator bid a price equal to their bid supply price. However, if you do, it changes the incentives of the bidders. You have given them a common incentive to gradually raise their bids, and that is exactly what they do. You would find yourself doing it if you were bidding under the discriminative rules. You would not be aware of it because you would not experience what would happen under the alternative uniform price rules. As noted so perceptively by Hayek, the proper focus of social science studies is *the study of what is not*. To understand what is, you must also understand what is not—what things would be like under alternative arrangements besides those we have experience with.

Our experimental colleagues, Robert Thomas and his co-authors at Cornell, got wind of a movement by a regional Independent System Operator (ISO) to change over to an "as bid" generator market. They went to the ISO and assigned the officials and staff to two independent experimental treatments: one with uniform pricing and the other with "as bid" discriminative pricing. The group succumbed to the differential incentives as would anyone, falsifying their own beliefs, and that, said the cat, was that—the ISO continued to use uniform price auctions. My colleagues and I have a paper that demonstrates this "bid creep" as generators gradually extract progressively more surplus from the buyers until the process is arrested by seller entry competition at these higher prices (S. Rassenti, V. Smith and B. Wilson, "Discriminatory Price Auctions in Electricity Markets: Low Volatility at the Expense of High Price Levels," *Journal of Regulatory Economics*, 23(2), 2003, see https://ideas.repec.org/a/kap/regeco/v23y2003i2p109-23.html). Nothing inherently evil here in what people do, it is what comes natural if you choose rules that reward high prices. These behavioral results are just a seller's auction version of the buyer-bid Treasury bond auction I studied back in the 1960s. It was not actually "settled" then, as a principle, and it keeps rising from the dead.

Some theorists believe that revenue will not differ in uniform versus discriminative price auctions because of the "Revenue Equivalence Theorem." That

theorem is for a single isolated auction exchange, not for repeated auctions across time, and this makes a difference as big as night versus day.

From this 1960s beginning, my thoughts gradually evolved and were influenced by literature and ideas from many others, such as Charles Plott and his co-authors in the late 1970s and into the 1980s. There was a continuous transformation of our thinking as we became more experienced with a great variety of different experimental and institutional contexts. Moreover, the community of scholars participating in that process was growing rapidly. Twenty years later, experimental economics was no longer a "hobby" that I did on the side. That process began as I moved up to associate professor in 1957 and full professor in 1961 at Purdue. Experimental economics was gradually becoming a way of life, as well as a robust research program. With tenure, I could work on whatever I wanted to, whether or not people thought it was economics. I knew I would follow where experiments led me.

The biggest impact on my thinking, about the scope of experiment made possible by a computer network, came through my joint work with Stephen Rassenti, beginning in the 1970s. The airlines were scheduled for deregulation, and Stephen was looking for a thesis topic. I pointed out to him that "deregulation" meant that individual airlines, not the government by approval (through the Civil Aeronautics Board, CAB), would choose their own operating routes; but airplanes still had to land and take off, and no one was thinking about the airport runway slot rights. New flexibility in route choice would impact airport runway utilization. Suppose a market was to be made in these rights. How would you do it? This led to our first case of a "smart" computer-assisted market and culminated in Stephen's important 1981 thesis "0-1 Allocation Problems: Algorithms and Applications." E-commerce in the laboratory, beginning in 1975, changed the way we thought about market design and test-bedding. It was now possible to combine the information advantages of decentralized decision—for example, bidding to supply or to buy—with the coordination advantages of the central processing of messages to achieve more efficient outcomes. The excitement of discovery was exhilarating.

David Grether, Mark Isaac, and Charles Plott had proposed a complex auction market for runway slots. They proposed that the slots at each airport be simultaneously and independently auctioned, followed by an aftermarket where people could re-trade to fill in the missed combination packages. We saw a way of doing it in one primary computer-assisted auction.

In this application, the buyers of airport landing and takeoff slot rights enter bids for each "package" combination of takeoffs and landings required to support each flight schedule. Each airport has a fixed capacity of these slots they can process per period across all airlines submitting package bids. Essentially, Stephen's algorithms accomplished two things: computed period prices for each airport's scarce slot capacity; simultaneously, maximized system net revenue—package bid value less slot price cost—across all bidders. Basically, the algorithms maximized the economist's concept of consumer or buyer's surplus (profit). Grether, Isaac, and Plott had provided a sequential procedure, whereby buyers could achieve this same objective through a sealed-bid provisional allocation of slots, and then a multilateral trading system, whereby buyers filled in the optimal combinations. This was a labor-intensive process. Stephen's algorithms did the matching, based on willingness to pay for each package of rights, much like UBER and LYFT match taxis and passengers for efficient movement of people in real time. The algorithms easily did the heavy lifting.

Moreover, this exercise generalized to the concept of "smart" computer-assisted markets, whereby algorithms designed for any complex market maximized buyer plus seller surplus based on individual dispersed willingness to pay and willingness to accept bids. Some thought the politicians would never buy it, but we could not have cared less because our constituency did not consist of politicos. Stephen, I, and later co-authors—Kevin McCabe, David Porter, Mark Olson, Jim Murphy, Jeff Banks, Bart Wilson, Elizabeth Hoffman, Mary Rigdon, and others—would apply these principles to gas pipeline, water, and electrical networks; to scheduling; and to the FCC spectrum auctions. In these applications, particularly electricity and the spectrum, you can avoid smarts, but only at the peril of the long-run betterment of citizens and their governments.

Here is how we expressed it in our 1982 paper (S. Rassenti, V. L. Smith and R. Bulfin), "A Combinatorial Auction Mechanism ...," *Bell Journal of Economics*, 1982, p. 62: To our knowledge, this study constitutes the first attempt to design a "smart" computer-assisted exchange institution. In all the computer-assisted markets known to us in the field, as well as those studied in laboratory experiments, the computer passively records bids and contracts and routinely enforces the trading rules of the institution. The "RSB mechanism" has potential application to any market in which commodities are composed of combinations of elemental items (or characteristics). The distinguishing feature of our combinatorial auction is that it allows consumers to define the commodity by means of the bids tendered for alternative packages of elemental items [bundles of property rights]. It eliminates the necessity for producers to antici-

pate, perhaps at substantial risk and cost, the commodity packages valued most highly in the market. The experimental results suggest that: (a) The procedures of the mechanism are operational, i.e., motivated individuals can execute the required task with a minimum of instruction and training; (b) the extent of demand under revelation by participants is not large, i.e., allocative efficiencies of 98–99% of the possible surplus seem to be achievable over time with experienced bidders. This occurred despite repeated early attempts by inexperienced subjects to manipulate the mechanism and to engage in speculative purchases.

Hence, beginning in the years from 1976 to 1980, test-bedding became an integral part of a much larger program in economic system design, including the developing of the smart computer-assisted market. The rapid advance in computer and communication technology seemed to me to make this development a slam dunk.

It would take a while, however, for other experimentalists to see and adopt the advantages of the computer–human interface, e.g., at Caltech that development was delayed into the 1980s, when Charlie commissioned MUDA (multiple-unit double auctions). This was a first-class double auction software program; you needed only to add written instructions to the code. Charlie supplemented with written and oral instructions, whereas our tradition was to include instructions directly in computer-screen presentation, which facilitated replication control by other users. At first, the reaction of experimentalists had been that "programming for experiments is a black hole for throwing money into" (I heard it at Caltech, of all places). Indeed, this was true, but as we learned to surmount the challenges, it started to pay off. Every advance always built on the experience of others—there was no need to repeat what turned out to be earlier shortcomings. Arlie found that out in 1976 with his own product and started over, reprogramming his Plato double auction software.

New technologies always foster enormous resistance from the status quo alternatives. I had experienced directly the resistance to the computerized trading of securities beginning in the 1960s, and that resistance would extend to derivatives and currencies—new products traded the old-fashioned way. After 20 or 30 years, computerization had started to make inroads, and then, the Internet began to take over, trading made more and more use of the new technology.

The experimental program at Arizona, particularly its e-commerce version, was operating at full speed by the early 1980s. Based on my earlier work on the Treasury bill auctions and the new experiments on single-object auctions with John Titus and Vicki Coppinger (nee Sandler), I had received

an NSF grant to extend the experimental study of auctions. Bruce Roberson had developed the Plato software to design rigorous computerized experiments allowing pairwise comparisons of Dutch, sealed-bid first-price and second-price auctions for single objects. Jim Cox had joined the Arizona faculty from the University of Massachusetts and asked if he could join me on the NSF project. I agreed. He and I worked on the theory, building on the innovations of Bill Vickrey. John Ledyard had derived an extension of the Vickrey risk-neutral Nash equilibrium bid function for constant relative risk-averse agents. There was an error in the derivation, however, a fact noted by Dave Porter, then a graduate student at U of A. Meanwhile, Bruce finished his Plato program and we started running experiments.

Jimmie Walker, who came to the U of A after finishing his Ph.D. at Texas A&M University, started to work on multiple-unit Plato versions of the uniform and discriminative auctions for experiments. Jimmie played a central role in all our subsequent studies of bidding behavior in auctions for single and multiple units with varying numbers of bidders. Lack of vision resulted in our inability to retain him. The U of A's big loss was an even bigger gain for Indiana University, where he took root in a fertile environment of political economy research with Elinor (Lin) Ostrom.

I was able to extend the derivation of the bid function to multiple units—one for each agent—in discriminative auctions with a uniform distribution of values, and Jim Cox came up with a derivation of equilibrium bid functions for the general class of log-concave utility functions. This led to a series of papers developing the theory and conducting experimental tests of single- and multiple-unit auctions.

Mark Isaac had also come to the U of A after completing his Ph.D. at Caltech, and he and I collaborated on several papers dealing with industrial organization and antitrust issues.

Arlie and I also worked on asset trading, which became a very significant new initiative in the early 1980s. I will begin with some background that led into that initiative.

At Caltech, Charlie Plott, Ross Miller, and I had done two auction experiments in which we cycled the level of demand in a regular shifting, repeat, "seasonal" pattern, low, high, low, high, and so on in successive trading periods. In one of these experiments, we had six buyers, six sellers, and two "traders." The buyers were assigned unit values, and the sellers assigned unit costs, with the traders having the exclusive right to buy in one period and carry over the units for resale in the next period. When demand was low, only the very lowest cost sellers were able to make profitable sales; when demand was high, many high cost sellers can sell profitably. The efficiency of

this market can be increased by speculative traders, who buy in the low price season and resell in the high price season. This action raises the price in the off-season, lowering the price in the on-season. Efficiency increases because in the off-season some of the units sold by low cost sellers are carried over and substitute for the higher cost units that otherwise would be sold in the high demand on-season. Speculation worked as predicted by theory: Price in both seasons tended toward the same level with traders buying excess units produced above demand in the low demand season and selling them in the high demand season.

The Miller, Plott, and Smith experiments were done by hand, and Arlie went to work modifying his e-commerce software to allow for the added activity of traders who could buy in any period and sell in another period. The main idea was to replicate and conduct many more experiments like the two reported in the original speculation experiments. New research contributions came out of that exercise. Notice, however, that we were introducing for the first time traders who could buy for resale. Their purchases were not for consumption as is and was the case for all other purchases.

Independent of this work, we had been talking about a research program in which we would study asset market trading—so far all our work had involved supply and demand markets with per-period flows across time. In effect, these were markets for consumer non-durables; in Chapter 21 below, I discuss our much later insights into the "two kinds of markets." One day it struck me that Arlie's program introducing speculative traders—who could buy for resale in a supply and demand setting—involved asset trading. Why not take that new code, add provision for "dividend" realizations from asset units held in a period, and develop a new stand-alone program dedicated to asset trading across time, based on initial assignments of "cash" and "shares" to each subject in an experiment. Arlie created the new software and we were off and running.

However, the results from the first asset experiments did not come close to satisfying our expectations. We had a lot to learn from our subjects about this new environment. Recall that my expectations were not fulfilled in the first supply and demand experiments, as well. Supply and demand theory had worked far better than anyone—certainly than I—had expected, and that led to new experiments intended to contradict and thereby to better understand those strong results and their limitations. In the new asset market experiments, the data did not converge quickly, as expected, to the fundamental value of the shares based on the dividends they earned. The experimental setup was simple: Each subject received an up-front endowment of shares and cash. Trading took place in a sequence

of 15 periods. At the end of each period, each share would receive a dividend with expected value specified by a dividend distribution, e.g., 0, 8, 28, 60 cents, with equal probability yielding a mean of 24 cents per share. Consequently, the fundamental dividend ("rational expectations") value of a share was $15 \times 24 = 360$ in period 1, 336 in period 2, and so on down to 24 in period 15 as the inventory of dividend drawing rights expired. In each period, the future expected dividend value of a share was well defined by theory, and a price below that value would provide a profitable opportunity for anyone to buy, a price above that value an opportunity to sell profitably.

Originally, we plan to begin with a protocol and environment so transparent that we would get a rational expectations outcome, as above. At the next stage planned to manipulate information and see if we could create bubbles. Those best laid plans got shot down with the very first "transparent environment experiments." They produced big bubbles—large deviations from the declining path of fundamental value. More experiments established the replicability of the first results. Others could not believe what we were observing—hardly a surprise, as we also hadn't initially believed them. Colin Camerer, then at Wharton, referred to our results as "Arizona phenomena," but when he tried them at the Wharton School they were surfacing in the Quaker State as well.

Charlie Plott did not believe our results. I suggested to Dave Porter that he go to Caltech and conduct one of our asset market experiments, which he did; better yet, he ran it in Charlie's class. Of course his class yielded pretty much a standard garden variety 15-period asset trading bubble—by then, we were getting used to it. Charlie had a new procedure he wanted to introduce. He gave the subjects a blank table and required them each period to write down what was the next period's fundamental value. Our Plato system software reminded the subjects each period what was the new dividend-adjusted value of a share in the next period, but Charlie thought he would make them write it down! Then, midstream in the experiment, Charlie looks out and notes they are not all writing it down. Dave walks to a subject failing to comply, reminds him, and is told: "You write it down I am busy trading."

We were also moved to other subject groups. We did asset trading using members of the Tucson Kiwanis Club—with whom the supply and demand experiment converged—corporate middle-level executives, and a group of over-the-counter stock traders in Chicago. We soon found the answer to the ubiquitous questions: What do undergraduates know? Plenty. What do people in the "real world" do? The same.

Many experiments and papers later failed to find treatments that squelched these bubble tendencies, except for subject experience; bring the same subjects back a second time, and a third time, and by the third time around the trades were finally approximating the decline in fundamental value. Finally, however, we (with Mark van Boening and Charissa Wellford) found a simple treatment that defanged bubbles from the start. We declared only one large randomly drawn dividend at the end of the experiment. There was no longer a flow of new cash each period into the market for the purchase of shares. The bubbles did not occur. So, we reasoned, suppose we do a series of experiments in which we pay half the regular declared dividend into their trading account each period; then, after trading ends, pay the other half in a single draw. This should yield bubbles in between the no-dividend and full-dividend payment per period protocols. It worked: We observed intermediate-level bubbles. Hence, the bubbles we were observing in the laboratory depended vitally on the inflow of new cash; stop the inflow and you stop the propensity to bubble (see our paper "Dividend Timing and Behavior in Laboratory Asset Markets," *Economic Theory*, 16 (3) 2000, pp. 567–583; reprinted in T. Cason and C. Noussair, eds., *Advances in Experimental Economics.* New York: Springer, 2000). Years later, the economy would crash due to a precipitate decline in housing prices against fixed mortgage debt. The house price decline was anticipated by a decline in the flow of new mortgage credit money into housing purchases (see S. Gjerstad and V. Smith, *Rethinking Housing Bubbles*, Chapters 2 and 3. Cambridge University Press, 2014).

These were busy years. Parallel with all these research/teaching activities, from 1975 to 1985, I was deeply involved in the development of a support base for experimental research at U of A; that base had two dimensions:

1. New faculty hires who were dedicated to experimental research. These included Jim Cox (1977), Mark Isaac (1980), Stan Reynolds (1982), and Kevin McCabe (1982).
2. External funding, primarily from the National Science Foundation, which had supported me at Purdue as early as 1962 and later at other universities. The administration at U of A was committed to support that effort, but it was up to us to define and make the funding appeals. My fledgling efforts were broadened and expanded by the new smart market experiments that I have reported above. During this nine-year period (1976–1985), the growing experimental group was awarded over 1.3 million dollars in research funds, mostly from public sources like the National Science Foundation.

It was during this period that I had occasion to return for a short period to a direct political activist role for the first (and last) time since 1936! The most immediate impetus for this turn (tinged madness?) came via an invitation from the Cato Institute to attend a conference/workshop at Rio Rico, Arizona (a "Luxury Resort" except that it was essentially bankrupt); this was in 1977 or 1978. Founded in Wichita in 1974 as the Charles Koch Foundation, in 1976 the name was changed to the Cato Institute.

Rio Rico was only 60 miles south of Tucson, so I accepted the invitation and drove down. That is where I first met Ed Crane, Charles Koch, Earl Ravenal, Leonard Liggio, and assorted other Cato Institute regulars, all speakers at this event. On Saturday, Alan Greenspan flew in for an after-dinner speech, so I met him. (Incidentally, all this eventually led me to read Ayn Rand, *Atlas Shrugged* surely making me one of the last to read this classic.) I recall Charles Koch speaking of his opposition to the Vietnam War, with stories of having to deal with some suspected politically motivated harassment problems with the Johnson administration.

Subsequently, Ed and Charles became active in the Libertarian Party. The National LP Convention was held in Los Angeles in 1979, and Carol—a long-time sympathizer—and I went to the convention. Carol had historic roots in libertarianism when she was working at the NBER in New York. She had dated Lanny Friedlander, who founded *Reason Magazine* before he sold it to Bob Poole, Manny Klausner, and my Chapman Colleague, Tibor Machan (18 March 1939–24 March 2016). It is a small world indeed. Jimmy Carter, the Democratic candidate, was considered a weak president—he was not yet fully appreciated for his outsized success in deregulating transportation. The Republican candidate Ronald Reagan, fresh from the governorship of California, looked like he would go down as the last of the big time spenders if he won; he won, but was not nearly the last of the big time spenders.

The Libertarian candidate was Ed Clark, a magnificent person with a lovely Hispanic wife, but, more important, I liked their program. Becoming an activist, I put together a group of 50 Economists for Ed Clark. Two of the members I recruited were editors of economics journals: The *Journal of Political economy* (Sam Peltzman) and *American Economic Review* (Robert Clower).

We were young enough—only in our fifties.

In 1984, The Arizona Corporation Commission (ACC) provided us with an unprecedented opportunity to examine state utility regulation and to consider its alternatives. It was a political accident. The ACC consists of a three-person elected commission. One of the commissioners had died in office; another, with higher political ambitions, had resigned. Arizona's Governor Babbitt appointed two replacement commissioners to sit until the next regular election. One was Marianne Jennings, a professor of business ethics at Arizona State University; the second was Junius Hoffman, professor of law at the University of Arizona. I knew neither of them at the time, but I later heard that both had been astonished to find out what transpires under

the heading of "utility regulation." You know how it is: Once you have seen sausage (including headcheese) made, you can't eat it.

At hearings, Junius passed the line of utility lawyers waiting in a row of chairs and was heard to ask, "Got your meters running, boys?" I also heard that neither of the new Babbitt appointments wanted to run for the office and continue as commissioners, but desired to have some influence on the process beyond their short tenures. Two of our graduate students were working at the ACC: Dave Porter and Glenn Vail. Their education of the ACC on the capabilities of experimental methods resulted in the Commission seeing an experimental research project as a way to bring fresh perspectives on regulation into the public domain. They were right, and they were successful in having a long-term impact on utility liberalization though to this day, not a particularly serious impact in Arizona.

As we defined it, the project had several parts, but two alternatives to rate-of-return regulation (RORR) were primary: incentive regulation and deregulation. Mark Isaac and Jim Cox pursued the study of incentive regulation; Stephen Rassenti and I, together with Dave Pingry, opted to examine deregulation and focus on the electrical power industry. Mark and Jim did some generic incentive mechanisms, ran many experiments, and eventually published the results. Our part of the work took much longer to be fully completed and published, but it provided a direct lead into the worldwide deregulation, privatization, and liberalization reforms of the late 1980s and 1990s. It became an influential research program that is still ongoing, though that seemed like a very remote possibility in 1985.

In the 1980s, it was almost universally believed that economies of scale, economies of coordination, and wasteful duplication of wires in distribution and transmission meant that electricity was inherently a natural monopoly.

The theory of natural monopoly stems from John Stuart Mill, although he did not use the term. Writing in 1848, Mill argued that it was obviously wasteful for more than one mail carrier to retrace the same pathways to deliver the mail, and likewise, it would be wasteful for two cities to be connected by two parallel railroad tracks. You may have noticed that in the nineteenth century many people got rich building multiple-path competing railroads, and more recently several companies—including UPS and Federal Express—have profited handsomely in competition with the U.S. Post Office, which is still a money loser. Of course the USPO has often argued that that is because the competitors are "cream skimmers," servicing only profitable routes and deliveries. Yes, but that begs the central question: Why has the USPO created so much cream to skim by underpricing some classes of service and overpricing others?

There were no more than a handful of academic and industry dissenters who saw the merits of deregulating electric generation—one of the latter turned out to be Ted Welp, president and CEO of Tucson Electric Power (TEP); also, I believe, William W. Barry, president of Virginia Electric Power, had favored deregulation. (See Daniel Southern, "Power Struggle in Virginia," *The Washington Post*, June 27, 1994.) Some TEP joker at this time suggested that TEP's name be changed to Western Electric Light and Power (WELP). The traditional and unchallenged assumptions of natural monopoly produced a world in which no one had asked, "If you were to deregulate electricity and allow markets to discipline prices, how would you do it, and how might it work?" If you don't ask, you won't think about or investigate the possible answers.

A year later, in 1985, we filed our report recommending that the "energy business" be separated from the "wires business." Generators would be sold or financially spun off with separate managements, say into five companies that would bid into a spot market—the Arizona Energy Exchange—to supply power to the network. Local distribution utilities do not have to produce their own energy any more than they need to manufacture the trucks used by their service people. In addition, we proposed that the Exchange be organized as a two-sided bidding mechanism, with demand-side wholesalers and other buyers empowered to bid any of their interruptible loads into the spot market.

In time, our experiments would show that strategic demand-side bidding easily controlled price spikes in wholesale markets like those seen in California, the Midwest, the South, and the East Coast. The retail customer's energy does not have to be provided by the local wires company any more than your car rental company needs to supply you with gasoline—you can buy your own in a separate market. We argued that the rental rate for the wires could or would continue to be regulated, but the utility would be prohibited from having the exclusive legal right to tie the sale of energy to the rental of the wires. Thus, we recommended repeal of the law requiring consumers to buy their energy and rent the wires from the same local utility. In the non-regulated sector of the economy, tie-in sales are illegal, so extending this principle to the utilities was hardly revolutionary. We were simply proposing that common features of any competitive business environment ought now to be extended to electricity, an industry that had long been protected from such forces.

To provide some contestability in the wires business, we also proposed that the franchised legal protection of the local wires monopoly be eliminated. Specifically, utility easements on all property would be declared

open to entry by alternative cable and pole users, subject only to the usual environmental and safety considerations. If electricity is truly a "natural" monopoly, it doesn't require any "unnatural" franchised legal protection. Right? Not quite: historically, legal protection from entry had been long part of the lobbying position of the industry, just in case the monopoly was not sufficiently "natural." If electricity had ever been a natural monopoly, technological change had undermined it, as it had its sister industry, telecommunications. Our proposed changes would have aligned the organization of the industry with contemporary technology.

Before filing the final report, we met with each of the major stakeholders. The utility sector meetings included TEP, where Ted Welp understood our study so well that he chimed into answer his own management team's objections to our proposals. The other regulated companies were Arizona Public Service and Salt River Project. We also met with the key people at the Regulated Utilities Consumer Organization (RUCO). This is the watchdog organization created and designed to protect the consumer's interest, and it heartily approved of our study. After the meeting, Mike Block—the overall administrator of our ACC project—and I met with the chairwoman of RUCO. Since she liked our proposal, we asked her if she could openly support our position after it was publically announced. She regretted to say that RUCO could not support us publicly. Here was the problem, she said: RUCO's budget was up for renewal by the legislative committee; one of the utilities had been exceptionally critical of RUCO's stance lately, and that she was concerned that her budget renewal might be endangered!

Wow, there it was; economists have a theory called the Capture Theory of Regulation. According to this theory, a regulated industry necessarily interacts constantly with the regulating organs of government. In time, the regulators are captured by the industry. In this case, the so-called independent watchdog, approved and set up to counteract this tendency, had also been captured by the industry. RUCO's chairwoman was bearing eloquent witness to this obscure and difficult-to-prove model of the regulatory process. Actually, the theory has some flaws. The two sides are better described as capturing each other, since they have a commonality of interest requiring their joint attention, with the consumer paying whatever the bill for costs incurred.

Capture theory is well supported by the following observation: Whenever there is serious discussion of deregulating any industry, it's the industry itself that opposes it along with the regulators; regulation is said to be needed in order to serve the "public interest." Except for United and Frontier Airlines, the entire industry opposed airline deregulation in the 1970s; the railroad and trucking industries opposed deregulation of the Interstate Commerce

Commission; and so on. You have to ask, why? Well, *they like it*, and defend and support regulation, although often not the particular actions of the regulators. The average person believes falsely that regulation protects the consumer, and politicians who want to be elected will support regulation if that causes their constituents to vote for them. Regulatory regimes tend to be founded firmly on the premise that it will be administered by a fantasized perfect consumer protection process, but regulation also serves to protect producer interest from the consequences of consumer choice. The paradigm here is illustrated in professional and trade licensing, e.g., prohibiting dental technicians from practicing separately from dentists, justified by "the need to protect quality." or, limits on opening new firms that cause oversupply.

We presented our final report to a reconstituted commission with the two newly elected commissioners freshly arrived to do regulatory battle with the forces of general business (and, specifically, utility) evil. They thought we were mad. In addition, one of the new commissioners suggested that our study was not properly balanced. We had not considered the alternative of state government ownership of the industry! (Interestingly, he called me a couple of years later, after seeing sausage made, to say that he now understood where we were coming from in our original report, so much for the regulatory savvy of elected officials.) Our report was submitted to the ACC in 1985 on the eve of the worldwide unraveling of command-and-control government-owned industries, but there was no unraveling of American-style rate-of-return regulation.

The unsympathetic reception of our proposal did not put an end to our work. As the worldwide liberalization movement gathered steam, the study was picked up by many, most prominently in the international community, followed by many in the domestic industry. Stephen Rassenti and I would eventually serve as research consultants to New Zealand and Australia private and public entities, to a few companies in the USA, and, with various co-authors, conduct many experimental studies of structural issues related to competitiveness in the industry.

When Stephen and I first went to Australia (1993), it was by invitation of Prospect Electricity (later, part of Integral Energy), the second largest energy distribution company in Australia. This trip was part of the Australian political debate on liberalizing the industry. It was essentially the buyer side of the industry—the industrial, commercial, and distribution company buyers of bulk power—that pushed to explore liberalization as a possible means of lowering their energy cost, and who were sponsoring our visit. We listened to concerns that energy costs in their energy-intensive export industries were crimping their ability to compete internationally. Each state owned its own

electricity-generating stations and was unfavorably disposed toward the idea of a wholesale power market. We faced many skeptics who believed it was impossible to make a market in electric energy.

The same issues had arisen in New Zealand. After speaking at a luncheon in Wellington, I recall a young woman stood up and said that markets would not work; it was an industry that necessarily must be owned by the government; "I know, because I am an engineer." I was able to respond to the effect that I was also an engineer; that I once believed what she was saying; I understood what was bothering her and was sure I could convince her otherwise from the experiments we had done.

In Australia, our approach was to place industry, government, distributors, and other participants at our workshops, into a wholesale market experiment consisting of three radial nodes—a central demand center with limited generating capability and two more remote smaller demand centers with excess generating capacity that could serve the center as well as their local demand. (Conceptually, it was the UK, with London served by large power sources to the north and smaller sources to the south.) In our follow-up, we could demonstrate that the group was quite effective in making an efficient market in power. The question: Is anything wrong with the experiment? How can we improve it? Implicitly, the burden of proof was on the skeptics to defend their beliefs in the face of this evidence, and we were open to changes if the experiment was flawed. Now is the time to explore any concerns with our low cost, test-bed technology. Essentially, this process won a series of battles and ultimately the war: Subsequently, the central government created the National Grid Management Council (NGMC) to plan and oversee a wholesale energy market embracing the states, integrated by an interconnected national grid.

Through our Australian contacts, we gained approval to conduct laboratory experiments with a prototype for the proposed market. We were consultants on software specifications and experimental design, but all development and experiments were to be conducted by the Australians. This enabled the Aussies to get hands-on experience in test cycles of experiment-feedback-redesign-experiment. The process culminated in a two-week (7 hours per day) electronic trading experiment using non-industry participants trained in the exchange procedures and earning significant cash profits based on induced costs and demands, using Australian generation and grid parameter characteristics. We had advised against using government or industry people—politically biased either for or against—and favored using independent subjects who in effect would become trained "professionals" in trading based directly on their experience.

Victoria and New South Wales began separate markets in power in 1996. These, along with the other Australian coastal states in the southeast, joined in the National Electricity Market on December 13, 1998. It was our proudest moment: proof-of-concept laboratory experiments merged seamlessly into design experiments and into the field. The result was a modern late twentieth-century smart computer-assisted market that has been continuously improved and updated in response to ongoing technological change since its inception.

There is an untold story related to our consulting for a California utility—a further chapter on the subject of Regulatory Capture Theory. We conducted several workshops at ESL in Arizona from 1995 to 1997. In groups of about twenty, utility executives from all over the USA came to the laboratory for a one-day workshop on wholesale power market design issues. They participated in an electricity spot market as supply-side generator owners, but also as wholesale bulk demand-side buyers of power for resale to retail customers or for industrial use. The experiments demonstrated that in a two-sided bidding market, five generating companies were enough to yield competitive allocations in an experimental design with a comparable number of demand-side wholesale bidders, each having a limited capability of interrupting a portion of their demand, especially on-peak demand. Therefore, given the great volatility in the marginal cost of a kilowatt-hour of energy throughout the day, week, and season, we emphasized the need for the local distribution company to get as many customers as possible on interruptible peak pricing contracts in return for big savings on their cost of reselling power for retail consumption. In this manner, wholesale buyers could discipline wholesale prices and help smooth out the common occurrence of price spikes in peak consumption periods of the day.

With few exceptions, most of the utility executives saw their main problem as striking a political deal with their commissions for an increase in average retail prices per kilowatt-hour to cover anticipated "stranded costs" due to deregulation. *Stranded cost* was a term used to describe the conjectured prospect that generation investments incurred in good faith by the utilities, in anticipation of a guaranteed price and fair regulated return on their investments, might not be recoverable in the new competitive market regime. Characteristically, in the regulatory mind-set of the industry, *stranded costs* were thought likely to occur, because the regulatory authorities were changing from the previous rules that assured a rate-of-return regulation (RORR) determined profit on investment, to new rules requiring them to bear risks previously borne by customers. Such assets, it was believed, would not command a price above their depreciated cost, and they wanted the anticipated losses covered as the political price for agreeing to deregulation. Utility executives think about the world the way they think about their regulated mini-world.

We did not see this issue as their immediate and most pressing problem, based on our learning from experiments and on our applied design work in New Zealand and Australia. Rather, we knew that the deregulation of wholesale power markets would bring high volatility in wholesale prices, because

of the great variation in the marginal cost of generating energy to satisfy off-peak and on-peak demand levels. To protect against this volatility, the main task of a local utility should be to restructure their demand to include more interruptible retail contracts that would allow them to reduce consumption in response to high on-peak prices.

Under regulation, during peak demand hours, the energy cost alone can rise far above the regulated retail price. The utility loses money by, in effect, subsidizing on-peak consumption; these losses have to be offset by the profit earned from off-peak and weekend sales when energy cost is well below the retail price. The utility is, in effect, taxing this off-peak consumption. With deregulation, we argued that it was not in the utility's immediate profit interest to continue this inefficient practice and they would be well advised to prepare for it.

We also saw the *stranded cost* argument as a red herring—a way for the utilities to extract a price for agreeing to deregulation, but irrelevant to their need to adapt to a new world. But it served to illustrate that the entire industry was oriented to historical experiences in cost recovery, not flexibility in meeting the future—a consequence of about 100 years of rate-of-return regulation. One of a few utility companies that saw potential merit in our argument was a California utility.

The California utility in question paid us consulting fees to come to California and rerun our experiments with other executives and staff besides the few who had come to the Arizona laboratory to participate in our workshops for them. We proposed to its top management that we design experiments based on that utility's particular grid and generator parameters and get data on the specific demand-side bidding problem that the utility faced. That would allow us to evaluate the savings benefit from an x percent increase in their retail demand responsiveness, which could be compared with the investment cost of achieving that retail flexibility. In the course of the roundtable discussion with the company's board members, many comments were favorable to this strategic plan. But their legal counsel objected, pointing out that "this is 'science,'" and noting that they could not control the outcome. Did the company really want to take that path? They looked to me for an answer. I agreed with the counsel's premise, saying, "It's precisely because you can't control the result that the experiments are of value. They give you the opportunity to learn about possible consequences and prepare for them."

In the end, counsel carried the day. We were invited, however, to support their political bid to the State Commission for a price increase to cover stranded costs. We respectfully declined to do this sort of "consulting," as we thought it would not serve their interest, which, after all, was our purpose for being there. Our expertise was not in the political economy of lobbying their State Commission, nor did we see that as their primary problem.

The consequences were recorded prominently in California history: The California utilities are estimated to have lost $15 billion buying at very high wholesale prices and reselling at the much lower retail rate they had negotiated as the price of their consent—some bargain! Of course, we never forecast, nor did we even dream, that the problem would become so severe. This unusual severity was due to the confluence of such events as abnormally warm weather, a nuclear plant offline, and low hydro reservoirs in the Pacific Northwest. All power systems in the world are vulnerable to extreme condi-

tions in which unresponsive demand strains the supply system and threatens power shortages and outages. That is the great danger of a regulatory system in which consumer prices are fixed, and all adjustment to hourly, weekly, and seasonal changes in demand is a must-serve mandate imposed on the supply system; generating companies love it, but retail distribution utilities are squeezed unmercifully if they don't recognize and prepare for it in the way we prescribed.

I should also note that the widespread anticipation by the industry, and the regulators, that there would be *stranded costs* turned out to be without empirical merit. One generator auction sale after another brought prices in excess of the fully depreciated historical cost of the generation assets. Why, given all the rhetoric about stranded cost, were these losses not revealed in the auction prices for those assets? I think there is a simple reason for this: When you sell an operating generator connected to a transmission network, you are also conveying that generator's established access rights to the network along with the generation plant. Those access rights are valuable, they are not part of the historical investment cost in the generator itself, but that network value will be reflected in the price that the generator will fetch. Market prices reflect system values, and this tells you one more way in which regulation, based on piecemeal historical cost, is irrelevant in determining economic worth and efficient pricing by management. System value, moreover, would have been enhanced by deregulation and exposure to broader more interconnected market opportunities.

Similarly, when an airline goes bankrupt or is sold to another airline, an important part of the value of the company's aircraft is that they are combined with the right to land and take off in airports served by the airline. These rights are part of the package value of the airline and may be more desired than any other asset. The airplanes have an active resale market and are likely to be competitively priced, but no such market exists for runway use rights, and they are highly valuable.

Just as aircraft are more valuable with joint airport landing rights, so also are generators with network connection rights. This analysis seems to solve the mystery of why the depreciated construction cost of generators undervalues their worth when auctioned with implicit hook-up rights to the grid!

I have covered here only the tip of an iceberg of developments that created the synergy that enabled the University of Arizona to emerge as a prominent center in experimental methodology with applications to the design of electronic markets. That success depended importantly on state and university support beginning in 1985. In that year, the Economic Science Laboratory (ESL) was officially founded by action of the Board of Regents and the Arizona State Legislature, who funded ESL as a research center by a direct budget line from the state. ESL support was in the form of a "Decision Package." This was part of a state vehicle for the competitive funding of research for several years by the state legislature. The program was

justified in expectation that such funds would generate additional research support from industry and government. Thus, *our mandate in the creation of ESL was to raise $2 to $3 for each dollar of state funding for ESL*, a goal that ESL surpassed, but not until a decade later, from 1995 to 2000.

As a vehicle for implementing the mandate, the university helpfully created an ESL "Revenue Account" in 1985 to which payments were to be made for rental fees whenever the ESL facilities were used by external commercial or government applied research contractors. Subsequently, the university invited ESL faculty to start a private company under a university program for "technology transfer." That company began paying $500/hour into this account for all its contract use of the facilities. In the late 1990s, one contract alone generated more than $200,000 in fees for the ESL/university account. We finally were beginning to perform on the mandate originating a decade earlier.

During the next fifteen years, many new milestones were passed. New experimental scholars included Brian Binger and Betsy Hoffman. (Betsy left to become Dean of Arts and Sciences at Iowa State University. She followed as Provost at the University of Illinois, Chicago. Finally, she became president of the University of Colorado.) Dave Porter came to the U of A from the Jet Propulsion Laboratory at Caltech; Mark Olson came from Purdue; Kevin McCabe was an ESL research scholar from 1988 to 1990, when he left for the University of Minnesota, but returned to the U of A as a tenured economics professor in 1995.

An early draft of this autobiography contained two long chapters, entitled respectively "Initial Flight of the Phoenix" and "Final Flight of the Phoenix." They dealt with the period 1985–2001, which ended when seven of us—all collaborators—left the U of A for George Mason University in Arlington, Virginia. As indicated above that story must be told, but this is not the place to tell it. You deserve to be spared those excruciating details. Versions of those two chapters and all the supporting e-mail and other documentation are at Duke University Libraries, which forty-odd years ago asked—and I consented—to administer the archive of my collected correspondence and professional papers.

Suffice it to say that the opportunities that brought us all to Arizona, sadly, were no longer available. This was very disappointing to us, but my colleagues and I did what we had to do to continue our work. Much remained to be accomplished, and I was only seventy-four years from birth in the move year of 2001.

I and three of my colleagues at Arizona were subjected to allegations of fraud and criminal conduct and repeated threats of "police action." None of these claims led to any legal action—an outcome that was absolutely inexcusable, of course, if those making the charges actually believed them based on facts or evidence. We were charged with bypassing the University Office of Sponsored Project (OSP) grant procedures in doing applied research through the private company the university had invited us to set up. However, we had followed the original Decision Package protocol and paid all ESL fees through the university-created Revenue Account set up for this purpose when ESL was founded. Eventually, it was confirmed that our use of the ESL Revenue Account had generated more funds for the university than if the grants in question had gone through OSP. After five years, the issues were settled through mediation in January 2004. In that process, we finally had direct contact with university administrators. We had been repeatedly denied such contact by university counsel, to whom the entire issue had been delegated by university administrators. In mediation, we could all talk, and they learned all the above facts. In response to the question, from the vice president of research, "Who set up this Revenue Account," I was able to say (and show), "You did"; obviously, I never could have done this except through university accounting and approval. People do not understand the nature of university bureaucracies wherein the left hand operates without a clue as to what the right hand is doing. Nor is it appreciated that all conversation between principals ceases once counsel enters as the "mouthpiece!" Moreover, counsel has their own incentives that are not necessarily well aligned with their principals.

So mediation produced the public announced result:

"*No admission of wrongdoing. The parties hereby agree, acknowledge and recognize that nothing contained in this Agreement shall constitute or be treated as an admission of liability or wrongdoing by the parties. It is expressly understood and agreed that the settlement made hereunder is by way of compromise and in full and final settlement of disputed claims. The parties hereto acknowledge that each disputes and denies all liability and damages claimed against them, and denies they were responsible in any way for the same, or for any damages allegedly resulting therefrom; and the parties hereto further acknowledge that this settlement made hereunder is not and should not be construed as an admission of liability by any of the parties hereto.*"

This was legalese for "somebody goofed," and their faces need to be saved; each side gets out of the corner into which the process has painted them.

Curiously, sometime in 2000–2001, a vice dean at Arizona had heard rumors that we all might leave; whereupon he told one of my colleagues that we would never leave Arizona because (1) "Vernon is too old to leave," and (2) "No one could afford seven people." In retrospect, these glib uninformed statements were symptomatic, I fear, of the basic problem with a tiny handful of key people at the university who were mind-blind, or threat-

ened by change and the accomplishments of others, or were simply ignorant of the circumstances. For us, it was a lesson in how far out of joint things can get once the delicate flower of trust is crushed and people start making up facts.

There are a great many fine people on the faculty of the University of Arizona, where—in spite of a continuing and much publicized "brain drain"—there are still many very distinguished scholars, scientists, and Regents' Professors. I wish them only the best and I hope that someday I will be given an opportunity to help their cause. The university administration simplistically blamed the substantial brain drain on the state government's refusal to provide the university with adequate funding. That cause, even if true for others who left, was not the reason for our departure. In spite of severe state funding problems, we had been able to raise our own student and other support funds, at no cost to the taxpayer, through ESL's "Revenue Account" and the International Foundation for Research in Experimental Economics (IFREE). The latter we founded in 1997. This success was not without big-time sour grapes.

The sadness of it is not lost in the following upbeat e-mail sent to me on October 10, 2002, by Ed Zajac, a longtime friend and former economics department head at the U of A.

Vernon,

CONGRATULATIONS!!!!

When I was department head, by every second Tuesday in October I would have prepared a little speech to give reporters when they called me for reactions to your getting the Nobel Prize. My speech revolved around the sound bite, "the first 'made in Arizona' Nobel."

Alas, I never had a chance to give my speech, and the U of A and the State of Arizona have squandered the opportunities that a "made in Arizona" Nobel would have brought them. But you've gotten a chance to give yours! And you're at a place that is seizing opportunities, not squandering them.

You abundantly deserve the Nobel, even though it's years and a day too late [the economics award date was moved to Wednesday]. *It must be gratifying to finally have recognized the many years of dedicated, hard work, often in the face of enormous obstacles.*

Keep up the great work. What you and your group are doing is fascinating and path breaking.

Having one Nobel should just spur you on to a second!!!

Ed

I wrote an article for the *Wall Street Journal* that embodied my learning from the many years my colleagues and I engaged in direct contract research with public and private entities. I reproduced in full:

Contract Killing

By VERNON L. SMITH

Updated Aug. 19, 2004

A controversial and divisive issue in U.S. higher education is the extent to which the public university should seek funding from private businesses for scientific research and its applications. Yet universities follow very lenient policies in permitting faculty to consult for private businesses. Consulting is actively encouraged on the grounds that it greatly enhances faculty competence and experience in real-world problem solving. An unadvertised advantage is that it is a source of faculty compensation, helping to stem any "brain drain" leakage of faculty to other universities, at no cost to the taxpayer.

The standard rule in American universities is that faculty on academic appointments may consult an average of one academic day per week, weekends, holidays, winter and spring breaks, and for three summer months. Moreover, there are universities whose policies explicitly allow for administrative approval of the use of facilities such as laboratories in consulting research, provided that there is suitable compensation for the use of such facilities by the outside interest.

How can existing consulting policies and practices be restructured into an instrument for the funding of university research? How can it be made compatible with, and used as an instrument for enhancing university education, research, and community outreach services? How can the independence and integrity of the university be assured?

The answer is the consulting research contract, struck between faculty research teams and external business entities, and not restricted to proprietary uses of the research results. Such contracts would allow for the payment of consulting fees negotiated between the faculty and the funding entity within the above consulting-time guidelines and the payment of commercial rental rates for the use of laboratory and other research facilities. Rental earnings would be paid into a university-established "revenue account" available to support students, basic research, laboratory development, postdoctoral visitors and educational programs initiated by the research team. For the incentives to be right, the research team that earns the revenue must be the primary entity that has access to the fund for the development of their science.

Such an account was set up for my Economic Science Laboratory faculty team in the 1980s when we were at the University of Arizona. In addition to experimental laboratory rental earnings, we volunteered to pay 10% of all our consulting billings into this account. This seemed reasonable to us as each of us was making use of the knowledge we had acquired jointly as a teaching and research team; also the university, through its Arizona Technology Development Corporation, owned a 10% share of the consulting company we had formed under an aggressive initiative of the university. The university then collected overhead at their normal rate as the funds from the account were expended.

But how can it be assured that such activity is fully compatible with the university's independent education and research mission? Very simply, all such consulting research results must be available for publication by the faculty, the data available to graduate students for dissertation research, and for conference and other public presentations and uses. This means that the research data and report cannot be proprietary. I do not believe that proprietary research can be conducted without compromising the university's independent scientific mission; faculty should either say "no" or do it as independent consultants. This is where conflicts of interest arise. Faculty should also say "no" to routine and unchallenging opportunities to earn consulting fees that involve no significant new learning. It is essential that the consulting research be devoted to creating new fundamental or applied science understanding and know-how.

I and my colleagues design markets for resource management; auctions and other electronic trading; and management systems for emission permits, logistics services, spectrum licenses, asset portfolios, energy in transmission networks, water supply networks, supply chain development, etc. We use cash-motivated human subjects to test, modify, redesign and refine these systems until they are ready for further development in the field. Every such market-design problem has its own unique institutional and technological constraints and features, and the research needs lots of industry-specific input from engineers and practitioners. To try to do such research without that input creates a much less practical product. It's all about acquiring know-how through practice, as well as developing basic principles governing incentives in decentralized interactive systems.

The resulting research exercises directly impact our teaching, at no cost to the university or to the taxpayers. In one project we were using the new network electricity trading "research" software we had developed for a client in our MBA classes before the final research report was delivered to the client! New research software quickly becomes part of graduate, undergraduate, and even high-school outreach teaching programs. Faculty learning advances in tandem with student learning as together both learn by doing. Long ago, I discovered that if you are not learning, you are not teaching.

Those who argue that the university should be a haven for intellectual purity uncontaminated by the world of practice are promoting a diminished and restricted climate for education. The vast majority of students have to find their way in the world of commerce, industry or government. Their most important lesson is to learn how to learn, and to be creatively useful in that world. Those students who remain in academic careers also benefit uniquely from these practical learning exercises.

Universities are well advised by Ben Franklin: "Tell me and I forget. Teach me and I remember. Involve me and I learn." As I see it, the three products of the university – education, research and public service – should be seamlessly connected by a common vision for creating and dispersing new knowledge.

Mr. Smith, a professor at George Mason and the Visiting Rasmuson Chair at the University of Alaska, Anchorage, is a 2002 Nobel laureate in economics.

At GMU, we designed and built new experimental laboratories located on the Fairfax Campus, an essential location for access to the mass of GMU student subjects. Originally, GMU had recruited us with the expectation that we would locate centrally on the Fairfax campus. Subsequently, GMU received a $3 million grant from the Koch Foundation to support our laboratory and experimental budget. This Koch had funded the Mercatus Center in Arlington, so this explains the switch that moved us, whereby we ended up moving from Arizona to the Arlington Campus. Our separation from the main laboratories in Fairfax was inconvenient, but residing near Washington DC had compensating easy-access advantages. Moreover, the graduate students much preferred Arlington to the "boring" Fairfax campus!

In retrospect, our experience at GMU takes on new significance in light of the recent anger-driven charges that the "Koch's" have attached "strings" to their GMU gifts and exercised undue influence on programs and hiring at GMU.[2] This new development is interesting because in 2001 we had no strings (except perhaps where we located), but that involved us in a different problem than the one now receiving all the noisy attention. The Koch money came into an account for the support of our Interdisciplinary Center for Economic Science (ICES). We encountered issues with the GMU Provost's office charging expenditures to the ICES account unrelated to ICES programs. We were able to fend off and correct these internal irregularities, but it gives you a hint of the problems encountered by donors if they do not contract to keep the recipient universities from misappropriating funds for purposes contrary to donor intent (a form of unintended fraud). (I say "unintended" because universities feel confident that they know best how to spend other people's money.) Moreover, contracts are without meaning unless monitored. Since 2001, the Koch Foundation seems to have learned from their past mistakes. From my reading of the emotionally charged media reports—I have no inside information—Koch learning might have led to an "overreach," responsibility for which resides with GMU (or any other university recipient) to protect their interest in the integrity of due process and academic freedom in faculty hiring and programs. Good contracts offer gains from exchange, but have to balance the various parties' interests where there are conflicts (Figs. 15.1, 15.2, 15.3, 15.4, 15.5, 15.6, 15.7, 15.8, and 15.9).

[2]The contacts in question were effective from 2003 to 2011 and are no longer in force. They gave donors the right to recommend faculty for hiring; final authority was with GMU. This is the substance of the claim that there are "strings attached" and "undue influence." https://www.nytimes.com/2018/05/01/us/koch-george-mason-university.html.

Fig. 15.1 Vernon and Paul Martin visit Lehner Ranch Mammoth Kill Site 1975

Fig. 15.2 In Situ Mammoth Bone at Ed Lehner Used Elephant Lot 1975

Fig. 15.3 2nd Generation PLATO Lab

Fig. 15.4 Discovery Arizona style

Fig. 15.5 Richard Kiser, Vernon, Praveen Kujal, Stephen, Spain Workshop

Fig. 15.6 ESA Westward Look Find Yourself

Fig. 15.7 Vernon and Jo Kwong, Liberty Fund, Lone Mtn

Fig. 15.8 Lone Mtn Ranch C&W, Find Yourself

Fig. 15.9 Candace, Cate, Vernon, Charlie, Maverick C&W

16

My Friends Were Finally Right

When this book is mould,
And a book of many
Waiting to be sold
For a casual penny,
In a little open case,
In a street unclean and cluttered,
Where a heavy mud is splattered
From the passing drays,

Stranger, pause and look;
From the dust of ages
Lift this book,
Turn the tattered pages,
Read me, do not let me die!
Search the fading letters, finding
Steadfast in the broken binding
All that once was I!

—Edna St. Vincent Millay,
"The Poet and His Book"

On October 9, 2002, I walked to my office in Arlington, arriving early, dressed as usual in jeans and a Western snap-button shirt. I immediately went to my computer to resume work on a paper I had been writing for several months, entitled "Constructivist and Ecological Rationality in Economics." Shortly before 9:30 a.m., the phone rang, and the caller identified himself as Torsten Persson, chairman of the Nobel Prize Committee for Economics.

V. L. Smith, *A Life of Experimental Economics, Volume II*,
https://doi.org/10.1007/978-3-319-98425-4_16

When I hung up, I knew who would be publishing the paper I had been working on: the Nobel Foundation.

All prize recipients are asked to write a paper related to the scientific contributions recognized by the prize committee. The paper I had been writing would be just right for this purpose, although that prospect had not been any part of my original expectations in writing it. I continued that project with the result that the ninety-odd-page typescript I submitted to the Economics Prize Committee became a 600-plus-page manuscript for a book published in 2008 by Cambridge University Press under the title *Rationality in Economics: Constructivist and Ecological Forms*. Laureates are also asked to write an autobiography, and I submitted a sixteen-page manuscript that has been expanded into this two-volume work.

The third assignment for me was to prepare a toast to be delivered at the Nobel Banquet on the evening of December 10, 2002, following the awards ceremony. A recipient in each of the prize areas—physics, chemistry, physiology and medicine, literature, and economics—is asked to prepare a toast. In October, soon after the announcement, I received the following e-mail message from my friend and co-laureate in economics, Danny Kahneman, to whom I responded affirmatively: "I am of course counting on you to do the toast, which some people find intimidating, but you won't. Let me know. See you in Stockholm!"

I guess Danny was right. I provide the text of that toast later in this chapter when I discuss the celebrative events in Stockholm, beginning with Candace and me arriving on December 8 and ending with our departure on December 16.

I had met Torsten Persson the previous December at the 2001 Nobel Centennial Conference on "Behavioral and Experimental Economics," one of many such conferences embracing all the various Nobel Prize areas convened in celebration of the 100 years since Alfred Nobel had created his foundation.

A year earlier, the decision by the committee to hold a celebrative conference on this topic had generated much speculation that in 2002 or 2003 the committee might indeed recognize the field of experimental economics and/or the field of judgment and decision in cognitive psychology, also sometimes called economic psychology, and now often referred to as behavioral economics. Earlier conferences held by the committee in various fields of economics, with only one exception (five out of six, as I recall), had led within two to three years thereafter to the recognition of one or more scholars in the field of the conference.

Although most people believe that the prize is intended to honor the person, published discussions have explained quite carefully that in fact the intention is to recognize an influential *contribution* to economics by naming the individual or individuals whom the committee feels are most directly identified with initiating or developing the contribution. This often leads to controversial outcomes because any new contribution must necessarily involve many people precisely because it is influential. Since the awards have a tendency to include early contributors, subsequent and very deserving contributors may not be included in the citation. This applies in my case, as there are several truly distinguished experimentalists who entered the scene later than I entered, and who might otherwise have been considered. Many potential candidates in any of the fields simply do not live long enough—no awards are given posthumously.

The genesis of experimental economics is relatively easy to articulate because there were so few originators and so few involved in its slow but (as it turned out) sure development, beginning approximately in the middle of the twentieth century.

There were two seminal contributors, both long deceased, who influenced the development of experimental economics: Edward Chamberlin and Sidney Siegel (whose influence and contributions I have discussed in Chapter 11). I did my first market experiment in January 1956 and continued throughout the 1950s, but I did not publish my first experimental paper until 1962. Simultaneously, Reinhard Selten in Germany was conducting the first oligopoly (competition among a few sellers) experiments that he would publish in 1959. Siegel, in conjunction with Larry Fouraker, did their first bargaining experiments in the 1950s and published them in 1960. Siegel, Fouraker, Shubik, and Harnett, working together at Pennsylvania State University (Shubik was at Yale), subsequently conducted many bargaining and oligopoly experiments that would be published in 1963 after Sidney died. Larry Fouraker finished their 1963 book, which had already appeared in the form of three thick Pennsylvania State working papers. My original copies of these treasures did not survive one of my many moves, but my longtime friend Martin Shubik, cast in concrete at Yale and an inveterate packrat who is careful to save everything, tells me he still has all of these wonderful papers. Others in this early period also made contributions including Austin Hoggatt, Lester Lave, and Roger Sherman. In 1963, James Friedman, under the influence of Siegel, Fouraker, and Shubik, published his thesis on oligopoly competition. Shortly thereafter, Charles Plott would be much influenced by the experiments that I, and various graduate

students, were doing at Purdue throughout the 1960s. After going to Caltech, Charlie became actively involved in experimental work, applying the induced value methodology to political economy, particularly to voting mechanisms and agenda processes in joint work with Mo Fiorina and Mike Levine, and to the study of mechanisms for the provision of public goods with Roger Noll and John Ferejohn. It would be my work at the beginning, plus that of Charles Plott subsequently, however, that would be most commonly perceived as having an early and sustained impact in the development of experimental economics.

At the University of Arizona, I organized a conference on experimental economics in 1977, and it would be repeated in 1978 with funding by the NSF. In 1986, the Arizona team of experimentalists sponsored the inaugural meeting of the Economic Science Association (ESA) at the Westward Look in Tucson. Membership has grown steadily in ESA, and today, it is international in scope and holds every third meeting in Europe. (The International ESA 2016 meeting was held in Jerusalem, July 9–11, where I gave one of the keynote addresses.) Some years ago, I lost contact as a regular participant because of an intense and very demanding research and travel schedule. Being unwilling to yield research and teaching time to new travel demands, I yielded regular attendance at various conferences.

I know only a few anecdotes about the promotion of behavioral economics as a subfield related to experimental economics and cognitive psychology; I will leave it to others to articulate that history. The form of cognitive psychology that emerged recently and became identified with the 2002 award is an outgrowth of the development over three or more decades of the judgment and decision-making research. The recognition of this research by psychologists, however, as well as experimental economists, was already in the cards, long in the making, and was not, I think, related to the recent emergence of the sub-discipline that has been labeled "behavioral economics." Just as Herb Simon, who was not an economist, was recognized, similarly I believe the work of the cognitive psychologists likely was due to be recognized. Experimental economists and cognitive psychologists had been studying behavior since mid-century, and consequently, "behavioral economics" was a confounding label in search of a more precise substantive definition. It is unfortunate that it has come to be represented as a collection of results that are contrary to the standard economic models. Incredibly, some have naively defined behavioral economics as the search for results contrary to standard models. I see cognitive psychology qua behavioral economics, at least in principle, as part of experimental economics; its enduring successes

represent potential for a reformulated general theory of decision in economics, after having originated as consciously simplified exercises in decision analysis.

Many of the behavioral findings in the study of two-person interactions by experimentalists challenge the standard economic models identified with neo-classical marginal analysis stemming from the 1870s (Jevons, Walras, and Menger). Recent scholarship, however, has shown that some of these findings are consistent with and were originally developed in the classical contributions of David Hume, and particularly by Adam Smith in his first book, *The Theory of Moral Sentiments* (1759). Smith's first work was essentially about modeling human sociality, in particular what today we identify with personal socio-economic exchange. Hence, experimental laboratory methods have redirected scholarly attention to its eighteenth-century roots in the Scottish Enlightenment, which also provided the classical foundation for economics in Adam Smith's better-known and more influential second book, *The Wealth of Nations* (1776). The latter focused on what we would today call impersonal exchange through markets, which enables specialization and explains the source of all economic betterment through wealth creation. For an examination of these issues, see my article "Adam Smith: From Propriety and Sentiments to Property and Wealth," *Forum for Social Economics*, Volume 42, Issue 4, 2013; also, with Bart Wilson, "Fair and Impartial Spectators in Experimental Economic Behavior," *Review of Behavioral Economics*, 2014, 1, pp. 1–26.

The following persons attended the 2001 Alfred Nobel Foundation Centenary Conference in Stockholm, each invited to present a paper or be discussants (in order of the presentations); about fifteen other attendees formed the small audience:

- Daniel Kahneman (psychology; Nobel, 2002);
  Discussant, James Mirrlees (economic theory; Nobel, 1996).
- Howard Rachlin (psychology; animal behavior);
  Discussant, George Akerlof (economic theory; Nobel, 2001).
- Vernon Smith (experimental economics; Noble, 2002);
  Discussant, Reinhard Selten (experimental economics and game theory; Nobel 1994).
- Charles Plott (experimental political economy and economics);
  Discussant, Jean-Jacques Laffont (economic theory).
- Alvin Roth (experimental economics and game theory; Noble 2012);
  Discussant, Thomas Palfrey (political economy, theory, and experiment).

- Paul Slovic (psychology);
  Discussant, Sendhil Mullainathan (behavioral economics).
- Richard Thaler (behavioral economics; Nobel 2017);
  Discussant, Robert Shiller (economics; Nobel 2013).
- George Loewenstein (psychology);
  Discussant, Dan Gilbert (psychology).
- Matt Rabin (economic theory),
  Discussant, Ariel Rubinstein (economic theory).
- David Laibson (behavioral economics);
  Discussant, Roland Bénabou (economics).
- Colin Camerer (experimental and behavioral economics);
  Discussant, Amnon Rapoport (psychology).
- Ernst Fehr (experimental and behavioral economics);
  Discussant, Charles Manski (economic theory).

I will not comment in detail on the content and emphasis of this program. Three of the presentations directly concerned the study and performance of markets: my talk, Charlie Plott's, and Al Roth's. The rest were on utility, social preferences, and individual choice or decision and were closely associated with psychology and economics. After I saw the preliminary program, I abandoned the original title I had proposed on personal exchange (Trust Games) and substituted a title on electricity market design. I thought that there was excessive emphasis on individual decision and two-person games of strategy, and not enough on the market issues that had propelled most of the early development of experimental economics. The discussion that ensued following each presentation provided me with entirely adequate opportunities for drawing on some of the results from trust games that I would have covered in my original title.

I believe the subsequent 2002 award to Daniel Kahneman was assured, independently of the emergence of behavioral economics, in recognition of the contributions of cognitive psychology; the substance of that contribution was part of a record that had long predated behavioral economics.

I have correspondence with Mie Augier, an associate of Herb Simon who was also his archivist and biographer, showing that Herb had nominated Kahneman, Tversky, me, and others, and was impatient with his ineffectiveness in having his nominations recognized. From an e-mail to me dated November 06, 2002:

…I had a few chances to talk with him [Herb] about this; and he gave me copies of his correspondence with Assar Lindbeck and his nominations in the past.

As with most other things, he had (very) strong views about Nobel prizes and economics; and he was often frustrated that his nominations didn't succeed. For instance, in a letter to Bertil Naeslund he wrote (about his previous correspondence with Lindbeck: "Over the years, I have proposed such names as Kahneman and Tversky,…Vernon Smith,…and others, with no indication that such nominations have obtained very much support".

Further, in his specific nominations of you, he wrote that you have "developed and applied a novel and powerful set of techniques for studying human behavior in economic markets, and subsequently for the more general study of industrial organization and decision making. …the importance of Smith's experimental methods and empirical findings are now widely recognized in economics, and experimental economics is practiced by a rapidly growing number of economists." (He then continues to talk about specifics about your work). I shall be happy to send you a copy of his letters nominating you, if you are interested. He would have been very pleased now!

(I do not recall if these letters were sent to me; if so they are likely in archives at Duke University. Herb's original letters would be in the Herbert Simon papers at Carnegie Mellon University.)

That the award to Danny was assured is also suggested strongly by the fact that when the Nobel was awarded to the three game theorists (John Harsanyi, John Nash, and Reinhard Selten) in 1994, the reported backup candidate—in the event that the first choice of the committee was rejected by the Academy—was Amos Tversky. For decades, Amos was part of the Kahneman–Tversky team, since deceased, which demonstrates the necessity of living a long time, as well as making Nobel-worthy contributions. (See Sylvia Nasar, *A Beautiful Mind*, for a discussion of Tversky's 1994 candidacy.)

Bill Vickrey (1914–1996) died only days after the announcement of his award in 1996, and the deserving Sid Siegel died in 1963 at age forty-five, fifteen years before experimental economics was a blip on the committee's radar screen. The blip known to me occurred in 1978, when I recall first receiving a request from Stockholm to make a nomination. The invitation implied that at least one person had nominated me. I responded by nominating Bill Vickrey in 1978, eighteen years before he received the award, which gives you an idea of how long the gestation period is once you are on the committee's agenda. It was in the early 1980s that I first began hearing rumors that I was alleged to be "in the running" for a Nobel Prize. Believe me, I was not holding my breath in anticipation. For me, it was a long shot, and it was precisely that for twenty-five years. Here is the citation I wrote for William S. Vickrey, when he was elected a distinguished fellow, American Economic Association, 1978:

"Many of us have had the experience of thinking we were the first to show the neutrality of a particular tax scheme, to prove the incentive character-

istics of a particular bidding institution, to deduce the redistributive implications of the expected utility hypothesis, to invent a demand revealing process, and so on, only to find that William S. Vickrey had done it earlier—sometimes much earlier—and whereas our 'original contribution' may have contained a minor or even a substantive error, Vickrey had done it correctly. Some great scholars receive recognition from the beginning, but, inscrutably, with others it takes a little longer. His numerous works, appearing in all the leading journals in economics, law, operations research, finance, and taxation, contain many seminal contributions, and many more that would have been seminal but for the fact that the profession was not yet ready for his ideas. Thus, his 'Counterspeculation, Auctions, and Competitive Sealed Tenders,' Journal of Finance, 1961, (1) invents a class of demand revealing processes for private goods, (2) develops with clarity the important concept of incentive compatibility, and (3) operationalizes these theoretical insights in the form of realizable auctioning institutions. Only later, after the profession had discovered solutions to the 'free-rider' problem, was it possible to adequately appreciate Vickrey's astonishing precursory insights. We are proud to recognize the creative, inspirational, and persistently operational character of William S. Vickrey's contributions to economic theory and economic policy."

It would have been inappropriate for me (or any other principal) to serve on the 2001 conference planning committee because the process is expected to avoid such self-serving actions. This, of course, did not rule out soliciting suggestions from all the principals as to who should be invited, which the conference planning committee in fact did. Camerer, a member of the conference committee, requested my recommendations, which included Stephen Rassenti, Kevin McCabe, and Charlie Holt; that obviously was shot down, although Holt was invited he was part of the small audience of non-participants, there to view what was referred to as "a beauty contest."

If I had been proposing a complete program, it would have included roughly half experimental economists and half psychologists and would have consisted of the earlier senior contributors to each field, with the younger contributors serving as discussants or audience participants. Why half psychologists? Only in part would I have done this in recognition of the legacy of Daniel Kahneman and Amos Tversky, as my list would have included others with a legacy in their own right: Ward Edwards, along with Paul Slovic and Sarah Lichtenstein, as part of the senior list of Kahneman and Tversky contemporaries. Both Amos Tversky and Paul Slovic had been students of Ward Edwards, so his inclusion would hardly have been out of synchrony with the program as I would have envisioned it.

Also, I would have asked Selten to give a paper. I felt strongly, and still feel, that Selten deserved consideration along with me as an originator. I have never felt that his Nobel Prize in game theory should eliminate him as a con-

tender in experimental economics. His early work paralleled mine, and he even published three years earlier than I did, if that counts for anything. He was unquestionably a father of experimental economics who was still living. It is conceivable that we are yet to see an award in experimental game theory that could include two or three from a group consisting of Selten (5 October 1930–23 August 2016), Werner Guth, Al Roth, and Ken Binmore (Al Roth was recognized in 2012). Another prospect: an award in experimental political economy that would include Plott and others as contenders in this emergent and highly successful offshoot from experimental economics.

I would have included my longtime friend Richard Thaler as a presenter or discussant at the 2001 conference because he clearly deserved to be heard, in spite of his uncontested and preeminent capacity to piss off even his closest associates. He could be counted on to be seen "strutting around as if he had already received the award," to quote one of the conference participants. But the worst disappointment of the 2002 awards that emanated from the 2001 conference took the form of a participant who remarked: "the committee trashed my work and contribution." Only one who has high self-absorption in the expectation of "winning" will have equally high and unjustifiable disappointments.

As to the award itself, why were Selten and Plott not included? Selten, I suspect, was out because he already had been recognized in game theory. Plott, as some think, may have had too many skeletons in his closet, and it is hard to change a lifetime's accumulation. As a close Caltech colleague once said, "Charlie thinks everyone is just like he is." But I did not rule out the possibility that Charlie was still in line for a different but fitting future citation. You have to keep in mind that the Nobel is a serious attempt to recognize influential contributions, not personalities.

If I had been making the decision to recognize economic psychology and experimental economics, I would have done it in back-to-back years, with separate and distinct prizes for each area, because their respective contributions and methodologies are so completely different and, in some cases, in conflict with each other in ways that I believe are not methodologically reconcilable. Combining them into one award caused much confusion on distinctions of substance. For example, if you read the article in *The New York Times*, and the statement on Joe Stiglitz's Web site, each describing the 2002 Economics Award, you will find that neither writer had a clue as to what my contribution was. Similarly, there were several public comments that failed to notice Kahneman's work.

My candidates in psychology would have been Danny Kahneman, Paul Slovic, and Ward Edwards, who were prominent among the early founders of the tradition of economics and psychology.

In experimental economics, Reinhard Selten and I make sense. Numerous people have received more than one prize in the sciences, and that action would have been appropriate for Reinhard, as well as pathbreaking for the prize committee in economics. I would also have considered Martin Shubik as well as Charlie. Why consider Martin? Because, trust me, no one has an excellent understanding of economic institutions, their essential embedded role in the economic processes of society, and their relationship to game theory. But I have not been sanguine about the ability of economists to see the central role of institutional analysis to economics in Martin's lifetime. He and I are the same age. I have little doubt that institutional economics and its many other contributors will be recognized.

I favor a completely separate recognition for political economy, where my candidate would be Charlie Plott, whose contributions are wide ranging and who is strongly associated with the field of experimental political economy, as well as the distinguished political scientist Elinor (Lin) Ostrom, from the University of Indiana. Lin's contributions are truly exceptional in terms of both laboratory and field empirical understanding of how people all over the world have generated self-organizing institutions for governing the commons. In 2009, Lin Ostrom was recognized along with Oliver Williams for their contributions to the study of economic governance.

On October 12, 2009, the Internet brought the news that Elinor Ostrom had been recognized by the Nobel Foundation Economics Committee. As one who had supported such an action, I was quite exceptionally pleased. The Internet was alive with commentary, including the uncomprehending response, "Elinor Who?" out of the University of Chicago, but also elsewhere. That very day, I was meeting my class, taught jointly with Bart Wilson, and we would be discussing one of her most important works, *Governing the Commons.* I was asked to write an evaluation by Tunku Varadarajan who I had met in 2002–2003 when he was on the editorial board of the *Wall Street Journal*. He had since left and at the time was with Forbes. Concerning Ostrom, he wrote me at 8:30 AM:

Vernon: We'd love to get an evaluative piece from you, 700 words or so….to run soonest today on the Opinions section of Forbes.com…

Can do? Hope you're well!

Best, tunku

I responded: *I will do it late the (sic, this) morning after my class..* I relished this opportunity and you can read my comments on her achievements at Forbes. (*Governing The Commons*, Forbes Commentary, October 12, 2009.)[1]

[1]https://www.forbes.com/2009/10/12/elinor-ostrom-commons-nobel-economics-opinions-contributors-vernon-l-smith&refURL.

Later, in December 2009, I was at Chicago for a conference celebrating the 100th birthday of Ronald Coase with co-authors Tom Hazlett and Dave Porter, where we gave our paper on Coase.[2] There was talk of Lin Ostrom, and I heard yet again opinions that "The award should be given only to better known people." I found this prideful insularity simply embarrassing. Lin was well-known everywhere else!

In time, perhaps soon, I anticipate specific recognition of the theory and application of auctions. My candidates would include: Stephen Rassenti, who more than anyone else invented and developed the combinatorial auction as well as the concept and applications of smart computer-assisted markets; Bob Wilson, for his work in the theory of auctions, in electricity market design, and for his attempted assault on modeling the continuous double auction, and in the process demonstrating the inherent limitations of the current toolkit for game-theoretic analysis—itself a key insight; and perhaps Paul Milgrom, who is widely identified with auction theory, and the use of auctions to distribute spectrum by the FCC.

Also approaching quickly for recognition is the field of "neuroeconomics," as my associate Kevin McCabe has named it, applying the new tools of neuroscience to individual, interactive, and market decision making. Kevin was responsible for one of the pioneering breakthrough contributions in applying FMRI techniques to interactive trust games. He named the field and is surely a strong viable founding candidate. There are many other entrants that are likely to be considered, such as Colin Camerer, who was not a founder, but has been active, with much Internet speculation that he was likely to be recognized for one of his areas of research. His award of a MacArthur prize fellowship has added to the speculation, but Nobel's have been rare among the thousands of Macarthur prize recipients, where the emphasis has been on recipients too young to be in the Nobel running.

None of my remunerations above take account of the politics of science in the Swedish Academy, of which I am ignorant. Hence, some of the above suggestions may not be feasible, although there is evidence that there are those in the Academy who favor broadening the prize to include the social sciences more generally. I believe this has great merit and suggests another reason why Danny Kahneman was a leading candidate in 2002. Sylvia Nasar reports in *A Beautiful Mind* that many in the Academy much regret having

[2]T. W. Hazlett, D. Porter, and V. L. Smith. 2011. "Radio Spectrum and the Disruptive Clarity of Ronald Coase," *Journal of Law and Economics*, 54(4), Part 2.

allowed the Bank of Sweden to recognize a prize in Economics. I think this position also has some merit: Economists do indeed have pretensions to knowledge that outrun their delivery capacity. The prize rewards contributions narrowly influential within the profession, as in the sciences, but people expect economics to have a much broader significance for society than is clear in the award citations. They don't expect to understand the awards in physics and therefore cut the awardees lots of slack. We economists are not afforded that privilege.

The only award that I believe is likely to have erred significantly—because it excluded Gordon Tullock—went only to Jim Buchanan in 1986 "for his development of the contractual and constitutional basis for the theory of economic and political decision making." Jim's award was highly deserved, but, I think most economists recognize that it should have been made jointly to Buchanan and Tullock. It was not, for the reason that people close to the decision, including Jim, believed that there was a high probability that Gordon would embarrass the Nobel Foundation and the Economics Committee. I think this is an incorrect assessment. As one who has borne the brunt of Gordon's incessantly teasing put-downs, I also know him to be very capable and responsible in evaluating people in professional evaluation processes, including women, whom he often teased in direct interactions. In Stockholm, I am sure that he would have contained himself.

I learned of my own citation by phone on October 9, 2002. Torsten Persson's call from Stockholm noted that he was on a speaker phone with the entire economics committee present, and proceeded to read the citation to me over the phone. He then asked, "How do you feel?" "Relieved" I replied, "My friends have been predicting this for twenty years, and I am glad that they are finally right." The problem with the well-intended predictions of your friends is simply stated: Although the process is totally out of your control, you have this sense that you have failed your friends when year after year their predictions are wrong.

Candace and I flew into Stockholm on December 6. We deplaned to find a welcoming committee of two, standing in the departure tunnel, at the door to the airplane: the president of the Swedish Academy; Steffen, the attaché assigned to us for the next ten days. It was the first and last time that I have been met at the door of an airplane. We were ushered down the stairwell to the limousine assigned to us for the week and parked directly below the airplane. There we met Boe, our driver, who introduced himself in perfect English, saying that he would be constantly at our service. "I hope to be your friend for life," he said. These Swedes really are incredible.

We proceeded in the car to a VIP room with soft drinks and snacks; we were briefed on upcoming events, while an assistant took our passports,

handled customs, and retrieved our luggage. Before we left for the Grand Hotel with our driver and Steffen, the president handed me a pair of boxed patent leather shoes to go with the formal attire that we had already arranged to rent and pick up in Stockholm. This was a precaution, a gentle Swedish hint that my cowboy boots would not be in order in the official ceremonies. More on that later, but for the record I had no intention of violating the Nobel protocol—when in Stockholm, do proudly as the Swedes do!

Boe gave us a tour of the city—it's situated on thirty islands! He did the same for my family and Candace's family in attendance, and was a peerless friend for the week—and I hope for life.

My family included my four children—Deborah, Eric, Torrie, and Joshua—my grandson, Tal, my wife, Candace, and two ex-wives and dear friends, Joyce (Harkleroad) Smith and Carol Breckner. Candace's family included her sister and brother-in-law, Sandy and Tracy. Little did we realize that except for a few official events, we would have no significant time to spend with these close family members.

Stockholm during the Nobel celebration is like Hollywood during the Academy Awards. Television and newspapers blast away constantly, and you cannot go on the street without being recognized.

Moreover, you are expected to be knowledgeable on all matters ranging from the human to the Divine. Fortunately, however, I was forewarned and humbled by the wise counsel of Hayek. His banquet speech (see Nobel Web site) expressed his reservations about the very existence of the Nobel Prize in Economics:

> Hayek notes that just because a person made important contributions of distinction to economics did not certify him to make competent pronouncements on every problem of society. He tends so to be treated by the press until the honoree comes to believe it himself. It becomes almost a public duty to make such pronouncements. Hayek expressed doubt that this is a healthy social process and called on the Academy to require of your laureates something of a Hippocratic Oath to forbear from public statements that exceed their limits of competence. Hayek ends with an oft-quoted comment by Alfred Marshall (attributed to A. C. Pigou): "*Evil is with them when all men speak well of them.*"

That is good advice from Hayek and Marshall. I hope people will see my speculative writing (as in much of this autobiography) as those of an individual distinct from narrower topics in economics that I have studied and researched most intensively; even there, wide is the gate that leads to error.

Attendance by two ex-wives would turn out to provide grist for attempted sensation by the Stockholm tabloids, but it went nowhere. When asked by

one of the journalists why I had invited two ex-wives, I said, "Why would I not? We're friends, not enemies." I think this helped to scotch it, but more significantly, Steffen weighed in heavily, issuing a stern warning for them to keep their distance. He made it clear that they were in trouble if they tried to make anything untoward out of this innocent family business. The mature young Swede—in training for the diplomatic corps—could say what I could not.

Quite by accident, Candace got the underground story behind the black patent leather shoes. Early in the week, a cocktail reception was scheduled. Although Candace had commissioned two beautiful formal dresses that were packed and ready for service along with the Hong Kong tailor-made suits I had bought her the previous year, and assorted other dress wear, she had "nothing"—as she put it, and every husband has heard—that qualified as a cocktail dress. She received instructions at the hotel desk on local shopping and walked to an upscale shopping district nearby. She found the right store and an attendant and explained what she needed.

> "What is the event?" asked the attendant.
> "My husband and I are attending a Nobel cocktail reception," was the reply.
> "Why are you invited?"
> "My husband is one of the laureates."
> "Which one is he?"
> "He is Vernon Smith in economics."
> "Is he the one with the pony tail?"
> "Yes."
> "Tell me—everyone in Stockholm is wondering—will he wear his boots to the award ceremony?"

That citizen's query summarized what was most important about the great scientific, cultural, and intellectual event of the year at the Mecca of all such events! For the record, I did not wear my boots at the ceremony, or at the banquet that followed, because I wanted to honor the Swedish tradition and follow its long-practiced protocol of celebration. I left Boe, however, with instructions to have my boots in the limo and to deliver them to me immediately after the banquet to do service at the Grand Ball beginning at midnight. I did not wear the patent leather shoes—I wore conventional black dress shoes, as did many others, purchased earlier for the occasion at the Western Warehouse in Tucson—but the reason was very practical: I tried my honest best with the patent leather shoes, but they were so slippery that I was afraid to wear them. It would not serve my distinguished and respected

hosts if I were to fall onstage as I approached the King of Sweden; nor did I want to fall at the banquet, where I knew I would have to ascend to the podium to give my toast. You have to show the proper respect!

Here is the long written version of my toast, cut slightly in its oral presentation:

Your majesties, the royal academy, my fellow laureates, ladies and gentlemen. I rise to offer a toast. In this toast

I wish to celebrate:

- *The royal family, for their grace and charm in this magnificent affirmation of the dignity of human kind.*
- *Daniel Kahneman, for his ingenuity in the study and understanding of human decision and its associated cognitive processes demonstrating that the logic of choice and the ecology of choice can be divergent.*
- *The pioneering influence of Sidney Siegel, Amos Tversky, Martin Shubik, and Charles Plott on the intellectual movement that culminated in the economics award for 2002.*
- *Humanity's most significant emergent creation: markets.*
- *Mandeville, who said: "the worst of all the multitude did something for the common good."*
- *The ancient Judeo commandments: thou shalt not steal or covet the possessions of thy neighbor, which provide the property right foundations for markets, and warned that petty distributional jealousy must not be allowed to destroy them. Neither shalt thou commit murder, adultery or bear false witness, which provide the foundations for cohesive social exchange.*
- *David Hume, who declared the three laws of human nature: the right of possession, its transference by consent, and the performance of promises, and taught that the rules of morality are not the conclusions of reason.*
- *The unknown author who said that if goods do not cross borders soldiers will.*
- *F. A. Hayek, for teaching us, that an economist who is only an economist cannot be a good economist; that fruitful social science must be very largely a study of what is not; that reason, properly used, recognizes its own limitations; that civilization rests on the fact that we all benefit from knowledge that we do not possess as individuals; and who saw emergent institutions as super-individual structures within which individuals found great opportunities that could take account of more factual circumstances than individuals could perceive, and in consequence is in some respects superior to or "wiser" than human reason.*
- *Ben Franklin, who said, "tell me and I forget, teach me and I remember, involve me and I learn."*

- *And finally, Kahlil Gibran, from whom we learn the truth that "work is love made visible."*

—The Royal Banquet
Blue Hall
Stockholm, Sweden
December 10, 2002

If you view the TV coverage of the banquet, you will see that the royal family was very pleased with this toast, and Candace and I like satisfied customers. I heard through some of my academic contacts in Sweden, however, that some politicians and leaders were not thrilled by my explicit call in support of the classical liberal tradition of economic freedom and the rule of law.

The last quotation from Gibran—long one of my favorites in his *The Prophet*—inspired an essay by my son when he was an undergraduate in 2002. Here it is, with his permission:

The Letter
By Joshua Smith

One Thursday afternoon I was driving home from school after attending my last class for the day. As I drove, I contemplated the actions I was taking in my life. Since the beginning of the semester I had been determined to do the best work I could in each course, and most of my energy went towards accomplishing this goal. During the times when I could get away from school studies, I was focusing the rest of my energy on creating music with my band. My efforts in these two projects seemed to be paying off so far. I felt that I was turning in good work at school, and my band was progressing and writing good material. But as I reflected in that hot car, I did not feel contentment. In fact, I was swimming in an abyss of confusion and disillusionment. The long, closed off study hours and late nights spent rehearsing with the band had caught up with me. I was thoroughly exhausted, both physically and emotionally. I felt lost inside of myself, unsure of what it was all for. It seemed my higher purpose was disappearing inside of a banal routine. I could feel my outward energies becoming increasingly negative and cynical.

I pulled into my apartment complex and parked in the front. As I got out of the car, I grabbed my heavy book bag from the passenger seat. On the way up to my apartment I checked my mailbox, and saw there was a letter from Arlington, Virginia. I knew it was from my father, since he is the only person I know who lives in Arlington. As I ascended the stairs towards room 223, I wondered what the letter would be about. My practical mind was expecting it to be the standard, just-keeping-in-touch letter usually sent by relatives, probably saying how nice it was to see me last, asking when I would be visiting again, and asking how school was going.

As I entered my apartment, I threw my book bag onto the bed. Standing in the middle of the room, I tore open the envelope and pulled out the card that was inside. On the front of the card were the words "Book of Life". I opened the card, and quickly recognized my father's handwriting. After reading only the first few words, it became clear that this letter was of a completely different nature than I had expected. The letter went like this: "On work: Work is love made visible, and if you cannot work with love but only with distaste, it is better that you should leave your work and sit at the gate of the temple and take alms of those who work with joy. For if you bake bread with indifference, you bake a bitter bread that feeds but half man's hunger."

As I read these profound words, tears of truth swelled in my eyes and ran down my face. My heart, which had been cold and doubtful, was filled with a warm rush of joy. I was suddenly delivered from confusion, and made to see clearly once again. My father's message symbolically related to my own life, and reminded me of why I did what I did: Out of love. I love new ideas, and that is why I strive in school. I love music, and that is why I spend late hours creating it. All I ever wanted was to do work that I was truly passionate about, to wage war against indifference and apathy.

My creator had written me at just the right time, for I had turned away from the light, and needed to be shown the way back. He was speaking to me in just the way a father should, utilizing his advanced age and wealth of life experience to give me significant and honest advice. I had a wonderful sense that everything was going to be alright, as long as I continued to do what I love and follow my dreams.

I gave many interviews and lectures during our stay in Sweden. One was the traditional lecture given by economic laureates at Uppsala University, where my banquet toast probably helped to get me a heckler in the audience who surfaced during the Q&A period. The chairperson was very permissive of the heckler's interruption of my attempt to answer his questions, but the audience was not. They burst out in applause when my more polite responses failed and I firmly announced, "You are wrong!"

I have given many lectures since October 9, 2002; participated in panel discussions, radio, newspaper, and television interviews; visited Mexico, South and Latin America, Norse Country, Italy, Austria, China, Australia, Taiwan, Singapore, South Korea, Republic of Georgia, Ukraine, etc.; speaking on experimental economics; but more broadly on the world economy, globalization, trade, liberalization, and issues related to reducing world poverty, disease, and conflict in which I always try to show connections to what we have learned in the laboratory, from economic history and from economic prehistory about the wellsprings of wealth creation through specialization and exchange, whether the latter be personal or impersonal.

In the space I have here, I can say little to convey the richness of those experiences. Increasingly, I was asked to opine on current events and to express my particularly strong feelings about the sequence of wars that have embroiled America in the wake of the end of the Cold War. Like almost all the others, especially on foreign soil (the American Revolution and World War II being important exceptions, as I have indicted elsewhere), the war in Iraq was a tragedy—a tragedy for my country, our friends and allies in the world, and for me. I hope that these conflicts, and this pattern of tragedy, will pass in my lifetime, though unfortunately many everywhere seek to fix blame when there is more than enough to go around.

Speaking of the Iraqi War, below is a (very dated) piece I wrote for the *Wall Street Journal* that was published on December 22, 2003. Although it is my understanding that there was strong internal and quasi-official support by the Provisional Transitional Government for an Iraqi Fund limited to oil royalty payments, there had been no announcement of official public support when I wrote the article. Subsequently, documents were released showing that there had been a Department of Treasury internal memo urging the announcement of an Oil Trust Fund.[3]

The basic problem with the proposal is that no one knows how to create the preconditions in culture and tradition that might enable it to be realized, and in this sense was, and is, idealistic for Iraq. I do not regret the efforts I made in this direction.

The Iraqi People's Fund

By Vernon L. Smith

With the capture of Saddam Hussein, President Bush has a great symbolic victory against his critics. However, the unfinished Iraqi economic reconstruction presents the president with a historic opportunity to craft a new geopolitical-economic paradigm that could—and should—become a world model for the movement of assets from governments to citizens.

The last decades have seen a world-wide transfer of state-owned assets to private entities, most often as governments have found themselves unable to afford their varying brands of socialism. However, this transfer of assets has served largely to generate funds for governments—sales to retire government debt, fund political priorities, or as an alternative to raising taxes—creating a funding system easier for politicians but more difficult for the public it serves.

[3] http://library.rumsfeld.com/doclib/sp/2899/2003-11-15%20from%20John%20Snow%20re%20Announcement%20of%20an%20Oil%20Trust%20Fund%20for%20the%20Iraqi%20People.pdf.

For long-term success, the enormous task of nation rebuilding in Iraq requires attention to more than the creation of a political democracy. No matter how well-intentioned and democratic it might be, the next government will be tempted by corruption, violation of rights, and expanded political power if it owns and controls the great economic wealth potential of Iraq. This is the time, and Iraq is the place, to create an economic system embracing the revolutionary principle that public assets belong directly to the public—and can be managed to further individual benefit and free choice, without intermediate government ownership in the public name.

In Iraq, the rights in question are to the former government's producing properties, transportation, terminal facilities, waterways, land and subsurface rights. These assets should first be declared transferred to the account of the citizens, recognizing the birthright of each citizen to a personal, empowering property right in the land and assets of the country of their birth. All citizens should have an equal share in this fund and be issued the same number of share claims to the fund.

Over a period of several decades, all Iraqi assets should be auctioned to the highest bidders in an individual, national and international business competition so that each asset or bundle of complementary assets is transferred to the bidders who value them most for production, development or exploration. The auction could begin by selling existing producing oil properties, refineries, pipelines, and gathering, separating and terminal facilities over the next several years, then move to mineral, oil and gas exploration leases, and to land surface rights.

The proceeds would be deposited in a giant mutual fund for investment in index securities of the world's stock markets and monitored—but not managed—by the U.N. Investing in stock indexes would minimize the need for discretionary financial management, and the prospect of the next government exercising or re-establishing any central control over Iraqi assets. The Iraqi Fund should be a closed-end fund whose shares are tradable and listed on world stock exchanges. The proceeds of each new property auction would be deposited to the account for investment in index funds. Redemptions at market value would go to any Iraqi citizen who elects at any time to cash out any portion of his shares.

There is a very important precedent, in part, for this action: the Alaska Permanent Fund. The state of Alaska elected to put a portion of its vast Prudhoe Bay annual royalty revenue into a citizens' Permanent Fund for investment in securities. Each year a dividend from this fund is paid to every Alaskan citizen. This Fund was the first to recognize the full rights of citizens to share directly in the income from public assets.

This Fund, however, had three important shortcomings that should not be repeated in the proposed Iraqi Fund.

- *First, Alaska did not put all of Prudhoe Bay state revenue into the people's account. A portion of it went to the state government. When oil prices went up, the state succumbed to the temptation to repeal its income tax and spend its oil income as if there were no tomorrow. Consequently, today the Alaskan government has a budget crisis and a deficit gap, but the 600,000 Alaskan citizens still share equally in the dividends from their Fund, now worth $27 billion.*

It is better, because it disciplines government spending, for the political process to have to pass through the eye of the needle of voter scrutiny of tax and spending policies than to have free priority access to what should be the people's earnings on their assets.

• Second, the Alaska Fund is not a ready source of private investment and venture capital for its individual owners. This is because there are no tradable certificate shares in its mutual fund. This lack of liquidity denies citizens access to capital markets: An individual citizen cannot sell some portion of his shares for investment in a private start-up business, or borrow against the shares for such investment. It serves as a serious impediment to individuals desiring to finance private economic developments and new ventures.

• Third, the Alaska Fund was a one-time-only event: Only the Prudhoe Bay revenues were committed to the Fund. There was no permanent recognition of citizen rights to have the proceeds of all future state-asset sales placed in the fund.

The Iraqi People's Fund would consist of tradable shares; all public property would be held, then sold, for the account of the fund; and the new government would be required to obtain its revenue from taxes levied on the citizens who are willing to elect them and finance their spending programs. The government could not raid the fund to finance its operations. All this could be made explicit in the Iraqi constitution.

There should be room in the proposal for a temporary transition mechanism. For example, sales of citizen shares in the fund might be limited at first, but gradually lifted as citizen registrations and claims were settled, and the auction/sales mechanisms became established. Also, an initial budget set-aside for financing the new government might be in order, but this budget should decline on a fixed planning schedule at 15-20 percent per year as the new government gets its tax and spending program together.

This action would launch the new Iraqi state as one based on individual human rights and the rule of law, and give it rock-hard credibility by giving every citizen a stake in that new regime. The objective is to undermine any citizen sense of disenfranchisement in the country's wealth, or economic and political future, and to galvanize citizen support for a democratic regime. Now is the time to act, before post-war business-as-usual creates de facto *foreign and domestic spoils-of-war property right claims, leaving out a citizenry brutalized by a totalitarian regime and in sore need of empowerment.*

Despite its simplicity, this proposal is by no means a modest venture—demonstrated by unsuccessful half-efforts at privatizing public assets around the world. However, the political devil in focusing on too many competing details is to risk missing the overarching principle of restoring individual ownership in Iraq. Like the Marshall Plan, the details must be subjugated to the principle. The details, if wrong, can later be repaired. The principle, once corrupted, can never be reinstated. The Iraqi Citizens' Fund requires that the principle of individual ownership be primary.

A central issue in Iraq—as well as in the United States and other countries—remains whether the people control government through voting and taxes or the government controls the people though a monopoly on natural resources. To break that monopoly, the Bush administration and the Iraqi Governing Counsel have a momentous opportunity to instate a new paradigm. Only an owner-people can ensure a prosperous Iraqi state.

My experience with attempts to fashion economic policy at the presidential level in the USA is very limited, so it is hard to make comparisons, but the Bush administration took seriously the idea of trying to implement some form of Iraqi Fund. Three or four people at Treasury actually worked on the proposal for a while, prior to the transition of government, to see if a pathway could be identified. The Bush administration asked me to come across the river to meet with Treasury. That initiative developed from an earlier connection with Jim Carter, who re-contacted me after I had moved to George Mason University. Seems like the last time I had gotten Greetings from the White House, it was to report for the World War II draft.

In what follows, I have inserted clarifications and current commentary in the parentheses:

From: James_E._Carter@opd.eop.gov
Sent: Tuesday, March 19, 2002 4:03 PM
Subject: Greetings from the White House

My name is Jim Carter. I am an associate director of the National Economic Council at the White House. During the last presidential campaign, nearly 600 economists jointly endorsed George W. Bush's economic agenda. [I endorsed the agenda publically but not the Bush candidacy because I avoid endorsing specific candidates and am a registered Independent with all the implications that category represents]. As I recall, you were one of those economists. If I have contacted you in error, please let me know and I will correct my e-mail list.

I am contacting you today because the President wants you to know that he still values your input. To that end, please feel free to contact me with your thoughts and suggestions. Further, I hope you will not mind if I contact you from time to time to keep you apprised of new policy developments and to ask for your input.

I look forward to speaking with you in the months and years ahead.

Sincerely,
Jim Carter

James Carter
Associate Director
National Economic Council
The White House
From: Vernon Smith vsmith2@gmu.edu Mon 3/25/2002 9:09 AM

Jim:

Yes, I am one who supported the main features of the President's agenda. I am now at George Mason University, Professor of Economics and Law, a fellow at the Mercatus Center, and was instrumental in moving our research group here–the Interdisciplinary Center for Economic Science—from the University of Arizona. We do auction and market design using the experimental laboratory to test bed the electronic trading or auction systems we

develop. Our work includes the following, all related [or at the time I thought could be related] to the President's program:

1. Electricity. What went wrong in California, and how to avoid such in the future? The key here is in the deregulation of the retail sector, opening it up to entry by alternative energy suppliers, to introduce time of use pricing and demand-price responsivity, and to encourage supply and conservation technologies that bypass the grid and locate closer to the end use consumer. [California had moved to deregulate only wholesale markets, an action certain to lead to wholesale price volatility, which should be the point because, as in airline deregulation, the idea is to bring about an adaptation of consumer prices to the true costs; the issues here are treated more fully in Chapter 15.] Our experiments show that a modest improvement here, with only 16% of demand selectively and voluntarily interruptible, eliminates price spikes, lowers prices and effectively controls so-called generator market power without the need for intervention.

2. Water markets. We think the next crisis in CA and elsewhere will be in water shortages due to inevitable droughts. Needed is a flexible trading system that will allow water to be transferred from low value uses to high value uses when supplies are tight. [Yes, badly needed, but we were wildly off-the-mark optimistic in supposing that drought was near-term likely. Though as I write in 2015, southern California reservoir levels have returned to dangerously low record levels. Much experience has been gained in the development of water markets—for example in Australia—that can inform California's needs. Regrettably, the state made no use of Australian experience in developing electricity markets, though it was available and offered. On October 8, 2014, we met with Rod Smith at Stratwater and are working on creating a market for the exchange of water rights in a California community; if we get that going the grand idea is to expand it into other regions in steps; regrettably, nothing tangible followed].

3. We have a proposal to privatize the $5 billion estimated overrun on the space station. The idea would be to auction a percent of Space Station resources to commercial and industrial investors, and avoid cutting back on the projected capability of the Station. This would alleviate a current government program funding problem and set an important precedent for private participation in the future. [Space X and other private initiatives have rapidly accelerated the private sectors contribution].

4. We have just participated in an aviation conference with representatives from the FAA, DOT, airports, airlines, etc. *The focus is on airport congestion and the development of auction markets for airport runway slots that compress the schedules at airports like LaGuardia. We suggest an exchange of rights that would reduce the overall utilization to more secure levels. This is revenue neutral in that it is a withi-industry reallocation. New capacity, as with new airports or added runways, would be auctioned. Also, there is need for an exchange market for reallocating schedules from lower to higher priority (willingness-to-pay) when one or more airports are under weather or other temporary Ground Delay Programs. [I believe that any attempt to bring the government into managing an auction process directly would be disastrous; all you have to do is look at the government's auction program as it has developed and grown within the FCC. The whole process has been captured by the economist rent-seekers who advise both the FCC's auction design program while likewise*

consulting with the industry bidders who will participate in spectrum auctions. Rather, I would prefer a more marginal approach; simply allow the airlines under an airport ground delay program to trade runway slots for take-off and landing to re-prioritize the normal utilization; then see if that capability grows in use beyond the ground delay episodes.]

This will give you an idea of a few of the things that are within our technical expertise, and the range of our interests that bear directly on White House programs. There are seven us now at GMU that are involved in this sort of market/economics/engineering systems development for the electronic age.... Vernon...

This interchange led ultimately to several issues on which we made some sort of contribution; among them was the Iraqi People's Fund concept that I believe was largely torpedoed by the Iraqi politicians—hardly surprising that the American military victory did not mean that political resistance could be overcome in Iraq. As in Mexico and elsewhere around the world, the "curse of oil" resides in the eagerness with which the political process latches onto all that inflow of revenue, becomes hooked on it and is reluctant to give it up. As I write this in July 2014, the Iraqi government is under siege yet again from Iraqi splinter groups. We can intervene and win military battles, and still may, but what do we know about managing the cultural divides that survive into the aftermath?

The Iraqi People's Fund was only one of many spin-offs derived from my tenure as the first visiting Rasmuson Chair in Economics at the University of Alaska, Anchorage. The chair arrangement had been made prior to the October 10, 2002, announcement. Although there was concern that after the Nobel announcement I might change my mind about the Rasmuson Chair. I had no such thoughts, and it was in Stockholm that I signed the official papers. The key person, who conceived of a chair for implementing the Rasmuson grant, was Steve Jackstad, Professor of Economics at UAA. He recruited me to the chair that would launch me into a wonderful learning experience about Alaska Native issues and state resource challenges. Here is Steve's memory of working to make that appointment possible, sent in response to my request in 2013.

My e-mail sent to Steve Jackstadt, November 10, 2013, stated that

I will be revising my memoir, Discovery and will want to have a chapter devoted to my UAA years 2002-2013.

For that chapter, it will be very helpful for me that I have whatever notes you would have used, including the Chauncey Croft confirmation, had you

attended this anniversary event in Anchorage. For this you have plenty of time, as I am only now looking ahead to making that revision, as God wills.

Steve answered, November 11, 2013

Vernon,

…Chauncey Croft…was instrumental in moving things along in dramatic fashion. As you know, many meetings preceded your appointment as Rasmuson Chair and at one, held after your Nobel was announced, President Hamilton said "Get this guy now (Meaning you) and don't nickel and dime him!" "How long will it take? he asked. The top university administrator at the meeting, replied "Four weeks or so." "Make it four days," replied Hamilton. Two weeks later I encountered Croft, who was Chairman of the Board of Regents at the time, on the back stairs at the Sheraton Hotel, following some kind of lunch. "How's that Rasmuson Chair thing going?" he asked. When I replied that it still hadn't been acted upon, Croft put down his briefcase, took out his cell phone, and made a call. He held up a finger, signaling me to wait. He turned and spoke into the phone. When he put the phone down he said "It's all been taken care of," and sure enough two days later it was announced that you had been made the Rasmuson Chair. Chauncey laughed when I reminded him of the encounter and of his contribution to expediting things. There are lots of little stories surrounding your elevation to the Chair and Paul Johnson is a great source, having been the Chair of the Economics Department at the time. When you toss $5 million in front of a bunch of academics you never know what will happen!

Sorry I won't be at your 10th Anniversary, but naming you to the Chair was certainly one of the very best things that ever happened to UAA!.

See Figs. 16.1, 16.2, 16.3, 16.4, 16.5, 16.6, and 16.7.

Fig. 16.1 2002 American Nobelists in Oval Office

Fig. 16.2 Nobel Prize Art and Gold Medal

Fig. 16.3 Vernon and Candace at Nobel

Fig. 16.4 Vernon and Ethelmae Humphreys, his favorite Missourian

Fig. 16.5 Candace and Sandy

Fig. 16.6 Carol and Josh

Fig. 16.7 Mary Louise Rasmuson, Vernon, Anchorage

17

Wives, Daughters, and Sons

-Let no man write my epitaph. Please, God, let a woman write it.
—My wife, Candace.

After I went to work for Boeing in June 1943, I had little social involvement in high school other than to parachute in and attend two classes per week and for gym requirements. The war seemed to be changing everything. With gas rationing, we all carpooled and commuted to work, and that is how I met my first love, Mildred Anderson. She appeared one night about 11:30 p.m. in my third-shift car pool in December 1943.

Mildred was from Evansville, Minnesota, and was a Swede. We dated regularly for about eight months. She had migrated to Wichita to work for the war effort. About the time I left Boeing to go to Friends University, she left Wichita to return to her home in Minnesota. I was just turning seventeen when I met her. She was twenty. First love is very strong and very special; you don't readily forget the feelings. We corresponded regularly, continuing until after I went to Caltech in the fall of 1945. Then, one day she wrote to say she was getting married to a longtime local acquaintance, back from the war, whom she had mentioned earlier. I called to protest, but she was under family pressure and the course was set.

On occasion, we continued to correspond. I was apprised of the birth of her two children and their progress growing up. She was happy. When Joyce and I drove to Alaska in 1965, from West Lafayette, Indiana, we drove through Evansville. I stopped and went to the Evansville Bank for some money. I knew that she worked for the bank. She was there, dumbfounded to see me; we talked for only a few minutes, I got the traveler's check cashed,

V. L. Smith, *A Life of Experimental Economics, Volume II*,
https://doi.org/10.1007/978-3-319-98425-4_17

and I was off for the Yukon and Alaska. Years later, I was at a conference in Minneapolis. I called her in Evansville, and she drove up to see me. She was now separated—divorce was not in the cards for a dedicated and devoted Lutheran. We had a good visit, reminiscing about the war years, all the intervening change, about the way we were, and so on. We were content with how things had worked out and were happy to have once loved, but we remembered the heartaches.

All's well that ends well.

I was back working for Boeing as a summer employee in dad's Boeing machine shop in 1946. Soon, I met and dated Margot Tompkins (I was nineteen, and she was twenty-seven). That turned out to be a romantic dead end; I spent the summer of 1947 on the road and working in Saskatchewan. I hitchhiked more than 5000 miles that summer. I first hitched to Minnesota, where I met up with a group of young liberal-socialist types organized by some Wisconsin "progressive" group. We were assembling in Minnesota to go to Saskatchewan and learn about their new socialist government. In the aftermath of a devastating war, and the precedent-establishing first use of nuclear weapons by our own country, we had this youthful commitment to changing it all. The socialists were gaining political strength in Canada, and we believed their commitment to freedom and democracy meant that the communist corruptions and abandonment of these ideals could be avoided. These memories are still fresh enough for me to easily empathize with the appeal of Bernie Sanders to young adults in the 2016 primary race.

This venture is how I met E. Scott Maynes (rhymes with Keynes). Scott was studying economics in college and had two books with him: Lord Beveridge, *Full Employment in a Free Society*, one of the "progressive" manifestos of the day, and John Maynard Keynes, *The Theory of Employment, Interest and Money*. I read the first and tried to read the second in my spare time. Scott and I talked about them, and in retrospect, it is clear that this was the beginning of my career interest in economics. He had brought two friends, girls (in the language of the day) with him from Connecticut in "the Juggernaut," Scott's Model A Ford, with rumble seat. They took me in, stacked the bags on the car's roof, and we headed northwest for Regina, Saskatchewan. We went through Evansville, but we kept moving rather than stop. The three passengers shared gas and took turns riding in the rumble seat. We slept all night on a railroad loading platform in Portal, North Dakota, fully clothed in our sleeping bags, trying to keep warm. It was cold as a witch's teat. We cuddled, which interfered not a whit with our Platonic relationship, but did help a little to keep us warm. I thought of the farm

when I got up in the middle of the night needing to take a leak, and realized that at 19 I was aging—I could not make it through the night in spite of monumental incentives to stay in the sack. That problem was still with me in spades at seventy-seven, but so far I have successfully avoided both the plumbers and the medications. I tried the latter with unacceptable side effects and dropped it like a hot potato.

In Regina, we met the others coming up from our rendezvous in Minnesota and met *en masse* with the legislature and with Socialist Premier Douglas; the plan was to follow the path of New Zealand, and other democratic socialist governments, and to create the first "socialist paradise" in North America. I remember that we all slept on the floor of a local Regina bar—it was hard but warm. After a few days of reveling in this climate of political fantasy to be, we headed for Moose Jaw. We were all invited to a big dance that Saturday night. It was great fun, especially when one of the local ladies was crowned Miss Moose Jaw of 1947.

The itinerary for our visionary group from the USA was ending, and we were each on our own. The girls took off. Scott and I decided to participate in a land clearing project 30 miles North of Carrot River that would be used for summer wheat crops (see Chapter 4 above on the highway combine trains; we were beyond the northern limit of these trains). Canadian veterans received allocations of land for farming and built their own homes from the lumber yielded by the trees they cut. We would be employed in a jerry-rigged mill that produced finish-cut lumber for homes. That is where I learned to make open-pot coffee from a Norwegian immigrant to Canada, who was the foreman on the lumber mill. He made the coffee in an empty "#10" institutional packing can with a wire coat hanger threaded through a couple of opposing holes near the top of the can; just 2/3 water, about four handfuls of regular grind coffee and lowered into a wood heating stove. He withdrew it when it sports a two-inch head—never, never boil coffee!

The Norwegian foreman fed rough-cut 2 × 4s through the mill, and they came out the other side finish-cut one and half by three and a half inches like you buy at Home Depot. We worked a 10-hour shift and the guy could not even imagine a break. The three of us loaded the pieces onto a wood sled, pulled it by tractor over to an outdoor storage area where the lumber was unloaded and stacked for dry-curing in the sun and air. The three of us were barely able to keep up with his steady production. Fortunately, he had to stop occasionally to change his settings and cutters to accommodate 1 × 6s or whatever, and we caught our breath. He was built like Little Abner and a hard steel-driving man.

One day we shut down the mill to build a road bridge across a drainage ditch. For the spans of the ditch, big logs had to be cut from the Queen's mast-sized trees two feet in diameter. No power saws here; you cut the trees with an eight-foot, two-man crosscut saw. What this meant is you had this BIG Abner guy on the one end and us taking turns on the other. God, you cannot even imagine his energy—the guy just keeps pumping away—your chest muscles are burning and we are turn-taking! No wonder we clobbered the Nazis with this guy on our side. We slept well every night and loaded up in the early morning on bacon and eggs and home-baked bread with locally grown Canadian raspberry or strawberry jam.

It was a great summer. Scott dropped me off somewhere back south as he headed east. I hitchhiked home and was soon off for the new academic year at Caltech. I hitched to Denver, visited with Margot—still a dead end—then hitched down through Pueblo to Amarillo, ate at a restaurant that advertised "Tender Coffee and Rich Steaks," slept in a pasture under the horizon-to-horizon Texas stars, and ended up taking the bus to Pasadena.

In the summer of 1948, I was back working for Boeing, this time in the engineering department. That is how I met Jane Hall. She was a civil engineer out of the Missouri School of Mines and was working full time at Boeing (I was twenty, and she was twenty-eight; for some reason, I was taking up with older women—I recommend it). She moved to Southern California just before I graduated from Caltech in 1949, so we were back together for a short time before I returned to Wichita, then went to KU in Lawrence and continued in economics. We stayed in touch after I was married to Joyce. When we moved to Cambridge, Jane was there studying architecture. When Torrie was born she brought a gift, one of those mobiles to attach to Torrie's crib. Torrie loved to shake the crib and laugh at all the birds jiggling on the threads that formed the mobile. Jane moved back to St. Louis, her original home. I used to look her up whenever I was in St. Louis on business. The last time I saw her was probably forty-odd years ago. She was married to an older retired gent. I went to their home to meet him. She and they were doing OK; not long thereafter, he joined the ranks of the deceased. Some years later, I called St. Louis information: There was no longer a listing for J. C. Hall. Living alone, she avoided advertising herself. So Jane has disappeared after six decades—longtime good friends just come and go.

But the first real love of my life was Joyce. As I said earlier, I met her in John Ise's class—Economic Systems (meaning capitalism, socialism, etc.). She sat in the row ahead of me and had beautiful, long, black hair. She lived in the women's co-op, Henley House, that gave us something in common besides taking the same class. We hit it off and started dating. Joyce's

mother was an ex-schoolteacher from Belle Plain, and her father was a telegrapher on the famous Rock Island Road (Rock Island and Pacific, whose Pacific ambition was never realized—as I have indicated, that's the railroad business), the railroad that also employed my great aunt's husband, Dub McCracken, as an engineer.

Joyce's parents lived in Whitewater, a small town on the Rock Island Railroad not far from Wichita. When we went home for family visits, we could easily get together, and we met each other's family. My dad and mom really liked Joyce; it was the right match. We were married on a lovely June day in 1950 at the KU chapel. It was a small affair with family and a few close friends. We found an apartment on Ohio Street, where we lived until we started the Couples Co-op, then moved down the street to 1334 Ohio, next door to the fourteen-carat asshole I discussed earlier—perhaps in more detail than he deserved—who lived next door to the Jayhawker Inn.

We were young and had much to learn, but we had a blastoff start eleven months later on May 5, when Deborah and Eric were born. Then, it was off to Harvard, and the next big family event was Torrie, born in Cambridge in April 1955, followed by the completion of my dissertation and the move to Purdue. While we were at Purdue and Joyce was commuting to Meadville Theological Seminary at the University of Chicago, she began discovering the poetry in herself. She was thoughtful and expressive, and she had found her calling. It was a great, if vicarious, experience for me, as I was helping to make it possible. As I have already written, she was a natural for her newfound ministerial track. Those were golden years.

The details do not matter, but after many years of strict monogamy, we strayed. We violated one of the "thou shalt nots," thinking that it mattered not if you were open and honest with each other and discussed it. If you are going to commit adultery, yes, level with each other about it, and try to convert it into a way of strengthening your marriage, but don't count on it working out that way in the long run. "The rules of morality are not the result of reason," as David Hume so wisely noted.

Ultimately, after Sherborn and Palo Alto, where we lived while I was at the Center for Advanced Study in the Behavioral Sciences in 1971–1972, we began drifting apart, and decided to separate. I met Carol at the Center, but that was not a fundamental cause of what was transpiring, which truly had begun in the early 1960s. Many years later, Joyce sent me a note acknowledging it all and "apologizing," but it was not an issue of fault; it was an issue of forgiveness for us both, and we forgave each other.

In Palo Alto, we decided to tell our children about the separation (although we did not actually separate until the next year, after we moved to

Pasadena). We did tell them, but only with Deborah and Eric in a four-way meeting; Joyce did not feel comfortable including Torrie. It was against my instincts, but Joyce's sense of it had to be respected.

Joyce undertook the task of finding a new ministry and succeeded the following year while we were living in our apartment in Pasadena. It was the leading Unitarian Church in the DC area. Joyce was getting her due, exactly what she deserved and had earned. Her calling budded into a big-time success. She went on, eventually, to become the Head of Ministry at the UUA in Boston—the number-two position behind the UUA president. This was a fitting outcome, given her fine record at Chicago's Meadville, and rejection in Washington State because they did not want a female minister—wow did Joyce ever prove them to be misguided. She ended up being part of the revolution in women rights without any sacrifice of her femininity.

Years after we separated and I had moved to Arizona, we were divorced—a $50 do-it-yourself job under California law. There was nothing to contest, and lawyers would have found something, so we eschewed them. We split our assets down the middle. We had been married for twenty-five years, from 1950 to 1975.

Margaret Mead was divorced after as many years, and someone asked her what was wrong with the marriage. She said, "Nothing, it was a perfectly good marriage. We just wore it out." With Joyce and me, I am more certain of the first part of Mead's response than I am of the last. Joyce is a wonderful person and woman, one who can love and be loved, a fact to which her children can testify.

From the beginning, Deborah was destined to do her father proud. There are crawlers, and there are walkers. Deborah was the former. There is no need to hurry into walking when you are a crawl expert, a climber, and excel on all four. Exhausted from crawling, she once fell dead asleep in Lawrence on the screen door sill, door and screen ajar, after managing to open the screen door, then trying to crawl through it. She would have made it, but she pooped out on the trail. That was a harbinger of her ability to reach, commit, and focus her own resources on any task.

Deborah's mental functioning is similar to mine in some ways, although it took decades for me to understand myself and as long to recognize it in her. She has to be left to herself to figure things out. Like me, she doesn't readily catch on or immediately see what others may see about a situation. They are sometimes impatient about "the way she is." To me that sounds close to my mental home. That is not slowness; it's being methodical. It's a neurophysiological style. It's the way her brain works, and no two brains work quite

alike. In school, she always could get it, not always fast, but persisted in getting it right.

School is not set up for the slow but thorough. School is set up for the quick, so they can get on to the next quick thing, and it sucks. It's not life; it's a faulty construction made out of imagining life, not living it. I wish that we knew, that I knew, how to tailor each person's education to the architecture of his or her particular brain, but also recognizing that the brain in part develops in the crucible of educational experience.

After finishing a degree in psychology (1975) and waiting tables, Deborah went back to school—part time, a couple of courses at a time—math, physics, chemistry, etc., at her own pace. She worked her ass off and mastered what she was determined to master, graduating in chemical engineering in 1982 at Washington University in St. Louis.

Best of all, she produced for me a grandson, Tal. I had given up ever having a grandson, and just flat-out had my own—Joshua, thirty years younger than the twins. Tal is named after the children's book, so important in my childhood, squarely out of my homeland. I don't know Tal well, which is my loss, but that is changing. He came to Stockholm with his mom, and I was very pleased to share that with him. He's quiet, though still water runs deep. In the summer of 2014, I decided to give Tal my 1997 J-30 Infinity. It was a good match, I felt, and best of all we kept that wonderful automobile in the family!

Deborah and Eric, born two months prematurely, weighed only 3.2 and 3.6 pounds, respectively. They were nurtured in hospital incubators for an extra four weeks, until they reached the take-home-formula weight of 5 pounds. In the incubators, the twins breathed in an oxygen-rich atmosphere, long known to increase the survival rate of "preemies." It seems that—if my memory of the pediatric feedback at the time is correct—along with the increased survival rate came a roughly 5% chance of blindness. Early on, the cause of the blindness was often confused with German measles, which leaves babies blind as well. With less-than-ideal records, and with great variability across hospitals, there was plenty of room for error. Then, it was noticed that there seemed to be a correlation with oxygen, and for a while, it was thought to be related to a lack of oxygen. At some point, it was discovered that the incidence was highest in the best hospitals. Of course, they were the ones best equipped with incubators. Anyway, whatever the exact medical history, a cause was identified: Apparently, the oxygen-rich atmosphere, in some babies, caused the blood vessels in the eye to dilate, rupture, and damage the retina: It was called retro-lentil fibroplasia (RLF) then, and now is called retinopathy of prematurity (ROP).

Recent explanations of ROP are available at various Web sites.[1] ROP may affect blood vessel growth on the retina in babies born prematurely and is a leading cause of childhood blindness. Historically, a risk factor was excessive oxygen supplied by the incubator, but better monitoring and control has greatly reduced this risk. In most ROP cases, the blood vessel growth slows down and babies get better. More severe cases are treated with Cryotherapy that freezes parts of the retina that are affected, or Laser Therapy to stop the unhealthy growth.

Every ophthalmologist who examines Deborah's eyes can see the ROP scars. Eric survived with partial vision, but he was still technically blind. Over the years, that vision has been lost.

Joyce came home three days after the birth, which had been easy as births go. She bought a bulb-operated hand pump, set the alarm to wake up every three or so hours during the night, and extracted breast milk. It was refrigerated, and twice a week I delivered it to St. E's to be autoclaved and fed to the twins. They thrived on mother's milk and gained weight steadily. I was known as the milkman, and the nurses were all very much impressed by Joyce's output, which rose steadily and on the final delivery day was one quart and six ounces. About the third delivery trip, and hearing repeatedly how well the twins were doing, I asked our pediatrician whether it was necessary to give them so much oxygen, since they seemed to be doing so well. No one suspected that it could be harmful, but as a poor graduate student, I was paying for the oxygen, and I just wondered if it was necessary. The pediatrician said, "Don't you have Blue Cross/Blue Shield?" I did not, and she replied "I will take them off, as they seem not to need it anymore." This is how medical practice works. A breakthrough discovery—like saving preemies with oxygen—leads to its intense utilization, but then side effects start to appear. I have often wondered if my inquiry, which led to the twins being taken off oxygen, saved Deborah's eyes, and if it prevented Eric from being totally blind, as was common for afflicted preemies at the time.

I won't belabor what Joyce and I went through over the next months, as we gradually learned of Eric's condition and worried about Deborah, whose eyesight emerged unscathed, although not without residual evidence of scarring. Eric's blindness affected his physical development. He was late in learning to walk, and he skipped the crawling stage. We learned that crawling is somehow related to seeing—seeing something the child wants then going

[1] https://www.verywellhealth.com/retinopathy-of-prematurity-rop-2748613.

after it to satisfy curiosity. But during that first year in our Cambridge apartment, months after Deborah had been walking we trained him to walk by holding his hands above his head and moving him forward.

One day, standing, as he was able to do well, Eric stepped ahead on his own. Uncertain and wavering, he somehow got it. You cannot imagine what a thrill that was for him and, of course, vicariously for us. He could not have been restrained or stopped. He held his head forward, walked forward to keep from falling, and laughed and screeched with delight from one end of the apartment to the other. When the inevitable occurred and he fell, he was not a bit dissuaded. The self-propelled motion was a joy to him that had to be repeated incessantly.

By the way, during those years, Eric and Deborah had a Cambridge pediatrician, whom they used to see regularly, and when necessary he did not hesitate to make house calls: It was T. Berry Brazelton, who subsequently became the famous pediatric medical author.

Eric was very precocious. After moving to West Lafayette, we had great family discussions at dinnertime, and Eric filled them with questions. He had inexplicable insights. For example, one day at the dinner table we discovered that he could add any two numbers that were the same up to the numeral 10. He was only four and had not the vocabulary to articulate to us how he was able to do it. So, seeing an opportunity, I pointed out to him that if he could add any two numbers that were the same, and if he could also count, then he could add any two numbers, period. That intrigued him. "How?" I gave him the algorithm: If 2 plus 2 is 4, then 2 plus 3 is simply 2 plus 2 plus 1 equals 5. Similarly, from 3 plus 3 is 6, you get 3 plus 3 plus 1 is 7. If $x + x$ is $2x$, $x + x + 1$ is $2x + 1$. You can fill in all the gaps between the even numbers by adding one to the output of an algorithm which he could use but perhaps not yet explain. It is a good example of the brain's ability to develop skills that the mind cannot penetrate—a principle illustrated over and over in the study of experimental markets and decision making. He was fascinated and went into contemplation mode. The next day at the table he could add any two numbers whose sum was not larger than 10. So we talked about the ten-digit cycles, 1–10, 11–20, 21–30, etc. The third day he had it down pat and could add any two numbers ad nauseam.

Eric was a teacher's dream in school: He was well mannered (he inherited that from Joyce), learned things quickly, and was well liked by his classmates and teachers because he developed a great sense of humor and an upbeat attitude. He always scored well, which teachers love, because they can share the credit. He graduated from high school a particularly deserving National Honors Society scholar and was in demand with colleges—likewise wanting

to share credit by recruiting students who score well. He decided to go away for college to Macalister College in St. Paul, Minnesota.

A fundamental problem with educational institutions is that there is no direct means of determining their value added. The value added by a high school, a college, or its parts, like a semester, or a course, is the value of the output less the value of the input. A college may have a great reputation for placing its graduates, for having accomplished graduates, but that is valuing output. What was the value that went in? If the student input was carefully selected to be high performing, then the output is almost certainly going to exhibit high performance. But the institution's contribution is still the value of the output less the value of the input.

Consequently, there is competition for getting the best input. This is good, but what is missing is clear and aggressive competition to create the most value added. That is the secret to creating wealth in the form of human knowledge in education, just as it is in the economy.

If you go to Singapore or Hong Kong you will see why these city-states are among the world's most aggressive creators of wealth based on free institutions and the ultimate resource—people. They have no mineral or energy deposits, yet consistently rank in the top 20 countries with highest GDP per capita. They are completely dependent on imports paid-for by exports. They also consistently rank near the top of the World Freedom Index.

Every business enterprise stands in the winds of difficulty in creating value added: the market value of its output less the market value of its inputs. The difference is its profitability, resisted by the willingness of customers to provide the firm with high-enough revenue, and of suppliers to accept low-enough costs. If there is no profitability, there is negative value added, the enterprise draws down the available start-up capital and eventually fails. The world is crowded with people who are anti-business and critical of profit because they do not understand this basic principle of economics: No positive profit means there is no net value added. This is why the vast majority of businesses are *involuntary* nonprofit entities—they are incorporated as for-profit entities, try to make a profit, but fail to produce value in excess of cost. An incorporated nonprofit foundation is *voluntarily* and intentionally unprofitable, constituted to pay out everything that comes in. That is easy; it tells you why there is so little value-creating discipline in the Foundation World. And why it is so difficult to give away money intelligently; more difficult even than to make it.

Eric grew up in a sighted world and went to school with sighted students because the sighted culture assumed without evidence that anyone who was partially sighted and could read—he used large print special books and, later, a scanner that enlarged the print—belonged in the sighted community and thus, there was no need to learn Braille. Interestingly, left to his natural self-propelling devices after moving to Minneapolis, he independently sought out and explored the blind community, choosing to be identified and associated with it. He became an activist in blind organizations, which,

as you can guess, were traditionally dominated, organized, and run by sighted people. All that has changed, and Eric was part of the revolutionary transition.

Eric loved the Twin Cities so much that, thirty-odd years later, he still lived there. One attraction was its celebrated system of public transportation, well suited to his needs for facilitating independence—it enabled him to help himself. He has had a career in Minneapolis in the Internal Revenue Service, starting out as an accountant, but with his personality, he ended up running the public relations section. What? Did you say the IRS is into public relations? It sounds like a contradiction in terms, but Eric has made it work. Who needs good public relations more than the IRS?

He and his wife Laura now live in Washington, DC. When Candace and I lived in Arlington, we were able to get together for dinner and theater on a more regular basis, and it was like old times. Sadly, living in California, we are able to do it only on our occasional trips to the DC area. A star IRS employee, Eric was recently recognized by IRS Commissioner John A. Koskinen at a National press club meeting, March 31, 2015: "I also want to mention the people behind these numbers. None of the work I just described could happen without the dedication, professionalism and expertise of our employees, and my admiration for them continues to grow. I've never seen such a dedicated workforce in my entire career…A great exemplar of the caliber of our work force is sitting up here on the dais with us: Eric Smith, who has spent nearly four decades at the IRS working with reporters, helping them put the complexities of the tax code into plain English. And he shows no signs of slowing down. His dedication to public service is what you find with employees in locations all across the country."

Torrie is the one on whom our many family camping trips made an indelible mark and became deeply rooted. This led to her participation in one of the Outward Bound programs in the Elk Ridge region of Colorado when she was sixteen. I cannot imagine any two-week experience that could have been more transforming in terms of building self-confidence: Day in and day out wading ice-cold high-altitude streams with a light pack and rations, a 4×4 light tarp, hiking, climbing, in small teams in which it is obvious to the others if you are not pulling your weight, and ending with a day and night of isolation totally separated from all other humans. You experience the very edge, and upon coming through there is the transformational sense that you can endure anything!

Torrie studied pharmacy and was most attracted to hospital pharmacy, where you can make the fullest use of your training in treatment regimens. Her first position was in the pharmacy department of a California hospital.

One of her tasks was to track the prescription drugs each patient takes—doses, side effects—and make sure the physicians have not put patients on a cocktail of substances that are interactively bad news—the sort of specialty information that a hospital did not expect the general physician to be up to speed in knowing. But she soon learned the realities of how things work. She was criticized for doing her job by questioning a particular combination of doses prescribed by one of the doctors. The head of pharmacy told her: "When a doctor around here says jump, your response should be, 'How high?'" She went into the Peace Corps and helped to set up a pharmacy program in St. Lucia, Caribbean. She met her husband-to-be, Jim Call, in the Peace Corps.

Jim and Torrie abundantly illustrate the principle that the global economy vastly expands the freedom to choose. In fact, it's possible to live a great distance from that economy, appearing to be no part of it, even "reject" it philosophically, politically, and morally, so long as you *export something of value to that economy.* You can isolate yourself from it all but benefit enormously from the extent of its cost lowering reach. They moved to a remote area of the mountains above the beautiful San Luis Valley in Colorado. I first visited Colorado in 1936, at age nine, when mom, dad, and I vacationed for two weeks in one of Snow's log cabins, on the edge of Officer's Ranch, many miles out of Creed (which had a lounge that claimed to have the original bar over which Robert Ford, who shot Jesse James, was himself shot in revenge). We returned again for two weeks in 1937. Visiting Torrie and Jim, I found the cabin where we had stayed in the 1930s. It was still standing, and the view of Bristol Head Peak was just as I had remembered it.

As a pharmacist, Torrie is highly employable, part or full time, particularly in regions that do not attract hordes of professionals aspiring to drive BMWs. So they live remotely, choosing their lifestyle. But you have to export something, and they export pharmaceutical services and import vehicles, gasoline, books, solar panels, deep well-water pumps, "earth-ship" home-building materials, garden plants and seeds, camping fabrics, and gear much improved by global technology, cell phones, goats for backcountry packing, and so on ad infinitum in symbiotic dependence on the global economic engine.

Jim and Torrie are now "retired" living near Colorado Springs, but busy as ever with volunteer work, and reading, learning, and discovering.

I met Carol in 1972–1973 at the Center, where I was a fellow. She was a research assistant in medical economics for Victor Fuchs, who was also a fellow that year. We became good friends, in time lovers, and I commuted regularly on weekends from 1973 to 1975 to the San Jose airport from

Pasadena/Burbank. With many reservations—one for each degree of Arizona heat above 80—she moved with me to Tucson, where she still lives.

We bought a home on Maria Drive in one of the few remaining residential areas in which the home builders had left the original vegetation undisturbed, tucking each home in among the trees, shrubs, coyotes, rattlesnakes, gila monsters, tarantulas, puma, scorpions, deer, javelina (looks like a kind of hog, but is a peccary), lovable roadrunners, and a paradise of other birds. (Bird watchers descend upon Tucson from all over the world to observe a great variety of Western and Mexican birds.)

I want to report what Gusse Thomas Smith (*Birds of the Arizona Desert*, 1941) writes about the roadrunner. Her little book, privately published in Phoenix under her own copyright, deserves to be a classic but is "unknown"; a few cheap used copies can be found online. Sometime in the 1970s after moving to Tucson, I stumbled upon a tattered old paperback copy in a used bookstore, beautifully illustrated in black and white. All 40 of her entries are charming, but this one is about a bird that is charming beyond your imagination; as I read it, it's impossible for me not to visualize the old roadrunner cartoons. According to Britannica, the famous cartoon was introduced by Chuck Jones (1912–2002), an animator, in 1949, and produced by Warner Brothers; that was eight years after Gusse Smith's book was published, so it is possible that she influenced the cartoonist. I offer her essay on the roadrunner in its full-unexpurgated form. Follow her advice: If you see one, note the time of day, and look for that bird the next day at that time, and you will be rewarded—I have done it, and it works. Her description of how a roadrunner handles a rattlesnake can now be watched on YouTube; just apply your favorite search engine and watch the fun.

Roadrunner-Chaparral Cock

Geococcyx californianus

Average Length 24 Inches

Now there's a bird for you—the desert clown, unique to this region and like nothing you ever saw on land or sea. The Roadrunner is a ground dweller of the cuckoo family, two feet long, with a bill longer than his head and an appetite longer than his tail. His wings are stubby and ineffective for distance flying, his plumage always looks shabby, and raising or lowering his bristle-tipped top-knot, he always looks absurdly surprised. Around his mocking eyes he has a naked yellow ring seemingly emphasized with mascara on his stiff eyelashes—yes, eyelashes. He is streaked with brown and white, glossed with steel blue, changing toward the tail to bronzy green. His long tail feathers are blue–black and green, tipped with white. His throat and underparts are a dirty white,

dashed with black. You'd think with all that to work with he could look quite dressy, but he never does. A Bedouin, a true son of the desert—but never a vagabond.

At first sight, he appears tail heavy—but his best stunts depend on that tail. He is a speed demon and will race anything from a horned toad to a Lincoln Zephyr; when he is quite sure he has won, he will turn off to the side, throw his tail in the air, dig his toes in the ground, and look back in impudent astonishment. Though wild and lawless, he makes an amusing pet, not at all shy, ready to eat anything he can swallow, and an inveterate show-off—quite a ventriloquist at times. Ranchers usually like to have him around the barnyard, and all Mexicans love him better than any other bird, their loyal "Paisano."

The nesting habits of roadrunners are about as irregular and unusual as their other traits. Really tame ones will invade a desert shack and try to settle behind the woodbox; or, in the barn; though under usual conditions, they'll throw together an immense mass of sticks and trash on the ground under low brush, or in the lower branches of mesquite or paloverde trees. They trample this down in the middle after a fashion and line the depression with snake skins, feathers, and any soft things they can steal off nearby clotheslines. At irregular intervals from three to nine, elliptical white eggs appear in the midst of this confusion. They are so erratic about delivering eggs, and the temperature of the desert is so warm that babies of various ages get mixed up with perfectly fresh eggs and mother's work is never done.

Although they never put on any style and are such a rough and tumble lot, roadrunners are courageous and self-supporting and do an enormous amount of good in the world. They eat incredible numbers of the very pests man wants most to be rid of—grasshoppers, crickets, caterpillars, beetles, centipedes, mice, lizards and, most important of all, snakes—lots of snakes, little ones, big ones—all kinds.

It is a thrilling, three-hour show to see a roadrunner kill a big rattler, literally worrying it to death leaping and dive bombing over its head to deliver stab after stab at the base of the brain. If the victim is not too large, not much longer than the bird, it is another three-hour show to watch him swallow it head first. It is a desperate business and a lot of snake is visible a long time—I could never quite stay to the end.

Roadrunners live the year round in a chosen locality, developing a regular schedule in covering their beat. One passed in front of my desert cottage at mid-morning every day, with practically no variation of time, for several months; and I, as regularly, watched for him. He was always in a terrific hurry and intensely concentrated on finding food—lots of food. He and his whole family were ridden by slave-driving appetites and the more I watched him fighting to live, the better I realized that if he occasionally stopped at a meadowlark's nest, he was forced by necessity rather than innate meanness. His own most dangerous enemy, the coyote, is a really mean hombre, who enjoys marauding nests. (At least I think so, but I don't know him so well.)

In Tucson, about the first week of July, and continuing to and sometimes through September, all hell can and often does break loose in monsoon lightning and rainstorms. Early on, living in that house, Carol and I were

sitting on the covered patio and watching a storm sweep in from southeast of the city past the Santa Rita mountains, envelop the valley in a deluge with great, crashing, cloud-to-cloud and cloud-to-ground thunder and lightning. It seemed like hundreds of lightning bolts, and indeed, the next day the *Tucson Citizen* confirmed that Tucson Electric Power (TEP) sensors recorded more than 900 ground strikes. It felt like Kansas, and the farm, with mountains replacing the "prairy erth". This became my adopted homeland.

Since the house was on a rocky foothill lot, there was no space for a proper garden. But I could plant tomatoes in the spring at the edge of the back patio facing the south. Never buy a house in Arizona, or even in the northern hemisphere, without a wide southern exposure. In ours, planting in late September, we enjoyed sugar snap peas with vines reaching to the roof in the warm winter sun, but able to survive frosts with minimal protection from the overhanging roof.

During our first two years in Arizona, Carol was in the M.B.A. program in the business college. It was the accounting and finance courses that stuck—the substance of the program—and she went to work for Tucson Electric Power in June 1977 (though not immediately, as we were delayed by low water, in returning from a Grand Canyon whitewater trip with Ken Slight).

I found Carol's job at TEP of unceasing interest; it gave me a direct plug-into an intriguing world of regulation, not always, shall we say, with the best incentives for productive activity. She kept me current on that fascinating world. An electric utility has a huge cash flow because it is capital intensive; the wage and material bills are small relative to the services of the capital stock, which is long-lived, with thirty-year plant investments. That cash flow became the substance of financial market creativity. TEP's president had a background in finance, and TEP soon became known as "the finance company with a utility subsidiary." Why? Here are a few reasons.

Carol's first task was cash management, but she soon graduated to managing TEP's entire cash flow. So, TEP cleared check payments to suppliers (accounts payable) through an obscure South Carolina bank. That slowed payment deliveries—increased their "float," as it is called in monetary economics. Meantime, for accounts receivable, all large sums are wire transferred, or even courier delivered. Speed up incoming payments, slow down outgoing payments. This swells working capital funds for investment in order to increase return on total capital. Those were the days when a great many firms were not aggressively managing their cash flow and were much too insensitive to the concept of opportunity cost. Carol knew it well, pushing the payment boundary into the future while pulling the receipts

boundary forward, generating as much working capital for the company as the system would allow.

Again, suppose a utility is required to install $100 million of pollution abatement equipment. To sweeten the incentives for purifying the environment, these investments are allowed to be financed from tax-exempt revenue bonds at lower interest rates. So you finance the equipment as far in advance as is allowable under the law and reinvest in bonds at the higher interest rate on taxable debt instruments, selling them only as you need cash to meet construction payments.

And then there was the famous, or infamous if you prefer, "two county rule," of which TEP and Carol took full advantage. This rule allowed any utility, all of whose operations happen to be in no more than two counties, to finance both operating and emission abatement assets with tax-exempt bonds. These special exceptions get into law when some legislator puts a rider on a bill to help one of his constituents somewhere (it's called "pork"). The provision, though narrowly defined to include that beneficiary, of course doesn't name any specific firm. Anyone can benefit from the provision by satisfying the fine-print conditions. The downside for everyone else is that it creates incentives for people to invest to change their operations in order to benefit from the rule rather than to create new products and services. All sorts of smart and productive people, like Carol for her company, are encouraged to seek out these high financial return margins that arbitrage the tax laws, but contribute little if anything to create new economic value for you and me. They are all doing their jobs well, but they are afforded misaligned incentives; it's the rules that are misguided, creating artificial profits that encourage wasted time, effort, and resources. This is the overriding justification for simplifying the tax laws and closing all these wasteful redistributive loopholes that serve only particular interests. The challenge is not just to get rid of them, but to keep them from gradually creeping back, one special benefit at a time.

One of Carol's and my major mileposts together came in early 1980 when we adopted—the truth is we were adopted by—a forlorn and homeless puppy. I was driving our 1972 Scout II south on Park Avenue on my way to the university. Crossing Grant, I fortuitously ran out of gasoline—I had been having trouble with a faulty gauge on a newly installed oversize tank and had not allowed a large enough margin for error. I rolled to the curb just south of Grant and left the Scout. A black puppy with a few white markings emerged from underneath an old car parked in the driveway of a run-down multiple-unit dwelling, smears of grease here and there on its head and body. It followed me to the corner. I sped up, not looking back,

thinking it would return to its place, but I could feel that puppy against my heels. I got a can of gas at the station and saw that she (I was already noticing what I had to deal with here) was terrified by the cars wheeling in and out. So I moved her out of the drive paths and began walking back, the puppy hot on my heels. I rounded the corner back at Park Avenue, returned to my Scout, and poured in the gas.

It was time to go, but the pup was still there at my heels, with tail wagging. What the heck, the puppy had no tag, and no friends. There was an old red Sears Roebuck sweater in the back of the Scout. I got it out, placed it on the floor of the passenger side, and lifted the greasy little pup onto that sweater. She lay down, happy as a pig in mud, and we left for my office. At the office, I put the sweater on the floor to the left of my kneehole desk and pulled open the bottom drawer just over her head, so that it was a bit like she was in her own roofed home. She was completely contented. I called Carol and told her the story in sequence, revealing only at the end that I had brought the pup to the office. I described the pup and said that we needed to decide what we were going to do. Carol said, "I'll drive over on my lunch hour." When she arrived, Carol had already selected a name, sight unseen: Our new acquisition was no longer just a forlorn puppy—she was now Lucy and would live with us for years to come.

Our neighbor across the street was Harold Jarcow, whose son Jim was a veterinarian—one of the very best in town, but more of a research scientist than a retail veterinarian; he was into zoo animals, tigers, and sick snakes, so dogs were easy because he could talk with them intelligently. Jim became Lucy's vet. We had Lucy, and now she had a doctor, who pronounced her in good health and believed she was part Australian shepherd. What was most evident to us all was that the rest of her was Labrador retriever. The little girl loved water!

A year later, Lucy acquired a companion. We had visited a pet shop looking for a dog bed. The shop had an Alaskan malamute pup, she and Lucy were introduced, they bonded with each other and us, and we drove Lucy and Sophie to their new home together in the foothills. Lucy lived until age eleven; Sophie until age fourteen. They were our first children, our only daughters, and we loved those girls as if we had whelped them ourselves.

Those were exciting times for Carol and me, but the biggest event was Joshua, born on August 31, 1981—a good year for us all. She returned to work after a few weeks' leave, and I helped-out at home, becoming a home parent. It was back to the breast pump.

After seven years at Arizona, I was qualified for a sabbatical leave in January 1982, so it was good timing, and I applied for it, planning to work

at home on various research papers and writing projects during the spring semester. Classes ended in mid-December when Josh was three months old, and I would be a home parent until the following August.

I remember and savored those months; working at my desk with a body sling around my shoulders, Josh asleep on my chest. Mothers log lots of body contact with their babies nestled in their arms, and I was experiencing that feeling as a father. He was with me constantly on weekdays, and I had the warm pleasure of nourishing him, if only through a bottle as a conduit to his mother's milk.

Josh was an unusual child (like each one of my kids, but each in their own marvelous way); his forte was that he could hold his own with adults in conversation even as a preschooler. He never seemed to be bored with adult conversations going on around him, such as when eating out in restaurants, which sometimes can be hell for both kids and adults. In those early years, he and I went fishing together for two days in the motor home we had purchased. We talked a lot, and on the return, just as we reached Camino Escalante and made the turn down the hill toward our cross street, Camino El Ganado, he turned to me and said, "Dad, you know, I like there being just two." I realized how true it was for him, just two, one-on-one; that was very insightful for a preschooler. He had the same involvement with his mother, but on completely different topics from those that engaged us. Josh is a people person—How do people, not things, work?—and it's all bound up with expression in verbal, written, and musical forms. He learned to play the guitar. He had some good instruction, but much of it was self-taught. He did not study music by learning to read music, but he can play, compose, and create—so who cares about reading it? I learned to read music, but could not play worth a tinker's damn. In truth, reading was a worthless undertaking for me because I could never create anything with it. That is where the action, the satisfaction, and the communication lie. Josh composes songs in his head and plays them directly. He just rolls it from his brain down and out of his fingers and onto the strings, and the strings connect directly back to processors in his brain, as if he were talking to you with words. Josh is sort of spacey sometimes, as I am, but in a completely different way.

Yes, I am spacey, and I have difficulty emerging from deep *mentalizing*, though people don't always know it. I reply to them as if I am in their world when I am not. My brain naturally resists interruption, clinging to communication within itself, while expressing recognition signals to ward off the invasion. It appears that this makes me hard to live with. But I had two very tolerating wives, bless them, and they stuck it out for twenty-five years each.

They were as different as two people could be, or so I thought at the time. I was wrong, as I learned meeting Candace; she is as different from them as Carol and Joyce were from each other.

I met Candace at the meetings of the Association for Private Enterprise Education (APEE). APEE has an annual Adam Smith Award, and I had been chosen by the board to receive it at one of their meetings. Following my after-dinner address, replete with overhead transparencies, she was one of several people I talked with. I later learned that she was on the board, and the board had chosen some guy (me) that she'd never heard of for the award. Subsequently, I was asked to join the board. I saw her once or twice a year at these meetings and at the APEE conferences.

We became friends, and after a few years, lovers, and I am married again—for the duration, perhaps much more than twenty-five years. We rented a U-Haul, and I moved her myself from Pueblo to Tucson. We towed her car behind, with Oscar sitting in the driver's seat every mile of the way. Oscar is a Boxer, fourteen years old at this moment and deaf as a post, but a happy member of our family. When we travel, Oscar lives with Carol and her yellow lab, Zoey. They often have guest dogs—three or more; it's bedlam. He hangs in there with the youngest of them. Oscar has become my role model. [He died after I wrote these lines. He had a series of strokes, lost consciousness, and then recovered for a time. At one point he got up, retrieved his leash, and wanted to go for a walk; there's nothing like dying with your boots on!]

Candace and I have a "contract" of sorts, wherein I'm to live to 106. It might even work, as one of my ancestors, Joseph Lomax, lived to be nearly 105 (1809–1914). We work closely together almost daily, planning and executing our events, of which there are many—panels, lectures, interviews, roundtables, conferences, and so on. These tend to be general non-academic audiences. She's good at finding out what people think they want from me, giving them feedback, and working out something that fits everyone. I can't do that on my own, but Candace and I are learning how to do it together. She knows what I can do and has a sixth sense for relating it to the needs of diverse sponsors and audiences; 2003, for example, the first year that I was the part-time visiting Rasmuson Chair at the University of Alaska Anchorage there were dozens of requests and invitations from Alaskan companies and organizations. She would ask each what they had in mind for me to do. Typically, the response would be that I was to give a thirty-or forty-minute talk and answer a few questions. She might say, "That's not the best way to make effective use of Vernon. We need a way to find out early, maybe up front, what questions are on people's minds and have Vernon

build the talk around those questions." She evolved this approach by observing that the Q&A sessions at the ends of formal talks often were the best parts of the meetings; there was a spontaneity, a freshness, and more important, a direct audience involvement that made those sessions high points. The idea was to find ways of starting that experience earlier in the session. Sometimes it's done with readings and questions in advance. This works best if I am a guest speaker in somebody's regular class—we've done two dozen or more of these. In the best of these, the instructor takes written questions in advance and the students are *graded* on the quality of the questions. (Bart Wilson and I teach classes together, and we've employed this method with good results. Think about it: Good questions are harder to create than good answers, and you never get good answers if the questions are not thoughtfully stated. The professional skill of asking good questions normally comes late, but why shouldn't it be nurtured at the beginning?) Another approach is for me to talk for ten or fifteen minutes on issues relevant to a particular group's interest and then move quickly into fielding the questions.

Candace is incredibly helpful. I once attended a workshop for schoolteachers hosted by Steve Jackstadt (the guy who recruited me to the visiting Rasmuson Chair), head of the UAA Center on Economic Education. Candace and I met with the teachers in three of the sessions. There were readings for each session. Candace started each session by getting the teachers to write or state their questions; then, I organized my talk around the questions. The best part in my experience came last, on the third day: She had them interacting in real time, writing summary questions on the whiteboard (that really helps since my hearing has deteriorated more than my vision); then, my talk built directly on that experience. It was fun.

But better than all of this is our intimacy. No wonder I love her so much.

In Tucson, we like to go dancing. Again, as the fellow said, there are just two kinds of music: Country and Western. It took some effort to get Candace to overcome her reluctance to learn C&W, but she did—fast and well: the two-step, swing, country waltz, and still learning. We have friends that we see at the Maverick, now a fifty-six-year-old institution in Tucson that has survived two fires. Candace is my favorite partner.

There is a new development. Candace was first a student, then an accomplished professional in etiquette. For Candace, manners are concerned with rules and order in social, dinning, and networking occasions. She has made a business as an etiquette consultant. In demand, I often go with her on jobs, and do my thing while she does hers.

Mother, father, wives, daughters, and sons—I love them all as painted, perhaps in a portrait that they might not recognize.

There is much more that could be said, but it will not be. You are getting all that's fit to print. It's been exciting all the way. An adventure, to be sure, though I've never felt far from home. Each move, each adventure, seems like an extension of my homeland (Figs. 17.1, 17.2, 17.3, 17.4, 17.5, 17.6, 17.7, 17.8, and 17.9).

Fig. 17.1 Joyce, Eric, Torrie, Deborah, and Yukon Hot Springs 1965

Fig. 17.2 Josh and family, the way we were 1981

Fig. 17.3 Dad with Josh feeling what it is like to be mom, 1981–1982

Fig. 17.4 Our loving Corporal Oscar in memorium

Fig. 17.5 Candace and Vernon, the way we are

Fig. 17.6 Deborah, Dad and Tal

Fig. 17.7 Torrie and Jim

Fig. 17.8 Dad, Marfa Texas, trip with Josh 2015

Fig. 17.9 Eric's IRS Lifetime Achievement Award from Commissioner May 2017

Part IV

Rethinking Recessions, Markets, Adam Smith and Religion

> Yet personal knowledge in science is not made but discovered…It commits us, passionately…to a vision of reality. Of this responsibility we cannot divest ourselves by setting up objective criteria of verifiability–or falsifiability, or testability…For we live in it as in the garment of our own skin
>
> —M. Polanyi, *Personal Knowledge*

Our move to Chapman University in Orange, California coincided with the economic disaster that plunged the economy into its worst downturn in nearly 80 years. That numbing economic downdraft and the support of Chapman University would launch me into a re-evaluation of what I thought I knew about the economy and experimental market behavior. The following year, I finished and published *Rationality in Economics*, and my memoir *Discovery*, but what came next for me was a very fresh bout of new learning, both unintended and unexpected. In Part IV, I tell that story.

18

Home Again: Chapman University

He may live on by compact and plan
When the fine bloom of living is dead
But God pity the little that's left of a man
When most of his dreams are dead.

—Carruth, *Ghosts of Dreams*

In December 2007, Candace and I moved to Orange, California. Chapman University had made several of us offers we couldn't refuse—an unprecedented opportunity to do our work and bring it into the classroom, three new state-of-the-art laboratories all directly across the hall from our offices, reminiscent of our Camelot spatial-scientific arrangement at Arizona. No dead dreams here! At Chapman, "us" included John Dickhaut, longtime member of the accounting faculty at the Carlson School of Business, University of Minnesota. John had been hanging out as a visitor with us at GMU. He had majored in English as an undergraduate at Duke. Unusual for an accountant, he was always keen to explore fundamental questions, like "Emergent Recordkeeping Changes the Course of Human History." For John, accounting was about measurement, language and purpose simultaneously lurking in the hidden recesses of human creativity.

From GMU, the move included Dave Porter and Stephen Rassenti who would become the foundation of the Economic System Design (ESD) program at Chapman University, and a new Master Degree in ESD. We all had appointments in the Argyros School of Business and Economics, but Dave and Stephen also had School of Science appointments in mathematics. Stephen would be the Director of ESI. We had been hired by Jim Doti,

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President, and Daniele Struppa, Chancellor. Then in 2016, Daniele became our new president. The Italians were everywhere.

Bart Wilson and I would have appointments in both the business and law schools, where we would continue to develop the law school course we had started at GMU, which now evolved into "Spontaneous Order and the Law." Particularly significant, we will become more deeply enmeshed in deriving some of the economics, and rules-and-order implications, of Adam Smith's *The Theory of Moral Sentiments* (1759). This work will have a transformational effect on how we model human sociability. Adam Smith had long threaded through the way we taught and learned from trust and other two-person games; now there came some breakthrough insights, but more of that in Chapter 21. Bart will become a new kind of philosopher of humanity's search for socioeconomic betterment and help revitalize literature as a source of understanding that quest. We will link up with Jan Osborn in the English Department eager to learn and teach by bringing her literature skills to the table. A "new" synthesis will combine literature and economics as it began in the eighteenth century.

Steven Gjerstad, who we had long known by reputation and co-authorship connections, came to ESI as a Visiting Research Associate and later will be appointed a Presidential Fellow Professor. In 2008, Steve and I launched our joint work on the Great Recession, Depression, and the twelve intervening recessions, finishing our book *Rethinking Housing Bubbles*, published in March 2014—an episode that will change many of our perspectives on economics.

ESI received its budget directly from Daniele Struppa's office, but administratively we had appointments in the Argyros School of Business and Economics. Although I had some reservations at the time about departing from our original discussions, this had been exactly the right decision, for it helped us to integrate research perspectives into undergraduate education. Our arrival coincided with a "buyers' market" in new PhD talent, and we soon hired a stable of young, truly outstanding, assistant professors in accounting, marketing, finance, and economics. Although these new faculties were attracted by our worldly reputation in experimental economics, many were not experimentalists and we heartily supported hiring the best and brightest, whatever their direction or methodology. We also added two full professors, Nat Wilcox and Dan Kovenock, giving us a commanding presence in "experiment-rics," game theory, and political economy. Nat reminds me of Bill Vickery and Tom Muench—on top of the topic of every visiting speaker regardless of subject, content, and technical level.

What actually made Chapman appealing at the time was that it had no graduate economics PhD program. Our group had such a program at GMU, but it was a challenge to get students appropriately prepared for our sequence and degree; we had to raise money to finance it and were already discussing the possibility of re-directing our funds into the support of a pre-doc, post-doc and visiting professor operation, and getting out of the PhD business altogether. Our annual Graduate Workshop in Experimental Economics, inaugurated in 1994 at the University of Arizona, brought us a couple dozen top graduate students from all over the world, and we could screen them for the kind of inner-motivated students we might invite as pre- or post-doc visitors. We did not need a PhD program to interface with graduate students and programs.

Daniele's recruiting skills and high-academic standards were the key to our move to Chapman University. He contacted us early in 2007. Since leaving GMU was furthest from our thoughts, we would likely have dismissed it, except that we had known him as our Dean at GMU. Tyler Cowen, David Knott, and Kevin Gentry had recruited us in 2001 with Daniele's backing. Dean Struppa had been a great supporter of our program, becoming one of the lead candidates for Provost at GMU. He was our candidate and had strong support for that position, but in the end lost out, and subsequently departed. I had not paid any attention to where he went. It was Chapman University, unknown to me. We were not looking, but we would listen to Daniele.

The more we talked the better it looked. As noted, we had already been thinking of a program built around laboratory-based teaching and research, with regular short- and longer-term visitors, post-docs, and so on. Chapman was willing to fund it and build us new labs. Our center, which became the Economic Science Institute, would be administratively under the Chancellor's Office; Chapman's President, Jim Doti, came out to meet with us, and by spring there was no turning back. There was nothing to negotiate with GMU. Our wonderful seven-year run at GMU would transfer to Chapman.

Stephen, who had already been through two laboratory design and construction projects for us (UA and GMU), worked with Chapman Facilities Management to create a state-of-the-art laboratory. Daniele would move out of his offices, and we would occupy the first floor of the building, with laboratories in the center, offices in satellite position round-about. It was the ideal science facility for us and other faculty, affiliates, and visitors.

We quickly came to love Chapman. John was never more productive and creative and often spoke of his utter contentment. Similarly, for me, people would ask, "How do you like living in California?" And it became instinctive for me to reply: "I am not here because of California; I am here to be at Chapman University, and it is in California!"

We had some early concerns in considering the move. Accustomed to operating a laboratory on large state university campuses with loads of student subjects, how can we make it on a campus with only a few thousand (4500 students, but 8000 by 2014) students? An initial proposal to locate us off the main campus would not be satisfactory, as it is essential that a busy laboratory for human subjects be near the main student pathways. We were in Daniele's office in Wilkinson Hall when the issue arose, and we said it was critical that we be where the main flow of students could easily drop into the laboratory. Daniele replied: "How about the first floor of this building? I can move out." The signs don't get any better than that; from there on, the climb upward was jet assisted. We soon had one-quarter to a third of the student body in our database, registered, and eager to participate.

We were also concerned that we would need administrative help. Daniele was superb in his support, and some of us flew out to interview some candidates in advance of our move. From a dozen or so candidates that we interviewed, we identified Sharon Kreuger, who we thought would be a great fit, able to cover well what we do poorly. We conform well to the proverbial story about faculty who are like herding a bunch of cats. The decisive element with Sharon was her response to the question in our interview: "What did you see as your most important role in your previous job?" Her response: "Closing the circle." Perfect! We also identified a second candidate, Jennifer Cunningham. Jennifer had experience in a science laboratory, and we saw in her enormous potential for learning experimental economics and transferring her science experience into our environment. So, can we hire two? Yes, and both worked out magnificently. They were here for five years and left only in response to independent external circumstances. Most important they built operating systems that could be smoothly taken over by replacements if their own careers advanced: Cyndi Dumas replaced Sharon, Megan Luetje replaced Jennifer, and we have new winners—the kind of people who see their jobs as whatever needs to be done.

Candace and I moved into a modest home just under a mile east of Chapman; perfect for me to walk to the campus seven days a week; and it had some space between bushes and flowers to grow tomatoes and veggies. I needed little workspace at home because my office was not far. The home had been for sale (this is December 2007 when buyers were disappearing) and was converted to "For Rent" status the day we walked into Old Towne Al's realty shop. It fit my maxim: "own at most one home"—we still had our desert home in Tucson and were holding on to our Merrill Lynch miracle interest-only loan at just 3%, thanks to the easy-money generosity of Ben Bernanke, Chairman of the Federal Reserve System. A year later we moved

to a Chapman University property we leased, and Old Town Al remarked that "We need to find you a more appropriate place east of Glassell." I disagreed. West of Glassell in Orange was the equivalent of the west side of the tracks in Wichita. Yet, it was perfect for us: Our unpretentious three-bedroom "Doll House" was exactly two blocks west of my office. I needed no home study office. I could walk to my office in about six minutes. Evenings, Saturdays, or Sundays—I loved it!

John Dickhaut would be our early and unrecoverable loss. John had experienced a couple of bouts with cancer, recovering from the original surgery and chemotherapy, and treatment for a re-occurrence. I saw him while attending the AEA 2003 meetings in San Diego, celebrating the 2002 awards to Danny and me. He was recovering from treatment: In that episode, he would do well, subsequently visiting at GMU and ultimately moving to Chapman. He was always a key figure in many collaborative efforts with his colleagues. Then the disease returned, gradually sapping his strength, but he continued to work regularly and creatively. He insisted that Shengle Lin, Dave Porter, and I get a first draft of our experiments on re-trading finished (Chapter 20 concerns the gestation of that work and its connections with my parallel investigation, with Steve Gjerstad, of the last 14 recessions in Chapter 19); we cobbled together a first draft, and Dave Porter and I took it over to his house at 5 p.m., April 9, 2010. He was very pleased and passed peacefully about 12 hours later. His wife Sheri called with the news about 8 a.m., and Candace and I went over to be with her and with John until they transported him to whatever was next. John's ashes are in the garden area north of the campus chapel and often Candace and I drop by for a peaceful visit, but mostly just to be with him.

Indeed, as with Pete Steele, we all live on borrowed time. As we age, the debt gets larger and no one knows how to refinance it, but I did manage to deepen its term (Figs. 18.1 and 18.2).

Fig. 18.1 Friends long standing ESA meetings 2016

Fig. 18.2 Innsbruck Bohm-Bawerk lecture with Jurgen Huber Left and Michael Kirchler June 22, 2015

19

Economic Collapse 2007–2008: Would 1929 Be Reborn in Anemic Growth?

> Neither a borrower nor lender be;
> For loan oft loses both itself and friend,
> And borrowing dulls the edge of husbandry.
>
> —Shakespeare (Hamlet)

As I moved to Chapman in December 2007, we had not yet officially entered the Great Recession, although the National Bureau of Economic Research (NBER), long in the business of officially dating what is known in economic parlance as "The Business Cycle," would eventually designate the fourth quarter of 2007 as the start date. A far better designation would be "The Consumer Housing Cycle," as I will try to clarify in this chapter. In that month the *Wall Street Journal* published the article that I had submitted under the title, "We Have Met The Enemy and He Is Us." The title was a literal quote from a *Pogo* cartoon produced by the incomparable Walt Kelly (1913–1973). The print media always exercises their right to re-title submissions; the editorial board re-titled it, quite unimaginatively: "The Clinton Housing Bubble." I felt that was an overstatement. As I saw it, Bill Clinton was merely the instrument for an exceptionally popular proposal that became the Taxpayer Relief Act of 1997, overwhelmingly supported by both parties in the House (90%) and in the Senate (92%), and serving to demonstrate the relevance of my original title. If you want to see my thoughts,

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nearer the beginning of seven years of research and reflection on the Great Recession, you can access the *WSJ* piece.[1]

I had much to learn and unlearn in the years to follow, although there are hints in the above article as to how it might ultimately come together, as well as my misreading's (in light of later developments), such as the possibility that the Fed's continued concerns about inflation in 2007 might have merit. It didn't, and we all soon learned that inflation was and would not be a problem. In memory, a corrected belief can subtly blur into your recollection of the original. Hence, the importance of having a record of when and what events changed or did not change what you believed earlier.

My purpose in this chapter is to narrate my personal transition and address the recurrent themes undergirding economic recessions; how and what we have missed as economists in trying to understand their nature; how and why our accustomed policies fail. That failure is likely to continue because of the unfortunate political necessity of focusing on the here and now, shorn of any attention to a program of creating individual incentives that support the social ends that are sought. I extracted some portions of an early draft of this chapter, and Steven and I edited into a three-part "mini-lecture" that was published by the Orange County Register.[2]

The narrative in this book will omit important documentation. If you want that, and everyone should, Steve Gjerstad and I have done our utmost to provide it in *Rethinking Housing Bubbles: The Role of Balance Sheets in Modeling Economic Cycles* (2014). In part, our intent in that book is to motivate others to check our argument against the facts we present, or the facts we omit, and to seek further relevant evidence.

Our learning can be stated most succinctly as propositions, thereby also making it easier for the reader to link up with the topics and documentation in our book through its table of contents and index. Twelve such propositions seem to allow me to cover the most critical issues that I believe require emphasis.

[1] http://online.wsj.com/news/articles/SB119794091743935595.

[2] http://www.ocregister.com/articles/prices-635784-homes-housing.html.

Proposition 1: Severe Economic Recessions Have Their Origin in Household and Bank Balance Sheet Crises

We began by examining the Great Recession, for which there is a mountain of data on the buildup and collapse of single- and multi-family home construction expenditures and their prices, 1997–2006, a collapse that brought down the economy. I remember my initial sense that this episode was surely an outlier, with its unusually strong and transparent link between housing and severe economic distress.

Then our research turned to the Depression, and we soon came to see the 1920s boom and the collapse of 1929–1933 as having housing fingerprints all over it. With some very early exceptions, that was not the storyline in the economics literature. I think that the Depression data looked much different to us than to prior examiners, because we had in mind a model of the buildup and collapse of the economy into the Great Recession. There are differences between the two, to be sure, but that outline provides a template for seeing the Depression similarities in bright relief.

Finally, we investigated all the intervening 12 recessions—see below in Proposition 9. Except for these 12 cases being far less severe slumps than the big ones on either end, housing was very prominent in most. To us, it was obvious; this was not rocket science. Ed Leamer at UCLA could see it. Bob Shiller had been instrumental in making the role of housing more transparent in the Case–Shiller housing index that we used. Econometric papers had shown that the business cycle in GDP was "Granger-caused" by housing. So it wasn't as if no one had noticed, but few were paying any attention to this regularity. For economists, policy makers, and central bankers, it was business as usual, namely worries about inflation, the adequacy of business investment; housing was simply what you inadvertently goosed with monetary policy or retarded whenever you targeted the control of inflation. Steven and I were not finding these mainstream concerns to be the driving features, independent of housing.

Here is the model, or the fundamental working hypothesis, that soon emerged. The stage is set for economic fallout and collapse, as in the Depression and Great Recession, when there is (1) an unusual expansion in the output and sales of new housing accompanied by the rising prices of homes, old and new, and (2) the increase in new home expenditures is outpaced by the increase in the flow of mortgage credit. The latter is an example

of what the principal founder of classical economics, Adam Smith, would have called "other people's money" and there is too much of it. The key point is that bubble trouble arises when housing expansion is driven by an excessive flow of mortgage credit with buyers not having enough skin in the game.

Here is the important sense in which the credit flow is excessive: The expansion in the production of homes is decoupled from the growth in the income of home buyers, which is the only sustainable support for homeowner expansion. A house is a very long-lived source of shelter services bought for future consumption. On a national scale, growth in credit in excess of growth in new home spending means that home sales today are coming out of sales that would otherwise be made tomorrow. (Local real estate booms are different, as the surge in demand is due to migration, and housing resources need to be drawn into that locality. We've seen that in North Dakota with its recent oil exploration boom.) Unsustainable, the precise turning point in the housing market is widely unpredicted and unpredictable, but leads to a self-reinforcing frenzy of decline in which credit is reduced in the face of falling prices, and thus leads to a still larger decline in prices.

The consequence for the economy is a negative impact on the balance sheets of both households and their lender banks: Housing asset values fall against the fixed long-term debt of households; hence, the underlying collateral value of mortgages held by the banks suffers a corresponding decline. Many households, especially the most recent entrants into the market, are plunged into negative equity. The resulting household balance sheet damage extends into the future, causing households to hunker down, stuck in debt-reduction mode. Banks and other financial institutions holding mortgages with reduced market value face similar losses in equity, causing them to severely restrict new loan activity.

Gradually, these damaged balance sheets will be restored, but the process can linger for years and can mire the economy for years in a state of subnormal growth, as we have seen after 2008. The good news is that such crises have been rare in the USA. We have had only two in 78 years: 1929 and 2007, measuring from their start years.

Proposition 2: Standard Economic Models Do Not Contain Balance Sheets

Neither the traditional general equilibrium nor macroeconomic models of the economy contain balance sheets for households and banks; nor are balance sheet considerations part of the thought models of how the

economy works. In the models, money is simply a means of payment and of measuring the value of goods, services, inputs, and outputs. As goods and services flow from sources to destinations in households and firms, money payments flow in the reverse direction. Where durable goods are represented by stocks that are used but not consumed, like houses and factories, their use is a service in the current period. But the models poorly account for how such investments are financed, and the dynamics of the debt and equity claims against them are not explicitly included.

In "Economics after the Crisis: New Model Army" (*The Economist*, January 19, p. 75, 2013), you can read an assessment of the efforts underway to correct the above flaws by introducing banks into macroeconomic models. According to this article: "[B]ig central banks are interested in these new ideas although staff economists are reluctant to abandon existing 'industry-standard' models." Existing models have always been thought to have had a desirable characteristic: internal dynamic stability. Stability has been a convenient property because then you can model the effect of external shocks on the variables in the model. Consequently, severe infrequent cyclical downturns (Propositions 3 and 4), erratic "normal" cycles (Proposition 9), and the systematic linkage of the economy to housing, mortgage markets, and monetary policy are beyond the reach of these models. The models account for economic flows most of the time, have been good for teaching and researching certain principles, but are sterile sources for inspiring a better grasp of operations where balance sheets matter.

Proposition 3: The Great Recession—A Balance Sheet Crisis

The Great Recession is our most glaring and recent example of a housing boom and bust that devastated the economy. The median national inflation-adjusted price of homes turned upward in 1997, rose steadily, and even accelerated. Already in 2001, this index rose above its previous all-time high set in 1989, well in advance of the outsized growth in derivatives, mortgaged-backed securities, and in the interest-only and sub-prime loans that were so much the target of blame-mongers. These later developments do not account for the bubble's origins and simply added gasoline to a blaze already in the roaring stage. The index continued to rise into 2006, steadied temporarily, then began its rapid descent. The rise in home prices relative to all other goods—the meaning of being "inflation-adjusted"—peaked 85% above their level in 1997. The clear implication of that second phase of the bubble,

2001 to 2006, was that a bubble, already of historic magnitude, had morphed into one of severe disequilibrium between house prices, income, and all other prices. Because home values then declined against fixed underlying mortgage debt, household equity (value minus debt) declined precipitously. Indeed, some 23% of all households were soon plunged into negative equity—meaning that many occupied homes were currently worth less than the outstanding mortgage debt owed to the bank, with many more close to that edge.

The Great Recession was later declared to have begun in the fourth quarter of 2007 (December). Measured relative to that benchmark quarter, new housing construction expenditures (single and multi-family) had peaked almost 80% higher in the first quarter of 2006, but would stand 60% lower by the second quarter of 2009, at historically low post-WWII levels.

Moreover, in city after city a partitioning of home prices into three tiers, lowest, middle, and highest, showed that the lowest third increased in price by the largest percentage, the middle third by the next largest percentage, and the highest third by the smallest percentage. This structural pattern held when the bubble collapsed; after 2006, the lowest tier showed the largest percentage decline. Hence, it was the families of modest means, the ones that our public and private policies were designed most directly to help, that were most hurt. We are measuring "hurt" here in terms of net wealth, not income; the relative disparity in incomes was smaller over the whole period, especially after you adjust for the many income subsidy programs. (The Congressional Budget Office report shows that the three lowest income quintiles received 2.3, 7.4, and 13% of market income, but 9.3, 11, and 14.3% of income after taxes and transfer subsidies.)

Distinguishing the effect of changes in wealth from changes in income is critical to understanding the deep malaise from which it has been so difficult to recover. A decline in net wealth implies an absolute loss relative to past achievements in economic welfare—households start over at a lower level, whereas a decline in income implies only a decrease in the rate of welfare improvement. Total national home equity fell by more than half (58%) from 2006 to 2009. Think about it. The average rate of savings out of current income is 5%. If your income is $50,000, and your house has lost $50,000 in value, you are rebuilding that loss inch by inch at $2500 per year—a two-decade haul.

Traditional economic theory, including welfare economics, is only about income and misses all the painful action arising from balance sheet losses in net wealth. How we feel, however, is quite negatively impacted when our assets are worth less than what we owe on them, even if we are working, earning an income, and trying to smile.

Proposition 4: The Depression—A Balance Sheet Crisis?

I think it is true—certainly a good bet—that the Depression had its origin in housing-mortgage markets, though the available evidence to support this conclusion is not as extensive nor as complete as it is for the Great Recession; it is never-the-less compelling.

It is widely accepted among economists that the Depression was a consequence of the failure of the Federal Reserve System to respond adequately to the liquidity needs of the banking sector, especially in 1931. This is the central thesis in the influential work of Milton Friedman and Anna Schwartz in their book *A Monetary History of the United States* (1963). The argument is that the monetary collapse (evidenced by the bank run) in 1931 was not the inescapable consequence of economic forces at play, but constituted an independent factor that could have been prevented. The economic events of 1929–1930 were seen as an ordinary (some have even said "garden variety") turndown, although Friedman and Schwartz did not rule out that the turndown might have been relatively severe, absent inaction by the Fed.

However, none of this important work of Friedman and Schwarz rules out housing-mortgage markets as the origin of the severe 1929–1930 downturn, occurring distinctly prior to the banking crisis in 1931. I believe that Friedman and Schwartz were right in their interpretation of Federal Reserve policy, but incorrect in supposing that the banks had only a temporary liquidity problem, rather than a more fundamental, and challenging insolvency problem.

From 1922 to 1925, annual new housing construction expenditures rose almost 60%, held steady in 1926, and then declined in 1927, 1928, and in 1929, when housing expenditure returned to its 1922 level. Housing expenditures fell another 88% between 1929 and 1933 before beginning to recover. Debt, relative to the value of new home construction, was surging in 1922–1925: The flow of mortgage funds grew 200%, while new construction was growing about 60%. Moreover, 85% of the mortgage loans by the commercial banks of that period were either interest-only or only partially amortized loans, for relatively short 3–4 year durations. Hence, by 1929–1930, loans made in those peak construction years of 1925–1926 would have had balloon payments due or have required refinancing.

These households and their banks faced an insolvency problem that was surely beyond the reach of temporary injections of liquidity.

At the turning point in the Depression, 1929–1930 (and similarly, 2007–2008 in the Great Recession), larger and more sustained injections of liquidity could not help those in need of refinancing without encouraging net new debt, and further suspending judgment day for the insolvent balance sheets. In this environment, the standard solution of supra-expansionary easy money will be ineffective. More debt to prop up asset collateral value simply kicks the can down the road. What is needed is a return to home ownership growth that does not outpace income growth, but a return to that soberer state of growth implies a really painful period of transition. The only way to prevent that pain is to have avoided creating the imbalance in the first place.

Households lost a third of their home equity in three years, 1929–1932, with no Fed intervention; they lost over half of their equity in the three years between 2006 and 2009, amid unprecedented intervention. If monetary policy made so little difference in the latter case, why would you expect it to have made a big difference in the former case?

Proposition 5: Policy Experts, Economists, Consumers, and Businesses Were Blindsided by the Great Recession

The first policy-transparent signs of trouble for the economy—signals that were destined to spark a reversal in expectations and aggressive, but very traditional, action by the Federal Reserve—came suddenly and unexpectedly in August 2007 when the interest rate at which banks lend to each other overnight soared. Illiquidity, a freeze-up in financial markets, means an unwillingness to exchange Fed Reserve demand deposits for promises to pay. The banks were signaling their reluctance to lend to each other in response to a concern that the default risk on bank assets had increased sharply. Think of it this way: Each bank looked at its own balance sheet and saw assets that were deteriorating in value; so, here is another bank wanting to borrow? Not from me: The people most willing to pay soaring rates must be the ones most in trouble! On August 10, 2007, the Fed announced their recognition of this pronounced signal of market "illiquidity" only three days after issuing a routine press release, on August 7, in line with the Fed's more optimistic previous statements. Seven days later, on August 17, the FOMC reinforced its recognition of the new reality that "financial market conditions have deteriorated." From that time forward until October 2008, there ensued a 14-month period of continuing and increasing Federal Reserve "liquidity

enhancement." This action was squarely in line with the Friedman-Schwartz view of what went wrong in the Depression, and a determination to avoid the same mistake (see Proposition 6).

In my view, this was a period in which a problem of bank insolvency, stemming from the collapse in household real estate equity, was mistakenly identified as a problem in deficient liquidity. Consequently, the so-called critical events in mid to late 2008, like the Lehman Brothers bankruptcy, the failure or near-failure of Bear Sterns, A. I. G., Merrill Lynch, Washington Mutual, and Wachovia, were all unavoidable manifestations of unprecedented national household-bank equity loss; no interventions were going to change that long festering and growing asset-debt imbalance. Which particular banks or financial institutions succumbed first was not particularly significant; all those who had departed from traditional due diligence lending had balance sheets weakened from mortgage investments gone sour, where the dike would break was not the big-picture issue.

That Fed policy makers were caught off-guard is transparent in their abrupt turnaround between August 7 and August 10, 2007. That consumers and businesses generally were blindsided is indicated by the resiliency of their spending in the pre-recession period: Firms' investment and consumer durables spending did not decline until GDP declined as well, in quarter four of 2007.

In the Great Recession, the best-laid plans of men kept going awry. I say "men" advisedly because there were two prominently clear-eyed women in public policy that resisted or openly fought the excesses that were happening on their watch. Brooksley Born, appointed by Bill Clinton to the Commodity Futures Trading Commission (CFTC) in 1994, became Chairwoman in 1998. As Chair, she stood against the wind by favoring public hearings for a re-examination of the "exempt" status of derivatives. She had been a litigant in bankruptcy cases involving credit default swap derivatives and knew the hazards of these instruments as uncollateralized "insurance," a consequence of their exemption from being classed as "futures" subject to registration, exchange listing, and margin requirements. As a partner in Arnold and Porter LLP, "She was the head of the firm's derivatives practice and represented domestic and international clients in legislative, litigation, regulatory, and transactional matters involving derivatives transactions and financial markets. Ms. Born also specialized in complex civil litigation and arbitration."[3]

This was 1998, well before these growing financial market instruments had taken off as an investment vehicle. However, instead of serving as a source of potential learning from her experience, she was shouted down by "the

[3]See http://www.arnoldporter.com/professionals.cfm?action=view&id=557.

boys" over at the Federal Reserve, Treasury, and the Securities and Exchange Commission. Brooksley Born should be among your favorite Democratic public servants.

Written by an unidentified person, the Clinton Library on April 18, 2014, released nine pages of handwritten notes from a meeting between Born, the Fed's Greenspan, Rubin and Summers from Treasury, and the SEC's Levitt, documenting for the first time what may have transpired.[4]

Here is an excerpt from the report:

In the notes, Born opens the topic by discussing what she called a "concept release"—a document that would ask a series of questions about possible regulations. "CFTC feels that they must go forward given how marketplace has evolved," the note taker writes.

Rubin warns that the financial community is "petrified" that Born's concept release implies swaps are futures. Because futures are required to be traded on exchanges, the implication would be that swaps not traded on exchanges are illegal and unenforceable. He says Born's plan "raises uncertainty over trillions of dollars of transactions," according to the note taker's paraphrasing.

Born: "Why should looking at CFTC's own regs cause uncertainty?"

Rubin: "Massive concern."

Levitt: "Better ways to air issue."

Summers: "Given that, rightly or wrongly, Treasury, Fed, SEC, & industry view concept release as being disastrous for market, is there not a better way to proceed?"

Then, a flash of emotion. Born is "pissed" about a breach of confidentiality: "(CONCEPT RELEASE WAS LEAKED!) by Treasury?" The word "pissed" is scratched out and replaced with "angry."

Rubin: "Doesn't disagree w/substance of CFTC's actions. Thinks there's a better way to proceed."

Born: "Rubin is asking CFTC not to uphold the law."

Rubin: "Not suggesting CFTC not attain goal, but rather that CFTC find better approach toward attaining goal—one that does not carry such risks… legal staffs of various agencies should get together."

Undaunted by the bruising debate, Born went ahead on May 7 with the concept release, stressing that it "does not in any way alter the current status of any instrument or transaction." Greenspan, Rubin, and Levitt issued a statement the same day expressing "grave concerns" about the CFTC's release. That helped persuade Congress to pass legislation that would block the CFTC from changing its regulation of swaps.

Born resigned in June 1999. On December 21, 2000, Clinton signed the Commodity Futures Modernization Act of 2000, which formalized the exemption of most over-the-counter derivatives from regulation as either futures or securities. Regulators were thus only dimly aware of the explosive growth in new products such as credit default swaps, which helped pump up the housing bubble and in 2008 brought down American International Group, then the

[4]See http://www.businessweek.com/articles/2014-05-01/clinton-officials-missed-chance-to-avert-2008-financial-crisis#p2.

world's largest insurer. Interviewed in 2010, Clinton told ABC News that his economic team got it wrong—"and I think I was wrong to take [their advice]."

Rubin, as represented in the above notes, seems to believe that the derivatives market is much larger than can be justified by later estimates. In his 2009 working paper "Credit Default Swaps and the Credit Crisis," R. Stulz, we see the following estimates: *"Because CDS are traded over-the-counter and are not regulated, there is no official record of CDS contracts. The Bank for International Settlements (BIS) has statistics on the CDS market only since the end of 2004 based on survey data. By then, the CDS market had been alive for roughly ten years, but its size in the 1990s was trivial—an estimate for 1998 is that at that time the size of the market was $180 billion. By 2004, the total notional amount was $6 trillion. At the end of June 2008, the size of the market, at $57 trillion, was almost ten times bigger."* (p. 13)

Born's statement that the current status of any instruments would not be altered was surely designed, especially in light of the meeting, to open the door to grandfathering rights already created. The opinions of Rubin and Summers reflected that of the financial community and were about immediate outcomes, while Born emphasized process which, from her experience, she knew could lead to undesirable future outcomes. The whole episode illustrates the problem of getting support for preventive action when current conditions are favored by so many. In 1998, house prices were moving up after a long slide that had peaked in 1989.

Shelia Bair, the 2006–2011 Chairwoman of the Federal Deposit Insurance Corporation (FDIC), was appointed by George W. Bush. She reports resisting many features of the two largest bank bailouts—Bank of America and Citibank—by the Fed and the Treasury. During her tenure, she presided over the orderly bankruptcy of nearly 400 small-to-medium-sized banks at the FDIC, mostly at no cost to the taxpayer (see Shelia Bair, *Bull by the Horns*, 2012). Shelia Bair should be among your favorite Republican public servants.

Proposition 6: Bernanke's 14 Months of "Liquidity Enhancement"—A Test of the Friedman-Schwartz Hypothesis that Liquidity Expansion Can Prevent Depression-Like Episodes?

Bernanke was distinguished as a scholar of the Depression, of the mechanisms whereby financial market stress is transmitted to the real economy, and of the work of Friedman and Schwartz. Knowing these lessons, Bernanke was not about to repeat the Fed's failure to provide liquidity as needed in the event of an economic turndown. His commitment, based on this knowledge, is explicitly stated at the end of his speech honoring Milton Friedman's 90th birthday: "Let me end my talk by abusing slightly my status as an official representative of the Federal Reserve. I would like to say to Milton and Anna: Regarding the Great Depression. You're right, we did it.

We're very sorry. But thanks to you, we won't do it again" (Bernanke spoke as a board governor).[5]

And, as we have seen in Proposition 5, true to his word, he didn't do it again. As Fed Chairman between August 7 and 10, 2007, believing that the Fed was suddenly confronted with an illiquidity crisis, monetary policy was abruptly reversed and liquidity enhancement followed for the next 14 months. The effect was to suspend immediate further deterioration in short-term loan markets, but household-bank insolvency problems persisted and grew as house prices declined. The other shoe dropped as LIBOR rates soared again in September 2008, and the failed episode of liquidity enhancement was replaced by massive Federal Reserve intervention. The LIBOR events coincided with the surfacing of the A. I. G. problems, but these were symptoms rather than causes. As I have indicated, which particular banks or financial firms were hit first was not significant given the continued widespread deterioration of household and financial institution balance sheets. The Great Recession was upon us.

In response to what was clearly a second round of monetary action, but which became known as Quantitative Easing (QE) I, the Fed intervened massively to rescue the largest banks, 2008–2009, with unprecedented monetary ease continuing into 2014. Forgotten and nameless, but of immense significance—think of it as "(QE) 0"—was the 14 months of liquidity action that had been the classic Freidman-Schwartz remedy and that had fizzled like a wet firecracker. During the entire episode, the FDIC quietly restructured the assets and liabilities of 489 small-to-medium-sized bank failures from 2008 through 2013. Investors, not depositors or taxpayers, took the primary hit in that restructuring. Two questions remain unanswered:

Did Fed action to save the large-bank investors and their CEO's—described and sold as saving the financial system—really serve to soften the depth and extent of the recession for the economy?

Alternatively, did Fed action only serve to stretch out the years of underperformance and low growth in the economy?

Or was it both?

We have no way to be sure of the answers to these questions, though the answer to the second may well be "yes." The large banks and far too many households continued to be hobbled by weak balance sheets that leave no mark on the national income accounts, but have an effect of uncertain magnitude on expectations, hiring, and lending behavior.

[5]See http://www.federalreserve.gov/BOARDDOCS/SPEECHES/2002/20021108.

Proposition 7: Monetary Policy Is Ineffective in a Balance Sheet Crisis

Once household and bank balance sheets are damaged by a collapse in home values against mortgage debt, monetary policy becomes ineffective as a stimulus. Fed expansion of the base money supply (bank reserves) is simply held idle because of the collapse in the demand for new loans. Money held by the public shrinks as payment on existing bank loans exceeds new lending. With a substantial loss in home equity and many in negative equity, household precautionary concerns divert earnings into debt reduction. Saving increases in a time when capacity expansion plans are on hold and investment expenditures decline. The consequence is a glut of homeowners waiting for equity recovery coupled with banks that are in lending reduction mode. Keynes, aware that such financial markets were stuck, dubbed the ineffectiveness of monetary policy the "liquidity trap," providing only a technical name for the ineffectiveness, but not an anatomy of the balance sheet source of the stress.

The general result is near-zero interest rates for extended periods, as in the Depression and the Great Recession.

Proposition 8: When Monetary Policy Is an Ineffective Economic Stimulant, So Is Government Deficit Spending, and for the Same Reason

Once you see the important role of damaged wealth account balances and of depressed and negative equity in understanding household and bank precautionary behavior in severe recessions, it becomes credible why government deficit spending is also an impotent stimulant under such conditions. Both monetary policy and government fiscal policy operate primarily on the flows of income, spending, and lending; neither monetary nor fiscal policy changes the net depressed-equity wealth position that explains household-bank decision behavior; rather the policies only adjust the margins of change in net wealth. You are in a deep hole, but at the margin things are improving!

The evidence for the false claim that government deficit spending provides an assured boot-strap lift for an economic recovery rests on a single data period: The US economy did not recover from the Depression

until 1940–1941, and the recovery was a result of the defense buildup for WWII. I first heard this deficit spending argument in Alvin Hansen's classes at Harvard in 1952–1953. Because of the liquidity trap, only the government's direct injections of spending could extricate us from the Depression, as Hansen claimed had occurred in 1940–1941. This was the wrong recovery policy lesson, based then as now on the absence of capital balance sheets from standard models and thinking about economic performance. The inferred causal conjunction of recovery with enlarged government spending failed to appreciate that by 1940, we had been in balance sheet repair and rebuilding mode for about 11 years. The cascade of bankruptcies, bank failures, private and public policies directed to resetting mortgage debt to levels closer to their market value, plus the gradual process of debt repayment, all combined to slowly restore health to balance sheets. The flow of mortgage debt that had turned negative, 1930–1931, finally turned slightly positive only in 1938. Hence, the evidence supports the conclusion that *if balance sheets are in distress, neither monetary nor fiscal policy is effective as a means of stimulating recovery.* The corollary is that recovery will be delayed until balance sheet health is restored through direct private and public restructuring efforts—most prominently, extensive bankruptcy processes.

Monetary and fiscal policies can be effective in the absence of significant balance sheet stress. The recovery lessons from many of the post-WWII minor recessions illustrate the truth of this proposition. The economy in this period, though subject often to temporary surges and declines in mortgage-housing markets, never was beyond the reach of interest rate and fiscal policy actions.

Proposition 9: Housing Expenditures Are a Leading Indicator of Most Economic Recessions; in Only the Depression and Great Recession Was the End of the Recession Not Accompanied by a Housing Recovery; Instead, We Remained Stuck in Low Growth

In 11 of the past 14 recessions, counting from the big one in 1929, new housing construction expenditures have served as a leading indicator of downturns. More striking, in every post-WW II recovery of GDP, with the exception of the Great Recession, new housing expenditures were on

the increase. But is the Great Recession really an exception you might ask? Good question for the reason that the "recovery" in GDP was particularly weak in comparison with the typical recovery. This time, in contrast with the Depression, intervention took the form of 14 months of liquidity enhancement followed by a rescue of the large banks from insolvency. Did that action simply stretch out the economic malaise?

My claim is that the Fed's too-big-too-fail (TBTF) rescue of the large banks, arising from the Bernanke Fed's extant understanding of Friedman-Schwartz, had the unfortunate and unintended effect of preventing the back-to-zero rebooting of bank equity positions. The Fed did more than protect depositors and prevent a collapse in the monetary payment system. Under the guise and rhetoric of depositor protection, incumbent investors in those banks did not face the normal bankruptcies that occur when their investments fail. The effect was to weaken the recovery by the continuing incumbent claims on any future return from new lending. Bank investors did not suffer the consequences of their investments gone sour, and this crippled a more normal recovery, in the five years or more that followed the massive intervention in October 2008.

Why is this the effect of TBTF?

Insofar as toxic asset collateral remained on large-bank books and bankruptcy avoided artificially, *it dilutes the return on new loan activity that would otherwise flow to the new suppliers of capital to the banks, and this dilution lowers their incentive to finance new economic activity.*

Insofar as the Fed and Treasury lifted toxic (money losing) collateral from the bank books, and required compensation for the action out of any return on new loans, the consequent hit on the banks simply delayed recovery. Whereas if the Fed and Treasury took the loss, the cost is born by taxpayers and, in turn, becomes a drag on future recovery through public claims on future output.

Too-big-to-fail, widely seen as an unfair distributional action (as indeed, it was—different rules for different players!), has a hidden and unintended cost in delayed recovery and new wealth creation. Actions to protect incumbent investors from losses merely shift those losses to future anticipated recovery revenues and delay that growth.

The political system will always protect incumbent investors; they are visible and squeaky; they contribute to election campaigns; they assist in the choice of Secretaries of Treasury and government financial advisers; and they are anxious to avoid suffering badly from balance sheet crises like the Great Recession and Depression.

Ed Leamer at UCLA is one of the few economists around who has understood the persistent role of housing in the so-called business cycle. His forecasts include the effects of housing as those effects have been historically reflected in the post-WWII macro-accounts. Why, if he had the right model, did his forecasts fail to reflect impending price declines in the peak year of 2006? Here is a report by Carolyn Said, "UCLA forecast says state housing prices unlikely to increase," *San Francisco Chronicle*, September 28, 2006:

"If you buy a home today, you could sell it for about the same price in five years," said Ed Leamer, director of the UCLA Anderson Forecast. Of course, after accounting for inflation, the house's real value would have declined, he noted. UCLA is predicting that consumer prices will rise 4.5% this year, 3.3% in 2007 and 2% in 2008.

Leamer predicts California real estate will remain sluggish for a long time—at least five years—but with little price deterioration other than the failure to keep up with inflation.

"Relative to the historical norm, California homes are about 60% overpriced," he said. "It takes five years to get rid of that." After that period, he expects home prices to appreciate at a rate about 2% above the inflation rate.

Based entirely on a post-WWII model of the ordinary cyclical behavior of housing, these were reasonable expectations as of September 2006. Not part of that model, however, was the severe balance sheet crunch that would come from the collapse of housing value against fixed debt—a phenomenon that had not occurred in any of the other post-WWII recessions. The resulting huge loss in household and bank equity greatly compounded the downturn; housing and the economy would be completely beyond the accustomed reach of ordinary Fed monetary policy, and out of reach even of housing-sensitive models fitted to post-WWII data. That data set was bereft of any consequences caused by balance sheet crunches, and therefore models fitted to the data could not predict outcomes that exceeded the post-WWII empirical experience. That which policy makers believed could not happen again became a reality built into the models, including even the models that properly included housing expenditure flows. If the housing cycle and mortgage market model estimates had included the data of the 1930s, the forecasts would have had a shot at anticipating the severity of the decline.

Proposition 10: Stock Market Crashes Do Not Impact Household and Bank Balance Sheets the Way Housing-Mortgage Market Crashes Do; Consequently, Loss in Stock Market Value Is Not a Good Indicator of Potential Damage to the Economy from Losses in Housing Value

Proposition 10 was not understood by the Federal Reserve in the end-stage period, 2005–2006, of the recent housing bubble.

In the "dotcom" stock market crash of 2001–2002 about ten trillion dollars came off the value of securities, but the impact on household and bank balance sheets, and the associated recession was quite mild compared with all other post-WWII recessions. The Fed attributed the recession to weakness in firms' capital investment spending, and the Fed responded with unusual monetary ease, later criticized as adding excessive fuel to the housing market. Earlier large stock market crashes, such as October 19, 1987, did not lead to a recession. Yet in 2006–2007, when the total value of all US homes declined by "only" four trillion dollars, the banking system was severely stressed, the Fed launched a sustained 14 months of unusual "liquidity enhancement" and the Great Recession was upon us. Why do we observe such a pronounced difference in economic impact between loss in the value of securities, and loss in housing values?

Sophisticated Fed policy analysts did not understand the difference between a securities price decline and a housing price decline, in terms of their impact on the economy. Yet, these analysts fully appreciated that we were in the midst of a housing bubble. In June 2005, the FOMC sponsored a conference on the topic of "housing valuations and monetary policy," with five presentations by senior Fed personnel. The leading research question, "Is there a housing bubble?" prompted the finding that home prices had risen unsustainably relative to home rentals—a measure of fundamental value—and therefore constituted a price bubble. The San Francisco Fed's Senior Vice President, John Williams, used the impact of the 2001–2002 stock market crash to estimate the fallout from a potential 20% drop in housing prices based on how changes in wealth might affect household spending; his conclusion:

> There is considerable uncertainty regarding the magnitude of the effects of changes in stock market and housing wealth on household spending; nonetheless, it seems clear the magnitude of the current potential problem is much smaller than, and perhaps only half as large as, that of the stock market bubble. (Williams 2005, pp. 17–18)

The failure revealed here was to distinguish the household balance sheet effects of stock market price declines from that of housing price declines. When stock market values decline, the combination of tough margin requirements and callable loans impacts only margin investors; in a crash, enough loans are called to maintain positive equity in margin accounts, and neither borrowers nor lenders carry the burden of negative equity into the future to hamper spending and lending. However, as we have seen in

the propositions above, when housing prices collapse against fixed long-term mortgage obligations, household equity falls dollar for dollar with the declining home prices and can burden balance sheets for many years into the future.

Mortgage loans became highly leveraged, especially in the final years of the recent housing bubble. The collapse in the market value of homes against fixed mortgage debt directly brought a 58% decline in household equity and that stress severely affected bank equity positions. Such leverage effects cannot occur in the stock market because margin rules require at least 50% collateral to be pledged and maintained on portfolio investment, and a margin loan is subject to call in 24 hours. For houses, however, an "underwater" mortgage goes unrecognized on bank balance sheets so long as the borrower is making loan payments. Consequently, lender risk accumulates as collateral market values erode against fixed historical book values, threatening financial system solvency and severe economic distress for all. Contrastingly, in stock market crashes, loan obligations are flexible downward with assets liquidated against debt day by day. Future decisions of both debtor and creditor are not burdened by the millstone of negative equity.

During the period 1928–1934, the development of stock market margin rules created new property right rules through a process of adaptation to the experience of boom and bust. Private sector brokers and banks began increasing the cash margin requirements to 50% in 1928–1929. In 1933, for the first time, the New York Stock Exchange required all broker members to impose margin requirements on their customers. Finally, in 1934, the Securities and Exchange Act introduced Regulation T, fixing a minimum margin requirement implemented by the Federal Reserve Board. First private sector institutions, then Congress, codified the property right rules that would stabilize the impact of stock market volatility on the economy. These rules are with us down to the present and have served to limit the collateral damage of stock market crashes on the credit system and the economy (Gjerstad and Smith 2014, pp. 219–220).

A parallel adaptation in mortgage markets ushered in amortization rules and due-diligence standards for mortgages in the 1930s (Gjerstad and Smith, p. 220). But after a long period of stability, these developments began eroding in the 1990s in response to the demand for eased standards to enable families of modest means to more easily become home owners.

Tough margin rules have not provided a guarantee against stock market bubbles and crashes. What they can do, in contrast to mortgage lending rules, is demonstrate clearly that they can limit the external damage to banks and the economy. Lax mortgage standards have brought great-unintended

harm, even for those we most hoped to make life better. The reasons are evident: Strong public and private constituencies existed to alter mortgage terms and down payments or "margin requirements." For politicians, it meant votes, because they were making it easier to own a home. For banks, mortgage originators, real estate brokers, and investors, it brought new business. The consequences were low, zero, and even negative down payments for homebuyers in the euphoria of the housing bubble. In addition, privileged exempt status for derivatives meant that they were neither listed nor required collateralization. These were housing-finance property right failures juxtaposed with the lasting property right successes achieved in securities markets. Both those who call for more "regulation" and those who call for less need to see the real problem as one of aligning property right incentives in the service of stable economic performance. Needlessly, complex and excessive forms of regulation can be just as damaging as imprecise and destabilizing regulation.

Proposition 11: Bankruptcy Facilitates Recovery from Negative Equity Balance Sheet Recessions; Artificial Avoidance of Bankruptcy Delays Recovery: The Primary Mechanism of Recovery Involves the Repair and Rebooting of Damaged Balance Sheets from New Sources of Growth

With only two balance sheet recessions in the last 80 years of US history, we have little experience against which to test the effectiveness of alternative policies and economic responses. Across the world, however, there are many investment booms, including housing, that have culminated in severe balance sheet crises. Many smaller countries from Thailand and South Korea to Finland, Poland, and Sweden, and the large economy of Japan provide observations on a range of responses. Most all of them suffered severe declines in GDP followed by slow recoveries.

In all cases, the best outcome would have been to have avoided the excesses in the first place, but that is hardly an option once trouble begins.

Thailand and Finland are examples in which the ending of a long investment-housing boom induced a sharp drop in their currencies, as the inflow of foreign capital that drove the boom reversed. Bankruptcies accelerated, and currency exchange rates declined, but this in turn caused prices and profits in the export industries to soar, starting a recovery turnaround.

Although the currency depreciation raised imports prices, imports tended also to rise, after an initial lag, because exports typically have important components that are imported. For example, Mexico is an important assembly point for over-the-road truck-tractors, with engines, axels, tires, and other components imported from abroad. In these countries, the stability of their debt-fueled relative contribution to world output came into question, and this manifest itself in the market for their currencies. The resulting economic devaluation jump starts the recovery process. Failed enterprises are part of that recovery process, as exports grow and imports decline.

Japan and Sweden are examples of larger economies that followed distinct pathways after crises in the early 1990s. In Japan, the economy floundered in slow growth for over two decades; Sweden recovered, if not spectacularly, far faster and better than Japan. I would argue that the difference was a direct consequence of following different policies in the treatment of bank distress. Japan's real estate market suffered a major decline in the early 1990s. Home prices peaked in the fall of 1990 and fell by 25% in two years. Japanese policy permitted banks to carry souring mortgage loans at original book value. Moreover, to enable borrowers to continue to meet their mortgage payments, lending was expanded to them. This response seems to have been perceived and rationalized, as a public–private mechanism to "smooth out the bump," but serves to camouflage its inevitable damage. Existing bank investors avoided failure by simply stretching out the return on their investment, relying on a presumed recovery from new growth in the economy.

Japanese forbearance, in the form of permissive bank behavior, was coupled with government deficit spending—tax revenue declined and expenditures rose—as a means of stimulating growth. However, the expected growth never materialized. Japan was caught in the black hole of too much negative equity, and let it fester: The banks, burdened with large inventories of bad loans, sank into debt-reduction mode and were reluctant to incur new debt, much as their household mortgage customers were mired in debt-reduction mode and did not spend. The result was a decade of lost growth that stretched into and absorbed a second decade of dismal performance. The policy "cure"—let the banks hide their de facto losses in the books and "save" their incumbent investors—created the sink that exceeded the pull of recovery forces. The same forces were prominent in the USA, where bank managers vigorously opposed the more stringent mark-to-market accounting standards that had been proposed by their private advisory group, the Financial Accounting Standards Board.

Japanese growth, truly impressive from 1976 to 1991, declined to less than one-third as much in the following 15 years. Japan embarked on an aggressive program to lower the value of their currency to stimulate exports in 2013. This top-down effort is not the same thing as a market-induced depreciation, but after two decades of balance sheet repair, this initiative might work. As of 2016, Japan had continued to grow modestly unperturbed by these more aggressive new measures.

Sweden's response to deep recession in the early 1990s was the opposite of Japan's: Bank shareholders were generally required to absorb loan losses, although the government financed enough of the bank losses on bad assets to protect bank bondholders from default. (I see this as a mistake; bondholders assumed the risk of default; and a bank's failure may require bondholder "haircuts" to get the full recovery benefit.) The result was favorable to Swedish economic recovery from a severe downturn. By 1994, loan losses had bottomed out and lending began a slow recovery that accelerated after 1999.

The bankruptcy model for failed banks is also the FDIC model. Some 489 banks—mostly small-to-medium-sized banks—failed from 2008 through 2013, generally at no cost to the taxpayers; where asset value was inadequate, depositors were made whole—subject to maximum deposit levels—through prepaid insured-deposit reserves.

As a scholar well versed in the Depression, Bernanke's research had included the study of mechanisms whereby financial stress was transmitted to the real sector of the economy. Bernanke zeroed in on the channels that transmitted financial sector stress to the industrial sector. One channel was the connection from failed or suspended banks to their borrowers: Businesses that had established relationships with a failed bank encountered reduced access to capital markets. See Bernanke, Ben S. (1983). "Nonmonetary Effects of the Financial Crisis in Propagation of the Great Depression." *American Economic Review*, 73, pp. 257–276. However, even solvent and surviving banks reduced their lending during the Depression, and, as we shall see, there should be no inference that bank bailouts will enable those lines of credit to continue.

Bernanke's argument provides no support for the effectiveness of public policies that prevent bank failure, either via American-style public expenditure bailouts or Japanese style permissive accounting standards that allow banks to avoid marking loans to their market value (that is, recognize reduced asset value on the accounting books). Banks do not like to do this because it reduces their reported accounting profit, share prices, and their

capital base. However, these realities are already in market prices and do not change because of accounting misinformation. Any new loan activity, if it is to serve the objective of growth and recovery, must be free of the burden of sharing its return with investors who bore risks that failed. This is the function of bankruptcy; intervention to avoid de facto failure and to rescue incumbent bank investors has a cost that extends far beyond the direct loss to the previous investors. Bank failures, and the restructuring of assets and liabilities, allow the new capital to flow directly into new enterprise activity at the cutting edge of technology—the source of new products, output, and employment which in turn provide the new growth recovery. Requiring new investment to share its return with investors that have been rescued from the consequences of their own decisions dilutes the return on the new activity; it is tantamount to requiring Henry Ford to share his return from investment in his new Model-T horseless carriage with the carriage makers, livery stables, and horse-breeding farms that his innovation rendered obsolete. In a dynamic economy, the exit of failed enterprises releases labor and material resources for employment in entering firms operating on the expansion frontier of new technologies. The new activity is invisible to policy makers; the old failures are highly visible.

Proposition 12: Rules That Focus on Incentive Compatibility Alone Carry the Realistic Hope That Stability Can Replace the Historical Instability That Has Been the Hallmark of Housing-Mortgage Markets

The deterioration in mortgage lending standards allowed credit to fuel the most recent housing bubble; underlying it all stood individual incentives that were incompatible with stability. Fix the incentives with the appropriate rules, and you fix the problem at its roots. Then, avoid messing with the rules.

Loan originators received their fee-for-service up front for finding and landing a borrower. After their issuance, these loans were sold to mortgage aggregators, then insured, and the security claims on these packaged mortgages sold to lender-investors. This fee structure disconnected the loan originator from the incentives that a lender would have for due diligence on the credit-worthiness of the borrower, nor is it consistent with a lender's preference for larger down payments, and for earlier rather than later principal reduction payments.

The media made much noise about "predatory lending," actions directly derived and resulting from this incentive-incompatible fee structure. Indeed, the complex Dodd-Frank Bill contained an entire section "Title XIV – Mortgage Reform and Anti-Predatory Reform Act," devoted to the prevention of predatory lending practices. Instead of this heavy-handed, end-state, approach to the symptoms of bad incentives, here is a simple rule for subjecting the loan origination process, at its beginning, to the same incentives that a lender would face. The market agreed-upon origination fee is paid in proportion to principal payments of the borrower escrowed into monthly payments by the loan servicer and paid to the mortgage originator. This is the way taxes and insurance have long been folded into the monthly payments and paid to the tax assessor or insurer. Thus, an interest-only loan, with no principal payment due for 10 years, would yield no fee recovery for 10 years until principal payments begin. Earlier rather than later principal payments, including down payments, would sweeten the origination of such loan terms, exactly as it would sweeten the loan from a lender.

There are many other examples of incentives gone awry in housing-mortgage markets because of rules that distorted incentives and led to huge market inefficiency. Thus, derivative contracts for insuring mortgages against default were a class of "exempt" futures contract (specifically, exempt from being standardized and exchange-traded like other futures contracts) and did not require the seller to post collateral in the event of defaulting on the insurance obligation. Sellers of insurance against natural hazards—fire, storm, and water damage—are required to have resources on call in the event that damage is incurred.

In conclusion, good incentive-compatible rules mean nothing more or less than good property rights in which the choices made by the actor do not benefit self without also benefitting others that are an integral part of the economic relationship. Since the market involved long-lasting durable goods, the incentive-incompatible rules governing their credit financing brought long-lasting inefficiency in the form of damaged balance sheets.

> Getting the rules right, however, is not a slam dunk. When people get swept up in expectations of rising prices, they have powerful incentives to find a way around attempts to prevent them from acting on those expectations. As I write (*Mayday*, 2014), China has been seeing an investment boom that prominently includes real estate. The banks have tightened credit, but there has been a spontaneous turn to company-to-company loans that bypasses the more stringent loan availability from the banks. (Wei, L. and D. McMahan, "China Firms Tap One Another for Cash." *Wall Street Journal*, May 1, 2014)

In Summary

The typical post-WWII recession was minor compared with the Depression, 1929–1933, whose effects reverberated until 1940, when a decade of balance sheet rebuilding allowed increased government spending to bring full recovery. The economic and policy consensus was that the primary cause of the Depression had been an inadequate provision of liquidity by the Federal Reserve. This framework was accepted with little understanding of the initiating economic causes of the Depression. Moreover, the overall good performance of the economy in the post-WWII decades reinforced the official belief that expansionary government monetary and fiscal policies could be relied upon to avoid a repetition of the Depression.

Left out of this comfortable malaise was an understanding of the role of balance sheets in severe economic downturns. In this chapter, I have argued that all that changed with the run-up of inflation-adjusted national home prices, 1997–2006, fueled by an unprecedented inflow of mortgage funds to the housing market, followed by a collapse in housing and the economy, from 2007 to 2009. Even after massive monetary intervention by the Fed, housing was slow to recover with economic output stuck in low growth.

This experience questions the consensus that the Depression was attributable only to Fed inaction. More plausibly, the Depression was a housing-caused balance sheet crisis, much like the Great Recession: A credit financed housing expansion borrowed deeply from future demand, leading to a correction that was beyond the range of Fed interest rate policy and government fiscal policy. Appropriate Fed action might have softened the impact of the Depression, as it may have done in the Great Recession, but the main lesson should be that such painful corrections require more courageous attention to avoiding the excesses that cause them. A necessary condition is surely that economic modeling and thinking require attention to creating incentives that are compatible with a more orderly production of long-lived durable goods like housing (Figs. 19.1, 19.2, and 19.3).

Fig. 19.1 KU Honors Great Opera Star Joyce Didonata; lucky me on the left 2014

Fig. 19.2 KU Honors Shelia Bair, heroine of action in Great Recession 2012

Fig. 19.3 Neighbors Candace, Vernon, Steve, and Stephanie

20

Markets: The Good, the Volatile and the Sometimes Ugly

My involvement in the study of recessions and their housing-mortgage market origins had completely unexpected consequences for my perspective on the supply and demand experiments that I began conducting in the late 1950s, and the subsequent asset market experiments in the early 1980s. Chapter 11 provides an unexpurgated narrative of my original perspective on the supply and demand experiments, and Chapter 15 includes a discussion of our assets market "bubble" experiments of the 1980s; neither of these accounts reflects the most recent changes in my understanding, which I now want to record.

In rethinking housing bubbles in the context of national economic data, the necessity of revisiting those original market experiments became evident. It was plain enough that there would be, and were, parallels between housing bubbles in the economy and the asset market bubbles we discovered in the laboratory in the 1980s. However, I would never have expected any change, let alone a major transformation, in my perspective on the supply and demand experiments of the 1950s and their expanded development through the rest of the century. The results of the original experiments, the data, stood firm. What changed in my thinking was their broader meaning and significance for the economy, and *pari passu* this led to a new perspective generally on understanding markets and their role in economic betterment. That change in perspective also had links to the experimental asset market bubbles in the 1980s, and the underlying sources of stability and instability in economic performance that was the more complex topic of Chapter 19.

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One of the things people teach and students learn in an introductory macroeconomic course is that aggregate consumption expenditures vary far less than aggregate investment expenditures. In our book, Steve Gjerstad and I dig deeper, to separate our analysis of the non-durable consumption goods, *C* (goods that don't last; e.g., hamburgers, and services like haircuts, the consumption of which is inseparable from their production), from durables, *D* (mostly automobiles). Similarly, we also separate housing expenditure (single and multi-family), *H*, from business plant and equipment expenditure, *I*. Non-durables *C* are not only very stable over economic cycles, but very large; if you subtract government expenditure from Gross Domestic Product, *GDP*, you get total private product, of which 75% is *C*. Hence, three quarters of privately consumed output is stuff like hamburgers, haircuts, and other services like hotel room space, air passenger trips, and so on. *None of these items are bought (or rented) to resell, only to consume or use.*

This description approximates conditions in the supply and demand experiments that I discussed back in Chapter 11, although I hadn't thought of that distinction as particularly important until sometime in 2008 or 2009. In my first S & D experiments, there were many buyers and sellers, but each trade was for a single unit between a particular buyer and seller. For each profitable purchase, the buyer's assigned value is above the price he/she pays the seller, otherwise it would be a money loser—you avoid buying a haircut in a salon that charges more than you are willing to pay. Similarly, the seller's assigned cost in the experiment is below the price received from the buyer, otherwise the sale would be a money loser. (Even if a particular sale is not fully net-profitable, if it is above your out-of-pocket cost, the surplus contributes to fixed overhead cost recovery). The buyer in the experiment immediately records the surplus (personal value minus price) from the purchase; the seller records the surplus from the sale (price minus personal cost). Each gain from the voluntary exchange, otherwise one or both would elect not to trade. These experimental procedures apply to perishable goods like restaurant food, retail services, and taxicab trips. Buyers and sellers are specialized in these roles, know their roles in advance of trading, and never re-trade the item (when have you gone to the local hamburger stand for the purpose of buying one to resell?); and the value is enjoyed (realized) as an integral part of the exchange.

Housing, a very long-lived consumer good with a large and active resale market, lies at the other extreme of consumption. Moreover, while the rate of expenditure on non-durables is very stable relative to *GDP*, expenditures

on new homes (*H*) is a roller coaster of volatility: measured as a percent of *GDP* in 1926 it hit 5.3% of GDP, dropping steadily to 0.5% in 1933; the post-World War II high was 5.3 again and a low of 0.9%. Housing market volatility corresponds to the discoveries revealed in the asset market experiments we had studied in the 1980s, which had surprised everyone by being so bubbly even though information on their fundamental value was transparent and available to all in each of the experiments. (See Chapter 15) "Transparency" of information was not sufficient for efficient outcomes; what mattered was the actual experience of buying above fundamental value or selling below, and finding it unprofitable, although the "herding" tendencies of inexperienced subjects do lead to unsustainable temporary profits from playing the bubble. We had stumbled upon a reliable "bubble factory" that defied all reasonable expectations, much as the housing bubble, 1997–2006, defied beliefs.

In both the laboratory and the economy, the critical difference between the performance of markets for durables and markets for non-durables, reduced to a single characteristic, is that durables could be re-traded and non-durables could not. This led to the hypothesis that market instability derives fundamentally from re-trading activity. To test this hypothesis, we needed to revisit the economic environment created in the original supply and demand experiments, but introduce re-trading to compare the results with the condition of no re-trade. The objective was to see if we (I, and co-authors) could upset the comforting tendency of those experiments to yield rapid convergence to a stable predicted equilibrium with private information on value and cost. The equilibrating stability manifest in the early experiments and in non-durable goods in the national accounts was not in dispute, but here was a chance to better understand the sensitivity of the results to the technological fact that perishability was necessarily linked to not being re-traded. We were on the trail of potential new learning, and it was exciting to see where it would lead, not least because it involved an old and presumably settled topic—equilibrium convergence under private information—but we were seeing it in the lens of a fresh new framework of thinking.

Our objective now was to return to that much earlier Purdue world of repetitive single-period supply and demand economic environments and to introduce re-trading, but only within each period. I and many other experimentalists had learned that asset re-trading across the entire horizon of the experiment had produced a bubble factory. I think I was surprised by this result, not only because I expected people would find it too risky to trade

at prices away from fundamental value—on which all were commonly informed—but implicitly I was overinterpreting the results of the earlier experiments. I was extending them beyond the special framework within which they had emerged.

As professional economists, we fall into the error of thinking about the world in terms of our models, not in terms of how people on the ground see the world and choose to act. Herb Simon used to implore us to ask how the decision maker sees the world and chooses to take action. Adam Smith took the same stance in the eighteenth century, which is why he is so important to study, and why I wrote Chapter 21.

This error—viewing the world only through the lens of our models—seems to arise because the models become the way we think about people's actions. The models, however, are clearly a construction that we erect out of our thoughts about particular phenomena in the world—thinking and imagining precedes modeling and is essential to modeling—but then we abandon the original thinking process! Yet we all know that the models are conscious abstractions from the richness of personal experience, justified because we are reaching for an essential coherent representation of our experience. That reduction to a simpler representation we know to be subject to error, but then we avoid continued exposure to that potential error by using the models not to aid our thoughts but to substitute for them. The modeling, properly interpreted, is an aid to the thinking process, rather than its substitute.

This is what Einstein—the master of experience gained via the "mental experiment"—meant when, in his study of physics (see Chapter 15), he had learned to "scent out that which was able to lead to fundamentals and to turn aside from everything else, from the multitude of things that clutter up the mind and divert it from the essential." Thus, as we shall see in Chapter 22, Einstein knew that his general theory necessarily must show the equivalence of gravity and acceleration. The proof, the inner conviction, came from a simple mental experiment: if you were in a sealed rocket ship there was no experiment that you could perform that could determine whether your weight on the floor was due to the rocket's acceleration under its onboard power thrust, or due to the gravitational pull of an external planet. This experiential thought experiment assured him that there must exist formal equations that would have this equivalence property.

I attended the 5th Lindau Nobel Meeting on Economic Sciences in August 2014 along with 17 other laureates. It struck me in listening to some of the 30-minute "mini-lectures" that one could describe few of the talks as concerned with the origins of the behavior represented. Thoughts were expressed in the vocabulary of the higher-level models presented. The models became the way of thinking. I am not saying that is wrong, only that if we seek sources of prediction error, it surely is not enough. Suddenly, sitting there listening to these outstanding scholars, I found that to be strange; that the consequences of agent action, not their causes were all that mattered. Yet, many papers were concerned with *policies that would change the consequences by acting through the causes.*

In the new experiments, the basic treatment comparison was "re-trade" versus "no re-trade," but at the close of each period all holdings disappeared, corresponding to consumption, their "utility value" realized. The aim was to isolate the effect of re-trading in the simple world in which there was no carryover of desired items from one period to the next. In the original experiments, there had been no exchange of units of "experimental money" for units of the good. Money was the measure of value and the measure of "profit" from a purchase or a sale, and prices were denominated in money. (As a buyer you profit when you pay less than the most you would be willing to pay.)

In the new version, we endowed each subject with *both cash and a capacity to buy or sell units* of a good that had a private money value equivalent (the maximum you would be willing to pay). With the trade represented as an exchange of abstract items of money for abstract items of commodity we could delay consumption, and let them be re-trade-ed in one treatment (or not let them be re-traded-ed in the other treatment) until consumption time came, at which point they disappear. Hence, we allow re-trade, or not, before the item disappears as consumption. The design introduces new sources of error, not present in the first experiments. Equilibrium might require you to hold your cash, sell some units (specialize as a seller), or hold your units, and buy additional units with cash (specialize as a buyer). Until the collective discovers equilibrium, any individual may err in either buying or selling.

Here is the problem in the new comparison experiment introduced by the re-trade treatment condition. Individuals can gain from re-trading a unit whose price rises above the amount paid, and not only by purchasing a unit for its personal consumption value. This speculative incentive to buy for resale may, and empirically does, cause resale value to rise above the equilibrium supply and demand for consumption. *Hence, speculation distorts equilibrium price discovery.*

What is the fundamental learning from this new experiment? That there is a lot of information embedded in the fact that in the world of non-durables you know before you go to markets whether you are a buyer or seller; you do not decide that based on the price! With durables, like houses and securities, a change in price may change your decision whether to buy or sell.

In the "re-trade" treatment condition, the items are freely exchangeable—bought, resold—before the period ends, at which time consumption is final for units retained by consumer-buyers. Perishability only prevents carryover between periods, and thus all profit or loss from re-trading is confined to

each period. Similarly, there is no carryover of cash. Each subject receives a replenished endowment of cash and units for resale by producers or units in demand by consumers, and the exact conditions repeated. This opportunity structure allows market value in trading to become disconnected (or not) from private consumption value within each independent period. As a buyer, you might buy two units, retain one to consume against its higher consumption value to you, and re-sell the extra unit in the active market before the period closes. Value, in exchange for consumption use, competes with market re-trading value.

Imagine, hypothetically, that you want a ticket to claim a seat to attend the Green Bay Packer game—a perishable product. The issuing price is well below the maximum you are willing to pay and is profitable for you to consume. You buy two tickets, one to consume, the other to re-sell in an active pre-game market. You sell that ticket, but the price rises above your subjective value of attending the game, so you sell the retaining ticket. The market price falls, so you buy back in and go to the game.

The purpose of the experiments is to enable us better to understand the effect of re-tradability on behavior and market performance. The purpose is not to reproduce the circumstances of any particular market, for the *circumstances may not exist*. Our imagined market is feasible for sports events, but I do not intend to describe any actual market. Examples are hard to imagine because of the nature of non-durables. As Hayek said, "The proper study of social science is the study of what is not" (*Law Legislation and Liberty, Vol. I, Rules and Order*, p. 17). In these experiments, as with many others, we study "what is not" to help us better understand "what is." We cannot come to understand phenomena in the world by always reproducing its conditions in the laboratory; rather, we ask what would occur if other conditions prevailed. If institutions change by adaptation in response to failed alternative forms then we must study the alternative forms. Similarly, we test the proposition—that the stable convergence of markets for perishables arises because they are not re-tradable—by introducing re-trading while retaining their essential end-of-period final consumption characteristic. Consequently, they are an "asset" that can be re-traded only in the interim before consumption. If we create that circumstance in the laboratory, does it interfere with equilibrium discovery compared to the standard case with no re-trade?

We repeat each period in the new experiments 10 times, with nothing carried over from period to period except participant experience, giving them an opportunity to learn and adapt their behavior. Each new period begins with the same initial endowments of cash, willingness to pay value, and willingness to accept "cost" value. In addition to the treatments,

"re-trade" and "no re-trade," there are two levels of cash endowments in each period, in a 2 × 2 design.

The comparative results clearly demonstrate that "re-trade" interferes with market performance: the efficiency of final consumption levels is reduced with re-trading, while prices and the volume of trading both increase. Moreover, higher cash endowments make the performance worse. The distortionary culprit is the prospect of gain from resale, rather than only the gains from producers selling to consumers. Experience across the 10 periods markedly improves performance, but more slowly over time when cash levels are high.

Markets are not all born equal, and the prospect of re-trade is a critical feature in distinguishing the two kinds of markets. (See my Paper with John Dickhaut, Shengle Lin, and Dave Porter, "Commodity Durability, Trader Specialization, and Market Performance," *Proceedings of the National Academy of Science*, Vol. 109, January 31, 2012, pp. 1425–1430.)

Critics of the market economy need to recognize that 75% of private product is very effectively organized and operated with no special attention to the rules governing exchange, once we have addressed property right issues of safety and truth in labeling for consumer goods. The key to understanding the success of that market order is the condition that these items cannot be re-traded. The challenge to those who understand the wealth-creating power of rule-governed markets is to better understand where and how markets can deviate from this desired result. We need better property rights for guiding incentives in the 25% of final consumer products that are re-tradable, but most particularly the production and financing of homes.

Similarly, those who voice the "free market" rhetoric need to realize that there is no such thing as a market free of governance by property rights—buyers must pay and sellers must deliver. Calls for less "regulation" must address the problem of incentive compatibility; i.e., the need to align individual incentives with the social objective to achieve stable and efficient market performance. That may imply less regulation, but also smarter "regulation" in the form of redefined property rights (Figs. 20.1, 20.2, and 20.3).

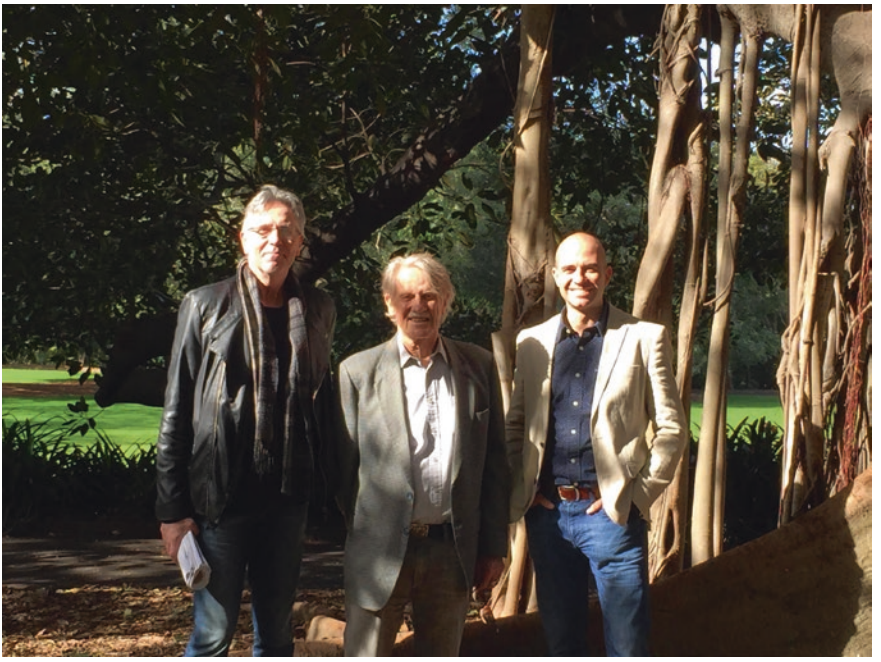

Fig. 20.1 Sydney friends Andreas left Marcos right July 30, 2015

Fig. 20.2 Vernon and Ken Arrow Jerusalem July 11, 2016

Fig. 20.3 Sydney interview July 29, 2015

21

Reconnecting Modern Economics with Its Classical Origins: Discovering Adam Smith

> Adam Smith was the first great post-Newtonian Scientist
> —Kenneth Boulding, in a seminar at the University of Arizona, 1976

The subtitle refers to discovering rather than rediscovering Adam Smith because we can't rediscover something that was never found. In this chapter, I want to discuss the Adam Smith that Bart Wilson and I found in the *Theory of Moral Sentiments* (hereafter referred to as *Sentiments*), published in 1759. (My quotations will be from the Dugald Stewart version reprinted by Liberty Fund.)[1]

At the time of his death, Adam Smith's two closest friends were James Hutton, who was a major contributor to the development of geological science, and Joseph Black, the physical chemist whose pioneering experiments established that air was composed of a mixture of different gases. Smith chose them as executors of his estate, instructing them to burn all his unpublished notes and papers. Smith, however, left open the possibility that they might publish his manuscript entitled *History of Astronomy*, which became a classic, not so much in providing a definitive history, but in its treatment of the human sentiments of wonder, surprise, and admiration in scientific discovery. Smith's decision to commit all his papers to the flames was quite deliberate. Smith had given the same instruction to David Hume

[1]The Stewart edition is freely available for downloading in the online Library of Economics and Liberty at http://oll.libertyfund.org/titles/smith-the-theory-of-moral-sentiments-and-on-the-origins-of-languages-stewart-ed.

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who was to have been his executor, but Hume died in 1776. If Smith was concerned that his unfinished work would be misunderstood, he failed to foresee that his *finished* work would be so badly misrepresented, particularly in the secondary literature inspired by his influence. In 1795, Smith's *History of Astronomy*, along with reprints of miscellaneous articles, and the first biography of Smith by Dugald Stewart were published as *Essays on Philosophical Subjects* (also published by Oxford Press). Below is the "advertisement" that his friends wrote to introduce the book.[2]

THE PRINCIPLES WHICH LEAD AND DIRECT PHILOSOPHICAL ENQUIRIES, ILLUSTRATED BY THE HISTORY OF ASTRONOMY

ADVERTISEMENT By the EDITORS

The much lamented Author of these Essays left them in the hands of his friends to be disposed of as they thought proper, having immediately before his death destroyed many other manuscripts which he thought unfit for being made public. When these were inspected, the greater number of them appeared to be parts of a plan he once had formed, for giving a connected history of the liberal sciences and elegant arts. It is long since he found it necessary to abandon that plan as far too extensive; and these parts of it lay beside him neglected until his death. His friends are persuaded however, that the reader will find in them that happy connection, that full and accurate expression, and that clear illustration which are conspicuous in the rest of his works; and that though it is difficult to add much to the great fame he so justly acquired by his other writings, these will be read with satisfaction and pleasure.

JOSEPH BLACK

JAMES HUTTON

The Adam Smith we discovered in *Sentiments* carries over into the interpretation of his much more famous book, *An Inquiry into the Nature and Causes of the Wealth of Nations* (1776; hereafter, I will call his second book *Wealth.* You can find it available online at the above liberty fund site).

At a colloquium in commemoration of Adam Smith, I delivered a lecture on "Humanomics of Adam Smith," August 7, 2012, at Old Kirk in the town of Kirkcaldy, Scotland, where Smith was born. I used the term "Humanomics" (Bart introduced me to this word; you won't find it in a dictionary yet, but in time you might) because that one word portrays the lifetime contributions of Adam Smith, in *Sentiments* and *Wealth.* After my presentation, Nick Phillipson, a recent and compelling biographer of *Adam*

[2]From an edition in the public domain http://oll.libertyfund.org/titles/theory-of-moral-sentiments-and-essays-on-philosophical-subjects.

Smith: An Enlightened Life, said to me that he had never heard anyone offer these particular observations on his biographical subject. If that be true—and trust me, Nick would know far better than I—it is only because Bart and I had read Smith against the background of questions to which we had not found satisfactory answers. Indeed, provisionally, we had thought the questions had been answered, and we were not searching through Smith for answers. We had no reason to take our unstated questions there, and harbored no expectation that better answers, and deeper questions would pop out so refreshingly from the pages of *Sentiments*. But they did. It changed my whole perspective on interactive 2-person games, and that is the background story I want to tell here. Moreover, our questions, and I believe the answers we found in *Sentiments*, provide for us a new and unanticipated perspective on *Wealth*.

My intent is to write a brief narrative on Smith's thought, as I see it and have learned from it—an essay for twenty-first-century readers. In papers and lectures, I usually like to let Smith speak in his own mid-eighteenth-century King's English, but to the unaccustomed ear that easily obscures the meaning that must be conveyed, and also the beauty, so I will not follow this rule exclusively. Also, to understand Smith's meaning, you need to have handy Samuel Johnson's Dictionary—the first dictionary of the English language (reviewed, incidentally, by no less capable a scholar than the Scotsman, Adam Smith). Along the way, I provide references with enough specificity that if you think the narrative is too inventive, I hope you root out what you think are my errors.

For motivation, I think I should start at the end, where Bart and I came out, indicating why, and working backward. The route was circuitous, but little of the nature of that exploration needs to be related in detail here. For modern economic readers, brought up in the dominant tradition that began with the neoclassical marginal revolution of the 1870s, it is essential to jettison the utility maximizing trappings of that tradition in order to understand *Sentiments*. That tradition has merit in understanding markets, as we have seen above, but it interferes with thinking about why people act as they do in their social groupings, which was what Smith was trying to understand in *Sentiments.* Moreover, utility maximization in markets in no way conflicts with the inherently social nature of people and gives it new significance. I will illustrate what I mean by asking you to consider Smith's opening salvo in his own words:

> How selfish soever man may be supposed, there are evidently some principles in his nature, which interest him in the fortune of others, and render their

> happiness necessary to him, though he derives nothing from it except the pleasure of seeing it. (*Sentiments*, p. 3)

To the student brought up on modern economics—some like to call it Samuelsonian economics—which stems entirely from the neoclassical economics modeling tradition, "pleasure" implies utility, while an interest in "the fortune (happiness, destiny, luck, fate) of others" suggests altruism. Smith was neither a utilitarian nor is he writing here about altruism, which also implies "selfless"; we need therefore to dig deeper. Indeed, for Smith, "pleasure" meant mutual sympathy. Moreover, he could not use the word ALTRUISM because it would be another century before it enters the English language, apparently as part of the process of secularizing a religious sentimentalism. When he refers to "mutual sympathy," the modern reader will appreciate that the context and elaboration in terms of fellow feeling clearly imply "mutual empathy." Again, the word EMPATHY does not enter English until 150 years later. If you infer from this that Smith's thinking was a century or so ahead of his time, I do not think this would be an exaggeration.

Of course, I am looking back and seeing all this. I do not want to leave the impression that I am saying that Smith saw this future in his ideas. Rather, he was simply interpreting the world around him based on careful and penetrating observations—a habit to which he was accustomed. His interpretations turned out to be deep and significant enough to "anticipate" (contribute to) later intellectual developments. I should add that I am surely open to the charge of seeing more through hindsight than was intended by Smith. Well, of course! Fair enough, but the charge misses a central point: You can surely say that about any prolific scholar of the past, and we lack placebo-like controls on any comparison. For me, what is important about Smith is how unique he appears in such comparisons. Other well-known and highly inventive figures of that period, such as Joseph Black, James Hutton, and Adam Ferguson—even the incomparable David Hume—are not for me quite in Smith's particular class. What is remarkable about Smith is that if you can learn his mode of thinking about human social experience, new insights begin to emerge. That mode of thinking grew on me, reading Smith against the background of decades of experimental work. He was constantly testing his understanding of social and political economic affairs against cases from history, law, theater, literature; in substance, his laboratory was all human-recorded experience with which he was familiar.

With this background, I will attempt this modern translation of Smith's opening sentence: *However selfish a person may be, there are principles of*

rule-following human sociality that govern his conduct. These principles allow him to seek the pleasures of mutual empathy and to find his deepest fulfillment in resonant fellow feeling with others. The whole of Smith's work in sociality concerns modeling the development and expression of mutual empathetic fellow feeling as he saw it in people.

Sentiments is thus a work in psychology, written a century and a quarter before psychology evolved into a field distinct from natural philosophy. More specifically, however, it is a work in *social psychology*. For Smith, there was no individual psychology, separable, distinct, and independent of our social upbringing and maturation.

Smith's vision of the individual derives from human sociability, which governs and shapes the individual who in turn contributes to that sociability.

The key ideas here are rich in metaphor and I will comment on them briefly below. Along with each summary point that I make, I will cite the appropriate references so that you can find Smith's text and enjoy the grace and rhythm, as well as the precision of his words—with occasional passages that, if I may say so, could have used additional editing. Smith's awareness of textual shortcomings is plainly demonstrated by his desire to commit unfinished work to the flames.

Smith nowhere better illustrates his conception of the role of socialization in human maturation than in what I like to call his social "*gedankenexperiment*," described in a passage in which he is explicating his persistent use of the metaphor of the impartial spectator. In this passage, he imagines a human being growing up to maturity in complete isolation from other people. Such a person could develop no idea of the merit or demerit of his conduct, or of any distortions in his social character, any more than he could acquire a sense of the deformity of his own face. Such objects he cannot see because he has no mirror in which to view them. Bring him up in the society of others, however, and that experience affords him the looking glass he needs to gain his social mind-sight. That mirror resides in the "countenance and behavior" of everyone whose space he intersects, and who invariably mark their approval or disapproval of the appropriateness or inappropriateness of his actions. Here is where the individual learns "the beauty and deformity of his own mind." It is how the individual acquires personhood. Otherwise, he would be a stranger to society and his joy (or sorrow) could not become a new source of joy (or sorrow). For Smith, a person's "own mind" is necessarily a social mind, without detracting in the least from the treasured independence of every individual—nowhere better illustrated than in Smith himself, who drew on so many sources, reworking them as he drew (*Sentiments*, pp. 162–163).

Our first ideas of what is acceptable conduct—the beginnings of moral right and wrong—are not derived from examining ourselves but from our observation of the actions and conduct of others, the feelings and reactions we have in response, and our judgments of what is deformed, acceptable, or proper (*Sentiments*, pp. 163–164).

From this knowledge and evaluation of others, we come to an intuitive operating understanding that others similarly evaluate and judge us (*Sentiments*, p. 163). In coming to see ourselves as others see us, we strive to do it as would any "fair and impartial spectator" (*Sentiments*, p. 162). The latter is better described as a third perspective—that of a judge in which we see ourselves more neutrally. I should emphasize that this "knowledge" derives from feelings, and perhaps subconscious thinking, not reason or any requirement of self-aware development. I like Bart Wilson's reference to the process as feeling, thinking, knowing. However, "thinking" need not imply conscious deliberation, and "knowledge" means "knowing how."

Smith often refers to the spectator, the impartial spectator, or in the passage above, to the fair and impartial spectator. The spectator is a metaphor for the process, whereby each of us draws on the external influence of and learning from others to achieve some measure of self-control over our expressions of self-interest (Smith would say, self-command over Stoic self-love), and thereby learn to be other-regarding and not only self-regarding in our social relationships. The ideal state of being impartial means specifically that we learn to acquire a certain measure of balance in making these judgments; otherwise, our sociability readily is marred by vanity, self-consciousness, and pretention.

The fundamental axiom in *Sentiments* is the Stoic principle of self-love—that each mature person is strictly self-interested and the most qualified to judge his or her own likes and dislikes, if not without error (*Sentiments*, p. 321). However, this axiom—the common knowledge that more is better, less is worse for each of us (non-satiation in modern choice theory)—does not lead Smith to base all individual actions on maximizing the utility of one's own outcome. That is the fundamental error in utility theory; it is why Smith was not a utilitarian.

How can individuals be both self-loving and thus self-regarding, but simultaneously exhibit other-regarding conduct toward others in their social worlds?

One easily infers from Smith's examples and propositions that common knowledge of self-love is what enables each person to judge whether, and for whom, an action is beneficial or hurtful, and to take account of those consequences in choosing to act. The action is beneficial if it awards more of

a resource (money, goods, or services) to another; it is hurtful if it provides less. The social context of an action is essential because the resulting outcomes can only acquire meaning relative to the available decision alternatives defined by the context.

Rules serve us by "correcting the misrepresentations of self-love concerning what is fit and proper to be done in our particular situation" (*Sentiments*, p. 226).

Here is an example from my personal experience that illustrates the importance of circumstances and context in the rules we follow—of Smith's concept of the freedom to pursue one's "own interest" in one's "own way." The monetary stakes are trivial, but the fitness of the rules is not trivial.

In the Tucson suburbs, you regularly see homeless persons selling newspapers on the islands at traffic intersections. Near my home resides a familiar figure, with straw hat and copies of the local newspaper, always on the same island (property rights recognized, we can suppose, by mutual consent?). I occasionally give him $5 for a $2-priced paper. However, I rarely read print text anymore, and one day I handed him $5 and said, "You can keep the newspaper." He pulled back his left outstretched hand reaching for the bill and his right hand reaching the paper to me, and gently noted that, "I only sell newspapers." Fortunately, I got the message! I immediately recovered from my mistake that had risked damaged credibility; I took the paper, he took the money, and we both smiled. Unwittingly, I had offended him. He was a businessman; he was not seeking a handout. It is ok for me to include a good-service gratuity in my payment, but it is not ok to refuse the paper. Be assured that in this exchange neither of us doubted that the other preferred more money to less. What mattered in this interaction was the context and interpretation of the exchange. In particular, I realized that his image of himself as a businessman would have been corrupted by his own action if he had accepted the money without delivering the paper. His impartial spectator examined and judged his own action knowing from experience how to see himself as others saw him, and weighing that against the loss of badly needed money. He avoided Smith's warning of the ever-present dangers of self-deceit, "the fatal weakness of mankind," and proudly maintained his credibility (*Sentiments*, pp. 223–224).

Two pillars of society govern our social actions: beneficence and justice. Both have emerged and evolved gradually through cultural change and adaptation.

Beneficence governs the intentionally chosen actions of an individual that benefit other(s), who naturally feel gratitude toward the actor, and this invokes an obligation to reward the person for her beneficent action. The outward consequence is that we observe reciprocity—kindness in response to kindness—most particularly in close-knit social groups, but more widely in stable communities. Justice derives from injustice as its complement, where

the latter concerns intentional actions that are hurtful toward other(s), provoke feelings of resentment and a desire to punish the perpetrator of the action. The strong universal negative reaction and proportionate punishment response tend to discourage hurtful actions, and through evolutionary acceptance and diffusion, such rules become the foundation of justice and constitute "the rule of law." Informally, this was the people's law, emerging by consent; formally, it was captured in the English common law with higher authority than the king.[3]

For Smith, in its origins, the roots of these social forces are in the emotions, or feelings, "in the breast." The work they do is manifest in the order, social stability, and human betterment manifest in society.

Smith's tightly reasoned monograph leads to several propositions that govern our choices of action. Here, in his own words is the first proposition on beneficence that we used to introduce the topic above:

> Actions of a beneficent tendency, which proceed from proper motives, seem alone to require reward; because such alone are the approved objects of gratitude, or excite the sympathetic gratitude of the spectator. (*Sentiments*, p. 112)

There is some complexity in the phrase "proper motives." I will translate it as meaning clearly intentional.

Unaware that we were testing Smith's proposition at the time, Kevin McCabe and I conducted an experiment in the 1990s and published it in a paper appearing at the turn of the millennium.[4] In these experiments, twenty-four pairs of undergraduates were matched anonymously to participate in the following two-stage decision process: The first-mover person in each pair, Player 1, can choose payoffs of $10 for herself and $10 for her paired counterpart, Player 2, that is (P1 = $10, P2 = $10). Alternatively, Player 1 can choose to pass her turn to Player 2; if she does, then the $20 pot becomes $40. Player 2 now chooses between two outcomes (P1 = $15, P2 = $25) and (P1 = 0, P2 = $40).

The standard self-interested model of choice predicted that Player 1 would not pass to Player 2. This is because, choosing in his self-interest, Player 2 would take all the money. Although earlier experiments had shown that people violated this principle in "trust games" with incentives similar to this one, we had deliberately designed this experiment to encourage self-in-

[3]Vernon Smith, *Rationality in Economics*. Cambridge: Cambridge University Press, 2008, pp. 192–198.
[4]See http://www.pnas.org/content/97/7/3777.full.

terested choice: not only were players guaranteed strict anonymity in their selections, but in this case, Player 2 faced two stark alternatives—take all the money, leaving none for Player 1, or split the gain 50–50. We thought the conjunction of anonymity and the prospect of losing all would seriously deter Player I from passing to Player 2 and strongly motivate defection by Player 2.

Badly misled by the standard utility analysis, we were wrong on both counts: Half of the 24 Player 1s passed to Player 2, and three-quarters of the Player 2s chose the \$15–\$25 option (P1 = \$15, P2 = \$25), rather than the \$0–\$40 split. This experiment shows the importance of stress-testing whatever it is that you think you know to determine how robust it is.

As an aside, this experiment also serves to provide counter-evidence concerning a different hypothesis that has gained a large following among behavioral and experimental economists: that individuals are predominantly averse to actions that lead to unequal outcomes—called "inequity aversion." Ernst Fehr has been a prominent pioneer of this hypothesis and has marshaled many studies confirming it. But in this trust game, observe that Player 1s choice of (P1 = \$10, P2 = \$10) is supported not only by a self-interested analysis of play, but also by aversion to inequity. If Player 1 passes to Player 2, the result is sure to be an unequal outcome. Combining the motivation of self-interest with that of inequity aversion implies predominant choice of this action. The evidence goes against these motivations implying that there are much different forces at work.

Adam Smith's proposition, stated above, provides a long-dormant prediction qua explanation of the cooperative tendencies in this trust game. Player 1 incurs substantial risk of gaining nothing from the interaction; hence, passing the choice to Player 2 rather than invoking the safer option surely constitutes, and signals, an intentionally beneficent action toward Player 2. Such "properly motivated" (see below) action invokes gratitude in Player 2, whose response is to reward the action.

Returning to the theme of the "fair and impartial spectator," we note that Smith uses the word FAIR in the sense of the sports metaphor: FAIR in the eighteenth century meant being "not foul." Smith is entirely about rules governing propriety and the consequences of breaking them; he was not thinking and writing about fairness in the sense of equal outcomes, as in the contemporary behavioral economics literature.

This is why it is so helpful, if we are to understand individual decision in terms of the sociability of conduct, to rid our accustomed thinking of the

trappings of utilitarian modeling. Eventually, there might be a case for introducing such modeling possibilities, but it will be a disservice to our understanding of Smith to do it up front for his argument was entirely different.

Smith's rule-following characterization of conduct, however, does not mean marching in time with rote formulae; an action chosen produces an outcome, but the essence of human sociality is in developing the capacity to read the meaning, the signal, and the intentions, in the action. A neoclassical utility theorist would say that the meaning in an action stems from the utility of the outcome, where that utility may depend on the outcome for both the actor and others—or "social preference." Not for Smith, he instructs us that the meaning of the action derives entirely from its circumstances, which includes the various actions that were available and could have been taken but were not. This is why it is an egregious error to attribute utilitarian "meaning" to the outcome resulting from an action. The circumstances (or context) are critical in judging the meaning and the intentionality conveyed in the action. Action, as a signal of intent, is obscure if it is supposed to reflect and be overridden by the subjective value of payoffs to others as well as the payoff to the actor (*Sentiments*, p. 134). Hence, people are not only sensitive to their own opportunity cost (foregone choice), but also to the cost to others because that is what enables the choice by others to be evaluated. To explicate these issues, consider a game in which we varied—across two sets of experiments—the choices of subjects in the role of Player 1s, while holding constant the options and joint payoff outcomes faced by the Player 2s.[5]

In each session, the pairs are randomly and evenly assigned either to Experiment I or Experiment II.

Experiment I. Player 1 chooses either payoffs (P1 = $20, P2 = $20) which will end the game, or she passes the final choice to Player 2. If Player 1's choice is to pass to Player 2, then the latter chooses between (P1 = $25, P2 = $25) and (P1 = $15, P2 = $30). As in the game above, both players see these possibilities. If Player 2 is strictly self-interested, she will choose (P1 = $15, P2 = $30). Player 1, seeing that this is the case, should not pass to Player 2. Contrary to this prediction, of 27 pairs who participate in this experiment, 17 pass to Player 2. Of these 17, 11 choose (P1 = $25, P2 = $25), and only 6 choose (P1 = $15, P2 = $30).

[5]See Http://www.altruists.org/static/files/Positive%20reciprocity%20and%20intentions%20in%20trust%20games%20%28Kevin%20McCabe%2C%20Mary%20Rigdon%2C%20Vernon%20Smith%29.pdf.

Experiment II. Suppose in the independent second game with 27 distinct subjects (no player participated in both games), Player 1 can only pass to Player 2; there exists no alternative choice opportunity, and Player 2 sees that this is the case. Now, the choice by Player 2 is predominantly the opposite of what we find in Experiment I, only 9 choose (P1 = $25, P2 = $25) while 18 choose (P1 = $15, P2 = $30).

As Adam Smith would say, the circumstance of action is what enables outcomes to convey intentions. In particular, in the Experiment I the action that Player 1 *could* have chosen, but did not, is what enables Player 2 to read the intentions of Player 1. Both players do better where Player 1 voluntarily exposes her choice to the risk of defection—precisely an action calling for reward and not defection. In accordance with Smith's proposition above, people naturally feel gratitude toward those who intentionally choose actions of a beneficent tendency. They cannot naturally feel any such gratitude in the Experiment II. This dynamic is lost in the utilitarian "social preference" model, which cannot distinguish the difference between the two experiments, except by taking into account the results and adding intentions as a post hoc parameter in the utility function. Hence, *social preference theory has only adapted the alleged utilitarian cause to the data after observing it.*

Behavioral and experimental economists did not predict the substantial levels of cooperation we observed in two-person anonymous interactions in the ultimatum, dictator, and trust games (like the one above) studied in the 1980s and 1990s. Moreover, they explored why their models failed, and among other findings learned that intentions mattered.

This tells you why *Sentiments* is so powerful; it is replete with succinctly stated core principles in the contemporary economic and social psychology of individual and small group decision—principles that have received empirical support in individual and two-person game experiments.

Many were quick to latch on to a "social preference," utility function to account for these unpredicted results. The hypothesized utility function that was maximized depended on the payoff outcomes for both oneself and the other. Hence, it could "explain" the observations. Indeed, it would have predicted that result, if we had only thought of it before. The "theory" attributed the anomaly of other-regarding action, to preferences that included an "altruistic" component of utility toward others. The question begged in this utility reconstruction of the data is how we humans come to be social and have these alleged preferences. In the above experiments, we see that the results from Experiment I can be attributed to a just so preference for a payoff to other and not only to one's self: Thus (P1 = $25, P2 = $25) must be preferred by Player 2 over (P1 = $15, P2 = $30), and therefore, according

to social preference aficionados, that is why it is chosen. But the same preference is not expressed in Experiment II, and together the results of the two sets of experiments contradict the explanation that a stable other-regarding preference accounts for other-regarding choice.

I think most experimental and behavioral economists now accept the "social preferences" hypothesis in explaining and modeling action in small group contexts. I believe it is methodologically and empirically incoherent, driven by rejection of the universality of neoclassical self-interest, but accepting its preference maximization methodology. This does not mean that there have not been important investigational findings inspired by the maintained hypotheses that human sociality is manifest in utility-motivated choice theory. My reading of the evidence is that these studies are consistent with norm-based rule-following actions that do not eliminate self-love, but control it in the interest of human social betterment. The results reported in the first trust games above do not require the subjects to have resorted to a "social preference" function, and Experiments I and II provide explicit tests that falsify any such interpretation based on the differential context and meaning attributed to actions in the two experiments.

In *Sentiments*, socialization is a process of maturation in which we learn to control our self-interested actions that are knowingly hurtful to others and to reward the intentionally beneficent actions of others; in this process, we follow rules or norms that define our group identity. The theory, based on the human capacity for moral sentiment, existed long before we started doing the experiments that falsified the model of strictly self-interested agents. We did not have to change the preference function to accommodate the experimental data. We had only to discover Adam Smith's theory and apply it to the experimental games to understand the results.

Next, I should elaborate on a related key proposition in *Sentiments*, the asymmetry between gains and losses revealed in decision-making experiments. Danny Kahneman, whose contributions were recognized with his Nobel Laureate award in 2002, was perhaps best known for establishing the empirical proposition that people will pay a premium to avoid an uncertain loss, but require a premium to be paid to induce them to accept the risk of an uncertain gain of a similar amount. The proposition predicts the results of risky choice, but more generally postulates that people act as though something given up is of greater value to them than acquiring it. I don't want to detail the evidence or the vast experimental literature and design issues involved in drawing this conclusion. I prefer rather to discuss the issue as it arises in *Sentiments.*

Smith entertains a version of this proposition as fundamental to an understanding of human emotions and conduct. Moreover, it also enters as a keystone principle in Smith's conception of property rights. Here is his clearest statement of the proposition, which is a remarkably crisp version of the Kahneman–Tversky empirical findings: "We suffer more…when we fall from a better to a worse situation, than we ever enjoy when we rise from a worse to a better" (*Sentiments*, p. 311).

For Smith, this characteristic feature of our experience with external events derives from our social experience and not only from personal health and fortune: He argues that foremost among the requirements of prudence is to avoid exposing our health, fortune, rank, or reputation to any hazard. We act to conserve our state of personal accomplishments, our reputational status, and to avoid risking their loss. "The methods of improving our fortune, which it [security] principally recommends to us, are those which expose to no loss or hazard" (*Sentiments*, p. 311). We see here how distorted and superficial is the popular image of Smith as a single-dimensional champion of individualism. He stood for individual opportunity and against privilege, as a means to personal as well as social and national economic betterment, but the individual always projected a social mind. Thus, if people were strongly prone to be averse to risking loss, this at least in part reflected a social attitude, a negative judgment by others, and accordingly people sought to avoid blame and blameworthiness through prudence.

This asymmetry in our gain–loss choices derives more fundamentally from an asymmetry between our experience of joy and of sorrow. For Smith, most people see or imagine themselves in a situation such that though "little can be added to this state, much may be taken from it…Adversity, on this account, necessarily depresses the mind of the sufferer much more below its natural state than prosperity can elevate him above it" (*Sentiments*, pp. 62–63).

Because social–psychological loss looms so large relative to gain, several important propositions follow that are critical to understanding Smith's concept of justice and the rule of law. In our cultural groups, long before the emergence of the civil order of law, we experienced and learned to resent hurtful actions improperly driven by intent. Smith accepts as axiomatic that we are all self-interested, but that does not make us grasping; indeed, this axiom is essential if we are to have common knowledge, or a sense of shared feeling, as to whether an action is hurtful or beneficial. From that common recognition that more is better and less is worse for all (self-interestedness), we learn that the resentment we feel from the intended hurtful actions of others implies that others resent our own intended acts of harm. As we grow

up, we learn to pre-screen and moderate our actions to show regard for others—to see ourselves as others see us.

Hence, through socialization we exercise more self-command over our basic self-interested predisposition. Resentment is an important defense mechanism that "prompts us to beat off the mischief, which is attempted to be done to us, and to retaliate that which is already done…" (*Sentiments*, p. 113). "Though it may be true…that every individual…naturally prefers himself to all mankind, yet he dares not look mankind in the face, and avow that he acts according to this principle…If he would act so as that the impartial spectator may enter into the principles of his conduct…he must… humble the arrogance of his self-love, and bring it down to something which other men can go along with" (*Sentiments*, p. 120). The very meaning of being social is to exercise self-command over our predisposition toward actions that serve only our self-interest.

Moreover, under Smith's guidance in *Sentiments*, the evolution of rules in culture explains the origin of property rights—the rule of law—in the civil order:

The greater the evil that is done to us by the intentionally hurtful actions of others, the greater is our sense of resentment, which in turn proportions our responses and punishments to the degree of intended harm. Thus, murder commands the greatest penalties in the civil order (*Sentiments*, p. 121). Robbery and theft take from us that which we already possess. Consequently, the resentment is higher and the punishment greater for such infractions, than for the violation of contract, which merely disappoints us of what we had only an expectation. Robbery and theft are criminal offences, while violation of contractual promises is civil offence allowing redress from damage, but is recognized as less of a breach than that of property (*Sentiments*, p. 121).

Smith understood that the original purpose of punishment for murder—intentional killing—was not focused on deterrence—that was a side effect. The purpose was to avenge the victim of the crime. We naturally sympathize with the loss felt by those close to the victim. This required the tribal or other local authorities to satisfy the family and friends of the victim who would specify the punishment levied upon the murderer. This might mean the death penalty, but instead might require specified forms of compensation in the form of wealth transfers or servitude. Only later, with the emergence of strong central governments did murder become a crime against the public and call for the death penalty. The public is "avenged" for the crime, but the main justification was to deter future violators.

Consider the text in Joshua 20. NKJV. "Appoint for yourselves cities of refuge, of which I spoke to you through Moses, that the slayer who kills a person accidentally or unintentionally may flee there; and they shall be your refuge from the avenger of blood. And when he flees to one of those cities, and stands at the entrance of the gate of the city, and declares his case in the hearing of the elders of that city, they shall take him into the city as one of them, and give him a place, that he may dwell among them. Then if the avenger of blood pursues him, they shall not deliver the slayer into his hand, because he struck his neighbor unintentionally, but did not hate him beforehand. And he shall dwell in that city until he stands before the congregation for judgment, and until the death of the one who is high priest in those days. Then the slayer may return and come to his own city and his own house, to the city from which he fled."

The prevailing practice is one in which the relatives and friends of the murdered victim are to be avenged. But God is here instructing Joshua to implement judgment procedures that make provision for distinguishing between accidental and intentional death. To do so requires a means for circumventing the standard practice, whereby a victim's family/friends rightfully, by tradition, avenge by blood. The proposed mechanism is to establish communities of refuge where an accused can be judged by a congregation (elders, judges) to determine the punishment. My interpretation is that this is an evolutionary story about law in which the new rules are more sensitive to alleviating circumstances, such as the distinction today between first-degree murder and manslaughter. The new rules emerge inscrutably out of shared experiences and are attributed to the unseen hand of God.

Finally, Smith explains why we punish acts of injustice but do not reward acts of justice: "The man who…merely abstains from hurting his neighbors, can merit only that his neighbors in their turn should respect his innocence" (*Sentiments*, p. 117).

Hence, no reward is given or expected when you stop at a red light, observe speed limits, or leave your neighbor undisturbed—these are your duty and you can hope that others, under the same system of rules, will perform their duty and be law-abiding.

The "rule of law" is an edifice built historically on identifying the set of actions that are ruled "out-of-bounds." Along with the elements in that set is an associated schedule of penalties proportioned to the severity of the hurt suffered by their intended victims. (As in Gilbert and Sullivan's *Mikado*: "My object all sublime I shall achieve in time — To let the punishment fit the crime…")

In this conception of society, we are all afforded wide-ranging freedom to move and conduct ourselves in the space of economic and social advancement and development, to enjoy the fruits of our innovations. We are

allowed to do *anything that is not excluded*—an order of rules exceptionally well geared toward discovery and innovation, as well as toward choosing a trade, craft, or professional practice in harmony with our personal circumstances and experiences. By focusing only on the foul-play boundaries, all enjoy maximum exploratory freedom. When our gains are the product of fouls that limit the similar pursuits of others, then the indulgence of the fair and impartial spectator is at an end. The offended party is a good man, every bit as good as the offender; he is fully protected from any violations of fair play (*Sentiments*, p. 120).

Some readers surely will object that the world of law defined by exclusion is long gone. As we are well aware, legislatures are prone to give truth to the expressed opinion: "No man's life, liberty or property are safe while the legislature is in session"—Judge Gideon John Tucker, Final Accounting in the *Estate of A.B._, 1, Tucker* (N.Y. Surrogate Court) 247, 249 (1866). It is true that Smith's world of negative law suffers from constant invasion in attempts at furthering positive outcomes like income or wealth redistribution—ignoring any possibility that particular forms of the policy may reduce the amount available to redistribute. These encroachments, however, are what make Adam Smith relevant for the twenty-first century. His work was driven by a compassion, very much as great as that of contemporary redistributionists, but tempered always by sensitivity to the incentive and property right rules that are necessary for achieving human social betterment through mutual empathetic fellow feeling (the subject of his first book) and economic betterment through markets and wealth creation (the topic of his second book).

Why has law emerged naturally in the form of rules that punish unjust actions? Why not have rules that incentivize just actions? The answer, following Smith's argument, is that our common experience of that which is harmful leads to common agreement on what must be limited or prevented by punishment. Such "justice" concerns consequences that will occur in the future, are uncertain, subject to unintended bad outcomes, and are subject to disagreement. Hence, they are not part of the common law tradition. (We find them in legislated law, e.g., the post–World War II public housing programs for the poor seemed to be political actions in the pursuit of "justice." But the programs failed and the policies were abandoned.)

When Smith wrote *Wealth*, he did not repeat the background history of the emergence of property developed in *Sentiments*. Implicitly, property rights were a necessary but not sufficient condition for wealth creation. Simultaneously, society also needed people who were free of the dead hand of privilege. As he stated in his *Correspondence* (p. 245):

> "That in every profession the fortune of every individual should depend as much as possible upon his merit, and as little as possible upon his privilege, is certainly for the interest of the public. It is even for the interest of every particular profession… (as genuine merit and honor rest) …on such liberal principles." (The *Works and Correspondence of Adam Smith* is published by Oxford University Press, 1987, letter #143, p. 178.)

In *Wealth*, this principle of liberty appeared, and Smith extended it in a variety of forms. There was a need to address problems in the enforcement of contracts, of slavery which Smith vigorously opposed, the need to reverse the long tradition of contempt toward commercial and industrial enterprise. There was excessive reliance on import and export duties; the problem of state granted corporate and monopoly privileges; and various entry controls over apprenticeships in the crafts.

Deirdre McCloskey[6] has emphasized that the takeoff in per capita income and wealth growth, beginning about the time that Smith was writing his first book, has accelerated down to the present and involved more than property rights. The turning point reflected a widespread change in attitude: The individual was not bound by tradition to do what family and society decreed or expected, but was free to pursue a self-actuated and self-defined destiny. My childhood neighbors well illustrated these values, even as individual families had to struggle to make ends meet.

This change in attitude, toward what the individual could aspire to become, and Adam Smith's personal views on merit versus privilege are illustrated by a younger contemporary of Adam Smith—the incomparably innovative genius, James Watt. He doggedly pursued a career of invention despite entry barriers that were effectively finessed by a liberal attitude at Glasgow University in which Adam Smith played a supportive part. His friend and fellow professor Joseph Black, as we shall see, also played a prominent part.

It was Watt's remarkable mechanical craft skills that led to his decision—a true "calling"—to qualify himself for the trade of mathematical instrument-maker. With this end in mind in 1755, at age 19, he went to London to apprentice as an instrument-maker. With some difficulty, and only by paying for an arrangement he could ill-afford, he managed to spend one year in the shop of a London instrument-maker, but he was excluded from tenure as a legitimate craft apprentice. (Interesting that while in London he avoided going into the streets at night, as young men were routinely seized and impressed into the Queen's Navy.)

[6]See her *Bourgeois Equality: How Ideas, Not Capital Or Institutions, Enriched the World*, 2016.

He then turned to Glasgow but encountered the same resistance that he had experienced in London. Being not the son of a burgess (one with full privileges, authority, or rights to town citizenship), nor having married the daughter of one, and not having been accepted into and completed a full craft apprenticeship, he was forbidden to establish his own shop in the Burgh of Glasgow. As in London, the town tradesmen, or "corporations," excluded him from membership in their club.

Fortunately, however, his unusual skills had come to the attention of Joseph Black and other professors at the University of Glasgow, whose attitude toward hiring a 21-year-old mechanical-artisan genius with no university education was not bound and gagged by conventional privilege. Here is how Joseph Black described the hiring of James Watt in his important documentary account of *The History of Mr. Watt's Improvement of the Steam-Engine*:

"I became acquainted with Mr. James Watt in the year 1757 or 1758 [it was in fact 1757], at which time I was Professor of Medicine and Lecturer of Chemistry in the University of Glasgow. About that time Mr. Watt came to settle in Glasgow as a maker of mathematical instruments; but being molested by some of the corporations, who considered him as an intruder on their privileges, the University protected him by giving him a shop within their precincts, and by conferring on him the title of Mathematical Instrument Maker to the University. I soon had occasion to employ him to make some things which I needed for my experiments, and found him to be a young man possessing most uncommon talents for mechanical knowledge and practice, with an originality, readiness, and copiousness of invention, which often surprised and delighted me in our frequent conversations together." (See *The Life of James Watt* by James Patrick Muirhead, p. 58; also Chapters IV, V and VI for more background on Watt's experiences in London and in Glasgow.)

None of this recount mentioned any role that Adam Smith might have played by the university having trumped the Glasgow corporation privileges with their own independent privileges. Indeed, no such reference is recorded by Black, although scholars and biographers, knowing his views have speculated that Smith had a part. There is, however, a link connecting Smith with Watt and his fellow faculty member Black, if you read Chapter 6 of N. Phillipson, *Adam Smith: An Enlightened Life*, on Smith's Glasgow University years, 1951–1959. Already by 1754, Smith was a respected manager of campus property. Thus, according to Phillipson, "He was responsible for rebuilding the Principal's house, in building a new natural philosophy classroom, for accommodating the Academy of Fine Arts, and for housing James Watt's workshop."

In *Wealth*, Smith added a key axiom to his development of the origin of property in *Sentiments*: The propensity to truck, barter, and exchange one thing for another. An exchange yields a price, and price facilitates comparisons. At this price, should I produce less corn and more hogs, acquiring the needed corn by purchasing it with the sale of hogs? Prices pose and solve the decision to make (at home) or buy (from other homes) through market prices

and thus represent differential skills and specialization *discovered* through trade. Why is this significant in view of the subsequent neoclassical marginal revolution? Because the latter launched a completely altered perspective that postulated an economy of *existing information* in the form of preferences, resource availability, and knowledge of technology (specialization). The function of markets became one of aggregating this dispersed information into prices, allocations that determine who specializes in what activity. What was lost for decades in this transition was Smith's idea of markets that facilitate specialization as a discovery process; the neoclassical approach did not supplement the classical model; rather, it displaced and substituted for that tradition. The neoclassical marginal analysis, down to many of its modern market forms, reversed the classical interpretation of causality.

In my personal experience, the Smith of *Wealth* was resurrected midcentury in my first supply and demand experiments, although that was not how I originally viewed them (see Chapter 11). The reality was that I unwittingly gave the subjects in my experiments a discovery tool—the sequential bid-ask double auction—that enabled individuals in a market setting quickly to receive trial-and-error correctional feedback. In retrospect, it was the double auction, as a market search process, that was the significant feature of the experiments, and not only that the participants converged to the efficient equilibrium. The latter is a corollary of the former. Economists had modeled static equilibrium in such markets, but had not been effective in articulating processes for finding equilibrium based directly on the message exchange process. It remained for Steven Gjerstad to provide a series of articles that achieved this significant breakthrough.

> Based on strictly rational individual action, game theory cannot explain why people trade. This seems strange to non-economists, so I want to discuss it, especially because Adam Smith, who also had no explanation for trade, did not find it inhibiting. But there is a literature on no-trade theorems in finance and economics.[7] The idea is that if anyone attempts to make a trade by announcing a bid to buy, or offer to sell, it must mean that he has special information. No one would accept because to do so would make them a loser relative to the fully rational equilibrium price.
>
> Before accepting the conclusion of this abstract exercise in logic, consider the first experiments I did at Purdue in the 1950s (Chapter 11). Buyers were each given a value for a unit of the item; if a unit was purchased, the buyer

[7]See, for example, https://en.wikipedia.org/wiki/No-trade_theorem.

earned the difference between her value and the price paid. Similarly, each seller was assigned a cost value and earned the difference between the selling price and its cost. People only knew their own value or cost. As in the no-trade theorem, all have strictly private information. So why does anyone make a bid or offer? It's simple, the clock is ticking and time is running. It is opportunity costly to do nothing in an environment in which you may gain for yourself by doing something. Moreover, you have complete control over the process, whereby you can guarantee that you will not be made worse off. So, a buyer knows his value and can announce a bid below that value, assuring its profitability if accepted. He can only gain, however near or far his bid is from "the equilibrium." (Of course, no one in those classes had yet learned about equilibrium.) If the bid is below all sellers' costs, none will accept, but any of them can counter with a high asking price offer and risk no loss. People trade because of expected potential gain, not because they expect optimal results. Each in so acting, however, leads predominantly to optimality over time—and end not part of their intentions. The only problem here is with non-trade theory, not the reality of what people do (including game theorists when they go shopping). This argument also applies to asset markets. However, the guarantee is only to assure purchase below your price expectations, which may not be realized, nor may ex post optimality be realized.

Adam Smith realized that because we have no explanation of why people trade, yet observe it everywhere, then we should begin with the proposition that people trade. In effect, the propensity to trade was Smith's first axiom of markets. For Smith, economics then became the study of the effects of that axiom, and his first important theorem was that specialization is limited by the extent of the market. He also believed that because specialization focuses the mind on certain tasks specialization fostered innovation.

Modeling market search processes, particularly as they apply to labor markets where search is costly, was recognized in 2010 by the Nobel Foundation's award to three theorists—Peter Diamond, Dale Mortensen, and Christopher Pissarides—other applications include housing markets. These models clarify, in a formal sense, that the rapid price discovery reported in the experimental markets was a direct consequence of the bid-ask open display and improvement rules in which search costs were effectively near zero. What makes the problem mathematically challenging is that the individual is not searching for something hidden in a stationary environment, like a pool of crude oil, or a buried treasure—conceptually easy cases, even if technically difficult. As you search in the job market, conditions and wage offers are subject to change as firms receive the information feedback from these search efforts. The models, however, cannot predict the speed of convergence any more than did Adam Smith.

From the perspective of *Wealth*, these modern examples constitute specific process models or implementations of Smith's axiom of discovery, the "propensity to truck, barter and exchange" as economics returned to these original themes armed with modern techniques. In Smith's conception, this axiom led to prices; once prices formed, whether prices were public information or diffused by gossip, individuals could more effectively make comparisons, more effectively ask whether they should grow more corn and less pigs or the reverse, depending on their circumstances. Price formation facilitated and incentivized search and the discovery of how labor should be specialized. Markets were not about aggregating a given state of information into prices, but about how price formation affected what people chose to do and to become. Hence, the basic theorem in *Wealth* that "the division of labor is limited by the extent of the market."

> "[T]he certainty of being able to exchange all that surplus part of the produce of his own labour, which is over and above his own consumption, for such parts of the produce of other men's labour as he may have occasion for, encourages every man to apply himself to a particular occupation, and to cultivate and bring to perfection whatever talent or genius he may possess for that particular species of business…[T]he very different genius which appears to distinguish men of different professions, when grown up to maturity, is not upon many occasions so much the cause, as the effect of the division of labour…" (*Wealth of Nations*, Vol. 1, Edwin Cannon, p. 17)[8]

This sequential connection from exchange (the primitive) to price formation, thence to calculation and the transformation of activity through market processes, reverses the neoclassical representation of markets. Developments in information and search theory eventually returned the focus to that causal sequence and implemented the natural order so prominently associated with Adam Smith.

I want to close with a short discussion of Smith's profound intellectual identity with what was becoming and became the "American experiment." Bear in mind that 1776 is the publication year for *Wealth.* The book was very influential with William Pitt (the younger) who served as Prime Minister of the British Parliament for over 18 years. Against the mainstream views of the British in his day, Smith opposed slavery, mercantilism

[8]See http://oll.libertyfund.org/titles/smith-an-inquiry-into-the-nature-and-causes-of-the-wealth-of-nations-cannan-ed-vol-1.

and privilege (today's "crony capitalism"), colonialism (dependence), empire (subjugation), and taxation without representation (violation of deliberative processes). These ideas, however, foreshadowed British changes in political economy over the next two and a quarter centuries.

Smith published his ideas in the form of what we would today call a "White Paper" on British government policy. The ideas appear as a response to a request by his friend, Alexander Wedderburn, who was an advisor to Prime Minister North on the government's American policy. The statement, published in Smith's *Correspondence*, which I referenced above, is in Appendix B, pp. 376–385: "Smith's Thoughts on the State of the Contest with America, February 1778."

Smith believed that if his country were successful against the American colonies, it would lead to a military government of inordinate expense to the mother country. He thought such an outcome would be improbable, but in any case the Americans world be "ten times more ungovernable" (p. 383). Smith's preference is clearly in the direction of a constitutional union. He greatly doubted that the submission of the energized Americans could be brought about only by consent through treaty even if offered advantages. That was not likely to change without military campaigns more successful than had so far been mounted. If so, and such a union were desired by the people and the Parliament, a constitutional union was not impossible. Such a plan if executed would be a boon to the prosperity and longevity of the empire. But except for a few solitary philosophers like himself such a policy had not a "single advocate" (pp. 381–382).

These considerations must have been uppermost in his mind when he wrote the closing sentences of *Wealth* and expressed the position that the existence of an empire west of the Atlantic had been entirely imaginary, an empire projected rather than achieved—a project better to be given up if it cannot be completed. The leaders of the fledgling new state in the west would have read those sentences with relish. Ultimately, that opinion turned out to be prophetic, as Smith's ideas concerning the true engine of wealth creation came to fruition on both sides of the Atlantic and beyond. Even more insightful was his proposition that the outcomes would be no part of the intention of those on the ground who would be the drivers of that reality, and that the good consequences came only so long as people did not violate the laws of justice. What Smith seems not to have anticipated is the tendency for the rules of the social order to encroach on those of the extended order of the market and vice versa (Figs. 21.1, 21.2, 21.3, 21.4, and 21.5).

Fig. 21.1 Bagpipes and Scottish color celebrate Adam Smith

Fig. 21.2 Pullig the string on Adam Simth; Queen declined in favor of an American July 4, 2008

Fig. 21.3 Me and Bobby burns

Fig. 21.4 Candace with Adam; now there is a pair

Fig. 21.5 Towering into the twenty-first century

22

Faith and the Compatibility of Science and Religion

Now faith is the substance of things hoped for the evidence of things not seen.
—Hebrews 11:1 KJV

This autobiography has frequently touched on themes in science and religion, particularly in the home and school atmosphere in which I was educated; in that world, science and religion were believed inherently in conflict. However, that stance is no longer credible, for "Science has outgrown the 'modern mistake' of discounting invisible realities…"[1]

In this parting chapter, I want to write briefly on how I see faith as it is manifest in scientific and religious experience. I particularly want to emphasize that in this exercise, I speak only as an individual searching for personal answers to ancient human questions. I think each of us has to seek our own answers in our own way while learning from the experiences of others. Truth-seeking, of course, is one of the guiding principles of Christian commitment, as well as of science.

What is faith, and how might it be relevant to both science and religion? The answer that I will build upon derives from a New Testament definition that I believe is readily applicable also to science. The Christian definition is in Hebrews (11:1): "Now faith is the substance (hypostasis—realization, being, reality) of things hoped for and the evidence (elenchus—proof, inner conviction) of things not seen." I want to examine the meaning expressed

[1]Houston Smith, *The Soul of Christianity* 2005, p. 14.

V. L. Smith, *A Life of Experimental Economics, Volume II*,
https://doi.org/10.1007/978-3-319-98425-4_22

here and indicate how I see it as applying just as forcefully to science as to religion.

As I have written in Chapter 5, my early exposure to religion was strongly influenced by the prevailing materialistic-agnostic interpretation of science at the time. My mother and her father had been attracted to Unitarianism before I was born. Unitarians tended to have a very strong scientific bent in crafting their religious beliefs, but this was the fashionable face of reason. The emphasis on science always was tempered and qualified by an inner private experience emanating from the deep Judaic-Christian and secular sources of poetic inspiration that left an influential mark upon me.

Though materialism is alive and well in the rhetoric of many scientists and other intellectuals today, I believe that the truth-seeking processes of science have undermined this belief system, making it obsolete. Indeed, a modern reading of Jesus, as reported in John, 8:32, conveys this message: "And ye shall know the truth, and the truth shall make you free." There is no inherent conflict between science and religion. Each can be at peace one with the other, although I am not predicting that they will be. Public debates, polarized on the issue of design versus a naturalistic rule-governed order particularly arise in public education, much controlled from top-down bureaucratic directives. All are expected to conform to a common policy and therefore are destined to generate heated controversy. Each side in this controversy fears that some child somewhere will have her mind permanently scarred by not being properly indoctrinated in their own version of what they consider the "truth."

It is interesting that my mother was not a believer. She also did not feel threatened by believers. I have since come to understand that people comfortable in their own search for meaning live well with those who have different views.

Returning to my main theme, the basic materialist faith was that physical science would enable us to determine the ultimate reductionist building blocks of matter, and in that discovery we would come to understand our universe at a depth that would subvert and replace any need for appealing to some spiritual or mystical entity to comprehend human existence. This personal expectation was implicit in my naïve childhood belief that everything would become knowable once you became an adult. I had yet to learn that along with the answer to any question came a host of deeper questions created by the answer. This fundamental property of our knowledge is revealed by the fact that in any epoch a child can force you to the outer limits of knowledge by asking "Why" a few times in a row. This questioning and

probing to find deeper levels of comprehension is unique to neither science nor religion, but is a manifestation of human curiosity and a characteristic feature of both.

In my view, this materialist conception of the universe started to unravel with two of Einstein's four famous 1905 papers, both deserving discussion: one on the special theory of relativity (originally entitled, "On the Electrodynamics of Moving Bodies"); the other on the photoelectric effect. The first paper concerned the equivalence of energy and matter (the equation was actually derived in still another of his famous 1905 papers), leading both to a revolutionary new understanding of physics as well as to practical nuclear engineering. The paper also led to Einstein's 1916 general theory of relativity, which reinterpreted our concept of gravity, space, and time and later formed the basis for the cosmology of an expanding universe starting with the Big Bang of creation. Today, these relativistic extensions of Newtonian physics are straightforward modifications of classical physics and live entirely embraced within the substance of the classical paradigm. At the time, these extensions were counter-intuitive; our intuition could not break free of our commonsense experience that space and time had independent existences.

His second paper of 1905, "*On a Heuristic Viewpoint Concerning the Production and Transformation of Light*" established that energy came in discrete packets that are governed by a form of probabilistic uncertainty; this paper was the one cited when he won the Nobel Prize in 1921, and jumpstarted the field of quantum mechanics. As the very articulate Einstein, with his characteristically intuitive simplicity, put it "…when a light ray is spreading from a point, the energy is not distributed continuously over ever-increasing spaces, but consists of a finite number of energy quanta that are localized in space, move without dividing, and can be absorbed or generated only as a whole."[2] Later, it was discovered that the new break-through quantum physics implied a reality of "spooky action at a distance" that Einstein could never accept, although it defined a new instrumental reality that eventually received experimental support many times over. He saw quantum physics as only provisionally correct. He thought the theory required modification to become more "complete." Others, unrestrained by the intuitive

[2]https://einsteinpapers.press.princeton.edu/vol2-trans/100#,#.

unreality of the new physics, would carry us into a new world that, by all previous standards, was quite mystical.

In 1929, Hubble's observations revealed that the stars and galaxies of the universe are expanding in all directions and at velocities that increase in proportion with their distance from us. The most prominent implication was that our space-time universe had a single region of origin. Originally, the idea had been proposed by the Catholic priest and accomplished physicist, Lemaître in the year of my birth (1927), but in 1949 the astronomer Fred Hoyle dubbed it appropriately, "The Big Bang," a label that stuck. For the next thirty years after Hubble's observations, scientists were resistant to the idea that all matter and energy in the universe must have once emanated from a particular historical region in space-time. Mathematicians called it a "singularity," massive compared with the singularities sprinkled in all directions throughout the universe like Swiss cheese, and associated with local imploded stars or black holes.

Why this resistance? I think it emanates from the Newtonian idea that the universe had always existed, which seemed more psychologically comforting and natural—no beginning, no end. If there was a beginning, then science—the search for truth in physical phenomena—had to face up to the psychologically overwhelming fact that before the beginning there was nothing: no matter, no energy, no space, and no time, just a monstrously pervasive nothing! I am using "nothing" here in the sense of classical physics, not necessarily in the sense of quantum physics, which, as I will discuss, is alive with creative activity. But if the universe had always existed, then it seemed that there was room aplenty for Einstein's impersonal God, the Deism of natural rules, order, and beauty, to say nothing of agnosticism and atheism.

Our ancestors had understood their world in terms of Genesis (1:2). Before creation "…a formless void and darkness covered the face of the deep…," while in our day, we have come to understand our world, technically, as originating at a massive singularity at which the equations, charting everything from stars and dark matter to particles, have no finite solution.

The ancient question of human existence—"Why is there something rather than nothing?"—could be avoided, if this something that we observe everywhere was thought to have always been. The belief is in direct contradiction to Genesis and to Hebrews, where it is stated that, "By faith we understand that the universe was ordered by the word of God, so that what is visible came into being through the invisible." But the new question for science, implicit in the Big Bang theory, "Why was their nothing that became something?" seemed to deepen the state of our ignorance and mystery. This is because the mystery of origins is beyond any conceivable science

and the whole apparatus of hypothesis testing. Creation—that is, the beginning—could be located in history, and in the limiting state of equations, proven repeatedly to have enormous experimental and astrophysical predictive power in locating events in our observable world of space-time, energy, and matter.

At its best, these developments, and those in quantum physics, are embarrassing for classical materialism. That the materialist rhetoric has changed little, tells you how deep its belief system has penetrated.

Stephen Hawking (*A Briefer History of Time*) allows that physics has put a new twist on these interpretations. The Big Bang began in an initial state that is not contained in Einstein's general theory of gravity. (Projecting the observed expansion backward, general relativity theory appears to break down at the beginning boundary of our experienced world.) Hawking asks how, if God chose the laws of nature, he managed to choose the initial state. By implication, Genesis has the same problem. The remedy offered in quantum theory allows space-time to be finite but with no boundary singularities, as on the surface of a billiard ball or doughnut. That representation requires a few more dimensions that we cannot translate into experience—I started to end this sentence with "intuition" then realized that we cannot account for intuition itself inside the box of our world experience; intuition must have higher dimensional representations, access to which cannot be credibly proven.

On March 23, 2015, I received a booklet from the Caltech Alumni Association announcing "Reunion Weekend" May 14–17. Kip Thorne was listed among those receiving Caltech's highest honor: the Distinguished Alumni Award.[3]

The usual number of high-quality lecture demonstrations, ranging from biology to seismology, was featured on this Seminar Day, with special emphasis on topics in basic physics. Clifford Cheung scheduled to speak on "Is Our Universe Fine-Tuned?" fits well the content of this chapter. I summarize his abstract:

Why have the fundamental constants take on the particular values observed? Scientists have long sought an answer to this question. Most recently, they have celebrated the discovery of the Higgs boson particle, as predicted by the Standard Model that describes all we observe in our world. "However, with this discovery comes an even greater puzzle that defies long-standing and cherished principles. In particular, the fundamental constants of nature appear to be exceptionally fine-tuned—and we do not know

[3]http://www.wsj.com/articles/the-weekend-interview-finding-our-place-in-the-stars-1416005859.

why" (Alumni Reunion Weekend, Caltech Alumni Association, May 14–17, 2015, p. 21).

If the universe did not exhibit the particular values we observe for the fundamental constants, then it follows that we would not be here to make the observations—the anthropic principle. The so-called fundamental constants—like the speed of light, the charge of the electron, Plank's constant—are in a sense not "pure," that is, they have dimensions of measurement; e.g., we measure light's speed in terms of length traversed per unit of time. So what does it mean to say they are constant? Essentially, that they do not change in the process of observing certain physical interactions. I would argue that we can imagine, in theory, a more general model of multiple universes that yields one with such constants as predicted observations, rather than pivotal crutches in our approximate accountings for the world of experience. What Cheung and the physicists call a "puzzle," others call God.

But what is this thing we call experience? Beyond science is a personal experience shared by all humans, a sense of the awe and mystery of existence. For me, for all humans, this experience must count as an observation, even if it is incommensurate with our rhetorical vision of the objective tests of science. The power to inspire awe is expressed magically, in Carruth's moving lines:

Like tides on a crescent sea beach,
When the moon is new and thin,
Into our hearts high yearnings
Come welling and surging in,
Come from the mystic ocean,
Whose rim no foot has trod,
Some of us call it Longing,
And others call it God.

Materialism ignored any references to experiences of awe and mystery. Such experiences are ruled out of consideration—not representing admissible observations, we do not have to account for them, much as we did not have to account for a beginning if the universe had always existed. What is not an observation cannot enter this worldview. Kahlil Gibran may have had such dismissals at heart, in his book, *The Madman* (1918), where he writes: "…we heard a voice crying, 'This is the sea. This is the deep sea. This is the vast and mighty sea.' And when we reached the voice it was a man whose back was turned to the sea, and at his ear he held a shell, listening to its murmur. And my soul said, 'Let us pass on. He is the realist who turns his back on the whole he cannot grasp and busies himself with a fragment.'"

Science has chosen to become more and more deeply informed on a fragment of the total human experience, with most scientists believing that the fragment constitutes, or comprehensibly represents, the whole. That belief is unshakable when coupled with the belief that contrary experiences do not count as observations. What is paradoxical is that the success of science has depended on the emergence of scientists with an open mind to phenomena, yet areas formerly seen as contaminated by belief error are not fit for openness.

It is possible that what I call "a sense of awe and mystery" is not a human universal shared by all people. If there are people who do not have such inner feelings, then it is not for them an observation that they are refusing to count as such. I did some searching online on this theme to see if I could come up with an explicit denial of such feelings by one of the well-known atheists. I found exactly the opposite from no less a central figure than Richard Dawkins! In an interview, he was asked whether he has ever felt a divine presence associated with inspiring music, or when in a church.

In his response, he said he preferred to avoid the "divine" description as it is easily misunderstood. Thus, Einstein, who was a spiritual person, did not accept belief in a personal God; rather, he subscribed to Spinoza's belief that God was nebulous. Dawkins says that when he thinks of his feelings of wonder about the physical universe, he does indeed have such a sense of wonder, but he "wouldn't call it 'divine.'"

"[W]hen I listen to Schubert or look at a great cathedral or look at the Grand Canyon, I do get a feeling which is probably akin to what religious people feel when they experience what they call a mystical experience." Dawkins, however, does not think there is anything supernatural about these feelings. "I think it's all going on in my material brain. But I wouldn't wish to be upstaged by a religious person when it comes to my ability to feel an emotional response to something like a beautiful piece of music or a beautiful object."[4]

This last quotation is almost tailor-made for my point for this whole chapter, for this entire book. Dawkins states that what "they call a mystical experience," is not for him "anything supernatural…I think it's all going on in my material brain."

No kidding, Richard, and tell me, please, what is this thing you call your material brain that allows you to have what others call a mystical experience, that you do not want to be upstaged by, and that you do not find offensive so long as you can choose your own word descriptions? You have no evidence, only faith in your righteousness. Like the snake swallowing its own tail, I think your verbiage has overtaken you.

Some call it a mystical experience, and others call it God. Those who are among the "some" and the "others" are all our neighbors to be loved.

[4] http://www.salon.com/2013/09/29/richard_dawkins_im_not_like_christopher_hitchens/.

What spooked Einstein about quantum theory is illustrated by the discovery that two quantum particles could interact instantaneously no matter where they were located. In experiments in Switzerland, such particles are studied at a separation of 18 kilometers! (*Nature*, August 14, 2008) Thus, if one particle is perturbed, there is an instantaneous synchronous effect on the other. This seemed to violate special relativity by allowing physics to embrace speeds greater than that of light. The best verbal description that could be mustered was the concept that two such particles are entangled—the term introduced by Schrödinger in 1935—a phenomenon subsequently found repeatedly to be consistent with indirect experimental observations. The common origin of the particles—a kind of twinning—binds them in a relationship that defies the logic and intuition of Einstein's new classical physics. But let me here emphasize that all perception, and all scientific observations, are indirect and are therefore necessarily, the "…evidence of things not seen," as in Hebrews (11:1).

Thus, even if quantum theory is "incomplete" and in some sense due for improvement, scientists now have faith that quantum-spooky interconnectedness is not going to be replaced because it is integral to the phenomena. Indeed, what does it mean to say that two entangled particles are subject to simultaneous effects? It actually means that the time required by any postulated signal passing between the two particles is below the detection limit of the instruments. What the Swiss experiments show is that any supposed signal passing between the entangled particles must be traveling at least 10,000 times the speed of light!

I have used above the phrase, "objective tests of science"; this carries the ring of "reality" to it, but it is a rhetorical distraction. "Reality," when penetrated by new instrumental probes, is never what it seems in our experience of the world, and no one was a greater champion of this principle than Einstein. For example, he used the term "ponderable matter" in contrasting elements of classical physics with his new space-time physics. His general relativity theory created an interpretation of space—a mathematical equivalence—that curved back on itself in a four-dimensional space-time continuum. Einstein had long known that the theory had to show that gravity and acceleration were indistinguishable. A simple mental experiment gave him the proof, the inner conviction, and the evidence that this was a truth. Imagine that you are in a sealed chamber within a rocket ship unable to observe anything outside of it. You feel and can measure your weight or force against the floor. You can perform no experiment that can distinguish whether you are accelerating under propulsion in empty outer space or pulled by the gravitational force of a planet.

Truth must come packaged in the form of experience. Experience can be imagined, based on mental constructs available from internal mental information, or it can be a construct built from external stimuli received through our senses. If your mind cannot tell the difference between internal and external signals, you may be afflicted with schizophrenia, but otherwise seem entirely "normal." (As Lily Tomlin once asked, "Why is it that when we talk to God we're said to be praying, but when God talks to us we're schizophrenic?")

Incredibly, as Einstein and others would show, the space curvature of general relativity allows for the theoretical possibility of "wormholes" through which various points in space are accessibly connected by shortcuts that, if traversable, do not violate special relativity but simply bypass it. In particular, gravity is not instantaneous, but is mediated by a finite signal which, through the warping of space-time, travels only at the speed of light. Keep in mind that we are talking of a theory, some implications of which have survived experimental tests of certain of its predictions, at least where observations could be brought to bear, beginning with the eclipse experiments of Eddington in 1919. The wormhole solution has remained speculative without observational support, and its main influence has been on science fiction. Yet, the equations of the theory exist whatever our attempts to interpret them. A new hot topic in theory seeks to link wormhole relativity with quantum entanglement and connects to "multiverse" (parallel universes) interpretations.[5] Some call it physics; others call it God.

What is mind-boggling—Feynman said that nobody understands quantum mechanics—is that uncertainty, unpredictability, is built into the micro-foundations that support all that we experience. In physics, we model relationships, changes in the position and momentum of a mass. The state of a billiard ball is entirely determined when we measure its position on the table and its momentum—mass times velocity, where speed can be determined by a radar gun. Our sense experience, leveraged with instruments, is that of a determinate position and momentum for the ball. But at the micro-level of measurement, applied originally by Heisenberg to a particle, we find an unresolvable uncertainty originally not thought to be shared by the billiard ball example. As Heisenberg proved, there are natural uncertainty limits on our ability independently to measure the position and momentum of a particle. At the micro-measurement level, the more cer-

[5]See http://www.livescience.com/41639-quantum-entanglement-links-wormholes.html.

tainly with which we determine position, the less certainly we determine momentum. The product of these uncertainties (probabilities) is constrained numerically by a value proportional to a universal constant, a contribution of Max Planck. The product of these probabilities cannot be smaller than h/4ʌ, where *h* is Planck's constant. This is because at the instant the particle's position is known; the photon (radar-gun-like) measurement process discontinuously jumps the level of the particle's momentum. Hence, at the instant that the position is known as the particle's momentum can be known only up to the level corresponding to that discontinuous jump. This is a consequence of the fact that any measurement device must use the electromagnetic spectrum to record the observed object; in this sense, our observation must influence the thing we observe.

So what? Isn't this only at the particle level, expressed by wave equations? Not quite. Any mass has an equivalent wave function representation. The propagation of that billiard ball has an equivalent wave-energy description. Measuring its position and momentum encounters exactly the same uncertainty as a particle *at the Planck level of smallness*. We are talking about what happens when we examine things at very micro-levels, whatever the thing might be. The principle is not altered by our single-minded insistence on ignoring small effects at billiard ball macro-levels. Think of there being a thin layer of quantum active support everywhere, underlying all that we experience and perceive in the world and that reality is beset with quantum unpredictability. This is the way that Nature—others call it God—works.

The world of our sense experiences is big enough that we are far, far above the Planck layer of smallness; simultaneously, our world involves speeds that are tiny with respect to the velocity of light. The latter property allows us to live blithely unaware that with increased speed masses grow heavier, clocks run slower, measuring rods shrink, space and time is space-time. Einstein, spooked by the implications for physics of Planck smallness, felt at home from the start with relativity, which spooked other physicists for decades. Einstein won the relativity battles with others, but not the quantum battle with himself.

Contemporary theorists have learned to take quantum incredibility at its equation value, if not at its face value. In less than a hundred years, after the special theory and the photoelectric effect, we encountered engineering miracles like atomic energy, all electronic devices, and lasers; the field of chemistry, and therefore biology, all rooted in that thin layer modeled by quantum theory. Therefore, we should not shoot from the hip in rejecting entangled objects and teleportation as the stuff only of fantasy science fiction. Indeed, teleportation in the sense of information transfer has been achieved in atoms

and molecules. At quantum levels, if you have copied all the information in an object, you have teleported that object. These fairytale-like stories are now serious physics and within the framework of contemporary science; some lead nowhere, yet others lead to breakthrough observations and devices.

The point I want to emphasize is that science is about physical and biological mechanisms; about discovering how things work; about engineering; and about theories that describe and can predict observations that we experience entirely through our senses or their extension through instruments. The instruments of science supply us with the indirect "evidence of things not seen." It's like Plato's Allegory of the Cave in which reality can only be experienced as shadows on a cave wall. An experimental physicist says that he measures the "spin" of an electron, but in actuality, he records certain effects on a screen and uses the theory to calculate its meaning as a measurement. Carl Anderson discovered the positron, but had actually photographed a streak in a cloud chamber (a bent trace in measured space caused by its energy), which was an implication, or predictable consequence of the theory. Science keeps getting better, in this instrumental task, on a scale beyond anything believed possible in 1905, let alone at the beginning of the Christian era.

In science, we observe nothing directly; only indirectly through instruments that record the secondary effects implied by conceptual models of objects—particles, waves, energy—whose postulated existence is not violated by our indirect observations. But you cannot derive the existence of those objects and the richness of the theory from the sparse indirect effects and the measurements we record—theory is resolutely under-determined by observation. You can only do the reverse: deduce from those constructed objects and models their implications for what we can expect to observe. The constructs of those theories come from mysterious flights of the imagination, from scientific intuition buttressed by mathematics. That under-girding structure, the theory, and the hopes that drive imagination are things you accept on faith, whose believability is reinforced by the occasional tests that one is lucky and ingenious enough to perform. Eventually, that faith is disturbed by contrary observations, or by a more comprehensive construct, able to account for new shadows on the cave wall of our experience.

Hence, it is inescapable that science is dependent on faith. The conceptual and theoretical constructs of science constitute the "substance of things hoped for" whose evidential support depends on instruments that record the "evidence of things not seen." As Einstein once said, "It is theory which first determines what can be observed." But prior to theory, there is what

we call "thinking"—a systematic form of consciousness, deeply driven by the unconscious that enables understanding and experimental predictions. The parallel is expressed in John (1:1), "In the beginning there was the Word, and the Word was with God, and the Word was God." For humans, all beginnings are in thought or reason—the Word.

This unseen reality of theory brings an operating understanding of how our world works and enables us to accomplish engineering miracles by trial, error, tinkering, and adjustment. Who living 100 years ago could even imagine the material marvels that would exist routinely today?

Science, however, cannot identify, nor can it disprove, purpose. Some prominent scientists and philosophers have claimed—seemingly with some intemperance—that science shows that there is no purpose in the universe. The denial of purpose goes hand in hand with the denial that a sense of mystery is an admissible observation. However, failing to find something does not allow one to conclude that it therefore does not exist. Scientists embrace this proposition wholeheartedly in space explorations, in the search for extraterrestrial intelligence (SETI), and in the various Mars probes a hot research topic is, and will be, whether there is evidence of life in some simple cellular form. Failing to find any smoking-gun evidence for life has not deterred continuance of the search, nor suppressed belief that life can be generated spontaneously from unfathomable physical processes. Scientists therefore accept that failing to find evidence of extraterrestrial life or intelligence does not prove their non-existence.

In religious matters, however, scientists tend to identify the lack of observable evidence for purpose as supporting the proposition that there is no purpose. Religions everywhere have sought to comprehend a universal purposeful human experience: a longing born of high yearnings that come welling and surging in, that indeed come from a mystic ocean on whose rim no foot has trod.

The ancients confounded their mystical experience and religious faith with explanations of everyday events. Science has invaded that everyday world of explanation and created marvels out of the new understanding of how things work. Ancient peoples believed that divine forces governed the formation and movement of clouds. Science demonstrates that clouds are a precipitate of water vapor under specified conditions of temperature, pressure, and saturation. The scientific explanation of a cloud does not make it any less mysterious or less "divine." The mystery is pushed to a more fundamental level. The success of science cannot be arbitrarily projected beyond the bounds of what science is capable of investigating. Scientists overstep their bounds when an illiberal attitude opposes the study of paranor-

mal, out-of-body, near-death, and other such testimonial experiences. Our accelerating ability to explain the workings of complex systems creates an illusion of comprehension, of control, even understanding, but it leaves in place the gap between engineering and purpose. That illusion is what maintains a wedge between science and religion. The scope of the engineering expands while deepening the mystery of meaning. Better, deeper, and ever more knowledge of how—consistent with but commonly not predicted by theory—does not answer why. Moreover, Quantum Theory models the ultimate phenomena that are simultaneously beyond our capacity to experience in space-time, and from which that space-time world demerged: Twentieth-century science has brought the search for reality face-to-face with the mysticism that it rejected.

An important new learning experience for me, beginning in 2007, has been my explorations with Candace of Christian history and theology. That history is largely in the form of the narrative literature from Acts, the letters of Paul, the four Gospels, and the other apostles. The NIV Archaeological Bible and the Teaching Company lecturers, Philip Cary, Luke Timothy Johnson, and others have been an important supplement to that reading. Biblical literatures are not histories, but discounting them on that basis is like denying that Shakespeare can express truth because it is narrative, fictional, and metaphoric. Truth comes from experience, and the varieties of experience come richly adorned in metaphor. The reliability of ancient text—often questioned—was tested with the discovery of the Dead Sea Scrolls that included a complete and earliest known copy of Isaiah, dating to 125–150 BC; the oldest copy at the time dated only from 1009 CE. Jewish scholars had added vowels to the original text from 500 to 1000 CE which had been handed down in oral traditions long before it was written. The two Isaiah written texts, separated by over 1100 years, differ only in minor words and spelling.

It was not until the second century CE that the new religion would begin to leave historical and archaeological footprints of its presence beyond the testimony of its leaders. The narratives, however, and the subsequent history into the fourth century CE, make it plain that Christianity grew at an astounding rate. Moreover, in its formative years, the movement was entirely decentralized, as the apostles and other followers of Jesus attracted a constituency throughout the Mediterranean area, with some Jewish adherents but primarily with Gentiles. The leaders in this movement were writers and readers of scriptures (and of letters interpreting scripture) to congregations, as were the Jews from whose heritage they had originated. Soon, scholars translated the Gospels into other languages of the region.

Ironically, insight into why the movement had so much appeal came from the writings of Celsus, a pagan critic of the Christian sect, who sneared that it is "a religion of women, children and slaves." These sub-groups were of the "ignorant classes" who had no social or political standing in Roman or Greek cultures. Christianity was a persecuted sect, neither Jewish nor pagan, with a reputation for troublemaking, whose leader suffered the most demeaning of all deaths. Since Jesus's ministry was most concerned with the "poor in spirit," an answer to the puzzle of Christian growth was that the sect appealed to "women, children and slaves" who were without institutional recognition in non-Christian cultures and were poorest in spirit. Particularly, powerful must have been the message of Resurrection that came from witnesses, and the converted persecutor, Paul. Parousia—the belief that Jesus would return to give life in a new world to come and that the poor would have their day—was surely part of the appeal, and sufficient for many of the faithful to sell their possessions and share them with the poor. The poor, without inheritance in this world, had unprejudiced access if not preference in the afterlife. Paul preached Parousia, although as time wore on it was harder to maintain that the Day of Judgment was near at hand. Since Jesus never dated His return, this was a belief without any scriptural foundation and Christian theology came to reflect this reality, while maintaining a sense of witness.

Today, people look back on Paul and see him as hostile toward women. In 1 Corinthians 14:33–34 Paul said: "For God is not a God of disorder but of peace—as in all the congregations of the Lord's people. Women should remain silent in the churches. They are not allowed to speak, but must be in submission, as the law says." In her book, *Paul Among the People*, 2010, Sarah Rubin asks, concerning women, "What are they doing in the churches?" In the surrounding Roman and Greek cultures of the day, women were not free to attend public meetings or to express opinions, as were men. Hence, given Paul's audience and environment, far from degrading women, he was pushing the edge for the people of his time, but advising caution in not attracting a crackdown; he was "being protective rather than chauvinistic" (see Rubin, Chapter 4).

I want to close by returning to my claim that our sense of mystery should count as an observation consistent with the religious faith of our fathers. Christian theology is, and has been, the counterpart to theory in science. Our experience of the Spirit is our evidence. In religious discussions, we compare notes on that experience and thereby give it an inter-subjective (qua objective) standing. Similarly, when confronted by new observations, scientists ask each other, "Do you see what I see?" The answers are commonly

controversial for extended periods but, in time, the discussion may settle on provisional agreement—an equilibrium. I have quoted Carruth, but I will draw now on some assorted quotations from the book by Kahlil Gibran, *Jesus the Son of Man, His words and His deeds as told and recorded by those who knew Him*.[6] It is a work of historical fiction; a series of characters, from the bible and the surrounding cultures, who speak from their experience in knowing Jesus directly or through the testimony of others. Gibran's narratives are richly poetic and have an appeal that reflects his remarkable mystical inspiration nineteen centuries later. Gibran uses his narratives to invoke a sense of witness to Jesus through the lives He touched in His brief visit.

From *Pontius Pilatus: Of Eastern Rite and Cults:*

Then I left them and went back into the Judgment Hall again, and I saw Him standing there alone, and His head was still high.

And I remembered… that a Greek philosopher said "The lonely man is the strongest man." At that moment the Nazarene was greater than his race.

…And I asked Him again, "Have not you said that you are the king of the Jews?"

…Then He answered with a quiet voice, "You yourself proclaim me king. Perhaps to this end was I born, and for this cause came to bear witness unto truth."

Behold a man speaking of truth at such a moment.

In my impatience I said aloud, to myself as much as to Him, "What is truth? And what is truth to the guiltless when the hand of the executioner is already upon Him."

Then Jesus said…, "None shall rule the world save with the Spirit and truth."

And I asked Him…, "Are you of the Spirit?"

He answered, "So are you also, though you know it not."

From *Claudius: A Roman Sentinel:*

After he was taken, they entrusted Him to me. And I was ordered by Pontius Pilatus to keep Him in custody until the following morning…

At midnight I left my wife and children and visited the arsenal…where He was held…

My soldiers…were making sport of Him…stripped Him of His garment, and… put a crown of last year's brier-thorns upon His head…they were dancing and shouting at Him…

I stood before Him…and I was ashamed. I knew not why.

I had fought in Gallia and in Spain, and with my men I had faced death. Yet never had I been in fear, nor been a coward. But when I stood before that man and He looked at me I lost heart…I could utter no word.

[6]Enjoy free access at http://gutenberg.net.au/ebooks03/0301451h.html.

And straightway I left the arsenal.

This chanced thirty years ago. My sons…are men now. And they are serving Caesar and Rome.

But often in counseling them I have spoken of Him, a man facing death with… compassion for His slayers in His eyes.

And now I am old. I have lived the years fully. And I think truly that neither Pompey nor Caesar was so great a commander as that man of Galilee.

And, from *Rachael: A Woman Disciple:*

And Jesus was often alone. He was among us yet not one of us. He was upon the earth, yet he was of the sky. And only in our aloneness may we visit the land of His aloneness.

From: A Man from Lebanon
Nineteen Centuries Afterward
… Your friends are yet with us for comfort and support,
And your enemies also, for strength and assurance.
Your mother is with us;
I have beheld the sheen of her face in the countenance of all mothers;
Her hand rocks cradles with gentleness,
Her hand folds shrouds with tenderness.
And Mary Magdalene is yet in our midst,
She who drank the vinegar of life, and then its wine.
And Judas, the man of pain and small ambitions,
He too walks the earth;
Even now he preys upon himself when his hunger find naught else,
And seeks his larger self in self-destruction…

And the woman who was taken in adultery,
She too walks the streets of our cities,
And hungers for bread not yet baked,
And she is alone in an empty house.
And Pontius Pilatus is here also:
He stands in awe before you,
And still questions you,
But he dares not risk his station or defy an alien race;
And he is still washing his hands.

Finis

See Figs. 22.1 and 22.2.

Fig. 22.1 Vernon and Candace, Christian Marriage Anchorage

Fig. 22.2 Sam Gregg and Vernon Acton Institute

Deliverance

Live, and seek to be the
Agent of change you would see in the world; the
Beauty you would want for this world; the
Child you would hope for this world; the
Deliverance sought in this world; the
Forgiveness so rare in this world; the
Gift you would make to this world; the
Heart that stands out on either side; the
Jesus who joins two or three who gather in His name; the
King who would rule with neither scepter nor sword; the
Love the world is a-need'n; the
Mother or Father missing to someone; the
Narnia of every child's dreams; the
Opportunity each might seize; the
Peace that He prayed be with you; the
Quiet mind, courage and gaiety for it is within reach; the
Rebirth you would wish for the world; the
Song of Songs for the day; the
Truth that history will never write; the
Unmasked who survives madness; the
Value that exceeds gold and diamonds; the
Work that is Love made visible; the
X that names the unknown; the
Yearning that some call human, others call God; and the
Zen that wells from the depth of the soul.

Author Index

A
Akerlof, George 91
Anderson, Carl 227
Anderson, Mildred 117
Augier, Mie 92

B
Bair, Shelia 161, 176
Banks, Jeff 60
Barry, William W. 68
Bénabou, Roland 92
Bennett, Jack 55–57
Bernanke, Ben 148, 171
Binmore, Ken 95
Black, Joseph 189, 190, 192, 205, 206
Block, Mike 69
Born, Brooksley 159–161
Borts, George 10
Boulding, Kenneth 189
Bowles, Sam 11
Brazelton, T. Berry 125
Buchanan, Jim 98
Bulfin, R. 60

C
Call, Jim 128
Camerer, Colin 64, 92, 94, 97
Cannon, Edwin 209
Carruth, W.H. 222, 231
Carter, Jim 107
Carter, Jimmy 66, 107
Cary, Philip 229
Cason, T. 65
Cessna, Clyde 17
Chamberlin, E.H. 89
Che Tsao 56
Cheung, Clifford 221, 222
Clark, Ed 66
Clinton, Bill 151, 159
Clower, Robert 66
Coase, Ronald 46, 97
Coppinger, Vickie(nee Sandler) 48, 51, 61
Coursey, Don 48, 51
Cowen, Tyler 147
Cox, Jim 10, 62, 65, 67
Crane, Ed 66
Croft, Chauncey 109, 110

V. L. Smith, *A Life of Experimental Economics, Volume II*,
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Cunningham, Jennifer 148

D
Dalton, George 10
Davis, Lance 32
Dawkins, Richard 223
Diamond, Peter 208
Dickhaut, John 145, 149, 185
Didonato, Joyce 175
Doti, Jim 145, 147
Dumas, Cyndi 148

E
Eddington, Sir Arthur 225
Edwards, Ward 94, 95
Einstein, Albert 49, 50, 182, 219, 220, 221, 223–227

F
Fehr, Ernst 92, 197
Ferejohn, John 32, 90
Ferguson, Adam 192
Fiorina, Mo 31, 32, 35, 90
Fouraker, Larry 89
Fouraker, Lawrence 89
Franklin, Ben 79, 81
Frederick, Bastiat 46
Friedlander, Lanny 66
Friedman, James 89
Friedman, Milton 52, 53, 157

G
Gentry, Kevin 147
Gibran, Kahlil 102, 222, 231
Gilbert, Dan 92
Gintis, Herb 1
Gjerstad, Steve 65, 146, 149, 152, 180, 207
Grandin, Temple 6
Greenspan, Alan 66
Grether, David 59, 60
Grey, Bob 8
Guth, Werner 95

H
Hall, Jane 120
Hansen, Alvin 164
Harkleroad, Joyce 99
Hawking, Stephen 221
Hayek, F.A. 46, 58, 99, 101, 184
Hayes, Samuel 57
Hazlett, Tom 97
Heisenberg 225
Heiser, Bob 31
Herbert Simon 93
Hoffman, Betsy 75
Hoffman, Elizabeth 60
Hoffman, Junius 66
Hoggatt, Austin 50, 89
Holt, Charlie 94
Hovde, Fred 47
Hoyle, Fred 220
Hughes, John 10, 46
Hume, David 91, 101, 121, 189, 190, 192
Hunsberger, Moyer 10
Moyer 11
Hutton, James 189, 190, 192

I
Isaac, Mark 59, 60, 62, 65, 67

J
Jackstad, Steve 109, 136
Jarcow, Harold 133
Jennings, Marianne 66
Johnson, Luke Timothy 229
Johnson, Paul 110
Johnson, Samuel 191

Johnson, Velma B. 28
Jones, Chuck 129
Judge Gideon John Tucker 204

K
Kahn, Joseph 27
Kahneman, Daniel 91, 92, 94, 101
Kahneman, Danny 88, 95, 97, 200
Kamien, Mort 46
Kelly, Walt 151
Kennedy, John 5
Ketcham, Jonathan 48, 51
Keynes, John Maynard 118, 163
Kihlstrom, Rich 10, 26
Klausne, Manny 66
Klein, Larry 10
Knott, David 147
Koch, Charlesn 66
Koskinen, John A. 127
Kovenock, Dan 146
Kreuger, Sharon 148
Kumler, Langdon 17

L
Laffont, Jean-Jacques 91
Laibson, David 92
Lave, Lester 89
Leamer, Ed 153, 166
Ledyard, John 62
Lemaître, Georges 220
Levine, Mike 31, 90
Lichtenstein, Sarah 94
Liggio, Leonard 66
Lin, Shengle 149, 185
Lindbeck, Assar 92, 93
Luetje, Megan 148

M
Machan, Tibor 66
Manes, Rene 47
Manski, Charles 92
Marshall, Alfred 99
Maynes, E. Scott 118
McCabe, Kevin 60, 65, 75, 94, 97, 196
McCloskey, Deirdre 205
Mead, Margaret 122
Merman, Lennie 10, 26
Milgrom Paul 97
Mill, John Stuart 67
Miller, Ross 32, 35, 62
Mirrlees, James 91
Mortensen, Dale 208
Mrs. Douse 9
Muench, Tom 146
Mullainathan, Sendhil 92
Munsinger, Gary 47
Murphy, Jim 60

N
Naeslund, Bertil 93
Nasar, Sylvia 93, 97
Noll, Roger 31, 32, 90
North, Doug 3

O
Oaxaca, Ron 10
Olson, Mark 60, 75
Osborn, Jan 146
Ostrom, Lin 62, 96, 97
Oz Tippo 12

P
Palfrey, Thomas 91
Peltzman, Sam 51, 66
Persson, Torsten 87, 88, 98
Phillipson, Nick 190, 206
Pigou, A.C. 99
Pingry, Dave 67
Pissarides, Christopher 208
Planck, Max 226

Plott, Charles 31, 35, 59, 60, 89–91, 95, 101
Plott, Charlie 17, 32–36, 62, 64, 92, 96
Poole, Bob 66
Porter, David 48, 60

Q
Quirk, Jim 32

R
Rabin, Matt 92
Rachlin, Howard 91
Rand, Ayn 66
Rapoport, Amnon 92
Rassenti, Stephen 48, 52, 58–60, 67, 70, 94, 97, 145
Ravenal, Earl 66
Reagan, Ronald 66
Reiter, Stanley 46
Resnik, Steve 11
Rigdon, Mary 60
Riker, Bill 32, 33
Roberts, John 10
Roosevelt, Franklin 19
Roth, Alvin 91
Rubin, Sarah 230
Rubinstein, Ariel 92

S
Sanders, Bernie 118
Schaefer, John 47
Schelling, Tom 46
Schultz, George 53, 56
Schwartz, Anna 157, 159, 161, 162, 165
Schwartz, Nancy 46
Selten, Reinhard 89, 91, 93–96
Sherman, Roger 89
Shiller, Robert 92
Shubik, Martin 89, 96, 101
Siegel, Sidney 89, 101
Simon, Herb 46, 90, 182
Slight, Ken 14, 19, 21, 22, 28, 30, 31, 131
Slovic, Paul 92, 94, 95
Smith, Adam 46, 91, 135, 146, 154, 182, 189–199, 201–212
Smith, Candace 29, 30, 85, 88, 98–100, 102, 112, 113, 127, 135, 136, 139, 145, 148, 149, 177, 214, 233
Smith, Deborah 3, 4, 7, 18, 22, 25, 26, 29, 30, 99, 120–122, 127, 128, 137, 140
Smith, Eric 3, 4, 7, 18, 22, 25, 26, 29, 30, 99, 120–122, 127, 128, 137, 140
Smith, Gusse Thomas 129
Smith, Houston 217
Smith, Joshua 102
Smith, Joyce 3, 4, 7, 18, 22, 25, 26, 29, 30, 99, 120–122, 127, 128, 137, 140
Smith, Torrie 3, 4, 7, 18, 22, 25, 26, 29, 30, 99, 120–122, 127, 128, 137, 140
Solow, Bob 10
Sonnenschei, Hugo 10, 26
Steele, Jackie 22, 28
Steele, Pete 17, 27, 29, 30, 149
Stewart, Dugald 189, 190
Stigler, George 32
Stiglitz, Joe 32, 95
Stilley, McClure 34
Stilly, McClure 23
Struppa, Daniele 146

T
Thaler, Richard 92, 95
Thorne, Kip 221
Titus, Jon 48, 51, 61

Tompkins, Margot 118
Tullock, Gordon 98
Tversky, Amos 92–94, 101, 201

V
Vail, Glenn 67
van Boening, Mark 65
Vannoni, Mike 48, 51
Vickrey, Bill 53, 62, 93
Vickrey, William S. 93, 94
Vignola, Tony 56

W
Walker, Jimmie 62
Wallich, Henry 53, 54, 56
Watt, James 205, 206
Wedderburn, Alexander 210
Weiler, Em 47
Wellford, Charissa 65
Welp, Ted 68, 69
White, Georgia 21
Wilcox, Nat 146
Williams, Arlington 48, 50
  Arlie 50, 51, 61–63
Williams, John 167
Williams, Oliver 96
Wilson, Bart 60, 91, 96, 136, 146, 189, 194
Wilson, Bob 97

Z
Zajac, Ed 77
  Betsy 75
Zwinger, Ann 21

Subject Index

A

Asset markets 63, 181, 208
  bubbles in lab 65, 179
  housing 154, 157, 164, 167, 174, 181, 208
  mortgage 155, 157, 164, 166, 168, 173
Auctions
  Dutch (uniform price) 53, 55–57
  experimental 62
  Henry Wallich 53, 54
  Milton Friedman 52, 53, 157, 161
  Salomon Brothers and 53, 54, 57
  securities 54, 55, 57
  Treasury and investment banks 54, 57
  Treasury bill 52, 53, 61
  Treasury experiments 53, 54, 57
Autism
  Temple Grandin and 6

B

Brain 50, 123

D

Depression 130, 153, 159, 161, 163–165, 171, 174
  Federal Reserve and 157, 174
  housing and 153, 174
Discrimination 56

E

Economics
  experimental 31, 32, 34, 36, 48, 49, 51, 59, 88–90, 95, 96, 103, 146, 148
  lab and field 33, 88, 103, 181
  macroeconomics 154
  markets 93
Electric power
  Australia 70–72
  California liberalization failures 108
  lab and field 52, 58
  market design 52, 59, 72, 92, 97
  New Zealand 72

V. L. Smith, *A Life of Experimental Economics, Volume II*,
https://doi.org/10.1007/978-3-319-98425-4

test-bedding 52, 59, 61

F
Faith
  Hebrews and 217, 220
  religion and 217–219, 229
  science and 217–219, 229

G
Games
  and Adam Smith 197, 200
  trust 92, 97, 196, 197, 199, 200
  ultimatum 199
Great Recession 146, 151–153, 156–159, 162–165, 167, 174
  bankruptcy, role of 169
  housing and 153, 155, 156, 164

K
Knowledge
  and education 126
  How 125, 194, 229

L
Learning 12, 49, 72

M
Materialism 218, 221, 222
Mind 21, 27, 95, 109, 125, 135, 153, 193, 201, 208–210, 218, 223, 225
Mysticism 229

P
Philosophy 193, 206

Q
Quantum theory 221, 224, 226, 229

R
Recessions
  housing role 153
  major vs minor recessions 164, 174
  mortgage funds 157, 174
Regulation 67, 69, 70, 73, 74, 131, 169
  property rights and 185
Relativity theory 221, 224
Religion
  science and 217, 218, 229

S
Science 56, 73, 148, 217–220, 223, 227, 228
Smith, Adam
  predictions 197
  propositions 194, 196, 197, 199, 208
  trust games and 196, 197, 199
  ultimatum games and 199
Social/Sociability/Sociality 9, 91, 92, 99, 117, 146, 192–195, 197–202
Supply and demand 34, 63, 64, 179, 181, 207

T
Teaching
and learning 35, 90, 147, 155

U
Utility theory 194
in asset markets 64, 179
supply and demand 63, 179, 180, 183

W
Work
early
Boeing 117, 118, 120
love made visible 102, 103
satisfaction 190