Experimentation Fundamentals 🧪

Brief introduction and intuitions

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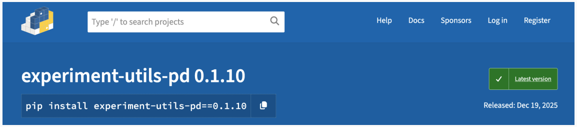
Outline 🗺️

- Introduction & Causal Inference Fundamentals
- Experimental Design & Randomization
- Power Analysis & Sample Size
- Sampling & Covariate Balance
- AB Analysis + Confounding & Non-compliance Simulations

Python Package: `experiment-utils-pd` 📦

A Python package for designing, analyzing, and validating experiments with advanced causal inference capabilities



experiment-utils-pd 0.1.10

```
pip install experiment-utils-pd==0.1.10
```

Released: Dec 19, 2025

Key components:

- `ExperimentAnalyzer` - AB test analysis, IV, IPW, regression adjustment, retrodesign
- `PowerSim` - Sample size & power calculations, retrodesign

Links: PyPI | GitHub

What problem are we trying to solve? 🤔

Goal: Measure the causal effect of a treatment/intervention

The Fundamental Problem of Causal Inference:

- For any individual, we can only observe one potential outcome
- Example: Did the medication work for patient *i*?
  - We observe: Patient took medication → recovered in 5 days
  - We cannot observe: Same patient without medication → ?
- Individual causal effects are fundamentally unobservable
- We need a counterfactual: What would have happened without treatment?

Solution: Randomized experiments (RCTs/A/B tests) create valid comparisons

Why Randomization Works 🎲

Without randomization:

- Treatment and control groups may differ in many ways
- Differences in outcomes could be due to pre-existing differences, not treatment
- Example: Sicker patients seek treatment $\rightarrow$ worse outcomes (confounding)

With randomization:

- Treatment assignment is independent of all other characteristics
- Groups are exchangeable: same distribution of characteristics
- Any difference in outcomes can be attributed to treatment

Key Assumptions for Valid Experiments 📝

1. Independence (Randomization)
   - Random treatment assignment creates exchangeable groups
2. Stable Unit Treatment Value Assumption (SUTVA)
   - No interference: Units don't affect each other
   - Consistency: Treatment uniformly defined
   - Violations: Network effects, marketplace spillovers
3. Compliance
   - Treatment received matches treatment assigned

Internal vs External Validity 🎯

Internal Validity: Can we trust the causal inference within our experiment?

- **Threats:** Selection bias, implementation errors (SRM), measurement errors, SUTVA violations

External Validity: Can we generalize beyond our experiment?

- **Threats:** Non-representative samples, novelty effects, seasonal/context factors

Experimental Designs: How to Assign Treatments? 🧪

Between-Subjects Design:

- Each unit receives only one treatment condition
- Compare different units: Group A vs Group B
- Example: 50% users see version A, 50% see version B
- **Key limitation:** More participants needed, lower power

Within-Subjects Design:

- Each unit receives all treatment conditions (at different times)
- Compare same unit against itself
- Example: All users see version A in week 1, version B in week 2
- **Key limitation:** Order/carryover effects, not irreversible treatments

Power Analysis & Sample Size

Power 💪

Statistical Power = Probability of detecting an effect when it exists

Key components:

- α (Type I error): False positive rate, typically 0.05
- β (Type II error): False negative rate, typically 0.20
- Power = $1 - \beta$, typically 0.80 (80%)
- **Effect size**: Magnitude of difference
- **Sample size**: Number of units per group

For more details look at: blog's post on power analysis

Minimum Detectable Effect (MDE) 🔎

MDE = Smallest effect size reliably detected at 80% power, α = 0.05, given sample size

How to define MDE:

1. **Business:** Minimum effect for ROI (e.g., 2% revenue lift)
2. **Resource-constrained:** Achievable with sample (e.g., 50K users $\rightarrow$ 3% MDE)
3. **Historical:** Based on past experiments (typically 1-5%)

If MDE unreasonable & limited sample:

- Avoid running under-powered experiments, risk of winner's curse (exaggerated estimates)
- Use quasi-experimental methods instead!

Small Effects + Limited Traffic: What To Do? 💡

1. **Reduce variance**: CUPED, regression adjustment, stratified randomization
2. **Use sensitive metrics**: surrogate/proxy metrics
3. **Redesign experiment**: within-subjects, responsive subpopulations, longer duration
4. **Accumulate evidence**: meta-analysis (`combine_effects`)
5. **Go quasi-experimental**: DiD, Synthetic Control, Geo-experiments, Interrupted Time Series

Multiple Comparisons Problem 🎣

Problem: Testing m metrics at $\alpha = 0.05$ expects 1 false positive per 20 tests

$$P(\text{at least 1 false positive}) = 1 - (1 - \alpha)^m$$

Solutions:

- Multiple comparison corrections
- Bonferroni, Holm-Bonferroni, Sidak, Benjamini-Hochberg (FDR)

Find Sample Size & Power 🧑‍💻

`PowerSim` uses a simulation approach, more useful for more complex designs and metrics (compliance, multiple variants, etc.)

```python
from experiment_utils.power_sim import PowerSim

p = PowerSim(metric='proportion',
            relative_effect=False,
            variants=2,
            nsim=1000,
            alpha=0.05,
            alternative='two-tailed',
            comparisons=[(1, 0), (2, 0), (2, 1)],
            correction='holm')
```

Find Sample Size & Power 🧑‍💻

```
result = p.find_sample_size(
    target_power=0.80,
    baseline=0.10,
    effect=[0.03, 0.05],
    # compliance=0.80,
    optimize_allocation=True)

Using sample size 17202 (driven by (0, 1))
Optimized sample sizes: {'control': 1455, 'variant_1': 7873, 'variant_2': 7873}
Achieved power: {'(0, 1)': 0.907, '(0, 2)': 1.0, '(1, 2)': 0.942}
```

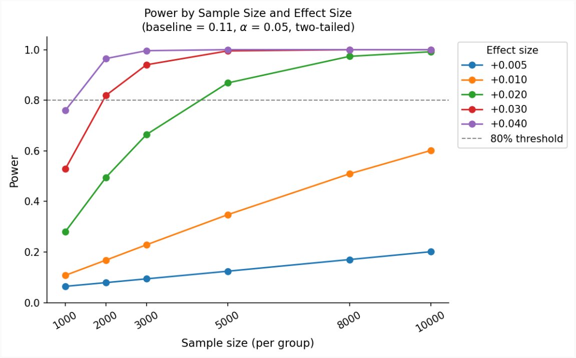
Power Analysis in Practice 🔍

1. Always explore multiple scenarios — not just one power calculation
   - Jointly optimize: 80% power + realistic MDE + acceptable duration
   - In 1–2% MDE territory: proxy metrics, CUPED
2. MDE is the most constrained parameter — rarely inflated to compensate
   - Should reflect minimum business-relevant effect, not statistical convenience
   - If MDE is unrealistic, the experiment probably shouldn't run (quasi-exp instead)
3. Heuristics as smart defaults
   - Keep power $\geq$ 80% (most respected threshold)
   - Cap MDE at 5%; two-tailed tests preferred unless strong prior

Exploring the Power Surface 📈

```
p = PowerSim(metric='proportion', relative_effect=False,
             variants=1, nsim=1000)

rr = p.grid_sim_power(
    baseline_rates=0.25,
    effects=[0.005, 0.01, 0.02, 0.03, 0.04],
    sample_sizes=[1000, 2000, 3000, 5000, 8000, 10000],
    plot=True)
```



Sampling Methods for Experiments ✂️

Why sampling matters:

- Limited resources (budget, time, traffic)
- Reduces variance - Better precision in estimates
- Increases credibility - Shows randomization worked properly

Two main approaches:

- Random Sampling - Simple random selection
  - Easy to implement, may result in imbalanced small segments
- Stratified Sampling - Sample within dimensions
  - Ensures representation across strata, better for heterogeneous populations
  - Can use proportional or equal allocation

Blocking / Stratified Sampling 🧑‍💻

```
from experiment_utils.utils import balanced_random_assignment

# simulated data with covariates (3000 rows):
age  previous_purchases  days_since_signup  treatment  conversion
0  29.143694                   5         464.228095  treatment           0
1  49.973454                   2         577.496020    control           0
2  42.829785                   6         806.725758  treatment           1
3  24.937053                   6        1324.618460    control           0
4  34.213997                   5         415.036894  treatment           0

# random allocation
treatment = balanced_random_assignment(
    df,
    variants=['control', 'treatment'],
    allocation_ratio=0.5,
    check_balance_covariates=['age', 'previous_purchases', 'days_since_signup'],
    # balance_covariates=['age', 'previous_purchases', 'days_since_signup']
)
----------------------------------------------------------------------
         covariate  mean_treatment  mean_control       smd balanced
               age       40.046056     40.415916 -0.036537        y
previous_purchases        3.088000      3.004000  0.048583        y
 days_since_signup      365.495181    358.738388  0.018646        y

Summary: 3/3 covariates balanced (|SMD| < 0.1)
Mean |SMD|: 0.0346
Max |SMD|: 0.0486
======================================================================
```

Blocking / Stratified Sampling 🧑‍💻

```
# stratified allocation
treatment = balanced_random_assignment(
    df,
    variants=['control', 'treatment'],
    allocation_ratio=0.5,
    check_balance_covariates=['age', 'previous_purchases', 'days_since_signup'],
    balance_covariates=['age', 'previous_purchases', 'days_since_signup']
)
```

```
----------------------------------------------------------------------
         covariate  mean_control  mean_treatment       smd balanced
               age     40.167258       40.297051 -0.012818        y
previous_purchases      3.062868        3.028513  0.019869        y
 days_since_signup    362.615133      361.600167  0.002801        y

Summary: 3/3 covariates balanced (|SMD| < 0.1)
Mean |SMD|: 0.0118
Max |SMD|: 0.0199
======================================================================
```

Regression with Pre-Treatment Covariates 🤖

Even with perfect randomization, regression adjustment helps:

- Reduced variance $\rightarrow$ narrower confidence intervals, higher precision
- Increased power to detect effects (especially with correlated covariates)
- Corrects for chance imbalances in small samples

When to use:

- Pre-treatment covariates strongly correlated with outcome (most of the time)
- Sample size < 1000 per group (chance imbalances more likely)

Regression Adjustment 🤖

```
from experiment_utils.experiment_analyzer import ExperimentAnalyzer

# no adjustment
analyzer_simple = ExperimentAnalyzer(
    df,
    treatment_col='treatment',
    outcomes='conversion'
)
analyzer_simple.get_effects()
analyzer_simple.results['standard_error']
0.0173

# covariate adjustment
analyzer_adjusted = ExperimentAnalyzer(
    df,
    treatment_col='treatment',
    outcomes='conversion',
    covariates=['age', 'previous_purchases', 'days_since_signup'],
    regression_covariates=['age', 'previous_purchases', 'days_since_signup']
)

analyzer_adjusted.get_effects()
analyzer_adjusted.results['standard_error']
0.0122
```

Winner's Curse 💀

Problem: Significant results often overestimate true effect

Why?

- Selection bias: Only "winners" reported
- Small samples + low power $\rightarrow$ worse exaggeration (2-3x for power < 0.50)

$$\text{Exaggeration (Type M)} = \left|\frac{\hat{\tau}}{\tau}\right|$$

$$\text{Relative Bias} = \frac{\hat{\tau}}{\tau}$$

When an underpowered experiment reports a significant effect, divide the estimate by the exaggeration ratio to get a more realistic sense of the true effect size.

Winner's Curse 💀

```
# reduce the sample size (~500 per group)
sdf = df.sample(1000)
analyzer_simple = ExperimentAnalyzer(
    sdf,
    treatment_col='treatment',
    outcomes='conversion'
)
analyzer_simple.get_effects()
analyzer_simple.results[['absolute_effect', 'pvalue']]
   absolute_effect    pvalue
0         0.076136  0.011805

# let's assess the potential bias (we know the MDE is 0.045)
analyzer_simple.calculate_retrodesign(true_effect=0.045)
cols = ['power', 'type_s_error', 'type_m_error', 'relative_bias', 'trimmed_abs_effect']
print(analyzer_simple.calculate_retrodesign(true_effect=true_effect)[cols])

   power  type_s_error  type_m_error  relative_bias  trimmed_abs_effect
0  0.336        0.0006        1.7265         1.7248             0.04414
```

More on analysis

Components 📏

Key components:

- Primary metrics
    - Pre-specified metrics (e.g., revenue, conversion)
    - Multiple tests?
- Guardrail metrics
    - Safety checks (e.g., load time, error rates)
    - Implementation metrics
- Sample Ratio Mismatch (SRM)
    - Does split match expectation?
    - SRM signals implementation issues or bias

Simple analysis 🧑‍💻

```python
from experiment_utils import ExperimentAnalyzer

analyzer = ExperimentAnalyzer(
    df,
    treatment_col='treatment',
    outcomes='conversion',
    bootstrap=True,
    exp_sample_ratio_col='expected_sample_ratio',
    outcome_models={'conversion':['ols', 'logistic']},
    # pvalue_adjustment='sidak',
)
analyzer.get_effects()
print(analyzer.results.round(3)[['model_type', 'absolute_effect', 'relative_effect',
    'rel_effect_lower', 'rel_effect_upper', 'srm_pvalue']])
```

```
  model_type  absolute_effect  relative_effect  rel_effect_lower  rel_effect_upper  srm_pvalu
0        ols            0.056            0.179             0.075             0.309       0.32
1   logistic            0.056            0.179             0.074             0.307       0.32
```

Distribution Assumptions: Don't Overthink It 🧠

Why not useful:

- CLT: Sample means $\approx$ normal for large n (>30-50), regardless of distribution
- Normality tests too sensitive; sampling distribution matters, not population

What to do:

- **Large samples:** Use standard t-tests / z-tests (rely on CLT)
- **Small samples/complex metrics:** Use bootstrapping
- **Extreme outliers:** Robust statistics (better models!)

What If the Experiment Is Broken? 😱

Common problems:

- **Implementation issues** - Bugs in assignment or logging
- **SRM** - Observed split differs from expected
- **Non-compliance** - Users don't receive assigned treatment

What can we do?

- **ITT** - Preserves randomization, underestimates effect
- **Regression adjustment / IPW** - Correct for imbalances
- **IV** - Assignment as instrument (ITT $\rightarrow$ LATE)

Simulation: Non-Compliance + Confounding 🧑‍💻

CS farming meeting scenario:

- Only a % of treated users attend (one-sided non-compliance)
- Attenders are more engaged, higher baseline revenue (confounding)

How to recover the true effect on bookings?

ITT	As assigned $\rightarrow$ underestimates effect
Regression	Covariate adjustment $\rightarrow$ reduces bias
IPW	Inverse probability weighting $\rightarrow$ corrects imbalance
IV	Assignment as instrument $\rightarrow$ LATE $\equiv$ ATT (one-sided non-compliance)

Naive Approach 🧸

Naive analysis: Compare attenders vs non-attenders

True effect = 5 bookings

```
naive = ExperimentAnalyzer(
    cdf,
    treatment_col='attended',
    outcomes='bookings',
)

naive.get_effects()

naive.results[['absolute_effect', 'abs_effect_lower', 'abs_effect_upper', 'pvalue']]

absolute_effect  abs_effect_lower  abs_effect_upper         pvalue
0         6.316905          5.921429          6.712381  3.823925e-215
```

Intend-to-Treat (ITT) ⚖️

ITT: Compare all treated vs control, regardless of attendance

True effect = 5 bookings

```
itt = ExperimentAnalyzer(
    cdf,
    treatment_col='assigned',
    outcomes='bookings',
)
itt.get_effects()

itt.results[['absolute_effect', 'abs_effect_lower', 'abs_effect_upper', 'pvalue']]
```

```
absolute_effect  abs_effect_lower  abs_effect_upper        pvalue
0         2.743512          2.359612          3.127413  1.418363e-44
```

Inverse Probability Weighting (IPW) 🥷

IPW: Adjust for compliance selection using covariates

True effect = 5 bookings

```
ipw = ExperimentAnalyzer(
    cdf,
    treatment_col='attended', outcomes='bookings',
    covariates=['engagement', 'account_age_months', 'monthly_usage'],
    adjustment='balance',  balance_method='ps-logistic', target_effect='ATT', overlap_plot=Tr

ipw.get_effects()
ipw.results[['absolute_effect', 'abs_effect_lower', 'abs_effect_upper', 'pvalue']]
   absolute_effect  abs_effect_lower  abs_effect_upper         pvalue
0         5.056775          4.652374          5.461177  1.212354e-132

# what about just regression adjustment?
reg_adj = ExperimentAnalyzer(
    cdf,
    treatment_col='attended', outcomes='bookings',
    covariates=['engagement', 'account_age_months', 'monthly_usage'],
    regression_covariates=['engagement', 'account_age_months', 'monthly_usage'])

reg_adj.get_effects()
reg_adj.results[['absolute_effect', 'abs_effect_lower', 'abs_effect_upper']]
   absolute_effect  abs_effect_lower  abs_effect_upper         pvalue
0         5.086338          4.766525          5.406151  2.597419e-213
```

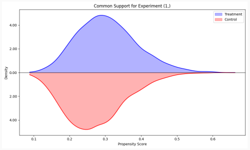
Overlap Assumption 🔍

Why it matters:

- IPW requires common support: every unit needs a non-zero probability of either treatment
- Without it, weights become extreme $\rightarrow$ high variance and bias

What to do:

- **Diagnose:** Inspect propensity score distributions by group
- **Trim:** Drop or down-weight units outside common support
- **Target:** Shift estimand from ATE $\rightarrow$ ATT



Instrumental Variables (IV) 🔑

IV: Assignment as instrument $\rightarrow$ One-sided non-compliance $\rightarrow$ LATE $\equiv$ ATT (compliers = attenders)

Since always-takers don't exist, every attender is a complier.

True effect = 5 bookings

```
iv = ExperimentAnalyzer(
    cdf,
    treatment_col='attended',
    outcomes='bookings',
    covariates=['engagement', 'account_age_months', 'monthly_usage'],
    instrument_col='assigned',
    adjustment='IV'
)
iv.get_effects()
print(iv.results[['absolute_effect', 'abs_effect_lower', 'abs_effect_upper', 'pvalue']])
   absolute_effect  abs_effect_lower  abs_effect_upper  pvalue
0         4.870595          4.381111           5.36008     0.0
```

We only want to use the part of attendance variation that came from the random assignment. I'm throwing away self-selection.

Fixed-Effects Meta-Analysis 📊

Goal: Pool effect estimates across multiple experiments into a single estimate

Inverse-variance weighting:

- Each experiment's estimate is weighted by $w_i = 1/\sigma_i^2$
- More precise experiments (smaller SE) get more weight

When to use it:

- Multiple experiments testing the same intervention
- Any single experiment underpowered on its own
- Experiments run across different regions / segments

Key assumption: All experiments share the same true effect (homogeneity)

Fixed-Effects Meta-Analysis 🧑‍💻

Setup: 5 experiments, baselines correlated with allocation → naive pooling is biased

```
# high-alloc -> low baseline; low-alloc -> high baseline
experiments = [
    {"name": "exp_1", "n": 3000, "alloc": 0.20, "baseline": 0.20},
    {"name": "exp_2", "n": 2000, "alloc": 0.30, "baseline": 0.25},
    {"name": "exp_3", "n": 1500, "alloc": 0.50, "baseline": 0.30},
    {"name": "exp_4", "n": 1000, "alloc": 0.70, "baseline": 0.40},
    {"name": "exp_5", "n": 800,  "alloc": 0.80, "baseline": 0.50},
]
# true_effect = 0.05 in all experiments

# naive pooled analysis: ignores experiment structure
naive = ExperimentAnalyzer(data=meta_df, treatment_col="treatment", outcomes=["conversion"])
naive.get_effects()
# absolute_effect = 0.1397   <-- 2.8x the true effect!

# correct: fixed-effects meta-analysis
analyzer = ExperimentAnalyzer(
    data=meta_df, treatment_col="treatment",
    outcomes=["conversion"], experiment_identifier="experiment",
)
analyzer.get_effects()
pooled = analyzer.combine_effects(grouping_cols=["outcome"])
```

Fixed-Effects Meta-Analysis 🧑‍💻

```
# per-experiment results
print(analyzer.results[["experiment", "absolute_effect", "standard_error", "pvalue"]].round(4
  experiment  absolute_effect  standard_error  pvalue
      exp_1           0.0758          0.0200  0.0001
      exp_2           0.0319          0.0221  0.1494
      exp_3           0.0333          0.0247  0.1774
      exp_4           0.0800          0.0342  0.0193
      exp_5           0.0203          0.0443  0.6464

print(pooled[["outcome", "experiments", "absolute_effect", "standard_error", "pvalue"]])
      outcome  experiments  absolute_effect  standard_error    pvalue
   conversion          5.0           0.0514          0.0115  0.000008

# True effect = 0.05
# Naive pooled  = 0.14  <-- 2.8x overestimate
# Meta-analysis = 0.05  <-- recovers true effect
```

Key takeaways ✨

- Power analysis is crucial for experiment design; MDE should reflect business relevance, not statistical convenience
- Sampling methods (random vs stratified) can improve balance and precision
- Regression adjustment reduces variance and corrects for imbalances, even with perfect randomization
- Non-compliance and confounding can be addressed with ITT, IPW, and IV methods
- Meta-analysis allows pooling across multiple experiments

Links

- https://github.com/sdaza/slides/blob/main/presenta-tions/experimentation/code/simulations.py
- https://github.com/sdaza/experiment-utils-pd