Prices and Portfolio Choices in Financial Markets: Theory and Experimental Evidence*

Peter Bossaerts
California Institute of Technology

Charles Plott
California Institute of Technology

William R. Zame
University of California, Los Angeles

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1 Introduction

Most asset-pricing models make strong predictions about portfolio choices as well as about asset prices. The static and intertemporal versions of the Capital Asset Pricing Model (CAPM and ICAPM) and (equilibrium versions of) Arbitrage Pricing Theory (APT), for example, predict that all investors hold the same mix of risky securities (a property usually termed *portfolio separation*). Even very much more general models, such as the Arrow-Debreu model with the assumption of expected utility, predict that wealth rankings across states are the same for all investors. Empirical studies of asset-pricing based on historical data, however, almost always focus on the pricing predictions, ignoring the portfolio predictions — perhaps because the pricing predictions seem consistent with the data while the portfolio predictions do not. Because the pricing predictions of these models *rest* on the portfolio predictions, these empirical studies offer a puzzle: How can asset-pricing theory be right if the portfolio choice theory on which it rests is wrong?

However, a multitude of difficulties make it hard to evaluate the evidence from historical data. Some of these difficulties arise from statistical considerations: Is the ubiquitous assumption of stationarity correct? Do asset returns satisfy the strong mixing conditions that are required for the central limit theorems on which inference is based? How should one account for feedback from earlier statistical inference on the same historical data? Other difficulties arise from imperfect observability of the main ingredients of the theory: What information did investors have? What preferences and beliefs did investors hold? What portfolio choices did investors make? Was the market complete or incomplete? What was the market portfolio?

Motivated by a desire to avoid many of these difficulties, we create and observe an *experimental asset market*. In this setting, the market structure, the true payoff distributions, and the market portfolio are imposed by (hence known to) the experimenter; portfolio choices can be observed; and the magnitude of the risks and rewards involved allows for informed guesses about preferences. All these factors make our task much easier. Some additional concerns to attach to the experimental setting, however: rewards and risks are relatively small (compared to the lifetime wealth of the participants), and participants are relatively inexperienced (compared to typical traders). We address the import of these concerns below.

In our experiments, 20-60 subjects trade risky and riskless assets in an electronic open-book market that closely resembles the Paris stock exchange. Because our asset markets are complete, we can apply standard general equilibrium theory to derive various predictions. Absent additional assumptions, we can predict that market prices should equilibrate, that equilibrium prices should not admit arbitrage, and that equilibrium market prices should depend on individual preferences and endowments. Additional assumptions are suggested by the nature of our experiments. Because risks and rewards are uncorrelated with other risks facings subjects, it is natural to assume that subjects behave as expected utility maximizers. Under this assumption, familiar arguments lead to the qualitative predictions that, at equilibrium, state prices should be negatively correlated with aggregate wealth across states, and that individual wealth across states should be positively correlated with aggregate wealth across states. Because risks and rewards are small in comparison to lifetime wealth, Taylor's theorem suggests that it is natural to assume that individuals maximize quadratic utility functions for certain consumption. Because the true state probabilities and asset payoffs are commonly known, these additional assumptions lead CAPM, which predicts that, at equilibrium, the market portfolio should be mean-variance efficient and that all individuals should hold a mixture of the riskless asset and the market portfolio of risky assets.

Our findings confirm some of these predictions but disconfirm others:

(i) market prices do equilibrate over time

(ii) equilibrium prices do not admit arbitrage

(iii) market prices reflect substantial risk premia

(iv) market prices equilibrate at different levels in different experiments

(v) market prices are consistent with ordinal predictions of the Arrow-Debreu model with expected utility preferences and with the cardinal predictions of CAPM

(vi) portfolio holdings are inconsistent with the ordinal predictions of the Arrow-Debreu model with expected utility preferences and with the cardinal predictions of CAPM

Of these findings, the first two echo findings of earlier experiments (in somewhat different settings); the remainder appear to be quite new. Findings (v) and (vi) indicate clearly that the pricing predictions of theory may hold even when the portfolio predictions do not, which re-emphasizes our original motivation: How can asset-pricing theory be right if the portfolio choice theory on which it rests is wrong?

We address this question by extending standard CAPM in a way familiar in applied work, to a model in which individual demands are the sum of two terms: the demands generated by quadratic utilities and a perturbation (error) term. If these perturbations are independent across individuals and have mean zero across the population, and if the number of traders is large, the law of large numbers implies that the perturbations wash out in the aggregate. Hence our extended model predicts the same market prices as does CAPM — but does is consistent with quite different portfolio choices. In particular, we provide a simple reason why the market portfolio should be mean-variance efficient even if *no* investor holds a mean-variance optimal portfolio. (In the standard CAPM, mean-variance efficiency of the market portfolio is an immediate consequence of mean-variance optimality of individual portfolios.)

Our theory says nothing about why actual demands should not equal theoretical demands, and hence says nothing about the sources of the perturbation terms. Several possibilities come to mind:

(a) **Perturbations reflect subject errors:** Subjects do not intend to optimize (because risks and rewards are too small to provide adequate incentives), or do intend to optimize but find it too difficult.

(b) **Perturbations reflect market imperfections:** Buy and sell orders cannot be filled (because the market is not sufficiently deep), so portfolio holdings do not reflect experimental demands.

(c) **Perturbations reflect unobserved heterogeneity:** The true utility functions are not quadratic, so the true theoretical demands are not the theoretical demands derived from quadratic utilities.

We cannot test any of these possibilities directly, but we can offer several indirect tests, all of which suggest that errors are not the most important source of perturbations.

- Rewards are roughly constant across experiments, but outside wealth is not. In one of our experiments (labelled 011126 in the tables), subjects were drawn from the undergraduate university population in Bulgaria, where the average winnings in the experiment represent 3-4 weeks wages for a typical worker. The findings of this experiment are not qualitatively different from the findings of experiments in which subjects were drawn from university populations of upper-class universities in the U.S.

- Because we observe subject choices from several different budget sets, we can ask whether subject choices satisfy the Afriat inequalities, and hence are consistent with the Strong Axiom of Revealed Preference. Our analysis suggests that, for a large majority of subjects, choices nearly satisfy the Afriat inequalities, hence display at most minor violations of the Strong Axiom of Revealed Preference, and so are substantively consistent with utility maximization.

- Revealed Preference analysis provides only a weak test, since it reveals only whether subjects made choices consistent with maximizing *some* utility function, not necessarily whether they made choices consistent with maximizing their *true* utility functions. Put differently: subjects might have made substantial errors which did not lead to substantial violations of the Afriat inequalities. We construct a stronger — but indirect — test by using the assumption of expected utility. Under this assumption, maximizing a utility function for risky consumption generates the same demand as maximizing a different utility function over

> portfolios. Using this equivalence, we construct certainty-equivalent experiments in which we *assign* utility functions, and for which equilibrium prices and portfolio choices — which we can calculate from our knowledge of endowments and utility functions — correspond to the predictions of CAPM in the original experiments. Because we *know* the utility functions, we can observe any instances in which individuals fail to optimize, and assess the importance of such failures in terms of foregone monetary gains. We find that foregone gains average more than \$350 per person at the beginning of an experiment, but decrease to less \$1.80 per person at the end. Since average gains in the experiments are about \$55 per person, it seems that subjects do learn to optimize quite well.

Thus, our work presents experimental evidence that the pricing predictions of theory can be correct even when the portfolio predictions are not, and theoretical analysis that explains why. Demand perturbations play an important role in our analysis — just as they do in much applied work — but not, hitherto, in the analysis of financial markets.

Following this Introduction, Section 2 describes the design of our experimental asset markets, Section 3 derives the predictions made by a standard theoretical analysis of these markets, and Section 4 describes the actual findings. Section 5 describes the expanded model with demand perturbations. Section 6 presents the revealed preference analysis of our experimental data, and Section 7 describes the certainty-equivalent experiments and findings. Section 8 concludes.

2 Experimental Asset Markets

In this Section, we describe the experimental environment and design.

In our market the objects of trade are risky assets; that is, random variables on the set of exogenous states of nature.

We treat a market in which the objects of trade are *assets*, which are state-dependent claims to wealth at the terminal time, and cash. Some of the assets are risky and some are riskless; we refer to the latter as *notes*. We write $A(s)$ for the *dividend (payoff)* of the asset A if the realized state of nature is s. The state of nature is drawn from a commonly known distribution.

At the opening of the market, the state of nature is not known. Experimental subjects (traders) are endowed with assets and cash; in part, these endowments are loans, which must be repaid. While the market is open, traders can buy and sell assets for cash. After the market closes, the state of nature is drawn, dividends are are issued on each asset, and loan repayments are demanded; net profits are credited to subject accounts. Between the opening of the market and the closing of the market, no information about the state of nature is revealed, and credits are made to subject accounts. (In effect, consumption takes place only at the close of the market.)

In the experiments reported here, there were three states of nature, labelled X, Y, Z. States were drawn randomly, with or without replacement (details are provided below).

Three assets could be traded. Two, labeled Securities A and B, had a risky payoff, determined by the random drawing of one of three "states," referred to as States X, Y and Z. The third asset, labeled Note, was riskfree, and unlike the risky securities could be sold short, up to eight units. Note that markets are complete, which has particular implications for theoretical predictions. Among other things, this implies that, in equilibrium, shortsale of the risky securities has not restrictive. In contrast, subjects' ability to shortsell the riskfree security is potentially important even in equilibrium. Subjects were endowed with a certain number of securities A and B. The Notes were always in zero net supply.

Subjects could offer for sale or purchase, or trade, in each of the securities during periods of fifteen to twenty-five minutes, after which the state was announced and dividends were paid depending on the final holdings of the securities. The assets were then taken away, and subjects were given a fresh supply of the risky securities, as well as a certain amount of cash. The times between initial allocation of securities and cash and final realization of the dividends will be referred to as *periods*. Each experiment involved up to nine periods.

Cash was given at the beginning of every period. At the end of each period, subjects had to pay the experimenter for the initial cash as well as the initial allocation of risky securities. That is, initial cash and risky securities were given "on loan," and the end-of-period payment to the experimenter was therefore referred to as "loan repayment." This loan repayment created leverage, causing a magnification of the risk involved in the holding of securities A and B.

All accounting was done in terms of a fictitious currency called francs, to be exchanged for dollars at the end of the experiment at a pre-announced exchange rate. In some experiments, subjects were also given an initial sign-up fee, theirs to keep if they succesfully finished the experiment (but fully exposed to any losses from holding risky securities).

Subjects were barred from further trading if they ran net cumulative losses more than two periods in a row. In the next section, we will explain how this bankruptcy rule induces risk aversion up to the penultimate period in an experiment even if subjects are risk neutral.

The payoff matrix for the seven experiments remained the same, namely:

State	X	Y	Z
Security A	170	370	150
Security B	160	190	250
Notes	100	100	100

Note that the payoffs on securities A and B are negatively correlated. In other words, purchases of B can readily be used to diversify the risk of A.

One experiment, 001113, differed from the previous ones only in terms of the number of securities and the payoff matrix. While the number of states remained the same (3), we added a security (security C) and gave subjects a simple payoff matrix:

State	X	Y	Z
Security A	200	0	0
Security B	0	200	0
Security C	0	0	200
Notes	100	100	100

Note that there is redundancy: a portfolio of one unit of each of the risky securities generates the same payoff pattern as two Notes. The risky securities are of a type that is fundamental to general equilibrium asset pricing theory: they are state securities, paying a positive amount in one state, and nothing otherwise.

Trade took place over the internet, in a set of parallel, continuous, computerized open book markets, one for each security. The open book system worked as the ones used in field markets, such as the Paris Bourse, with one major exception: no orders could be hidden. The identity of the traders was not revealed; only an ID number identified orders in the book. The trading system, referred to as *Marketscape*, was developed at Caltech. It had been used succesfully in other experiments, also aimed at studying competitive equilibrium theory. The reader who would like to try out the mechanics of Marketscape should visit http://eeps3.caltech.edu/market-demo/.

The remaining data and parameters for all eight experiments are displayed in Table 1. Notice the variation in initial endowments across experiments. As will become clear in the next section, theory predicts that pricing will therefore be different across experiments, even if subjects' risk aversions remains the same. One interesting empirical implication, therefore, is whether pricing did indeed change from one experiment to another.

With the exception of the first experiment, endowments also differed across subjects within an experiment. Subjects were not told other subjects' endowments (they were not even told how many subjects participated). This is a deliberate design feature. We wanted to avoid that subjects could easily deduce the nature of the aggregate risk (in the jargon of the most popular model: the payoff pattern of the market portfolio). Once aggregate risk is known, subjects could use standard models (like the CAPM) to price securities if they cared to do so. Indeed, the reader will be reminded in the next section that aggregate risk determines pricing in financial markets when they are complete as in our experiments. The purpose of the experiments is to generate the predictions of asset pricing theory, not through subjects' deliberate pricing on the basis of the theory that they may have heard about, but through the fundamental economic forces that the theory claims are behind its pricing results. Note also that subjects need not know the nature of aggregate risk for the predictions of general equilibrium theory in complete markets to be valid. In general equilibrium theory, prices are the only piece of marketwide information that agents are given.

The actual payoffs obviously depended on the sequence of states that happened to be drawn during the experiment. Because each experiment involved only up to 9 periods, the frequency of occurence of each state deviated from the ex ante probabilities (1/3 for each state), but all states ended up occuring in each experiment. While subjects were told that states were random and equally likely, some of them clearly ignored this and started gambling on the occurence of a state that had not appeared yet. This evidently affected prices in two experiments (referred to as the 990407 and the 991111 experiments).[1]

We did not have full control over the number of participants in the experiments. Subjects could register in a database several days before the actual experiment. After that, they could retrieve an ID and password with which to log on to the Marketscape website from which the experiment would be run, and execute some trades as practice. In general, however, subjects were slow to register. Hence, the number of subjects varied substantially. In two experiments, we had under 20 subjects (the experiments referred to as 990211 and 000804). Previous experience had revealed that thin markets do

[1] More detail can be found in [5].

not generate the theoretical pricing predictions as cleanly as do thick markets. See [4]. In part, this is because the usual trading environment (a set of parallel, unconnected markets, where one cannot submit orders that are conditional on events in other markets) makes it hard for subjects to readily rebalance entire portfolios. We know this indirectly: a system that allows subjects to directly trade portfolios of several securities has been shown to work better. See [3].

Another reason for the predictions of asset pricing theory to emerge more cleanly in thicker markets will follow from the theoretical developments later in this paper. These will be introduced to explain the main question that we started with in the Introduction, namely, how asset pricing theory can be correct if its allocational predictions do not hold. We will postpone a discussion of market thickness and accuracy of equilibrium asset pricing predictions until after we develop the heory to explain the price-allocation paradox.

Subjects were given clear instructions, which they could read on the website for each experiment, and which we did not alter across experiments. These instructions included a description of some portfolio strategies. They did not mention a strategy whereby the subject is invited to gamble against the experimentor's random drawing of states. It should be pointed out that most subjects had familiarity with financial markets. This is obvious for the MBA students we used in, e.g., the first experiment (referred to below as "Yale experiment" and labelled 981007). Caltech students had been exposed to financial markets through a class in introductory finance, often complemented with a class in investments analysis. Claremont and Occidental students were taking economics/econometrics classes. Bulgarian students (experiment 011126) had no prior exposure to finance.

The interested reader can consult the website for one of the experiments for more detail:

http//eeps3.caltech.edu/market-011126 ("011126")

The website provide the reader access to instructions, the trading interface, and the trading history (including pricing).[2]

[2] Anonymous login requires the ID 1 and password a.

3 Theory

Here we use general equilibrium theory, together with natural assumptions about utility functions of the traders, to generate various predictions which we can test against the observed data.

As in our experiments, we consider a security market with two dates, S states of nature at the second date, and a single consumption good. Consumption takes place only at the terminal date. J assets $A_1, \ldots, A_J$ (state-dependent claims to consumption at the terminal date) are traded at the initial date. If $\theta \in \mathbf{R}^J$ is a portfolio of assets, we write div θ for the *dividends* of θ; that is,

$$\text{div } \theta(s) = \sum_{j=1}^{J} \theta_j A_j(s)$$

There are N traders. Trader n is characterized by an endowment ν^n, which is a portfolio of assets, and a utility function U^n over state-dependent terminal consumptions. (Thus, we assume that traders care about portolio choices only through the consumption they yield.) Write $M = \sum \nu^n$ for the *market portfolio.* An *equilibrium* consists of prices $q \in \mathbf{R}^J$ for each asset and portfolio choices θ^n for each trader such that

- choices are budget feasible: for each n

$$q \cdot \theta^n \leq q \cdot \nu^n$$

- choices are budget optimal: for each n

$$\eta^n \in \mathbf{R}^J, U^n(\text{div } \eta^n) > U^n(\text{div } \theta^n) \Rightarrow q \cdot \eta^n > q \dot{} \nu^n$$

- markets clear: for each j

$$\sum_{i=1}^{N} \theta_j^n = M$$

  (Note that assets are in positive supply.)

If an equilibrium exists, then equilibrium asset prices cannot admit arbitrage.

- **No Arbitrage** For $\theta \in \mathbf{R}^J$

$$\begin{aligned} &\text{if div } \theta \geq 0 \quad \text{then} \quad q \cdot \theta \geq 0 \\ &\text{if div } \theta > 0 \quad \text{then} \quad q \cdot \theta > 0 \end{aligned}$$

Given asset prices q that do not admit arbitrage, the Fundamental Theorem of Asset Pricing guarantees that there are *state prices* $\pi \in \mathbf{R}^S_+$ such that

$$q \cdot \theta = \sum_s \pi_s \text{ div } \theta(s)$$

for every portfolio θ. We assume throughout that a riskless asset, yielding one unit of consumption in each state, is available; it is convenient to normalize so that the price of that asset is 1.[3] In that case π is a probability measure, frequently called the *risk neutral measure*, (see [8]) and the price of each portfolio is its expected dividend with respect to π.

Asset markets are *complete* if for every consumption pattern $x \in \mathbf{R}^S$ there is a portfolio $\theta \in \mathbf{R}^J$ such that div $\theta = x$. If asset markets are complete then an equilibrium in the asset market is equivalent to an equilibrium in the underlying Walrasian economy in which claims to state-dependent consumption are traded directly, and the state prices can be identified with the Walrasian prices. By construction, our experimental asset markets are complete.[4]

In this generality, absence of arbitrage is the only implication that can be drawn from the theory. To derive observable implications beyond the absence of arbitrage, we need to make some assumptions about utility functions. Assume first of all that each trader maximizes state-independent expected utility according to the objective probability distribution β on S. That is

$$U^n(x) = \sum_s \beta_s u^n(x_s)$$

where u^n is some strictly concave, strictly increasing function. At equilibrium, state prices equate marginal utilities for state dependent consumption,

[3] In our experiments, both cash and Notes are riskless.

[4] Because we do not allow traders to hold short positions in the risky assets, this is not quite true. However, it is true whenever equilibrium portfolio choices of risky assets are interior, which is almost always the case in our experiments.

so we obtain the familiar conclusion that individual consumption is positively correlated with aggregate consumption. Since aggregate consumption is just the dividend on the market portfolio, we obtain:

- **State Consumption Ranking** If div $M(s) >$ div $M(s')$ then div $\theta^n(s) >$ div $\theta^n(s')$ for each n.

In our experiments, $S = 3$ and $\mu_s = 1/3$ for each s so

- **State Price Ranking** If div $M(s) >$ div $M(s')$ then $\pi_s < \pi_{s'}$.

Note that these are purely ordinal predictions. To derive cardinal predictions we need stronger assumptions on preferences. In our experiments, risk and reward are small, and are uncorrelated with outside risk, so it seems reasonable to ignore the error between utility functions and their quadratic Taylor approximations; that is, we assume $u^n(x) = x - \frac{1}{b_n}x^2$ for some $b_n > 0$. (Note that b_n is the coefficient of absolute risk aversion, which we assume non-zero.) Because markets are complete and individuals hold the true probabilities, the assumption of quadratic utilities implies that an interior equilibrium (that is, an equilibrium in which each trader's equilibrium consumption is strictly greater than 0 in each state) satisfies the conclusions of the Capital Asset Pricing Model (CAPM).

To express these conclusions succinctly, suppose that the first $K < J$ assets are risky (in our experiments, cash and Notes are riskless). Write z^n for trader n's equilibrium holding of the risky assets and $Z = \sum z^n$ for the market portfolio of risky assets. Write $\mu = (E(A_1), \ldots, E(A_K))$ for the vector of expected returns of risky assets and $\Delta = [\text{cov}(A_j, A_k)]$ for the covariance matrix. CAPM guarantees that equilibrium prices q satisfy

$$q = \mu - \left(\frac{1}{N}\sum_{n=1}^{N}\frac{1}{b_n}\right)^{-1}\Delta\left(\frac{1}{N}Z\right)$$

and that equilibrium portfolio choices satisfy

$$z^n = \frac{1}{b_n}\Delta^{-1}(\mu - q)$$

Recall that b_n is trader n's coefficient of absolute risk aversion; its inverse $\frac{1}{b_n}$ is usually called trader n's *risk tolerance*, so $\left(\frac{1}{N}\sum\frac{1}{b_n}\right)$ might be viewed as the *market risk tolerance*.

Since we do observe individual or market risk tolerances, the pricing conclusion of CAPM cannot be observed directly, but an immediate consequence of the pricing formula is that the market portfolio of risky assets Z is mean-variance efficient; that is, the expected excess return $E(\text{div } Z) - q \cdot Z$ on the portfolio Z is highest among all portfolios having variance Var Z. (Keep in mind that M, Z differ only by riskless assets, so mean-variance efficiency could equally well be expressed in terms of the entire market portfolio M.) Thus we have two observable implications of CAPM.

- **Mean Variance Efficiency** At equilibrium, the market portfolio of risky assets is mean-variance efficient.
- **Portfolio Separation** At equilibrium all traders choose a portfolio of risky assets that is a multiple of the market portfolio of risky assets.

(In particular, Portfolio Separation implies that individual consumptions are perfectly correlated with aggregate consumption, which is State Consumption Ranking.)

Bounds on individual and market tolerances will yield sharper predictions on equilibrium prices. If we assume that u^n is the quadratic approximation to a utility function which displays constant relative risk aversion (a class of utility functions commonly used in macroeconomics and finance), then we can use estimates of the present value of lifetime wealth V, the coefficient of relative risk aversion r, and the magnitude of the risk to derive estimates for individual risk aversion and hence for the market risk tolerance. We estimate $V \geq \$500,000$ because most of our experimental subjects are students who expect to become successful professionals; we estimate $r \leq 5$ because this allows a degree of risk aversion greater than that assumed in most studies; and we estimate the risk in the experiment to be no greater than \$100 because this amount exceeds the median gain of all subjects in all our experiments. These lead to estimates that individual risk aversion does not exceed 10^{-5}

and hence that market risk tolerance is at least 10^5. Substituting in the pricing formula, we can compute that the equilibrium price for the risky assets A, B satisfy (in the case where states are equally likely):

$$229 \leq q_A \leq 230 \ , \ 199 \leq q_B \leq 200$$

This suggests equilibrium pricing should be approximately risk neutral (with respect to the true probabilities), or equivalently that risk premia ((the difference between expected payoffs and prices of risky securities) should be approximately 0.

- **Approximate Risk Neutral Pricing** In equilibrium, asset prices should approximately equal expected dividends.

If subjects were *exactly* risk neutral, equilibrium prices would *exactly* equal expected dividends, all zero net cost trades would be utility neutral — and portfolio predictions would completely disappear. As we shall see, however, risk neturality is decisively rejected in our experiments, and asset prices are far from expected dividends.

4 Data

In the subsections below, we discuss the extent to which the six theoretical predictions are consistent with the data from our experiments. In order to keep the reader oriented, we begin each discussion with the experiment involving Yale MBA students (referred to as 'Yale experiment" and labelled 981007 according to the year, month, and day on which the experiment was conducted), and then discuss the extent to which the data from other experiments presents a different picture. Figure 1 presents the complete history of transactions in the Yale experiment; each observation records a trade in one of the three assets. Complete data for all the experiments is presented in tables in the Appendix.

4.1 No Arbitrage

Since Notes pay 100 in every state, No Arbitrage predicts that the equilibrium price of notes must be $q_N = 100$. A quick glance at Figure 1 reveals that this prediction is confirmed at the end of each trading period — although it is frequently violated near the beginning of each trading period. The latter observation is not surprising; the structure of our market requires "cash in advance" for all transactions, and therefore generates a transactions demand for money. Put differently: cash and Notes are perfect substitutes at the end of each period but not before then.

More generally, straightforward computations (which we suppress) show that end-of-period asset prices in the Yale experiment satisfy No Arbitrage. The same conclusion can be drawn in all our experiments.

4.2 Risk Neutral Pricing

Figure 1 reveals that that the transaction prices for risky assets are almost always well below the expected asset payoffs. Indeed, of the roughly 500 transactions involving the risky asset A, *every single transaction* takes place at a price *below* the expected payoff of 230, and the vast majority of transactions take place below 220. We take this to be overwhelming rejection of Risk Neutral Pricing, and confirmation that risk aversion plays an important role in experimental financial markets.

Similar conclusions can be drawn in each of the other experiments, although not quite so sharply (see Table 2). The vast majority of transactions for risky assets take place at prices well below expected payoffs, although we do see occasional transactions at prices above expected payoffs.

4.3 State Price Ranking

Given the market portfolio and the payoff matrix, aggregate wealth is lowest in state X, intermediate in state Z, highest in state Y. Since states were equiprobable in 981007, theory therefore predicts that the state price probability π_X should be highest, the state price probability π_Z should be intermediate, and the state price probability π_Y should be lowest.

To check this prediction, we use the prices of the traded assets to infer state prices. In Figure 1, each observation corresponds to a transaction in one asset; we use that transaction to determine the price of the traded asset, and set the prices of the other assets equal to the last previous prices. The evolution of state price probabilities in the Yale experiment is displayed in Figure 2. In each period, the ranking of the state price probabilities is wrong at the beginning of the period but evolves in the "correct" direction through the period. In the first two periods, the initial error is so large that it is not corrected by the end of the period; in the last four periods, the rankings are just as predicted by theory. [5] provides statistical tests that confirms the visual impression given by Figure 2. The results of the Yale experiment are largely confirmed in other experiments. See Table 3.

4.4 Mean Variance Efficiency

It is convenient to express the deviation from Mean Variance Efficiency in terms of Sharpe Ratios. Recall that, fiven asset prices q, the *excess return* on a portfolio θ is the difference between the expected return on θ and the return on a riskfree portfolio having the same price: $E(\text{div } \theta) - q \cdot \theta$. By definition, the *Sharpe ratio* of θ is the ratio of its expected excess return to its variance

$$\text{Sharpe Ratio}(\theta) = \frac{E(\text{div } \theta) - q \cdot \theta}{\text{Var}(\text{div } \theta)}$$

Mean Variance Efficiency is the prediction that the market portfolio has the largest Sharpe Ratio among all portfolios, so we can examine the extent to which this prediction is consistent with the data by plotting the difference between the Sharpe ratio of the market portfolio and the maximum possible Sharpe ratio.[5]

Figure 3 shows the evolution over the Yale experiment of the difference between the Sharpe ratio of the market portfolio and the maximum possible Sharpe ratio. (As before, each observation corresponds to a transaction in one asset; we use that transaction to determine the price of the traded asset, and set the prices of the other assets equal to the last previous prices.) Mean Variance Efficiency is the prediction that the difference between the Sharpe ratio of the market portfolio and the maximum Sharpe ratio is zero. As Figure 3 shows, this difference is quite large at the beginning of the experiment but evolves in the direction of zero. Again, [5] formalizes this visual impression. Table 4 summarizes the evidence for the other experiments, displaying the average difference of Sharpe ratios over the last ten transactions in each period.

[5] In computing the maximum possible Sharpe ratio, we take shortsale constraints on the assets A, B into account by considering only portfolios for which holdings of A, B are non-negative.

4.5 State Wealth Ranking

Figure 4 provides evidence on the State Consumption Property in the Yale experiment. For each subject (shown on the x-axis) and each period (shown on the z-axis), we show (on the y-axis) the correlation between the ranking across states of the subject's end-of-period wealth with the ranking across states of aggregate end-of-period wealth. A rank correlation of +1 means the observation is in perfect conformity with the prediction of theory; a rank correlation of 0 means the observation has no relation with the prediction of theory; a rank correlation of -1 means the observation is opposite to the prediction of theory. Figure 4 reveals that for many subjects, end-of-period wealth rankings do not conform with theoretical predictions. Indeed, end-of-period wealth rankings are frequently opposite to the predictions of theory. Moreover, rank correlations do not appear to increase over time — even as the ranking of state prices changes in accordance with the theory.

The failure of State Wealth Ranking in the Yale experiment is all the more dramatic considering that in this experiment (and only in this experiment) each subject's endowment of risky assets coincided with the market portfolio.[6] (Of course subjects do not know this; indeed they do not know the market portfolio.) In particular, the ranking across states of each subject's beginning-of-period wealth *is* in perfect conformity with the ranking across states of aggregate wealth — and subjects move *away* from these rankings as the market evolves.

4.6 Portfolio Separation

Since State Wealth Ranking fails, Portfolio Separation — which is stronger than State Wealth Ranking — must also fail, and it does, most spectacularly. Figure 5 displays the distribution of final risk exposure in each of the six periods. Plus signs show the fraction of individual risky wealth (that is, the value of risky assets held) invested in asset A at the end of each period;

[6]That is not to say that endowments are Pareto optimal, since subject's endowments of cash, hence total wealth, differed.

circles show the fraction of social risky wealth invested in asset A at the end of each period.[7] Portfolio Separation predicts that the fraction of individual risky wealth held in asset A should be the same for all investors. In fact, however, this fraction is 0 for some investors and 1 for others, and appears quite random.[8] Moreover, holdings appear to conform more closely to the predictions of Portfolio Separation neither near the ends of periods or nor near the ends of experiments. Table 5 confirms that such is the case in all the experiments.

[7]Although the market portfolio is constant across periods, the fraction of social risky wealth invested in asset A is different across periods because prices are different across periods.

[8]Subject to the constraint that it averages to the social fraction.

5 Demand Perturbations

We focus on the CAPM, because it makes stricter assumptions than the more general Arrow-Debreu model (it assumes that utility functions are quadratic). We advance conditions such that CAPM pricing (the Sharpe Ratio Property) obtains in the limit as the number of agents increases, while CAPM allocations (the Portfolio Separation Property) do not. The arguments can readily be changed to prove that the State Price Property can obtain in the limit if even the State Consumption Property is violated.

Unlike in standard asset pricing theory, yet inspired by standard practice in applied economics, our starting position is that parametric formulations of preferences generate only a crude approximation of actual demands in the marketplace. There could be multiple sources for this error term. Foremost, it captures shortcomings in the preference model from which demands are obtained (quadratic utility in the case of CAPM). Such shortcomings are not limited to, say, failures of expected utility theory. Among other things, our preference models almost never take into account effort in computing and implementing optimal demands. Second, the error term could also reflect lack of understanding. It has often been suggested that subjects in market experiments generally do not exhibit the savvy that participants in the corresponding field markets are claimed to have. Later on, however, we will be able to provide some evidence that subjects do understand (at least in our experiments), and therefore, that confusion is not the predominant source of the error term needed to square prices and allocations in asset markets experiments.

So, we expand the model such that actual portfolio choice is represented as a random perturbation of the theoretical (CAPM-optimal) portfolio choice. Formally, we suppose that actual demand z_n is the sum of theoretical demand $\tilde{z}_n$ and an error term ϵ_n:

$$z_n = \tilde{z}_n(p) + \epsilon_n \tag{1}$$

We *do not* assume the error term is small. We only impose that error terms are independent across n, with mean zero

$$E[\epsilon_n] = 0, \tag{2}$$

and finite variance V

$$E[\epsilon_n^2] = V > 0. \tag{3}$$

The condition of cross-sectional independence and zero mean is inspired by applied economics. Under the condition, the error term can be viewed as capturing *unobserved heterogeneity*, terminology that is also used in applied economics.

Direct computation shows that equilibrium prices will be:

$$p = \mu - \left(\sum_{n=1}^{N} \frac{1}{b_n} \right)^{-1} \Delta z^0 + \left(\frac{1}{N} \sum_{n=1}^{N} \frac{1}{b_n} \right)^{-1} \Delta \frac{1}{N} \sum_{n=1}^{N} \epsilon_n \tag{4}$$

Hence the difference between equilibrium prices p and CAPM equilibrium prices $\tilde{p}$ is

$$|p - \tilde{p}| = | \left(\frac{1}{N} \sum_{n=1}^{N} \frac{1}{b_n} \right)^{-1} \Delta \frac{1}{N} \sum_{n=1}^{N} \epsilon_n | \tag{5}$$

Now consider adding subjects, ensuring that the endowments average z^0 remains finite, risk tolerances $1/b_n$ have a finite non-zero mean, and errors ϵ_n continue to be independent with zero mean and finite variance. To emphasize the dependence on the number of subjects, write $\tilde{p}^N, p^N$ for equilibrium prices in the CAPM economy and perturbed economy with N investors. The Law of Large Numbers implies that, as $N \to \infty$

$$|p^N - \tilde{p}^N| \quad \to \quad 0 \;\text{ a.s.}, \tag{6}$$

$$\frac{1}{N} \sum_{n=1}^{N} [z_n - \tilde{z}_n]^2 \quad \to \quad V \;\text{ a.s.}. \tag{7}$$

That is, for large N equilibrium prices in the true economy will (with high probability) be close to equilibrium prices in the benchmark CAPM economy, but equilibrium portfolio choices in the true economy will *not* be close to equilibrium portfolio choices in the benchmark CAPM economy. (More specific assumptions about the distribution of error terms lead, via the Central Limit Theorem, to estimates on the rates of convergence.)

While the mathematics behind the result in (6) and (7) is simple, the intuition is not. It essentially means that it is possible for the market portfolio (aggregate supply of risky securities, and, in equilibrium, the aggregate demand) to become mean-variance optimal even if *no* individual demand is mean-variance optimal. Indeed, the pricing result in (6) is equivalent to the market portfolio becoming mean-variance optimal as the number of subjects increases.

Our introduction of error terms at the demand level is a deliberate modeling choice (we could have introduced them, e.g., at the utility level). It ensures that the pricing results (CAPM pricing) are not invalidated, at least as long as there is a large number of subjects. Note also that one can rotate the asset space (substitute securities whose payoff matrix is a linear transformation of that of the original securities). CAPM pricing will still obtain in the limit, while portfolio choices need not converge to CAPM predictions.

Our version of the CAPM provides an explanation for the empirical results in the experiments, where one observes that CAPM pricing can obtain even if holdings move no closer to satisfying the portfolio separation predicted by the traditional CAPM. CAPM pricing emerges because subjects' demands can successfully be modeled as a mean-variance optimal demand plus a mean-zero error term. In [6], we provide formal tests of this explanation, by means of χ^2 statistic derived from our model.

Our version of the CAPM also predicts that CAPM will not explain pricing as well in experiments with a smaller number of subjects. While this prediction is not immediately obvious from Table 4, where there are only two experiments with less than twenty subjects, more extensive evidence can be found in [4]. The prediction is based on the following. Even if demand errors relative to CAPM are mean zero and uncorrelated across subjects, the sampling error in the actual average error will be larger when there are fewer subjects. Hence, the pricing error (relative to CAPM) will be bigger. There are, however, other plausible explanations for poorer performance of the CAPM as a pricing model is smaller-scale experiments. One is related to the nature of the trading mechanism and is explored in [3].

6 Incentives to Optimize

As we have noted in the Introduction, it is sometimes suggested that market behavior in the experimental laboratory has little to tell us about market behavior in the field because subjects in the laboratory are do not receive sufficiently strong incentives. The experiments conducted in Bulgaria — in which rewards corresponded to 3-4 weeks' wages but behavior is not qualitatively different from other experiments; see the evidence in the Tables; the Bulgaria experiment is referred to as 011126 — suggest that this objection is not well-founded. In this Section we describe two tests which also suggest that this objection is not well-founded; the first is based on revealed preference analysis, the second on "equivalent" experiments in which there was no uncertainty.

6.1 Revealed Preference

If the incentives offered subjects are too weak, we should not expect them to exert the effort required to optimize. In our experiments, we cannot directly observe preferences directly, so cannot directly test whether subjects optimize. However, because we observe choices at different prices, and hence in different budget sets, we can test whether subjects choices are consistent with the Strong Axiom of Revealed Preference, and hence are consistent with maximizing *some* utility function. The idea of applied revealed preference in this context was suggested by [2], and our methodology extends the methodology employed there.

Consider a single subject making choices in I periods. Let p_i, x_i denote the prices prevailing and the choice made in period i. If x_i is affordable at prices p_j (that is, $p_j \cdot x_i \leq p_j \cdot x_j$) then x_j is *revealed preferred* to x_i (because x_j was chosen when x_i was available). If the observed choices are consistent with maximizing a (strictly concave) utility function then the revealed preference relation must be irreflexive, so that there cannot be periods i, j for which x_j is revealed preferred to x_i and x_i is revealed preferred to x_j. (Choices with this property are usually said to satisfy the *Weak Axiom of Revealed Pref-*

erence.) More sharply, in order that the observed choices be consistent with maximizing a (strictly concave) utility function it is necessary and sufficient that the revealed preference relation must be transitive, so there cannot be periods $i_1, i_2, \ldots, i_K$ such that x_{i_k} is revealed preferred to $x_{i_{k+1}}$ for each k and x_{i_K} is revealed preferred to x_{i_1}. (Choices with this property are usually said to satisfy the *Strong Axiom of Revealed Preference.*)

[1] demonstrates that choices satisfy the Strong Axiom of Revealed Preference if and only if there exist numbers $U_i \geq 0$ and $\lambda_i \geq 1$ such that

$$\begin{aligned} U_i - U_j &\leq \lambda_j p_j \cdot (x_i - x_j) \\ &\qquad\qquad \text{for } i, j = 1, ..., I; i \neq j \end{aligned} \tag{8}$$

(In the usual formulation, the requirement is only that $\lambda_i > 0$. However, the usual inequalities are positively homogeneous, so requiring $\lambda_i \geq 0$ entails no loss.) We measure *failure* by means of a slack variable ε, which is the solution to the following nonlinear programming problem.

$$\begin{aligned} \min \varepsilon \quad & \quad \text{subject to} \\ U_j - U_i &\geq \lambda_j \left[p_j \cdot x_j - \varepsilon - p_j \cdot x_i \right] \\ &\qquad\qquad \text{for } i, j = 1, ..., I; i \neq j \\ \varepsilon &\geq 0 \\ \lambda_j &\geq 1 \text{ for } i = 1, ..., I \\ U_i &\geq 0 \text{ for } i = 1, ..., I \end{aligned} \tag{9}$$

If $\varepsilon = 0$, the Afriat inequalities can be satisfied exactly. If $\varepsilon > 0$, the Afriat inequalties cannot be satisfied, and ε is the minimum income that has to be taken away from the subject in at least one period (but at most $I-1$ periods) for the resulting inequalities to be satisfiable. Speaking loosely, we refer to ε as the *violation of the Afriat inequalities.*

Table 6 provides details of the distribution of violations of the Afriat inequalities for eight experiments together. Of 316 subjects (the table is based on subjects from the first 6 experiments only), 78 (24.7%) display no violations at all, 211 (66.8%) display violations of no more than 25 francs (less than \$1), and 291 (91.8%) display violations of no more than 100 francs (\$3-4).

Although it is not obvious how to provide an economic interpretation of the sizes of these violations, we view this analysis as suggesting that observed choices are (approximately) consistent with maximizing *some* utility function. Of course, it might be that observed choices are not consistent with maximizing subjects' *actual* utility functions; it is possible that subjects behaved irrationally or made gross errors which were nonetheless just happened to be consistent with the Strong Axiom of Revealed Preference. The next subsection provides a sharper test.

6.2 Certainty-Equivalents

The assumption that traders care about portolio choices only through the consumption they yield means that, at least in theory, a market for risky assets is equivalent (in the sense of generating the same demand functions, hence the same equilibrium prices and choices) to a markets in which traders derive utility directly from portfolio holdings (so that there is in fact no risk); we refer to the latter market as the *certainty equivalent* market. If trader n's utility function for risky consumption is

$$U(x) = \sum_s \beta_s \left[x_s - b_n x_s^2\right]$$

(so that his utility function for certain consumption is quadratic with absolute risk aversion $b_n > 0$) then his /her equivalent utility function for portfolios is

$$\tilde{U}^n(\theta) = \sum_s \beta_s \left[\sum_j \theta_j A_j(s) - b_n \left(\sum_j \theta_j A_j(s)\right)^2\right] \tag{10}$$

We can create a experimental version of the certainty equivalent market in the laboratory simply by paying subjects a monetary award proportional to their utility function $\tilde{U}^n$.

In reality, we do not *observe* the coefficient b_n of absolute risk aversion so we cannot *observe* the utility function certainty equivalent utility function

$\tilde{U}^n$. However, we can calibrate the certainty equivalent market to the *imputed* risk aversions in our experimental markets for risky assets. Having done so, we can measure directly the extent to which subjects do or do not optimize by measuring foregone gains, and the extent to which these foregone gains decrease over time.

The experimental environment for the certainty equivalent experiments is precisely the same as the environment for the the first seven uncertainty experiments, but instead of paying state-dependent dividends on portfolios of assets, we pay subjects on the basis of assigned utility functions. For tractability, we do not use the utility functions $\tilde{U}^n$ (which are, inconveniently, functions of 4 variables), but rather the mean-variance utility functions

$$\hat{U}^n(\theta) = E(\text{div } \theta) - \hat{b}_n \text{Var}(\text{div } \theta) \tag{11}$$

These mean-variance utility functions are convenient because they are quasi-linear in the riskless assets, and we can choose the coefficients $\hat{b}_n$ so that $\tilde{U}^n, \hat{U}^n$ generate the same demands for risky assets.

We assigned subjects one of three levels for the risk aversion coefficient $\hat{b}_n$, chosen in such a way as to generate equilibrium prices similar to the observed equilibrating prices in the uncertainty experiments. Table 7 provides details of the experimental design.[9]

Table 8 displays the evolution of the average amount of money left on the table, expressed in francs (ignoring shortsale constraints). That table gives per-subject average differences between actual end-of-period gains and ideal gains if subjects had truly optimized, given the most recent transaction prices. Early in these experiments, foregone losses can be enormous — more than 6,000 francs ($360) — but display substantial variation across subjects. As the experiment evolves, these losses decrease dramatically; by the end of an experiment, foregone losses are less than 60 francs ($3.6) and average less than 30 francs ($1.8).

[9]Instructions and screens for the experiments can be viewed at http://eeps2.caltech.edu/market-000511. Use identification number=1 and password=a to login as a viewer.

7 Conclusion

It is possible for securities prices to arrange in accordance with standard predictions in general equilibrium theory even if portfolio choices remain vastly different. The paradoxical situation can emerge if theoretical demands are only an approximation of actual demands, yet error terms are mean zero. Large-scale financial markets experiments confirm the theoretical possibility and shed light on the nature of the demand error term: it does not seem to be related to noise, confusion, inattention or inexperience among subjects, leaving only the possibility of preference mis-specification as an explanation. The findings underscore the importance of the inclusion of error terms in demand modeling, to capture preference heterogeneity, including the plethora of preference patterns suggested in behavioral finance. Such inclusion is standard in applied economics but has hitherto rarely been considered in empirical finance.

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Table 1: Experimental Design Data

Date	Draw Type [a]	Subject Category (Number)	Signup Reward (franc)	Endowments A	B	C	Notes (franc)	Cash (franc)	Loan	Exchange Rate $/franc
981007	I	30	0	4	4	N/A[b]	0	400	1900	0.03
981116	I	23	0	5	4	N/A	0	400	2000	0.03
		21	0	2	7	N/A	0	400	2000	0.03
990211	I	8	0	5	4	N/A	0	400	2000	0.03
		11	0	2	7	N/A	0	400	2000	0.03
990407	I	22	175	9	1	N/A	0	400	2500	0.03
		22	175	1	9	N/A	0	400	2400	0.04
991110	I	33	175	5	4	N/A	0	400	2200	0.04
		30	175	2	8	N/A	0	400	2310	0.04
991111	I	22	175	5	4	N/A	0	400	2200	0.04
		23	175	2	8	N/A	0	400	2310	0.04
000804	I	8	175	5	4	N/A	0	400	2200	0.04
		7	175	2	8	N/A	0	400	2310	0.04
001113	I	29	125	7	13	9	0	400	2200	0.04
		30	125	8	11	12	0	400	2310	0.04
011114	D	21	125	5	4	N/A	0	400	2200	0.04
		12	125	2	8	N/A	0	400	2310	0.04
011126	D	18	125	5	4	N/A	0	400	2200	0.04
		18	125	2	8	N/A	0	400	2310	0.04
011205	D	17	125	5	4	N/A	0	400	2200	0.04
		17	125	2	8	N/A	0	400	2310	0.04

[a] I=states are drawn independently across periods; D=states are drawn without replacement, starting from an urn with six balls of each type (state).

[b] N/A = not applicable.

Table 2: Prices (Average Of Last Ten Transactions); For 011114, 011205, 011126, Ratios Of Prices To Expected Payoffs

Date	Security	Period									
		1	2	3	4	5	6	7	8	9	10
981007	A	220	215	215	218	219	202				
	B	191	196	193	194	194	194				
	Notes	95	97	99	97	99	99				
981116	A	215	202	210	212	189	202				
	B	187	194	196	194	190	188				
	Notes	99	100	98	99	100	99				
990211	A	220	228	213	202	218	224	230			
	B	184	181	185	178	188	181	193			
	Notes	94	95	96	98	96	99	97			
990407	A	219	208	206	200	200	212	202	208		
	B	195	198	202	206	215	201	204	215		
	Notes	99	99	100	99	99	99	99	99		
991110	A	204	212	214	214	210	206				
	B	167	172	180	189	191	189				
	Notes	96	97	97	98	98	100				
991111	A	225	217	225	225	230	233	212	208		
	B	196	199	182	182	187	189	189	190		
	Notes	99	99	99	99	98	99	99	99		
000804	A	219	225	210	219	219	231	232			
	B	185	188	189	182	188	191	194			
	Notes	98	99	98	99	100	99	99			
001113	A	63	76	66	68	76	63	75	106	71	
	B	60	66	69	53	49	39	52	32	36	
	C	66	60	68	69	65	67	52	55	75	
	Notes	100	99	99	99	100	100	99	99	100	
011114	A	.98	.92	.89	.89	.99	.98	.99	1.00	.91	.93
	B	.94	.97	.96	.96	.93	.96	1.01	.94	.96	.90
	Notes	.99	.99	.99	.99	.99	.99	.99	.98	.99	.97
011126	A	.78	.78	.83	.84	.85	.84	.83	.86		
	B	.74	.93	.91	.90	.94	.93	.95	.90		
	Notes	.90	1.10	.99	.98	.98	.98	1.02	.99		
011205	A	.91	.93	.95	.89	.87	.94	.97	1.00	.90	.87
	B	.97	.93	.92	.93	.90	.94	.94	.95	.96	.92
	Notes	.99	1.00	.99	.99	.99	.99	.99	.99	.99	.99

Table 3: State Prices Probabilities (Average, Based On Last Ten Transactions); For 011114, 011205, 011126, State Price Densities (Ratios Of State Price Probabilities To Probabilities)

Experiment	State	Period									
		1	2	3	4	5	6	7	8	9	10
981007	X	0.30	0.34	0.43	0.35	0.44	0.47				
	Y	0.35	0.30	0.27	0.31	0.24	0.20				
	Z	0.35	0.36	0.30	0.35	0.31	0.33				
981116	X	0.50	0.49	0.39	0.44	0.58	0.54				
	Y	0.26	0.19	0.25	0.25	0.12	0.20				
	Z	0.24	0.31	0.36	0.31	0.29	0.27				
990211	X	0.36	0.41	0.45	0.63	0.39	0.54	0.32			
	Y	0.35	0.37	0.28	0.20	0.32	0.30	0.37			
	Z	0.29	0.21	0.26	0.17	0.30	0.16	0.31			
990407	X	0.40	0.39	0.37	0.33	0.22	0.35	0.34	0.19		
	Y	0.29	0.24	0.23	0.21	0.22	0.26	0.21	0.26		
	Z	0.32	0.37	0.41	0.47	0.57	0.39	0.44	0.55		
991110	X	0.69	0.64	0.53	0.44	0.44	0.57				
	Y	0.22	0.25	0.27	0.27	0.25	0.19				
	Z	0.08	0.11	0.19	0.28	0.31	0.24				
991111	X	0.37	0.36	0.54	0.53	0.44	0.43	0.49	0.49		
	Y	0.32	0.28	0.30	0.30	0.34	0.35	0.25	0.23		
	Z	0.31	0.36	0.16	0.17	0.22	0.23	0.26	0.28		
000804	X	0.47	0.46	0.46	0.44	0.49	0.41	0.35			
	Y	0.29	0.31	0.25	0.28	0.27	0.34	0.35			
	Z	0.23	0.23	0.29	0.28	0.23	0.25	0.29			
001113	X	0.33	0.38	0.32	0.36	0.40	0.37	0.42	0.55	0.39	
	Y	0.32	0.33	0.34	0.28	0.26	0.23	0.29	0.17	0.20	
	Z	0.35	0.30	0.33	0.36	0.34	0.40	0.29	0.29	0.41	
011114	X	1.50	1.41	1.51	1.60	1.58	1.46	0.94	1.76	1.77	2.14
	Y	0.87	0.71	0.54	0.55	0.92	0.91	0.99	0.97	0.69	0.73
	Z	0.64	0.90	0.88	0.90	0.60	0.79	1.04	0.65	0.85	0.51
011126	X	2.61	2.76	1.80	1.76	1.40	1.68	1.84	1.83		
	Y	0.43	-0.15	0.40	0.36	0.48	0.50	0.15	0.46		
	Z	-0.05	0.39	0.64	0.61	0.94	0.72	0.80	0.36		
011205	X	1.33	1.66	1.52	1.55	1.80	1.26	1.20	1.31	1.49	1.55
	Y	0.73	0.81	0.84	0.60	0.59	0.89	0.99	1.10	0.64	0.56
	Z	0.94	0.53	0.46	0.67	0.32	0.68	0.69	0.42	0.93	0.56

Table 4: Average Absolute Difference Between Market Sharpe Ratio and Maximal Sharpe Ratio (Average Based On Last Ten Transactions; Maximum In Parentheses).

Experiment	Period								
	1	2	3	4	5	6	7	8	9
981007	0.13	0.02	0.03	0.01	0.02	0.01			
	(0.71)	(0.31)	(0.34)	(0.27)	(0.13)	(0.05)			
981116	0.06	0.00	0.01	0.01	0.04	0.03			
	(2.74)	(0.16)	(0.02)	(0.09)	(0.16)	(2.04)			
990211	0.10	0.20	0.05	0.08	0.03	0.16	0.24		
	(13.17)	(0.67)	(0.29)	(0.39)	(0.09)	(0.21)	(0.56)		
990407	0.02	0.00	0.03	0.11	0.39	0.03	0.08	0.45	
	(0.35)	(0.37)	(0.17)	(0.34)	(0.55)	(0.53)	(0.22)	(0.67)	
991110	0.20	0.19	0.11	0.02	0.01	0.04			
	(0.74)	(0.30)	(0.31)	(0.23)	(0.22)	(0.11)			
991111	0.01	0.02	0.16	0.15	0.14	0.14	0.03	0.02	
	(0.49)	(0.35)	(0.20)	(0.30)	(0.27)	(0.29)	(0.33)	(0.13)	
000804	0.09	0.10	0.02	0.03	0.07	0.12	0.13		
	(0.33)	(0.20)	(0.26)	(0.05)	(0.10)	(0.14)	(0.40)		
001113	0.11	0.05	0.06	0.09	0.04	0.28	0.03	0.00	0.20
	(0.16)	(0.06)	(0.12)	(0.13)	(0.33)	(0.32)	(0.11)	(0.09)	(0.28)
011114	0.10	0.01	0.00	0.01	0.14	0.07	0.10	0.17	0.04
	(0.21)	(0.15)	(0.17)	(0.09)	(0.22)	(0.20)	(0.34)	(0.24)	(0.11)
011126	0.24	0.06	0.03	0.03	0.01	0.01	0.00	0.05	
	(4.28)	(0.53)	(0.14)	(0.14)	(0.03)	(0.29)	(0.36)	(0.07)	
011205	0.00	0.08	0.12	0.03	0.07	0.07	0.09	0.14	0.01
	(0.52)	(0.12)	(0.15)	(0.13)	(0.10)	(0.13)	(0.18)	(0.45)	(0.21)

Table 5: Average Absolute Deviation between The Fraction of Subjects' End-of-period Risk-exposed Wealth Invested in A and the Market Portfolio Weight of A

Experiment	Period								
	1	2	3	4	5	6	7	8	9
981007	0.17	0.29	0.15	0.21	0.22	0.16			
981116	0.22	0.23	0.22	0.18	0.23	0.21			
990211	0.22	0.31	0.32	0.28	0.20	0.26	0.26		
990407	0.31	0.24	0.24	0.27	0.30	0.32	0.31	0.31	
991110	0.24	0.27	0.27	0.24	0.26	0.23			
991111	0.20	0.27	0.21	0.26	0.26	0.28	0.21	0.18	
000804	0.19	0.18	0.17	0.16	0.24	0.24	0.27		
001113	0.32	0.22	0.16	0.18	0.21	0.16	0.15	0.32	0.21
011114	0.23	0.23	0.23	0.23	0.21	0.19	0.20	0.22	0.21
011126	0.24	0.24	0.28	0.24	0.28	0.23	0.28	0.27	
011205	0.20	0.25	0.24	0.17	0.22	0.26	0.32	0.29	0.21

Table 6: Violations of Afriat Inequalities – Experiments 981007 to 991111

Size of Violations (ε), in francs[a]						
$\varepsilon = 0$	$0 < \varepsilon \leq 25$	$25 < \varepsilon \leq 50$	$50 < \varepsilon \leq 100$	$100 < \varepsilon \leq 200$	$200 < \varepsilon \leq 300$	$300 < \varepsilon$
78	133[b]	46[c]	34[d]	20[e]	4[f]	1[g]

[a] ε in the nonlinear programming problem in (9)
[b] Optimization algorithm did not converge for 9 subjects.
[c] Optimization algorithm did not converge for 3 subjects.
[d] Optimization algorithm did not converge for 2 subjects.
[e] Optimization algorithm did not converge for 2 subjects.
[f] Optimization algorithm did not converge for 1 subject.
[g] Value of ε for this subject: 357.

Table 7: Experimental Design Data – Certainty Equivalent Experiments.

Experiment	Subject Category (Number)	b_n[a]	Signup Reward (franc)	Endowments A	Endowments B	Endowments Notes	Cash (franc)	Loan Repayment (franc)	Exchange Rate \$/franc
000320	8	$2.3.10^{-3}$	0	6	2	0	400	1675	0.06
	9	$2.8.10^{-4}$	0	2	6	0	400	2020	0.09
	8	$1.5.10^{-4}$	0	6	2	0	400	2120	0.09
	8	$1.5.10^{-4}$	0	2	6	0	400	2015	0.09
000509	14	$2.3.10^{-3}$	0	2	8	0	400	2340	0.06
	13	$2.8.10^{-4}$	0	8	2	0	400	2480	0.06
	14	$1.5.10^{-4}$	0	2	8	0	400	2365	0.06
000509	15	$2.3.10^{-3}$	0	2	8	0	400	2340	0.06
	14	$2.8.10^{-4}$	0	8	2	0	400	2480	0.06
	15	$1.5.10^{-4}$	0	2	8	0	400	2365	0.06

[a]Coefficient in the payoff function (11) assigned to subject.

Table 8: Evolution of Size of Subjects' Mistakes Over Time – Certainty Equivalent Experiments

Experiment	Periods[a]							
000320	53	34	39	26	28	25	25	13
	(92)	(75)	(89)	(73)	(69)	(68)	(69)	(23)
000509	6414	2834	815	50	73	73	63	12
	(5207)	(2375)	(693)	(35)	(61)	(66)	(66)	(17)
000511	1211	760	379	274	189	181	100	57
	(911)	(568)	(301)	(219)	(153)	(154)	(90)	(59)

[a]Average difference in payoff between the actual end-of-period subject holdings and optimal holdings given end-of-period prices. Standard deviations in parentheses.

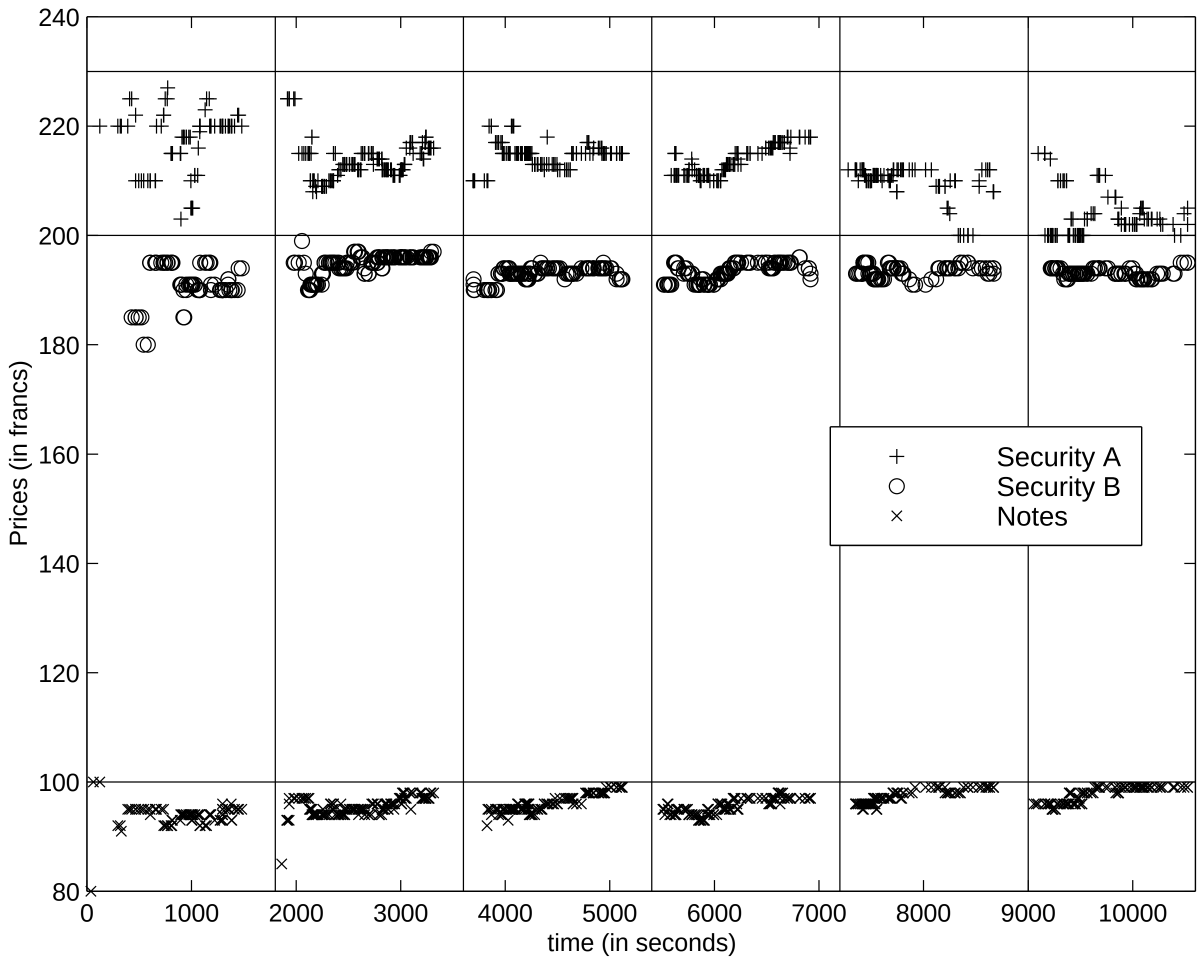

Figure 1: Evolution of securities prices in the 981007 experiment.

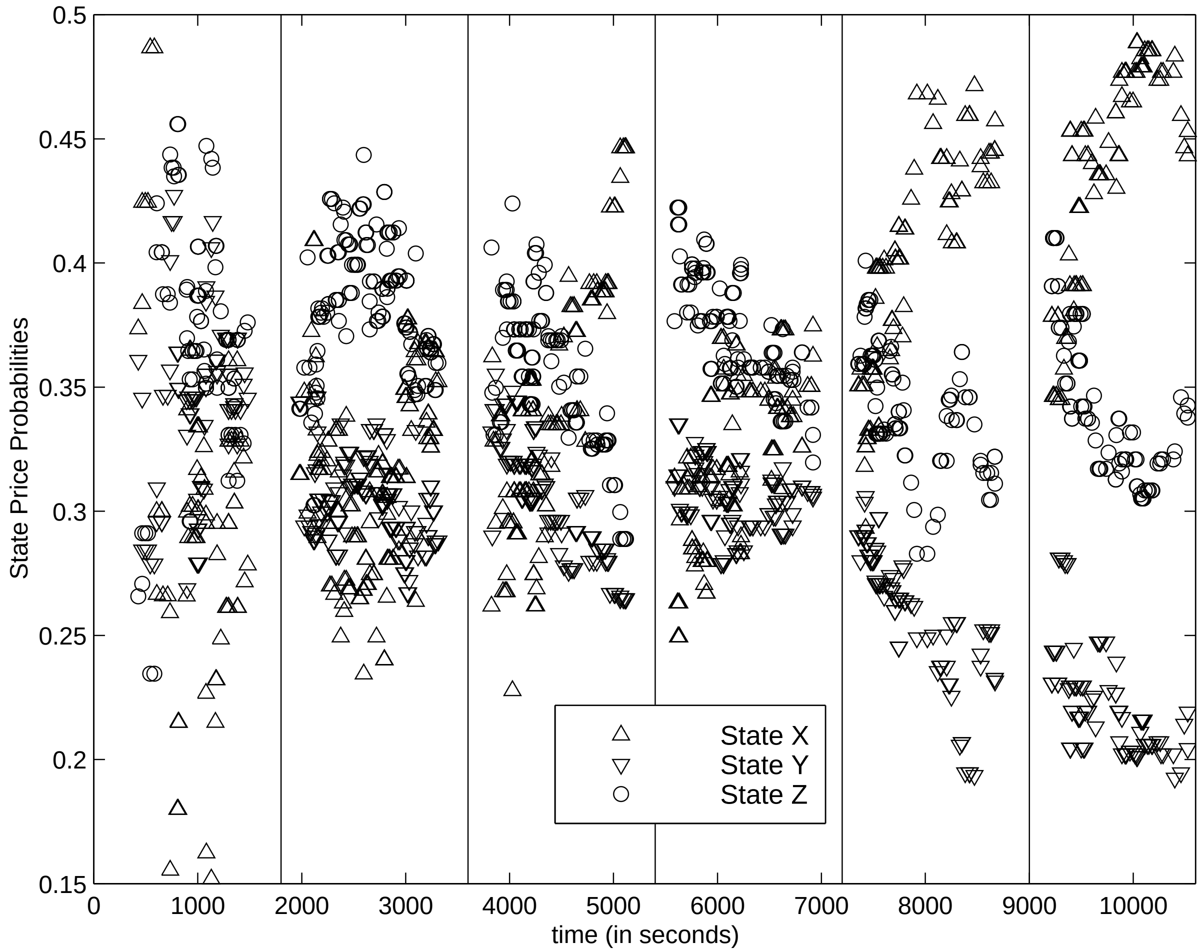

Figure 2: Evolution of the state-price probabilities in the 981007 experiment.

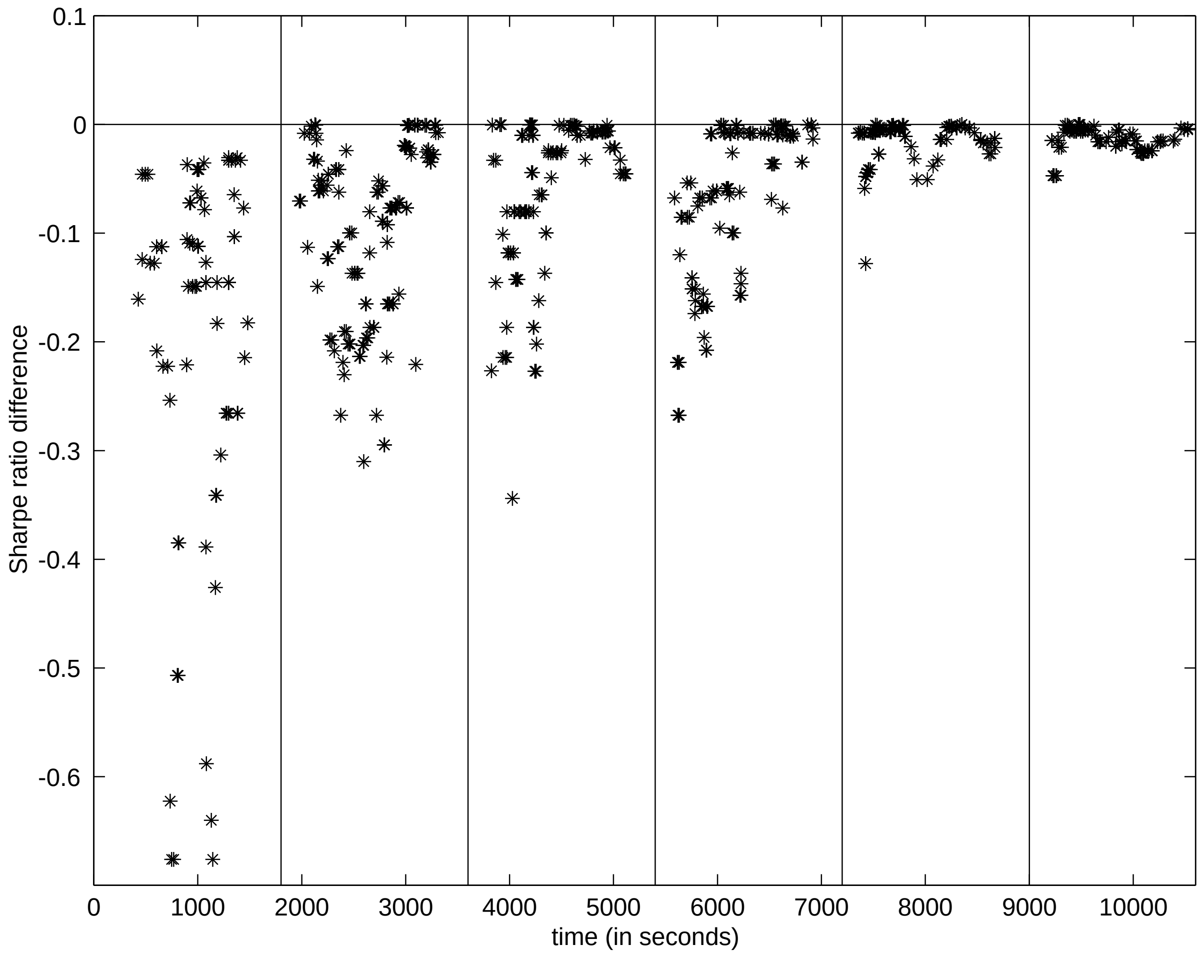

Figure 3: Evolution of the difference between the Sharpe ratio (expected return in excess of the riskfree rate, divided by the standard deviation) of the market portfolio and the maximum possible Sharpe ratio, 981007 experiment.

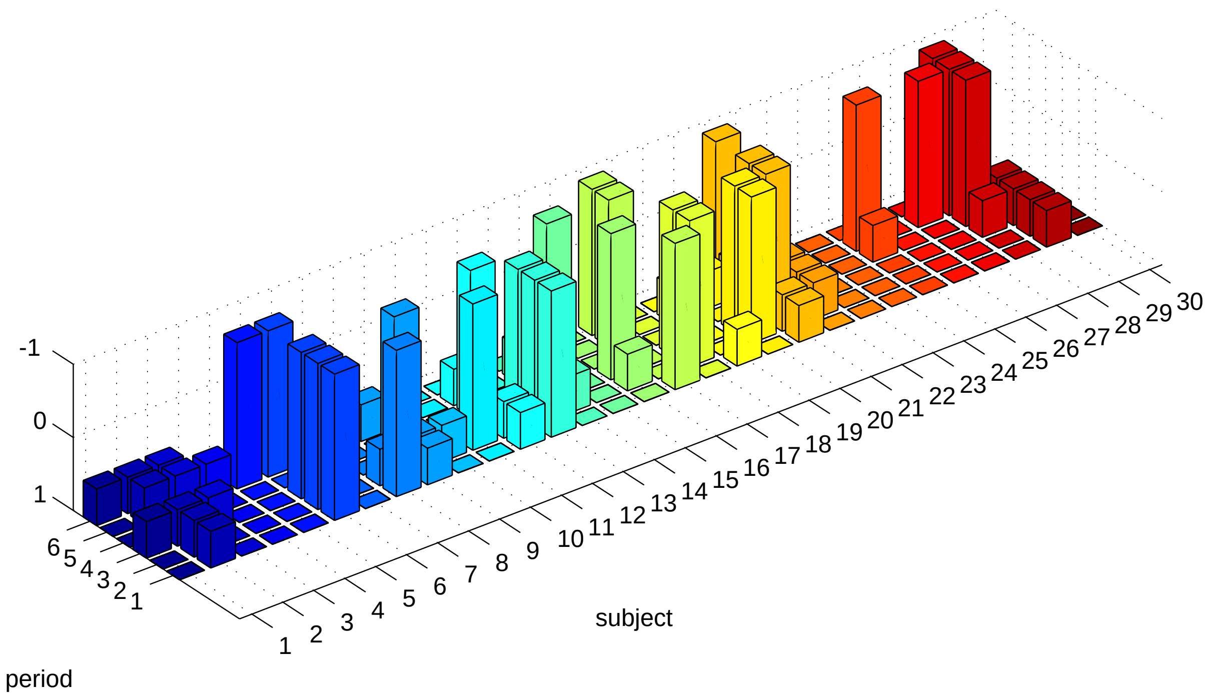

Figure 4: Rank correlation beteen earnings expected from individual subjects' end-of-period holdings and the aggregate dividend, experiment 981007. Note the inverted scale on the vertical axis.

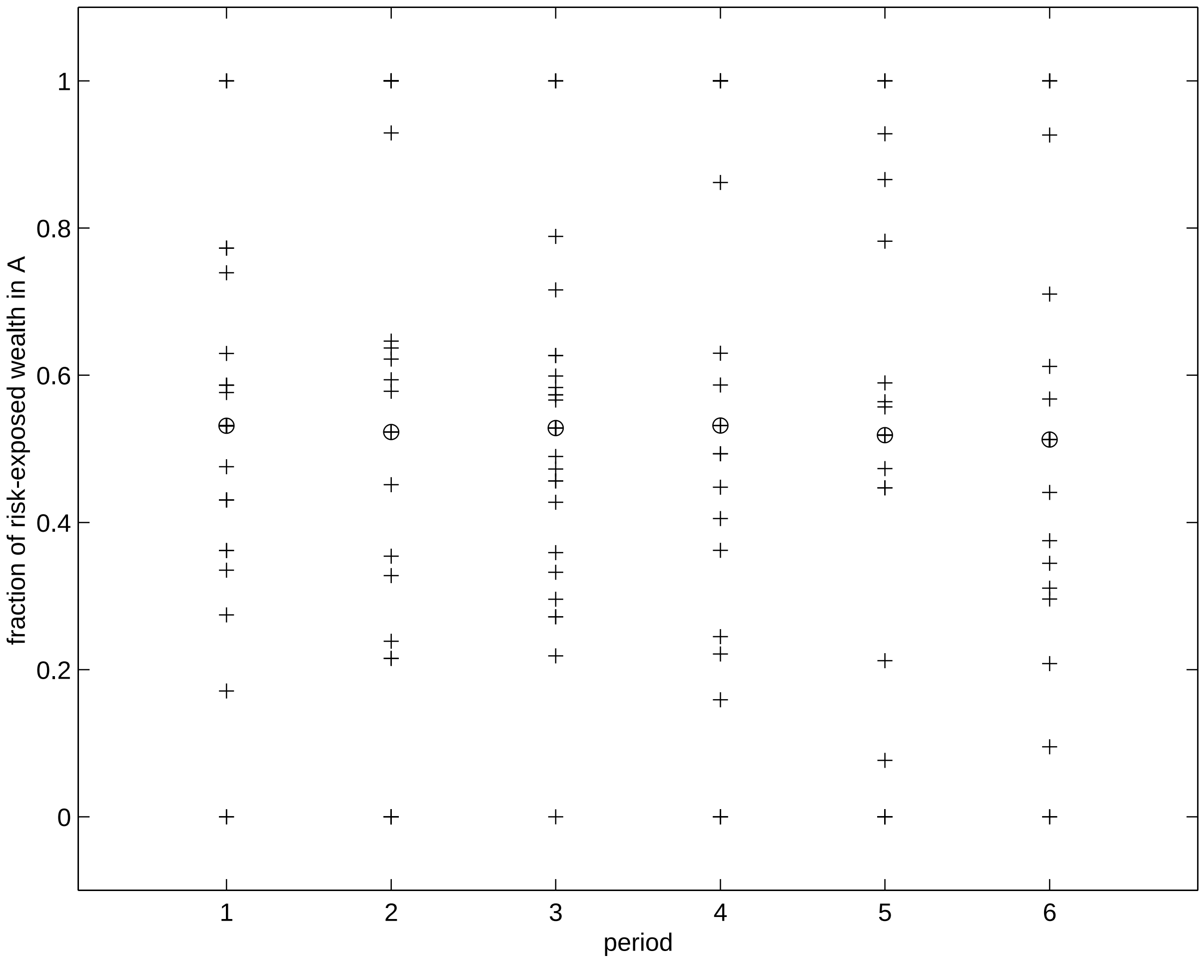

Figure 5: Plus signs indicate weight on security A in subjects' portfolios of risky securities at the end of each period, experiment 981007. For comparison, market portfolio weights are also depicted (circles).