

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

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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?

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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

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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 \Rightarrow

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

of standard economic analysis, but focus on introducing psychological factors heretofore under-emphasized by economists.



- Not everybody likes the term "behavioral economics". \Rightarrow I don't!. \Rightarrow
 - Slander against other economists, who also study behavior. \Rightarrow
 - 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. \Rightarrow
 - My complaint: I am very interested in welfare, not just behavior.

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Introduction

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.

A short history of “Behavioral Economics” \Rightarrow

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

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

Third Wave: Fully integrate into mainstream economic analysis. \Rightarrow

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

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

A table we'll return to later:

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Introduction

What combinations of preferences and types of mistakes could explain the general pattern of modest-scale risk preferences we observe? \Rightarrow

	DMU(W)	“Classical PT”	“News-U PT”	
Fully rational	X	X	(✓)	\Rightarrow
plausible errors	X	(✓)	✓	
Implausible errors	✓	✓	✓	

- X \equiv **can't** explain \Rightarrow
- ✓ \equiv could explain most/all instances. \Rightarrow
- (✓) \equiv could maybe explain in a scientifically serious way some of what we observe.

Introduction

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.

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Introduction

3. Beyond explaining behavior, better understand normative/hedonic effects of observed behavior. \Rightarrow

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

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

- Trade-off economists do all the time with familiar assumptions \Rightarrow
- I can credibly claim to need to dumb down my models. \Rightarrow
- Can structural IO economists with kray computers, and theorists writing Baroque models? \Rightarrow
- But also often by making our models less complicated and more tractable, especially in the long term after \Rightarrow
 - we develop our skills with new models, and \Rightarrow
 - 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 \Rightarrow

- Most economic models: take theory of individual as *input* \Rightarrow

Economic models as I see them: \Rightarrow

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

Short articles with these perspectives: \Rightarrow

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

Before more, a source of inspiration:

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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? \Rightarrow

- It changed (and improved!) economics. \Rightarrow

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

- Formulate a modification of existing models that let you make alternative predictions **across domains**, \Rightarrow **limiting yourself as much as possible to the information—RHS variables—used in existing research**, \Rightarrow and using as close to zero degrees of freedom in applying the new model \Rightarrow
- Almost all cases fail to achieve this ideal, but aim to come close.

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Introduction

My lectures (& my life ...): \Rightarrow

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

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

- “deppa”, ρ .

$\rho \rightarrow$

Introduction

Reframe the pre-existing model as implicitly or explicitly assuming some value for P (usually 0, 1, or ∞). \Rightarrow

- Normal-science empirics on mean and confidence interval of P . \Rightarrow

Theory: \Rightarrow

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

Examples:

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Modifying Preferences \Rightarrow

- Becker, Fehr and Schmidt, Bolton and Ockenfels, Charness and Rabin on Social Preferences: ρ, σ \Rightarrow
- Kahneman & Tversky, Machina, etc. on probability weighting \Rightarrow
- Bell, Loomes and Sugden, Gul, etc. on disappointment aversion \Rightarrow
- Kahneman & Tversky, then Kőszegi and Rabin on Reference Dependence and Loss Aversion: λ, η, γ \Rightarrow

Self Control \Rightarrow

- Strotz, Thaler, Loewenstein, and Laibson on Present Bias: β \Rightarrow
- Strotz, O'Donoghue and Rabin on Naivety about Present Bias: $\hat{\beta}$.

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Errors in Statistical Reasoning (Old School!) \Leftarrow

- Camerer, Grether and Plott, Benjamin, Bodoh-Creed, and Rabin on Base-Rate Neglect: α \Leftarrow
- Rabin and Vayanos on belief in LSN: (α, δ) \Leftarrow
- Benjamin, Rabin, and Raymond on non-belief in LLN: ψ \Leftarrow
- Benjamin, Bodoh-Creed, and Rabin on

Quasi-Maximization Models \Leftarrow

- Loewenstein, O'Donoghue, & Rabin on underappreciating taste change: α \Leftarrow
- Barberis and Huang, then Rabin and Weizsacker on narrow bracketing: v

“Behavioral Game Theory” \Rightarrow

- Stahl, Camerer and Ho, Crawford, on cognitive hierarchies. \Rightarrow
- McKelvey and Palfrey on QRE. \Rightarrow
- Eyster and Rabin on informational under- and mis-inference: χ, v . \Rightarrow

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

- Let's start to take our best shot:

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Introduction

Let's start being ambitious ... \Rightarrow

- Under- and Naive Inference: \Rightarrow
 - Classical: $\chi = 0, v = 0$
 - Better: $\chi = .5, v = .3\Rightarrow$
- Projection Bias: \Rightarrow
 - Classical: $\alpha = 0$
 - Better: $\alpha = .5\Rightarrow$
- LA and DS over changes, not just absolute levels. \Rightarrow
 - Classical: $\eta = 0, \lambda = .618, \alpha = 1.618, \gamma = 2.618$
 - Better: $\eta = 1, \lambda = 3, \alpha = .88, \gamma = .6\Rightarrow$
- Present bias and naivety about it: \Rightarrow
 - Classical: $\beta = \hat{\beta} = 1$
 - Better: $\beta = .7, \hat{\beta} = .9\Rightarrow$
- Etc.

- In judging models, \Rightarrow
 - both old and new, and in formulating your own new models... \Rightarrow
- Realize all (useful) models are false ... try to improve \Rightarrow
- Ask two things of both others' and own theories: \Rightarrow
 - What do they rule out? What is inconsistent with them? \Rightarrow
 - What do they say outside the exact context they are illustrated in? \Rightarrow
- **Models should be general in their applicability and specific in their implications.** \Rightarrow
 - Not the other way around. \Rightarrow
- And two more questions should ask ... \Rightarrow
 - Are they true? (in the sense of improving upon previous models) \Rightarrow
 - Are they quantitatively important in important economic contexts? \Rightarrow
- And now for odd-sounding question ...

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

- What does the question mean? \Rightarrow
- Comes out of many (friendly) arguments: \Rightarrow
- My vision of what “we” are up to: \Rightarrow
 - Improving existing theory \Rightarrow
 - Not creating perfection \Rightarrow
- Defensiveness of a theorist: \Rightarrow
 - Of course our models are not fully realistic \Rightarrow
 - All models are false — some are useful. \Rightarrow
- Prickly/puzzled about emphasis on testing theories \Rightarrow
 - Discovering the theory is false! \Rightarrow
- But especially prickly when \Rightarrow
 - It is false in ways that **all** models are false. \Rightarrow
 - Especially when conceived as improved parameters.

Introduction

Frustrating empirics: \Rightarrow

- Old type of non-identification test: \Rightarrow
 - Rejecting a behavioral model when classical explanation makes the same (good) prediction. \Rightarrow
- New type of non-identification test \Rightarrow
 - Rejecting a behavioral model when both it and the classical model make bad predictions. \Rightarrow
- At least realize how these translate into classical econometrics. \Rightarrow
 - “I don’t need $\beta < 1$ to explain this” is not evidence against $\beta = .7\dots$ \Rightarrow
unless $\beta = .7$ does **worse** than $\beta = 1$. \Rightarrow
 - “You can’t explain my data with $\beta < 1$ ” is not evidence against $\beta = .7\dots$ \Rightarrow
unless $\beta = .7$ does **worse** than $\beta = 1$. \Rightarrow
 - “ $\beta < 1$ can’t explain this self-control looking thingy” and “ $\chi > 0$ this example of WC-looking thingy” have proper scientific role. \Rightarrow
 - And should be used to provoke further improvements \Rightarrow
 - But when used to favor status quo, they are bad science.



What does this have to do with the question? \Rightarrow

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

But I am much more bothered by: \Rightarrow

- The massive number of explanations & new alternatives that do way *worse* than classical models in *massive* number of domains. \Rightarrow
- And near universal lack of focus on that.

\Rightarrow

Almost no culture of that in our community: \Rightarrow

- In an anomalies-driven program, a tempting logic: \Rightarrow
 - The thing we are trying to explain is the **holes** in classical theory. \Rightarrow
 - Not the successes. \Rightarrow
- But unless attend to the non-holes, you'll under-appreciate: \Rightarrow
 - How good the existing models are \Rightarrow
 - How bad your model is \Rightarrow
- And won't be developing theories that can replace current theories. \Rightarrow
 - It may not be the goal to come up with general improvements. \Rightarrow
 - But if it is, it is manifest nonsense to ignore the non-holes.

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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”

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Concrete examples? \Rightarrow

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

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Introduction

Conjectures (vague—and genuinely unsure) \Rightarrow

- Applied **globally**, probably \Rightarrow
 - Full rationality better than reinforcement learning \Rightarrow
 - Nash equilibrium better than cognitive hierarchy \Rightarrow
 - even in “first-time” play! \Rightarrow
 - Behindness aversion worse than pure self-interest \Rightarrow
 - probably all existing social preferences models strictly worse. \Rightarrow
- Applied globally, \Rightarrow
 - $\beta = .7, \hat{\beta} = .9$ is often better, and never worse than, $\beta = \hat{\beta} = 1.$ \Rightarrow
 - QRE is better than Nash \Rightarrow
- Probably/Maybe globally ... \Rightarrow
 - cursed vs. Nash (but communication, etc.) \Rightarrow
 - expectations-based reference dependence \Rightarrow
 - NBLLN

Introduction

One big problem with my claims ... \Rightarrow

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

The case **for** 8,000% theories. \Rightarrow

- It is early days \Rightarrow
 - If explanations do feel right in particular domains, then clearly good science to articulate. \Rightarrow
 - Then later we work on boundary conditions \Rightarrow
 - And often extreme forms of models useful \Rightarrow
 - Fine, but maybe more emphasis on boundary conditions \Rightarrow
- Maybe we really *are* about explaining the holes ... \Rightarrow
 - Grand theories get us in trouble \Rightarrow
 - Maybe we ought to go down menu of theories while looking at the data

Another genre: \Rightarrow

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

“Theories” that **can't possibly** be **general** \Rightarrow

- More choice is bad \Rightarrow
- Incentives always backfire \Rightarrow

Research agenda **must** start moving to quantification/boundaries. \Rightarrow

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

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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**: \Rightarrow

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

Prediction: \Rightarrow you *will* be held to these higher standards, \Rightarrow

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

But ... \Rightarrow

- While monster big fan, some caveats from an economic theorist: \Rightarrow
 - 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? \Rightarrow

Even the very best methodology seems ... \Rightarrow

- working from a metaphor of standards for human trials for drugs \Rightarrow
- But is much of evidence more like animal trials? \Rightarrow
 - Yes, learning something. \Rightarrow
 - Very hard to interpret effect sizes!

\Rightarrow

Desperate need for quantitative hypotheses and theories... \Rightarrow

- 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: \Rightarrow
 - Specify clearly its domain of applicability \Rightarrow
 - Measure its size in a scientifically serious way. \Rightarrow
- To use these insights, **must** make progress on answering: \Rightarrow
 - when? \Rightarrow
 - how much? \Rightarrow
- And theory and empirics necessary allies in this battle
 - Without theory, measurement can be meaningless. \Rightarrow
 - 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: \Rightarrow

- They are wrong. \Rightarrow

The Fundamental Theorem of Imprecise Theories: \Rightarrow

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

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

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

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When qualitative ... \Rightarrow

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

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