Gray Eminence?

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Introduction

When the editors invited me to contribute an essay to *Eminent Economists II – Their Life Philosophies*, I found the adjective intimidating. My hesitance grew as I reviewed the list of contributors to the first *Eminent Economists* volume and reread their magnificent, moving essays. I wondered, "Why not *Salient Economists*? That I could live with." My muse (a librarian and sometime bookseller) replied: "Yeah…just think how many copies *that* would sell!"

My title is an attempt to reduce the gap between expectations and results.[2] But even this reframing of the task left me uncomfortable with the idea of writing about something as self-conscious as a life philosophy. Until recently unaware even of having had one, I doubted my ability to articulate one worth reading about. Besides, didn't those who would be interested already know me well enough to infer what my philosophy must be?

Several factors made me agree to write the essay. First, the invitation came as I finished up a third of a century (literally 33 1/3 years) at the University of California, San Diego, in preparation for starting a second job at Oxford. That seemed a salient time to summarize what I had learned about my profession. I also had a vivid memory of my colleague Sir Clive Granger, whose 2009 death prevented him from contributing to *Eminent Economists II*, at a UCSD gathering in his honor. He explained the even more ambitious turn his research had lately taken by saying, "Winning a Nobel prize is one of those things that builds self-confidence." I thought, "What in the world are the *other* things?" When the invitation came I saw that it might be one—importantly, one that didn't require anything as difficult as discovering cointegration.

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[1] This essay will appear in the collection, *Eminent Economists II – Their Life Philosophies*, eds. Michael Szenberg and Lall Ramrattan, to be published by Cambridge University Press. I owe thanks to Zoë Crawford for her encouragement and valuable advice, on this essay and over the past three decades. Thanks are also due to J. Wesley Clark and Avinash Dixit for good suggestions; and to my former students and perennial co-authors Miguel Costa-Gomes, Nagore Iriberri, and Juanjuan Meng for valuable advice and support.

[2] According to Wikipedia (http://en.wikipedia.org/wiki/%C3%89minence_grise), "An éminence grise…is a powerful advisor or decision-maker who operates secretly or unofficially. This phrase originally referred to François Leclerc du Tremblay, the right-hand man of Cardinal Richelieu. Leclerc was a Capuchin friar who wore grey, or rather brown, robes. Brown or light brown (now called 'beige') was called grey in that era. The phrase 'His Eminence' is used to describe a Cardinal in the Roman Catholic Church."

Next, my muse answered my main concern by turning it on its head: "You should do it! You're not very introspective, and you might learn something about yourself" (delivered like a dissertation advisor complimenting a student on a particularly clever choice of research topic). Finally, I discovered that there was a safety net: Rereading the first *Eminent Economists* volume suggested that contributors unaware of having a life philosophy, or unable to articulate one, could get by with a professional philosophy.[3] Perhaps data-mining the advice I had been in the habit of giving to students, colleagues, and authors would yield something passable.

This essay never strays far from the net. I start by discussing my research orientation toward game theory and its applications in economics, and how I came by it.[4] By a happy coincidence, my orientation and birth cohort allowed me to participate in not one but two scientific revolutions: the game-theoretic revolution of the 1970s and 1980s, and the still continuing "behavioral" revolution that followed. I next discuss in more detail a representative strand of my work, on strategic communication, whose motivation and development spans both revolutions. The openness to evidence and theoretical adaptability this continuing strand required was an important professional lesson. I conclude by trying to distill some other generalizable lessons from my experience in what Tony Hillerman once called "the professor business".

The Road to Game Theory and Economics

I came to game theory and then economics by what anyone who has read graduate application essays will see as the normal route: via an early love of mathematics followed by a desire to apply it to something human. I assumed from about age eight that I would find a job in research, without knowing what that meant.[5] I was no clearer about the field than the job description, but thought vaguely of something in mathematics, engineering, or natural science.[6]

Until college my love of mathematics was pure, with little thought of application. (I did prefer concrete and visualizable ideas over abstract ones; and perhaps for that reason discrete over continuous mathematics.) My pre-college formal education did nothing to change this.

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[3] Indeed, one contributor—a beloved teacher at the Massachusetts Institute of Technology—seemed to have written about precisely what he pleased, claiming at the end to have forgotten what the assignment was.

[4] Academic astrologers might attribute my orientation to having been born "under the star" of John F. Nash, Jr.'s (1950), "The Bargaining Problem," *Econometrica* (April 1950), 155 – 162.

[5] I date this from a gift of a toy printing set, which I used to set a stamp that read "Vincent Crawford, Ph.D."

[6] All this without clear examples: I learned a lot from my family, but those who worked at jobs closest to mine (engineers) died before they could teach me about their disciplines. So much for the importance of role models!

There was no economics beyond brief, pro-forma mentions in civics and history, and no game theory other than the commentaries on military strategy in Caesar and Xenophon.[7]

My pre-college informal education contained one crucial exception, which greatly expanded my view of what mathematics was for. My parents had the prescience to give me, at age ten, a copy of James R. Newman's wonderful anthology, *The World of Mathematics* (New York: Simon and Schuster, 1956). I devoured all 2500+ pages, including Richard B. Braithwaite's article, "Theory of Games as a Tool for the Moral Philosopher".[8] Even without knowing what moral philosophy was, I was surprised and delighted that mathematics could be used to think about human as well as physical phenomena. I do not remember learning much of the substance of game theory from Braithwaite's article; and because I had no easy way to find more to read about game theory, I put it on the back burner. But with hindsight it seems likely that the mere knowledge of the existence of a theory of strategic behavior made me look harder for general principles underlying the behavior patterns I saw "in the wild". I now give several examples, chosen mainly to illustrate principles that later became important in my research.

One of the first organizations I belonged to was the Boy Scouts, a rich source of strategic experience.[9] The most interesting scouting example concerns an orienteering competition, in which one had to take a compass bearing across a field and then follow the bearing to the goal, blindfolded but permitted to look down under the blindfold at the compass. The need to allow us to look at the compass made it easy to cheat, and this was common knowledge. Cheating was in fact so easy that it would have made the contest a boring waste of time. (There was no prize.) I therefore resolved not to cheat, thinking that other scouts would do the same.

I happened to be the first contestant, and was pleased to come within a few feet of the goal. Then I watched, amazed, as most of the remaining scouts hit the goal exactly on target. I

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[7] In grade school or high school in the 1960s, game theory was out of the question. In high school there might have been economics, but my Jesuit prep school prided itself on curricular conservatism. I now recognize their allegiance to the Ratio Studiorum (http://en.wikipedia.org/wiki/Ratio_studiorum) as a blessing, because it taught me things I later would have lacked the patience to learn, but virtually nothing I later had to unlearn. It also once allowed me to surprise an educationally traditionalist dinner companion by quipping that I had been educated in the 17th century.

[8] This I now see was his inaugural lecture as the Knightbridge Professor of Philosophy at the University of Cambridge, originally published in book form (Cambridge, U.K.: Cambridge University Press, 1955).

[9] I experienced another rich source only as a spectator, when my parents, then active in local politics, bridged the gap between the ends of my school day and their workdays by using the spectator's gallery of the Maryland state legislature as a day care center. Perhaps because I was not directly involved, I extracted few if any concrete insights. But watching the legislature in action did give me a feel for the semi-structured maneuvers that are more characteristic of politics and economics as practiced than as usually modeled in noncooperative game theory.

remained amazed throughout the contest, but the source of my amazement evolved. At first I was amazed at the skill of the scouts who came after me, for it had been quite difficult to come even within a few feet. Then, after the second or third scout hit the goal exactly, I marveled at how obvious it was that they must have cheated. I was next amazed that so few scouts saw how obvious this would be. Finally, I was amazed that it did not occur to any of them to feign small errors.[10] These lessons were driven home to all of us soon afterward, when the scoutmasters promoted me—not otherwise an exceptional scout—to the highest scout office in the troop.

There were two game-theoretic/economic lessons here, which I articulated to myself only much later, when I acquired the required conceptual vocabulary. The first concerns statistical verification of compliance in agency relationships. It is now well understood, though with no help from me. The second and in my view equally important lesson was how difficult people find it to put themselves into others' shoes and how much simpler their models of others are than their models of themselves. This, my first step toward behavioral game theory, plays a leading role in my current research, including the work on strategic communication discussed below.

Another especially rich source of strategic experience was competitive sailing, with its clear rules and clearly specified objectives. Here I give four of the most interesting examples.

The first concerns the general principle that when one is ahead in any kind of race, it is beneficial to reduce the variability of changes in the size of one's lead, because (for well-behaved distributions) this increases the probability of still being ahead at the finish. One corollary is obvious: In sports that have deadlines, it is beneficial to stall when ahead.

In sailboat racing the principle has an important but less obvious corollary: When ahead, it is beneficial to increase the correlation between shocks to one's own and others' performance, because this reduces the variability of changes in one's lead. Its most common application concerns sailing a windward leg, in which the most important shocks are unpredictable shifts in wind direction. There the way to increase the correlation between one's own and others' shocks is to "cover", that is to stay on the same side of the course (relative to the direction of the goal) as others do, so that wind shifts, favorable or not, will affect all boats' performances approximately

[10] Years later this lesson was further reinforced by the experience of a graduate school classmate. Fresh out of a British university soon after the end of the colonial era, he worked for an international organization that gave him the task of creating the first national income accounts for a developing country. Quickly realizing that collecting accurate data would be difficult, perhaps impossible, he took the "creating" in his task description more literally than his employers intended, generating synthetic data by simulating a then-popular growth model. Cleverer than my fellow scouts, he added normal errors with modest variance; and his dissimulation has apparently escaped detection.

equally. Covering is strategically important in multi-boat races, but it is especially important in two-boat match races like the America's Cup. Dennis Conner taught the non-sailing public how important covering can be when, with a 57-second lead in *Liberty* at the start of the fifth leg of the last race of the 1983 Cup, he failed to cover and lost the race, and the Cup, to *Australia II*.

This covering principle is more interesting than most because it depends on a rich view of uncertainty. Both stalling and covering have clear analogs in economic competitions. The analog of stalling is well understood in the literature on patent races. To my knowledge the analog of covering has not yet been studied, though possibly its practical importance is limited on land.

A second example illustrates the importance of seeing patterns in data when one's understanding is shaped but not dictated by theory. Pop quiz: You are racing in a large fleet, with all of the boats struggling to make progress in a light breeze. They are apparently succeeding, but not by much. Then, suddenly, one boat starts to pull ahead at a small but clearly noticeable rate, just as if it had switched on a tiny, silent engine (not allowed, of course). Soon afterwards, another boat starts to pull ahead, at exactly the same rate. What should you do, and why?

The (uniquely optimal!) answer is that you should drop your anchor, surreptitiously. The crew of the first boat to pull ahead has figured out that even though they were making progress through the water, the current was strong enough to make them lose ground over the bottom. Anchoring, surreptitiously, stopped that loss while slowing the spread of their insight.

This kind of race, which sailors jokingly call an "anchoring duel", conveys two game-theoretic/economic lessons, which I again failed to articulate to myself until later. The first is the now widely recognized importance of social learning: In some settings we can learn as much from others' responses to experience as from the experience itself. The second lesson is that even with vague knowledge of the structure, attention to data may compel a particular interpretation. (The "smoking gun" here is that the second boat starts to pull ahead at exactly the same rate as the first, which makes alternative explanations of the change in performance highly implausible.)

It seems remarkable that data can be so compelling even when the new interpretation requires expanding the universe of possible models—violating Leonard J. Savage's "small worlds" assumption (*The Foundations of Statistics*, New York: John Wiley and Sons, 1954). A meta-lesson concerns research strategy. The first crew probably learned that they should anchor (long before electronic navigation) by taking a land bearing, something which does not always occur to sailors who are making apparent progress through the closer and much more salient

water. That crew (not mine!) was disciplined enough to look for disconfirming evidence even when its favored theory seemed to be performing well, a choice that goes against human instinct. The second crew (still not mine) was more alert than most, but managed to replace its favored theory by the correct one only when confronted with overwhelming evidence of the cost of holding on to it. Even that takes some discipline, but in an anchoring duel it doesn't take long for the rest of the fleet to adopt and act on a better theory. It is no less important for economists to be intellectually flexible enough to accept disconfirming evidence; but their adjustments often seem to be slower, perhaps because the penalties for being persistently wrong are weaker.

Another sailing example has to do with a tactic that I saw only later, in college. Races start at a pre-specified time, after which the boats may cross a line usually marked by buoys. One of "my" skippers had an unusual starting method, in which he sailed along the "wrong" side of the line, timing it to swing around the buoy at the favored end and cross the line from the correct side, just on time. This would not have worked in equilibrium, because a boat making a near-perfect orthodox start has the right of way (via the "anti-barging" rule) and can legally block a barger like my skipper from starting on time. With my skipper, however, it worked every time, because no one ever anticipated his ploy well enough to block his start.[11] He is now a tax attorney, and I can only imagine the revenue losses caused by his tax avoidance schemes.

This example is intriguing because it depends on deviating from Nash equilibrium to exploit anticipated suboptimal choices by opponents. Robert E. Lee (quoted in Douglas Southall Freeman, *R. E. Lee: A Biography*, New York and London: Charles Scribner's Sons, 1934) once explained why he had failed to exploit a mistake by the opposing Union general, saying "It is proper for us to expect [the enemy] to do what he ought to do." Prescient as this half-definition of Nash equilibrium was, I think this was one of few instances in which General Lee was wrong.

A final sailing example, again from college, has to do with the contest between reason and emotion in decision-making. Early in a five-race series for the MacMillan Cup, our boat was forced, by another boat fouling us, to foul a third boat. Unlike common law, the rules of sailing make no distinction between voluntary and involuntary violations; or more precisely (much as in the Catholic requirement to avoid "occasions of sin") they treat failure to avoid a situation where one is forced to commit a foul as equally culpable as a freely chosen foul. Thus, we were as sure

[11] The ploy worked better when others hadn't seen it before, but it worked even when they had. It can be argued that adapting equilibrium to incorporate decision errors, as in quantal response equilibrium, explains its success; but it worked even in top-level races where decision errors were small. He was the U.S. national champion of his class.

to be disqualified as the boat that forced us to commit the foul. In discussion after the race, it was noted that we would get one more point if we withdrew voluntarily than if we contested the foul and lost. We decided to contest it anyway, because the foul had not been "our fault" and (though this was unstated) we thought the point was unlikely to decide the series. Unsurprisingly, we lost the hearing and the point. Surprisingly, three days later we lost the Cup, by exactly one point. I would like to think that we all learned to control our emotions better, but I cannot swear to this.

Game Theory and Economics

I began my formal study of game theory and economics almost simultaneously, when I enrolled at Princeton in 1968. I enrolled in my first economics courses and didn't like them very much; but I greatly enjoyed Paul A. Samuelson's textbook, *Economics: An Introductory Analysis* (New York: McGraw–Hill, 7th edition, 1967). I also enrolled in the honors mathematics sequence and learned in the first few weeks —at one of the best places in the world to learn that one doesn't really want to be a pure mathematician—that I needed to look elsewhere for my field of research. I still had no formal instruction in game theory, but I started reading about it more seriously—starting with Anatol Rapoport's *Two-Person Game Theory: The Essential Ideas* (Ann Arbor: University of Michigan Press, 1966)—and also reading more about economics—starting with Samuelson's *Foundations of Economic Analysis* (Cambridge: Harvard University Press, 1947) and John R. Hicks' *Value and Capital* (Oxford: Clarendon Press, 2nd edition, 1946).

Needing more guidance, I went to Oskar Morgenstern's secretary to make an appointment.[12] I had planned only to make an appointment, and so was dressed normally for me at the time rather than properly. To my chagrin his secretary waved me in to see him immediately, and he (dressed more than well enough for both of us) was very kind. He wrote me a short reading list, which I still have. There were four references: Morton D. Davis, *Game Theory: A Nontechnical Introduction* (New York: Basic Books, 1970); John D. Williams, *The Compleat Strategyst: Being A Primer On The Theory Of Games Of Strategy* (New York: McGraw-Hill, 1966); R. D. Luce and H. Raiffa, *Games and Decisions: Introduction and Critical Survey* (New York: John Wiley & Sons, 1957); and John von Neumann and Oskar Morgenstern, *Theory of Games and Economic Behavior* (Princeton University Press, 3rd edition, 1953). The

[12] This did not seem pushy at the time. Although John Nash was a recognized, legendary figure on campus, he was not on the faculty and he had not yet recovered to the point where students thought that they could interact with him.

thing I remember most clearly was his advice: "Don't start with my book with von Neumann!" My next stop was the bookstore; and needless to say, I didn't follow that part of his advice.

Princeton offered even more than this to a budding game theorist. Despite my fiasco in honors mathematics, there were many mathematics courses a non-genius could take, and I took most of them. I indulged my love of discrete mathematics by taking several courses from Albert W. Tucker. His discussion of his students' David Gale and Lloyd S. Shapley's "College Admissions and the Stability of Marriage," *American Mathematical Monthly* (January 1962), 9-14, and the recognition that their models were markets even though they had no prices, led me a decade later to write my first papers on matching markets.

More importantly, and probably inspired by Rapoport's book, I did part of my Economics Department junior independent work on the (non-)convergence of adaptive learning processes to mixed-strategy Nash equilibrium. Later this led to my first published paper, "Learning the Optimal Strategy in a Zero-Sum Game," *Econometrica* (September 1974), 885-891.

Most importantly, a dormitory neighbor in my senior year, having just returned from two (voluntary) years of combat in Vietnam, gave me his copy of Thomas C. Schelling's *The Strategy of Conflict* (Oxford University Press, 1960), saying "You'll get more out of this than I did." Even without bullet holes, the book looked like it had been in combat; and my neighbor's jungle marginalia often revealed spirited disagreement with Schelling's arguments. *The Strategy of Conflict* was like nothing I had read, and it opened up a new world for me, full of rich analyses of strategic thinking and applications whose realism and institutional detail went far beyond the toy examples I had seen before. If one book can change an intellectual life, that was it for me.[13]

In graduate school at the Massachusetts Institute of Technology I learned a great deal of economics, which was enormously important to my research. I was also allowed to write an entirely game-theoretic dissertation, one with no mention of prices. But what I learned has comparatively little to do with the research I discuss here, so I do not discuss it.

Strategic Communication through Two Scientific Revolutions

As I said earlier, my birth cohort and orientation toward game theory and its economic applications allowed me to participate in two scientific revolutions. The first was the game-

[13] I try to repay part of my debt in "Thomas Schelling and the Analysis of Strategic Behavior," in Richard Zeckhauser, editor, *Strategy and Choice* (Cambridge, Massachusetts: MIT Press, 1991).

theoretic revolution of the 1970s and 1980s, in which researchers in economics took game theory over from its previous owners in mathematics, used it (mostly noncooperative game theory) to formulate economic models that everyone had assumed were intractable only a few years before, and then used equilibrium to analyze the models. These advances made it possible to do strategic microeconomics with the rigor and clarity that had previously been thought possible only for perfect competition and monopoly. Something that bears emphasis for younger readers is how great an achievement it was just to get the logic of equilibrium analysis of those models right.

My most important contribution to the strategic revolution was the analysis of strategic communication via natural language (now called "cheap talk") in Vincent P. Crawford and Joel Sobel, "Strategic Information Transmission," *Econometrica* (November 1982), 1431-1451. Cheap talk refers to communication that has no direct payoff consequences: It can influence players' welfare, but only by influencing decisions that do have direct payoff consequences. Thus lying involves no personal cost to the liar, as is commonly assumed in economics.

When Sobel and I started thinking about strategic communication, most people assumed—encouraged by Michael Spence's emphasis on the importance of direct signaling costs in "Job Market Signaling," *Quarterly Journal of Economics* (August 1973), 355–374—that cheap talk could not credibly convey information in equilibrium, as the phrase "talk is cheap" suggests. That intuition is correct in two-person games of pure conflict, for if there were an equilibrium in which cheap talk conveyed information, the receiver's response to the sender's message would make the receiver better off, which would thus make the sender worse off, so that he would prefer to make his message uninformative. Nonetheless, it was clear from Schelling's *The Strategy of Conflict* that cheap talk could be very effective in pure coordination games. The difference in cheap talk's effectiveness between the extremes of pure conflict and coordination made me want to know what happens in the mixed-motive games in between.

Sobel and I posed this question in a class of what are now called sender-receiver games, in which the sender observes a one-dimensional piece of private information and sends a message based on it to the receiver, who then takes an action that determines the welfare of both. The receiver cannot commit to a mechanism as in the agency literature: Thus, in a sequential equilibrium he must take an action that is optimal given his beliefs after hearing the message. We assumed that the sender's and receiver's preferences differ in a limited, realistic way, in that a

fully informed sender and receiver would both prefer a higher action, the higher the sender's private information; but the sender would always prefer a higher action than the receiver.

Sobel and I characterized the relationships between the sender's observed signal and the receiver's choice of action that are possible in a sequential equilibrium.[14] The key issue is how much information can be transmitted in equilibrium, and how the amount is influenced by the difference between the sender's and receiver's preferences.

We showed that all equilibria are "partition equilibria", in which the sender, in effect, partitions the set of states into contiguous groups and tells the receiver only which group his observation lies in. When talk is cheap, in equilibrium the receiver reads the sender's message as meaning "I like what you do when I say this (better than anything I could get you to do by saying something else)". Our analysis showed that, despite the prevailing intuition at the time, if preferences are not too different this reading of the message conveys some useful information.

For any given difference in preferences we showed that there is a range of equilibria, from a "babbling" equilibrium with one partition element to equilibria with more elements that exist if the preference difference is small enough. Under reasonable assumptions there is a most informative equilibrium, which has the most partition elements and gives both sender and receiver the highest ex ante expected payoffs of any equilibrium. As the preference difference decreases, the amount of information transmitted in the most informative equilibrium increases. But unless preferences are identical even the most informative equilibrium has a coarse partition, and so conveys the sender's information only with a certain intentional vagueness. Although both sender and receiver would be better off ex ante if the sender could commit to tell the truth, his inability to so commit makes any equilibrium involve some degree of vagueness.

It was very satisfying to show that cheap talk can convey information when the difference in preferences is not too large, and to confirm the intuition that the preference difference limits the amount of information that can be transmitted in equilibrium. But we were disappointed that our analysis shed no light on lying or deception: In equilibrium the receiver's beliefs on hearing the sender's message must be an unbiased (though noisy) estimate of the signal. Thus, in equilibrium the sender's message cannot systematically fool the receiver; it can only add noise to the sender's information.

[14] Because cheap talk signals have no direct payoff consequences, equilibrium does not determine the signaling strategies that support these relationships; but the possible relationships are independent of how they are supported.

There are varying remedies for this, one of which is explored in Sobel's "A Theory of Credibility," *Review of Economic Studies* (October, 1985), 557-573, which studies the equilibrium implications, in repeated play, of the receiver's uncertainty about the sender's motives. But I was also concerned that predictions of senders' and receivers' strategies based on our equilibria seemed to miss systematic patterns in subjects' behavior in experiments. In Hongbin Cai and Joseph Tao-Yi Wang, "Overcommunication in Strategic Information Transmission Games," *Games and Economic Behavior*, (July 2006), 7-36, and several prior experiments, senders told the truth more than predicted in any equilibrium ("overcommunication"); and receivers were more credulous than in any equilibrium. Yet despite these deviations, our equilibrium comparative statics prediction that the closer the sender's and receiver's preferences, the more information is transmitted, was strongly confirmed.

How could these results be reconciled with a sensible theory? Having survived the struggle to get the logic of the equilibrium analysis right, I had to adapt, moving from the strategic revolution to a second, "behavioral" revolution. Behavioral game theory alloys the decision-theoretic rationality of traditional game theory with psychologically more realistic models of how people think about others' responses to incentives. The part of the theory I shall discuss here concerns initial responses to games, where people's beliefs about others' responses must come from strategic thinking rather than learning.

Equilibrium can and often is viewed as a model of strategic thinking, but an important experimental regularity is that people's thinking often fails to follow the fixed-point logic that equilibrium usually requires. Instead people seem to follow strategic rules of thumb based on models of others that are systematically simpler than their models of themselves—like my fellow scouts' models. That doesn't mean that people never behave as if in an equilibrium: The evidence suggests that the most common rules of thumb mimic equilibrium in sufficiently simple games, while deviating systematically in some more complex games. Nor does people's aversion to fixed-point logic mean that with enough stationary experience, they cannot converge to equilibrium strategies an analyst would need fixed-point logic to characterize: It only means that fixed-point logic does not *directly* describe people's thinking in their initial responses to games.

A series of experimental analyses with normal-form games, culminating in Miguel Costa-Gomes, Crawford, and Bruno Broseta, "Cognition and Behavior in Normal-Form Games: An Experimental Study," *Econometrica* (September 2001), 1193–1235, and Costa-Gomes and

Crawford, "Cognition and Behavior in Two-Person Guessing Games: An Experimental Study," *American Economic Review* (December 2006), 1737-1768, seeks to identify the most common rules of thumb people use in lieu of equilibrium logic. A robust finding is that many people use "level-*k*" rules, in which they start by anchoring their beliefs in a naïve model of others' instinctive reactions to the game, and then massage their beliefs by iterating best responses a small number of times: one, two, or at most three. The resulting level-*k* models provide an empirically well supported structural alternative to equilibrium models of strategic thinking.

In "Lying for Strategic Advantage: Rational and Boundedly Rational Misrepresentation of Intentions," *American Economic Review* (March 2003), 133-149, I adapted level-*k* models to sender-receiver games in which a sender could communicate his intentions about how he would play an underlying two-person zero-sum game. Here, as explained above, in any equilibrium the sender's message must be uninformative, and the receiver must ignore it. Yet in real-world games with communication of intentions, we often observe messages that attempt to deceive, and such attempts sometimes succeed. My 2003 paper showed that a level-*k* model in which people anchor their beliefs in the literal meaning of messages gives a simple explanation of several commonly observed patterns in such settings.

My 2003 analysis is readily adapted to games in which the message concerns private information as in Crawford and Sobel's analysis, rather than intentions. In such a model our equilibrium-based comparative statics result that the closer the sender's and receiver's preferences, the more information is transmitted, continues to hold for empirically realistic distributions of level-*k* players. Earlier I stressed the importance of being open to disconfirming evidence. It is also important to be open to confirming evidence! Building on Cai and Wang's 2006 analysis of a sender-receiver game with one-sided private information as in Crawford and Sobel's analysis, Wang, Michael Spezio and Colin F. Camerer, "Pinocchio's Pupil: Using Eyetracking and Pupil Dilation To Understand Truth-telling and Deception in Sender-Receiver Games," *American Economic Review* (June, 2010), 984–1007, have now found strong experimental support for such a level-*k* model. The model gracefully reconciles subjects' tendencies toward overcommunication, excessive credulity, and other systematic deviations from equilibrium with the persistent support for Crawford and Sobel's comparative statics prediction.

Further Generalizable Lessons

What further lessons might be gleaned from all of this? (I take it that I am allowed to advise you to follow rules I haven't always followed myself.) Here are some simple rules.

1. Know the literature; but as much as you can, get your ideas from the world.

2. Put your ideas carefully into the context of others' work, giving them all due credit. We stand on the shoulders of giants, and medium-sized and short people too. If you give a clear, accurate summary of how ideas evolved, you will probably get at least your share of the credit anyway.

3. Make 99% sure your ideas are right, and then fight for them. Tough-mindedness is particularly necessary for original ideas. One of my students persisted for a year trying to prove a result I was convinced wasn't true, and I am sorry to say I did not encourage him: quite the contrary. He was right, and the result was worth the year. Even worse, years before that I had proposed a significant generalization of one of my own results to an advisor who immediately conveyed his belief that the generalization wasn't true, saying "Forget it!" I uncritically took his advice, only to discover years later that my generalization was an easily proven extension of my earlier result.

4. But even when 99% sure you are right, listen to others' criticisms and learn from them. And pick your fights: If you fight too often, unless you are very lucky your credibility will suffer.

5. Be kind to your juniors. One Saturday in graduate school I received a reprint in Russian, whose English title and abstract might as well have read "Chapter 2 of Vince Crawford's dissertation". I phoned one of the two people I knew who spoke Russian, a senior faculty member, and asked if he would translate it for me. He said, "Sure. Come in Monday morning." I said, "But I meant *now*." Without batting an eye, he told me his address. His translation of the first few paragraphs made clear that it wasn't really Chapter 2. I will never forget his generosity. Many years later I had an opportunity to repay part of it by helping him to get a difficult paper into shape for publication; but I still think I owe him. Good students are one of the best things about the professor business: The joy of being kind to them should be enough reward; but even if not, they will repay any kindness you show them many times over. (Stephen Jay Gould, *Wonderful Life*, New York and London: W.W. Norton, 1989, 139-141, has a wonderful passage on the reciprocal relationship between mentors and students.)

6. Be kind to your seniors. They can be fun. Besides, someday you will be a codger yourself.

7. Last but not least: Be kind to your contemporaries. This follows from rules 5 and 6 by continuity—or is it convexity?

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