Sign Congruence, External Validity, and Replication

Tara Slough—NYU Scott A. Tyson—Emory

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The external validity problem

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A central problem for:

- Policymakers who want to use evidence
- Social scientists who want to understand general phenomena

The conventional wisdom: replicate it!

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- Without attention to design, replications can mislead.
- We provide formal definitions of external validity...
- ... and provide guidance on how to evaluate it.

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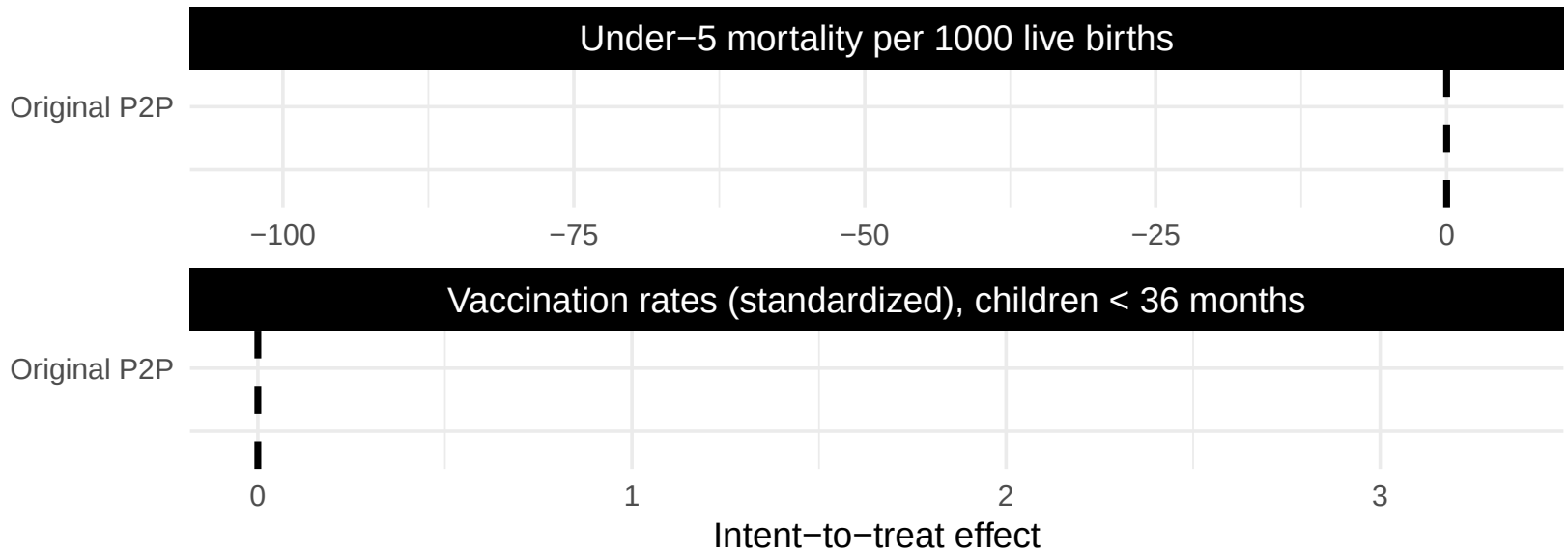

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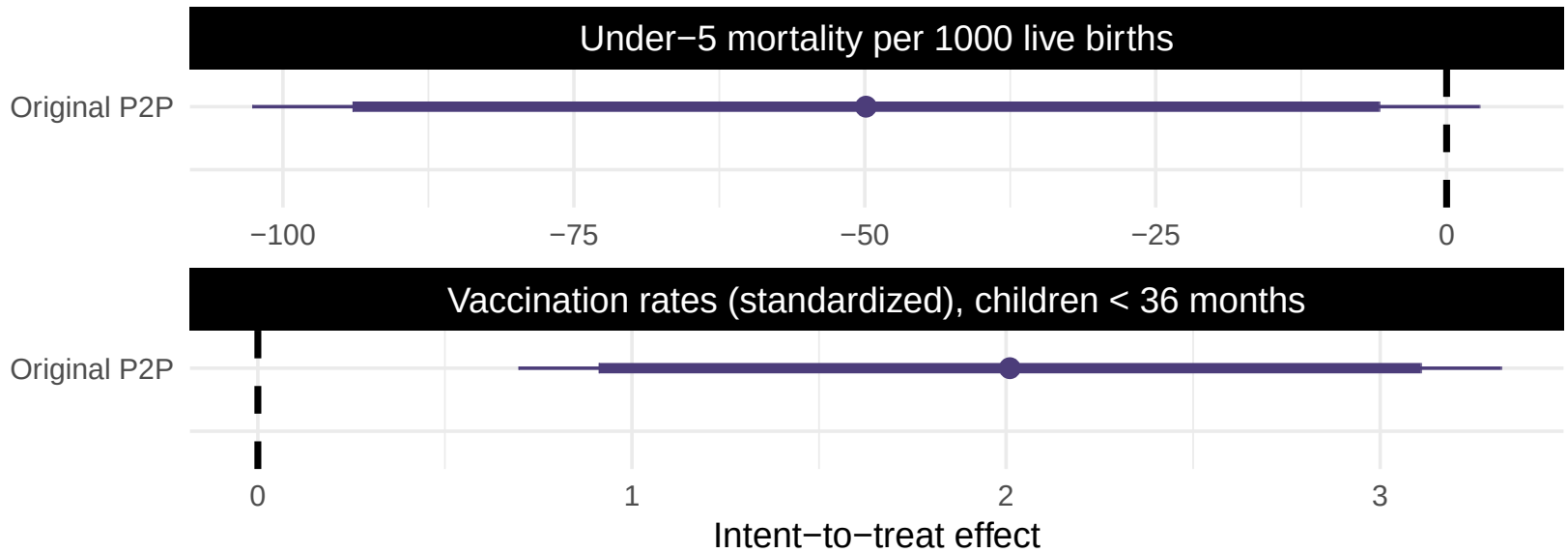

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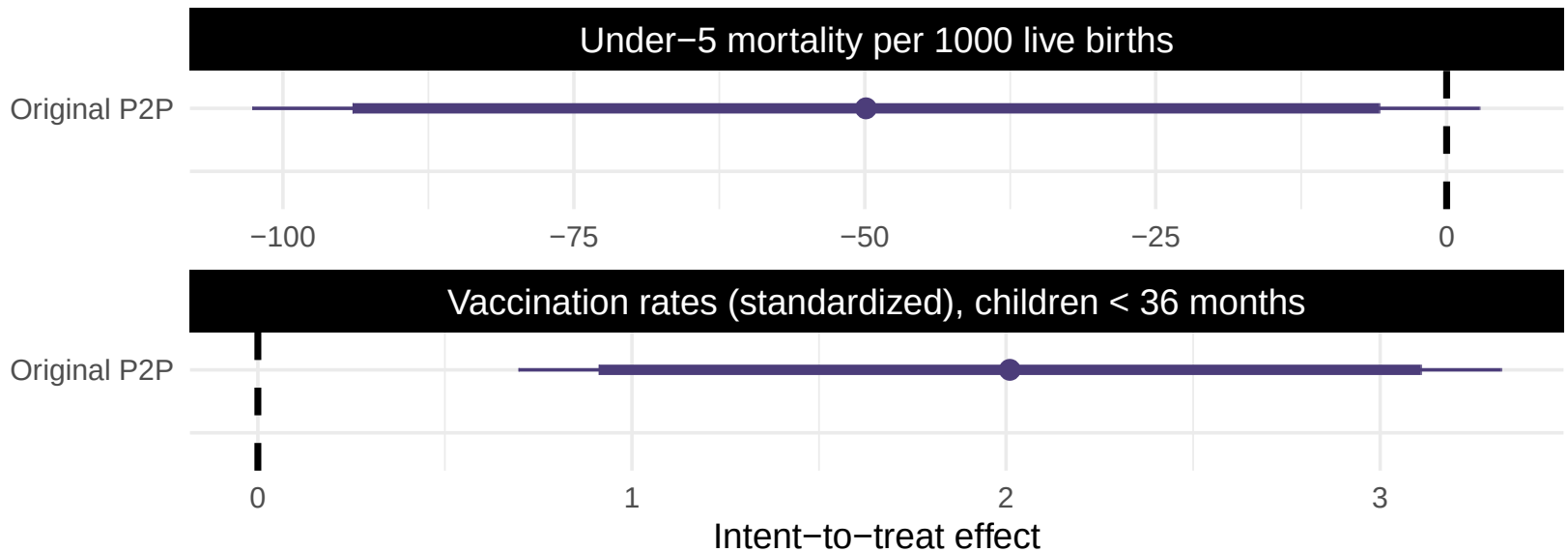

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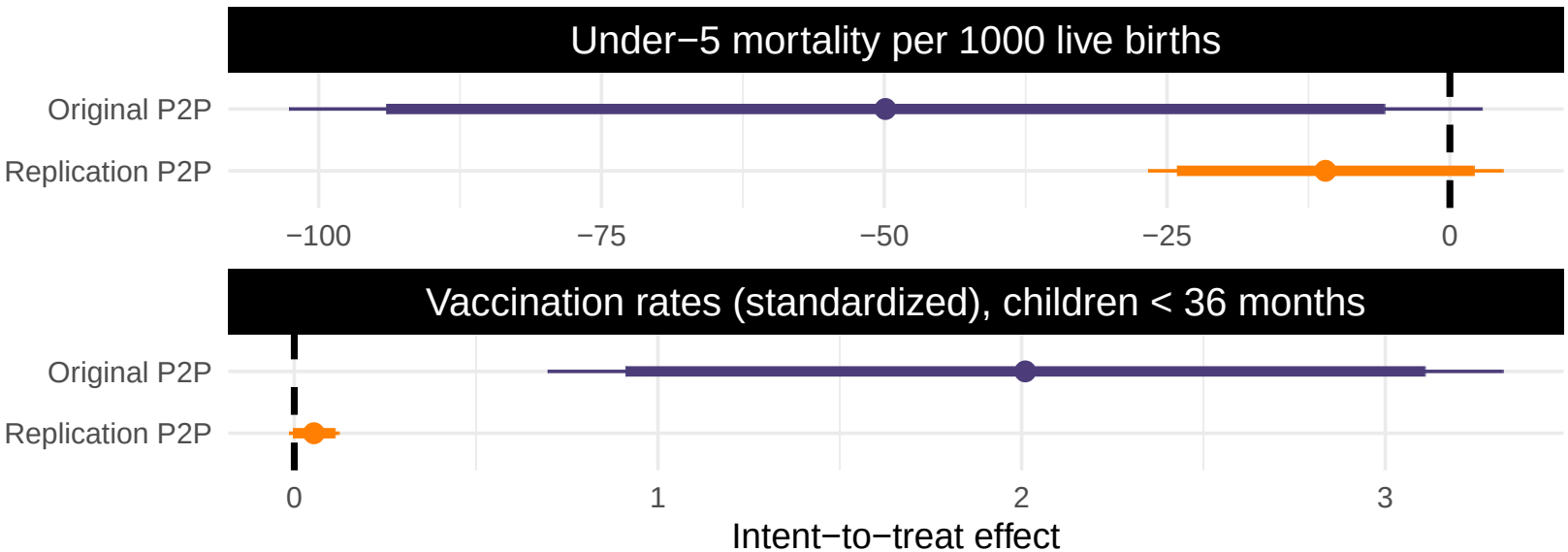

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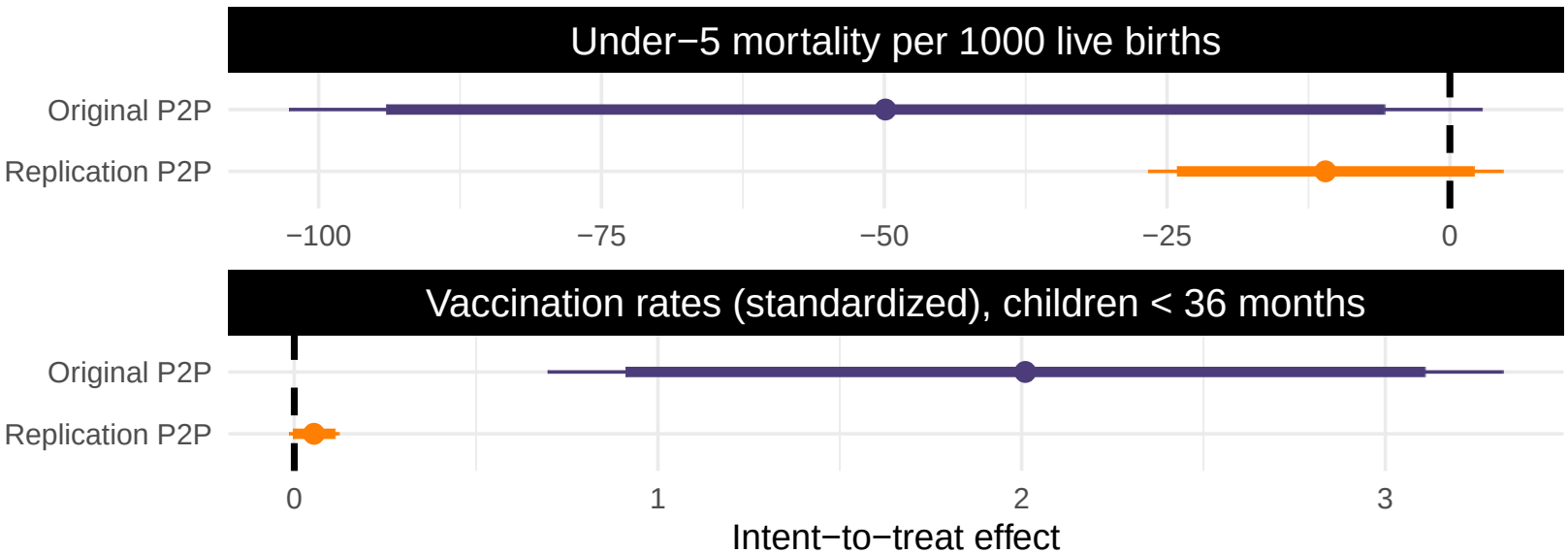


Their interpretation: effect of P2P lacks **external validity**.

Overview of talk

When does the comparison of results from replication studies provide information about **external validity**?

External validity

Statistical tests

Overview of talk

Missing ingredient: **empirical targets** depend on choices of treatment(s) and outcome(s) in each study.

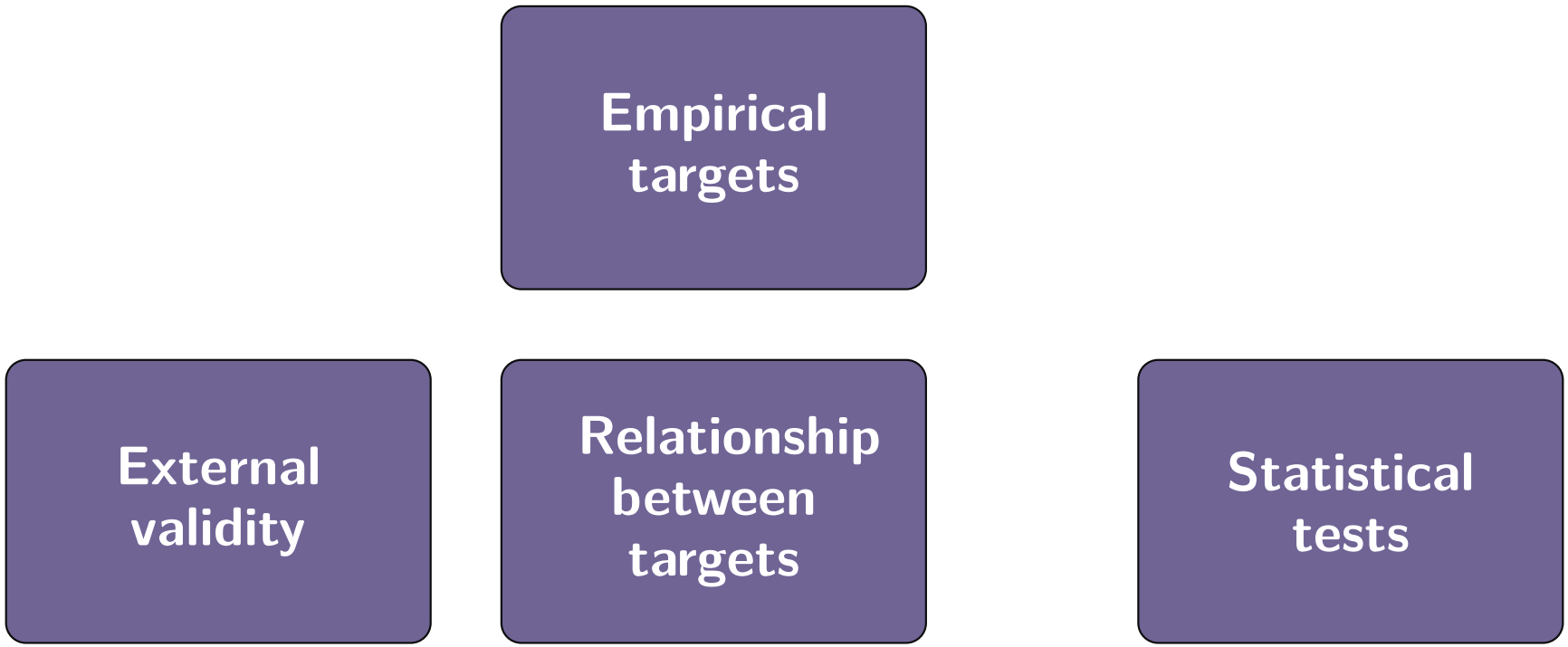
Empirical targets

External validity

Statistical tests

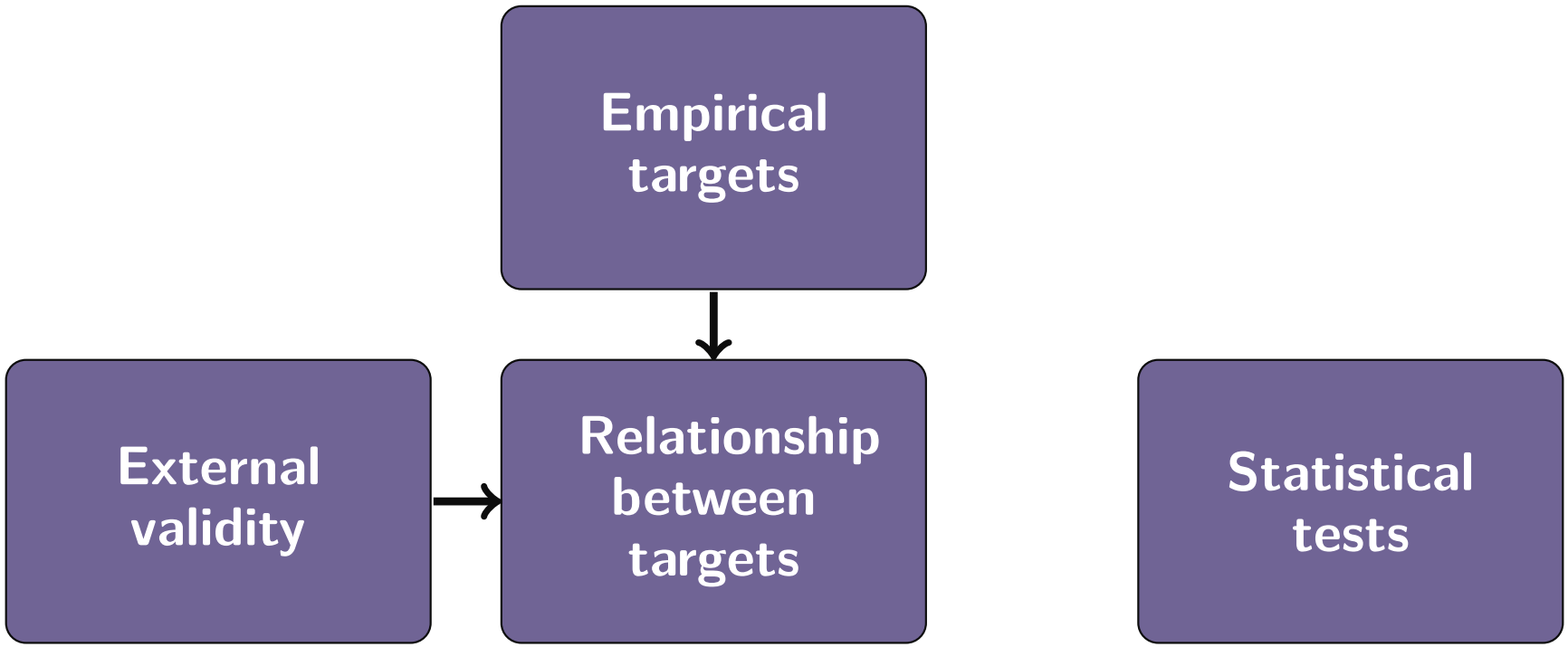
Overview of talk

To compare, we need to know how targets relate to each other.



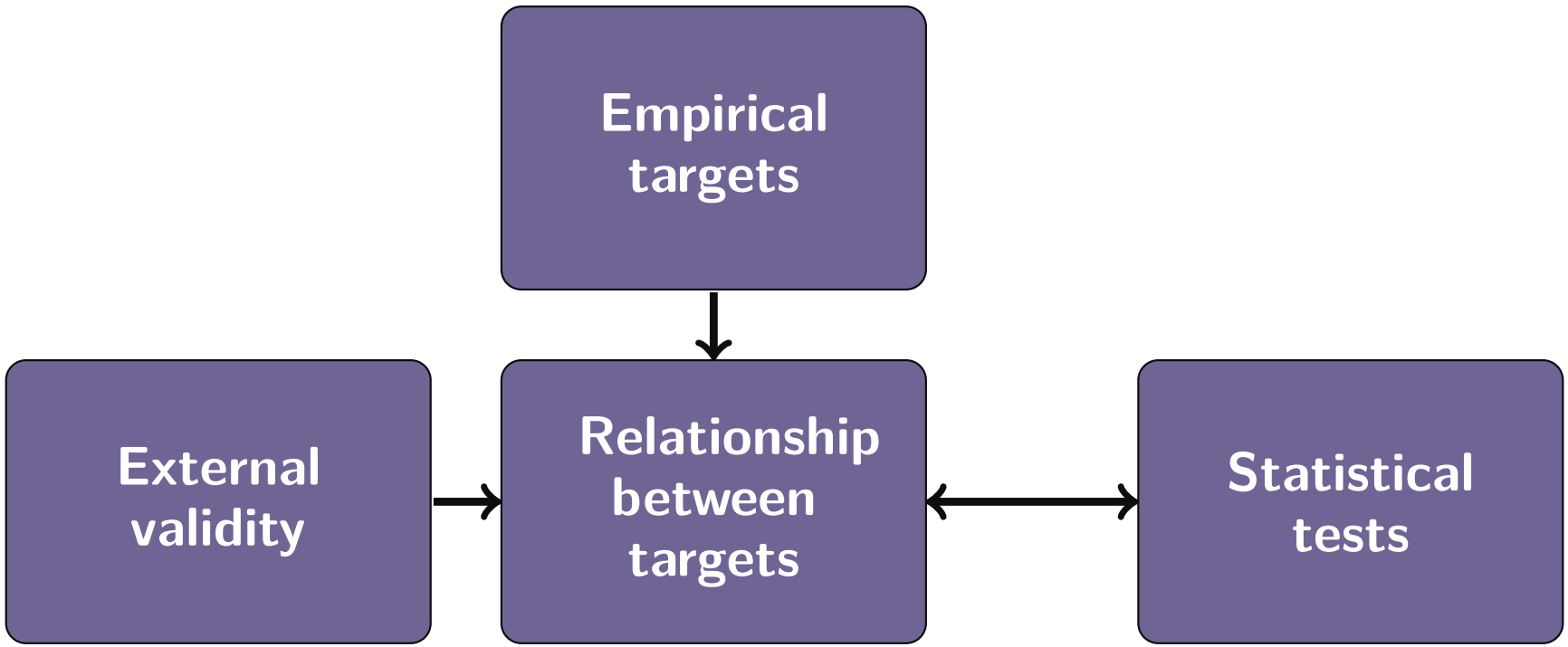
Overview of talk

Relationship between targets depends on both **external validity** of the mechanism and **empirical targets** (research design).



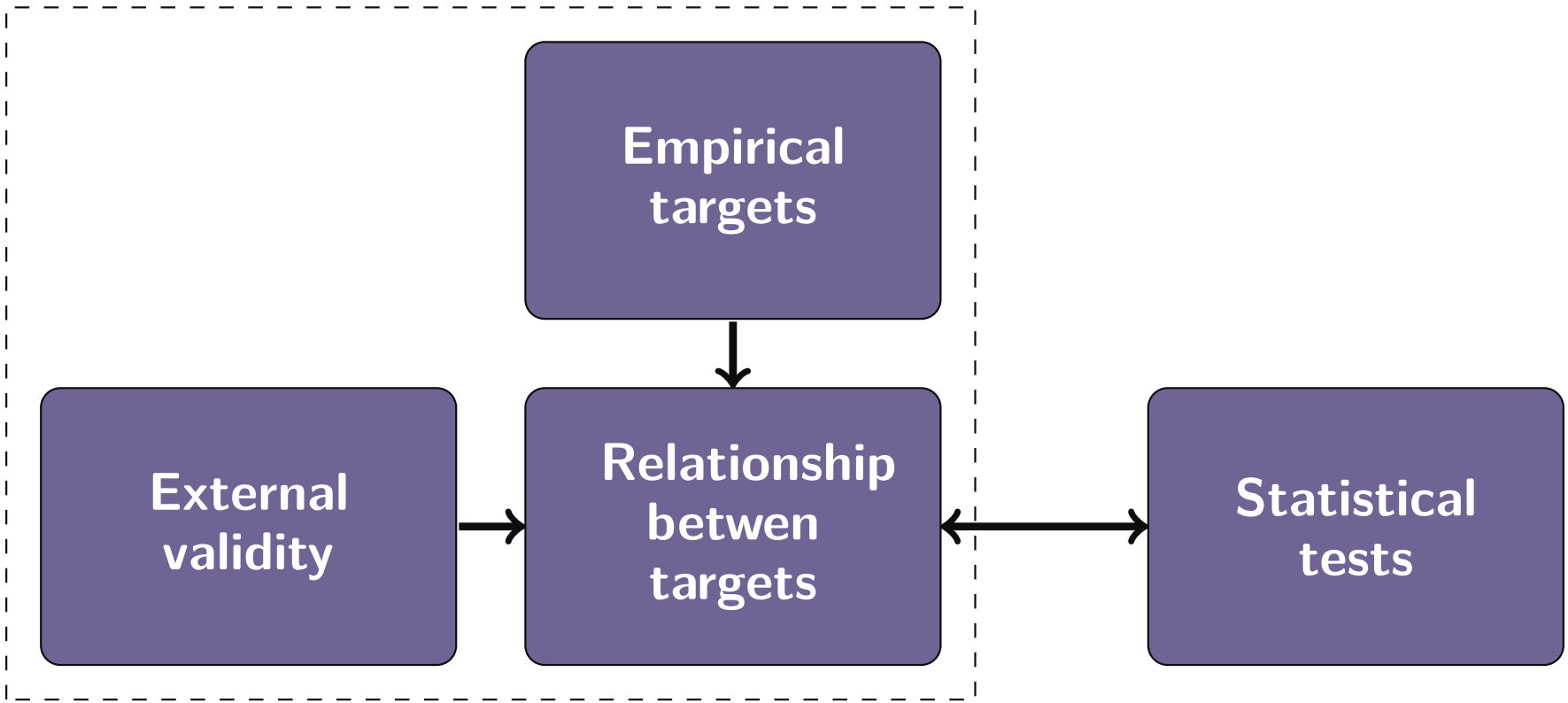
Overview of talk

We provide conditions under which statistical tests used in replications provide information about **external validity**.



Overview of talk

Framework suggests two approaches for the design of replications.



Our contribution to the literature

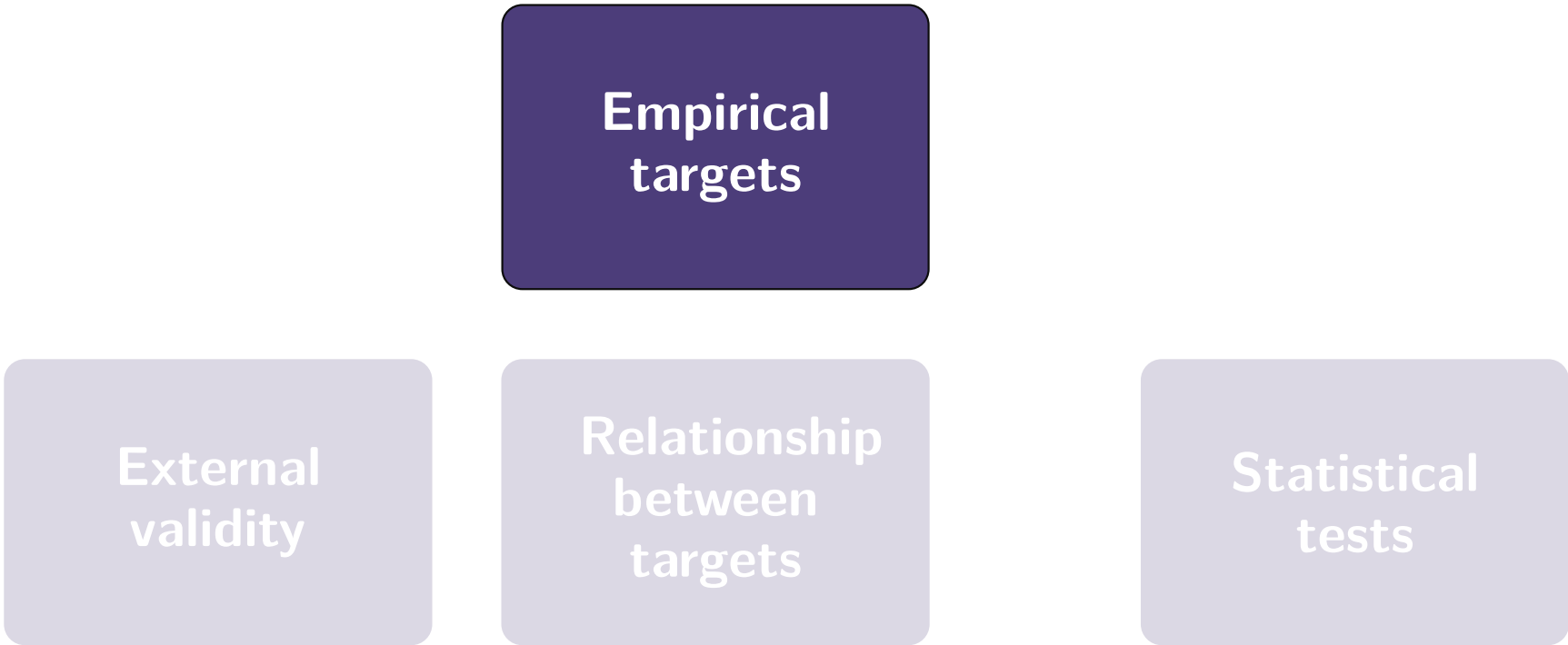
- Concepts of **external validity** (Smith, 1982; Shadish, Cook and Campbell, 2002; Guala 2005; Deaton 2010; Pearl and Bareinboim 2011, 2014; Bisbee et al., 2017; Findley, Kikuta, and Denley 2021; Dehejia, Pop-Eleches, and Samii, 2021; Egami and Hartman, 2022)
- Theoretical framework for **multi-study** research design (Meager 2019; Gechter and Meager 2021; Slough and Tyson, 2022; Wilke and Samii, 2022)
- Practical guidance for **replication** (Collins, 1992; Guala, 2005; Schmidt, 2009; Nosek and Errington, 2017)

Framework for Research Design

A Conceptual Framework for Research Design

Requires a framework that incorporates study-level and cross-study design features.

- Builds upon Slough and Tyson (2022).



A study

A study is a triple:

1. A **setting**, θ
   $\rightarrow$ Contextual features, population, time, etc.
2. A **measurement strategy**, m
   $\rightarrow$ Outcome choice and measurement components
3. A **contrast**, (ω', ω'')
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Two studies are **harmonized** if the measurement strategy and contrast are the same.

Empirical target

Treatment effects measure the influence of a mechanism.

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3. Its derivative has **full rank** in measurement strategies and contrasts.
   $\rightarrow$ Measured effects depend on research design.

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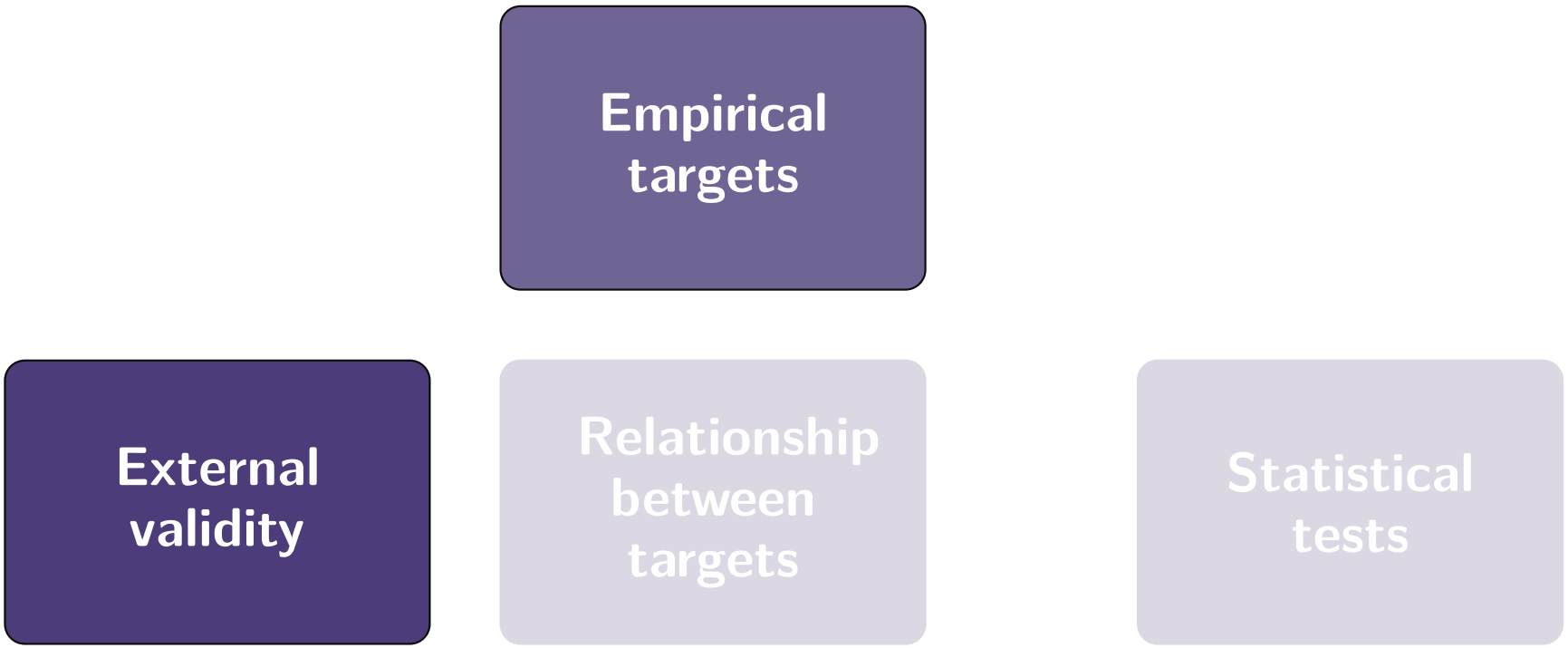
- $\varepsilon_j^{n_j}$ is observation error
- **Unbiased** when $\mathbb{E}[\varepsilon_j^{n_j}] = 0$
- **Consistent** when $\mathbb{E}(\varepsilon_i^{n_i} - \mathbb{E}[\varepsilon_j^{n_i}])^2 \to 0$ (in probability) as $n_i \to \infty$.

Concepts

Concepts of External Validity

External validity is a cluster of concepts.

- Cross-sectional concepts: external validity, sign-congruent external validity.



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Definition (Slough and Tyson, 2022)

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- (Evaluated by comparison that we make.)

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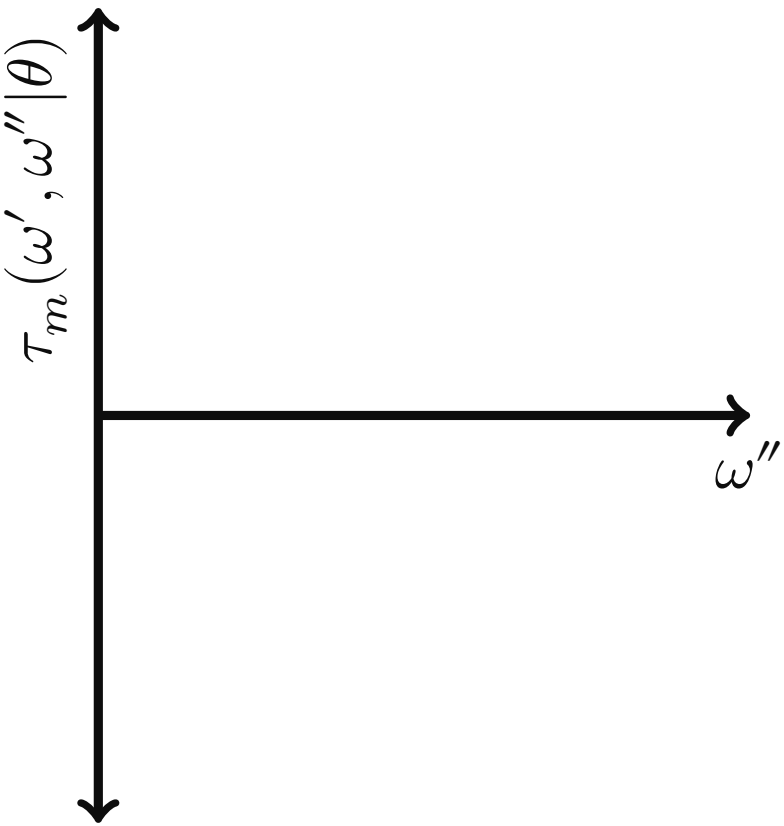

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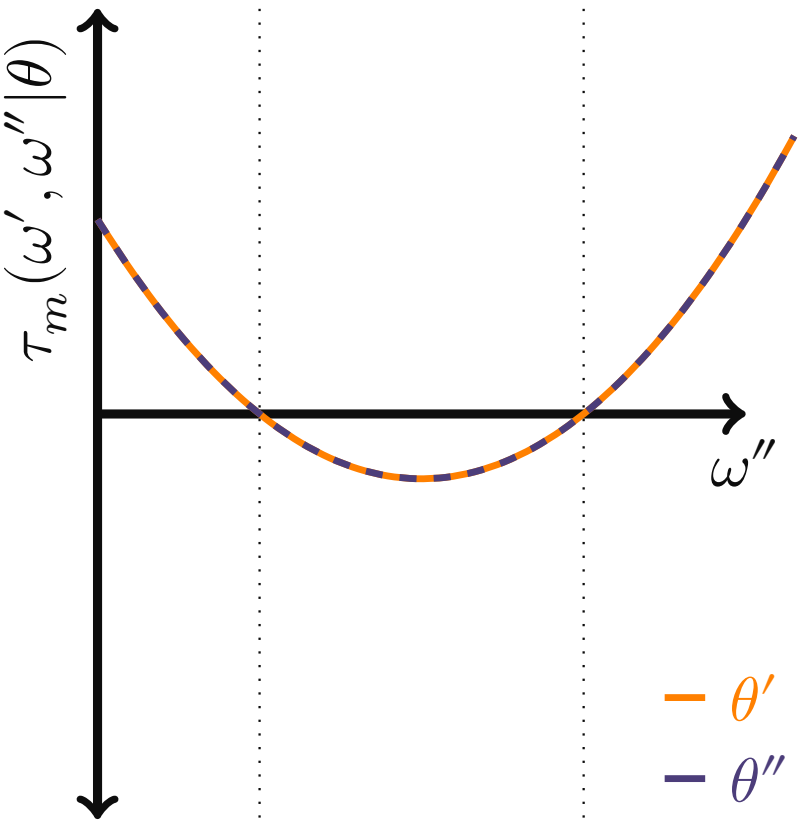

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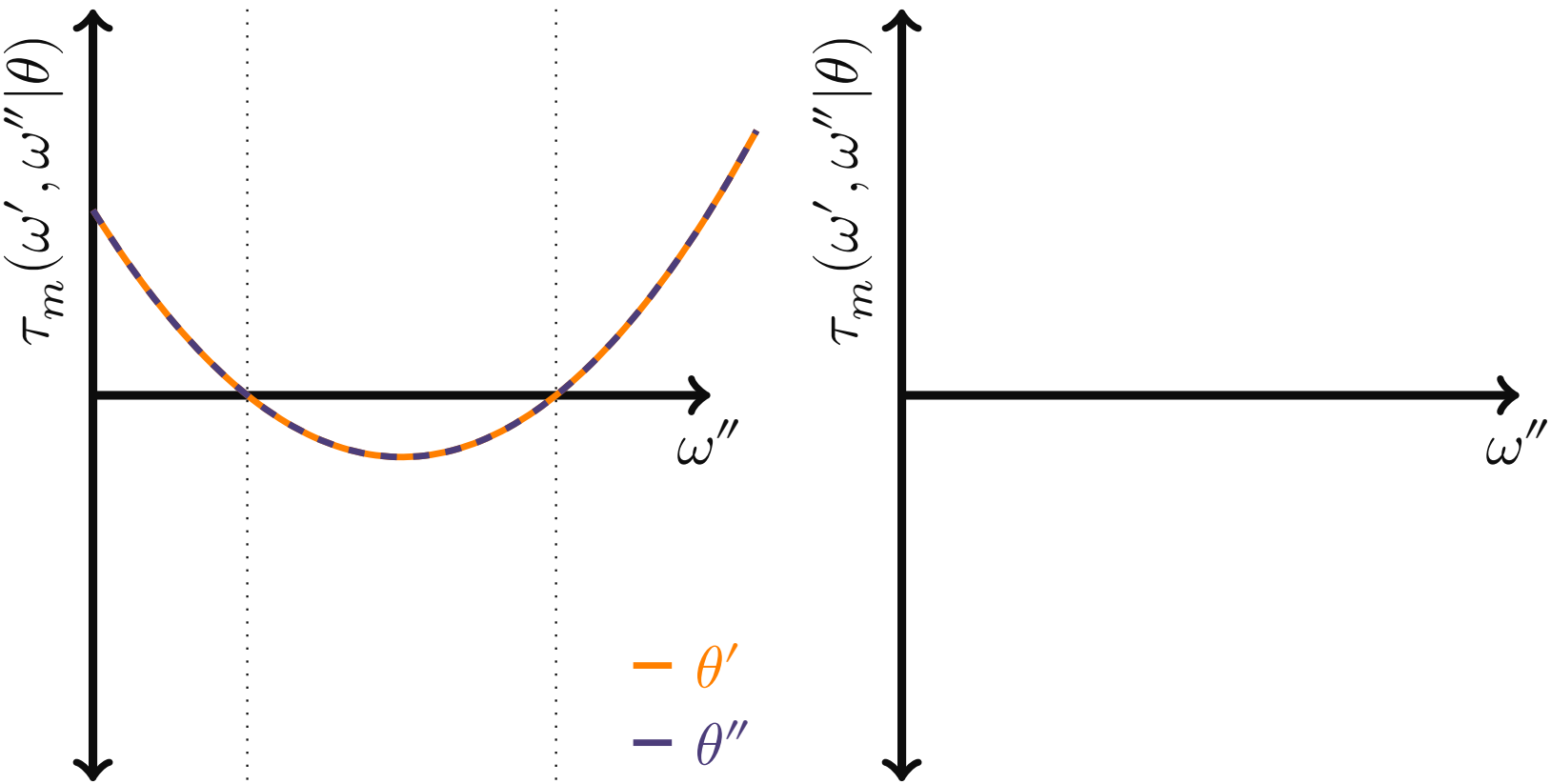

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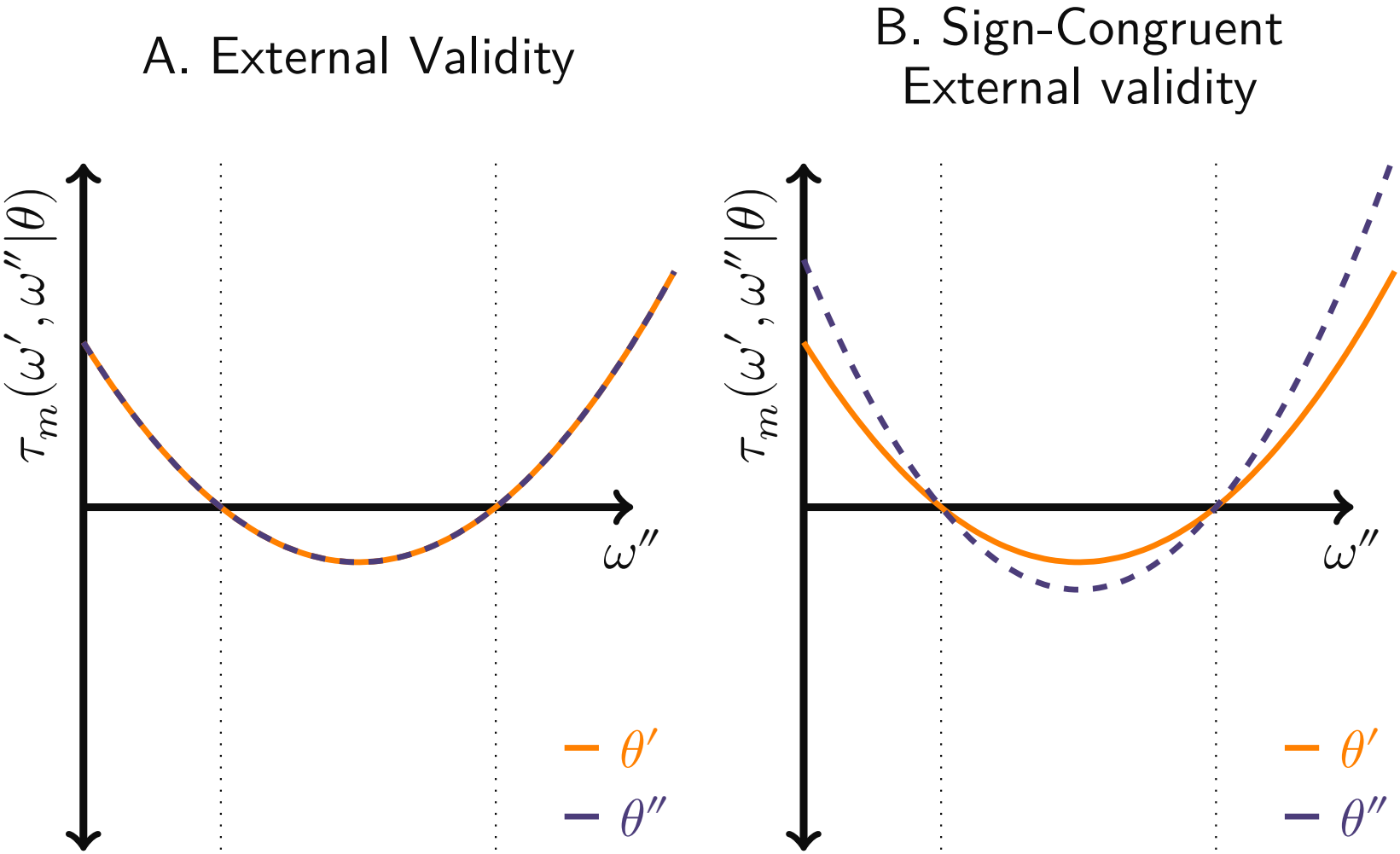
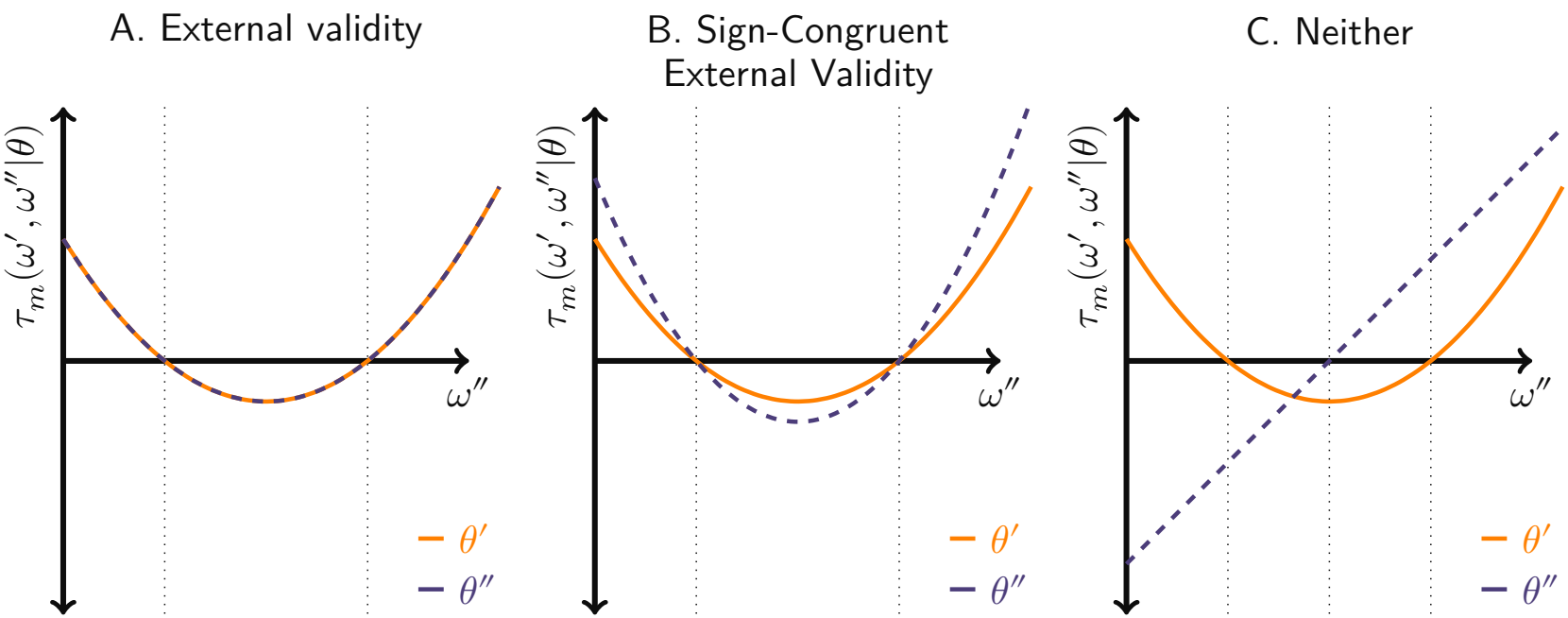


Comparing notions of external validity



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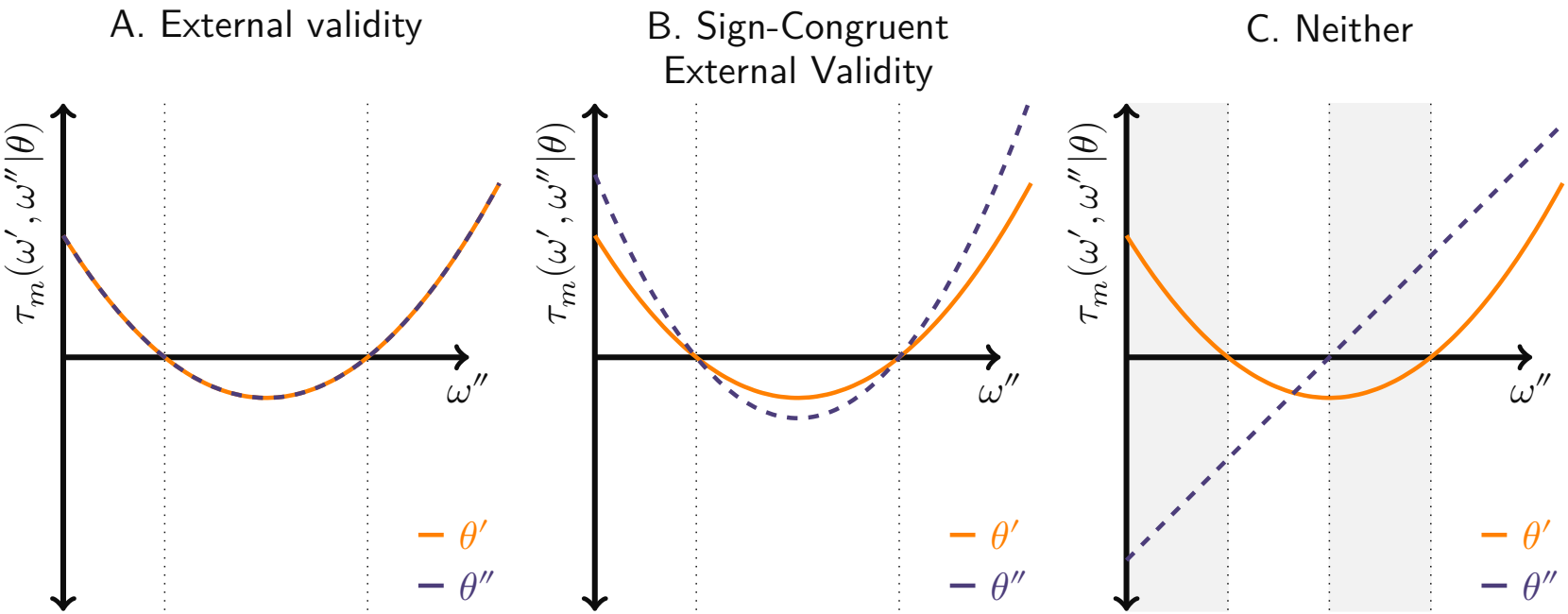
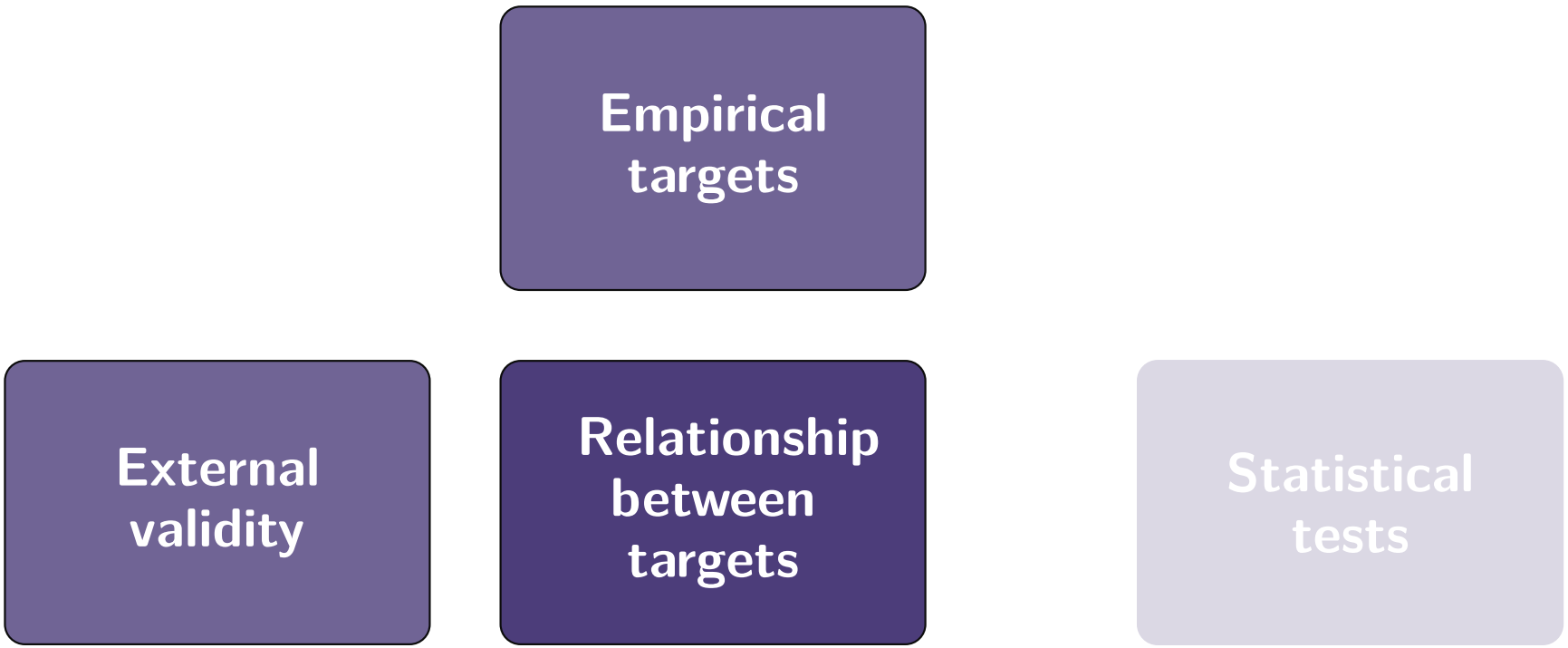


When **sign-congruent external validity** does not hold, the set of research designs where a harmonized design will produce effects with different signs in different settings has positive measure.

Relationship between targets

How do the empirical targets across studies relate to each other?

- Concepts of **target equivalence** and **target congruence**
- **Discrepancies** between targets



Target equivalence and congruence

Consider two studies: $\mathcal{E}_1 = \{m_1, (\omega_1{}', \omega_1{}''), \theta_1\}$ and $\mathcal{E}_2 = \{m_2, (\omega_2{}', \omega_2{}''), \theta_2\}$:

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*$\mathcal{E}_1$ and $\mathcal{E}_2$ are **target-equivalent** if*

$$\tau_{m_1}(\omega_1{}', \omega_1{}'' \mid \theta_1) = \tau_{m_2}(\omega_2{}', \omega_2{}'' \mid \theta_2).$$

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Target discrepancies

The **target discrepancy** from setting θ to θ' is

$$\Delta_{m,(\omega',\omega'')}(\theta,\theta') = \tau_m(\omega',\omega'' \mid \theta) - \tau_m(\omega',\omega'' \mid \theta').$$

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Measure of departures from **external validity**.

- Target-equivalence implies **zero** target discrepancies.
- Target-congruence is when they take a **particular form**.

Artifactual discrepancies

For a fixed setting θ, the **artifactual discrepancy** is

$$\mathcal{A}_{ij}(\theta) = \tau_{m_i}(\omega_i{}', \omega_i{}'' \mid \theta) - \tau_{m_j}(\omega_j{}', \omega_j{}'' \mid \theta).$$

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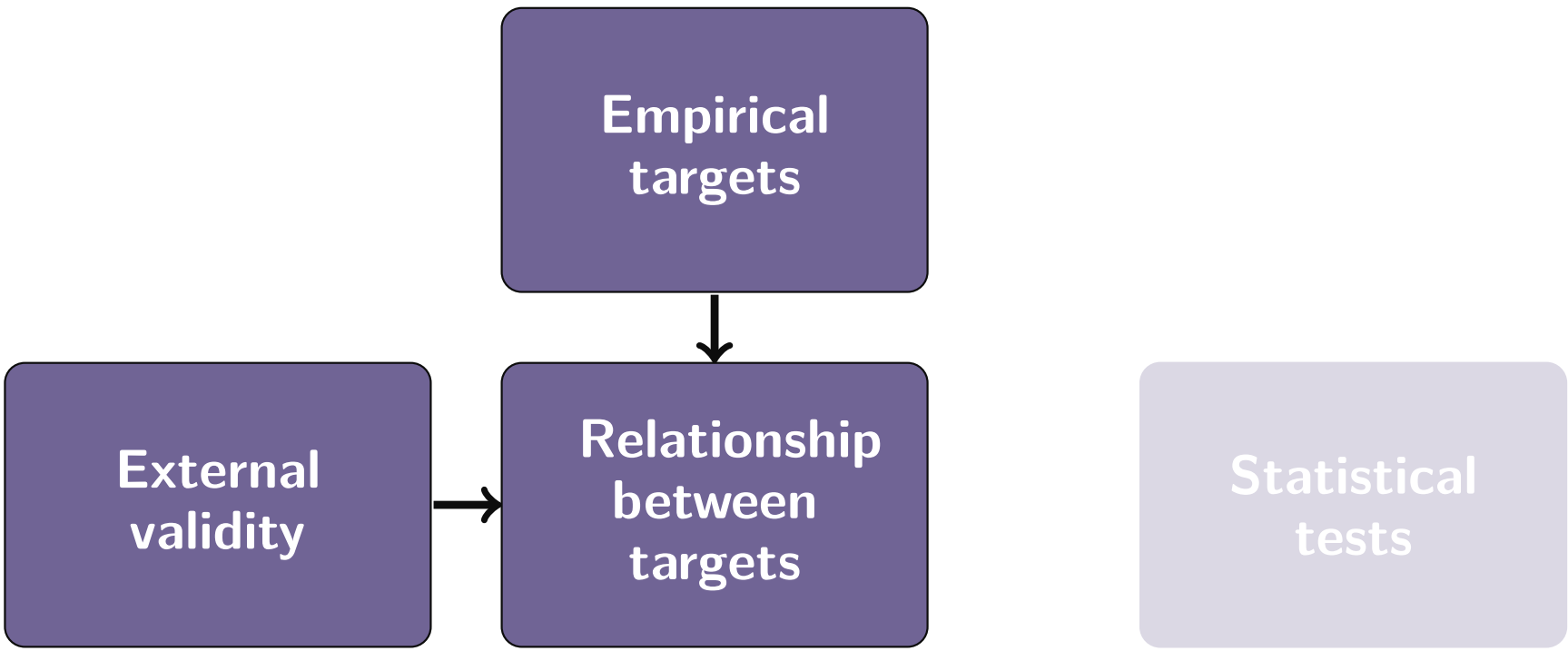
Remark: $\mathcal{A}_{ij}(\theta) = 0$ for almost every θ if and only if i and j are **harmonized**.

Results

Our goal

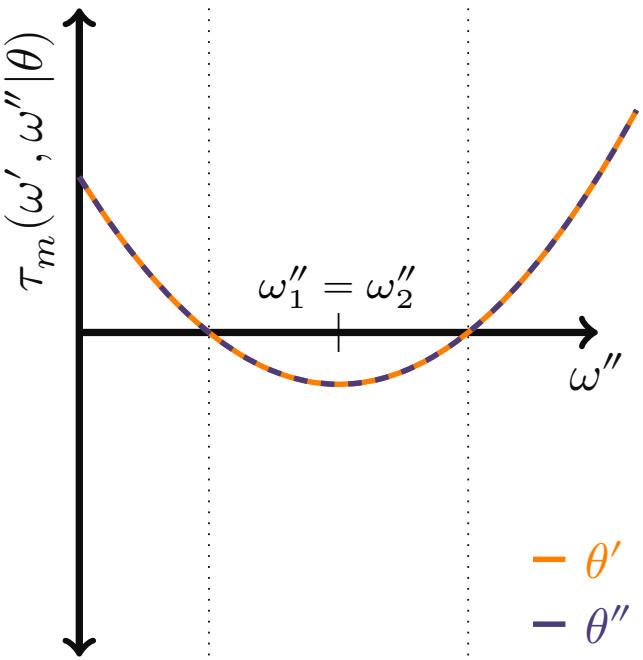
Under what conditions do we achieve **target equivalence** or **target congruence**?



Achieving target equivalence

Theorem

*Target-equivalence holds across a collection of studies if and only if the mechanism satisfies **external validity** and all studies are **harmonized** (almost everywhere).*



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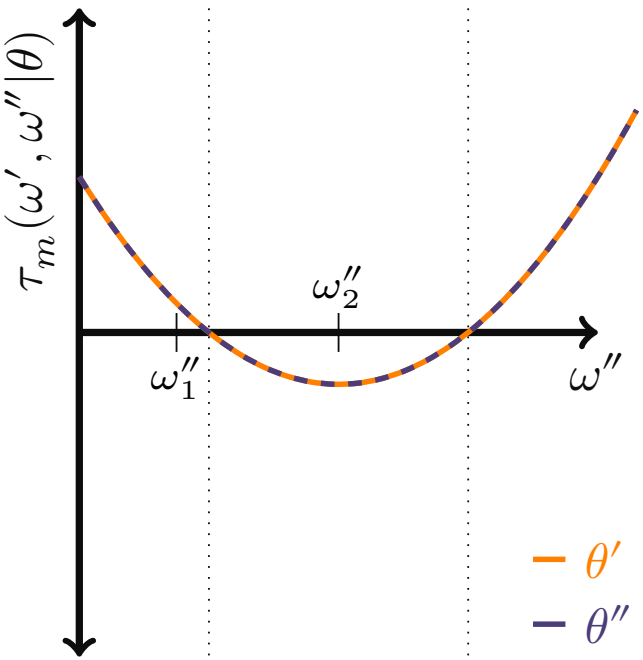

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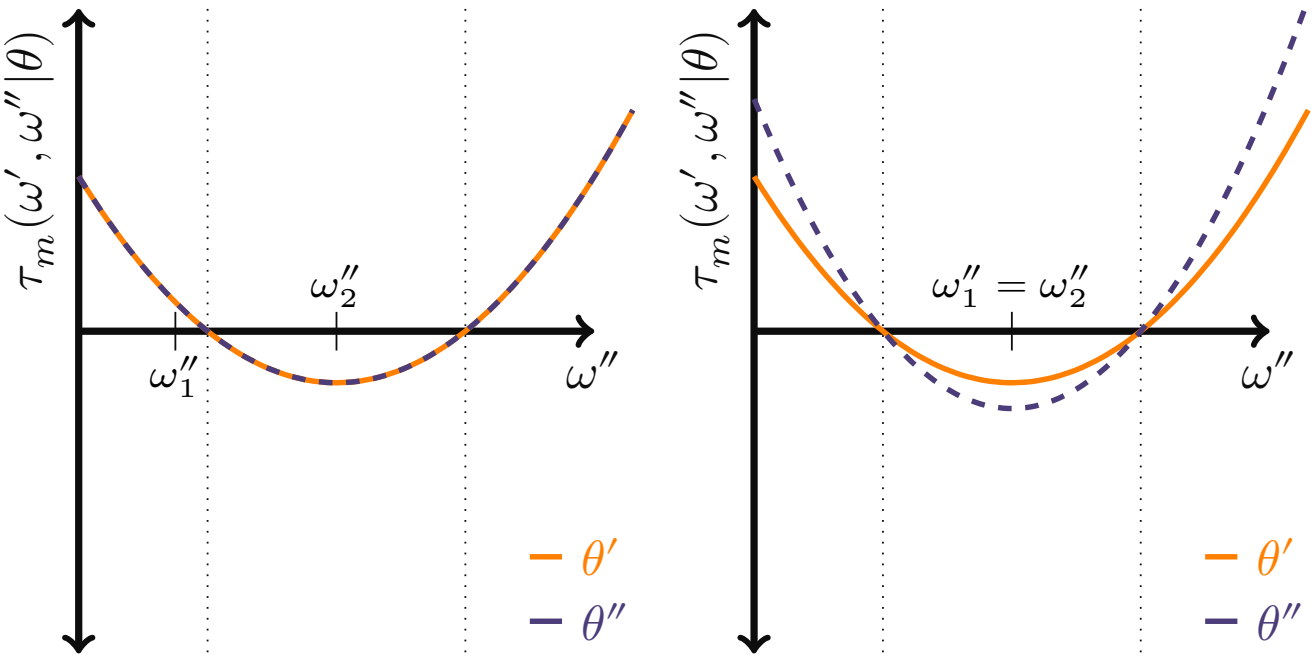

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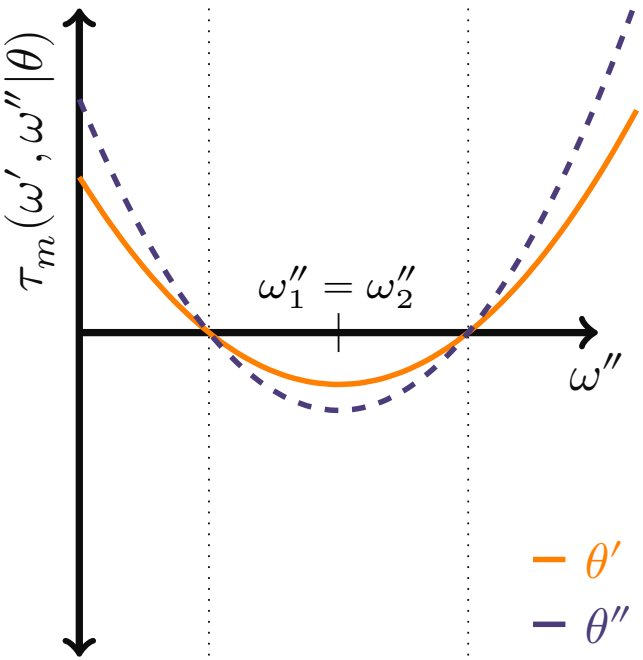

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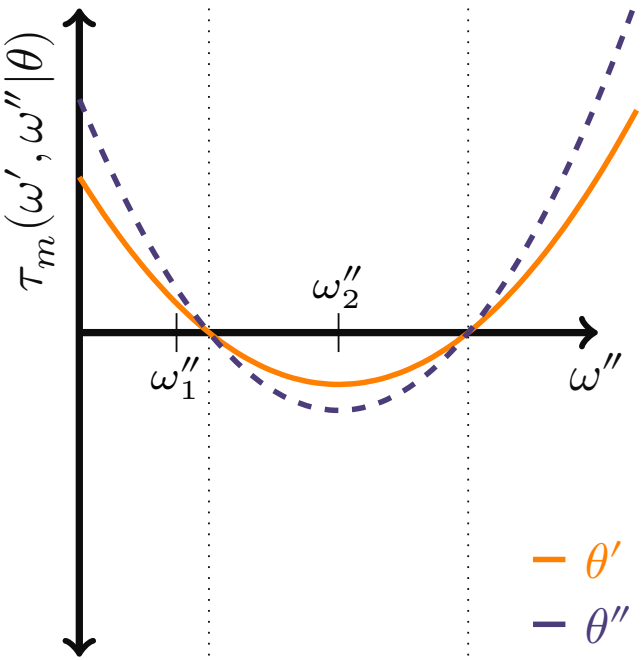

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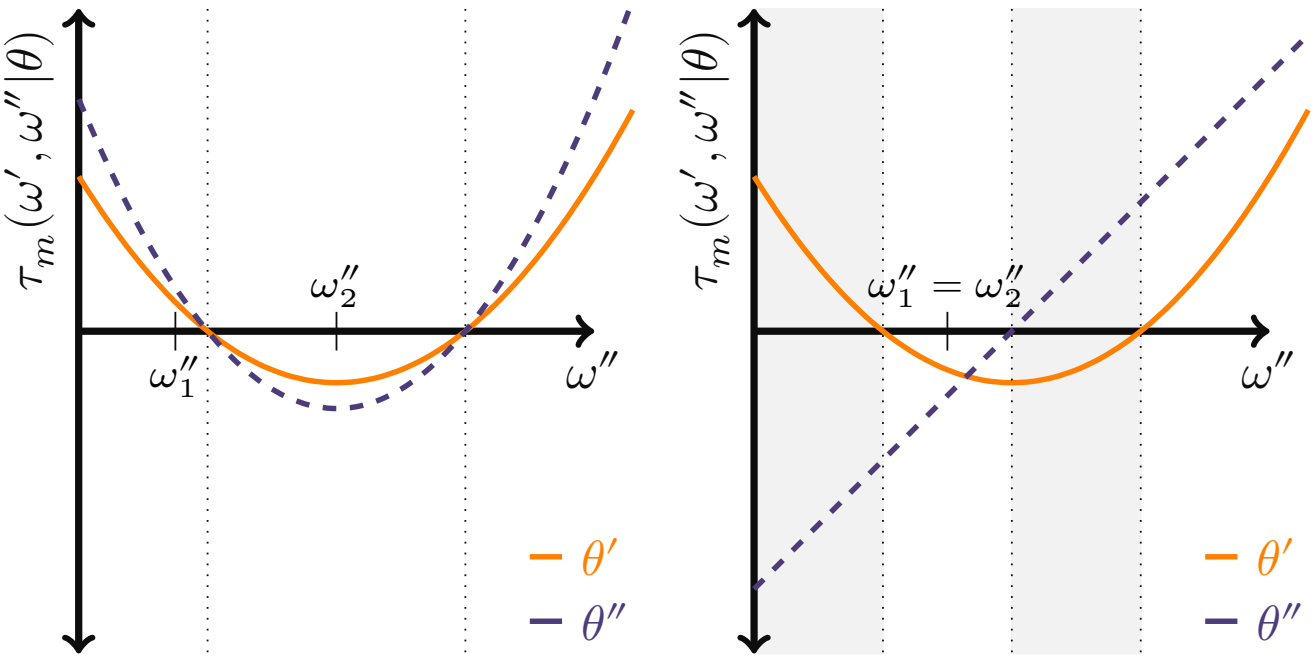

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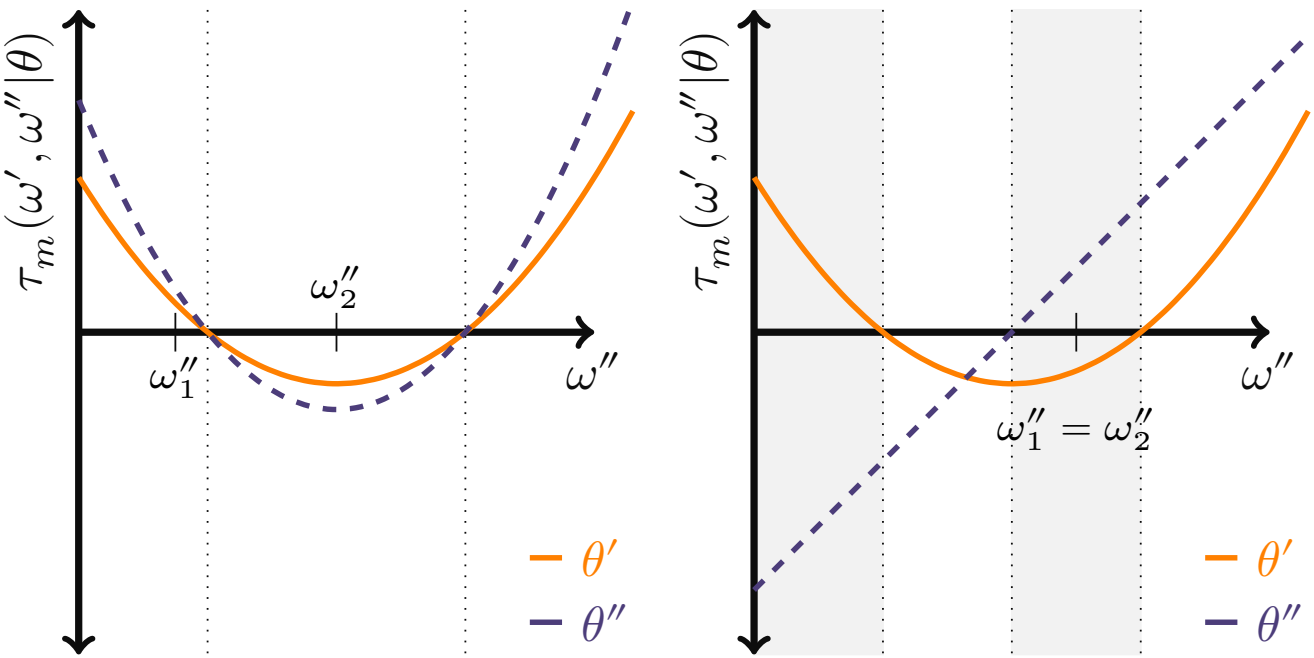

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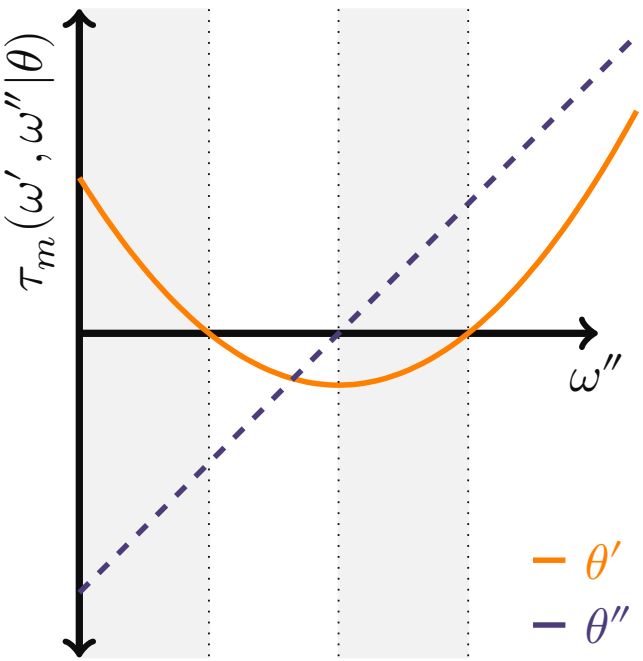

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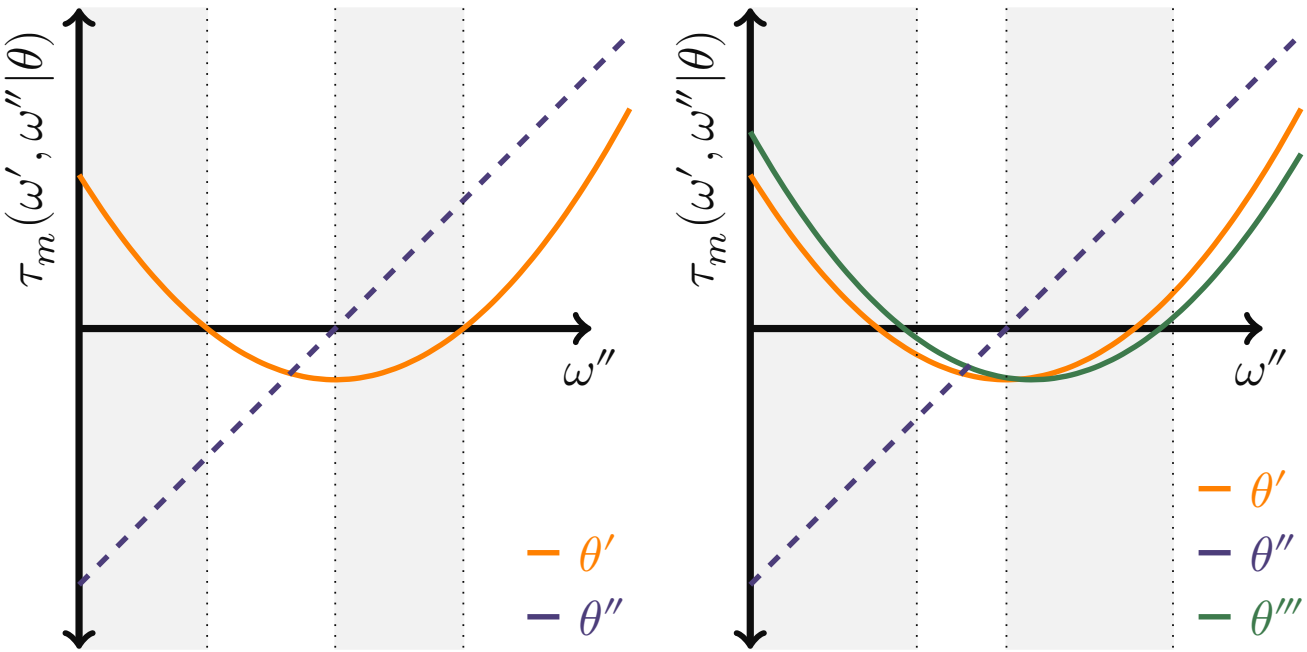

Why it matters

Recall the P2P studies from Uganda:

- 2004-5 study (θ') $\rightarrow$ larger reduction in under-5 mortality
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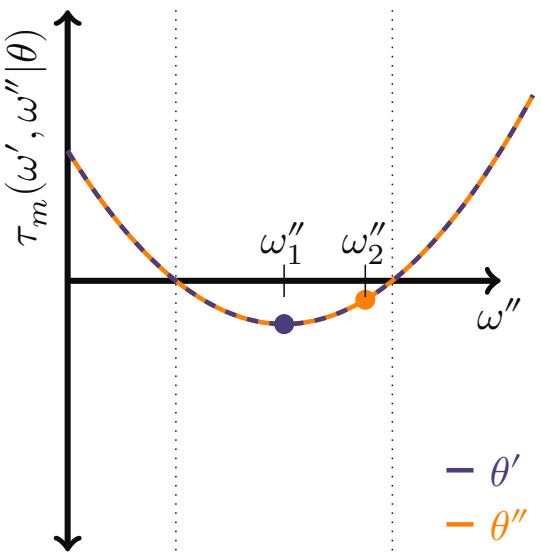

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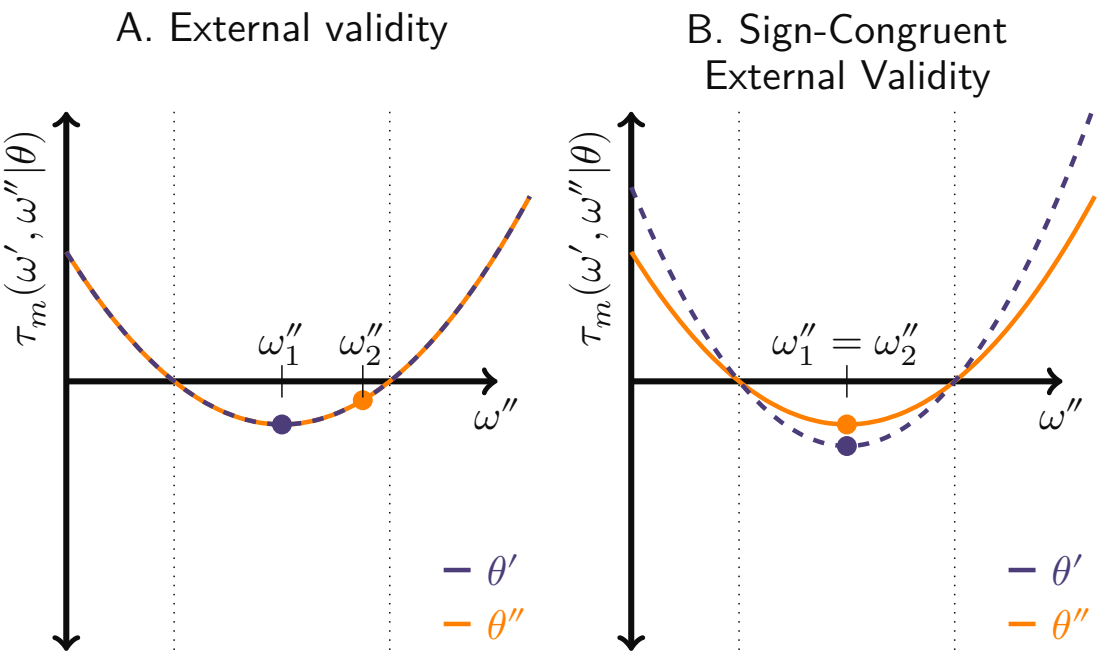

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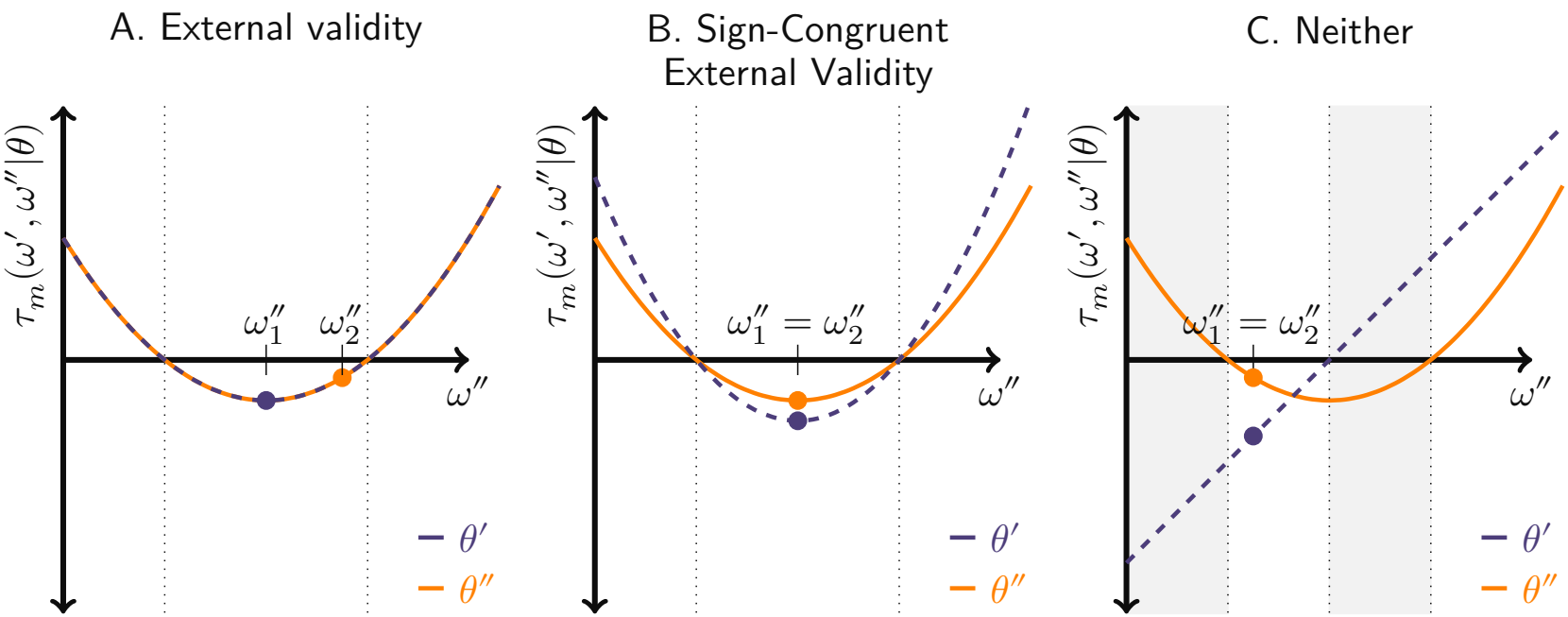

Taking Stock

When research designs are **harmonized**:

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Without harmonization, relationship between empirical targets of studies is ambiguous, even when external validity holds.

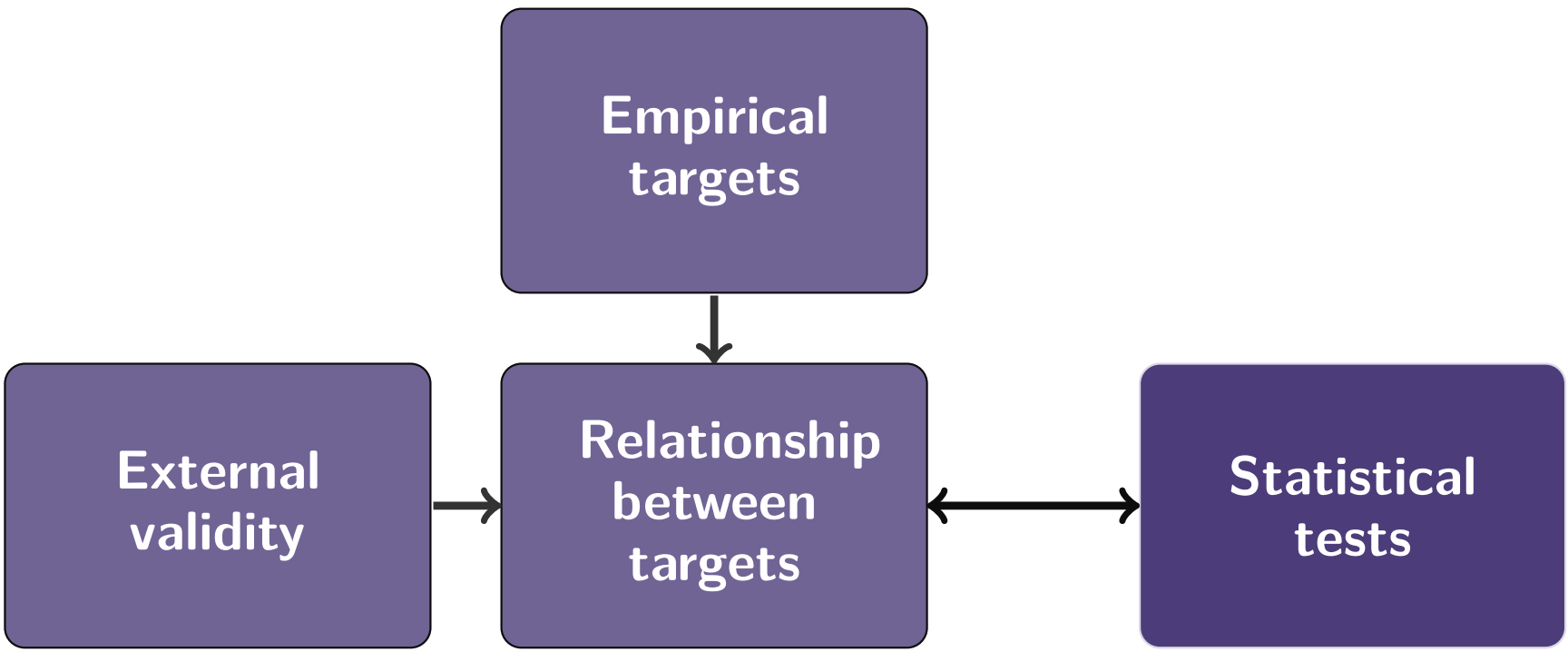
Doing more replications can only exacerbate these problems.

- These problems are non-statistical.

Comparing Estimates

Making Comparisons

- Two comparisons pursued in replications are:
  - Comparison of **point estimates** $\rightarrow$ target equivalence
  - Comparison of **estimate signs** $\rightarrow$ target congruence



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Estimated treatment effects in two studies will always differ:

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2. The sign-comparison test computes:

$$\mathcal{Z} = e_1 \cdot e_2,$$

and test the null hypothesis of **target congruence**, which occurs when $sign(\tau_{m_1}(\omega_1', \omega_1''|\theta_1)) \cdot sign(\tau_{m_2}(\omega_2', \omega_2''|\theta_2)) > 0$.

What does the estimate-comparison test evaluate?

Proposition

If two studies have unbiased and consistent estimation errors, then

1. *If the studies are harmonized, then the estimate-comparison test assesses a null hypothesis that the mechanism is* ***externally valid****;*
2. *If the mechanism has external validity, then the estimate-comparison test assesses a null hypothesis that the studies are* ***harmonized****.*

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Key idea: learning about **external validity** is not automatic!

- We have to worry about cross-study design as well.
- Sometimes we prefer to learn about how effects vary in study design.

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Key idea: learning about **sign-congruent external validity** is not automatic!

- We have to worry about cross-study design as well.
- A weaker concept of external validity limits what we could learn about artifactual discrepancies.

Two Approaches to Replication

Structural approach

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Drawback: inconsistent with notions of causality invoked within-study.

Design-based alternative

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Design-based approach to conceptual replication is **sequential**

1. Design-**harmonized** replications
   → measure target discrepancies (under one design).
2. **Single-setting** replicatons varying design
   → measure artifactual discrepancies (in one setting).
3. **Non-harmonized multi-setting** design
   → With steps #1 and #2, evaluate whether artifactual discrepancies vary in settings.

Limits to design-based approach to conceptual replication

Sequential nature requires that effects of mechanisms are stable over time.

- More likely for some interventions, settings than others.

Problems in the organization of research:

- Limited researcher incentives for replication.
- In principle, we favor replication by independent teams.
  - Requires more transparent communication of precise design, link to constructs than is current practice.

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Formal **conceptual frameworks** as a necessary complement to advances in estimation.

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Nagging question: What could we have learned?

Applied $\rightarrow$ methodological work: three directions

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Designing field experiments $\rightarrow$ formal treatments of ethics:

- "The Ethics of Electoral Experiments: Design-Based Recommendations" (working paper)

Thank you!

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Supplementary Information

Contents

Treatment effects and potential outcomes framework

Define potential outcomes:

$$Y_i^m(\omega''|\theta) \text{ and } Y_i^m(\omega'|\theta)$$

The treatment effect function is given by:

$$\tau_m(\omega', \omega''|\theta) = f_{\mathcal{D}}(Y_i^m(\omega''|\theta) - Y_i^m(\omega'|\theta)),$$

where:

- $f(\cdot)$ is a function or operator.
- $\mathcal{D}$ is a set of units for whom treatment effects are estimated

C-validity

Egami and Hartman (2022) formalize C-validity as:

$$Y_i(T = 1, c) - Y_i(T = 0, c) = Y_i(T = 1, c^*) - Y_i(T = 0, c^*)$$

with our (potential outcome) notation, this can be written:

$$Y_i^m(\omega''|\theta) - Y_i^m(\omega'|\theta) = Y_i^m(\omega''|\theta') - Y_i^m(\omega'|\theta')$$

Recall that:

$$\tau_m(\omega', \omega''|\theta) = f_{\mathcal{D}}(Y_i^m(\omega''|\theta) - Y_i^m(\omega'|\theta)).$$

As such, C-validity implies that:

$$\tau_m(\omega', \omega''|\theta) = \tau_m(\omega', \omega''|\theta'),$$

which is the definition of **external validity**. But external validity does not imply that C-validity holds.

T-validity and Y-validity

T-validity holds that:

$$\mathbb{E}_{\mathcal{P}}[Y_i(T_i = 1, c) - Y_i(T_i = 0, c)] = \mathbb{E}_{\mathcal{P}}[Y_i(T_i^* = 1, c) - Y_i(T_i^* = 0, c)],$$

- Ruled out by symmetry assumption (i.e., $T_i^* = 1 - T_i$), except for case when $\mathbb{E}_{\mathcal{P}}[Y_i(T_i = 1, c) - Y_i(T_i = 0, c)] = 0 \forall T_i$, which is ruled out by full-rank assumption.

Y-validity holds that:

$$\mathbb{E}_{\mathcal{P}}[Y_i(T_i = 1, c) - Y_i(T_i = 0, c)] = \mathbb{E}_{\mathcal{P}}[Y_i^*(T_i = 1, c) - Y_i^*(T_i = 0, c)],$$

- Ruled out by full-rank assumption.

Fisher: Concepts in (applied) statistics

> *"...the obscurity which envelops the theoretical bases of statistical methods may perhaps be ascribed to two considerations. In the first place, it appears to be widely thought, or rather felt, that in a subject in which all results are liable to greater or smaller errors, precise definitions of ideas or concepts is, if not impossible, at least not a practical necessity. In the second place...it is customary to apply the same name...to both the true value we would like to know, but can only estimate, and to the particular value at which we happen to arrive by our methods of estimation; so in applying the term probable error, writers sometimes would appear to suggest that the former quantity, and not merely the latter, is subject to error."*
>
> R.A. Fisher (1922, p. 311)

Sign-comparison test: inference

Let $\varepsilon_i^{n_i}$ be normally distributed with mean 0 and let the standard error of e_i be se_i.

- The p-value of the null hypothesis of sign-congruence is:

$$\begin{aligned} p &= \Pr(e_1 > 0)\Pr(e_2 > 0) + \Pr(e_1 < 0)\Pr(e_2 < 0) \\ &= \Phi(\frac{e_1}{se_1})\Phi(\frac{e_2}{se_2}) + (1 - \Phi(\frac{e_1}{se_1}))(1 - \Phi(\frac{e_2}{se_2})), \end{aligned}$$

where $\Phi(\cdot)$ is the cdf of the standard normal distribution.

Rejection regions

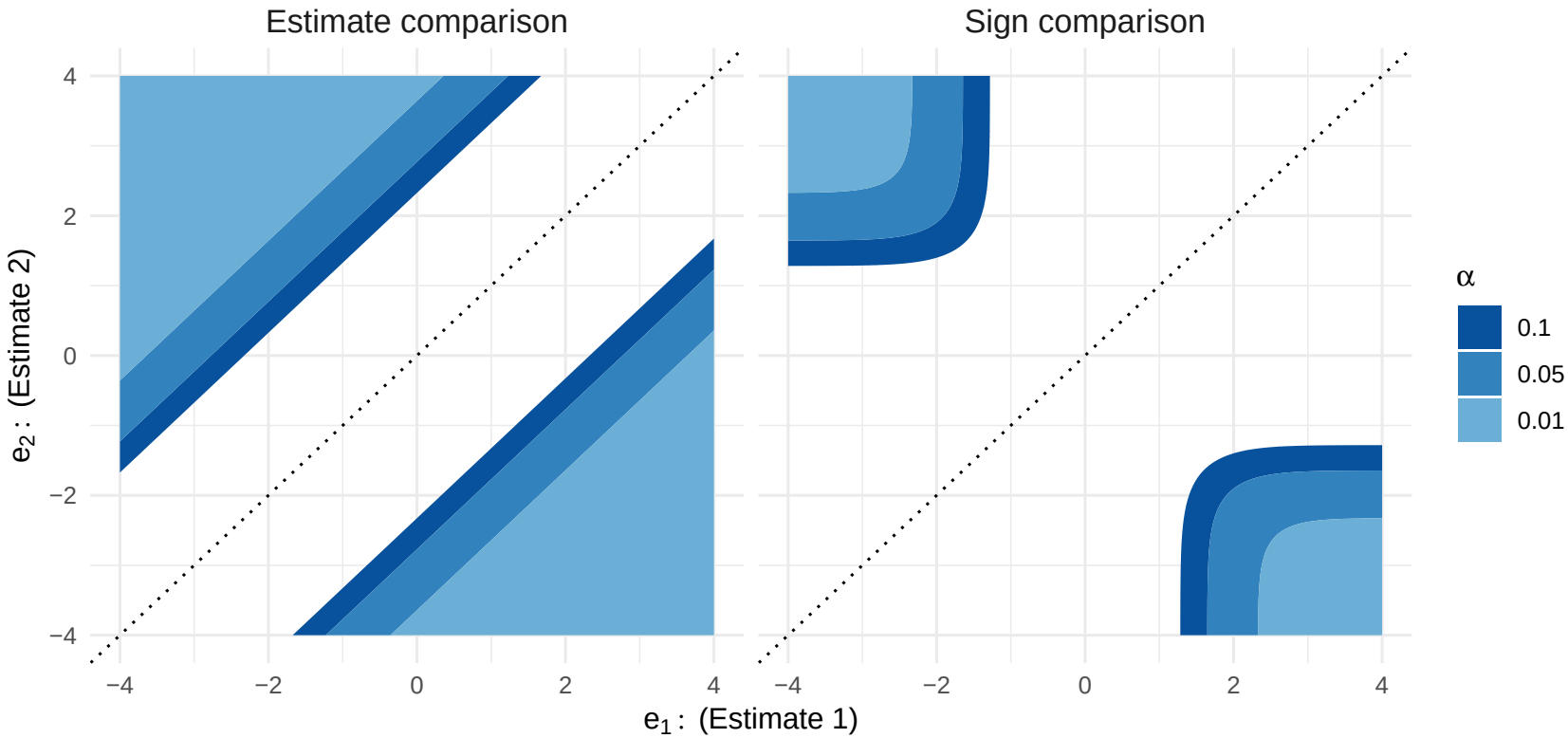

Figure: Rejection regions of the estimate- and sign-comparison approaches for Type-I error rates, $\alpha \in \{0.01, 0.05, 0.1\}$. Both plots fix $se_1 = se_2 = 1$ in order to visualize these regions in two dimensions.

Classification of replication designs

Class	Sub-class	Studies differ in... Samples	Settings	Design
Exact		–	–	–
Direct		✓	–	–
Conceptual	Harmonized	✓	✓	–
Conceptual	Single-setting	✓	–	✓
Conceptual	Non-harmonized, multi-setting	✓	✓	✓

Table: Mapping between conventional classification of replication studies and our framework. Note that the disaggregation of conceptual replications into sub-classes is non-standard in existing literature.

Additional estimands, tests in OSF (2015)

Suppose we have $N > 1$ replication studies, where each study consists of a pair of estimates.

OSF (2015) additionally compute:

- Share of replications with $p < 0.05$ in the same direction
- Effect size difference
- Meta-analytic estimate
- Original effect within replication 95% CI
- Subjective "yes" did it replicate