Introduction

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Economics 276a,
Lecture 1

What is Experimental Economics?

The use of treatment intervention and induced preferences with real human subjects and financial incentives to pose questions in the social sciences that are difficult to answer using theory or field data.

What it isn't:

- Behavioral economics (though the two overlap significantly).
- A complete substitute for field data (though it is often a necessary substitute).
- Adversarial to economic theory (though it has a lot to say about theory's default assumptions).

What Does a Good Experiment Do?

Most have one of three broad motivations:

1. Use treatment interventions to test a hypothesis against an open alternative.
   - Compare competing neoclassical theories or predictions.
   - Compare neoclassical theory against a "behavioral" alternative.
   - Compare several "behavioral" theories in environments where neoclassical predictions are known to fail.

2. Use induced preferences to structurally identify an underlying behavioral parameter.
   - Preferences
   - Cognitive parameters
   - Learning parameters

3. Learn about realistic, real world phenomena too complex for theoretical study

Where do Good Experiments Come From?

From the **theory**.

- Test a point prediction.
- Test a comparative static prediction.
- Test a theoretical isomorphism.
- Empirically narrow the equilibrium set.

From the **literature**

- Resolve an outstanding controversy/ambiguity
- Improve on a previous methodology
- Point out an error/ambiguity in previous work
- Test robustness of results to new subject pools, stakes etc.

From the **world**

- Measure a "homegrown" parameter
- "Grow" something in the lab that exists in the world but not in theory
- Evaluate institutions on the cheap
- Study environments too complex for theory to model

An Interpretation of Theory

An interpretation of theory that is implicit in a lot of experimental economics:

At its heart much (most?) economic theory is a description of

- Correct beliefs and decisions (**optima**) OR
- mutually consistent beliefs and decisions (**equilibria**).

Theory takes a decision environment and set of preferences and describes a state in which it is desirable to change neither beliefs nor actions (a stable state).

I think theory is on very well established ground doing social science this way.

Getting There (and Which One)

(Much) less well established is an account of how (and whether) you get to these stable states. A few possibilities are contemplated in the history of economics.

1. Rationality/Beliefs: People are substantively rational (i.e. solve math problems in their head, apply Bayes' rule, think their way to common knowledge).
2. Adaptation: People follow heuristic adjustment rules that (may) terminate in equilibria or optima.
3. Selection: The Market Selection Hypothesis, one of the powerful traditional answers (and I think under-explored in the lab)

Economics was forged in (2) and (3), but modern economists are sometimes partial to (1) – Economic Creationism: How could rational outcomes emerge without hyper rational agents constructing them?

What behaviors emerge when multiple outcomes are stable?

Preferences

Also less well established is the actual structure of preferences. In the traditional absence of evidence, economists adopted a fairly empirically ad hoc set of defaults:

- Sociality: Narrow self interest
- Context dependence: Payoff irrelevant frames don't matter
- Learning: Bayes' rule
- Uncertainty: Expected utility theory
- Time: Time consistency (geometric discounting)

These have nice normative properties (at least the last four do!) and are good first guesses but there was never a huge empirical reason to assume them.

It is hard to fault economists for building defaults, but experimental economics allows us to move beyond assumptions in some areas.

Gaps in Economic Theory

Even if one more or less buys into the project of traditional theory (as I do), hard not to conclude that theory was forced (by lack of evidence) to be fairly ad hoc in three critical areas:

- How people reason, make inferences and form beliefs (reasoning).
- How people adjust behavior and beliefs out of equilibrium (adaptation).
- How people value things and what they value (preferences).

I interpret most of experimental economics as aimed at filling in these gaps and view this endeavor as fundamentally complementary to economic theory.

Why not just use field data?

Field data features strong external validity

- Observe behavior at actual (possibly unobserved) structural parameter settings (i.e. effect of policy XYZ given real world backdrop of parameters).
- Recover actual (non-behavioral) structural parameters (i.e. calculate a demand elasticity based on observed decisions).

Laboratory experiments feature strong internal validity

- Exogeneity (can apply experimental control without worrying about causality)
- Sterility (can simplify economic system to the level of a tested model)
- Observability (can control and therefore observe most important parts of the model)
- Measurement (can usually avoid measurement error)
- Replicability (results can be nearly exactly replicated by other researchers)

Why not just use field data?

External validity is the primary criticism of experimental economics

- Subject pools too unsophisticated
- Stakes too small
- Environments too artificial

These criticisms have driven methodological innovations

- Experiments with wider ranging subject pools
- Experiments with wider varying stakes
- Field experiments

Note that these criticisms are sometimes levied by theorists but generally not by (classical versions of) theory!

Why not just use field data?

Some responses on external validity

- Most applied exercises with field data involve subsamples or historical examples subject to related external validity concerns.
- Many important questions raised by economic theory can be tested in the field but many more are untestable without the control of the lab.
    - Is no evidence better than imperfect evidence?
- Experiments involve real people making real decisions with real incentives. If economic theory doesn't work here, there is a strong burden on the theory to explain why.

Still, there is also a burden on laboratory experimentalists to be very careful and thoughtful about the interpretation and counterfactual projection of their own results.

Hertwig and Ortmann (2001)

Experimental economics has several elements that distinguish it generically from experimental psychology:

- Pay for Performance
- Repetition
- Clear Instructions
- No Deception

I'd add that experimental economics avoids hypotheticals, distinguishing it sharply from early behavioral economics ala Kahneman and Tversky.

1940s and 1950s

The most striking fact about the history of modern economic theory is how quickly its findings were followed by experimental tests. Soon after von Neumann and Morgenstern (1944) researchers were in the lab running experimental tests of its main two components:

- Starting in 1948, experimentalists begin running tests of expected utility theory (notable early efforts include Mosteller and Nogee (1951), Allais (1953))
- Dresher and Flood researchers run the first game theory experiments in 1950 (Flood, 1952)

At the same time Chamberlin (1948) publishes first market experiment (though without monetary incentives or repetition).

The Prisoner's Dilemma

The first ever publication of the prisoner's dilemma was actually a 1952 experimental paper by one of its inventors (Flood, 1952).

A 100-times repeated prisoner's dilemma run on two subjects (one of whom was Armen Alchian)

This paper also published John Nash's comments in which he:

- Describes the unravelling of finitely repeated prisoner's dilemmas and the sustainability of cooperation in an infinitely repeated game for the first time and
- promptly dismisses this is an implausible prediction (and gently scolds the subjects for being "irrational" by not cooperating more)!

1960s-1970s: The Field Emerges

- In the early 1960s Vernon Smith, Sydney Siegel and Reinhard Selten begin independently running market experiments that start to resemble modern methodology.
- Smith begins teaching the first graduate seminars on experimental economics in the early 60s, first grad students write experimental papers.
- Charlie Plott and Vernon Smith begin doing experimental work on topics in public economics in the 1970s
- First computerized experiments emerge in the late 1970s, moving methodology further towards its modern
- Smith (1976, later expanded in 1982) establishes some of the methodological roots of experimental economics.

1980s: The Field Picks Up Steam

- Theoretically grounded bargaining literature by Roth and coauthors start in late 1970s (e.g. Roth and Moulof, 1979) and continue through the 80s; ultimatum game introduced by Guth et al. in 1982; a vigorous debate on nature of these results continues to this day.
- Study of public goods games takes off (see Isaac and Walker (1988) for an oft cited early example; also Andreoni's work in the late 80s/early 90s).
- Experimental auction theory takes off with important work especially being done on common value auctions at Pittsburgh and private value auctions at Arizona.
- Experimental financial markets finds evidence of information aggregation (Plott and Sunder 1982, Friedman and Harrison, 1984) and asset market bubbles (Smith et al. 1988).
- Lots of other important stuff going on, too much to mention.

1990s: The Field Takes Off

- Nagel (1995) introduces the guessing game and the notion of k-level thinking.
- McKelvey and Palfrey (1995) introduces QRE, a generalization of Nash inspired by experimental evidence.
- Handbook of Experimental Economics and Arizona grad student workshop spread interest in field.
- Reciprocity and fairness research builds steam, stoked by gift exchange games (Fehr et. al 1993) and trust/investment games (Berg et. al 1995)
- Coordination game research program on equilibrium selection in coordination games begins with van Huyck et al. (1990).
- Experimental study of learning and evolutionary games takes off in the mid 90s with Dan Friedman, Ido Irev, Al Roth, Colin Camerer and Tek Ho being some leading researchers on the topic.

2000s: The Field Becomes (more) Mainstream

- 2002 Nobel Prize awarded jointly for Experimental Economics and Behavioral Economics (Smith and Kahnemann)
- Field experiments get off the ground thanks especially to pioneering efforts by John List and David Reilly.
- Rise in interest in social preferences lead by a large flood of papers by Ernst Fehr and coauthors in Europe and an influential paper by Charness and Rabin (2001).
- New methodological tools, neuro-imaging and examination of effects of chemicals on decision making.
- Gender/demographic research starts building steam with Andreoni and Vesterlund (2001).
- Rise of work in behavioral game theory (see Camerer, 2003) as experimentalists try to build generalizations and alternatives to standard theory that incorporate limited reasoning, noisy inference, learning processes and alternative preferences.
- Study of communication, group decision making and identity takes root.

This brings us to the window of our readings...

Two Projects

In order to make these methodology lectures more concrete, throughout this class I am going to illustrate using:

- A project published last year (on continuous time decision making).
- A project published a few years ago (on subjects' valuation of real options).

My hope: By having two projects we're constantly referencing, I'm hoping you'll get a good feel for the key considerations that go into experimental design and the lifecycle of experimental research.

Throughout these lecture notes I'll be putting these example vignettes on red slides.

Continuous Time Project

Many actual strategic decisions unfold in continuous time but virtually all dynamic games unfold in discrete time. How do people make strategic decisions in continuous time?

Simon and Stinchombe (1989) provide a theory. A surprising result is that continuous time often generates very different equilibria than closely related dynamic discrete games.

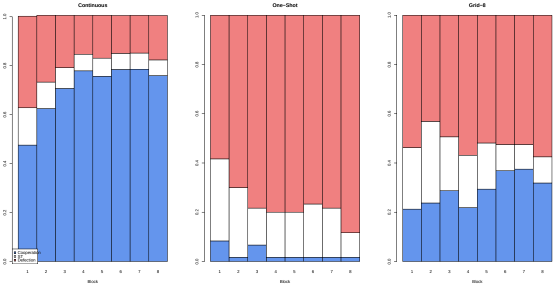
Two questions arise:

1. Does SS's theory accurately predict behavior? It has never been directly tested.
2. Does the wide gap between discrete time and continuous time emerge in practice?

Good reason to doubt the second – arbitrarily fine discrete grids are ϵ-close to continuous equilibrium for an arbitrarily small ϵ.

- Alternative hypothesis to (2): Subjects play ϵ-equilibria.

Continuous Time Project



- Oprea and Friedman (2012) show really high cooperation rates in continuous time prisoner's dilemmas.
- Discrete time variants have less cooperation (but more than zero!).
- Varying grid sizes consistent with one of many epsilon equilibria.
- Problem 1: Really complex data / ϵ-equilibria
- Problem 2: Endogenous reaction lags mean don't study "true" continuous time.

Continuous Time Project

SS propose a striking and simple example using a simple timing/preemption game:

- Two players, a and b each choose a time $t_i \in [0,1)]$ to enter a market (WLOG $t_b \leq t_a$) incurring a cost $C(t) = c(1-t_i)^2$
- Agent b earn flow profits π^m while in the market alone and a fraction $[0.5 - 0.5(t_a - t_b)]$ of π^d while sharing the market.

SS highlight a case with $\pi^m = 2$ and $\pi^d = 3$.

- In continuous time there is a continuum of SPNE $t_a = t_b \in [0, 0.25]$.
- However iterated elimination of dominated strategies leaves a unique survivor at $t_a = t_b = 0.25$ (the payoff maximizer for a symmetric strategy profile).

Can test this precisely by freezing time when first decision is made.

Continuous Time Project

However, with even an arbitrarily fine discrete time grid, the unique SPNE is $t_a = t_b = 0$ (an outcome Pareto dominated by the Continuous profile).

- Strictly, this prediction is insensitive to the fineness of the grid
- however the deviation from best response necessary to play 0.25 falls towards zero as the grid approaches the continuous limit.

One of Simon and Stinchombe's major theorems points out however that the continuous prediction is always ϵ-close to a best response for and ϵ that shrinks with the fineness of the time grid.

- We would expect varying this required ϵ to influence the likelihood of subjects playing as in continuous time.

This paper was published as Calford and Oprea, 2017 Econometrica

Real Options Project

Lot of theory on ideal investment rules when costs are irrecoverable. Can't use the NPV rule, must consider the option value – best known is deferral option (MacDonald and Siegel, 1986).

Suppose an investor can sink a fixed cost C to earn a present value V that evolves forever according to a geometric Brownian motion

$$dV = \alpha V dt + \sigma V dz, \tag{1}$$

where $\alpha < \rho$, $\sigma > 0$ and z is a stander Wiener process. The investor observes $V(t)$ at each t. Investor can make one irreversible investment at t, yielding $(V(t) - C)e^{-\rho t}$, where ρ is the discount rate.

The expected wealth maximizing decision involves waiting until $V(t)$ hits a threshold $V^*(t) = (1 + w^*)C$ where the option premium w^* is:

$$w^* = \frac{1}{B-1}, \colon \ \texttt{where } B = \frac{1}{2} - \frac{\alpha}{\sigma^2} + \sqrt{\left[\frac{\alpha}{\sigma^2} - \frac{1}{2}\right]^2 + \frac{2\rho}{\sigma^2}} > 1. \tag{2}$$

Real Options Project

Lots of high profile attempts in the literature to empirically assess whether human beings properly value real options using field data.

Very, very difficult to get good evidence because the parameters determining the option premium is unobservable. But we can control (and therefore observe) this stuff in the lab.

So our questions are:

- Do subjects (learn to) properly value real options?
- Do they react to variation in the parameters in qualitatively optimal ways?
- If not, why not?
- If so, how do they learn to do it?

This paper was published as Oprea, Friedman, Anderson, 2009 REStud

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