Behavioural Biases and Chief Executive Officers' Compensation

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To my mother

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Abstract:

This thesis consists of three essays. In the first, we document *illusory correlation* in CEO compensation decisions by demonstrating that golf handicaps of CEOs are uncorrelated with corporate performance, but related to CEO compensation. Golfers earn more than non-golfers and pay increases with golfing ability. In the second essay we propose a *fundamental attribution bias*-based explanation of the recent explosive growth in CEO pay. Analysis of aggregate time series data and cross sectional data from the late 1990s stock market bubble period suggests that shareholders overattribute prominent increases and decreases in the prices of corporate stocks to the leadership and skill of the CEOs and underestimate the role of stock market fluctuations that are beyond CEO control. In the third essay we show that increases in the number of Initial Public Offerings reliably predicts in-sample and out-of-sample decreases in subsequent equally weighted aggregate stock returns and the return differential between small and big firms.

Resúmen

Esta tesis consiste de tres ensayos. En el primero, documentamos la *correlación imaginaria* entre las decisiones de compensación de los ejecutivos (CEO) al demostrar que el hándicap de los ejecutivos que juegan al golf no está correlacionado con su desempeño en la empresa mientras que sí lo está con su compensación. Los golfistas ganan más que los que no juegan al golf, y las pagas se incrementan con la habilidad en este juego. En el segundo ensayo explicamos la reciente espiral de las compensaciones de los ejecutivos basados en el *sesgo de atribución fundamental*. El análisis de las series temporales agregadas y de datos de sección cruzada correspondiente a la burbuja del mercado accionario en los noventa sugiere que los accionistas exageran al atribuir las subidas y bajadas de los precios de las acciones corporativas a las aptitudes de liderazgo del ejecutivo mientras que subestiman el rol de las fluctuaciones del mercado accionario que se encuentran fuera del control de estos. En el tercer ensayo demostramos que un gran número

de Ofertas Públicas Iniciales predice sistemáticamente, tanto dentro como fuera de la muestra, el subsiguiente bajo rendimientos agregado y ponderado, y la diferencia de rendimientos entre las pequeñas y grandes firmas.

Foreword

This dissertation is a collection of three essays. We present empirical evidence supporting the idea that behavioural biases have an impact on prices and behaviours observed on various markets. The first two essays study Chief Executive Officers' (CEOs) compensation, i.e., the price tag attached to the value of services that the CEO provides to the firm. In the third essay, we present evidence that firms go public when investor sentiment is favorable and equity valuations are higher than justified by fundamentals.

Illusory correlation refers to the use of information in decisions that is uncorrelated with the relevant criterion. In the first essay, we document illusory correlation in CEO compensation decisions by demonstrating that information, that is uncorrelated with corporate performance, is related to CEO compensation. We use publicly available data from the USA for the years 1998, 2000, 2002, 2004 and 2006 to examine the relations between golf handicaps of CEOs and corporate performance, on the one hand, and CEO compensation and golf handicaps, on the other hand. Although we find no relation between handicap and corporate performance, we do find a relation between handicap and CEO compensation. In short, golfers earn more than non-golfers and pay increases with golfing ability. We relate these findings to the difficulties of judging compensation for CEOs. To overcome this – and possibly other illusory correlations – in these kinds of decisions, we recommend the use of explicit, mechanical decision rules.

One robust finding from controlled experiments done by social psychologists is that people have the tendency to excessively attribute observed outcomes to internal dispositions and characteristics of agents (ability, skill, effort) and to downplay situational factors (exogenous shocks, luck). This phenomenon has been labeled *fundamental attribution error* in the social psychology literature and more recently when pertaining to leaders *illusion of leadership* in the economics literature.

In the second essay we suggest that the fundamental attribution bias is a natural suspect for the explosive growth in CEOs compensation observed since the beginning of the 90s. Shareholders confused the bullish stock market (which was mainly driven by the advent of the internet technology and by shareholders' own rosy expectations) for CEOs' effort and skill, and therefore tolerated astronomic CEOs' paychecks for no good reason.

We recast the hypothesis that shareholders over-attribute prominent increases and decreases in the prices of corporate stocks to the leadership and skill of the CEOs and underplay the role of stock market fluctuations which are beyond CEOs control as a simple endogeneity test. In the aggregate time series data we cannot reject the null hypothesis that market fluctuations mechanically cause changes in CEOs' compensation with no detectable reverse causality. Further we find that in the aggregate data increases in CEOs' pay decrease corporate profits.

To complement the time series evidence, we use 4-factor model risk-adjusted returns as a direct measure of CEO skill. We show that in the cross section the relationship between individual CEO pay and skill is very weak (economically small and statistically insignificant).

Finally, the years surrounding the peak of the bubble (years 1997 to 2001) offer a convenient quasi-natural experiment with a clearly identifiable control and treatment groups. The market value of "old economy" firms (the control group) kept growing at a steady rate. The market value of "new economy" firms (the treatment group) more than doubled and then halved back (all of this in less than four years). It is hard to attribute this spike to any reasonable fundamental valuation considerations and even less so to the "new economy" CEOs effort or skill. Hence we use the spike to identify non-parametrically pay for luck and asymmetries in pay for luck. The evidence here is even more supportive of the fundamental attribution bias being the main driver of CEO compensation growth.

Overall in the second essay we conclude that in the late 90s stock market bubble period shareholders were taken for a ride and ended up paying huge amounts of money to their CEOs for no rational reason.

In the third essay, we show that increases in the number of Initial Public Offerings (IPOs) reliably predicts subsequent decreases in equally weighted aggregate stock returns and subsequent shrinking in the small minus big return differential, both in-sample and out-of-sample. In other words, in months following months that feature relatively many IPOs, we observe relatively low overall returns, and small firms do relatively badly compared to big firms. The forecasting patterns are consistent with a behavioural story featuring investor sentiment and limits to arbitrage.

Managers time the public equity market and take their firms public when investor sentiment is high and equity is overvalued. Subsequently as investor sentiment mean reverts or as arbitragers gradually bring values back to levels justified by fundamentals, the market experiences low aggregate returns. The effect is concentrated among small capitalisation stocks, i.e., firms that are more subject to sentiment or more difficult to arbitrage.

Contents

Chapter 1

ILLUSORY CORRELATION IN THE REMUNERATION OF CHIEF EXECUTIVE OFFICERS: IT PAYS TO PLAY GOLF, AND *WELL*

joint work with Robin M. Hogarth

1.1 Introduction

Decisions concerning the remuneration of the chief executive officers (CEOs) of large corporations are important. Curiously, however, few studies have focused on the underlying judgmental processes that lead to these decisions. Instead, there seems to be an underlying belief that market forces will act to ensure that appropriate decisions are taken.[1]

Whereas it would be foolish to ignore the corrective power of market forces, it would be equally foolish to ignore the fact that judgments involving the remuneration of CEOs are fraught with difficulties. Not least of these is that such judgments – like many other complex, decision tasks – depend on information that is only probabilistically related to the criterion of interest (Brunswik, 1952; Kahneman, Slovic, and Tversky, 1982). For example, imagine estimating the future performance of a potential CEO. Clearly,

[1] CEO's remuneration is just the price tag attached to the value of services that the CEO provides to the firm. According to the efficient market hypothesis (Samuelson, 1965; Fama, 1970) prices fully reflect all the available (relevant) information.

some information – or cues – will be more important than others, the track-record of the candidate, say, or the type of problems facing the corporation. However, no one cue will be a perfect predictor and humans typically consider and aggregate several (Karelaia and Hogarth, 2008).

The quality – or accuracy – of human judgment depends on factors that characterize the environment in which judgments are made and people's actions (Simon, 1956; Hammond, 1996). First, the predictive ability of available cues sets an upper limit on how well anyone can predict the criterion. Second, how people use these cues in their judgments, as well as their consistency in doing so, affects relative success. Ideal tasks for accurate judgment involve just a few highly predictive cues, repeated occasions on which judgments are made, and good feedback on outcomes (Karelaia and Hogarth, 2008).

Judgmental tasks concerning the remuneration of CEOs are not "ideal." There are few good, predictive cues, the task is not repeated frequently (compare judging distances when driving), and feedback is unclear (e.g., delayed and/or distorted by extraneous factors). In these circumstances, the psychological literature suggests that people will be susceptible to different types of bias in the selection and weighting of different sources of information (Einhorn and Hogarth, 1978; Gilovich, Griffin, and Kahneman, 2002).

One such phenomenon has been termed "illusory correlation" (Chapman and Chapman, 1967), and relates to using information systematically that is unrelated to the criterion of interest. This phenomenon was originally identified (and named) in the area of clinical psychology but it is not hard to see how it pervades many aspects of life. There have, for example, been many demonstrations of how physical height is used as a discriminating cue in situations such as job selection and political elections even when there is no basis to assume a veridical correlation between height and, say, competence (for a comprehensive review, see Judge and Cable, 2004). Parenthetically, we add that people may or may not be aware that they are using illusory correlations. In some cases, these could arise from conscious beliefs that are just misconceived. In other cases, people might lack awareness about which cues affect their judgments.

This paper documents the role of illusory correlation in decisions about CEO remuneration. In short, in common with economic theory we assume that CEOs' remuneration should be related to the performance of the companies they manage. Second, we document a cue (or information about CEOs) that is unrelated to corporate performance. Third, we maintain that this cue is available to those making decisions about CEO performance, and that (a) those CEOs who exhibit the cue earn more than those that don't, and (b) remuneration is larger for CEOs who display more desirable values of the cue. The cue in question is the CEO's golf handicap, a measure of how well a person plays the sport of golf. In short, possession of a handicap indicates whether or not a person plays golf on a regular basis and the actual handicap indicates ability.[2] Our argument that this cue is available to those making remuneration decisions is based on the fact that, in addition to providing recreational facilities, golf clubs in the USA serve as important venues where wealthy investors, top business executives, board members and other relevant luminaries can meet and discuss. A CEO, therefore, can choose to be visible in such circles of influence.

This paper is organized as follows. Before describing the data used to test for illusory correlation, we discuss some related literature. Our actual tests follow two steps. The first is to show that there is no relation between golf handicap and corporate performance. The second is to document that CEOs with handicaps earn more than those that don't as well as the relation between handicap and remuneration. We conclude by discussing implications.

1.2 Related literature

This paper is most closely related to the small literature on pay for luck in CEO compensation in that it shows that this largely depends on a factor that has no place in standard principal-agent models. Indeed, one has to think hard and have a rich imagination to come up with a rational explanation for

[2]Handicaps are administered by golf clubs or national associations such as the United States Golf Association. A handicap reflects how many more shots an amateur is expected to take to complete a round of golf than a hypothetical excellent player (or "par") – thus, the lower the handicap, the better the player.

the empirical facts we report. Bertrand and Mullainathan (2001) show that CEO pay responds as much to a lucky dollar as to a general dollar, contrary to what the basic principal-agent model predicts. As measures of luck in their analysis they use oil prices (for firms in the oil industry), average industry performance and exchange rate movements (for firms in the traded goods sector). Further they find that firms with stronger governance (e.g., firms where a large shareholder is present on the board of directors) pay less for luck.

Garvey and Milbourn (2006) demonstrate asymmetries in pay for luck – CEOs are rewarded for good luck, but are not punished for bad luck. The measure of luck they use is average industry performance.

Kolev (2008a) shows that CEO pay is affected by the conditions in the public equity market, reflected in the number of IPOs and first day IPO returns. He argues that this is a manifestation of another judgmental bias, the "fundamental attribution error"[3] – shareholders confuse good public equity market conditions for CEO leadership and skill.

There are extensions to the basic principle-agent model, which can make pay for luck potentially optimal – see, e.g., Oyer (2004), Himmelberg and Hubbard (2000), and Celentani and Loveira (2006). The key feature of these models is that CEO marginal productivity or the value of a CEO's outside options fluctuates. As these fluctuations can be potentially correlated with the state of the economy, pay for luck is not necessarily suboptimal – the state variables that we label "luck" are plausibly beyond the CEO's control, yet they might reflect the CEO's marginal productivity or values of outside options.

Blanchard, Lopez-de-Silanes, and Shleifer (1994) provide the most convincing evidence that CEO compensation in the USA has nothing to do with efficient compensation models and is a result of badly functioning corporate

[3]Classic studies demonstrating the fundamental attribution error, i.e., people's tendency to attribute observed outcomes to internal dispositions and characteristics of agents (ability, skill, effort) as opposed to situational factors (exogenous shocks, luck) are Jones and Harris (1967) and Ross, Amabile and Steinmetz (1977). More recent and more relevant for the CEO compensation literature are Weber, Camerer, Rottenstreich, and Knez (2001) and Durell (2001).

governance. They study the effect of cash windfalls, in the form of won or settled lawsuits, on CEO compensation. They start with a sample of 110 firms with settled lawsuits, and exclude all firms for which awards can be potentially connected to the firms' marginal Tobin's Q, thereby reaching a final sample of 11 firms. This method rules out the possibility that the effect of the cash windfall on CEO compensation is due to a change in the marginal productivity of the CEO. Further, their luck variable – cash windfall – is firm specific, hence the possibility that the effect of luck on pay is due to changing values of outside options is also discarded. Blanchard et al. (1994) show that a median of 16% of the net award is given to the top three executives in the form of extra cash over the three years following the award. This increases cash compensation over the three years following the award by 84% compared to the three preceding years. Median management ownership rises from 14.5% before the award, to 16.5% after the award. The empirical results in Blanchard et al. (1994) cast serious doubts on the empirical relevance of the models in Oyer (2004), Himmelberg and Hubbard (2000), and Celentani and Loveira (2006). Incidentally, none of the latter three papers quotes the former.

1.3 Data

The magazine *Golf Digest* compiles data on CEOs' golf handicaps biennially. For 1998, the *Golf Digest* ranking covers CEOs from the top 300 firms in the Fortune 500 list, and only data on those having US Golf Association handicap indices are included. For 2000, the ranking covers the 230 CEOs with the lowest handicaps (i.e., the 230 best players). For 2002, the *Golf Digest* CEO handicap ranking lists the top 270 golfers among Fortune 500 and S&P 500 companies. For 2004, the ranking contains the top 234 golfers, again among Fortune 500 and S&P 500 companies. For 2006, the *Golf Digest* CEO handicap ranking lists the top 200 golfers among Fortune 1000 companies.

We merged the data for the years 1998, 2000, 2002, 2004 and 2006 from issues of *Golf Digest* with *Execucomp* data on CEO compensation, stock returns and other control variables. To study how playing golf affects CEOs' remuneration and shareholders' returns we define three regressors.

Handicap is the exact golf handicap of the CEO in the given fiscal year as reported in the corresponding year report of *Golf Digest*. *No handicap* is a dummy variable equal to 1 if the CEO does not appear in any *Golf Digest* ranking, and equal to 0 otherwise. Summary statistics for the main variables of interest are contained in Table 1.1.

Table 1.1: Summary statistics for the main variables of interest

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Total expected CEO compensation♣ in thousands USD	5361.557	13747.155	0	655447.998	8680
Log(total comp. expected)	7.871	1.247	-6.908	13.393	8665
Total current CEO compensation♢ in thousands USD	1367.206	1660.385	0	32208.334	8770
Log(total current comp.)	6.853	1.039	-6.908	10.38	8710
NO handicap♡	0.836	0.37	0	1	8770
Handicap♠	14.12	5.588	0.3	35.1	839

♣ comprised of Salary, Bonus, Other Annual, Total Value of Restricted Stock Granted, Total Value of Stock Options Granted (using Black-Scholes), Long-Term Incentive Payouts, and All Other Total (tdc1 item in *Execucomp*).
♢ comprised of salary and bonus (total_curr item in *Execucomp*).
♡ a dummy variable equal to 1 if the CEO is not present in any of the *Golf Digest* rankings.
♠ the golf handicap for the given year as reported in the relevant *Golf Digest* ranking.

We also compute the mean golf handicap for each CEO over the years in which he[4] appears in rankings (e.g., if the CEO appears only in year 2000

[4]The vast majority of CEOs were male.

ranking, then his mean golf handicap is the handicap for year 2000; if he appears in both the 2000 and 2002 rankings, his mean handicap is the average value of the handicap for year 2000 and the handicap for year 2002). We classify CEOs according to their mean golf handicap and define two dummy variables taking the value of 1 if the given CEO falls in the middle or the top tercile, respectively, of the mean golf handicap distribution. In the instrumental variable regressions the dummies denoting in which tercile of the mean handicap distribution the CEO falls are used as instruments for the exact golf handicap, i.e., we use them to compute Wald (1940) type of instrumental variable estimator.

1.4 Results

1.4.1 CEO handicaps and shareholder returns

The first step in our analysis is to establish that CEO golf handicap is not a relevant cue regarding CEO's ability to generate shareholder returns. We present results of an event study of stock price reaction to appointments of CEOs, as described e.g. in MacKinlay (1997). We relate the daily abnormal returns around the succession date to the *No handicap* dummy, and to the golfing ability of the CEOs who appear in the *Golf Digest* rankings summarised by the tercile in the handicap distribution in which the golfing CEO falls. Further we compute mean excess risk-adjusted returns, i.e., Jensen's alphas, from Jensen-Fama-French-Carhart 4-factor models (Jensen, 1968; Fama and French, 1993; Jegadeesh and Titman, 1993; Carhart, 1997). We construct portfolios which are long in the stocks of CEOs not appearing in *Golf Digest* rankings, and short in the stock of the CEOs appearing in the rankings. We also construct portfolios which are long in the stocks of CEOs who fall in the top and the middle tercile of the mean handicap distribution (not exceptionally good golf players), and short in the stock of the CEOs appearing in the bottom tercile of the mean handicap distribution (good golf players).

As a first step we present simple cross sectional correlations unconditional on other covariates except time.

Table 1.2 compares contemporaneous and one year ahead shareholders' returns for the group of CEOs appearing in the *Golf Digest* ranking to shareholders' returns for the group of CEOs who are not in the rankings. If golfers are better shareholder value maximizers, we should observe that they generate higher returns. Table 1.2 shows that this is not the case. CEOs who do not appear in any *Golf Digest* ranking appear to outperform the rest, and the effect is significant for one year ahead returns.

Table 1.3 shows that among the CEOs who appear in the *Golf Digest* ranking, higher golf handicap, i.e., worse golfing ability is related to larger shareholders' returns. We cannot reject the null hypothesis that golf handicap and shareholders' returns are unrelated.

Table 1.2: Mean shareholders' returns (%) for firms where CEO does and does not have a golf handicap. In columns 1 and 2 the returns are for the current fiscal year. In columns 3 and 4 the returns are for the one year ahead fiscal year, i.e., in columns 3 and 4 we forecast future yearly returns.

	(1) Return 1 yr	(2) Return 1 yr	(3) Future 1yr ret	(4) Future 1yr ret
NO handicap	12.5644		31.6801**	
	[39.1286]		[14.4070]	
Year=1998	-0.2854	14.9048***	-8.3214	10.2639**
	[32.6365]	[2.5743]	[11.8969]	[4.1123]
Year=2000	15.9252	20.9861***	50.4924	4.0116*
	[32.5225]	[3.2099]	[41.0392]	[2.1446]
Year=2002	27.1728	-11.6691***	35.7335**	38.6243***
	[45.3150]	[1.7177]	[15.2767]	[3.2390]
Year=2004	150.8325*	165.2709	-13.9284	10.0484***
	[89.6989]	[145.1799]	[11.9362]	[1.6826]
Year=2006	10.8815	16.9461***	1.2024	2.4436
	[33.8671]	[1.5652]	[25.2955]	[2.4676]
NO handicap X yr1998		-5.6704*		8.6926*
		[3.1622]		[4.8898]
NO handicap X yr2000		6.4439		88.4587
		[5.3468]		[62.7878]
NO handicap X yr2002		59.8675		28.1228*
		[50.7117]		[16.2132]
NO handicap X yr2004		-4.7916		2.6534
		[183.7769]		[2.1442]
NO handicap X yr2006		5.5365		30.2557
		[3.9005]		[30.8057]
Observations	8661	8661	7146	7146
R^2	0.001	0.001	0.001	0.001
number of CEOs	3886	3886	3282	3282

Note: We regress shareholders' returns in percentage form (*Execucomp* data item trs1yr) for the fiscal year (columns 1 and 2) and for the next fiscal year (columns 3 and 4) on a full set of time dummies (without a constant) and an indicator for whether the CEO does not appear in any *Golf Digest* golf handicap ranking (column 1 and 3). Hence in columns 1 and 3 the estimated coefficients on the time dummies are the mean returns for CEOs present in the *Golf Digest* golf handicap rankings and the estimate on the *NO handicap* dummy reflects the differential return for CEOs not present in the ranking. In columns 1 and 3 the differential return is constrained to be the same across years. In columns 2 and 4 full set of interactions is included. Hence the estimated coefficients on time dummies reflect the mean returns for CEOs present in any ranking in the given year, and the estimated coefficients on the (No handicap X year) interactions reflect the differential performance of CEOs not appearing in any ranking for the given year.
Standard errors [in brackets] consistent in the presence of arbitrary within CEO autocorrelation and heteroskedasticity (see Wooldridge 2002, eq. 7.26). Significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 1.3: Measures of association between shareholders' returns (%) and golf handicaps of CEOs. In columns 1 and 2 the returns are for the current fiscal year. In columns 3 and 4 the returns are for the one year ahead fiscal year, i.e., in columns 3 and 4 we forecast future yearly returns.

	(1) Return 1 yr	(2) Return 1 yr	(3) Future 1yr ret	(4) Future 1yr ret
Handicap	0.0596		0.1668	
	[0.2391]		[0.3394]	
Year=1998	15.1907***	10.1639	12.0228	39.4293
	[5.5034]	[12.5544]	[10.6560]	[28.2516]
Year=2000	7.9141	19.8486	3.2237	3.8700
	[5.9844]	[13.9500]	[5.7471]	[7.3610]
Year=2002	-14.3048***	-16.0500***	37.5261***	26.9578***
	[3.9379]	[5.6414]	[4.6990]	[9.5611]
Year=2004	17.8363***	16.0714***	8.1648	12.0403**
	[3.6041]	[3.5595]	[5.3956]	[5.7073]
Year=2006	17.5396***	5.5720	-2.5614	-9.4187
	[3.2769]	[6.8471]	[4.8329]	[10.8811]
Handicap X yr1998		0.3852		-1.6591
		[0.7439]		[1.5016]
Handicap X yr2000		-0.7116		0.1229
		[0.7700]		[0.4298]
Handicap X yr2002		0.1771		0.8803
		[0.3734]		[0.7800]
Handicap X yr2004		0.1844		-0.1050
		[0.2423]		[0.3388]
Handicap X yr2006		1.1716*		0.8036
		[0.6514]		[0.9977]
Observations	833	833	691	691
R^2	0.152	0.157	0.115	0.123
number of CEOs	463	463	389	389

Note: We regress shareholders' returns in percentage form (*Execucomp* data item trs1yr) for the fiscal year (columns 1 and 2) and for the next fiscal year (columns 3 and 4) on the golf handicap for the given year (column 1 and 3). In columns 2 and 4 full set of interactions (Handicap X year) is included.
Standard errors [in brackets] consistent in the presence of arbitrary within CEO autocorrelation and heteroskedasticity (see Wooldridge 2002, eq. 7.26). Significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

1.4.2 An Event study of stock price reaction around CEO successions

We employ the event study methodology, e.g., as described in MacKinlay (1997), to study how stock price reaction around CEO successions relates to whether the incoming CEO appears in any *Golf Digest* ranking, and to the golf handicap of the incoming CEO if he appears in some *Golf Digest* rankings. We start by extracting the dates when the CEOs entered office from *Execucomp*, as given in the *becameceo* item for the years between 1997 and 2007. We compute abnormal returns using daily data and the market model

$$AR_{it} = R_{it} - (\widehat{\alpha}_i + \widehat{\beta}_i R_{mt})$$

where AR_{it} is the abnormal return for firm i at day t in the event window, R_{it} is the raw return for firm i at day t in the event window, $\widehat{\alpha}_i$ and $\widehat{\beta}_i$ are the parameters of the market model estimated over the estimation window for each firm, and R_{mt} is the market return on date t. The market return is the equally weighted return on the universe of *CRSP* stocks, an index readily provided by *CRSP*.

We take the event window to be from 30 days before the day the new CEO takes over office to 30 days after the event. Date 0 is the date given in the *becameceo* variable in *Execucomp*. We use as nonoverlapping estimation window stock returns from 150 days before the CEO entered office to 30 days before the CEO entered office. Therefore we use at maximum 120 days' returns to estimate the parameters of the market model. If the returns are not available for at least 30 days in the estimation window, we drop the firm.[5]

Once we have calculated the abnormal returns in the event window AR_{it}, we regress them on explanatory variables reflecting the newly hired CEOs

[5] As robustness checks we tried also estimation window from -360 to -45, and different benchmarks – Fama-French three factor model, Fama-French-Carhart four factor model, and value weighted stock market returns. The results turned out very similar and are not reported here.

characteristics:

- *No handicap* dummy is equal to 1 if the CEO does not appear in any *Golf Digest* ranking and 0 otherwise.
- *tercile2* dummy is equal to 1 if the CEO appears in at least one *Golf Digest* ranking and falls in the second tercile of the mean golf handicap distribution, the latter being just the distribution of the average golf handicap for each CEO, the average taken over all years he appears in the rankings. So the *tercile2* dummy is equal to 1 if the CEO is a regular golf player but of average golfing ability and 0 otherwise.
- *tercile3* dummy is equal to 1 if the CEO appears in at least one *Golf Digest* ranking and falls in the third tercile of the mean golf handicap distribution. So the *tercile3* dummy is equal to 1 if the CEO is a regular golf player but of low golfing ability and 0 otherwise.
- *female* dummy is equal to 1 if the CEO is female and 0 otherwise.
- *outsider* dummy is equal to 1 if the CEO has not worked for the firm before his becoming a CEO, and 0 otherwise.

When researchers conduct an event study of CEO successions, they typically take the first announcement in the newspapers of the succession to be the event date. This methodology is questionable for two reasons:

- The search for a new CEO is a lengthy process followed by lengthy negotiations before the new CEO formally agrees with the terms and conditions offered by the firm. It is not clear at all that the markets learn of the *intention* to hire a particular new CEO when the newspapers report it.
- There is a lot of time between the moment the newspapers report that there is intention to hire a new CEO, and the moment the new intended CEO actually becomes a CEO.[6] Meanwhile a lot of unexpected events can take place, e.g., the intended CEO can die in an accident.

[6]For example on 18th of August 2009 the *Wall Street Journal* reported a planned CEO succession in Sony-Ericsson, which is about to take place on the 15th of October 2009.

We take the date on which the newcomer actually becomes the CEO to be the event date. Obviously this approach is not perfect either, as the markets in all probability have learned about the planned change before it actually takes place. Therefore we take a very wide event window, 30 days before and after the event date, and report regressions where we gradually shrink the window to 1 day before and after the event date.

In the following six tables we are mostly interested in the coefficients on the *No handicap* dummy, and the coefficients on the *tercile2* and *tercile3* dummies. As these are binary variables their coefficients are interpreted most easily in columns (2) and (3) where they are the only regressors. In column (2), the Constant is the average daily abnormal return in the event window for CEOs who appear in at least one ranking (i.e., *No handicap* = 0) and the coefficient estimated on the *No handicap* regressor is simply the *difference* in the average daily abnormal return in the event window for the CEOs who do not appear in any ranking (i.e., *No handicap* = 1), relative to the ones who appear in at least one ranking. For example if the Constant is 2 and the coefficient on *No handicap* is 3, then the CEOs appearing in at least one ranking generate 2% daily abnormal return in the event window on average, and the CEOs who do not appear in any ranking generate 5% = 2% + 3%.

In column (3) the Constant is the average daily abnormal return in the event window for CEOs in the first tercile of the mean handicap distribution (i.e., the best golfers) and the coefficients on the *tercile2* and *tercile3* dummies are the difference for each of these two groups of CEOs relative to the first tercile CEOs. For example if the the Constant is 2 and the coefficient on *tercile2* is 3 and the coefficient on *tercile3* is 4, then CEOs who are the best golfers (first tercile) generate average daily abnormal return in the event window of 2%, CEOs who are average golfers (second tercile) generate average daily abnormal return in the event window of 5%=2%+3%, and CEOs who are the worst golfers still appearing in any ranking (third tercile) generate average daily abnormal return in the event window of 6%=2%+4%.

The coefficients on the dummies are still interpreted as differences in the presence of other covariates, but the interpretation of the Constant is a little bit more complicated. For example the Constant in column (4) is the average daily abnormal return for a hypothetical CEO who does appear in at least one ranking, is male, is insider and has age equal to the sample average age (the age regressor is sample demeaned).

If CEOs who play golf regularly are better shareholder value maximisers than the ones who do not play, we expect a more positive price reaction when the former enter office. If CEOs of higher golfing ability are better shareholder value maximisers we expect more positive price reaction for CEOs who are in the first tercile in the mean handicap distribution, i.e., they are the best golfers among all the CEOs playing regularly golf.

Table 1.4 already shows that this is not the case. In fact the ordering of the effects is just the opposite – CEOs who do not play golf regularly generate more positive price reaction than the regular golf players, columns (2) and (4), and the worse the CEO is placed in the mean golf handicap distribution, the more positive stock price reaction he generates, columns (3) and (5). The effects are not significant in this window, though. Notice that female CEOs generate more negative price reaction, as known from Lee and James (2007). Outsiders generate more positive stock price reaction. The female and outsider effects are significant, although the event date used in this study is different, i.e., the date on which the manager actually becomes the CEO, as opposed to the date on which the announcement is found in the newspapers.

Table 1.5 shows the results for a narrower window, from 20 days before, to 20 days after the event. The ordering of the effects is the same, and the golf related variables of interest are still insignificant.

Table 1.6 shows the results for a still narrower window, from 10 days before, to 10 days after the event. The ordering of the effects of interest is the same, and the golf related variable *tercile3* is significant. So the worst golfers, among the set of CEOs who are regular golf players, generate significantly more positive price reaction compared to the best golfers who are in the first tercile (the omitted category in the regression) and compared to average golfers in the second tercile. Average golfers in the second tercile generate more positive price reaction than the best golfers, but the effect is not significant.

The results for the -5 to 5 days window are similar – the effect for the third tercile is relatively large and significant. The effect becomes even larger for the -2 to 2 days window.

Finally in the -1 to 1 days window the significance for the variables of interest is lost.

To summarise the results of the event study:

- CEOs who are not regular golf players generate more positive stock price reaction, but the effect is not significant
- CEOs in the second tercile of the mean handicap distribution, i.e., average golfing ability, generate more positive stock price reaction than the best players, but the effect is not significant
- CEOs in the third tercile of the mean handicap distribution, i.e., low golfing ability, generate the most positive stock price reaction and the effect is significant.

Table 1.4: The dependent variable is the Daily Abnormal Return in % form, the Event Window is -30 to +30 days relative to the event date

	(1)	(2)	(3)	(4)	(5)
NO handicap		0.0224		0.0157	
		[0.0347]		[0.0347]	
2nd tercile			0.0086		0.0355
			[0.0774]		[0.0777]
3rd tercile			0.0641		0.0614
			[0.0749]		[0.0747]
female				-0.1274*	-1.2650***
				[0.0746]	[0.0655]
outsider				0.1456***	0.1672*
				[0.0357]	[0.0996]
age				-0.0022	-0.0003
				[0.0021]	[0.0046]
age^2				-0.0004	0.0004
				[0.0002]	[0.0005]
Constant	0.0076	-0.0122	-0.0361	-0.0209	-0.0826
	[0.0132]	[0.0316]	[0.0511]	[0.0332]	[0.0554]
Observations	102489	102489	12301	99301	12259
R^2	0.000	0.000	0.000	0.000	0.001
number of CEOs	2362	2362	275	2289	274

Standard errors in brackets

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors robust to heteroskedasticity and arbitrary within CEO correlation

No handicap is binary = 1 if the newly hired CEO does not appear in any *Golf Digest* ranking

tercile2 is binary = 1 if the CEO falls in the 2nd tercile of the mean handicap distribution

tercile3 is binary = 1 if the CEO falls in the 3rd tercile of the mean handicap distribution

female is binary = 1 if the newly hired CEO is female

outsider is binary = 1 if the CEO has not worked for the firm before becoming the CEO

Table 1.5: The dependent variable is the Daily Abnormal Return in % form, the Event Window is -20 to +20 days relative to the event date

	(1)	(2)	(3)	(4)	(5)
NO handicap		0.0379		0.0299	
		[0.0417]		[0.0419]	
2nd tercile			0.0380		0.0496
			[0.0916]		[0.0931]
3rd tercile			0.1021		0.0971
			[0.0931]		[0.0918]
female				-0.1185	-0.2460***
				[0.0861]	[0.0792]
outsider				0.1585***	0.1682
				[0.0426]	[0.1162]
age				-0.0037	-0.0016
				[0.0025]	[0.0062]
age^2				-0.0004	0.0009
				[0.0003]	[0.0008]
Constant	0.0055	-0.0279	-0.0741	-0.0364	-0.1337*
	[0.0158]	[0.0380]	[0.0635]	[0.0401]	[0.0684]
Observations	68274	68274	8178	66151	8151
R^2	0.000	0.000	0.000	0.000	0.001
number of CEOs	2362	2362	275	2289	274

Standard errors in brackets

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors robust to heteroskedasticity and arbitrary within CEO correlation

No handicap is binary = 1 if the newly hired CEO does not appear in any *Golf Digest* ranking

tercile2 is binary = 1 if the CEO falls in the 2nd tercile of the mean handicap distribution

tercile3 is binary = 1 if the CEO falls in the 3rd tercile of the mean handicap distribution

female is binary = 1 if the newly hired CEO is female

outsider is binary = 1 if the CEO has not worked for the firm before becoming the CEO

Table 1.6: The dependent variable is the Daily Abnormal Return in % form, the Event Window is -10 to +10 days relative to the event date

	(1)	(2)	(3)	(4)	(5)
NO handicap		0.0188		0.0135	
		[0.0544]		[0.0548]	
2nd tercile			0.0602		0.0617
			[0.1100]		[0.1130]
3rd tercile			0.2712**		0.2418**
			[0.1154]		[0.1136]
female				-0.1544	0.2079**
				[0.1299]	[0.1026]
outsider				0.2018***	0.1576
				[0.0616]	[0.1639]
age				-0.0005	0.0073
				[0.0034]	[0.0070]
age^2				-0.0004	0.0009
				[0.0004]	[0.0009]
Constant	-0.0191	-0.0357	-0.1447**	-0.0496	-0.1993**
	[0.0230]	[0.0481]	[0.0694]	[0.0503]	[0.0780]
Observations	34880	34880	4192	33796	4177
R^2	0.000	0.000	0.002	0.001	0.003
number of CEOs	2362	2362	275	2289	274

Standard errors in brackets

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors robust to heteroskedasticity and arbitrary within CEO correlation

No handicap is binary = 1 if the newly hired CEO does not appear in any *Golf Digest* ranking

tercile2 is binary = 1 if the CEO falls in the 2nd tercile of the mean handicap distribution

tercile3 is binary = 1 if the CEO falls in the 3rd tercile of the mean handicap distribution

female is binary = 1 if the newly hired CEO is female

outsider is binary = 1 if the CEO has not worked for the firm before becoming the CEO

Table 1.7: The dependent variable is the Daily Abnormal Return in % form, the Event Window is -5 to +5 days relative to the event date

	(1)	(2)	(3)	(4)	(5)
NO handicap		-0.0639		-0.0826	
		[0.0775]		[0.0782]	
2nd tercile			0.2210		0.2245
			[0.1343]		[0.1371]
3rd tercile			0.4041**		0.3550*
			[0.1873]		[0.1818]
female				-0.2328*	0.3592***
				[0.1351]	[0.1113]
outsider				0.3139***	0.3506
				[0.0888]	[0.2783]
age				0.0016	0.0101
				[0.0045]	[0.0103]
age^2				-0.0004	0.0018
				[0.0005]	[0.0011]
Constant	0.0215	0.0777	-0.1265	0.0444	-0.2378**
	[0.0309]	[0.0698]	[0.0926]	[0.0712]	[0.1099]
Observations	17822	17822	2148	17267	2141
R^2	0.000	0.000	0.004	0.001	0.007
number of CEOs	2362	2362	275	2289	274

Standard errors in brackets

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors robust to heteroskedasticity and arbitrary within CEO correlation

No handicap is binary = 1 if the newly hired CEO does not appear in any *Golf Digest* ranking

tercile2 is binary = 1 if the CEO falls in the 2nd tercile of the mean handicap distribution

tercile3 is binary = 1 if the CEO falls in the 3rd tercile of the mean handicap distribution

female is binary = 1 if the newly hired CEO is female

outsider is binary = 1 if the CEO has not worked for the firm before becoming the CEO

Table 1.8: The dependent variable is the Daily Abnormal Return in % form, the Event Window is -2 to +2 days relative to the event date

	(1)	(2)	(3)	(4)	(5)
NO handicap		0.0176		-0.0140	
		[0.1160]		[0.1161]	
2nd tercile			0.2035		0.2205
			[0.1913]		[0.1963]
3rd tercile			0.6469**		0.5321**
			[0.2602]		[0.2379]
female				-0.2371	0.1032
				[0.2025]	[0.1632]
outsider				0.4269***	0.4172
				[0.1598]	[0.4286]
age				0.0090	0.0399**
				[0.0069]	[0.0155]
age^2				-0.0007	0.0003
				[0.0007]	[0.0017]
Constant	0.0666	0.0511	-0.2342*	0.0068	-0.3022**
	[0.0497]	[0.1022]	[0.1209]	[0.1029]	[0.1447]
Observations	8436	8436	1027	8173	1023
R^2	0.000	0.000	0.009	0.002	0.019
number of CEOs	2361	2361	275	2288	274

Standard errors in brackets

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors robust to heteroskedasticity and arbitrary within CEO correlation

No handicap is binary = 1 if the newly hired CEO does not appear in any *Golf Digest* ranking

tercile2 is binary = 1 if the CEO falls in the 2nd tercile of the mean handicap distribution

tercile3 is binary = 1 if the CEO falls in the 3rd tercile of the mean handicap distribution

female is binary = 1 if the newly hired CEO is female

outsider is binary = 1 if the CEO has not worked for the firm before becoming the CEO

Table 1.9: The dependent variable is the Daily Abnormal Return in % form, the Event Window is -1 to +1 days relative to the event date

	(1)	(2)	(3)	(4)	(5)
NO handicap		-0.0559		-0.1062	
		[0.1560]		[0.1551]	
2nd tercile			0.0683		0.1387
			[0.2658]		[0.2692]
3rd tercile			0.4711		0.3479
			[0.3432]		[0.3108]
female				-0.3300	-2.2297***
				[0.2871]	[0.2247]
outsider				0.6261***	0.8259
				[0.2221]	[0.5494]
age				0.0100	0.0353*
				[0.0093]	[0.0199]
age^2				-0.0009	0.0008
				[0.0010]	[0.0018]
Constant	0.1004	0.1495	-0.0360	0.0737	-0.2013
	[0.0695]	[0.1357]	[0.1698]	[0.1370]	[0.2008]
Observations	5247	5247	644	5082	641
R^2	0.000	0.000	0.005	0.003	0.022
number of CEOs	2299	2299	275	2229	274

Standard errors in brackets

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors robust to heteroskedasticity and arbitrary within CEO correlation

No handicap is binary = 1 if the newly hired CEO does not appear in any *Golf Digest* ranking

tercile2 is binary = 1 if the CEO falls in the 2nd tercile of the mean handicap distribution

tercile3 is binary = 1 if the CEO falls in the 3rd tercile of the mean handicap distribution

female is binary = 1 if the newly hired CEO is female

outsider is binary = 1 if the CEO has not worked for the firm before becoming the CEO

1.4.3 Long term performance attribution regressions

We carry out calendar time performance attribution regressions (Jensen, 1968; Carhart, 1997) to study the long term impact of golf playing on shareholders' returns. For each month from January 1997 to December 2007, for a total of 132 months, we compute the equally (columns 1, 2 and 3) and value weighted (columns 4, 5 and 6) returns of portfolios which are

a) long in firms with CEOs who do not appear in any *Golf Digest* ranking and short in firms with CEOs who appear in at least one ranking (Table 1.10). For example, for equally weighted returns this would be

$$\sum_{i \text{ s.t. No handicap}=1} r_{it} - \sum_{i \text{ s.t. No handicap}=0} r_{it} \equiv R_t^1,$$

where r_{it} is the monthly return for firm i in month t.

b) long in firms with CEOs who are in the second tercile of the mean golf handicap distribution (good but not exceptional golf players) and short in firms with CEOs who are in the first tercile of the mean golf handicap distribution (exceptionally good golf players) (Table 1.11). For example, for equally weighted returns this would be

$$\sum_{i \text{ s.t. CEO in 2nd tercile}} r_{it} - \sum_{i \text{ s.t. CEO in 1st tercile}} r_{it} \equiv R_t^2,$$

where r_{it} is the monthly return for firm i in month t.

c) long in firms with CEOs who are in the third tercile of the mean golf handicap distribution (relatively bad golf players) and short in firms with CEOs who are in the first tercile of the mean golf handicap distribution (exceptionally good golf players) (Table 1.12). For example, for equally weighted returns this would be

$$\sum_{i \text{ s.t. CEO in 3rd tercile}} r_{it} - \sum_{i \text{ s.t. CEO in 1st tercile}} r_{it} \equiv R_t^3,$$

where r_{it} is the monthly return for firm i in month t.

For value weighted returns the summations in the above expressions are weighted by the value of the firm in the previous month, calculated as the number of shares outstanding times the share price.

The mean golf handicap distribution is the distribution of the average golf handicap of each CEO (the average taken across all the years in which the CEO appears in the rankings). The terciles are computed from this mean golf handicap distribution, and each CEO is classified as either belonging to the first, second or third tercile of the mean golf handicap distribution. The whole sample of firms on which the computations are based are the successful matches resulting from merging *Execucomp* to *CRSP* data. Merging was done on *gvkey*, through the *CRSP/Compustat Merged* monthly database.

For each portfolio in a), b) and c) this procedure results in time series of monthly returns, which are regressed on the monthly time series of returns of a set of "risk" factors. In the one factor model the only risk factor is the return on the value weighted market portfolio minus the risk free rate. In the three factor model (Fama and French, 1993) the risk factors are the returns on the value weighted market portfolio minus the risk free rate, the high book to market minus low book to market firms portfolio and the small firms minus big firms portfolio. In the four factor model the momentum factor (Jegadeesh and Titman, 1993; Carhart, 1997) is added to the previously mentioned three factors.[7] For example, for the four factor model, for $j = 1,2,3$ we run the following time series regressions

$$R_t^j = \alpha^j + \beta_1^j MktRf_t + \beta_2^j SMB_t + \beta_3^j HML_t + \beta_4^j MOM_t + \varepsilon_t^j.$$

The returns on the portfolios are regressed on the risk factors and the constant term in this regression, known as Jensen's alpha, represents the average risk adjusted abnormal return the portfolio generates after controlling for the known risk factors. For example, if the portfolio strategy long in bad golf players (top tercile, the same as 3rd tercile) and short in good golf players (bottom tercile, the same as 1st tercile) generates positive statistically signif-

[7] Time series of the factor returns are downloaded from Kenneth R. French's website (http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html).

icant and economically large Jensen's alpha, we can conclude that bad golf players outperform good golf players in the long run and generate abnormal returns even after appropriately controlling for risk.

The following three tables show that Jensen's alphas are never significant at conventional levels when value weighted returns are used as the regressand.

Table 1.10: The dependent variable in columns 1, 2 , and 3 is the equally weighted Monthly Return in % form difference between the portfolio of CEOs who do not appear in any *Golf Digest* ranking (long position) and the portfolio of CEOs who do appear in at least one ranking (short position). The dependent variable in columns 4, 5 , and 6 is the value weighted Monthly Return in % form difference between the portfolio of CEOs who do not appear in any *Golf Digest* ranking (long position) and the portfolio of CEOs who do appear in at least one ranking (long position).

	(1)	(2)	(3)	(4)	(5)	(6)
Mkt-rf	0.2401***	0.0858***	0.0668**	0.2157***	0.1092***	0.1329***
	[0.0390]	[0.0257]	[0.0286]	[0.0442]	[0.0376]	[0.0395]
High-Low		-0.1405***	-0.1506***		-0.0844	-0.0719
		[0.0311]	[0.0317]		[0.0591]	[0.0590]
Small-Big		0.4339***	0.4452***		0.3255***	0.3116***
		[0.0345]	[0.0352]		[0.0562]	[0.0506]
Momentum			-0.0455**			0.0565
			[0.0227]			[0.0353]
Constant	0.2395	0.2774***	0.3277***	-0.0952	-0.0802	-0.1426
	[0.1903]	[0.0823]	[0.0876]	[0.1813]	[0.1338]	[0.1318]
Observations	132	132	132	132	132	132
R^2	0.190	0.849	0.858	0.173	0.569	0.585

Standard errors in brackets

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors robust to heteroskedasticity

However in Table 1.10 we see that CEOs who are not regular golf players *outperform* the regular golfers with a significant risk adjusted yearly excess return of roughly 3.9324% = 0.3277% * 12.

Similarly in Table 1.11 we see that CEOs who fall in the second tercile of the mean golf handicap distribution (regular golfers of average golfing ability)

Table 1.11: The dependent variable in columns 1, 2 , and 3 is the equally weighted Monthly Return in % form difference between the portfolio of CEOs who fall in the second tercile of the mean handicap distribution (average ability regular golf players, long position), and the portfolio of CEOs who fall in the first tercile of the mean handicap distribution (excellent regular golf players, short position). The dependent variable in columns 4, 5 , and 6 is the value weighted Monthly Return in % form difference between the portfolio of CEOs who fall in the second tercile of the mean handicap distribution (average ability regular golf players, long position) and the portfolio of CEOs who fall in the first tercile of the mean handicap distribution (excellent regular golf players, short position).

	(1)	(2)	(3)	(4)	(5)	(6)
Mkt-rf	0.1134***	0.0634	0.0467	-0.0745	-0.0755	-0.1012
	[0.0322]	[0.0406]	[0.0428]	[0.0580]	[0.0681]	[0.0709]
High-Low		-0.0853**	-0.0941**		0.0170	0.0034
		[0.0410]	[0.0417]		[0.0931]	[0.0881]
Small-Big		0.0570	0.0669		0.0404	0.0556
		[0.0435]	[0.0441]		[0.0832]	[0.0769]
Momentum			-0.0400			-0.0614
			[0.0250]			[0.0520]
Constant	0.1737	0.2214*	0.2655**	-0.0077	-0.0234	0.0444
	[0.1253]	[0.1268]	[0.1286]	[0.2112]	[0.2385]	[0.2277]
Observations	132	132	132	132	132	132
R^2	0.110	0.188	0.207	0.019	0.023	0.040

Standard errors in brackets

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors robust to heteroskedasticity

Table 1.12: The dependent variable in columns 1, 2 , and 3 is the equally weighted Monthly Return in % form difference between the portfolio of CEOs who fall in the third tercile of the mean handicap distribution (relatively low ability, regular golf players, long position) and the portfolio of CEOs who fall in the first tercile of the mean handicap distribution (excellent regular golf players, short position). The dependent variable in columns 4, 5 , and 6 is the value weighted Monthly Return in % form difference between the portfolio of CEOs who fall in the third tercile of the mean handicap distribution (relatively low ability regular golf players, long position) and the portfolio of CEOs who fall in the first tercile of the mean handicap distribution (excellent regular golf players, short position).

	(1)	(2)	(3)	(4)	(5)	(6)
Mkt-rf	0.0114	0.0227	0.0115	-0.0852	-0.0188	-0.0134
	[0.0264]	[0.0312]	[0.0318]	[0.0566]	[0.0572]	[0.0603]
High-Low		0.0111	0.0052		0.1661*	0.1690*
		[0.0420]	[0.0416]		[0.0907]	[0.0922]
Small-Big		-0.0299	-0.0233		0.0345	0.0313
		[0.0306]	[0.0316]		[0.0665]	[0.0691]
Momentum			-0.0267			0.0129
			[0.0214]			[0.0548]
Constant	0.0682	0.0646	0.0940	0.3289	0.2186	0.2043
	[0.1097]	[0.1110]	[0.1124]	[0.2060]	[0.2013]	[0.2152]
Observations	132	132	132	132	132	132
R^2	0.002	0.014	0.026	0.028	0.073	0.074

Standard errors in brackets

* $p < .10$, ** $p < .05$, *** $p < .01$

Standard errors robust to heteroskedasticity *outperform* CEOs who fall in the first tercile of the mean golf handicap distribution (regular golfers of high golfing ability) with a significant risk adjusted yearly excess return of roughly 3.1860% = 0.2655% * 12.

In Table 1.12 we see that the worst regular golfers outperform the best golfers both when returns are value and equally weighted, but the Jensen's alphas are very small and insignificant.

To summarise the results

- CEOs who do not appear in any *Golf Digest* ranking outperform the rest. The effect is significant only when equally weighted returns are used.
- Average golfers (second tercile) outperform excellent golfers (first tercile). Again the effect is significant only when equally weighted returns are used.
- Bad regular golfers (third tercile) outperform excellent golfers (first tercile). The effect is not significant, but is of the same sign both for equally and value weighted returns.

Overall we conclude that good golf playing skills are not informative for good long term performance of the CEOs. If anything, bad golfers outperform good golfers, but the evidence is not overwhelming.

1.4.4 CEO handicaps and compensation

We start our analysis of the relation between golf handicap and CEO compensation by establishing that it exists. CEOs whose handicaps are good enough to warrant presence in the *Golf Digest* ranking are better paid and the effect is significant in all specifications at better than 1% significance level. Among the set of executives present in the ranking, the ones who have higher handicaps (i.e., are worse golfers) are paid less.

Tables 1.13 and 1.14 establish these facts for the log of total direct compensation [log(tdc1) item in *Execucomp*], and Tables 1.15 and 1.16 do the same

Table 1.13: Regression of log of total compensation on playing golf. The dependent variable is the log of total compensation, comprised of Salary, Bonus, Other Annual, Total Value of Restricted Stock Granted, Total Value of Stock Options Granted (using Black-Scholes), Long-Term Incentive Payouts, and All Other Total (tdc1 item in *Execucomp*). The main regressor of interest *NO handicap* is a dummy variable equal to 1 if the CEO does *not* appear in any of the *Golf Digest* golf handicap rankings.

	(1)	(2)	(3)	(4)
NO handicap	-0.2170***	-0.2179***	-0.1461***	-0.1440***
	[0.0459]	[0.0460]	[0.0473]	[0.0472]
Log(mktvalue)	0.3925***	0.3913***	0.4149***	0.4318***
	[0.0119]	[0.0120]	[0.0213]	[0.0205]
Log(book/mkt value)			0.0826***	0.1109***
			[0.0218]	[0.0222]
S&P 500 dummy			0.0653	0.0506
			[0.0631]	[0.0619]
Return 1 yr			0.0000***	0.0000**
			[0.0000]	[0.0000]
Return 3 yrs			0.0013***	0.0016***
			[0.0004]	[0.0005]
Return on Assets			-0.0047***	-0.0051***
			[0.0011]	[0.0011]
Sales growth 3yrs			0.0004	0.0002
			[0.0003]	[0.0003]
Employees			0.0005	0.0006*
			[0.0003]	[0.0003]
Dividend yield			-0.0084	-0.0002
			[0.0066]	[0.0023]
Price/earnings			-0.0001	-0.0001
			[0.0000]	[0.0000]
age				0.0008
				[0.0025]
age^2				0.0001
				[0.0002]
tenure				0.0008
				[0.0030]
tenure^2				-0.0005***
				[0.0002]
Time dummies	No	Yes	Yes	Yes
2 digit SIC dummies	No	No	No	Yes
Observations	8450	8450	7731	7384
R^2	0.319	0.325	0.347	0.388
number of CEOs	3786	3786	3521	3335

Standard errors [in brackets] consistent in the presence of arbitrary within CEO autocorrelation and heteroskedasticity (see Wooldridge 2002, eq. 7.26). Significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 1.14: Regression of log of total compensation on golf handicap. The dependent variable is the log of total compensation, comprised of Salary, Bonus, Other Annual, Total Value of Restricted Stock Granted, Total Value of Stock Options Granted (using Black-Scholes), Long-Term Incentive Payouts, and All Other Total (tdc1 item in *Execucomp*). The main regressor of interest is *Handicap*, the golf handicap for the given year as reported in the relevant *Golf Digest* ranking.

	(1)	(2)	(3)	(4)
Handicap	-0.0138**	-0.0128*	-0.0119*	-0.0191***
	[0.0067]	[0.0070]	[0.0072]	[0.0063]
Log(mktvalue)	0.3329***	0.3296***	0.3569***	0.3470***
	[0.0440]	[0.0440]	[0.0743]	[0.0662]
Log(book/mkt value)			0.0973	0.0763
			[0.1020]	[0.0922]
Return 1 yr			0.0036*	0.0038**
			[0.0019]	[0.0016]
Return 3 yrs			0.0029*	0.0027**
			[0.0017]	[0.0013]
Return on Assets			-0.0029	-0.0025
			[0.0109]	[0.0113]
Sales growth 3yrs			-0.0038	-0.0027
			[0.0036]	[0.0031]
Employees			0.0001	0.0009
			[0.0004]	[0.0007]
Dividend yield			0.0021	0.0247
			[0.0253]	[0.0237]
Price/earnings			0.0003	0.0004
			[0.0002]	[0.0002]
S&P 500 dummy			0.0145	-0.0024
			[0.1394]	[0.1087]
age				0.0166**
				[0.0076]
age^2				-0.0005
				[0.0008]
tenure				-0.0011
				[0.0092]
tenure^2				-0.0016
				[0.0010]
Time dummies	No	Yes	Yes	Yes
2 digit SIC dummies	No	No	No	Yes
Observations	811	811	777	764
R^2	0.173	0.181	0.211	0.344
number of CEOs	447	447	430	423

Standard errors [in brackets] consistent in the presence of arbitrary within CEO autocorrelation and heteroskedasticity (see Wooldridge 2002, eq. 7.26). Significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 1.15: Regression of log of total current compensation on playing golf. The dependent variable is total current compensation comprised of salary and bonus (total_curr item in *Execucomp*). The main regressor of interest is *NO handicap*, a dummy variable equal to 1 if the CEO is not present in any of the *Golf Digest* rankings.

	(1)	(2)	(3)	(4)
NO handicap	-0.2637***	-0.2451***	-0.1679***	-0.1530***
	[0.0444]	[0.0445]	[0.0478]	[0.0465]
Log(mktvalue)	0.2304***	0.2346***	0.2671***	0.2532***
	[0.0127]	[0.0129]	[0.0206]	[0.0213]
Log(book/mkt value)			0.1345***	0.0960***
			[0.0185]	[0.0200]
S&P 500 dummy			-0.0173	0.0024
			[0.0602]	[0.0598]
Return 1 yr			0.0000	0.0000
			[0.0000]	[0.0000]
Return 3 yrs			0.0013***	0.0009**
			[0.0004]	[0.0004]
Return on Assets			0.0017**	0.0005
			[0.0008]	[0.0007]
Sales growth 3yrs			-0.0007	-0.0008
			[0.0007]	[0.0006]
Employees			0.0007	0.0009*
			[0.0004]	[0.0005]
Dividend yield			-0.0005	0.0015
			[0.0017]	[0.0021]
Price/earnings			-0.0001*	-0.0001
			[0.0000]	[0.0000]
age				0.0090***
				[0.0025]
age^2				-0.0002
				[0.0002]
tenure				0.0051**
				[0.0024]
tenure^2				-0.0003**
				[0.0001]
Time dummies	No	Yes	Yes	Yes
2 digit SIC dummies	No	No	No	Yes
Observations	8492	8492	7762	7419
R^2	0.185	0.204	0.227	0.279
number of CEOs	3806	3806	3534	3348

Standard errors [in brackets] consistent in the presence of arbitrary within firm autocorrelation and heteroskedasticity (see Wooldridge 2002, eq. 7.26). Significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 1.16: Regression of log of total current compensation on golf handicap. The dependent variable is total current compensation comprised of salary and bonus (total_curr item in *Execucomp*). The main regressor of interest is *Handicap*, the golf handicap for the given year as reported in the relevant *Golf Digest* ranking.

	(1)	(2)	(3)	(4)
Handicap	-0.0045	-0.0045	-0.0056	-0.0086*
	[0.0061]	[0.0061]	[0.0066]	[0.0051]
Log(mktvalue)	0.2102***	0.2102***	0.2095***	0.1641***
	[0.0346]	[0.0346]	[0.0630]	[0.0399]
Log(book/mkt value)			0.1560*	0.1006
			[0.0819]	[0.0636]
Return 1 yr			0.0052***	0.0050***
			[0.0016]	[0.0013]
Return 3 yrs			0.0026	0.0024
			[0.0021]	[0.0015]
Return on Assets			0.0100*	0.0137***
			[0.0056]	[0.0049]
Sales growth 3yrs			-0.0010	0.0002
			[0.0031]	[0.0027]
Employees			0.0005	0.0012*
			[0.0004]	[0.0007]
Dividend yield			0.0039	0.0140
			[0.0162]	[0.0144]
Price/earnings			0.0001	0.0002
			[0.0002]	[0.0002]
S&P 500 dummy			0.0564	0.1260
			[0.1224]	[0.0881]
age				0.0122**
				[0.0061]
age^2				-0.0011
				[0.0007]
tenure				0.0015
				[0.0071]
tenure^2				-0.0017*
				[0.0009]
Time dummies	No	Yes	Yes	Yes
2 digit SIC dummies	No	No	No	Yes
Observations	809	809	775	762
R^2	0.161	0.161	0.219	0.363
number of CEOs	446	446	429	422

Standard errors [in brackets] consistent in the presence of arbitrary within CEO autocorrelation and heteroskedasticity (see Wooldridge 2002, eq. 7.26). Significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

with respect to the log of total current compensation [log(total_curr) item in *Execucomp*].

1.4.5 CEO handicaps and compensation, conditional on other covariates

Tables 1.13-1.16 show the results of regressing CEO compensation on golf handicap and other controls. Moving from column 1 to column 4 in each table, more regressors are included. Column 1 contains the bare minimum of controls relevant in this context – the size of the firm measured by the log of the market value. Column 2 adds a full set of year dummies. Column 3 adds other controls which might be relevant for explaining compensation – a dummy variable equal to 1 if the firm belongs to the S&P 500 index, log of book to market ratio to proxy for firms' growth opportunities, 1 and 3 year stock returns (including dividend distributions), return on assets, number of employees, 3 year sales growth, price to earnings ratio, and dividend yield. Column 4, which is our preferred specification, additionally includes a full set of industry fixed effects at the 2 digit SIC level, and quadratics in the CEO's age and tenure.[8]

Table 1.13 explains the log of total CEO compensation [Salary, Bonus, Other Annual, Total Value of Restricted Stock Granted, Total Value of Stock Options Granted (using Black-Scholes), Long-Term Incentive Payouts, and All Other Total, i.e., tdc1 item in *Execucomp*] with a regressor which is a dummy variable taking the value 1 if the CEO does not have a golf handicap, or the handicap was not good enough to merit inclusion in any of the *Golf Digest* rankings. CEOs who are not regular golf players receive about 14% less[9] in total ex-ante compensation and the effect is significant at the 1% significance level (Table 1.13, column 4).

[8]The regressors age and tenure are sample demeaned for ease of interpretation. Therefore the estimated coefficients on, e.g., age (disregarding age^2) is interpreted as the percentage increase in pay resulting from a year increase in the regressor, evaluated at the sample mean of age.

[9]More precisely, the marginal effect on CEO compensation from switching the No golf handicap dummy from 0 to 1 is $100*[\exp(-0.144) - 1] = -13.41\%$

Table 1.14 is limited to the set of executives appearing in the *Golf Digest* rankings and presents regressions of the log of total CEO compensation on the CEO's golf handicap. Better golfers are paid more: an increase of one point in handicap (i.e., being a marginally worse player) results in 1.91% decrease in total ex-ante pay (Table 1.14, column 4). The effect is statistically significant at the 1% significance level and economically large.

Table 1.15 presents regressions of the log of total current compensation comprised of salary and bonus (total_curr item in *Execucomp*) on a dummy variable equal to 1 if the CEO does not have a handicap or if the handicap is not good enough to merit inclusion in the rankings, and other controls. Not playing golf regularly costs about 15.3% in total current compensation (Table 1.15, column 4), and the effect is significant at the 1% significance level. The sizes of the estimated effects of not playing golf for total current compensation are fairly similar to the estimated effects for total direct compensation.

The evidence supports our claim that CEOs who are regular golfers earn more than those who are not. At the same time, we stress that the effect is economically large – 15% less in pay just because the CEO does not play golf or does not play golf regularly enough to have a decent handicap. Table 1.16 presents regressions of the log of total current compensation on golf handicap and other covariates. Among the CEOs who have good golf handicaps – and hence appear in the *Golf Digest* rankings – an increase of one handicap point (i.e., being a marginally worse player) results in a decrease in salary and bonus of about 0.86% (Table 1.16, column 4). The effect is only statistically significant at the 10% significance level but economically quite large.[10]

Finally, the mean CEO golf handicap in our sample is 14.12 with a standard deviation of 5.58. Hence an increase in golf handicap of one standard deviation (i.e., becoming a worse golfer) leads to about 10.66% decrease in total ex-ante compensation and about an 4.8% decrease in salary plus bonus. This

[10] A good but not outstanding golfer might have a handicap of, say, 15. An outstanding golf player might have a handicap of 2 (i.e., plays nearly at par). Thus, a decrease in handicap from 15 to 2 , which is a move from being a good to an excellent golf player, results in about a 0.86*13 = 11.18% increase in total current compensation. This is a large effect.

is strong evidence in support of our claim concerning the relative effects of golf handicap on remuneration.

1.4.6 CEO handicaps and compensation, instrumental variable regressions

We argue that good golf playing abilities confer a "halo" effect on the CEO. The presence of the illusory belief that golf playing abilities correlate with shareholder value maximisation abilities prompts the relevant decision makers (board of directors, compensation committee members) to confer higher pay on CEOs who are good golfers. Hence the thought experiment we have in mind is to elicit and somehow aggregate the opinions of all relevant decision makers regarding how good a golfer their CEO is, and to correlate this (infeasible) measurement of CEO golf playing abilities with CEO compensation.

As this experiment is infeasible in practice, the best measurement of how good a golfer a CEO is in the eyes of the relevant decision makers, is the golf handicap in the fiscal year in question. The theoretical variable we wish we could have regarding golf playing abilities is a weighted average of the opinions of the people deciding how much the CEO should be paid, where the weights reflect how important each person is in the decision making process. Therefore the golf handicap is an imperfect measurement of the theoretical variable we are interested in, even if the true golf handicap is measured without error in our data for the year in question.

If the decision makers' estimates of the CEOs' golf playing abilities diverge from the golf handicap in a random manner, as in the classical errors in variables model, i.e., the noise is uncorrelated with the golf handicap and with the error term in the estimating equation, our regressions of CEO remuneration on golf handicap would suffer from attenuation bias and we would underestimate the true effect of golf playing abilities on CEO pay. To investigate this issue, and correct for potential attenuation, we estimate instrumental variable regressions following a suggestion by Wald (1940). We use the tercile to which a CEO belongs in the handicap distribution (of CEO average handicaps) as an instrument for the golf handicap in the given year in question.[11]

We find some evidence that our OLS handicap regressions are subject to attenuation. In the instrumental variable regressions the marginal effects of the golf handicap on CEO compensation become larger and statistically significant both for total direct compensation and total current compensation. For total direct compensation the instrumental variable estimate of the marginal effect of a unit increase in golf handicap is -2.49% (Table 1.17, column 4) versus the OLS estimate of -1.91% (Table 1.14, column 4). For total current compensation the instrumental variable estimate of the marginal effect of a unit increase in golf handicap is -1.4% (Table 1.18, column 4) versus the OLS estimate of -0.86% (Table 1.16, column 4). In the instrumental variable regression, the golf handicap appears as a significant predictor for total current compensation too, whereas this effect was only marginally significant in the OLS regression.

1.5 Plausibility of alternative (rational) explanations of our results

We consider two alternative explanations of our results that relate to reverse causality and unobserved productivity, respectively.

1.5.1 Reverse causality – better paid CEOs are able to afford to play more golf

In our sample, a CEO at the 10th percentile of the distribution of total compensation receives about \$650 thousands. A CEO at the median receives

[11] We firstly compute the average golf handicap by CEO, e.g., if the CEO is present in the handicap rankings for years 2000, 2002 and 2004 we take the average of the three; if he is present only for year 2000 we take the handicap for this year. Then we create dummy variables equal to 1 if the CEO belongs to the first, second or third tercile in this average handicap distribution, and use these dummy variables as instruments for the exact golf handicap in a given year. The idea is that this procedure provides another measurement of how good a golfer the CEO is in the eyes of the relevant decision makers.

Table 1.17: Instrumental variable regression of log of total compensation♣ on golf handicap.

	(1)	(2)	(3)	(4)
Handicap	-0.0175**	-0.0180**	-0.0170*	-0.0249***
	[0.0084]	[0.0088]	[0.0093]	[0.0080]
Log(mktvalue)	0.3331***	0.3298***	0.3575***	0.3486***
	[0.0440]	[0.0439]	[0.0738]	[0.0682]
Log(book/mkt value)			0.0946	0.0730
			[0.1021]	[0.0950]
Return 1 yr			0.0036*	0.0038**
			[0.0018]	[0.0016]
Return 3 yrs			0.0029*	0.0028**
			[0.0017]	[0.0013]
Return on Assets			-0.0025	-0.0020
			[0.0109]	[0.0117]
Sales growth 3yrs			-0.0038	-0.0029
			[0.0036]	[0.0032]
Employees			0.0001	0.0009
			[0.0004]	[0.0007]
Dividend yield			0.0026	0.0251
			[0.0252]	[0.0243]
Price/earnings			0.0003	0.0003
			[0.0002]	[0.0002]
S&P 500 dummy			0.0124	-0.0106
			[0.1383]	[0.1111]
age				0.0173**
				[0.0080]
age^2				-0.0004
				[0.0008]
tenure				-0.0017
				[0.0096]
tenure^2				-0.0015
				[0.0010]
Time dummies	No	Yes	Yes	Yes
2 digit SIC dummies	No	No	No	Yes
Observations	811	811	777	764
R^2	0.172	0.180	0.211	0.343
number of CEOs	447	447	430	423

♣ comprised of Salary, Bonus, Other Annual, Total Value of Restricted Stock Granted, Total Value of Stock Options Granted (using Black-Scholes), Long-Term Incentive Payouts, and All Other Total (tdc1 item in *Execucomp*). The main regressor of interest is *Handicap*, the golf handicap for the given year as reported in the relevant *Golf Digest* ranking. We compute the mean golf handicap for each CEO across the years in which he appears in the sample. The *Handicap* variable is instrumented with two dummy variables which take the value of 1 if the CEO falls in the top or middle terciles respectively of the distribution of the mean golf handicaps.
Standard errors [in brackets] consistent in the presence of arbitrary within CEO autocorrelation and heteroskedasticity (see Wooldridge 2002, eq. 8.33). Significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 1.18: Instrumental variable regression of log of total current compensation♣ on playing golf.

	(1)	(2)	(3)	(4)
Handicap	-0.0000	-0.0088	-0.0099	-0.0140**
	[0.0070]	[0.0071]	[0.0079]	[0.0063]
Log(mktvalue)	0.2224***	0.2104***	0.2100***	0.1656***
	[0.0352]	[0.0344]	[0.0625]	[0.0411]
Log(book/mkt value)			0.1536*	0.0973
			[0.0822]	[0.0662]
Return 1 yr			0.0052***	0.0050***
			[0.0016]	[0.0013]
Return 3 yrs			0.0026	0.0024
			[0.0021]	[0.0016]
Return on Assets			0.0103*	0.0141***
			[0.0056]	[0.0051]
Sales growth 3yrs			-0.0011	0.0000
			[0.0031]	[0.0028]
Employees			0.0005	0.0012
			[0.0004]	[0.0008]
Dividend yield			0.0044	0.0144
			[0.0163]	[0.0148]
Price/earnings			0.0001	0.0002
			[0.0002]	[0.0002]
S&P 500 dummy			0.0546	0.1182
			[0.1213]	[0.0895]
age				0.0129**
				[0.0064]
age^2				-0.0011
				[0.0007]
tenure				0.0009
				[0.0076]
$tenure^2$				-0.0017*
				[0.0010]
Time dummies	No	Yes	Yes	Yes
2 digit SIC dummies	No	No	No	Yes
Observations	809	809	775	762
R^2	0.108	0.160	0.218	0.362
number of CEOs	446	446	429	422

♣ comprised of salary and bonus (tcc item in *Execucomp*). The main regressor of interest is *Handicap*, the golf handicap for the given year as reported in the relevant *Golf Digest* ranking. We compute the mean golf handicap for each CEO across the years in which he appears in the sample. The *Handicap* variable is instrumented with two dummy variables which take the value of 1 if the CEO falls in the top or middle terciles respectively of the distribution of the mean golf handicaps.
Standard errors [in brackets] consistent in the presence of arbitrary within CEO autocorrelation and heteroskedasticity (see Wooldridge 2002, eq. 8.33). Significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

more than \$2.5 million. Such levels of annual income are clearly not all spent on consumption.[12] Hence even the poorest CEOs in our sample are rich enough to afford playing as much golf as they want – let alone notice the accompanying expense.

Prima facie evidence that CEOs are not really optimising golf-playing related expenses is the fact that most belong to more than one golf club.[13] Lastly, there is casual evidence that golf club memberships are considered a legitimate business expense and are often paid by the corporation (for examples, see the article quoted in the last footnote; for systematic evidence on this issue we will have to wait for improved *SEC* requirements for disclosure of executive perquisites).

1.5.2 Golf playing abilities correlated with unobserved productivity

We admit that this is always a possibility and challenge readers to come up with a plausible explanation. What we have shown is that golf playing abilities are contemporaneously uncorrelated with a measurable and relevant criterion, shareholders' returns. Moreover, in the cross section, golf playing abilities do not meaningfully predict shareholders' returns one year ahead.

We find more positive stock price reaction to appointments of bad golfers, CEOs who are in the 3rd tercile of the mean golf handicap distribution.

We also find some weak evidence that bad golfer outperform good golfers on average in the long.

Overall, we conclude that the two alternative rational explanations are not plausible.

[12]Notice that buying a multi-million dollar mansion at the waterfront is not consumption, but investment as it will appreciate in value with the passage of time.

[13]An article in USA Today (July 11, 2006) entitled "CEOs belong to fore – or 5 or even 6 golf clubs" states: "a USA TODAY analysis of 115 CEOs and chairmen of Fortune 1,000 companies who also score good to excellent at golf found 51 who belong to at least two clubs, and 25 who belong to three or more." This could be an underestimate, as the *Golf Digest* survey for 2006 reports that 65% of CEO golfers who run Fortune 1,000 companies belong to at least two private country clubs and 45% belong to four or more.

1.6 Conclusions and implications

Our results show clearly that information – or a cue – that is unrelated to corporate performance is related to the remuneration of CEOs. The presumption therefore is that this cue is used in remuneration decisions whether or not those making the decisions are conscious of its influence. We emphasize that given the inherent difficulty of assessing CEO compensation, it should come as no surprise that the underlying process of judgment is subject to bias. This is simply the nature of human information processing and leads to two questions. The first is why this particular cue – ability to play golf – plays an inappropriate role in these decisions. The second is what might be done to alleviate this, and possibly other biases, in the decision making process.

Given the social context in which CEO remuneration decisions are made, the underlying judgments undoubtedly involve a host of tangible and intangible measures ranging from concrete indicators of past performance to the observation of "soft" social skills and even physical appearance. Moreover, in the USA golf clubs provide locations in which the relevant actors socialise and can judge each other on a variety of dimensions. In this milieu, then, we suspect that being a good golfer is a positive attribute, generating its own "halo" effect.[14]

Since golf handicap does not predict corporate performance, what might be done about this – and possibly other – illusory correlates? Our suggestion goes back to clinical psychology (where illusory correlation was identified) and the classic work of Meehl (1954) who showed that, even for complex diagnostic tasks, predictive ability is improved if human judgment is replaced by simple, explicit statistical rules. Moreover, as demonstrated by a meta-analysis involving some 140 studies (Grove et al., 2000), these findings have only been reinforced with time. As stated by the authors:

[14] Interestingly, an article in the Economist (April 10, 2008) entitled "Addressing the ball" states: "Many chief executives are obsessed with golf. Warren Buffet and Bill Gates are both keen players. Jack Welch, a former boss of General Electric, considered handicaps a good measure of business acumen."

> This study confirms and greatly extends previous reports that mechanical prediction is typically as accurate or more accurate than clinical prediction...
>
> Even though outlier studies can be found, we identified no systematic exceptions to the general superiority (or at least material equivalence) of mechanical prediction. It holds in general medicine, in mental health, in personality, and in education and training settings. It holds for medically trained judges and for psychologists. It holds for inexperienced and seasoned judges (Grove et al., 2000, p. 25).

This does not, of course, mean that no human judgment is involved in mechanical prediction. People still need to identify the variables that are used in formulas. Thus, if decision makers believe that golf handicap is a relevant variable for CEO compensation, it should be explicitly included in the equation. Given the inherent uncertainty in corporate performance, no decision rule – clinical or mechanical – can be a perfect predictor. However, to maximize expected shareholder value, one should clearly use the "best" rule available.

Chapter 2

THE STOCK MARKET BUBBLE, FUNDAMENTAL ATTRIBUTION BIAS AND CEOs COMPENSATION

2.1 Introduction

I present[†] a behavioral theory and empirical evidence relating the recent explosive growth of Chief Executive Officers(CEOs) pay to the stock market bubble of the late 90s. The average total realized compensation of the top 100 CEOs perfectly tracks the stock market index (Figure 2.1). My main finding is that fluctuations in the public equity market conditions mechanically cause CEOs pay and that reverse causality running from CEO pay to the company valuations cannot be detected. I conclude that as the late 90s stock market bubble was developing, shareholders mis-attributed the appreciation of their shares to superior CEOs' leadership and skill, and tolerated excessive CEOs pay for no rational reason. Further, a simple finite distributed lag model shows that increases in CEOs pay are associated with decreases in aggregate profits in the corporate sector, i.e., a negative tradeoff is present.

One might be concerned that these aggregate time series regressions miss important cross sectional relationship between CEOs pay and leadership/skill.

[†]Most of the results in this chapter already appeared in print in Kolev (2008a). The results in Section 2.5 are new with respect to Kolev (2008a).

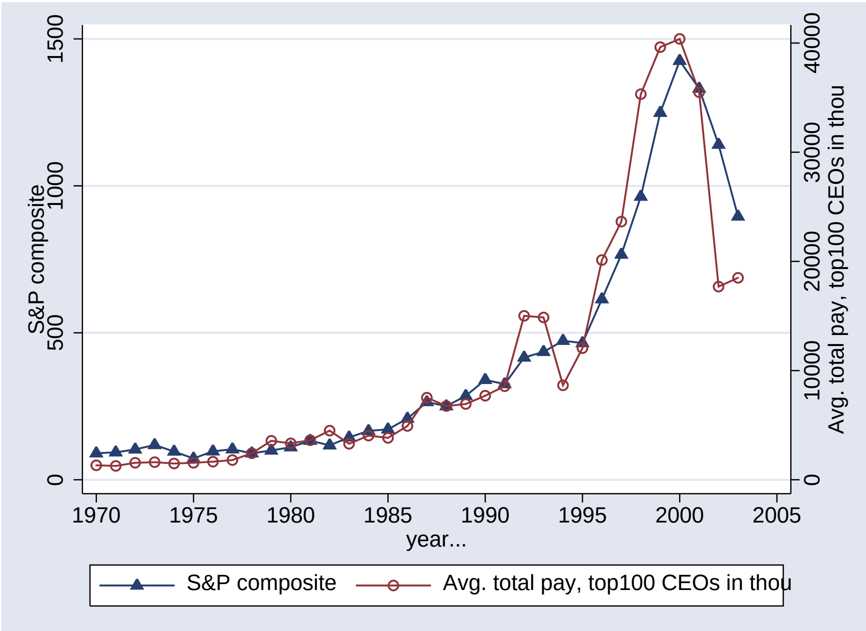

Figure 2.1: S&P composite index and Average total realized pay for the 100 highest paid CEOs in thousands of constant year 2000 dollars.

To explore this issue I construct a direct measure of individual CEO skill, namely risk-adjusted abnormal returns computed from a 4-factor Fama-French-Carhart model. I show that in the cross section CEO pay is very weakly related to skill. I.e., CEOs who generate higher risk-adjusted abnormal returns for their shareholders are better paid, however the effect is economically negligible and statistically insignificant.

Finally, I use the peak of the "new economy" stock market bubble (years 1997 to 1999) and the burst of the bubble (years 1999 to 2001) as a convenient quasi-natural experiment with a clearly identifiable control group ("old economy" firms) and treatment group ("new economy" firms) to compute differences-in-differences estimates of the effect of the bubble on company stock values and on CEOs pay. Further I combine the reduced form differences-in-differences estimates into Wald (1940) estimates of pay for luck where I use the exogenous variation in stock valuations induced by the bubble to study how CEOs pay responds to changes in company stock valuations which are plausibly *not* related to CEOs effort and skill. I study asymmetries in pay for luck by analyzing the development and the burst of the bubble phases separately.

I use an episode of sharp discontinuous changes where the source of exogenous variation in market values is well understood. The market values of "new economy" firms soared sky-high and then spectacularly came back down to earth in about four years due to investors irrational exuberance or due to genuine investors valuation mistakes. Either way it was due to investors' folly and in well functioning corporate governance system[1] CEOs pay must not directly depend on investors folly. I do not need to resort to instrumental variable techniques with tenuous exclusion restrictions as previous studies do. The exclusion restrictions employed in previous research are somehow more tenuous because it might very well be that CEO pay *has to* fluctuate with the state variables employed as instruments (oil prices, average industry performance, IPOs etc) because the reservation wage of the CEO is a function of these state variables reflecting the fortunes of the industry (see Oyer 2004 for a model in these lines). This critique also holds for the aggregate time series instrumental variable regressions that I present.

Two added benefits of the methodology I use are that firstly, it is uncontroversial in this context what is good luck (bubble developing) and bad luck (bubble bursting) from the point of view of the "new economy" CEOs. Secondly, I do not need to assume linear compensation contract as is commonly done in the literature. The differences-in-differences and Wald estimates I report are simple fully nonparametric comparisons of group means.

In Figure 2.2 I plot the market value of equally weighted portfolios of "new economy" and "old economy" firms. I follow the classification of Murphy (2003).[2] In Figure 2.3 I plot value weighted portfolios. Either way, it is clear from these figures that new and old economy firms exhibit more or less similar growth in value, except for the bubble years. In other words,

[1]The US corporate governance system is still claimed by many US academics and practitioners to be a well functioning one, from which the rest of the world should learn, and which the rest of the world should mimic.

[2]Previous studies of the bubble period and how it affected CEO compensation in "new economy" firms include Anderson, Banker and Ravindran (2000), Ittner, Lambert and Larcker (2003), and Murphy (2003).

there is a case for taking the “new economy” firms as being similar to “old economy” firms in the only respect that should matter – shareholder value creation over the long run. Yet “new economy” firms experienced overvaluation and subsequently correction stages, while “old economy” firms did not. This allows us to study how mistaken valuations propagate into CEOs compensation.

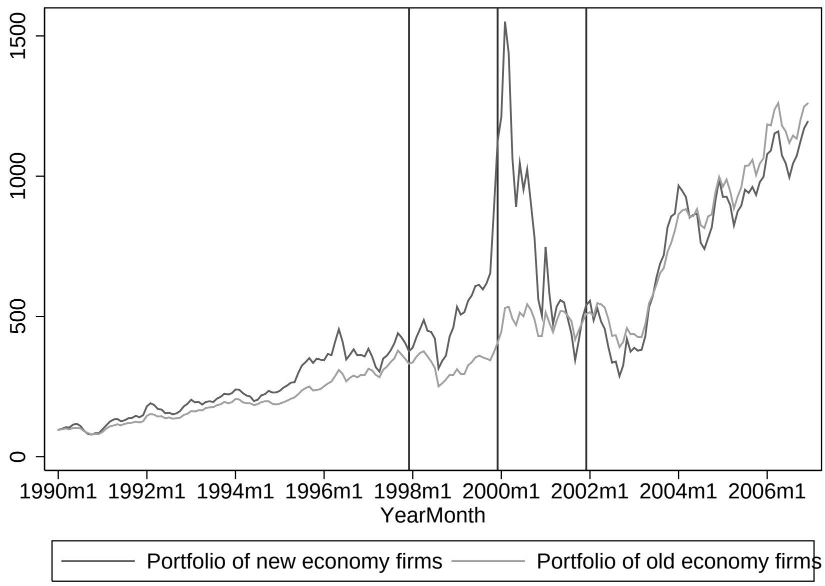

Figure 2.2: Monthly *equally weighted* portfolios of “New economy”(primary SIC codes 3570, 3571, 3572, 3576, 3577, 3661, 3674, 4812, 4813, 5045, 5961, 7370, 7371, 7372, and 7373) and “Old Economy” firms(SIC codes less than 4000 not otherwise categorized as new economy). Source: CRSP.

As we can clearly see from the figure, the market value of “old economy” firms (the control group) kept growing at a steady rate. The market value of “new economy” firms (the treatment group) more than doubled and then halved back (all of this in less than four years). It is hard to attribute this spike to any reasonable fundamental valuation considerations and even less so to the “new economy” CEOs effort or skill.

Fundamental attribution error (see, e.g., Weber, Camerer, Rottenstreich,

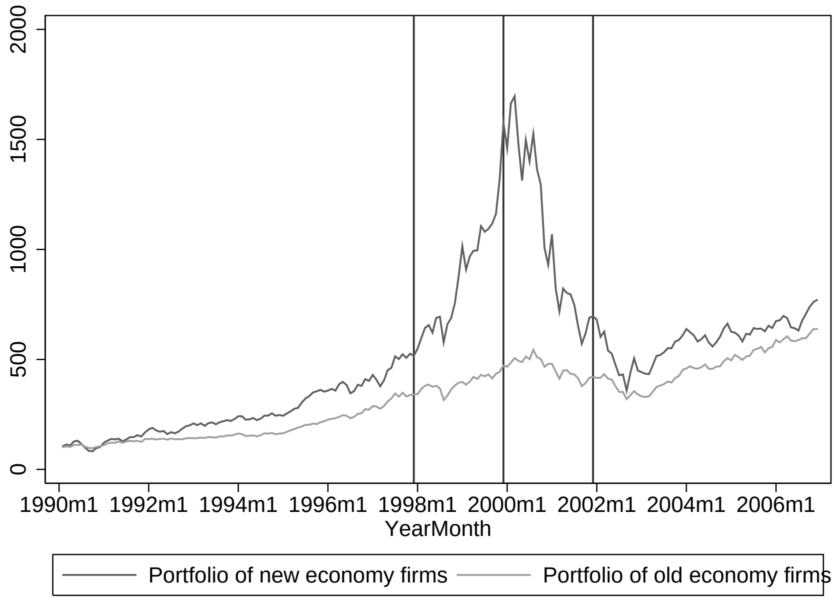

Figure 2.3: Monthly *value weighted* portfolios of "New economy"(primary SIC codes 3570, 3571, 3572, 3576, 3577, 3661, 3674, 4812, 4813, 5045, 5961, 7370, 7371, 7372, and 7373) and "Old Economy" firms(SIC codes less than 4000 not otherwise categorized as "new economy"). Source: CRSP.

and Knez, 2001; Durell, 2001, and references therein for experimental studies documenting this phenomenon) in the context of CEOs pay implies that when shareholders and boards of directors have to evaluate their CEOs and to judge their performance in the noisy market environment, they will tend to attribute the observed outcome to the CEOs ability and effort and to underweight external market factors. Therefore when the stock of the company is doing well just because the whole market is on a rise,[3] shareholders and boards would see superior CEOs' performance. When the stock price is declining because of unfavorable market conditions, as it happened in year 2000 when it became apparent that the internet technology will not bring about as large productivity increase as expected and the bubble burst, they would tend to blame it on their CEOs. This bias is rooted in human beings' cognitive limitations. People find it impossible to take into account the whole complex interplay between economic, sociological and political factors. Therefore as a convenient shortcut, outcomes are attributed to prominent persons.

If the stock market bubble coupled with shareholders' fundamental attribution bias caused excessive CEOs pay, then in a statistical model explaining CEOs total pay as a function of firm market value, the firm value must be *exogenous*. If, on the other hand, the sky-high CEOs pay and the world practice of awarding stock options reflects optimal incentives contracts, where shareholders have to increase the level of CEO pay so that the participation constraint is satisfied, then the firm market value must be *endogenous*. One must be able to detect reverse causality running from the CEOs pay to stock market performance, because shareholders have retained the best CEOs, who have both the incentives and the ability to increase shareholders' value.

I proceed as follows. In section (2.2) I present the literature on the fundamental attribution error and link its implications to CEOs compensation. In section (2.3) I present the aggregate time series evidence. First, I show that one cannot reject the null hypothesis that aggregate CEOs pay responds

[3]The reason for steeply increasing stock market value can be the advent of a new technology or a stock market bubble. Ex post it seems clear that both these effects were in place in the late 90s.

only to exogenous public equity market fluctuations. Second, increases in aggregate CEOs pay are associated with decreases in aggregate corporate profits. In section (2.4) I show that in the cross section individual CEO pay is not meaningfully related to CEO skill, measured as excess returns from a 4-factor Fama-French-Carhart model. In section (2.5) I present the evidence from the quasi-natural experiment offered by the peak and trough of the "new economy" bubble. I the last section I conclude.

2.2 Fundamental Attribution Error

In a corporation shareholders are the principals. In the traditional view of CEO pay, by assumption shareholders are represented by their board of directors. This assumption does not fit the facts in large corporations with dispersed shareholders, as explained in Bebchuk and Fried (2004) and Bebchuk, Fried and Walker (2002). Board of directors have closely aligned interests with the CEO, and unfortunately these interests are orthogonal to shareholders' interests. Therefore what probably happens in practice is that the board of directors and the CEO collude and extract the maximum possible rents from the shareholders subject to the *outrage constraint*. If this is the case, then the shareholders' fundamental attribution error would act through relaxing the outrage constraint.

Conversely, if the traditional view is a correct description of reality, the fundamental attribution error would act through incentive schemes which do not filter out overall market effect beyond CEOs control and through excessive *levels* of CEOs pay in the market bubble period, as the shareholders incorrectly believe that they have super CEOs to be retained at all cost.

In my empirical tests I cannot distinguish between the traditional and the rent extraction views, as long as the shareholders commit the fundamental attribution error, which is my null hypothesis. They provide financing but otherwise delegate the responsibility of running the day to day business to the CEO, who is their agent. Shareholders and board of directors' judgements of the CEO abilities and performance are essential for the hiring, firing, promotion and remuneration decisions. In turn, these decisions are essential for corporate efficiency. However, the shareholders and the boards of directors have to evaluate CEO performance in extremely noisy environment. CEO performance depends partially on his ability and effort, but also it depends to a great extent on the market conditions.

Experimental studies on how people evaluate ability in noisy environment suggest that people do not adjust their assessment sufficiently to account for the exogenous effects that are beyond agents' control. This systematic excessive attribution of outcome to internal characteristics of the agent, e.g., ability, leadership, skill, effort, etc., and the underplay of the situation factors is known as *fundamental attribution error*.

Jones and Harris (1967) were the first to establish people's tendency to over-emphasize agents' intrinsic dispositions and under-emphasize the role of situational influences. In a classic experiment they had their subjects read pro- and anti- Castro essays. Even when their subjects were specifically told that the essay writers were randomly assigned the task to write pro- or anti-Castro essays and *instructed* to support the position in question, the subjects still believed that the essay writer held the viewpoint of the essay.

Ross, Amabile and Steinmetz (1977) randomly assigned people to the role of a questioner or a contestant. In each pair the questioner had to generate 10 questions and the contestant had to answer these questions. Then a third subject (the observer) had to rate both the questioner's and the contestant's general knowledge. Although it was obvious that the questioners had role advantage and could not possibly look stupid as they generated the questions, the observers still systematically judged the questioners to be much more knowledgeable than the contestants.

In both of the quoted studies essay writers and quiz participants were randomly assigned to their roles, the observed outcomes were mainly determined by these roles and the subjects that had to make the evaluation knew that. Nevertheless, subjects attributed outcomes to stable internal characteristics, rather than to environmental/role factors.

In the context of firm performance and CEOs pay, shareholders and the board of directors observe an outcome – the price of the shares of the company and company profits – and have to evaluate to what extent this outcome is due to the CEO leadership, effort and ability, and to what extent the outcome is due to general stock market fluctuations beyond CEO control. On the basis of this evaluation shareholders and the board of directors decide on the appropriate CEO pay and whether they should retain this particular CEO or fire him and hire a new, better qualified CEO.

The evaluation process is extremely difficult because the shareholders and boards of directors cannot observe the counterfactual. As a concrete example take a company with internet related business operating during the internet bubble episode of late 90s. The shareholders and the board of directors would observe the stock of the company spectacularly increasing in value under the leadership of their CEO. What they cannot observe is a) an alternative scenario where a different, e.g., randomly selected CEO is running their company and b) another scenario where the actual CEO is in charge but there is no stock market bubble developing. The important question is "do shareholders and board of directors make on average unbiased evaluation of their CEOs or their attribution of performance is biased in a particular predictable manner?"

More recently and more relevantly to the issue we have at hand, Durell (2001) shows that subjects predict better performance and are willing to pay more to hire agents with good scores even on easy quizzes. Subjects had ample feedback, experience with the task and incentives for accurate assessment. Nevertheless biased attribution persisted.

Weber et al. (2001) have groups of subjects play a *weak-link coordination game.* Previous experimental studies of the weak-link game, e.g., Van Huyck et al. (1990) and Knez and Camerer (1996), have shown that coordination on the Pareto efficient outcome is much easier for small groups of players than for large groups of players.[4]

[4]Large groups never manage to coordinate on the efficient equilibrium. In Van Huyck et al. (1990) experiments, out of seven sessions played with groups of size 14 to 16 people, after the third period all groups ended up in the worst possible Nash equilibrium. On the other hand, 12 out of 14 small groups of size of 2 people managed to coordinate on the Pareto efficient outcome. Therefore there is a strong group size effect.

Weber et al. (2001) use the group size as a situation variable. They randomly assign their subjects to two conditions – small group condition (2 people) and large group condition (9 or 10 people). After two or three rounds of each group playing the game, one subject was randomly assigned to be the "leader" and was instructed to make a speech exhorting the other players to coordinate on the efficient Nash equilibrium. Authors found that "subjects underestimate the strength of the situation effect (group size) and attribute cause to personal traits of the leaders instead" a phenomenon authors label "illusion of leadership." Therefore their subjects felt that the randomly assigned leaders are to be blamed for the inability of the large groups to coordinate on the efficient outcome and credited for the success of the small groups in their coordination on the Pareto efficient Nash equilibrium. Subjects' fundamental attribution error affected also their behavior, as in a second experiment subjects voted more often to replace their leaders (at a small cost to themselves) in the large group condition.

The study of Weber et al. (2001) puts the empirical results I present in the next sections into perspective. Authors use an environmental variable (group size) which determines outcomes and can be manipulated at will in experimental set up. In large groups which are bound to achieve bad performance subjects blame it on the person randomly assigned as the leader. In small groups likely to achieve good performance subjects incorrectly credit the randomly assigned leader with the success. Making the reasonable assumption that shareholders are prepared to pay more to what they believe to be good leaders, the findings of Weber et al. are in perfect accord with Figure 2.1 – the realized average total pay of each year's 100 superstar CEOs tracks the stock market perfectly.

Unlike the group size, the stock market cannot be manipulated at will. To overcome this problem I use exogenous stock market shifters, IPO underpricing and the number of IPOs, to separate the part of the variation in the stock market index which is unrelated to the leadership and skill of the top 100 CEOs, and use only this variation to explain CEOs pay. Similarly, I do not have the benefit to randomly assign people as CEOs and therefore unlike Weber et al. I cannot ensure that the top 100 CEOs are not systematically better leaders than the other CEOs. However, as the 100 highest paid CEOs run the companies with roughly the largest market capitalization and weight in the stock market index, their potential systematic superior leadership should manifest through reverse causality going from top 100 CEOs pay to their market performance. This is testable, if one believes that the IPO variables are valid instruments.

2.3 Aggregate Time Series Evidence from the Long Run

2.3.1 Data Description

I assemble a time series data set at yearly frequency containing the following variables:

- *Avg_top100* – total average realized pay of the top 100 CEOs (in terms of pay). Total pay includes salary and bonus, stock options exercised during the year, the value of restricted stock awarded, and the value contingent pay. Emmanuel Saez constructed this series from Forbes magazine survey of 800 CEOs of the largest US corporations from 1970 to 2003. The series is available on his web site in the updated data set from his paper Piketty and Saez (2003). Executive pay is in thousands in terms of year 2000 constant dollars.

- *SandP* – S&P composite index. The data on S&P composite index is taken from Robert Shiller's web site.

- *nipo* – yearly number of Initial Public Offerings(IPOs).

- *ripo* – average first day IPO returns in a given year. The last two series use Jay Ritter's data, cover the years 1962 to 2004, and are available on Jeffrey Wurgler's web site in relation to his paper Baker and Wurgler (2005).

- *Profits* – Profits before tax in the corporate sector without Inventory valuation adjustment and Capital consumption adjustment, in constant, year 2000 billions of dollars, for year 1929 to 2004, downloaded from the web site of the Bureau of Economic Analysis.

- *Hours* – average weekly hours of production workers from the Current Employment Statistics survey, years 1964 to 2004, downloaded from the web site of the Bureau of Labor Statistics.

I firstly conduct Dickey-Fuller tests with trend on each of the time series above using all available years for a given series. For all of the variables, except for *nipo* and *ripo* which I use as instruments, the null hypothesis of unit root cannot be rejected. Therefore I take first differences of all the variables except *nipo* and *ripo*. Then I conduct Dickey-Fuller tests with trend on all first differenced series. The unit root null hypothesis can be rejected in all differenced series.

In the regressions I report from now on, the sample is restricted to the time span for which all the variables are available, which is determined by the shortest time series I have, *Avg_top100*. Therefore my sample covers the years 1970 to 2003 for a total of 34 observations.

2.3.2 CEOs Pay and the Stock Market

The time series of the average total realized pay of the 100 highest paid CEOs tracks closely the S&P composite index, Figure 2.1. These top 100 CEOs receive huge amounts of money (in year 2000 the average total pay for the group peaked at more than $40mln) supposedly because they have great leadership skill. Let us assume that we can describe the time variation in the average total realized pay of the top 100 CEOs (*Avg_top100*) by the following linear equation:

$$Avg\_top100_t = \alpha + \beta * SandP_t + \gamma * Leadership_t + \varepsilon_t \tag{2.1}$$

where *SandP* is the S&P composite index and *Leadership* is the average ability in the group of top 100 CEOs to maximize shareholder value and ε_t is white noise. If the money that the shareholders pay to their CEOs is wisely spent to retain the best leaders and induce them to maximize shareholders' value, then *SandP* must be endogenous in the following estimable equation:

$$Avg\_top100_t = \alpha + \beta * SandP_t + \zeta_t \tag{2.2}$$

where $\zeta_t \equiv \gamma * Leadership_t + \varepsilon_t$. The 100 highest paid CEOs preside over the companies with roughly the largest market capitalizations and therefore weight in the S&P composite index. Enlightened shareholders' value maximization in these companies should have a positive effect on the value of the S&P index. In the same time, if the compensation contracts are structured to motivate CEOs to do what is best for their shareholders, high values of average total realized pay should result in years in which the top 100 CEOs did on average great job, i.e., years in which *Leadership* was high.

As *Leadership* is not observable and we leave it in the error term in the estimable equation (2.2), the Ordinary Least Squares estimator $\hat{\beta}_{OLS}$ should be a *biased* estimator of the structural parameter β due to endogeneity.[5] We cannot observe *Leadership*, so direct estimation of (2.1) is not feasible. Yet we can obtain unbiased estimator $\hat{\beta}_{IV}$ of the structural parameter β by an Instrumental Variable (IV) regression applied on equation (2.2).

Having $\hat{\beta}_{OLS}$ and $\hat{\beta}_{IV}$ in hand, we can compare them and see whether they are statistically different, i.e., we can carry out Hausman's test of endogeneity. Note that we do not require $\beta = 0$ – it might be that the outside opportunities of the CEOs improve when the economy is doing well, so that the market index has an impact on CEOs pay even if *Leadership* is held fixed. However we do require that $\hat{\beta}_{OLS}$ and $\hat{\beta}_{IV}$ be statistically different, or in other words that *SandP* be endogenous in (2.2).

If shareholders commit the fundamental attribution error, then *Leadership* would not matter either because the 100 highest paid CEOs do not have any

[5]In this context endogeneity simply means that *SandP* and ζ_t are positively correlated, because *SandP* and *Leadership* are positively correlated and $\zeta_t \equiv \gamma * Leadership_t + \varepsilon_t$.

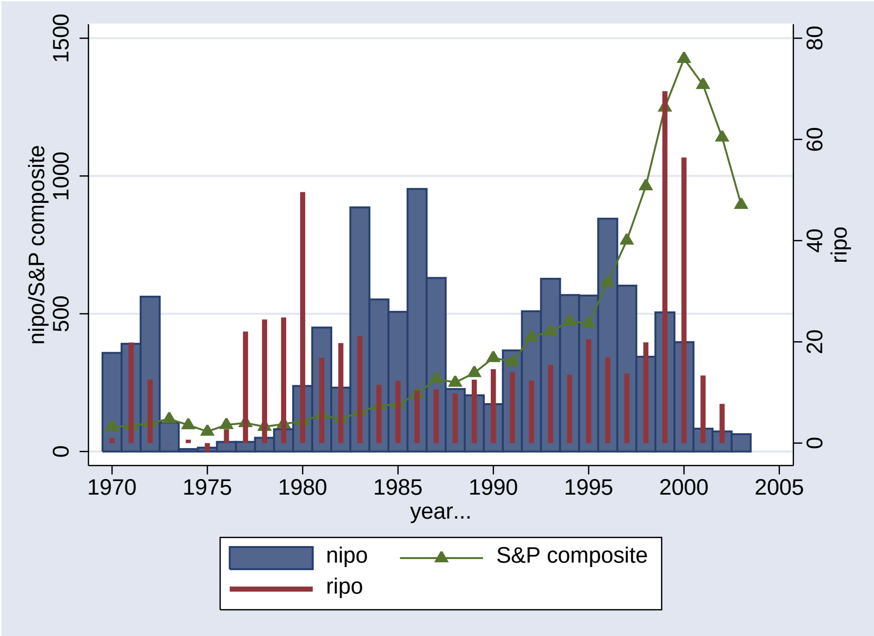

Figure 2.4: S&P composite index, Number of IPOs(nipo) and Average first day IPO returns(ripo).

superior abilities, i.e., they are just random people, or because shareholders get it all wrong and reward their CEOs for market fluctuations beyond CEOs' control and not for true leadership. S&P composite index would be exogenous in equation (2.2), i.e., $\zeta_t = \varepsilon_t$ would be white noise. In this fundamental attribution bias based model the two estimators $\hat{\beta}_{OLS}$ and $\hat{\beta}_{IV}$ should be different only up to sampling error. Shareholders would tolerate excessive CEOs pay when the market is doing well, whether or not their CEOs are superior leaders.

I use the number of IPOs (*nipo*) and the average first day returns of IPOs (*ripo*) as instruments for *SandP*. Figure 2.4 visualizes the time variation in the three variables.

To be valid instruments, *nipo* and *ripo* must be

1. correlated with *SandP*
2. uncorrelated with *Leadership*.

Little is known about the fundamental causes of time series variation in the number of IPOs and IPO underpricing. The following three are natural explanations:

1. The number of IPOs varies with the business cycle. During economic expansions the demand for capital is higher and more firms go public.
2. The number of IPOs might be driven by aggregate changes in investor optimism: "When investors get bearish, you can't go public. But when they go bullish, just about anyone can go public(Wall Street Journal, April 19, 1999)."
3. The number of IPOs might reflect changes in the aggregate uncertainty, i.e., the aggregate severity of the lemons problem.

These explanations were investigated in Lowry (2003). She concludes that the first two are both statistically significant and economically important determinants of IPO volume. Adverse selection costs (the third explanation above) appears to be statistically significant, but not economically important.

The **first stage regression** confirms that the instruments are correlated with the stock market index:

$$\begin{array}{llll} \widehat{dSandP} = & -78.286 & +3.008 * ripo & +.138 * nipo \\ & (26.506) & (.933) & (.051) \\ N = 33 & R^2 = .408 & & \end{array}$$

where *dSandP* is the first differenced time series of the S&P composite index (I took first differences to make the series stationary) and the usual Ordinary Least Squares (OLS) standard errors are reported in parentheses below the estimated coefficients. Both instruments are significant at the 1% significance level and together they explain about 40% of the variation in the stock market index.

The assumption that *ripo* and *nipo* are uncorrelated with *Leadership* involves a variable which is not observable, so it cannot be directly verified.

I conduct over-identifying restriction test, which is implementable as I have one more instrument than I need to identify the model. The test shows that the hypothesis that the instruments are valid cannot be rejected.

The two variables *ripo* and *nipo* reflect decisions taken by the set of firms which go public in a given year. Most importantly, this set is *different* from the set of firms which are presided over by the 100 best paid CEOs. Therefore how good leaders on average are the 100 highest paid CEOs is not related to how much firms that go public underprice their shares and to when these firms decide to go public.

I argue that even if the IPO firms and the firms with the highest paid CEOs were the same, which they are not, still *ripo* and *nipo* are good instruments. Assume for the sake of the argument that IPO variables are bad instruments and are in fact correlated with leadership. The first stage regression, together with the fact that good leadership has positive effect on the stock market index, shows that if CEOs underprice more on average and go public when many other firms go public, they are better leaders. Leadership means enlightened shareholder's value maximization. It is hard to see why leaving more money on the table, i.e., underpricing more, should be good for the existing shareholders and should reflect superior leadership on the side of the CEO.

Loughran and Ritter (2004) argue that the increase in IPO underpricing in the 90ies is due to changing objectives, where the decision makers of the firms going public care more about analyst coverage and directly benefit from underpricing through side payments (at the expense of their shareholders). Krigman, Shaw and Womack (1999) show that first day IPO returns predict subsequent performance and that institutional investors apparently have the ability early on to get rid of subsequently under-performing IPOs. Authors conclude that underwriters pricing errors are intentional. Note that on average for a given year IPOs are always underpriced, so on average the firms going public always lose from underwriters intentional pricing errors.

Similarly, the strategy of going public when the market is hot and many other firms go public is fairly mechanical. Considering this follow-thecrowd strategy enlightened leadership, which is to be rewarded by multi-million compensation, seems far fetched.

Estimating equation (2.2) by two stage least squares using the number of IPOs and the average first day IPO returns as instruments for S&P composite index gives the following fitted equation:

$$\begin{array}{lll} \widehat{dAvg\_top100} = & -52.576 & +23.473 * dSandP \\ & (680.562) & (10.282) \\ N = 33 & R^2 = .391 & \end{array}$$

where *dAvg_top100* is the first differenced time series of the average realized total pay of the 100 highest paid CEOs. Differencing was applied to both the regressor and the regressand to make them stationary. The coefficient of S&P composite index is significant at the 5% level. Therefore the structural parameter β in equation (2.1) is different from 0 – the stock market has an effect on CEOs pay even when *Leadership* is taken into account.

Estimating equation (2.2) by OLS gives:

$$\begin{array}{lll} \widehat{dAvg\_top100} = & -212.164 & +30.011 * dSandP \\ & (642.102) & (6.465) \\ N = 33 & R^2 = .41. & \end{array}$$

To check whether the OLS and IV estimates are statistically different we can compare directly only the coefficient of interest (Wooldridge 2002, page 120):

$$\text{Hausman } t = \frac{\hat{\beta}_{IV} - \hat{\beta}_{OLS}}{\sqrt{[\mathrm{se}(\hat{\beta}_{IV})]^2 - [\mathrm{se}(\hat{\beta}_{OLS})]^2}} = \frac{23.473 - 30.011}{\sqrt{10.282^2 - 6.465^2}} = -0.8177$$

with an associated p-value of 0.4135. Therefore we cannot reject the null hypothesis of exogeneity at any reasonable level of significance. The IV and OLS estimates differ only due to sampling error, the S&P composite index is exogenous and purely mechanically causes excessive CEOs pay. This evidence is consistent with shareholders' fundamental attribution bias and inconsistent with the view that CEOs pay reflects optimal incentives contracts.

I carry out overidentifying restrictions test regressing the IV residuals on the instruments and using the $N * R^2 \sim_a \chi^2(1)$ as a test statistic (N is the number of observations and R^2 is the usual R-squared from this regression) as explained in Wooldridge (2002, pages 122-124). It turns out that $N * R^2 =$ .604, which is less than the critical value of the χ^2 distribution with 1 degree of freedom for any reasonable significance level (the p-value of the test is 0.437). Therefore this test does not reject the hypothesis that the instruments are valid.

Throughout this section I have reported the usual standard errors which are derived under homoskedasticity. The validity of the versions of endogeneity and over-identifying restrictions tests that I have implemented also rely on this assumption. To check whether heteroskedasticity is a problem here I conduct White's test regressing the squared IV residuals on the two instruments, the squares of the two instruments and their cross product. The F-statistic for the joint significance of the regressors in this auxiliary regression is 0.23 (the p-value of the test is 0.94) and therefore heteroskedasticity is not a problem here, i.e., the variance of the error is not a function of the exogenous variables.

The over-identifying restrictions test gives credibility to my instruments if the instruments are chosen by the same logic. One can argue that the IPO underpricing and the number of IPOs reflect different phenomena. Therefore I repeated the analysis above in one case using *nipo* and lagged *nipo* as instruments and in the other using *ripo* and lagged *ripo*. The results were qualitatively the same. The S&P index always appeared exogenous and in neither case I could reject the hypothesis that the set of instruments is valid. When *nipo* and lagged *nipo* are used as instruments, the IV estimate of the coefficient on the S&P index is 31.754, much closer to the OLS estimate, with a standard error of 13.574 . When *ripo* and lagged *ripo* are used as instruments, the IV estimate of the coefficient on the S&P index is 19.562 with a standard error of 12.969.

2.3.3 Aggregate Corporate Profits and CEOs pay

In this subsection I demonstrate that in the aggregate data CEOs compensation has substantial *negative* impact on corporate profits. As bad a measure of shareholder value as they might be, corporate profits have two virtues – they do not depend on investors' perceptions and optimism, and they are a measure of value which is fully based on fundamentals.

I fit a simple finite distributed lag model. I have differenced all variables to make them stationary. The dependent variable is *dProfits*, first difference in corporate profits in billions of constant year 2000 dollars, and the regressors are *dAvg_top100* first difference of the total realized pay of top 100 CEOs in thousands of constant year 2000 dollars (contemporaneous and two lags) and *dHours* first difference in average weekly hours of all production workers.

I have chosen the number of lags that maximizes the adjusted R^2. One can think of this specification as describing a simple linear technology which transforms CEOs input (determined by their total pay) and the variable labor input (average hours worked) into aggregate profits in the corporate sector. In preliminary analysis I included also capital as an additional regressor, but it appeared insignificant in all specification and I dropped it. Theoretical justification for omitting capital in the specification is that it is usually thought of as a *fixed input* which does not change much from period to period.

The results of the estimation follow:

$$
\begin{array}{lllll}
\widehat{dProfits}_t = & 39.419 & -.0018 * dAvg\_top100_t & -.0021 * dAvg\_top100_{t-1} & - \\
 & (10.032) & (.0019) & (.0021) & \\
 & & -.0046 * dAvg\_top100_{t-2} & +205.032 * dHours_t & \\
 & & (.0021) & (47.293) & \\
N = 31 & R^2 = .489. & & &
\end{array}
$$

The usual OLS standard errors are reported in parentheses. The regression explains about 49% of the variation in corporate profits. The coefficients on executive compensation variables are all negative casting doubts on the idea that it is in shareholders' best interest to pay their CEOs millions of dollars. The coefficients on the contemporaneous and one period lagged compensation are insignificant. The coefficient on the two periods lagged compensation is significant at the 5% level. This is not surprising and happens often in finite distributed lags models. The problem is that different lags of compensation are highly multicollinear, and cannot be precisely estimated separately. The three compensation variables are *jointly* significant at the 5% significance level (F-statistic of 3.05 with the p-value of 0.04). There are no signs of first order serial correlation (which I interpret as evidence that the model is correctly specified) nor heteroskedasticity.[6]

The most interesting quantity in a finite distributed lag model is the long run multiplier, i.e., the effect of a permanent unit increase in executive compensation on profits. The long run multiplier is given by the sum of the contemporaneous effect and the effects of the lags. In the finite distributed lag model estimated above the long run multiplier is highly significant, equal to $-.0085$ with a standard error of .0031. This is a huge negative effect implying that \$1,000 permanent increase in the average compensation of the top 100 highest paid CEOs leads to \$8.5 mln decrease in corporate profits.

This is strong evidence for the claim that excessive CEO compensation leads to inefficiencies, beyond the redistribution of money from shareholders to CEOs.

2.4 Fama-French-Carhart 4-factor Model Excess Returns and CEO Pay

In this section I use a direct measure of CEO skill – excess risk-adjusted returns from Fama-French-Carhart 4-factor model (Fama and French,1993;

[6]Denote by $\hat{e}$ the residuals from the finite distributed lag model and by $d\widehat{Profits}$ the fitted values. To test for first order serial correlation I reestimate the finite distributed lag model with $\hat{e}_{t-1}$ added as an additional regressor. The t-statistic for the test that the coefficient on $\hat{e}_{t-1}$ is equal to 0 is 1.40, therefore there is no evidence of first order serial correlation. When I test for heteroskedasticity, to conserve degrees of freedom I implement the version of White's test where in the auxiliary regression $\hat{e}^2$ is regressed on $d\widehat{Profits}$ and $d\widehat{Profits}^2$. The F-statistic for joint significance of $d\widehat{Profits}$ and $d\widehat{Profits}^2$ is 0.97, therefore there is no evidence of heteroskedasticity.

Jegadeesh and Titman,1993; Carhart,1997) – to see how CEO pay varies in the cross section with this measure of skill.[7]

2.4.1 Cross sectional data

I transcribe data on total realized pay (salary, bonus, other compensation and value of options exercised) for each of the 197 CEOs who have at least 6 years long tenure with their company in year 2003 (variable name $totalcomp_i$) from the *Forbes* CEO compensation survey. I hand-match these data to company monthly returns spanning the period from January 1998 to December 2003, company returns computed from data provided by *Datastream*, I denote this variable by R_t^i in equation (2.3).

I am not able to unambiguously match the names of the companies provided by *Forbes* to names of companies in *Datastream* for all cases, and hence I end up with a sample of 184 observations. Further I match these data to *Forbes* data on total realized pay for the year 2001. Some observations are lost in this process too and I have 162 observations with compensation data for years 2001 and 2003 matched to company returns data. Compensation data for year 2001 is translated in terms of constant 2003 dollars using the CPI index. Summary statistics are presented in Table 2.1.

2.4.2 Methodology and Results

I estimate the time-series regression in equation (2.3) at monthly frequency for each of the 184 CEOs who have been with their companies for at least 6 years as of year 2003. This is not a random sample – boards and shareholders had ample feedback on the performance of these CEOs, a feedback which is not available in the whole population. However this biases the

[7]For robustness check, I redid this exercise by constructing skill from risk-adjusted returns from a 3 factor Fama-French model, i.e., omitting the momentum factor and from a CAPM model. It turns out that the momentum factor does not make much of a difference for this sample. CAPM gives different results in terms of risk-adjuster returns, but regarding the relationship between pay and skill, one again reaches the conclusion that they are only weakly related (if related at all).

Table 2.1: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
totalcomp, (year2001 in \$000)	15,045.032	61,876.798	104	734,320.08	162
$\hat{\alpha}$, (year2001,from 3years in% monthly)	1.126	1.927	-2.721	9.656	162
skill, (year2001,from 3years in% monthly)	0.787	2.385	-5.96	9.931	162
totalcomp, (year2003 in \$000)	9,084.734	15,228.641	0	147,970	184
$\hat{\alpha}$, (year2003,from 6years in% monthly)	0.906	1.293	-0.858	8.456	184
skill, (year2003,from 6years in% monthly)	0.067	1.416	-3.221	6.867	184
$\hat{\alpha}$, (year2003,from 3years in% monthly)	0.613	1.347	-4.711	6.16	184
skill, (year2003,from 3years in% monthly)	0.111	1.527	-3.628	6.783	184

totalcomp is the total realized CEO pay in terms of thousands of 2003 dollars, in parentheses is given the year to which the number refers.
$\hat{\alpha}$ is the estimated excess risk-adjusted return for each CEO, from the 4-factor model equation(2.3). In parentheses is given the end year of the sample and the number of years of monthly data which have been used in the estimation. E.g., (year2001,from 3years in% monthly) means "monthly returns in percentage form from January 1999 to December 2001 have been used to estimate the $\hat{\alpha}$ in equation(2.3)."
skill is the estimated $\hat{\alpha}$ plus the average residual from the regression in equation(2.3) for the year in parentheses. E.g., (year2003,from 6years in% monthly) referring to *skill* means "6 years of monthly returns in percentage form from January 1998 to December 2003 were used to estimate equation(2.3), *skill* was constructed by adding to the $\hat{\alpha}$ the average residual for year 2003."

results *towards* finding a relationship between skill and pay. If boards of directors cannot get the compensation for these CEOs right, they would never be able to get it right for the other CEOs for whom performance record is not available.

$$R_t^i - r_t^f = \alpha^i + \beta_1^i MktRf_t + \beta_2^i SMB_t + \beta_3^i HML_t + \beta_4^i MOM_t + \varepsilon_t^i \qquad (2.3)$$

In this equation R_t^i is the monthly return for company i, r_t^f is the risk-free rate (one-month Treasury bill rate), $MktRf_t$ is the value-weight return on all NYSE, AMEX, and NASDAQ stocks in excess of the risk-free rate, SMB_t (Small Minus Big) is the average return on the three small portfolios minus the average return on the three big portfolios, HML_t (High Minus Low) is the average return on the two high book-to-market portfolios minus the average return on the two low book-to-market portfolios and MOM_t (momentum factor) is the average return on the two high prior return portfolios minus the average return on the two low prior return portfolios. Factors were downloaded from Kenneth French's webpage and details about the construction of the portfolios can be found there.

To construct the *skill* variable I estimate equation (2.3) on the previous 3 years of monthly data, then skill is the intercept plus the average residual for the given year. E.g., to construct *skill* for year 2003, I estimate the regression from January 2000 to December 2003 for the measure based on 3 years (and from January 1998 to December 2003 for the measure based on 6 years) to get estimates of the parameters in equation(2.3). Then

$$skill^i = \sum_{t=\text{Jan2003}}^{t=\text{Dec2003}} \left[(R_t^i - r_t^f) - (\hat{\beta}_1^i MktRf_t + \hat{\beta}_2^i SMB_t + \hat{\beta}_3^i HML_t + \hat{\beta}_4^i MOM_t) \right]$$

Analogous procedure is applied to measure skill for year 2001 (only skill measure based on 3 years of data is available here). This methodology parallels Carhart (1997, page 67). Summary statistics for the skill measures are presented in Table 2.1.

In what follows I interpret the results as if the 4 factors are risk factors and hence the term

$$\hat{\beta}_1^i MktRf_t + \hat{\beta}_2^i SMB_t + \hat{\beta}_3^i HML_t + \hat{\beta}_4^i MOM_t$$

is a proper measure of the cost of capital for firm *i*. I.e., the return in excess of the risk free rate the investors will *require* for bearing the systematic risk given by the factor loadings for company *i*. Yet it is important to point out that this is a matter of interpretation only. Carhart (1997, page 61) writes:

> The 4-factor model is consistent with a model of market equilibrium with four risk factors. Alternatively, it may be interpreted as a performance attribution model, where the coefficients and premia on the factor mimicking portfolios indicate the proportion of mean return attributable to four elementary strategies: high versus low beta stocks, large versus small market capitalization stocks, value versus growth stocks, and one-year return momentum versus contrarian stocks.

Having constructed proxy for CEO skill I fit bivariate regressions where the dependent variable is CEO total pay and the regressor is the measure of skill. Results are reported in Table 2.2.

In none of the regressions one can reject the null that the coefficient on *skill* is 0. More importantly, the coefficient on *skill* is economically negligible. Take for example the regression in the third column, which gives the largest coefficient on *skill*. What it says is that in year 2001 a CEO who barely broke even at the cost of capital (and hence in this framework there is no sense in which he has any exceptional skill) received the stunning total pay of above $13mln.

In the same time a CEO who generated an abnormal risk-adjusted return of 1% per month, which is an outstanding performance, received only additional $2mln. Note that when the skill measure is based on 6 years of data (the first column) the coefficient on the *skill* variable is actually *negative*.

Table 2.2: CEO compensation and *skill*

	Dependent variable is *totalcomp*(in \$000)				
	year2003	year2003	year2001	Pooled OLS	FixedEffects
skill, from 6 years	-210.624				
	(796.890)				
skill, from 3 years		950.252	2,124.914	1,751.361	163.411
		(735.652)	(2,044.337)	(1,519.578)	(2,908.304)
year2003				-4,776.347	-5,803.596
				(5,460.296)	(6,497.735)
intercept	9,098.923	8,978.866	13,371.820	13,665.960	14,916.360
	(1,126.814)***	(1,123.618)***	(5,119.936)***	(5,428.287)**	(4,833.137)***
N	184	184	162	346	324
R^2	.0004	.009	.007	.011	.559

Dependent variable in each regression is the total realized CEO pay in thousands of constant 2003 dollars. In the first column data only from year 2003 is used and the *skill* regressor is the $\hat{\alpha}$ estimated from equation (2.3) on the previous 6 years of monthly returns, plus the average residual for year 2003. In the second column only data from year 2003 is used and the *skill* regressor is the $\hat{\alpha}$ estimated from equation (2.3) on the previous 3 years of monthly returns, plus the average residual for year 2003. In the third column only data from year 2001 is used and the *skill* regressor is the $\hat{\alpha}$ estimated from equation (2.3) on the previous 3 years of monthly returns, plus the average residual for year 2001. In the fourth column pooled data from years 2001 and 2003 is used and the *skill* regressor is the $\hat{\alpha}$ estimated from equation (2.3) on the previous 3 years of monthly returns, plus the average residual for the year for which skill refers to. Estimation procedure is Ordinary Least Squares. In the fifth column pooled data from years 2001 and 2003 is used and the *skill* regressor is the $\hat{\alpha}$ estimated from equation (2.3) on the previous 3 years of monthly returns, plus the average residual for the year for which skill refers to. Estimation procedure is CEO Fixed Effects panel data regression.

In parentheses below the coefficients heteroskedasticity consistent standard errors are reported. The symbol *** means the coefficient is significant at the 1% level.

In the last column I estimate a fixed effects model

$$totalcomp_{it} = \gamma * skill_{it} + \tau * Year2003_{it} + \psi_i + \varepsilon_{it}$$

where *Year2003* is a dummy equal to 1 if year is 2003 and ψ_i are CEO specific fixed effects.

This appears to be the correct model as I can reject the null that all fixed effects are equal (heteroskedasticity consistent F(161,160)-statistic= 5.68). Note that the fixed effects model yields the smallest coefficient on the *skill* variable and the largest intercept from all the specifications. The intercept reported in the fixed effects model is given by

$$\widehat{intercept} = \overline{totalcomp} - \hat{\gamma} * \overline{skill} - \hat{\tau} * \overline{Year2003}$$

where bars denote the grand means of the variables, i.e., computed from the pooled data from years 2001 and 2003.

To summarize, CEO skill measured by risk-adjusted abnormal returns, which is probably the only thing well diversified shareholders care about, does not seem to determine CEO pay.

2.5 The "New Economy" Stock Market Bubble and Evidence for Fundamental Attribution Bias from Quasi-Natural Experiment

2.5.1 Data

Execucomp is the source of the data used in this subsection. The compensation variables are taken directly from there, as well as firm market values and returns on common equity including dividend distributions. Corporate governance index data (G-index) is taken directly from Andrew Metrick's home page.

2.5.2 Methodology and Notation

The methodology I employ is differences-in-differences analysis (see e.g., Meyer 1995 for a survey). To allow for asymmetries, the analysis is done separately for the overvaluation stage of the bubble (from year 1997 to year 1999) and the correction stage (year 1999 to year 2001). The control group is the subset of "old economy" firms (not affected by the bubble) and the treatment group is the subset of "new economy" firms (the ones affected by the bubble). Let V_y^x denote the market value of the firm in year x and in group y, and let W_y^x denote the CEO compensation in the firm, where $x = \{1997, 1999, 2001\}$ and $y = \{$"new economy firm" (denoted by n), "old economy firm" (denoted by o)$\}$. Let

$dV_n^u = V_n^{1999} - V_n^{1997}$: Change of market value, new economy, overvaluation stage.
$dV_n^d = V_n^{2001} - V_n^{1999}$: Change of market value, new economy, correction stage.
$dV_o^u = V_o^{1999} - V_o^{1997}$: Change of market value, old economy, overvaluation stage.
$dV_o^d = V_o^{2001} - V_o^{1999}$: Change of market value, old economy, correction stage.

where the mnemonics in superscript of dV stay $\{u$ for market up$\}$ and $\{d$ for market down$\}$. Differences are taken within firms across time (hence *firm fixed effects are wiped out*). Let similarly

$dW_n^u = W_n^{1999} - W_n^{1997}$: Change of CEO compensation, new economy, overvaluation stage.
$dW_n^d = W_n^{2001} - W_n^{1999}$: Change of CEO compensation, new economy, correction stage.
$dW_o^u = W_o^{1999} - W_o^{1997}$: Change of CEO compensation, old economy, overvaluation stage.
$dW_o^d = W_o^{2001} - W_o^{1999}$: Change of CEO compensation, old economy, correction stage.

So far all the quantities defined have been firm and market (overvaluation/correction) specific. Lets denote by overbars averages of firms within type of firm/market cells. E.g.,

$$\overline{dV_n^u} = \frac{\sum_{\text{if firm new economy And market up}} dV_n^u}{\#\text{terms in the summation}}.$$

The **reduced form** differences-in-differences estimators for firm market value and CEO compensation are defined respectively as

$\widehat{\Delta_V^u} = \overline{dV_n^u} - \overline{dV_o^u}$	Up-market difference between mean new and mean old economy (within firm differenced) market values.
$\widehat{\Delta_V^d} = \overline{dV_n^d} - \overline{dV_o^d}$	Down-market difference between mean new and mean old economy (within firm differenced) market values.
$\widehat{\Delta_W^u} = \overline{dW_n^u} - \overline{dW_o^u}$	Up-market difference between mean new and mean old economy (within firm differenced) CEO compensation.
$\widehat{\Delta_W^d} = \overline{dW_n^d} - \overline{dW_o^d}$	Down-market difference between mean new and mean old economy (within firm differenced) CEO compensation.

They are estimators of the causal impact of the "new economy" stock bubble in the market upturn and in the market downturn. The first difference within firms and across time takes care of the firm fixed effects. The second difference between averages for new and old economy firms takes care of systematic factors(apart from the bubble) common to all firms and varying only across time. The old economy firms are the control group affected by this systematic factors but *not* by the bubble. The new economy firms are the treatment group affected by the systematic factors *and* by the bubble.

The **instrumental variable estimator** of pay for luck here simplifies to the Wald estimator, because the instrument is binary

$$\widehat{\Psi^u} = \frac{\widehat{\Delta_W^u}}{\widehat{\Delta_V^u}} = \frac{\overline{dW_n^u} - \overline{dW_o^u}}{\overline{dV_n^u} - \overline{dV_o^u}} \text{ , Pay for Good luck}$$

$$\widehat{\Psi^d} = \frac{\widehat{\Delta^d_W}}{\widehat{\Delta^d_V}} = \frac{\overline{dW^d_n} - \overline{dW^d_o}}{\overline{dV^d_n} - \overline{dV^d_o}} \text{ , Pay for Bad luck}$$

These estimate the causal impact of market valuation on CEO compensation, where causal means that this would be the impact if we had the freedom to assign market values to companies at random, and then observe how CEO pay reacts. In yet other words, this is the impact of market values on CEO pay, where only the variation in valuation induced by the market bubble is used to predict variation in CEO pay.

2.5.3 Reduced Forms: the Causal Impact of the Bubble on Market Values and on CEO pay

In the first panel of the following Table 2.3 differences-in-differences estimates are presented for the year 1999 when the market bubble was developing (good luck reduced form estimates, denoted by $\widehat{\Delta^u_V}$ and $\widehat{\Delta^u_W}$ above). I compute $\widehat{\Delta^u_V}$ and $\widehat{\Delta^u_W}$ by regression analysis. Within firm differences in variables from year 1997 to year 1999 are regressed on a dummy variable "New" which is 1 for "new economy" firms and 0 otherwise.

Dependent variables are respectively change in market value (in millions of dollars), change in total current compensation (salary plus bonus), change in total compensation valued at grant date (*Execucomp* TDC1 variable) and change in total realized compensation (*Execucomp* TDC2 variable), compensation variables in thousands of dollars. The coefficient on the Const is the mean change for "old economy" firms and the coefficient on New is the difference between mean change in new and old economy firms (i.e., the $\widehat{\Delta^u_V}$ and $\widehat{\Delta^u_W}$).

In the first column we see that the effect of the bubble peak on "new economy" firms was roughly \$11.6 billion increase in market value. The impact on total CEO pay valued at grant date (column 3) was an increase of roughly \$4.4 million. Both differences-in-differences estimates are significant at any level. Total realized CEO pay for "new economy" firms increased by \$6.7 million due to the bubble (last column), even more than the increase in the

Table 2.3: Reduced form differences-in-differences estimates

	Year 1999, stock market bubble developing:			
	dMktVal	dTotCurrComp	dTotCompExAnte	dTotCompExPost
	Coeff/[95%CI]	Coeff/[95%CI]	Coeff/[95%CI]	Coeff/[95%CI]
New	11602.278***	-44.467	4410.622***	6708.388***
	[8155.339,15049.216]	[-233.935,145.000]	[1804.652,7016.593]	[3864.138,9552.639]
Const	1093.805	209.290***	713.461	624.902
	[-392.371,2579.981]	[127.010,291.570]	[-417.056,1843.977]	[-610.268,1860.072]
#Obs.	624	631	627	631
	Year 2001, stock market correction:			
	dMktVal	dTotCurrComp	dTotCompExAnte	dTotCompExPost
	Coeff/[95%CI]	Coeff/[95%CI]	Coeff/[95%CI]	Coeff/[95%CI]
New	-5930.354***	56.202	3345.489	-932.449
	[-7790.878,-4069.829]	[-308.948,421.352]	[-265.363,6956.341]	[-3430.048,1565.150]
Const	314.421	71.966	663.173	339.354
	[-615.082,1243.925]	[-112.492,256.424]	[-1169.664,2496.010]	[-922.325,1601.033]
#Obs.	613	627	621	627

grant date value.

In the second panel of Table 2.3 are presented $\widehat{\Delta_V^d}$ and $\widehat{\Delta_W^d}$ the differences-in-differences estimates for the same items but for the correction stage.

Two puzzling results are apparent in the correction stage differences-in-differences estimates. Firstly, and in disagreement with Figures 2.2 and 2.3, it seems that the correction stage did not take the market values of "new economy" firms all the way down to where they started. The decrease is significant at any level and huge, $-\$5.9$ billions of dollars compared to the control group of "old economy" firms. Yet it is half of the magnitude of the increase in the bubble peak stage, and the two estimates are statistically different (the absolute values of their 95% confidence intervals do not overlap).

Second, the differences-in-differences estimates of the effect of the bubble trough on "new economy" firms CEO compensation is *positive* (except for ex post total pay). The point estimates for ex ante total pay are huge albeit insignificant, an increase of \$3.3 million further and beyond the increase that the control group of "old economy" CEOs experienced. Therefore while shareholders in the "new economy" firms were losing fortunes, their CEOs kept on being showered with money more generously than "old economy" CEOs.

2.5.4 Instrumental Variable estimates of Pay for Luck

In this subsection I present the Wald estimates of Pay for Luck. For computational purposes I use instrumental variable regression, however when the instrument is binary, this estimator is equivalent to the Wald estimators $\widehat{\Psi^u}$ and $\widehat{\Psi^d}$ defined above. Mechanically, changes in CEO compensation variables (total current compensation, total compensation ex ante and total compensation ex post) are regressed on the predicted values coming from the first stage regression of change in market value on the binary instrument New (1 for new economy firms, 0 for old economy firms). The exercise is performed separately for the good luck year 1999, and for the bad luck year

2001, to allow for asymmetries in pay for luck.

Year 1999, pay for good luck:

	dTotCurrComp Coeff/[95%CI]	dTotCompExAnte Coeff/[95%CI]	dTotCompExPost Coeff/[95%CI]
dMktVal	-0.004	0.386***	0.594***
	[-0.021,0.013]	[0.160,0.613]	[0.349,0.838]
Const	214.873***	285.806	-28.682
	[121.682,308.065]	[-984.634,1556.246]	[-1387.470,1330.105]
#Obs.	624	620	624

In the second column we see, that if we had the the power to increase the value of a company by $1mln, by say just transferring the amount to firm's bank account, the total ex ante valued compensation of the firm's CEO is predicted to increase by $386, the estimate is significant at any level. This estimate is huge, implying that, e.g., the CEO manages to grab 0.0386% of every windfall booty accruing to the company. This is consistent with the illusion of leadership hypothesis. Shareholders commit the fundamental attribution error and let the CEO receive large share of the spoils even when he has nothing to do with the generation of the surplus. Shareholders tend to identify the CEO with the company he is running and this tends to propagate company value into CEO pay, even when the CEO has nothing to do with value creation.

For comparative purposes, the OLS estimate of the effect of market value on CEO total ex ante compensation is 0.204 (95% confidence interval [0.147 , 0.260]). It is somehow close to the IV estimate of 0.386. However both are estimated fairly precisely, and the Hausman's endogeneity test (see Wooldridge 2002, page 120, for a version of the test that compares only the coefficients of interest) is pretty close to rejecting the null hypothesis that they are the same at the 10% significance level – year 1999, Hausman t = 1.635 (p-value 0.102).

The most interesting empirical result comes from the pay for bad luck estimates.

Year 2001, pay for bad luck

	dTotCurrComp Coeff/[90%CI]	dTotCompExAnte Coeff/[90%CI]	dTotCompExPost Coeff/[90%CI]
dMktVal	-0.021	-0.604*	0.164
	[-0.075,0.032]	[-1.177,-0.032]	[-0.187,0.515]
Const	82.998	914.064	339.700
	[-67.117,233.112]	[-707.490,2535.618]	[-649.425,1328.825]
#Obs.	613	608	613

For year 2001, the bubble trough, we find a Wald estimate

$$\widehat{\Psi^d} = \frac{\widehat{\Delta_W^d}}{\widehat{\Delta_V^d}} = \frac{\overline{dW_n^d} - \overline{dW_o^d}}{\overline{dV_n^d} - \overline{dV_o^d}} = -0.604 \text{ , (Standard Error} = 0.347)$$

for ex ante total CEO pay, column 2. The estimate is marginally significant (p-value = 0.083), say it would be significant at the 10% level, but not at the 5% level. What is says is that at the market trough there was economically large pay for luck, which was even bigger in absolute value than the pay for good luck estimate (which was 0.386). However, the coefficient has the wrong sign – while the shareholders in the "new economy" firms were losing big from the stock market correction compared to the control group of "old economy" firms, their CEOs kept on getting richer at faster rate compared to the control group CEOs.

To interpret this estimate, imagine that we take away $1 million from the company, then the estimate suggests that the CEO will get $604 *more* in pay. Of course we obtain this shocking result in the context of the "new economy" bubble trough. Whether we can generalize it to other times and populations of firms and executives is another question.

For comparison, OLS regression of the change in total CEO ex ante compensation on the change in firm value gives a slope estimate of 0.308 (Standard Error = 0.076). As it could be expected, Hausman's test decisively rejects the null that the IV and OLS estimates are the same – year 2001, Hausman t = -2.689 (p-value 0.007).

2.5.5 Reverse Pay for Luck and Rotten Governance

So is the reversed pay for luck estimate not a strong evidence against the shareholders' fundamental attribution bias? Not necessary, because we might have bad governance superimposed on the shareholders' biases. It might be that when the economy is booming and things go well, CEOs ride on the shareholders' attribution error. When the things go wrong, the CEOs receive protection from friendly boards at the corporations where the corporate governance is bad. In other words it must be the case that the reverse pay for luck coefficient is largely driven by firms with rotten governance.

It turns out that indeed rotten corporate governance is behind the reverse pay for luck coefficient. As a measure of the quality of corporate governance I use the famous and widely used Gompers-Ishii-Metrick G-index. The index is constructed in a straightforward manner from Investor Responsibility Research Center (IRRC) data. A few important anti-shareholders provisions are identified in the corporate charter and bylaws, e.g., poison pill provisions, supermajority requirements for charter amendments, golden parachutes etc., and a point is added to the index if the anti-shareholders provision is present (see Gompers, Ishii and Metrick, 2003 for the details). So high values of the G-index indicate entrenched management and low values of the index indicate strong shareholders.

Strictly speaking this is an anti-takeover index, reflecting how strong are shareholders of a particular corporation. In the relevant literature researchers call the G-index "governance index" and this is a misnomer that is inconsequential when shareholders are assumed to be rational. However if the shareholders commit the fundamental attribution error low levels of the index reflect strong shareholders, but not necessary good governance. In corporations with low G-index shareholders will have their way, but what they want might deviate from the rational first best. Hence I will call the G-index anti-takeover or strength of the shareholders rights index.

To preserve the non-parametric nature of the analysis in this section, I control for the quality of corporate governance in the following way. I sort the firms according to their G-index and group them into 5 roughly equally

Table 2.4: Pay for luck for the “Democracy portfolio” and “Dictatorship portfolios”

	Pay for Good luck, year 1999		Pay for Bad luck, year 2001	
	Democracy dTotCompExAnte Coeff/[90%CI]	Dictatorship dTotCompExAnte Coeff/[90%CI]	Democracy dTotCompExAnte Coeff/[90%CI]	Dictatorship dTotCompExAnte Coeff/[90%CI]
dMktVal	0.317**	-0.003	0.300	-0.672**
	[0.054,0.580]	[-0.084,0.077]	[-0.391,0.990]	[-1.128,-0.216]
Const	358.180	1424.321***	798.582	757.389
	[-1681.028,2397.387]	[808.573,2040.069]	[-2040.848,3638.013]	[-396.186,1910.965]
#Obs.	191	159	167	139
Hausman’s				
t-stat	-.657	-.272	-.790	-2.954
p-value	.510	.785	.429	.003

sized groups, the break points are the quintiles of the G-index. The set of the first two groups is a “Democracy portfolio” (i.e., firms with strong shareholders) and the set of the last two groups is a “Dictatorship portfolio” (i.e., firms with entrenched management and weak shareholders) in the parlance of Gompers, Ishii and Metrick (2003). Then I compute the pay for good luck and pay for bad luck estimates separately for the Democracy portfolio and Dictatorship portfolio.

The results in the Table 2.4 above are fully consistent with the hypothesis that shareholders commit the fundamental attribution error. Among firms with strong shareholders (the Democracy portfolio, columns 1 and 3) the pay for good luck and pay for bad luck estimates are numerically equivalent down to the first digit after the decimal point. Just as in the controlled experiments shareholders reward and punish their agents for shocks beyond their control. However this happens only when shareholders have strong rights.

The reverse pay for bad luck coefficient is completely driven by firms with weak shareholders rights (the Dictatorship portfolio, column 4). I.e., these are firms at which shareholders simply cannot have their way, and hence their biases and preferences are irrelevant.

Hausman’s endogeneity test cannot reject the hypothesis that pay for luck is the same as the pay for performance in all the cases, except for the case where we have bad luck combined with weak shareholders’ rights and where we obtain the perverse sign of the coefficient.

2.6 Conclusion

The optimal contracting view teaches us that the ex post observed sky-high levels of CEOs pay are the outcome of optimal contracting between rational agents and a side effect of alignment of CEOs incentives with shareholders’ value maximization. In this view both ex post observed high market valuations and high CEOs pay are resulting from CEOs leadership, ability and wise decisions which have achieved what is best for the shareholders. The second explanation, which I propose, is that shareholders and boards of directors commit the fundamental attribution error. They uncritically attribute prominent increases and decreases in the prices of corporate stocks to the leadership and skill of the CEOs, where in fact stock market fluctuations which are beyond CEOs control are the major driving force.

I recast these competing hypothesis as a simple endogeneity test and I cannot reject the view that market fluctuations mechanically cause changes in CEOs compensation with no detectable reverse causality, consistent with the fundamental attribution bias explanation of CEOs pay. Further I find that in the aggregate data increases in CEOs pay decrease corporate profits. I also employ a direct measure of CEO skill (4-factor model risk-adjusted returns) and show that in the cross section CEO pay is unrelated to CEO skill. Exploiting the "new economy" stock market bubble quasi-natural experiment I show that when shareholders have strong rights, they reward and punish their CEOs for factors beyond CEOs' control, just like controlled experiments have previously shown.

I conclude that in the late 90s stock market bubble period shareholders were taken for a ride and ended up paying huge amounts of money to their CEOs for no rational reason.

My results are consistent with Bertrand and Mullainathan (2001) who find that "CEO pay is as sensitive to a lucky dollar as to a general dollar." As I use different methodology from theirs it appears that the finding is robust and is not an artefact of the methodology employed. My analysis also clarifies how the shareholders fundamental attribution bias and the strength of shareholders rights interact and determine CEOs compensation.

In conclusion, one has all reasons to worry about the top CEOs pay setting practices. The multimillion compensation packages that top CEOs manage to extract from their companies do not look like an outcome of a rational decision making process that has shareholders' value maximization for an objective. Boards of directors and shareholders should exercise caution and should resist the natural temptation to take the shortcut and attribute performance directly and uncritically to the CEO. Systematic analysis on a case by case basis should help – firm value changed, but how did it come: were the political, macroeconomic and public equity market conditions favorable, did the CEO do something which can be unambiguously linked to company performance... Thinking in these lines will probably not eradicate fundamental attribution bias, yet it might substantially reduce it.

Chapter 3

FORECASTING AGGREGATE STOCK RETURNS USING THE NUMBER OF INITIAL PUBLIC OFFERINGS AS A PREDICTOR

3.1 Introduction

The number of Initial Public Offerings (IPOs) reliably predicts subsequent aggregate stock returns both in-sample and out-of-sample at monthly frequency.[†] Increases in the number of IPOs forecast significant decreases in the returns on equally weighted portfolio of all stocks in CRSP database. The effect for the value weighted market portfolio has the same sign, however it is statistically insignificant even in-sample. Increases in the number of IPOs forecast low returns among NASDAQ traded firms, however the effect is statistically significant both in and out-of-sample only when the index is constructed as equally weighted. Increases in the number of IPOs predict remarkably well shrinkages in the return differential between small and big firms (Fama and French's *smb*) judged by both in-sample and out-of-sample criteria.

The forecasting patterns are consistent with a behavioral story featuring investor sentiment and limits to arbitrage (Baker and Wurgler, 2007). Rational managers time the public equity market and take their firms public when investor sentiment is high and equity is overvalued. Subsequently as investor

[†] The results reported in this chapter were published in Kolev (2008b).

sentiment mean reverts or as arbitragers gradually bring values back to levels justified by fundamentals, the market experiences low aggregate returns. The effect is concentrated among firms that are more subject to sentiment or more difficult to arbitrage. Arguably small capitalization and high tech growth stocks are more difficult to value and arbitrage than large capitalization mature firms, and hence the former could be expected to be more affected by investor sentiment.

Investor sentiment seems a plausible explanation for the empirical patterns I document (Baker and Wurgler 2000, 2006, 2007). Yet some other fully rational mechanism might be at work. The fact that the number of IPOs predicts reliably in-sample and out-of-sample aggregate returns, where the effect is concentrated among small capitalization and high tech stocks, is of certain interest of its own right. This result comes in the midst of a recent large scale reexamination of the predictive ability of variables earlier proposed in the literature. This reexamination reaches conclusions ranging from the view that the evidence is somewhat mixed (Rapach and Wohar, 2006) to the view that stock return predictability is not at all an empirical fact that one should rely upon (Goyal and Welch, 2007).

The result that one could predict future aggregate returns with the number of firms going public closely relates to Baker and Wurgler (2000). The latter paper shows that increases in the equity share in new issues, a variable very similar in spirit to the number of IPOs, predict subsequent decreases in aggregate stock returns. Although the two variables most likely reflect the same underlying phenomenon, they are sufficiently distinct both in terms of raw correlation (= 0.2787) and in terms of forecasting patterns they present. The finding that the number of IPOs predicts returns more pronouncedly among small capitalization and high tech stocks relates to Baker and Wurgler (2006). The authors construct an index of investor sentiment, part of which is also the contemporaneous number of IPOs, and show that in the cross section investor sentiment mostly affects valuations among stocks that are hard to value or hard to arbitrage.

I proceed as follows. In Section 3.2 I outline the methodology that I use. In Section 3.3 I describe the data. In Section 3.4 I present the results. In

Section 3.5 I show that the in-sample results are not an artifact of small-sample bias. In Section 3.6 I show that the number of IPOs predicts the small minus big return differential. In the last section I conclude.

3.2 Methodology

Following much of the extant literature I estimate by Ordinary Least Squares bivariate predictive regressions where the gross real return on aggregate stock index is regressed on a constant and a lagged value of a predictor

$$R_t = \beta_0 + \beta_1 X_{t-1} + u_t. \tag{1}$$

In different specifications R is the gross real return on value and equally weighted portfolios of the universe of CRSP stocks and the value and equally weighted portfolios of stocks traded on the NASDAQ stock exchange. The predictor X is the number of IPOs and for comparative purposes the equity share in new issues. βs are population parameters to be estimated and u is a disturbance term.

The in-sample predictive ability of X is assessed via the t-statistic corresponding to b_1 , the OLS estimate of β_1 in eq. (1). Under the null hypothesis that X_{t-1} does not help in predicting R_t the expected returns are constant and $\beta_1 = 0$. Although the reasoning outlined in the Introduction suggests that β_1 should be less than 0, I take the alternative hypothesis to be double sided, $\beta_1 \neq 0$.

To generate out-of-sample predictions I use a recursive scheme. I split the sample into two halves with roughly equal number of observations. Let the total number of observations be T and let the first half used for in-sample estimation contain $(T1-1)$ observations. Let the second half used for out-of-sample predictions contain $(T2+1)$ observations. Denote the null model prediction by $R_{pn,t} = b_{0,t-1}$ and the alternative model prediction by $R_{pa,t} = b_{0,t-1} + b_{1,t-1}X_{t-1}$. The mnemonics in the subscript pn stand for "prediction with the null imposed," i.e., b_1 constrained to be 0, and pa for "prediction under the alternative," i.e. eq. (1). The bs are estimated by OLS with data available only up to one period before the forecast is made, e.g., the first prediction under the alternative model eq. (1) is $R_{pa,T1} = b_{0,T1-1} + b_{1,T1-1}X_{T1-1}$ where the bs are estimated using only data points from the 1st though the $(T1-1)$th.

As an informal measure of out-of-sample performance of the predictive regression I report the out-of-sample R-squared of Campbell and Thompson (2006)

$$\text{R-sqos} = 1 - \frac{\Sigma_{t=T1,..,T}(R_t - R_{pa,t})^2}{\Sigma_{t=T1,..,T}(R_t - R_{pn,t})^2}. \tag{2}$$

To formally test the null hypothesis that eq. (1) does *not* improve upon the historical average return I employ the Clark and West (2007) Mean Squared Prediction Error-adjusted (MSPE-adj) statistic

$$\text{MSPE-adj} = \frac{\Sigma_{t=T1,..,T}\{(R_t - R_{pn,t})^2 - [(R_t - R_{pa,i})^2 - (R_{pn,t} - R_{pa,i})^2]\}}{T2+1}. \tag{3}$$

Clark and West (2007) observe that under the null that $\beta_1 = 0$ the alternative model in eq. (1) estimates additional parameters whose population values are 0 and that the estimation induces additional noise. Hence under the null hypothesis the MSPE of the alternative model is expected to be larger than the MSPE of the null model.

They propose an adjustment to the alternative model's MSPE. The term in square brackets in eq. (3) is the adjusted MSPE of the alternative model. Clark and West (2007) show that if the t-ratio associated with MSPE-adj exceeds the critical value of +1.282 then one can reject the null of *no* returns predictability in favor of the alternative model eq. (1), and that the size of the test is somewhere between 0.05 and 0.10 (i.e., the probability that we mistakenly reject a correct null is at most 0.10).[1]

[1] I implement the test as proposed in Section 2 of Clark and West (2007). I define the quantity in curly brackets in eq. (3) and regress it on a constant. The t-statistic from this regression is reported in Table 2.

3.3 Data

Monthly returns for equally and value weighted indices on all CRSP stocks are taken directly from CRSP. I construct monthly returns on equally and value weighted indices of stocks that are traded on the NASDAQ stock exchange.[2] The monthly number of IPOs series covering January 1960 to December 2006 is downloaded from Jay Ritter's web page http://bear.cba.ufl.edu/ritter/. The series is an update of Ibbotson, Sindelar and Ritter (1994). The monthly share of equity in new issues is downloaded from Jeffrey Wurgler's web page http://pages.stern.nyu.edu/ jwurgler/. I use only the period which overlaps with the number of IPOs, from January 1960 to March 2006. Returns are converted to real terms using the CPI index downloaded from the web page of the Bureau of Labor Statistics.

Table 1, Summary statistics
nipo is the monthly number of IPOs, *s* is the equity share in new issues. The rest of the variables are gross real monthly returns in percentage form, including dividends distributions: *ewre* is the equally weighted return on all CRSP stocks, *vwre* is the value weighted return on all CRSP stocks, *nsdqewre* is the equally weighted return on all stocks last observed trading on the NASDAQ stock exchange, and *nsdqvwre* is the value weighted return on all stocks last observed trading on the NASDAQ stock exchange.

Variable	Obs	Mean	Std. Dev.	Min	Max
nipo	564	27.5	24.04455	0	122
s	555	.1838175	.1074827	.0208771	.6348659
ewre	563	100.889	5.600332	72.58585	129.4272
vwre	563	100.5982	4.390141	77.26484	115.418
nsdqewre	563	100.7537	6.407584	72.42509	129.5718
nsdqvwre	563	100.4484	6.288268	71.92438	122.7501

[2]NASDAQ was opened in the beginning of the 70ies. The IPO data starts in year 1960. To avoid losing 10 years of data from this mismatch I include a stock in the index if the last exchange where the stock is observed trading is NASDAQ, i.e., I include stocks for which CRSP variable HEXCD=3.

The time series of monthly number of IPOs (*nipo*) appears to be stationary. The Dickey-Fuller test rejects the null that a unit root is present at any significance level. Visual inspection of the series does not reveal time trend, and the trend term is insignificant if included in any of the specifications. The augmented Dickey-Fuller test with 48 lags of the first differenced variable rejects the null that a unit root is present at the 5% significance level (p-value = 0.0287).

3.4 Results

Table 2, Returns predictions

The regressand being predicted is gross real monthly return in percentage form, including dividends distributions: *ewre* is the equally weighted return on all CRSP stocks, *vwre* is the value weighted return on all CRSP stocks, *nsdqewre* is the equally weighted return on all stocks last observed trading on the NASDAQ stock exchange, and *nsdqvwre* is the value weighted return on all stocks last observed trading on the NASDAQ stock exchange. The predictors are *nipolag*, one month lagged number of IPOs, or *slag*, one month lagged equity share in new issues. The statistics in the three columns labeled In-sample are computed using the full sample, January 1960 to December 2006. For the statistics in the columns labeled Out-of-sample, the sample is split into two roughly equal halves (containing respectively 281 and 283 observations when *nipolag* is the predictor) and *recursive* predictions are generated for the second half of the sample. The formulas for the computed statistics are given in Section 3.2.

		In-sample			Out-of-sample		
regressand	predictor	b_1	t-stat	R-sq	MSPE-adj	t-stat	R-sqos
ewre	*nipolag*	-.0327	-3.35	0.0198	1.3721	2.16	.0085
	slag	-6.6362	-3.10	0.0161	.6388	1.28	-.0005
vwre	*nipolag*	-.0077	-0.92	0.0018	-.0486	-0.24	-.0111
	slag	-3.5794	-2.11	0.0076	.3048	1.31	.0058
nsdqewre	*nipolag*	-.0413	-3.82	0.0241	2.1206	2.37	.0069
	slag	-8.9361	-3.64	0.0223	1.2001	1.59	.0025
nsdqvwre	*nipolag*	-.0254	-2.36	0.0095	.6225	0.86	-.0112
	slag	-7.5581	-3.13	0.0165	1.1928	1.55	.0075

The results are contained in Table 2. Increases in the monthly number of IPOs and in the equity share in new issues predict decreases in the next month aggregate stock returns. Both predictors perform better (in terms of goodness of fit measures and strength of the rejection of the null of no predictability) when equally weighted returns are forecasted, where the effect is particularly pronounced for the number of IPOs. The marginal decrease in subsequent returns for a marginal increase in the number of IPOs and for a marginal increase in the equity share in new issues is also larger for equally weighted returns.

Both predictors reveal statistically significant in-sample predictive ability, except in the case when the number of IPOs is used to predict value weighted returns for all CRSP stocks. When prediction of equally weighted returns is the objective, the number of IPOs performs slightly better in-sample than the equity share in new issues. The equity share in new issues forecasts well both equally and value weighted aggregate returns, regardless of whether performance is judged by in-sample or out-of-sample criteria.

The number of IPOs forecast reliably equally weighted aggregate returns both in-sample and out-of-sample. The t-statistic associated with the MSPE-adj is 2.16 and 2.37 for equally weighted, respectively all CRSP stocks and only NASDAQ stocks. It is well above the critical value of 1.282 and therefore the null hypothesis of no out-of-sample aggregate stock returns predictability is decisively rejected in favor of the one sided alternative that the number of IPOs is a superior predictor compared to the unconditional stock returns mean. The out-of-sample R-squared (labeled R-sqos) of 0.0085 and 0.0069 are remarkably high compared to the ones reported in other papers.[3]

3.5 In-sample predictions: Is there small-sample bias?

Regression coefficients and standard errors, obtained from predictive regressions employing a highly persistent predictor whose innovations are correlated with the innovations in the predictand, might exhibit severe small sample biases (Mankiw and Shapiro, 1986; Stambaugh, 1986,1999; Nelson and Kim, 1993). In this subsection I study whether the in-sample results in Table 2 are not an artifact of this small-sample bias. The model is defined over the whole sample $t = 1, 2, .., T$

$$R_t = \beta_0 + \beta_1 X_{t-1} + u_t \tag{4}$$

$$X_t = \mu + \rho X_{t-1} + w_t \tag{5}$$

where the disturbances (u_t, w_t) are serially independently and identically distributed as bivariate normal, and the autoregressive coefficient in eq. (5) is less than 1. I follow the bias correction methodology of Amihud and Hurvich (2004). As a matter of notation, a superscript c always denotes a bias corrected estimator in what follows. First, I estimate eq. (5) to obtain the OLS estimator r of ρ. From r , I compute the bias corrected estimator of ρ

[3]Campbell and Thompson (2006) Table 1, column 5 and Goyal and Welch (2007) Table 3, column 4 report R-sqos. However the comparison is only suggestive, as their definition of returns is different and their sample period is different too.

$$r^c = r + \frac{(1+3r)}{T} + \frac{3(1+3r)}{T^2}. \tag{6}$$

The bias corrected estimator r^c is used to compute corrected residuals $\hat{w}_t^c$ for eq. (5)

$$\hat{w}_t^c = X_t - (m + r^c X_{t-1}),$$

where m is the OLS estimator of μ.[4]

Second, I run an auxiliary regression of R_t on intercept, X_{t-1} and $\hat{w}_t^c$. Denote by b_1^c the OLS estimator of the slope parameter on X_{t-1} and by f^c the OLS estimator of the slope parameter on $\hat{w}_t^c$ in this auxiliary regression. b_1^c is the bias corrected estimator of β_1 in which we are interested.

Finally, to conduct inference on β_1, we need the bias corrected standard error of b_1^c, which is given by the formula

$$[\mathrm{SE}^c(b_1^c)]^2 = [f^c]^2 * [1 + 3/T + 9/T^2]^2 * [\mathrm{SE}(r)]^2 + [\mathrm{SE}(b_1^c)]^2, \tag{7}$$

where $\mathrm{SE}(r)$ denotes the usual OLS standard error of r produced by any regression package and $\mathrm{SE}(b_1^c)$ denotes the usual OLS standard error of b_1^c, which comes as a direct output from the auxiliary regression of R_t on intercept, X_{t-1} and $\hat{w}_t^c$.

Table 3: In-sample bias corrected statistics from the predictive regressions: The regressands and the predictors are as in Table 2. b_1^c is the Amihud and Hurvich (2004) bias corrected estimator of β_1 in eq. (4) and $\mathrm{SE}^c(b_1^c)$ is its bias corrected standard error. t^c-stat $= b_1^c$ /[$\mathrm{SE}^c(b_1^c)$]. r is the OLS estimate of the autoregressive parameter ρ in eq. (5). r^c is the bias corrected estimator of ρ. Finally, f^c is unbiased estimator of [$\mathrm{Cov}(u_t, w_t)$]/[Var w_t] (Amihud and Hurvich, 2004, Lemma 1).

[4]The choice of estimator m is inconsequential for the bias in the predictive regression slope.

		In-sample bias corrected			Auxiliary statistics		
regressand	predictor	b_1^c	$\mathrm{SE}^c(b_1^c)$	t^c-stat	r	r^c	f^c
ewre	*nipolag*	-.0323	.0097	-3.32	.8588	.8652	.0498
	slag	-6.6777	2.2155	-3.01	.7369	.7427	-1.6567
vwre	*nipolag*	-.0074	.0077	-.9618	.8588	.8652	.0472
	slag	-3.6040	1.7446	-2.06	.7369	.7427	-2.1743
nsdqewre	*nipolag*	-.0409	.0111	-3.68	.8588	.8652	.0538
	slag	-8.9843	2.5264	-3.55	.7369	.7427	-.1496
nsdqvwre	*nipolag*	-.0249	.0109	-2.27	.8588	.8652	.0760
	slag	-7.6106	2.4848	-3.06	.7369	.7427	1.1295

Comparison of Table 3 and the left panel of Table 2 (in-sample results) reveals that the corrections for the finite sample bias do not make a difference. Hence finite sample bias is not a problem when the number of IPOs is used as a predictor of stock returns.

The results in this section are not surprising. Baker, Taliaferro and Wurgler (2006) show that the small-sample bias has negligible consequences for managerial decision variables, e.g., for the equity share in new issues that is studied here as well.[5]

Therefore by now it is well known that the model in eq. (4) and (5) has very different stochastic properties depending on whether the predictor X is a managerial decision variable (e.g., number of IPOs or equity share in new issues), or a valuation ratio (e.g., aggregate dividend to price ratio, or aggregate book to market ratio).

[5] Baker, Taliaferro and Wurgler (2006) use different statistical techniques to demonstrate this – bootstrap and simulations under worse scenarios.

3.6 Forecasting the small minus big return differential

The number of IPOs forecasts well equally-weighted aggregate stock returns, but not value-weighted returns (Table 2). I suggest that this is because managers time the market, and take their firms public when investor sentiment is high – subsequently as sentiment mean reverts or arbitragers correct mispricing, firms which are difficult to arbitrage[6] or difficult to value (e.g., small firms) experience low returns.

If this is the case, the number of IPOs must forecast the return differential between small and big firms even better than the return on the aggregate equally weighted portfolio. Big firms, which are easy to value and arbitrage, are present in equally weighted portfolio too (albeit their impact is downplayed by weighting) and if the sentiment/limits to arbitrage story is true, big firms make the forecasting job of the number of IPOs harder.

In this section I show that the number of IPOs forecasts the return on the small minus big (*smb*) portfolio of Fama and French,[7] and the forecasting performance is remarkably better than the forecasting performance on the equally-weighted aggregate returns. The *smb* is the return differential between three portfolios including only small firms and three portfolios including only big firms. Within each portfolio, firms' returns are *value weighted*.

Table 4: The regressand is *smb* the return differential between small and big firms. The rest of the table has the same structure as Table 2, except that bias corrected t-statistics are added in square brackets bellow the usual t-statistics.

[6]By "difficult to arbitrage" I mean that arbitrage is gradual, and cannot correct prices instantly.

[7]I downloaded *smb* from Kenneth French's web site http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html. In terms of gross returns, the mean of *smb* from the years that overlap with available data on number of IPOs is 100.21%, and the standard deviation is 3.15%.

		In-sample			Out-of-sample		
regressand	predictor	b_1	t-stat [t^c-stat]	R-sq	MSPE-adj	t-stat	R-sqos
smb	*nipolag*	-.0258	-5.16 [-4.71]	0.0387	.9161	3.81	.0379
	slag	-2.9944	-2.71 [-2.42]	0.0103	-.0112	-0.09	-.0143

Table 4 reveals remarkable ability of the number of IPOs to predict the small minus big return differential. For example both in-sample and out-of-sample R-squared with *smb* as a predictand are about 3 times higher than the R-squared in the regression where equally weighted returns are the predictand. This result is in accord with the effects that should be expected if the conjectured market timing mechanism is at play.

3.7 Conclusion

A behavioral story featuring investor sentiment and limits to arbitrage suggests that increases in the number of IPOs should predict subsequent decreases in stock returns, and that the effect should be concentrated among stocks that are difficult to value or arbitrage (e.g., small stock or high tech stocks).

I show that at monthly frequency, the number of IPOs predicts reliably equally weighted aggregate stock returns and predicts remarkably well the return differential between small and big firms, judged by both in-sample and out-of-sample criteria.

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