Three interviews, fictitious in varying degrees, are used to explore some fundamental questions of economic methodology. The role of mathematics in economic reasoning, the question of truth in assumptions, and whether rigorous arguments will necessarily lead to important results are examined. The use of models in science is usually justified on grounds of prediction or explanation. The general theme of this paper is that many mathematical models in economics fail on both criteria.

Introduction

Last year I began to feel I had been spending too much time in the field teaching farmers and not enough time reading journals and keeping up with new research results. Resolving to do better, I took a brief study leave, dusted off a few copies of the American Journal of Agricultural Economics, and got to work.

Now mind you, I wasn't looking forward to this task. I'd never been one to question Carlyle's judgment that economics is the "dismal science." And even though the AJAE is widely regarded as the top journal in my field, the president of the American Agricultural Economics Association, when faced with a membership that couldn't read its own journal, chose a "let them eat cake" defense — the journal was "of more importance to writers than readers" anyway (Padberg).

So imagine my surprise when reading the AJAE turned out to be thoroughly enjoyable. Rather than worry us with predictions of gloom and doom, the economists I read seemed content to spend their time imagining worlds which, if nothing else, were thoroughly amusing. "Why this is no dismal science at all!" I remember thinking. "This is a whimsical science."

The first article I read promised to analyze "the implications of income, profit, and consumption taxes on the economic decisions of farmers." I was barely into the second page of the article when I read:

\[
\max_{c, L_1, L_2} \int_0^\infty \{u(c(t), H - L_1(t) - L_2(t))e^{-\delta t}dt
\]

subject to

(i) \[\dot{E} = \rho[E(t), L_1(t), v] + wL_2(t) + y(T) - c(t)\]

(ii) \[E(0) = E\]

All of a sudden, images of the farmers I had been working with came to me. I saw them finishing up a hard day in the fields with a bout of equation solving. You know, "Gol' darn, it, Martha. First the truck breaks down, then a sick cow, and now we've got to stay up all night solving this blasted equation." I couldn't stop laughing for the longest time.

The more I looked into it, the more often I came upon economists who claimed that farmers routinely tackled even the most intractable of equations. For example, the 1988 AAEA Outstanding Journal Article award went to the developer of an econometric model which required data "assumed to be generated by farmers solving a single-period maximization problem." The particular farmers in question were not, by the way, on sabbatical in the Harvard Mathematics Department; they were peasant rice farmers in a village in India.

Some more recent examples show that using irrigation requires solving not one, but
two, equations bristling with Greek letters and arcane symbols. And as for the mathematics of deciding when to apply nitrogen, well, let's just say it's not a pretty sight — five full journal pages that at first blush look like a flight plan to Jupiter.

An article on how cheating (moral hazard, if you prefer the parlor term) must be considered in designing all-risk agricultural insurance policies gives both farmers and insurers the usual complement of equations to occupy their idle hours. But it doesn't stop there. All of reality seems up for grabs. Here are a few highlights:

* All farmers are identical. This, coupled with the consistent reference to farmers as "she," could lead to some serious barroom brawls with the macho heroes of herbicide ads.

* Farm profits do not depend on government programs. This should go over big with farm-state legislators struggling to justify billion-dollar subsidies.

* Farmers are indifferent between income from honest farming and that gained by cheating insurance companies. We have apparently come a long way from the days of agrarian fundamentalism and the view that farmers were somehow good folks.

* Insurers have no preexisting wealth with which to finance claims. Perhaps a fantasy, perhaps a premonition of the S&L crisis — I'm not sure here.

These are but a few of the many surprising observations to be found in the article. Most are merely whimsical and, one supposes, harmless enough if not taken too seriously. A few, though, like that of farmers' indifference to cheating might best have been examined carefully before publication to avoid libel suits from farm groups.

I am also indebted to this particular article for introducing me to "stylized" facts. Its author briefly speculates that, "the gains from risk sharing in agriculture are smaller than the gains from risk sharing in other industries . . . especially given the presence of large government programs and active futures markets." As reasonable as this might seem, it "does not fit with the stylized facts of agriculture as usually perceived by agricultural economists" and is summarily dismissed from further consideration.

Now, I'm not going to try and convince you that economists in general, much less agricultural economists, don't have style. A visit to any annual association meeting clearly shows they do. But "stylized facts?" Perhaps a new journal, modelled after Gentleman's Quarterly or Cosmopolitan, is in order that can give these stylish facts the treatment they deserve.

Whims sometimes collide as journal articles pass in the night, but no one seems to care. For example, a recent AJAE article claims that only the amount of water a plant takes up determines yields. Forget the bugs, hail, and equipment breakdowns. In the same issue of this journal, another article claims that crop yields are a function of not one, but two things, water and nitrogen. The author of the first article pointed out that the case of water and fertilizer was tried, but "little insight was gained" and "the costs in terms of analytical complexity were substantial."

I'm the first to admit that a little disagreement never hurt anyone, and I'm the last to argue for consistency at all costs. I draw the line, however, at fairness. It hardly seems right that some farmers get by having to solve relatively simple single-period equations, while others must solve nasty-looking monsters that cover several lines and require knowledge of everything from now until infinity. This is, after all, a country in which all are created equal, and I think that should be respected.

From time to time, I've been tempted to show some of the farmers I've worked with the equations from our journals and ask if they really solve them. To be honest, I just don't have the guts to do it. No telling what might happen. Then, out of the blue, the AJAE announced that students, too, solve equations in order to allocate study time. I seized my chance.

The particular equation students solve is this:

$$
\max \Omega = \phi(L, M) h(z_1, z_2, w_1, w_2) - \left[ z_1 + z_2 \right] v(z_1 + z_2)
$$
Meanwhile, their instructors are struggling with this one:

$$\max \phi = \rho(L, l) \ast h(z_1, z_2, w_1, w_2) - [x_1 + x_2] \omega(x_1 + x_2)$$

As you might expect, the instructor's equation is more complicated than that used by students; it also has four mathematical constraints that are not shown here.

I prepared a questionnaire for the students in a graduate course I was attending on the philosophy of social science. All 16 students present that day, as well as the professor, agreed to provide data.

First, I showed them the equation they were supposed to have solved. My initial questions revealed not only had none of the students ever solved it; none had ever seen it before. Inquiring further, I learned that only two of the students thought they had the mathematical ability to solve the equation, but neither could recall having solved any equation whatsoever in allocating their study time. The instructor was no better. He had never seen his equation nor anything like it, and said he couldn’t solve it anyway.

I also wanted to check if the students had a well-behaved "learning production function" of the type suggested in the article. First, I asked the question, "If you study one extra second, do you always learn more?" One said "yes," 15 said "no." I also asked which best described the units in which they allocated their study time: nanoseconds, seconds, minutes, hours, or days. One chose minutes, one said hours sometimes, days others, and the remaining 14 said hours. To have functions of the type used in the article, you need what mathematicians call "continuity." The results I obtained don’t support the assumption of continuity at all.

One assumption buried among the equations and symbols seemed especially cynical — learning has diminishing utility. Hamburgers have diminishing utility, I suppose. The first one tastes great, the second is a bit less enjoyable, and the tenth one is just about more than you can handle. But learning? I have always hoped that those I taught felt as I do — the more you know, the more you value learning. So I asked the students directly. "The more I learn, the less I value learning — true or false?" One said true, 15 said false.

Surely, not all of economics could be as whimsical as this, I thought. I headed back to the library to get some copies of the even more prestigious American Economic Review. But on the way, I remembered that some well-known general economists, (Leontief and McCloskey came to mind), have from time to time come before our association. Their message, incredible as it now seemed, was that in comparison to AER and similar journals, agricultural economists had done a commendable job of preserving their connection to the real world.

What was going on? I was tired of reading and decided to ask some experts.

Dona W. Hardy

According to one of the papers presented at the 1987 meetings of the American Agricultural Economics Association, many people think professional journals are "filled with mathematical erotica" (Johnson). What constitutes the erotic is obviously a personal judgment, for search as I might, I find nothing remotely titillating in the back issues of my journals. What I do find, however, is enough mathematics to make my old calculus books seem readable by comparison. In fact, a few years ago when physicists met economists at a showdown over who had the fanciest mathematical models at the Santa Fe Institute (Pool), the underdog economists put up a surprisingly good fight. The economists were properly shocked when a leading physicist admitted (with apologies, one hopes) that he had stooped to experimentation rather than using proofs in trying to figure out how the world works.

Every article in my latest AJAE had enough symbols and such to keep hieroglyphically-impaired academic riff-raff such as myself safely at bay. Could all this math be disguising more whims, or, better yet, erotica? I would clearly need a translator to find out. The name Dona W. Hardy came to mind. I had attended a seminar she gave in Washington, D.C. on the
relationship between mathematics and logic a few years back. I could hardly understand a word she said, but couldn't help thinking Dr. Hardy was an impressive mathematician by any standard.

I'm not sure which was more difficult, getting Dr. Hardy to agree to speak with an agricultural economist about mathematics, or getting my department head to fund a trip to England when our budget was facing its annual pruning by the legislature. Miraculously, both were accomplished and I was on my way to meet her.

I don't know what I expected, but nothing can fully describe the sense of academic tradition permeating Assumption College. The mathematics department was in a centuries-old stone building covered, as you might expect, with ivy. Dr. Hardy's office was on the sixth floor of a building that appeared to have only five. A spiral staircase led to a room in a turret; floor-to-ceiling bookcases surrounded both her desk and the single overstuffed chair across from it. A photograph of an elegant-looking cat sat on her desk. A portrait of an even more elegant-looking gentleman, G.H. Hardy, dominated the only wall space not covered with books.

Dona W. Hardy, tall and thin, shockingly red-haired, rose from her desk to greet me. "My great uncle," she said. "We Hardys are a long line of mathematicians. Of course, he's the most famous, but both of my parents are mathematicians as well."

I asked if she had found time to look over the single issue of AJAE I had sent earlier. Not only had she read it; she seemed genuinely interested.

"I was absolutely amazed at what you economists are doing with mathematics," she declared. "I had no idea."

"Most of us in my profession have a lot of trouble reading the math in these articles," I said. "I don't suppose that was a problem for you."

"Actually, you're wrong about that. I had a beastly time reading most of it. There is so much mathematics to digest, and the text is a constant distraction. Just as soon as I would get one side of my brain engaged for mathematics, I'd have to stop and change gears for some digression on fertilizer."

"That wasn't the worst of it, though. When I finally got to the end of an article, I kept looking for what of significance had been said. I didn't find much."

"What do you mean?" I asked.

"I admit to knowing very little about agriculture, but I have studied education rather extensively. The article in your journal about teaching innovations was, in all honesty, the most bizarre article I've ever read on the subject. On the one hand, you have to struggle through clock-wise rotations of curves, and on the other, you have conclusions that are completely obvious. Why do you publish such articles, anyway?"

I allowed that such decisions weren't up to me, but I was sure there must be some good reasons. Trying to dodge further implications of responsibility on my part, I asked, "What do you think about the level of mathematics used in the journal?"

"I found it completely uninteresting. That's understandable, though. I'm only interested in mathematical results, not applications of hundred-year-old techniques. Applications in general are not highly regarded around here. I once fell in love with a physicist and my parents almost disowned me. To this day, I hope my father's heart attack wasn't a direct result of my bringing Abraham home for his approval. Well, you didn't come all the way to England to hear about me."

"What is your area of specialization?"

"Abstract algebra, group theory, that sort of thing. As best I can tell, it has no application, but you can never be too careful these days. Everybody wants to apply mathematics. It's ruining a perfectly beautiful subject."

"I see. What else struck you about the journal I sent?"

She thought for a minute, then said, "Well, there is one other thing. Remember, now, I'm no expert on agriculture, but the way everything from continuity to convexity to differentiability was assumed at the drop of a hat bothered me a bit. These are all very technical concepts, and I would be surprised if they very accurately described much in agriculture. I had the feeling
that a lot of the assumptions were being made solely so that mathematics could be applied."

"That may be," I admitted. "But as long as mathematics is used, at least we can be assured of accurate results. That no doubt motivates much of the work I have shown you."

Dr. Hardy surprised me by responding, "There's no reason to expect accuracy from this sort of thing. Let me give you an example from this short paper on poultry production that was stuck in the journal you sent."

So that's where that paper was! It must have slipped in there by accident. "Drumstick Production in a Mississippi Chicken Processing Plant: A Mathematical Approach" by Dr. L.S. Legus may seem simple now, but 15 years ago it created quite a sensation. While some jokingly refer to L.S. Legus as the ChD, the "Doctor of Chickens," most credit him with fathering the revolution in methodology that changed Poultry Husbandry into Poultry Science.

In his seminal paper, Legus recognized what everyone knew, that each chicken processed would yield two drumsticks. To use his notation,

\[ D = 2 \times C \]

Of course, the problem facing the processing plant was not one of seeing how many drumsticks they would have if a certain number of chickens were processed. Rather, they needed to know how many chickens to process to get a certain number of drumsticks. The paper was the first to use mathematical methods to derive the now-famous Legus Equation:

\[ C = \frac{1}{2} \times D \]

What had originally been a tedious trial-and-error method of trying to guess how many chickens it would take to get a certain number of drumsticks became a matter of applying a single formula.

"This approach, while in some sense mathematically correct, can easily lead to wrong answers," she continued.

"That's a very famous result," I said. "How could it give wrong answers?"

"Simple. To get 25 drumsticks I'd have to process 12.5 chickens. Chickens don't come in halves. The answer is wrong."

"Maybe in some technical sense, but not practically," I objected. "All you need to do is round to the nearest whole chicken."

"Perhaps," she said thoughtfully. "You see, we seldom face those problems in pure mathematics. We use real numbers that don't need rounding. In pure mathematics, at least, I'd be very careful about rounding anything off. Strange things can result. How would you round off 12.5?"

"To 13, of course," I responded.

"Why not 12?" she asked. "It's as close to 12.5 as 13."

"Obviously," I responded, "because you wouldn't have enough drumsticks. Everyone knows that."

"Maybe everyone. But not mathematics. Mathematics knows nothing about chickens, and that's why you can't trust it to give you a right answer outside of the world of pure mathematics."

I was starting to get worried for Dr. Legus. He had recently received a large grant from the Highway Department to study road-killed poultry. The project was considered a stroke of genius. Legus turned road-kill study on its head by proposing to study causes, not effects. While everyone else looked into ways to dispose of dead birds, Legus proposed to use mathematical models of poultry behavior to determine why chickens crossed the road in the first place. But if mathematics had nothing to say about chickens, what would happen?

I tried to change the subject once again. "I heard your seminar on mathematics and logic a few years ago. It seems we might be more careful about accuracy when doing computations, but most of our work with mathematics is of a different nature. In most of our work, mathematics is a logical tool which guides our reasoning about economic matters."

"I object to the way you are using the term 'reasoning,'" she said. "'Rationalizing' is more accurate. You see, since mathematics has nothing to say about any of the topics you apply it to in your journals, it can't lead to any conclusion that is not first arrived at by other
methods. You first state a problem, then determine a conclusion, then fill in the middle with mathematics. But if you didn't already have the conclusion, you would have no idea where to take the mathematics. That's why I call it 'rationalizing'. And mathematics is so abstract, that virtually anything can be rationalized in this way. I wouldn't call any of it reasonable, though.

"As I was reading the articles, I couldn't help but think that this might explain why they came to such trivial conclusions," she continued. "The mathematics is not only offering no help in finding surprising results, it positively gets in the way of rationalizing even simple ones. As obvious as some of the conclusions are in the articles you showed me, it was clear that someone had gone to a lot of work to get even that far with mathematics."

"What would you have us do?" I asked. "Trust your own profession more and mathematics less. After all, I'm not coming to you in search of ways economics or poultry science might tell me more about mathematics. Why should you be coming to me thinking I can be of more help to you? I'm sure you can think of better ways than mathematics to describe and reason within your profession. Save the mathematics for the simpler tasks, like counting chickens."

Simple things, indeed. Wasn't she aware of Dr. Legus's article, "Counting Chickens Before They Are Hatched: A Probabilistic Approach?" Probably not, I surmised. The plane ride home was a long one, and I couldn't get Dr. Hardy's claim that articles in the journal led to trivial conclusions off my mind. She didn't seem the type to recognize an important finding in agriculture anyway, I thought.

I turned to the next article, and there in the abstract I read, "Thus, conservation may be a key to solving resource scarcity problems." Two articles later, I learned that, "the evidence indicates that agricultural banks that failed in the 1980s took relatively high risks and suffered the consequences in an agricultural downturn." Later, "multinomial logit techniques," whatever they are, revealed "older farmers are less likely to use computers and educated farmers are more likely to use them."

As I read still more seemingly obvious conclusions, the AJAE was working its dependable cure for insomnia. Drifting off to sleep, I remembered something that happened to me way back in the dark ages when I was just starting out as an extension farm management specialist. I was making a presentation to a bunch of rough-looking farmers in Florida and apparently said something that was obvious to all but me. Worse yet, I said it as if it should have been news to everyone. One crusty old cowboy up front, his eyes squinting under his cap, looked up and, in a loud voice, said, "The hell you say." It left a lasting impression on me.

Michael D. Root

What makes practitioners of the whimsical science choose one whim over another? Certainly not considerations of what is true, because their imaginations are freed from such concerns. And how can "rigorous arguments," the style of choice among fashionable journal articles, lead to meaningful conclusions if they start with whims? In hopes he might help me with these questions, I visited my friend Michael Root.

Dr. Root is the Director of Graduate Studies at the University of Minnesota's Philosophy Department. His long-standing interest in economic theory has given us a chance to co-direct a few graduate students. I've also sat in on the Philosophy and Social Science course he teaches and have reviewed his excellent manuscript, The Liberal Sciences: Seeking the Facts and Hiding the Values.

We met for lunch in the University of Minnesota's faculty club. As we reached the end of the serving line, he politely asked the cashier...
to put both meals on his bill. One of my longest held beliefs from economics — there is no such thing as a free lunch — had been seriously challenged, and we hadn't even started our interview.

Dr. Root began by thanking me for letting him borrow my most recent copy of the *American Journal of Agricultural Economics* and suggested we discuss an article that developed a general mathematical model for studying technologies that conserve inputs. When applied to irrigation of crops, the model showed that introducing a pollution tax is likely to reduce both water use and pollution. The conclusion may not have been too surprising, but there was more than enough mathematics in the article to qualify it as rigorous in my book.

"One thing puzzles me," he began. " Barely three sentences into the description of the model, the authors choose to look at only two irrigation technologies. I don't know that much about agriculture, but aren't there more ways to water crops?"

"Yes," I admitted. "Even in the example used later in the paper, four types of irrigation are considered. But as the authors clearly say, they're only trying to simplify the problem they have at hand."

"I see." Root now directed my attention to the next paragraph of the article in which it was assumed there was only one crop, that returns to scale were constant, and that there was only one input used in growing the crop.

"Do farmers usually have only one crop they can possibly grow?" he asked.

"Actually, this study was done in California where there is tremendous variety in crop production," I answered.

"Do you think the authors were aware of that fact?" he innocently asked.

"No doubt. I'm sure that this assumption, too, was for simplicity."

"Ah," he said. "I'm just guessing now, but isn't it generally the case that farmers can affect water use and pollution by changing crops?"

"Of course," I answered.

"Can I then gather that a whole range of solutions to the pollution problem have been eliminated for simplicity?"

"It would seem that way," I had to admit.

"And don't constant returns to scale rule out any argument a person might make that small farmers use techniques that pollute less than those used by larger farmers?" he continued.

"Yes, it does," I said, "but the authors say right here in the footnote that it doesn't matter because all of the farms in the area were large anyway."

"Isn't it conceivable that this could be changed?" he asked.

"I suppose. Are you suggesting that not considering farm size was also a matter of simplicity?"

"Now you're getting the picture," he said with satisfaction. "Can you see how this would also be the case with the assumption that there is only one input, water?"

This was a good point. Here in Minnesota, at least, much of the research on controlling pollution from agriculture involves sustainable systems which vary practices on a system-wide basis. Changing only one input is considered a fine way of missing the whole point of environmentally benign farming. When I explained this to him, he pointed out that once again a whole class of options for dealing with the pollution problem had been eliminated.

"So, what the article says 'for ease of exposition', that only water matters in crop production, carries a high price tag in terms of contradicting facts and in the way it limits how to look at the problem. In my field of philosophy, ease of exposition has never carried much weight as a value."

No argument there. One of the few things I remembered from the philosophy classes I took in college was that the more difficult someone's writing was to understand, the more highly regarded it seemed to be.

At least we had gotten through page two of the article. But, alas, we were a long way from being in the clear. For at the top of the next page, the assumption was made that the farmer was a pure profit maximizer.

"Why do you think this assumption was made?" he asked. "Do the authors think farmers should be completely greedy and act with complete disregard for the well-being of their neighbors? I suspect that at least some
agricultural economists, farmers too, for that matter, go to churches that teach otherwise."

"The authors don't come right out and say why they chose this way of looking at farmers, but it seems likely to me that it was done to simplify the analysis. I doubt the authors think farmers really act this way, but treating them like they do makes life a bit easier for the researchers," I surmised.

"I see," he said. "And how about the mathematics they use to find the maximum profits. Does it have any assumptions built into it for simplicity?"

"Yes," I explained. "The assumptions are so common they are not always spelled out. But without them, analysis of this type would be difficult, if not impossible. The assumptions, of course, are not strictly true."

"What do you make of the way selecting an irrigation system is predicated upon the farm being able to cover land rent?" he asked.

"It's a standard and obvious assumption," I said.

"Does the article offer any general way of saying whether such costs will be covered, or is the issue of farm survival also tossed out the window of simplicity?"

It seemed to be, I granted. As we continued our journey through the article, he stopped me every paragraph or so to ask questions. At last, we made it through to the conclusion that "in sum, introduction of a pollution tax is likely to reduce water use and pollution."

"Now this is interesting," he announced. "We made it all the way to a conclusion such as this in a supposedly scientific article and, until I have missed it, have never once made reference to anything factual. In fact, several things that are readily admitted as false figure heavily into the chain of reasoning that led to the conclusion."

"What does that matter?" I asked.

"At the very least, the conclusion should read 'If there are only two irrigation systems, and if there is only one crop, and if farm size doesn't matter, and if only water use affects crop production, and if farmers act only out of greed, and if the calculus invented for use in physics also applies to farmer behavior, and if the tax doesn't cause the farmer to go out of business, then introduction of a pollution tax is likely to cause one farmer to choose the one (of two) irrigation system that reduces water use and pollution.'"

"And there's more," he went on. "The authors don't mention it until the conclusion, but we must also add three more ifs to the list: if land quality does not vary in a field, if there is no problem that all farmers taken together will act just like any one farmer does, and if nothing important changes over time."

"That's quite a mouthful," I said. "Maybe they chose to say things as they did for simplicity."

"If so, I object," said Dr. Root. "The reason is that the matter being discussed is one of consequence. Someone might actually take the study seriously and add to the tax burden of California farmers. I suspect the farmers would find this rather distasteful."

"Yes, but at least pollution would be reduced," I reminded him.

"You miss the point. There is no way to know from this study if pollution would be reduced or not by adding to farmers' tax bills. What if farmers had been characterized differently? What if they were assumed to be concerned about pollution resulting from their operations and were doing everything they possibly could to prevent it while still staying in business? In this view, taxing their irrigation water would leave them with less money to carry out their desire to minimize the pollution resulting from their operations. And the study gives us no evidence whatsoever to support one view of farmer behavior over the other. All we know is that one is simpler."

"How should we regard the conclusion that raising taxes will reduce pollution, then?"

I asked.

"Not as having been shown to be true, because it follows from several premises admitted to be false. I suppose many economists may find the tax-effect to be somehow self-evident, a matter of faith, so to speak. But anyone who needed convincing will find no facts to back up the claim, only a long list of curiously simplifying assumptions. In no sense, however, should the relationship between taxes
and pollution be characterized as having been supported by objective study."

Even though Dr. Root was convincing on this article, I thought he might be overlooking one of the mainstays of research in agricultural economics, empirical work that rests solely on data, not whims. While it is common to have a whimsical introduction in an article, many sooner or later get down to objective analysis of data.

"Do you remember the teaching article I used in the survey of your students?" I asked him.

"Yes," he replied. "Your results did little to support the assumptions used in the article."

"But there was a second part of the article in which data were used to support the conclusions. Did you have a chance to read that?"

"Yes, I did. It, too, was interesting. As I recall, the mathematical model was based on variables for which no data existed. Rather than stop there, the article resorted to 'proxies and subjective data'. These data were collected for only one class, and then only at the end of the class, even though changes in learning were the main focus of the paper. Are practices such as these common?"

"Compromise is the price you pay when working in the real world," I said.

"So, thanks to compromise, there are data on cumulative grade point average, test results, and ethnic background of students. By the way, why were these data converted to logarithms?"

"The authors don't say," I answered, "but it is common to try various functional forms in a regression to get a better fit with the data."

"Does the R² of .387 reported in the article mean a good fit was obtained?"

"No, the author admits it was not especially high." Actually, I thought it was shockingly low. The tests of variable significance weren't very encouraging, either.

"But still he pressed on?" he asked.

Pressed on, indeed. Dr. Root and I reviewed the regression results together. Some variables behaved themselves the way good statistics should; others didn't and were unceremoniously expelled from further consideration. A negative correlation turned up between learning and minority status, both U.S. and foreign. Dr. Root perked up.

"A significant result at last! Teaching in the classroom is clearly biased against minorities!"

His excitement faded as I explained this was hardly the point of the paper. Instead, we went on to the hypothesis concerning how students allocate their study time to innovations. There was not much in the way of data here, either. Where continuous variables of infinite range were employed in the theoretical model, the students surveyed had only four choices: Disagree Strongly, Disagree, Agree, and Agree Strongly. We turned our discussion to the statistical analysis of these data.

There were four variables the theoretical model claimed would be important. Of these, the first two were not significantly different from zero. No matter, though, because they were otherwise "consistent with theoretical expectations." The article assured us that the statistical problem arose from weaknesses in the data set, not the theoretical model. The third variable was "consistent with theoretical expectations and is significant at the 10 percent level."

"Couldn't there be data problems with the third variable, too?" asked Root.

"I suppose," I said, "but that was the only one of the four variables that acted as expected. The fourth variable was worst of all — it was statistically significant and of the wrong sign."

In light of the unrealistic assumptions of the theoretical model, the statistical problems encountered, and the author's own concerns about the one-and-only data set applied, both Michael and I expected a somewhat tentative conclusion at best. Not so — "The empirical results support the notion that economics plays a role in learner time allocation."

"Well, I've got a class to teach, so I'd better run. There's one last question, though. What would it have taken for the empirical results to reject the hypotheses of the article?"

He had me on that one.

Thomas J. Jackson

Although I never knew him well, Tom Jackson has always intrigued me. I took a
philosophy course from him as part of my undergraduate program at the University of Florida in the late 1960's. Some 15 years later, I returned to the University of Florida on the Food and Resource Economics faculty. I was surprised to hear he was working with the Economics Department to establish a Center for Study of Economic Methodology. As things turned out, I moved to Maryland before the project really got off the ground, and never heard much about the center.

Professor Jackson seemed an ideal candidate to discuss some of the philosophical questions left hanging by the whimsical science, so I decided to look him up. After some effort, I located him at his modest house in the Cross Creek area of Florida, just south of Gainesville. He was tying his Ranger Bass Boat to the dock in front of his house as I greeted him. Tom was much the way I remembered him - a little grayer in the beard, a little more tan, and a little broader at the belt line - with one major exception. Where his left hand used to be, there was an awkward-looking mechanical contrivance.

"Gator," he said. "A great white one, at least 16 feet. Got me late last year in the dark of night while I was reaching down to land the biggest bass I've ever seen, much less caught. Twenty pounds, maybe more. Damn gator took my hand, the fish, everything, and was gone as fast as he came up. Thank God I had a buddy with me and the Ranger here is a fast boat. Otherwise I'd have bled to death."

"Not that it's any of my business, but aren't you a little worried to be out fishing, knowing that gator's still out there?"

His eyes directed mine to the 12-gauge up under the front seat of the boat. "I've changed my equipment since then," he said wryly. "Come on up and have a beer with me. It's hot today."

We settled down on the screened porch and began our discussion. I showed him two predictions I'd brought along. The first was from Countess Sophia Sabak's column in the Weekly World News. She advised "Lovelorn Guy in Syracuse" that "You'll meet the woman of your dreams on February 14. Her initials are J.G." The second was from a flyer for "noted New Age researcher" Brad Steiger's book *Overlords of Atlantis and the Great Pyramid*. In the book, Mr. Steiger predicts that Atlantis will rise during this decade.

"Predictions play a big role in discussions about modern economics. What is the difference between predictions like these and those which economists so often use to justify their methods?" I asked.

"I'll tell you what the difference is," he answered. "These predictions are a lot better than anything you're going to get out of your typical academic economist. At least you can tell if these predictions are true or not. Just wait until February 14 or the end of the decade and see what happens. It's that simple."

"Not so with economists. If one of their predictions turns out to be true, they go running around with a smug 'I told you so' written all over them. But if things don't fall as planned, they give you dozens of reasons why the real world didn't behave itself, or their 'everything else being equal' didn't hold water - anything but their models failed, and failed miserably. During my two years at the economics department, not once did I hear anyone say something like 'We cooked up this ridiculous theory, but nothing we got out of it made any sense, so we dropped it'. Not once."

I must admit, I expected a slightly different response from a man who devoted so much time to setting up a Center for Study of Economic Methodology. "How did your efforts with the center turn out?" I asked.

"What center? After two years of working with the only people in the world who think all issues of scientific methodology are either resolved or uninteresting, I took an early retirement rather than continue. It cost me, not getting in those last few years, but it was more than worth it. And besides, if I'd landed that big bass, I could have made more money endorsing crankbaits and coolers in a month than a philosophy professor makes in ten years."

"You mean your decision to retire here was basically an economic one? You weighed the costs and benefits of continuing, discounted the risk factors associated with catching big bass, and maximized your utility accordingly?"

"Listen here. One more crack like that, and this interview is over. You understand? It
was just that kind of nonsense that I could never stand. I fish because I like to fish. That's it, pure and simple. And the last thing I want is some economist coming along and telling me otherwise."

Now he really had me worried. "Please forgive me, but such notions could predict your presence here today, couldn't they?"

"The Countess could have predicted my presence here today, too, I suppose. So what? The idea that predictions are all there is to science went out with the nineteenth century. At least for everyone but economists. They still seem to think Milton Friedman said all there was to say."

"If not predictions, then what is it we should be after in science?" I continued.

"Explanations. Why things happen. What causes things to happen. Understanding. In short, all the things that go out the window when you focus exclusively on one-upping the Countess."

"Can't you both explain and predict?"

"Only if you haven't given yourself a license to fictionalize the world. Now all sciences do some of this, granted. It's called using 'ideal types'. We studied that way back when you were a student. You remember, using frictionless surfaces in physics, that sort of thing."

"But these idealizations," he continued, "while not completely accurate, show respect for reality. What your man Friedman did was take things overboard. For him, since no statement was going to be completely accurate, why worry? Just assume anything you want."

"What's the problem with that?"

"For one thing, you lose your ability to explain anything. Imagine going before some Congressional Agricultural Committee with this story: 'Well, it's like this. We have more wheat this year because the first order conditions of the multiperiod maximization problem, given limited inputs and convex risk surfaces, indicate that, all other things being equal, there should be a rise in output'. Where there should be an explanation, we only have gibberish."

"What economics has set itself to doing is the lowest of all scientific pursuits, that of building a barometer. A barometer will tell you when a storm is coming, but tells you nothing about why the storm is coming and does nothing to increase your understanding of storms. The big difference is that at least barometers are reliable, which is more than I can say for economic predictions. Even the big corporate economics shops are closing because no one can predict any better than the Countess. Meanwhile, economists go on justifying all manner of methodological outrages based on their predictive power."

I somehow thought a fisherman like Tom would show more enthusiasm for barometers. "Can't barometers also be used to predict when fish will be feeding?" I asked.

"Some people think so, and they'll get no quarrel from me. But you don't seriously think whoever invented the barometer had so much as seen a trophy bass, do you? The relationship between barometric pressure and fish feeding was learned by crafty bassers, not deduced from some high theory."

"Of course, there's probably some economist out there right now devising a theory of how bass maximize the utility of feeding subject to the risk of getting caught. Once we figure out what equation it is and get as good at math as a common bass, wham!, you know what a fish should be doing to meet her goals, and then she's as good as in the boat."

"Sounds crazy, you say? Do you know what an economist who should have known better told me back in my center days?"

I admitted I did not.

"That birds know physics, otherwise they couldn't fly. And here we have whole departments trying to teach college students what birds already know. Yessir, everybody's pretty well hard-wired in the world of economics. The same method applies to choosing fertilizer as to choosing college courses, to boardrooms as to bedrooms. Nothing is sacred. It's a damn insult, that's what it is. An insult to Creation and Creator alike."

"I've never heard an argument against economic methods quite like yours."

"Yes you have," he told me, "if you would only listen. It all goes back to Thomas Carlyle and the dismal science. All you economists seem
to think Carlyle nailed you because of someone or another's dismal predictions. That's not how I see it at all."

"You see, the Industrial Revolution in England was Carlyle's worst nightmare. He saw society becoming increasingly inhuman, measuring every obligation by money and profitability. When he put his thoughts in writing as the Latter-Day Pamphlets, he managed to offend nearly everyone. Even his friends thought he had taken to whisky."

"In the first of the Pamphlets, an essay on slavery with the unfortunate title the 'The Nigger Question', he argued with the dismal economists who thought how much work people did was no more than a matter of supply and demand. Carlyle's religious background held that people had a moral right to work regardless of what economics might say. For him, people had a calling higher than the laws of supply and demand, and governments should strive to more than letting people alone."

"No, I don't think Carlyle found the economists' predictions dismal. It was the attempt to discover laws like those of physics that governed the behavior of the individual that was so dismal. And, if he could see the farmers, students, and everyone else who solve equations, thereby revealing their behavior to the skilled economist, he would no doubt say economics is more dismal than ever."

It was getting late, and I knew our interview would soon be over.

"Darkness coming," he said. "That's where you'll find the big ones. I'm going fishing. Besides, you probably won't be able to use any of my rantings anyway."

"Oh, I don't know about that." He was already walking toward the boat when I called out, "Good luck, and watch out for that gator. No telling what equation it's going to be solving tonight."

Conclusion

I still look back fondly to my boyhood in rural Florida. My friends and I sat around the radio for hours, listening for our favorite song — "My Heros Have Always Been Ag Economists":

*I grew up dreaming of being an ag economist And working on computers all day, Solving them problems that nobody cares about And assuming the others away."

How could police officers and fire fighters compete with heros such as these? we asked ourselves as we dressed in discarded ties and made complex-looking calculating devices from old cardboard boxes.

When Willie Nelson revised the song to one about cowboys, I suppose I should have known something was amiss, but boyhood dreams can be amazingly persistent. As for my graduate student days, there was so little time for music that "Mamas Don't Let Your Babies Grow Up to Be Ag Economists" went by completely unnoticed. How my life might have been different had I heard even the first verse!

"Mamas don't let your babies grow up to be ag economists.
Don't let them make assumptions and drive them state cars,
Make them be cowboys and tenders of bars."

Throughout my readings and interviews, two questions about the whimsical science bothered me the most. One was "So what?" and the other was "Why?"

"So what?" seems seldom addressed by whimsical scientists. They are much more likely to discuss the rigor of their arguments. But I see little value in rigorous arguments, mathematical or otherwise, that can't reach beyond trivial conclusions. That other conversational standby, predictive power, also seems to run aground on the reefs of triviality. So what if something can be predicted, if it is not important? And how, I ask myself, are arguments based on whims ever to be persuasive? Even if the argument somehow comes to something true, can the suspicions that surround dowsers and astrologers ever be completely put aside?

As for "Why?" my sad conclusion is this: The entire method of whims is geared toward the publishing of articles. No wonder academics, pressured as they are to "publish or perish," are so eager to jump on the whimsical honey wagon. The world is viewed as being completely determined by mathematical laws; messy observations and data collection become unnecessary; and the vast part of an article can be devoted to whims and rigorous manipulations of them. The subjects of the research, farmers or otherwise, are confined to acting "as if" they were
cynical cartoon figures. "Simplicity" is a virtue of the highest order. And, lest we forget, the most obvious of conclusions are fair game if they are presented in a sufficiently pious way.

While this may be an ideal environment in which to write articles, it does little for those who try to read them. Commenting on the *AJAE*, a recent president of the American Agricultural Economics Association noted that "many (most?) members can’t or don’t read it becomes less of a bother." On this point, I disagree – it is a bother.

What’s to worry about is best illustrated by a letter a non-academic member of the American Agricultural Economics Association wrote to the president in 1990. The writer was putting the association on notice that this would be the last year in which he would pay dues. He sent a copy to me, and, with his permission, here is an excerpt:

"I would be more inclined to pay the $60 (dues) with a smile, however, were the *AJAE* a little more relevant to those of us out here in the real world, instead of being dominated by a cast of misplaced mathematical economists and statistical theoreticians for whom agricultural problems are just convenient examples for use in their various unintelligible, irrelevant, almost universally unrealistic models. They may need to publish this nonsense to advance in the academic circles where this sort of thing is thought to be important; that’s too bad for them; but it is no justification for diverting the bulk of the space in our perfectly good journal to these sterile and recondite exercises in methodological obscurantism, making the rest of us into victims of their ‘writing’. It is a failure of leadership to allow this to continue."

Must we bid farewell to whims? I raise this question with some sadness, for the cost of a whimless economics will be frightening if measured in terms of foregone professional amusement. Arguments will have to go back to the old-fashioned ways of Logic 101 and begin with true premises. Common sense, long held in contempt by modern methods, must be restored to respectability. And mathematical modelling, with its seemingly insatiable appetite for whims of the "simplifying assumption" variety, will have to play a much smaller role in the methods chosen by economists. Without whims, economics would require simple methods for dealing with complex reality, not complex methods for dealing with simple reality.

Voltaire’s story of *Candide* is one of my favorites. After a long and tormenting journey, Candide and his companions settled down to farming. In this setting, Martin says, "Let us work without theorizing, 'tis the only way to make life endurable." There follows one last flourish of philosophizing by the good Dr. Pangloss, to which Candide replies, "'Tis well said, but we must cultivate our gardens."

And so we must. But that, I'm afraid, requires experience and common sense, not the rigorous application of whims to imaginary problems.

**References**


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