Camp Outline & More Perspectives

I. Logistics, Etc. (with repetition of both David and documents sent)
   A. Camp Etiquette
   B. Schedule
   C. Office Hours
   D. Questions?
II. Topics Covered and Uncovered
III. Orientation of Camp/Field
Introduction

Camp Etiquette

Names
- Address David and me and visitors by first names. And you are?
  - Wear name tags — *throughout* — for us and for speakers
- Seating...
- be forgiving of the aged

More on Bears!
- Stay distant ... when in Rome, stand where the Romans stand.
- The evolutionary story that wasn’t ...
  - We find babies of dangerous species ugly, not tempted to approach!
- If comes towards you
  - Try blasphemous cussing! (doesn’t work)
  - Make noise (does work) Fight back (won’t be needed)
Talks and Schedule

- 9:30 means ... 9:30 (not 9:31+).
  - W/o us having to herd.
- 11:00 means ... 11:00 (not 11:01+).
  - W/o us having to herd.
- 2:00 means ...

End times are end times.

- (So 10:40 means 10:40, etc.)
- (Note: David and I will allow ourselves “spillovers”)

Breaks: Short ones are short (and let speakers get to rest, and to restrooms)

Speakers set rules during talks.

- Several Q&A sessions, ask any questions
Office Hours

- Will all have individual “office hours” to discuss your research, or any questions that would benefit from focused, one-on-one discussion.
  - None of us will be able to read material ahead of time.
  - Come reasonably prepared.
- Sign up with sheets provided.
  - Deadline: Today 3.30 sharp.
  - When speakers over-subscribed, we assign slots by your expressed priority, topic match, fairness, zodiac sign, and randomness.
  - Up to 32 hours 30-minute slots of individual office hours!
- 7.5 hours of group/drop-in office hours
  - Formats/topics announced.
  - Sign-up 11.00 am day of sessions.
  - Come with questions.
  - Mine tomorrow: questions about $U(x)$, (after that) own research on prefs.
Reminders:

- Talk during meals, etc. is primary.
- Please don’t ask visiting faculty for special meetings.
- But separate scheduling with home-institution faculty permitted!

Questions on logistics?

- E.g., whether must be on time to sessions?
Topics Covered

- Psychological Evidence ... 
  - All of us!

- Modeling, and Methods
  - David, me, Stefano, Stefano, Gautam, Kelly

- Economic and Policy Implications
  - Richard, David, Stefano, Muriel, Gautam, Kelly, Antoinette, Eric
  - ... and even me
Introduction

Much not covered enough because of time constraints

Things not covered because less on topic

- Classical rational-choice models.
  - Missing because you already know it, not because unimportant.
  - If this were the only nine days of your economics education, we would teach you virtually no behavioral economics.

- Evolutionary theory/deriving human nature from “first principles”.
  - Feel unnecessary to figure out how humans became this way.
  - Not behavioral to derive psych from (whose?) “first principles” rather than evidence.
  - Any hypothesis about humans now that is of economic or social interest, no matter source, is a good thing.

- “Non-psychological bounded rationality”.
Introduction to “Behavioral Economics”

A repeat-lots-of-what-David-said Approach

Use and embrace the

- **substance**
- **techniques**, and
- **goals**
  - search for tractable models with economic consequences,
  - not mere psychological accuracy,
  - and taste for comparative statics,
  - calibrational relevance, and
  - empirical implementability

of standard economic analysis, but focus on introducing psychological factors heretofore under-emphasized by economists.
• Not everybody likes the term "behavioral economics". I don’t!
  • Slander against other economists, who also study behavior.
  • Whiff of “behaviorism” a la B.F. Skinner. And, in fact, a perspective here reflects a past one in psychology—we’ll find it useful to talk about motives, thoughts, etc., not just observed behavior.
  • My complaint: I am very interested in welfare, not just behavior.
Behavioral economics is *not* defined by a method (e.g., lab experiments) but by substance—disposition to integrate psychological factors historically unemphasized by economists but which we think matters for economics.

- Laboratory experiments ... great when they can help.
- Field experiments ... great when they can help.
- Natural experiments ... great when they can help.
- Structural analysis observational data ... great when it can help.
- Formal models ... great when they can help.
- Etc.

Lest you think I am too wishy washy ...

- Empirical uncuriosity or low empirical standards ... always bad.
- Loud gum chewing ... always bad.
Introduction

A short history of “Behavioral Economics”

First Wave: Identify “anomalies” — ways that economic theory has been importantly wrong, and identify some alternative conceptualizations.

Second Wave: Formalize some of the alternatives in precise models, and identify some empirical validations of these models.

Third Wave: Fully integrate into mainstream economic analysis.

- I’m a 2nd-waver.
  - Hoping to become an anachronism
  - Now entered the 3rd Wave.
  - But still a lot of 1st and 2nd wave.

- Laibson (1994) launched 3rd Wave?
Introduction

New assumptions don’t mean abandoning traditional methods.

- Same set of tools
- Same tolerance for imperfections
  - Of course our models are not fully realistic — they couldn’t be.
  - All models are false — including those I present!
    - Are they improvements?
- Same intolerance of mistaking vagueness for perfection!

Nor does it even mean abandoning traditional assumptions.

- Limits to the correctness and applicability of these assumptions does not mean that they aren’t often appropriate
- Develop new models **without destroying the insights of old models** by embedding them together into the same models.
Approaches to Incorporating Limits to Rationality

The biggest approach of all ...

1. Don’t!
   - Our models can often be improved by improving our understanding of $U$.
   - Against common image of battles over BE, much of it is about convincing economists certain behaviors are rational, **not** mistakes.
   - Don’t cling desperately to bad rational explanations.
   - *Nor* cling desperately to familiar assumptions about preferences, and label all “anomalous” behaviors as mistakes.
“De Gustibus” for 21st Century:

- Try model preferences people actually have.
- Not mechanically either
  - what we always assumed or
  - what needed to call observed choices 100% rational.
- Just what seems true enough, tractable enough, or important enough, based on good science, good economics, and good psychology.
- Lots of cases where evidence seems clear one or the other.
- Theory that accommodates a range of preferences and of errors
- Methods for identifying decomposition of behavior into each

A table we’ll return to later:
What combinations of preferences and types of mistakes could explain the general pattern of modest-scale risk preferences we observe?

<table>
<thead>
<tr>
<th>DMU(W)</th>
<th>“Classical PT”</th>
<th>“News-U PT”</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fully rational</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Plausible errors</td>
<td>X</td>
<td>(✓)</td>
</tr>
<tr>
<td>Implausible errors</td>
<td>✓</td>
<td>✓</td>
</tr>
</tbody>
</table>

- X \equiv \text{can’t explain}
- ✓ \equiv \text{could explain most/all instances.}
- (✓) \equiv \text{could maybe explain in a scientifically serious way some of what we observe.}
Big big big (not so big at all) aside on how I am going to think about “preferences”, utility, errors, etc.

- Utility/happiness exist separate from observed choice.
  - (So do beliefs!)
- Using choice to ID preferences does not mean it defines them.

Utility theory as *foundations* for choice theory, not the other way around.

- We choose orange over apple because we like the orange better.
  - Liking orange better not a representation of fact that we choose it.
- Satisfaction from seeing somebody you hate trip not a representation of the fact that you will trip him if can get away with it.
  - In fact, you may not—but still get satisfaction.
- Retiring $200,000 poorer from constant stock trading ought not be represented by utility function decreasing in retirement wealth;
  - Ought be “represented” by u-function increasing in retirement wealth
Ways greater psychological realism can improve economic analysis

1. Explaining behavior studied by economists that traditional analysis has had difficulties explaining.
   - Equity-premium puzzle, low saving, nominal-wage rigidity, etc.

2. Explaining behavior that one would have thought economists would have been studying—but haven’t been.
   - My favorite category in some ways
     - Reflects deep fact that theory influences topics of research.
     - Role of status quo and defaults.
   - Economists were not examining role of defaults and claiming small,
     - We were not looking.
Two types of “not looking at”:

- we never set our eyes on, or
- quickly avert eyes when realize we couldn’t say anything coherent.

Implicit and explicit small-scale insurance.

- Extended warranties, rental insurance, etc.
- products → loss leaders!

Short-term, high-interest-rate borrowing.

- credit-card debt
- huge pawn industry, payday loans, rent-to-own furniture.
3. Beyond explaining behavior, better understand normative/hedonic effects of observed behavior.

- In many domains, I think that this is the more important insight.

4. Often by making our models more complicated but more realistic

- Trade-off economists do all the time with familiar assumptions
- I can credibly claim to need to dumb down my models.
- Can structural IO economists with kray computers, and theorists writing Baroque models?
- But also often by making our models less complicated and more tractable, especially in the long term after
  - we develop our skills with new models, and
  - we realize we can nix certain complexities in classical models that have been added to try to match the evidence.
Ways greater psychological realism can harm economic analysis.

- Destroys illusion that any model is literally correct.

- A sad sentence in an introduction to a book (with biographical sketches of some eminent people): “The history of the Victorian Age will never be written: we know too much about it. For ignorance is the first requisite of the historian - ignorance, which simplifies and clarifies, which selects and omits, with a placid perfection unattainable by the highest art.” - Lytton Strachey, preface to Eminent Victorians

- Don’t use abandonment of ignorance as abandonment of appreciation for general insight.

- “… let him bear in mind that his novel is not a transcript of life, to be judged by its exactitude; but a simplification of some side or point of life, to stand or fall by its significant simplicity.” - Robert Louis Stevenson (from A Humble Remonstrance)
An approach to developing more realistic theories

- Most economic models: take theory of individual as input

Economic models as I see them:

- Take well-specified model of individuals, plug them into well-specified model of the environment, and see predictions.
- Theoretical comparative statics.
- Theory-guided empirics.

Short articles with these perspectives:

- May 2013 *AER*, June 2013 *JEL*

Before more, a source of inspiration:
The Genius of David Kreps (and Wilson, Nash, Selten, Cho, Pearce, Bernheim, Fudenberg, and Levine, etc.)

- Kreps has made predictions in thousands of economic situations!
  - Why do I say that?
  - Hint: he’s never even thought about most of those situations.
- Developed Solution Concepts/Refinements, completely portable.
- Refinements different than alterations we aspire to.
  - But the spirit of portable improvements of insights.
- Pearce, Bernheim (rationalizability), Fudenberg and Levine (self-confirming) are "crudements", meant to ID bad refining.
  - But also portable and often precise.
Model ourselves after refinement literature?

- It changed (and improved!) economics.

My own taste, is “PEEMS” — portable extensions of existing models.

- Formulate a modification of existing models that let you make alternative predictions across domains, limiting yourself as much as possible to the information—RHS variables—used in existing research, and using as close to zero degrees of freedom in applying the new model.

- Almost all cases fail to achieve this ideal, but aim to come close.
My lectures (& my life ...):

- Take current model of individual, with all its assumptions.
- Read and interpret *broad* evidence.
- If you think you notice patterned and important shortcoming:
  - Pick an un-used Greek letter...
  - Toss it in with a bunch of clear RHS variables ...
  - And model away.

To illustrate the process, let us consider a hypothetical Greek letter:

- “deppa”, Ψ.
Reframe the pre-existing model as implicitly or explicitly assuming some value for $\varphi$ (usually 0, 1, or $\infty$).

- Normal-science empirics on mean and confidence interval of $\varphi$.

Theory:

- fixing environment, comparative statics on $\varphi$.
  - And then, fixing new, improved $\varphi$, can engage in the once and future core activity of economic theory:
    - comparative statics on environment.

Examples:
Modifying Preferences

- Becker, Fehr and Schmidt, Bolton and Ockenfels, Charness and Rabin on Social Preferences: $\rho$, $\sigma$
- Kahneman & Tversky, Machina, etc. on probability weighting
- Bell, Loomes and Sugden, Gul, etc. on disappointment aversion
- Kahneman & Tversky, then Kőszegi and Rabin on Reference Dependence and Loss Aversion: $\lambda$, $\eta$, $\gamma$.

Self Control

- Strotz, Thaler, Loewenstein, and Laibson on Present Bias: $\beta$
- Strotz, O’Donoghue and Rabin on Naivety about Present Bias: $\hat{\beta}$
Introduction

Errors in Statistical Reasoning (Old School!)

- Camerer, Grether and Plott, Benjamin, Bodoh-Creed, and Rabin on Base-Rate Neglect: $\alpha$
- Rabin and Vayanos on belief in LSN: $(\alpha, \delta)$
- Benjamin, Rabin, and Raymond on non-belief in LLN: $\psi$
- Benjamin, Bodoh-Creed, and Rabbin on

Quasi-Maximization Models

- Loewenstein, O’Donoghue, & Rabin on underappreciating taste change: $\alpha$
- Barberis and Huang, then Rabin and Weizsacker on narrow bracketing: $\nu$
“Behavioral Game Theory”

- Stahl, Camerer and Ho, Crawford, on cognitive hierarchies.
- McKelvey and Palfrey on QRE.
- Eyster and Rabin on informational under- and mis-inference: $\chi, \nu$.

Early days, but goal in long run be a degree of freedom.

- Let’s start to take our best shot:
Let’s start being ambitious ...

- Under- and Naive Inference:
  - Classical: $\chi = 0, \upsilon = 0$
  - Better: $\chi = .5, \upsilon = .3$

- Projection Bias:
  - Classical: $\alpha = 0$
  - Better: $\alpha = .5$

- LA and DS over changes, not just absolute levels.
  - Classical: $\eta = 0, \lambda = .618, \alpha = 1.618, \gamma = 2.618$
  - Better: $\eta = 1, \lambda = 3, \alpha = .88, \gamma = .6$

- Present bias and naivety about it:
  - Classical: $\beta = \hat{\beta} = 1$
  - Better: $\beta = .7, \hat{\beta} = .9$

- Etc.
In judging models, both old and new, and in formulating your own new models...

Realize all (useful) models are false ... try to improve

Ask two things of both others’ and own theories:
- What do they rule out? What is inconsistent with them?
- What do they say outside the exact context they are illustrated in?

Models should be general in their applicability and specific in their implications.
- Not the other way around.

And two more questions should ask ...
- Are they true? (in the sense of improving upon previous models)
- Are they quantitatively important in important economic contexts?

And now for odd-sounding question ...
Introduction

Better to explain 8% or 8,000% of how current theories wrong?

- What does the question mean?
- Comes out of many (friendly) arguments:
- My vision of what “we” are up to:
  - Improving existing theory
  - Not creating perfection

- Defensiveness of a theorist:
  - Of course our models are not fully realistic
  - All models are false — some are useful.

- Prickly/puzzled about emphasis on testing theories:
  - Discovering the theory is false!

- But especially prickly when:
  - It is false in ways that all models are false.
  - Especially when conceived as improved parameters.
Frustrating empirics:

- Old type of non-identification test:
  - Rejecting a behavioral model when classical explanation makes the same (good) prediction.

- New type of non-identification test:
  - Rejecting a behavioral model when both it and the classical model make bad predictions.

At least realize how these translate into classical econometrics:

- “I don’t need $\beta < 1$ to explain this” is not evidence against $\beta = .7...$ \text{ unless } $\beta = .7$ does worse than $\beta = 1$.
- “You can’t explain my data with $\beta < 1$” is not evidence against $\beta = .7...$ \text{ unless } $\beta = .7$ does worse than $\beta = 1$.
- “$\beta < 1$ can’t explain this self-control looking thingy” and “$\chi > 0$ this example of WC-looking thingy” have proper scientific role.
  - And should be used to provoke further improvements
  - But when used to favor status quo, they are bad science.
Introduction

What does this have to do with the question?

- These new genre of non-identification tests are all about picking on theories for not explaining everything.
  - Only getting 8% of what’s wrong with classical theory ...

But I am much more bothered by:

- The massive number of explanations & new alternatives that do way worse than classical models in massive number of domains.
- And near universal lack of focus on that.
Almost no culture of that in our community:

- In an anomalies-driven program, a tempting logic:
  - The thing we are trying to explain is the **holes** in classical theory.
  - Not the successes.

- But unless attend to the non-holes, you’ll under-appreciate:
  - How good the existing models are
  - How bad your model is

- And won’t be developing theories that can replace current theories.
  - It may not be the goal to come up with general improvements.
  - But if it is, it is manifest nonsense to ignore the non-holes.
New theories that explain 8,000% are bad general theories.

- The old theory is superior 79 times as often as the new theory.
  - If your theory were applied, would it destroy the stuff Economics 101 gets right? E.g., would all of consumer theory be shut down?
  - If prices don’t fit in your theory, what could economists do with it?
    - Etc.

And of course be skeptical of the other type of 8,000% theories:

- Vague ones that are all about degrees of freedom
  - one behavior is observed in a particular situation
  - 80 different behaviors in given context can be “explained”
Concrete examples?

- Why do bounded-rationality models (complexity-type, and some inattention-type) arguments leave me so cold?
  - E.g., bizarre ones for status-quo effects, u-game rejections
  - Because I can think of the 79 examples of equally complex (simple) tasks that people get right
Introduction

Conjectures (vague—and genuinely unsure)

- Applied **globally**, probably
  - Full rationality better than reinforcement learning
  - Nash equilibrium better than cognitive hierarchy
    - even in “first-time” play!
  - Behindness aversion worse than pure self-interest
    - probably all existing social preferences models strictly worse.

- Applied globally,
  - \( \beta = .7, \hat{\beta} = .9 \) is often better, and never worse than, \( \beta = \hat{\beta} = 1 \).
  - QRE is better than Nash

- Probably/Maybe globally ...
  - cursed vs. Nash (but communication, etc.?)
  - expectations-based reference dependence
  - NBLLN
Introduction

One big problem with my claims ...

- There are lots of deep and important papers that essentially are 8,000% theories.

The case for 8,000% theories.

- It is early days
  - If explanations do feel right in particular domains, then clearly good science to articulate.
  - Then later we work on boundary conditions
  - And often extreme forms of models useful
  - Fine, but maybe more emphasis on boundary conditions

- Maybe we really are about explaining the holes ...
  - Grand theories get us in trouble
  - Maybe we ought to go down menu of theories while looking at the data
Another genre:

- Theories with forces that tend to go opposite classical or other forces.
- Especially interested in the valid ones

“Theories” that can’t possibly be general

- More choice is bad
- Incentives always backfire

Research agenda must start moving to quantification/boundaries.

- Will senior discounts on restaurants decrease senior demand?
  - Primes them to think about their age
  - So they will walk too slowly to the restaurant ... and never get there.
Scientific Seriousness

- Not guerrilla warfare
- We are not one Ted Lecture away from knowing the right policy when people make mistakes.
  - (Assuming no mistakes takes a great deal of theory and evidence!)
- Nor one experiment away from knowing economic relevance & size.
- This literature & these lectures convey principles we believe to be true and important.
  - Where we think economics gets things importantly wrong.
  - Experiments and theory needed for empirical research.
  - But the core of Economics is empirical evidence & measurement.
  - We need lots more research on this.
- “Existence-proof evidence” and new theories:
  - the start of economic insight, not the culmination.
Introduction

Theme of camp: scientific seriousness: 

- even if “unfair”, hope you are all subject to much higher empirical standards of 
  - fully disclosure of order of hypotheses, 
  - specification mining, 
  - p-hacking, 
  - theoretical assumption-hacking, 

- than previous researchers. 

Prediction: you will be held to these higher standards, 

- In psychology, in EE, in BE, etc. 
  - good for science, 
  - good for your souls 
  - but maybe not for careers
But ...

- While monster big fan, some caveats from an economic theorist:
  - How reconcile the is-it-true-or-is-it-false? approach to theory testing with the all-theories-are-false and move-beyond-existence-proof-to-effect-size approach?

Even the very best methodology seems ...

- working from a metaphor of standards for human trials for drugs
- But is much of evidence more like animal trials?
  - Yes, learning something.
  - Very hard to interpret effect sizes!
Desperate need for quantitative hypotheses and theories...

When an identified effect goes opposite direction of an OBVIOUSLY existing effect, then, once providing ‘existence proof’ or ‘proof of concept’, we **MUST** actively start to:

- Specify clearly its domain of applicability
- Measure its size in a scientifically serious way.

To use these insights, **must** make progress on answering:

- when?
- how much?

And theory and empirics necessary allies in this battle

- Without theory, measurement can be meaningless.
- The best (intentionally) funny sign in the world ...
Introduction
General empirical wish list of an enthusiastic consumer

- Report all hypotheses and ‘pilots’.
- Hypotheses, questions, and tests either:
  - ex ante, or labeled as ex post, and timelined.
  - Learning things, making mistakes, seeing unexpected provocative patterns worth reporting. Just be clear ...
- Report your decision rules for whether to gather more data.

Grammar Lesson of the day:

- "Past Fudgitive": Verb tense used when the order of past events is obscured. Survives in modern English solely in the context of empirical research, and almost exclusively with the verb "hypothesize".
- Usage: "We hypothesized ... Our hypotheses were confirmed."
- Say explicitly: did you hypothesize that before results and as part of design?
The Fundamental Theorem of Precise Models:

- They are wrong.

The Fundamental Theorem of Imprecise Theories:

- They are wrong too!
- When too vague to commit to precise a priori predictions, researchers often never expose themselves to how they are wrong.

Anonymous psychologist (I will call him “Danny K”) defending economists to a roomful of psychologists (who were giggling at some of the sillier assertions of economists):

- “One of the ways that psychologists avoid ever being completely wrong is that we avoid ever being completely clear.”
When qualitative ...

- It almost never matters qualitatively whether mistake/departures 100% vs less than 100%.
- The qualitative difference almost always 0% vs more than 0%.
- So if you are interested in whether people use some information they should normatively use, don’t look for whether they ignore it.
- Look instead for under-use relative to the normative amount of use.
  - Base-rate Neglect
  - Koehler’s (1991) diatribe against "base-rate neglect"