Ever Since Allais*

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The Allais critique of expected utility theory (EUT) has led to the development of theories of choice under risk that relax the independence axiom but adhere to the fundamental/conventional axioms of ordering (completeness and transitivity) and monotonicity (with respect to first-order stochastic dominance). Unlike experimental work designed to test independence, our experiment is comprehensive—testing the entire set of axioms on which EUT is based. Our econometric analysis is also nonparametric and performed at the level of each individual subject. For the vast majority of subjects departures from independence are small relative to departures from ordering and/or monotonicity.

JEL Codes: D81, C91.

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1 Introduction

Expected utility theory (EUT) lies at the very heart of economics so it is natural that experimentalists would want to test the empirical validity of the axioms on which EUT is based. Empirical violations of EUT bring into question the rationality of individual behavior and, more specifically, raise criticisms of the familiar von Neumann and Morgenstern (1947) *independence* axiom as the touchstone for rational decision making under risk. Such criticisms have motivated various theoretical alternatives to EUT, and the experimental investigation of these theories has resulted in new empirical regularities.

For the most part, generalizations of EUT weaken the independence axiom but embody ordering (completeness and transitivity) and monotonicity with respect to first-order stochastic dominance (FOSD).[1] To test EUT and its various generalizations, laboratory experiments typically use several pairwise alternatives, à la Allais, that these various theories rank differently, while making a presumption that subjects adhere to the (more) fundamental/conventional axioms of ordering and monotonicity.

Given that EUT is part of the core of economics—and not something that one can or should abandon lightly—we wish to provide a comprehensive assessment of all the axioms on which EUT is based, and not just the independence axiom. Our overall objective is to provide a better, positive account of choice behavior under risk by evaluating the performance of EUT (as well as non-EUT models) in a choice environment where all axioms underpinning these models can be evaluated.

To do this, we use an experiment where, through a graphical "point-and-click" design, subjects choose an allocation of contingent commodities from a budget set; the user-friendly interface makes it possible to present subjects with a large array of heterogeneous budget sets. Crucially, the rich information collected in this way allows us to perform (statistical) tests at the level of each individual subject. From these tests, we can ascertain each subject's degree of compliance with the different components of EUT. While budgetary experiments are not new, the vast majority of such experiments involve subjects choosing from two-dimensional (2D) budget lines (in particular, see Choi *et al.* (2007a,b)), while in our experiment, subjects

[1] Violations of FOSD are commonly regarded as mistakes/errors in decision making, and so monotonicity with respect to FOSD is a generally accepted principle in decision theory, as pointed out by Quiggin (1990), Wakker (1993), and Starmer (2000), among others.

choose from three-dimensional (3D) budget sets. The experiment involving three states and three associated securities has a number of important advantages over earlier experiments:

- While demand is typically studied within the context of complete and transitive preferences, it can be well-defined even when one or both of these properties are absent (Anderson, 1981). A fundamental result attributable to Rose (1958) and extended by Banerjee and Murphy (2006) states that with only two goods, the *weak axiom of revealed preference* (WARP) and the *generalized axiom of revealed preference* (GARP) are observationally equivalent—any violation of GARP (cyclical inconsistency) must contain a violation of WARP (pairwise inconsistency). Since WARP is an implication of completeness, Rose's (1958) result tells us that with only two goods we cannot separate incompleteness from intransitivity. With three (or more) goods, by contrast, choices can satisfy WARP (pairwise consistency) and violate GARP (cyclical inconsistency), indicating complete yet nontransitive preferences. Therefore, an experimental design with three states allows us to provide a discriminating test of the ordering axioms (completeness and transitivity) before jointly testing any additional properties, such as independence, which places strong restrictions on the precise form of preferences.

- Within the context of choices from 3D budget sets, prominent non-EUT models give rise to distinct utility specifications, which yield empirically discriminating restrictions on observed behavior. However, these differences are no longer prominent within the context of choices from 2D budget lines. For example, the rank-dependent utility (Quiggin, 1982, 1993) and disappointment aversion (Gul, 1991) models reduce to the same form with two equally likely states (see the Appendix for details). The greater empirical separation among non-EUT models in 3D choice data allows for a more rigorous test of EUT (by testing it against a richer set of alternatives). This is consistent with our power analysis of the experimental design/test, which shows that data from 3D budget sets provide a stronger test in terms of power—especially of EUT versus non-EUT models that respect FOSD—than data from 2D budget lines.

Our analysis builds on the nonparametric revealed preference approach to testing a broad class of models of decision making under risk—including EUT and prominent non-EUT

alternatives—developed by Polisson, Quah, and Renou (2020). While Polisson, Quah, and Renou (2020), as well as recent studies by de Clippel and Rozen (2023) and Echenique, Imai, and Saito (2023), utilize existing experimental data from 2D budget lines, our analysis extends to 3D budget sets. This extension is crucial because, as noted above, in 3D data: (1) WARP and GARP are not observationally equivalent, allowing us to distinguish between incomplete and nontransitive preferences, and (2) the test of EUT is stronger as it is compared against a richer set of non-EUT alternatives. Additionally, we develop a novel nonparametric statistical test to compare the consistency scores between models, specifically of EUT versus non-EUT alternatives. The rich data generated by our experiment allow us to apply this statistical test to individual-level data, rather than pooling data or assuming homogeneity across subjects.

Our empirical analysis is in the revealed preference tradition of Afriat (1967, 1973), Diewert (1973), and Varian (1982, 1983a, 1990). Afriat's (1967) theorem tells us that if a finite dataset generated by an individual's choices from linear budget sets (as in our experiment) obeys GARP, then the data can be rationalized by a continuous and increasing utility function. This result gives a practical way of checking whether a dataset is *rationalizable* in this fundamental/basic sense. There are also extensions of Afriat's (1967) theorem that allow us to test whether a dataset can be rationalized by a utility function with stronger properties. In particular, we could test whether a dataset is *FOSD-rationalizable*, in the sense that it is consistent with the maximization of a continuous utility function that is increasing with respect to FOSD and whether a dataset is *EUT-rationalizable*, in the sense that it is consistent with the maximization of a continuous utility function of the expected utility form.

When subjects make decisions, they often make mistakes; and these mistakes manifest themselves in inconsistent choices. Since GARP provides an exact test (either choices satisfy GARP or they do not) and choice data almost always contain at least some errors, we assess how nearly the data comply with GARP by calculating Afriat's (1973) *critical cost-efficiency index* (CCEI), which measures the extent by which each budget constraint must be reduced in order to remove all violations of GARP (thereby rendering the data rationalizable). The CCEI, denoted by e^*, is bounded between 0 and 1 and can be interpreted as saying that the decision maker is leaving (as much as) a fraction $1-e^*$ of the money on the table due to errors because the chosen allocation is utility-maximizing only when compared to allocations in the

reduced budget set. The CCEI is thus the "least costly" adjustment required to remove all errors. In this sense, it measures the overall "noise" in individual behavior.

Using recent refinements of the CCEI (in particular, see Polisson, Quah, and Renou (2020)), we can also measure the extent to which budget sets need to be reduced in order for a dataset to be FOSD-rationalizable or EUT-rationalizable. Thus, for any dataset collected from an individual subject's choices, three CCEI-type scores can be calculated: e^* for (basic) rationalizability, e^{**} for FOSD-rationalizability (which can be no greater than e^* since FOSD-rationalizability is the more stringent requirement), and e^{***} for EUT-rationalizability (which can be no greater than e^{**} since EUT-rationalizability is the more stringent requirement).

The use of the same measure for all three models we consider has the very important advantage that we can decompose violations of EUT and compare the magnitudes of violations of the different axioms from which EUT can be derived. Perfect consistency with EUT implies that $1 = e^* = e^{**} = e^{***}$, whereas perfect consistency with any of the familiar non-EUT alternatives that respect FOSD but not EUT itself implies that $1 = e^* = e^{**} > e^{***}$.

Figure 1 depicts the distributions of the e^*, e^{**}, and e^{***} rationalizability scores. The horizontal axis shows the score values and the vertical axis presents the fraction of subjects with scores above each value. Only 16.1 percent of subjects are perfectly rationalizable ($e^* = 1$), but none are perfectly FOSD-rationalizable ($e^{**} = 1$) or EUT-rationalizable ($e^{***} = 1$). If we set a more permissive score value threshold of 0.95, we find that 63.1 percent of subjects are rationalizable ($e^* > 0.95$), 28.0 percent are FOSD-rationalizable ($e^{**} > 0.95$), and 16.1 percent are EUT-rationalizable ($e^{***} > 0.95$). This means that at a score value threshold of 0.95, the number of subjects who fail FOSD-rationalizability is about six times greater than those who are FOSD-rationalizable but not EUT-rationalizable. Across the other score value thresholds displayed in Figure 1, this ratio ranges between three and six. Overall, the difference between *perfect* rationalizability and FOSD-rationalizability ($1-e^{**}$) is greater than the difference between FOSD-rationalizability and EUT-rationalizability ($e^{**} - e^{***}$) for 85.1 percent of our subjects. We thus conclude that violations of ordering and/or monotonicity—underlying both EUT and non-EUT alternatives—are much more pronounced in our data than violations of independence alone.

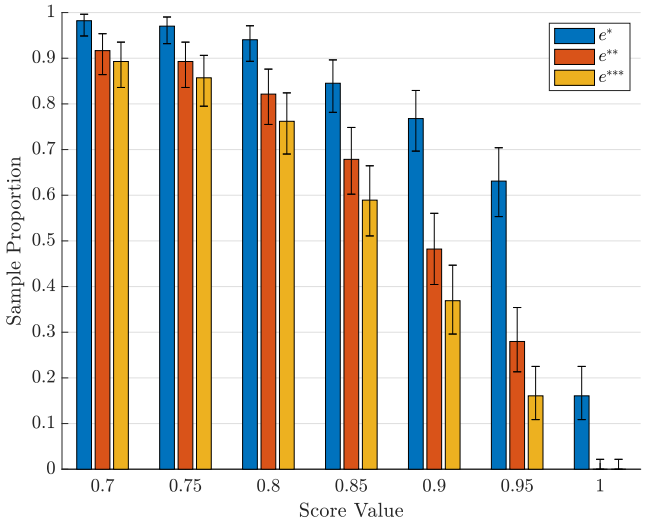


Figure 1: Distributions of Rationalizability Scores

The horizontal axis shows the score values and the vertical axis presents the fraction of subjects with scores above each value for rationalizability (e^*), FOSD-rationalizability (e^{**}) and EUT-rationalizability (e^{***}). The braces indicate 95 percent confidence intervals on these proportions.

Our rich data also allow us to make statistical comparisons at the individual level of the difference between perfect rationalizability and FOSD-rationalizability and the difference between FOSD-rationalizability and EUT-rationalizability, using a purely nonparametric econometric approach making no functional form assumptions about subjects' underlying preferences or on the error structure. For each subject, we randomly draw 1,000 datasets of 25 observations each. For each subsample, we calculate FOSD-rationalizability and EUT-rationalizability scores, denoted by $e^{\dagger\dagger}$ and $e^{\dagger\dagger\dagger}$, and the resulting difference-in-differences $(1 - e^{\dagger\dagger}) - (e^{\dagger\dagger} - e^{\dagger\dagger\dagger})$. (We use $\dagger$ rather than $*$ to denote rationalizability scores based on 25 rather than 50 observations). We then calculate the sample mean of the difference-in-differences and generate a sampling distribution of difference-in-differences using a standard bootstrapping procedure. We find that the sample mean of the difference-in-differences is positive and statistically significantly different from zero for 84.5 percent of subjects, which includes nearly all subjects for whom $(1-e^{**})-(e^{**}-e^{***})$ is positive using all 50 observations.

Violations of EUT thus appear to run much deeper than violations of merely independence, challenging many of the most prominent non-EUT alternatives which respect FOSD. Furthermore, of the 83.9 percent of subjects who are not perfectly rationalizable, no subject has only GARP violations which are free of WARP violations; and the CCEI scores required to remove all violations of WARP and GARP are identical for all but 4 subjects

(2.4 percent). Therefore, models of choice under risk based on (complete and) nontransitive preferences—as proposed by Bell (1982), Fishburn (1982), and Loomes and Sugden (1982)—cannot rationalize our data. While these models can accommodate violations of GARP, they cannot accommodate violations of WARP. This distinction is only possible within the context of choices from 3D (or more) budget sets.

While there are alternative cost-efficiency measures of violations of rationalizability, none of the various indices are viable given our empirical exercise. We adopt the CCEI as a measure of rationalizability since it is straightforward to interpret and computationally feasible for moderately large datasets and for all classes of models we consider. Partly for those reasons, the CCEI is also the most commonly used measure in empirical revealed preference research. Nevertheless, we also develop an alternative distance-based approach which yields similar empirical conclusions, albeit in a somewhat limited empirical exercise.

The emphasis in our paper is to provide a *comprehensive* and *nonparametric* test of complete representations of preferences under risk rather than focusing on individual axioms. Our main result—that violations of EUT are relatively small after accounting for violations of ordering and monotonicity with respect to FOSD—is what Quiggin (1982) calls an "undesirable result" as ordering and monotonicity are more fundamental principles than the independence axiom, and are embodied in the most prominent non-EUT theories of choice under risk. As Starmer (2000) notes, economists have taken the view that the independence axiom needs to be weakened on the grounds of predictive validity and psychological realism, but have generally left ordering and monotonicity unchallenged.

The rest of the paper is organized as follows. The next section provides more background and motivation. Section 3 describes our tests of rationalizability, and Section 4 outlines the experimental design and procedures. Section 5 summarizes the experimental results. Section 6 explains how the paper is related to the literature, focusing on recent revealed preference papers on choice under risk. Section 7 outlines what we think theorists, experimentalists, and other economists should take away from the paper. In the interests of brevity, all technical details that are not essential for understanding the results are relegated to the Appendix.

2 Background and Motivation

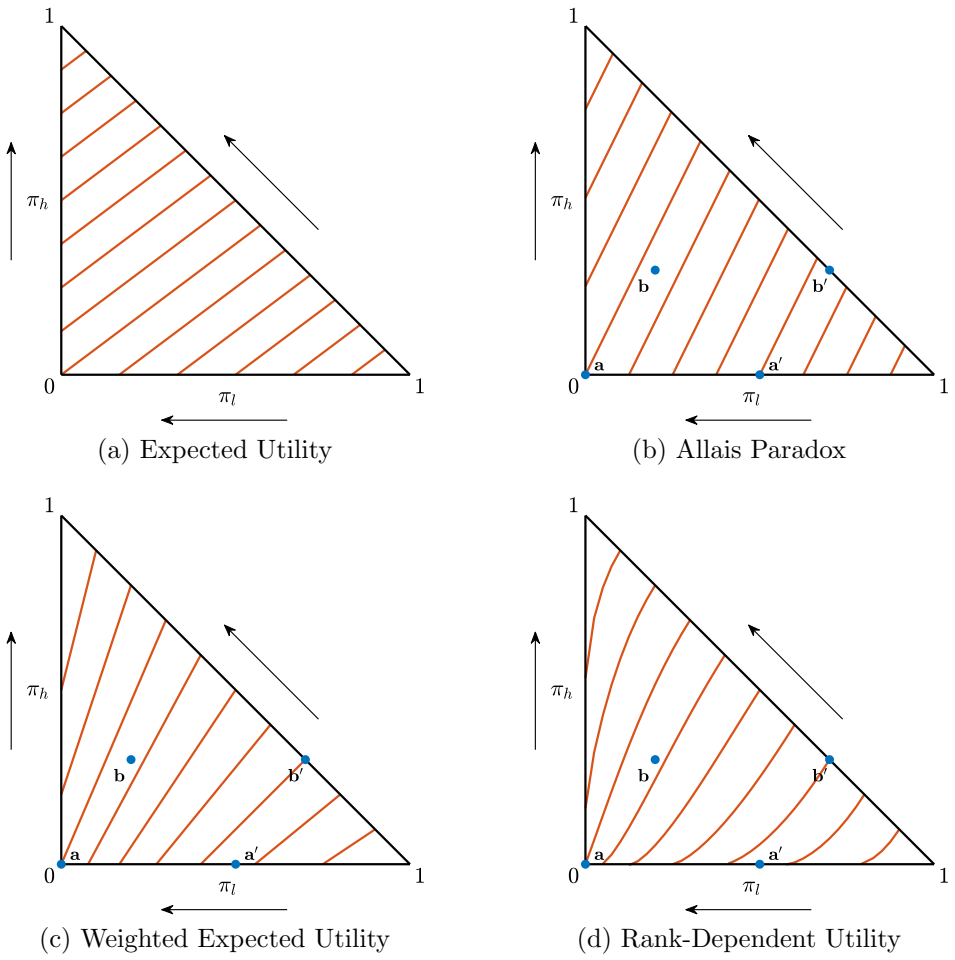
Much of the experimental evidence of "anomalies" in choice behavior suggests that EUT may not the right model of choice under risk. To understand the role of each of the axioms on which EUT is based, suppose that there are three mutually non-indifferent outcomes $x_h \succ x_m \succ x_l$, such as monetary consequences where $x_h > x_m > x_l$. The probability triangle in Figure 2 depicts the set of all possible lotteries—each point in the triangle represents a lottery (π_h, π_m, π_l) over the outcomes (x_h, x_m, x_l), where $\pi_h = 0$ on the horizontal edge, $\pi_m = 0$ on the hypotenuse (because $\pi_h + \pi_l = 1$), and $\pi_l = 0$ on the vertical edge.[2]

Ordering (completeness and transitivity) plus continuity imply that there exists a map of (non-intersecting) indifference curves on the probability triangle. Monotonicity with respect to first-order stochastic dominance (FOSD) implies that preferences are increasing from right to left along horizontal lines, from bottom to top along vertical lines, and from bottom-right to top-left along lines parallel to the hypotenuse. Assuming that ordering and monotonicity hold, the independence axiom then implies that preferences admit an expected utility representation, so that the indifference curves in the triangle are parallel straight lines (Figure 2a). Viewed within the context of the triangle, independence is a strong requirement, leaving only the slope of the indifference lines undetermined (with steeper lines corresponding to higher risk aversion).

An example of the famous Allais (1953) paradox can be illustrated by a pair of binary choices—between lotteries $\mathbf{a}$ and $\mathbf{b}$ and between lotteries $\mathbf{a}'$ and $\mathbf{b}'$ (Figure 2b). The imaginary straight lines connecting lotteries $\mathbf{a}$ and $\mathbf{b}$ and lotteries $\mathbf{a}'$ and $\mathbf{b}'$ are parallel to each other and flatter than the indifference lines so $\mathbf{a} \succ \mathbf{b}$ and $\mathbf{a}' \succ \mathbf{b}'$. But experimental subjects often make choices revealing that $\mathbf{a} \succ \mathbf{b}$ and $\mathbf{b}' \succ \mathbf{a}'$ (or $\mathbf{b} \succ \mathbf{a}$ and $\mathbf{a}' \succ \mathbf{b}'$), which is commonly taken as evidence against independence. This persistent finding has led to a large literature with the objective of developing new models of choice under risk that weaken the independence axiom (see Camerer (1995) and Starmer (2000) for comprehensive reviews).

In weighted expected utility (Dekel, 1986; Chew, 1989), for example, all indifference curves are again straight lines but they typically "fan out"—that is, they become steeper

[2]The probability triangle was introduced by Marschak (1950) and popularized by Machina (1982) as a way of representing the choice space over lotteries.



(a) Expected Utility (b) Allais Paradox

(c) Weighted Expected Utility (d) Rank-Dependent Utility

Figure 2: Indifference Curves in the Probability Triangle

The probability triangle depicts the lottery space as a set of probability weights (π_h, π_m, π_l) over three fixed outcomes (x_h, x_m, x_l). (a) Ordering (completeness and transitivity) plus continuity guarantee non-intersecting indifference curves; monotonicity (with respect to FOSD) guarantees that preferences are increasing as shown (see arrows). Adding independence gives rise to EUT, characterized by indifference curves that are parallel straight lines. (b) The Allais paradox arises because EUT requires that $\mathbf{a} \succ \mathbf{b}$ and $\mathbf{a}' \succ \mathbf{b}'$, but experimental subjects often make choices revealing that $\mathbf{a} \succ \mathbf{b}$ but $\mathbf{b}' \succ \mathbf{a}'$. Prominent non-EUT models that adhere to ordering and monotonicity but relax independence to accommodate the Allais paradox include (c) weighted expected utility and (d) rank-dependent utility.

(corresponding to higher risk aversion) when moving northwest in the triangle (Figure 2c). In loss/disappointment aversion (Gul, 1991), the indifference curves are also straight lines but "fan in" for lotteries better than x_m (top part of the triangle) and "fan out" for lotteries worse than x_m (bottom part of the triangle). In rank-dependent utility (Quiggin, 1982, 1993) and prospect theory (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992), by contrast, the indifference curves are not straight lines and they can "fan out" or "fan in," especially near the triangle boundaries (Figure 2d). Each of the conventional alternatives

to EUT gives rise to indifference curves with distinctive shapes in the triangle, but with the common feature that they avoid the Allais paradox.

In many experimental studies, the main criterion used to evaluate a given theory is the fraction of choices that it correctly predicts. Some studies have also estimated parametric utility functions for individual subjects. Generally speaking, these experiments involve collecting a small number of decisions from each subject, with the decisions involving specific choices that are narrowly tailored to test the independence axiom against its various generalizations. The accumulated experimental evidence against independence has prompted theorists to develop formal alternatives to EUT and, apart from a few notable exceptions, non-EUT models have relaxed the independence axiom while maintaining ordering and monotonicity with respect to FOSD. However, our basic contention is that we ought to have a wider view of the relative performance of EUT and therefore *all* the assumptions which underpin the model deserve close scrutiny.

In this paper, we test EUT and its generalizations in a canonical setting of decision making under risk—the portfolio choice problem of the consumer. Within this setting, we develop tests of rationalizability that are *comprehensive*, in the sense that we test whether a given model—taken as a whole—succeeds or fails in explaining the data, rather than focusing on specific individual axioms. Furthermore, by evaluating a set of progressively restrictive models using a common measure of performance, we can compare the relative impact of the different EUT axioms. Another important feature of our tests is that they are *nonparametric*, in the sense that we make no auxiliary functional form assumptions on the utility function.

3 Theory

In this section, we describe the theory on which the experimental design is based. In the experimental task we study, subjects make decisions under conditions of uncertainty about the objective parameters of the environment. There are three equally likely states of nature ($s = 1, 2, 3$) and an Arrow security for each state. An Arrow security for state s is defined to be a promise to deliver one token (the experimental currency) if state s occurs and nothing otherwise. Let $\mathbf{x} = (x_1, x_2, x_3)$ denote a portfolio of securities or bundle of contingent commodities, where $x_s \geqslant 0$ denotes the number of units of security s. Without essential

loss of generality, assume the individual's endowment is normalized to 1. The budget set $\mathcal{B} = \{\mathbf{x} \in \mathbb{R}^3_+ : \mathbf{p} \cdot \mathbf{x} \leqslant 1\}$ is then all the bundles that are affordable given this endowment and given the price vector $\mathbf{p} = (p_1, p_2, p_3)$, where $p_s > 0$ denotes the price of security s.

The experimental design requires subjects to solve a sequence of 50 decision problems. Each decision problem is defined by a budget set, represented graphically on a computer screen. The subject uses the mouse to select a portfolio from the feasible set by "pointing and clicking." Subjects were informed that the states are equally likely, eliminating any ambiguity. For each subject, we have a set of 50 observations $\mathcal{D} := \{(\mathbf{p}^i, \mathbf{x}^i)\}_{i=1}^{50}$, where $\mathbf{p}^i = (p_1^i, p_2^i, p_3^i)$ denotes the i-th observation of the price vector and $\mathbf{x}^i = (x_1^i, x_2^i, x_3^i)$ denotes the corresponding allocation. The experiment thus provides a large amount of choice data consisting of many individual decisions over a wide range of three-dimensional budget sets.

3.1 Rationalizability

The most fundamental/basic question to ask of each individual-level dataset obtained in our experiment is whether it is consistent with utility maximization. We refer to a utility function defined over contingent commodities, $U : \mathbb{R}^3_+ \to \mathbb{R}$, as *well-behaved* if it is continuous and increasing. An individual-level dataset $\mathcal{D}$ is said to be *rationalizable* if there is a well-behaved utility function U that rationalizes $\mathcal{D}$, in the sense that $U(\mathbf{x}^i) \geqslant U(\mathbf{x})$ for all

$$\mathbf{x} \in \mathcal{B}^i = \{\mathbf{x} \in \mathbb{R}^3_+ : \mathbf{p}^i \cdot \mathbf{x} \leqslant 1\}.$$

We want to know whether each dataset $\mathcal{D}$ is rationalizable in the sense just defined—that is, whether it could have been generated by an individual maximizing a well-behaved utility function. The canonical test for this involves checking the *generalized axiom of revealed preference* (GARP). A well-known result, due to Afriat (1967), tells us that a dataset $\mathcal{D}$ is rationalizable if and only if $\mathcal{D}$ satisfies GARP.[3] GARP requires that if allocation $\mathbf{x}^i$ is revealed preferred to $\mathbf{x}^j$, then $\mathbf{x}^i$ must cost at least as much as $\mathbf{x}^j$ at the prices prevailing when $\mathbf{x}^j$ is chosen, $\mathbf{p}^j \cdot \mathbf{x}^i \geqslant 1$. Put precisely, allocation $\mathbf{x}^i$ is said to be *directly revealed preferred* to $\mathbf{x}^j$, denoted $\mathbf{x}^i \, R^D \, \mathbf{x}^j$, if $\mathbf{p}^i \cdot \mathbf{x}^j \leqslant 1$ (equivalently, $\mathbf{x}^j \in \mathcal{B}^i$) and *directly revealed*

[3]It is straightforward to show that choice data generated by the maximization of a locally nonsatiated utility function must obey GARP. Conversely, and somewhat more difficult to show, if an individual's choice data obey GARP, then they can be rationalized by a well-behaved and concave utility function.

strictly preferred if the inequality is strict. The *revealed preference* relation, denoted R, is the transitive closure of the direct revealed preference relation.[4] GARP requires that if $\mathbf{x}^i \, R \, \mathbf{x}^j$ then $\mathbf{x}^j$ is not directly revealed strictly preferred to $\mathbf{x}^i$. To verify GARP, it is thus necessary to have an efficient way to compute the transitive closure R of the direct revealed preference relation R^D and check that, for every pair of allocations $\mathbf{x}^i$ and $\mathbf{x}^j$ satisfying $\mathbf{x}^i \, R \, \mathbf{x}^j$, we do not have $\mathbf{x}^j$ being directly revealed strictly preferred to $\mathbf{x}^i$.

GARP fails whenever $\mathcal{D}$ contains a revealed preference cycle: a sequence of allocations $\{\mathbf{x}^k\}_{k=1}^K$ with $\mathbf{x}^1 = \mathbf{x}^i$ and $\mathbf{x}^K = \mathbf{x}^i$, such that $\mathbf{x}^k \, R^D \, \mathbf{x}^{k+1}$ for every $k = 1, \ldots, K-1$, where at least one direct revealed preference relation is strict; we refer to such a revealed preference cycle as a GARP violation. The *weak axiom of revealed preference* (WARP) can then be seen as a weakening of GARP which only requires that if $\mathbf{x}^i \, R^D \, \mathbf{x}^j$ then $\mathbf{x}^j$ is not directly revealed strictly preferred to $\mathbf{x}^i$. WARP fails whenever $\mathcal{D}$ contains a pairwise revealed preference cycle $\mathbf{x}^i \, R^D \, \mathbf{x}^j$ and $\mathbf{x}^j \, R^D \, \mathbf{x}^i$, where at least one direct revealed preference relation is strict; we refer to such cycle as a WARP violation. It is straightforward to verify that WARP holds if demand is generated by a complete, but not necessarily transitive, preference ordering (see the Appendix for details).

Choices from two-dimensional budget lines—as collected in previous experiments—cannot satisfy WARP and violate GARP since every GARP violation must contain a WARP violation (see Rose (1958) and Banerjee and Murphy (2006)). This is particularly important because choice data which violate not only GARP but also WARP cannot be rationalized even by a complete but *nontransitive* preference ordering, which excludes some important models of choice under risk (see, for example, Bell (1982), Fishburn (1982), and Loomes and Sugden (1982)).[5] This is not the case if choices are from three-dimensional (or more) budget sets, where it is indeed possible for choice data to satisfy WARP and violate GARP. We can thus provide a more discriminating test of rationalizability by allowing for the separation of

[4] That is, $\mathbf{x}^i \, R \, \mathbf{x}^j$ if there exists a sequence of allocations $\{\mathbf{x}^k\}_{k=1}^K$ with $\mathbf{x}^1 = \mathbf{x}^i$ and $\mathbf{x}^K = \mathbf{x}^j$, such that $\mathbf{x}^k \, R^D \, \mathbf{x}^{k+1}$ for every $k = 1, \ldots, K-1$.

[5] Notice that demand can be well-defined even when preferences are incomplete; in that case, the primitive is an irreflexive binary relation $\succ$ interpreted as a strict preference and $\mathbf{x}$ is a demand bundle in a budget set $\mathcal{B}$ if there is no bundle $\mathbf{y} \in \mathcal{B}$ such that $\mathbf{y} \succ \mathbf{x}$. As Anderson (1981) shows, demand can be well-defined if the preference is incomplete so long as it obeys standard convexity and continuity axioms. A well-known model of incomplete preferences in the context of choice under uncertainty is Bewley (2002), with further generalizations by Galaabaatar and Karni (2013). The existence of demand in Bewley (2002) is guaranteed by Anderson (1981) under mild additional assumptions.

incomplete from complete but nontransitive preferences, a crucial step before jointly testing the additional axioms underpinning EUT.

Since GARP provides an exact test of rationalizability and individual choices almost always involve at least some noise—subjects may optimize incorrectly, or implement optimal choices with imprecision, or err in any other of many possible ways—Afriat (1972, 1973) proposes the notion of the *critical cost-efficiency index* (CCEI) to measure these mistakes/errors. The CCEI is the fraction by which each budget constraint must be reduced in order to remove all violations of GARP. By definition, the CCEI is between 0 and 1: indices closer to 1 mean the data are closer to satisfying GARP and hence to perfect consistency with utility maximization.

Put precisely, given a number $e \in (0, 1]$, we say that a dataset $\mathcal{D}$ is *rationalizable at cost-efficiency* e if there is a well-behaved utility function U such that $U(\mathbf{x}^i) \geqslant U(\mathbf{x})$ for all

$$\mathbf{x} \in \mathcal{B}^i(e) = \{\mathbf{x} \in \mathbb{R}^3_+ : \mathbf{p}^i \cdot \mathbf{x} \leqslant e\}.$$

This concept is a less stringent requirement than (exact) rationalization since $\mathcal{B}^i(e)$ is a subset of $\mathcal{B}^i$ (except when $e = 1$, in which case the concepts coincide). Afriat's (1973) CCEI, denoted e^*, which is associated with the dataset $\mathcal{D}$ is the greatest possible cost-efficiency e at which $\mathcal{D}$ is rationalizable. We can calculate the CCEI e^* based on a modified version of GARP. In the notation introduced above, for any number $e \in (0, 1]$, we define the direct revealed preference relation $R^D(e)$ as $\mathbf{x}^i \, R^D(e) \, \mathbf{x}^j$ if $\mathbf{p}^i \cdot \mathbf{x}^j \leqslant e$ (equivalently, $\mathbf{x}^j \in \mathcal{B}^i(e)$), and we define $R(e)$ to be the transitive closure of $R^D(e)$. We say that $R(e)$ satisfies GARP if, for every pair of allocations $\mathbf{x}^i$ and $\mathbf{x}^j$ where $\mathbf{x}^i \, R(e) \, \mathbf{x}^j$, we have $\mathbf{p}^j \cdot \mathbf{x}^i \geqslant e$. The largest value of e such that the relation $R(e)$ satisfies GARP coincides with e^*.[6]

How should we interpret the CCEI? Suppose a dataset $\mathcal{D}$ is not rationalizable by a well-behaved utility function U. Then, choices must involve at least one error—that is, a budget set $\mathcal{B}^j$ and an allocation $\mathbf{y} \in \mathcal{B}^j$ such that $U(\mathbf{y}) > U(\mathbf{x}^j)$. A money-metric measure of the severity of this error is the fraction of the budget wasted by choosing $\mathbf{x}^j$ instead of $\mathbf{y}$, which is $1 - \mathbf{p}^j \cdot \mathbf{y}$. The CCEI e^* is then obtained by minimizing the budget overspent among *all* well-behaved utility functions. In this sense, for every observation of $(\mathbf{p}^i, \mathbf{x}^i)$, there exists a

[6]By a variation of Afriat's (1967) theorem, we know that $\mathcal{D}$ is rationalizable at cost-efficiency e if and only if $R(e)$ satisfies GARP (Afriat, 1973).

well-behaved utility function U such that $U(\mathbf{x}^i) \geqslant U(\mathbf{x})$ for any allocation $\mathbf{x}$ which is more than $1 - e^*$ percent cheaper than $\mathbf{x}^i$ at the prices $\mathbf{p}^i$ prevailing when $\mathbf{x}^i$ is chosen. The CCEI thus provides the "least costly" adjustment of budget sets that accounts for mistakes/errors. It is a theoretically disciplined measure of noise and it also has a well-established economic interpretation. As Afriat (1973) puts it, the CCEI captures the idea that the decision maker "has a definite structure of wants," but "programs at a level of cost-efficiency e." For a fuller discussion of the interpretation of the CCEI see Polisson and Quah (2024).

3.2 FOSD-Rationalizability

Afriat's (1967) theorem is just the first of a long list of results with the following pattern: an individual-level dataset $\mathcal{D}$ is rationalizable by a well-behaved (continuous and increasing) utility function U belonging to some family if and only if $\mathcal{D}$ obeys some property. For our purposes, two families are particularly important. The first is the family of utility functions that are continuous and increasing with respect to first-order stochastic dominance (FOSD); the latter means that $U(\mathbf{x}'') \geqslant U(\mathbf{x}')$ whenever $F_{\mathbf{x}''} \leqslant F_{\mathbf{x}'}$, where $F_{\mathbf{x}''}$ and $F_{\mathbf{x}'}$ are the resulting (cumulative) payoff distributions, with the inequality being strict if FOSD is strict. We refer to such utility functions as *FOSD-increasing*. Note that every FOSD-increasing utility function is well-behaved since such a utility function must be increasing.[7]

Violations of FOSD might reasonably be regarded as errors in decision making, regardless of underlying risk attitudes—that is, as a failure to recognize that some allocations yield payoff distributions with unambiguously lower returns. Violations of FOSD are therefore compelling as mistakes/errors, and so monotonicity with respect to FOSD is widely embodied in models of decision making under risk.[8] In the Appendix, we cover some prominent examples of FOSD-increasing utility functions—including expected utility theory (EUT) and

[7] A utility function U that is FOSD-increasing must also be increasing (in the sense that $U(\mathbf{x}'') > U(\mathbf{x}')$ whenever $\mathbf{x}'' > \mathbf{x}'$) but the converse is not true. Suppose that there are two equally likely states. Then $U(1,3) > U(2,1)$ if U is FOSD-increasing because $(1,3)$ strictly first-order stochastically dominates $(2,1)$, but no relationship between $U(1,3)$ and $U(2,1)$ is implied by U being increasing.

[8] This is true, for example, of weighted expected utility (Dekel, 1986; Chew, 1989), rank-dependent utility (Quiggin, 1982, 1993), cumulative prospect theory (Tversky and Kahneman, 1992), and (under certain restrictions) reference-dependent risk preferences (Kőszegi and Rabin, 2007). Notably, the original formulation of prospect theory (Kahneman and Tversky, 1979) allows for violations of monotonicity but, partly for this reason, it was reformulated as cumulative prospect theory (Tversky and Kahneman, 1992) to exclude such behavior. For an exception to this rule see, for example, Manzini and Mariotti (2008).

also generalizations of EUT such as the rank-dependent utility (Quiggin, 1982, 1993) and disappointment aversion (Gul, 1991) models—as well as their relationships to one another.

We say that an individual-level dataset $\mathcal{D}$ is *FOSD-rationalizable* if it can be rationalized by an FOSD-increasing utility function. Nishimura, Ok, and Quah (2017) extend Afriat's (1967) theorem to provide a necessary and sufficient condition for $\mathcal{D}$ to be FOSD-rationalizable by strengthening GARP to rule out a larger set of revealed preference cycles. In the case where the states are equally likely (as in our experiment), requiring a utility function to be FOSD-increasing is equivalent to requiring it to be increasing and *symmetric*. An implication of this property is that $\mathcal{D}$ must be free of (within-observation) FOSD violations—clearly, any decision that involves allocating fewer tokens to the cheaper security is incompatible with the maximization of an increasing and symmetric utility function.[9,10] Whenever $\mathcal{D}$ is not exactly FOSD-rationalizable, we can check whether it can be rationalized at cost-efficiency e by an FOSD-increasing utility function and calculate the corresponding CCEI, denoted by e^{**}. Since the family of FOSD-increasing utility functions is contained within the family of well-behaved utility functions, it must be the case that $e^{**} \leqslant e^{*}$.

3.3 EUT-Rationalizability

Within economics there is a vast amount of experimental work which has led to the development of various theoretical alternatives to EUT. The second important family of well-behaved utility functions we analyze therefore contains utility functions that are compatible with EUT. In our experiment with three equally likely states, EUT requires the existence of a continuous and increasing *Bernoulli index* $u : \mathbb{R}_+ \to \mathbb{R}$, such that

$$U(\mathbf{x}) = \tfrac{1}{3}u(x_1) + \tfrac{1}{3}u(x_2) + \tfrac{1}{3}u(x_3),$$

[9]For example, suppose that with two equally likely states, the allocation $(1, 2)$ is chosen at prices $(1, 2)$. However, the allocation $(2, 1)$ is stochastically equivalent to the allocation $(1, 2)$ and yet costs strictly less. This is incompatible with the maximization of an FOSD-increasing utility function since $(2 + \varepsilon, 1 + \varepsilon)$ is affordable for $\varepsilon > 0$ sufficiently small and strictly superior to $(1, 2)$.

[10]Utility functions representing reference-dependent risk preferences (specifically the choice acclimating personal equilibrium model of Kőszegi and Rabin (2007)) can fail to be FOSD-increasing if loss aversion is sufficiently high (see Masatlioglu and Raymond (2016)); however, these preferences are always locally nonsatiated and, in our experimental setting, symmetric. For reasons explained in greater detail in the Appendix, utility functions that are symmetric and locally nonsatiated cannot rationalize any behavior that cannot also be rationalized by a symmetric and increasing utility function. Thus the explanatory power of reference-dependent risk preferences does not extend beyond that of FOSD-increasing utility functions.

for all contingent commodity allocations $\mathbf{x} = (x_1, x_2, x_3)$. We say that an individual-level dataset $\mathcal{D}$ is *EUT-rationalizable* if it can be rationalized by a utility function U taking the expected utility form. Since every such U must be FOSD-increasing, EUT-rationalizability is a stronger requirement than FOSD-rationalizability.

Polisson, Quah, and Renou (2020) develops a test called the *generalized restriction of infinite domains* (GRID) which can be used to characterize individual-level datasets that are EUT-rationalizable. The GRID method replaces the true contingent commodity space (which in our experiment is $\mathbb{R}^3_+$) with a finite set $\mathcal{G}$ of allocations in $\mathbb{R}^3_+$ constructed in a certain manner; a dataset $\mathcal{D}$ is EUT-rationalizable if and only if it can be rationalized by a utility function taking the expected utility form, where only allocations in $\mathcal{G}$ are included in the consumption space (see the Appendix for details). The latter condition gives a viable test of EUT-rationalizability because it can be converted into a problem of solving a finite system of linear inequalities. Using this method, one could also calculate e^{***}, the CCEI corresponding to EUT-rationalizability. Since this family of utility functions is contained within the family of FOSD-increasing utility functions, it must be the case that $e^{***} \leqslant e^{**}$.

3.4 Comparing Scores

To recap, given any individual-level dataset $\mathcal{D}$ we can calculate three rationalizability scores corresponding to three nested models, with

$$1 \geqslant e^{*} \geqslant e^{**} \geqslant e^{***} > 0.$$

There are, of course, other families of utility functions besides these three, and there will be rationalizability scores corresponding to those families as well. In particular, the families of FOSD-increasing utility functions which generalize EUT will *necessarily* have rationalizability scores between e^{**} and e^{***}. The great advantage of measuring—on the same scale—a dataset's consistency with three increasingly stringent models is that it allows us to determine the *source* of the subject's departure from EUT. A subject who is perfectly consistent with EUT will have $1 = e^{*} = e^{**} = e^{***}$, while a subject who is perfectly consistent with any of the familiar non-EUT alternatives that respect FOSD will have $1 = e^{*} = e^{**} > e^{***}$.

Typically, values of e^{***} will be strictly less than one. Crucial to our analysis, the corresponding values of e^{**} will then allow us to make comparisons between the difference

between *perfect* rationalizability and FOSD-rationalizability ($1 - e^{**}$) and the difference between FOSD-rationalizability and EUT-rationalizability ($e^{**} - e^{***}$). A subject for whom $1 - e^{**}$ is (much) smaller than $e^{**} - e^{***}$ could indeed be violating the independence axiom, but such behavior could potentially be explained by a FOSD-increasing utility model which relaxes the independence axiom; on the other hand, a subject for whom $1 - e^{**}$ is (much) larger than $e^{**} - e^{***}$ may or may not be violating the independence axiom but would require a more substantial departure from the standard paradigm to explain such behavior.

3.5 Alternative Measures

The CCEI is by no means the only way of measuring departures from exact rationalizability (or FOSD-rationalizability or EUT-rationalizability). We focus on this index in our empirical analysis for two related reasons. First, it has a natural economic interpretation (see Varian (1990)), and it is the most commonly used measure in the revealed preference literature.[11] Second, it is easy to calculate for the three (increasingly narrow) families of utility functions we examine. We are not aware of any other index where there are computationally efficient ways of calculating its values for the three families of utility functions under consideration.[12]

That said, we *do* carry out some analysis using an alternative, distance-based approach to noise/error, which is somewhat more in line with conventional econometric analysis. In this approach, we measure the distance k_U^i between the observed allocation $\mathbf{x}^i = (x_1^i, x_2^i, x_3^i) \in \mathcal{B}^i$ and another allocation $\mathbf{z}^i = (z_1^i, z_2^i, z_3^i) \in \mathcal{B}^i$, where $\mathbf{z}^i$ maximizes the well-behaved utility function U in the budget set $\mathcal{B}^i$. For example, one could measure the distance between $\mathbf{x}^i$ and $\mathbf{z}^i$ by the greatest difference in expenditure among the three goods, in which case

$$k_U^i := \max_{s=1,2,3} |p_s^i(z_s^i - x_s^i)|.$$

We say that an individual-level dataset $\mathcal{D}$ is *rationalizable* at distance k if there is a well-behaved utility function U such that $k_U^i \leqslant k$ for each observation of $(\mathbf{p}^i, \mathbf{x}^i)$ and we define k^* as the smallest distance k at which $\mathcal{D}$ is rationalizable. Put differently, the dataset $\mathcal{D}$

[11] A small subset of the many studies using the CCEI includes Harbaugh, Krause, and Berry (2001) on children's preferences, Andreoni and Miller (2002) and Fisman, Kariv, and Markovits (2007) on social preferences, and Choi *et al.* (2007a, 2014) and Carvalho, Meier, and Wang (2016) on risk preferences.

[12] Several other cost-efficiency indices have been suggested to measure departures from exact rationalizability (see, for example, Varian (1990)). The relationships between the CCEI and other cost-efficiency indices are discussed in Choi *et al.* (2014) and Halevy, Persitz, and Zrill (2018).

can be made rationalizable by perturbing the observed choices by any distance greater than k^*. In a similar way, we can define k^{**} and k^{***} as the smallest distances k at which $\mathcal{D}$ is FOSD-rationalizable and EUT-rationalizable.

There are known procedures for calculating k^* and k^{**} precisely (see Hu *et al.* (2021) for a description). However, there is no known procedure to calculate k^{***} and so we rely on a method that only gives an upper bound on its value; furthermore, this approximate method works only when datasets have sufficiently few observations. These difficulties limit the scope of our empirical analysis using this index. However, the conclusions from our limited empirical exercise using the distance-based index are broadly consistent with what we obtain from our efficiency-based analysis using the CCEI (see the Appendix for details).

4 Experiment

The experimental procedures described below are identical to those described by Choi *et al.* (2007b) and used by Choi *et al.* (2007a) to study a portfolio choice problem with two states of nature ($s = 1, 2$) and two associated contingent commodities, except that this experiment incorporates three states ($s = 1, 2, 3$) and three contingent commodities.[13,14,15] We conducted the experiment at UC Berkeley and UCLA. The subjects in the experiment were recruited from undergraduate classes at these institutions. Each experimental subject faced 50 independent decision problems. These decision problems were presented using a graphical interface. On a computer screen, subjects saw a graphical representation of a three-dimensional

[13]We are building on the expertise that we have acquired in previous work using the experimental method across different types of individual choice problems. The two-dimensional budget lines graphical interface was introduced by Choi *et al.* (2007b), and used by Choi *et al.* (2007a) with student subjects and by Choi *et al.* (2014) with subjects from a nationally representative sample. The datasets of Choi *et al.* (2007a, 2014) have been analyzed in many papers, including Halevy, Persitz, and Zrill (2018), Polisson, Quah, and Renou (2020), de Clippel and Rozen (2023), and Echenique, Imai, and Saito (2023), among others.

[14]A series of papers employ a similar methodology to study social preferences with different pools of subjects: Fisman, Kariv, and Markovits (2007), Fisman *et al.* (2015), Fisman, Jakiela, and Kariv (2015, 2017), Li, Dow, and Kariv (2017), Li *et al.* (2022), and Fisman *et al.* (2023). The two-person budget line dictator experiment of Fisman, Kariv, and Markovits (2007) is identical to Andreoni and Vesterlund (2001) and Andreoni and Miller (2002) except for presenting the choice problems graphically, allowing a much wider range of budget lines than can be tested using a pencil-and-paper questionnaire method.

[15]The experimental method with three-dimensional budget sets has been used by Ahn *et al.* (2014) to study ambiguity aversion, but so far has not been used to study risk. Halevy and Mayraz (2024) introduces an interface to study even higher-dimensional budget sets. In general, the experimental literature involving budgets has shifted towards using user-friendly graphical interfaces that allow for the quick and efficient elicitation of many decisions per subject. See, for example, Andreoni and Sprenger (2012) on time preferences.

budget set and chose portfolios through a simple "point-and-click."

For each subject, the computer selected 50 budget sets randomly from the set of planes that intersect all axes at or above the 10 token level and at or below the 100 token level, with at least one intercept at or above the 50 token level. The budget sets selected for each subject were independent of one another and of the budget sets selected for other subjects. Subjects were not informed of any state that was actually realized until the end of the experiment. This procedure was repeated until all 50 rounds were completed. At the end of the experiment, the computer randomly selected one of the 50 decision rounds to carry out for payoffs, and token allocations were converted into dollars. The round selected depended solely on chance. Full experimental instructions, including the computer program dialog window, are available in the Appendix.

In order to ascertain the power of an experiment in testing basic rationalizability, Bronars (1987) proposes as a benchmark the choices of a simulated subject who randomizes uniformly among all allocations on each budget set. In our case, we illustrate the power of the experiment in testing EUT-rationalizability by adapting the Bronars (1987) procedure. Specifically, we present the simulated subject with 50 randomly generated budget sets (like the actual subjects); choices are then drawn randomly from these budget sets, but with the additional restriction that the choice data are perfectly FOSD-rationalizable ($e^{**} = 1$). Precise details of the simulation procedure can be found in the Appendix.

Figure 3 depicts the distributions of EUT-rationalizability scores generated by 1,000 such simulated subjects in the two-dimensional (2D) and three-dimensional (3D) budget set experiments. The horizontal axis shows the EUT-rationalizability score values and the vertical axis presents the fraction of simulated subjects with scores above each value. If we choose $e^{***} = 0.9$ as our critical value, we find that just over 20 percent of simulated subjects are EUT-rationalizable above this threshold ($e^{***} > 0.9$) in the 3D experiment. Thus it is clear that the design of our experiment does *not* guarantee that a dataset is (nearly) EUT-rationalizable simply because it is FOSD-rationalizable. It is also noteworthy that the power of the 2D experiment is significantly lower: in that case, more than 80 percent of simulated subjects have $e^{***} > 0.9$.

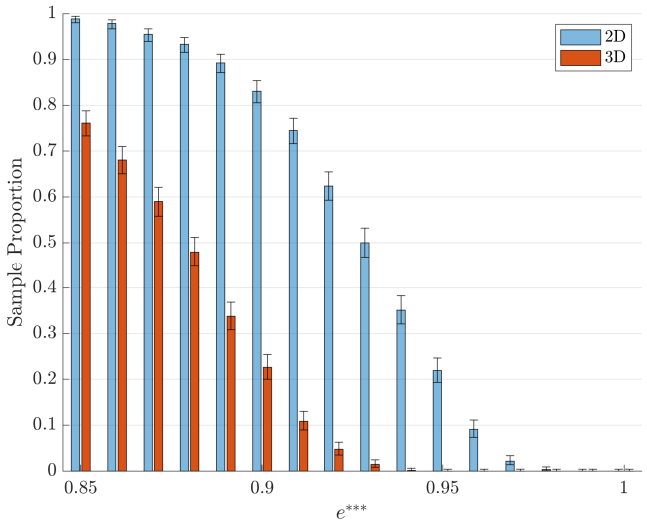


Figure 3: Power of Testing EUT-Rationalizability in 2D vs. 3D Experiments

Simulated subjects whose choices are uniformly distributed on each budget set subject to perfect FOSD-rationalizability ($e^{**} = 1$). Each simulated subject made 50 choices from randomly generated budget sets in the same way as the human subjects. The horizontal axis shows the EUT-rationalizability score values and the vertical axis presents the fraction of simulated subjects in 2D and 3D experiments with scores above each value. The braces indicate 95 percent confidence intervals on these proportions.

5 Experimental Results

In this section, we present the experimental results. The data from the experiment contain observations on 168 individual subjects. For each subject, we have a set of 50 observations $\mathcal{D} := \{(\mathbf{p}^i, \mathbf{x}^i)\}_{i=1}^{50}$, where $\mathbf{p}^i = (p_1^i, p_2^i, p_3^i)$ denotes the i-th observation of the price vector and $\mathbf{x}^i = (x_1^i, x_2^i, x_3^i)$ denotes the corresponding allocation. The experiment therefore provides a large amount of data consisting of many individual choices over a wide range of budget sets. This is a crucial point because the (within-subject) power of the experiment depends upon two factors: the frequency of the intersection of the budgets and the number of decisions.

5.1 Illustrative Subjects

In the introduction, we provide an overview of the important aggregate features of our experimental data, which we summarize by reporting the distributions of our cost-efficiency indices of rationalizability (e^*), FOSD-rationalizability (e^{**}), and EUT-rationalizability (e^{***}). But the aggregate data tell us little about the choice behavior of individual subjects. To get some idea of the wide range of observed behaviors, we present in Figure 4 scatterplots depicting all

50 choices for five illustrative subjects. We have chosen subjects whose behavior corresponds to one of several prototypical choices and illustrates the striking regularity within subjects and heterogeneity across subjects that is characteristic of our data. These selected case studies also help us get a broad sense about some of the common challenges to rationalizability.

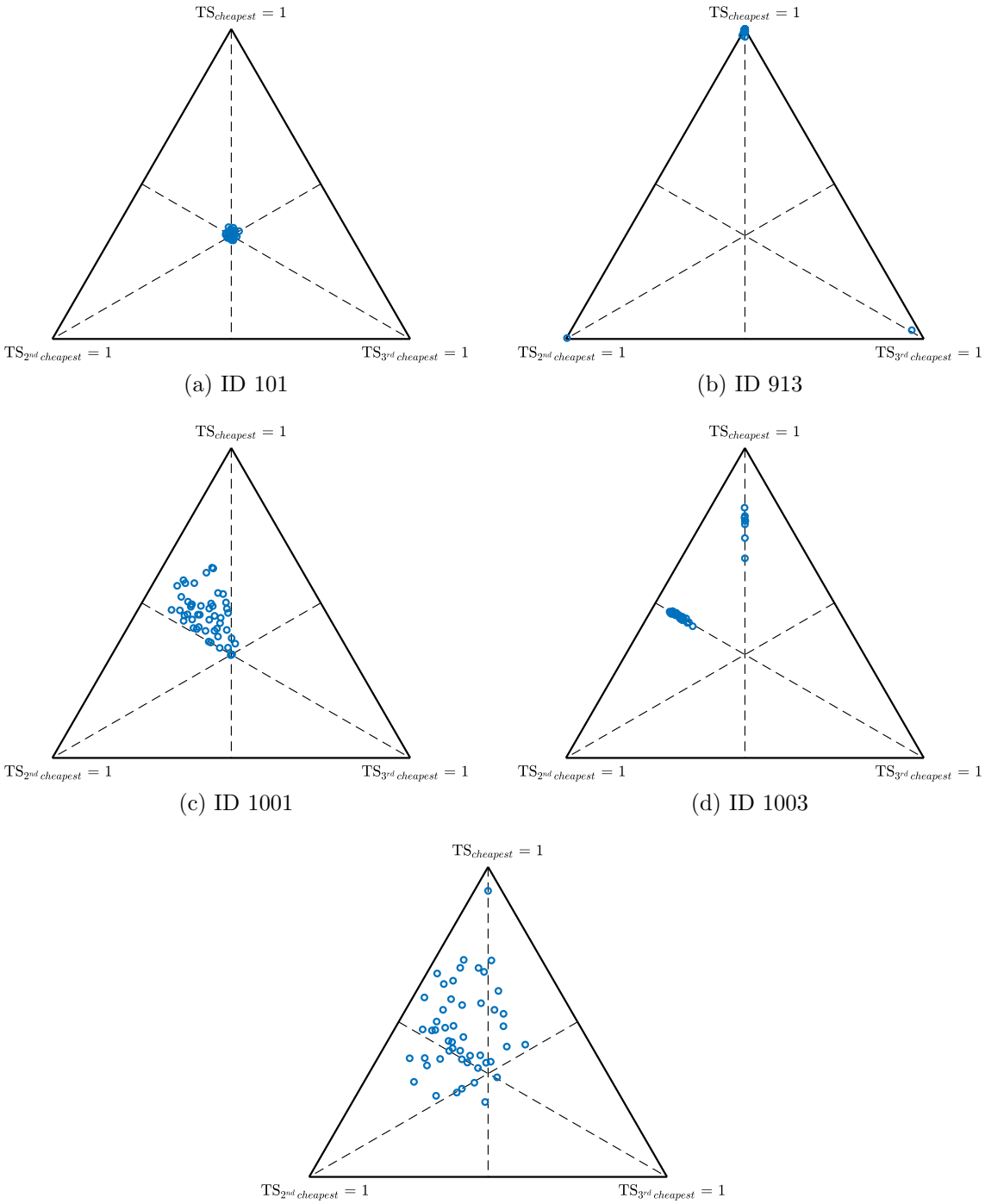
Figure 4 depicts the choices in terms of token shares for the three securities as points in the unit simplex. For each allocation $\mathbf{x}^i = (x_1^i, x_2^i, x_3^i)$, we relabel the states $s = 1, 2, 3$, so that $p_1^i < p_2^i < p_3^i$ and define the *token share* of the security that pays off in state s to be the number of tokens payable in state s as a fraction of the sum of tokens payable across states

$$\bar{x}_s^i = \frac{x_s^i}{x_1^i + x_2^i + x_3^i},$$

and $\bar{\mathbf{x}}^i = (\bar{x}_1^i, \bar{x}_2^i, \bar{x}_3^i)$ is the vector of token shares corresponding to the allocation $\mathbf{x}^i$. Each panel of Figure 4 contains a scatterplot of the token share vectors corresponding to the 50 allocations chosen by one of the five illustrative subjects. The vertices of the unit simplex correspond to allocations consisting of one of the three securities, and each point in the simplex represents an allocation as a convex combination of the extreme points.

The behaviors of the first three subjects are roughly EUT-rationalizable. In the scatterplot for subject ID 101 (Figure 4a), all of the vectors of token shares lie near the *center* of the simplex where $\bar{\mathbf{x}}^i = \left(\frac{1}{3}, \frac{1}{3}, \frac{1}{3}\right)$; this behavior is consistent with infinite risk aversion. In the scatterplot for subject ID 913 (Figure 4b), the token shares are all concentrated on (or, in a few cases, adjacent to) the top *vertex* of the simplex where $\bar{\mathbf{x}}^i = (1, 0, 0)$; this behavior is consistent with risk neutrality. A more complex behavior is illustrated in the scatterplot for subject ID 1001 (Figure 4c). The choices of this subject roughly equalize expenditures $p_1^i x_1^i = p_2^i x_2^i = p_3^i x_3^i$, rather than tokens, across the three securities; this behavior is consistent with maximizing a logarithmic von Neumann-Morgenstern expected utility function.

The next two subjects are *not* EUT-rationalizable. In the scatterplot for subject ID 1003 (Figure 4d), all token shares lie roughly along the *bisectors* of the angles of the simplex where $\bar{x}_1^i = \bar{x}_2^i$ or $\bar{x}_2^i = \bar{x}_3^i$; this behavior—equalizing the demands for two out of the three securities for a non-negligible set of price vectors—is FOSD-rationalizable (because $\bar{x}_1^i \geqslant \bar{x}_2^i \geqslant \bar{x}_3^i$ while $p_1^i < p_2^i < p_3^i$) but not EUT-rationalizable. However, preferences generated by, for example, rank-dependent utility (Quiggin, 1982, 1993) could give rise to such choices. Finally, in the



(a) ID 101

(b) ID 913

(c) ID 1001

(d) ID 1003

(e) ID 1105

Figure 4: Subject Behavior

Each plot shows all 50 choices for a single subject in terms of token shares. Each vertex of the unit simplex corresponds to a full allocation to one of the three securities. Some subjects are roughly EUT-rationalizable: (a) ID 101 is consistent with infinite risk aversion; (b) ID 913 is consistent with risk neutrality; (c) ID 1001 is consistent with the maximization of logarithmic von Neumann-Morgenstern expected utility. Some subjects are distinctly *not* EUT-rationalizable: (d) ID 1003 is FOSD-rationalizable and could be explained by rank-dependent utility; and (e) ID 1105 is not FOSD-rationalizable.

		Violations			CCEI Scores	
		WARP	GARP	GARP not WARP	WARP	GARP
Percentile Values	1	0	0	0	0.668	0.668
	5	0	0	0	0.787	0.787
	10	0	0	0	0.828	0.828
	25	1	1	0	0.916	0.913
	50	4	7	0	0.969	0.968
	75	8	318	0	0.996	0.996
	90	14	181,655	1	1.000	1.000
	95	25	$\geqslant 10^7$	7	1.000	1.000
	99	55	$\geqslant 10^7$	24	1.000	1.000

Table 1: Number of WARP/GARP Violations and CCEI Scores

Percentile values of the numbers of WARP violations, GARP violations, and GARP violations that do not contain a WARP violation, along with the CCEI scores required to eliminate all WARP and GARP violations. Due to computational constraints, we are unable to fully compute the number of GARP violations that do not contain a WARP violation for three subjects (1.8 percent), each of whom has more than 10^7 GARP violations (even after removing WARP violations). For these subjects, we provide a lower bound on the number of GARP violations that do not contain a WARP violation.

scatterplot for subject ID 1105 (Figure 4e), the token shares are not confined to the top left subset of the simplex where $\bar{x}_1^i \geqslant \bar{x}_2^i \geqslant \bar{x}_3^i$; such behavior is not FOSD-rationalizable (and thus also not EUT-rationalizable).[16]

5.2 WARP and GARP

Before analyzing the various measures of rationalizability, we first provide a basic description of the individual-level revealed preference violations. Table 1 reports percentile values of the numbers of WARP violations, GARP violations, and GARP violations that do not contain a WARP violation, along with the CCEI scores required to remove all violations of WARP and GARP. Recall that the number of GARP violations is the number of distinct revealed preference cycles, and that a WARP violation is a special case of a GARP violation involving a pairwise revealed preference cycle (see Section 3.1). With only two goods, all GARP violations must contain one or more WARP violations, which is not the case with three (or more) goods.

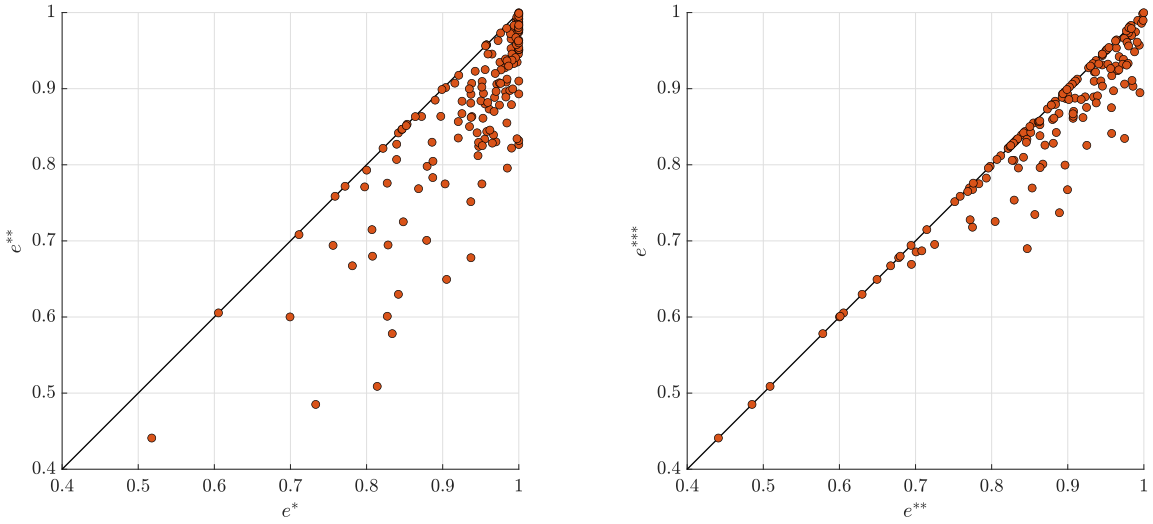
[16]We have shown just a small and selected subset of the full set of subjects, and these are of course special cases where regularities in the data are very clear. For most subjects, the behavioral regularities are much less clear. However, a full review of the data reveals both regularities within subjects and heterogeneity across subjects. The scatterplots for the full set of subjects are available upon request.

We see from Table 1 that the median number of WARP violations is 4 and the median number of GARP violations is 7. While the number of GARP violations is very high among a small number of subjects, the vast majority of these GARP violations contain WARP violations. In fact, only 23 subjects (13.7 percent) have violations of GARP that do not contain a violation of WARP. Furthermore, *all* of the 27 subjects (16.1 percent) who are free of WARP violations are also free of GARP violations. In other words—and importantly for our analysis—every subject who violates GARP also violates WARP, and so cannot be rationalized by a complete preference, even if it is allowed to be nontransitive. More generally, we can (for each subject) calculate the (usual) CCEI and also the CCEI which measures the amount by which each budget constraint needs to be reduced in order to remove all violations of WARP. The latter must (by definition) be weakly greater than the former, but the scores turn out to be identical for all but 4 subjects (2.4 percent). From the last two columns of Table 1 we see that the distributions of the two CCEIs are almost identical, suggesting once again a limited role for models that allow for nontransitive preferences.

5.3 Rationalizability Scores

As a first basic check on the rationalizability (e^*), FOSD-rationalizability (e^{**}), and EUT-rationalizability (e^{***}) of individual subjects, Figure 5 shows scatterplots of e^* against e^{**} (Figure 5a) and of e^{**} against e^{***} (Figure 5b). By definition, $e^* \geqslant e^{**} \geqslant e^{***}$, so all points in both scatterplots must lie on or below the 45-degree line. An individual subject who is perfectly EUT-rationalizable will have $1 = e^* = e^{**} = e^{***}$. When e^{***} is strictly less than one, the corresponding values of e^* and e^{**} will then allow us to isolate the source of the subject's departure from EUT.

Out of our 168 subjects, the choices of only 27 subjects (16.1 percent) are perfectly rationalizable ($e^* = 1$), but the choices of *none* of our subjects are perfectly FOSD-rationalizable ($e^{**} = 1$), and hence perfectly EUT-rationalizable ($e^{***} = 1$). Most interestingly, only 11 subjects (6.5 percent) fall along the 45-degree line in the scatterplot of e^* against e^{**} (Figure 5a); the choices of these subjects are not necessarily perfectly rationalizable but they are not less FOSD-rationalizable than they are rationalizable ($e^* = e^{**}$). By contrast, 65 subjects (38.7 percent) fall along the 45-degree line in the scatterplot of e^{**} against e^{***} (Figure 5b);



(a) e^* vs. e^{**} (b) e^{**} vs. e^{***}

Figure 5: Individual-Level Rationalizability Scores

Rationalizability scores for individual subjects. (a) e^* (horizontal axis) vs. e^{**} (vertical axis) and (b) e^{**} (horizontal axis) vs. e^{***} (vertical axis). By definition, $e^* \geqslant e^{**} \geqslant e^{***}$, so all points in both scatterplots must lie on or below the 45-degree line.

the choices of these subjects are not perfectly FOSD-rationalizable but they are not less EUT-rationalizable than they are FOSD-rationalizable ($e^{**} = e^{***}$). Only 3 subjects (1.8 percent), fall along the 45-degree line in both scatterplots; the choices of these subjects are not less EUT-rationalizable than they are rationalizable ($e^* = e^{**} = e^{***}$).

Furthermore, we compare the *magnitudes* of differences between scores. Figure 6 shows a scatterplot of the difference between *perfect* rationalizability and FOSD-rationalizability ($1 - e^{**}$) against the difference between FOSD-rationalizability and EUT-rationalizability ($e^{**} - e^{***}$). Out of our 168 subjects, 143 (85.1 percent) fall below the 45-degree line in the scatterplot, so the difference between perfect rationalizability and FOSD-rationalizability ($1 - e^{**}$) is larger for these subjects than the difference between FOSD-rationalizability and EUT-rationalizability ($e^{**} - e^{***}$). Furthermore, for 125 subjects (74.4 percent) the difference between perfect rationalizability and FOSD-rationalizability ($1 - e^{**}$) is *twice* as large as the difference between FOSD-rationalizability and EUT-rationalizability ($e^{**} - e^{***}$). Finally, 65 subjects (45.5 percent) fall along the horizontal axis ($e^{**} = e^{***}$). For these subjects, there is no difference between FOSD-rationalizability and EUT-rationalizability.

Hence, for the vast majority of our subjects there is only a small (or no) difference between FOSD-rationalizability and EUT-rationalizability ($e^{**} - e^{***}$), whereas the difference

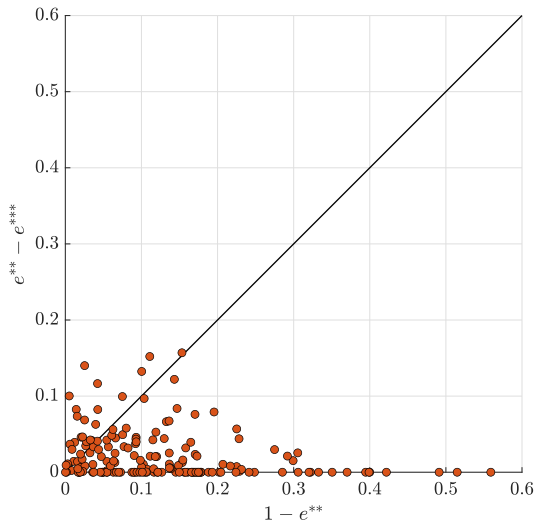


Figure 6: Differences-in-Differences in Individual-Level Rationalizability Scores

Differences between perfect rationalizability and FOSD rationalizability ($1 - e^{**}$) (horizontal axis) vs. differences between FOSD-rationalizability and EUT-rationalizability ($e^{**} - e^{***}$) (vertical axis).

between perfect rationalizability and FOSD-rationalizability ($1 - e^{**}$) is much larger. For these subjects, there is little scope for the most prominent non-EUT alternatives that relax the independence axiom, such as weighted expected utility, rank-dependent utility, or reference-dependent risk preferences, to explain observed behavior, since they all postulate FOSD-rationalizability ($1 = e^{*} = e^{**} > e^{***}$).

5.4 Difference-in-Differences

Our individual-level data also allow us to compare for each subject the gap between perfect rationalizability and FOSD-rationalizability to the gap between FOSD-rationalizability and EUT-rationalizability, using a *statistical* difference-in-differences approach. Our econometric test is purely nonparametric, in the sense that we make no functional form assumptions about subjects' underlying preferences or on the error structure. To construct a test statistic and sampling distribution at the individual level, we leverage a repeated subsampling of half a subject's data, with each subsample consisting of 25 observations.

Specifically, for each subject we begin by randomly drawing 1,000 subsets of 25 observations sampled *without replacement* from the full dataset of 50 observations. We then calculate for each subset r of 25 observations a pair of FOSD-rationalizability ($e_r^{\dagger\dagger}$) and EUT-rationalizability ($e_r^{\dagger\dagger\dagger}$) scores. Let $\bar{e}^{\dagger\dagger}$ and $\bar{e}^{\dagger\dagger\dagger}$ then denote sample averages over the

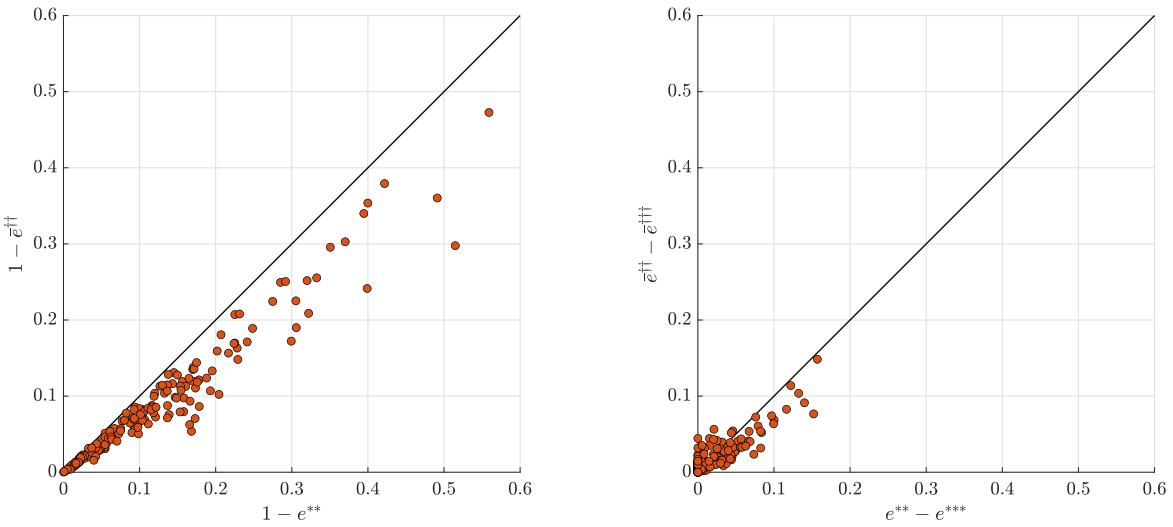
1,000 scores $\{(e_r^{\dagger\dagger}, e_r^{\dagger\dagger\dagger})\}_{r=1}^{1,000}$ for each subject, from which we can calculate the sample mean of the difference-in-differences $(1-\bar{e}^{\dagger\dagger})-(\bar{e}^{\dagger\dagger}-\bar{e}^{\dagger\dagger\dagger})$.

To understand the relationship between the difference-in-differences based on 25 observations $(1-\bar{e}^{\dagger\dagger})-(\bar{e}^{\dagger\dagger}-\bar{e}^{\dagger\dagger\dagger})$ and 50 observations $(1-e^{**})-(e^{**}-e^{***})$, note that $\bar{e}^{\dagger\dagger} \geqslant e^{**}$ and $\bar{e}^{\dagger\dagger\dagger} \geqslant e^{***}$ since the rationalizability scores can be no lower when based on a subset of the data. Importantly, the difference between the distributions of $\bar{e}^{\dagger\dagger}$ and e^{**} is actually very similar to the difference between the distributions of $\bar{e}^{\dagger\dagger\dagger}$ and e^{***}. The mean and median of $\bar{e}^{\dagger\dagger}$ are 0.908 and 0.929 compared to 0.875 and 0.898 for e^{**}, and the mean and median of $\bar{e}^{\dagger\dagger\dagger}$ are 0.888 and 0.906 compared to 0.852 and 0.874 for e^{***}. Hence, the differences between the means and medians are all in the range of 0.031 to 0.036. The rationalizability scores using 25 and 50 observations are also very highly correlated—the correlation coefficient between $\bar{e}^{\dagger\dagger}$ and e^{**} is 0.970, and between $\bar{e}^{\dagger\dagger\dagger}$ and e^{***} the correlation coefficient is 0.978.[17]

To better illustrate the relationship between the difference-in-differences based on 25 and 50 observations, Figure 7a shows a scatterplot of $1-e^{**}$ against $1-\bar{e}^{\dagger\dagger}$ (the differences between perfect rationalizability and FOSD-rationalizability) and Figure 7b shows a scatterplot of $e^{**}-e^{***}$ against $\bar{e}^{\dagger\dagger}-\bar{e}^{\dagger\dagger\dagger}$ (the differences between FOSD-rationalizability and EUT-rationalizability). Since $\bar{e}^{\dagger\dagger} \geqslant e^{**}$ and $\bar{e}^{\dagger\dagger\dagger} \geqslant e^{***}$, $1-e^{**}$ must be as large as $1-\bar{e}^{\dagger\dagger}$, whereas $e^{**}-e^{***}$ can be larger or smaller than $\bar{e}^{\dagger\dagger}-\bar{e}^{\dagger\dagger\dagger}$. Thus, all points in Figure 7a must lie on or below the 45-degree line, while points in Figure 7b can lie below or above the 45-degree line. The mean and median of $1-\bar{e}^{\dagger\dagger}$ are 0.092 and 0.071 compared to 0.125 and 0.102 for $1-e^{**}$. In contrast, the mean and median of $\bar{e}^{\dagger\dagger}-\bar{e}^{\dagger\dagger\dagger}$ and $e^{**}-e^{***}$ are nearly identical—0.020 and 0.023 compared to 0.023 and 0.024. Thus, while using half a subject's data necessarily reduces the difference between perfect rationalizability and FOSD-rationalizability, it need not reduce the difference between FOSD-rationalizability and EUT-rationalizability, making it more difficult to demonstrate that the former is greater than the latter.

We are now ready to present our individual-level difference-in-differences statistical test. For each subject, we define the difference-in-differences

[17] As a practical note, the high concordance in the two sets of scores—$(e^{\dagger\dagger}, e^{\dagger\dagger\dagger})$ and (e^{**}, e^{***})—suggests that subjects' mistakes/errors are unlikely to be due to occasionally lapsing into quasi-random behavior and/or adopting a low-effort heuristic that would generate more dispersion between their scores for half the dataset $(e^{\dagger\dagger}, e^{\dagger\dagger\dagger})$ and for the full dataset (e^{**}, e^{***}). We also note that datasets containing 25 observations include enough decisions across a wide range of budget sets to conduct powerful tests of consistency.



(a) $1 - e^{**}$ vs. $1 - \bar{e}^{\dagger\dagger}$ (b) $e^{**} - e^{***}$ vs. $\bar{e}^{\dagger\dagger} - \bar{e}^{\dagger\dagger\dagger}$

Figure 7: Individual-Level Differences in Rationalizability Scores (50 vs. 25 Decisions)

(a) Differences between perfect rationalizability and FOSD-rationalizability ($1 - e^{**}$) with 50 decisions (horizontal axis) vs. average differences ($1 - \bar{e}^{\dagger\dagger}$) with 25 decisions (vertical axis). (b) Differences between FOSD-rationalizability and EUT-rationalizability ($e^{**} - e^{***}$) with 50 decisions (horizontal axis) vs. average differences ($\bar{e}^{\dagger\dagger} - \bar{e}^{\dagger\dagger\dagger}$) with 25 decisions (vertical axis). By definition, $\bar{e}^{\dagger\dagger} \geqslant e^{**}$ and $\bar{e}^{\dagger\dagger\dagger} \geqslant e^{***}$, so all points in (a) must lie on or below the 45-degree line, while points in (b) can lie below or above the 45-degree line.

$$D_1 = (1 - \mu_{e^{\dagger\dagger}}) - (\mu_{e^{\dagger\dagger}} - \mu_{e^{\dagger\dagger\dagger}}),$$

where $\mu_{e^{\dagger\dagger}}$ and $\mu_{e^{\dagger\dagger\dagger}}$ denote the expected FOSD-rationalizability and EUT-rationalizability scores for a subject over all possible datasets consisting of 25 observations. For each subject, we test the null hypothesis that $D_1 = 0$. That is, that there is no difference between $1 - \mu_{e^{\dagger\dagger}}$ (the mean difference between perfect rationalizability and FOSD-rationalizability) and $\mu_{e^{\dagger\dagger}} - \mu_{e^{\dagger\dagger\dagger}}$ (the mean difference between FOSD-rationalizability and EUT-rationalizability).

To estimate D_1, we use the repeated subsampling procedure described above to create the sample averages $\bar{e}^{\dagger\dagger}$ and $\bar{e}^{\dagger\dagger\dagger}$, which serve as estimators for $\mu_{e^{\dagger\dagger}}$ and $\mu_{e^{\dagger\dagger\dagger}}$. By definition, the expected values of the scores are equal to the mean scores: $\mathbb{E}[e_r^{\dagger\dagger}] = \mu_{e^{\dagger\dagger}}$ and $\mathbb{E}[e_r^{\dagger\dagger\dagger}] = \mu_{e^{\dagger\dagger\dagger}}$. By linearity of the expectation function, the sample averages are also equal in expectation to the mean scores: $\mathbb{E}[\bar{e}^{\dagger\dagger}] = \mu_{e^{\dagger\dagger}}$ and $\mathbb{E}[\bar{e}^{\dagger\dagger\dagger}] = \mu_{e^{\dagger\dagger\dagger}}$. The estimator is thus defined as

$$\bar{D}_1 = (1 - \bar{e}^{\dagger\dagger}) - (\bar{e}^{\dagger\dagger} - \bar{e}^{\dagger\dagger\dagger}),$$

where $\mathbb{E}[\bar{D}_1] = D_1$.

Because the subsampled scores $e_r^{\dagger\dagger}$ and $e_r^{\dagger\dagger\dagger}$ are not statistically independent, the dis-

tribution of $\bar{D}_1$ is not analytically known. We thus use a bootstrap procedure to generate an empirical distribution approximating the sampling distribution of $\bar{D}_1$. To obtain the bootstrapped distribution, we re-sample *with replacement* from the 1,000 pairs of scores $\{(e_r^{\dagger\dagger}, e_r^{\dagger\dagger\dagger})\}_{r=1}^{1,000}$ to create a bootstrapped sample of size 1,000. For this sample we calculate the bootstrapped sample averages and the test statistic $\bar{D}_{1,b}$. We repeat this procedure 10^6 times to get a bootstrapped distribution $\{\bar{D}_{1,b}\}_{b=1}^{10^6}$, which is centered on $\bar{D}_1$ and has a standard deviation which we can use to conduct a t-test under our null hypothesis.

Out of our 168 subjects, $\bar{D}_1$ is positive and statistically significantly different from zero at the 1 percent level for 142 subjects (84.5 percent). The $\bar{e}^{\dagger\dagger}$ scores for 112 (66.7 percent) and 58 (34.5 percent) of our subjects are above 0.9 and 0.95, respectively. Even out of these highly FOSD-rationalizable subjects with $\bar{e}^{\dagger\dagger}$ scores above 0.9 and 0.95, $\bar{D}_1$ is positive and statistically significantly different from zero at the 1 percent significance level for 87 (77.7 percent) and 38 (65.5 percent) subjects, respectively.

We strengthen our difference-in-differences test by using a "double-differencing" strategy under the null hypothesis is that $1 - \mu_{e^{\dagger\dagger}}$ is *twice* as large as $\mu_{e^{\dagger\dagger}} - \mu_{e^{\dagger\dagger\dagger}}$. We thus define

$$D_2 = (1 - \mu_{e^{\dagger\dagger}}) - 2(\mu_{e^{\dagger\dagger}} - \mu_{e^{\dagger\dagger\dagger}})$$

and test the null hypothesis that $D_2 = 0$ using an analogous procedure to that described above for testing the hypothesis on D_1. We find that $\bar{D}_2$ it is positive and statistically significantly different from zero at the 1 percent level for 118 out of 168 subjects (70.2 percent). To summarize our analysis, Figure 8 shows the rationalizability score differences for individual subjects. Subjects are depicted in blue if only $\bar{D}_1$ is positive and statistically significant at the 1 percent level, in red if both $\bar{D}_1$ and $\bar{D}_2$ are positive and statistically significant, and in gray if neither.

5.5 Departures from FOSD-Rationalizability and EUT-Rationalizability

Our nonparametric individual-level difference-in-differences analysis indicates that, for the vast majority of subjects, the gap between FOSD-rationalizability and EUT-rationalizability ($e^{\dagger\dagger} - e^{\dagger\dagger\dagger}$) is small compared to the gap between perfect rationalizability and FOSD-rationalizability ($1 - e^{\dagger\dagger}$). The important implication of this result is that while prominent non-EUT models, which are monotone with respect to FOSD, must weakly outperform EUT

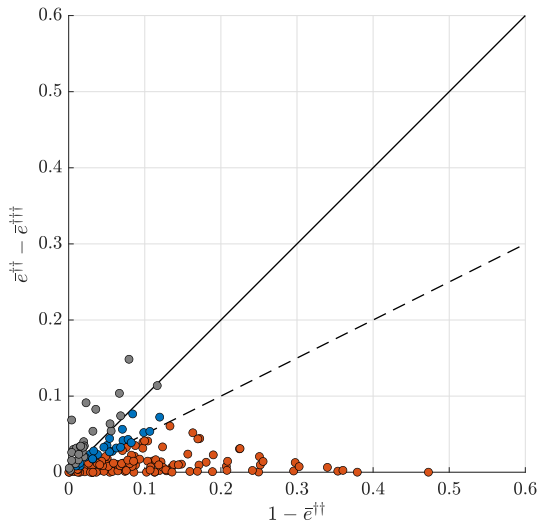


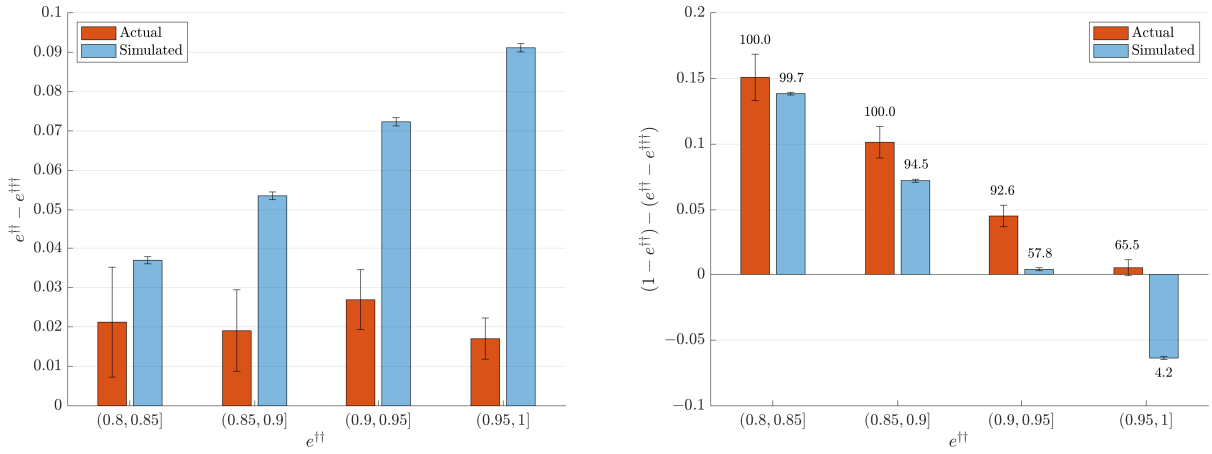
Figure 8: Statistical Tests of Difference-in-Differences in Individual-Level Rationalizability Scores

Differences between perfect rationalizability and FOSD rationalizability ($1 - \bar{e}^{\dagger\dagger}$) (horizontal axis) vs. differences between FOSD-rationalizability and EUT-rationalizability ($\bar{e}^{\dagger\dagger} - \bar{e}^{\dagger\dagger\dagger}$) (vertical axis). The solid line represents the D_1 test, while the dashed line represents the D_2 test. Subjects are depicted in blue if only $\bar{D}_1$ is positive and statistically significant, in red if both $\bar{D}_1$ and $\bar{D}_2$ are positive and statistically significant, and in gray if neither, all at the 1 percent level.

(since EUT is a special case of these models), their superior performance can only be marginal for the vast majority of subjects. This can occur because subjects do not systematically deviate from EUT-rationalizability in ways that non-EUT alternatives capture, and/or because subjects either idiosyncratically or systematically deviate from FOSD-rationalizability on which the prominent non-EUT models are also based.

To discern between departures from FOSD-rationalizability and EUT-rationalizability, we generate a benchmark comparison using simulated subjects whose choices are uniformly distributed on each budget set, conditional on different FOSD-rationalizability score $e^{\dagger\dagger}$ thresholds. In other words, the simulated subjects' choices are consistent with maximizing a utility function, subject to some degree of error as measured by $e^{\dagger\dagger}$. The simulated subjects make choices from 25 budget sets drawn from the same distribution as the actual subjects. For each simulated subject, we calculate the EUT-rationalizability score $e^{\dagger\dagger\dagger}$, the difference between FOSD-rationalizability and EUT-rationalizability $e^{\dagger\dagger} - e^{\dagger\dagger\dagger}$, and the resulting difference-in-differences $(1 - e^{\dagger\dagger}) - (e^{\dagger\dagger} - e^{\dagger\dagger\dagger})$. Precise details of the simulation procedure can be found in the Appendix, and the results are shown in Figure 9.

The horizontal axes in both panels of Figure 9 present FOSD-rationalizability score $e^{\dagger\dagger}$



(a) $e^{\dagger\dagger}$ versus $e^{\dagger\dagger} - e^{\dagger\dagger\dagger}$ (b) $e^{\dagger\dagger}$ versus $(1 - e^{\dagger\dagger}) - (e^{\dagger\dagger} - e^{\dagger\dagger\dagger})$

Figure 9: Departures from FOSD-Rationalizability and EUT-Rationalizability for Actual vs. Simulated Subjects

The horizontal axis in both panels show different threshold FOSD-rationalizability $e^{\dagger\dagger}$ score value bins. The vertical axis in (a) represents the mean difference between FOSD-rationalizability and EUT-rationalizability $e^{\dagger\dagger} - e^{\dagger\dagger\dagger}$, while the vertical axis in (b) represents the mean of the resulting difference-in-differences $(1 - e^{\dagger\dagger}) - (e^{\dagger\dagger} - e^{\dagger\dagger\dagger})$. The braces in both panels represent 95 percent confidence intervals around the mean. The numbers above the bars in (b) are the fractions of actual and simulated subjects for whom the difference-in-differences is positive.

value bins: $(0.8, 0.85]$, $(0.85, 0.9]$, $(0.9, 0.95]$, and $(0.95, 1]$. For each score value bin, we generated 5,000 simulated subjects. The numbers of actual subjects in each bin are 10 (6.0 percent), 28 (16.7 percent), 54 (32.1 percent), and 58 (34.5 percent), respectively. The bars in Figure 9a represent the mean difference between FOSD-rationalizability and EUT-rationalizability $e^{\dagger\dagger} - e^{\dagger\dagger\dagger}$, and in Figure 9b the mean of the resulting difference-in-differences $(1 - e^{\dagger\dagger}) - (e^{\dagger\dagger} - e^{\dagger\dagger\dagger})$. The braces in both panels of Figure 9 represent 95 percent confidence intervals. The numbers above the bars in Figure 9b are the fractions of actual and simulated subjects for whom the difference-in-differences is positive $(1 - e^{\dagger\dagger} > e^{\dagger\dagger} - e^{\dagger\dagger\dagger})$. We note that for the actual subjects, all of whom made 50 choices, we use the average scores $\bar{e}^{\dagger\dagger}$ and $\bar{e}^{\dagger\dagger\dagger}$ obtained from drawing 1,000 subsamples of 25 choices.

Figure 9a shows that the mean difference between FOSD-rationalizability and EUT-rationalizability $e^{\dagger\dagger} - e^{\dagger\dagger\dagger}$ is larger for the simulated subjects at all FOSD-rationalizability score $e^{\dagger\dagger}$ threshold levels. Clearly, because within each bin the actual and simulated subjects have the same FOSD-rationalizability scores $e^{\dagger\dagger}$, the difference is due to the latter having lower EUT-rationalizability scores $e^{\dagger\dagger\dagger}$. We thus conclude that the behavior of

the actual subjects cannot be explained by FOSD-rationalizability subject to some error—characteristic of the simulated subjects—as this would result in lower EUT-rationalizability scores. Due to their greater consistency with EUT-rationalizability—for each level of FOSD-rationalizability—the mean difference-in-differences $(1 - e^{\dagger\dagger}) - (e^{\dagger\dagger} - e^{\dagger\dagger\dagger})$ among our actual subjects is significantly higher than that of the simulated subjects, as shown in Figure 9b.

Overall, 85.7 percent of actual subjects have positive difference-in-differences but there is considerable heterogeneity across FOSD-rationalizability threshold values, as also shown in Figure 9b. In the lowest bin $(0.8, 0.85]$, the difference-in-differences is positive for all actual subjects and nearly all simulated subjects. In the highest bin $(0.95, 1]$, it is positive for 65.5 percent of actual subjects and 4.2 percent of simulated subjects. Additionally, 83.3 percent of the actual subjects have $e^{\dagger\dagger}$ scores above 0.85 and 66.7 percent have scores above 0.9, where the experiment discriminates best between the actual and simulated subjects.[18]

5.6 Further Analysis

The broad conclusion from our analysis of rationalizability scores is clear: there could be multiple sources of EUT violations even for a single subject and, for a large majority of subjects, violations of ordering and monotonicity are prominent and greater in magnitude than violations of the independence axiom. This finding limits the applicability of the most prominent non-EUT alternatives, such as weighted expected utility or rank-dependent utility, to explain observed behavior since they all postulate FOSD-rationalizability. In this subsection we discuss further analysis that confirms the robustness of these findings.

5.6.1 Distance-Based Indices

In additional to our analysis using the CCEI, we also carry out a difference-in-differences analysis using the distance-based index (explained in Section 3.5). Such an analysis requires us to calculate the index for FOSD-rationalizability and EUT-rationalizability. The former can be obtained following Hu *et al.* (2021), but we are not aware of any computationally efficient method of calculating the index for EUT-rationalizability. To get around this dif-

[18] In recent work using our data, Ellis, Kariv, and Ozbay (2024a,b) calculate the completeness (Fudenberg *et al.*, 2022) of parametric EUT and non-EUT models. These results are consistent with the findings of our nonparametric analysis as we discuss in the related literature in Section 6.

ficulty, we divide each subject's 50 observations into five subsets of 10 observations each. For each of these "mini datasets" we have a way of calculating an upper bound on the index for EUT-rationalizability. We then obtain, for each subject and on each subset of 10 observations, the index for FOSD-rationalizability k^{**} and an *upper bound* on the index for EUT-rationalizability k_u^{***}. Taking an average over the five subsets of 10 observations, we obtain for each subject, $\bar{k}^{**}$ and $\bar{k}_u^{***}$. Obviously, the average value of the index for EUT-rationalizability, $\bar{k}^{***}$, must satisfy $\bar{k}^{***} \leqslant \bar{k}_u^{***}$.

Since EUT-rationalizability is more stringent than FOSD-rationalizability, we must have $\bar{k}^{***} \geqslant \bar{k}^{**}$. The central question is about the relative size of the gap—at its most extreme, if $\bar{k}^{***} \gg \bar{k}^{**} \approx 0$, then we conclude that the agent could be maximizing an FOSD-increasing utility function but is not EUT-rationalizable. Out of our 168 subjects, we find that $2(\bar{k}_u^{***} - \bar{k}^{**}) < \bar{k}^{**}$ (hence $2(\bar{k}^{***} - \bar{k}^{**}) < \bar{k}^{**}$ since $\bar{k}^{***} \leqslant \bar{k}_u^{***}$) for 124 subjects (73.8 percent), and $4(\bar{k}_u^{***} - \bar{k}^{**}) < \bar{k}^{**}$ for 99 subjects (58.9 percent). Therefore, for most subjects the additional perturbation required to guarantee EUT-rationalizability beyond FOSD-rationalizability is relatively modest (see the Appendix for details).

5.6.2 Two-Dimensional Data

We also analyze data from 956 subjects making portfolio decisions in two-state experiments with equally likely states.[19] The results we obtain from the two-state experiments are broadly similar to those obtained from the three-state experiment. In particular, for the vast majority of subjects violations of basic ordering and monotonicity are more prominent than violations of independence. Indeed, for 850 out of 956 subjects (88.9 percent), $\bar{D}_1 = (1-\bar{e}^{\dagger\dagger}) - (\bar{e}^{\dagger\dagger} - \bar{e}^{\dagger\dagger\dagger})$ is positive and statistically significant at the 1 percent level (see the Appendix for details).

Finally, there is also a small pool of 46 subjects (taken from Choi *et al.* (2007a)) where the two states occur with *unequal* probabilities. For reasons which we provide in the Appendix, this asymmetric environment is not ideal for carrying out our difference-in-differences analysis. However, even in this case, departures from ordering and monotonicity are just as significant as departures from independence (see the Appendix for details).

[19]The data include data collected by Choi *et al.* (2007a), similar data using subject pools collected by Zame *et al.* (2024) and Cappelen *et al.* (2023), as well as new data. These experiments are identical to the one in this paper, except that there are two states rather than three.

6 Related Literature

We will not attempt to review the vast theoretical and experimental body of work on decision making under risk.[20] Instead, we focus attention on some recent papers that are particularly relevant to our study. Choi *et al.* (2007a) employs graphical representations of budget lines containing bundles of state-contingent commodities, which allows for the collection of very rich individual-level datasets. In contrast to earlier work, the purpose of Choi *et al.* (2007a) is not to uncover violations of particular axioms, but rather to provide a positive account of choice under risk in a rich choice environment that allows for a general characterization of the patterns of individual behavior.

For each subject in their experiment, Choi *et al.* (2007a) tests the data for consistency with GARP and estimates a parametric utility function, which can be motivated by loss/disappointment aversion (Gul, 1991) and embeds EUT as a parsimonious and tractable special case.[21] But testing EUT as a restriction on a non-EUT utility function has an obvious drawback—it depends on assumptions over functional form and the specification of the error structure, as shown by Halevy, Persitz, and Zrill (2018).

While consistency with GARP is implied by—and guarantees—choice from a consistent preference ordering, *any* such preferences that are locally nonsatiated are admissible. In particular, choices can be compatible with GARP and yet fail to be reconciled with the maximization of a utility function that is monotonic with respect to FOSD, which is not normatively appealing. One is thus naturally led to go beyond basic consistency and to ask whether choices are also compatible with a utility function that has some special structure, in particular one which is monotonic with respect to FOSD and/or adheres to EUT.

Originating in the works of Varian (1983a,b, 1988) and Green and Srivastava (1986), some recent papers that pursue these questions include Diewert (2012), Bayer *et al.* (2013), Kubler, Selden, and Wei (2014), Echenique and Saito (2015), Chambers, Liu, and Martinez (2016),

[20] See Camerer (1995) and Starmer (2000) for excellent surveys. Camerer and Weber (1992) and Harless and Camerer (1994) also summarize the experimental evidence from testing the various utility theories of choice under risk and under uncertainty, and Kahneman and Tversky (2000) collects many theoretical and empirical papers that emerged through their work on prospect theory.

[21] Following the seminal work of Hey and Orme (1994) and Harless and Camerer (1994), a number of other papers have estimated parametric utility functions. Harless and Camerer (1994) fits aggregate data, while Hey and Orme (1994) estimates functional forms at the level of the individual subject using decisions from a large menu of binary choices.

Chambers, Echenique, and Saito (2016), Nishimura, Ok, and Quah (2017), Echenique, Imai, and Saito (2023), Polisson, Quah, and Renou (2020), and de Clippel and Rozen (2023). We compare our approach and contribution to the existing work along four key dimensions—methods, measures, tests, and power.

Methods. In comparison to previous work, our paper differs in the methods used for the collection and analysis of the data. The data from the experiment reported here involve three states with three associated securities whereas the data used in earlier experiments involve only two states and two associated securities. With only two states, WARP and GARP are observationally equivalent so incompleteness and intransitivity can only be tested jointly. With three states, by contrast, we can separate incompleteness from intransitivity. In the case of three states, prominent non-EUT models generate different structures for the utility function, thus yielding a larger set of empirical restrictions on observed behavior against with EUT can be tested.

Our test of EUT-rationalizability relies on the GRID method developed in Polisson, Quah, and Renou (2020). With the exception of the GRID method, all other revealed preference tests of EUT involve a *concave* Bernoulli index. The GRID method, by contrast, neither assumes nor guarantees concavity. This distinction is by no means cosmetic, since it has empirical implications. Although concavity of the Bernoulli index, which is equivalent to risk aversion under EUT, is widely assumed in empirical applications, we avoid imposing any further requirements that are not, strictly speaking, a part of EUT as such.[22] This feature of our analysis is an important part of our claim that our tests are purely nonparametric, with no extraneous assumptions on the parametric form or shape of the utility function.

Measures. Revealed preference relations generate exact tests while choice data almost always contain some violations. Given this, any serious empirical investigation requires an index to measure a model's goodness-of-fit, or (in other words) the extent to which a subject's choices are (in)compatible with the model. In this paper, we use Afriat's

[22]Indeed, there are datasets which are EUT-rationalizable but only with a nonconcave Bernoulli index. For such an example, see Section A4 of the Online Appendix in Polisson, Quah, and Renou (2020). A fuller discussion of these distinctions can also be found in Polisson, Quah, and Renou (2020).

(1973) CCEI to measure a subject's consistency with (basic) rationalizability (e^*), FOSD-rationalizability (e^{**}), and EUT-rationalizability (e^{***}). Since the models are nested, the indices must be ordered for any given subject, with $1 \geqslant e^* \geqslant e^{**} \geqslant e^{***} > 0$. The use of a common index across different models means that we can perform a comprehensive test of each model (in which all of the axioms underpinning the model are tested in combination) and at the same time cleanly identify the incremental impact of additional axioms.

We employ the CCEI (rather than some other index) for two related reasons (as discussed in Section 3.5): it is straightforward to compute for the models under consideration and it is economically interpretable (see Varian (1990)). See also Dziewulski (2020) for a behavioral interpretation of the CCEI based on a decision maker's cognitive inability to distinguish between similar bundles. For these reasons, the CCEI is the most commonly used measure of goodness-of-fit in the revealed preference literature. Another index proposed by Varian (1990) is closely related to the CCEI and has been used in some important work (see, for example, Halevy, Persitz, and Zrill (2018)). There are known methods for calculating this index for the different models that we consider, but its calculation is much more computationally demanding than the CCEI (especially for EUT-rationalizability) and therefore it is not practically implementable for us, given the size of our datasets and the scope of our empirical exercise.[23,24]

Tests. We implement novel individual-level econometric tests. The approach builds only on revealed preference techniques and it is purely nonparametric, making no assumptions about the form of the subject's underlying utility function or on the error structure. That is, for each individual subject we obtain the (empirical) distribution

[23]For more on the computation of Varian's (1990) index to measure rationalizability, FOSD-rationalizability, and EUT-rationalizability, see Polisson, Quah, and Renou (2020). One advantage of Varian's (1990) index is that it is generally less sensitive to a single errant observation as compared to the CCEI. We address this sensitivity issue through our sub-sampling procedure.

[24]de Clippel and Rozen (2023) proposes a new index to measure goodness-of-fit that applies to different families of utility functions; roughly speaking, the index is based on the size of the departures from the first-order conditions. Building on the methodology in Echenique, Imai, and Saito (2020) within the context of intertemporal choice, Echenique, Imai, and Saito (2023) proposes essentially the same index as de Clippel and Rozen (2023) for expected utility, albeit with a somewhat different motivation. This index (or collection of indices) relies on a first-order (condition) approach, which is only applicable to models representable by quasiconcave utility functions (defined on the space of contingent consumption). We avoid imposing a concave Bernoulli index (or, more generally, a quasiconcave utility function) as a rationality requirement.

function for the test statistic under the null hypothesis—that the difference between *perfect* rationalizability and FOSD-rationalizability and the difference between FOSD-rationalizability and EUT-rationalizability are equal—using a purely nonparametric difference-in-differences econometric approach.

Power. A number of recent papers—including Polisson, Quah, and Renou (2020), de Clippel and Rozen (2023), and Echenique, Imai, and Saito (2023)—analyze the experimental data from Choi *et al.* (2014). This experiment is identical to Choi *et al.* (2007a), except that it consists of 25, rather than 50, decision problems involving two (equally likely) states of nature and two associated securities. Echenique, Imai, and Saito (2023) also analyzes the experimental data from Carvalho, Meier, and Wang (2016) and Carvalho and Silverman (2024), which also consist of 25 problems. The Choi *et al.* (2007a) data have also been extensively analyzed, including by Halevy, Persitz, and Zrill (2018) and Polisson, Quah, and Renou (2020).

The experiment reported in this paper consists of 50 decision problems involving three equally likely states and three associated securities. Collecting 50, or even 25, individual decisions is more than is usual in the experimental literature on choice under risk and, as Choi *et al.* (2014) show, it does provide a rich enough individual-level dataset for a powerful test of (basic) rationalizability. Furthermore, our power analysis indicates that having three states significantly enhances the discriminatory power of the experiment, especially with respect to EUT-rationalizability.

To conclude this section, we compare our empirical findings vis-à-vis a few closely-related recent papers. Both de Clippel and Rozen (2023) and Echenique, Imai, and Saito (2023) develop new methodologies and apply their techniques to existing experimental data which (unlike our newly collected data) is obtained from 2D experiments. Notwithstanding the use of a different measure of rationalizability, de Clippel and Rozen (2023) draws a similar conclusion to ours, namely that the gap between FOSD-rationalizability and EUT-rationalizability is small for many subjects; however, as acknowledged by the authors, power issues cast doubt on the robustness of their empirical conclusions. Echenique, Imai, and Saito (2023) finds that subjects who are more rationalizable (as measured by the CCEI) are not necessarily more EUT-rationalizable (as measured by their index). However, these two rationalizability

measures are not formally comparable, so their analysis is not directed at separating the empirical validity of each of the axioms on which EUT is based, which is the principal interest in our exercise.

The principal aim of Polisson, Quah, and Renou (2020) is to develop the GRID method as a nonparametric revealed preference test for a broad class of models (including EUT); to demonstrate its practicality, the GRID method was applied to existing 2D choice data under risk. We share Polisson, Quah, and Renou's (2020) aim to evaluate the empirical validity of the various axioms underlying theories of choice under risk and we also use the GRID method (among others), but our empirical analysis goes much further. The primary contribution of this paper is a combination of richer experimental data and new analytical/statistical methods. This experimental-analytical combination provides much richer guidance for understanding risk preferences and the choices that implement them. Specifically, one key advantage of our 3D experiment over earlier 2D experiments is that with only two goods incompleteness cannot be separated from intransitivity, which is not the case with three goods.[25] Furthermore, there is much greater separation among non-EUT models in the 3D experiment, which thus provides a much stronger test in terms of the power to distinguish between EUT and non-EUT.

A rapidly expanding body of recent research focuses on evaluating the prediction accuracy and flexibility of *parametric* models using the dual measures of completeness and restrictiveness (Fudenberg *et al.*, 2022; Fudenberg, Gao, and Liang, 2023). The completeness of a model is the fraction of the predictable variation in the data that the model captures. The restrictiveness of a model discerns completeness due to the "right" regularities by evaluating its distance to synthetic data. A more complete model better captures the regularities in the data, but it might also be flexible enough to accommodate any regularity. An unrestrictive model is complete on any possible data, so its completeness on the actual data is uninstructive. The completeness and restrictiveness of nested models such as EUT and non-EUT can be easily compared—the completeness/restrictiveness of EUT (the nested model) can only be lower/higher than that of non-EUT (the associated nesting model).

[25]In this paper we develop (and implement) a method to find all WARP violations, GARP violations, and GARP violations that do not contain a WARP violation. As a result, we can tell apart incomplete from nontransitive preferences, which cannot be done using data from previous 2D experiments.

Ellis, Kariv, and Ozbay (2024a,b) calculates the completeness and restrictiveness of parametric EUT and non-EUT models—as well as several machine learning models—using the 2D and 3D budget set data collected from the experiments reported in this paper. These data enable the calculation of individual-level completeness scores. Using the 3D data, Ellis, Kariv, and Ozbay (2024b) finds that the average completeness of EUT is essentially the same as that of non-EUT—85.1 percent versus 85.2 percent. This parametric result aligns with the conclusion of our nonparametric analysis: departures from EUT-rationalizability are small after accounting for departures from FOSD-rationalizability, which also underpin non-EUT models. Otherwise, non-EUT models would achieve higher completeness.[26]

Fudenberg *et al.* (2022) calculates the completeness of models in predicting *aggregate-level* data (of certainty equivalents for binary lotteries), whereas Ellis, Kariv, and Ozbay (2024a,b) calculates *individual-level* completeness measures for each model. This underscores the advantages of our rich experimental data. An additional related point is that using 2D data, Ellis, Kariv, and Ozbay (2024a) finds that the average completeness of both EUT and non-EUT models increases only slightly. However, the restrictiveness of both models is reduced by more than half—from 48.1 percent to 18.6 percent for EUT and from 47.5 percent to 16.6 percent for non-EUT when using the 3D and 2D data, respectively. This aligns with our power analysis, which shows that the 3D data provide a stronger test in terms of power than the 2D data.

In a separate strand of recent research, Nielsen and Rehbeck (2022) tries to separate the normative from the descriptive value of a theory by allowing subjects to revise their choices; if many subjects choose to revise their decisions after being alerted to their violations of (say) the independence axiom, then the axiom has normative appeal even if it may not be descriptively accurate.[27] The goal of our paper (and indeed of all the papers cited above) is

[26]Ellis, Kariv, and Ozbay (2024b) also compares the completeness scores of human subjects to the scores of two types of simulated subjects: (*i*) those who maximize a rank-dependent utility function, systematically departing from EUT-rationalizability but not from FOSD-rationalizability; and (*ii*) those who maximize an expected utility function with idiosyncratic (logistic) noise, departing from FOSD-rationalizability and, consequently, from EUT-rationalizability as well. The conclusion from these comparisons is that the human subjects do not exhibit a combination of systematic departures from EUT-rationalizability and frequent idiosyncratic departures from FOSD-rationalizability.

[27]In a famous encounter between Allais and Savage in 1952, Savage (in response to questions by Allais), first makes choices that contradict one of the core axioms of his subjective expected utility theory, only to acknowledge the mistake and correct his choices upon reflection. See Dietrich, Staras, and Sugden (2021) and the references therein for an account of this famous interaction and the issues it raises.

different—we want to evaluate EUT and related models as *descriptive* theories. To the extent that mistakes (in the sense of Nielsen and Rehbeck (2022)) are part of typical choice behavior and their variable severity across subjects is reflected in subjects' variable economic outcomes outside the lab (Choi *et al.*, 2014), it is certainly not our objective to remove such mistakes from the experiment.[28] That said, separating a theory's normative from its descriptive value is an interesting issue and developing experimental approaches that allow for this separation in the context of budgetary decision making is an important extension.

7 Concluding Remarks

The standard model of choice under risk is based on von Neumann and Morgenstern's (1947) EUT. It is meant to serve as a normative guide for choice and also as a descriptive model of how individuals choose. However, much of the experimental and empirical evidence of "anomalies" in choice behavior suggests that EUT may not the right model. While EUT embodies three important axioms—ordering, monotonicity (with respect to FOSD), and independence—independence is the only axiom which the seminal alternatives to EUT relax.

It is thus natural that experimentalists should want to test the empirical validity of the independence axiom, and the overwhelming body of evidence against independence has raised criticisms about its status as the touchstone of rationality in the context of decision making under risk. In response to these criticisms, various generalizations of EUT have been developed, and the experimental examination of these theories has led to new empirical regularities in the laboratory. Starmer (2000) calls this the "conventional strategy"—theories/experiments designed to permit/test violations of independence (and weakened forms of independence) while retaining the more basic axioms of ordering and monotonicity.[29]

By combining theoretical tools, experimental methods, and nonparametric econometric techniques, our study examines all the axioms of EUT using an individual-level experimental

[28] In Breig and Feldman (2024) subjects are given random opportunities to revise their decisions but, unlike Nielsen and Rehbeck (2022), violations of axioms are not pointed out. The paper finds that various measures of decision quality improve with revisions. We could modify our experiment to allow for random revision opportunities, and that could improve rationalizability scores, but there is no reason to believe that this modification will upset our findings, which are about the *relative* performance of different models.

[29] Bell (1982), Fishburn (1982), and Loomes and Sugden (1982) (simultaneously) propose a model of nontransitive risk preference. Loomes and Sugden (1982) develop a version of this model that involves regret with pairwise choice. Starmer (2000) provides an overview of these models and relates them to other non-EUT alternatives.

dataset that is richer than any previously used. The data are well-suited to purely nonparametric revealed preference tests which allow for the reality that individual behavior may not be perfectly consistent with well-behaved preferences. This is crucial because if choice data exhibit large deviations from rationalizability or FOSD-rationalizability, then the standard approach of postulating some parametric family of utility functions (typically respecting FOSD), and estimating its parameters leads to model misspecification. Consequently, the estimated preference will not reflect the true underlying preference, if such a preference ordering even exists, and positive predictions and normative welfare conclusions based on these models will be misleading.[30]

It also matters because of the potential implications for public policy; for example, in the practice of light paternalism, which is aimed at steering people toward better choices (Camerer *et al.*, 2003; Thaler and Sunstein, 2003; Loewenstein and Haisley, 2008). Clearly, decision makers that only violate independence merit greater deference from policy makers than the more boundedly rational ones that violate ordering and monotonicity because the choices of the former, unlike the latter, maximize a well-behaved utility function and are thus of a higher quality (Kariv and Silverman, 2013).

Finally, we aim to reconcile our finding—that departures from EUT-rationalizability are small after accounting for departures from FOSD-rationalizability—with the ample experimental evidence that rejects EUT in favor of non-EUT alternatives that are FOSD-rationalizable. We first note that, based on data from 81 experiments across 29 studies, Blavatskyy, Ortmann, and Panchenko (2022) concludes that "the Allais Paradox is a fragile empirical finding," likely to be observed when subjects choose between lotteries near edges of the triangle (involving small probabilities) and/or in experiments with (high) hypothetical payoffs. We also note that the absence of comprehensive tests for the entire set of axioms underlying EUT—typical of experiments à la Allais—is particularly striking given the evidence

[30]Halevy, Persitz, and Zrill (2018) parametrically estimates preferences for the dataset collected by Choi *et al.* (2007a) involving two states and two associated securities. They find *significant quantitative and qualitative differences* between the preferences induced by parametric estimation and the revealed preferences implied by choices, due to model misspecification.

on nontransitive choices, dating back at least to Tversky (1969).[31,32]

Furthermore, so-called multiple-switching behavior in multiple price list experiments (Holt and Laury, 2002)—which have emerged as a simple and popular method for eliciting risk preferences—are often linked to violations of ordering and/or monotonicity. Charness, Gneezy, and Imas (2013), for example, concludes about multiple-switching behavior that "such inconsistent behavior is difficult to rationalize under standard assumptions on preferences." In fact, given the prevalence of multiple-switching behavior, many researchers using multiple price list experiments restrict subjects to switching only once, thus suppressing violations of ordering and/or monotonicity.

We hope that our empirical results might also stimulate new theories of how subjects choose contingent consumption bundles from budget sets, given the performance of the standard model with an FOSD-increasing utility function. One possible avenue consists of models where the chosen bundle is optimal among all alternatives in the budget according to some preference, but where the preference is not stable; an example would be a model where the agent's preference is formed around a reference point (such as the risk-free portfolio allocation) that varies with the budget set. Another avenue consists of models where the chosen bundle is optimal (according to a stable preference) among some but not all alternatives in the budget, because the agent is employing simplifying heuristics or has a consideration set (formed according to some rules) which is a strict subset of the budget.[33] We suspect that a more descriptively accurate theory needs to incorporate features from such approaches.

The experimental platform and analytical techniques that we have used are applicable to many other types of individual choice problems and decision domains. One important

[31]Starmer's (2000) explanation/justification for this practice is that "... many have taken the view that the standard independence axiom of EUT can be sacrificed for the sake of explaining the data. Transitivity, however, may be another matter. It might be tempting to think that transitivity is so fundamental to our ideas about preference that to give it up is to depart from theories of preference altogether."

[32]Experimental studies that have tested transitivity have found various forms of nontransitivities—some predicted by regret theory (Loomes, Starmer, and Sugden, 1989, 1991) and others not (Starmer and Sugden, 1998). More recently, Halevy, Persitz, and Zrill (2018) finds "substantial numerical differences with respect to the recovered parameters that in some cases imply significant quantitative and qualitative differences in preferences" when inconsistencies are considered in the structural estimation of risk preferences.

[33]Barseghyan, Molinari, and Thirkettle (2021) use a model with expected utility and random consideration sets to help explain households' deductible choices across different types of insurance coverage (which is essentially a choice problem over a finite set of risky alternatives). The use of limited consideration sets in their paper was motivated in part by the frequent observation of households making choices which are dominated (in a certain sense related to FOSD).

direction is to study choice under ambiguity. In a separate paper, we apply the GRID method and other revealed preference techniques to the analogous data of Ahn *et al.* (2014) which similarly allow for a rigorous test of individual-level decision making under ambiguity. In ongoing work, we also study patterns of economic rationality in intertemporal choice—specifically non-constant time discounting—by replacing the state-contingent assets in the experiment reported here with time-dated accounts.

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