

# IPO POST-ISSUE MARKETS: QUESTIONABLE PREDILECTIONS BUT DILIGENT LEARNERS?

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*Abstract*—There appear to be no anomalies in the aftermarket of a sample of 4,848 U.S. IPOs over the period 1975 to 1995, except issues offered below \$6. Risk is priced in the aftermarket in accordance with Rubinstein's asset-pricing model. Unlike under the efficient markets hypothesis (EMH), however, market priors about the probability of future default are not unbiased at the IPO date. Still, subsequent learning is rational: the market uses Bayes' law with a correct-likelihood function (of news given the eventual fate of an issue). That is, the hypothesis of an efficiently learning market (ELM) cannot be rejected. We produce direct evidence in support of these statements, based on a new class of tests. We also provide indirect evidence, by documenting a gradual convergence of IPO prices towards EMH as issues mature.

## I. Introduction

EVER since Ritter's seminal empirical study (Ritter, 1991), the post-issue performance of IPOs has been considered to be a puzzle. In the long run, IPOs significantly underperform standard benchmarks or equity in appropriately matched firms. The puzzle has been confirmed in numerous follow-up studies. (See, for example, the Spring 1993 issue of *Financial Management*, Jain and Kini (1994), and Loughran and Ritter, 1995.) The evidence is now generally interpreted as suggesting that the market is too optimistic when pricing young issues. It realizes its mistakes slowly, adjusting prices as the issues mature.

Although some have argued that the biases in the market's prior at the issue date are a natural consequence of shortsale restrictions (Miller, 1977; Morris, undated), it could also be a mere sign of the beliefs at a particular point in time. Indeed, most studies focus on IPOs executed during the 1970 and 1980s. That priors over this period were biased does not necessarily imply irrationality, because the bias was demonstrated to be there only *ex post*, that is, with the benefit of hindsight.

Instead, it seems much more fruitful to ask whether subsequent changes in the market's beliefs were rational. If beliefs can be expressed in terms of the chance numbers of classical probability theory, we know precisely what this means: changes should obey the rules of conditional prob-

ability (Bayes' law).<sup>1</sup> We will also take this to mean that the market knows the likelihood of the signals it receives given the eventual fate of an issue (will it default?). We set out to test this weaker restriction on market beliefs.

Although the market will be assumed to know the likelihood function, we do allow for biases in the market's prior at the IPO date, which means that it need not be confirmed in subsequent realizations. Therefore, we deviate from the standard view that market beliefs are unbiased in all respects (the efficient markets hypothesis (EMH)). But the deviation is minimal: we let the market have biased priors only at the issue date.

Both Bayesian updating and the use of correct-likelihood functions are an integral part of the learning that is assumed under EMH. Therefore, we will refer to our model of market beliefs as the hypothesis of an efficiently learning market (ELM). ELM differs from EMH only in that prior beliefs may be biased.<sup>2</sup>

This article tests ELM in the IPO aftermarket with a methodology that requires little or no information about the actual market prior at the issue date and how priors varied across issues. The methodology was originally developed by Bossaerts (1996, 1998) and successfully applied to experimental winner-take-all markets by Bondarenko and Bossaerts (2000), to digital option prices implied by index call and put option prices by Bondarenko (1997), and to straight index call options by Bossaerts (1998). The applications have one thing in common: they concern securities with a clear bankruptcy state, like the equity contracts studied here.

The specific framework of analysis is the following. At the launch date, it is known that a certain number of IPOs eventually fail (default), but it is not known exactly how many and, if the issue at hand does fail, at what time. For simplicity, the recovery rate conditional on bankruptcy is set equal to zero.

Initial priors about the probability of bankruptcy are arbitrary and may vary across IPOs. Price changes in the

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<sup>1</sup> A similar view is expressed by Lewellen and Shanken (1998).

<sup>2</sup> It may be useful to give a nonfinancial example of the meaning of ELM. Imagine an individual who is dropped one day at some random point on earth where she has never been before. She is asked to predict whether it is going to rain the next day. Of course, because our individual is visiting the place for the first time, she may not be well informed about the unconditional probability that it would rain the next day. She may have been dropped in a beautiful garden in the midst of a desert, and infer incorrectly that it must rain often. Still, her experience with rain elsewhere should allow her to update this (incorrect) prior correctly. She knows that rain is often preceded by a certain type of clouds. These clouds are far less likely when the next day is rain-free. That is, she knows the likelihood of the state of nature (type of clouds) given that it would rain the next day, or given that it would not rain the next day. With this information (state and likelihood), she updates her prior and announces her prediction.

aftermarket reflect rational updating of these priors from news about the fate of the company. The market is supposed to understand how the news relates to bankruptcy. (It knows the likelihood of the signals given the bankruptcy status.) Likewise, the market correctly predicts the expected value of equity in the company conditional on no default. (If default occurs, the market of course knows that this value will be zero.)

The tests that we use to verify this belief model (an example of ELM) are simple and powerful. A novelty is that they require the empiricist to split the available sample in a winner category (companies that did not default) and a loser category (companies that defaulted). Standard returns have to be modified slightly and weighted appropriately. If rational updating is rejected, the sign of the statistic provides information on the nature of the inefficiency: whether the market overreacts or underreacts to new information.

The need to split the sample into winner and loser categories (in this article, we exclusively investigate the former) turns sample-selection bias into a virtue: the results will not suffer from survivorship bias by construction. That is, even if one does not know the exact proportion of winners and losers (perhaps because some histories of losers became unavailable), our tests remain valid.

Our methodology tests for correct updating of priors about the likely default of each company separately. The methodology is of the event-study type: each history is mapped in event time, with a common event time zero (the IPO date); one loses potential information from the knowledge that two histories occurred sequentially in calendar time. This implies, in particular, that our methodology does not investigate whether the default history of companies floated earlier in calendar time were correctly reflected in the priors at the issue date of subsequent IPOs. Our methodology allows there to be such updating (priors can vary arbitrarily across IPO histories), but it does not study its rationality.<sup>3</sup>

In a rigorous and comprehensive way, this article tests the conjecture made by Jegadeesh (1998), namely, that negative aftermarket IPO excess returns should be rationalizable in terms of the relatively negative news that the market received about IPOs. Jegadeesh documented that a large fraction of the aftermarket underperformance can be explained by three-day price changes around earnings announcements. However, he was not able to determine to what extent these price changes were “correct”; moreover, he focused on price reactions to well-identifiable events (earnings announcements). The methodology of this article tests whether price changes following any news event are rational, in the sense that they reflect Bayesian updating with a correct-likelihood function.

<sup>3</sup>Loughran and Ritter have argued that priors at the IPO date have recently gotten more in line with the actual post-issue performance, indicating that priors are being updated between two issue dates. See Loughran and Ritter (1995, p. 49).

Unlike in Ritter (1991), we do not compare post-issue IPO returns with contemporaneous returns on benchmark portfolios (value-weighted or equally weighted market indices, and size-based portfolios) or matched securities (similar market capitalization and industry). Instead, we use an explicit intertemporal asset-pricing model that has been shown to be successful in other contexts, namely, Rubinstein’s model (Rubinstein, 1976). His model uses a pricing kernel that is a simple nonlinear transformation of the return on a value-weighted market index.

Our data set is approximately triple the size of Ritter’s, and covers 4,848 U.S. IPOs in the period 1975 to 1995. Although risk adjustment appears not to be necessary in the earlier part of our sample (covering Ritter’s period of 1975–1984, when the risk premium recorded for U.S. stock markets was historically exceptionally low), it becomes important in the second part.<sup>4</sup>

It is also crucial that a value-weighted market index is used in Rubinstein’s pricing kernel, in full consistency with the model: we will demonstrate that an equally weighted index is misspecified. In particular, as IPOs mature, their prices converge to Rubinstein’s model with a value-weighted portfolio as benchmark. In contrast, the fit of Rubinstein’s model with an equally weighted index does not improve with the age of IPOs.

The remainder of the article is organized as follows. The next section discusses the data and summarizes past evidence. Section III presents our approach and methodology. Section IV discusses the aggregate empirical results. Section V reports tests conditional on issue information. Section VI corroborates the findings by documenting how fast the market learns, and, hence, eliminates biases in the pricing. Section VII concludes.

## II. The Evidence

Our results are based on a merging of Ritter’s and van Bommel’s sample<sup>5</sup> of IPOs in the 1975–1995 period and the CRSP 1998 NYSE/AMEX/NASDAQ monthly return tape. The two data sets were merged on the basis of PERM number, or CUSIP when the PERM number was not available.

The following three IPOs were eliminated.

- All the IPOs for which the *EXCHANGE* variable in Ritter’s dataset is equal to 4, corresponding to non-Nasdaq OTC issues.
- IPOs for which the CRSP variable *SHRCD*, the share code, differs from 10 or 11. These are certificates,

<sup>4</sup>Earlier versions of this article focused on IPOs in the period 1975–1984. Our theory fit the data well without risk adjustment. The period 1985–1995 was added later on and provided a ten-year out-of-sample test. The theory continued to fit remarkably well, but risk adjustment became necessary.

<sup>5</sup>An extensive analysis of Jos van Bommel’s data set can be found in van Bommel (1999).

ADRs, SBI (shares of beneficial interest), units, companies incorporated outside the United States, American primes and scores, closed-end funds, closed-end fund companies incorporated outside the United States, and REITS.

- IPOs of which the first digit of the SIC code equals 6 or 9, corresponding to financial institutions, insurance, savings and loans (6), and utilities (9).

The latter exclusion was decided on because many IPOs in that category were in fact well-established firms that issued stock on the NYSE/AMEX/NASDAQ because of regulatory changes. The nature of these IPOs differs dramatically from that of the typical one, wherein a young company is floated, usually in a new area of industrial activity.<sup>6</sup>

The final sample contained 4,848 IPOs, about triple Ritter's data set. For each IPO, trading and delisting information, as well as a vector of 120 post-issue monthly returns were extracted from the CRSP tape (together with 120 synchronous CRSP equally weighted and value-weighted index returns). This means that ten-year post-issue performance histories for each IPO were available, except of course for the issues from 1989 on, for which only partial histories were available (provided they survived until the end of the return data, namely, December 1998).

A large fraction of these histories are incomplete, due to delisting. The delisting information is important for the tests to be reported later. CRSP provides delisting codes (DC), which we grouped as follows.

- *Active* issues: DC 100
- *Mergers*: DCs 200–203
- *Exchanges*: DC 300–390
- Liquidations and forced delistings: DCs 550–588, 400, and 700 (to be referred to as *Liquidations*)
- Inactive but *Unknown* status: DCs 500–520

Active issues, mergers, and exchanges will be classified in a category that we will refer to as *winner*s: these are the successful IPOs. In contrast, liquidations and unknowns will be categorized as *loser*s.

We recorded the delisting status at the end of each twelve-month period after the issue date. This status is determined as follows. Consider the end of the  $j$ th twelve-month period. If the corresponding point in calendar time is before the recorded delisting date or the issue was never delisted, then DC is set equal to 100 (active issue). Otherwise, it is set equal to the DC in the CRSP tape.

<sup>6</sup> We used the SIC code that CRSP assigned to an issue on the 1998 tape to determine exclusion. This code often deviates from Ritter's, which was constructed to better reflect industry affiliation on the IPO date itself. Hence, we ended up with some companies that were in the excluded industries at the IPO date. Because they were later reclassified by CRSP, or considered to be in a different industry altogether, these companies were deemed to be more typical IPOs, and, hence, retained in our study.

Table 1 provides descriptive information of the distribution of delisting status for each twelve-month period. Obviously, the number of active issues decreases as time passes. Only 1,037 issues have ten-year active trading histories. This does not mean that the other issues went sour: 1,035 IPOs were merged at one point in the ten-year history, and 63 disappeared through an exchange of stock.

Still, 1,237 IPOs were listed as liquidations at the ten-year mark. In other words, the probability of failure is rather high (35.0% of the sample). Many of the 165 unknowns should also be considered losers. So, roughly three out of ten IPOs will not be a success.

This rather high failure rate makes all the more dramatic Ritter's observation that the post-issue performance of IPOs has been dismal. His cumulative average return, however, went out to only three years after the issue date, and one wonders what happened further. After three years, about one-third of the failures (losers) had left the sample, and, hence, do not affect the average returns anymore. On the other hand, many mergers and exchanges had also occurred prior to the three-year mark. A large fraction of these generated substantial abnormal returns, which will not affect the IPO post-issue performance measure after the merger or exchange.

The evidence of underperformance does not disappear when extending the horizon beyond Ritter's three-year window. Figure 1 displays the cumulative average return (CAR) in excess of the return on the equally weighted and value-weighted CRSP indices up to ten years after the issue date. Only after approximately 72 months (six years) is there any tendency to reversal.

Figure 1 confirms Ritter's finding: the underperformance of IPOs in the period 1975–1995 was pronounced. For instance, at sixty months (five years), the CAR relative to the value-weighted index is –40%, which means that an investment in the average IPO would have generated a level of wealth after sixty months that is 40% below that of an investment in the value-weighted CRSP index.

The fact that the CAR plot with value-weighted benchmark levels off after 72 months (six years) may indicate that the remaining (2,087) IPOs have finally become "established firms," which would mean that the market has learned how to price those issues correctly. In fact, this is an explanation in terms of learning, very much in the spirit of this article. We will confirm this point later.

Although we do not want to jump ahead too fast, it may be worthwhile to point out here that we will be able to explain the underperformance anomaly with a value-weighted benchmark. This is significant, because the CAR plot is worst for precisely this benchmark. That attempts to use an equally weighted benchmark fail can already be seen from figure 1: the CAR increases after sixty months, implying that the market does not correctly price IPOs even after they have become mature (relative to an equally weighted benchmark). We will demonstrate this explicitly at

TABLE 1.—DISTRIBUTION OF IPO STATUS AT VARIOUS POINTS UP TO TEN YEARS AFTER ISSUE DATE; BASED ON 4,848 U.S. IPOs IN THE PERIOD 1975–1995; DELISTING STATUS AS RECORDED ON THE 1998 CRSP TAPE

Reference Point	Active <sup>a</sup>	Mergers <sup>b</sup>	Exchanges <sup>c</sup>	Liquidations <sup>d</sup>	Unknown <sup>e</sup>
1 Year	4,780 (98.6) <sup>f</sup>	22 (0.5)	3 (0.1)	29 (0.6)	14 (0.3)
2 Years	4,388 (90.5)	179 (3.7)	13 (0.3)	205 (4.2)	63 (1.3)
3 Years	3,932 (81.1)	360 (7.4)	22 (0.5)	419 (8.6)	115 (2.4)
4 Years <sup>g</sup>	3,260 (71.3)	529 (11.6)	33 (0.7)	609 (13.3)	139 (3.0)
5 Years <sup>h</sup>	2,649 (61.4)	675 (15.6)	45 (1.0)	794 (18.4)	153 (3.5)
6 Years <sup>i</sup>	2,087 (51.9)	785 (19.5)	54 (1.3)	941 (23.4)	158 (3.9)
7 Years <sup>j</sup>	1,675 (43.9)	865 (22.7)	60 (1.6)	1,058 (27.7)	160 (4.2)
8 Years <sup>k</sup>	1,396 (38.1)	924 (25.2)	60 (1.6)	1,125 (30.7)	163 (4.4)
9 Years <sup>l</sup>	1,204 (33.5)	979 (27.2)	61 (1.7)	1,192 (33.1)	163 (4.5)
10 Years <sup>m</sup>	1,037 (29.3)	1,035 (29.3)	63 (1.8)	1,237 (35.0)	165 (4.7)

<sup>a</sup> CRSP delisting code (DC) 100.

<sup>b</sup> DCs 200–203.

<sup>c</sup> DCs 300–390.

<sup>d</sup> Includes forced delistings; DCs 550–588, 400, and 700.

<sup>e</sup> Includes inactive issues; DCs 500–520.

<sup>f</sup> Percent of total.

<sup>g</sup> From year 4 on, the sample excludes 278 IPOs floated in 1995.

<sup>h</sup> From year 5 on, the sample excludes 254 IPOs floated in 1994 as well.

<sup>i</sup> From year 6 on, the sample excludes 291 IPOs floated in 1993 as well.

<sup>j</sup> From year 7 on, the sample excludes 207 IPOs floated in 1992 as well.

<sup>k</sup> From year 8 on, the sample excludes 150 IPOs floated in 1991 as well.

<sup>l</sup> From year 9 on, the sample excludes 69 IPOs floated in 1990 as well.

<sup>m</sup> From year 10 on, the sample excludes 62 IPOs floated in 1989 as well.

the end of the article. Of course, asset-pricing theory requires one to use a value-weighted index and not an equally weighted index. In other words, the finding that the equally weighted index does not work provides further support of asset-pricing theory.

The motivation for this article is that the plot in figure 1 does not necessarily convey much evidence of an anomaly unless one is willing to endow the market with a questionable level of forecasting accuracy. Here is why.

Figure 1 is meant to display the performance of a portfolio of IPOs at different horizons. The composition of this portfolio changes substantially over time. Table 1 documents the complexity of the dynamics of the composition: firms continuously exit, because of success (mergers and exchanges) or failure (liquidations and unknowns).<sup>7</sup> In previous studies, “correct” pricing of this portfolio is interpreted as implying that the cumulative excess return should be zero on average, independent of the horizon. But this would require a high amount of foresight in the mind of the market. Not only must the market know beforehand what proportion of IPOs will exit (for example, that approximately 35% will eventually exit because of liquidations (see table 1)), but also how the exit is distributed over time. For instance, the

<sup>7</sup>Hensler, Rutherford, and Springer (1997) use an accelerated failure time model to explain the complex dynamics of IPO survival in the aftermarket.

market must have forecasted correctly that the number of exits because of liquidations peaks at four years (190), and that the maximum number of exits because of mergers occurs earlier, at two years (181).<sup>8</sup>

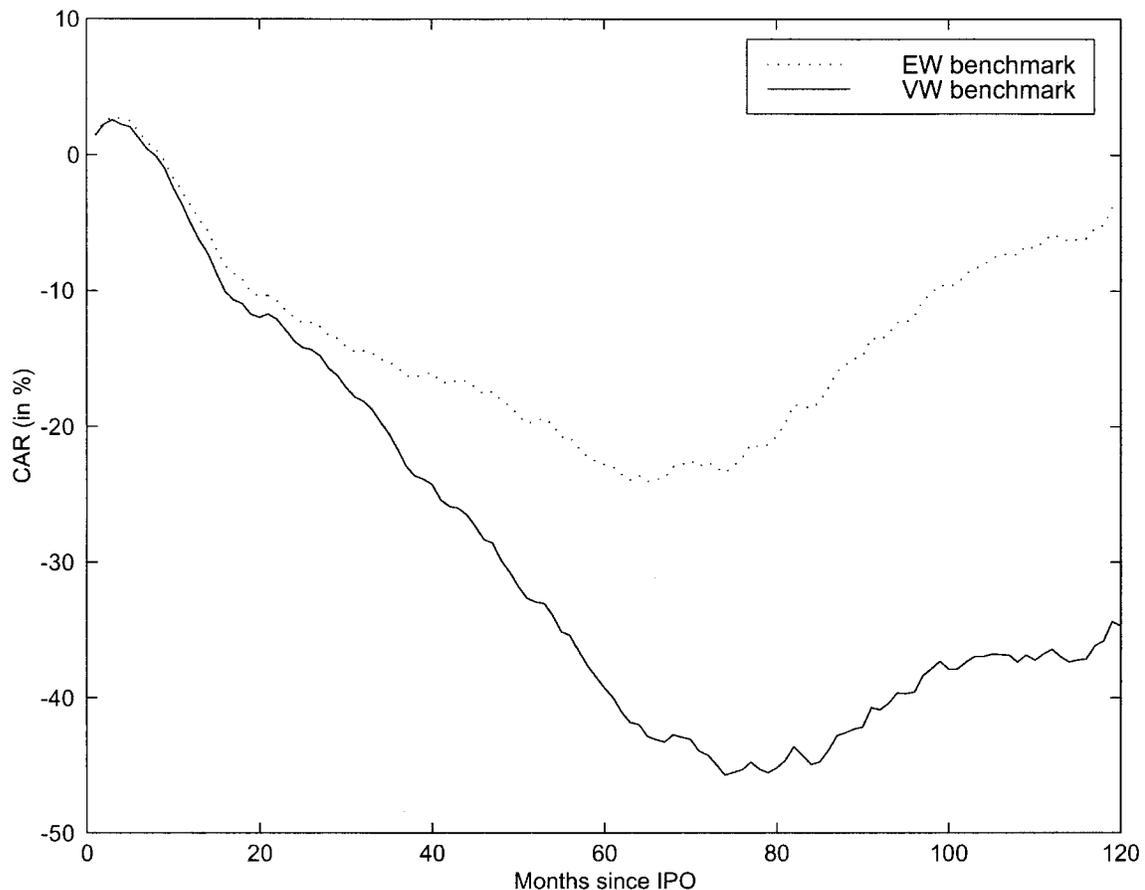
Unless the empiricist believes that the market correctly predicts not only the number and nature of exits out of the portfolio, but also their distribution over time, anomalous behavior in plots like figure 1 can be expected. The result could merely reflect surprises about the actual patterns of exit from the portfolio. But this would mean that the market’s priors about default were initially biased. This conjecture is precisely what our article studies.

Ritter also studied the aftermarket performance conditional on issue information, such as level of IPO underpricing. Our study continues the tradition. In particular, post-issue return histories are stratified according to the following criteria.

- *Underpricing*: Issues are sorted by size of underpricing (difference between first aftermarket price and IPO offer price), and then arranged in ten deciles, num-

<sup>8</sup>Of course, table 1 provides only estimates, but the sample size reduces the error to a minimum. For instance, with a sample of 4,848 observations, the standard error on the estimate of the probability of liquidation in the first four years, 13.3%, is  $\sqrt{0.133(1 - 0.133)/4848}$ , (that is, 0.5%).

FIGURE 1.—CUMULATIVE AVERAGE RETURNS IN EXCESS OF THE RETURNS ON (i) THE EQUALLY WEIGHTED (EW) CRSP INDEX, (ii) THE VALUE WEIGHTED (VW) CRSP INDEX, UP TO 120 MONTHS (TEN YEARS) AFTER ISSUE DATE



Based on 4,848 U.S. IPOs in the period 1975–1995.

bered 1 to 10, with category 10 containing the most-underpriced issues, (that is, those with the highest aftermarket price relative to offer price).

- *Industry classification:* Issues are arranged in classes depending on the first digit of their three-digit SIC code at IPO date.
- *Price level:* Issues are sorted by offer price, and then arranged in ten deciles, numbered 1 to 10, with category 10 containing the most-expensive issues.

With the underpricing categorization, one can investigate whether the IPO anomaly is specific to extremely underpriced issues. Likewise, the industry-classification and price-level categorizations enable one to relate aftermarket underperformance to industry and offer price, respectively, or to study whether the anomaly is specific to early issues in an industry.

### III. Theory

#### A. *Mere Optimism or Fads?*

Ritter's findings have been interpreted as evidence of optimism, or even fads. The term *fads* reflects something

irrational: the market overprices the issues and stubbornly holds on to its beliefs, until finally correcting in the face of evidence of failures. But optimism is not necessarily irrational. It may merely be a characterization of someone's beliefs at a point in time. Optimism becomes irrational only if it is not corrected (updated) properly as contrary evidence emerges. So, optimism is not the problem; instead, the obstinacy is. The aim of this article is to investigate whether IPO underperformance is merely a consequence of optimistic beliefs at the issue date of the IPOs, or whether it also reflects obstinacy (or its opposite, overexuberance). The latter would be evidence of irrationality.

The article does so in the context of a framework that allows the empiricist to be agnostic of the actual priors of the market at the issue date and to avoid specification of the payoff-generating process beyond the proposition that there is a bankruptcy state in which the issue becomes worthless.

The methodology was originally developed by Bossaerts (1996, 1998) and will be summarized here in terms of the IPO application. The framework is presented first; the tests are discussed next.

### B. Framework

Let  $t$  index (event) time, measured in, say, months ( $t = 0, 1, 2, \dots$ ). We want to test rational learning in a period up to  $T$  months after the issue date, based on the status of the IPO at some future reference point  $T^* > T$ .  $T$  is either a fixed point in event time, such as eighteen months after the issue date, or it is the date of the last available price if delisting occurred prior to this fixed point.

Assuming that no dividends are paid or that dividends are continuously reinvested, the price at  $t$ ,  $p_t$ , predicts the value of equity (with dividends reinvested) in the future. Let  $V_{T^*}$  denote the value of equity at the future reference point  $T^*$ . Let  $\phi_t$  denote the market's information at time  $t$ .

Assuming that an infinite-lived representative investor exists, holding logarithmic preferences, Rubinstein showed that equilibrium prices should satisfy the following stochastic Euler equation (Rubinstein, 1976):

$$E^s \left[ \frac{1}{R_{M,t+1}} p_{t+1} \middle| \phi_t \right] = p_t, \quad (1)$$

where  $R_{M,t+1}$  denotes the return on the (value-weighted) market portfolio over the period  $(t, t + 1)$ . The conditional expectation in this expression is computed on the basis of the market's subjective beliefs (whence the superscript  $s$ ). This subjective expectation need not coincide with the "true" probability, which should be interpreted as the one that can be estimated from the actual frequency of future events. It will be specified shortly to what extent the market's beliefs may be incorrect.

Because equation (1) holds for all  $t$ , it is more convenient to work with scaled prices, defined as follows:

$$\tilde{p}_0 = p_0,$$

$$\tilde{p}_t = \frac{1}{R_{M,t} R_{M,t-1} \dots R_{M,1}} p_t, \quad (t = 1, \dots, T^* - 1), \quad (2)$$

$$\tilde{p}_{T^*} (= \tilde{V}_{T^*}) = \frac{1}{R_{M,T^*} R_{M,T^*-1} \dots R_{M,1}} V_{T^*}.$$

The stochastic Euler equation in (1) then becomes

$$E^s [\tilde{p}_{t+1} | \phi_t] = \tilde{p}_t, \quad (3)$$

and recursion generates the following pricing equation:

$$\tilde{p}_t = E^s [\tilde{V}_{T^*} | \phi_t], \quad (4)$$

as if investors were risk neutral and had a zero discount rate.<sup>9</sup>

<sup>9</sup> The informed reader will recognize that we implemented the deflation procedure that has become standard in mathematical finance.

By time  $T^*$ , the issue may have defaulted, in which case the recovery rate is assumed to be zero, and, hence,  $\tilde{V}_{T^*} = 0$ .<sup>10</sup> Absent bankruptcy,  $\tilde{V}_{T^*} > 0$ . The market may hold incorrect beliefs about the frequency of occurrence of bankruptcy. Conditional on no default, however, the market's beliefs about the (scaled) value  $\tilde{V}_{T^*}$  are correct.

The actual biases in the market's prior about bankruptcy are left unspecified. They can be arbitrary. Moreover, they can vary across issues. We do require, however, that the market learn rationally about the potential bankruptcy of individual issues, using its information  $\phi_t$ . In other words, the market uses the rules of conditional probability (Bayes' law) to update its beliefs, based on the correct likelihood of signals given the final outcome (default or not).

It was mentioned that the position of the reference date  $T^*$  is arbitrary: any date after  $T$  would do. It must lie after  $T$ , however ( $T^* > T$ ), because we cannot allow for perfect revelation that the company defaulted during the period for which returns are computed ( $t = 1, \dots, T$ ). Our methodology will separate winners (firms that have not defaulted by  $T^*$ ) from losers (those that defaulted by  $T^*$ ). Firms that announce default on or before  $T$  will record a return of  $-100\%$  (given our assumption of a zero recovery rate). Hence, conditional on default, returns are predictable. We will also use what we refer to as modified returns, which are computed with the end-of-period price as basis. Upon default, the price (even when scaled as in equation (2)) drops to zero, and, hence, the modified return will be unbounded.<sup>11</sup>

In our IPO data set, the recovery rate of IPOs is not always zero. We only use this assumption because it simplifies matters substantially. Moreover, recent evidence suggests that the recovery rate on delisted firms is far below what CRSP has traditionally been reporting. See Shumway (1997).

Likewise, some IPOs in our data set may have been known to have defaulted prior to  $T$ . We leave a six-month period between  $T$  and  $T^*$  to mitigate problems caused by early revelation of default.<sup>12</sup> Given how clean our results are, the reader will be able to confirm that our empirical results appear not to have been affected much by early (potential) revelation.

In our world, it will no longer be true that actual changes in prices (properly scaled) will be unpredictable, even if ex ante expected changes are. Mathematically,

<sup>10</sup> It is possible to accommodate a nonzero recovery value. It is necessary, however, that the recovery value be fixed. The test results that are reported in this article should be adjusted slightly if the recovery value is argued to be nonzero.

<sup>11</sup> See Bossaerts (1996) for an in-depth analysis of the problems caused by early revelation of default.

<sup>12</sup> Delisting announcements of distressed firms invariably occur after the last available price on the CRSP tape. The end of the time series from which returns are computed,  $T$ , must necessarily be based on available price data. Hence,  $T$  cannot be beyond the point of the last recorded price on the CRSP tape and therefore occurs before potential delisting. Nevertheless, default could still be announced way before the delisting date, potentially invalidating our tests.

$$E\left[\frac{\tilde{p}_{t+1} - \tilde{p}_t}{\tilde{p}_t} \mid \phi_t\right]$$

may be nonzero. In contrast with equation (3), the conditional expectation is now computed from the actual (true) distribution of price changes over month  $t + 1$  across issues. (The absence of the superscript  $s$  should make the distinction clear.) Defining the return to be

$$\tilde{r}_{t+1} = \frac{\tilde{p}_{t+1} - \tilde{p}_t}{\tilde{p}_t},$$

this can be translated to mean that the return may be nonzero on average. Mathematically, the following need not hold:

$$E[\tilde{r}_{t+1} \mid \phi_t] = 0. \tag{5}$$

This restriction only obtains under EMH, that is, when the market holds unbiased priors. Existing studies of IPOs have focused on testing equation (5) (albeit based on other risk-adjustment models, not necessarily Rubinstein’s). The tests reject.

### C. Methodology

We allow beliefs about the probability of bankruptcy to be arbitrary, and even to vary across issues. Moreover, we remain agnostic about the processes generating payoffs and information, although we do require that the market use the correct-likelihood function in updating its beliefs. In such a context, one would reasonably conjecture that rational learning does not impose falsifiable restrictions. In other words, any set of price histories could be explainable in terms of some set of biases in the market’s prior and some payoff- and information-generating processes.

Bossaerts (1996, 1998) proves that this conjecture is wrong. Learning does restrict the dynamic behavior of prices, even in this fairly generic environment. The restrictions are not apparent from a study of returns as in past studies of IPOs.

Foremost, the restrictions emerge only after splitting the sample in two subsamples, one of histories of winners (issues that did not default by  $T^*$ ) and those of losers (issues that defaulted by  $T^*$ ).

Further, the selection bias introduced by studying the subsamples of winners and losers separately implies that returns will not necessarily be equal to zero, even if priors happen to be correct. Indeed, returns on winners, for instance, can be expected to be positive on average. As a consequence, performance measures that differ from the traditional return will have to be studied.

Here are the main restrictions. To simplify the discussion, we will discuss only restrictions involving winners.<sup>13</sup>

First, define the modified return:

$$\tilde{x}_{t+1} = \frac{\tilde{p}_{t+1} - \tilde{p}_t}{\tilde{p}_{t+1}}. \tag{6}$$

The modified return differs from the traditional return in that the future (scaled) price is used as basis.

For  $W$  issues that did not default before  $T^*$  (indexed  $i = 1, \dots, W$ ), compute the average modified return:

$$\frac{1}{W} \sum_{i=1}^W \left( \frac{1}{T-1} \sum_{t=1}^{T-1} \tilde{x}_{i,t+1} \right). \tag{7}$$

**Fact 1:** *For winners, the expected average modified return is nonpositive. If the scaled payoff upon winning is correlated with information about default status, the expected average modified return is strictly negative. Otherwise, it is zero.*

To build some intuition as to why fact 1 is true, consider the average traditional return. If this is computed on the basis of histories of winners only, one expects a positive bias, at least if the market correctly reacted to news that the issues were going to be winners. To offset this bias, positive returns should be multiplied by a factor smaller than 1, and negative returns should be multiplied by a factor larger than 1. The ratio  $\tilde{p}_t/\tilde{p}_{t+1}$  is such a factor. Multiplying the traditional return with this factor produces the modified return. The factor more than offsets the bias: the resulting variable, namely, the modified return, will be nonpositive on average. It will be strictly negative if the payoff is correlated with news about eventual success (for example, signals that indicate a reduction in the probability of default also imply a higher payout conditional on no default).

Fact 1 is an implication of ELM. In particular, the strict negativity of the modified return obtains not just because the market reacted favorably to news that the issues at hand eventually became winners, but precisely because this reaction was correct, that is, in accordance with Bayes’ law, and on the basis of the correct likelihood.

It deserves emphasis that market priors about the likely default of the issue may be arbitrary; in particular, they need not be correct, and they can vary across issues. Intuition for why the prior does not matter can be obtained indirectly, from observing that fact 1 is based only on the winners: no information about the losers (timing, numbers) is involved. To test whether the market’s prior is unbiased, one would need to know at least how many losers there were. Because fact 1 does not use this information, it must be testing something that is independent of any bias in the prior. Indeed, it does: it merely tests whether the updating is correct (use of Bayes’ law and a correct-likelihood function).

A specific weighting scheme makes the average modified return zero. Let  $\tilde{V}_{i,T^*}$  denote the time- $T^*$  scaled value on

<sup>13</sup> Previous versions of this article discussed restrictions on losers as well; see also Bossaerts (1996, 1998).

winner issue  $i$  ( $i = 1, \dots, W$ ). Consider the following weighted average modified return:

$$\frac{1}{W} \sum_{i=1}^W \tilde{V}_{i,T^*} \left( \frac{1}{T-1} \sum_{t=1}^{T-1} \tilde{x}_{i,t+1} \right). \quad (8)$$

**Fact 2:** *For winners, the expected weighted average modified return is zero.*

The intuition is as follows. The weighting scheme gives more weight to issues with a large payoff at  $T^*$ . These are the issues that must also have had the larger price run-up (that is, higher (modified) returns) in anticipation of the larger payoff. Indeed, we assume that market prices correctly incorporate the generally favorable information. Issues with lower  $T^*$  values have had lower modified returns, but are weighted less in the weighted average modified return. Altogether, the weighted average modified return is expected to be zero.

Again, it should be underscored that fact 2 obtains irrespective of the market's prior about default: it may be biased, and it may vary across issues.

Some may find fact 1 to be redundant, in view of the cleaner restriction in fact 2. Fact 2, however, uses a weighting that may affect the power of the test, depending on the data at hand. We will discuss the issue of power shortly.

The two facts were expressed in terms of unconditional averages. They also obtain if conditioning on issue information, such as IPO underpricing or IPO offer price.

**Fact 3:** *Facts 1 and 2 continue to hold when the averages are computed for subclasses of winning IPOs formed on the basis of issue information.*

If the averages are not computed from the first aftermarket trade on, but from some future initial date  $\tau < T$ , facts 1 and 2 also hold when conditioning on information that emerged after the issue date, up to time  $\tau$ .

The three facts can easily be tested, because they restrict the behavior of readily available statistics, based on simple price information. We chose not to use absolute price information, but wealth ratios based on an investment at IPO of \$1, with reinvestment of possible dividends. That way, the future values  $\tilde{V}_{i,T^*}$  become normalized, reducing their cross-sectional variation, and improving the power of tests of fact 2.

#### D. Meaning of Rejections

Before turning to the results, it is important to delineate the information that would be conveyed by rejections of the facts.

Most information could be conveyed by tests of fact 2, because, unlike fact 1, the alternative hypothesis would include two cases: positive and negative weighted average modified returns. Here is an interpretation in terms of a

market whose price reaction may be correct in the long run, but either too slow or too fast in the short run.

Remember the intuition behind facts 1 and 2: the traditional return on winners is positive, because of the selection bias and the market's learning; to offset this, returns must be multiplied by a factor that is below 1 for positive returns and above 1 for negative returns; if the market learns rationally, the factor that does the job is the ratio of today's price over tomorrow's (deflated) price; it converts the standard return into the modified return.

Positive weighted average modified returns are a sign of underreaction of the market to new information. In other words, price changes amount only to a fraction of the full Bayesian update; further adjustments are made later. It implies that price movements are less extreme than in the rational case, making the factor that transforms standard returns into modified returns less variable. As a result, the bias caused by looking only at winners is not entirely offset.

Negative weighted average modified returns are a sign of overreaction of the market to new information. Price changes overcompensate for the Bayesian update, creating the necessity for future reversals. It implies that price movements are more volatile than in the rational case, making the factor that transforms traditional returns into modified returns more variable. Hence, the selection bias is overadjusted.

In classical tests of EMH, return autocorrelations convey signals of overreaction and underreaction of the market to new information. Under ELM, priors may be biased, and return autocorrelations do not provide the right signal. Moreover, autocorrelations only provide information about the behavior of deviations of returns from the mean; they do not test the appropriateness of the mean return. In contrast, the weighted average modified return of winners must be zero even if priors are biased (fact 2). Moreover, it conveys information about the correctness of both price changes net of the average change and the average change itself.

Likewise, in standard tests of EMH, significant projections of returns on past information (Teoh, Welch, & Wong, 1998) provide evidence against the theory. Under ELM, such projections do not convey conclusive evidence. Only projections of weighted modified returns on past information (such as issue information) do.<sup>14</sup>

#### E. Cross-Sectional Independence

Our tests of the facts will be based on the averages in equation (7) and (8). The validity of the test statistics (simple  $z$ -statistics, asymptotically normally distributed) relies on cross-sectional independence. That is, we assume

<sup>14</sup>In the appendix of earlier versions of this article (the interested reader can obtain a copy at <ftp://hss.caltech.edu/pub/pbs/ipool.pdf>), we constructed a simple model of an overreacting or underreacting market. The construction was the basis for a Monte Carlo analysis of the power of tests based on fact 2. It revealed that autocorrelations may be low even in a substantially irrational market. In contrast, tests based on fact 2 had power.

that modified returns (in equation (7)) and weighted modified returns (in equation (8)) are independent across the winners used to compute the averages. However, to the extent that there is clustering of IPOs in time, this assumption may not be correct.

Although there are some peak periods for IPO activity, our sample covers twenty years, which diminishes the potential effect of clusters. Still, at one point we will stratify our sample according to industry affiliation. IPOs within an industry are notoriously clustered in time (for example, oil exploration IPOs in our sample almost invariably occurred in the late 1970s), potentially invalidating our independence assumption. Therefore, the results based on industry stratification must be interpreted with caution.

Because we scale prices using the return on the market portfolio before computing returns, we effectively do eliminate correlation caused by marketwide price movements. In traditional event-study analysis, the analogous technique would be to use market model residuals.

Clustering biases our tests against the null hypothesis. If IPOs are clustered in time, and there is still cross-sectional dependence after elimination of the market component, we expect average returns (whether weighted, modified, or not) to be significant.

Cross-sectional dependence caused by clustering in calendar time is of course a well-known problem in event studies. Notice, however, that we need cross-sectional dependence in returns (after correcting for the market return) only for winners. In previous studies of IPO aftermarket performance (which were also event studies), independence had to hold across winners and losers, because both types of IPOs were used in the tests. This means, among others, that there could not be any cross-sectional dependence in default. Yet, it is well known that default occurrence is correlated. If one company defaults, it is more likely that another one defaults, and so forth. Because we use only winners in our tests, the inference is immune to correlation in defaults. We really only need independence of returns (adjusted for the market return) conditional on being a winner.

#### IV. Tests Based on the Performance of All the Winners

We first report results from unconditional tests (facts 1 and 2). Figure 2 provides graphical evidence. The plots provide point estimates (horizontal dashes) and 95%-confidence intervals (vertical line segments).

The figure reports averages for different post-issue reference points ( $T^*$  in the previous section), spaced at one-year intervals. For each reference date, averages were computed using return data from the first aftermarket trade up to six months before the reference date at the latest. (In terms of the notation of the previous section,  $T$  is positioned up to six months before  $T^*$ .) IPOs were followed up to ten years after

issue date; hence, there were ten reference points, at the end of years 1, 2, . . . , 10.<sup>15</sup>

The averages displayed in figure 2 are computed on the basis of overlapping periods (the averages for the period ending after two years are based on observations from the first year as well). Hence, the results are not independent across reference points. This continues a tradition of the event-study literature in general and the empirical IPO literature in particular.<sup>16</sup>

In addition to the average modified return, the average (traditional) return and the weighted average modified return, figure 2 shows results for the weighted average traditional return. This statistic is computed from equation (8) after substituting the traditional return  $\tilde{r}_{i,t+1}$  for the modified return  $\tilde{x}_{i,t+1}$ . It gives a good benchmark to gauge the power of the test. (Of course, the presence of rejections in itself will confirm power.) We expect the average traditional returns, weighted or not, to be strictly positive, because of the bias from winner selection. The power of the tests may be questioned, however, if the weighted average return is seldom found to be significantly positive. Our reporting of the (equally weighted) average traditional return in figure 5 plays the same role: one expects it to be significantly positive; if not, there is perhaps too much noise in the data for our tests to be powerful.

Altogether, the plots in figure 2 are meant to provide evidence about the hypothesis of ELM, in conjunction with the asset-pricing model used to adjust for risk (Rubinstein's model).

First, the upper-left plot in figure 2 provides overwhelming support for fact 1: we always reject that the average modified return is nonnegative. Likewise, the restriction of fact 2, that the weighted average modified return ought to be zero, cannot be rejected, except with a horizon of one year, where it is significantly positive. See the lower-left plot of figure 2.

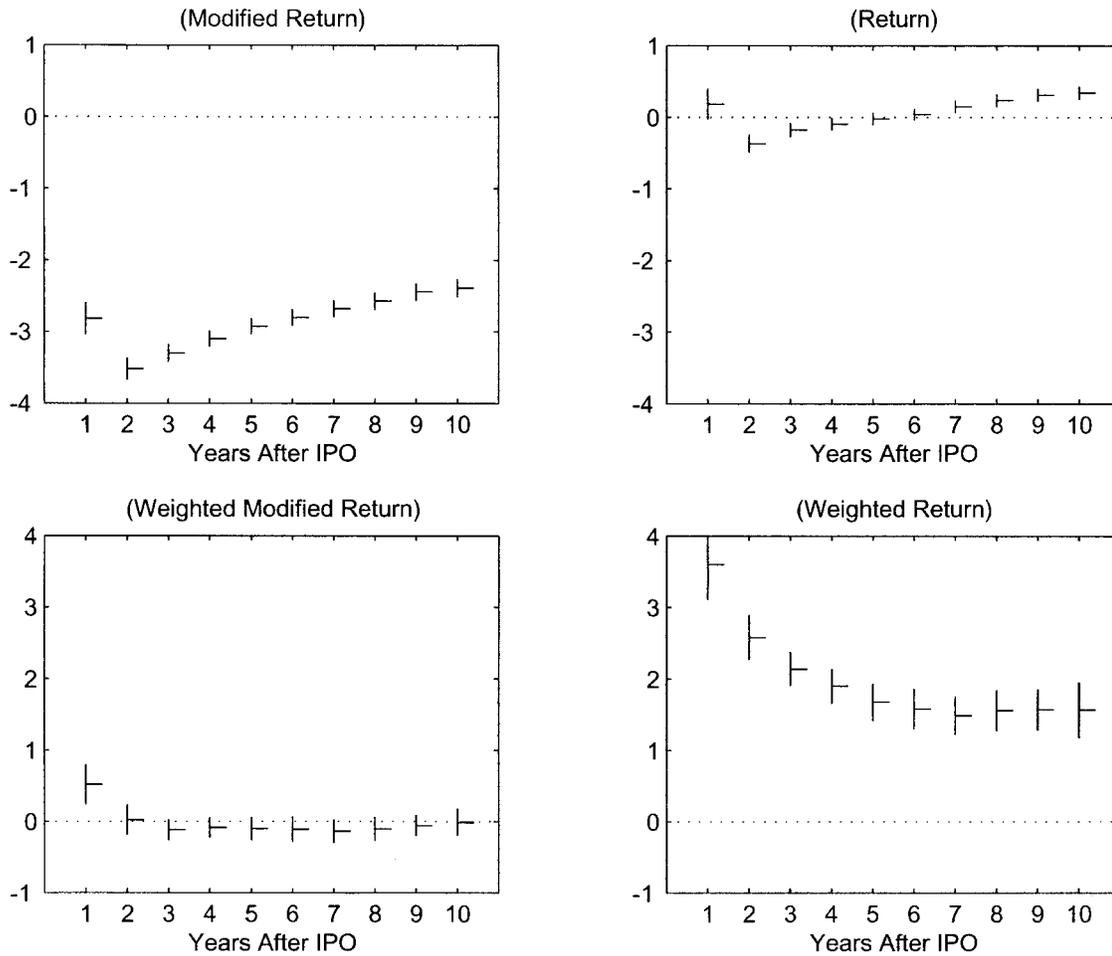
The insignificance of the weighted average modified returns cannot be attributed to the variability of the weights: the lower-right plot of figure 2 demonstrates that the weighted average standard returns are always highly significant. The latter are positive, because of the selection bias: the averages are computed on the basis of winners only.

But the selection bias is not severe enough for the equally weighted average standard return to be significantly positive except for horizons beyond six years. See the upper-right plot of figure 2. The reason is simple: the average performance of U.S. IPOs, whether winner or loser, has been

<sup>15</sup> Reference point values ( $\tilde{V}_{i,T^*}$  in the previous section) for issues that merged or were exchanged (mergers and exchanges) were computed by risk-free reinvesting (at the three-month T-bill rate) all the wealth accumulated up to the date of the last available return on the CRSP tape, and scaling in accordance with equation (2).

<sup>16</sup> In principle, one could correct standard errors for the overlap by jointly modifying the statistics in such a way that a functional central limit theorem can be applied.

FIGURE 2.—ONE-MONTH RISK-ADJUSTED RETURNS (HORIZONTAL BARS) AND 95%-CONFIDENCE INTERVALS (VERTICAL BARS) ON 4,848 U.S. IPOs IN THE PERIOD 1975–1995, FOR DIFFERENT HORIZONS, WINNERS ONLY



Risk adjustment is based on Rubinstein's model, using the value-weighted CRSP index.

so bad (see figure 1) that even selection bias hardly annihilates it.

Based on the evidence from the unconditional tests, it is fair to conclude that the post-issue price series do not reflect any irrationality. Priors may have been biased, but this did not keep the market from correctly learning about future values, in accordance with ELM. Simultaneously, the market appears to price IPOs as in Rubinstein's model of asset pricing. That is, prices are set as if there exists a representative investor with logarithmic preferences.

**V. Conditioning On Issue Information**

We now report results on price histories sampled on the basis of information from the IPO itself. This should verify fact 3. We will focus on the weighted average modified return, which is the most convenient and cleanest statistic to gauge the validity of ELM in the aftermarket.

*A. Stratification by Industry*

Let us first split the sample of winners by industry. Table 2 lists the nine industries with their (two-digit SIC) code.

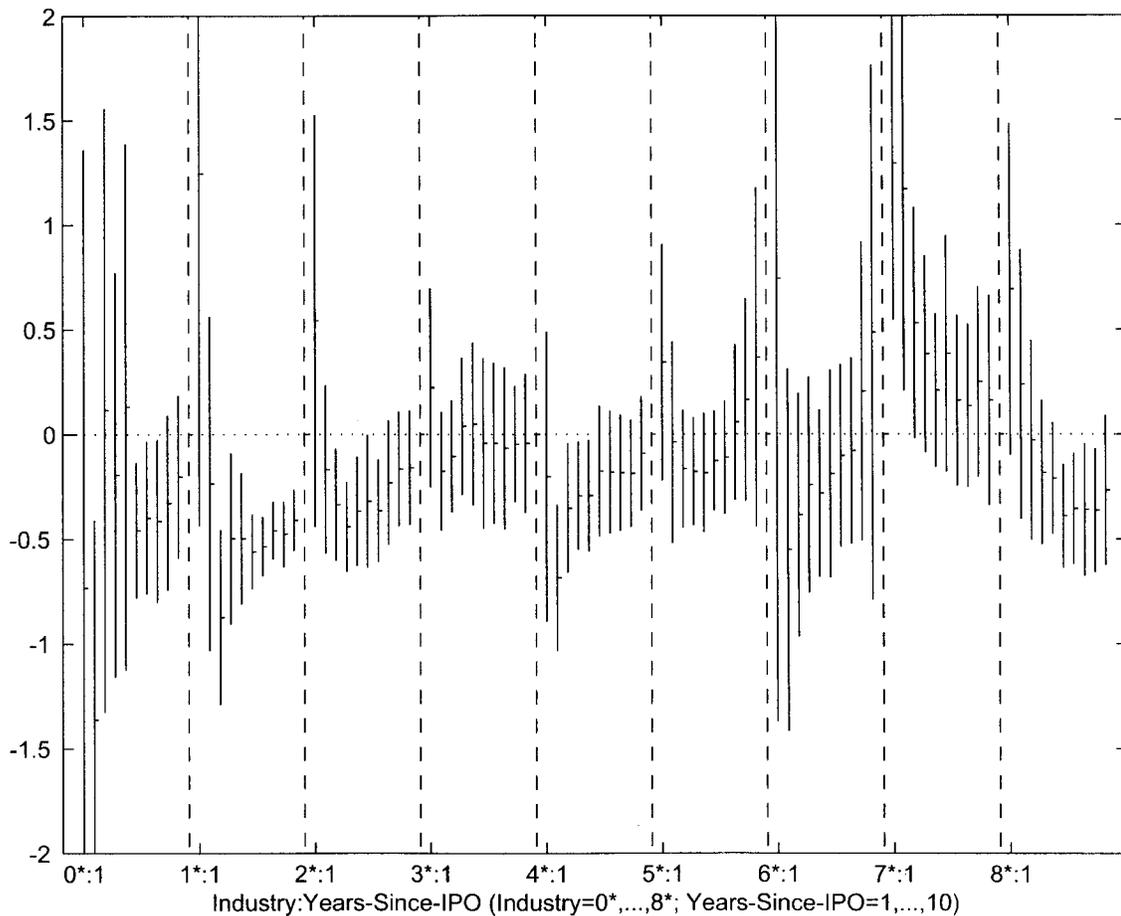
The reader should be warned beforehand that stratification per industry may introduce serious cross-sectional dependence, to the extent that the fate of IPOs within an industry is highly correlated. Hence, the results should be interpreted with caution, and one may want to rely more on stratification that has less tendency to generate cross-sectional dependence.

Figure 3 replicates the lower-left plot of figure 2 after splitting the sample into industry-based subsamples. There are anomalies (rejections of the null that the weighted average modified return equals zero), but the pattern is not

TABLE 2.—INDUSTRY CLASSIFICATION CODES (SIC)

SIC	Description
0*	Agriculture and forestry
1*	Natural resources and construction
2*	Pharmaceutical, chemical, food, wood, and textile products
3*	Industrial and consumer durable goods
4*	Transportation, communication, and energy
5*	Wholesale and retail
6*	Finance, insurance, and real estate
7*	Services
8*	Health care, accounting, and legal services

FIGURE 3.—ONE-MONTH RISK-ADJUSTED RETURNS IN PERCENT (HORIZONTAL BARS) AND 95%-CONFIDENCE INTERVALS (VERTICAL BARS) ON 4,848 U.S. IPOs IN THE PERIOD 1975–1995, FOR DIFFERENT HORIZONS, STRATIFIED BY INDUSTRY (FIRST DIGIT OF SIC CODE), WINNERS ONLY



Risk adjustment is based on Rubinstein's model, using the value-weighted CRSP index.

clear. We do expect to see a certain number of rejections, but there are a rather large number of them in industries 1\* and 2\*. The former includes a fair number of oil and gas exploration issues from the late 1970s that went sour in the early 1980s, after a dramatic drop in crude-oil prices. The latter includes a large number of biotech issues from the late 1980s that also fared poorly.

In fact, the anomalies in industries 0\*, 1\*, and 8\* turn out to be price-level anomalies in disguise. That is, the anomalies that will be discussed next and that are related to the issue price level are far more systematic (and presumably also less affected by potential cross-sectional dependence). Because industry and issue price level are correlated, the price-level anomalies show up scattered across industries. We will document the correlation between price level and industry later on.

#### B. Stratification by Issue Price Level

We split the sample of winners according to issue price level. Decile 1 includes the IPOs with the 10% lowest price, decile 2 includes IPOs in the next 10% pricing category, and so on. Figure 4 displays the results.

There is a very clear pattern in the weighted average modified returns in figure 4: they are highly significantly negative for deciles 1, 2, and 3, for virtually all horizons. These include all IPOs with an issue price below \$6. Beyond \$6, there is no systematic anomaly. The occasional rejections could be expected by chance.

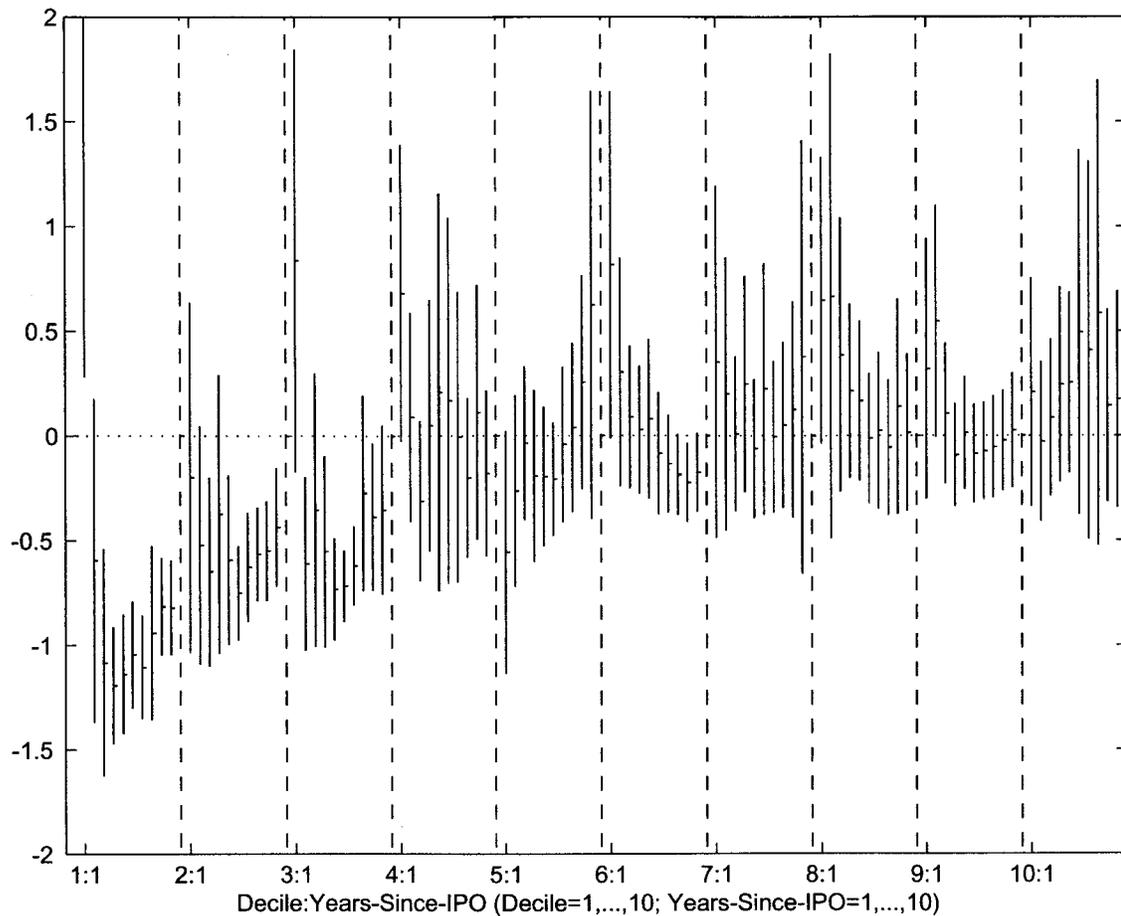
As explained before, significantly negative weighted average modified returns are a sign of overreaction: the volatility is much higher than expected given the market's prior and likelihood (under the condition that the latter is correct).

Table 3 demonstrates that the issue-price effect will partly translate into an industry effect because IPO prices are systematically lower in certain industries. The table documents that the median issue price is below \$6 in industries 0\* and 1\*. The next-lowest median can be found in industry 8\*. These three industries together generated most anomalies. (See figure 3.)

#### C. Stratification by Underpricing

It is surprising that stratification of the winner sample by issue underpricing (defined as the difference between the

FIGURE 4.—ONE-MONTH RISK-ADJUSTED RETURNS IN PERCENT (HORIZONTAL BARS) AND 95%-CONFIDENCE INTERVALS (VERTICAL BARS) ON 4,848 U.S. IPOs IN THE PERIOD 1975–1995, FOR DIFFERENT HORIZONS, STRATIFIED BY ISSUE PRICE LEVEL, WINNERS ONLY



Risk adjustment is based on Rubinstein's model, using the value-weighted CRSP index.

TABLE 3.—CONDITIONAL DISTRIBUTION OF ISSUE PRICE LEVEL BY INDUSTRY; BASED ON 4,848 U.S. IPOs IN THE PERIOD 1975–1995

SIC	Number	Min	Q1	Median	Mean	Q3	Max	Std
0*	32	0.10	1.00	5.00	5.65	9.13	15.00	4.62
1*	347	0.10	1.00	5.00	7.14	11.25	200.00	12.33
2*	546	0.04	6.00	10.00	11.86	14.00	700.00	30.31
3*	1,625	0.04	5.50	9.50	11.69	13.00	1,250.00	47.13
4*	380	0.05	5.38	10.00	9.99	14.25	28.00	5.69
5*	717	0.04	5.38	10.00	9.69	13.50	30.00	5.48
6*	76	0.10	4.75	10.00	10.23	16.00	34.00	7.48
7*	740	0.10	5.00	9.50	9.49	13.00	25.00	5.22
8*	384	0.13	5.50	8.38	8.91	12.00	26.00	4.86

closing price on the first day of trading and the issue price) did not generate any major anomalies, except in the largest decile, usually referred to as the “hot” issues (10% IPOs with largest underpricing).<sup>17</sup>

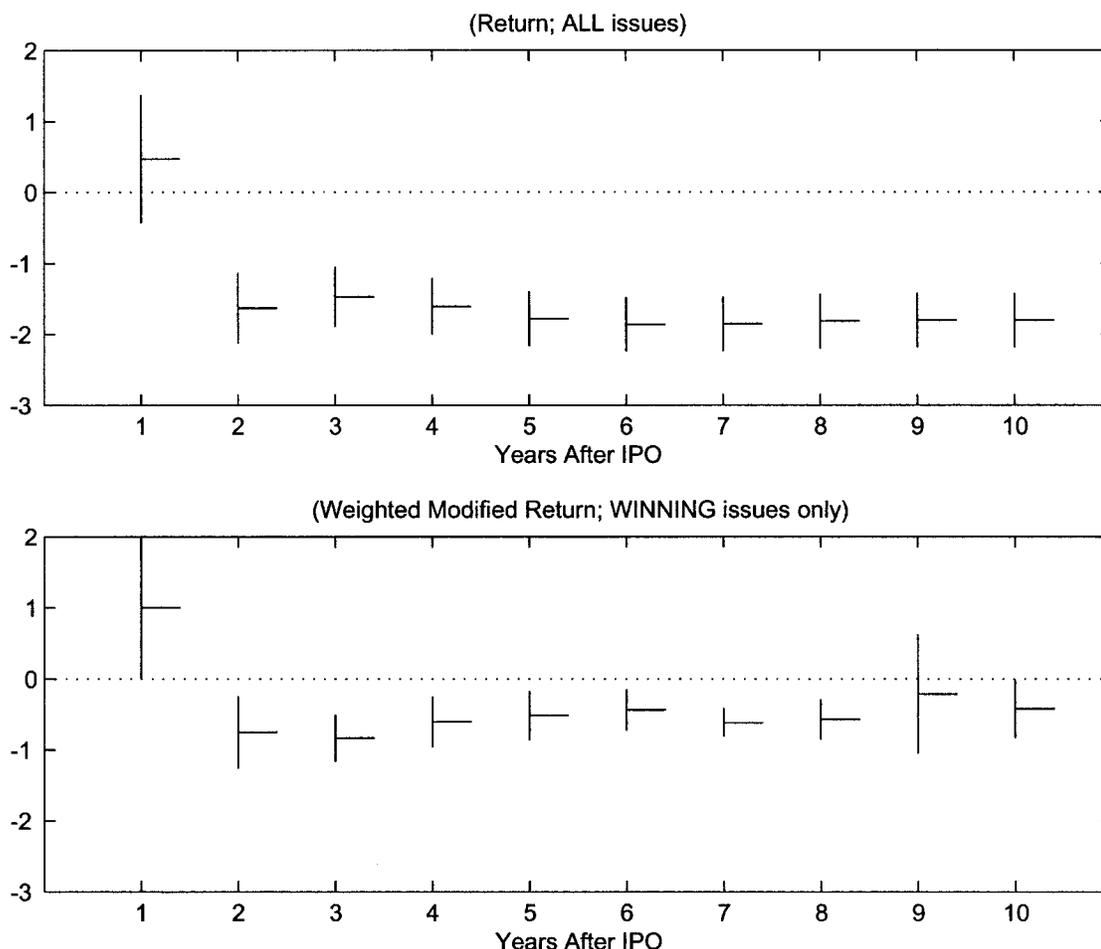
Figure 5 shows the results for this category of IPOs. For comparison, the equally weighted standard returns of all hot issues (both winners and losers) are displayed as well. Although not entirely correcting for the extreme underperformance of all hot issues (upper plot), the weighted average

modified return of winners only (lower plot) is substantially closer to zero, indicating that most, but not all, of the hot-issue anomaly is in fact caused by biases in the market's prior about default at the issue date, in accordance with ELM.

Table 4, however, demonstrates that the hot-issue anomaly is also a price-level anomaly in disguise: both the mean and median issue price of hot IPOs are below \$6. Compare this to the median IPO (the issue in the fifth decile in terms of underpricing), which has a median issue price of \$9, and a mean of \$9.69. Hence, the lower plot of figure 5 is but a selection of figure 4.

<sup>17</sup>In our sample, the hot issues had an underpricing of 52% or more.

FIGURE 5.—ONE-MONTH RISK-ADJUSTED RETURNS IN PERCENT (HORIZONTAL BARS) AND 95%-CONFIDENCE INTERVALS (VERTICAL BARS) ON 485 “HOT” U.S. IPOs IN THE PERIOD 1975–1995, FOR DIFFERENT HORIZONS, ALL ISSUES (UPPER PANEL) AND WINNERS ONLY (LOWER PANEL)



“Hot” IPOs belong to the top decile in terms of issue underpricing (minimum underpricing of 52%). Risk adjustment is based on Rubinstein’s model, using the value-weighted CRSP index.

*D. Summary*

Altogether, there seem to be few anomalies (relative to ELM and Rubinstein’s asset-pricing model) in the U.S. IPO aftermarket. The notable exception are issues priced below \$6. The anomalies for low-priced issues show up in other categories (such as hot issues), because of correlation in the categorizations.

It is surprising that something as arbitrary as issue price has any predictive power at all. Still, this is not unlike the finding of anomalies in changes in the market value of low-priced call options written on the S&P 500 index. (See Bossaerts (1998).) Low-priced call options are almost invariably out-of-the-money. They are the equivalent of a highly levered position in the underlying security—an in-

expensive bet. Future research should indicate whether low-priced IPOs are similarly perceived to be cheap bets.

Because of the low issue price, the reader may suspect that market microstructure effects (finite tick size and relatively large bid-ask spreads) have contributed to the poor performance, even under ELM. But these must be of less relevance for the weighted average modified return, which is computed only for winners. Certainly at longer horizons, winners generally trade at much higher prices, where market microstructure impact should be negligible.

The fact that we do find occasional rejections is good news for the methodology that is used in this article: they demonstrate that the tests have power. A formal analysis of power can be found in the appendix to previous versions of

TABLE 4.—CONDITIONAL DISTRIBUTION OF ISSUE PRICE LEVEL FOR “MEDIAN” AND “HOT” ISSUES; BASED ON 4,848 U.S. IPOs IN THE PERIOD 1975–1995

Type	Number	Min	Q1	Median	Mean	Q3	Max	Std
Median	492	1.00	6.00	9.00	9.69	12.50	28.00	4.63
Hot	485	0.04	0.50	4.00	5.53	9.00	35.00	5.94

TABLE 5.—*t*-STATISTICS COMPUTED FROM IPO RETURNS OVER MONTHS *k* TO 120 AFTER ISSUE DATE, FOR *k* = 1, 16, . . . , 106

<i>k</i>	Benchmark <sup>a</sup>		
	None <sup>b</sup>	Equally Weighted CRSP Index	Value Weighted CRSP Index
1	9.16	-1.77	-5.25
16	10.86	-0.61	-4.44
31	10.21	0.97	-3.22
46	10.39	1.90	-1.60
61	12.44	4.15	0.74
76	12.25	5.34	2.72
91	11.20	3.91	2.41
106	7.19	1.71	0.86

In column 2, only time value of money is adjusted for, by subtracting the three-month T-bill rate. In columns 3 and 4, Rubinstein's model is used to adjust for risk, with two choices for the market portfolio: (i) the CRSP equally weighted index, (ii) the CRSP value weighted index.

<sup>a</sup> Based on 4,848 U.S. IPOs in the period 1975–1995.

<sup>b</sup> Benchmark index used in risk adjustment.

<sup>c</sup> Returns are measured in excess of the three-month T bill rate.

the paper (available on the Internet at <ftp://hss.caltech.edu/pub/pbs/ipool.d.pdf>).

Even more comforting is the fact that the rejections occur in a category of issues (below \$6) that has been the subject of regular inquiries on price manipulation by the Securities and Exchange Commission (SEC). The statistics confirm what the SEC suspected. That category is now generally ignored in new studies of IPO performance (for example, Ritter (2000), who excludes issues with an offer price below \$5).

## VI. Do Financial Markets Really Learn?

The foregoing has demonstrated that ELM, in conjunction with Rubinstein's asset-pricing model, explains the post-issue performance of U.S. IPOs over the period 1975–1995, with the exception of low-priced issues. ELM allows markets to have biased beliefs about the probability of default of an issue at the IPO date, but requires them to update these beliefs rationally (based on Bayes' law and the correct likelihood).

Can we generate other evidence that the market indeed learns to price these IPOs correctly as they mature? The simplest way to do so would be to verify whether prices ultimately reflect the actual probability of default, that is, whether prices eventually converge to EMH. To test this, we could run a standard asset-pricing test, but based only on later returns, that is, excluding the early return history.<sup>18,19</sup>

Table 5 displays results of a standard test of Rubinstein's model on all returns from month *k* on, where *k* = 1, 16, 31, . . . , 106. The test simply verifies equation (5), using a standard *t*-test. Of course, we expect the test to reject for low values of *k*, but we should ultimately accept for large *k*.

<sup>18</sup> Richard Roll suggested this test.

<sup>19</sup> The reader may conjecture that inclusion of the early return history would not affect the inference in large samples anyway (because the market learns to price issues correctly), and, hence, that our exercise is pointless. Bossaerts (1995), however, proves that the conjecture is wrong: the asymptotic distribution of tests of EMH is generally affected by learning, even when the latter is transient.

Rubinstein's model uses the value-weighted market portfolio as benchmark. For comparison, table 5 also replicates the test with the equally weighted CRSP index and the risk-free rate (three-month Treasury bill rate) as benchmark. Under either benchmark, we should reject.

According to Table 5, the underperformance relative to Rubinstein's model (and EMH) disappears after deleting 45 months (*k* = 46). There is slight overperformance for *k* = 76 and *k* = 91, but the reader should be warned that the number of observations (months) is reduced to only 55 (*k* = 76) and 30 (*k* = 91).

Using an equally weighted market benchmark, one does not record a rejection when *k* = 1. Of course, that could already have been read from figure 1, where the CAR against the equally weighted CRSP index reverts back to zero by year 10 (120 months). But the equally weighted benchmark generates rejections for values of *k* from 46 on. With the risk-free rate as benchmark, Table 5 documents rejections for all levels of *k*.

Altogether, table 5 corroborates our earlier findings, that Rubinstein's model, in conjunction with ELM, explain the U.S. IPO post-issue performance. The weighted average modified returns on winners proved this directly. Indirect evidence was provided by the change in the mean return as we deleted more and more observations early after the IPO.

## VII. Conclusion

In the aggregate, we find little evidence against rational aftermarket price behavior for U.S. IPOs that were floated between 1975 and 1995. Prices are set in accordance with Rubinstein's model. If, with hindsight, early aftermarket price levels seemed on average to be too high, this must be the consequence of initial optimism. There is certainly no evidence that the market overreacted or underreacted as news about the fate of each individual issue emerged. At a more microscopic level, however, we find solid evidence against rational price behavior in the aftermarket of low-priced issues. The sign of the weighted average modified returns indicates that the market of low-priced issues is excessively volatile.

Before closing, it should be emphasized that the findings of this article in no way contradict more recent studies that have looked at IPO underperformance, such as Teoh et al. (1998). Using standard methodology, these studies attempt to better identify the nature of anomalous IPO pricing. (Teoh et al., for instance, relate post-issue returns to pre-issue earnings numbers management.) Even when IPO markets satisfy our hypothesis of efficient learning (ELM), anomalies will continue to be found as long as EMH does not hold and as long as researchers insist on using a testing methodology that is valid only under EMH. What our study does is to explain the IPO aftermarket performance puzzle entirely in terms of biases in the market's prior about default risk.

It may deserve emphasis that our methodology is immune to survivorship bias by construction. The fact that our tests

are based only on the winners is by design: in no way does this reflect lack of data on IPOs that defaulted.

The methodology in the present article could be applied to event studies of other financial markets, such as the markets for corporate or sovereign bonds. In general, it can be used to study the prices of any financial contract that features a clear “default state,” in which a fixed and known payout occurs. This includes seemingly unrelated securities, such as straight call options, as illustrated in Bossaerts (1998).

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