Incomparability and Status Quo

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PRELIMINARY. NEWER VERSION COMING SOON

Abstract

Status quo bias is one of the leading robust behaviors that challenge standard economic modeling, but its source has been debated in theoretical and experimental studies. We provide experimental evidence that accounting for incomplete preferences is crucial in diagnosing SQB. We develop an identification strategy that bounds the behavior of a very general class of complete preferences and uses violations of these bounds as indication of incomparability. Implementing this strategy in a laboratory experiment, we find that incomplete preferences are widespread, with roughly one third of subjects exhibiting incomparability. These subjects are significantly more likely to retain the status quo when facing incomparable alternatives, and display substantially higher status quo bias than subjects whose choices are consistent with complete preferences. Furthermore, we demonstrate that ignoring preference incompleteness is empirically consequential: once subjects with incomplete preferences are excluded, the evidence against the Coase Theorem is no longer robust, and the case for the Endowment Effect largely disappears.

Keywords: Incomplete Preference, Riskless Choice, Experiment, Status Quo Bias, Endowment Effect

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1 Introduction

The completeness axiom is a maintained assumption in almost all economic research on choice. Yet completeness is an empirically meaningful restriction: it requires that every pair of alternatives be comparable and that the decision maker be always decisive. If some alternatives are instead incomparable, standard inference from choice data may be misleading. Whether ignoring incomplete preferences is consequential depends on two questions: How prevalent are incomplete preferences in economically relevant choice environments, and how do individuals with incomplete preferences behave when forced to choose?

In the current paper, we ask two related questions: how prevalent are incomplete preferences in a riskless choice environment, and how do subjects choose between a status quo and an incomparable alternative? The second question has a natural theoretical logic: when an agent cannot rank two alternatives, she has no well-founded basis for abandoning her current position in favor of the alternative. Status quo retention in this case is conceptually distinct from loss aversion. Empirical studies on incomplete preferences in riskless environments are scarce, and the second question has not been evaluated empirically.

Identifying incomplete preferences based on choice data alone is conceptually challenging. Our approach is indirect and does not rely on any assumed behavioral signature of incompleteness (e.g., choice deferral or delegation). Instead, we set-identify choices that are consistent with completeness and very weak normative assumptions, namely monotonicity in money and dominance-transitivity (which is significantly less demanding than full transitivity). We interpret choices that do not abide by these bounds as indicating incomparability. Because our focus is on riskless choice, we are also free from the confound between incompleteness and convex preferences that complicates identification in the domain of imprecise beliefs (Halevy et al., 2025; Cettolin and Riedl, 2019; Agranov and Ortoleva, 2025).

We implement this identification strategy in a controlled preregistered laboratory experiment in which subjects face choices over bundles of cookies, socks, and money — goods chosen for their attributional dissimilarity, which maximizes the potential for genuine incomparability. To identify incomparability within each pair of bundles, we employ two choice list questions. In the first, Bundle A – containing cookies and money, is held fixed, while the subject chooses between it and a varying option containing the same number of socks as Bundle B plus a varying monetary payment; the switching point estimates the minimum monetary compensation that makes Bundle B at least as attractive as Bundle A. In the second MPL, Bundle B is held fixed, and the varying option contains the same cookies as Bundle A with increasing monetary payment; the switching point estimates the analogous compensation in the opposite direction. Under any complete preference satisfying our ax-

ioms, the two switching points must satisfy a consistency condition: loosely speaking, the two switching points must reflect a consistent preference between bundle A and bundle B. Violations of this condition cannot be rationalized by any complete preference in our class and constitute our identification of incomparability. To study choices with a status quo, subjects additionally face two-stage exchange problems adapted from Dean et al. (2017) and Samuelson and Zeckhauser (1988), in which one bundle is randomly assigned as the status quo and subjects choose whether to retain it or trade it for the alternative.

The experimental data shows that at the aggregate level, 11.7%–19.9% of observations are inconsistent with the restrictions, indicating incomparability. At the individual level, 27.1%–34.6% of subjects exhibit incomparability in at least one pair — a substantial share that cannot be dismissed as noise. Subjects facing an incomparable alternative retain the status quo at rates of 58.9%–61.9%, statistically significant above the 50% benchmark implied by random selection. Subjects who exhibit incomparability in at least one pair display status quo bias that is 15.5–20.6 percentage points higher than subjects without incomparability. Three bodies of auxiliary evidence support the interpretation that these violations reflect genuine incompleteness rather than decision errors or confusion: subjects who exhibit violations take a longer time to make their decisions, report lower confidence, and perform no differently on comprehension quizzes than subjects who do not violate the bounds. Furthermore, the elevated status quo retention among incomparable subjects is not accompanied by a symmetric increase in switching away from a strictly preferred status quo, an asymmetry that is difficult to reconcile with stochastic choice but is consistent with genuine incomparability.

These findings carry methodological implications for experimental tests of theories. We illustrate this through Kahneman et al. (1990), who interpret the insufficient trade as evidence of loss aversion and the endowment effect. This interpretation rests on an implicit assumption that all subjects hold complete and well-defined preferences over the objects being traded. Once this assumption is relaxed, status quo retention admits a third explanation alongside rational preference and loss aversion. Namely, that the subject simply cannot rank the status quo against the alternative. In our data, this third case is of comparable empirical magnitude to the case where the status quo is kept against a preferred alternative. When we restrict attention to subjects with complete preferences, the evidence against the Coase Theorem is no longer robust, weakening the case for loss aversion and the endowment effect. This suggests that a non-trivial share of the low trading rate documented by Kahneman et al. (1990) could be attributed to incomplete preferences rather than the endowment effect.

The remainder of the paper is organized as follows. Section 2 reviews the related literature. Section 3 presents the theoretical framework, including the key axioms and iden-

tification propositions. Section 4 describes the experimental design and implementation. Section 5 reports the main results. Section 6 presents robustness checks. Section 7 applies our findings to reexamine the experimental evidence on the endowment effect and the Coase Theorem. Section 8 concludes. Additional theoretical results are provided in the Supplemental Appendix.

2 Literature Review

This paper is related to three strands of literature.

Theoretical literature on incomplete preferences Generally, this paper is related to the theoretical literature on incomplete preferences. Some works model behavior when preferences are incomplete (e.g., Bewley, 2002; Karni, 2024). Some works study the foundations and mathematical properties of incomplete preferences (e.g., Aumann, 1962; Ok, 2002; Dubra et al., 2004; Ghirardato et al., 2004; Nau, 2006; Gilboa et al., 2010; Ok et al., 2012; Galaabaatar and Karni, 2013; Faro, 2015; Riella, 2015). This paper does not build on a specific model of incomplete preferences, but focuses on exploring the testable implications for very general classes of complete preferences in the domain of riskless choice. A related theoretical approach to identifying incomplete preferences is provided by Eliaz and Ok (2006).

Experimental evidence on incomplete preferences. A small but growing experimental literature has sought to identify incomplete preferences directly from choice data. Cettolin and Riedl (2019) and Halevy et al. (2025) study incomplete preferences when beliefs are imprecise, that is in the domain of ambiguity. Agranov and Ortoleva (2025) study deliberate randomization and conjecture that incompleteness may be related to it. Cettolin and Riedl (2019) and Agranov et al. (2020) use choice list which allow mixing of two options in a row. Similarly, Halevy et al. (2025) use a modified BDM which allows subjects to report a non-degenerate interval. A key challenge in the domain of uncertainty is that incomplete preferences and complete but convex preferences can generate observationally similar choice patterns. Cettolin and Riedl (2019) use willingness-to-pay for randomization to differentiate a general ambiguity-aversion preference and incomplete preferences. Halevy et al. (2025) use binary bets and the probability equivalents for two complementary events to distinguish very general class of complete preferences and incomplete preference. Because our paper focuses on riskless choice, we are free from this confound and need not distinguish incomplete preferences from convex preferences. Analogously to Halevy et al. (2025), we identify incomplete

preferences by bounding the behavior of a very general class of complete preferences and using violations of those bounds as identification; our bounds are derived from completeness, dominance transitivity–a weaker requirement than regular transitivity–and monotonicity in money.

Other literature uses choices deferral as a behavior signature to identify incomplete preferences (e.g., Costa-Gomes et al., 2022; Danan and Ziegelmeyer, 2006; Gerasimou, 2026). Costa-Gomes et al. (2022) and Gerasimou (2026) use both forced and non-forced (choice deferral) treatments. Costa-Gomes et al. (2022) only allows single choice and use the greater consistency in the non-forced treatment as evidence for hesitation or genuine indecisiveness. Gerasimou (2026) extend this design by allowing subjects to select multiple alternatives simultaneously, which enables the data to distinguish between indifference and indecisiveness. Danan and Ziegelmeyer (2006) elicit ranges of certainty equivalents under a design that permits choice deferral, interpreting intervals that cannot be collapsed to a single certainty equivalent as evidence of indecisiveness under risk. The use of choice deferral introduces some alternative explanations, such as preference for flexibility, learning of preference, etc. Nielsen and Rigotti (2026) elicit incomplete preferences over monetary gambles using a non-forced choice environment, in which subjects are permitted to decline a choice rather than being compelled to select one of the available alternatives and being paid by an algorithm unknown to the subjects. However, their design is not incentive compatible.[1]

Our experiment addresses the limitations of existing designs. We employ an incentive-compatible mechanism that does not impose any assumption about how subjects with incomplete preferences behave, such as choice deferral as a behavioral signature of incompleteness. Instead, we bound the behavior rationalizable by very general classes of complete preferences and use violations of those bounds as indicating incompleteness.

Endowment Effect and Status Quo Bias A classical interpretation of the endowment effect and status quo bias is that they manifest loss aversion (e.g., Knetsch, 1989; Kahneman et al., 1990; Tversky and Kahneman, 1991). There is a growing body of literature that explores alternative sources other than loss aversion.

A strand of theoretical literature establishes that incomparability between alternatives can rationalize choice avoidance and status quo retention. Bewley (2002) develops a model of decision making under Knightian uncertainty in which the agent accepts a deviation from the status quo only if it yields a higher expected utility for every prior in a set of beliefs, embedding inertia directly into the preference structure. Mandler (2004) argues

[1]In their paper, they mention that the design is not theoretically incentive compatible but behavioral incentive compatible.

that status quo maintenance need not reflect loss aversion or psychological attachment to the current position, but can instead be rationalized by incomplete preferences: an agent retains her status quo whenever no available alternative is unambiguously superior to it across all dimensions of evaluation. Masatlioglu and Ok (2005) develop a revealed-preference model and integrates SQB into the model by expanding the domain to problems with or without a designated status quo and by allowing the underlying preference to be incomplete. Masatlioglu and Ok (2014) extends this line of work by axiomatizing choice behavior in which the presence of a status quo imposes psychological constraints on the feasible set, leading to status quo bias naturally. Gerasimou (2016) proposes a model of partially dominant choice in which an alternative is selected only if it is not worse than anything in the menu and strictly better than at least one other option, generating systematic choice avoidance when alternatives are incomparable. More recently, Ip and Nei (2025) model trade-off aversion as a source of indecision, showing that aversion to making trade-offs produces choice avoidance and choice overload, and that self-awareness of this tendency can mitigate its consequences. Our paper complements this theoretical literature by providing direct empirical evidence on the relationship between incomparability and status quo retention.

Tversky and Shafir (1992) suggests that the difficulty of choice leads to inertia. They show that when options are difficult to compare because competing attributes create internal conflict, individuals disproportionately defer or stick with the status quo. The "difficulty of choice" can be interpreted as incomplete preference. Dean (2008) complements this by showing that SQB strengthens as choice problems become harder (e.g., with larger or more complex menus), consistent with the idea that increasing comparison difficulty raises the likelihood of incompleteness. Our experiment design generates "hard" tradeoffs in minimal menus (pairs) to isolate incompleteness from choice overload.

Meanwhile, the endowment effect can rise when preference is complete without loss aversion. In Cerreia-Vioglio et al. (2024), agents are uncertain about trade-offs between goods and entertain a set of utility functions, evaluating options by their worst-case monetary equivalent. This caution generates the endowment effect, loss aversion, and the certainty effect within a unified framework. The model completes an underlying incomplete preference ranking through a cautious completion without assigning a special role to the status quo and produces endowment effect when the alternatives are comparable without invoking asymmetric valuation of gains and losses. Dean et al. (2017) offer an alternative, attention-based rationalization: defaults influence consideration rather than underlying valuations, so status-quo choices can arise even with complete preferences when the status quo is more likely to enter the consideration set. Our paper contributes to the literature by empirically demonstrating that status quo retention may reflect incomplete preference rather than

classical loss aversion.

3 Theory

Consider an individual who consumes three goods. Two of the goods are to be interpreted as two different consumption goods while the other is a composite commodity, e.g. money. In the experiment, we choose cookies and socks as the consumption good. Bundles are denoted $A = (a_1, a_2, a_3)$, $B = (b_1, b_2, b_3)$, $C = (c_1, c_2, c_3)$, and so on, with $A, B, C \in \mathbb{Z}^+ \times \mathbb{Z}^+ \times \mathbb{R}$. For a generic bundle, the first component denotes the number of cookies, the second denotes the number of socks, and the third denotes an amount of money. We denote the set $\mathbb{Z}^+ \times \mathbb{Z}^+ \times \mathbb{R}$ by $\mathcal{C}$. Money is divisible and continuous, cookies and socks are indivisible and discrete. Let $\succsim$ be a binary relation on $\mathcal{C}$ representing a binary relation over bundles. As usual, define the $\sim$ and $\succ$ the symmetric and asymmetric parts of $\succsim$, respectively.

The approach we take to revealing incompleteness is indirect: we identify choices that are consistent with a very general class of complete preferences and interpret choices that violate these identifying restrictions as indicating incompleteness. Specifically, we assume that the decision maker's preferences are strictly monotone in money and transitive when restricted to dominant bundles. For simplicity, in the body of the paper we assume continuity in money. We relax the latter assumption in the appendix.

Axiom 1 (Completeness)**.** $\forall A = (a_1, a_2, a_3), B = (b_1, b_2, b_3) \in \mathcal{C}$, either $A \succsim B$ or $B \succsim A$ or both.

Axiom 2 (Dominance Transitivity)**.** For any three bundles $A = (a_1, a_2, a_3)$, $B = (b_1, b_2, b_3)$, $C = (c_1, c_2, c_3)$ such that $a_1 \geq b_1$, $a_2 \geq b_2$, and $a_3 \geq (>) b_3$:

1. If $B \succsim C$, then $A \succsim (\succ) C$.
2. If $C \succsim A$, then $C \succsim (\succ) B$.[2]

Axiom 3 (Continuity in Money)**.** For any $(a_1, a_2), (b_1, b_2) \in \mathbb{Z}^2$ and $b_3 \in \mathbb{R}$, the upper and lower contour sets $\{a \in \mathbb{R} : (a_1, a_2, a) \succsim (b_1, b_2, b_3)\}$ and $\{a \in \mathbb{R} : (b_1, b_2, b_3) \succsim (a_1, a_2, a)\}$ are closed.

Corollary 1 (Monotonicity in Money)**.** *Under Axiom 1(Completeness) and Axiom 2 (Dominance Transitivity), for any* $(x_1, y_1, z_1), (x_1, y_1, z_2) \in \mathbb{C}$ *with* $z_1 \geq (>) z_2$, $(x_1, y_1, z_1) \succsim (\succ)(x_1, y_1, z_2)$.

The *Tradeoff Equivalent (TE)* measures the tradeoff between two consumption goods in monetary terms. Intuitively, $TE_{12}(a_1, b_2, a_3)$ is the monetary amount that, together with

[2]Part (2) follows from part (1) together with Axiom 1 (Completeness).

b_2 units of the second good, makes the decision maker indifferent between this bundle and holding a_1 units of the first good with a_3 in money. Symmetrically, $TE_{21}(a_1, b_2, b_3)$ is the monetary amount that, together with a_1 units of the first good, makes the decision maker indifferent between this bundle and holding b_2 units of the second good and b_3 in money.

Definition 1 (Tradeoff Equivalent)**.** For any $(a_1, 0, a_3), (0, b_2, b_3) \in \mathbb{C}$:

(i) $TE_{12}(a_1, b_2, a_3)$ is the monetary value of trading off first consumption goods for the second consumption goods, defined implicitly by $(a_1, 0, a_3) \sim (0, b_2, TE_{12}(a_1, b_2, a_3))$; [3]

(ii) $TE_{21}(a_1, b_2, a_3)$ is the monetary value of trading off the second consumption good for the first consumption good, defined implicitly by $(0, b_2, b_3) \sim (a_1, 0, TE_{21}(a_1, b_2, b_3))$.

Proposition 1. *If $\succsim$ satisfies Axiom 1-3,*

1. $TE_{12}(a_1, b_2, a_3) \geq (>) b_3 \iff TE_{21}(a_1, b_2, b_3) \leq (<) a_3$.
2. $TE_{12}(a_1, b_2, a_3) \leq (<) b_3 \iff TE_{21}(a_1, b_2, b_3) \geq (>) a_3$.

Proof. Start with the proof of the first bullet.
$\Longrightarrow$ If $TE_{12}(a_1, b_2, a_3) \geq b_3$, since $(a_1, 0, a_3) \sim (0, b_2, TE_{12}(a_1, b_2, a_3))$, by dominance transitivity: $(a_1, 0, a_3) \succsim (0, b_2, b_3)$. Therefore, by definition of $TE_{21}(a_1, b_2, b_3)$, $a_3 \geq TE_{21}(a_1, b_2, b_3)$. If $TE_{12}(a_1, b_2, a_3) > b_3$, by dominance transitivity $(a_1, 0, a_3) \succ (0, b_2, b_3)$, $a_3 > TE_{21}(a_1, b_2, b_3)$.
$\Longleftarrow$ If $TE_{12}(a_1, b_2, a_3) \leq b_3$, by dominance transitivity: $(0, b_2, b_3) \succsim (a_1, 0, a_3)$. Therefore, by definition of $TE_{21}(a_1, b_2, b_3)$, $a_3 \leq TE_{21}(a_1, b_2, b_3)$. If $TE_{12}(a_1, b_2, a_3) > b_3$, by dominance transitivity $(a_1, 0, a_3) \succ (0, b_2, b_3)$, implying that $a_3 > TE_{21}(a_1, b_2, b_3)$. The proof for the second bullet is analogous to the first one. ■

Identification of Incomplete Preference We interpret violations of Proposition 1 as indicating incomplete preferences under a weak auxiliary assumption of dominance transitivity, which is evaluated separately in the experiment. Incomparability denoted as: $(a_1, 0, a_3) \bowtie (0, b_2, b_3)$. The absence of violations is not equal to complete preference. Decision makers with incomplete preferences may nonetheless make choices consistent with Propositions 1. As a result, violations of the proposition provide sufficient but not necessary evidence of incompleteness, and the identified prevalence should be interpreted as a lower bound under the maintained assumptions.

[3] Axiom of continuity ensures that the minimal of the set $\{b \in \mathbb{R}_+ | (0, b_2, b) \succsim (a_1, 0, a_3)\}$ and the maximum of the set $\{b \in \mathbb{R}_+ | (a_1, 0, a_3) \succsim (0, b_2, b)\}$ are well-defined. Axiom of dominance transitivity ensures that $min\{b \in \mathbb{R}_+ | (0, b_2, b) \succsim (a_1, 0, a_3)\} \geq max\{b \in \mathbb{R}_+ | (a_1, 0, a_3) \succsim (0, b_2, b)\}$. Axiom of completeness ensures that $min\{b \in \mathbb{R}_+ | (0, b_2, b) \succsim (a_1, 0, a_3)\} = max\{b \in \mathbb{R}_+ | (a_1, 0, a_3) \succsim (0, b_2, b)\}$. Therefore, $TE_{12}(a_1, b_2, a_3))$ is well-defined. The same holds true for $TE_{21}(a_1, b_2, b_3)$.

4 Experiment Design and Implementation

There are three parts in the experiment. The order of the three parts is randomized. The order of questions within each part is also randomized. The incentive is RIS(random incentive system). The subjects are told that one of the questions will be selected for payment. We use 3 pairs of bundles: $A = (a_1, 0, a_3) \quad v.s. \quad (0, b_2, b_3)$. Table 1 shows the specifications of the 3 pairs.

Pair	**Bundle A**			**Bundle B**		
	x (cookies)	y (socks)	z (money)	x (cookies)	y (socks)	z (money)
1	3	0	9.75	0	2	10.25
2	3	0	10.5	0	2	9.5
3	3	0	9.5	0	1	10.5

Table 1: Specification of 3 Pairs

We designed choice pairs to maximize the likelihood of incomparability between bundles, thereby increasing our power to detect incomplete preferences. Pairs 1 and 2 both comprise bundles of 3 cookies and 2 pairs of socks, differing only in which bundle receives more monetary compensation (Bundle B in Pair 1, Bundle A in Pair 2). Pair 3 compares 3 cookies to 1 pair of socks. Since preliminary evidence indicates subjects generally prefer 3 cookies to 1 pair of socks, we assign the monetary compensation to Bundle B to counterbalance this preference differential and preserve potential incomparability.

In Part 1, we employ six choice list questions to elicit $TE_{12}(a_1, b_2, a_3)$ and $TE_{21}(a_1, b_2, b_3)$ across all three pairs and identify incomparability between bundles within each pair. Consider $TE_{12}(a_1, b_2, a_3)$ as an example. The fixed option $(a_1, 0, a_3)$ remains constant across rows. In each row, the decision maker (DM) chooses between two options: $(a_1, 0, a_3)$ and $(0, b_2, b)$, where b takes values from an ascending list. A random row r is selected, and payment is determined by the DM's choice in that row. The switching point provides an estimate of $TE_{12}(a_1, b_2, a_3)$.

Because the list is discrete rather than continuous, we cannot elicit $TE_{12}(a_1, b_2, a_3)$ precisely. Instead, we identify its infimum and supremum, $\underline{TE_{12}}(a_1, b_2, a_3)$ and $\overline{TE_{12}}(a_1, b_2, a_3)$, as defined in Definition 2. Specifically, the last row in which the fixed option is chosen estimates $\underline{TE_{12}}(a_1, b_2, a_3)$, while the first row in which the alternative option is chosen estimates $\overline{TE_{12}}(a_1, b_2, a_3)$.[4]

[4]By definition, the last row where the fixed option is chosen provides a lower bound for $\underline{TE_{12}}(a_1, b_2, a_3)$, and the first row where the alternative option is chosen provides an upper bound for $\overline{TE_{12}}(a_1, b_2, a_3)$. In practice, we identify incomplete preferences using Proposition 2 in the appendix, which are equivalent

By eliciting $TE_{12}(a_1, b_2, a_3)$ and $TE_{21}(a_1, b_2, b_3)$, we are able to identify violations of the Proposition 1[5]. In addition, we are able to elicit the preference between bundle A and bundle B. [6] We record the response time for each choice list question based on the timestamp of the final click. We add a comprehension quiz before the choice list questions. Each subject needs to answer the comprehension quiz correctly before they move on to answer the real questions. After each choice list question, subjects are asked to report their confidence in their choice on a scale from 0% to 100%.

Notice that in Pairs 1 and 2, the physical goods are identical—3 cookies versus 2 pairs of socks — differing only in the monetary amounts assigned to each bundle. Because the underlying goods are the same, the tradeoff equivalent $TE_{12}(a_1, b_2, a_3)$ and $TE_{21}(a_1, b_2, b_3)$ elicited across these two pairs pertain to the same preference ordering over the same consumption goods, we can use either $TE_{12}(a_1, b_2, a_3)$ and $TE_{21}(a_1, b_2, b_3)$ elicited within each pair or $TE_{12}(a_1, b_2, a_3)$ and $TE_{21}(a_1, b_2, b_3)$ elicited across pair 1 and 2 to constitute violations of Proposition 1. To see this concretely: if $TE_{12}(a_1, b_2, a_3)$ and $TE_{21}(a_1, b_2, b_3)$ within Pair 1 violate Proposition 1, we identify $(3, 0, 9.75) \bowtie (0, 2, 10.25)$; if $TE_{12}(a_1, b_2, a_3)$ from Pair 1 and $TE_{21}(a_1, b_2, b_3)$ from Pair 2 violate Proposition 1, we identify $(3, 0, 9.75) \bowtie (0, 2, 9.5)$; and if $TE_{12}(a_1, b_2, a_3)$ from Pair 2 and $TE_{21}(a_1, b_2, b_3)$ from Pair 1 violate Proposition 1, we identify $(3, 0, 10.5) \bowtie (0, 2, 10.25)$. Each case identifies a distinct pair of bundles as incomparable. Cross-pair comparison genuinely extends the scope of incomparability detection beyond what within-pair comparisons alone can reveal.

Based on the pattern of violations, we categorize incomplete preferences into two types. For Pairs 1 and 2, the shared goods structure admits both within-pair and cross-pair comparisons, giving rise to two distinct notions of incomparability:

1. **Narrow Incomparability** (Pairs 1 and 2): the indicator equals one if $TE_{12}(a_1, b_2, a_3)$ and $TE_{21}(a_1, b_2, b_3)$ elicited *within* the same pair violate Proposition 1, identifying incomparability under a single monetary configuration.
2. **Broad Incomparability** (Pairs 1 and 2): the indicator equals one if $TE_{21}(a_1, b_2, a_3)$ and $TE_{yx}(a_1, b_2, b_3)$—whether drawn from the same pair or across Pairs 1 and 2—

to Proposition 1 under relaxed continuity assumptions. This approach ensures that the identification of incomplete preferences is conservative and guarantees a difference of at least two rows.

[5] An additional design feature strengthens our identification strategy. The monetary amounts in the two bundles of each pair are positioned on opposite sides of $10.00, the midpoint of each choice list. If subjects employ midpoint heuristics—systematically selecting the midpoint as their switching point—their choices would automatically conform to Proposition 1. Consequently, any observed violations of these propositions cannot be attributed to midpoint-based decision-making, providing clean identification of incomplete preferences.

[6] The monotonicity is imposed by the interface. For instance, if a subject chooses the right option in one row, the interface will automatically choose the right option in every row below. Similarly, if a subject chooses the left option in one row, the interface will automatically choose the left option in every row above.

violate Proposition 1. This captures incomparability across the full range of monetary configurations sharing the same underlying goods.

By construction, Narrow Incomparability implies Broad Incomparability, but not vice versa. A subject flagged under Broad but not Narrow Incomparability appears decisive within each individual monetary configuration, yet reveals inconsistency when their valuations are compared across configurations with different monetary allocations, suggesting that incomparability is more sensitive to the specific monetary environment in which preferences are elicited. For Pair 3, only within pair violations are possible by construction, since no counterpart pair shares the same underlying goods. Therefore, the narrow incomparability and broad incomparability are identical in Pair 3.

To the best of our knowledge, no prior experiments identify incomparability in the domain of deterministic products. Accordingly, this analysis is exploratory, and the observed proportion cannot be directly compared to existing estimates. We report the prevalence of incomparability in this setting as descriptive evidence.

Part 2 contains 3 exchange questions to measure choices given a status quo. To create a status quo for the subjects, we adopt the two-stage design from Dean (2008) and Dean et al. (2017) where the choice in the first stage serves as the status quo in the second stage. In the first stage of each question, half of the DMs will be asked to choose among $A = (a_1, 0, a_3)$, $A' = (a_1, 0, a_3 - 0.25)$, and $A'' = (a_1 - 1, 0, a_3)$, which are Bundle A and the dominated variants of Bundle A. We expect most of them to choose A. The other half of the DMs will be asked to choose among $B = (0, b_2, b_3)$, $B' = (0, b_2, b_3 - 0.25)$, and $B'' = (0, b_2 - 1, b_3)$, which are the Bundle B and the dominated varatiants of Bundle B. We expect most of them to choose B. The assignment of first stage menu is randomized across subjects and questions. In the second stage, they will be asked if they want to keep their choice in stage 1, or switch to Bundle B if Bundle A is in the first stage, or switch to Bundle A if Bundle B is in the first stage. The choice in the first stage creates a status quo for subjects. Therefore, we can observe choices under status quo. In addition, given that the preferences between Bundle A and B are elicited in part 1, we are able to study the correlation between incomplete preferences and the choice in settings with the status quo.

We have two preregistered hypotheses related to incomplete preference and status quo retention.

- **H1:** DMs are more likely to keep the status quo when the alternative is incomparable with the status quo. That is, Pr(keep SQ | incomparable) > 0.5.
- **H2:** Subjects with ≥ 1 incomparable pair exhibit higher status quo bias than those with no incomparable pair.

The status quo bias is measured using the probability of choosing the status quo despite strictly preferring the alternative. Therefore, $Pr(\text{keep SQ} \mid \text{strictly prefer the alternative})$ is higher for subjects who have incomplete preferences in at least one pair.

Hypothesis 1 concerns the role of incomparability in shaping choices when no preference exists. Prediction 2 instead examines whether incomplete preferences are associated with a greater tendency to choose the status quo even when a strict preference for the alternative is revealed, which corresponds to the classic definition of status quo bias.

Part 3 consists of nine binary choice problems. For each pair of bundles, subjects make three binary choices. The first menu is $A = (a_1, 0, a_3)$ versus $B = (0, b_2, b_3)$. The second menu is $A = (a_1, 0, b_3)$ versus $B' = (0, b_2, b_3 + 0.25)$, and the third menu is $A' = (a_1, 0, a_3 + 0.25)$ versus $B = (0, b_2, b_3)$. These three choices are designed to test dominance transitivity directly.

For example, if a subject chooses A in the first menu but switches to B in the second menu, this pattern constitutes a violation of dominance transitivity. A caveat is that observed violations of dominance transitivity may reflect underlying incomparability rather than genuine failures of transitivity. The measurement of dominance transitivity violations is therefore inherently confounded with incomparability. Nevertheless, unlike the choice list questions in Part 1, monotonicity is not imposed by the interface in the binary choice question: subjects are free to choose either option in every menu. Therefore, the binary choice question is a more straightforward test of dominance transitivity. The design allows us to distinguish the primary source of violations of Proposition 1. If failures of dominance transitivity mainly drive such violations of proposition 1, we should also observe violations of dominance transitivity.

The experiment is pre-registered at Open Science Framework in September 2025. We conducted 12 sessions in September and October at the Toronto Experimental Economics Lab(TEEL). Subjects are mainly undergraduate students from the University of Toronto. 214 subjects attended.

5 Experiment Results

5.1 Proportion of Incomparability

We use violations of Proposition 1 to identify incomparability. Table 2 summarizes the proportion of violations at the aggregate level. Out of 642 observations, 128 (19.9%) exhibit Global Violations and 75 (11.7%) exhibit Local Violations. Table 3 summarizes violations at the individual level. Among 214 subjects, 74 (34.6%) exhibit Global Violations in at least

one pair, and 58 (27.1%) exhibit Local Violations in at least one pair.

	N (%)
strictly prefers Bundle A	210 (32.7%)
strictly prefers Bundle B	291 (45.3%)
Indifference	13 (2.0%)
Global Violation	128 (19.9%)
Local Violation	75 (11.7%)

Notes: Percentages are calculated relative to the full sample.

Table 2: Proportion of Violations of Propositions 1 and 2 (Aggregate Level)

5.1.1 Dominance Transitivity

Dominance transitivity is the weak auxiliary assumption in our identification of incomplete preference. As discussed in the previous section, we use three binary choices to test dominance transitivity. Violations of dominance transitivity in the binary choice question can shed light on the primary source of violations of Proposition 1 followed in the choice list questions.

Tables 4 and 5 summarize violations of dominance transitivity by whether Proposition 1 is violated globally. At the aggregate level, 128 out of 642 observations (19.9%) violate Proposition 1, out of which only 20 (3.1%) observations violate dominance transitivity. At the individual level, 74 out of 214 subjects (34.6%) violate Proposition 1, out of which only 22 (10.3%) violate dominance transitivity. Restricting attention to the 603 observations satisfying monetary monotonicity, 108 (17.9%) still exhibit Global Violations. A similar pattern holds at the individual level. The proportion of violations of Proposition 1 decreases only marginally after excluding non-monotonic observations, confirming that violations of Proposition 1 are not primarily driven by failures of dominance transitivity.

	Local Violation	Global Violation
No Violation	156 (72.9%)	140 (65.4%)
Violations in at least 1 pair	58 (27.1%)	74 (34.6%)
Violations in 1 pair	44 (20.6%)	32 (15.0%)
Violations in 2 pairs	11 (5.1%)	30 (14.0%)
Violations in 3 pairs	3 (1.4%)	12 (5.6%)

Notes: Percentages are calculated relative to the full sample.

Table 3: Proportion of Violations of Propositions 1 (Individual Level)

Taken together, these results indicate that violations of dominance transitivity account for only a small share of the violations of Proposition 1.

	Transitive	Intransitive
No Violations	495 (77.1%)	19 (3.0%)
Global Violation	108 (16.8%)	20 (3.1%)

Notes: Percentages are calculated relative to the full sample. Column 1 includes observations that satisfy dominance transitivity. Column 2 includes observations that violate dominance transitivity.

Table 4: Tabulation of Violations of Proposition 1 and Dominance Transitivity – Aggregate

	Transitive	Intransitive
No Violations	126 (58.9%)	14 (6.5%)
Global Violation	52 (24.3%)	22 (10.3%)

Notes: Percentages are calculated relative to the full sample. Column 1 includes individuals who satisfy dominance transitivity in all the 3 pairs. Column 2 includes individuals who violate dominance transitivity in at least one pair.

Table 5: Tabulation of Violations of Proposition 1 and Dominance Transitivity – Individual

5.1.2 Monotonicity

Dominance transitivity and completeness imply strict monotonicity in money. Therefore, we do the same exercise to address the violation of monotonicity in money. Monotonicity in money can be tested using the first stage of the exchange question, as described in the experimental design.

Tables 6 and 7 summarize violations of monotonicity by whether Proposition 1 is violated globally. 19 out of 128 observations that violate Proposition 1 globally also violate monotonicity in money. Similarly, 13 out of 74 subjects that violate Proposition 1 globally also violate monotonicity in money.

Restricting attention to the 602 observations satisfying monetary monotonicity, 109 (18.1%) still exhibit Global Violations. A similar pattern holds at the individual level. The proportion of violations decreases only marginally after excluding non-monotonic observations, confirming that violations of Proposition 1 are not primarily driven by failures of monetary monotonicity.

	Monotonic	Non-monotonic
No Violations	493 (76.8%)	21 (3.3%)
Global Violation	109 (17.0%)	19 (3.0%)

Notes: Percentages are calculated relative to the full sample. Column 1 includes observations that satisfy monotonicity in money. Column 2 includes observations that violate monotonicity in money.

Table 6: Tabulation of Violations of Proposition 1 and Monotonicity in Money – Aggregate

	Monotonic	Non-monotonic
No Violations	130 (60.7%)	10 (4.7%)
Global Violation	61 (28.5%)	13 (6.1%)

Notes: Percentages are calculated relative to the full sample. Column 1 includes individual that satisfy monotonicity in money in all 3 pairs. Column 2 includes individuals who violate monotonicity in money in at least one pair.

Table 7: Tabulation of Violations of Proposition 1 and Monotonicity in Money – Individual

5.1.3 Auxiliary Evidences- Decision-making Time, Self-reported confidence level, Comprehension Quiz

To strengthen the identification of incomplete preference via the violation of Proposition 1 further, we examine some auxiliary measurements, including decision-making time, self-reported confidence levels, and comprehension Quiz results. We find supporting evidence.

Decision Time We record the decision-making time of each choice list question. [7] Table 8 reports differences in response time by violation status of Proposition 1, at both the aggregate and individual level.

At the aggregate level, observations exhibiting Local or Global Violations spend 2.80–5.89 more seconds on each choice list question relative to non-violating observations. The upper bound of 5.89 seconds is statistically significant, representing a 19.7% increase over observations without violations. At the individual level, subjects exhibiting Local or Global Violations in at least one pair spend 8.24–8.37 more seconds per pair relative to comparable subjects without violations, corresponding to an increase of 28.6%–29.7%. These patterns

[7] We require subjects to spend at least 30 seconds on each *MPL* question. Therefore, the time spent on each *MPL* page may overestimate actual deliberation time. Instead, we use the timestamp of the final mouse click in the choice list to measure the time spent on each *MPL* question.

are consistent with the interpretation that the identified violations reflect genuine difficulty in comparison the two bundles.

	Dependent Variable: Response Time			
	Aggregate Level		Individual Level	
	(1)	(2)	(3)	(4)
Narrow Incomparability	2.80		8.24***	
	(2.39)		(2.30)	
broad incomparability		5.89***		8.37***
		(2.22)		(2.15)
Constant	30.7***	29.9***	28.8***	28.1***
	(1.07)	(1.07)	(1.15)	(1.18)
Observations	642	642	214	214

Significance: * 10%, ** 5%, *** 1%.

Table 8: Response Time of *MPL* questions

Self-reported Confidence After each choice list question, subjects report their confidence level on a scale from 0 to 100. Since incomplete preferences are closely related to difficulty in ranking alternatives or low confidence in choice, we compare self-reported confidence levels between violation types.

At the aggregate level, the average confidence level for observations without violations ranges from 83.9 to 84.2 percentage points. Observations exhibiting Local or Global Violations report significantly lower confidence, with a reduction of 3.72–4.47 percentage points; both are statistically significant. At the individual level, subjects exhibiting Local or Global Violations in at least one pair report a statistically significant reduction in confidence of 4.28–4.77 percentage points relative to subjects without violations. These patterns corroborate the response time evidence: the identified violations are associated with greater decision difficulty, consistent with the interpretation that they reflect genuine incomparability rather than random decision errors.

Comprehension Quiz To address the concern that violations of Proposition 1 may be driven by a lack of understanding of the choice list mechanism, we compare comprehension quiz performance between violation types. Every subject is required to pass the comprehension quiz to proceed. Some pass on the first attempt, while others take more attempts. Passing on the first attempt can serve as a measurement of their understanding of the payment mechanism. Table 10 reports the first-attempt pass rate on the comprehension quiz,

	Dependent Variable: Self-reported Confidence			
	Aggregate Level		Individual Level	
	(1)	(2)	(3)	(4)
Narrow Incomparability	-4.47**		-4.28*	
	(2.18)		(2.46)	
broad incomparability		-3.72**		-4.77**
		(1.86)		(2.30)
Constant	83.9***	84.2***	84.6***	85.1***
	(1.12)	(1.18)	(1.28)	(1.35)
Observations	642	642	214	214

Significance: * 10%, ** 5%, *** 1%.

Table 9: Self-reported Confidence of *MPL* questions

separately by violation status. Across both Local and Global Violation classifications, first-attempt pass rates are similar between violating and non-violating subjects, clustering around 90% in all cases. This null result suggests that the observed violations are unlikely to reflect confusion about the choice list mechanism, lending further credibility to the interpretation that they capture genuine incomparability.

	Global Violation	Local Violation
No Violation	91.7%	91.4%
Violations in at least 1 pair	89.7%	90.5%

Notes: Percentages are calculated relative to the sample in each cell.

Table 10: Percentage of Pass Comprehension Quiz at First Attempt

Taken together, these results alleviate concerns that the observed violations of Proposition 1 are driven by failures of monotonicity, failures of dominance transitivity, or a misunderstanding of the choice list mechanism. Subjects who violate the propositions show similar comprehension quiz performance to those who do not, indicating a comparable understanding of the task. At the same time, the lower self-reported confidence and longer decision times among violating subjects suggest that these choices are more difficult, consistent with the interpretation of incomplete preferences. Overall, the evidence supports the view that the observed violations reflect genuine preference incompleteness rather than noise or confusion. For the remainder of the paper, we refer to these patterns simply as "incomparabilities" or "incomplete preferences," rather than as violations of Proposition 1.

5.2 Association Between Incomparability and Status Quo

We observe choices under the status quo using a second-stage exchange question. Given that we can infer preference or lack of preference through responses to the choice list question[8], we are able to study the relationship between the two.

5.2.1 Hypothesis 1

Hypothesis 1 predicts that, when the alternative and the status quo are incomparable, individuals will choose the status quo with probability greater than 0.5. Table 11 summarizes the proportion of choices in which the status quo option was retained in the second stage of the exchange questions, conditional on the alternative and the status quo being incomparable. Each row represents a different categorization of incomparability. Under Narrow Incomparability, the proportion of status quo retention is 58.9%, significant at the 10% level. Under Broad Incomparability, the proportion is 61.9%, significant at the 1% level. Both are statistically significantly greater than 50% based on a one-sided binomial test, consistent with Prediction 1.

There is heterogeneity across the two forms of incomparability: Broad Incomparability is associated with a higher rate of status quo retention than Narrow Incomparability. At first glance, this pattern seems counterintuitive. By definition, the gap between Broad and Narrow Incomparability consists of observations that exhibit only a global violation but no local violation, and one might expect that observations exhibiting both local and global violations would display the stronger effect.

We propose a possible explanation. A closer look at the bundle specifications reveals that bundle A in Pair 1, $(3, 0, 9.75)$, and bundle B in Pair 2, $(0, 2, 9.5)$, are very close in monetary value, differing mainly in the consumption goods offered. Similarly, bundle A in Pair 2, $(3, 0, 10.5)$, and bundle B in Pair 1, $(0, 2, 10.25)$, are also very close in monetary value, again differing mainly in consumption goods. The incomparability identified via the two tradeoff equivalents across Pair 1 and Pair 2 therefore tends to capture incomparability arising from the difficulty of comparing two consumption goods directly, rather than incomparability arising from comparing money against a consumption good. More generally, the difficulty of comparing two bundles may stem from two distinct sources: difficulty in comparing two consumption goods against each other, and difficulty in comparing money

[8]If a subject selects the option with fewer cookies or socks in the first stage of the exchange question, we cannot reliably infer their preference or incomparability between the status quo and the alternative from their choice list responses. Since this section focuses on the relationship between incomparability and status quo retention, we restrict the analysis to the subsample of observations in which the dominant choice or the option with less money was selected in the first stage.

against consumption goods. It is plausible that status quo retention is stronger when incomparability arises from the consumption-good tradeoff than from the money-good tradeoff, which would account for the pattern above: because Broad Incomparability disproportionately captures cross-pair (consumption-good) incomparability, while Narrow Incomparability captures within-pair (both money-good and consumption-good) incomparability, the former exhibits a stronger status-quo effect even though it is not the subset satisfying both criteria simultaneously.

	proportion	one-sided p-value	one-sided 90 % CI	N
Narrow Incomparability	0.589*	0.080	[0.507,∞)	73
Broad Incomparability	0.619***	0.005	[0.559,∞)	126

Significance: * 10%, ** 5%, *** 1%. Column 1 reports the proportion of choices in which the status quo option was selected in the second stage of the exchange questions, conditional on the alternative and the status quo being incomparable. Column 2 reports the p-value from a one-sided binomial test with the alternative hypothesis $H_1 : p > 0.5$. Column 3 reports the corresponding 90% one-sided Clopper–Pearson confidence interval. Column 4 reports the sample size within each category of incomparability.

Table 11: Proportion of Choosing Status Quo under Incomparability

5.2.2 Hypothesis 2

Hypothesis 2 states that individuals who exhibit incomparability in at least one choice pair are more likely to display status quo bias than individuals who do not. Table 12 summarizes the difference in status quo bias between these two groups. Group 1 consists of individuals who exhibit incomparability in at least one pair, while Group 0 includes individuals who do not exhibit incomparability in any pair. Column 1 reports the proportion of choices in which the status quo was retained in the second stage of the exchange questions, conditional on the alternative being strictly preferred to the status quo, for subjects in Group 1. Column 2 reports the corresponding proportion for Group 0, and Column 3 reports the difference between the two groups. Each row represents a different categorization of incomparability.

Across both categorizations, individuals in Group 1 exhibit higher status quo bias than those in Group 0. Under Narrow Incomparability, the difference is 15.5 percentage points, with a baseline status quo bias of 20.6% for Group 0. Under Broad Incomparability, the difference is 20.6 percentage points, with a baseline of 17.7% for Group 0. Both differences are statistically significant, at the 5% and 1% levels respectively, and are substantial in magnitude relative to the baseline.

Given that each individual makes multiple decisions, a simple t-test fails to account for repeated observations. We therefore estimate a logit model at the aggregate level, where

	proportion (Group 1)	proportion (Group 0)	difference in propotion	one-sided p-value	one-sided 90 % CI	N
Narrow Incomparability	0.361	0.206	0.155**	0.021	[0.058,∞)	235
Broad Incomparability	0.383	0.177	0.206***	0.000	[0.127,∞)	235

Significance: * 10%, ** 5%, *** 1%. Column 4 reports the p-value from a one-sided t-test with the alternative hypothesis $H_1 : p_1 > p_0$. Column 5 reports the corresponding 90% one-sided confidence interval. Column 6 reports the number of observations in which the alternative is strictly preferred to the status quo in the second stage of the exchange questions.

Table 12: Status Quo Bias by Completeness of Preference – Aggregate Level

the outcome is an indicator for whether the status quo is retained, restricting the sample to observations in which the alternative strictly dominates the status quo, with standard errors clustered at the individual level.

Table 13 reports the results. Column (1) uses an indicator for Narrow Incomparability in at least one pair as the explanatory variable; Column (2) uses an indicator for Broad Incomparability in at least one pair.[9] Coefficients are reported as odds ratios. Across both specifications, individuals who exhibit incomparability in at least one pair display significantly higher levels of status quo bias, with odds ratios of 2.178 and 2.888 under Narrow and Broad Incomparability, respectively. Both coefficients are statistically significant. Taken together, these results are consistent with Hypothesis 2.

Again, there is heterogeneity across the two forms of incomparability: Broad Incomparability has a stronger association with status quo bias than Narrow Incomparability. The same explanation applies here.

5.2.3 Consistency Between Choice List Question and Binary Choice Question

Hypothesis 2 draws on both the preferences elicited via the choice list questions and the choice behavior observed in the exchange questions. Because these two elicitation methods differ, a natural concern is that the choice list questions may introduce systematic bias or error into the elicited preferences, thereby leading to an incorrect conclusion. We address this concern by examining a within-subject tabulation of the preferences elicited in the choice list questions against subjects' choices in the second stage of the exchange question. We find that subjects with a clear preference in the choice list question act on that preference in the exchange question.

Table 14 reports the within-observation tabulation between the preference inferred from the choice list questions and the subject's choice between the status quo and the alternative

[9] We use the full sample of responses in the choice list questions to construct the incomparability indicators at the individual level.

	Dependent Variable: Status Quo Bias	
	(1)	(2)
Narrow Incomparability=1	2.178*	
	(0.904)	
Broad Incomparability=1		2.888***
		(1.001)
Constant	0.259***	0.215***
	(0.0502)	(0.0467)
Observations	235	235

Significance: * 10%, ** 5%, *** 1%. Standard errors in parentheses. Standard errors are clustered at the individual level. Odds ratios are shown as coefficients.

Table 13: Association between Incomparability and Status Quo Bias at Individual Level

in the second stage of the exchange question. Each row corresponds to one of the four categories identified in the choice list —strictly prefers Bundle A, strictly prefers Bundle B, indifference, and incomplete preference—and reports, for that category, the number and row percentage of subjects who subsequently chose Bundle A versus Bundle B in the exchange question.

The tabulation shows that the two elicitation methods are highly consistent for subjects with a strict preference. Among subjects inferred to strictly prefer Bundle A in the choice list, 88.3% subsequently chose Bundle A in the exchange question; among those inferred to strictly prefer Bundle B, 82.5% subsequently chose Bundle B. By contrast, subjects classified as indifferent show a much more even split between the two choices (58.1%/41.9%), close to what one would expect if the choice list question had not identified a clear strict preference for these subjects. This pattern is consistent with the choice list elicitation capturing meaningful information about subjects' underlying preferences, rather than reflecting systematic bias or elicitation error.

	Choose Bundle A	Choose Bundle B
Strictly prefers Bundle A	182 (88.3%)	24 (11.7%)
Strictly prefers Bundle B	49 (17.5%)	231 (82.5%)
Indifference	25 (58.1%)	18 (41.9%)
Incomplete preference	37 (59.7%)	25 (40.3%)

Percentages are calculated relative to the sample size in each row.

Table 14: Association between Incomparability and Status Quo Bias at Individual Level

6 Robustness Checks

We conduct two robustness checks to validate our main findings: (1) analyzing a subsample that excludes observations violating dominance transitivity, and (2) analyzing a subsample that excludes observations violating monotonicity in money.

6.1 Subsample Excluding Violations of Dominance Transitivity

Our main analysis retains all observations, including those that violate dominance transitivity in the binary choice questions. In this robustness check, we exclude such observations and examine whether the positive association between incomparability and status quo retention persists. If the main results remain qualitatively unchanged, this provides evidence that violations of dominance transitivity do not drive our findings.

Table 15 summarizes the proportion of choices in which the status quo option was retained, conditional on the alternative and the status quo being incomparable. Under Narrow Incomparability, the proportion is 60.3%, significant at the 10% level. Under Broad Incomparability, the proportion is 62.3%, significant at the 1% level. Both are comparable to those reported in Table 11 (58.9% and 61.9%, respectively), and both remain statistically significantly greater than 50% based on one-sided binomial tests, consistent with Prediction 1.

Table 16 summarizes the difference in status quo bias between individuals who exhibit incomparability in at least one pair (Group 1) and those who do not (Group 0). Under Narrow Incomparability, the difference is 11.8 percentage points, significant at the 10% level, with a baseline status quo bias of 16.8% for Group 0. Under Broad Incomparability, the difference is 12.0 percentage points, significant at the 5% level, with a baseline of 15.8% for Group 0. Both differences are comparable in magnitude to those reported in Table 12 (15.5 and 20.6 percentage points, respectively). [10]

Table 17 reports the results of the logit regressions. The odds ratio under Narrow Incomparability is 1.986 but is not statistically significant, partly due to reduced sample size. Under Broad Incomparability, the odds ratio is 2.046, significant at the 10% level, and comparable to the 2.888 reported in Table 13. In sum, both Prediction 1 and Prediction 2 remain supported after excluding observations that violate dominance transitivity, confirming that our main results are not driven by such violations.

[10] Given that the indicators of incomparability are defined at the individual level in the tests for Prediction 2, we exclude individuals who violate dominance transitivity in at least one pair.

	proportion	one-sided p-value	one-sided 90 % CI	N
Narrow Incomparability	0.603*	0.074	[0.511,∞)	58
Broad Incomparability	0.623***	0.007	[0.556,∞)	106

Significance: * 10%, ** 5%, *** 1%. Removes observations that violate dominance transitivity. Corresponds to Table 11.

Table 15: Proportion of Choosing Status Quo under Incomparability

	proportion (Group 1)	proportion (Group 0)	difference in propotion	one-sided p-value	one-sided 90 % CI	N
Narrow Incomparability	0.286	0.168	0.118*	0.093	[0.004,∞)	194
Broad Incomparability	0.278	0.158	0.120**	0.047	[0.028,∞)	194

Significance: * 10%, ** 5%, *** 1%. Removes observations that violate dominance transitivity. Corresponds to Table 12.

Table 16: Status Quo Bias by Completeness of Preference – Aggregate Level

	Dependent Variable: Status Quo Bias	
	(1)	(2)
Narrow Incomparability=1	1.986	
	(1.124)	
Broad Incomparability=1		2.046*
		(0.874)
Constant	0.201***	0.188***
	(0.0415)	(0.0423)
Observations	194	194

Significance: * 10%, ** 5%, *** 1%. Standard errors in parentheses. Standard errors are clustered at the individual level. Odds ratios are shown as coefficients. Removes observations that violate dominance transitivity. Corresponds to Table 13.

Table 17: Association between Incomparability and Status Quo Bias at Individual Level

6.2 Subsample Excluding Violations of Monotonicity in Money

Our main analysis retains all observations, including those that violate monotonicity in money in the first stage of the exchange questions. In this robustness check, we exclude such observations and examine whether the positive association between incomparability and status quo retention persists. If the main results remain qualitatively unchanged, this provides evidence that violations of monotonicity do not drive our findings.

Table 18 summarizes the proportion of choices in which the status quo option was re-

tained, conditional on the alternative and the status quo being incomparable. Under Narrow Incomparability, the proportion is 56.5%, but is not statistically significant ($N = 62$). Under Broad Incomparability, the proportion is 57.9%, significant at the 10% level, and comparable to the 61.9% reported in Table 11. The pattern supporting Prediction 1 thus persists for Broad Incomparability in this subsample, while the result for Narrow Incomparability loses significance due to reduced statistical power.

Table 19 summarizes the difference in status quo bias between Group 1 and Group 0. Under Narrow Incomparability, the difference is 2.4 percentage points but is not statistically significant. Under Broad Incomparability, the difference is 10.4 percentage points, significant at the 10% level, with a baseline status quo bias of 15.7% for Group 0. The Broad Incomparability result remains comparable in magnitude to the 20.6 percentage point difference reported in Table 12.[11]

Table 20 reports the results of the logit regressions. The odds ratio under Narrow Incomparability is 1.167 and is not statistically significant. Under Broad Incomparability, the odds ratio is 1.900, significant at the 10% level, and comparable to the 2.888 reported in Table 13. The loss of significance for Narrow Incomparability across all three tests is consistent with reduced statistical power following sample restriction, rather than a reversal of the underlying pattern. In sum, the positive association between incomparability and status quo bias persists after excluding observations that violate monotonicity in money, with results for Broad Incomparability remaining statistically significant across all three tests.

The point estimates for Narrow and Broad Incomparability remain positive throughout, consistent with Prediction 2. The weaker significance for Narrow Incomparability partly reflects the composition of excluded observations: subjects who violate monetary monotonicity tend to exhibit greater overall status quo retention, and their exclusion reduces the measured status quo bias among Narrowly Incomparable subjects. Broad Incomparability, drawing on a larger pool of both within-pair and cross-pair comparisons, is less sensitive to this compositional shift. In sum, the positive association between incomparability and status quo bias persists after excluding observations that violate monotonicity in money, with results for Broad Incomparability remaining statistically significant across all three tests.

6.3 Discussion on Stochastic Choice

Stochastic choice and incomplete preferences are conceptually distinct but related. A growing theoretical literature shows that incomplete preferences can themselves be a source of stochastic choice. Eliaz and Ok (2006) show that a stochastic choice function can be the

[11] Given that the indicators of incomparability are defined at the individual level in the tests for Prediction 2, we exclude individuals who violate monotonicity in at least one pair.

	proportion	one-sided p-value	one-sided 90 % CI	N
Narrow Incomparability	0.565	0.187	[0.475,∞)	62
Broad Incomparability	0.579*	0.061	[0.513,∞)	107

Significance: * 10%, ** 5%, *** 1%. Removes observations that violate monotonicity in money. Corresponds to Table 11.

Table 18: Proportion of Choosing Status Quo under Incomparability

	proportion (Group 1)	proportion (Group 0)	difference in propotion	one-sided p-value	one-sided 90 % CI	N
Narrow Incomparability	0.200	0.176	0.024	0.387	[-0.082,∞)	212
Broad Incomparability	0.261	0.157	0.104*	0.052	[0.022,∞)	212

Significance: * 10%, ** 5%, *** 1%. Removes observations that violate monotonicity in money. Corresponds to Table 12.

Table 19: Status Quo Bias by Completeness of Preference – Aggregate Level

	Dependent Variable: Status Quo Bias	
	(1)	(2)
Narrow Incomparability=1	1.167	
	(0.606)	
Broad Incomparability=1		1.900*
		(0.690)
Constant	0.214***	0.186***
	(0.0418)	(0.0419)
Observations	212	212

Significance: * 10%, ** 5%, *** 1%. Standard errors in parentheses. Standard errors are clustered at the individual level. Odds ratios are shown as coefficients. Removes observations that violate monotonicity in money. Corresponds to Table 13.

Table 20: Association between Incomparability and Status Quo Bias at Individual Level

observable signature of an underlying incomplete preference relation rather than of either genuine indifference or error; Karni (2024) proposes an "Irresolute Choice Model" in which random choice arises because a decision maker facing noncomparable alternatives awaits an unspecified signal to resolve her indecision; Ok and Tserenjigmid (2020) examines stochastic choice model that can be induced by lack of strict preference.

We do not interpret our data as evidence against stochastic choice per se. Instead, our goal is more precise. First, we show that a specific family of stochastic choice models, the Random Utility Model, under a set of plausible assumptions, cannot account for the

patterns we observe. Second, we show that violations of Proposition 1 exhibit an asymmetric association with exchange behavior: such violations are positively associated with status-quo retention when the alternative is strictly preferred, but are not associated with a corresponding increase in trade when the status quo is strictly preferred.

Together, these two pieces of evidence indicate that the violations of Proposition 1 observed in our experiment are not driven by random error or noise. This leaves open the possibility that a different account of stochastic choice, including one rooted in incomplete preferences rather than random error, could instead explain the data.

6.3.1 Random Utility Model

If stochastic choice were driving the violations of Proposition 1 in the choice list questions, it would have to generate noise of a specific magnitude in the binary-choice questions as well. To evaluate this possibility, we adopt a random utility model in which a person's choice in any given row of a choice list, or in any given binary-choice question, is generated jointly by her underlying utility function $v : Z^+ \times Z^+ \times R^+ \to R$ and a random term ϵ[12], such that given $A = (a_1, a_2, a_3)$ $B = (b_1, b_2, b_3)$

$$\rho(A, B) = \mathbf{Pr}(v(A) + \epsilon_A > v(B) + \epsilon_B)$$

, where $\rho(A, B)$ is the probability of A being chosen in comparison with B.

Denote $\rho^1(A, B)$ as the probability of choosing A in the comparisons A vs. B in a choice list question where A is the fixed option. Denote $\rho^2(A, B)$ as the probability of choosing A in the comparisons A vs. B in a choice list question where B is the fixed option. Denote $\rho^3(A, B)$ as the probability of choosing A in the comparisons A vs. B in a binary choice question. Analogously for ϵ^1_A, ϵ^2_A, and ϵ^3_A as well as ϵ^1_B, ϵ^2_B, and ϵ^3_B. v is constant across questions.

We make two assumptions on the random function ϵ.

Assumption 1. $\epsilon_A - \epsilon_B$ across questions are independent draws.

Assumption 2. Given the same comparison between $A = (a_1, a_2, a_3)$ and $B = (b_1, b_2, b_3)$, $e^1_A - e^1_B$,$e^2_A - e^2_B$, and $e^3_A - e^3_B$ follows the same distribution.

Assumption 1 concerns independence across questions only: noise across different rows within the same choice list question may be correlated. Assumption 2 only requires the same distribution given the same comparison. Together, Assumption 1 and Assumption 2 are weaker than the i.i.d. assumption in the Fechnerian Random Utility model.

[12]When the set of alternatives is finite, the additive structure of error is without loss of generality (Strzalecki (2025)).

Choice List. With A fixed and b^k the money amount of B in row k, the DM makes a binary choice between A and $(0, b_2, b^k)$ at every row; the data reveal the row at which she switches from A to B. Symmetrically, with B fixed and a^k the money amount of A in row k, the DM makes a binary choice between $(a_1, 0, a^k)$ and B at every row, and the data reveal the row at which she switches from B to A.

Binary choice 1 (A vs. B). The DM chooses A or B.

Binary choice 2 ($A' = (a_1, 0, a_3 + 0.25)$ vs. B). The DM chooses A' or B.

Binary choice 3 (A vs. $B' = (0, b_2, b_3 + 0.25)$). The DM chooses A or B'.

In the binary choice questions, two patterns violate dominance transitivity. Pattern 1: choose B between A and B, and choose A between A and B'. Denote the probability of Pattern 1 by P_1. Pattern 2: choose A between A and B, and choose B between A' and B. Denote the probability of Pattern 2 by P_2. In the choice list questions, we focus on the analogous two patterns that violate Proposition 1. Pattern 3: in the A-fixed list, choose A at the row corresponding to $b_3 + 0.25$ (i.e., have not yet switched to B by that row), and in the B-fixed list, choose B at the row corresponding to a_3 (i.e., have not yet switched to A by that row); Denote the probability of Pattern 3 by P_3. Pattern 4: in the A-fixed list, choose A at the row corresponding to b_3 (i.e., have not yet switched to B by that row), and in the B-fixed list, choose B at the row corresponding to $a_3 + 0.25$ (i.e., have not yet switched to A by that row). Denote the probability of Pattern 4 by P_4.

Deriving P_1 and P_2. Pattern 1 requires choosing B in the A vs. B question and choosing A in the A vs. B' question. By Assumption 1,

$$P_1 = \left(1 - \rho^3(A, B)\right) \rho^3(A, B').$$

Similarly,

$$P_2 = \rho^3(A, B) \left(1 - \rho^3(A', B)\right).$$

Deriving P_3 and P_4. Since Pattern 3 uses only one row from each of two distinct lists, never two rows from the same list. Hence, by Assumption 1,

$$P_3 = \left(1 - \rho^2(A, B)\right) \rho^1(A, B').$$

Similarly,

$$P_4 = \rho^1(A, B)\left(1 - \rho^2(A', B)\right).$$

By assumption 2, $P1 = P2$, $P3 = P4$. However, 21 observations show Pattern 1, while 35 observations show Pattern 3, which is the opposite of the prediction, with a p-value of 0.056. In addition, 18 observations show pattern 2 and 35 observations show pattern 4, which is again against the prediction with a p-value of 0.017. Therefore, this Random Utility Model cannot explain the behaviors we observe in the experiment.

6.3.2 Asymmetric Association between Incomplete Preference and Exchange Behaviour

For observations with a strict preference, two forms of irrational behaviour are possible: (i) retaining the status quo while strictly preferring the alternative (status quo bias), and (ii) choosing to trade while strictly preferring the status quo (switch bias[13]). If violations of Proposition 1 are driven by stochastic choice rather than genuine incomparability, both anomalies should be equally elevated among individuals who exhibit incomparability in at least one pair. The results for Prediction 2 document a significant increase in status quo bias for such individuals, consistent with pattern (i). However, Table 21, which reports the difference in switch bias between individuals with incomparability in at least one pair (Group 1) and those without (Group 0), reveals no significant difference between the two groups. The asymmetry between the two patterns — elevated status quo bias but no elevated switch bias — is difficult to reconcile with a pure stochastic choice explanation, and instead supports the interpretation that the identified violations reflect genuine incomparability.

	Dependent Variable: Switch Bias	
	(1)	(2)
Narrow Incomparability=1	1.396	
	(0.692)	
Broad Incomparability=1		1.884
		(0.825)
Constant	0.119***	0.104***
	(0.0287)	(0.0276)
Observations	274	274

Table 21

[13]There is no such terminology. We denote this kind of anomaly by switch bias for simplicity

7 An Application: Endowment Effect or Incomplete Preference?

The findings documented in this paper carry broader methodological implications for experimental tests of economic theories that involve binary choice between a status quo and an alternative. In such settings, a subject who retains the status quo is conventionally interpreted as either preferring it or exhibiting a behavioral bias against trading. This interpretation implicitly assumes that preferences are complete — that every subject can rank the status quo against the alternative. When this assumption fails, retention of the status quo admits a third interpretation: the subject faces genuine incomparability and defaults to the status quo in the absence of a well-defined preference ordering. Ignoring this possibility conflates two conceptually distinct phenomena and may lead to systematically misleading conclusions about the prevalence and sources of behavioral biases. Since our experiment directly studies the association between preference incompleteness and status quo retention, we illustrate this concern through the lens of Kahneman et al. (1990) (referred to as KKT in the following). Their experimental test of the Coase Theorem is among the most influential applications of the binary exchange paradigm, and as we show below, its conclusions are consequently sensitive to whether preference incompleteness is accounted for.

Kahneman et al. (1990) provides an experimental test of the Coase Theorem, which predicts that, in the absence of transaction costs, the final allocation of resources is independent of the initial assignment of property rights. Since endowments are randomly assigned, gains from trade exist for approximately half of all subject pairs, implying that roughly 50% of endowed goods should change hands in equilibrium. KKT consistently observes trade volumes significantly below this benchmark and attributes the undertrading to the endowment effect, rooted in loss aversion: subjects demand substantially more to relinquish an object than they are willing to pay to acquire it.

We argue, however, that this interpretation rests on an implicit assumption that has gone unexamined — namely, that all subjects hold complete and well-defined preferences over the objects being traded. Once this assumption is relaxed, three exhaustive reasons emerge for why a subject might retain the status quo rather than trade: (1) the status quo is strictly preferred to the alternative; (2) the alternative is strictly preferred to the status quo, yet the subject nevertheless retains it — the classical status quo bias; or (3) the subject faces incomparability between the status quo and the alternative and defaults to the status quo. The first case is consistent with rational behavior and is not of interest here. The second case is the phenomenon KKT document. The third case is conceptually distinct: retention of the status quo reflects not a bias over a well-defined preference ordering, but the absence

of such an ordering altogether.

In our data, the counts for each case are as follows. Under narrow incomparability, cases (1), (2), and (3) account for 181, 44, and 44 observations, respectively; under broad incomparability, the corresponding counts are 166, 54, and 54. The near-identical magnitudes of cases (2) and (3) indicate that the two phenomena are of comparable empirical importance. Conflating them — as any analysis that does not elicit preferences and incomparability must necessarily do — risks attributing to loss aversion what is in fact a consequence of preference incompleteness.

The experimental design in KKT does not elicit subjects' underlying preferences or identify incomparability; it is structurally unable to distinguish between cases (2) and (3). Our design directly elicits the preference relation between the status quo and the alternative for each subject in each round. This allows us to test whether the failure to draw this distinction leads to consequentially different conclusions.

Following KKT, our key empirical test compares observed trade volume to the volume predicted by the Coase Theorem. We implement this test on two samples: (i) the full sample, and (ii) the subsample restricted to subjects who exhibit no incomparability in any round. In the full sample, the Coase Theorem predicts a trade rate of 50%, and we find that observed trade volume is 41%, which falls statistically significantly below this benchmark, replicating the KKT result.

The predicted trade rate in the complete-preference subsample requires more care. Let N_A denotes the number of observations in which the alternative is strictly preferred to the status quo, N_I the number of observations in which the subject is indifferent[14], and N^* the total number of observations in the subsample. The Coase Theorem predicts a trade rate of $\frac{N_A+\alpha N_I}{N^*}$, where $\alpha \in [0,1]$ denotes the probability of trading under indifference. The Coase Theorem is silent on the value of α; we therefore report results for both $\alpha = 0$ and $\alpha = 1$ as bounds.[15]

As reported in Table 22, the Coase Theorem is decisively rejected in the full sample, consistent with KKT. In the complete-preference subsample, however, the results are sensitive to the choice of α: the theorem cannot be left when $\alpha = 0$, but is rejected when $\alpha = 1$. Given the absence of a theoretical prior on α and the empirical evidence that α is closer to 0 in our data, there is no robust basis on which to reject the Coase Theorem in this subsample. Consequently, there is no robust evidence for loss aversion or status quo bias once subjects with incomplete preferences are excluded. This finding suggests that the KKT result may

[14] N_I are 20, 13, 13 across samples

[15] In our data, when subjects are indifferent between the status quo and the alternative, 15 out of 20 choose to retain the status quo in the full sample. This suggests α is empirically small and closer to 0.

be partly an artifact of ignoring preference incompleteness, rather than a pure reflection of the endowment effect.

It is important to acknowledge a key distinction between the current experiment design and the trading environment that the Coase Theorem most directly speaks to. A complete test of the Coase Theorem would require a market or bargaining institution — as in Kahneman, Knetsch, and Thaler (1990). The current design offers subjects a costless switch between their assigned object and an alternative with no trading environment. This design difference has two implications for interpreting our results. First, our experiment tests a condition that is logically prior to the Coase Theorem: costless reallocation suffices to render initial assignment irrelevant to final memorizes. Second, and conversely, our design cannot speak to whether introducing a market would restore efficient allocations, as Plott and Zeiler (2005) has explored in related contexts. Therefore, our test is more conservative, given that we make no claim about market outcomes.

	$\alpha = 0$		
	Full Sample	Exclude Narrow Incomparability	Exclude Broad Incomparability
Trading Rate	0.410	0.426	0.429
Coase Benchmark	0.500	0.455	0.450
p-value	0.000***	0.106	0.207
Reject Coase?	Yes	No	No
	$\alpha = 1$		
	Full Sample	Exclude Narrow Incomparability	Exclude Broad Incomparability
Trading Rate	0.410	0.426	0.429
Coase Benchmark	0.500	0.485	0.483
p-value	0.000***	0.006***	0.017**
Reject Coase?	Yes	Yes	Yes
N	602	437	389

Table 22: Test of Coase Theorem

8 Conclusion

This paper provides experimental evidence on incomplete preferences and their behavioral consequences. We develop a framework based on the Equivalent Gain to identify incomparability between bundles that differ in both the consumption good and the monetary component, and implement it in a controlled experiment. Using violations of Proposition 1 as a conservative lower bound, we find that a substantial share of subjects exhibit incomplete

preferences: 34.6% display Broad Incomparability and 27.1% display Narrow Incomparability in at least one pair. We test two predictions linking incomparability to status quo retention. Prediction 1 establishes that, conditional on facing an incomparable alternative, subjects retain the status quo at rates significantly above 50%, ranging from 58.9% to 61.9% depending on the categorization of incomparability. Prediction 2 establishes that subjects who exhibit incomparability in at least one pair display significantly higher status quo bias than those who do not, with differences of 15.5 to 20.6 percentage points. Both predictions are robust to excluding observations that violate dominance transitivity or monetary monotonicity. Furthermore, the increase in status quo bias is not accompanied by a corresponding increase in switch bias, an asymmetry that supports the interpretation that incomparability — rather than stochastic choice — is the primary driver of the observed status quo retention.

These findings carry broader methodological implications for experimental tests of economic theories that rely on binary exchange paradigms. We illustrate this through the lens of Kahneman et al. (1990), whose experimental test of the Coase Theorem attributes undertrading to loss aversion and the endowment effect. We argue that this interpretation conflates two conceptually distinct phenomena: retaining the status quo due to a preference bias over a well-defined ordering, and retaining it due to the absence of such an ordering altogether. In our data, these two cases are of comparable empirical importance — under Narrow Incomparability, each accounts for 44 observations, and under Broad Incomparability, 54 observations each. When we restrict attention to subjects with complete preferences, the evidence against the Coase Theorem is no longer robust, and consequently, the evidence for loss aversion and the endowment effect largely disappears. This suggests that the KKT result may be partly an artifact of ignoring preference incompleteness rather than a pure reflection of the endowment effect. More generally, ignoring incomplete preferences in experiments is consequential: it risks attributing to behavioral biases what is in fact a consequence of a fundamentally different phenomenon — the absence of a well-defined preference ordering.

Rather than specifying a behavioral model of how subjects with incomplete preferences make choices, our approach proceeds by refutation: we derive bounds on behavior that must hold for any complete preference relation satisfying dominance transitivity, and use violations of these bounds as indicators of incomparability. This strategy is deliberately conservative as it accommodates a very general class of complete preferences without imposing additional structure. But it comes at a cost. We cannot characterize how incomparable subjects actually make choices, only that their choices are inconsistent with any complete preference in this class. As a result, the identified prevalence of incomparability should be interpreted as a lower bound, and the approach is silent on the decision process that governs behavior when

preferences are genuinely incomplete. Developing a richer behavioral model of choice under incomparability remains an important direction for future research.

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Appendix

When we relax continuity, the definition of *Equivalent Gain (G)* need to be updated accordingly.

Definition 2 (Tradeoff Equivalent(TE):)**.** For any $(x, 0, z_x), (0, y, z_y) \in \mathbb{C}$:

1. $\overline{TE_{xy}}(x, y, z_x, z_y) = inf\{z \in \mathbb{R}_+ | (0, y, z) \succsim (x, 0, z_x)\}$
2. $\underline{TE_{xy}}(x, y, z_x, z_y) = sup\{z \in \mathbb{R}_+ | (x, 0, z_x) \succsim (0, y, z)\}$
3. $\overline{TE_{yx}}(x, y, z_x, z_y) = inf\{z \in \mathbb{R}_+ | (x, 0, z) \succsim (0, y, z_y)\}$
4. $\underline{TE_{yx}}(x, y, z_x, z_y) = sup\{z \in \mathbb{R}_+ | (0, y, z_y) \succsim (x, 0, z)\}$

Proposition 2. *If $\succsim$ satisfies Axiom 1-2, $i, j \in \{x, y\}$, then following statements are true:*

1. $\underline{TE_{ij}} > z_j \Rightarrow \overline{TE_{ji}} \leq z_i$
2. $\overline{TE_{ij}} < z_j \Rightarrow \underline{TE_{ji}} \geq z_i$.

Proof. By definition, $\forall z > \overline{TE_{xy}}$, $(0, y, z) \succ (x, 0, z_x)$. $\forall z < \overline{TE_{xy}}$, $(x, 0, z_x) \succ (0, y, z)$. $\forall z < \underline{TE_{xy}}$, $(x, 0, z_x) \succ (0, y, z)$. $\forall z > \underline{TE_{xy}}$, $(0, y, z) \succ (x, 0, z_x)$. Symmetrically, $\forall z > \overline{TE_{yx}}$, $(x, 0, z) \succ (0, y, z_y)$. $\forall z < \overline{TE_{yx}}$, $(0, y, z_y) \succ (x, 0, z)$. $\forall z < \underline{TE_{yx}}$, $(0, y, z_y) \succ (x, 0, z)$. $\forall z > \underline{TE_{yx}}$, $(x, 0, z) \succ (0, y, z_y)$.

Assume without loss of generality that $i = x$.

step 1 (prove (1)): If $\underline{TE_{xy}} > z_y$, $(x, 0, z_x) \succ (0, y, z_y)$. Assume for contradiction that $\overline{TE_{yx}} > z_x$, $(0, y, z_y) \succ (x, 0, z_x)$. Contradiction.

step 2 (prove (2)): If $TE_{xy} < z_y$, $(0, y, z_y) \succ (x, 0, z_x)$. Assume for contradiction that $\underline{TE_{yx}} < z_x$, $(x, 0, z_x) \succ (0, y, z_y)$. Contradiction. ■