HOW TO BUILD AN ECONOMIC MODEL IN YOUR SPARE TIME

by Hal R. Varian*

Abstract

Most of my work in economics involves constructing theoretical models. Over the years, I have developed some ways of doing this that may be worth describing to those who aspire to practice this art. In reality the process is much more haphazard than my description would suggest—the model of research that I describe is an idealization of reality, much like the economic models that I create. But there is probably enough connection with reality to make the description useful—which I hope is also true for my economic models.

1. Getting Ideas

The first step is to get an idea. This is not all that hard to do. The tricky part is to get a *good* idea. The way you do this is to come up with lots and lots of ideas and throw out all the ones that aren't good.

But where to get ideas, that's the question. Most graduate students are convinced that the way you get ideas is to read journal articles. But in my experience journals really aren't a very good source of original ideas. You can get lots of things from journal articles—technique, insight, even truth. But most of the time you will only get someone else's ideas. True, they may leave a few loose ends lying around that you can pick up on, but the reason they are loose is probably that the author thought about them a while and couldn't figure out what to do with them or decided they were too tedious to bother with—which means that it is likely that you will find yourself in the same situation.

My suggestion is rather different: I think that you should look for your ideas outside the academic journals—in newspapers, in magazines, in conversations, and in TV and radio programs. When you read the newspaper, look for articles about economics. . . and then look at the ones that aren't about economics, because lots of the time they end up being about economics too. Magazines are usually better than newspapers because they go into issues in more depth. On the other hand, a shallower analysis may be more stimulating: there's nothing like a fallacious argument to stimulate research.[1]

Conversations, especially with people in business, are often very fruitful. Commerce is conducted in many ways, and most of them have never been subjected to a serious economic analysis. Of course you have to be careful not to *believe* everything you hear—people in business usually know a set of rules that work well for running their own business, but they often have no idea of where these rules come from or why they work, and this is really what economists tend to find interesting.

In many cases your ideas can come from your own life and experiences. One of my favorite pieces of my own work is the paper I wrote on "A Model of Sales." I had decided to get a new TV so I followed the ads in the newspaper to get an idea of how much it would cost. I noticed that the prices fluctuated quite a bit from week to week. It occurred to me that the challenge to economics was not why the prices were sometimes low (i.e., during sales) but why they were ever high. Who would be so foolish as to buy when the price was high since everyone knew that the item would be on sale in a few weeks? But there must be such people, otherwise the stores would never find it profitable to charge a high price. Armed with this insight, I was able to generate a model of sales. In my model there were two kinds of consumers: informed

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consumers who read the ads and uninformed consumers who didn't read the ads. The stores had sales in order to price discriminate between the informed and uninformed consumers.

Once I developed the model I had a research assistant go through a couple of years' worth of the *Ann Arbor News* searching for the prices of color TVs. Much to my delight the general pattern of pricing was similar to that predicted by the model. And, yes, I did manage to get a pretty good deal on the TV I eventually bought.

2. Is Your Idea Worth Pursuing?

So let's assume (a favorite word of economists) that you have an idea. How do you know if it is any good? The first test is to try to phrase your idea in a way that a non-economist can understand. If you can't do this it's probably not a very good idea. If you can phrase it in a way that a noneconomist can understand, it still may be a lousy idea, but at least there's hope.

Before you start trying to decide whether your idea is correct, you should stop to ask whether it is interesting. If it isn't interesting, no one will care whether it is correct or not. So try it out on a few people—see if they think it is worth pursuing. What would follow from this idea if it is correct? Would it have lots of implications or would it just be a dead end? Always remember that working on this particular idea has an opportunity cost—you could be spending your time working on a different idea. Make sure that the expected benefits cover that opportunity cost. One of the primary purposes of economic theory is to generate insight. The greatest compliment is "Ah! So that explains it!" That's what you should be looking for—forget about the "nice solid work" and try to become a Wizard of Ahs.

3. Don't Look at the Literature Too Soon

The first thing that most graduate students do is they rush to the literature to see if someone else had this idea already. However, my advice is to wait a bit before you look at the literature. Eventually you should do a thorough literature review, of course, but I think that you will do much better if you work on your idea for a few weeks before doing a systematic literature search. There are several reasons for delay.

First, you need the practice of developing a model. Even if you end up reproducing exactly something that is in the literature already you will have learned a lot by doing it—and you can feel awfully good about yourself for developing a publishable idea! (Even if you didn't get to publish it yourself. . .)

Second, you might come up with a different approach than is found in the literature. If you look at what someone else did your thoughts will be shaped too much by their views—you are much more likely to be original if you plunge right in and try to develop your own insights.

Third, your ideas need time to incubate, so you want to start modeling as early as possible. When you read what others have done their ideas can interact with yours and, hopefully, produce something new and interesting.

4. Building Your Model

So let's skip the literature part for now and try to get to the modeling. Lucky for you, all economics models look pretty much the same. There are some economic agents. They make choices in order to advance their objectives. The choices have to satisfy various constraints so there's something that adjusts to make all these choices consistent. This basic structure suggests a plan of attack: Who are the people making the choices? What are the constraints they face? How do they interact? What adjusts if the choices aren't mutually consistent?

Asking questions like this can help you to identify the pieces of a model. Once you've got a pretty good idea of what the pieces look like, you can move on to the next stage. Most students think that the next stage is to prove a theorem or run a regression. No! The next stage is to work an example. Take the simplest example—one period, 2 goods, 2 people, linear utility—whatever it takes to get to something simple enough to see what is going on.

Once you've got an example, work another one, then another one. See what is common to your examples. Is there something interesting happening here? When your examples have given you an inkling of what is going on, *then* you can try to write down a model. The critical advice here is KISS: keep it simple, stupid. Write down the

simplest possible model you can think of, and see if it still exhibits some interesting behavior. If it does, then make it even simpler.

Several years ago I gave a seminar about some of my research. I started out with a very simple example. One of the faculty in the audience interrupted me to say that he had worked on something like this several years ago, but his model was "much more complex." I replied "My model was complex when I started, too, but I just kept working on it till it got simple!"

And that's what you should do: keep at it till it gets simple. The whole point of a model is to give a simplified representation of reality. Einstein once said "Everything should be as simple as possible. . . but no simpler." A model is supposed to reveal the essence of what is going on: your model should be reduced to just those pieces that are required to make it work.

This takes a surprisingly long time—there are usually lots of false starts, frustrating diversions, and general fumbling around. But keep at it! If it were easy to do, it would have already been done.

5. Generalizing Your Model

Suppose that you've finally made your model as simple as possible. At this point your model is probably *too* simple to be of much interest: it's likely just an example or a special case. But if you have made your model as simple as possible, it will now be much easier to see how to generalize it since you know what the key pieces are that make the model work.

Here is where your education can be helpful. At last you can use all those techniques you learned in graduate school. Most of the time when you were a student you probably studied various canonical models: things like consumer choice, and producer choice, general equilibrium, game theory and so on. The professor probably told you that these were very general models that could encompass lots of special cases.

Well, it was true. Over the last fifty years economists have come up with some very general principles and models. Most likely your model is a special case of one of these general models. If so you can immediately apply many of the results concerning the general model to your special case, and all that technique you learned can help you analyze your model.

6. Making Mistakes

This process—simplify to get the results, complexify to see how general it is—is a good way to understand your model. Most of the time that I spend modeling is involved in this back-and-forth process. Along the way, I make a lot of mistakes. As Piet Hein puts it:

> The road to wisdom? We'll it's plain
> and simple to express:
> Err
> and err
> and err again
> but less
> and less
> and less.

This back-and-forth iteration in building a model is like sculpting: you are chipping away a little bit here, and a little bit there, hoping to find what's really inside that stubborn block of marble. I choose the analogy with sculpting purposely: like sculpture, most of the work in building a model doesn't consist of adding things, it consists of subtracting them.

This is the most fun part of modeling, and it can be very exciting when the form of the idea really begins to take shape. I normally walk around in a bit of a daze at this stage; and I try not to get too far away from a yellow pad. Eventually, if you're lucky, the inner workings of your model will reveal itself: you'll see the simple core of what's going on and you'll also understand how general the phenomenon really is.

7. Searching the Literature

At this point you can start doing your literature search. Tell your professors about what you've discovered—nine times out of ten they'll tell you to look in the "1983 *AER*" or "*Econometrica 77*" or some textbook (maybe even one of mine). And lots of the time they'll be right. You'll look there and find "your" model—but it will be much better done, much more fully developed, and much clearer.

Hey, no one said research would be easy. But this is a point where you really have a chance to learn something—read the article(s) carefully and ask yourself "Why didn't I do that?" If someone started with the same idea as you and carried it further, you want to see what you missed.

On the other hand, if you really followed the advice I gave you above to keep it simple, you may have come up with something that is much clearer than the current treatments. Or, maybe you've found something that is more general. If so, you may have a worthwhile insight. Go back to your advisor and tell him or her what you have found. Maybe you've got a new angle on an old idea that is worth further exploration. If so, congratulations—you would never have found this if you did the literature search right away.

Maybe what you've figured out is not already in the literature. The next possibility is that you are wrong. Maybe your analysis isn't right, maybe the idea is just off the wall. This is where your advisor can play a big role. If you've really made your analysis as simple as possible, it is a) less likely to contain an error, and b) any errors that remain will be easier to find.

This brings me to another common problem. When you've worked on a topic for several months—or even several weeks—you tend to lose a lot of perspective. . . literally. You're just too close to the work to really get a picture of what is going on. This lack of perspective takes one of two forms: first, you may think something is obvious when it really isn't. It may be obvious to *you*, but you've been thinking about this issue for several months—it probably isn't so obvious to someone who doesn't have the benefit of that experience.

The other possibility is that you may think, something is complicated when it is really obvious—you've wandered into a forest via a meandering path. Maybe there's a nice clear trail just a few feet away that you've totally missed.

So at this point you've got to start getting some independent judgement of your work. Talk to your advisor, talk to your fellow students, talk to your wife, husband, girlfriend, boyfriend, neighbor, or pet. . . whoever you can get to listen. And here's what you'll find: they've got no idea what you are talking about (especially your pet). So *you* have to go back to trying to figure out what you really are talking about: what *is* the fundamental idea of your model?

8. Giving a Seminar

After you've bored your friends, relatives and pets to death, you should give a seminar. This is a really important phase: the more you can talk about your work, the better the final paper will be. This is because a talk forces you to *get to the point*. If you want your audience to listen to you, you've got to make your idea clear, concise, and organized—and the experience that you gain by doing this is extremely useful for writing your paper.

I listen to a lot of stupid ideas—but that's what I'm paid to do. Lots of people listen to stupid ideas from me, too: my colleagues get paid to do it, and the students get examined on it. But most people don't have to listen to you. They don't have to read your paper. They won't even have to glance at the abstract unless they have a reason to.

This comes as a big shock to most graduate students. They think that just because they've put a lot of work and a lot of thought into their paper that the rest of the world is obliged to pay attention to them. Alas, it isn't so. Herb Simon once said that the fundamental scarcity in the modern world was scarcity of attention—and brother, is that the truth. There are demands for everybody's attention, and if you want someone to pay attention to you, you have to give them a reason to do so. A seminar is a way to get them to pay attention, so be sure to exploit this opportunity to get people to listen to you.

The useful thing about a seminar is that you get immediate feedback from the audience. An audience won't put up with a lot of the things that authors try to write in papers: turgid prose, complex notation, and tedious details. And, believe it or not, readers won't put up with these things either! The trick is to use the seminar to get all those things out of your paper—that way, it may actually get read.

Controlling the audience

I've seen it claimed that one of the greatest fears that most people have is speaking before a group. I imagine that most assistant professors have this problem, but after many years of giving lectures before several hundred students it goes away.

In fact, lecturing can become downright addictive (as my family often reminds me). As the mathematician R. H. Bing once said: "When I was young, I would rather give a lecture on

mathematics than listen to one. Now that I am older and more mature I would rather give *two* lectures on mathematics than listen to one." Giving lectures is a bit like eating oysters. Your first one requires some courage, but after you develop a taste for them, it can be hard to stop.

There are three parts to a seminar: the introduction, the content, and the conclusion. My advice about introductions is simple: don't have one. I have seen many seminars ruined by long, pretentious, contentless introductions. My advice: say a few sentences about the big picture and then get down to business: show them what you've got and why it's important. The primary reason to get down to business right away is that your audience will only remember about twenty minutes of your talk—and that is usually the *first* twenty minutes. So make sure that you get some useful information into that first twenty minutes.

As for conclusions, the most common problem is letting the seminar trail off into silence. This can ruin a good talk. I always like to spend the last couple of minutes summarizing what I accomplished and why the audience should care. After all, this is what they will walk away with, so you might as well tell them what they should remember rather than make them figure this out for themselves.

Nowadays everyone seems to use overheads for their lectures. The downside of this is that the seminar isn't very spontaneous—but the upside is that the seminar is usually better organized. My advice is to limit yourself to one or two slides for an introduction and one for a conclusion. That way you will be forced to get to *your* contribution sooner rather than later. And make your overheads *big*; use large type and don't try to say too much on each one.

There are two things to avoid in your presentation: don't let your audience go to sleep, and don't let them get too lively. You want the audience to hear what you have to say. They won't hear your message if they are sleeping, and they won't hear your message if they are talking more than you are. So don't lose control of your seminar!

The key to maintaining control is to establish credibility early on. The way to do so is to go into great detail in the presentation of your first result—a theorem, a regression, a diagram, whatever. Spell out each aspect of your result in excruciating detail so no one can possibly misunderstand. When you do this you will certainly get questions like "Will this generalize to n agents?" or "Have you corrected for heteroskedasticity?"

If you know the answer to the question, go ahead and answer it. If you don't know the answer—or the question is totally off the wall—say "That's a good question; let me come back to that at the end of the seminar." (Of course you never will.) Don't get sidetracked; the point of going through the initial result in great detail is to establish credibility.

Once you've presented your result and you see that the audience has understood the point—their heads are nodding but not nodding off—you can go on to the generalizations and elaborations. If you've done a good job at establishing your credibility initially now the audience will believe anything you say! Of course you shouldn't abuse this trust, but it is useful to exploit it in the rest of your presentation. This is the fundamental reason for starting simple: if you start out with a delicate argument, it will be hard for the audience to understand and you will never establish trust.

When you are done with your talk you should take a few minutes to jot down some notes: What was difficult for people to understand? What questions did they ask? What suggestions did they make? What references did they give you? You may think that you will remember these points, but quite often you won't. The audience is a very useful resource for clarifying your thoughts—make sure you use it well.

9. Planning Your Paper

Almost everyone writes on computers these days. I know that computers are great time savers: I get almost as much work done now as I got done before I started using computers.[2]

I thought that I would spend a bit of time talking about how I use computers, not because it is all that important, but because no one else in this collection will discuss such mundane matters. Since I am well known as a computer nerd, people always ask me what I use, and I figure I can save time by pointing them to this article. Undoubtedly this will all look incredibly archaic in a few years, but that's the cost of being on the bleeding edge of technology.

I currently use a UNIX machine, but most of what I say applies equally well to other

environments. I have a directory on my computer called `Papers` and when I start to work on a new topic I create a subdirectory under papers. (For example, this paper is in a directory `Papers/how-I-work`.) When I create the directory I create a `notes.txt` file: this contains my initial ideas, a rough outline, whatever. For example, the `notes.txt` file for this paper initially had entries in it like:

```
*read the newspaper
*simplify
*write and talk
**if you don't grab them in the first
  page, they won't read it
```

I create a notes file like this when I first start to work on a topic—I jot down the initial ideas I have, which are usually pretty sketchy. In the following days and weeks I occasionally take a look at this outline. When I look at it I move things around, add material and so on. I rarely take anything out completely—I just move material to the end of the file. After all, I might want those notes again.

After organizing these ideas for several weeks or months I am ready to write the first draft on paper. I usually try to do this in a day or two, to keep it all fresh. I normally put the notes in one window and the paper in the other and write the paper while I refer back and update the notes to keep them in sync with the paper.

Once the paper is written I put it aside for a couple of weeks. Papers need to age like fine cheese—it's true that mold might develop, but the flavor is often enhanced. More importantly, it gives your subconscious mind a chance to work on the idea—maybe it will come up with something your conscious mind has missed.

When I come back to the paper I try to read it with a fresh mind, like someone who has never seen it before.[3] On rare occasions I like what I read, but usually I have lots of criticism. Whenever I have to pause and think "what does that mean?" I rewrite—I add more explanation, change the notation, or whatever is necessary to make the paper clearer. When I'm done with this process I have a first draft.

I next check this draft into a revision control system. This is a piece of software that keeps track of the revisions of a paper. It documents all of the changes you make and allows you to restore any previous version of a paper. I use the UNIX utility `rcs` but I know there are many other systems available. Revision control systems are especially valuable if you are working with a coauthor since they keep track of which person made which changes when.

I then repeat the process; let the paper sit for a few more weeks or months, then come back to it, read it with a fresh mind and revise it accordingly.

It is particularly useful to do a revision right after you give a seminar. Remember those notes I told you to write after your seminar ended? Sit down with the paper and go over the questions the audience had and the suggestions they made. Can you answer their questions in your paper? Can you incorporate their suggestions? Be sure to modify the notes/outline/slides for your talk when you incorporate the audience's suggestions.

Bibliographic software

One very useful computer tool is a bibliographic system. This is a piece of software designed to manage a list of references. There is a master database of references that is stored in your computer. You assign a key to each article like `Arrow70` or `ArrowRisk`. When you want to refer to a paper you use the key, by saying something like `\cite{Arrow70}`. The bibliographic program then looks up the appropriate citation in your database and puts it in the list of references at the end of your article.

I use the system called BibTEX, since it works well with TEX. However, there are many other systems available that work for other word processing packages. It's a good idea to get in the habit of using a system like this. Over the years you will build up a comprehensive bibliography for the areas you work in.

But where do you get your references in the first place? Well, one way is to ask people: your advisor, your colleagues, your friends, and so on. This is still one of the most reliable ways. But nowadays there are a number of computerized databases available online or on CDs that allow for easy search. You can open the CD for *Journal of Economic Literature*, type in a few key words like "price discrimination" and get the last 10 years worth of abstracts published articles that contain the words "Price discrimination." As you look at these articles you will see a few "classic" articles cited. When you identify these classic articles go to

the *Social Science Citation Index* and search for all the recent papers that have cited these classics. This process should give you an up-to-date bibliography pretty quickly. Often you can download the citations you get directly into your bibliography database program.

10. The Structure of the Paper

There's an old joke about academic papers. They are all supposed to have three parts. The first part, everyone can understand. The second, only a handful of readers can understand. The last part no one can understand—that's how the readers know it's a serious piece of work!

The big mistake that authors make these days is to leave out the first part of the paper—that part that everyone can understand. But the introduction is the most important part of the paper. You've got to grab the reader on the first page. No matter how brilliant the rest of the paper is, it won't be noticed if no one reads it. And no one will read it if you don't get their interest in the first few paragraphs. If you really know what your paper is about, you shouldn't find it hard to explain this to your reader in a couple of paragraphs.

My basic advice is to make your paper look like your talk. Get to the point. Use examples. Keep it simple. Tell people why what you did is important after you've done it. Put the tedious stuff in the appendix. End with a summary of what you have accomplished. If you have really written a good paper, people won't have to listen to your seminar to find out what you have done: they can just read it in your paper.

11. When to Stop

You can tell when your work is getting ready for publication by the reactions in the seminars: people stop asking questions. (Or at least, the people who have read your paper stop asking questions.) If you've followed my advice, you've already asked their questions—and answered them—in your paper.

Once you've made your point, stop. Lots of papers drag on too long. I said earlier that people only remember about 20 minutes of your seminar (if you're lucky), and they only remember about 10 pages of your paper. You should be able to say most of what you want to say in that length.

Once your paper is written, you can submit it to a journal. I don't have too much to say about this; Dan Hamermesh has written a nice article that describes the procedure better than I can.[4] All I can say is to echo his advice that you go over the article with a fine tooth comb before sending it in. Nothing turns off an editor or a referee more than to find typos, missing references and sloppy editing in the articles they deal with.

12. Writing Textbooks

Most of what I've had to say so far has to do with writing articles. But I suppose I really should say a bit about the other kind of writing I've done: textbooks.[5]

My first text, *Microeconomic Analysis,* really wasn't planned; it just happened. When I first started my professional career at MIT in 1973 I was asked to teach the first year graduate micro course. The text, such as it was, consisted of about 20 pages of notes written by Bob Hall, maybe 40 pages of notes from Don McFadden and Sid Winter, and a few journal articles. The notes were awfully sketchy, and the journal articles were much too advanced for first year students. So I had to write my own notes for the students.

The first year I wrote about 50 pages; the next year another 50, and the year after that another 50. The students who used them were great. They read them carefully and told me what was wrong: where the obscurities were, where the errors were, what was too advanced, and what was too simple. I owe much of the success of that book to the fact it was class tested before a highly critical audience.

During this period I happened to meet Richard Hamming, an electrical engineer who had written several texts. He gave me a key piece of advice: "Get together the problems that you want your students to be able to solve after they've read your book—and then write the book that will teach them how to solve them."

This was great advice. I followed it to some degree with the graduate text, but later, when I wrote the undergraduate text, I followed it religiously—but more about that below.[6]

One day a publisher came into my office and asked (as they often do) "Are you writing a book?"

I said that would be a silly thing for an assistant professor to do—but as a matter of fact, I did have some class notes that I had been working on for a few years.

Next thing I knew, I had several publishers interested in my notes. I spent a semester at Berkeley in 1977 and used that opportunity to hammer them into shape. Much to my surprise the notes eventually became a book and ended up being very widely used. I did a second edition in 1983 and I *should* have done a revision in 1987 or so—but instead I decided to write an undergraduate text.

I wanted to write an undergraduate book because I was fed up with the books I had been using. I had tried several different ones, but couldn't find any I really liked. I remember one semester I sat down and tried to write a midterm exam—but the book I had been using was so vapid that I couldn't think of any problems that the students could solve using the tools that had been presented in the book! At that point I figured I could produce something better.

About the same time one of my undergraduates had picked up a workbook by Marcia Stigum called, I believe, *Problems in Microeconomics*. The student found this very helpful in understanding the concepts of economics, and I remembered what Hamming had told me about how to write a textbook. So I asked my colleague Ted Bergstrom if he would like to work with me to create a serious workbook.[7] Ted created problems as the text was being written, and I had to make sure that the text contained everything necessary to solve the problems he created. I created problems too, but those were automatically coordinated with the textbook—the external stimulus imposed by Ted's problems was much more important in shaping the contents of the book. If the students weren't able to solve the problems, I had to add explanations to the text until they could—and if we couldn't create a problem to illustrate some point, the point probably wasn't important enough to put in the text.

It's a pity that most workbooks are created as afterthoughts. Creating the workbook really should be an integral part of the writing process, as Hamming suggested. You want the students to be able to *use* the material you teach them, so the first order of business is to figure out what it is that you want them to be able to do. The latest buzzword in education is "learning by doing" but as far as I'm concerned that's always been the only way to go.

The undergraduate text turned out to be pretty successful as well. And the workbook has ended up selling two or three times as much as any of its competitors—which goes to show that there still is a market for a quality product in the textbook market.

13. Summary

I said that every talk should have a summary—so I suppose I have to follow my own advice. Here are the points to take away:

- Look for ideas in the world, not in the journals.
- First make your model as simple as possible, then generalize it.
- Look at the literature later, not sooner.
- Model your paper after your seminar.
- Stop when you've made your point.

And now my points have been made, so I'm duty bound to stop. Go forth and model!

Notes

1. But which sources to read? I read the *New York Times*, the *Wall Street Journal* and the *Economist*; these are probably good places to start.
2. If a train stops at a train station, what do you think happens at a work station?
3. This is much easier once you reach middle age.
4. Daniel S. Hamermesh, "The Young Economist's Guide to Professional Etiquette," *Journal of Economic Perspectives*, 6:1, 169–180.
5. The reader may recall Disraeli's warning: "An author who speaks about his own books is almost as bad as a mother who talks about her own children."
6. The general principle that I followed (and still follow) with the graduate text is that it should give the student the information they need to know to read a microeconomics paper in the *American Economic Review*. Every now and then I go through a few issues of the *AER* and note topics that should go in the next edition of the book.
7. As it turned out, it wasn't quite as serious as I had expected—in fact, I think it is quite funny, but that is due to Ted's unique sense of humor rather than my intentions.