The Effects of Experience on Investor Behavior: Evidence from India's IPO Lotteries*

Santosh Anagol† Vimal Balasubramaniam‡ Tarun Ramadorai§

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Abstract

We exploit the randomized allocation of stocks in 54 Indian IPO lotteries to 1.5 million investors between 2007 and 2012 to provide new estimates of the causal effect of investment experiences on future investment behavior. We find that investors experiencing exogenous gains in IPO stocks (the treatment) are more likely to apply for future IPOs, increase trading in their portfolios, exhibit a stronger disposition effect, and tilt their portfolios towards the sector of the treatment IPO. Treatment effects vary with the characteristics of the treatment (size, variability, and salience of the gain), and are stronger for smaller and younger accounts. Treatment effects persist for larger and older accounts, suggesting that experiencing gains exerts a powerful force even on sophisticated players.

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†Anagol: Wharton School of Business, Business Economics and Public Policy Department, University of Pennsylvania, and Oxford-Man Institute of Quantitative Finance. Email: anagol@wharton.upenn.edu

‡Balasubramaniam: Saïd Business School, Oxford-Man Institute of Quantitative Finance, University of Oxford, Park End Street, Oxford OX1 1HP, UK. Email: vimal.balasubramaniam@sbs.ox.ac.uk

§Ramadorai: Saïd Business School, Oxford-Man Institute of Quantitative Finance, University of Oxford, Park End Street, Oxford OX1 1HP, UK and CEPR. Email: tarun.ramadorai@sbs.ox.ac.uk

1 Introduction

Workhorse economic models typically assume that agents have stable preferences and well-founded beliefs. In these models, preferences are "deep" parameters that are not influenced by states of the world, and beliefs are defined using all past data and updated according to Bayes rule. More recent work in economics, however, takes the view that the preferences and beliefs of individuals are more malleable. One interesting approach in this vein has been to model agents' preferences and beliefs as being particularly influenced by their own personal experiences (see, for example Kőszegi and Rabin, 2007; Rabin and Weizsäcker, 2009; Camerer and Ho, 1999; Roth and Erev, 1995; Ellison and Fudenberg, 1993).

Empirical validation for the effects of personal experience on economic decision-making has been growing, especially in the area of investments, where data on agents' choices involving risk is readily available. One strand of this emerging empirical literature relates personal economic experiences to long run risk-taking in financial markets, finding that investors living through periods of low stock returns, inflation, and unemployment suffer declines in stock market participation and allocations to risky assets even decades after the experience (Malmendier and Nagel, 2011, 2009; Knüpfer et al., 2014). A second strand studies how recent portfolio experiences shape short-term decisions, finding that savings and stock trading decisions appear to respond to investors' personal experiences.[1]

A fundamental challenge confronting empirical work on experience effects on short-term financial decision-making is the fact that most investment experiences are determined endogenously by the investor. For example, if we observe investors who have recently experienced gains and exhibit subsequent changes in investment behavior, we might be tempted to conclude that these return experiences have changed these investors' risk preferences. However,

[1]A large literature relates investors' experienced asset returns to future investment behavior. One strand focuses on how prior gains and losses affect risk-taking, see Thaler and Johnson (1990) for one of the first analyses of this nature, and Gamble and Johnson (2014). Another strand looks at specific types of investor experiences in certain asset classes, see Andersen et al. (2014) for a recent review. For recent work using Indian data in this context, see Campbell et al. (2014).

it is entirely possible in this scenario that the initially experienced gains were themselves a result of an increase in risk-taking by the investor, caused by an unobservable change in the investor's risk preferences, or of a change in the investor's skill, an attribute which is notoriously difficult to measure. Another challenge confronts work on experience effects on longer-run risk-taking, namely that it is unobservable whether specific investors were interested in economic matters, or physically invested in the stock market or other risky assets during important historical episodes. In this sense, averaging outcomes across all investors alive during a particular historical event produces a potentially downward-biased measure of experience treatment effects.

Empirical work in this area has been careful to control for various investor and time characteristics in an attempt to isolate the experience-behavior relationship. However, it is ultimately impossible to test whether unobservable confounding factors have been suitably controlled for. The ideal research design in this case would be to find a setting in which investment experiences are randomly assigned to investors, and to then track how this random assignment of experience affects future behavior.[2]

This paper introduces a new research design for estimating the causal relationship between investor experiences and future behavior. We exploit the fact that (owing to excess demand) shares in initial public offerings (IPOs) are often allocated to retail investors using randomized lotteries. By comparing allocated versus non-allocated applicants, we can identify the causal effect of how the experience of IPO initial returns (which are often high, and vary substantially across IPOs) changes future investment behavior.

We apply this research design to India, where we have data from 54 different IPOs in which 1.5 million investor accounts experienced randomized allocation in lotteries between

[2]An assumption underlying many of the specifications estimated in the literature is that variations in expected returns due to risk-taking or skill are likely to be swamped by variations in unexpected returns caused by luck. However it is worth noting that this is simply an assumption, which can be tested if econometricians have access to truly random variation in investment gains and losses and are able to track outcomes in response to these random gains and losses.

2007 and 2012. For all 469,288 treatment and 1,093,422 control accounts, we are able to track the details of investment in their equity portfolios on a monthly basis both prior to and following treatment. Given the large number of IPO experiments that we observe, we also have substantial power to test how different types of return experiences affect investors. Moreover, we are able to estimate heterogenous treatment effects, i.e., estimate how investors' responses to experience vary with investor characteristics such as the size of the pre-existing portfolio of the investor and the "age in the market" of the investor. To our knowledge, this is the first paper to estimate the causal effect of return experiences using the randomized allotment of real securities to particular investors.[3]

We begin our analysis by testing whether investors that are randomly allotted shares are more likely to apply for future IPOs. Our results confirm previous non-experimental results that personal experience in the IPO market appears to lead to reinforcement learning (see Kaustia and Knüpfer, 2008; Chiang et al., 2011). We find that conditional on applying for an IPO, investors who "win the IPO lottery," i.e., those that are randomly allocated IPO shares with positive returns, are significantly more likely to apply for future IPOs, and investors randomly allocated IPOs with negative returns are significantly less likely to apply for future IPOs.

We next test whether investors' randomized IPO return experiences cause substantially different trading decisions in their *non-IPO* portfolios. As we explain more fully below, we believe this analysis best exploits our experimental design. We find that the exogenous shock of receiving a gain in an IPO security strongly increases treated investors' propensity to trade stocks, exacerbates the disposition effect, and causes small but precisely estimated increases in the fraction of the investor's portfolio that is invested in the industry sector of

[3]While our specific data and analysis focus on India, we also note that this research design could be applied to many countries that use lottery systems to allocate IPO shares, including Bangladesh, Brazil, China, Germany, Hong Kong, Singapore, Sweden, and Taiwan. In addition, several brokerages, such as TD Ameritrade and E-Trade in the United States, allocate shares to individual investors using random assignment; our methodology could also be applied to data from such individual brokerages.

the treatment IPO stock. We also find that there is a small but significant increase in the total number of stocks held by investors experiencing IPO lottery wins.[4]

Treatment effects are not homogenous across investors or experiences. Treatment effects inherit the sign of the first-day IPO return, meaning that they are generally negative for negative treatment returns. If our results were simply explained by investors rebalancing towards optimal portfolios, losses experienced on IPOs with initial negative returns should also increase trading volume as simple predictions on rebalancing are symmetric across loss and gain domains. We find, instead, that negative treatment returns lead to statistically insignificant, though negatively signed changes in investor trading volume. Moreover, our results on disposition and familiarity are inconsistent with such optimizing behavior.

Treatment effects also appear stronger for more extreme positive treatments, i.e., IPOs with extremely high returns. Treatment effects are also larger for IPOs with less initial variability in returns, and for IPOs raising a large amount of capital, suggesting that there are important roles for the perception of risk in experiences, as well as for the salience of experiences.

The effects of experience appear to be stronger for smaller accounts, suggesting that more financially sophisticated, larger investors are less susceptible to these effects. However, we find that even for investors with average portfolio sizes in excess of US$ 10,000, small gains in IPO lotteries (averaging roughly US$ 70) continue to produce economically and statistically significant effects on a range of outcome variables. The fact that the treatment effects do not disappear for large accounts suggests that our results are unlikely to be driven by wealth effects. They also suggests that these experience effects exert a powerful influence on investor behavior even on sophisticated investors.[5]

[4]All these results are estimated removing the direct allocation of the IPO stock that treatment accounts have because they were “winners” of the lottery.

[5]Wealth is generally considered to be highly correlated with sophistication in work on household finance (see, for example, Campbell, 2006).

Finally, we find that treatment effects remain significant for investors of all account ages – they are substantially greater for rookies in the market, but continue to exist for those with substantially longer exposure to the stock market. These patterns are interesting in light of prior work on the relationship between experience and market anomalies (as in List, 2003, 2004).

Overall, we view our results as important for the development of both behavioral and rational theories of investor behavior. Many previous behavioral models assume that investors narrowly frame stocks separately when evaluating performance, and in this sense ignore the potential for cross-security effects within investor portfolios (see, for example, Barberis et al., 2006). For example, current models of "realization utility," the idea that investors receive utility jolts at the time of selling an investment, generally assume that utility is defined at the asset level and generally ignore the possibility that there may be cross-asset realization utility effects (see Barberis and Xiong, 2012; Frydman et al., 2014). However, it seems plausible that realizing a gain in one stock might make an investor more willing to realize a loss in another because utility jolts are bracketed together. Our findings suggest that experiences arising from one stock in a portfolio has a *causal* effect on decisions regarding other securities, or put differently, we find that there can be contagion effects even *within* an investor's portfolio.

On the other hand, it is difficult to square our results with fully rational theories of economic decision-making, as it is difficult to explain them using mechanisms such as wealth effects or rational portfolio rebalancing. Much like the related literature in this field that uses non-experimental variation for identification, we find strong evidence consistent with reinforcement learning behavior by investors in financial markets.

Finally, our results are also related to the recent literature (see, for example, Parker et al., 2011; Agarwal et al., 2007; Bertrand and Morse, 2013) which uses micro-data to study the consumption response to unanticipated income shocks. Most of these studies harness the power of experimental or quasi-experimental variation to reject the predictions of the rational expectations life-cycle/permanent income hypothesis. Our results are similar in approach, and complement this literature by showing that there are good reasons to believe that shifts in beliefs and preferences caused by exogenous variation in gains and losses have effects on investment (and not just consumption) behavior.

The next section describes the natural experiment that we study, describing the details of the Indian IPO lottery process. Section (3) describes the data that we employ, Section (4) describes how we estimate treatment effects on a range of investment behaviors using these lotteries, Section (5) describes the results, Section (6) explores the heterogeneity of our estimated treatment effects, and finally, Section (7) concludes.

2 The Experiment: India's IPO Lotteries

2.1 Details of Regulation and the IPO Process

As with many other details of regulation in the country, the Indian regulatory process for IPOs is quite complex. Several papers (e.g., Anagol and Kim, 2012; Campbell et al., 2015) have used this complexity of the Indian regulatory process to cleanly identify a range of economic phenomena.

Our experiment uses the Indian retail investor IPO lottery as an identification mechanism. This lottery arises in situations in which an IPO is oversubscribed, and the use of a proportional allocation rule to allocate shares would violate the minimum lot size set by the firm. In such cases, the lottery is run to give investors their proportional allocation *in expectation*. The outcome of the lottery is that some investors receive the minimum lot size (this is the treatment group) and others receive zero shares (the control group).

The fundamental reason for the lottery is that in India, regulations require that a firm must set aside 30% or 35% of its shares (depending on the type of issue) to be available for allocation to retail investors at the time of IPO.[6] For the purposes of the regulation, "retail investors" are defined as those with expressed share demands beneath a pre-set value.[7] At the time of writing, this pre-set value has been set by the regulator at Rs. 200,000 (roughly US $3,400); this value has varied over time.[8]

The share allocation process in an Indian IPO begins with the lead investment bank, which sets an indicative range of prices. The upper bound of this range (the "ceiling price") cannot be more than 20% higher than the lower bound (or "floor price"). Importantly, a minimum number of shares (the "minimum lot size") that can be purchased at IPO is also determined at this time. All IPO bids (and ultimately, share allocations) are constrained to be integer multiples of this minimum lot size.

Retail investors can submit two types of bids for IPO shares. The simplest type of bid is a "cutoff" bid, where the retail investor commits to purchasing a stated multiple of the minimum lot size at the final issue price that the firm chooses within the price band. To submit a cutoff bid, the retail investor must deposit an amount into an escrow account, which is equal to the ceiling of the price band multiplied by the desired number of shares. If the investor is allotted shares, and the final issue price is less than the ceiling price, the difference between the deposited and required amounts is refunded to the investor. In our sample 93 % of IPO applicants elect to submit cut-off bids.

[6]We provide more details on these regulations in the online appendix.

[7]In practice, each brokerage account is counted as an individual retail investor for the purposes of the regulation, meaning that a single investor could in practice exceed this threshold by subscribing using multiple different brokerage accounts. However, this is not a concern for us as we can identify any such behavior in our data. This is because our data are aggregated across all brokerage accounts associated with the anonymized tax identification number of the investor.

[8]The Indian regulator, SEBI, introduced the definition of a retail investor on August 14, 2003 and capped the amount that retail investors could invest at Rs. 50,000 per brokerage account per IPO. This limit was increased to Rs. 100,000 on March 29, 2005, and once again increased to Rs. 200,000 on November 12, 2010. This regulatory definition technically permits institutions to be classified as retail when investing amounts smaller than the limit, but over our sample period, we verify using independent account classifications from the depositories that this hardly ever occurs, and accounts for a miniscule proportion of retail investment in IPOs. We simply remove these aberrations from our analysis.

Alternatively, retail investors have the option to submit a "full demand schedule," i.e., the number of lots that they would like to purchase at each possible price within the indicative range. As in the case of the cutoff bid, the investor once again deposits the maximum monetary amount consistent with their demand schedule at the time of submitting their bid. If allotted shares, the investor's order will be filled at the stated share demand associated with the final issue price, and a refund is processed for the difference between the final price and the amount placed in escrow. 7 % of our sample submits full demand schedules.

Once all bids have been submitted, the firm and investors jointly determine the level of retail (and total) investor oversubscription. The two inputs to this are total retail demand, and the firm's total supply of shares to retail investors, including any excess supply from other categories (for example, if employees and/or non-institutional investors participate in amounts less than they are offered, this can "overflow" into additional retail supply).[9]

We define "retail oversubscription" as the ratio of total retail demand for a firm's shares to total supply of shares by the firm to retail investors, i.e., the total number of shares made available by the firm for retail investors to purchase.

There are then three possible cases:

1. Retail oversubscription is less than or equal to one. In this case, all retail investors are allotted shares according to their demand schedules.

2. Retail oversubscription is greater than one, and shares can be allocated to investors *in proportion to their stated demands without any violation of the minimum lot size constraint*. There is no lottery involved in this case.

[9]Of course, total firm supply is restricted by the overall number of shares that the firm decides to issue, which is fixed prior to the commencement of the application process for the IPO.

3. Retail oversubscription is *far* greater than one (the issue is substantially oversubscribed), and a number of investors under a proportional allocation scheme would receive an allocation which is lower than the minimum lot size. This constraint cannot be violated by law, and therefore, all such investors are entered into a lottery. In this lottery, the probability of receiving the minimum lot size is proportional to the number of shares in the original bid.

This third case, in which the lottery takes place, constitutes our experiment. Far from being an unusual occurrence, in our sample alone (which does not even cover all IPOs in the Indian market over the sample period), roughly 1.5 million Indian investors participate in such lotteries over the 2007 to 2012 period in the set of 54 IPOs that we study.

Note that the minimum allocation (minimum lot size times issue price), along with the listing return, i.e., the difference between the price at listing and the issue price, together determine the experimental stake. The minimum allocation of shares is the base on which gains and losses for the treatment group are accrued, relative to the control group.

We now provide a more formal description of the process, and illustrate it with a specific example from an Indian IPO.

2.2 The Probability of Treatment

Let S be the total supply of shares that the firm decides to allocate to retail investors. Let $c = 1, ..., C$ index "share categories," which are integer multiples of the minimum lot size x for which investors can bid. The set of possible numbers of shares for which investors can bid is therefore: x,$2x$,...,Cx.[10] Let a_c be the total number of applications received for share category c. The total demand D for an IPO with C share categories is then:

[10] Note that the minimum lot size is also the mandatory lot size increment.

$$D = \sum_{c=1}^{C} cxa_c. \tag{1}$$

Retail oversubscription v is then defined as:

$$v = \frac{D}{S}. \tag{2}$$

As described in case (1) above, if $v \leqslant 1$ at the ceiling price, then all investors get the shares for which they applied, and if $v > 1$, one of cases (2) or (3) will apply.

In the latter two cases, the first step is to compute the allocations for each share category under a proportional allocation rule, and compare these allocations to the minimum lot size x.

Let $J \leq C$ be the share category such that share categories $c \in [J, ..., C]$ receive proportional allocations which are greater than or equal to x, and share categories $c' \in [1, ..., J)$ receive proportional allocations which are less than x. If $J = 1$ then we are in case (2), otherwise we are in case (3).

In either case, investors in share categories $c \geqslant J$ receive a proportional allotment $\frac{cx}{v}$, and a total number of shares equalling $\sum_{c=J}^{C} \frac{cx}{v} a_c$. However, investors in share categories $c' \in [1, ..., J)$ cannot receive the minimum of x shares (since J is the cutoff share category, i.e., $\frac{(J-1)x}{v} < x$). Let Z be the remainder of shares to be allotted, i.e.,[11]

$$Z = S - \sum_{c=J}^{C} \lfloor \frac{c}{v} x a_c \rceil. \tag{3}$$

These are the shares allocated by lottery in case (3). Note that in this lottery, the possible outcomes are winning the minimum lot size x with probability p_c, or winning nothing with probability $1 - p_c$.

[11]By regulation, the shares to be allotted $\sum_{c=J}^{C} \frac{c}{v} x a_c$ is rounded to the nearest integer.

By regulation, the probability of winning in share categories $c' \in [1, ..., J)$ must be exactly proportional to the number of shares applied for, meaning that in expectation, investors will receive their proportional allocation. That is, for share categories $c' \in [1, ..., J)$:

$$\frac{p_{c'}}{p_{c'-1}} = \frac{c'x}{(c'-1)x} = \frac{c'}{c'-1}. \tag{4}$$

The combination of equation (4) and the fact that the total remaining shares are described by equation (3) gives us:

$$\sum_{c'=1}^{J-1}(p_{c'})xa_{c'} + \sum_{c'=1}^{J-1}(1-p_{c'}) \times 0 = Z. \tag{5}$$

Solving (5), we get that $p_{c'} = \frac{c'}{v}$ of winning exactly x shares in share categories $c' \in [1, ..., J)$. We show the solution in an appendix to the paper.

In general, the probability of winning increases proportionally with the number of share lots bid for c, and decreases with the overall level of over-subscription v. This implies that the probability of winning will vary across share categories within IPOs, as well as across IPOs. In other words, there may be some self-selection of investors into share categories – that is, by applying for more share lots, they increase the probability of winning. However, conditional on two investors applying for the *same* share category in the same IPO, the investor chosen to actually receive the shares will be random. In other words, the relevant control group is the set of investors *within* the same share category who were unsuccessful in the lottery. As we explain more fully below, it is precisely this within-share-category experimental variation that we exploit in estimating the effects of winning (or losing) the IPO lottery on subsequent portfolio decisions.

2.3 An Example: Barak Valley Cements IPO Allocation Process

We now provide an example to illustrate this process. Barak Valley Cements' IPO opened for subscription on October 29, 2007, and remained open for subscription through November

1, 2007. The stock was simultaneously listed on the National Stock Exchange (NSE) and the Bombay Stock Exchange (BSE) on November 23, 2007. The listing price of the stock was Rs. 42 per share, and the stock closed on the first day of listing at Rs. 56.05 per share, for a 33.45% listing day gain. The retail oversubscription rate v for this issue was 37.62. Given this high v, all investors that applied for this IPO were entered into a lottery, i.e., $J = C$.

Table 1 shows the official retail investor IPO allocation data for Barak Valley Cements.[12] Each row of column (0) of the table shows the share category c, associated with a number of shares bid for given in column (1), which, given the minimum lot size $x = 150$ for this offer is just cx. In this case, $C = 15$, meaning that the maximum retail bid is for 2,250 shares. This is because $C = 16$ would give a number of 2,400 shares, and a maximum subscription amount of Rs. 100,800 at the listing price of Rs. 42. This maximum subscription amount would violate the prevailing (in 2007) regulatory maximum retail investor application constraint of Rs. 100,000 rupees per IPO. Column (2) of the table shows the total number of retail investor applications received for each share category, and column (3) is simply the product of columns (1) and (2).

Column (4) shows the investor allocation under a proportional allocation rule, i.e., $\frac{cx}{v}$. As $v = 37.62$, this proportional allocation is less than the firm's minimum lot size of 150 shares per investor for all share categories, i.e., $J = C$. By regulation, the firm is now required to conduct a lottery to decide share allocations.

Column (5) shows the probability of winning the lottery for each share category c, which is $p = \frac{c}{v}$. For example, 2.7% of investors that applied for the minimum lot size of 150 shares will receive this allocation (this is the treatment group in this share category), and the remaining 97.3% of investors applying in this share category (the control group) will receive no shares. In contrast, 40.6% of investors in share category $c = 15$ receive the minimum lot

[12] These data are obtained from `http://www.chittorgarh.com/ipo/ipo_boa.asp?a=134`

size $x = 150$ shares. For this particular IPO, *all* retail investors are entered into a lottery, and will ultimately receive either zero or 150 shares of the IPO.

Column (6) shows the total number of shares ultimately allotted to investors in each share category, which is the product of x, column (2), and column (5). Columns (7) and (8) show the total sizes of the treatment and control groups (number of retail investors) in each share category for the Barak Valley Cements IPO lottery. Across all share categories, 12,953 investors are treated, and 55,669 are in the control group.[13]

As described briefly earlier, it is perhaps easiest to think of our data as comprising a large number of experiments, in which each experiment is a share category within an IPO. *Within* each experiment the probability of treatment is the same for all applicants, and we exploit this source of randomness, combining all of these experiments together to estimate the causal effect of experiencing the IPO listing return on future investment behavior. We explain this more fully in the methodology section, following the data description below.

3 Data

To understand the causal effects of experience on investment behavior in this setting, we require two major sources of data. First, we need data on the full set of investors who applied for each IPO, i.e., both successful and unsuccessful applicants. These data are used to define our treatment and control groups. Second, we require investor-level data on portfolio allocations and trades to measure how investing behavior changes in response to the treatment, i.e., the experience in the IPO lottery.

[13]By regulation the firm allocates shares to investors rounded upwards to the nearest integer and will appropriately increase the total number of shares to accommodate the rounding off additions.

3.1 Data on IPO Applications

When an individual investor applies to receive shares in an Indian IPO their application is routed through a registrar. In the event of heavy oversubscription leading to a randomized allotment of shares, the registrar will, in consultation with one of the stock exchanges, perform the randomization to determine which investors are allocated. We obtain data on the full set of applicants to 54 Indian IPOs over the period from 2007 to 2012 from one of India's largest share registrars. This registrar handled the largest number of IPOs by any one firm in India since 2006, covering roughly a quarter of all IPOs between 2002 and 2012, and roughly a third of all IPOs over our sample period.

For each IPO in our sample, we observe whether or not the applicant was allocated shares, the share category c in which they applied, the geographic location of the applicant by pin-code,[14] the type of bid placed by the applicant (cutoff bid or full demand schedule), the share depository in which the applicant has an account (more on this below), whether the applicant was an employee of the firm, and other application characteristics such as whether the application was supported by a blocked amount at a bank.[15]

[14]PIN codes in India are postal codes managed and administered by the Indian Postal Service department of the Government of India. They are similar to postcodes in the UK, although cover a larger region in India.

[15]An application supported by blocked amount (ASBA) investor is one who has agreed to block the application money in a bank account which will be refunded should she not be allocated the shares in an IPO. The alternative is paying by cheque, i.e., in either case, the money is placed in escrow prior to the allotment process, but in the case of ASBA, any refunds are processed a few days faster.

3.2 Data on IPO Applicants' Equity Portfolios

Our second major data source allows us to characterize the equity investing behavior of these IPO applicants. We obtain these data from a broader sample of information on investor equity portfolios from Central Depository Services Limited (CDSL). Alongside the other major depository, National Securities Depositories Limited (NSDL), CDSL facilitates the regulatory requirement that settlement of all listed shares traded in the stock market must occur in electronic form. CDSL has a significant market share – in terms of total assets tracked, roughly 20%, and in terms of the number of accounts, roughly 40%, with the remainder in NSDL. While we do also have access to the NSDL data (these data are used extensively and carefully described in Campbell et al., 2014), we are only able to link the CDSL data with the IPO allocation information, as we describe below.

The sensitive nature of these data mean that there are certain limitations on the demographic information provided to us. While we are able to identify monthly stock holdings and transactions records at the account level in all equity securities in CDSL, we have sparse demographic information on the account holders. The information we do have includes the pincode in which the investor is located, and the type of investor. We use investor type to classify accounts as beneficial owners, domestic financial institutions, domestic non-financial institutions, foreign institutions, foreign nationals, government, and retail accounts. This paper studies only the category of retail accounts, as the IPO lottery only applies to this group of investors.

As described in Campbell et al. (2014), the share of direct household equity ownership in India in total equity investment is very large (roughly 80%-95%), relative to the share of indirect equity holdings using mutual funds, unit trusts, and unit-linked insurance plans. This means that we observe roughly the entire equity portfolio of the household in our analysis, allowing us to interpret the treatment effects of experience that we estimate as effects on household equity portfolio choice. This distinguishes our study of investment behavior from those attempting to detect effects of experienced returns on trading behavior, such as Seru et al. (2010) and Strahilevitz et al. (2011).

3.3 Constructing the Final Sample

In order to match the application data to the CDSL data on household equity portfolio choice, we obtain a mapping table between the anonymous identification numbers of household accounts from both data sources. We verify the accuracy of the match by checking common geographic information fields provided by both data providers such as state and pincode.

Every applicant for an IPO must register (or already have) an account with either of the two depositories (CDSL and NSDL), as the option to receive allocated shares in an IPO in physical form does not exist. For all applicants with accounts in CDSL, we observe accounts that applied for an IPO and were allotted in the lottery, i.e., the treatment group, as well as those that applied, but due to randomized allocation did not get allocated any share in an IPO. The latter group is the universe of counterfactuals in the IPO randomized lottery, i.e., the control group.

Since our data additionally permit us to observe all allocations made to investors in IPOs *after* the selection process managed by share registry firms in CDSL data, we observe allotments (but not applications) to particular household accounts, which we use in some of our analysis below.

All CDSL trading accounts are associated with a tax related permanent account number (PAN), and regulation requires that an investor with a given PAN number can only apply once for any given IPO.[16] Consistent with this, we observe that there are no two trading accounts in any single IPO that are associated with the same (anonymized) PAN number. Thus no investor account may simultaneously belong to both the control and treatment group, or be allocated twice in the same IPO. However, it is possible that a household with

[16] In July 2007 it became mandatory that all applicants provide their PAN information in IPO applications. SEBI circular No.MRD/DoP/Cir-05/2007 came into force on April 27, 2007. Accessed at http://goo.gl/OB61M2 on 19 Sep 2014.

multiple members with different PAN numbers could submit multiple applications for a given IPO in an attempt to increase the household's likelihood of treatment. While we do not have a direct way to control for this possibility, given our sample size, we do not believe that this is likely to affect our inferences materially.

3.4 Summary Statistics

Between March 2007 and March 2012, the common sample period for our total dataset, we observe 85 IPOs (of a total of roughly 240). Figure 1 shows the coverage of IPOs in our sample relative to that in the total universe of IPOs. Our sample coverage closely tracks aggregate IPO waves, with a severe decline in 2009, and high numbers of IPOs in 2008 and 2010. In our sample of 85 IPOs, 54 IPOs have at least one share category with a randomized lottery allocation, compared to the universe of 176 IPOs with randomized allocations over the period.[17]

Table 2 presents summary statistics on the 54 IPOs with randomized allotments in our sample. The preponderance of IPOs in our sample, 31, are in the manufacturing sector, with 17 in the service sector, 4 in the technology sector, and 2 retail sector IPOs. The table shows that these IPOs account for 22% of all IPOs over this period by number, and US$ 2.65 BN or roughly 8% of total IPO value over the period, varying from a low of 0.72% of total IPO capital in 2009 to a high of roughly 25% in 2011.

Between 32% and 35% of shares in these IPOs are allocated to retail investors who are not employees of the IPO firm.[18] The average IPO in our sample is 12 times oversubscribed, leading to an average of 8,691 treatment accounts and 20,248 control accounts per IPO, for a total of 1,562,706 accounts in our experiment. We observe a total of 383 randomized

[17] We only consider IPOs that both undertake a randomized allocation and are mentioned in public sources such as www.chittorgarh.com in our analysis.

[18] This is slightly below the mandatory 35% allocation to retail investors because we do not include employees in this calculation as employees are not randomly assigned shares. For further details, refer to the online appendix.

share categories (or experiments) across 54 IPOs, of which 323 randomized share categories experienced positive first-day listing gains in the stock market. We naturally expect different results based on whether IPO delivered a positive or negative experience. As a result, in the majority of our analysis, we focus on IPOs with positive first day returns as our main sample. We also discuss results obtained using the 60 share categories from the 14 IPOs with non-positive first-day returns in the results, but in a separate table from the remainder of the analysis.

Figure 2 plots the mean and distribution of first-day returns for our 54 IPOs across the five years of our sample. The figure shows that our sample contains significant dispersion in experiences, with IPOs generating both high negative ($< -50\%$) and high positive returns ($> 150\%$) and a range in-between. The second panel shows the first day variability of the IPO stocks in our sample, measured by the first day high price minus the first day low price divided by the issue price. Our IPO stocks also show large dispersion in first day return volatility, with intra-day dispersion of 50% not uncommon. We explore how cross-IPO variation in first-day returns and first-day return variability affects outcomes in our analysis.

Table 3 characterizes the treatment experience the investors in our analysis received upon being randomly chosen to receive IPO shares. Column (1) of the table shows the mean across all investors in the treatment groups or IPOs in our 323 share category experiments (for the positive first-day return IPOs) for each of the variables listed in the row headers.[19] Columns (2) through (6) present the percentile of each variable in terms of the distribution across all of the experiments.[20]

On average, applicants put approximately US$1,803 in escrow to apply for the IPOs in our sample, although this amount varies substantially from 163 to 2,174 dollars based on

[19]The weighting across the different share categories is done in exactly the same way as the regression framework we use weights the individual treatment and control groups. See Section 4 for details.

[20]We first calculate the mean within each experiment, and then report the corresponding percentile across the experiments. For example, the median share category experiment had a mean application amount of 847 dollars (first row of Table 3).

the number of shares applied for and the issue price of the IPO. The mean probability of treatment is 35%, and this also varies substantially across experiments – as discussed earlier, this is because probability of treatment is proportional to the number of shares applied for by investors.

The mean value of the share allotment from the lottery is 150 dollars. This is very similar across all of our experiments – recall that all treatment applicants in a randomized share category receive the same number of shares, the minimum lot size, regardless of how many shares were applied for. This implies that within an IPO the value of allotment is always the same across share categories; the value of allotments across IPOs also tend to be similar as there tend to be similar numbers of share categories in total and the maximum investment amount is typically 100,000 rupees.

We measure the gain to the treatment group as the difference between the IPO issue price and the closing price of the IPO in the market at the end of the first day's trading. This is tantamount to an assumption that the control group can access the IPO shares only at the beginning of the first day (note that the control group is refunded the money placed in escrow roughly two trading weeks following the allocation), but we note that the exact measurement of this gain does not affect our inferences about outcomes except in terms of calculations about the magnitude of the stimulus. Using this definition of the first day gain, the mean treatment across IPOs with positive first-day returns is a 43% gain relative to the IPO issue price, which translates into a US$ 67 gain at the end of the first day (ranging from US$ 9 at the 10th percentile to US$ 142 at the 90th percentile).

Despite the average percentage gain on the IPO being large, the absolute dollar gains are quite small relative to the application amounts required – this is again because the treatment group only gets allotted the minimum lot size in the case they win the lottery. These experimental gains are similar in size to the US$ 300 tax stimulus payments studied in Parker et al. (2011) and are also relatively small compared to the cross-sectional mean of the time-series median portfolio value of US$ 1,866.

In general the size of these experimental stakes have two effects. First, it is difficult to interpret any results we find as arising from wealth effects or portfolio rebalancing given the low fraction of total invested equity portfolio wealth that these experimental gains represent. Second, and more generally, the smaller the experimental stakes, the greater the bias against finding any strong results from winning the IPO lottery.

4 Methodology

4.1 Estimating Treatment Effects

We can view each randomized share category in each IPO as a separate experiment with a different probability of being allotted shares. The idea of our empirical specification is to pool all of these experiments in order to maximize statistical power, while ensuring that we exploit only the randomized variation of treatment status within each IPO share category. Our strategy is similar to that employed in Black et al. (2003), who estimate the impact of a worker training program that was randomly assigned within 286 different groups of applicants.

Intuitively, this approach proceeds by stacking the different applicants from all of the experiments together into a single dataset, and then including a fixed effect for each experiment. These experiment-level fixed effects ensure that our identification of the treatment effect stems solely from the random variation in treatment within each experiment.[21]

In particular, we estimate the causal effect of the experience of winning an IPO lottery on an outcome variable by estimating the cross-sectional regression in each (event) month t:

$$y_{i,j,c,t} = \alpha + \rho_t I_{\{success_{i,j,c}=1\}} + \gamma_{j,c} + \beta X_{i,j,t} + \epsilon_{i,j,c,t}. \tag{6}$$

[21] See Chapter 3 of Angrist and Pischke (2008) for a discussion of how regression with fixed effects for each experimental group identifies the parameter of interest using only the experimental variation.

Here, $y_{i,j,c,t}$ is an outcome variable of interest (for instance, the number of times the individual i applies for subsequent IPOs) for applicant i in IPO j, share category c, at event month t (we measure time in relation to the month of the lottery). $I_{\{success_{i,j,c}=1\}}$ is an indicator variable that takes the value of 1 if the applicant was successful in the lottery for IPO j in category c (investor is in the treatment group), and 0 otherwise (investor is in the control group). ρ_t are the estimated treatment effects in each event-month t. As we discuss more fully below, we estimate all treatment effects for $t \in [-6, ..0, .. + 6]$ where $t = 0$ is the month in which the lottery takes place, with leads and lags of up to 6 months. $X_{i,j,t}$ are account-level control variables – in our empirical implementation these include dummies for whether the investor bid using the cutoff or full demand schedule mechanisms, and whether the investor funded the application using ASBA or cheque payment.

$\gamma_{j,c}$ are fixed effects associated with each experiment, i.e., each IPO share category in our sample. Angrist et al. (2013) refers to these experiment-level fixed effects as "risk group" fixed effects. Conditional on the inclusion of these fixed effects, variation in treatment is random, meaning that the inclusion of controls should have no effect on our point estimates of ρ_t. Nevertheless, we include these controls to soak up additional variation in the dependent variable to increase the statistical precision of our estimates. Specification (6) identifies ρ_t as the causal impact of the experience of winning the IPO lottery on the outcome variable $y_{i,j,c,t}$.

Angrist (1998) shows that our estimated treatment effect ρ_t is a weighted average of the treatment effects from each separate share category experiment. In particular, the weights are constructed as:

$$w_c = \frac{r_c(1 - r_c)N_c}{\sum_{k=1}^{323} r_k(1 - r_k)N_k} \tag{7}$$

where r_c and N_c are the probability of treatment and sample sizes in share category

c, and we have a total 323 share category experiments. Intuitively, the regression weights give more importance to experiments in which the probability of treatment is closer to $\frac{1}{2}$, and experiments with larger sample sizes. The basic idea is that the "good" experiments are ones in which there are many accounts in both treatment and control groups. This weighting scheme implies that our regression estimate only exploits purely random variation in treatment induced by the lotteries, since treatment versus control comparisons are only performed *within* share categories and given the fact that ρ_t is a weighted average of these share-category-specific effects.

We cluster all standard errors by calendar-month, to pick up potential correlations of the error terms $\epsilon_{i,j,c,t}$ across all IPOs occurring in the same month, as well as correlations across share categories within IPOs. As mentioned earlier, we estimate all treatment effects for $t \in [-6, ..0, .. + 6]$ where $t = 0$ is the month in which the lottery takes place, with leads and lags of up to 6 months.

The +1 to +6 window identifies the causal impact of the experience on future outcomes. Estimating equation (6) for time periods for event-times -1 to -6 outcome variable serves as a useful "placebo" test. If the lottery is truly randomized, we should find that receiving treatment at time zero does not, on average, predict outcomes in time periods *before* treatment was actually assigned. This placebo test is particularly useful because many outcomes are highly serially correlated over time, so we would be likely to pick up any selection into treatment (if it exists) by inspecting the behavior of treatment and control groups in the pre-treatment periods. For example, if particular applicants figure out a way to "game" the lottery then we might find that their treatment at time zero actually predicts their behavior in the -1 to -6 window.

Table 4 presents summary statistics and a randomization check comparing our treatment and control groups. Columns (1) and (2) present the means of variables listed in the row headers in treatment and control groups respectively, and Column (3) presents the difference across the two samples with ***,** and * indicating statistically significant differences at the 1%, 5%, and 10% levels.[22] All of these variables are measured in the month prior to the treatment IPO. If the allocation of IPO shares is truly random, we would expect few statistically significant differences across treatment and control groups prior to the assignment of the IPO shares. Column (4) calculates the percent of our 323 share category experiments (note: these are only the IPOs with positive first day returns, as described earlier) in which the treatment and control groups were significantly different at the 10% level. Under the null hypothesis that treatment status is random, we expect that roughly 10% of these experiments will exhibit a significant difference at the 10% level.

Our first outcome variable of interest is whether investors randomly allocated IPO shares are more likely to apply for IPOs in the future. The construction of this outcome variable warrants further explanation. In the case of IPOs for which our data provider was the registrar, we can directly measure whether or not an account *applied* to an IPO in each of periods +1 to +6. For IPOs where our data provider was not the registrar, we can observe whether the account was *allotted* shares since we see allotments for the entire universe of IPOs from the CDSL data. We set the outcome variable to one in either case – if we see an application for IPOs for which our data provider was the registrar, or if we see an allotment for IPOs not covered by our registrar – and zero otherwise.[23] Table 4 shows that virtually identical fractions (38%) of both treatment and control investors applied to an IPO with our registrar, or were allotted shares in an IPO not covered by our registrar, in the month prior to treatment.

The next set of variables describe the trading behavior of our treatment and control samples. We focus on the total amount of trading value, which is calculated as the sum of

[22]These means are calculated using the weights defined in equation (7), which are the same weights that our main estimating equation uses to combine the share category by share category experimental results in to one treatment effect estimate.

[23]For the set of IPOs for which we can observe allotments but not applications, our measure is noisy, because although an account had to apply to receive shares, there are also accounts which applied but did not receive shares. We focus on this combined measure because it includes all of the information available to us, but we note that our results likely under-estimate the full impact of IPO experiences on future IPO application behavior.

the value of stocks bought and sold in a month, and corresponds on average to roughly US$ 203 including zeros. Approximately 29% of accounts made no trades in the month prior to the IPO; this distribution is also U-shaped. In particular, it is striking to note that nearly half of the accounts observed traded more than US$ 1,000 in the month prior to treatment – in general, the investors in the sample trade substantial amounts.

The next two rows of the table show statistics about two important outcome variables. The first one is the percentage of the portfolio which is invested in stocks in the same industry sector as that of the IPO lottery stock.[24] A large literature documents that investors demonstrate a preference for familiarity, i.e., they tend to invest in firms that are located physically close to them, or those that have some relationship with the investor's occupation (see, for example, Coval and Moskowitz (2001)). A simple way for investors to become familiar with a sector is to simply own a stock in that sector. Consistent with this, Huang (2012) finds using data from a large discount broker in the U.S. over the period 1991 - 1996, that individuals are more likely to buy a stock in an industry in which they previously experienced a gain. Our design allows us to test this idea using exogenous variation in sectoral experience that is unlikely to be conflated with other investor or time-varying characteristics.[25] In the month prior to treatment, on average, both treatment and control investors have roughly 6% of their portfolios invested in the same sector as that of the IPO.

The second of these variables is the disposition effect. A large empirical literature (see, for example, Shefrin and Statman, 1985; Odean, 1998; Grinblatt and Keloharju, 2001) documents the disposition effect across a wide variety of contexts. However, there is little empirical work testing how the disposition effect responds to exogenous variation in investor

[24]Sectoral allocation is defined by the Indian National Industrial Classification Code (NIC code) as of 2004 for all sectors of the Indian economy. Using the NIC classification, we use the third-level aggregation to define 42 sectors in the economy. The details of this classification is available at http://mospi.nic.in/Mospi_New/upload/nic_alphabetic_5digit2004.html

[25]An alternative explanation for this familiarity effect is that investors believe they have private information about stocks that they are familiar with (although whether they actually out-perform in those stocks is unclear – see Massa and Simonov (2006); Seasholes and Zhu (2010)).

experiences. We believe that such inferences may be valuable to separate different potential causes for this effect, including loss-averse preferences (see, for example, Barberis and Xiong, 2009), or an irrational belief in mean-reversion (Weber and Camerer, 1998). For example, if the disposition effect is driven by investors' irrational belief in mean reversion, we should see no difference in the disposition effect across our treatment and control investors, because in terms of information sets, these groups should be exactly the same; both chose to apply for the IPO in question, but one was simply lucky to have been allotted. It seems implausible that the experience of receiving an exogenous gain in an IPO would cause an investor to start believing more (or less) in mean reversion.

We define the disposition effect as the percent of paper gains in the portfolio realized during the month minus the percent of paper losses in the portfolio realized during the month. For example, suppose an account had 4 stocks on paper with gains, and 5 stocks on paper with losses at the beginning of the month. Further suppose that the account sold 1 stock of both gains and losses respectively. Then, our disposition effect measure would be 5%, i.e., 25% of gains realized minus 20% of losses realized. We find that this measure averages to roughly 10% for both treatment and control investors in the month prior to the IPO treatment.

The next few rows of Table 4 present summary statistics on the application characteristics of control and treatment investors. 93% of these investors submitted an application with a "cutoff" bid, i.e. they specified their demand for shares regardless of what the final chosen price was, and 4% used ASBA rather than cheque payment to fund the application. The geographic distribution of investors is concentrated in states with major economic activity, in particular Gujarat, Maharashtra, Rajasthan and Delhi.

The next two blocks of rows of Table 4 show statistics about the distribution of investor portfolio values and the "age" of investors, i.e., the amount of time they have spent in the market. In much work in household finance (see, for example, Campbell (2006)), investor wealth is strongly associated with sophistication, suggesting that any treatment effects that we detect should attenuate or even disappear for larger accounts. The amount of time investors spend in the market is equally interesting, in light of important work in this area (see, for example, List, 2003, 2004), which posits that increasing experience of market interactions should cause market participants to behave increasingly rationally in these interactions. If this hypothesis is correct, treatment affects should once again attenuate or even disappear for "aged" accounts relative to "rookies".

The table shows that 78% of treatment and control investors had an account value greater than zero in the month prior to the IPO. Portfolio value amounts are highly skewed so we transform this variable using the inverse hyperbolic sine function[26] – we find that portfolio values, which are on average US$ 530 including zero values, are not significantly different across treatment and control accounts. The next rows show the fractions of treatment and control accounts that fall into the range of portfolio values described in the row headers. The distribution of portfolio values is roughly U-shaped in both treatment and control accounts, with a relatively large number of accounts with zero value (some of these correspond to new market entrants, or "rookies" as we identify below), few accounts with portfolio value between US$ 500 and 1,000, and roughly a quarter of the accounts with portfolio values over US$ 5,000.

In terms of account age at the time of the treatment IPO, approximately 30% of accounts are less than six months old, 30% are between 7 and 25 months old, and 40% are over 25 months old. We later explore how heterogeneity in both portfolio size and account age affects the treatment effects that we estimate.

Overall, we find that the differences across treatment and control groups are small, and importantly statistically insignificant. The fraction of experiments with greater than ten percent significance is around ten percent. Given the similarity of treatment and control

[26] $\sinh^{-1}(z) = \log(z + (z^2 + 1)^{1/2})$. This is a common alternative to the log transformation which has the additional benefit of being defined for the whole real line. The transformation is close to being logarithmic for high values of the z and close to linear for values of z close to zero. See, for example, Burbidge et al. (1988), and Browning et al. (1994).

groups across this wide set of background characteristics, the IPO shares do appear to be randomly assigned to investors.

5 Results

Table 5 presents our main estimates of equation 6 for our outcome variables of interest. Each row delineated by lines (we refer to these as panels) in the table corresponds to a distinct outcome variable, and shows results for a set of applicants for the month $t \in [-6, ..., 0, ..., +6]$ where $t = 0$ is the month of the lottery. The first set of numbers within each panel shows the coefficients ρ_t, which are the estimated treatment effects from the cross-sectional regressions estimated for each event-time t in the window shown in the column header. The second row of numbers in each panel shows standard errors, and the third row of numbers in each panel shows the mean of the outcome variable for the control group, which we use to interpret the magnitudes of the treatment effects.

Across our outcome variables of interest, we find that there is one statistically significant relationship between treatment status in the outcome prior to treatment (event months -1 to -6). However, there is no systematic pattern amongst these coefficients that suggest that the treatment and control groups are systematically different from from one another after including risk-group fixed effects.[27]

[27]Note that by chance some of the pre-period treatment effects are likely to show up as significant, but as seen in Table 4, these are not systematic and not particularly economically meaningful.

5.1 Treatment Effects on Future IPO Subscription

We begin by testing how receiving a randomized allocation of IPO stock affects an investor's propensity to apply for other IPOs in the subsequent six months. This outcome has been studied in previous work, (see, for example, Kaustia and Knüpfer, 2008; Chiang et al., 2011), but always in non-experimental contexts in which randomized variation of the type that we exploit is not available. As a result, this outcome variable is a useful cross-check on whether our empirical approach confirms the results in prior work.

Panel 1 of Table 5 shows that in the month of treatment, accounts that received a randomized allocation are 0.17 percentage points (p.p.) more likely to apply to an IPO in that month. In the month after treatment, treated accounts are 0.94 p.p. more likely to have applied for an IPO, and this effect is significant at the one percent level. This corresponds to a roughly 2% increase in the probability of applying for an IPO relative to the base rate probability of applying in the control group (46.36%). The effect size in month two is substantial, raising the probability of applying relative to the base rate by 3%. The effect sizes in months three through five are smaller in levels (between 0.19 and 0.32 p.p. when significant), but are similar in magnitude to the effect sizes in the first few post-treatment months relative to the base rate of applying for IPOs (they all represent roughly a 2% increase in the base rate of applying). Cumulatively, simply assuming that these probabilities are independent, we see an increase in the probability of applying to a future IPO of roughly 12% relative to the base rate in the control group (in month zero) over the six months following the IPO.[28] This suggests a significant causal effect of exogenous IPO experience on future IPO applications, and constitute a useful validation of our estimation approach given the qualitative similarity of our results to previous work using non-randomized allocation of IPOs.

[28] As mentioned earlier, these are likely under-estimates of the true effect as we only observe allotments and not applications for IPOs that were not handled by our data provider.

5.2 Treatment Effects on Trading Activity

We now move to testing whether the experience of the IPO lottery allocation has an impact on the investor's portfolio outside the narrow sphere of the IPO market. We view these as our most interesting analyses because testing for experience effects beyond IPO subscriptions takes the greatest advantage of the experiment that we study. When using non-experimental variation in experiences, we might become more concerned about unobservable investor or time characteristics as we try to explain behavior that is further removed from the original experience. For example, if we find that IPO investors who had positive experiences in this setting are more likely to subscribe to future IPOs, it seems plausible that learning from personal experiences is the main driver of this result, rather than unobservable investor or time heterogeneity. However, if we find using non-experimental data that successful IPO investors are more likely to increase their future trading volume across all stocks, we would quite naturally be more concerned about whether our inferences are contaminated by unobserved investor or time heterogeneity. Even if it is true that investor experience in the IPO market greatly influences a broad variety of investment behaviors, identification remains a challenge as it is ultimately very difficult to control for all of the factors that might jointly determine IPO experiences and trading behavior. The random assignment of experiences in our design allows us to precisely identify experience effects on a wide range of investor decisions.

These tests also allow us to explore to what extent experiences in particular stocks spillover to other parts of an investor's portfolio. In the specific domain which we consider, namely, retail investor portfolio choice, our results help to shed light on whether investors are better modeled as making separate stock-by-stock decisions (i.e. they "narrowly bracket" their utility changes from the IPO allocation in the sense of Rabin and Weizsäcker (2009) from those experienced on other components of their portfolio), or whether there are within-portfolio utility spillovers. When we find the latter, we go further when analyzing heterogenous treatment effects in an attempt to understand how these within-portfolio spillovers manifest themselves.

We check whether the treatment makes investors trade more in stocks *other than the IPO stock*. The literature has suggested overconfidence as a possible explanation for the large amount of trading volume, especially among retail investors in equity markets (see, for example, Statman et al., 2006; Odean, 1999; Barber and Odean, 2001). There are also numerous models which cite positive feedback trading as a likely explanation of the relationship between trading volume and stock returns (see, for example, Shiller, 2015; Barberis et al., 1998; De Long et al., 1990) based on the assumption that investors have extrapolative expectations. Testing for the presence of such expectations using price and investment flow data is difficult because in most models, prices, and investment decisions are jointly determined in equilibrium.[29] Having the ability to utilize exogenous variation in gains and losses in the portfolio confers a significant advantage in this setting, as prices and trading volume are jointly determined in equilibrium.

In Panel 2 of Table 5 the dependent variable is the inverse hyperbolic sine (IHS) of the total value of purchase and sale transactions made during the month, excluding the value of trades made in the IPO stock itself. We find that the value of transactions in the non-IPO stock portfolio treatment group relative to the control group increases by approximately 2% in the month of the IPO, and increases to almost 7.5% greater in the two months after the IPO. The increased trading reduces somewhat between three and six months after the IPO, but the treatment group still has a trading value which is 3.5% higher in the sixth month after the IPO.

When we include the IPO stock we see that the amount of trading activity increases substantially in month zero – treated investors trade roughly 47% more than the control group.[30] This suggests that a large fraction of treated investors sell the stock immediately,

[29]Note that this mechanism is not mutually exclusive to the others mentioned above; for example it is possible that positive experiences make investors overconfident, which then leads to greater trading volume as in Statman et al. (2006) and Barberis et al. (2015a).

[30]These are presented in Table V in the online appendix.

crystallizing the gains – a version of the disposition effect. These effects slowly decline as treated investors sell their allocation in the months following treatment.

Before we conclude that this result is purely behavioral in nature, we need to consider rational explanations such as portfolio rebalancing. As discussed earlier, any such explanation is complicated by the small size of the treatment effects – with gains in the experiment averaging roughly US$ 67, it is difficult to square this number with the substantial and prolonged increases in trading volume that we see up to 6 months following the treatment. Moreover, as we demonstrate later in the paper, portfolio rebalancing explanations have symmetric implications – exogenous losses in the IPO lottery should also be associated with increases in trading volume if portfolio rebalancing is the underlying cause. We show that this implication does not hold true in the data, as IPO lottery losses appear to be associated with declines in trading volume for treated investors.

Overall, this result has a number of interesting implications for models of trading and liquidity, since it says that exogenous variation in gains and losses (for example, those engendered by cash-flow relevant news releases) are associated with changes in investors' trading activity.

5.3 Treatment Effects on the Disposition Effect

The third panel of Table 5 shows that in the month following the IPO, there is a 0.82 p.p. increase in treated investors' disposition effect relative to a base rate of 10% in the control group. In other words, there is roughly an 8% increase in the realized disposition effect across the other stocks in a treated investor's portfolio caused by the randomly experienced gains in the IPO security. Treated investors are more prone to realizing gains, and less inclined to realize losses, behaving as if they were more loss averse following the positive IPO return realization.

One possible interpretation of this finding is that exogenously experienced gains have the effect of shifting investors' utility "reference-points" (see Tversky and Kahneman, 1991) up across the board for all stocks. This finding echoes that of Campbell et al. (2014), who find that overall account outperformance relative to the market is associated with increases in the disposition effect, using a different (non-experimental) approach.

5.4 Treatment Effects on Familiarity in Portfolio Choice

The fourth through sixth panels of Table 5 test whether treated investors are more likely to invest in the sector of the randomly allocated IPO lottery stock. First, we find that while the effects are positive, there doesn't appear to be a statistically significant increase in the probability that treated investors hold stocks in the same sector as the IPO stock (Panel 4 of Table 5). However, as Panel 5 shows, there is a small but statistically significant increase in the fraction of the portfolio that treated investors invest in the sector of the IPO stock. This effect is most prominent in months two through six following the IPO, and corresponds to a 5 to 9 basis point increase in the fraction of the portfolio in the sector. As a percentage of the base rate, which is the control group average allocation to the corresponding sector of approximately 8%, this corresponds to a 1% increase in the fraction of the portfolio allocated to this sector for the treatment group relative to the control group. These effects do appear to be quite persistent despite being small in magnitude. These results lend credence to models that assume that investors extrapolate their experiences to their beliefs about other related securities, such as that of Barberis et al. (2015b).

We also test whether treated households are more likely to buy the IPO stock that they were originally allocated (Panel 6, Table 5), as the random allocation of the stock could plausibly make the investor more familiar with it.[31] We find that the treatment group is less likely to re-purchase the IPO stock in the month of, and the month directly following, the random assignment of the IPO stock. We find that this result appears to be driven by the fact that some members of the control group try to participate in the IPO experience

[31] Note that this is not taken into account in the measures in Panel 4 and 5 of Table 5.

by purchasing the stock on the secondary market in the months immediately following the IPO. However, in months three through six after the IPO, we find that the treatment group is between 14 and 26 basis points more likely to re-purchase the IPO stock; this is a small effect in levels, but is large relative to the fact that very few members of the control group purchase the IPO stock (between 0.24% and 0.5% of the control group purchase this stock in months three through six after the IPO).

5.5 Treatment Effects on the Number of Stocks Held

Table 5 reports results on the effect of the randomized IPO allocation on the number of stocks that investors hold. Our idea is that this is a simple working definition of diversification. We find little evidence that our treatment and control groups are unbalanced on this measure of diversification in the months prior to receiving treatment. Note again that our dependent variable does not include the randomly allocated IPO stock.

We find that treated accounts hold approximately 0.69 p.p. more stocks in the month after the IPO allocation, increasing to 0.8 p.p. more stocks two months after the allocation, decreasing to approximately 0.6 p.p. more stocks six months after treatment (Panel 7, Table 5). These results while signalling a tiny increase in this measure of diversification, are nonetheless precisely estimated.

These results are interesting in light of work on reference-dependent risk attitudes (see Kőszegi and Rabin, 2007). If experienced gains affect attitudes towards risk, causing expectations of future risk to reduce, then buying an additional stock viewed in isolation, i.e., as an additional gamble, is expected utility increasing. Put differently, if you are randomly allocated a loss in the IPO lottery, this might increase your expectation of future risk in stock investing, somewhat perversely causing future gambles to be more aversive in the language of Kőszegi and Rabin (2007), and thus reducing diversification overall.

5.6 Treatment Effects on Overall Portfolio Value

Malmendier and Nagel (2011) find that investors who have positive experiences associated with the stock market increase their exposure to risky asset markets. Panels 8 and 9 of Table 5 explore how experiencing gains in the IPO lottery changes a household's overall account value after removing the IPO stock. We find that receiving the IPO stock neither increases the probability that the account will continue to participate in the market (i.e., the probability that the household will invest non-zero amounts in the market) in the 6 months following the IPO, nor do we find that the total value of the investments in the account increase.

We acknowledge that these results are most likely an artefact of the relatively short time window (6 months) following the IPO for which we track these outcomes. Participation effects manifest themselves over long time-scales, meaning that a longer-run historical study such as Malmendier and Nagel (2011) is more likely to pick up such effects.

6 Heterogenous Treatment Effects

6.1 Heterogeneity of Results Across IPOs

Our total sample consists of 54 IPOs, with widely differing first day returns, return variability, and issue sizes. Table 6 explores how these different treatments affect the outcome variables in our study. Inevitably, as we move to treatment heterogeneity, our inferences will become noisier for two reasons. First, there is the inevitable attenuation of sample size and the attendant loss of power. Second, there is the issue that heterogeneity (especially across investors) cuts across experiments, meaning that we are inevitably restricted to subsets of treatment and control within each experiment. This also causes a lack of precision. Nevertheless, there are interesting insights to be garnered from this heterogeneity, so we proceed in this direction, keeping the caveat about precision in mind. It is also important to note that we view this heterogeneity analysis as descriptive, in the sense that there may be some selection of the types of investors who participate in these different types of IPOs (and therefore some selection in how these different investors respond to the randomized treatments). Nonetheless, it is interesting to document how the treatment effects vary based on differences in these treatments.

Table 6 reports treatment effects for our nine outcome variables separately for IPOs with different return and value characteristics described in the table header. We simply estimate equation 6 separately for each sub-sample of IPOs and all of our outcome variables; where possible, we aggregate our outcome variables over the six months after the treatment IPO.[32] The bottom of the table reports the number of observations in each sub-sample, as well as the mean values of the different return and value characteristics.

The first column of the table simply repeats our baseline results for the 323 share categories and 40 IPOs with positive returns. The second column estimates the treatment effect for the 60 share categories and 14 IPOs with non-positive first-day returns in the sample. The sample size is severely reduced, but we can still observe several statistically significant results. Treated investors are substantially less likely to apply for future IPOs over the 6 months following the negative treatment (the effect size is larger than the one from the positive treatment). They are also significantly less likely to hold stocks in the same sector as the IPO, and reduce their portfolio weights on average in stocks in that sector. Perhaps surprisingly, they appear slightly more likely to repurchase the IPO security in which they experienced losses, suggesting that there may be an attention-related story associated with this behavior. Barber and Odean (2008) show that retail investors purchase both extreme loss-making and extreme-gain stocks and attribute this to the fact that these are more salient for investors.

[32] We present the results for 6 months after treatment for “stock” variables such as portfolio value, number of securities held, and the weight on the IPO sector. We measure disposition one month after treatment.

One of the possible drivers of the treatment effect on trading volume is that investors tend to rebalance towards their optimal portfolio, thus increasing the extent of market activity. If portfolio rebalancing is indeed the explanation, it should occur for both positive and negative returns experienced on a stock. However, the trading volume result for negative return IPOs in the second panel and second column of Table 6 suggests that the treatment effect is statistically insignificant, but negatively signed. It is also worth noting that this negative sign occurs despite the increase in trading activity likely associated with the new portfolio tilts (albeit with a small and precisely estimated 11 basis points) away from the IPO sector. Taken together, it appears unlikely that trading activity is fully explained by the portfolio rebalancing requirements of investors.[33]

The remaining columns of Table 6 are also interesting. Column 3 of the table shows the treatment effects arising from the IPOs with the highest positive returns (top quartile based on the first day listing gain of the IPO stock). These effects do appear somewhat stronger than those for all positive return IPOs, especially for the repurchase of the IPO security, the effects on disposition, and the likelihood of portfolio value being greater than zero.

Columns 4 and 5 consider the treatment effects for IPOs falling into the upper and lower quartiles of first-day variability in returns, computed as the percent difference between the listing day high and low over the issue price of the stock. The table shows that lower variation in the first-day return experience (Column 4) causes a far greater impact on an array of outcome variables. Treated investors who experience low variability are approximately twice as likely to participate in future IPOs, and have twice the volume of trades placed in the stock market compared with those who experience very high variability in first-day IPO returns.

[33] A growing body of literature also suggests that individual investors are very sluggish rebalancers, demonstrating inertia in this and other markets in which they participate. For instance, see (Calvet et al., 2009) and (Andersen et al., 2015).

Finally, columns 6 and 7 categorize IPOs by total IPO value at issuance (not merely the portion for retail investors) and estimate treatment effects using the top and bottom quartile of this value distribution. We find that larger IPOs have far stronger effects on investor behaviour – for instance, the effects on familiarity and disposition appear to be driven by the experiences of investors in the very largest IPOs.

6.2 Heterogeneity of Results by Investor Portfolio Value

Another possible driver of the treatment effects that we detect is that the additional wealth gain associated with treatment could allow investors to apply for future IPOs, increase trading activity and so on. There are two potential scenarios to consider here. The first is that households are initially liquidity constrained, and the treatment gain increases their liquid wealth, leading to the effects that we detect. However, this channel of liquidity/wealth constraints is rendered somewhat implausible by the fact that all households need to place a significant amount of cash in escrow in order to apply for the IPO in the first place.[34] The treatment gain is an order of magnitude lower than this amount of cash. The second possibility is that the treatment gain has the same effect as a pure cash transfer, i.e., any results we see are a pure wealth effect. While we once again view this as implausible given the relatively small sizes of the treatment gain, if this were the case, it would be an extremely interesting channel to explore, as it is rare to have an experiment in which we can detect the *marginal propensity to invest in stocks* in response to a wealth shock. Many authors (Parker et al., 2011; Juster et al., 2006; Gourinchas and Parker, 2002, to name a few) have

[34]For example suppose an IPO had an issue price of 10 rupees and the minimum lot size was 10 shares. Both lottery winners and losers had to have deposited 100 rupees (or more) to enter the lottery. The only difference is that the winners get this back in shares and the losers get this back in cash. Thus, the only wealth effect here would be if the IPO stock appreciates in value; if it appreciated by 10 % in the first month then the wealth effect would only be 10 rupees. Note also that technically, control investors could immediately buy the stock in the secondary market. This would limit the wealth gains to the difference between the initial listing price and the issue price of the IPO.

undertaken work on the marginal propensity to consume or save in response to unanticipated wealth shocks.

To further explore the question of liquidity constraints, Table 7 considers how the treatment effects vary across the size of the amount placed in escrow, i.e., the size of the initial application for the IPO. Admittedly this is an imperfect proxy, since it is potentially also correlated with the wealth of the household making the application. Nevertheless we use this proxy as a useful one in combination with other household characteristics that we believe are more closely correlated with wealth, such as the total size of the household's equity portfolio.

Table 7 estimates equation 6 separately for accounts in different quartiles of the application size distribution, corresponding to average application sizes of US$ 256, US$ 913, US$ 2,174, and US$ 2,843. The bottom of table reports the number of observations in each quartile, the number of experiments corresponding to each of these quartiles, and the mean probability of treatment associated with each quartile – note that this is increasing in application size as described when we outlined the lottery process earlier.

The table shows that while there is an attenuation of the effects as we move to larger application sizes, they continue to be strong and statistically significant, especially for trading volume, disposition, and our measures of familiarity, even for the largest application quartile. The likely correlation of application size with total investor wealth also admits other interpretations than attenuation purely on account of the alleviation of liquidity constraints. We consider these more carefully with other proxies below.

In most work in household financial behaviour (for example, Campbell, 2006), investor wealth is strongly associated with investor sophistication – this suggests that any treatment effects observed on average should attenuate or even be non-existent for wealthier households. To investigate this further, we separately estimate effect sizes based on the size of the total equity portfolio of each household. Table 8 reports treatment effects for our nine outcome variables separately for accounts in different deciles of the portfolio value distribution as of the month prior to the IPO. We split the full sample of accounts into 10 groups based on the portfolio value; the first two deciles are grouped together, since this contains all accounts that had a portfolio value of zero in the month prior to treatment (23% of the full sample), and the remaining accounts are split across the remaining deciles. We once again estimate equation 6 separately for each sub-sample and all of our outcome variables over the six months after the treatment IPO. The bottom of table reports the number of observations in each decile as well as the mean portfolio value in dollars in the month prior to the treatment IPO.

In the first panel of the table, we find that the treatment effect on applying to future IPOs is similarly sized across accounts, ranging from those with a zero balance prior to the treatment, all the way up to accounts in the 70-80th percentiles of account value (average portfolio size of US$ 4,720). Even in the very largest accounts, with mean portfolio value of US$ 83,294, there continues to be a strong and statistically significant treatment effect. The similarity in the magnitude of this effect across this enormous range of portfolio values is difficult to reconcile with any story of wealth effects driving our results. Indeed, it also appears that for this measure, the propensity to apply for future IPOs, it is very difficult to conclude that investor sophistication (to the extent that this is correlated with wealth) drives out the effects of experienced gains. The evidence appears to point towards a simple story that experienced gains have strong effects on investor psychology.

In the second panel of Table 8, we do find that effects on trading volume decrease substantially across the portfolio value distribution. These effects decline from a roughly 13% increase for the very smallest accounts to approximately 1.3% for accounts in the 80-90th percentiles of the portfolio size distribution. However these magnitudes make clear the implausibility of appealing to a wealth effects channel. Such an explanation would imply that households are extremely sensitive to small changes in wealth. The wealth gain due to the IPO first day return is approximately US$ 70 on average. For households in the 80-90th percentile of the portfolio size distribution, this is only 0.75% of average total portfolio value.

That this small wealth gain causes a 1.3% increase in trading activity for this group seems implausible, and an appeal to more behavioral forces seems necessary. More generally, the large size of the effect has important implications for theories relating trading volume to returns in the equity market.

Experience effects on disposition (Panel 3) attenuate from 2.19% to 0.21% across as we move along the portfolio size distribution. It is worth noting though, that in all of these cases, despite this strong reduction in the size of the effect, the statistical significance of the effect is still unchanged even for the very top of the portfolio size distribution. Clearly, experienced gains are a powerful force on investment decision-making. This is also clear for the effect of familiarity – investors' propensity to hold stocks in the IPO sector is 42 basis points higher for treated investors in the second highest decile of the portfolio size distribution, those with a mean portfolio value of US$ 9,360.

Figure 3 provides a graphical summary of these results across the portfolio value spectrum, showing that for four main outcome variables, there is a significant reduction in the size of the effect as we pass the portfolio size threshold of roughly US$ 5,000. However, the treatment effect sizes, though considerably smaller, remain statistically significant all the way to the top of the portfolio size distribution.

6.3 Heterogeneity of Results by Investor "Age in the Market"

Several papers (as in List, 2003, 2004) suggest that increasing experience of market interactions should result in more rational behaviour when engaging in market transactions. This hypothesis is deeply interesting, since it suggests that in "age in the market" may be a useful proxy of financial sophistication, and one which is not necessarily captured fully by wealth or other demographic characteristics. We therefore check whether the treatment effect of experienced gains in the IPO lottery decreases with increasing age in the market in Table 9. We estimate treatment effects for account age deciles using a similar procedure as for portfolio size heterogeneity, under the assumption that the amount of time investors spend in the market measures their increasing experience of market interactions.

The table shows that the treatment effect heterogeneity is similar to that of the portfolio size heterogeneity discussed above. Figure 4 plots selected results from Table 9 across the spectrum of new and old accounts. For example, in terms of future IPO participation (Panel 1), the effect appears to settle at a positive constant of 54 basis points for the very top of the age distribution, for accounts with an average age of $5\frac{1}{2}$ years in the market. The effect continues, however to be strongly statistically significant. Similarly, the effects on trading volume decline with the age of the account, but remain large and significant for accounts in the 80−90th percentile of age (for accounts with an average age in the market of just under 4 years). The effects are similar for disposition and familiarity.

Overall, these results suggest that longer exposure to market interactions does not eliminate the importance of short-term gain experiences for investor behavior, despite having a strong attenuation effect.

7 Conclusion

Our paper exploits the randomized allocation of stocks in 54 Indian IPO lotteries to 1.5 million investors between 2007 and 2012, and provides new estimates of the causal effect of investment experiences on future investment behavior. To our knowledge, this is the first paper to estimate the causal effect of experience on investment behavior using the randomized allotment of real securities.

We find that investors experiencing exogenous gains in IPO stocks (the treatment) are more likely to apply for future IPOs, increase trading in their portfolios, exhibit a stronger disposition effect, and tilt their portfolios towards the sector of the treatment IPO. We also find that these treatment effects are stronger for smaller accounts and accounts that have spent less time in the market, and increase in magnitude with the experience itself, i.e., the size of the first-day gain from treatment. We view our results as having implications for a wide range of empirical and theoretical work on the effects of experience on economic decision making.

A Appendix

A.1 Calculating the Probability of Winning

We begin from equation (5) in the paper. In that equation, we substitute for Z from equation (3) and use equation (4) to re-express $p_{c'}$ for share categories $c' \in [1, ..., J)$ in terms of p_1. We then substitute for S from equation (2) to arrive at:

$$p_1 = \frac{\frac{1}{v}\sum_{j=1}^{C} a_c cx - \sum_{c=J}^{C} a_c \frac{cx}{v}}{\sum_{c'=1}^{J-1} c' x a_{c'}}, \tag{i}$$

which gives $p_1 = \frac{1}{v}$, and $p_{c'} = \frac{c'}{v}$ for randomized share categories $c' \in [1, ..., J)$.

This probability is well defined, i.e., $0 < \frac{c'}{v} < 1$. Recall that the regulation requires randomization when the proportional allocation cannot allocate at least the minimum lot size of shares. Consider $c' = (J-1)$, which is the final share category in which proportional allocation is not possible, and random allocation must take place. That is:

$$\frac{(J-1)x}{v} < x \Longrightarrow (J-1) < v \tag{ii}$$

This will also be true for all values of $0 < c' < (J-1)$. Further, since $v > 0$ and $c' > 0$, $\frac{c'}{v} > 0$. Thus, $0 < \frac{c'}{v} < 1$.

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Table 1: Example IPO Allocation Process: Barak Valley Cement IPO Allocation

Share Category	Shares Bid For	# Applications	Total Shares	Proportional Allocation	Win Probability	Shares Allocated	# Treatment group	# Control group
(c)	($c \times x$)	a_c	$a_c \times c \times x$	$\frac{cx}{v}$	$\frac{c}{v}$		$\frac{c}{v} \times a_c$	$(1 - \frac{c}{v}) \times a_c$
(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1	150	14,052	2,107,800	4	0.027	57,000	380	13,672
2	300	9,893	2,967,900	8	0.054	80,250	535	9,358
3	450	5,096	2,293,200	12	0.081	61,950	414	4,682
4	600	4,850	2,910,000	16	0.108	78,750	525	4,325
5	750	2,254	1,690,500	20	0.135	45,750	305	1,949
6	900	1,871	1,663,900	24	0.162	45,450	304	1,567
7	1050	4,806	5,046,300	28	0.189	136,500	910	3,896
8	1200	2,900	3,480,000	32	0.216	94,050	628	2,272
9	1350	481	649,350	36	0.244	17,550	117	364
10	1500	1,302	1,953,000	41	0.271	52,800	352	950
11	1650	266	436,900	45	0.298	11,850	79	187
12	1800	317	570,600	49	0.325	15,450	103	214
13	1950	174	339,300	53	0.352	9,150	61	113
14	2100	356	747,600	57	0.379	20,250	135	221
15	2250	20,004	45,009,000	61	0.406	1,217,700	8119	11,885

Note: Columns (7) and (8) are obtained after applying the regulation defined rounding off methodology as described in section 2.3.

Table 2: IPO Characteristics

	2007	2008	2009	2010	2011	All
IPOs in sample						
Number of IPOs in sample	12	10	2	22	8	54
Percentage of all IPOs in India	12.04	31.58	11.76	32.84	20.51	22.13
Value of IPOs in sample ($ bn)	0.28	0.42	0.03	1.58	0.34	2.65
Percentage of total value of IPOs in India	3.00	8.77	0.72	11.01	24.62	7.71
Percentage issued (Retail investors excl. employees)	33.01	34.33	34.88	32.71	35.00	33.50
Over-subscription ratio	21.95	12.63	2.11	10.10	6.72	12.06
No. of randomized share categories ("Experiments")	109	55	2	177	40	383
Total no. of share categories	178	152	28	398	227	983
No. of IPOs from different sectors						
Technology	1	1	0	2	0	4
Manufacturing	8	6	2	12	3	31
Other Services	2	3	0	8	4	17
Retail	1	0	0	0	1	2

Table 3: Characterizing Treatment Experience

Treatment Characteristics		Percentile Across Experiments				
	Mean	10	20	50	75	90
	(1)	(2)	(3)	(4)	(5)	(6)
Application Amount ($)	1803.41	163.27	392.44	846.99	1524.57	2174.07
Probability of Treatment	0.35	0.09	0.18	0.35	0.63	0.82
Allotment Value ($)	150.38	123.81	134.11	145.57	157.73	165.72
First Day Gain/Loss (%)	42.28	6.00	11.54	21.73	40.00	87.80
First Day Gain ($)	67.03	8.68	14.28	29.58	65.30	141.62
Median Portfolio Value ($t-1$, $)	1866.05	805.74	1126.76	1632.09	2466.32	3208.12

Table 4: Randomization Check

	Treatment	Control	Difference	% Experiments > 10% significance
	(1)	(2)	(3)	(4)
Applied/Allotted an IPO	0.379	0.379	0.000	8.97
IHS Gross Transaction Value	5.619	5.616	0.003	11.45
0	0.287	0.288	-0.001	8.97
0 to 500$	0.183	0.183	-0.001	9.90
500 to 1000$	0.127	0.127	0.000	9.59
1000 to 5000$	0.287	0.285	0.002**	14.55
> 5000 $	0.116	0.117	-0.001*	8.97
IPO Stock Sector Portfolio Weight	0.063	0.063	0.000	12.69
Disposition	0.098	0.098	-0.000	7.12
Cutoff bid	0.928	0.928	0.000**	11.1
Application by Blocked Amount (ASBA)	0.048	0.048	-0.001	4.0
States of India				
Gujarat	0.350	0.348	0.002*	12.60
Maharashtra	0.216	0.216	0.000	9.30
Rajasthan	0.148	0.149	-0.001	8.70
Delhi	0.045	0.046	0.000	8.0
Portfolio Value > 0	0.778	0.778	0.000	9.28
IHS Portfolio Value	6.575	6.573	0.002	13.00
0	0.222	0.221	0.000	10.52
0 to 500$	0.143	0.143	-0.001	8.66
500 to 1000$	0.097	0.097	0.000	8.63
1000 to 5000$	0.285	0.285	0.000	9.59
> 5000 $	0.252	0.252	-0.001	10.21
IHS Account Age	3.134	3.131	0.003*	12.38
New Account	0.041	0.041	0.000	5.00
1 Month old	0.067	0.066	0.001	6.80
2-6 Months old	0.201	0.203	-0.002**	8.35
7-13 Months old	0.143	0.143	0.000	9.00
14-25 Months old	0.151	0.152	-0.001	9.90
> 25 Months old	0.378	0.376	0.002	11.10

Table 5: Experience Effects on Investor Behaviour

	Event-time												
	-6	-5	-4	-3	-2	-1	0	1	2	3	4	5	6
1. Future IPO	0.0006	0.0018**	0.0003	0.0005	-0.0009	-0.0001	0.0017	0.0094***	0.0071**	0.0029**	0.0019**	0.0032**	0.0013
participation	(0.0008)	(0.0009)	(0.0012)	(0.0008)	(0.0006)	(0.0010)	(0.0011)	(0.0015)	(0.0030)	(0.0015)	(0.0009)	(0.0012)	(0.0011)
	[0.2034]	[0.3108]	[0.2043]	[0.2172]	[0.3324]	[0.3786]	[0.4850]	[0.4636]	[0.2242]	[0.1283]	[0.0959]	[0.1341]	[0.0605]
2. Gross transaction	0.0084	0.0070	0.0085	0.0057	0.0000	0.0034	0.0212**	0.0746***	0.0742***	0.0447***	0.0333***	0.0345***	0.0345***
value	(0.0070)	(0.0070)	(0.0091)	(0.0059)	(0.0077)	(0.0058)	(0.0093)	(0.0121)	(0.0082)	(0.0118)	(0.0083)	(0.0089)	(0.0066)
	[0.1860]	[0.2436]	[0.2983]	[0.4496]	[0.9908]	[1.6807]	[1.6114]	[1.5832]	[0.9868]	[0.3052]	[0.2147]	[0.4525]	[0.2522]
3. Disposition	-0.0002	-0.0003	0.0006	-0.0005	-0.0003	-0.0004	0.0037*	0.0082***	0.0007*	0.0005	0.0013***	0.0006	0.0008
	(0.0005)	(0.0006)	(0.0004)	(0.0006)	(0.0004)	(0.0004)	(0.0023)	(0.0014)	(0.0004)	(0.0004)	(0.0003)	(0.0004)	(0.0005)
	[0.0523]	[0.0440]	[0.0523]	[0.0651]	[0.0647]	[0.0983]	[0.0644]	[0.0997]	[0.0311]	[0.0497]	[0.0560]	[0.0560]	[0.0491]
4. Propensity to hold	0.0009	0.0003	0.0011	0.0013	0.0005	0.0005	0.0002	0.0006	0.0010	0.0013	0.0018	0.0015	0.0016
IPO sector stocks	(0.0011)	(0.0010)	(0.0011)	(0.0007)	(0.0008)	(0.0009)	(0.0009)	(0.0014)	(0.0013)	(0.0013)	(0.0014)	(0.0015)	(0.0011)
	[0.2502]	[0.2649]	[0.2764]	[0.2731]	[0.3478]	[0.3170]	[0.3659]	[0.3662]	[0.3966]	[0.3946]	[0.4038]	[0.4109]	[0.4063]
5. Weight in IPO sector	0.0004	0.0002	0.0003	0.0004	0.0005	0.0001	0.0003	0.0001	0.0005**	0.0008***	0.0009**	0.0008***	0.0006***
	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0004)	(0.0002)	(0.0003)	(0.0004)	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0002)
	[0.0529]	[0.0571]	[0.0534]	[0.0499]	[0.1042]	[0.0629]	[0.0769]	[0.0708]	[0.0822]	[0.0811]	[0.0823]	[0.0851]	[0.0808]
6. Re-purchase of IPO	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	-0.0013**	-0.0027**	0.0026***	0.0017**	0.0014***	0.0018***	0.0022***
security	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0005)	(0.0010)	(0.0008)	(0.0007)	(0.0005)	(0.0004)	(0.0007)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0065]	[0.0132]	[0.0050]	[0.0035]	[0.0024]	[0.0025]	[0.0028]
7. No. of securities	0.0021	0.0016	0.0020	0.0018	0.0007	0.0003	0.0028	0.0069***	0.0078***	0.0064***	0.0057***	0.0059***	0.0062***
held	(0.0026)	(0.0026)	(0.0025)	(0.0024)	(0.0026)	(0.0022)	(0.0026)	(0.0016)	(0.0013)	(0.0017)	(0.0019)	(0.0018)	(0.0019)
	[3.6157]	[3.8247]	[4.0955]	[4.3544]	[4.8554]	[5.4630]	[6.5410]	[7.0343]	[7.3669]	[7.1642]	[7.1391]	[7.1692]	[7.1050]
8. Portfolio value >0	0.0013	0.0009	0.0007	0.0010	0.0002	-0.0003	-0.0005	-0.0003	-0.0001	0.0005	0.0003	0.0003	0.0006
	(0.0009)	(0.0009)	(0.0012)	(0.0009)	(0.0010)	(0.0007)	(0.0008)	(0.0007)	(0.0005)	(0.0003)	(0.0005)	(0.0004)	(0.0004)
	[0.5959]	[0.6137]	[0.6550]	[0.6809]	[0.7342]	[0.7787]	[0.8260]	[0.8762]	[0.8891]	[0.8902]	[0.8764]	[0.8786]	[0.8765]
9. Portfolio value	0.0132	0.0118	0.0113	0.0114	0.0054	0.0018	-0.0023	-0.0002	0.0025	0.0071	0.0057	0.0065	0.0089
	(0.0090)	(0.0084)	(0.0105)	(0.0067)	(0.0092)	(0.0078)	(0.0073)	(0.0076)	(0.0067)	(0.0063)	(0.0076)	(0.0073)	(0.0075)
	[0.9585]	[1.1207]	[1.5212]	[1.9287]	[3.1209]	[4.1797]	[6.3721]	[8.0207]	[8.7253]	[9.0154]	[8.0666]	[7.6502]	[7.5205]

Significance: *** 0.01 ** 0.05, *0.10,(clustered robust std. errors), [Mean Dep. Variable - Control group], Observations: 1,473,073

Units: Rows 1,2,3,7,8,9 in basis points. Means for rows 4,6 in 100s of US Dollars and Row 5 in No. of securities.

Table 6: Heterogeneity by ipo Characteristics

	IPO characteristics						
	Positive Returns	Negative Returns	Top Quartile Percent Returns	Bottom Quartile Returns variability	Top Quartile Returns Variability	Bottom Quartile ipo Value	Top Quartile ipo Value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1. Future IPO Participation	0.0117***	-0.0142**	0.0118***	0.0187***	0.0092***	0.0085**	0.0114***
Time: (t+1) to (t+6)	(0.0013)	(0.0039)	(0.0029)	(0.0059)	(0.0023)	(0.0039)	(0.0023)
2. Gross Transaction Value	0.0717***	-0.0210	0.0570***	0.0645***	0.0403***	0.0335**	0.0516***
Time: (t+1) to (t+6)	(0.0071)	(0.0192)	(0.0108)	(0.0238)	(0.0098)	(0.0173)	(0.0118)
3. Disposition	0.0082***	-0.0013	0.0123**	0.0079**	0.0126***	0.0066	0.0123***
Time: (t+1)	(0.0020)	(0.0029)	(0.0062)	(0.0040)	(0.0035)	(0.0057)	(0.0012)
4. Propensity to hold IPO sector stocks	0.0022	-0.0064**	0.0121***	0.0070*	0.0038**	-0.0012	0.0047***
Time: (t+1) to (t+6)	(0.0015)	(0.0029)	(0.0031)	(0.0038)	(0.0016)	(0.0040)	(0.0012)
5. Weight in IPO sector	0.0006***	-0.0011**	0.0004	0.0008*	0.0002	-0.0001	0.0002
Time: (t+6)	(0.0002)	(0.0064)	(0.0064)	(0.0005)	(0.0004)	(0.0005)	(0.0005)
6. Repurchase of IPO security	0.0039	0.0110***	0.0054***	0.0001	0.0073***	0.0044*	0.0081***
Time: (t+1) to (t+6)	(0.0025)	(0.0029)	(0.0019)	(0.0033)	(0.0015)	(0.0023)	(0.0015)
7. No. of securities held	0.0062***	-0.0052	-0.0007	0.0118*	0.0069	0.0023	0.0112***
Time: (t+6)	(0.0019)	(0.0092)	(0.0038)	(0.0065)	(0.0046)	(0.0055)	(0.0031)
8. Portfolio value > 0	0.0013***	0.0012	0.0038**	0.0023**	0.0006	0.0009	0.0001
Time: (t+1) to (t+6)	(0.0004)	(0.0014)	(0.0015)	(0.0012)	(0.0005)	(0.0009)	(0.0011)
9. Portfolio value	0.0089	-0.0154	-0.0106	-0.0113	0.0123	-0.0096	0.0244***
Time: (t+6)	(0.0075)	(0.0209)	(0.0128)	(0.0173)	(0.0137)	(0.0127)	(0.0069)
Observations	1,473,073	89,637	410,013	131,000	615,059	400,684	527,318

Significance: *** 0.01 ** 0.05, *0.10,(clustered robust std. errors), Units: basis points

Note: Table I in the online appendix presents the balance tests for this table.

Table 7: Heterogeneity by Initial Application Value

	Application value quartile range			
	0-25	25-50	50-75	75-100
1. Probability of Applying to Future IPO	0.0265***	0.0178***	0.0070***	0.0056***
Time: (t+1) to (t+6)	(0.0044)	(0.0025)	(0.0010)	(0.0013)
2. Gross Transaction Value	0.0925***	0.0772***	0.0442***	0.0440***
Time: (t+1) to (t+6)	(0.0197)	(0.0175)	(0.0068)	(0.0090)
3. Disposition	0.0052***	0.0077***	0.0097***	0.0079***
Time: (t+1)	(0.0012)	(0.0012)	(0.0029)	(0.0033)
4. Propensity to hold IPO sector stocks	0.0053*	0.0064**	-0.0033	0.0038***
Time: (t+1) to (t+6)	(0.0030)	(0.0027)	(0.0020)	(0.0014)
5. Weight on IPO sector	0.0008	0.0012	-0.0001	0.0007**
Time: (t+6)	(0.0007)	(0.0009)	(0.0005)	(0.0003)
6. Repurchase of IPO security	0.0087***	0.0081**	0.0024	0.0010
Time: (t+1) to (t+6)	(0.0034)	(0.0036)	(0.0021)	(0.0035)
7. No. of securities held	0.1136	0.0114**	0.0024	0.0077
Time: (t+6)	(0.0073)	(0.0046)	(0.0033)	(0.0073)
8. Portfolio value > 0	0.0027*	0.0027***	0.0005	0.0003
Time: (t+1) to (t+6)	(0.0016)	(0.0007)	(0.0007)	(0.0005)
9. Portfolio value	0.0067	0.0176**	-0.0053	0.0197
Time: (t+6)	(0.0184)	(0.0089)	(0.0086)	(0.0144)
Observations	377,937	359,706	385,609	349,821
Mean Application Value ($)	245.61	912.67	2173.98	2842.46
No. of Experiments	98	126	76	23
Mean Probability of Treatment	0.1437	0.2421	0.3812	0.4134

Significance: *** 0.01 ** 0.05, *0.10,(clustered robust std. errors), Units: basis points

Note: Table II in the online appendix presents the balance tests for this table.

Table 8: Heterogeneity by Portfolio Size

	Portfolio Value (t-1) Percentile Range								
	0-23	23-30	30-40	40-50	50-60	60-70	70-80	80-90	90-100
1. Future IPO participation	0.0128***	0.0153***	0.0100***	0.0135***	0.0118***	0.0081***	0.0106***	0.0044***	0.0036***
Time: (t+1) to (t+6)	(0.0025)	(0.0039)	(0.0031)	(0.0029)	(0.0026)	(0.0025)	(0.0027)	(0.0012)	(0.0013)
2. Gross Transaction Value	0.1312***	0.1620***	0.0884***	0.0439***	0.0689***	0.0443***	0.0160	0.0130**	0.0180
Time: (t+1) to (t+6)	(0.0287)	(0.0306)	(0.0121)	(0.0078)	(0.0110)	(0.0111)	(0.0101)	(0.0057)	(0.0148)
3. Disposition	0.0192***	0.0219***	0.0131***	0.0092***	0.0127***	0.0057**	0.0032*	0.0040***	0.0021*
Time: (t+1)	(0.0054)	(0.0072)	(0.0049)	(0.0030)	(0.0020)	(0.0027)	(0.0018)	(0.0014)	(0.0012)
4. Propensity to hold IPO sector stocks	-0.0012	0.0094	0.0151***	0.0090	0.0108***	0.0077*	0.0105***	0.0042**	0.0013
Time: (t+1) to (t+6)	(0.0017)	(0.0066)	(0.0020)	(0.0056)	(0.0038)	(0.0042)	(0.0036)	(0.0022)	(0.0020)
5. Weight on IPO sector	0.0009	-0.0030	0.0024**	0.0002	0.0009	0.0004	0.0004	0.0009	-0.0001
Time: (t+6)	(0.0008)	(0.0023)	(0.0012)	(0.0009)	(0.0011)	(0.0006)	(0.0008)	(0.0006)	(0.0004)
6. Repurchase of IPO security	0.0076***	0.0043*	0.0064***	0.0019	0.0044	0.0011	0.0003	0.0015	0.0036
Time: (t+1) to (t+6)	(0.0022)	(0.0025)	(0.0008)	(0.0017)	(0.0032)	(0.0034)	(0.0033)	(0.0031)	(0.0047)
7. No. of securities held	0.0130***	0.0033	0.0042	-0.0006	0.0004	-0.0033	0.0031	0.0048	0.0179***
Time: (t+6)	(0.0035)	(0.0088)	(0.0054)	(0.0037)	(0.0052)	(0.0031)	(0.0040)	(0.0044)	(0.0064)
8. Portfolio value > 0	0.0105***	-0.0003	0.0003	-0.0004	0.0000	0.0000	-0.0002	-0.0003	0.0000
Time: (t+1) to (t+6)	(0.0039)	(0.0013)	(0.0006)	(0.0004)	(0.0005)	(0.0004)	(0.0003)	(0.0003)	(0.0002)
9. Portfolio value	0.0347	-0.0202	-0.0168	-0.0070	-0.0153	-0.0108	0.0002	0.0193*	0.0269*
Time: (t+6)	(0.0128)	(0.0208)	(0.0188)	(0.0061)	(0.0137)	(0.0101)	(0.0089)	(0.0117)	(0.0153)
Observations	338,584	103,372	147,281	147,319	147,285	147,307	147,307	147,310	147,308
Mean Portfolio Value (t-1) in Sample ($)	0	129	411	841	1545	2672	4720	9360	83294

Significance: *** 0.01 ** 0.05, *0.10,(clustered robust std. errors), Units: basis points

Note: Table III in the online appendix presents the balance tests for this table.

Table 9: Heterogeneity by Account Age

	Account age percentile range									
	0–11	11–22	22–32	32–41	41–51	51–60	60–70	71–81	81–91	91–100
1. Future IPO participation	0.0159***	0.0166***	0.0093***	0.0062**	0.0089***	0.0107***	0.0087**	0.0083***	0.0076***	0.0054**
Time: (t+1) to (t+6)	(0.0021)	(0.0020)	(0.0034)	(0.0026)	(0.0023)	(0.0031)	(0.0040)	(0.0029)	(0.0025)	(0.0023)
2. Gross Transaction Value	0.1189***	0.0811***	0.0744***	0.0720***	0.0720***	0.0583***	0.0545***	0.0537***	0.0668***	0.0276*
Time: (t+1) to (t+6)	(0.0408)	(0.0113)	(0.0171)	(0.0215)	(0.0245)	(0.0160)	(0.0165)	(0.0131)	(0.0099)	(0.0158)
3. Disposition	0.0112***	0.0084*	0.0086***	0.0161***	0.0117***	0.0101***	0.0124***	0.0075***	0.0034	0.0038*
Time: (t+1)	(0.0052)	(0.0050)	(0.0036)	(0.0035)	(0.0028)	(0.0033)	(0.0021)	(0.0017)	(0.0023)	(0.0021)
4. Propensity to hold IPO sector stocks	-0.0004	0.0025*	0.0021	0.0068*	0.0102***	0.0138**	0.0108***	0.0092***	0.0035	0.0059**
Time: (t+1) to (t+6)	(0.0045)	(0.0014)	(0.0040)	(0.0040)	(0.0030)	(0.0062)	(0.0042)	(0.0032)	(0.0045)	(0.0027)
5. Weight on IPO sector	0.0023**	-0.0004	-0.0010	0.0008	0.0008	-0.0004	0.0018**	-0.0019**	0.0021**	0.0010
Time: (t+6)	(0.0010)	(0.0010)	(0.0013)	(0.0008)	(0.0008)	(0.0006)	(0.0009)	(0.0009)	(0.0010)	(0.0007)
6. Repurchase of IPO security	0.0110***	0.0059***	0.0070***	0.0045**	0.0046**	0.0038	0.0034*	-0.0001	0.0003	-0.0021
Time: (t+1) to (t+6)	(0.0025)	(0.0022)	(0.0021)	(0.0022)	(0.0022)	(0.0028)	(0.0019)	(0.0027)	(0.0035)	(0.0036)
7. No. of securities held	0.0109*	0.0129**	-0.0106*	0.0082	0.0058	-0.0012	-0.0049	-0.0044	0.0232***	0.0143**
Time: (t+6)	(0.0065)	(0.0053)	(0.0055)	(0.0076)	(0.0121)	(0.0081)	(0.0050)	(0.0087)	(0.0073)	(0.0.0071)
8. Portfolio value > 0	0.0070	0.0016	0.0029*	0.0025	0.0035***	0.0009	0.0001	0.0009	0.0019*	0.0005
Time: (t+1) to (t+6)	(0.0045)	(0.0021)	(0.0016)	(0.0021)	(0.0013)	(0.0013)	(0.0014)	(0.0008)	(0.0010)	(0.0004)
9. Portfolio value	0.0312	-0.0006	-0.0229	0.0019	-0.0030	-0.0162	-0.0185	-0.0108	0.0566	0.0339**
Time: (t+6)	(0.0223)	(0.0207)	(0.0111)	(0.0234)	(0.0294)	(0.0203)	(0.0142)	(0.0226)	(0.0189)	(0.0146)
Observations	164,040	157,243	146,158	128,922	161,233	126,731	147,894	158,499	142,463	139,890
Mean Account Age(t-1) in Sample (Months)	0	1.4	4	7.3	12.4	18.6	25.8	34.5	44.1	67.5

Significance: *** 0.01 ** 0.05, *0.10,(clustered robust std. errors), Units: basis points

Note: Table IV in the online appendix presents the balance tests for this table.

Figure 1: IPO Frequency

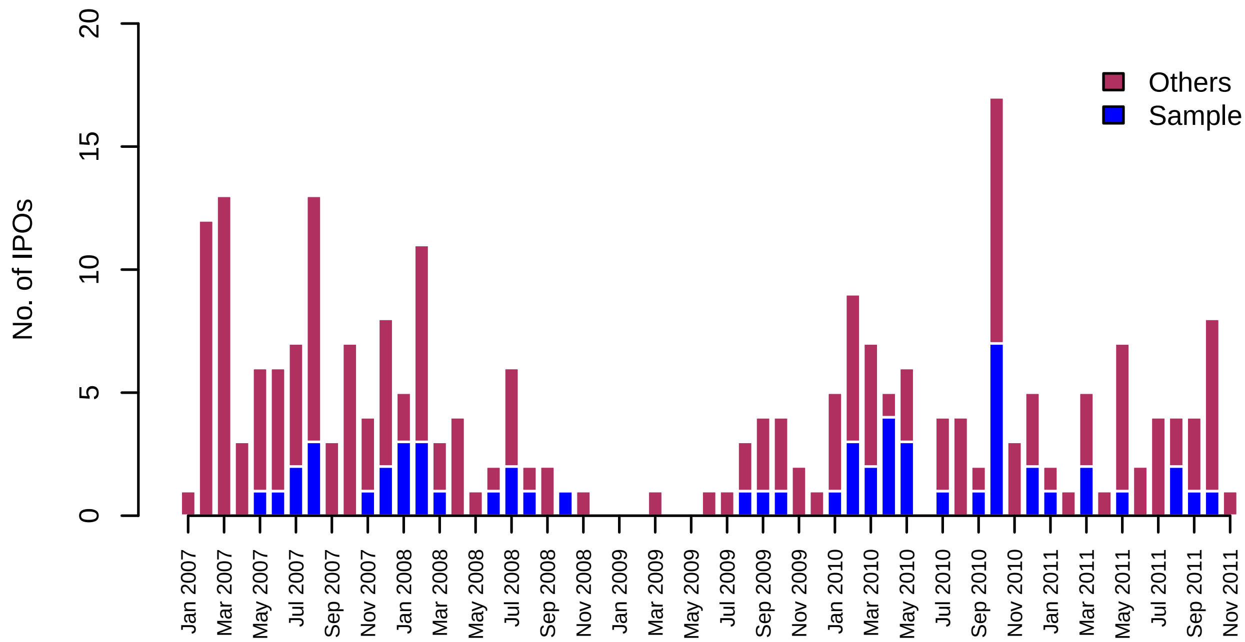
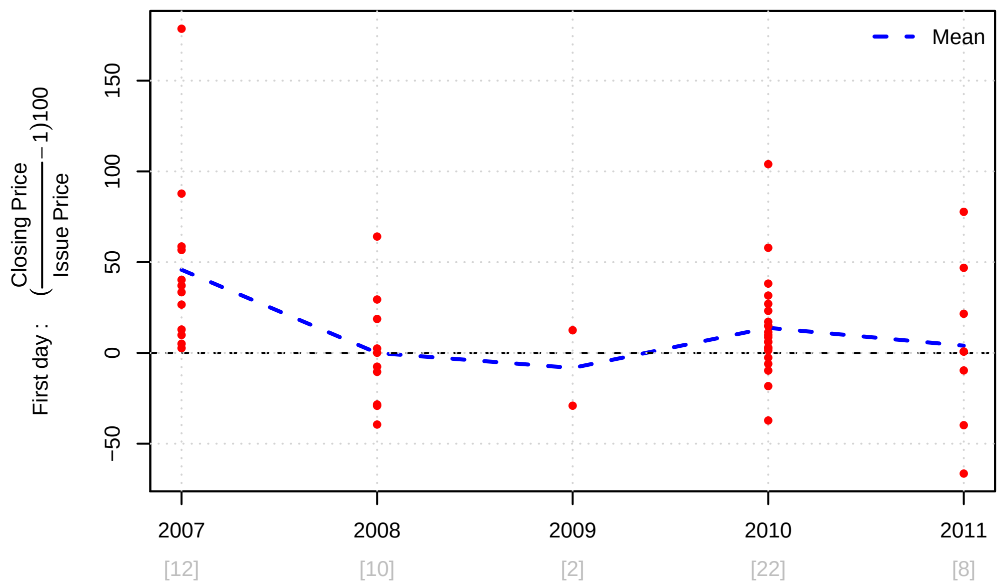


Figure 2: IPO Experience

First-day returns



First-day returns variability

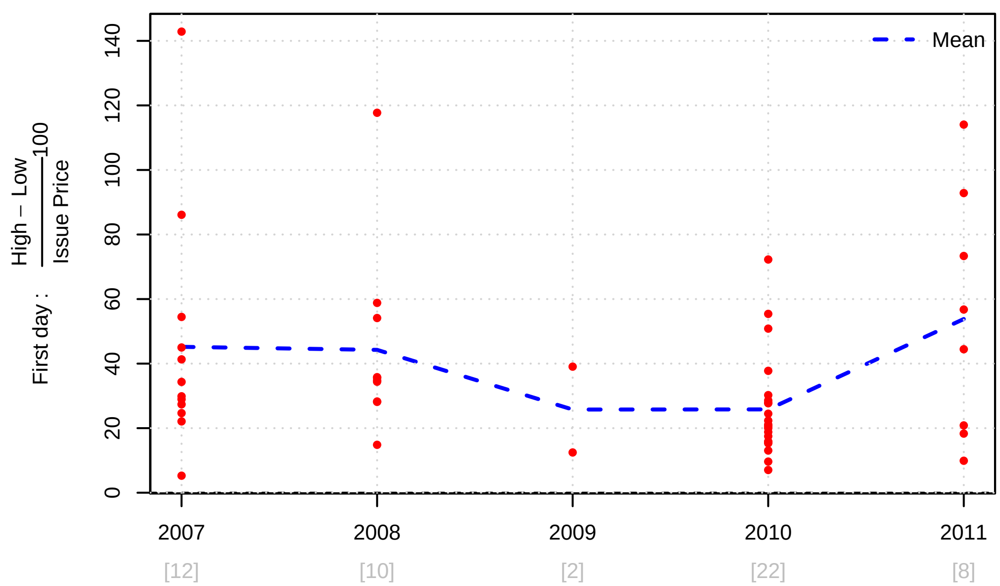


Figure 3: Heterogeneity by Portfolio Value

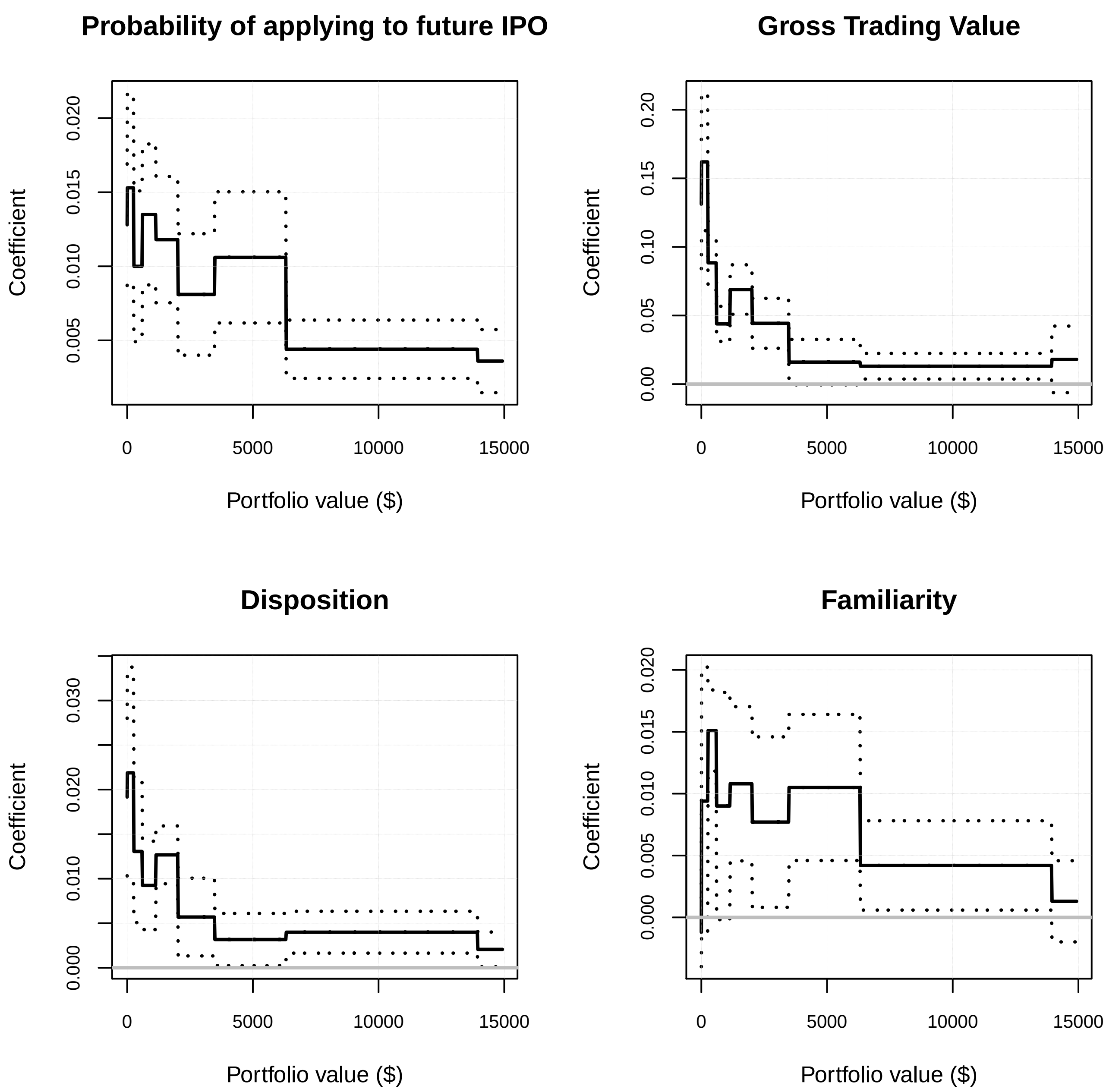


Figure 4: Heterogeneity by Account Age