Design II: Inference

Ryan Oprea

University of California, Santa Barbara

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The second characteristic of a good design is that it enables a maximally clear test of these questions. It should:

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- Avoid confounds by clearly mapping how the design enables hypothesis tests prior to data collection.
- Avoids causal ambiguity by using exogenous treatment and parameter variation to directly test as many of the questions as possible.

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Finally, a good design accomplishes all of this efficiently without wasting time and money.

Some standard terminology

Subject: An individual being observed in the experiment.

Session: A single meeting for data collection using a single set of subjects.

Period: A repetition of the decision task (or tasks) within a session (the meaning of the term differs from experiment type to experiment type).

Parameter: A (usually numerical) variable in a model that may or may not be changed in the experiment (the connotation in using the term is usually that it will be changed from treatment to treatment).

Treatment Variable: A set of conditions in the experiment that will take on more than one level over the course of the investigation. Similar in meaning to parameter.

Treatment: A collection of settings of the treatment variables.

Experiment: A collection of treatments designed to answer a question.

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3. Statistical Design: A choice about how treatment variables will be applied and arrayed with respect to one another (the factorial design) and with respect to sessions and subjects.

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3. Statistical Design: A choice about how treatment variables will be applied and arrayed with respect to one another (the factorial design) and with respect to sessions and subjects.
4. Power Considerations and Sample Size: A choice about how many sessions to run, how many subjects will be in the session and how many periods to run.

The rest of this lecture is mostly about the considerations that go into these choices.

An Experiment is a Living Model

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- List players, actions space, payoff functions, time, information, information sets etc.
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The model you implement in the lab will probably come from one of these places:

- You start with a model from the experimental literature.
- You start with a model from the theoretical literature.
- You start with a question and search for the right model from the theoretical literature.
- You start with a question and build the right model from scratch.

Designing a Video Game

The key to figuring out how to implement an experiment is to decide in exact detail how you would transform your model into (essentially) a video game (or a hand run game):

- What choices will subjects face?
- What feedback will they get.
- How will subjects interact?
- How will the decision problem, preferences etc. be described to subs?

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Might seem silly but thinking about this in every detail early on can be hugely clarifying:

- Especially important for complex experiments.
- Helps you determine feasibility of project.
- Helps make tough design decisions very early on.
- Thinking through subject experience often raises important alternative hypotheses.

Real Options Video Game

Subject choices: Subjects choose in real time when to make an investment

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Subject Interaction: Single agent model so no real need for interaction. Still

- Social Learning: Could show other subjects' decisions from the same session.
- Ballinger et al. (2003) show this improves decisions a lot in other dynamic stochastic games.
- Cost of this is I lose statistical independence from subject to

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Subject Interaction: Model spells out how strategy profile impacts earnings.

- Could give them tools outside the model that model says shouldn't matter (like chat communication).
- Again, could create scope for social learning by showing decisions from other pairs.

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- Automate decisions you're not interested in
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Big Idea: Complex equilibria or optimization problems often have several interdependent parts. If uninteresting parts can be automated, inference over the interesting parts becomes much easier.

Classic Boiling Examples

Ultimatum game:

- Grew out of questions about willingness to punish inequitable distributions in alternating offer bargaining games.
- The repeated structure of these games makes for complicated inference.
- Guth et al. (1982) noticed the game could be boiled down to a simple 2-stage game, focusing crisply on a single central question.

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Oligopoly problems:

- Early experiments examined posted offer markets by having one set of subjects induced as firms and the other as buyers.
- But all interesting action comes from the price setters (buyers do fairly predictable things in these games).
- Common now to use "robotic" buyers.
- Standard in fact in most contemporary IO experiments (i.e. Cournot experiments).

Straying from the Model

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Especially: translation of **continuous elements** of the model into simplifying discretizations. These can wreak havoc, destabilizing old equilibria and creating new ones:

- Continuous time
- Continuous action spaces
- Continuous populations
- Brownian motion

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Always examine a complicated model via simulation.

- Program robot agents to make decisions in your exact implementation.
- Have robots make range of decisions including optimum.
- Are reward functions / best responses maximized as in the model?

Straying in Our Examples

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- Whenever forced to stray, let theory be a guide.
- Well known binomial approximations already in the literature that can come arbitrarily close to true Brownian motion.

Let V iterate up or down by a fixed proportion h with probability p over a tiny time interval Δt. Then we have approximate Brownian parameters α and β approximated well with small Δt

$$\alpha = \lim_{\Delta t \to 0} \frac{(2p-1)h}{\Delta t}. \quad (1)$$

$$\sigma^2 = \lim_{\Delta t \to 0} \frac{4p(1-p)h^2}{\Delta t} \quad (2)$$

Simulating Real Options

Real Options Example: Are my approximations “close enough” to recover theoretical optima?

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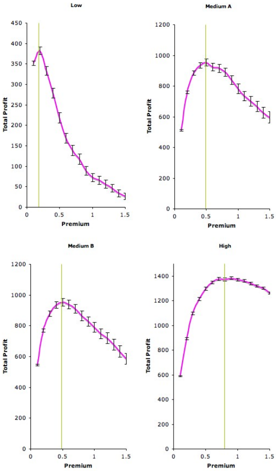

Ask Yourself These Questions

- Is there a simpler model that allows me to get at my question? Use it instead.
- Are there players in the model whose decisions have nothing to do with my question(s)? Automate them.
- Are there parts of players' decisions that fail to illuminate my problem? Automate them.
- Once I've reduced my model, is the player's decision problem trivial? Backtrack.
- In order to turn this model into an experiment, do I have to deviate from the model and do these deviations matter?
- Does the resulting model, if brought to the lab, avoid the key Inducement problems discussed in the last lecture set?

All boil down to the (slightly) **unhelpful advice**: Make the model as simple as possible but no simpler.

Choosing Treatment Variables

A treatment variable is just something you plan to vary during the experiment. What motivates treatment variables? Some of these items overlap:

- Comparative statics from a model
- Suspicious theoretical isomorphisms from theory
- Payoff irrelevant elements that seem to matter in the field but not in theory (like frames, focal points etc.)
- Other things that seem to have effects in the field but not in theory
- Variation needed for structural identification
- Need to show robustness (stay out of a narrow corner of the parameter space)

Most importantly, treatment variables flow directly from your central question(s).

Treatment Variables

Continuous Example:

- Time: Discrete vs. "Freeze Time" Continuous
- Epsilon: Vary π_M, c and number subperiods (T) to vary the costs of playing Continuous strategies in Discrete time.

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Real Options Example

- Binomial parameters: p, h and q.
- Could also vary the nature of feedback to subjects.

Calibrating Parameter Values

Most models feature a lot of greek symbols that will need to be filled in before you take it to the lab. How do you choose?

- Make sure you're looking at a representative portion of the parameter space of interest.
- If this isn't possible, consider studying your experiment under a wide set of parameters, perhaps varying them over periods.
- Too much parameter variation however can interfere with inference by thinning data.
- If you're going to be varying parameters to test a comparative static predictions, make sure the predicted effect is large enough to be observable in your noisy data.
- If possible, consider studying different parameter sets that generate the same prediction (but for different reasons) to sharply test theory.
- Make sure deviations in action space represent substantial deviations in payoff space under your parameters.

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Fixed Parameters: Main one to choose is Δt – picked 0.003 minutes, largely because it “looks” continuous to human eyes.

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Treatment Parameters: Vary (h, p, q) to fulfill a number of the criteria above.

- Low: $(0.0155, 0.513, 0.007)$ generates a $w = 0.18$
- Medium A: $(0.0155, 0.524, 0.003)$ generates a $w = 0.49$
- Medium B: $(0.03, 0.524, 0.007)$ also generates a $w = 0.49$
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Get a lot of parameter coverage, get relatively steep payoff hills, get large predicted effects (Low vs. High) and two very different parameter vectors that yield same prediction.

Parameters in Continuous Example

We have to choose a parameter vector (π_R, π_M, c) and values of T to take to the lab. Simon and Stinchombe give us a nice starting point of $(3, 2, 1)$ but has really flat payoffs.

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Treatment Parameters: Probably will vary the vector (π_M, T) in the design.

- Note for any $T <$ inf discrete Nash predictions are invariant to (π_M, T)
- but ϵ threshold needed to play continuous strategy instead varies in both parameters.
- **Idea:** Fix two ϵ-threshold values, ϵ_L and ϵ_H.
- Choose several combinations (π_M, T) that generate these ϵ_i in different ways.
- Test (i) whether ϵ matters and (ii) whether ϵ is all that matters.

Factorial Design

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Idea: observe subjects under a set of interactions between treatment variables

- **Factor:** A set of variables that will be varied in the experiment.
- **Level:** A setting for a variable.
- **Cell:** A combination of level settings across all factors.

Full Factorial Designs

A full factorial design consists of the full combination of factors and levels.

- The convention is to describe the size of the design as a {factor} x {level} or $k \times n$.
- This will lead to n^k cells of the design (a cell in this context is a treatment).
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The most important thing is to observe level changes with all else held constant on the treatments you are most interested in.

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The so-called **sparcity-of-effect principle** suggests that only main effects and lower order interaction effects are likely to be important.

Factorial Design in Continuous Example

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	Low- ϵ	**High-** ϵ
Discrete	L-D	H-D
Continuous	L-C	H-C

Where $\epsilon = f(\pi_M, c, \overline{T})$

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Discrete	L-D	H-D
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Where $\epsilon = f(\pi_M, c, \overline{T})$

In addition to point predictions of model, can test following hypotheses using this data to get at main experimental questions:

- Continuous is different from Discrete: $\frac{t^{L-D}+t^{H-D}}{2} < \frac{t^{L-C}+t^{H-C}}{2}$
- ϵ influences wedge between Continuous and Discrete: $|t^{L-D} - t^{H-D}| > |t^{L-C} - t^{H-C}|$
- ϵ increases wedge between Continuous and Discrete: $t^{L-C} - t^{L-D} < t^{H-C} - t^{H-D}$

Which are main effects and which are interaction effects?

Fractional Factorial Designs

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This is a big topic, but an example can illustrate. Suppose you are interested in 3 factors, each at 2 levels. A full fractional design requires $2^3 = 8$ cells. You can run a $2^{3-1} = 4$ cell design instead and if done properly can recover the main effects (but none of the interactions). To avoid confounds of main effects:

- Run every level in exactly two cells.
- Make sure no two cells have two factors sharing the same levels.

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- Make sure no two cells have two factors sharing the same levels.

Doing this will confound main effects with interaction effects so it only works if interactions are thought to be unimportant.

Half Factorial Design in Real Options Example

Real Options example uses a half-factorial design.

Treatment τ	Step Size h	Uptick Prob p	Expiration Prob q	Option Prem w^*_τ
Low	0.0155	0.513	**0.007**	0.179
Medium A	0.0155	**0.524**	0.003	0.490
Medium B	**0.03**	**0.524**	**0.007**	0.499
High	**0.03**	0.513	0.003	0.804
Effect on Premium	0.317	0.003	-0.308	

Main Effects:

$$\bar{h} = \frac{w^*_{High} + w^*_{MedB}}{2} - \frac{w^*_{Low} + w^*_{MedA}}{2} = 0.317 \quad (3)$$

$$\bar{p} = \frac{w^*_{MedA} + w^*_{MedB}}{2} - \frac{w^*_{Low} + w^*_{High}}{2} = 0.003 \quad (4)$$

$$\bar{q} = \frac{w^*_{Low} + w^*_{MedB}}{2} - \frac{w^*_{High} + w^*_{MedA}}{2} = -0.308 \quad (5)$$

Between and Within Design

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In many experiments, some of the factors/treatment variables are applied between subject while others are within subject.

Within Subject Design

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Pro:

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Con:

- Behavior under one treatment may impact decisions under the other.
- Because treatments must be applied in sequence, possible to confound learning with treatment effects.
- (Can be partially eased by varying the order of treatment across subjects or following an ABABAB... design)

Blocked Designs

A within subject design is an example of a statistical principle called "blocking".

The idea is, if you can find "nuisance factors" (individual subjects or sessions bring many of these with them) that are sources of variability, you can run all of your treatments under each nuisance factor to increase precision (by reducing the degree to which variance in nuisance factors drives variance in measured treatment difference).

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Some common blocking variables

- Sessions/subjects
- Lab/experimenter
- Demography (i.e. gender, culture)
- Time/Order

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Between and Within in Our Examples

In **Real Options example**:

- I need a lot of variation in stochastic realizations to identify behavioral rules.
- I really want to get a feel for long term learning.

Both suggest I should avoid a within-subject design.

In **Continuous example**:

- I want ample opportunity to get a feel for joint payoff maximizer (continuous prediction).
- ϵ doesn't effect this if I design carefully.

Suggests I *could* vary either of these within session but I would want to block carefully with time to avoid confounding time with treatment:

- If Continuous/Discrete is applied between-subject run in each: High-ϵ / Low-ϵ / High-ϵ / Low-ϵ / ...
- else a fully within-design: L-D / H-D /H-C / L-C/ L-D / H-D/...

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Will subjects will be repeating the experimental task? Most experiments feature repetition and for three possible reasons:

- Improve salience: Subjects often get a feel for the game based on a bit of experimentation.
- Study the learning process: In many studies how subjects learn is the center of the investigation.
- Enable within-session treatment variation: You can't achieve within-subject variation without having subjects make more than one decision!

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- **Stationary repetition:** Subjects do the exact same thing in the exact same environment repeatedly.
- **Within-session treatment application:** As above but elements of the design are changed from period-to-period
- **Time-blocking:** Treatments are varied within a block of periods and these blocks are repeated in order to avoid confounding experience with treatment.

Matching

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- **Partner matching:** Experiments explicitly studying supergames and reputations match subjects with the same partner(s) repeatedly.
- **Strangers matching:** Experiments aimed at gathering information on one shot games often randomly and anonymously rematch subjects with one another from period to period to short circuit supergame/reputation effects.
    - "Zipper" designs are matching protocols that attempt to prevent subjects from being rematched with each other at all during the experiment.
    - Healy (2007) offers a useful caution: group reputations are possible even in such settings.

Matching

Other increasingly popular matching strategies:

- **Repeated-repeated:** Since Selten and Stoecker (1986, at least) and increasingly recently experimentalists have studied repeated supergames. Subjects are matched with new partners in each period but periods are divided into a number of "subperiods" during which subjects stay matched with the same partner.
- **Population matching:** In some experiments the entire session's worth of subjects must interact simultaneously in each period (i.e. in most market experiments, population games).
- **Silos:** Subjects within session are divided into subgroups and matched only with other members of the sub-group (to generate more fully independent data).

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Obviously no matching decisions necessary for real options example

How Many Subjects?

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Statistical Power: The probability a statistical test rejects a false null hypothesis.

- If probability of a Type I error is β then power is $1 - \beta$
- A low powered experiment is ill suited to test a hypothesis.
- With very low power, it is very likely you will fail to find effects that are meaningful and real.

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Under assumption of approximate normality, can calculate power $(1 - \beta)$ using standard methods (or using any statistical software package).

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Post hoc power analysis: figure out how confident you should be in your results

- Get Δ from means differences in data.
- Get σ from your data.
- Calculate power (and credibility of your results).

Getting It Right

Fixing $1 - \beta$, you can reduce required N with careful design:

- Use a Within-Design and study across-treatment differences ($\downarrow \sigma$)
- Block on as many factors as possible ($\downarrow \sigma$)
- Include controls in statistical tests ($\downarrow \sigma$)
- Induce values carefully – minimize confusion, use steep payoffs, eliminate sources confounding dominance ($\downarrow \sigma$)
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Failures of independence complicate all of this significantly

- Multiple observations on the same subject are **not** independent sources of data.
- Subject interactions reduce and sometimes entirely destroy independence.
- Means you may need more data.

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A really useful exercise (again) is to simulate your full dataset

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Doing this does a few things:

1. It makes your future data concrete and will surprisingly often reveal problems with your design.
2. It will reveal subtle barriers to parameter recovery in structural exercises.
3. Overall, it forces you to think about your design in terms of your future data analysis.

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Anticipate criticisms. What would a skeptical reader think of your argument? Build your design with a critical reader in mind.

- Are there objectionable elements in the way you've induced preferences?
- In the best case scenario, are there remaining ambiguities in the interpretation of data under your design?
- What follow-up treatments might you run in various contingencies to convincingly explain your data? Be as prepared as possible from the beginning as this may inform your initial design.

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