Thoughts on Building an Academic Career

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We have many routes to success in agricultural economics: extension education, resident teaching, advising, research, public service. In selecting problems to study we must be sensitive to needs of all our clientele. Several production economics concepts are relevant to allocating our own efforts. Noticing, recognizing, and experiencing surprise aid scientific discovery. We need to use heuristics, intuition, deduction, and induction, though consideration of science's ideal and real types shows that all these mental processes are fallible. We need special theories that have broad application. Replication deserves high priority. A few thoughts on the manuscript review process are presented.

Key words: comparative advantage, ideal types, mental processes, real types.

Nobel Prize winner Peter Medawar urged that scientists study (pp. 126–27) "the behavioral and intellectual structure of everything that goes into the enlargement of our knowledge and understanding of nature." Such a study should be congenial to economists' interests because it recognizes that developing a science and building a successful scientific career require scientists to allocate their own resources effectively. This article presents some ideas acquired from the kind of study that Medawar urged. They may be useful as you decide how to manage your own career and allocate your own resources. My primary thesis is that there are topics other than economics, statistics, and mathematics that are relevant to an economics career.

Many Roads to Professional Success

After the Awards Ceremony at the 1987 summer meetings of the American Agricultural Economics Association (AAEA), President Havlicek expressed the hope that members would read the Fellows' citations to see the many ways to succeed in agricultural economics. To show what he meant, I quote from the various citations some of the reasons for granting these Fellowships: "a distinguished career in research, teaching, extension, and service to industry, his universities, and the profession," "popular and effective teacher," "university statesman," "service to AAEA," "success in developing practical solutions to difficult problems," "outstanding extension educator," "unusually imaginative and original research worker," "contributions to regional research activities," "served in a wide variety of counseling, committee, and extension roles," "teaching and counseling skills," "consultant to the presiding bishops of the Episcopal Church on world food issues," "superior scholarship, teaching, and service." You see, our profession does honor accomplishments in many different professional activities; it does provide many routes to success.

Economic Principles for Managing an Economics Career

We can use economic principles in managing our own efforts.

Pay attention to your own utility function. Spend a significant part of your time on work

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you like to do. You will enjoy life more and you will also be more productive.

You can have a portfolio of research projects: some safe projects with sure payoffs and some risky ones with smaller probability of success but with some possibility of dramatic results. The mix depends upon your risk aversion or risk affection. Working on several projects also provides an escape hatch when you become bored or frustrated with one. Having a portfolio of teaching and research responsibilities also is beneficial.

A researcher must use a limitational production input: scientific daring, a willingness to take a chance on something new. It is limitational because your productivity is zero if you lack it. Along with this must also go an ability to handle feelings of vulnerability: the feeling that you are an unprotected target for everyone who has never tried what you are trying to do. In universities it is easier to dare if you have the support of your dean, director, and department head.

A key idea in production theory is marginal productivity. In choosing the research that you will do, compare your knowledge, skills, and interests with those of other economists, and consider where are the greater unmet needs for information. In this way you identify the projects on which your anticipated marginal productivity and comparative advantage are greatest.

Find and use your comparative advantages. A comparative advantage arises from a difference. If you pattern yourself too closely after others, you have no comparative advantage over them. Some sources of comparative advantage are training, job experience, and intellect. Comparative advantage may also arise from your personality, the kind of person—not the kind of economist—that you are. I can best show this by using myself as an example.[1]

In reminiscing on his research in scientific discovery, Simon (1989) emphasized the importance of the phenomenon of surprise in discovering new problems or new solutions; noticing something surprising is often the first step in discovery. A capacity for surprise is an important (perhaps a necessary) aid to discovery. Simon also wrote that noticing and recognizing are mental activities that contribute to scientific discovery: to recognize the same idea or pattern under different names, to notice unsuspected interrelationships or identities. Surprise, recognition, and noticing are a unit. You cannot experience surprise unless you recognize that something is surprising. And you cannot recognize until you notice. The ability to notice, recognize, and experience surprise is a valuable asset.

I enjoy synthesis more than analysis, and my mind seems naturally to try to synthesize things without any conscious commands from me. I enjoy putting together ideas from different sources, integrating firm theory and animal breeding, using my knowledge of factor analysis to develop a model of prices and demands for product characteristics. I enjoy studying psychology. If I had lacked the interest in psychology, the enjoyment of synthesis, or the curiosity about behavior of economists, I would not have studied intuition, imagination, the creative mental processes. The enjoyment of synthesis and the resulting reading on a variety of subjects have helped me to notice and to recognize.

I have long believed that I am "a good noticer." My work on imagination (Ladd 1979, 1987) started by my noticing that economists rely on unconscious mental processes but never publicly admit doing so. And my study of intuition has been facilitated by noticing and synthesizing.

I am not a farm boy, and I have no formal training in agricultural economics. Because my training was different, I sometimes see things differently than others see them. Sometimes the different perception is helpful to me. Sometimes not.

Economists who have strong faith in their theories have little capacity for surprise. When they don't see what the theory tells them to see, they are disappointed that the data do not conform to their theory. They assure themselves that something must be wrong with the data, or the statistical procedure, or . . . But they never recognize the surprising possibility that the theory is really false or inapplicable or that there are phenomena that the theory says nothing about.

I am a skeptic and am convinced that "Anything that everyone knows is almost certainly wrong." My skepticism is even better than a capacity for surprise. To be surprised implies

[1] Note our profession's penchant for happy names: "greatest comparative advantage" in preference to "least relative disadvantage," "negative economic growth" in preference to "decline" or "recession." Why do we not use "negative recession" instead of "prosperity"?

that one has noticed the unexpected and has recognized its possible implication. Studies of perception have shown the difficulty of recognizing the unexpected. My skepticism makes it unnecessary for me to accomplish the difficult job of recognizing the unexpected. I expect the unexpected, but I also expect the expected. I am not surprised to find evidence that contradicts a theory that many accept. But people have good reasons for accepting their theories, and so I am not surprised when observations are consistent with theory. Believers can easily find confirmation of a theory but have trouble recognizing contradiction of the theory. Nonbelievers can find contradiction but have trouble recognizing confirmation. For a skeptic, recognition of confirmation and recognition of contradiction come equally easily. One who is not a skeptic can be prepared to recognize both confirmation and contradiction by knowing contradictory theories without being wedded to any one theory. I will return to this need to know more than one theory.

Herbert Simon identifies what I think is the best comparative advantage of all. In his Nobel Prize acceptance lecture, Simon (1979) stated:

> It is a vulgar fallacy to suppose that scientific inquiry cannot be fundamental if it threatens to become useful, or if it arises in response to problems posed by the everyday world. The real world, in fact, is perhaps the most fertile of all sources of good research questions calling for basic scientific inquiry. (p. 494)

A person who recognizes the "good research questions" that arise in the real world and states them in tractable formulations without sacrificing their critical elements exercises the best comparative advantage of all.

I value this trait so highly because its possessor makes fundamental contributions to basic knowledge while also providing "useful and practical information," which is a legislatively mandated responsibility of Agricultural Experiment Stations (AES). Section 1 of the Act of 1887 Establishing Agricultural Experiment Stations (Hatch Act) states the Act's purpose to be ". . . to aid in acquiring and diffusing among the people of the United States useful and practical information on subjects connected with agriculture. . . ." Section 4 states ". . . one copy [of bulletins or reports] shall be sent . . . to such individuals actually engaged in farming as may request the same. . . ." The Agricultural Marketing Act of 1946 imposes similar responsibilities upon those of us engaged in marketing research.

Usually, we must formulate these new real-world problems (Ladd 1987, pp. 59–70), i.e., we must convert ill-structured problems (Simon 1973) into well-structured problems. Whenever you attack a novel real-world problem, you are likely to need to introduce some novel abstractions (to be called novel "ideal types" later in this article). One reason that it is a novel task is that the appropriate conceptual abstractions have not yet been constructed.

Notice that we in AES are charged with providing "useful and practical information" to "the people of the United States." Hence we must reach other audiences in addition to our fellow professionals. And it is our responsibility to take the initiative in reaching these audiences.

You can work on practical problems and have refereed publications. Two of my colleagues, C. Phillip Baumel and Marvin Hayenga, regularly publish refereed journal articles that report results of applied research.[2] A number of my refereed publications report work that I did in response to felt practical needs of people in agribusiness.

Intuition, Deduction, and Induction

Three kinds of mental operations that we use are intuition, deduction, and induction. (For convenience I use "intuition" and "imagination" interchangeably to mean "intuition, imagination, hunch, and unconscious mental processes in general.") You frequently must rely on intuitive judgments, so you should develop proficiency and enjoyment in exercising your own intuition. I know, we must not trust intuition too far because it is fallible and it does not prove anything about the real world. I also know that deduction (including mathematics) and induction (including econometrics) are fallible and do not prove anything about the real world.

[2] Their vitae support this contention. Each has authored or coauthored numerous refereed publications as well as extension reports, papers in trade journals, and reports to producer associations. As specific examples, Hayenga has published in refereed journals and in extension reports and trade publications on price-risk management, and Baumel has published refereed papers and extension reports and trade journal articles on restructuring rural road systems. Cooperative extension service publications, articles in trade journals, and written reports to producer associations (such as the National Pork Producers Council) can reasonably be assumed to present "useful and practical information" on real-world problems.

Deduction (Including Mathematics) Is Fallible

Science deals with two completely different kinds of entities and creates connections between them. Machlup (chapters 5–9) contrasted "ideal types" or "theoretical or pure constructs" with "real types" or "operational concepts" and contrasted the clarity of the "ideal type" of "price" with the ambiguity of the "real type" of "price of steel." An ideal type is not perfect; it is ideal because it is purely an idea; it is hypothetical, idealized, invented, exact. Real types are based on observation, experimentation, statistical procedures. An ideal type is part of the mind-created world. A real type is part of a mind-independent world which is the object of our theories. Corresponding to the two types of concepts are two types of truth: ideal truth (ideally true) and real or operational truth (or true in reality). This distinction between ideal and real types throws light on the debate that Levins initiated with his paper in *Choices.*

To do empirical or applied work we must find ideal types that correspond to the real types. The selection of the appropriate ideal type depends upon the nature of the problem under investigation (Machlup, pp. 244, 420–21). In econometric work we must match economists' ideal types, statisticians' ideal types, and the world's real types. Simon's (1989) activities of noticing and of recognizing are keys to successfully matching ideal types with real types. We have no rules of correspondence that tell us how to do this matching. I maintain, with Warnock, that finding or creating correspondences between ideal and real types is done by perception, imagination, intuition, analogy, metaphor, simile, and not by deductive logic (Ladd 1987, pp. 90–91).

Among the ideal types of our theory of consumer behavior are "prices," "quantities consumed," "income." After presenting five different definitions of income, Hicks (1950, p. 177) wrote, ". . . income is a *subjective* concept, dependent on the particular *expectations* of the individual in question" [emphasis mine].

Even such a simple concept as "retail price of a 16-ounce can of cut green beans that contains 8.75 ounces net weight of beans" is an abstraction. I once collected prices of canned foods in two grocery stores in Ames. The price of a 16-ounce can of cut green beans that contained 8.75 ounces net weight of beans ranged from 33¢ to 46¢ per can. The mean price was 37.5¢ per can. Which was "the price"? Which "price" did consumers who decided not to buy cut green beans not pay?

For quantity, do we measure consumption or purchases? Measures of consumption make more sense in the utility function, but measures of purchases make more sense in the budget constraint. In a study of monthly demand, how do we handle items purchased one month but consumed in later months? What are the imputed prices of vegetables from home gardens? How do we treat gifts?

In theorizing and conceptualizing we use ideal types; we create our own ideal types. If we avoid contradictions and logical errors, we cannot make false statements about them. They are whatever we say they are and our conclusions are ideal truth, but may be false in reality. Logical argument provides only hypotheses about real types. The hypotheses may be true or false in reality. A statement can be true and false in the same article: ideally true where a conceptual model is developed and operationally false where the model is used to study policy consequences. Contradictory statements can be ideally true if they refer to different ideal types that possess the same name but different characteristics.

It is impossible to prove anything about the real world by deduction because it is not possible to prove deductively that real types have the properties attributed to ideal types. This is so because it is impossible to derive logical conclusions on any subject unless one's premises deal with the subject (Kemeny, pp. 233–34; and Nagel, pp. 373–74). Any deductive proof that a real type possesses certain properties must start with assumptions that describe properties of real types. And how do we know the assumptions are operationally true? If we prove them from more basic assumptions, how do we know that the more basic assumptions are really true? Do we make another deductive argument from still more basic assumptions? If so, how do we know that the still more basic assumptions are operationally true? Either we continue this process forever, proving each set of assumptions from more basic assumptions, or we quit at some point by agreeing to use assumptions that are not proven but are accepted on grounds of reasonableness, plausibility, introspection, mathematical convenience, shared experience, or whatever. Consequently, logical conclusions that real types possess certain properties are ultimately derived from unproved assumptions. For example, the assumptions of completeness, reflexivity, transitivity, monotonicity, nonsatiation, and convexity of preference are used to demonstrate the existence of consumer utility functions, which are used to derive properties of demand functions. These assumptions have not been deductively derived from some more fundamental assumptions.

Because there is no logical necessity to believe the real truth of unproven assumptions, there is no logical necessity to believe the real truth of the conclusions.

True assumptions need not lead to true conclusions. If the assumptions individually present real truth but collectively present only part of the truth, the conclusions may be false. And if our assumptions present the whole real truth, we do not have a theory because we have not abstracted; we simply have a small-scale duplication of all the "blooming, buzzing confusion of reality" in all its incomprehensibility. For a graphic depiction of the fallibility of deduction, see Hofstadter (especially pp. 192–93). Also see Ladd (1987, p. 133).

The impossibility of proving anything about real types by deduction becomes more easily acceptable when we realize that we obtain contradictory conclusions from deductive arguments simply by changing from one set of plausible assumptions to another.

One implication of my argument is that "impressive" is not synonymous with "useful." A mathematically impressive paper may contribute nothing to our understanding of the real world.

Induction (Including Econometrics) is Fallible

Induction and econometrics also are fallible guides to truth and knowledge and cannot demonstrate that real types have the properties possessed by ideal types.[3] One reason is expressed in the Duhem-Quine thesis. This thesis is a consequence of the fact that every hypothesis is derived from several assumptions, not from one assumption. As a result, observations that contradict a hypothesis tell us that at least one assumption is wrong but do not identify the incorrect ones. Every statistical test is a test of a joint hypothesis that consists of all of the assumptions that were used in deriving the test, and a statistically significant test tells us probabilistically that some hypothesis is wrong, but does not tell us which one is wrong. It is easy to provide examples of the Duhem-Quine thesis. Johnston (pp. 214–21, 246–49, and 281–82) shows that heteroskedasticity, autoregressive errors, and errors of measurement can cause Type I or Type II errors. Theil (pp. 215, 326–33) shows how choice of incorrect functional form, exclusion of relevant variables, or use of false restrictions lead to specification bias and statistical errors.

A statistically significant outcome of a test of the hypothesis that consumer demand functions are homogeneous may arise from any or all of the following: (*a*) functions are not homogeneous; (*b*) consumers were not in static equilibrium; (*c*) they did not know all prices; (*d*) their preferences changed; (*e*) prices or quantities were improperly measured; (*f*) incorrect functional forms were used; (*g*) wrong levels of statistical significance were used; and (*h*) incorrect assumptions were made about the error terms. Hence, if you want to believe the hypothesis that consumer demand functions are homogeneous, you can find all sorts of reasons for refusing to reject that hypothesis even though statistical tests yield highly significant outcomes. As McCloskey (p. 487) observes, "Falsification is not cogent."

But failure to falsify is not cogent either. It is possible that failure to reject a false homogeneity hypothesis results from some of the conditions I just listed or from combinations of the conditions. For more discussion of Duhem-Quine, see Caldwell (pp. 126, 156–57) and Cross.

A little-recognized characteristic of hypothesis tests, which is independent of the Duhem-Quine thesis, is that rejection is probabilistically unambiguous, whereas nonrejection is ambiguous. For any selected critical level, a large (absolute) value of the test statistic, say b_1/s_{b_1}, leads unambiguously to rejection of the null hypothesis, $\beta_1 = 0$; it is inconsistent with that hypothesis (if we ignore Duhem-Quine). By contrast, an (absolutely) small value is consistent with $\beta_1 = 0$ but is also consistent with its contradiction, $\beta_1 \neq 0$.

Suppose we have chosen a 1% critical level, and the 99% confidence interval for β_1 is $.2 \leq$

[3] You may believe that we do not test hypotheses or models for truth but only for applicability or for predictive power. For you, I revise this sentence to read, "Induction is a fallible guide to applicability and to predictive power and cannot demonstrate that our ideal types are applicable to study of real types nor that our ideal economic types correctly predict behavior of real types."

$\beta_1 \leq .5$. This is inconsistent with $\beta_1 = 0$ and leads to rejection of $\beta_1 = 0$. Now suppose the 99% confidence interval is $-.2 \leq \beta_1 \leq .5$. This leads to nonrejection of $\beta_1 = 0$ because it is consistent with the null hypothesis. But it also leads to nonrejection of the contradictory hypothesis, $\beta_1 \neq 0$, because the confidence interval includes nonzero values. Thus, the test statistic that confirms the null hypothesis that consumer demand functions are homogeneous also is consistent with the alternative hypothesis and with all the models in which demand functions of rational, well-informed consumers are nonhomogeneous.

Because deduction and induction are fallible guides to truth and understanding and neither proves anything about the real world, I have a recommendation to students. You do need to remember what we professors say—at least remember it until you have passed our examinations. You do need to remember what we say; but you don't need to believe what we say.

Manuscript Review Process

The distinction between ideal and real types is not merely epistemological nit-picking. It has practical importance. Many journal manuscripts are rejected because the authors do not understand the distinction between ideal types and real types, nor the role of each in science. Two examples: (*a*) A person ignores genetics in an economic study of animal breeding. As a consequence, the economist's ideal types bear little resemblance to the ideal types that animal breeders have found that they need, and the economist studies a world that animal breeders and livestock producers do not recognize. (*b*) An investigator uses a complicated econometric procedure designed to compensate for the lack of certain data even though the data is available. The basic error of these authors, I believe, arises from the mistaken belief that anything that is ideal truth is also real truth. Philosophers call this "reification": confusing an abstraction with the real thing.

Sometimes as I review a manuscript, I find myself asking, "Is this a poorly written report of well-done research or an accurate report of poorly done research?" Obviously, I recommend that the paper not be published.

The manuscript review process might best be viewed as a stochastic process. Brorsen's guide to the publication process provides ways to reduce the random element in your favor.

You young people are wrong if you think that established members of the profession can get anything published. The rate of rejection of my papers is about the same as it always has been. Over the course of my career, about two-thirds of the papers I have submitted to journals have been rejected.[4]

Heuristic Reasoning

In addition to intuition, deduction, and induction, we also use heuristic reasoning. Polya (p. 112) has written, "Modern heuristic endeavors to understand the process of solving problems, especially the mental operations typically useful in this process."

The informed, rational judgment of productive scientists uses heuristics. One set of heuristics for problem solving consists of search methods. Chapter 2 of Hayes includes an informative presentation of proximity methods of searching for problem solutions (hill-climbing, means-end analysis, fractionation, and knowledge-based methods) and of finding problem solutions by pattern matching. The "recognition" that Simon (1989) found to be so important in scientific discovery often takes the form of pattern matching, of recognizing, e.g., that a current task can be described by the same logical or mathematical pattern that described a task that already has been solved. Cognitive scientists who have studied scientific discovery regularly write about heuristic thinking. Kulkarni and Simon wrote about biochemical discovery. But their rules are suggestive of general heuristics useful in any scientific research. They present heuristic rules for: choosing problems, generating problems, proposing experiments, setting expectations, generating hypotheses, modifying hypotheses, modifying confidence levels, choosing a hypothesis, and choosing a strategy. The psychologist Wicker presented four heuristic strategies for generating new insights: "playing with ideas, considering contexts, probing and tinkering with assumptions, and clarifying and systematizing the conceptual frame" (p. 1094).

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[4] This may be a typical rejection rate. One reviewer expressed the suspicion that "... *nearly all* agricultural economists' rejection rates are running about two-thirds of submissions ..." [emphasis in original].

Devoting time to serious study of the work of cognitive scientists on problem solving will benefit you at least as much as devoting equal time to study of economic journals.

When I was teaching a graduate course in linear economic models and was regularly using linear programming in my research, I found the simple treatment of symbolic logic in Kemeny, Snell, and Thompson helped me understand and devise proofs in matrix and set theory. Studying Solow's compact treatment of mathematical thought processes can help you understand and create mathematical proofs.

Theory or Metatheory?

To handle the great diversity and changeability of the world's real types that we encounter in studying practical problems, we need a variety of special theories designed for specific conditions. Consequently, I believe that we could benefit by spending less time on received theory and more time on developing and comparing alternative theories. One reason for my belief is the disturbing observation that many young peoples' knowledge—of consumer theory, firm theory, welfare economics, whatever—is an inch wide and a mile deep. Concerning the textbook consumer theory, they know all about reflexivity, transitivity, nonsatiation, strict quasiconcavity, monotonicity, homogeneity, Cournot and Engel additivity, symmetry, negative semidefiniteness, every boring detail, and they cherish every one of them. But they are not even aware of any other consumer behavior theory; they are ignorant of the contributions of Pfouts; Kalman; Tintner; Kalman and Intriligator; Pollak; Fox and Van Moeseke; and Becker. It is possible that the one theory they do know is worse than any of the theories they do not know. They cannot be sure that the one they know is better than any of the others because they have never compared their one theory with other theories. What is even worse than their ignorance of alternatives is their failure to recognize that it is legitimate and necessary to have alternatives and their lack of curiosity about what an alternative might look like. Their attitude seems to be, "Who needs two hypotheses? I already have one."

I think that these economists fail to distinguish ideal truth from real truth. And they are willing to accept the first ideal truth they are taught as being real truth. This reflects excessive faith in their professors. Have we educated these people by providing them such narrow training? Or have we brainwashed them?[5]

Another reason for recommending more metatheory: theorists are looking for qualitative answers from qualitative inputs, while we are usually looking for quantitative answers from quantitative inputs. In one way theorists have more freedom than we do. They can freely create their ideal types. We are restricted to study of ideal types that resemble real types in important ways. But in another way, we have more freedom. Theorists have powerful motivation to stick to situations that are simple enough to be logically and mathematically tractable. With our computer programs we can easily handle situations that are beyond the capabilities of their mathematics.

In our impatience to exercise our mathematical skills upon our assumptions, we ignore the fact that the assumptions have human implications. Because we are a social science, we ought to spend some time considering the human meaning of our assumptions. Spend a few minutes wondering, "What am I like if I am the kind of person that I assume everyone to be?" Let us use our textbook-consumer assumptions. The answer is that you are an unattractive person. An egoistic hedonist, your utility depends only upon your own consumption. You are completely indifferent to everyone else's consumption and welfare. Your diet is important to you, but you do not care whether other people are well-fed or starving. It is a matter of complete indifference to you whether others are homeless and naked during the blizzards of January or are sheltered and clothed. You have no friends; anyone who is so completely unaffected by anyone else's well-being cannot be a friend to anyone else, and to have a friend you must be a friend. You are also stupid, rational perhaps, but stupid certainly. The only things that affect your utility are things that you individually can buy in a market. You individually cannot buy clean rivers or clean lakes so, even though fishing or boating or skin diving may be hobbies of yours, you do not care how dirty the lakes are. You cannot buy

[5] One reviewer reacted to this by pointing out, "It is not possible to avoid constraints in one's graduate training . . . ." but then added, ". . . presumably all but the most unimaginative of us go through the experience of breaking out of the constraints of our graduate training."

a highway, so you are indifferent between taking an auto pleasure ride on a crowded freeway or on that empty highway that is used for TV commercials for new cars.

Economics has long been called the dismal science. The myth is that it acquired that title because of the dismal predictions of Malthusian theory: that population must outstrip resources. The correct reason for labeling economics as the dismal science is the dismal view we have of human nature.

Do We Reward Dishonesty?

This section is the outgrowth of a coincidence. Shortly after reading Manderscheid's "President's Column—Data Ethics," I watched a public television (WGBH) *NOVA* program entitled, "Do Scientists Cheat?" The answer was, "Yes." The next day I came across a paper by Telser in which he assumed (p. 29) ". . . someone is honest only if honesty, or the appearance of honesty, pays more than dishonesty. Hence, if someone thinks he can gain by dishonesty with impunity then he will be dishonest." (Other authors have made this same assumption.) What are the implications of these reports?

One implication is that we have dishonest people in the profession. Another is that we need to reorder our priorities to place more value on replication. When I was a student, we were taught that replication was a necessary part of the scientific process. Now anyone who tries to replicate is accused of duplication and hence of waste. Our motto seems to be, "Nothing worth doing well is worth doing twice." Such a situation places a premium on cheating. If I am an anxious assistant professor, or a full professor protective of my reputation, and I have a lovely theory that is not quite consistent with my empirical results and that would merit publication if they were consistent, I can "gain by dishonesty with impunity" by fudging the numbers. I will not be caught. Nobody will replicate my study; replication has no professional payoff. Replication would not prove that I cheated (even if I did). But it would protect the profession against being led into serious error by my dishonesty, and it would assure that I cannot build a favorable reputation upon lies.

If I were younger, it would make me unhappy to think that some scientists with whom I am compared when I am considered for promotion are dishonest and are developing a national reputation by cheating. High status is a scarce item. If the dishonest acquire it, there is none left for the honest.

We need development of new models and replication of tests also because there is spatial variation and temporal change in economic structure. Economic theories need to be tested under diverse social conditions. In their international comparison of macro models, Shapiro and Halabuk argued that proper econometric model construction depends importantly upon an economy's resources, industrial pattern, institutional arrangements, behavioral characteristics, and objectives of economic agents. Replication is needed over time to identify outdated theories because as Hicks (1975) stated,

> Our facts are not permanent, or repeatable, like the facts of the natural sciences; they change incessantly and change without repetition. . . . There are theories which at particular times are fairly appropriate, but which are subsequently rejected, or neglected, not because they have been superseded by a more powerful theory but because in the course of time they have become inappropriate. . . . (pp. 320–21)

Also, see Hutchison (chapter 11) on revolutions in economics.

Anyone who values intuition and innovation highly must also value replication highly. Why? Because our intuitions and innovations are affected by our values. Also, we usually publish only plausible empirical results and plausibility is also a product of our values. Without replication we see only the results that fit one author's set of values (Ladd 1983). Replication allows us to compare innovations and plausible results under different sets of values. Replication is also necessary to obtain objectivity. Ackoff argued,

> Objectivity is *not* the absence of value judgments in purposeful behavior. . . . *Objectivity is a systematic property of science taken as a whole, not a property of individual researchers or research.* It is obtained only when all values have been taken into account. It is *value-full,* not value free [emphasis in original]. (p. 103)

Without replication, it is impossible to take all values into account, hence, impossible to achieve objectivity.

Two Gems of Wisdom

I have two recommendations for you when work is not going well; it took me many years to formulate these. (*a*) Don't just sit there, do something; (*b*) don't do anything, just sit there. On the first: don't just sit, try something; draw a graph, write an equation, rewrite the equation, find an economic interpretation of the terms in the equation, list and define variables, list and define parameters, do something. If you don't do anything, you won't do anything right.

On the second: don't consciously think about anything. Quit trying so hard, relax, let your mind wonder, speculate, daydream. Establish "the core of silence that provides the best background for intuition" (Goldberg, p. 152).

A third secret of success is to know the rule that tells when to follow (*a*) and when to follow (*b*). Unfortunately, I do not know this rule.

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(Because this list is long, one reviewer recommended identifying a few of the most useful references. I believe that Brorsen; Hayes (chapter 2); Kulkarni and Simon; Ladd (1987); Machlup (chapters 5–9); Polya; and Wicker are the most helpful.)

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