Identifying the Impact of Hypothetical Stakes on Experimental Outcomes and Treatment Effects

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Abstract

Recent studies showing that some outcome variables do not statistically significantly differ between real-stakes and hypothetical-stakes conditions have raised methodological challenges to experimental economics' disciplinary norm that experimental choices should be incentivized with real stakes. I show that the hypothetical bias measures estimated in these studies do not econometrically identify the hypothetical biases that matter in most modern experiments. Specifically, traditional hypothetical bias measures are fully informative in 'elicitation experiments' where the researcher is uninterested in treatment effects (TEs). However, in 'intervention experiments' where TEs are of interest, traditional hypothetical bias measures are uninformative; real stakes matter if and only if TEs differ between stakes conditions. I demonstrate that traditional hypothetical bias measures are often misleading estimates of hypothetical bias for intervention experiments, both econometrically and through re-analyses of three recent hypothetical bias experiments. The fact that a given experimental outcome does not statistically significantly differ on average between stakes conditions does not imply that all TEs on that outcome are unaffected by hypothetical stakes. Therefore, the recent hypothetical bias literature does not justify abandoning real stakes in most modern experiments. Maintaining norms that favor completely or probabilistically providing real stakes for experimental choices is useful for ensuring externally valid TEs in experimental economics.

Keywords: Interaction effects, meta-analysis, generalizability, bootstrap. JEL: C18, C90, D91.

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1 Introduction

Incentivizing experimental choices with real stakes is a key feature of experimental economics. This approach is a long-standing norm in experimental economics, as participants' desire to optimize real-world outcomes can improve the generalizability of experimental behavior by overpowering biases known to emerge in experimental environments (see Smith 1976; Smith 1982; Roth 1995; Camerer & Hogarth 1999; Hertwig & Ortmann 2001; Schram 2005; Bardsley et al. 2009; Charness, Gneezy, & Halladay 2016; Svorenčík & Maas 2016; Clot, Grolleau, & Ibanez 2018). However, this norm is starting to shift. Top economics publications are becoming increasingly open to publishing results from hypothetical-stakes experiments, and large-scale general-population surveys such as the Global Preferences Survey are now eliciting economic preferences using hypothetical-stakes experiments (e.g., see Golsteyn, Grönqvist, & Lindahl 2014; Cadena & Keys 2015; Kuziekmo et al. 2015; Alesina, Stantcheva, & Teso 2018; Falk et al. 2018; Sunde et al. 2022; Stango & Zinman 2023). Recent research also shows that some outcome variables do not statistically significantly differ on average between real-stakes and hypothetical-stakes conditions (Brañas-Garza, Kujal, & Lenkei 2019; Brañas-Garza et al. 2021; Matousek, Havranek, & Irsova 2022; Alfonso et al. 2023; Brañas-Garza et al. 2023; Enke et al. 2023; Hackethal et al. 2023). Citing some of this recent hypothetical bias research (in particular Matousek, Havranek, & Irsova 2022), the announcement for *Experimental Economics*' special issue on incentivization states: "There is good rationale for incentivized experiments, but recently there has been evidence that incentivization may not always matter."[1]

This paper shows econometrically and empirically that the existing hypothetical bias literature does not statistically support omitting real stakes in most modern experiments. I begin by distinguishing two types of experiments. In 'elicitation experiments', no intervention is varied, and treatment effects (TEs) are not of interest. In contrast, 'intervention experiments' vary at least one intervention with the goal of measuring its TE. Elicitation experiments dominated early experimental economics research, and though they remain important to this day, most modern economic experiments are intervention experiments.

[1] See https://link.springer.com/journal/10683/updates/26740876. Accessed on 19 September 2024.

Econometrically, traditional tests for hypothetical bias do not identify the hypothetical biases that matter for intervention experiments. I show that the hypothetical bias relevant for intervention experiments is the interaction effect between hypothetical stakes and the treatment of interest. However, most traditional hypothetical bias experiments cannot identify this interaction effect. Typically, these experiments randomize participants into either real-stakes or hypothetical-stakes conditions, elicit an outcome, and test whether the difference in average outcomes between the two conditions is statistically significant (e.g., Brañas-Garza et al. 2023; Hackethal et al. 2023). I show that this difference is the average marginal effect of hypothetical stakes on the outcome, which has no general relationship with the interaction effect between hypothetical stakes and any treatment of interest. This makes sense for two reasons. First, a researcher cannot identify an interaction effect if all the researcher knows is the average marginal effect of one of the two variables in the interaction. Second, it is unrealistic to expect hypothetical stakes to affect every possible intervention's TE on a given outcome in the exact same way.

Empirically, TE-irrelevant hypothetical bias measures often meaningfully misidentify TE-relevant hypothetical biases. I re-analyze replication data from three recent hypothetical bias experiments that vary both a treatment of interest and hypothetical stakes. These experiments allow me to directly estimate the interaction effects between hypothetical stakes and treatments of interest, and to compare these interaction effects with the TE-irrelevant hypothetical bias estimates typically produced in hypothetical bias experiments. I find that TE-irrelevant hypothetical bias measures often yield different conclusions than TE-relevant hypothetical bias measures. In some cases, TE-irrelevant hypothetical bias measures even exhibit sign flips when compared to TE-relevant hypothetical bias measures. That is, TE-irrelevant hypothetical bias estimates are sometimes positive even when TE-relevant hypothetical biases are negative (and vice versa).

These findings raise doubts about the practical value of recent advances in the hypothetical bias literature. My econometric results show that recent studies finding no statistically significant differences in certain outcomes between real-stakes and hypothetical-stakes conditions do not justify the broader conclusion that real stakes 'do not matter' for all TEs on those outcomes. Researchers who abandon real experimental stakes in their intervention

experiments based on these findings may be misled, and TEs estimated in these experiments may be confounded by meaningful hypothetical biases. Because ruling out hypothetical biases for a given intervention's TE on a given outcome functionally requires a factorial hypothetical bias experiment on that specific outcome and intervention, it is also unproductive and uninformative to conduct hypothetical bias experiments with the goal of 'paving the way' for future researchers to abandon real stakes in their experiments. Therefore, it remains useful to maintain existing norms in experimental economics that favor incentivizing experimental choices with real stakes. Because incentivizing all experimental choices for all participants is too expensive for some researchers, it is also likely beneficial to augment these norms by allowing researchers to incentivize experimental choices with real stakes probabilistically.

This paper is structured as follows. Section 2 provides a taxonomy of experiments that clarifies the relevant differences between elicitation experiments and intervention experiments, and establishes notation for the paper. Section 3 discusses how hypothetical bias is measured in the historical literature. Section 4 establishes econometrically why these traditional methods for measuring hypothetical bias fail to identify TE-relevant hypothetical biases. Section 5 provides three empirical applications demonstrating that TE-relevant and TE-irrelevant hypothetical bias measures often differ. Section 6 discusses the implications of my findings for norms in experimental economics and the future of hypothetical bias research. Section 7 concludes.

2 Terminology and Notation

I start by establishing a simple taxonomy of experiments. Let $Y_i \in \mathbb{R}$ be the outcome variable of interest, and let $D_i \in \{0, 1\}$ be an experimental intervention of interest. For this paper, a 'real-stakes' condition is one in which participants' experimental choices map onto real-world payoffs or consequences. In contrast, 'hypothetical-stakes' conditions do not link experimental choices to real-world consequences.

I distinguish between two types of experiments, the first of which is an 'elicitation experiment.' This sort of experiment does not apply any intervention, and there are no TEs to estimate. The primary aim of an elicitation experiment is to use experimental procedures to

obtain descriptive statistics concerning Y_i, usually sample means or medians. For example, a researcher interested in learning the average consumer's willingness to pay for a product may run an experiment employing the Becker, DeGroot, & Marschak (1964) procedure to obtain an incentive-compatible measure of participants' willingness to pay. This is undoubtedly an experiment, but there is no TE to speak of; the researcher is just interested in descriptive statistics on willingness to pay. This is thus an elicitation experiment.

The second type of experiment is an 'intervention experiment.' Unlike an elicitation experiment, an intervention experiment employs an intervention of interest D_i, and the researcher is interested in the TE of this intervention. To extend the previous example, suppose that the researcher wants to know the effect of a specific product characteristic on willingness to pay. They could repeat the same Becker-DeGroot-Marschak experiment, but randomly assign half of the participants to consider a product with that characteristic. The researcher can then estimate the TE of that characteristic on willingness to pay by taking the difference in average willingness to pay between the two halves of the sample. This would be an intervention experiment.[2]

In general, 'hypothetical bias' can be defined as the difference in the statistic of interest resulting from a change in stakes condition S_i, which is parameterized here as a dummy variable indicating that participant i faces real stakes with probability p' instead of probability p. That is, for $p, p' \in [0, 1]$ with $p \neq p'$, I define

$$S_i = \begin{cases} 0 \text{ if participant } i\text{'s stakes are real with probability } p \\ 1 \text{ if participant } i\text{'s stakes are real with probability } p' \end{cases} . \tag{1}$$

Typically, $p = 1$ and $p' = 0$, meaning $S_i = 1$ indicates pure hypothetical stakes whereas $S_i = 0$ indicates pure real stakes. I use this definition of S_i throughout the remainder of this paper for simplicity. However, this framework can be extended to examine potential biases arising from switching between any pair of probabilities that stakes are real. Because of this

[2]The researcher may still be interested in descriptive statistics about Y_i in an intervention experiment. For instance, the experiment described in this paragraph is still an intervention experiment even if the researcher also wants to know the mean willingness to pay for products both with and without the characteristic of interest. So long as the experiment employs an intervention whose TE is of interest to the researcher, it is an intervention experiment.

generalizability, the statistical framework that I introduce throughout this paper can also be used to explore hypothetical biases arising from probabilistic incentivization. I return to this point in Section 6.4. The specific bias induced by switching between stakes conditions depends on the statistic of interest.

3 Historical Measurement of Hypothetical Bias

Many early seminal contributions in experimental economics are elicitation experiments. A preponderance of economic experiments published prior to 1960 focus heavily on testing the predictions of prevailing economic theories and documenting empirical regularities observed in laboratory experiments (Roth 1995). This was largely done using elicitation experiments to measure various economic preferences and behaviors, including indifference curves for different bundles of goods (Thurstone 1931; Rousseas & Hart 1951), risk and ambiguity preferences (Allais 1953; Mosteller 1953), strategies in games (Flood 1958), and prices in experimental markets (Chamberlin 1948). This is not to say that no intervention experiments were conducted in experimental economics' early years, but elicitation experiments certainly played a leading role.

This historical context is important because the preponderance of elicitation experiments in experimental economics' early years influenced the statistical parameters that experimental economists were interested in when disciplinary norms on experimental stakes first emerged. The influential 'Wallis-Friedman critique' of hypothetical choice menus was already published in 1942, and played a key role in prompting leading experimental economists to incentivize their experiments with real stakes (see Wallis & Friedman 1942; Svorenčík & Maas 2016; Ortmann 2016). As a result, by the end of the 1950s, experimental economists were already predominantly incentivizing their experiments with real stakes (Roth 1995). The fact that experimental economists at this time were often more interested in descriptive statistics about people's basic economic preferences than the TEs of economically-relevant interventions influenced the reasons why experimental economists cared about real stakes, as well as the ways in which they measured bias when real stakes were not provided.

Two key rationales for incentivizing experiments with real stakes emerged from this early

literature. First, experimental economists believe that hypothetical stakes may affect the average preference or behavior elicited from a sample. This implies that hypothetical stakes bias the expected value of Y_i.

I refer to hypothetical biases on average elicited outcomes as 'classical hypothetical bias (CHB)', which can be written as

$$\text{CHB} \equiv \mathbb{E}\left[Y_i(p') - Y_i(p)\right]. \tag{2}$$

In other words, CHB is the average marginal effect of changes in stakes conditions on the outcome of interest. When the statistic of interest is the sample mean of Y_i, CHB can be easily parameterized in a linear model of the form

$$Y_i = \alpha + \delta S_i + \epsilon_i. \tag{3}$$

If S_i is randomly assigned, then one can invoke unconfoundedness condition $\epsilon_i \perp S_i$ to examine the causal effect of hypothetical stakes in the following simple potential outcomes framework (see Rubin 1974; Rubin 2005):

$$Y_i(1) - Y_i(0) = (\alpha + \delta) - \alpha = \delta, \tag{4}$$

where $Y_i(S)$ is the potential outcome of Y_i depending on stakes condition $S \in \{0, 1\}$. It then holds trivially that

$$\text{CHB} = \mathbb{E}\left[Y_i(p') - Y_i(p)\right] = \mathbb{E}\left[Y_i(1) - Y_i(0)\right] = \mathbb{E}\left[\delta\right] = \delta. \tag{5}$$

In other words, under experimental randomization of stakes conditions and linear models commonly applied when analyzing experiments, CHB can be identified as the difference in mean outcome values between hypothetical-stakes and real-stakes conditions.

CHB is a well-documented factor in economic experiments. Camerer & Hogarth (1999) provide systematic evidence of CHB, reviewing 36 studies that compare a hypothetical-stakes

condition with a real-stakes control.[3] 26 of these studies (72%) show that hypothetical stakes affect the central tendency of at least one outcome. Similarly, Harrison & Rustrom (2008) review 35 studies measuring CHB in experiments on willingness to pay. Only two of these studies (5.7%) report zero CHB, and 16 studies (45.7%) report statistically significant CHB. Smith & Walker (1993) and Hertwig & Ortmann (2001) provide similar systematic evidence.

Significant CHB is found in a variety of experimental settings. These include ultimatum games (Sefton 1992), public goods games (Cummings et al. 1997), auctions (List 2001), and multiple price lists (Harrison et al. 2005). CHB is particularly severe in contingent valuation experiments. Experimental participants routinely overstate their willingness to pay for public goods such as environmental services (see Hausman 2012). Meta-analytic estimates of CHB in contingent valuation range from 35% (Murphy et al. 2005) to 200% (List 2001). Even though some recent studies find that experimental outcomes do not statistically significantly differ between hypothetical-stakes and real-stakes conditions (Brañas-Garza, Kujal, & Lenkei 2019; Brañas-Garza et al. 2021; Matousek, Havranek, & Irsova 2022; Brañas-Garza et al. 2023; Enke et al. 2023; Hackethal et al. 2023), a large body of literature demonstrates substantial risks of CHB in many experimental contexts.

The second rationale for incentivizing experiments with real stakes is reducing noise. Experimental economists believe that participants motivated by real stakes make more careful and deliberative choices than participants facing hypothetical stakes, and thus that real stakes reduce noise in experimental outcomes (see Bardsley et al. 2009). Smith & Walker (1993) survey 31 hypothetical bias studies and find that in virtually all, the variance of outcomes around theory-predicted values decreases when stakes are real. Camerer & Hogarth (1999) note nine experiments where hypothetical stakes change the variance or convergence of experimental outcomes (usually by increasing variance or decreasing convergence). Hertwig & Ortmann (2001) identify two additional experiments where similar effects are observed.

However, the measurement of these 'noise reduction' effects is not systematic and differs between studies. Some studies focus on changes in the standard deviation (SD) or variance

[3]This is a subset of the 74 experiments reviewed by Camerer & Hogarth (1999), specifically focusing on studies with a '0 vs. L' treatment, or a '0' treatment with some real stakes control. My list excludes Scott, Farh, & Podsakoff (1988) because participants were unaware of the real stakes until after the experiment concluded (Camerer & Hogarth 1999).

of outcomes between stakes conditions (e.g., Wright & Anderson 1989; Ashton 1990; Irwin, McClelland, & Schulze 1992; Forsythe et al. 1994). Others assess noise by examining deviations from some theory-predicted value, such as price deviations from a competitive market price (see Edwards 1953; Smith 1962; Smith 1965; Jamal & Sunder 1991; Smith & Walker 1993). Furthermore, changes in variance between stakes conditions are typically not accompanied by a precision measure, such as a standard error (SE), to qualify the magnitude of these between-condition variance shifts.[4] It is thus unclear whether observed differences in outcome variances between stakes conditions reflect genuine effects or are simply artefacts of sampling variation.

For the purposes of this paper, I parameterize the effect of hypothetical stakes on noise as an 'outcome SD bias (OSDB)', which can be written as

$$\text{OSDB} \equiv \mathbb{E}\left[\sigma_{Y_i}(p') - \sigma_{Y_i}(p)\right]. \tag{6}$$

A point estimate of this bias can be obtained by simply taking the difference in outcome SD σ_{Y_i} between stakes conditions. The SE of this estimate can be obtained via bootstrap (see Section 5 for examples). I define noise in this way because not all experimental outcomes have clear values that theoretically 'should' be observed in experimental data, whereas SDs can be used to measure noise across experimental contexts.

Most studies on the effects of real stakes in experiments focus exclusively on CHB and OSDB. Brañas-Garza, Kujal, & Lenkei (2019) meta-analytically find that scores on the cognitive reflection test do not statistically significantly differ between real-stakes and hypothetical-stakes settings (though see Yechiam & Zeif 2023). Brañas-Garza et al. (2021) use equivalence testing to show statistically significant evidence that the count of safe choices made on a Holt & Laury (2002) multiple price list does not differ between real-stakes and hypothetical-stakes conditions (see also Fitzgerald 2024). Matousek, Havranek, & Irsova (2022) find that the meta-analytic average individual discount rate does not statistically significantly differ between real-stakes and hypothetical-stakes experiments. Brañas-Garza

[4]Recent attempts to qualify the significance of differences in variance between stakes conditions often take non-parametric approaches such as Kolmogorov-Smirnov tests (e.g., see Brañas-Garza et al. 2023; Hackethal et al. 2023). However, such non-parametric tests only identify significant differences in *distributions*, which are defined not just by parameter variances, but also by centrality measures and other moments.

et al. (2023) find that the means and SDs of time discounting factors are not statistically significantly different between real-stakes and hypothetical-stakes conditions, and Hackethal et al. (2023) find that the same is true of the number of risky choices that participants make in a multiple price list experiment. Enke et al. (2023) find no statistically significant differences in the number of correct answers on the cognitive reflection test, a base rate neglect test, or a contingent reasoning test between real-stakes and hypothetical-stakes conditions. These studies are reporting estimates of CHB, with Brañas-Garza et al. (2023) and Hackethal et al. (2023) also reporting evidence on OSDB.

Though CHB and OSDB are fully informative measures of hypothetical bias in elicitation experiments – which played early leading roles in experimental economics when norms on real stakes first emerged – most modern work in experimental economics (and experimental social sciences more broadly) is not limited to elicitation experiments. Although elicitation experiments remain important today, many researchers are now more focused on obtaining clean causal TEs from experiments than they are in simply obtaining descriptive statistics. Such experimental TEs were, and still are, crucial antecedents of the credibility revolution in economics (Angrist & Pischke 2010). However, as the next section shows, CHB and OSDB are completely uninformative measures of hypothetical bias for experimental TEs.

4 Hypothetical Bias for Treatment Effects

4.1 Treatment Effect Point Estimates: IHB

CHB is irrelevant for describing hypothetical bias on TEs. In fact, Equation 3 shows that CHB can be modeled and estimated while completely ignoring intervention D_i. Any statistical framework used to identify the effect of real stakes on TEs must incorporate D_i, and must allow the possibility that stakes condition S_i can influence TEs.

My econometric framework for modeling the impact of hypothetical stakes on TEs considers a simple 2x2 factorial experiment where both treatment D_i and stakes condition S_i are randomized with equal probability across participants. Following Guala (2001), I model

the effects of D_i and S_i using a simple heterogeneous treatment effects framework:

$$Y_i = \alpha + \beta_1 D_i + \beta_2 S_i + \beta_3 (D_i \times S_i) + \mu_i. \tag{7}$$

Randomization of D_i and S_i confers unconfoundedness: $\mu_i \perp \{D_i, S_i\}$. Participant i's treatment effect τ_i – the marginal effect of D_i on Y_i – can thus be modeled in the following potential outcomes framework:

$$\tau_i = Y_i(1, S) - Y_i(0, S) = \begin{cases} \beta_1 \text{ if } S_i = 0 \\ \\ \beta_1 + \beta_3 \text{ if } S_i = 1 \end{cases}. \tag{8}$$

Here $Y_i(D, S)$ represents the potential outcome of Y_i depending on intervention status $D \in \{0, 1\}$ and stakes condition $S \in \{0, 1\}$. For what follows, suppose that the statistic of interest is the average TE $\tau \equiv \mathbb{E}[\tau_i]$.

The hypothetical bias on the point estimate of τ can be derived as a simple difference-in-differences, which I refer to as 'interactive hypothetical bias (IHB)':

$$\begin{aligned} \text{IHB} &\equiv \mathbb{E}[\tau_i(p') - \tau_i(p)] && (9) \\ &= \mathbb{E}[Y_i(1,1) - Y_i(0,1)] - \mathbb{E}[Y_i(1,0) - Y_i(0,0)] && (10) \\ &= (\beta_1 + \beta_3) - \beta_1 = \beta_3. && (11) \end{aligned}$$

This implies that hypothetical stakes bias the TE's point estimate if and only if $\beta_3 \neq 0$. This yields an intuitive conclusion: in a factorial experiment that randomizes both an intervention and hypothetical stakes, any hypothetical bias in the point estimate of the intervention's TE is fully captured by the interaction effect between the intervention and hypothetical stakes.

IHB is a fully informative measure of hypothetical bias in intervention experiments, but CHB does not identify this term. Under the data-generating process in Equation 7, the

marginal effect of S_i on Y_i is

$$\delta_i = Y_i(D,1) - Y_i(D,0) = \begin{cases} \beta_2 \text{ if } D_i = 0 \\ \beta_2 + \beta_3 \text{ if } D_i = 1 \end{cases} . \tag{12}$$

The *average* marginal effect of S_i on Y_i can be defined by taking an expectation over Equation 12:

$$\mathbb{E}[\delta_i] = \beta_2 + \mathbb{E}[D_i]\beta_3. \tag{13}$$

As discussed in Section 3, CHB is the average marginal effect of S_i on Y_i. This implies that CHB $= \beta_2 + \mathbb{E}[D_i]\beta_3$. CHB thus does not identify IHB, only identifying a linear combination of IHB with other parameters.

Researchers thus cannot credibly identify IHB in typical hypothetical bias experiments, which only vary S_i. Isolating IHB (β_3) from the CHB parameter estimated in most hypothetical bias experiments ($\beta_2 + \mathbb{E}[D_i]\beta_3$) requires the researcher to know at least two of the three following parameters: β_2, β_3, and $\mathbb{E}[D_i]$. However, $\mathbb{E}[D_i]$ is undefined in an experiment where no D_i is varied. Additionally, the researcher cannot identify β_3 alone without knowing the interaction effect between S_i and D_i, which is not estimable if no D_i is varied. This implies that identifying IHB requires a factorial experiment that varies both intervention D_i and stakes condition S_i in a way that permits unconfounded estimation of these treatments' individual and joint effects on Y_i.

Trying to infer IHB from CHB can yield misleading conclusions, including both magnitude and sign errors. By Equation 13, if $|\beta_2|$ is large and $\beta_3 = 0$, then CHB will be large even though IHB is zero. Likewise, if $\beta_2 = -\mathbb{E}[D_i]\beta_3$, then CHB will be zero no matter how large IHB is. In a similar vein, if β_2 is sufficiently negative, then IHB can be positive while CHB is negative, and if β_2 is sufficiently positive, then IHB can be negative while CHB is positive. In fact, CHB and IHB almost always differ.

Proposition 1. *Whenever* $\beta_3 \neq \frac{\beta_2}{1-\mathbb{E}[D_i]}$*, CHB and IHB differ.*

Proof.

$$\beta_3 \neq \frac{\beta_2}{1 - \mathbb{E}[D_i]}$$

$$\beta_3 \neq \beta_2 + \mathbb{E}[D_i]\,\beta_3$$

$$\mathbb{E}[Y_i(1,1) - Y_i(0,1)] - \mathbb{E}[Y_i(1,0) - Y_i(0,0)] \neq \mathbb{E}[Y_i(D,1) - Y_i(D,0)] \text{ (Equations 9-13)}$$

$$\text{IHB} \neq \text{CHB (Equations 5 and 10)}$$

□

The sufficient condition in Proposition 1 holds almost always, as the interaction effect between an intervention and some moderator is virtually never exactly the same as the average marginal effect of the moderator itself.

Recent research on hypothetical bias in experiments – which focuses almost exclusively on CHB – must be understood in this context. Though Brañas-Garza, Kujal, & Lenkei (2019), Matousek, Havranek, & Irsova (2022), Brañas-Garza et al. (2023), and Hackethal et al. (2023) respectively find no statistically significant CHBs on cognitive reflection test scores, discount rates, time preferences, and risk preferences, this does not imply that hypothetical stakes induce zero bias for any intervention TEs on these outcomes. Further, for a given outcome variable, there is no 'one true' IHB for all interventions, as different interventions likely exhibit different IHBs for the same outcome.

4.2 Treatment Effect Standard Errors: TESEB

Hypothetical bias on TE SEs can be identified in a similar fashion to hypothetical bias on TE point estimates. I parameterize hypothetical bias on TE precision as 'TE SE bias (TESEB)':

$$\text{TESEB} \equiv \mathbb{E}[\text{SE}(\tau(p')) - \text{SE}(\tau(p))]. \tag{14}$$

In practice, point estimates for TESEBs can be obtained by taking the differences in TE SEs between stakes conditions. SEs for TESEB point estimates can be estimated via bootstrapping (see Section 5 for examples).

OSDB does not identify hypothetical biases on TE precision. The best way to show this is through a simple counterexample where OSDB and TESEB have opposite signs. Figure 1 displays data points from two simulated datasets, each of which contain 20 observations. In both datasets, the simulated intervention is assigned such that $D_i = 0$ for $i \in \{1, 2, \cdots 10\}$ and $D_i = 1$ for $i \in \{11, 12, \cdots 20\}$. The first dataset arises from the data-generating process

$$Y_i = \begin{cases} 0.05 + 0.1(i-1) \text{ if } i \in \{1, 2, \cdots 10\} \ \ (D_i = 0) \\ -0.05 + 0.15(i-10) \text{ if } i \in \{11, 12, \cdots 20\} \ \ (D_i = 1) \end{cases}, \tag{15}$$

and the second dataset is constructed using the data-generating process

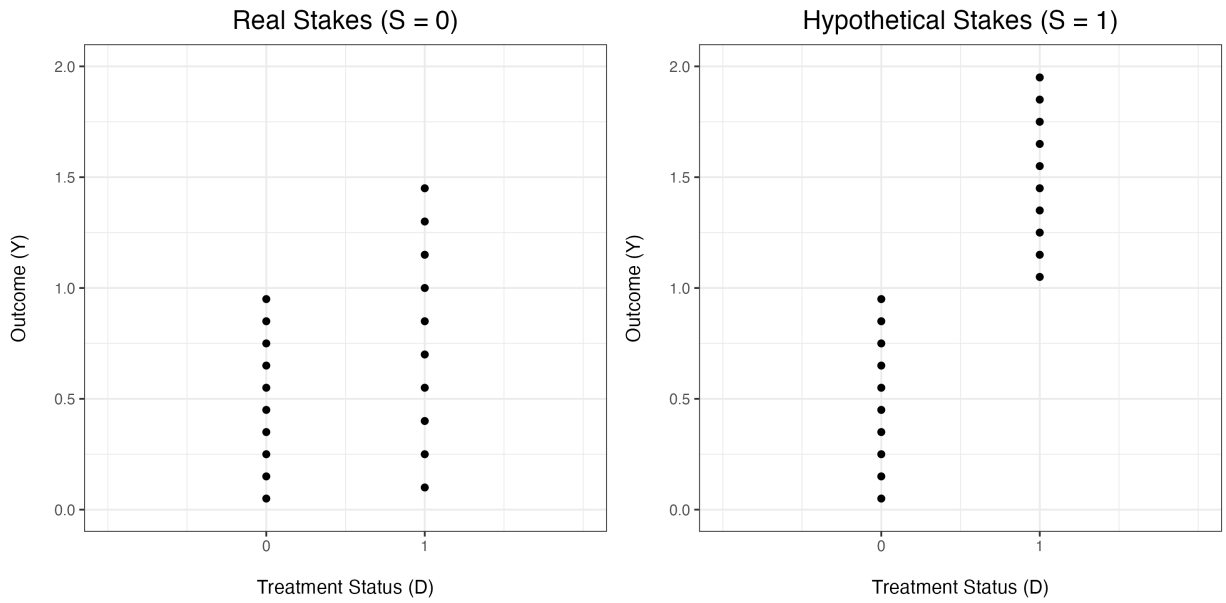
$$Y_i = \begin{cases} 0.05 + 0.1(i-1) \text{ if } i \in \{1, 2, \cdots 10\} \ \ (D_i = 0) \\ 1.05 + 0.1(i-11) \text{ if } i \in \{11, 12, \cdots 20\} \ \ (D_i = 1) \end{cases}. \tag{16}$$

For purposes of exposition, suppose that these two simulated datasets represent two halves of an experimental dataset where D_i and S_i are both randomized. Let the first half of the dataset (generated by the process in Equation 15) belong to a real-stakes sample (i.e., $S_i = 0$), and let the second half of the dataset (generated by the process in Equation 16) belong to a hypothetical-stakes sample (i.e., $S_i = 1$). It is clearly visible from Figure 1 that the outcome SD for the hypothetical-stakes sample (0.592) is higher than that in the real-stakes sample (0.401), so OSDB is positive. However, the TE SE from a simple linear regression model of Y_i on D_i is smaller in the hypothetical-stakes sample (0.135) than in the real-stakes sample (0.173), so TESEB is negative.[5] This example demonstrates that OSDB does not identify TESEB, and that interpreting OSDB estimates as evidence of how hypothetical stakes affect 'noise' in TE estimates can yield misleading conclusions.

4.3 Meta-Analytic Approaches

One approach that hypothetical bias researchers sometimes use to directly estimate IHB is meta-analytically comparing TEs from studies with and without real stakes. For instance,

[5]When HC3 heteroskedasticity-robust SEs are employed (see MacKinnon & White 1985), the TE SE in the hypothetical-stakes sample (0.143) is still smaller than that in the real-stakes sample (0.182).



Note: The graphs plot data points from two simulated datasets. The left graph's data points arise from the data-generating process in Equation 15, whereas the right graph's data points arise from the data-generating process in Equation 16.

Figure 1: An Example Where OSDB and TESEB Hold Opposite Signs

Li, Maniadis, & Sedikides (2021) conduct a meta-analysis of studies investigating anchoring effects on willingness to pay/accept. They find no statistically significant differences between the anchoring effects observed in studies with and without real stakes, and therefore conclude that real stakes have no discernible impact on anchoring effects. A similar approach could be used to estimate TESEBs by comparing meta-analytic averages of TE SEs under different stakes conditions, though Li, Maniadis, & Sedikides (2021) do not make this comparison.

Meta-analyses like this do not provide clean causal estimates of the impact of real stakes, as the choice to incentivize an experiment with real stakes is endogenous. In Equation 11, the identification of IHB as a simple interaction effect between treatment D_i and stakes condition S_i relies on a joint unconfoundedness assumption over both the treatment and the stakes condition, $\mu_i \perp \{D_i, S_i\}$. This is readily achieved *within* a factorial experiment when both the intervention and hypothetical stakes are randomly assigned. However, this unconfoundedness condition is not generally satisfied when comparing TEs *across* experiments, as experimental stakes conditions are typically not randomly assigned, and are likely correlated with other factors that simultaneously influence TEs and their SEs.

One important factor that likely confounds meta-analytic IHB estimates is academic dis-

ciplines. Naturally, some disciplines are more likely to provide real experimental stakes than others, and these disciplines meaningfully differ on various important dimensions, including participant pools and procedural norms in experimentation (see Hertwig & Ortmann 2001; Bardsley et al. 2009). To fix a simple example, consider a meta-analytic dataset where all experiments employing real stakes are run by economists, whereas all experiments employing hypothetical stakes are run by psychologists. Further, suppose that the economics experiments recruit economics students, whereas the psychology experiments recruit psychology students. In order to credibly interpret the difference in TEs between these groups of experiments as a causal effect of hypothetical stakes, one must be willing to assume (among other things) that economics students respond to treatment in the exact same way as psychology students. However, this assumption is untenable. Economics students differ from psychology students in important dimensions, and the same treatment can affect economics students and psychology students in significantly different ways (Van Lange, Schippers, & Balliet 2011; van Andel, Tybur, & Van Lange 2016). Therefore, meta-analytic differences between TEs do not generally provide clean identification of IHBs. For similar reasons, meta-analytic differences between TE SEs do not generally provide clean identification of TESEBs.

5 Empirical Applications

Finding experiments where clean estimates of IHB and TESEB can be respectively compared with CHB and OSDB is challenging. Most hypothetical bias experiments only vary stakes conditions without introducing additional interventions (e.g., see Walker & Smith 1993; Camerer & Hogarth 1999; Hertwig & Ortmann 2001; Harrison & Rutström 2008; Brañas-Garza et al. 2023; Hackethal et al. 2023). This makes it impossible to obtain IHB or TESEB estimates in these studies (see Sections 4.1 and 4.2). Even when experiments vary both an intervention and hypothetical stakes, interaction effects between these treatments are rarely reported. This is likely due to publication bias against null results (see Fanelli 2012; Franco, Malhotra, & Simonovits 2014; Andrews & Kasy 2019; Chopra et al. 2024). Interaction effects such as IHBs are notoriously noisy and difficult to sufficiently power (Muralidharan, Romero, & Wüthrich 2023). Many IHB estimates are thus likely statistically insignificant,

Article	Outcome	Treatment	CHB	IHB	OSDB	TESEB	N
Ceccato et al. (2018)	% of endowment transferred (0-100)	Give (vs. take) framing (0/1)	9.28 (2.575)	-9.547 (5.16)	2.312 (1.55)	-2.39 (1.423)	348
Fang et al. (2021)	Purchasing yogurt (0/1)	Virtual reality (0/1)	0.182 (0.04)	0.049 (0.08)	-0.162 (0.034)	0.579 (1.439)	1024
Enke et al. (2023)	Numerical answer (0-100)	Numerical anchor (0-100)	6.04 (2.338)	0.019 (0.074)	0.985 (1.356)	0.154 (2.052)	626

Note: CHB denotes 'classical hypothetical bias', IHB represents 'interactive hypothetical bias', OSDB denotes 'outcome standard deviation bias', TESEB denotes 'TE SE bias', and N is the effective sample size. SEs are presented in parentheses.

Table 1: Empirical Estimates of Hypothetical Bias Measures

meaning that many likely go unreported. Estimating IHB and TESEB in suitable studies that do not report estimates of these biases requires replication data. However, virtually no published articles provide full data and code unless their journal mandates data-sharing, many data-sharing policies are fairly recent, and many journals still do not mandate data-sharing (Askarov et al. 2023; Brodeur, Cook, & Neisser 2024).

To assess whether TE-irrelevant hypothetical bias measures misidentify TE-relevant hypothetical biases in practice, I re-analyze data from three of the first hypothetical bias experiments I can find that allow for direct estimation of CHB, IHB, OSDB, and TESEB. These studies have publicly available replication data and use factorial designs that simultaneously manipulate both hypothetical stakes and another intervention. The results of my three empirical analyses are presented in Table 1. In the remainder of this section, I provide an overview of each experiment, explain how CHB, IHB, OSDB, and TESEB are computed for each experiment, and discuss how my results show that TE-irrelevant hypothetical bias measures often misidentify TE-relevant hypothetical bias measures.

5.1 Ceccato et al. (2018)

Ceccato et al. (2018) conduct an experiment in which participants play double-anonymous dictator games. Participants are randomly assigned either to a real-stakes room or to a hypothetical-stakes room. Once assigned to a room, participants are randomly seated. Dictators face two envelopes, one titled "Your Personal Envelope" and the other titled "Other Participant's Envelope". Dictators must allocate a five-euro endowment between these two envelopes. Dictators can receive a seat with 'give' framing, where the endowment is initially

stored in "Your Personal Envelope", or a seat with 'take' framing, where the endowment is initially stored in "Other Participant's Envelope". The experiment also takes steps to manipulate the gender of the dictator and the passive player, but for the purposes of this replication, I focus exclusively on the effect of the 'give' framing treatment (compared to the 'take' framing control) on dictator transfers. Replication data for the experiment reported in Ceccato et al. (2018) is provided by Schwieren et al. (2018).

For this experiment, I first estimate IHB in an ordinary least squares model of the form

$$\%\text{Trans}_i = \alpha + \beta_1 \text{Give}_i + \beta_2 S_i + \beta_3 \text{Give}_i S_i + \mu_i.$$

$\%\text{Trans}_i$ is the proportion of the endowment transferred by dictator i (in percentage points), Give_i indicates that dictator i faces the 'give' framing treatment, S_i indicates that dictator i is assigned to a hypothetical-stakes room, and β_3 is the IHB estimate of interest. From this model, I compute CHB using the `avg_slopes()` command in the `marginaleffects` R package to obtain the average marginal effect of S_i on $\%\text{Trans}_i$ (see Arel-Bundock, Greifer, & Bacher 2024). SEs for both CHB and IHB are computed using the HC3 variance-covariance estimator (see Hayes & Cai 2007).

I obtain a point estimate of OSDB by simply subtracting the within-sample SD of $\%\text{Trans}_i$ for dictators assigned to real-stakes rooms from that same SD for dictators assigned to hypothetical-stakes rooms. I then run ordinary least squares models of the form

$$\%\text{Trans}_i = \alpha_H + \tau_H \text{Give}_i + \mu_i, S_i = 1$$
$$\%\text{Trans}_i = \alpha_R + \tau_R \text{Give}_i + \mu_i, S_i = 0.$$

That is, I separately regress $\%\text{Trans}_i$ on Give_i for the dictators facing hypothetical and real stakes (respectively). My TESEB point estimate is simply $\text{SE}(\hat{\tau}_H) - \text{SE}(\hat{\tau}_R)$. To obtain SEs for both OSDB and TESEB, I repeat my procedures for obtaining OSDB and TESEB point estimates on 10,000 bootstrap resamplings of dictators. My SE estimates for OSDB and TESEB are respectively the SDs of the OSDBs and TESEBs from my bootstrap sample.

Table 1 shows that in Ceccato et al. (2018), CHB and IHB exhibit opposite signs. CHB

is significantly positive: hypothetical stakes cause dictators to transfer over nine percentage points more of their endowment to recipients. This is intuitive, as people tend to overstate their generosity when stakes are not real (e.g., see Sefton 1992). However, the IHB for the impact of 'give' framing on endowment transfers is *negative*, and is even larger in magnitude than the CHB on endowment transfers (though this IHB is quite imprecise).

OSDB and TESEB also take on opposite signs in Ceccato et al. (2018). The dispersion of endowment transfers is larger when stakes are hypothetical. However, the SE of the 'give' framing's TE on endowment transfers is smaller when stakes are hypothetical. That said, estimates of both OSDB and TESEB are imprecise for this experiment.

5.2 Fang et al. (2021)

Fang et al. (2021) examine whether virtual reality marketplaces can reduce hypothetical bias in choice experiments. Participants choose between purchasing an original strawberry yogurt, a light strawberry yogurt, or neither product. Participants are randomized into one of five between-participant conditions. The first is a hypothetical-stakes condition where participants make product choices based on photos of the products. In the second and third conditions, participants choose between the products based on nutritional labels, with one condition employing hypothetical stakes and the other using real stakes. In the fourth and fifth conditions, participants make product decisions in a virtual reality supermarket. Stakes are real in one of these two virtual reality conditions whereas stakes are hypothetical in the other. Once randomized to a condition, each participant makes purchase decisions four times, each time facing a different price menu.

Because it is the primary target of the Fang et al. (2021) experiment, I focus on the effect of virtual reality on the decision to purchase. I estimate IHB in a panel data random effects model of the form

$$\text{Buy}_{i,p} = \alpha + \beta_1 \text{VR}_i + \beta_2 S_i + \beta_3 \text{VR}_i S_i + \mu_{i,p},$$

where i indexes the participant and p indexes the price menu. I code $\text{Buy}_{i,p}$ as a dummy indicating that participant i chooses to purchase either the original or light yogurt when

facing price menu p, VR_i as a dummy indicating that participant i faces one of the two virtual reality treatments, and S_i as a dummy indicating that participant i is facing one of the three conditions with hypothetical stakes. As in my re-analysis of Ceccato et al. (2018), β_3 is the IHB parameter of interest, and I compute CHB using the `avg_slopes()` command in the `marginaleffects` R suite to obtain the average marginal effect of S_i on $\text{Buy}_{i,p}$. SEs for both IHB and CHB are computed using the HC3 variance-covariance estimator, with SEs clustered at the participant level.

As in my re-analysis of Ceccato et al. (2018), I obtain a point estimate of OSDB by subtracting the SD of $\text{Buy}_{i,p}$ for the sample facing real-stakes conditions from that same SD for the sample facing hypothetical-stakes conditions. I run random effects panel data models of the form

$$\text{Buy}_{i,p} = \alpha_H + \tau_H \text{VR}_i + \mu_{i,p},\ S_i = 1$$
$$\text{Buy}_{i,p} = \alpha_R + \tau_R \text{VR}_i + \mu_{i,p},\ S_i = 0$$

and compute the TESEB point estimate as $\text{SE}(\hat{\tau}_H) - \text{SE}(\hat{\tau}_R)$. To estimate SEs for OSDB and TESEB, I repeat the procedures to obtain point estimates for OSDB and TESEB in 10,000 cluster bootstrap samples (where participants i, instead of rows $\{i, p\}$, are resampled with replacement). I respectively compute the SEs of OSDB and TESEB as the SDs of the OSDB and TESEB point estimates in my bootstrap sample.

Table 1 shows that TE-irrelevant hypothetical bias measures yield completely different conclusions than TE-relevant hypothetical bias measures in Fang et al. (2021). CHB is significantly positive in this experiment: hypothetical stakes increase participants' likelihood of choosing to purchase one of the two yogurts by 18 percentage points. This reflects the intuitive and well-documented fact that people often overstate their willingness to pay when stakes are hypothetical (see List 2001; Murphy et al. 2005; Harrison & Rutström 2008; Hausman 2012). However, the IHB estimate in this experiment is less than one third the size of the CHB estimate, and is not statistically significantly different from zero.

OSDB and TESEB take on opposite signs in Fang et al. (2021). Hypothetical stakes significantly decrease the dispersion of purchase decisions, decreasing the SD of $\text{Buy}_{i,p}$ by

over 16 percentage points. However, the TESEB estimate is positive, and is roughly 3.5 times larger than the OSDB estimate. Despite its larger size, the TESEB estimate is too imprecise to yield confident statistical significance conclusions.

5.3 Enke et al. (2023)

Enke et al. (2023) investigate hypothetical biases for a variety of commonly-elicited experimental outcomes. Participants first complete two out of four possible tasks without any real stakes at play. Participants are then randomized in between-participants fashion to either a low-stakes or high-stakes condition where stakes are real to repeat these same two tasks. For three of the four tasks, no interventions are implemented.[6] For these three tasks, it is not possible to estimate IHB or TESEB. However, Enke et al. (2023) also examine the impact of stakes in an anchoring context, where there is a clear TE to examine (i.e., the anchoring effect). It is thus possible to estimate IHB and TESEB in the anchoring task.

Participants facing the anchoring task in Enke et al. (2023) must answer two of four randomly-assigned numerical questions whose answers range from 0-100.[7] Each participant receives an anchor, constructed using the first two digits of their birth year and the last digit of their phone number. For each anchoring question, participants are first asked whether the numerical answer to the question is greater than or less than their anchor, and thereafter must provide an exact numerical answer to the question. The first anchoring question is answered with no real stakes at play. The second anchoring question is answered for (probabilistically) real stakes: participants can earn a monetary bonus if their answer to the question is within two points of the correct answer.[8] My replication of Enke et al. (2023) focuses only on the sample facing the anchoring task. To get as close as possible to examining extensive-margin effects of real vs. hypothetical stakes, I exclude participants subjected to the high-stakes

[6]These outcomes include scores on the cognitive reflection test, answers for a base rate neglect question, and answers for a contingent reasoning question.

[7]Questions include "Is the time (in minutes) it takes for light to travel from the Sun to the planet Jupiter more than or less than ANCHOR minutes?" and "Is the population of Uzbekistan as of 2018 greater than or less than ANCHOR million?" See Appendix B.3 in Enke et al. (2023).

[8]Enke et al. (2023) employ a probabilistic incentivization strategy where only one of the two tasks completed by each participant is randomly selected to be payoff-relevant. Participants facing the anchoring task only receive a bonus for the anchoring task if their answer is sufficiently accurate *and* the anchoring task is selected as payoff-relevant.

treatment. Replication data for Enke et al. (2023) is provided by Enke et al. (2021).

Estimation procedures for Enke et al. (2023) closely mirror those for Fang et al. (2021). IHB is computed in a panel data random effects model of the form

$$\text{Answer}_{i,c} = \alpha + \beta_1 \text{Anchor}_i + \beta_2 S_c + \beta_3 \text{Anchor}_i S_c + \mu_{i,c},$$

where i indexes the participant and c indexes the stakes condition. Anchor_i represents participant i's anchor and S_c is a dummy indicating that the participant is answering the first anchoring question, where no real stakes are at play. After estimating this model, I use the `avg_slopes()` command in the `marginaleffects` R suite to compute CHB as the average marginal effect of S_c on $\text{Answer}_{i,c}$. SEs for both IHB and CHB are computed using the HC3 variance-covariance estimator, with SEs clustered at the participant level.

I compute the OSDB point estimate by subtracting the SD of numerical answers to questions faced without real stakes from the same SD for questions faced when real stakes are at play. I then run random effects panel data models of the form

$$\text{Answer}_{i,c} = \alpha_H + \tau_H \text{Anchor}_i + \mu_{i,c}, \;\; S_c = 1$$
$$\text{Answer}_{i,c} = \alpha_R + \tau_R \text{Anchor}_i + \mu_{i,c}, \;\; S_c = 0$$

and obtain TESEB point estimate $\text{SE}(\hat{\tau}_H) - \text{SE}(\hat{\tau}_R)$. As in my re-analysis of Fang et al. (2021), I then re-estimate the OSDB and TESEB point estimates in 10,000 cluster bootstrap samples. SEs of OSDB and TESEB are respectively computed as the SDs of the OSDB and TESEB point estimates in the bootstrap sample.

My replication of Enke et al. (2023) shows that TE-irrelevant hypothetical bias measures can misidentify TE-relevant hypothetical bias not just in terms of qualitative conclusions, but also in scale. The CHB estimate is significantly positive: participants appear to offer numerical answers roughly six points higher (out of 100) when stakes are real. However, the IHB estimate is minuscule in comparison, and is not statistically significantly different from zero. This partially reflects the fact that hypothetical stakes and numerical anchors impact numerical answers at completely different scales. It makes sense that a one-point

increase in a 0-100 numerical anchor will have a relatively small impact on numerical answers compared to a binary switch from real-stakes to hypothetical-stakes conditions. Likewise, the TESEB estimate is less than one sixth the size of the OSDB estimate, which may reflect similar differences in scale (though both OSDB and TESEB estimates are imprecise here). Similar scale differences may emerge between TE-relevant and TE-irrelevant hypothetical bias measures in many other experimental settings.

6 Discussion

6.1 Practical Implications of Hypothetical Bias Research

The practical reason why a researcher would like to be able to use statistically insignificant CHBs to 'rule out' hypothetical bias for a given experimental outcome is clear: some researchers would like to be able to run cheaper intervention experiments by omitting real stakes for experimental choices. Many researchers see hypothetical bias studies that report insignificant CHB as evidence that this methodological choice is justified, trusting these studies when they report that real stakes 'do not matter' for eliciting certain outcomes. For example, at time of writing, Web of Science reports that Brañas-Garza et al. (2021) and Brañas-Garza et al. (2023) already have 36 unique citations between them. One third of these articles are citing Brañas-Garza et al. (2021) or Brañas-Garza et al. (2023) as justification to use data from hypothetical-stakes experiments.

However, this interpretation is not justified. My identification results in Section 4 and my empirical results in Section 5 show that TE-irrelevant hypothetical bias measures (namely CHB and OSDB) can wildly misidentify TE-relevant hypothetical bias measures (specifically IHB and TESEB, respectively). The finding that CHB for a particular outcome is not statistically significantly different from zero does not imply that all (or any) treatments targeting that outcome will exhibit negligible IHB. Researchers cannot credibly argue that real stakes 'do not matter' in their intervention experiment unless they have *a priori* knowledge that both IHB and TESEB will be negligible across all combinations of interventions and outcomes in their experiment. Given the limited research on IHB and TESEB in the current

literature, it is unlikely that researchers possess this knowledge *a priori* when running an hypothetical-stakes experiment.

6.2 How Useful Is This Research Agenda?

The usefulness of hypothetical bias studies depends in part on their ability to inform researchers about whether it is 'safe' to omit real incentives in their intervention experiments. Part of the reason why recent hypothetical bias studies have gained traction is because their findings have been misinterpreted as being widely applicable. For example, it is cost-effective to run a hypothetical bias experiment on time preferences if statistically insignificant CHB estimates on time preferences in one experiment truly mean that omitting real stakes does not matter for all future experiments that examine (TEs on) time preferences.

However, results from individual hypothetical bias studies are not widely portable to other intervention experiments. Hypothetical biases for one intervention's TE on a given outcome will almost never be exactly the same as the hypothetical biases for another intervention's TE on that outcome. Likewise, hypothetical biases for one intervention's TEs on a given outcome are almost never the same as the hypothetical biases for that intervention's TEs on any other outcome. For an older study's findings on IHB and TESEB concerning outcome Y_i and intervention D_i to be relevant for a newer experiment, that newer experiment must use the same Y_i and D_i under similar experimental conditions. Therefore, unless outcome Y_i is often combined with treatment D_i across experiments, evidence on IHB for the TE of D_i on Y_i is not likely to be relevant for future experiments. Even in this case, there is no guarantee that statistically insignificant IHB in one experiment will translate into statistically insignificant IHB in another experiment.

6.3 Statistical (In)significance

Even if a researcher has evidence that all hypothetical biases relevant to their experiment are not statistically significantly different from zero, this is still not credible evidence that hypothetical stakes have negligible consequences for their TE estimates. Much of the current hypothetical bias literature mistakenly interprets *statistically insignificant* hypothetical bias

estimates as evidence of *practically negligible* hypothetical bias. This is a widely-known misinterpretation of statistical (in)significance, which can yield high Type II error rates if applied generally (see Altman & Bland 1995; Wasserstein & Lazar 2016; Fitzgerald 2024). Further, statistically insignificant hypothetical biases can still change experimental conclusions, as the difference between a statistically significant estimate and a statistically insignificant estimate is not always statistically significant itself (Gelman & Stern 2006).

Misinterpreting statistically insignificant hypothetical bias as *ipso facto* evidence of practically negligible hypothetical bias yields a particularly high risk of Type II errors for TE-relevant hypothetical bias measures, which tend to be considerably underpowered. For example, IHBs are interaction effects, which are notoriously imprecise. In a simple heterogeneous treatment effects framework, if a main effect is sufficiently powered with N observations, and the interaction effect is half the size of the main effect, then it will take $8N$ observations to sufficiently power that interaction effect (Muralidharan, Romero, & Wüthrich 2023).

This property can be observed in my empirical results. For instance, Table 1 shows that the CHB on endowment transfers in Ceccato et al. (2018) is statistically significant, with a t-statistic exceeding 3.5. However, even though the IHB estimate for the framing effect on endowment transfers is larger than this CHB estimate, the IHB estimate is not statistically significant because its standard error is double that of the CHB estimate. If one considers this experiment's CHB estimate to be practically significant, then it is inconsistent to simultaneously regard the IHB estimate as negligible simply because it is less precisely estimated. Additionally, the SEs of OSDBs and TESEBs in my replication results are quite imprecise, which suggests that similar power issues may also affect OSDB and TESEB estimates.

6.4 Probabilistic Incentivization

Though the discussion so far implies that current norms favoring the use of real stakes for experimental choices protect against potential hypothetical biases that are difficult to detect through standard tests, there is a counterpoint: these norms can have exclusionary effects on scholars who lack sufficient research funding (Bardsley et al. 2009). This limitation contributes to the overrepresentation of researchers and samples from Western, educated, industrialized, rich, and democratic (WEIRD) countries in the published experimental economics

literature (see Henrich, Heine, & Norenzayan 2010). Given that TEs observed in WEIRD countries do not always generalize in non-WEIRD countries, this exclusionary consequence partially decreases the generalizability of TEs observed in the experimental economics literature (Henrich, Heine, & Norenzayan 2010).

One meaningful change in methodological norms that would decrease costs while still potentially maintaining the external validity provided by real stakes is disciplinary permission to use probabilistic incentivization. This involves honestly informing all participants that only a randomly-selected subset of their experimental choices will have real-world consequences, and/or that only a randomly-selected subset of the sample will face real-world consequences for their choices. Probabilistic incentivization has gained traction in recent years, and has been highlighted in recent methodological recommendations (see Charness, Gneezy, & Halladay 2016; Voslinsky & Azar 2021).

Unfortunately, the empirical literature on probabilistic incentivization faces the same limitations as the empirical hypothetical bias literature. Principally, most experiments on probabilistic incentivization vary no interventions other than stakes conditions, and only report evidence of CHB (see March et al. 2016; Clot, Grolleau, & Ibanez 2018; Anderson et al. 2023; Umer 2023). My identification results in Section 4 demonstrate that identifying TE-relevant hypothetical biases from probabilistic incentivization requires factorial experiments that vary both the intervention(s) of interest and stakes conditions. Further, conclusions on hypothetical biases from probabilistic incentivization experiments are only relevant for the specific combinations of interventions and outcomes tested in those experiments. This makes experiments on probabilistic incentivization just as uninformative for future experiments as traditional hypothetical bias experiments (see Section 6.2). Finally, as I discuss in Section 6.3, the statistical insignificance of hypothetical bias estimates in one study is not credible evidence that hypothetical biases are practically negligible. This is true regardless of whether these hypothetical bias estimates come from experiments on probabilistic incentivization or from experiments on completely omitting real stakes.

Instead of waiting on costly empirical evidence on hypothetical biases in probabilistically-incentivized experiments that will probably be uninformative anyways, it is likely more productive for experimental economics to just establish explicit norms accepting probabilistically-

incentivized experiments. This is not a significant departure from current practice. Many experimental economists already operate under the implicit understanding that probabilistic incentivization creates decision frames for participants that ensure externally-valid treatment effects. For example, the seminal Holt & Laury (2002) multiple price list for risk preference elicitation employs probabilistic incentivization. For participants in real-stakes conditions, only one of the ten lottery choices is randomly selected to be played out for real stakes. This multiple price list is in widespread use, with Web of Science reporting over 2900 citations on Holt & Laury (2002) at time of writing. Thousands of TE estimates on risk aversion parameters, and thousands of other TE estimates from models that control for risk aversion, rely on the probabilistically-incentivized Holt & Laury (2002) multiple price list or subsequent adaptations. Any economist confident in the generalizability of these TEs should be similarly confident in the generalizability of TE estimates from other probabilistically-incentivized experiments. This is a context where norms, rather than empirics, will provide better guidance for experimental practice. Norms in favor of probabilistic incentivization accommodate incentivization schemes that strike a balance between ensuring externally-valid experimental TEs and making experimental economics more accessible to scholars around the world.

7 Conclusion

This paper shows that the recent hypothetical bias literature does not justify abandoning real stakes for experimental choices in most modern experiments. I provide a new taxonomy of experiments, distinguishing between 'elicitation experiments' where TEs are not of interest and 'intervention experiments' where TEs are of interest. I show econometrically and empirically that though classical hypothetical bias measures identify relevant hypothetical biases in elicitation experiments, they can wildly misidentify TE-relevant hypothetical biases in intervention experiments. Traditional methods for investigating hypothetical bias thus typically produce results that are uninformative for future experimental practice, and may mislead researchers about the consequences of omitting real stakes in their experiments.

Experimental economics' norms in favor of providing real stakes for experimental choices are useful for ensuring that experimental TEs are externally valid. Researchers can often sub-

stantially reduce the costs of running experiments by completely omitting real stakes. However, the experimental economics literature is rich with examples where real stakes meaningfully impact TEs on human decision-making. To provide a recent example, Campos-Mercade et al. (2024) find that stated and revealed preferences for vaccination strongly positively correlate. However, they find that though the impact of donation-based incentives on stated vaccination preferences is significantly negative, the impact of the same treatment on actual vaccination behavior is significantly positive.

Given that 'incentives matter' is one of the fundamental tenets of economics, it is useful for experimental economists to presume that stakes conditions may meaningfully impact experimental TEs, and thus to functionally require real stakes for experimental choices before experimental TEs are trusted. These real stakes may be provided for all participants and all experimental choices, or for a randomly-chosen subset of participants and/or tasks. What is important is that experimental participants make choices with the expectation that these choices may have real-world consequences. This ensures that behaviors observed in experiments are more reflective of people's behavior in the real world.

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