When Learning Pays:

Evidence from Naturally Occurring Auctions

Jan Hanousek *

and

Evžen Kočenda *

Abstract

Learning is a subject of intense research in economics. We present persuasive evidence that learning took place among uninformed heterogeneous agents during a large-scale naturally occurring set of auctions that by size, incentives, and variation belongs among the largest experiments ever conducted. To detect and quantify learning we develop new measures of individual performance during the bidding process when prices of goods vary over succeeding stages of bidding.

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* CERGE-EI, Prague; CEPR and WDI

1. Introduction

We present persuasive evidence that learning took place among uninformed agents who had strong incentives to learn during a course of naturally occurring set of auctions whose stakes compounded, on average, to several months of pay. By virtue of the experiment's design with multiple market periods, which allow agents to learn based on accumulated experience, our results lend insights into the findings of Smith (1962) and List (2004c).

There exists a considerable theoretical literature on learning (see Weibull, 1995; Fudenberg and Levine, 1998; and Vega-Redondo, 2003, among others, for overviews), yet empirical evidence is still rudimentary and almost exclusively driven by the experimental approach.[1] Results from learning in laboratory experiments are reviewed by Camerer (2003, p.265) who argues for the use of experimental data since they "are a good way to test models of learning because control over payoffs and information means we can be sure what subjects know (and know others know, and so on), what they expect to earn from different strategies, what they have experienced in the past, and so forth."

Results from laboratory experiments suffer from understandable drawbacks. They are nearly always performed with relatively small samples of university students, groups that typically exhibit a relatively high degree of homogeneity, despite variation in gender, for example.[2] In addition, monetary rewards for active participation in such experiments are frequently limited, which raises the question to what extent these experiments capture learning.[3] Further, experiments that are small in size in terms of participants and in terms of earnings are usually also small in the choice set that participants face.[4]

We avoid these methodological problems by using data from a series of naturally occurring auction stages on a quasi-stock market in the Czech Republic. The aim of the set of auctions was to privatize more than 1600 state-owned enterprises.

[1] Non-experimental based evidence of learning is given by Thompson (2001), who offers estimates of the contribution of learning to the rapid increases in labor productivity.

[2] These problems are quite understandable since the costs of experiments with large groups of individuals are extremely high and the conduct of such experiments poses other nontrivial problems: credible arrangement for all participants, and managing large numbers of individuals, to name but only two (for a related account see Harrison and List, 2004 or Carbone, 2005).

[3] Researchers have raised doubts whether a payment of, say $30, provokes enough activity to warrant real world results. This critique could be simply restated as "low gain for low effort".

[4] In line with the above limitations, arguments stressing that incentives must be salient for experimental data to have meaning were voiced by Smith (1976, 1982), Harrison (1989), Smith and Walker (1993), Wilcox (1993), and Hertwig and Ortmann (2001).

Participants in this experiment—ordinary citizens—were counted in the millions; a participation rate of 87% of eligible citizens minimizes possible sample selection bias.[5] Our study is based on complete individual bidding data for a randomly drawn heterogeneous sample of 5000 citizens from a population of 2.2 million; this sample size allows us to estimate population proportions with a precision of ±1%.[6] To the best of our knowledge, these data constitute one of the largest samples available worldwide that can trace each step of each individual during such a set of naturally occurring auctions.[7] The rewards for participation in these auctions were substantial. The expected returns were several months' average salary, with the maximum returns being several years of average salary.[8] In addition, individuals paid a participation fee equal to about one week of salary. Participating individuals clearly had strong incentives to learn.

Our analysis contributes to the debate on learning in three ways. First, we supplement the theoretical literature on learning with evidence that learning took place among uninformed heterogeneous individuals during the real-life auction-like process. Second, we base our evidence on a data set from a naturally occurring set of auctions that by size, incentives, and variation belongs among the largest experiments ever conducted. Third, in order to detect and quantify learning we develop a new measure of individual bidding performance based on its distance from performance of the benchmark (market portfolio). These measures account for varying prices of goods (shares) available in succeeding stages of bidding.

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[5] A study by Harrison, Lau and Rutström (2005) shares such a bias.

[6] The data on 5000 individuals corresponds to the maximum size of the representative sample we could obtain given the amount of machine time we received from a government agency to collect the data. Such a sample size allows us to estimate very well any probability of occurrence within the whole population. For example, Newcombe (1998) presents the confidence intervals for sample proportion using normal approximation as $[p_{LB}, p_{UB}]$ where $p_{LB} = \hat{p} - z_{1-\alpha/2}\sqrt{\frac{\hat{p}(1-\hat{p})}{n}}$ and $p_{UB} = \hat{p} + z_{1-\alpha/2}\sqrt{\frac{\hat{p}(1-\hat{p})}{n}}$.
As is standard, z stands for the quantile of the standard normal distribution and n denotes the sample size. Precision of ±1% in estimating population proportions follows directly from the above.

[7] There are previous experiments that use large samples. In early work Suppes and Atkinson (1960) reported gaming results from more than 1000 subjects. Harrison, Lau, and Williams (2002) estimated individual discount rates among 268 people that were sampled so as to be representative of the Danish population. List (2003) observed the trading decisions across 148 subjects. Carbone (2005) engaged 498 participants in a comparison of the actual consumption strategy with the fully optimal strategy. In a fund-raising campaign designed as a field experiment List (2004a) engaged 1946 individuals. The experimental design in List (2004b) includes data gathered from more than 1100 market participants.

[8] These incentives are higher than those in Kachelmeier and Shehata (1992), where in the highest payoff condition, subjects earned three times their normal monthly revenue in the course of a two-hour experiment that was conducted in another transition economy, the People's Republic of China; or in Slonim and Roth (1998) where in an ultimatum game experiment in the Slovak Republic, financial incentives were varied by a factor of twenty-five.

Using the non-parametric Wilcoxon (1945) sign rank test for paired data we compare individual outcomes over different action stages with respect to the benchmark, account for changing prices of auctioned commodities, and show that significant learning is observed among relatively uninformed individuals. We conjecture that the extraordinary incentives that participants faced were the key factor driving our results.

The paper is organized as follows: Section 2 describes the natural experiment and the data; Section 3 defines learning formally and reports the empirical results. Section 4 briefly concludes.

2. Set of Naturally Occurring Auctions and Data

The data are from a set of naturally occurring auctions: the bidding process to acquire shares in privatized firms known as the voucher scheme (see Section 2.1 for details). The set of auctions could be considered a natural experiment in a way similar to Metrick (1995), though the stricter definition of Harrison and List (2004, p. 1041) might consider this label inappropriate due to the absence of a baseline.[9] In any event, natural experiments are appealing sources of data since "they reflect the choices of individuals in a natural setting, facing natural consequences that are typically substantial" (Harrison and List, 2004, p. 1042). We take the liberty to label our set of naturally occurring auctions as an experiment.

The subject pool contained all Czech citizens 18 years and older[10], who were provided with instructions on the bidding process and public information on privatized firms. Thus, bidding was based on publicly known rules. The individual investors were bidding to acquire a specific commodity: shares in privatized firms that could be sold for money after the end of the privatization scheme. The shares represented substantial (future) monetary value, hence the bidding incentives were high. Bidding was performed on an artificial market to proxy for the stock market.

So far all comparisons done on the voucher privatization scheme have used aggregated data, i.e., bids of all individuals were treated as the outcome of one (uninformed) agent. Such specifications do not take into account that the population of those individuals successfully placing their bids could vary significantly between

[9] Harrison and List (2004, p. 1041) state that "a natural experiment takes place when naturally-occurring comparisons of one or more treatments with a baseline are observed". For early work using natural experiments see for example Frech (1976) and Deacon and Sonstelie (1985).
[10] Due to high participation rate virtually no truncation is involved in our case.

rounds. Therefore, in order to analyze if individuals were able to learn and adjust their bidding behavior between rounds, it is imperative to deal with individual bidding data.

2.1 Voucher scheme as a set of naturally occurring auctions

The set of privatization auctions was part of the massive privatization program administered in the Czech Republic in the first half of the 1990s under three different schemes: restitution, small-scale privatization, and large-scale privatization. The first two schemes began in 1990 and were most important during the early years of the transition. Large-scale privatization, by far the most important scheme, began in 1991, was completed in early 1995, and allowed for various privatization techniques.[11] Small firms were usually auctioned or sold in tenders. Many medium businesses were sold in tenders or to predetermined buyers in direct sales. Most large and many medium firms were transformed into joint stock companies and their shares were distributed through voucher privatization (almost one-half of the total number of all shares of all joint stock companies was privatized in the voucher scheme), sold in public auctions or to strategic partners, or transferred to municipalities.

The voucher scheme was part of the large-scale privatization process. Two waves of voucher privatization took place in 1992-93 and 1993-94, respectively.[12] Both waves were administered in the same manner and there were no differences in their set-up. During the scheme, a total of 1664 firms were privatized: 988 in the first wave, and 676 firms in the second wave.[13] Our data come from the second wave. All Czech citizens over the age of 18 who resided in the Czech Republic[14] could participate in the voucher process. For each wave every eligible citizen was authorized to buy a voucher book that contained 1000 investment "points" for 1000 crowns (about a week's wage). The voucher book contained investment points in values of 100, 200, 500 and 1000; therefore the smallest bid was 100 points (in other words an individual's portfolio could

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[11] The privatization process has been extensively described and analyzed. See e.g., Kotrba (1995); Valbonesi (1995); Hanousek and Kroch (1998); Kočenda (1999); and Filer and Hanousek (2001) among others.

[12] Privatization of each state-owned firm was decided on the basis of an officially accepted privatization project. According to the law, all state-owned enterprises were selected either for the first or the second privatization wave, or they were temporarily exempted. Each selected firm had to submit an official privatization proposal that was usually crafted by the firm's management under the tutelage (and responsibility) of its sectoral ministry. Any domestic or foreign corporate body or individual was allowed to present a competing project that was to be considered on an equal footing to the official one.

[13] 185 firms were privatized in both waves in various proportions of their assets.

[14] For the first wave in the Slovak Republic, as well, since only in 1993 Czechoslovakia was split into two independent nations: the Czech and Slovak Republics.

be diversified up to 10 different items). Before the bidding started, individuals had the option of assigning none, some, or all of their points to Privatization Investment Funds (PIFs): newly established financial firms vaguely similar in their scope of activities to closed-end mutual funds. These PIFs had to publish basic facts about both the founder and the investment strategy. This part of the scheme is usually called "pre-round" or "zero round".

When entering the voucher scheme, individuals had two basic strategies to choose from. One was to bid for a particular firm to exercise shareholder's rights of control. However, the limited number of voucher points that were available for each individual during the bidding process effectively prevented individual bidders from exercising control over a privatized firm.[15] Individuals' awareness about this fact thus made shares of any given firm perfect substitutes. Thus, the second strategy was to maximize cash revenues from the future sale of shares, receive dividend payments, or a combination of both.

Before the start of the bidding process, the public was given basic financial information about each enterprise to be transferred. The information included employment, wages, capital, sales, costs, profit or loss, liabilities, foreign trade, ownership structure, etc. and was published in the special periodical "Kuponová Privatizace" (i.e. Voucher Privatization). During the bidding process citizens and PIFs used their voucher points to buy shares of available firms in a series of price-administered bidding rounds.[16] To avoid end of game problems, the total number of rounds was not set. Nevertheless, observers suspected that the total number of bidding rounds should be between 3 and 7. The bidding scheme was by no means a standard auction, since investors' bids were quantities and the prices were in fact administered by the privatization authority.[17]

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[15] After the voucher scheme ended, this assumption proved to be accurate. Resulting ownership was simply too dispersed to allow individual shareholders to exercise control.

[16] A Pricing Committee in the Ministry of Finance adjusted prices between each round primarily using an excess demand rule. The sequential character of the bidding and closed-economy character of the auction prevented incentives for speculation from materializing. The announced goal of this commission was to adjust prices so that by the end of the process citizens had used all their points while distributing as many shares as possible. For details see Filer and Hanousek (2001).

[17] Czech voucher privatization thus resembled, but was not identical to, any classical market mechanism design. It was not a Walrasian tâtonnement since demands were satisfied prior to determination of the equilibrium price and there was no recontracting. It bears some resemblance to a multi-unit Dutch auction, although there were several key differences including the fact that the initial price was set at a supposed approximation of the true equilibrium price rather than a price higher than the reservation price of any individual bidder. For more details see Hanousek and Kroch (1998) and Filer and Hanousek (2001).

Each bidding round was divided into four stages. In the first stage participants were told the administered price of the shares of each firm and the number of shares offered. Participants then bid for shares of their preferred firms. The third stage meant collecting, matching and analyzing the bids. The last stage, in fact, coincided with the first stage of the next round; the results of the bidding were announced and the Pricing Committee set the prices for the next round.

The bidding rounds continued until the privatization authority revealed the end of the wave when a negligible proportion of unsold shares along with disposable investment points remained. The final stage of voucher privatization was the real transfer of the purchased shares. For each participant, a share account at the Central Register was created. Those individuals who allocated part or all 1000 points to PIF(s) obtained the shares of PIFs immediately after the issue. Shares of firms obtained by individuals during the bidding process were traded on the capital market after the end of the privatization scheme.

From the above outline it is evident that the bidding scheme was a way to establish market prices where there was no market, by using—in a sequential process—a market response to adjust administered prices. Although the prices are not market prices, one can consider observed demand to be a market response to the set prices.

2.2 Auction design (rules for accepting bids)

The auction design of the Czech privatization scheme was relatively simple and can be described by the following rules:

1. The shares of privatized firms were offered in a sequence of auction stages (rounds). Prices in the first round were equal for all stocks since the number of shares issued was determined by a firm's book value; specifically, the first round price of one share of any firm was set at 50 voucher points (artificial currency). The number of points entering the sequence of auctions is known and no new points enter the auction after the first round has begun.
2. In each successive round, prices were adjusted up or down as a function of the excess demand for or excess supply of stock in the previous round. Thus, if there were a large excess supply (demand) in round r, the price was reduced (increased) in round $r+1$. Price -- number of points per share -- was administered by the Pricing Committee, which never publicly revealed its algorithm for adjusting share prices

between rounds. It was generally observed that, indeed, prices rose for shares in excess demand and fell for shares in excess supply.[18]

4. If bids for a firm did not exceed its supply of shares, the demand was satisfied and the remaining shares were deferred to the next round.
5. If the demand for a firm's shares exceeded supply by less than 25%, then individual investors had their demand met while bids from investment funds were rationed in proportion to their bids, provided that the funds were allocated at least 80% of their original bids. In such a case, all shares were sold and the firm was not available for purchase in the succeeding rounds. Once the auction began, the above proportions were made public and kept constant during the auction sequence.
6. In all other cases no shares were sold and all shares were carried over to the following round.
7. Between rounds the public was updated by providing information on revised prices and the total demand for each stock by individuals and investment funds in the previous round. Specifically, once the auction began, the authorities after each round publicly revealed the following set of information: demand for a particular firm's shares in the previous round (separately by individual investors and PIFs), amount of shares available for the upcoming round, prices achieved in the past round, and prices set to begin with in the upcoming round.

2.3 Auction data

Our data come from the series of auction stages that were performed during the second wave of the voucher scheme. The timing of the second wave is depicted in Table 1. When we consider the amount of state property to be privatized through this method it is clear that the voucher scheme was an extremely fast and dynamic process. The time span from the beginning of the bidding process to the distribution of the shares was about a year. A total of 6.16 million individuals participated in the second wave of the voucher scheme. Out of this total, 3.96 million (64.3%) assigned some or all of their points into PIFs; 3.87 million individuals assigned all 1000 points to PIFs (3.46 million put them into a single PIF of their choice), while a negligible number of 0.09 million individuals opted to allocate some points to a PIF and use some for independent

[18] On a discretionary basis prices were increased in some cases for those shares whose excess supply was small relative to the mean market's excess supply.

bidding. On the other hand, out of the total, about 2.2 million (35.7%) individuals bid on their own (and did not allocate any of their points to PIFs).

Thus, the bidding process involved two groups of agents. The first group was formed from ordinary citizens who can be characterized as uninformed heterogeneous agents since they possessed only limited prior knowledge of the process itself and had access only to the publicly available facts about firms. PIFs, as the second group, can be considered informed agents: the scope of their business activity was to invest points (artificial currency) *en masse* and to do so they were equipped with teams of analysts. The PIFs had technical as well as expert advantage in processing public information over the individual investors.

Our data sample consists of detailed individual bidding information of 5000 citizens randomly selected out of the 2.2 million who decided to bid all their points individually and not through the PIFs.[19] This way we ensure that every individual in our sample had exactly the same starting conditions in terms of 1000 points available.[20] We assume that, in general, these individuals were eager to participate actively in the scheme and that they had confidence in their own judgment.[21] The bidding data trace in detail every move of each individual during the bidding process. As complementary data we have aggregate information on bidding made by PIFs. Since the PIFs possessed better abilities to process all available information plus possible private information, they are a convenient representation of an informed investor.

Hanousek and Kroch (1998) found that the demand for a specific firm by individuals was strongly influenced by demand for that firm by investment funds in previous rounds (information that was widely available in the local daily press) even

[19] Random sampling was random also in the sense that selection did not involve the characteristics of individuals. Sampling was based on anonymous ID numbers of voucher books, which certifies the representativness of our sample. In effect our sample covers 0.23% of the population, which means that every 440th member of the population is included in our sample.

[20] From random selection we exclude those 0.09 million individuals that bid in combination through PIFs and by themselves, since this minority had less than 1000 points available for individual bidding (the rest went to PIFs). By excluding them we avoid the problem of different starting conditions and different budget constraints in our sample.

[21] Many of these individuals might have participated in the first wave of the voucher scheme. Unfortunately, we are not able to identify them. Given the higher participation rate of individuals in the second wave, we conjecture that the majority of these individuals participated also in the first wave. Therefore, there is a high likelihood that most of the participants were familiar with the bidding procedure from the first wave (of the voucher scheme); this attribute is similar to a warming-up phase common in laboratory experiments before an experiment officially begins.

after controlling for price and other public information.[22] This suggests that individuals may have believed that the market was not efficient and that funds had additional non-public information.

The aggregated behavior of the individual investors is illustrated in Tables 2-4. The bidding process started each round with the number of voucher points that were at the disposal of the individual investors. Table 2 shows proportions of *points available* at the beginning of each round. Obviously, at the beginning of the first round all individuals possessed the complete quota of 1000 points. We see that with each round the proportion of individuals who still had some points to bid decreases, which means that these individuals were able to allocate their points. The relative proportion of those who did not bid up to the last (sixth) round decreases and the proportion of those left with fewer than 100 points (minimum required to make a bid) increases over time. In the latter case we are talking about fewer than 30 individuals, though. Since the voucher scheme was a "closed economy" in the sense that voucher points could be used *only* during this scheme and after the final round the points had zero value, it was better to allocate the points wrongly in the last round than not to allocate them at all.

At the beginning of each subsequent round the new proportion of available points was determined by two steps. First, based on the information available, individuals made their decisions and bid their points in order to obtain shares of various firms; the proportions of these bids are shown in Table 3 (*points bid*). Panel A shows these proportions related to the whole sample of individuals, while panel B accounts for proportions of points related to the part of our sample that still possessed at least 100 points needed to make a bid. Thus, in panel B we do not consider those individuals who already spent all their points below the 100 point threshold. For completeness we also present basic statistics (mean and standard deviation) of absolute numbers of points that were bid in each round.

Second, bids were thereafter processed in accordance with the bidding rules. Table 4 shows the proportion of the bids that were actually processed and for which the shares were sold; these are *points spent* in each round and represent the reduction of available points for the subsequent round. The remaining points after this step naturally constitute the proportion of available points for the next round. Again panel A in Table

[22] Hung and Plott (2001) show in a simplified case that when a clear pattern is present, individuals tend to discount their private signal and follow the herd instead. Our results (see below) do not support the herding effect, e.g. individuals do not bid only for firms for which the PIFs bid initially.

4 shows proportions related to the whole sample and panel B shows these proportions with respect to individuals who still had enough points to bid. As in the previous layout, the bottom part of each panel contains basic statistics (mean and standard deviation) of absolute numbers of points that were spent in each round.

3. Learning

3.1 Definition and measures

In general terms learning is defined by Camerer (2003, p.265) as an “observed change in behavior owing to experience”. If the individuals in our set of auctions were learning, then, owing to experience, they should change their bidding strategy over the course of successive rounds to improve their expected performance.[23] In order to detect (or refute) the presence of learning we have to define appropriate measures of individual performance. In doing so we have to account for the fact that an individual could start the bidding in any round and continue until the very end of the scheme or until an individual had no points left, and that prices and selection of available shares were changing across rounds. From Tables 2-4 we see that individuals were able to allocate their points in a manner that would not waste these points. However, from the Tables we cannot derive information about the potential “profitability” of these allocations, or about whether the individuals have been learning. To do so we would need to connect the standard definition of learning above with some measures that capture the learning based on our data.

The voucher scheme used a specific “currency” – voucher points – with which participants could buy shares of privatized companies. After the scheme, unspent vouchers had zero value. Unless all individuals bid only for the same set of shares, we cannot measure and compare their performance between rounds using voucher points. However, there exists a natural way to assess individual bids, which is in the spirit of Erev and Roth (1998), who study both the ex post ('best fit') descriptive power of learning models, and their ex ante predictive power. Following this approach, we can

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[23] In our auction setting, there can be two ways learning takes place: learning about other bidders’ information (as the individual bidders are less informed) and also about other bidders’ strategies. In our context we consider only the former case (see point 7, subsection 2.3 on information that individual bidders had after each auction stage). Learning about strategies would occur if the bidders were not following some kind of equilibrium strategies (coordination is not an issue here). If individual bidders were learning about strategies, then informed bidders would be learning too. Theoretically, the informed bidders might choose to mislead the uniformed bidder through their bids. Practically, since all placed bids were binding and could not be revoked, attempting such misleading behavior would require coordination among hundreds of privatization funds. For this reason we consider the latter case irrelevant.

compare the values of acquired shares using their (*ex post*) prices on the secondary market after the bidding scheme ended. There is also an economic rationale for doing so: when bidding, individuals were in fact motivated by the vision of substantial rewards that they could collect in the secondary market instituted by the government. Their bidding strategy was aimed at collecting valuable shares of privatized firms (and to sell them later on the stock market). Thus, under the conditions of our experiment, learning should factor into the course of actions through which individual investors improved over time in terms of the future value of their bids on the stock market.

Therefore, we opt to assess individuals' performance in terms of the acquired shares valued with prices these shares carried on the secondary market after the bidding process ended. In order to have representative and relatively smooth data even for less frequently traded stocks, we use for each share the average price over the three month period (April - June 1995). This means that in our valuation we employ the share prices close to the period after the bulk of shares from the second wave of the voucher scheme entered the market. Also, during this period the majority of trades were initiated by individual sellers, liquidity was relatively high, and the prices should correspond to the expected payoff for the majority of participants. Further, by choosing this period we do not consider the medium and long run effect on stock prices caused by changes in ownership structure and corporate governance after the end of voucher privatization. As in the standard approach in financial literature, we allow for a one-month "settle-down period".[24]

Before evaluating the performance of individual investors, we have to take into account quantitative differences between successive auction rounds. First, the composition of each round differs in terms of the set of shares that are available as well as in terms of their quantities. Some shares (of particular firms) were sold out and were no longer available in the next rounds, while some were sold partially and thus were reduced in quantity available. Second, individual investors placed their bids in various quantities across rounds, e.g. the value of their bids differs with each individual. For both of the above reasons, we have to standardize the value of their bids by considering the value of shares obtained per one voucher point. In order to homogenize the value of shares offered in each round, we link each round with a particular performance

[24] Thus, we do not consider prices from March 1995, when trading of shares began. The second reason for doing so is the fact that shares were put on the secondary market gradually during this month; from April all of them were available for trading. We also checked against inclusion of March and did not find any significant difference.

benchmark (defined later). Typical candidates for a performance benchmark would be the weighted averages of values of shares purchased in a particular round. In other words, our benchmarks (mean value or return assigned to each round) will be equal to some portfolio value.

In order to define value per point (*bid* or *spent*) we consider the following three kinds of investors: uninformed (*U*), i.e. an individual investor in our sample; informed (*I*), i.e. a privatization fund; and market portfolio (*M*), i.e. passive investor bidding according to market capitalization measured in voucher points (a.k.a. index portfolio). The market portfolio is the value weighted portfolio, similar to the Capital Asset Pricing Model of Sharpe (1964), Lintner (1965), and Mossin (1966). Following Sharpe (1970, p. 82), for each separate round of our experiment the market portfolio (*M*) is defined as the sum of proportions invested in the *i* firms being privatized (X_i^M). Formally, the proportions are defined as

$$X_i^M = \frac{P_i Q_i}{\sum_{j=1}^{N} P_j Q_j} \qquad \text{for } i = 1, 2, \ldots, N \tag{1}$$

where P_i represents the price of a share of privatized firm *i* expressed in voucher points for a particular round where such a price is the result of the bidding mechanism, and Q_i represents the number of shares outstanding (e.g. number of shares allocated in each round). Hence, our market portfolio *M* is defined as

$$M = \sum_{i=1}^{N} X_i^M \ . \tag{2}$$

In our experiment there is no riskless asset (e.g. in our context there is no privatized firm for which a successful bid could be made without risk and with guaranteed return), so the market portfolio in our case does not contain such an instrument. Also, our market portfolio is calculated separately for each round so that we can make a dynamic comparison over the rounds. Thus, the market portfolio in our experiment is a collection of share holdings in which the voucher point value of each holding is proportional to the market capitalization of the respective company measured in voucher points.

To detect and quantify the hypothesized learning from our data, we define measures of learning based on the value of shares achieved per point spent in the bidding. In terms of our set of auctions, *learning is taking place when an individual investor outperforms the benchmark or, for an underperforming individual, the distance* *from a benchmark is getting smaller over time*. In order to make such an assessment, let us denote value per point (spent or bid) for uninformed (individual) *j*-th investor in round *t* as $V_U(j,t)$; value per point for market portfolio in round *t* as $V_M(t)$; and value per point for informed investor in round *t* (average value of all informed investors) as $V_I(t)$. All values per point are evaluated at the secondary market prices.

An observed change due to experience (e.g. learning) means that individual investors perform better over time in the bidding process, in the sense that individual performance either exceeds or is getting closer to the selected benchmark. Thus, we could observe how the value $V_U(j,t)$ developed over time; in a similar fashion we could observe the development of the value of the ratios $V_U(j,t)/V_M(t)$ or $V_U(j,t)/V_I(t)$. The ratios eliminate the effect of a particular auction stage but they alone would not allow us to detect (or refute) learning. Moreover, it is expected that yield would increase in higher rounds. Table 5, which contains the values of the market portfolio in each round, confirms such expectations. Note that very high maximum payoffs can be observed in rounds 2-4. This pattern in understandable since in round 1 the prices were set in a uniform pattern based on the book values of the firm; these book values were founded on the accounting principles from the command economy. We conjecture that during later rounds the potential payoff increases dramatically (due to an excess demand rule) and corresponds to the high motivation of individuals. Due to the fact that the price setting mechanism is based on an excess demand rule, the prices in later rounds move closer to their equilibrium, and the market portfolio is close to the optimal bidding strategy. The decrease in prices over the succession of auction stages is also due to increasing market efficiency. The above pattern, in which an excessive rent is vanishing over time and prices converge to equilibrium, conforms to the classic argument of Smith (1962).

For the above reasons it is imperative to observe the evolution of the distance between the value of an individual's performance (uninformed investor) and that of the informed investor or the market portfolio. Changes in this distance allow for accurate assessment since it is free from changes in values of shares in successive rounds. Following the above line of reasoning, we define the measure of performance of an individual in the bidding process in the form of performance distance from the benchmark: the market portfolio.

The distance of individual relative performance from market portfolio performance is defined as

$$D_M(j,t) = \max\left(1 - \frac{V_U(j,t)}{V_M(t)}; 0\right). \tag{3}$$

For those individuals outperforming the chosen benchmark we set the distance equal to zero and we do not scrutinize the extent of such "outperforming". Nevertheless, the extent of performance can be assessed from Figure 1 (presented in Section 3.1), which also includes its distribution over the individual bidders.

In the context of our performance indicator, learning occurs when the performance indicator does not deteriorate over time.[25] Specifically, we formulate our null hypothesis and its alternative as:

$$H_0 : D_M(j,t) \geq D_M(j,t+1), \quad H_A : D_M(j,t) < D_M(j,t+1) \tag{4}$$

Thus, learning is detected when we are not able to reject the null hypothesis.[26]

3.2 Statistical inference

To verify our hypotheses we have essentially two options:

1) One is to estimate a model and compare relevant coefficients. In such a case, we would exploit the dynamic panel structure of our data, assume normality of residuals, and inspect time-varying coefficients between bidding stages, which would be equivalent to conventional *t*-tests. Let us note that a standard panel estimation procedure (involving dynamic structure of the data) typically leads to biased estimates (for an exposition see Wooldridge, 2002).[27] Moreover, a formal model that would capture the

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[25] As mentioned earlier this approach is more restrictive for those individuals performing below the selected benchmark and less restrictive for those that outperform the given benchmark.

[26] Analogously, we can define the distance of individual relative performance from performance of the informed investor as $D_I(j,t) = \max\left(1 - \frac{V_U(j,t)}{V_I(t)}; 0\right)$.

The null and its alternative is then $H_0 : D_I(j,t) \geq D_I(j,t+1), \quad H_A : D_I(j,t) < D_I(j,t+1)$. However, for practical reasons we prefer the performance indicator defined with the market portfolio. Privatization fund (informed investors) performance was a good benchmark in earlier rounds of the auction, but in later rounds the funds faced legal restrictions on percentage of shares they were allowed to hold in any single firm and rebalanced their portfolios accordingly. This may impede the accuracy of such a benchmark.

[27] Well-known procedures, which account for error dynamics, either require a good set of additional instrumental variables (which we do not have) or are based on application of the Arellano-Bond procedure (see Arellano and Bond, 1991). The Arellano-Bond approach is based on differences of variables, and two lagged variables in levels are employed as valid instruments. We would lose the first two bidding rounds of data if we employed the Arellano-Bond procedure. Hence, neither of the two methods above is a viable option in our case.

complexity of our experiment could easily be misspecified, which might result in biased coefficients and a less than accurate inference.

2) To detect learning, as it is specified above, we are principally interested in movements of coefficients up and down (increase or decrease in performance) rather than in estimating specific values of such coefficients. Therefore, it is better to use a panel data structure to properly match individual performance between rounds and test whether we observe an improvement in performance. Moreover, we can relax any distribution assumptions and employ a non-parametric approach.

For the above reasons we do not build the model and we use the second option. We chose to verify the postulated hypotheses by conducting a battery of the Wilcoxon (1945) sign rank test for paired data. This is a robust non-parametric approach that has been used as an alternative to the paired *t*-test for a long time[28]; Lehmann (1998) provides a general statistical exposition of the test. For our purpose, the test is very suitable since it accounts for magnitude in differences of individual performance from a benchmark that varies in different auction stages. The procedure has been frequently used in the empirical finance literature on stock or exchange rate returns.[29]

The approach outlined above allows us to track the performance of each individual investor over time at each round and compare such performance with that of our benchmark.[30] Rejecting the null hypothesis means that learning is detected.

3.3 Empirical observations of learning

By virtue of bidding rules and limited resources (1000 points per individual only) individual investors could not revoke their accepted bids; nor could PIFs revoke their bids. For this reason any sign of learning detected by our empirical tests would be a lower-bound measure of potential learning in a less constrained setting. However, that would mean that if learning is detected, it is a strong indication of learning.

The condensed results of the tests are presented in Tables 6-8. Row *i* and column *j* in both tables correspond to rounds *i* and *j* in which sub-populations of our sample

[28] In this approach we treat our data as a panel with fixed effects. When analyzing differences between two rounds, such differences eliminate individual effects as well as bias and heterogeneity of agents. In this way we are able to avoid major problems that plague experimental analysis, namely bias due to heterogeneity of agents (see Wilcox 2005).

[29] For use of the sign and the sign rank tests see Flores (1986); Zivney and Thompson (1989); Corrado and Zivney (1992); Abrevaya (2000); Fatum and Hutchinson (2003), among others.

[30] Thanks to the extensive panel structure of our data we are able to trace learning of individuals across rounds as well.

successfully placed their bids. The numbers presented in this (*i, j*) intersection correspond to the sign test of the hypothesis that there is no learning effect found between corresponding round *i* and *j*. To be specific, for example, the first cell in Table 6 contains the following symbols and numbers ↑ **261** **, ↓ 80, p-value: 0.001. This means that there were 341 (=261+80) individuals which successfully placed their bids in rounds 1 and 2; for 261 individuals the distance between the benchmark (the market portfolio) decreases while for 80 individuals the distance from the benchmark increases; p-value of the underlying sign test was lower than 0.001, bold face letters highlight that there was significant increase in relative performance between round 1 and 2, and finally (**) marks the level of significance (1%).

The diagonal cells of Tables 6-8 present results of tests for performance between two successive rounds. As complementary evidence, in the other cells of the upper right triangle of each table we provide results of learning between two non-successive rounds.

When learning is measured with the help of the distance of individual relative performance from market portfolio performance (D_M (j,t), Table 6)) the evidence of learning between consequent rounds (diagonal cells) is overwhelming. In all cells on the diagonal, learning is detected at the highest significance levels. This we can identify as an instantaneous effect of learning.[31] Further, the number of individual investors who bid in each round and actually allocated their bids successfully increases up to the fourth round.[32]

The strict budget constraints (all bidders had vouchers of 1000 points) may evoke the possibility of an alternative explanation for the narrowing of portfolio performance over time by speculating that informed bidders bid more aggressively (and at a higher price) in earlier rounds, thus reducing their number of points, their flexibility of portfolio choice, and their advantage over uninformed bidders. Such an alternative explanation is, however, faulty. Informed bidders (privatization funds, PIFs)

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[31] As in Berk, Hughson, and Vandezande (1996), learning during the bidding experiment reduces the frequency of errors and may be interpreted as evidence of bounded rationality. Hossain (2004) analyzes the effect of learning from a posted price in a dynamic second-price auction with a structure similar to those of Bajari and Hortacsu (2003) and Roth and Ockenfels (2002). For these reasons, his and our results are not comparable.

[32] Additionally, we performed the tests for evidence of learning as measured by the distance of individual relative performance from informed investor (fund) performance. The tests yielded qualitatively identical results as when the market portfolio is used as a benchmark. However, given the different objectives of the privatization funds (mostly control of the companies), institutional barriers (a PIF could control only up to 20% of the company) and different behavior at the end of the game, we do not report the details (available upon request).

managed a total of 3.96 billion points (63.5% of all points available). Out of these available points the funds spent 19.7% and 42.2% of the points during the first and second round, respectively. Thus, their advantage over the uninformed individuals was not eroded at all. This is complemented by the fact that the number of points available to individual bidders decreased steadily with each round (Table 2).

Nevertheless, we explore the behavior of the uninformed bidders who faced a realistic budget constraint from round to round as their allotment of points decreased and relative value was affected by changes in the price of different shares. The results in Tables 7-8 account for the "budget constraint" of 200 and 300 points being available as minimum for two compared bidding rounds. We impose such a budget constraint since we hypothesize that it could affect or interfere with learning.

The 200-points-constraint does not significantly change the results from the no-constraint case. The distance between the benchmark (market portfolio) increases for a somewhat smaller number of individual investors when compared with the no-constraint case (Table 6), but the evidence of learning between consequent rounds (diagonal cells) is again substantial. In all cells on the diagonal, learning is detected at the highest significance levels, which we identify as an instantaneous effect of learning even when the budget constraint (200 points) is considered. Further, as in the no-constraint case, the number of individual investors who bid in each round and who successfully allocated their bids increases up to the fourth round

The same pattern emerges when we augment the budget constraint of available points between successive rounds to 300 points. The significant increases in relative performance are less frequent than in the no-constraint and 200-point-constraint cases, but the evidence of learning is again solid. The exception to this pattern is lack of significance related to bidding in the sixth round. Such an outcome should be expected, though. Given the announcement of the privatization authorities, individuals know that the sixth round is the last one and that unspent points will be worthless afterwards. Thus, with the imposed budget constraint, individual investors tend to bid for any shares still available regardless of their price (up to their available points) and learning should not be expected in a situation like this.

In addition, we also consider a stronger version of learning by analyzing individuals' behavior between two non-consecutive rounds. The relevant hypothesis would be formulated in the following form:

$$H_0 : D_M(j,t) \geq D_M(j,t+s), \quad H_A : D_M(j,t) < D_M(j,t+s), \qquad s > 1 \quad (5)$$

In fact, in hypothesis (5) we ask to what extent learning has a long-term effect. As we can see from Table 6, in none of the possible cases were we able to reject hypothesis (5). Moreover, eight out of ten possible tests show significant effects of learning even in the case of a bigger time distance between compared rounds. This result is strengthened by the fact that there was not a single case in which we would be able to reject the long-term effect of learning.

The Wilcoxon sign rank test takes into account the magnitude of the differences, e.g. the magnitude of the movements of individual bidders towards the benchmark. Thus, the test effectively eliminates cases when, for example, some of the individuals could have an extremely suboptimal bidding strategy in, say, round 1 and alter it insignificantly, but in the right direction, in round 2. Further, learning took place in a consistent manner across individual bidders since learning was detected not only between successive rounds (on the diagonal in Tables 6-8) but also between non-consecutive rounds (off the diagonal). If individual bidders followed the above hypothetical bidding strategy on a large scale, such a pattern would not emerge in the first place.

More illustrations of the magnitude of the learning process can be drawn from Figure 1, where distributions of bidders by their distances from the market portfolio benchmark are presented for each bidding stage. With the exception of round 6 (end of the auction) the distributions follow a similar pattern. Distributions across all bidding stages are stable and document that there were no dramatic changes in performance of individual bidders. This fact combined with the results presented in Tables 6-8 provide evidence that learning occurred across all bidders: both better and worse performers were able to reduce the distance from the benchmark. The majority of bidders was able to bid very close to the benchmark, but such a proportion decreased with successive bidding stages. This pattern is quite understandable: as the auction stages progressed, the market became more efficient and prices reflected available information. Thus, the extent of learning is correspondingly lesser at higher stages as documented by the decreasing proportion of close-to-benchmark bidders in Figure 1.

Our results are in line with the findings of List (2004c), who concludes that "market experience plays a role in the distribution of rents: experienced market players earn more rents than inexperienced agents." Further, the design of our experiment contains multiple market periods that allow agents to learn based on their accumulated experience.[33] We have shown that learning occurred in between successive as well as non-successive auction stages. In this context, our results lend a new perspective to the findings of Smith (1962) and List (2004c).

4. Conclusions

We have tested for learning among relatively uninformed heterogeneous individuals engaged in bidding on a proxy stock market. For that we have used an extensive data set taken from a series of naturally occurring auctions through which privatization of state owned enterprises occurred in the Czech Republic (1993-1994). We developed new measures of individual performance to accommodate for varying prices of goods (shares) available in six successive stages of bidding.

Specifically, we have used the equivalent of a market portfolio as a benchmark and we have scaled performance of individual investors to this benchmark. Since the benchmark increases over time, an individual investor learns if performing better than the benchmark. As a robust check we also imposed budget constraints of a minimum number of points to be available for two compared (consecutive) bidding rounds, which we hypothesized would negatively affect learning. We performed all possible pair-matched Wilcoxon sign rank tests. In none of these tests were we able to reject the learning hypothesis. Moreover, learning was found significant in nearly all tests performed for both successive and non-successive rounds. The exceptions are insignificant results with respect to the sixth bidding stage, especially when a significant budget constraint was imposed.

Our analysis supplements the theoretical literature on learning by providing evidence of the learning that took place during the naturally occurring auction-like process. We have used a large dataset from an experiment that by size, incentives, and variation is superior to any laboratory experiment we know of. Since individuals had to pay a fee to participate, and since the potential gain was in the magnitude of several months' salary, we conclude that large incentives were driving our results.

[33] Since uninformed bidders observed the behavior of informed bidders (by knowing *ex post* what the PIFs invested in) and to an extent factored it into their bidding, they show the features of social learning as described by Ballinger, Palumbo and Wilcox (2003).

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Table 1. Voucher Privatization — Time Framework

Steps in Voucher Scheme	Second Wave
Preparation	January–September 1993
Voucher Book issue	since October 1993
Registration	October – December 1993
0–round (Vouchers to Funds)	December 1993 – March 1994
1st–6th Round	April – December 1994
Official End	December 31, 1994
Transfer of Shares	February 1995
Trading of Shares Started	March 1995
First PIF Shares Issued	April 1995

Source: Ministry of Privatization.

Table 2. Points available

A. Distributions: Points available							
Variable	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6	The end
Points distribution							
0		5.4%	17.4%	30.3%	62.6%	78.7%	88.8%
100		0.0%	0.8%	1.6%	3.0%	3.6%	2.2%
200		0.3%	2.2%	3.9%	6.8%	5.1%	1.8%
300		0.2%	1.0%	2.2%	2.7%	2.0%	1.7%
400		1.0%	3.2%	6.5%	5.3%	2.7%	1.6%
500		6.6%	10.2%	10.7%	5.5%	2.6%	1.1%
600		2.3%	4.5%	6.6%	2.5%	1.1%	0.6%
700		1.1%	1.6%	2.2%	0.7%	0.4%	0.4%
800		5.4%	6.5%	5.6%	1.2%	0.6%	0.6%
900		1.2%	1.3%	1.1%	0.5%	0.7%	0.8%
1000	100.0%	76.5%	51.3%	29.4%	9.2%	2.7%	0.5%
Descriptive statistics							
mean	1000	879	687	499	199	90	46
std. dev.	0	264	387	406	323	221	158

B. Individuals who did not bid in a particular round, nor in preceding rounds							
Variable	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6	The end
Not bid	1009	307	100	64	23	0	--

Sample size: 5000 individuals.

Note: In the first round all individuals have 1000 points available. Since every individual could allocate the voucher points only in multiples of hundreds, the table above gives detailed distribution of points available at the beginning of each round (this information can be used to construct a similar distribution of the points spent). The fact that *x* % of participants have *y* points available in the second round means that *x* % of participants spent their (1000-*y*) points in the first round. A similar decomposition could be used for later rounds.

Table 3. Points bid

A. Distributions: Points bid						
Variable	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6
Points distribution						
0	20.2%	17.9%	25.1%	40.9%	69.4%	85.5%
100	0.1%	0.0%	1.4%	1.9%	3.1%	3.0%
200	0.1%	0.3%	2.4%	3.6%	6.4%	4.6%
300	0.1%	0.3%	1.2%	2.0%	1.7%	0.7%
400	0.2%	1.1%	3.2%	5.3%	4.1%	1.6%
500	1.1%	7.0%	8.9%	8.4%	3.9%	1.4%
600	0.6%	2.6%	4.6%	6.3%	2.3%	0.7%
700	0.1%	1.2%	1.3%	1.5%	0.3%	0.1%
800	0.7%	5.4%	6.1%	5.4%	1.0%	0.1%
900	0.2%	1.2%	1.1%	0.6%	0.0%	0.0%
1000	76.6%	62.8%	44.7%	24.3%	7.7%	2.2%
Descriptive statistics						
mean	785	749	612	418	159	57
std. dev.	403	387	422	414	300	184

B. Distributions: Points bid (for those who had at least 100 points)						
Variable	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6
Points distribution						
0	20.2%	13.3%	9.4%	15.4%	18.7%	32.6%
100	0.1%	0.0%	1.7%	2.7%	8.3%	14.1%
200	0.1%	0.4%	2.9%	5.2%	17.0%	21.5%
300	0.1%	0.3%	1.5%	2.9%	4.6%	3.4%
400	0.2%	1.2%	3.9%	7.6%	11.0%	7.3%
500	1.1%	7.4%	10.8%	12.0%	10.3%	6.7%
600	0.6%	2.7%	5.6%	9.0%	6.2%	3.1%
700	0.1%	1.3%	1.6%	2.1%	0.7%	0.7%
800	0.7%	5.8%	7.4%	7.7%	2.7%	0.6%
900	0.2%	1.3%	1.4%	0.8%	0.1%	0.2%
1000	76.6%	66.4%	54.0%	34.7%	20.3%	9.9%
Descriptive statistics						
mean	785	790	737	597	420	258
std. dev.	403	352	344	370	355	305

Sample size: 5000 individuals.

Note: Participants of the bidding scheme could allocate their vouchers only in multiples of hundreds; therefore, the table above provides detailed information on bidding patterns. Since individuals spent their points across rounds, their available points differ significantly since the second round. To account for points already spent we present both distributions. The upper part of Table (A) depicts a regular bidding pattern (unconditional distribution), while the lower part of Table (B) contains a conditional distribution of bidding for those individuals that have at least 100 available points.

Table 4. Points spent

A. Distributions: Points spent						
Variable	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6
Points distribution						
0	76.5%	68.0%	63.5%	50.1%	75.4%	87.2%
100	1.2%	1.3%	2.5%	2.1%	3.3%	3.0%
200	5.4%	5.2%	7.3%	5.4%	5.9%	4.2%
300	1.1%	1.1%	2.1%	2.5%	2.1%	0.9%
400	2.3%	3.7%	5.5%	7.8%	3.8%	1.2%
500	6.6%	6.2%	6.2%	7.8%	2.9%	1.2%
600	1.0%	2.1%	3.2%	5.6%	1.4%	0.5%
700	0.2%	0.6%	0.7%	1.2%	0.3%	0.1%
800	0.3%	1.6%	1.2%	3.2%	0.9%	0.3%
900	0.0%	0.6%	0.4%	0.4%	0.1%	0.2%
1000	5.4%	9.5%	7.3%	13.8%	3.9%	1.2%
Descriptive statistics						
mean	121	192	188	300	109	44
std. dev.	264	330	306	367	242	152

B. Distributions: Points spent (for those who had at least 100 points)						
Variable	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6
Points distribution						
0	76.5%	66.1%	55.9%	28.5%	34.1%	40.1%
100	1.2%	1.4%	3.0%	3.1%	8.8%	14.2%
200	5.4%	5.5%	8.9%	7.8%	15.9%	19.5%
300	1.1%	1.2%	2.6%	3.6%	5.6%	4.3%
400	2.3%	4.0%	6.6%	11.2%	10.1%	5.5%
500	6.6%	6.6%	7.5%	11.2%	7.7%	5.6%
600	1.0%	2.3%	3.9%	8.1%	3.9%	2.5%
700	0.2%	0.6%	0.9%	1.7%	0.9%	0.7%
800	0.3%	1.7%	1.5%	4.6%	2.4%	1.3%
900	0.0%	0.7%	0.5%	0.6%	0.2%	0.8%
1000	5.4%	10.1%	8.9%	19.8%	10.5%	5.5%
Descriptive statistics						
mean	121	203	227	430	292	209
std. dev.	264	337	323	370	321	273

Sample size: 5000 individuals.
Note: Participants of the bidding scheme could allocate their vouchers only in multiples of hundreds; therefore, the table above provides detailed information on patterns related to the points spent. The upper part of Table (A) depicts the distribution of points spent at the beginning of each round (this part of the table can be easily derived from Table 1, which shows the distributions of points available). Since individuals spent their points across rounds, their available points differ significantly since the second round. To account for points already spent we present both distributions. The upper part of Table (A) depicts the pattern of points spent (unconditional distribution), while the lower part of Table (B) contains the conditional distribution of points spent for those individuals that have at least 100 available points.

Table 5. Descriptive statistics -- Performance of Portfolios in CZK per 1 point

Round	Market portfolio	Informed investor portfolio	Individuals		
	mean	mean	mean	maximum	95% confidence interval
1	4.285	8.013	7.330	25.149	(7,070 ; 7,590)
2	6.514	12.514	15.043	313.010	(13,535 ; 16,551)
3	6.060	7.895	10.543	227.540	(9,717 ; 11,369)
4	7.384	9.418	7.522	126.400	(7,304 ; 7,739)
5	10.077	11.416	10.307	55.897	(10,047 ; 10,566)
6	14.048	17.312	13.463	34.337	(12,958 ; 13,969)

Sample size: 5000 individuals
CZK is the abbreviation for the Czech currency unit.
Note: In each bidding round the market portfolio has been constructed as an index portfolio (see equations (1)-(2)), while the informed investor portfolio represents an aggregated portfolio of all privatization investment funds (PIF) that acquired shares in a given round.
Values of portfolios presented in this table are valued at prices on the secondary market after the natural experiment ended. In order to account for less frequently traded stocks, we use for each share the average price over the three month period April - June 1995. This means that in our valuation we employ the share prices close to the period after the bulk of shares from our natural experiment (the second wave of the voucher scheme) entered the market (March 1995), allowing for a one-month "settle-down period".
For ease of comparison the values of all portfolios are measured in CZK per 1 point. Since all participants have 1000 points available, the mean payoffs of individuals (and their counterparts) are obtained by multiplying by 1000. Very high maximum payoffs can be observed in rounds 2-4. Since in round 1 the prices were set uniformly in an artificial pattern, we conjecture that during later rounds the potential payoff increases dramatically and corresponds to the high motivation of individuals. Due to the fact that the price setting mechanism is based on an excess demand rule, the prices in later rounds move closer to their equilibrium, and the market portfolio is close to the optimal bidding strategy.

Table 6. Summary of tests of learning in future rounds. Benchmark is performance (distance) with respect to the market portfolio in each round.

	Round 2	Round 3	Round 4	Round 5	Round 6
Round 1	↑ **261** ** ↓ 80 p-value: 0.001	↑ **267** ** ↓ 94 p-value: 0.001	↑ **284** ** ↓ 206 p-value: 0.001	↑ **135** ** ↓ 78 p-value: 0.001	↑ 56 ↓ 44 p-value: 0.136
Round 2		↑ **354** ** ↓ 127 p-value: 0.001	↑ **330** ** ↓ 237 p-value: 0.001	↑ **170** ** ↓ 96 p-value: 0.001	↑ 65 ↓ 70 p-value: 0.697
Round 3			↑ **503** ** ↓ 390 p-value: 0.001	↑ **248** ** ↓ 157 p-value: 0.001	↑ **104** * ↓ 76 p-value: 0.022
Round 4				↑ **398** ** ↓ 206 p-value: 0.001	↑ **163** ** ↓ 111 p-value: 0.001
Round 5					↑ **180** ** ↓ 127 p-value: 0.002

↑ denotes learning, ↓ no learning observed (opposite). Below we list p-value of the sign test.
* and ** denotes 5% and 1% significance level, respectively.

Total sample size: 5000 individuals.

Note: Rows *i* and column *j* in this table correspond to rounds *i* and *j* in which sub-populations of our sample placed their bids. The numbers presented in this (*i, j*) intersection correspond to the sign test of the hypothesis that there is no learning effect found between corresponding round *i* and *j*. For example, the first cell contains the following symbols and numbers ↑ **261** **, ↓ 80, p-value: 0.001. This means that there were 341 (=261+80) individuals who successfully placed their bids in rounds 1 and 2; for 261 individuals the distance between the benchmark (the market portfolio) decreases while for 80 individuals the distance from the benchmark increases; p-value of the underlying sign test was lower than 0.001, bold face letters highlight that there was significant increase in relative performance between rounds 1 and 2, and finally (**) marks the level of significance (1%).

Table 7. Summary of tests of learning in future rounds. Benchmark is performance (distance) with respect to the market portfolio in each round.
Budget constraint restriction: at least 200 points available in each compared round.

	Round 2	Round 3	Round 4	Round 5	Round 6
Round 1	↑ **260** ** ↓ 80 p-value: 0.001	↑ **261** ** ↓ 93 p-value: 0.001	↑ **276** ** ↓ 199 p-value: 0.001	↑ **110** ** ↓ 71 p-value: 0.001	↑ 34 ↓ 25 p-value: 0.149
Round 2		↑ **353** ** ↓ 124 p-value: 0.001	↑ **310** ** ↓ 224 p-value: 0.001	↑ **147** ** ↓ 82 p-value: 0.001	↑ 49 ↓ 52 p-value: 0.697
Round 3			↑ **481** ** ↓ 378 p-value: 0.001	↑ **216** ** ↓ 137 p-value: 0.001	↑ **71** * ↓ 51 p-value: 0.043
Round 4				↑ **350** ** ↓ 186 p-value: 0.001	↑ **163** ** ↓ 111 p-value: 0.001
Round 5					↑ **141** ** ↓ 88 p-value: 0.001

↑ denotes learning, ↓ no learning observed (opposite). Below we list p-value of the sign test.
* and ** denotes 5% and 1% significance level, respectively.

Total sample size: 5000 individuals.

Note: Rows *i* and column *j* in this table correspond to rounds *i* and *j* in which sub-populations of our sample placed their bids. The numbers presented in this (*i, j*) intersection correspond to the sign test of the hypothesis that there is no learning effect found between corresponding round *i* and *j*. For example, the first cell contains the following symbols and numbers ↑ **260** **, ↓ 80, p-value: 0.001. This means that there were 340 (=260+80) individuals who successfully placed their bids in rounds 1 and 2; for 260 individuals the distance between the benchmark (the market portfolio) decreases while for 80 individuals the distance from the benchmark increases; p-value of the underlying sign test was lower than 0.001, bold face letters highlight that there was significant increase in relative performance between rounds 1 and 2, and finally (**) marks the level of significance (1%).

Table 8. Summary of tests of learning in future rounds. Benchmark is performance (distance) with respect to the market portfolio in each round.
Budget constraint restriction: at least 300 points available in each compared round.

	Round 2	Round 3	Round 4	Round 5	Round 6
Round 1	↑ **257** ** ↓ 80 p-value: 0.001	↑ **250** ** ↓ 82 p-value: 0.001	↑ **244** ** ↓ 175 p-value: 0.001	↑ **69** ** ↓ 41 p-value: 0.005	↑ 14 ↓ 12 p-value: 0.422
Round 2		↑ **320** ** ↓ 108 p-value: 0.001	↑ **272** ** ↓ 199 p-value: 0.001	↑ **107** ** ↓ 52 p-value: 0.001	↑ 23 ↓ 26 p-value: 0.716
Round 3			↑ **442** ** ↓ 338 p-value: 0.001	↑ **141** ** ↓ 88 p-value: 0.001	↑ 30 ↓ 27 p-value: 0.396
Round 4				↑ **239** ** ↓ 114 p-value: 0.001	↑ 38 ↓ 34 p-value: 0.362
Round 5					↑ 60 ↓ 51 p-value: 0.224

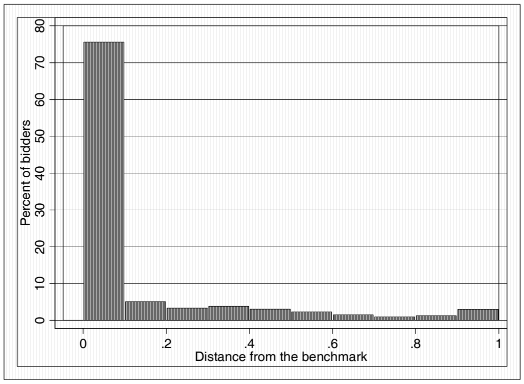
↑ denotes learning, ↓ no learning observed (opposite). Below we list p-value of the sign test.
* and ** denotes 5% and 1% significance level, respectively.

Total sample size: 5000 individuals.

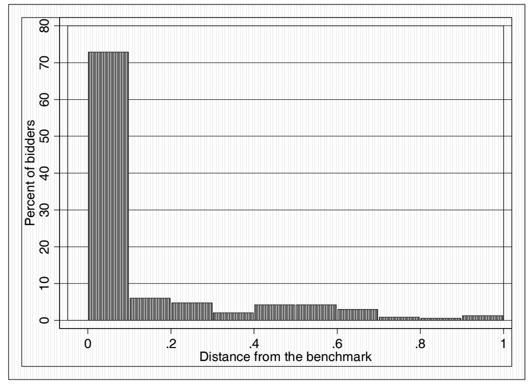
Note: Rows *i* and column *j* in this table correspond to rounds *i* and *j* in which sub-populations of our sample placed their bids. The numbers presented in this (*i, j*) intersection correspond to the sign test of the hypothesis that there is no learning effect found between corresponding round *i* and *j*. For example, the first cell contains the following symbols and numbers ↑ **257** **, ↓ 80, p-value: 0.001. This means that there were 337 (=257+80) individuals who successfully placed their bids in rounds 1 and 2; for 257 individuals the distance between the benchmark (the market portfolio) decreases while for 80 individuals the distance from the benchmark increases; p-value of the underlying sign test was lower than 0.001, bold face letters highlight that there was significant increase in relative performance between rounds 1 and 2, and finally (**) marks the level of significance (1%).

Figure 1. Distribution of bidders by their distance from the benchmark (market portfolio) in each round.

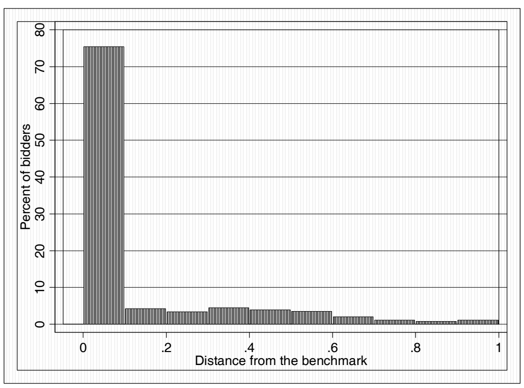
Round 1



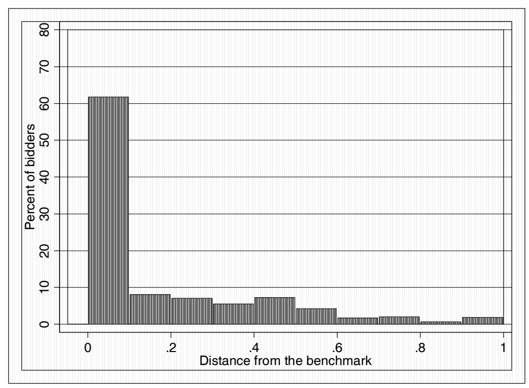
Round 2



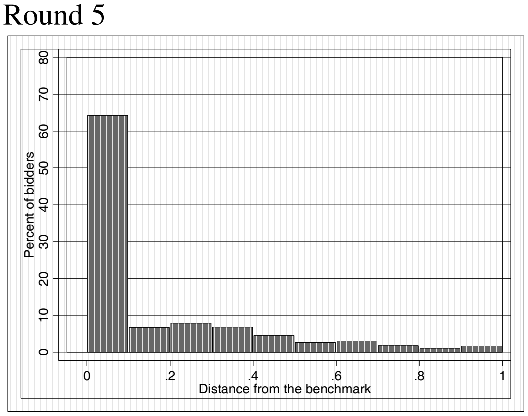
Round 3



Round 4



Round 5



Round 6

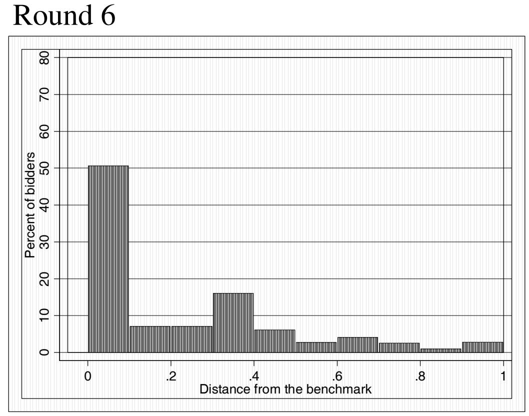


Figure 1 illustrates the magnitude of the learning process: distributions of bidders by their distances from the market portfolio benchmark are presented for each bidding stage. The highest bar in each panel indicates that majority of bidders was able to bid very close to the benchmark; this proportion decreased with successive bidding stages. Distributions across all bidding stages are stable and document that there were no dramatic changes in performance of individual bidders.