Confessions of an Economic Gadfly[*]

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How do I work? Hard and long. Why do I do it? I don't know, but then there are many things I do for reasons unknown. Actually, I am not totally the directionless, clueless, person the above answer suggests. I have a number of conjectures about why I work hard and long. One is that I'm an inquisitive person who, like my three-year-old, keeps asking "Why?" until I come up with *an answer that satisfies me*. Combine that inquisitiveness with a dogged persistence that abhors fudges in answers unless they are called what they are—fudges—and you have the makings of a gadfly like myself.

The Yeah Criterion

In explaining what I mean by “an answer that satisfies me” I could discuss the nature of satisfaction, the Duhem Quine Thesis, proofs, refutations, and lines of demarcation in this essay, but that would be misleading since what I mean by “satisfy” is guttural, not intellectual. A satisfactory explanation for me involves an inner sense—an intuition—which tells me “Yeah, that’s right; that the way it works.” I will call it the “Yeah criterion.” For an intuitive economist the “Yeah criterion' is central. [1]

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[*] I would like to thank Harry Landreth, Michael Szenberg, and Tom Mayor for helpful comments on earlier drafts.

[1] Tom Mayor pointed out to me upon reading this essay that the Yeah criterion is similar to Fritz Machlup’s “ahaness.” I suspect that it appears under other names for other intuitively oriented economists and scientists.

In no way am I saying that the Yeah criterion is a criterion of truth. I recognize that what makes sense to me is structured by my training, my biases, and my vision of the world. As I learn more, my common sense changes and what is a satisfying explanation changes—sometimes the unsatisfying becomes satisfying, and sometimes the unsatisfying becomes more unsatisfying. For that reason the Yeah criterion is not a stand alone criterion; for it to work requires an understanding of the literature and the thinking of both past and present experts. As I read the literature I often discover that some problems that have bothered me have bothered researchers before me. This is why the history of thought and literature studies have been so central to my study of economics. In earlier writers I can often find pointed discussions of the problems I am having with the intuition, and explanations of why they did what they did.

Intuition, Ego, and the Yeah Criterion

For the Yeah criterion to work, one needs an enormous ego, and an ability not to be influenced by the crowd. Most non-egotistical people will reason, often implicitly, that if an explanation is good enough for the enormously bright individuals who have considered an issue previously, it is good enough for them. In considering issues, I try to keep such reasonable considerations from my mind, and avoid letting other people's acceptance of an argument—either positively or negatively—influence my consideration of that issue.

For example, the standard cost curve analysis in the textbook does not meet the Yeah criterion for me, and I have been working off and on for the past 15 years to understand why it doesn't in a way that I can explain to others. My intuition tells me that Jacob Viner's famous mistake—telling the draftsman to do the impossible—was not a mistake, but was instead a misunderstanding by Viner about the existence of discontinuities as one moved from the long to the short period. [2] Viner wanted a smooth transition between the two, while his intuition was dealing with discrete jumps. If I am correct, Viner's recantation was misplaced, and a reconsideration of what structural aspects of the basic model of the firm will make his goof no goof at all. That reconsideration will give us a better understanding of numerous microeconomic issues.

The standard AS/AD analysis is another analysis that did not meet my Yeah criterion and my continued attacking of the standard AS/AD analysis (most recently, Colander 1995) has led many economists to consider me non-mainstream. But, if accepted (a big if), my reinterpretation of AS/AD analysis will play a role in changing the profession's thinking about what the central aspects of the Keynesian revolution are.

I have no great sense that acceptance of my ideas is imminent; changing established beliefs, especially when they are deeply built in and little thought about, is not easy; it requires a strong reliance on and belief in one's understanding. I expect most gadflies rely heavily on their egos and their Yeah criteria.

The MIT and Chicago Approaches to Economics

I think the majority of people in the world approach understanding using something similar to my "Yeah" approach. Most contemporary economists, at least in their stated methodology, don't, which is why I am considered a gadfly. Actually, I

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[2] For those who do not know of Viner's mistake, it was telling the draftsman to draw the short run marginal cost curves through the minimum point of the short run average costs curves and to simultaneously draw them through the point where the short run cost curve is tangent to the long run average cost curve. Scholar that he is, Viner left the mistake for all to see, along with his admission that it was a mistake, when his famous article which set up the standard cost analysis was reprinted. See Jacob Viner, (1931).

should clarify the above statement since the intuitive approach is often associated with the Chicago approach to economics, and, while that Chicago approach is in decline, it is still around, especially at the introductory level of economics. In fact, I suspect that many people are attracted to economics because of its ability to give one "Yeah" highs. (This is especially true of those who learn "Chicago economics" early on, as I did.)

I quite agree that Chicago economics is wonderful at producing superficial Yeah highs. In fact, if you really get into the Chicago model, you have the Yeah sense for everything you look at. A well-trained Chicago economist can explain everything with a simple economic model.

But like many highs, for most people the highs from the simple economic model wear off, and doubts start emerging. The problem is the Chicago model explains too much. There are other factors that are determining what happens, which should fit into the explanation, but don't. When this realization hit me, as it did in my junior year in college, I was ruined as a Chicago economist. I had lost the faith.

I believe something similar happened to the economics profession over the last seventy-five years. The non-mathematical intuitive approach lost favor as it became associated with laissez-faire policy recommendations that were claimed to come from economic theory. The claim that laissez-faire policy conclusions followed from economic theory did not fit an informed person’s Yeah criterion. But since many intuitive economists said they did, economic researchers went about showing formally that the intuitive economists were claiming far too much for their intuition and for laissez faire.[3]

[3] J.B. Clarke’s relating of marginal product and justice is an example of the type of problem that existed. Obviously not all non-mathmatical intuitive economists have believed that markets solve all problems. For example, in the early 1900s institutionalism was strong. But, by the 1920s the more doctrinaire laissez faire economists were an important part of the inner circle of the profession.

As this formal work showed the major failings of earlier economists' intuition, formal work acquired a higher and higher stature. Intuitive understanding based on informal models was looked down upon. For lack of a better term I call this formal approach "the MIT approach." It understands the economy through simple, but formal, models. In the 1990s this MIT approach has replaced the Chicago intuitive approach except in a few market niches. (With the death of George Stigler, and with Milton Friedman moving to the Hoover Institute, the MIT approach has even largely replaced the Chicago approach at Chicago.)

Thus, in the 1990s the MIT approach is the mainstream economic approach, and any intuitive approach to understanding economic issues that carries over from the early economic courses (one of the niches where the Chicago approach still is strong) is frowned upon and discouraged. Most economists have it brainwashed out of their minds. Those few who do not succumb to the brainwashing, and who continue to approach economics using the Yeah criterion, are selected out of the profession by the institutions that determine who advances and who doesn't. The Yeah criterion doesn't cut it with most journal editors or tenure committees.

The MIT approach is, in my view, sterile and highly limiting for most economists. By eliminating, or at least significantly surpressing, the Yeah criterion, it eliminates the passion in doing economics and instead directs economists' goals toward financial gain and institutional success. Economics becomes a job, not a vocation.

The above discussion will get me in hot water with both Chicago and MIT economists. Chicago economists will argue that their approach has no ideological slant, and MIT economists will argue that the MIT training does not diminish intuition—it simply tries to raise the level of intuition to a higher level. I won't argue with either side here, other than to say that an approach must be judged not by what its best practitioners say it is, but by its fruits—what does the standard person trained in that tradition come away with. Judged by their consequences, I have no trouble with either of the above judgments, nor do I think a neutral observer will have a problem with them. In fact, Robert Solow recently said as much—that the problem with economists today is that they don't use their intuition enough. (Solow 1994.) I agree. What Solow will object to is my argument that it is the MIT approach to training that has eliminated that intuitive approach.

Let me give an example of what the MIT approach does. A while back I went to dinner with some economic professors. At dinner I was describing the reasoning behind the market anti-inflation plan that Abba Lerner and I had been working on. An MIT-trained economist asked me if I had a formal model of it, and when I said "No," he said that he couldn't discuss it. For him, the Yeah criterion was irrelevant; understanding had to go through a prism of a formal model. In the MIT approach the standard student comes away with a belief that if an issue doesn't have a formal model, it cannot be discussed or thought about.

When I have pushed MIT economists on the role of intuition, they agree with me that economists should be able to deal with issues on an informal intuitive level, and those who cannot are bad economists. They point to economists such as Paul Krugman and George Akerlof who combine both. I agree, Paul and George are superb economists. They can rise above the models, because they have superb intuitions, and a different vision than many other economic researchers. But they both play the game by MIT rules. What's modelable guides their research and their intuition. They have made important contributions, but imagine what they could do if their intuitions were freed from the formal modeling shackles.

I think that contrasting my approach to studying economic problems with that given by Paul Krugman in his essay in this book is a useful way of seeing the difference between my approach and what I would call the best of the MIT approach. I'm the extreme opposite of Paul. Modelability, for me, is a technical issue to be dealt with only after one has chosen what to study by the Yeah criterion. It’s a way of demonstrating, checking, and refining what one already “knows.” I deal with ideas on an intuitive level, not on a formal model level. Formal modeling, for me, is useful to answer fine points, not to create and understand theory.

Paul follows the MIT approach; he understands things such as the importance of non-linearities and increasing returns, and then puts them to the model criteria. If it doesn’t make the model criteria, it doesn’t meet his understanding criteria. That’s why many intuitive economists don’t see Paul’s work as innovative, and they see him as claiming far more originality for his work than it deserves. In the MIT approach, he is correct; in the intuitive approach his critics are correct. I follow the intuitive approach and put existing models to the intuitive understanding criteria. If a model doesn't make intuitive sense it must be wrong, and I focus my work on explaining why.

Simple formal models that MIT economists find so enlightening often grate on my intuition. True, they may be an improvement on the existing simple, formal model, but often they simply add one new twist formally—a twist that informal, intuitive, economists have long understood. Moreover, often the intuitive economist will have recognized that the relevance of this particular twist can only be understood by adding seven or eight additional twists concurrently. The MIT approach doesn’t see an issue in an alternative way unless it is in a formal model, whereas I see any simple formal model as far too limiting to the twists I intuitively believe are necessary.

Put simply, I do not believe that most of the economic events I am analyzing can be explained by a simple, formal model without the addition of enormous institutional detail that simple, formal models cannot accommodate. Krugman argues the MIT line that we should "simplify, simplify." I follow Einstein—"Models should be as simple as possible, *but not more so*." My vision of the economy is one of complexity, and any explanation that fits my Yeah criterion must incorporate that complexity, or at least tell me why the complexity isn't going to affect the analysis. When I try to conceive of a general mathematical statement of the economic problem, I come up with an extraordinarily complicated set of interrelated dynamic equations that lead to chaotic, super-non-linear dynamic models.

The MIT vision sees it as possible to reduce that chaos--without formally modeling the institutions--to simple, formal models with linear dynamics, and deterministic results. They have to do so to arrive at a tractable model. Tractability runs roughshod over intuition, and creates a set of models that, for me, do not meet the Yeah criterion. The only way I can see an economy such as ours as working is with institutions limiting changes and creating some stability out of chaos. Somehow in the educational process of children enormous limitations on individual's choices are placed on them by institutions and social pressures. Society shapes us to fit into a workable marketplace. Whenever I see analyses—such as the standard analyses of production or of distribution—that don't include that shaping process and the institutions that play such an important role in shaping us, I cringe. I cringe a lot when reading economics.

The Possibility of Trade between High Level Theorists and Intuitive Economists

The problem with simple, formal models is that formal models constrain one's intuition. They embody within them implicit assumptions that one doesn't even know exist. The mathematics one uses in those models is a language and languages are limiting. There are two ways to confront this problem. One way is to delve deeper and deeper into the math—dealing with the complex issues in a highly abstract way so that the few implicit assumptions that remain are clear. Some of the complex game theoretic work fits this approach, as does some of the recent work on chaos and non-linear dynamics. At that level one can integrate one's intuition with formal modeling and the results can be impressive. The models that such economists develop are far from the simple policy-oriented MIT models that Krugman exalts; these are models that have no policy implications because, either, they generally have no analytic solution —at least not yet, or they are so abstract that they have no obvious relation to reality.

Relating such abstract, formal models with real-world observations is extraordinarily difficult, and for most people, impossible. Thus, while I try to follow the work of modern researchers such as Buzz Brock, and look to it for inspiration, I make no pretense of dealing at that level myself. I go to the other extreme and deal informally with loose ideas that better fit observed reality, but which oftentimes hide logical relationships. Such specialization opens up the possibility of trade and ideally, economics would have two types of economic researchers making trades—formal theorists dealing with highly complex and abstract analysis almost devoid of institutions, and intuitive institutionally-based theorists dealing with real-world institutions and informal abstract analysis. The MIT approach of simple formal models would make sense if there were not increasing returns to scale in research, but it seems obvious that there are increasing returns, so not to take advantage of them and not to encourage specialization is, in my view, a highly inefficient approach to understanding. If you are going to be formally abstract, then go all the way and don't let the real world issues contaminate the purity of your analysis. If you are going to be concerned with the real world, don't formalize more than the least precise real-world element. To do so is to violate the law of significant digits.

To make sure that I am being clear, and to get me in as much trouble as possible, let me state my position more bluntly. I would say that the MIT economics approach has played an important role in bringing economics to its current sterility. I say this regretfully because I also believe that MIT economics has played a significantly positive role and that it was necessary to get the blatant ideological aspects of earlier intuitive economics out of the models.

My Road to Becoming a Gadfly

Having arrived at the view of simple, formal models described above, I found myself in a difficult position in my graduate work in economics. I did fine in the mathematics they taught us, but I was not an ultra-mathematician, and did not want to be one. I had been attracted to economics by the intuitive understanding it gave me of events, and its ability to supply me with yeah highs. But I had rejected the Chicago Creed that the market was inherently good and beyond question.

Faced with my disillusionment with both the Chicago approach and the MIT approach I was in a bind.—a bind that I resolved initially by not considering it. Instead, I focused on more immediate concerns, such as getting my dissertation done, and getting a job. That meant following the MIT approach which, interpreted down to a third year graduate student level, meant that the best, quickest, way to a dissertation was to take a simple, formal model and permutate it.

Optimal taxation was hot at the time, and Ned Phelps and Bill Vickrey two of the most interesting professors of economics at Columbia, were interested in it. So it seemed like a good idea to write a dissertation on optimal taxation, especially since they would allow me to write three essays which I could easily translate into articles. The math in my essays would look impressive and the topic was hot. It was the perfect combination for a thesis. It wasn't a very good thesis, but I soon had two essays done, and was working on my third and final essay. That was in 1974; I was on my way to becoming a mainstream economist.

The decision to become a gadfly was made, as are most decisions, in a sequentially rational way. The first step along the path occurred in 1974, when one of those defining events of one’s life happened. While I had suppressed my intuitive approach to economics, I had not totally annihilated it, and one day I was sitting around thinking about inflation, trying to understand why we were having so much inflation and what could be done about it, when I conceived of an economy in which there were property rights in prices.[4] In such an economy individuals wanting to change their nominal price would pay someone else to change their nominal price in the opposite direction. Only relative price changes would be allowed in such an economy; inflation would be impossible. It was an intriguing idea to me since it allowed the society to control the price level, but it left all relative prices free to fluctuate.

The idea led to numerous questions such as: What price would these rights to change price sell for?, and: How would that price vary with change in aggregate demand? I played around with the idea in my mind for a while, and one afternoon sat down and wrote it up in a piece I called "The Free Market Solution to Inflation." I sent copies around to a few people, including Bill Vickrey. I soon got a letter back from Bill (with a

[4] I often try to conceptualize fundamentally different systems as a way of gaining insight into our current system; thus I have worked through in my mind multiple goods-monies economies, economies in which all consumption is joint and all production is individual, and economies in which production, not consumption, is the goal.

copy to my chair) telling me the idea was brilliant. Now it isn't often that one gets such a letter from one's advisor who himself is an innovative economist, and it led me to make a fateful decision: to dump my thesis on optimal taxation (which was almost finished) and to expand this short paper on the free market solution into a thesis.

Actually, the decision wasn't quite so gutsy as it sounds; I explained the situation to Vickrey and asked him if he felt I could finish a thesis on the topic in a year, the time I needed to have it done if I was to stay at Vassar where I was then teaching. He said I could. I then went to Phelps and told him that Vickrey felt I could expand the paper into a thesis in a year, and asked if he felt it was a reasonable plan. He also said yes. So essentially, I had gotten tentative approval from both my advisors before I began.

And a good thing, too, because a year later, when I handed in my thesis, it was done, but not very good. The title was "Microeconomic Stabilization Policy for an Economy with Simultaneous Inflation and Unemployment." It was provocative and imaginative; it was also vague, incoherent in parts, and incomplete in others. Still, they let me through, perhaps because they had made almost no criticism when I handed in successive drafts. Whether this was because they hadn't had the time to read the drafts carefully, or because they didn't know how to comment on such a vague and incoherent thesis, I don't know.

The only real hurdle I faced was my oral defense, and luckily for me, Sidney Morgenbesser, a well-known philosopher who cared little about formal economic models, was one of the outside examiners. At the beginning of the defense he suggested that the rules be changed—that the thesis looked like something Vickrey had worked out and that we should have Vickrey, not me, defend it. This provoked laughter and pleasant discussion, leaving little room for piercing questions.

I suspect that that thesis decision set me on my gadfly path, because while the thesis wasn't much, it contained the seeds of the ideas for most of my later work. Many of those seeds are still germinating, which gives you an idea of how incomplete the thesis was.

The chance to plant the seeds of new ways of looking at problems is something that few modern economists have, and I am eternally grateful to Bill Vickrey and the almost-directionless Columbia Ph.D. program for allowing me that chance.

Of course, seeds of ideas aren't going to get one a job, so I still had the job problem to deal with. I should have been scared to death, but in my immaturity, and with my almost total lack of knowledge about the system, I wasn't. After all, Vickrey had told me my paper was brilliant, and when I asked if he thought I could get it published in a top journal, he had responded, "Yes—no problem." I started to get worried when I got my first rejection (from the AER); it wasn't even polite. It said, essentially, that the paper was garbage, poorly written, incomprehensible and wrong. After a couple more rejections, I began to suspect that I was in trouble, and that maybe that fateful step into following, and trying to develop, my intuition, was a step into a deep abyss.

I began to consider other options; I was selected as a Brookings Policy Fellow and went on leave at Vassar to work at the G.A.O. on cost analysis, one of the many areas my thesis had touched on. I did a study there that argued essentially that, technically, it was impossible to distinguish a fee from a tax, and that when handing out limited entitlements, one had to base the fees on scarcity costs, not on costs as they were currently being interpreted in the law.

I further argued that when scarce entitlements were involved, costs could only be defined in relation to demand—since the value of the scarce resource was determined by demand. Demand elasticities had to be taken into account in allocating joint costs. While my arguments made good economic sense, they were not what most politicians wanted to hear, and I quickly discovered that I did not have the temperament to play the political economy game in government. So much for that option.

I suspect that many gadflies arrive at a similar stage in their careers, and leave the profession. I certainly considered leaving it, and at that point I seriously considered going to work for a management consultant firm. The pay was much better, and the likelihood of success much higher. I might well have done that too, had it not been for Sidney Weintraub and Abba Lerner.

With a Lot of Help from Friends

While a Brookings policy fellow I intermittently continued my work on my free market solution to inflation, but it wasn't going anywhere fast; I gathered another couple of nasty rejections, which told me what I know knew very well— that the paper didn't have the right form—that it talked about an idea without a formal model! I now knew that was not allowed in any mainstream journal. It was at that point that I began considering non-mainstream outlets.

One place I looked was to the Post-Keynesians. Sidney Weintraub, together with Henry Wallich, had a TIP (tax-based incomes policy) proposal that was something like my free market solution to inflation. The difference was that theirs was a tax-based policy, and mine a market-based policy. Theirs was designed as a policy that would work; mine was designed as a theoretical policy with no concern about how it would work out in practice. Sidney was also editor of the *Journal of Post Keynesian Economics*, and as a last resort I sent my paper there. He did not reject it outright, but he did reject it as too abstract, and too theoretical. He suggested I write a new paper which emphasized

TIP more, and then touched on my idea of a market plan. I jumped at the chance, and got a paper accepted. My academic life was not a total failure.

I met Sidney later, and liked him personally, but on economics we didn’t agree on many issues. There were major differences in our thinking about anti-inflation policy; he was concerned about practical matters and my concern was about the way nominal price setting institutions could be integrated into a general equilibrium system: thus his analysis of TIP was partial equilibrium; mine was general equilibrium. He also focused on wages and he took it as given that a wage/price markup had been, and would remain, constant. My proposal focused on value added, and I argued that one couldn't take any wage/price markup as constant when imposing a policy affecting wages. Despite our differences Sidney was generally supportive. I think it is important to note that gadflies exist in the profession only because of nurturing of existing economists such as Sidney. I will be forever grateful to him.

The second fortuitous event was a seminar that Brookings held on TIP, which occurred because Art Okun was there and was interested in TIP. Unfortunately, I was not asked to prepare anything since Art saw my work as off in left field. He was concerned with politics and getting something implemented. He felt, I suspect rightly since his political instincts were impeccable, that my discussion of a new market in some abstract concept would have killed the practical hopes for the TIP plan. His focus was on policy. I was disappointed.

Nonetheless, that Brookings seminar in April was another turning point in my career. The reason was that Abba Lerner came, I think almost uninvited, and discussed what he called WIP (Wage Incomes Policy). This proposal was very similar to mine in that it was a proposal to control inflation by creating property rights. A major difference was that his proposal was a modification of the wage-based TIPs and hence it focused on wages, not prices. A second difference was that he was interested primarily in practical issues (to the degree that Abba could be concerned about practical issues), while I was interested in the underlying theory and what it implied for macroeconomic theorizing.

After the seminar, Ned Phelps introduced me to Abba and told him I had a proposal somewhat similar to his. Abba nodded. Actually, I believe, I had sent Abba a copy of my early proposal, but I doubt he had read it, or if he had, that he had thought much about it. A couple of days later Abba was on the program at the Eastern meetings and I decided to attend; we spoke briefly and I outlined the differences between my proposal and his. He was pleasant, but otherwise noncommittal.

The next time I saw Abba was at the AEA meetings in Chicago. TIP was politically hot then, and there was a session with Abba, Sidney, and Henry Wallich, who was on the Fed board. The room, which held 300 or so, was full; I sat there listening, depressed that my work was being ignored. At the end of the session I got recognized by the chair and asked Abba three questions that I felt showed the weaknesses of his analysis and the strengths of mine. One seldom gets answers at such events, and I didn't, but at least I had had my say, and I felt better for it.

I was presenting a paper on the general topic at the last session of the meetings. There were two people in the audience—friends of the presenters. But then, right after the beginning of the session, in walked Abba. He sat down and listened to the presentations and afterwards came up and said that he had been thinking about some of the questions I asked, and that he thought that I might be right. He said that we should talk. I was delighted, and asked: "When?" He responded, "Now," and so he and I spent the next three days holed up in a Chicago hotel room, arguing technical points about our anti-inflation plans, and talking about economics in general.

At the end of the three days I had convinced him that my value-added price control approach was more general than his wage control approach, and that I had thought of a number of issues and nuances of the idea that he had not. His openness and total commitment to understanding was both unexpected and delightful. We could talk about highly abstract ideas that didn't have formal models. He suggested we should do a book together, spelling out the ideas we had discussed. I asked, "When?"; he said "*This* December," so that December I flew from Europe, where I was a research fellow at Oxford, to Tallahassee to work on the book. There, I rose at 6 AM every morning, and we worked until 9 PM. I'd write a draft; Abba would rework it, and we'd continue working like that throughout the day. Although Abba was in his 70s then, he still lived and breathed economics. At the end of December, a draft of an article and of the book was complete.

Abba and I got along fantastically; our views of economics were almost identical. We differed primarily in two ways: First, Abba was an unabashed utilitarian, and I was not. Second, while he was much more interested in the idea as a practical policy than I was, he was amazingly naive about politics. I was far more politically aware than he: he felt good ideas rose because they were good; I, at that point, was far more jaundiced, and felt that everything happens because it is in the relevant people's interest, and good ideas are often not in the relevant people’s interest.

We were, however, quite different in temperament. Abba was the perfectionist who would work over every word and phrase; I was interested in the grand conception—the specifics were simply a boring job that had to be done. This difference in temperament made the collaboration even more fruitful, and it was a delight working with him. (His wife Daliah, made it even more of a delight; she put up with us and made the technicalities of life disappear.)

In writing jointly with someone, one of the two must have final say, and given our different positions, it was clear that Abba would have final say. This presented no problem on most issues, since we agreed, but there was one area of disagreement where our conception of what we called MAP (the market anti-inflation plan) differed, even after long discussion. Abba saw our market anti-inflation plan as simply a way to control the price level, and that when imposed, the price of changing price would quickly go to zero, since all people would be doing would be setting relative prices. I argued for a quite different conception in which the nominal and real sectors were intertwined and that, depending on aggregate demand pressure, there would be a different equilibrium level of output with each different equilibrium price of raising price. Abba saw a knife edge equilibrium, except for frictions; I saw a multitude of equilibria, and the likelihood that given existing institutions the aggregate equilibrium the economy reached was an excess supply equilibrium.

This was a major theoretical difference. My interpretation required a radical rethinking of macro theory since it meant that the nominal price setting institutions influenced real economic variables in a systematic way. His interpretation saw MAP as fitting in nicely with existing macro theory. It was simply a way to control the price level.

In our joint work we followed Abba's conception; in my individual work I spelled out my conception and we continually discussed the differences when we were together. The last serious discussion we had about it was in England in 1980 where we were attending a conference on TIP. That evening we sat around arguing and I related the idea back to Abba's seminal 1934 article on degrees of monopoly. It was as if a light bulb went off in his head, and he finally tentatively agreed with me. We agreed to discuss it more when he returned from Israel, which was the next leg of his journey. Unfortunately he had a stroke in Israel that impaired his language ability, and we never had that conversation. Happily, he remained physically well after his stroke, but we never again could work together. We remained close friends until his death in 1982.

The importance of Abba to my career as an economist is inestimable. It is entirely possible that I would have left the profession had not Abba picked me up, encouraged me, and made it possible for me to publish. The reception accorded to our joint work was fundamentally different than the reception my solo work got; it was considered; people actually talked about the idea, even though it didn't flow from a formal model. The reason was that Abba was known as the generator of odd schemes, and was also seen as an icon from the past. But Abba was also known for being quite impractical and far out, so the idea was not taken seriously. But, at least, it was discussed, and it made a nice follow-up to discussions of TIPs—it was the market equivalent to TIPs, just as marketable permits for pollution are the market equivalent to pollution taxes.

Abba lived and breathed economics, and would take me around with him to the inner-circle cocktail parties where the insiders of the profession meet and informally talk economics. It is here where the old-timers meet the newcomers. Abba would introduce me to people, and say very nice things about me. Sometimes that introduction would cause people to remember my name, and treat me a bit differently. I became known as Abba's protégé and many thought that I had been his student. Put simply, Abba made it possible for me to exist in the economics profession.

But while Abba had access to the inner circle, Abba was not an inner-circle economist; he never was; instead, he was a tangential iconoclast who in his old age was adopted by the profession much more than he was when he was young.

In many ways Abba was a bit of an embarrassment to the inner circle of the profession in that he continued to come up with politically hopeless schemes to improve the efficiency of the economy. It was rumored that Abba was on the short list for the

Nobel Prize—I suspect our work on MAP played a role in his not getting the Prize, since MAP was far too controversial, and he would likely have used the Nobel speech as a podium for telling people about it, and claiming it was the solution to society's ills. That is not what I suspect the inner circle would have wanted from a Nobel Prize winner. Thus, Abba's high regard for me was both a blessing and a curse since, if Abba liked me, I, too, must be a tangential iconoclast. It was a curse I could live with.

My association with Abba catapulted me from struggling outsider to a small-time, known, but somewhat strange, gadfly economist. Being so known within some circles meant that I could get published reasonably easily within a restricted range of journals. It also led to my being offered an endowed distinguished chair at Middlebury College, which I accepted. I have enjoyed teaching there immensely.

Abba and I worked together for only four years; Abba had his stroke and I was on my own again. But life after Abba was quite different than life before Abba. The chair gave me some influence and respect, as well as a small budget to run conferences. At these conferences I tried to bring economists of different persuasions together and to look at issues from a slightly different perspective. They focused on ideas, not models. Those conferences, and the volumes I edited based on them, gave me a chance to meet, and carry on, the acquaintances I had made while with Abba, and to develop an independent reputation as a non-partisan heterodox economist.

The move to Middlebury also helped change my research agenda—from one focusing on abstract theory to one focusing on teaching and methodology. The reasons for this change were twofold. First, when I came to Middlebury, I tried to teach an upper-level course on the micro foundations of macro; I had three students sign up, two of whom were totally mathematically unprepared. Moreover, the passion of the one who was mathematically prepared was for music, not math. (She has since become a professional harpsichordist.) It was clear to me that if I were going to keep an active research agenda and be a good teacher, these two areas of my life must be combined. So I began concentrating more on what I call "the translation problem"—reducing the high-level theory to teachable models that convey the essence of the high-level theory.

As I studied this issue I became convinced that, as a profession, we were doing a horrendous job in that translation, and that the models we taught to undergraduates were not the models we believed, and that the empirical work we were doing and teaching students, was, not the way we convinced ourselves of the validity of propositions. The recognition of this conflict led to my work on the profession which has been far better received by the profession than my theoretical work has been.

As I was doing this work on models I discovered that I had a knack for textbooks. Textbooks gave me a wider forum for my ideas, and were profitable. I have come to believe that what goes into textbooks probably plays a more important role in the future direction of the profession than just about anything else economists do. Textbooks' tone and the vision they convey play an important role in selecting who chooses to continue studying economics, and what vision they carry with them. It is in textbooks that the foundation of the future of economic research is laid.

With each successful book and article more offers to write come in, and I am now at the stage of my career where I am having to learn to say no to invitations to write and speak. I am learning to do this in an attempt to maintain my sanity and my creativity. The reason I say the latter is that there is a perverse connection between the requests one gets to write and how well known one’s views on a subject are. The better known one’s views, the more requests to express them one gets. So I am now trying to follow the philosophy of turning down most request to write on the profession where my views are relatively well known among economists, and am concentrating on areas where my views aren't well known, and where I will likely get rejected. My latest work on the macrofoundations of micro falls within this classification. That work, however, meets my Yeah criterion so I will predict that within ten years some variation of it will be all the rage in macro. When I've had my say there, I will then turn my attention to the cost problem and to showing the profession that Jacob Viner's intuition was right after all.

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Confessions of an Economic Gadfly

by David Colander

Middlebury College

This paper is an invited essay for a book, to be edited by Michael Szenberg, entitled Passion and Craft, How Economists Work. The purpose of the book is "to get a sense of how creative economists choose their research topics, how they go about their work, and the processes that set them apart form less creative individuals."

Probably my biggest difference with most economists is that I have a different vision of the economy, and hence of the grand laws governing the economy.

Most mainstream economists

How I Got into Analyzing the Profession

How I Got Into Economics

I have not always been in search of "yeah." My first experience in economics was not useful in providing "yeah" highs.

I never planned to go into economics, either as a graduate student or an undergraduate student. It was serendipity that got me into the profession. In college I was a lousy student who got good grades for a lousy student.

Learning for the sake of learning was something quite foreign to me; I was in it for the grades, and maybe a tidbit or two for a mixer (the quaint name we had back then for dances that weren't really dances but places where one met or mixed with members of the opposite sex). I worked at a job 40 hours a week when I went to school; classes were a sideline, something to do between living and working. I didn't do as well as I wanted in my first economics class—I think I got an A-. One of the reasons was that I tried to reason things out rather than learn them from the Samuelson text. One event sticks in my mind. On the exam, I put quantity on the vertical axis and price on the horizontal axis. I wish I could say it was from deep understanding that since quantity demanded was the independent variable and price was the dependent variable, the axes belonged that way, but it wasn't; instead it was simply because of insufficient studying and lack of concern for what went on the axes. Nonetheless, I got the relationships right and the reasoning right.

But the professor marked it wrong and wouldn't change it when I argued with him. He said I should have done it the way the book did it. That's really the first time I thought about going into teaching. I would let the ability to reason, not the ability to replicate the text, determine the grades.

I didn't think much about going into teaching; I was going into business and make a million dollars. I only really started thinking about graduate school because of the Vietnam War.

In September of that year I got married; my wife had won a Rhodes scholarship so I used my connections with Phelps and Vickrey to get a research appointment at Nuffield College. Since I had resigned my job, I needed some money, so I agreed to write a report on tax -and market-based income policies for the Joint Economic Committee, and also agreed to edit a book on *Solutions to Inflation* for Harcourt Brace. These projects came about partially through luck, and partially through contacts. I think it is important to anyone studying the profession to recognize how important these are. One does not make it on one's own; one makes it because other people push you ahead—because you convinced the right people you have something important to say, or that they otherwise want to help you. For someone whose uncles or parents are well-known economists, these contacts come easy; for others, such as me, they come like stray light beams. A good contact doesn't develop often, but the possibility of a contact increases the more time one spends in economics. Thus the fact that I work long and hard, and am almost continually involved in economics activities increases the possibility of contact. Getting out of a liberal arts school—Vassar—and spending two years at Brookings and Oxford was also an essential element of exposure for me.

Ninety-five percent of the people you meet will nod pleasantly and continue on automatic pilot; if you send them a paper afterward, it will at best get skimmed. Ideas, approaches, need to be sold, and the more different the approach, the more it needs to be sold, because it is very difficult for people to get out of their traditional ways of looking at things. People who operate within another's framework, doing a piece of the puzzle that the other knew needed doing, but either hadn't gotten around to it, or was too lazy to do it, will find it easy to get their work read. The more different one's approach, the more costly it is for someone to get into it, and the less likely they are to consider it. Thus, making a jump in thinking is extraordinarily difficult, and it seems as if the whole profession is against one. I don't think there's any inherent bias; there's simply a large lumpy cost of deviating from one's established framework, and most are unwilling to spend that cost unless, somehow, they have been convinced that the payoff will be high. They will seldom be convinced by an article, because they won't read it. However, if in conversations where they can get someone to focus on the problems they are thinking about, and show how a new framework can add insight to those problems, they might be intrigued enough to look at an article and start to consider the idea. These costs differ among individuals; there are some what might be called multi-dimensional thinkers who, because they use multiple frameworks generally, can jump in and out of different frameworks. Others are uni-dimensional thinkers who can be superb at understanding an issue in one framework but who find it extremely costly to enter into other frameworks. The more technically framework-specific one's work is, the more costly it is to adopt another framework.

Thus, a large part of the work of a person suggesting a different framework involves what might be called "sales"—getting individuals with lower costs of switching frameworks to consider their framework. If they fail in that, they will fail in the profession.

To do this it is easiest if one is a multi-dimensional person who can translate between frameworks, but such skills are rare.

and do the broad thinking. They are too busy initially getting tenure and advancement.

Thus, when you have someone like Krugman, who is enormously creative and insightful, who works his way through the system—he actually almost believes that until an idea is reduced to a simple model it doesn't exist. So the fact that he doesn't attribute an idea to early writers doesn't bother him—before it was translated into a formal model, it didn't exist.

The alternative to the MIT approach is the Chicago approach, and in many ways I fit much better into that Chicago tradition than I do into the MIT tradition. But, for Chicago, I have my fatal flaw. I lack the belief that government is inherently inefficient and bad and the market is good. In fact, as I have argued elsewhere, (Colander 1995) it was that tendency of Chicago economists to insert there creed into their analyses that led many to reject the intuitive Chicago approach and to follow the formal MIT approach. All those who didn't believe in the market solved all problems went to MIT where one showed formally that the market didn't solve all problems.

Given this choice, most economists who did not agree with the Chicago conclusions initially went to MIT and its clones.

My problem was that it was obvious to me that the market didn't solve all problems, so I had little desire to learn to show formally that it didn't. But there was no middle ground.

Once I get to the point of deciding that an issue deserves closer study, then and only then does it seem reasonable to move to formal modeling. Formal models are to be dealt with *after the ideas are reasonably clear*. If I am addressing an interesting problem and I determine that the idea doesn't meet the yeah criterion, I drop it, even if people say it is the "hot model." Much of the work on efficiency wages models as an explanation for unemployment, or partial equilibrium microeconomic explanations for unemployment, fall into that category. There's definitely some ideas there, but they are nowhere that I've seen developed sufficiently to a level that warrants formal modeling.

Another difference in my approach compared to others' is in the way I attempt to understand theory. Most economists, I believe, try to understand a theory by working out the mathematics and understanding each step of a model along the way. I don't, at least initially. Instead, I try to understand the problem the researcher is trying to solve and the way he or she is trying to solve it. Thus, first I try to think: is it an interesting problem? Of the literature I survey in economics, approximately 80-90 percent isn't. Only then do I start working a bit more formally. And then, I usually don't deal with the latest work, but instead with the initial work, because the assumptions are usually better spelled out in that early work.

Now, it should be clear that the approach I am discussion is descriptive, not prescriptive. I think brains operate somewhat differently; I know mine does—it fades out for many parts of life, going on automatic, and it loses itself in thought about a problem I've been working on. For example, I can be doing the dishes, or sleeping, but my mind is working through again and again the 200-300 ideas that I've toyed with at some time, and suddenly an idea comes to me on. So what works for me might not for someone else.

I think in many ways the MIT approach has economics training backwards. Instead of encouraging ideas initially, it encourages technical competence; when students should be thinking broadly—explored new ideas—we have them lost in studying some technical aspects of models that may or may not be relevant. Unfortunately, given the nature of advancement in the profession, most never have time to escape the formal models.

My rejection of the MIT modeling approach goes deeply into the vision of the economy I have. Using an intuitive approach the first thing one must decide is at what level of simplicity will the economy be modelable. This is what might be called the drop of water/ gravity choice.

The MIT approach pictures the economy as "modelable" in a meaningful way—the economy, like the world, is ordered. I don't. It doesn't mean that I don't believe the economy is ordered, or orderable; it simply means that I think the order is of a higher dimension than simple models can take into account. For example, say you're a physicist trying to understand when a drop of water will fall. Can that be modeled simply? Or is it far more complex to model that then it is to model the trajectories of planets. Most economic events are for me more like the drop of water than the movements of planets. Yes, there is some general laws of economics, but the application of those laws to real world events is the equivalent to translating Einstein's theory of relativity to explain when a drop of water will fall.