Slides for

Improving Theory with Experiments, Improving Experiments with Theory,

all so as to

Improve Traditional Economics

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My talk:

1. Explaining my own vision of integrating psychology into normal-science economic theory.

2. More generally: arguing for an (excruciatingly old-fashioned) vision of economics (as being explicitly or implicitly about comparative statics) and the role it suggests for experimental work, theory, and the interplay between the two.

3. But whether you find my approach or vision appealing normatively, I will present my empirical impression: unless it engages economic theory on a deeper level than many seem inclined, I can't see how the lessons of experimental work become integrated into other economics.

Despite the task at hand of discussing theory, I will also comment very briefly, as an outsider & fan of both, on empirical styles in experimental economics as compared to what I see in mainstream, within-paradigm microeconomic empirical work.

My gentle (?) claim is that the microeconomic empirical standards are, as an empirical matter (not comparing idealized versions or the potential of each approach), epistemologically more satisfying outside experimental work than within it. While it may or may not be true that econometric or data-set-size standards are higher elsewhere in economics than in experimental work, this claim is different: the level of engagement within most empirical microeconomics with alternative hypotheses and true identification of broadly interpretable claims about mechanisms at play is higher than in any social science. As a reader of both, and a huge fan of the best psychological research, I think the epistemological standards of experimental economics are typically higher than psychological research. But I think it is noticeably lower than elsewhere in empirical microeconomics.

The short history of "behavioral economics" I always give:

First Wave: Identify "anomalies" — ways that economic theory has been importantly wrong in its assumptions about individuals, 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 economic analysis by embedding old and new assumptions as special cases of general models, formulate new theoretical results, empirical tests, and *applications* in entirely normal-science ways, with attention to all the desiderata of economic models (parsimony, predictiveness, generality, relevance, insight, etc.)

And then I claim we are in the 2nd wave, segueing to 3rd.

(Worry now: that is just wishful thinking.)

Two premises of third-wave behavioral economics

Premise 1: Adding untraditional assumptions doesn't at all mean abandoning traditional methods.

> *"Untraditional" or unfamiliar assumptions (including those implying limits to rational utility maximization) can and should mostly be studied using exactly the same set of tools and approaches economists are used to (i.e., formal mathematical models and statistical tests using both laboratory and field data), using exactly the same scientific criteria (good predictions, parsimony, etc.) as economists are used to. The sole difference in methods and goals of most economists is the broader array of aspects of human nature we study.*
>
> *There are some domains where the newer assumptions do lead to newer approaches, and should not be shy when that is the case. But (as is even clearer when studying some other social sciences) the traditional theoretical and empirical methods of economics have huge advantages.*

Premise 2: Adding untraditional assumptions doesn't mean abandoning traditional assumptions.

> *Not only are familiar economic methods great, but to a very large extent so are familiar economic assumptions. The fact that there are limits to the correctness and applicability of these assumptions does not mean that they aren't often exactly the appropriate assumptions—nor that they aren't tremendously useful even when not exactly right. This new approach is not meant as a replacement of, but as an enhancement of—and eventual component of—mainstream economics.*

So we see some imperfect theories out there. What to do?

Reactions to shortcomings of existing models?

1. Excitement at its insights

   You should have experienced that. If you've never had moments of feeling superior to non-economists for the sharpness that the classical rational model helps penetrate the logic of some social situations, then you don't understand economics.

2. Celebration of its sufficiency

   *That is a *very* different thing. When economists or anybody with a model joyously brag that their theory "explains" anything you could tell them without realizing that that is not such a good thing, rather than that their theory informs the topic, such complacent celebration is not very healthy.*

3. Desire to make it better

4. Celebration of its insufficiency

Prefer 1 and 3, and think we should continuously improve our models, and not attack any part of it without offering an alternative. (The first word of the title of my first paper in this area is "Incorporating".)

And my view as a theorist has always been not to attack formal theories for being flawed (of course they are). It is to incrementally supply improvements.

Note: Just because all good models are flawed and false, it is of course not true that all flawed and false models are good. There are reasons to criticize formal models. I have criticized models for one of three reasons: a) they have virtually no explanatory power because they do not imply what is claimed, or b) the evidence they are based on is clearly confounded with alternative explanations; once those alternative explanations are rigorously included, the explanatory power of the purported explanation disappears, or c) the existing or easily gathered direct evidence in fact contradicts the theory, and it is only put forward because some of its indirect implications are supported in data.

Roughly put, you should criticize models because what is included in the model is fundamentally the wrong explanation for what it claims to explain. When they include things that are right but missing additional elements that are right, you should praise them and improve them.

And to have these improvements be real and functional, and engaged with what economists actually do with models.

Which brings us to the main topic:

The role of experimental and theory work

Vast majority of theoretical (until recently) and empirical economics is not about developing new assumptions about people or new methods of investigation, etc.

And it shouldn't be.

It is about seeing how extant assumptions play out theoretically and empirically in different economic situations.

And it should be.

That is, most of economics fixes the set of reasonable assumptions about people and does comparative statics and empirical measurement of the way the actual economy works.

In light of this, it is an empirical fact that mainstream economic theory and mainstream economic empirics needs formal and portable theories of individuals.

"Tweaking" existing theoretical and empirical models is how I think we are going to make the major progress. Formal, portable improvements of models of individuals that do not destroy the correct insights of extant models are what can be plugged into what the vast majority of theoretical (until recently) and empirical economists do.

With no shame, I see myself as a servant to applied theorists and empirical researchers in this intellectual production function.

And with no insult, I see experimentalists and behavioral researchers as servants (hmm... I'll find better wording) to those of us tweaking economic models.

Not sure this vision of EE as an *input* into broader empirical, applied theory, and policy analysis will be shared by experimentalists.

Predicting the behavioral and welfare responses to raising taxes on cigarettes by $1 a pack, or extending unemployment insurance, or mandating insurance, effects of economic growth, effects of pollution on health and agricultural activities, etc., effects of non-discrimination laws on employment, predict employment from a merger, etc. are not things we are going to do in the labotary. They are ecological empirical questions.

I have always wanted the lab to help inform me about how to help empirical economists ask these questions outside the lab. To feed theories of fairness, procrastination, errors in reasoning, etc., into improved theories and studies of taxes, policy, growth, pricing, etc.

The insufficiency of lab experiments and field experiments to answer some of the core questions in economics seems manifest. And the insufficiency of informal, non-measurable, and context-specific theories of human nature to help guide such work seems manifest.

Most economists do formal theoretical and empirical models with well-defined and well-conceptualized right-hand-side variables, and no amount of listening to new evidence and talking to psychologists and experimental economists changes that.

If it is hard to see how to translate empirical findings or theoretical arguments into useful economics, then not clear who should care and how we can use.

So: I come today as a consumer of psychology and of experimental insights and empirical work who sees himself as a producer of theory of individuals that economists can use to improve economics. From that angle, I comment, sometimes critically, on the orientation of much of the research in experimental (and behavioral) economics.

The point won't be "everybody should be creating formal models". By a long shot it is not *solely* the actual theory-tweaking stage that is crucial — without inspired experimental and empirical uncovering of new hypotheses, we theory-tweakers would not know how to change the model.

> *Unlike lots of theorists, I could not imagine being complacent enough about the realism of my assumptions (or arrogant enough to think theory-office introspection is the best road to realism) to think I could do it without psychologists and experimental and other empirical economists providing me evidence.*

It is that portable precise models are a necessary component for bringing the fruits of EE into mainstream economics, and so just as I do theory with an eye towards the rest of economics, I think it makes sense for EE's to do EE with an eye towards improved theory.

So I will speak from this perspective on the value of experimental research.

A sincere disclaimer: for virtually all styles of research, there are papers I like and find valuable. Even if I don't like the approach, smart and creative people have a way of doing smart and creative research. And the production function really is non-linear. And people should do what they enjoy and are good at.

So an overly structured notion of methodology, like I will sound like, doesn't really do justice to various research.

An even bigger disclaimer: I *am* a committed consumer of masses of experimental work. If talk sounds frustrated at times it is because I want to use even more.

Outline of remainder of talk:

I. The case for formal theory

- A. Tolerating simplification/inaccuracy
- B. Two crucial features of good models: power and scope

II. A framework for improving economic models of individuals

III. Critiques of experimental work based on this approach

- A. Two simple theorems
- B. Some different theory-testing modes that puzzle/frustrate me.
- C. Sentences I'd like to see in all experimental papers ...

1. The Case for Formal Theory

How can psychological factors previously ignored or underplayed by economics can improve mainstream economics with all the tools and all the goals of mainstream economics.

My dream (since I was a toddler) is for aspirations of theoretical rigour in improving assumptions.

Dick Thaler assigned me a talk at the first ever Russell Sage "behavioral camp" (16 years ago ...), "Is Behavioral Economic Theory an Oxymoron?"

The taste for ever greater realism on issues that matter — *along side* the taste for clarifying abstraction and mathematical and economic rigour. These goals seem to me not at all contradictory.

Propaganda/appeal to authority for tolerating simplification and formalism. Do we really need theory? I think yes. Insight *requires* that you not (try to) get things exactly right, but tolerate and care about the general principles.

Tacky and pointless authority citing ...

"Whoever despises theory, let him give himself what airs of wisdom he may, is self-convicted of being a quack." - John Stuart Mill (from "Coleridge")

A literary critic put more succinctly what I feel is true of much anti-theory sentiment

"Hostility to theory usually means opposition to other people's theories and an oblivion of one's own." - Terry Eagleton (from *Literary Theory*)

And formality means simplifications, and tolerance for not capturing everything every time, and for incremental progress.

> *People should not piss all over economists or economic theory for simplifying human nature. Simplifying is way too inevitable, and it is way, way, way too useful. And it is too easy to dismiss models for their imperfections. But to say a model is an imperfect rendering of the real world is a tautology ... it would be the real world. Modeling means abstraction, and means skill in rendering essential truths into simplified settings. A novelist argues to young novelists how we might:*

"... 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*)

Economists have always understood the benefits and art of simplification and clarity. And a frustration I felt with my friends across the disciplinary divide is that lack of understanding.

The historic refrain of (even friendly) psychologists in dialogue with economists:

E: “My model is this.”
P: “It is more complicated than that.”

Theorem: Let “that” be a model. Let “it” be a reality. Then for all it’s and all that’s, “it” is more complicated than “that”.

Anonymous psychologist (“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.”

The Fundamental Theorem of Precise Models: They are wrong.

And I've regretted that so much emphasis by experimentalists is on whether a model is "right" (?) or "wrong".

For any theory, the difference between showing the theory is true and that it is false is how hard you've tried to falsify it—and how generously you'v defined "true". ("The theory fits surisingly well.", etc.)

All models are wrong. Some are useful.

Either people say that they don't want models at all (then why test them?), or the language and techniques has to move beyond testing whether a model is true or not. It can't be literally true.

It is intellectually immature to condemn a model for having the general attributes of a model. Either decide that all models are bad, or don't condemn a model for a feature that is inevitable from the fact that it is a model. And being "wrong" is an inevitable feature.

The flip side of wrongness of precisely stated models: the enticing false feel of insight from vague theories:

The Fundamental Theorem of Non-Precise Assertions about the Implications of Findings: they are wronger than the asserters realize.

Putative insights always look better when loosely stated.

Every single PhD theory student in this history of Doctoral programs has discovered by the process of actually trying to formalize his claims: that his intuition was wrong wholly or substantially.

My claim is not that experimentalists should leave implications to some set of people who are more talented at modeling. It is that experimentalists would have to be 3 times smarter than Paul Samuelson to see the mapping from experiment to its economic implications as clearly as connoted sometimes.

Indeed, most economics-trained experimental economists are in fact quite reasonable and cautious; the boldness of the claims is among those who in fact have engaged in the act of carefully drawing conclusions least. People who haven't thought enough about implications of findings are the ones who make the boldest claims about it.

As an avid consumer of psychology who did not expect them to be good economists, the predictably irrational overconfident pronouncement by psychologists about the implications of their experiments amused rather than offended me.

But when experimental economists do it, it is far more jarring.

Which brings us to something that is especially jarring (although this is my reaction to many observational-data empirical studies that present models, too):

The Fundamental Theorem of Data-Set Specific Models: They feel way, way more insightful and right than they are.

Which in turn brings us to two crucial criteria I'd love to see emphasized more in theories. These are **not** the only two criteria to judge models on. I am emphasizing them because they are the two valuable criteria which are most noticeably missing these days.

Besides tolerance of the necessary evil of being imperfect when you are precise, there are two crucial criteria for theories (even when not formal) should aim for:

I want to speculate two very big desiderata of theories, besides correctness, that in some circles (it seems) these days have almost no traction.

This holds for verbal or math theories put forward by many experimentalists, and separately by many theorists.

Power and scope:

1. Power and restrictiveness

Among many proposing new theories, almost complete neglect of the "power"/restrictiveness of theories: do they actually tell you significant stuff *not* to expect? Do they make different or sharper predictions than competing theories?

In some theoretical circles, it is virtually a forgotten goal. Or worse: the less your model actually tells us, the more you brag it is “general”.

> *(the sin of assuming a functional form is considered much, much, much, much worse of a sin than non-predictiveness, in large part because non-predictiveness is considered no sin at all.)*

I am not so much worried that no restrictions means not "falsifiable", as is often emphasized. (Since all theories are false, framing this as a goal seems slightly off to me.) So the point is not just its empirical testability.

The point is more obvious but somehow rarely appreciated: if a theory is not restrictive, it is useless.

> *E.g., If you explain your data set by imagining huge errors (say), but have not stood by a particular theory of those errors, have you pointed what outcomes you could not see from equal-sized errors?*

The fact that your theory is not falsifiable means that it has no implications. (This is not true for welfare considerations; of the infinite number of normative theories that are consistent with any given traditional data set, we may wish to speculate openly—rather than buriedly—as to which ones seem reasonable.)

2. Scope

This is clearly missing as a goal from so many experimental theories.

Do we want a theory of humans who are herding? A theory of humans who are playing the ultimatum games? A theory of humans who are bidding in auctions? Etc. that are separate from each other?

Different things *matter* in different contexts (e.g., departures from self interest don't matter in many contexts.). But the motivations and cognitive styles shouldn't. In this light, it is really really weird to come up with a new theory of humans for each new experimental game.

> *What if instead of running a herding experiment or auction experiment on two separate days, you randomly assigned subjects to two different games when they entered.*

What if you propose a theory of cognition in herding that ruins your predictions in other settings?

Experiment-by-experiment menus of models may beat Bayesian Nash equilibrium in each experiment, but each of them could be dramatically inferior to BNE accross games.

Theories are supposed to have some portability and make empirical predictions or aid with interpretation of the world across situations that economists care about, rather than replicate data in a specific context.

If you have no ambition to be a serious part of the process of making models usable to predict in other contexts, don't bother. [Comments about people's theories of the ultimatum game deleted.]

Again, the problem is not just that you can't test your theory in other contexts if it is not specified in other contexts. It is that your theory doesn't matter. We've seen the data in your context already; we don't need to redescribe it with a model unless we want the model to mean something besides the specific data.

When it **does** have implications in other contexts, you should admit it, and be interested, and open. And use it to double check your intuition.

Here's a worrisome fact, which is true of lots of "theories" (admittedly, some I like) I see proposed: if they were applied across situations of interest to economists, the model would do worse on average than the one it replaced.

E.g., the popular "idiocy theory" that comes in many guises:

- Explain away some departure from pure self-interest in binary choices as failed attempts to maximize self interest because people can't optimize between two choices, or (mysteriously) don't bother choosing the right button or right word because their choice may not matter.
- Or they think they are in a bargaining situation 100% of the time.
- The growing mini-industry of explaining every behavioral anomaly by contemplation costs or cognitive limitations, people don't bother taking a better-than-fair bet despite liking money because it is too complicated, or "yes" takes more effort than "no". (And then they seek out extended warrantees because?)

I think a lot of these theories (*as explanations for errors in non-complicated situations*) are silly, easily testable, and easily rejectable as improvements on rationality, and misleading.

But if they are meant to be serious hypotheses, they would change economics as we know it.

If you think people are throwing out money (e.g., selfish people slipping and rejecting offers, non-loss-averse people slipping and rejecting profitable risk) in lots of binary choices with blatant consequences, then you've got more to say.

If people were as incapable of implementing their wants in unfamiliar situations as some theories suggest, with unfamiliarity defined as broadly as it is, it would change consumer theory as we know it.

Heuristic: if you explain some experimental behavior with an explanation of human decisionmaking and behavior that if applied elsewhere in economics would ruin every result ever derived in any economics class and economics article, and if true about people would shut down every economy in the world, don't be too happy with it.

But if you don't articulate a theory of human nature in laboratory herds that at least extends to laboratory auctions or markets etc., or you propose a theory of laboratory ultimatum games that does not extend to other simple and simpler games and choices, I worry.

And if you don't want to ask those questions, and aren't conceiving of your theory as anything but an accommodation of the particular data set you have generated, you leave me puzzled as an economist, and you have just announced that nobody who doesn't want to study your particular experiment per se should care about your result.

Combining these two—power of a theory, and scope of a theory—the ideal, in my mind, is that theories should be

1. *general (unrestrictive) in their applicability*, and
2. *specific (restrictive) in their predictions*.

Not the other way around.

Too many people these days, especially in experimental (and behavioral and psychological economics) have no taste for the second, often literally not telling you a single thing their theory rules out. (In the case of the verbal theories—like knowingly observing that people are "boundedly rational" as an alternative to existing formal models of bounded rationaly—illustrating things your description of people rules out across settings would be especially useful confidence-builder that the theory is not just pointing out the existence of error terms, since readers have not been provided the means of working out implications .)

And my strong impression of experimentalist culture is that there is very little engagement with the first.

Framing I'll return to: theories of the individual ought to be a coherent (ideally formal) statements mapping a situation into the person's behavior, with the domain as broad as possible, with either familiar, widely used RHS variables, or a clear, focussed attention on whether and how to ID the new RHS variables.

To use an example that is apt for experimentalists, and that I'll return to: have you specified your theory in your context in a way that is defined in all simple Bayesian games? That is, it is a mapping from dollar payoffs, informational and strategic structure, into behavior? Many models pass this test. A huge number don't.

Final, blatantly self-serving comment: even if you are not interested in that exercise of specifying your theory beyond your own data set, be aware of the epistemological and methodological oddness of comparing a non-portable, data-specific theory to one that aspires to portability and broad applicability.

If you want to know why many economists fail in *your* goal of tracking your data, it is because it is not their goal.

2. An approach to developing more realistic theories

In my view: behavioral economics has the potential to contribute to the resurgence of theory into empirical economics. Recent years a lot of (good) empirical tests that don't engage very intimately with theory. This is partly because greater open-ness by economists to different questions, and (quite healthy) open-ness to the fallibility of traditional models.

But when no theories explain the data, hard to be guided by theory.

BE will help provide greater guidance and discipline to some lines of inquiry.

To facilitate this, my own taste, is "PEEMS" — portable extensions of existing models. Can you 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 in some ways, but we shall aim to come close. And then be in place to commit radical acts of normal science by turning it all into boring numbers.

Let's mock the tweaker's formula ...

- Take current model of individual, with all its assumptions.
- Read some Kahneman and Tversky paper, or Dan Levin experiment.
- 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", Þ.

Reframe the pre-existing model as implicitly or explicitly assuming some value for Þ, usually 0 or 1.

Empirically: normal-science research on mean and confidence interval of Þ.

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

(Note: I conceive of policy analysis as CS, with welfare as LHS.)

Take old model:

$$y = \text{Þ}_0 x_0 \; + \epsilon.$$

PEEM: Contend that instead

$$y = \text{Þ}_0 x_0 + \text{Þ}_1 x_1 \; + \text{Þ}_2 x_0 x_1 + \epsilon, \; .$$

PEEM(1): x_1 already a universal input into existing economic models.

PEEM(2): x_1 is a new factor, but observable, and preferably exogenous.

P-MEM: Say instead that existing model has wrong value of some Þ. Better value is $\text{Þ}'$.

PEEM(1)s and P-MEMs: Use existing data for empirical work, and existing economic questions. PEEM(2)s need to expand domain, but once do, full comparability to old theories.

Strotz, Thaler, Loewenstein, and Laibson: β.

Strotz, O'Donoghue et al: $\widehat{\beta}$.

Loewenstein, O'Donoghue, et al: α

Becker, FS, BO, Charness et al: ρ, σ

Kahneman & Tversky, then Koszegi et al: λ, η, γ.

Vayanos et al on belief in LSN: (α, δ)

Eyster, et al: χ, υ.

Benjamin et al on non-belief in LLN: ψ.

Barberis and Huang, then Weizsacker et al on narrow bracketing: υ

K&T, Machina, Prelec, etc. on probability weighting

Stahl, Camerer and Ho, Crawford, etc. on cognitive hierarchies.

(Calibrationally disciplined) QRE.

Types of theories? Minimal portability: RHS variables incorporated allow for all RHS we regularly include (although great and powerful if insist some coefficients are zero)

Closed Models: Strong portability: you include *only* standard RHS variables, and make predictions based solely on that. Examples? See above.

Open Models: includes all RHS variables and an internal additional assumption that isn't in RHS, but is a) conceptually tight, and b) doesn't 'want' to be different assumption each time. Examples?

> *Level K, choose the K, and the Level-0 behavior*
> *Uncertainty Aversion? Choose subjective uncertainty?*
> *Confirmatory bias?*

Frameworks: includes all RHS variables, plus additional RHS that actively self-identify as being fresch assumptions beyond the assumptions. Examples?

> *Analogy-based equilibrium? Categories?*

Perfectly good science to do merely "frameworks" or open models. But: don't compare frameworks to models, or open models to closed models, and declare victory. One is making precise new predictions, the other is accommodating new predictions.

I would tend to reserve the word "explain" for when you do the model, where you are actually making the prediction rather than accommodating what you observe. That is, you are ruling out what you don't observe.

But forcing articulation in my terms forces us to see the difference between a theory and a framework. Just tell me what your model says in each situation. If you can't, tell me what additional things you need to "close" the model, and pay a bit of attention as to whether we can rule anything out in any situation without closing the model.

At least give examples of closing the model.

Can somebody take your finding NOT to do another variant of your experiment, but to do theoretical or empirical economics? Could YOU? (Mightn't you?)

And make sure people understand that this will not in the long run be a degree of freedom.

- Present bias and naivety about it:
  - Parameters: $\beta \leq \widehat{\beta} \leq 1$, δ.
  - Classical: $\beta = \widehat{\beta} = 1$, $\delta \in [.2, .96]$
  - Better: $\beta = .7$, $\widehat{\beta} = .8$, $\delta = .96$

- LA and DS over contrasts, not just absolute levels.
  - Parameters: $\eta \geq 0$, $\lambda \geq 1$, $\alpha \leq 1$
  - Classical: $\eta = 0$, $\lambda = 1.618$, $\alpha = .618$, CRRA, $\rho \in [0, 10, 000]$ (or $w \in$ [30 minutes,90 years])
  - Better: $\eta = 1$, $\lambda = 3$, $\alpha = .88$, CRRA $\rho = 2$, $r =$ beliefs.

- Etc.

These are all PEEM(1)s, except Charness et al more of a PMEM on saying that FS/BO had wrong sign on one of their parameters, and Koszegi et al on specifying a different reference point.

- Eyster and Rabin: underattentiveness to and naivety about informational content of others' behavior
  - Parameters: $\chi \in [0, 1]$, $\upsilon \geq 0$
  - Classical: $\chi = 0, \upsilon = 0$
  - Better: $\chi = .5$, $\upsilon = .3$

- Loewenstein, O'Donoghue, and Rabin: projecting current preferences despite predictable changes.
  - Parameter: $0 \leq \alpha \leq 1$
  - Classical: $\alpha = 0$
  - Better: $\alpha = .5$

This over states the PEEMishness. (need, e.g., cardinal assumption; and non-simplified form of projection bias is sensitive to conceptualization of states).

But this reframes different theories as nothing less and nothing more than different parameter values, modifying the exact same set of observable variables.)

Advantage 1: Commiting Radical Acts of Normal Science

The normal-science part: theories that map observable RHS variables into LHS variables that economists care about.

The radical part: once you get into expanded models posed in terms of coefficients on well-defined RHS, you don't accept any asymmetries in hypotheses. Empirically, point estimates and confidence intervals ... no treating particular parameter values as the nulls by dint of having a longer history of appearing in journals, unless evidence is there.

Turn debate over new theories vs. old into fight over a parameter, and empirically over mean and confidence interval of that parameter.

Add scientific discipline to those resisting new theories: Once we can articulate it, no status quo bias. No miscounting "number of assumptions". No fantasy agnosticism, pretending that you want to wait and see before making any assumption. When you are really advocating keeping the current parameter value until "proven".

No empirically incoherent claims that one of the theories is unstable ... ummm. If the coefficient estimate is unstable, then it means that you are missing something. But ridiculous to preserve old value. To correctly fret about estimates $\widetilde{Þ}_1$ not stabilizing $\in [0, .8]$ is hardly an argument for sticking with $Þ_1 = 0$.

Fairness is not stable if and only if self-interest is not stable.

If β varies from one situation to another, then always assuming particular $\beta < 1$ imperfect, and always assuming $\beta = 1$ is more so.

Once we turn this into normal science then the scientifically pernicious beasts of “You can't reject my null" and "your theory is not perfect" get tamed.

No loose theoretically incoherent "it doesn't matter" when in fact it does.

> *Here's the CC industry on $\beta = 1$. Here's the CC industry on $\beta = .7$, $\widehat{\beta} = .9$. It matters. And it is truer. In fact, clearly BE is leading to rebirth of market analysis and price theory, and all the serious work on what the market implications of new assumptions are happening by BEists, not anti-BEists (maybe Barro's "Laibson-meets" is an exception). Once we have these models, we are going to see a reinvigoration of market analysis that we are already seeing, rather than this sub-genre of market mysticism that independent of any economic reasoning, somehow magically any hypothesis proposed by Kahneman and Tversky gets wiped out by markets.*

And no policy analysis based on worse estimate rather than better. E.g., Gruber-Koszegi, O'D-Rabin: Map beliefs about distributions of beta's into optimal tax. No two-step shenanigans. $\beta < 1$ unproven ... so set optimal tax by $\beta = 1$!

If this were all normal-science discussions, would force everybody to map beliefs about parameter into prescriptions, predictions, tests, etc. No anti-scientific asymmetries.

But also add scientific discipline to those dissing old theories and (ideally) proposing new theories.

> *Fine science to point out shortcomings and nothing more. But more useful and better to propose alternatives. And to have those alternatives be clear enough and implementable enough to do something with.*

Lots and lots of theories, including some behavioral ones, looked artificially better than classical model because not pinned down, and are selective about contexts applied.

Healthy open-ness to context dependence amongst BEs should not be pretext for complacency as to what general lessons there are.

Advantage 2 is more the topic today;

Advantage 2: clarifies theory and experimental economics

I'm going to give some theorems regarding theories, and based on these and more generally talk about experimental tests that puzzle me.

The point: I think once you regard theories as mappings from situations (RHS variables) to some description of an outcome; theoretically and in terms of regression analysis, it shows weirdness of some common tests.

All my frustrations and puzzlements relate to the basic perspective of seeing a model as a hypothesis about a parameter value. And seeing that every model any of us write down are about extending and modifying previous models. So anything besides saying that the proposed new parameter Þ modifying a variable x doesn't take on the value claimed. Showing that there are other variables missing means another improvement is called for. And criticisms of the model using data that don't actually speak to the value of Þ. So these are empirical tests that in fact are not part of classical theory techniques or classical regression analysis.

Two Theorems, especially relevant to experimentalists because they design and build their own data sets.

Suppose that the true model of y is such that there are two variables x_1 and x_2 such that $\text{Þ}_1 \geq 0$ and $\text{Þ}_2 > 0$.

That is: suppose that there is more than one thing that effects some LHS variable.

E.g., sharing is caused by Rawlsian preferences and altruism/efficiency and reputational concerns etc.

Or harming others caused by envy or retaliation

Theorem 1: There exists data X such that letting $Þ_1 > 0$ has no explanatory power. You'd do as well allowing $Þ_1 = 0$ as $Þ_1 > 0$.

Proof: Choose X *such that* x_2 *varies but* $x_1 = 0$*. Q.E.D.*

Corollary: there exists data set where $Þ_2 > 0$, $Þ_1 = 0$ wins a horse race vs. $Þ_1 > 0$ and $Þ_2 = 0$.

My real corollary: dissing a model for not having explanatory power in constructed data set in and of itself is a really misleading thing.

A would-be miracle theory has value added over prior theory *in all settings* only if no omitted variable has any explanatory power.

Theory-testing styles that puzzle/frustrate me #1a: showing that a theory doesn't help explain some anomaly. Theory-testing styles that puzzle/frustrate me #1b: Horse Races

Theory-testing styles that puzzle/frustrate me #2: Showing some RHS isn't the sole determinant of some LHS variable.

Theory-testing styles that puzzle/frustrate me #3: Testing how much some theory matters on experimental data rather on how big it would be if true on real data.

Theorem 2: There exists data X such that $Þ_1 > 0$ and $Þ_2 = 0$ is as consistent with the data as $Þ_1 = 0$ and $Þ_2 > 0$.

Proof: Choose fully colinear X.

My real corollary: dissing a model by saying that you know of a set of situations where an alternative can explain it is weird on two counts.

1. "Theory A and Theory B make the same prediction, therefore Theory A is better" is a puzzling epistemology. You can always trace the status-quo theory or the prejudice of the researcher by his ability to make such a statement. Anybody looking at dozens of papers with the punchline "Here's a situation where some error some researchers claim is prevalent would predict a behavior. But I show it can be explained by rationality" could easily figure out it that it came from a discipline historically attached to the rationality assumption

2. You don't search out colinear data in any normal-science research. You separate.

And defending your model by saying that there exist *constructed situations* where it is a good proxy for the right explanation is a really misleading thing to do.

Arguing that one explanation/variable (especially a simplification) is a good proxy for another must be based on what situations you think are prevalent. Experiments don't test what situations are prevalent. (They can't). Being a good proxy is domain specific. The relevant domain for judging whether two models are good proxies is the non-experimental distribution of RHS, not the experimental one.

E.g., behindness aversion may or may not be a a good proxy for retaliation in the ultimatum game. It is an awful proxy in prevailing economic situations.

Theory-testing styles that puzzle/frustrate me #4: Rejecting previous models by proposing a theory that explains LHS values with different RHS values than actually determine it. Type 4a: provide no evidence that your theory has traction. Type 4b: provide evidence that your theory has traction.

Theory-testing styles that puzzle/frustrate me #5: Rejecting the theory by absence of smoking gun

Theory-testing styles that puzzle/frustrate me #6: by attacking the increment rather that basic model

Theory-testing styles that puzzle/frustrate me #7: Look for *THE* RHS variable that determines LHS value, and interpreting a theory as trying to be a theory of the LHS rather than the RHS variables and coefficient.

Theory-testing styles that puzzle/frustrate me #8: "confound complacency"

Theory-testing styles that puzzle/frustrate me #9: testing an implication of your theory, but not listing any credible theory that does NOT make the same prediction

Sentences I would like to see in every experimental paper

Until it is fully and maturely absorbed that the point of a theory is not whether it is false, but rather whether it is an improvement over previous theories, as completely understood, should begin with the 1.5 sentences "All theories are false. In light of that fact, ..."

> *Why? So that the paper doesn't merely reject a theory.*

Every introduction should say: "Our hypotheses are ... If we find X, we would conclude A ... If we find not X, we will conclude B."

> *Why? So that you make sure you're evidence has any power in lending support to your explanation, and that your straw men get exposed. If you are only rejecting randomness or contrarian behavior, etc., say so.*

And every introduction should say: "We could not think of a simpler more direct test of our hypotheses"

> *I have no idea why it is acceptable as it is to propose hypotheses for complicated games when obvious it could be tested on simpler ones. I have started to reserve the right to be skeptical. Author may be purposely adding confounds. More often, the hypothesis was ex post, not ex ante.*

The concluding section should say either:

We cannot think of any other likely confounds to explain our result.

Or:

Our interpretation is confounded with Theory Z, but we could not think of a design to distinguish the two."