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IN DEFENSE OF FOXES

There is a line among the fragments of the Greek poet Archilochus which says, "The fox knows many things, but the hedgehog knows one big thing." . . . Taken figuratively, the words can be made to yield a sense in which they mark one of the deepest differences which divide writer and thinkers, and it may be, human beings in general. For there exists a great chasm between those, on one side, who relate everything to a single central vision, one system, less or more coherent or articulate, in terms of which they understand, think and feel—a single, universal, organising principle in terms of which alone all they are and say has significance—and on the other side, those who pursue many ends, often unrelated and even contradictory, connected, if at all, only in some *de facto* way, for some psychological or physiological cause, related by no moral or aesthetic principle.

—Isaiah Berlin, *The Hedgehog and the Fox*

At the disciplinary level, sociology is, itself, foxlike, containing individuals with multiple theoretical perspectives and methodological approaches. It also considers any aspect of social behavior as within the legitimate domain of its analysis. Of course, these multiplicities are something for which sociology is often criticized. It is a field that lacks a single coherence.

Sociology's lack of coherence means that it is a discipline, unlike, say, economics, in which foxes can thrive. Because of the multiplicity of its perspectives, it is able to tolerate individuals who singly reveal multiple commitments that is foxes, although within sociology there are always questions of where their loyalties, if any, truly lie.

In his book *The Hedgehog and the Fox*, the English political theorist Isaiah Berlin examines whether Tolstoy was a fox or a hedgehog.[1] He argues that although Tolstoy was deeply and genetically a fox, he thought one should be a hedgehog. Berlin suggests that the key issue for Tolstoy in *War and Peace* (as well as in his other work) was the role of free will and determinism in history: "he is above all obsessed by his thesis—the contrast between universal and all-important but delusive experience of free will, the feeling of responsibility, the values of private life generally, on the one hand; and on the other the reality of inexorable historical determinism."[2]

In this chapter, I reflect, in part, on my self-understanding as a fox. I built my reputation as a high-tech, quantitative type. Within the quantitative world, there are two quite distinct perspectives, that of a statistician and that of the econometrician, both fields I identify with. The statistician believes in the primary reality of the data themselves and wants to understand their structure, whereas the econometrician more typically sees data as being more epiphenomenal in character, having been generated by underlying behavioral structures that he seeks to uncover. Talk about two radically different views of the world. Thus, even a quantitative type can be a fox.

My goal for this chapter is to argue for the methodological and scholarly virtues of the fox. Before engaging in that argument, however, I examine two basic questions. First, how and/or why does one become a fox? Second, what does it take to succeed as a fox? These two questions are essential since it makes little sense to talk about the virtues of foxes if we don't first understand why some individuals are foxes and others hedgehogs and, second, if we don't understand what it takes to be a fox. I discuss these issues primarily in the context of my life and professional career. I do so not out of any great sense of self-importance but because it is in terms of my own life that I have, in part, come to find answers to these two questions.

THE GENESIS OF ONE FOX

Berlin makes it clear that Tolstoy would prefer to be a hedgehog but that he has no choice—he is a fox. This seems correct. We cannot choose to see the world in one or in multiple ways. We may, of course, deny that we see things from a certain perspective, but this is denial. The question is not what we acknowledge seeing but what we actually see. Why, then, do some individuals see like foxes and others like hedgehogs?

Few of my academic colleagues are aware that I grew up in a family of therapists, though colleagues occasionally have told me that I am one of the most touchy-feely quantitative social scientists that they have ever met. My father and stepfather are both psychiatrists, and my mother and two of my three sisters are psychiatric social workers. As I was growing up, dinner conversations were always focused on some individual, perhaps an anonymous patient or someone known to the family in the community. The question was to understand why they had so many problems and to discover a way in which they might resolve them.

I loved these conversations. If my parents did not offer up some poor soul for analytic sacrifice, I was ready to suggest a friend or classmate. One of the things that made these conversations fascinating was the socially and economically diverse set of patients my parents saw. We lived in New Britain, an old Connecticut factory town that was as economically, socially, and ethnically diverse as America itself. Although I grew up in the wealthy West End, I went to grade school with a full span of children, including those from the local orphanage. New Britain was a most fruitful initial field site for studying processes of social stratification.

For many years, I also wanted to be a therapist, and I believe I would have been a good one. I enjoyed thinking about people and what made them act as they did. Moreover, the deluge of presents received by my family each Christmas by grateful patients provided some evidence that through therapy it might be possible to actually help people. My parents had devised a life in which they had somehow created that magical combination—they were quite well off financially and yet were also contributing significantly to the social good.[3]

I often jokingly tell people that having grown up in a family of therapists, my adolescent rebellion was to become a quantitatively oriented sociologist. Actually, there is much truth in this quip, though I was certainly rebellious in many ways as an adolescent during the late 1960s. The transformation, however, was not sudden. Although I had decided by the eleventh grade to become a sociologist, had taken a sociology course that summer at the local community college, and in my senior year had carried out a lengthy survey of my entire boarding school (Hotchkiss), I planned to become both a psychiatrist and sociologist. I somewhat naively saw nothing problematic with a career in which one was equally committed to two quite different intellectual and professional perspectives. I went to Dartmouth as a freshman with shoulder-length hair and a full beard; a box of IBM punch cards, containing the data from the survey I had done; and the full intention of doing both premed and sociology.

Although I still find the psychological-therapeutic perspective compelling, as a college student I became disillusioned with it for several reasons. Certainly, first and foremost, I was frustrated with the idea that persons should always be understood at the individual level. When my family and I talked about individual problems, more often than not they concerned relationships. Yet, the diagnoses were always in terms of the individual.[4]

Second, the idea that there was a deep intentionality in all behavior seemed wrong. Whether in trying to analyze the behavior of someone at the dinner table or explaining why a member of the family had done something, my family always assumed that there was some deep, hidden motivation. There were never any unintended consequences.

But why choose to be a quantitative- or mathematically oriented sociologist? I had always been extremely good in math but quite weak in languages. In fact, I am moderately dyslexic, something that wasn't fully diagnosed until I was in graduate school; it was simply an unknown problem when I was growing up. For most of my youth, I was considered to be lazy when writing and spelling. In fact, Harrison White, my graduate advisor, accused me of this fault.

Math was a language with which I felt comfortable. It was easy to see when one made mistakes. Also, when I went to Dartmouth in 1968, math modeling had become a hot area in the social sciences and in sociology in particular. Unbeknown to me, I had come to a university with a number of the leaders in this new area of research. At Dartmouth were James Davis, Robert Norman, John Kemeny, Laurie Snell, and later Joel Levine.

I did a double major in sociology and mathematics. My thesis provided a solution to a long-standing problem in balance theory: how to extend Heider's theory (a friend of a friend is a friend; my enemies' friends are my enemies) to the situation in which there were degrees of enmity and friendship. I later published the thesis in the *Journal of Mathematical Sociology*.[5] As a piece of mathematics, the paper is elegant. As a piece of writing, it is horrid.

The point of this personal story is that I had no choice but to be a fox. My family literally, though not unhappily, forced me to see the world from a therapeutic perspective. Unless I had been born with different genes or simply was never given the opportunity to learn any math, I was also going to understand the world in mathematical terms. There was never any possibility that I would see things from only one perspective.

In *The Hedgehog and the Fox*, Berlin describes the host of negative reviews that *War and Peace* received when it was published. Tolstoy was accused of charlatanism and intellectual feebleness. His philosophy of history and his philosophical musings were generally considered superficial. He was attacked for his lack of facticity with respect to the historical record. Unsurprisingly, Berlin goes on to defend Tolstoy as being grossly misunderstood. He argues that the core problem is that Tolstoy's critics fail to appreciate that he is attempting to understand the same phenomena from multiple perspectives.

I went through graduate school rapidly, perhaps too rapidly—three and one-half years. Instead of writing a dissertation, I submitted three published articles as a thesis. I learned some sociology, though not enough. I did take quite a number of courses in statistics and economics. The primary article in my thesis was "The Allocation of Time Among Individuals,"[6] which took a formal model of an economy from General Equilibrium Theory in economics and showed how, when it was assumed that prices were fixed, it could be used to model the way in which people allocate time with each other. "Prices" were fixed to "one" since the amount of time I spent with you by necessity was equivalent to the time you spent with me.

My experience in the job market was a disaster. I was interviewed by twelve departments but received no offers—surprising, perhaps, in that I am now a full professor at Harvard. Everywhere I went a few mathematical economists loved my paper on time. The few sociologists who were supportive of a math-modeling approach also liked it. The vast majority of sociologists, however, had little idea of what I was doing and most certainly believed it irrelevant to sociology. No offers were forthcoming. I retrenched and was offered (by an economist) a one-year postdoctorate at the Institute for Research on Poverty at the University of Wisconsin, which was then followed by a postdoctorate for two years at the National Opinion Research Center (NORC) at the University of Chicago. I went to the Midwest to receive the training in sociology that I had failed to obtain at Harvard. Although I knew quite a bit of sociology, statistics, and economics, I hadn't learned how to speak coherently to each group individually, much less how to speak across groups. Most important, I had no ability to show sociologists how the perspective of another group, in this case, economics, might provide useful insights.

During my second year at NORC, I received a phone call from Ackie Feldman, then chair of sociology at Northwestern. He wanted to know whether I would consider a position in sociology there. At the time, sociology as a discipline was very much in the middle of the methods wars. Departments were divided internally and externally by whether they believed that a qualitative, ethnographic approach or a quantitative, statistical approach was the correct way to proceed. Northwestern had the reputation of being radically qualitative. Howie Becker and Arlene Daniels were there. John Kitsuse had been there in the past. Dick Berk had left a few years earlier on less-than-good terms with the department. Although Northwestern was a mere fifteen miles away, it took Ackie Feldman three telephone calls to convince me to visit. The visit went extraordinarily well. Howie Becker and I sat and talked about the virtues of Kemeny and Snell's *Finite Mathematics*.[7] I gave a reasonable talk about the problem of measuring inequality based on recent work in economics.

The position at Northwestern was joint with the undergraduate program in Mathematical Methods in the Social Sciences. My dual interests in sociology and math modeling were suddenly an asset. I went to Northwestern, was promoted with tenure three years later, and promoted to full professor four years after that. By the time I left for Harvard in 1992 (having previously turned it down in 1986), I had chaired the program in Mathematical Methods in the Social Sciences, as well as the sociology department. I had helped start the statistics department, held a courtesy appointment in economics, and had been a long-term member of the Center for Policy Research (now the Institute for Policy Research). Northwestern was a place where a strategy of multiple academic selves was not only possible but also a recipe for success. I had also learned how to be multilingual. Even though the multiple disciplines I was in couldn't be integrated into a single coherent theoretical approach, I at least had learned how speak within the perspective of each.

THE VIRTUES OF THE FOX

We may admire real foxes for their cleverness and perhaps their sense of roguishness. The idea, however, that they are virtuous seems peculiar. Similarly, academic foxes may be admirable, but they hardly seem virtuous. They lack a coherent perspective or commitment to any intellectual perspective. And then there is the question of loyalty.

I want to argue for the virtues of the fox in the scientific enterprise. Obviously this is, in part, self-serving. I do, however, think that foxes make an essential contribution, and thus their behavior should be tolerated, if not rewarded. My argument has four parts. First, I contend that an important source of new insights is the migration of ideas from other disciplines. Second, I argue that different types of research problems require different methodological approaches. Third, I suggest that the ability to hold multiple perspectives provides a powerful means for self-criticism. Finally, I argue that the ability to maintain multiple perspectives provides at least a partial means of dealing with the problem of objectivity.

BORROWING IDEAS

In her book *How Institutions Think*, Mary Douglas[8] argues that no one really has a new idea. Rather, academic research proceeds when individuals first recognize the importance of undervalued ideas and then promote them by showing how they can contribute to solving outstanding intellectual problems. But where do undervalued ideas come from? One possibility is from earlier work in the field. Many researchers have been successful by mining the works of earlier scholars. Douglas herself relies extensively on Durkheim in her own scholarship.

To look only to one's own field for undervalued ideas, however, is highly restrictive. It amounts to returning to old mines to see if any gold or silver has been left behind. The other possibility is to explore new fields, that is, other disciplines, for ideas. Of course, searching here may be quite unrewarding since the intellectual agendas of other disciplines are often quite different than those of sociology. At times, however, one can find gold. Let me give an example from my own experience.

When I was a postdoctoral student at NORC, I spent a lot of time working with and studying the works of Jim Heckman. At this time there was considerable interest in sociology, particularly in structural equation models and path analysis. One of the outstanding problems at the time was how to do path models when one had discrete variables. In fact, some scholars claimed that it wasn't possible to do a true path analysis in that case. My frequent collaborator, Rob Mare, who was then at the University of Wisconsin, Madison, and I were working through Heckman's 1978 article on dummy endogenous variables.[9] Heckman was interested in estimating the effect of a treatment when assignment to treatment was endogenous. Over the years this developed into an extensive line of research for which, in 2001, he received the Nobel Prize in Economics. There are significant rewards for being a hedgehog.

In reading Heckman's article, it was clear to us that there were two different ways to think about discrete variables. In one case, a discrete variable was simply a crude measure of an underlying continuous variable. In the other case, it was truly discrete, which implied that it had to be handled as a nonlinear variable. Doing path models in the first case was absolutely straightforward and simply involved estimating the coefficients for the underlying continuous variables. In the second case, one needed to recognize that the standard formula in path analysis was simply a special case of the chain rule in calculus, a formula I had learned in my second math course at Dartmouth. Having been a math major paid off. The insight that the chain rule could be used to carry out a path analysis when someone had nonlinear equations was something Rafe Stolzenberg[10] had just finished an article on. Given the insights of Heckman and Stolzenberg, the solution to path analysis with discrete variables seemed so obvious that it almost was not worth writing up. We did so, however, and much to our surprise it was considered a path-breaking piece of work.

USING THE RIGHT TOOL

One of the dumbest fights that has ever occurred in sociology has been the debate over whether quantitative or qualitative methods are the "true" method for this discipline. In terms of intellectual politics, the fight is quite understandable. Starting in the late 1960s, quantitative methodology began to dominate sociology, and qualitative-oriented sociologists—for both intellectual and personal reasons—were concerned that their type of sociology was being pushed to the sidelines. As a result, they fought back, and departments became deeply divided. Certainly, one of my vulnerabilities when I was first in the job market was that I was seen as someone who only knew math; that is, I was an extreme quantitative type.

Unfortunately, the methods battle was defined as which method was the "true way" of doing sociology. Somehow the idea that different methods provided different ways of understanding a phenomenon was not considered. Yet, this should be a key methodological doctrine for sociologists.

I see this in the work I am currently doing on Boston's efforts during the 1990s to deal with the problem of youth violence. During this decade, homicide rates fell by 80% in Boston. Both the local press and the national press have attributed this drop to work done jointly by a group of black ministers known as the Ten Point Coalition and by the Boston Police Department. The ethnographic evidence also seems to support this belief. A close examination reveals that the police and ministers were dealing with critical gang issues together. There is one problem with this argument. Homicide rates dropped precipitously in a number of large cities during the 1990s without any such partnerships. The quantitative data challenge the qualitative data in critical ways.

If, however, we simply examine the quantitative data, key insights are also missed. As in many evaluation problems, the important effects of a program are often different from those it was designed to have from or what people think it does. What I have shown in my research on the Ten Point Coalition is that the involvement of ministers has been instrumental in two ways: (1) creating legitimacy for the police when they do act in ways that are in the best interests of the community and (2) improving community-police relations. New York City's homicide rates also fell dramatically during the 1990s. However, in Boston, community-police relations are now the best they have been in decades, whereas in New York they have probably never been worse than during this period.[11]

What this example illustrates is that quantitative and qualitative methods can help us discover different types of truths. In doing so, they may force us to consider how the findings obtained through each method can be rectified. I hope that the result is a more holistic and accurate understanding of the phenomena we are investigating. Each is an imperfect tool in discerning the truth. By using methods in complementary ways, we can achieve a better, though almost certainly still quite imperfect understanding of the topic we are studying.

SELF-CRITICISM

Allowing one's research to be criticized is key to high-quality research. The criticism can come from others or from oneself. One of my current areas of research is counterfactual models of causality, which has two parts. First, there has been a set of papers and now a book in progress with my former student, Steve Morgan, aimed at explicating recent research in statistics and econometrics on counterfactual causal models.[12] Second, I have written a series of articles with my colleague Marty Rein at MIT on the policy misuses of causal reasoning in the social sciences.[13] The initial impetus for the work was the extensive and often poorly argued criticisms of *The Bell Curve*.[14] Ultimately, it resulted in a broad criticism of the use of causal reasoning by social scientists titled, "The Dangers of Strong Causal Reasoning."[15] One of the most effective ways of criticizing is by being fully cognizant of both sides of an argument.

THE ELUSIVENESS OF OBJECTIVITY

I have just argued that as sociologists we need to study problems by multiple methods. Implicit in my argument has been the position that we need to be willing to see phenomena from different perspectives. How, then, are we to get at the objective truth? Such a question assumes that there is a position from "nowhere," or a "God's-eye" view that can be used to determine what in fact constitutes the objective truth. I don't see how any social scientist today can believe that such a position is tenable. Is objectivity, then, impossible?

I have always found the East Indian story of the elephant and the blind men to be particularly useful. The essence of the story is that each man believes the elephant to be something quite different since each has felt a different piece of the elephant's anatomy. This story provides two key insights. First, just because each individual perceives the elephant differently does not mean that every person's perceptions are equally correct. If the man holding the elephant's tail describes it as being like a tree, that is a fairly poor description. If other men feel the elephant's tail, they may well disagree with his description. The analogy may also lead to poor predictions how the elephant will behave. Second, understanding what an elephant in fact is involves "seeing" it as a whole. This might be done if the blind men discuss their perceptions and are willing to assume that no one of them actually has the "truth." The other possibility is for them to trade places and for each to feel different parts of the elephant. Neither strategy will allow one to perceive the elephant from a position of "nowhere," but both are good strategies for coming up with a reasonable holistic account of what an elephant is.

The idea that we should move around and study social life from different vantage points is, of course, the strategy of a fox. It is similar to that of a set of start-up internet companies that David Stark at Columbia and Monique Girard at Columbia have been studying in Silicon Alley in New York City.[16] What is interesting about these companies is that they are very nonhierarchical. Girard and Stark describe their structure as a hetrarchy. In addition, these companies do not have a long-term business plan, nor do they have well-defined products. Rather, they are set up around work teams that sometime overlap. The job of a work team is to explore some portion of the Internet business environment in an attempt to discover what might be potentially lucrative business niches. Essentially these are businesses that are set up to succeed in an information-poor environment, where no one really knows what will succeed.

It impresses me that if social science is to thrive, it needs at least in part to act like these companies. Science, by its nature, involves exploring problems where we don't know the answers.[17] Of course, there are dangers. It may be difficult to explain or justify to the world what one is doing, and of course, one may come up empty-handed.

STRENGTH IN DIVERSITY

When I had my disastrous first experiences in the job market, I often thought about changing fields and becoming an economist. Economics was and is a field where mathematical ability is highly valued. However, every time I started to seriously consider this alternative, I was repelled by the intellectual narrowness of economic thought. Economics is not narrow in that it only studies how an economy works. Far from it. Economists have been willing to study anything that sociologists do, from the family to religion. Rather, it is that economists typically attempt to understand all social phenomena in the same way—as the choices made by utility-maximizing individuals. For me, becoming an economist was a recipe for intellectual claustrophobia.

Sociology's intellectual diversity is one of its greatest strengths. Unfortunately, it, too, often becomes something that divides us, as scholars become committed to one or the other approach as the "right" way to do research. What the elephant anecdote shows is that we can only get at the truth by "seeing" things from multiple perspectives. This methodological philosophy suggests that objectivity is not obtained by taking a neutral stance. Rather, objectivity is an ideal that is striven for by attempting to understand a phenomenon from many different perspectives. This is something that we should pursue both as individuals and as a community of scholars.

How, then, does one see from different perspectives? To truly see well, we need to be willing to commit ourselves at least temporarily to different perspectives. One needs to fully emphasize, if not identify with, different positions. There is no more effective way of doing this than by allowing oneself to have multiple academic selves, that is, to be willing to be a fox. The hedgehogs, of course, will always complain that they are doing the real work. Their contributions may appear to be more substantial and they may reap more rewards, but, oh, what fun it is to be a fox.

NOTES

1. Marty Rein introduced me to Isaiah Berlin's wonderful little book *The Hedgehog and the Fox: An Essay on Tolstoy's View of History* (Chicago: Ivan R. Dee, 1978), 3.
2. Ibid., 30.
3. In his fifty years of practice, my father essentially built the mental health structure of central Connecticut, that is, the area between Hartford and New Haven. He established a psychiatric floor in New Britain's General Hospital and created more than a half-dozen public psychiatric outpatient clinics in surrounding towns, as well as

equal number of mental health programs in local public schools. He is still practicing at eighty-three.

4. In the late 1960s, the idea that mental illness involved family and group dynamics was just starting to come into vogue in the works of Gregory Bateson, R. D. Laing, and Murray Bohen, among others.

5. Christopher Winship, "A Distance Model for Sociometric Structure," *Journal of Mathematical Sociology* 5 (1977): 21–39.

6. Christopher Winship, "The Allocation of Time among Individuals," in *Sociological Methodology 1978*, ed. Karl Schuessler (San Francisco: Jossey-Bass), 75–100.

7. John G. Kemeny, Hazlehon Mirkil, J. Laurie Snell, and Gerald L. Thompson, *Finite Mathematical Structures* (Englewood Cliffs, N.J.: Prentice-Hall), 1958.

8. Mary Douglas, *How Institutions Think* (Syracuse, N.Y.: Syracuse University Press, 1987).

9. James J. Heckman, "Dummy Endogenous Variables in a Simultaneous Equation System," *Econometrica* 46 (1978): 931–61.

10. Ross Stolzenberg, "The Measurement and Decomposition of Causal Effects in Nonlinear and Nonadditive Models," in *Sociological Methodology 1980*, ed. Karl Schuessler (San Francisco: Jossey-Bass, 1979), 4549–88.

11. Christopher Winship and Orlando Patterson, "Boston's Police Solution" (editorial), *New York Times*, March 3, 1999. In the work I have been doing in Boston's inner city over the last seven years as part of my research on the Ten Point Coalition, I have been struck by the similarities between the families I encounter and those my parents spent their professional lives dealing with in the various public mental health clinics where they worked. Boston does not have any truly "bombed-out" areas, such as those found in the South Bronx or West and South Sides of Chicago. For several decades, with some short exceptions, it has also enjoyed a low unemployment rate. Yet Boston certainly has its ghettos, where poor and often highly dysfunctional families live. To my surprise, I find myself seeing much of what I observe in these communities through my parents' eyes.

12. Christopher Winship and Stephen L. Morgan, "The Estimation of Causal Effects from Observational Data," *Annual Review of Sociology* 25 (1999): 659–707.

13. Martin and Rein and Christopher Winship, "The Dangers of Causal Reasoning in Social Policy," *Society* 25 (1999): 657–707; Winship and Rein, "Policy Entrepreneurs and the Academic Establishment: *The Bell Curve* Controversy," in *Intelligence, Political Inequality, and Public Policy*, ed. Elliot White (Westport, Conn.: Praeger, 1997), 17–49; Winship and Rein, "The Dangers of 'Strong' Causal Reasoning in Social Policy," *Society* 36 (July/August 1999): 38–46.

14. Richard J. Hernstein and Charles Murray, *The Bell Curve* (New York: Free Press, 1994).

15. Christopher Winship, "The Dangers of 'Strong' Causal Reasoning: Root Causes, Social Science, and Poverty Policy," in *Experiencing Poverty*, ed. Jonathan Bradshaw and Roy Sainsbury (Burlington: Ashgate, 2000) 26–54.

16. Monique Girard and David Stark, "Distributing Intelligence and Organizing Diversity in New Media Projects," *Environment and Planning* 34 (2002): 1927–29.

17. When I was in college, I was a serious rock climber. I had two very different kinds of climbing experiences. When I was following a route that had already been set by someone else, the logic was always one of "working it out." However, establishing new routes often involved intensive exploration since one literally didn't know where one was going. Not too surprisingly, putting in new routes was considerably more psychologically taxing than simply following an established one.