ESSAYS ON MANAGEMENT COMPENSATION AND DIVIDEND POLICY

by

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ABSTRACT

In this dissertation, I consider effects of two corporate policies on shareholder wealth. First, I examine whether paying high dividends in an economy such as the United States, where tax on dividend income is higher than tax on capital gains, results in higher stock required rate of return to compensate investors for higher tax burden. Higher required rate of return, in turn, lowers equity valuation and decreases shareholder wealth. This view is supported by the positive relationship between dividend yield and stock return in the U.S. data. However, I document the above positive relationship in the Hong Kong market, where neither dividend incomes nor capital gains are taxed. My result shows that there are unknown factors that affect both stock required rate of return and dividend policy. In this case, paying high dividends might be a part of an optimal corporate policy, and thus does not necessarily decrease shareholder wealth.

Second, I examine the question of whether the practice of using peer groups in setting Chief Executive Officers' (CEO) compensation results in unjustified pay, and thus transfers shareholder wealth to the CEOs. I examine this issue using the mandated disclosure of compensation peers that began in 2006. Although peers are largely selected based on characteristics that reflect the labor market for managerial talent, I find that peer groups are constructed in a manner that biases compensation upward, particularly in firms outside the Standard & Poor's (S&P) 500. Pay increases close only about one-

third of the gap between the pay of the CEO and the peer group, however, suggesting that the wealth transfer to the CEOs is relatively small.

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CHAPTER 1

INTRODUCTION

Firms acting in the best interests of shareholders develop and implement policies that increase shareholder wealth. In this dissertation, I consider effects of two corporate policies on shareholder wealth. First, I examine the effect of dividend policy. Firms can choose to follow either high- or low-dividend-yield policies. I investigate whether paying high dividends in an economy such as the United States, where tax on dividend income is higher than tax on capital gains, results in higher stock required rate of return to compensate investors for higher tax burdens. Higher required rate of return, in turn, lowers equity valuation and decreases shareholder wealth. This view is suggested by a theoretical work in Brennan (1970), the CAPM with tax, and empirically verified by the positive relationship between dividend yield and stock return in the U.S. data.

However, some evidence exists that challenges the first view. The documented dividend yield effect is too large to be solely attributed to tax, and the effect does not vary across different tax regimes. In addition, the effect is not present in large firms. This evidence leads to a second view, that there are omitted factors that affect both dividend policy and required rate of returns. As consequence, the yield-return relationship appears spurious. In this case, paying high dividends might be a part of optimal corporate policy, and thus does not necessarily decrease shareholder wealth.

In Chapter 3, I examine the yield-return relationship in the Hong Kong market, where neither dividend income nor capital gains are taxed. In such a market, paying high dividends doesn't result in higher tax burdens to investors and thus, according to the first view, doesn't result in a higher required rate of return. Alternatively, finding a positive relationship between dividend yields and stock returns in Hong Kong provides evidence to support the second view that there are omitted factors.

I document a robust dividend yield effect in the Hong Kong market. One percent difference in dividend yields relates to 1.266% and 1.262% differences in risk-adjusted returns using the CAPM and Fama-French models respectively. My result is consistent with the second view, that there are unknown factors that affect both stock required rate of return and dividend policy. In this case, paying high dividends might be a part of an optimal corporate policy, and thus does not necessarily decrease shareholder wealth.

Second, I examine the effect of compensation policy. Specifically, I consider whether the practice of using peer groups in setting Chief Executive Officers' (CEO) compensation results in inflated pay, and thus transfers shareholder wealth to the CEOs. Many critics contend that the use of compensation peer groups has resulted in pay that cannot be justified based on economic fundamentals. Still others argue that peer groups are an efficient way for the board of directors to determine a competitive pay level that is necessary to both retain and motivate top executives. Chapter 2 was developed with the help of two coauthors: John Bizjak of Texas Christian University and Michael Lemmon of the University of Utah. In this chapter, we examine the use of compensation peer groups using the mandated disclosure of peers that began in 2006.

We document evidence supporting both sides of the debate, and find that the wealth transfer to CEOs is relatively small. On the one hand, we find that peers are largely selected based on characteristics that reflect the labor market for managerial talent. Firms tend to choose peers that are of similar size and in the same industry, and those that exhibit similar accounting performance and similar market-to-book ratios. In addition, firms are more likely to choose peer firms with similar credit ratings and similar geographic or product diversity. Finally, firms are more likely to choose peers from industries that either supply or hire managerial talent from the firm's own industry.

On the other hand, we also find that peer groups are constructed in a manner that biases compensation upward. In particular, firms still favor including larger firms in the peer groups as larger size is associated with higher pay. In addition, firms tend to set pay target at or above the peer-group median pay. The degree of biases is larger for firms outside the Standard & Poor's (S&P) 500. On average, the difference in total pay between the target peers and the sample firms is 7.5% for S&P 500 firms and 27% for non-S&P firms. However, pay increases close only about one-third of the difference, suggesting that the wealth transfer to the CEOs is relatively small.

CHAPTER 2

ARE ALL CEOS ABOVE AVERAGE? AN EMPIRICAL ANALYSIS OF COMPENSATION PEER GROUPS AND PAY DESIGN[1]

2.1 Introduction

> *When shareholders question lush pay, they are invariably met with a laundry list of reasons that businesses use to justify such packages. Among that data, no item is more crucial than the "peer group," a collection of companies that corporations measure themselves against when calculating compensation.*[2]

Arguably few economic topics stir as much passion, controversy, and debate as CEO pay. As the above quote illustrates, for many firms one of the driving factors in setting both levels of pay and pay structure is the use of compensation peer groups. One of the biggest concerns with this practice, however, is that peer groups can be used to inflate pay levels. For example, according to RiskMetrics, the compensation peer group used in 2007 by the hairstyling company Regis Corp., which owns Vidal Sassoon and Supercuts, included Starbucks and H&R Block—firms that are much larger, in different industries, and with significantly higher CEO pay than Regis.[3] In general, critics of the use of peer group benchmarking argue that powerful CEOs and co-opted boards

[1]This chapter was coauthored by John Bizjak of Texas Christian University and Michael Lemmon of the University of Utah. Reprinted from Journal of Financial Economics, volume 100, issue 3, June 2011, 538-555, with permission from Elsevier.
[2]"Peer pressure: Inflating executive pay," by Gretchen Morgenson, New York Times, November 26, 2006.
[3]http://www.riskmetrics.com/press/articles/20080518_st.html.

opportunistically choose peer firms in a way that inflates CEO pay (e.g., Bebchuk and Fried, 2004). Moreover, the critics also contend that, given the prevalence of benchmarking, the opportunistic choice of peer firms has led to an upward ratcheting of pay levels over time.

Alternatively, the use of competitive benchmarking can play an important economic role in the pay-setting process. Properly structured, peer groups provide information to boards of directors for determining the competitive pay level that is necessary to both retain and motivate top executives. As in any other labor market, the forces of supply and demand are an important determinant of wages for managers. One effective way to gather information on prevailing market wages is to compare the salary of executives at one firm with those at other firms. In fact, the importance of the use of peer groups in determining competitive wages led Holmstrom and Kaplan (2003) to argue that "we need more effective benchmarking not less of it." In light of these competing views, the purpose of our paper is to provide evidence regarding the extent to which competitive benchmarking is used opportunistically (i.e., to inflate CEO pay) or whether peer groups are primarily structured to provide the board with useful information to determine the relevant market wage for the CEO.

The Securities and Exchange Commission (SEC) recently adopted new proxy disclosure rules that require firms to report the peer groups they use to set managerial compensation as long as the use of peer groups is material in determining pay.[4] We gather data on compensation peer groups for firms in the ExecuComp database with fiscal-year ends between December 2006 and May 2007. For this sample of firms, we

[4]The new SEC disclosure requirement became effective for fiscal years on or after December 15, 2006. See SEC final rules 33-8732a, Item 402(b)(2)(xiv).

find that 69% (808 out of 1,178 firms) report the composition of their compensation peer groups.

We begin our study by examining the characteristics of firms that are selected for inclusion in the compensation peer group. Consistent with the description of the compensation process in Murphy (1999) and with the discussion in corporate proxy statements, our analysis indicates that firms tend to favor peers that are of similar size and in the same industry, and those that exhibit similar accounting performance and similar market-to-book ratios. When peers are chosen from outside the industry, they tend to be drawn from industries that have higher stock return correlations with the firm's own industry. In addition, firms are more likely to choose peer firms with similar credit ratings, similar geographic or product diversity, and firms in the S&P 500 are more likely to choose other S&P 500 firms as peers. Finally, firms are more likely to choose peers from industries which either supply or hire managerial talent from the firm's own industry.

While these findings suggest that the selection of compensation peers reflects labor market considerations, the ultimate choice of the peer group is often a joint decision between the board, firm executives, and compensation consultants. The potential for influence and conflicts of interest among these parties allows for the possibility that peer groups are chosen largely based on economic considerations but in a manner that biases compensation upward. We identify and examine three ways that peer groups can potentially be manipulated to inflate pay. First, firms can target pay at higher percentiles of the peer group pay distribution in order to benchmark pay against firms that have higher compensation. Second, firms may systematically choose peer firms that are larger and have better performance, since compensation is correlated with firm size and performance. Finally, holding labor market factors constant, firms may choose peer firms with higher compensation levels.

With respect to the first channel, all but two of the firms in our sample target total pay at or above the 50^{th} percentile of the peer group pay distribution. For S&P 500 (non-S&P 500) firms, 32% (27%) report using pay targets above the 50^{th} percentile and the mean pay target is 56.2% (54.7%). Targeting pay above the peer group median may be justified if, for example, the firm is larger or more complex than the majority of firms in the peer group (or in the industry). We do not find any evidence, however, that firm size or complexity is associated with higher than median pay targets, which casts doubt on this economic rationale for targeting pay above the peer group median.

To further examine whether firms systematically choose compensation peers in a manner that justifies higher pay levels, we compare the characteristics of each sample firm with those of the selected peer group. For S&P 500 firms, the median peer firms are approximately the same size, in terms of sales revenues, and also have similar pay levels. In contrast, for non-S&P 500 firms the median peer firms are approximately 25% ($172 million) larger in terms of sales revenue, and have total compensation that is approximately 16.5% ($365,000) higher than that of the sample firms. These findings suggest that there are systematic biases in peer group selection, but that these biases differ considerably between S&P 500 firms and non-S&P 500 firms.

The differences in compensation between the peer firms and the sample firms that we find reflect two potential sources of bias. The first is the possibility that the peer firms are systematically larger than the sample firms, which is consistent with the size differences we document for non-S&P 500 firms. The second is the possibility that holding constant differences in size, performance, and other observable characteristics, firms can favor peers with higher pay. To explore this latter issue, we use propensity score matching (PSM) to identify a matched peer group for each firm in the sample, and compare the characteristics of the actual peer group with those of the matched peer group. Based on this analysis, we find little evidence that after controlling for the biases in firm size and performance, firms tend to favor higher paid peers.

A natural question to ask is whether the biases in peer group composition are related to the quality of corporate governance. We find no consistent evidence that firms with weak governance exhibit larger biases in peer group selection. Instead, at least for non-S&P 500 firms, the inclination to select peers opportunistically appears to be widespread.

The fact that smaller firms in particular exhibit larger biases in peer group selection is interesting in that much of the criticism of peer groups has been motivated by the pay of CEOs in large firms. One possible explanation for the differences between S&P 500 firms and the rest of the sample is that S&P 500 firms are more visible and attract greater scrutiny compared to other firms, thus making it more difficult for CEOs to significantly influence the choice of the benchmark. In addition, it may simply be more difficult to make significant adjustments to the benchmark in S&P 500 firms because these firms are already among the largest firms and have the most highly paid executives.

By benchmarking themselves against larger and more highly paid peers, CEOs can attempt to negotiate larger pay increases than can be justified by economic fundamentals. If boards are aware of biases in the peer group composition, however, they may exercise discretion when setting pay that mitigates the effects of peer group bias. Consistent with the latter view, we find that boards make only partial adjustments to pay in response to pay differences between the comparison group and the CEO. On average, the annual increase in compensation closes about one-third of the difference in pay between the CEO and the peer group in S&P 500 firms and about 27% of the pay gap in non-S&P 500 firms. The fact that the upward bias in peer group pay is most evident in non-S&P 500 firms, but that the adjustment coefficient is smaller in these firms, suggests that the overall benefit to these CEOs of inflating peer group pay is relatively small.

In the final analysis in the paper, we examine the stability of peer groups over time by comparing the peer groups reported in 2006 (the first year of the SEC regulation) with those reported in 2007. Approximately 25% of the firms made substantial changes to their peer groups between the two years and those making large changes appear to do so in ways that reduce the biases in peer group choice. We view this as preliminary evidence that the new disclosure requirements have prompted firms to be less opportunistic in the selection of peers, but a more definitive answer to this question will require additional years of data.

Our work is related to several largely contemporaneous papers that also examine the effect of peer group benchmarking on CEO pay (Faulkender and Yang, 2010; Albuquerque, De Franco, and Verdi, 2009; and Cadman and Carter, 2009). We differ from these studies on a number of dimensions: (i) We identify and examine three separate ways that firms can potentially manipulate the peer group to influence pay and how biases in peer group composition differ across S&P 500 and non-S&P 500 firms; (ii) we incorporate information on pay targets in our analysis; (iii) we examine a more extensive set of labor market factors associated with peer firm choice; (iv) we examine to what degree biases in peer group composition lead to increases in CEO pay; and (v) we provide evidence on changes in peer group composition in response to the new disclosure requirements.

The remainder of the paper is organized as follows. Section 2.2 describes the data used in the analysis. Section 2.3 provides a discussion of how peer groups are used by boards to set compensation and lays out our general research questions and hypotheses. Section 2.4 examines how firms choose peer groups and begins our analysis on whether peers are chosen opportunistically or to provide information about managerial labor markets. Section 2.5 provides further analysis of opportunistic peer group selection. Section 2.6 examines how peer groups affect managerial compensation. Section 2.7 provides some preliminary evidence on the effectiveness of the SEC regulation by analyzing changes in the composition of peer groups from 2006 to 2007. Section 2.8 concludes with a brief summary and discussion.

2.2 Data collection and summary statistics

We begin our analysis of peer groups with the list of firms with compensation information reported in ExeCucomp with fiscal-year ends between December 2006 and May 2007.[5] For each of these firms, we examine the corporate proxy statement for 2007 and gather information on the peer group that is used to set executive compensation. Wherever possible, we collect compensation data for all firms in the peer group. When

[5]Firms were required to report information on their compensation peer groups for filings at companies with fiscal year-ends on or after December 15, 2006. Prior to 2006, some firms did voluntarily report the use of their peer groups, but it was rare. For example, Faulkender et al. (2010) report that only 83 firms in the S&P 500 reported their peer group in 2005.

peer group firms are not part of the ExecuComp database, we collect the compensation data from corporate proxy statements and other sources.[6] In addition to the data on the firms comprising the peer group, we also collect data on the target pay percentiles used by the firms. Data on governance characteristics of the firms come from the Investor Responsibility Research Center (IRRC) database. Financial data are collected from Center for Research in Security Prices (CRSP) and Compustat. The data reflect the peer groups that were used to set compensation for the 2006 fiscal year. Thus, boards would rely on compensation and financial data from the 2005 fiscal year in forming their comparisons with the peer firms. Throughout the paper we adopt a similar timing convention and base our comparisons on data from fiscal year 2005.

Out of 1,178 firms in the ExecuComp database for which we obtained proxy data for fiscal year 2006, we find that 808 firms report the peer group they use for setting executive pay. We exclude 10 sample firms that report zero CEO compensation in either fiscal years 2005 or 2006.[7] We further exclude 91 sample firms for which we are unable to obtain compensation data for <u>all</u> of the reported peers. Peer firms with missing compensation data are generally either foreign or private firms or subsidiaries of other firms. The final sample of 707 firms which we use for the majority of our analysis consists of 259 firms from the S&P 500 index and 448 firms outside of the S&P 500.

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[6]Note that the reporting format for compensation changed in 2006, and that the reporting changes affect the way in which total compensation is reported in ExecuComp. In most of our analysis, we focus on compensation data for 2005 (2006 proxy year), and thus, rely on data prior to the reporting changes. In some analysis, we do examine changes in compensation from 2005 to 2006. In these cases, the comparisons may partially reflect the reporting changes rather than real changes in compensation. The changes in total compensation as reported by ExecuComp are described at http://wrds.wharton.upenn.edu/support/docs/exec/ExecutiveCompensation1.pdf.

[7] These CEOs are generally those facing special situations. For example, the two top executives of Google take no explicit compensation, but have a large ownership stake. Although peer groups are not used to set CEO compensation in these firms, they are used to set the pay of other top executives.

Table 1 defines the variables used in our analysis and table 2 reports summary statistics comparing the characteristics of firms that report using peer groups for which we have data on all peers to those that do not report using a peer group. Firms that do not report compensation peer groups are about half as large in terms of sales and assets as firms with complete peer data. In addition, these firms tend to have higher CEO ownership, longer CEO tenure, and slightly more insiders on the board of directors. Finally, firms that do not report peers have lower median levels of salary and bonus (S&B) and total pay, but somewhat higher median ratios of salary and bonus and total pay scaled by sales revenue. Firms not using peer groups also have higher ratios of salary and bonus scaled by total pay. Overall, firms that do not report peers are smaller in size, are more closely held, and are firms where the CEO potentially has a greater control over compensation. For these types of firms, compensation peers may be less crucial in determining CEO pay.

Table 2 also compares the characteristics of the sample of 707 firms with complete data for all peer firms and the 91 firms without compensation and sales data for all of the peer firms. For the most part, there are no significant differences in firm characteristics between the two samples with one notable exception. Firms without complete data for all peers have slightly higher pay, both in terms of S&B and total pay. The pay measures scaled by sales revenue, however, are not different between the two samples. Overall it does not appear that there are significant selection issues associated with having complete data for all peer firms.[8]

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[8]Including firms in the analysis with missing peer data can potentially lead to incorrect inferences regarding the extent to which peer groups are composed to bias pay comparisons upward. For example, if peers with missing data tend to have low pay, this will bias the peer group median pay level upward for these firms.

2.3 The compensation process and peer group benchmarking

For many publicly traded companies, compensation is set by the firm's compensation committee, which is comprised of members of the board of directors. In many cases, the compensation committee selects a peer or comparison group of firms that it uses to gather information on pay practices and pay levels. In most firms, salary, bonuses, option pay, and total compensation are in some form anchored to the peer group. Firms typically target the various components of pay at the median pay level of the comparator group. It is not uncommon, however, for firms to target pay above the median (e.g., at the 75th percentile).[9] Understanding how firms choose compensation peers provides insight into the role they play in determining managerial compensation.

2.3.1 Peer groups as a gauge of managerial labor markets

If managerial ability is an important factor in determining firm performance, then the nature of the managerial labor market will play a significant role in assessing the amount of compensation that is necessary to retain and motivate executives. One of the most important ways that a board can get information on the labor market for managers is to look at compensation practices in firms that it competes with for talent. If peer group benchmarking is used to evaluate the reservation wage necessary to attract and retain qualified executives, then we expect peer firms to be selected based on factors related to supply and demand conditions in the managerial labor market.

[9]According to RiskMetrics, 99.5% of firms in the S&P 1500 targeted pay at or above the median of their peer group. We report data on the target pay percentiles used in our sample in Table 5.

Consistent with this view, corporate proxy statements frequently mention that the purpose of compensation peer groups is to provide information on the managerial labor market. For example, the 2007 proxy statement of ATC Technologies Inc. states that the primary criterion for the selection of compensation peers is to provide “a broad view of the executive labor market against which we compete for executive-level talent.” In what follows, we discuss a number of characteristics that should proxy for commonalities in the managerial labor market between the firm and its selected peers.

2.3.1.1 Industry. A natural source for compensation peers are firms in the same industry. Firms that provide similar products and that are competitors for customers are also likely to be firms where a company will look to when trying to recruit executives. Corporate proxy statements also indicate that industry is an important factor when picking compensation peers. For example, the 2007 proxy statement for Fossil Inc. states that their compensation program is “designed to be competitive with the companies in the industry in which we must vie for talent.”

2.3.1.2 Firm size. Firm size is an indicator of organizational complexity and scope. Consequently, we expect firms to select as compensation peers other firms that are similar in size. A reading of corporate proxy statements supports the notion that firm size plays an important role in peer selection. For example, Fossil Inc. states that “the peer group is comprised such that the median revenue size of the peer group is at or close to our annual revenue.”

While size and industry are important factors in the selection of peer groups, firms often go outside their own industry when picking peers. For example, the 2007 proxy statement of Biogen states, “The named peer group is reviewed annually by the

Committee for appropriateness, considering such factors as size (e.g., revenue and market capitalization), complexity (e.g., multiple marketed products), geographic scope of operations (e.g., global versus domestic-only presence), etc." There are a number of reasons a firm may not pick peers from the same industry. Large firms that dominate a particular industry may choose not to include other peers within the industry if the firm is significantly larger than its industry competitors. Other relevant labor market factors include:

2.3.1.3 Firm performance. Firm performance may be used to identify firms for inclusion in the compensation peer group. Firms with similar market-to-book ratios may share similarities in profit models or organizational structure (Smith and Watts, 1992), and firms with similar profitability may be exposed to similar demand shocks.

2.3.1.4 Customers and suppliers. A firm's customers and suppliers are familiar with the firm's operational structure and serve as a natural source for recruiting executives. For example, executives at auto parts suppliers may have a thorough understanding of automobile manufacturing and provide for a pool of talent that automobile manufacturers can hire from. Pharmaceutical companies might hire from hospitals where they sell their products. To the degree that a firm's customers and suppliers provide a resource for managerial talent, we would expect that these firms would be appropriate for selection as part of the compensation peer group.

2.3.1.5 Capital markets. Companies that are competitors for equity or other types of financial capital may also serve as relevant peers. For example, the 2007 proxy statement for Avon Products Inc. states that peers are "selected based on the fact that the Company competes with these organizations for employees, customers and **shareholders** (bold added)." For firms that are dominant in an industry or that have few direct competitors, another potential source of peer firms comes from other companies that investors might view as substitutes in their portfolios. For example, the top ten firms in the S&P 500 have few organizations in the same industry that are similar in terms of size and operating characteristics. While these firms may not be direct competitors, one thing they have in common is the need to raise and manage enormous sums of capital. These firms may look to each other as peers or to other firms they compete with for equity capital.

2.3.1.6 Diversified firms. Diversified companies are often more complex organizations that require a specific set of managerial skills. Because of this, we expect that diversified firms will be more likely to look to other diversified firms for executive talent and to be more likely to include these firms in their peer group. Diversification may be measured across either product lines or across geographic regions.

2.3.1.7 Labor flows. Flows of executive talent between firms are a direct measure of the degree of substitutability of human capital. We expect that compensation peers will be more likely to come from industries which either supply (demand) talent to (from) the firm's industry.

2.3.2 Peer groups and managerial opportunism

Bebchuck and Fried (2004) argue that CEOs have significant ability to influence their own compensation because boards are co-opted or are simply ineffectual. With respect to the use of compensation peer groups, Jensen, Murphy, and Wruck (2004) note that initial recommendations on pay usually emanate from the firm's human resource department, often working in conjunction with compensation consultants whose primary job is to provide survey information for competitive benchmarking of pay. Moreover, in many instances compensation consultants cross-sell other services to the firms they work for (Murphy and Sandino, 2010; Cadman, Carter, and Hillegeist, 2010). In addition, the CEO often participates in deliberations of the compensation committee except those specifically dealing with the CEO's own pay.[10] While the use of compensation peer groups can provide valuable information to boards for determining appropriate compensation, the potential for weak boards and conflicts of interest in the process by which peer firms are chosen leaves scope for the possibility that the composition of the peer group will be biased in order to justify higher pay.[11]

There are primarily three ways that managers can potentially influence the peer group selection process in a manner that enhances compensation. Since it is well-documented that pay is correlated with firm size and performance, executives can justify or seek higher pay levels if they can select firms in the peer group that are larger and better performing. Besides putting larger firms in the peer group, firms can directly pick peers with higher levels of pay since the rules that companies use to justify inclusion of a firm into a peer group can be amorphous. For example, according to the 2007 proxy statement of Best Buy Inc., the firm selects peers because of "admiration within their industry" and those that have a "track record of innovation." While both qualities might

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[10]The 2007 proxy statement of Schnitzer Inc. discusses the role that the CEO and management plays in the selection of peers:

> "The CEO, with the assistance of Towers Perrin, analyzes survey data and makes recommendations to the Committee regarding compensation for the executive officers. The CEO participates in Committee meetings at the Committee's request to provide background information regarding the Company's strategic objectives and his evaluation of the performance of and compensation recommendations for the other executive officers. With respect to his own compensation, the CEO responds to requests from the Committee."

[11]Alternatively, Hayes and Schaefer (2009) show that boards may desire to rationally inflate pay to influence market perceptions.

be admirable, this type of selection criteria can lead to opportunistic behavior on the part of management. Finally, managers may argue that pay should be benchmarked against higher percentiles than the median pay in the peer group (e.g., the 75th percentile).

If managers are opportunistically selecting firms in the peer group in order to inflate pay, we anticipate that the composition of the peer group will deviate from the peer group that would be chosen purely based on the economic criteria discussed above that describe the relevant managerial labor market. Instead, we expect that managers will systematically select peer firms that are larger and have higher levels of compensation.

2.4 The selection of firms for the peer group

To explore how companies select compensation peers, we begin with an analysis of the characteristics of the firms included in the peer group. Next, we examine the determinants of peer firm selection.

2.4.1 Peer group characteristics

Table 3 reports summary information on the size and composition of peer groups. The average (median) size of the peer group is around 16.4 (15) firms. S&P 500 firms include more firms in their peer groups compared to firms not in the S&P 500, but the differences are small. The majority of firms in the peer group come from the same industry. Using the Fama and French (1997) 49-industry classification, we find that, on average (median), 63% (73%) of firms in the peer group are in the same industry as the sample firm. Non-S&P 500 firms select more firms from the same industry compared to firms in the S&P 500. To get a feel for how size also affects the selection of peer firms, we examine the fraction of peer firms in the same industry that also has sales revenue between 50% and 200% of that of the sample firm. Using this taxonomy, we find that, on average, 37% of peer firms are in the same industry-size classification. Firms outside of the S&P 500 tend to have more of their peers chosen from the same industry-size group. The fact that S&P 500 firms are more likely to include peer firms outside of their industry-size group likely reflects the fact that these firms are among the largest in the economy and have few same-industry peers that are comparable in terms of size and compensation.

As a basis for comparison, Faulkender and Yang (2010) report an average (median) size of peer groups to be 18.2 (16) which is slightly larger compared to our findings.[12] Using two-digit Standard Industrial Classification (SIC) codes, they find that the average (median) fraction of firms in the peer group that are in the same industry as the sample firm is 46% (44%). Faulkender and Yang limit their sample to the S&P 900 (S&P 500 plus the S&P midcap 400) and do not separately examine firms in different size groups. Our results indicate the potential for distinct differences in peer group composition in large (S&P 500) and small (non-S&P 500) firms and suggest it is important to partition the data based on firm size. From here on we report results separately for these two groups of firms.

2.4.2 The determinants of peer group composition

Next, we estimate multivariate logit regressions to identify factors that firms use when selecting peers. The set of potential peer firms includes all sample firms and all of

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[12]The difference is driven by the fact that we limit our sample to those firms with complete data on all peers, and firms with larger peer groups are more likely to contain peer firms for which we cannot obtain compensation data.

the peer firms disclosed by these firms. The dependent variable is one if the firm is selected as a member of the compensation peer group at a particular firm and zero otherwise. Based on the discussion in Section 2.3, the independent variables in the regression attempt to capture firm and industry traits related to supply and demand conditions in the managerial labor market.

The independent variables include an indicator equal to one if the firm and the potential peer share the same Fama and French 49 industry classification. To assess how firm size affects peer firm selection, we include two relative size variables. The first is equal to the difference in log sales between the potential peer and the firm when this difference is positive, and is set equal to zero otherwise. The second is equal to the difference in log sales between the potential peer and the firm, when the distance is negative, and zero otherwise. This specification allows for asymmetry in how relative size affects the choice of peer firms.[13] We form similar asymmetric measures for relative accounting performance and for market-to-book. Allowing for asymmetry in the size and performance measures allows us to examine whether firms have a tendency to pick peer firms that are larger and with better performance.

To capture commonalities across industries, we include the correlation of returns between the firm's industry and the potential peer firm's industry, where the correlation is measured using industry daily returns over the period 2004-2005.[14] We also include a measure of whether the potential peer firm is in an industry with significant customer or supplier relationships with the firm's own industry. To measure supply chain

[13]Note that by using the difference in log sales, we are essentially examining differences in relative size in percentage terms, which is consistent with how firms describe peer group choice in the proxy statements.
[14]We obtain data on industry returns from Ken French's Web Site http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.

relationships, we use the commodity flow data from the input-output (I/O) tables provided by the Bureau of Economic Analysis (BEA).[15] For each firm and potential peer, we compute the fraction of output that the firm's industry sells to the potential peer's industry and the fraction of input that the firm's industry purchases from the potential peer's industry (Lemelin, 1982; and Fan and Lang, 2000). To capture labor flows between industries, we compute the fraction of external hires in the firm's industry that come from or leave for the potential peer firm's industry. We compute this measure using data from ExecuComp over the five-year period 2001 through 2005.[16]

To proxy for other potential labor market linkages, we include a dummy variable equal to one if the firm and the potential peer are both in the S&P 500 and a dummy equal to one if they are both not in the S&P 500. We conjecture that S&P 500 firms are more likely to compete with other firms in the S&P 500 for executive talent. We include a dummy equal to one if the firm and potential peer share the same credit rating to proxy for common capital market characteristics. Finally, to capture commonalities in business complexity, we include dummy variables equal to one if both the firm and the potential peer report multiple business segments or multiple geographic segments. The variable definitions are provided in the Appendix. Finally, the regression also includes firm fixed effects to account for the differences across firms in the unconditional probabilities of a firm being chosen as a peer firm that arise because the size of the peer group differs across firms. The *p*-values based on robust standard errors clustered at the firm level are

[15] The Web Site for the I/O data is http://www.bea.gov/industry/index.htm#benchmark_io.
[16] To gather information on the firms/industries that the company either hired or lost talent to, we examined executive turnover over the last five years using the ExecuComp data. Note that this measure does not capture labor movements across firms not included in the ExecuComp data, and is therefore a noisy measure of labor flows.

reported in parentheses below the coefficient estimates and the marginal effects are reported in brackets.[17]

Focusing on model 1 in Table 4, all the coefficient estimates in the regression with the exception of the variables measuring the fraction of output (input) that the firm's industry sells to (purchases from) the potential peer's industry are statistically significant and nearly all of them have the expected sign. Moreover, many of the effects are economically large. For example, the results indicate that firms in the same industry are 42% more likely to be chosen as peer firms, and when firms do go outside the industry, they tend to select firms from industries that have higher stock return correlations with their own industry. With respect to firm size, the likelihood of a given firm being chosen as a peer declines as the absolute difference in log sales between the firm and the potential peer gets larger. The effect, however, is asymmetric. Conditional on the potential peer firm being larger (smaller) than the firm of interest, the marginal effect indicates that a one unit increase (decrease) in relative size decreases the probability of the firm being chosen as a peer by 18% (34%).[18] Thus, although firms are less likely to choose firms as peers that are either larger or smaller than themselves, when they do choose peers that are different in size, they are *more likely* to choose larger firms as peers and *less likely* to choose smaller firms. Similar results hold for the variables measuring relative performance and market-to-book. Firms are more likely to choose peers that have better performance and higher market-to-book ratios compared to potential peers

[17]The marginal effects reported in brackets in Table 4 are calculated as the partial derivative of the event probability and are computed at the following values of covariates: indicator variables 3, 15, 17, 18, and 20 set to one, indicator variables 16, 19, and 21 set to zero; all distance variables (1–2, and 5–10) set to zero; the remaining variables (4, 11–14) set at the respective means.

[18]Note that for the coefficient α_6, the independent variable (difference in log size between the peer and the firm conditional on the firm being larger than the potential peer) is negative, and thus, a positive coefficient estimate indicates that the probability that a particular firm is chosen as a peer is decreasing as the firm gets larger than the potential peer.

with lower relative performance. Faulkender and Yang (2010) do not find evidence of asymmetry in the effects of size on peer firm selection. The primary reason for this appears to be because they measure size using two indicator variables equal to one if the potential peer firm's sales are between 50% and 100% or 100% and 200% of the sample firm's sales. We also find much less evidence of asymmetry in the effects of firm size if we use similar indicators instead of the continuous measures of relative size.

Consistent with labor market considerations, compensation peers are also more likely to come from industries which either hire or supply executive talent to the firm's industry. Other notable findings are that firms in the S&P 500 are more likely to choose peers that are also in the S&P 500. Firms outside the S&P 500, however, are less likely to choose other non-S&P 500 firms as peers, all else equal. Firms that report multiple business or geographic segments are more likely to choose other diversified firms, and firms are more likely to select peers that share the same credit rating.

To provide some preliminary evidence on whether, all else equal, firms favor potential peers with higher relative pay, in model 2 we add measures of relative pay equal to the difference in log total compensation between the potential peer and the firm. Similar to our investigation of relative size and performance, we allow for asymmetry in how relative pay affects the choice of peer firms. The coefficient estimates indicate that firms do tend to favor potential peers with higher relative pay compared to those with lower relative pay, although the effects are not economically large. For example, the marginal effect indicates that a one unit change in relative pay when the pay of the potential peer is above (below) the pay of the sample firm decreases the probability of the firm being chosen as a peer by 2.4% (5.6%).

Finally, in unreported results we also find that the basic findings remain similar to those reported for the full sample when we separately estimate the regressions for S&P 500 and non-S&P 500 firms. One exception is that S&P 500 firms are more likely to include customer firms in their peer group, but are less likely to include suppliers.

Overall, the analysis indicates that, for the most part, when firms pick compensation peers the goal is to identify other firms that share common labor market characteristics. Nevertheless, the results also suggest some potentially opportunistic behavior in the selection of peers. In particular, the asymmetric effects of relative firm size and firm performance indicate that firms tend to favor larger and better performing firms when choosing the peer group, while the asymmetric effects of relative pay indicate that firms also favor peers with higher compensation, all else equal. In the next section we attempt to quantify the extent of these biases in peer group composition.

2.5 Biases in peer group selection

There are potentially three ways that peer groups can be manipulated to influence pay. First, firms can target pay at higher percentiles than the median of the peer group pay distribution in order to benchmark pay against firms that have higher compensation. Second, consistent with the results from the logit analysis above, firms may systematically choose peer firms that are larger and have better performance, since compensation is correlated with firm size and performance. Finally, holding other factors constant, companies may favor peer firms with higher compensation levels.

2.5.1 Pay targets

We gather data from proxy statements and report summary statistics on the pay targets used by firms in Table 5. Firms sometimes report different targets for different components of pay and we focus on the targets for total pay. As shown in Panel A, of the 707 firms in our sample, 229 do not disclose their pay target. For these firms, we assume that the pay target is the 50^{th} percentile of the peer group pay distribution. For the 478 firms that disclose information on their pay targets, 103 report a range for target pay. In these cases, we use the middle of the target range. In nine cases, the bottom of the range is below the 50^{th} percentile (for two firms the middle of the range is less than the 50^{th} percentile and seven firms have the middle range at or above the 50^{th} percentile). In all other cases, the bottom of the range is at or above the 50^{th} percentile. The 375 remaining firms disclose a specific pay target, and of these firms, 268 (107) target at (above) the 50^{th} percentile of total pay. Panel B of the table reports the mean pay targets as well as the fraction of firms with pay targets above the median. As shown in the table, for S&P 500 firms, 32% of the firms target total pay above the 50^{th} percentile and the average pay target is the 56^{th} percentile. For non-S&P 500 firms, 27% of firms have pay targets above the 50^{th} percentile, and the mean pay target is approximately the 55^{th} percentile. The difference across the two groups in the level of the mean pay target is statistically significant, but the difference in the fraction of firms targeting above median pay is not statistically different across the groups.

One reason that boards might choose a pay target above the median is because their firm is larger or has greater complexity than the median firm in the peer group. For example, the 2008 proxy for JB Hunt states: “Given the peer group’s size disparity, the

Committee decided that the appropriate comparative compensation target should be at the 75th percentile of the peer group." To explore whether firm size or complexity explain the use of pay targets above the median, Panel C of Table 5 reports results from a logit regression where the dependent variable equals one if the target pay percentile is above the median and zero otherwise. As proxies for size and complexity we include the natural log of sales revenue, an indicator equal to one if the firm has multiple business segments, an indicator equal to one if the firm has multiple geographic segments, and an indicator for S&P 500 firms. As seen in the table, none of the coefficient estimates are statistically significant, which does not support the hypothesis that larger and more complex organizations are more likely to target pay above the median. Instead, the results suggest that at least some firms might use high pay targets to inflate CEO pay.

2.5.2. Size, performance, and pay biases

Table 6 compares size, accounting performance, and compensation between the sample firms and their peers. Using the peer groups corresponding to each sample firm, we compute the median value of the characteristic of interest. The table then reports the median value of these medians across the sample firms. We report results for S&P 500 firms in Panel A and for non-S&P 500 firms in Panel B. For S&P 500 firms, we find that peer groups appear to be constructed such that the median size (sales revenue) and accounting performance return on equity (ROA) of the peer firms are similar to those of the sample firms. In terms of pay, the peer firms have total pay levels when measured in logs that are not statistically different from those of the sample firms. In dollar terms, however, there is some evidence that peer firm pay is lower than that of the sample firms.

The median difference in dollar total compensation between the peer groups and the sample firms is -$161,000 (*p*-value < 0.05).[19] Finally, S&P 500 firms tend to have a similar pay mix (salary and bonus /total pay) as the peer firms.

In contrast, for non-S&P 500 firms, we find significant differences between the characteristics of the peer firms and those of the sample firms. The median difference in terms of sales between the peer firms and the sample firms is about 25% (computed as exp(0.221)-1),[20] or $172 million. Similarly, the median difference in accounting performance between the peers and the sample firms is also positive, but is not statistically significant. Consistent with these size and performance differences, the peer firms also have higher pay levels compared to the sample firms. At the median, the total pay of peer firms is about 16.5% ($365,000) higher than that of the sample firms. Part of the explanation for this could be that the selected peers have more equity-based pay than the sample firms. For example, the median ratio of salary and bonus to total compensation is 49.7% for the sample firms compared to 44.9% for the peer firms. To the extent that equity-based pay requires a higher risk premium, pay levels will be higher in the peer firms. The difference in the amount of equity-based pay, however, seems small relative to the differences in total compensation. Overall, in comparison to S&P 500 firms these differences in pay appear to be economically important.

As noted in the prior subsection, about one-third of the firms in our sample use pay targets above the median. To provide some evidence on how pay targets affect the pay comparisons with the peer group, we also compute the difference in total pay between the peer firm at the target pay percentile and the sample firm (results not

[19]Note that column 3 reports the median difference, which is not generally equal to the difference in medians in columns 1 and 2.
[20]Throughout the paper we adjust the differences in logs to correspond to percentage differences.

tabulated). For S&P 500 firms, the median difference in total pay between the target peers and the sample firms is 7.5% ($524,000), and for non-S&P 500 firms the difference is 27% ($542,000). Both differences are statistically significant and provide evidence that setting pay targets above the median increases the bias in peer group pay as expected.

Based on this analysis, S&P 500 firms appear to choose peers that are generally similar in terms of size, performance, and pay. Even when pay is compared to the target pay percentile, the differences in pay are modest relative to the high pay levels in these firms. In contrast, non-S&P 500 firms tend to pick peers that are systematically larger and that have considerably higher levels of compensation. Firms outside the S&P 500 appear to choose peer firms in a manner that could lead to unjustified pay increases.

2.5.3. Peer group pay biases

A final source of potential bias is picking peers that have high pay, holding constant differences in observable characteristics like size and performance. Consistent with this possibility, the logit models in Table 4 show that firms tend to favor peers with higher relative pay, all else equal. To quantify this effect, we use the coefficient estimates from model 1 in the logit regressions estimated in Table 4 to identify a matched peer group for each sample firm. To identify the matched peers, we first compute propensity scores (i.e., the predicted probability of being chosen as a peer firm) from the logit model for each of the firms in the reported peer group.[21] For each reported peer we then identify another potential peer firm that has the closest propensity score and denote this as the PSM-matched peer firm. For each sample firm the set of potential peer firms includes all of the sample firms and their reported peers with the exception of the peer

[21]For a description of propensity score matching, see Wooldridge (2002, Ch. 18).

being matched. We then rank the actual and PSM-matched peers by total pay and compare the characteristics of the actual peers and the PSM-matched peers. We report the comparisons for both the target pay-percentile and the median.[22] In our analysis, we use the 50th percentile for firms that do not report a target percentile, and for firms that report a range of target pay percentiles we use the middle of the target range.

Note that our methodology differs somewhat from the propensity score matching (PSM) used by Faulkender and Yang (2010), who exclude all of the actual peers from the set of potential peers when performing their PSM analysis. For S&P 500 firms, we find that excluding the actual peer firms when selecting the matched peers results in PSM peers that are not well matched on size (the PSM peers are significantly smaller) and have much lower pay compared to both the sample firms and the actual peers. Failure to obtain a good size match can produce differences in pay levels between the actual and matched peer groups that are driven by size differences rather than by differences in actual pay practices between the two groups.[23]

The results of our PSM analysis are reported in Table 7. For the S&P 500 firms, both at the 50th percentile and at the target pay percentile, the actual peers are about 8.2% ($500 to $600 million) larger than the matched peers in terms of sales. The accounting performance of the matched peers is not statistically distinguishable from that of the actual peer firms. These results indicate that the matching technique does a good job of controlling for performance differences, but that some differences remain with respect to firm size. Comparing the actual peers to the matched peers, there is some evidence that,

[22]Some firms have two firms at the target pay percentile (median). In these cases we take the average value of the characteristic of interest. In cases where we report log values, we take the log of the average value.
[23]Faulkender and Yang (2010) do not report size comparisons between the actual peers and their PSM peers. When we conduct our analysis on only the S&P 900 firms excluding all actual peers in the PSM matching, we find a poor size match between the actual peers and the PSM peers.

after controlling for observable characteristics, S&P 500 firms select higher paid peers. Total compensation is between 6% and 8% ($414,000 and $536,000) higher compared to the matched peers at the 50th percentile and the target pay percentile, respectively. All of the differences are statistically significant. While these results suggest that even after controlling for the biases in firm size and performance, S&P 500 firms tend to favor higher paid peers, we urge some caution in interpreting this result. Given that the PSM-matching technique does not completely eliminate size differences between the matched peers and the actual peers, any differences in compensation could be because of a failure to obtain a good size match.

The results for the non-S&P 500 firm in Table 7 indicate that the differences in size and performance between the matched peers and the actual peers are not statistically significant indicating that the matching technique does a good job of controlling for these characteristics. Comparing the pay levels of the actual peers and the matched peers, none of the differences in pay are statistically significant with the exception that the actual median peer firms have compensation that is $24,000 higher than the corresponding PSM peers (p-value < 0.10). For non-S&P 500 firms, it appears that most of the bias in peer group pay comes from choosing systematically larger and better performing peers and through targeting above median pay, and not from selecting peers with higher pay, all else equal.

2.5.4 Peer group biases and corporate governance

We next examine whether the biases in peer group selection that we show above are systematically related to corporate governance. We focus on three measures of corporate governance that are often associated with managerial entrenchment: i) CEO tenure, ii) the fraction of the board that was hired after the CEO took office, and iii) the Gompers et al. (2003) measure of the strength of shareholder rights (GIM index). If powerful CEOs are able to influence the selection of the compensation peer group, we expect firms with weaker governance—longer CEO tenure, more directors hired after the CEO took office, and a higher GIM index—to exhibit larger biases in peer group composition.

We conduct basic univariate analysis to study the effects of governance on peer group bias and the results are presented in Table 8. For each governance variable, we split firms into two groups based on the median value of the characteristic of interest. For each group we report the fraction of firms with pay targets above the 50th percentile, the difference in log sales between the median peer firm and the sample firm, and the difference in log total compensation between the medians of the actual and PSM-matched peer groups. Results are reported separately for S&P 500 and non-S&P 500 firms in Panels A and B, respectively.

Overall, there is no consistent evidence that weak governance is systematically associated with greater peer group biases. For S&P 500 firms there is no evidence that any of the peer group bias measures are statistically different across firms with strong and weak governance when we measure governance based on either CEO tenure or the GIM index. In contrast, firms with a high fraction of board members hired after the CEO are slightly more likely to have pay targets above the 50th percentile (p-value < 0.10). For non-S&P 500 firms, those with longer CEO tenure are more likely to have above median pay targets (p-value < 0.05) as predicted by the weak governance hypothesis. In contrast, however, firms with more entrenched managers as measured by the GIM index are less likely to have above median pay targets (p-value < 0.05). None of the other measures of peer group bias are systematically correlated with the governance measures with the exception that the size bias is larger in firms where more board members are hired after the CEO (p-value < 0.10).

2.5.5 Discussion

To summarize, we find evidence of systematic biases in peer group composition that are consistent with peer groups being constructed in ways that inflate CEO pay. For S&P 500 firms, the biases in peer group pay are relatively modest compared to the high pay levels in these firms. In contrast, non-S&P 500 firms exhibit significant biases in peer group composition that allow managers to benchmark themselves with firms that have significantly higher pay. In non-S&P 500 firms the source of bias largely comes from benchmarking against firms that are considerably larger and through targeting pay above the 50th percentile. While there is some evidence that peer group biases are larger in firms with weak governance, the evidence is not consistent across different governance measures.

The differences that we find between S&P 500 and non-S&P 500 firms are interesting in that much of the criticism of peer groups has been motivated by the pay of CEOs in large firms. One possible explanation for the differences between S&P 500 firms and the remaining firms in the sample is that S&P 500 firms are more visible and attract greater scrutiny compared to other firms, thus making it more difficult for CEOs to significantly influence the choice of benchmark. In addition, it may simply be more difficult to make significant adjustments to the benchmark in S&P 500 firms because these firms are already among the largest firms and have the most highly paid executives.

2.6 The effect of peer groups on CEO pay

In this section, we provide evidence on the extent to which CEOs actually benefit from the biases in peer group construction that we document above. By benchmarking themselves against larger and more highly paid peers, CEOs can attempt to negotiate larger pay increases than can be justified by economic fundamentals. If boards are aware of biases in the peer group composition, however, they may exercise discretion when determining pay levels that mitigates the effects of peer group bias.

To examine this issue, our analysis follows Bizjak et al. (2008) and is designed to mimic the way in which firms use peer groups to benchmark compensation as described in Murphy (1999) and in proxy statements. The results are reported in Table 9. The dependent variable is the change in the log of total compensation between 2005 and 2006. Independent variables include log sales, change in log sales (2005 to 2006), and firm volatility. Also included are measures of current (2006) and prior (2005) stock price and accounting performance. Finally, we also include log pay in 2005 to account for any mean reversion in pay. For example, if firms grant options every other year, the low pay in a given year with no option grants would be correlated with a large change in pay in the subsequent year.

To provide a benchmark to assess the effect of peer group benchmarking on pay changes, we introduce a "naïve" peer group into the empirical model. The naïve peer group is constructed using all of our sample firms and all of the reported peer firms. The naïve peer group contains all firms in the same industry (Fama and French 49-industry groups) with sales revenues between 50% and 200% of those of the sample firm—which is a common criterion stated in proxy statements.[24] For each firm we compute the difference in log compensation between the median of the naïve peer group and the sample firm based on compensation data in 2005, which is the compensation data that the board could observe when determining compensation for 2006. This distance measure thus captures how much the manager is paid relative to the median firm within the same industry. The coefficient estimate on this variable measures how the firm adjusts the manager's compensation in 2006 as a function of the manager's pay relative to the naïve peer group in 2005.

In model 1, the change in pay is positively related to both the level and change in sales and to contemporaneous stock and accounting performance, although the coefficient estimates on the performance variables are not statistically significant. The coefficient estimate on the peer group distance variable indicates that a manager with pay 1% below the naïve peer group median pay level receives an increase in pay that is approximately 0.22% larger compared to a manager with pay equal to the naïve peer group median. The adjusted *R*-squared of the regression is 34%. In model 2, we substitute the difference in pay between the firm at the target pay percentile in the <u>actual</u> peer group and the sample firm for the pay difference measured relative to the naïve peer group. The adjusted *R*-squared of the regression increases to 36% and the coefficient estimate on the actual peer group distance measure indicates that a manager with pay 1% below the pay target receives an increase in pay that is approximately 0.31% larger compared to a manager

[24]We lose ten observations because there are no potential peer firms in the same industry with revenues between 50% and 200% of those of the sample firm.

with pay equal to the pay target. In model 3, we include both the naïve and actual peer group distance measures. In this model, the actual peer group measure drives out most of the explanatory power of the naïve peer group measure. Models 4 and 5 report similar results for subsamples of S&P 500 and non-S&P 500 firms, respectively. Interestingly, the coefficient estimate on the distance measure is smaller for non-S&P 500 firms, which are the firms with the largest biases in peer group compensation.

Overall, the results indicate that the use of peer group benchmarking is a significant determinant of CEO pay changes. To the extent that peer group comparison pay is biased upward, managers will receive higher raises than they would otherwise. Nevertheless, the results suggest that boards exercise discretion and do not fully adjust compensation for differences in relative pay. On average, the annual increase in compensation closes about one-third of the difference in pay between the CEO and the peer group. The fact that the upward bias in peer group pay is most evident in non-S&P 500 firms but that the adjustment coefficient is smaller in these firms is consistent with the idea that boards act to mitigate the effects of peer group biases and suggests that the overall benefit to these CEOs of inflating peer group pay is relatively small.

2.7 Changes in peer group composition over time

Lastly, we examine how the composition of peer groups has changed between 2006 and 2007. To explore this issue, we collect data on peer group composition for firms in our sample in fiscal year 2007 (2008 proxy year). The data are available for 651 firms in our original sample. This analysis provides some insight into whether the new SEC regulation requiring firms to report their peer groups has had any effect on the selection of peers. If the new reporting requirements invite additional scrutiny regarding peer selection, we expect that when firms change the composition of their peer group, they will be more likely to pick new firms in the following year (2007) in a manner that reduces any size or compensation bias in peer selection from the previous year (2006).

Table 10 provides evidence on the characteristics of the peer firms in 2006 and 2007. We break up the sample into quartiles based on the amount of similarity in peers in 2006 and those chosen in 2007. Panel A of Table 10 shows that overall, 81% of the peer firms remain constant between 2006 and 2007. There is substantial variation, however, in changes in peer group composition across firms. For the firms in the quartile with the most changes in the composition of the peer group, approximately 49% of the peer firms are either new or additional peer firms in 2007.

Panels B and C compare the change in peer group biases across years between firms with the largest changes in peer group composition (the bottom quartile in Panel A) and the remaining firms. The reported results focus on medians, but inferences based on means are similar. In Panel B we compare biases in firm size, as measured by the difference in the log of sales revenue, between the median peer firm and the sample firm, and in Panel C, we compare the bias in total pay measured as the difference in the log of total compensation between the peer group median and the sample firm.

Although none of the differences are statistically significant, two patterns in the data are notable. First, firms with the largest amount of peer group change exhibit larger biases in both firm size and pay in 2006 compared to firms with few peer group changes. For example, the bias in size (pay) is 0.158 (0.141) in firms with large peer changes compared to 0.114 (0.041) in other firms. Second, firms with the most peer group change exhibit larger reductions in bias between 2006 and 2007. Firms with the most peer group change reduce their size bias by about 2.4%, while firms with the least change in peer group composition reduce the size bias by 0.1%. The reduction in pay bias is 4.3% for firms with large changes in peer group composition compared to an increase in pay bias of 0.6% for firms with small changes.[25]

In general, the results are consistent with the view that the increased disclosure required by the new regulation has had some effect on the incentive of firms to opportunistically pick peer firms for compensation comparisons. Firms making large changes to their peer groups in 2007 appear to do so in a manner that reduces the biases in size and compensation that were evident in 2006 (the first year in which the new regulations became effective). An alternative hypothesis, however, is that the set of firms making large changes to their peer groups are those that have experienced recent changes in firm characteristics that have reduced comparability with the existing peer group. In unreported results we find no evidence that this is the case. Nevertheless, additional data will be needed to draw more definitive conclusions.

2.8 Conclusion

The level of pay at the compensation peer group is one of the important inputs for determining executive pay. Critics contend that powerful CEOs and co-opted boards opportunistically choose the benchmark peer group in a manner that inflates CEO pay. Moreover, given the prevalence of competitive benchmarking, they argue that this practice has led to an upward ratcheting of CEO pay over time. Alternatively, there are

[25] We also examined whether firms systematically change their target pay levels. We do not find evidence that firms have systematically reduced the target percentiles they use for benchmarking pay.

sound economic reasons for using peer group benchmarking. Firms must compete in the labor market for managerial talent and it is hard to imagine how to set overall compensation without reference to supply and demand conditions in the managerial labor market. Consequently, understanding how peer groups are formed is critical to understanding compensation practices.

We find that, on average peer, firms are chosen largely based on economic factors that reflect the managerial labor market in which the firms compete. Compensation peer groups contain firms that are in the same industry, are similar in size and scope, and that reflect other commonalities related to labor market factors. Nevertheless, we find that firms appear to exercise significant discretion in choosing peer firms. We show that when firms deviate from the economic model of peer firm choice, they tend to pick larger firms and firms with higher CEO pay. These biases in peer group selection are more evident in smaller, less visible firms where arguably management has more discretion in selecting the peer group. Interestingly, we find little consistent evidence that the peer group biases we show are systematically related to corporate governance. Despite the evidence of peer group biases that we find, boards appear to only partially adjust pay in response to differences in compensation between the comparison group and the CEO, suggesting that boards exercise discretion that mitigates the effects of peer group bias on pay increases. Finally, we provide some evidence suggesting that the increased disclosure promulgated by the SEC regulation has reduced the biases in peer group choice over time.

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Table 1
Variable definitions.

All the numbers in parentheses refer to the annual Compustat item number. Other data sources are given in the variable definition.

Variable names	Variable definition and data sources
Firm/peer characteristics:	
Sales revenue	Sales (12)
Log of sales revenue	Log(Sales revenue)
Total assets	Total assets (6)
ROA (%)	Return on assets = 100*Operating income after depreciation (178)/ book assets (6)
Market-to-book	Market-to-book (MTB) = [market equity + total debt + preferred stock liquidating value (10) – deferred taxes and investment tax credits (35)]/ book assets (6)
	where Market equity = Stock price (199) * Shares outstanding (25)
	Total debt = Long term debt (9) + Short term debt (34)
Salary and bonus	Salary + Bonus, ExecuComp
Total compensation	Total compensation = (Salary + Bonus + Other annual + Restricted stock grants + LTIP payouts + All other + Value of options granted for 2005 fiscal year; and Salary + Bonus+ Non-equity incentive plan compensation + Value of options granted + Grant-date fair value of stock awards + Deferred compensation earnings reported as compensation + Other compensation for 2006 fiscal year), ExecuComp
CEO ownership (%)	CEO ownership = 100*(shrown_tot/1000)/shrsout, ExecuComp
Board size	Number of directors, IRRC Directors
Fraction of the board that is independent	Fraction of board that is independent directors, IRRC Directors
CEO tenure	CEO tenure, ExecuComp
GIM index	Gompers-Ishii-Metrick (2003) index, IRRC Governance
Fraction of board hired during CEO’s tenure	Fraction of board hired after the CEO comes to office, ExecuComp

Table 1 (continued)

Firm/peer characteristics (continued):	
Total shareholder return	One-year total return to shareholders in percentage (dividends reinvested), ExecuComp
Stock price volatility	Volatility (60 month) used to calculate Black-Scholes values, ExecuComp

Peer group variables:	
Fraction of peers in the same Fama-French industry	Fama and French (1997) 49-industry classification
Fraction of peers in the industry-size group	A peer is considered in the sample firm's industry-size group if the peer is in the sample firm's industry and has sales between 0.5 to 2.0 times that of the sample firm's sales
Change in log(Total compensation)	Log(Total compensation 2006) – Log(Total compensation 2005)
Difference in log compensation between *Naïve* peer group and the firm	Log(Naïve peer group median compensation 2005) – Log(Firm compensation 2005)
Difference in log compensation between *Actual* peer group and the firm	Log(Actual peer group compensation target 2005) – Log(Firm compensation 2005)

Firm-peer variables:	
Correlation of firm's industry return and potential peer's industry return	Correlations were calculated using 2004–2005 industry daily return. Data are from Ken French's website.
Customer/Supplier relation	Data from Bureau of Economic Analysis, 2002 Benchmark Input Output account, 'USE' table.
Executive transfers	Turnover data from ExecuComp, 2001–2005.
Credit market characteristics	Firm credit rating is determined based on Compustat data 280 'S&P LT Domestic Issuer Credit Rating.' Credit rating has four possible values: 'investment grade' if data280 in [2,12], 'junk' if data280 in [13,23], 'default' if data280 in [27,29], and 'unrated' if data280 is missing.
Product diversification and Market diversification	Data from Compustat segment dataset.

Table 2
Summary statistics.

Summary statistics of i) firms reporting peer groups and firms that did not report peer groups and ii) firms that report peers where we have sales and compensation data on all peers and firms that report peers where we do not have either sales or compensation data for all peers. Out of 1,333 ExecuComp 2006 fiscal year firms that reported under the new SEC rule (roughly, firms that have fiscal-year end from December 2006 to May 2007), 1,178 have annual proxy statements available. 808 firms reported peer groups and 370 did not. The requirements that sample firms have non-zero CEO compensation in both 2005 and 2006 fiscal years reduce the sample to 798 reporting firms and 357 non-reporting firms. Out of 798 firms, 707 have all peers with non-missing compensation and sales revenue; and 91 have some peers with missing compensation or sales revenue. The 707 reporting firms together report 11,570 peers (2,630 distinct peers). The table reports data for 2005 fiscal year. The union of the 707 sample firms and their 2,630 chosen peers has 2,678 firms. ROA and market-to-book are winsorized at the 1^{st} and 99^{th} percentiles of the distribution of the sample union. Variable definitions are provided in table 1. ***, **, And * indicate significance at 1%, 5%, and 10% confidence levels using the Wilcoxon rank-sum Z-test.

	Comparison of firms that report peers with firms not reporting		Comparison of firms with full data on all peers with firms without full data on all peers	
	Firms reporting peers *N=707*	Firm not reporting peers *N=357*	Firms with full peer data *N=707*	Firms without full peer data *N=91*
	Median	Median	Median	Median
Financial characteristics				
Sales ($ million)	2028	1006***	2028	2559
Total assets ($ million)	3080	1269***	3080	3589
ROA (%)	8.08	8.01	8.08	8.36
Compensation				
Salary & bonus ($ 000s)	1538	1009***	1538	1809**
Salary & bonus/Sales	0.76	1.04***	0.76	0.84
Total compensation ($ 000s)	3880	2326***	3880	4324**
Total compensation/Sales	1.85	2.15***	1.85	1.81
Pay mix (salary & bonus/total compensation)	0.42	0.49***	0.42	0.41
Governance characteristics				
CEO ownership (%)	0.91	1.25***	0.91	0.97
Board size	9	9***, [a]	9	9
Fraction of the board that is independent	0.75	0.70***	0.75	0.77
CEO tenure	5.24	6.16*	5.24	5.98
GIM index	9	9*, [b]	9	9
Fraction of the board hired during the CEO's tenure	0.50	0.45	0.50	0.50

[a]: Firms reporting peer groups have larger rank-sum value. The means of the two groups are 9.449 and 8.914, and significant at 1%.
[b]: Firms reporting peer groups have larger rank-sum value. The means of the two groups are 9.447 and 9.057, and significant at 5%.

Table 3
Statistics on the size and composition of peer groups.

This table present evidence on peer group size and 45omposition. The sample covers ExecuComp 2006 fiscal year firms that report under the new SEC rule (roughly, firms that have fiscal-year end in December 2006 and later) and that report the use of peer groups in determining executive compensation and have all peers with non-missing compensation and sales. Peer group data were hand-collected from corporate proxy statements. In identifying industry, we use the Fama-French 49-industry classification. A peer is considered in the sample firm's industry-size group if the peer is in the sample firm's industry and has sales between 0.5 to 2.0 times that of the sample firm's sales. The table reports means with medians reported in parentheses.

	Number of observations	Number of firms in peer group	Fraction of peers in the same industry	Fraction of peers in the same industry-size group
All firms in the sample	707	16.4 (15)	0.63 (0.73)	0.37 (0.33)
S&P 500 firms	259	17.3 (15)	0.58 (0.56)	0.32 (0.29)
Non-S&P 500 firms	448	15.8 (14)	0.66 (0.80)	0.39 (0.40)

Table 4
Logit analysis.

Logit regressions of the factors that determine the characteristics of the firms that are contained in the compensation peer group. The dependent variable is one if a potential peer is chosen as a peer by the sample firm and zero otherwise. The sample for the analysis consists of 707 firms with complete data on all peers. The set of potential peers includes the union of all sample firms and their chosen peers.[a] The marginal effect is defined as the partial derivative of the event probability.[b] *p*-Values are reported in parentheses and marginal effects are reported in brackets.

		Dependent variable is one if a potential peer is chosen as a peer by the sample firm and zero otherwise	
		(1)	(2)
	Intercept	-7.579	-7.423
		(0.000)	(0.000)
	Compensation measure:		
α_1	Log peer total pay – Log firm total pay		-0.120
	when Firm total pay < Peer total pay, = 0		(0.000)
	otherwise.		[-0.024]
α_2	Log peer total pay – Log firm total pay		0.285
	when Firm total pay > Peer total pay, = 0		(0.000)
	otherwise.		[0.056]
	Industry variables:		
α_3	Dummy equal to one if both firm and peer	2.140	2.142
	are in the same Fama-French industry	(0.000)	(0.000)
		[0.422]	[0.424]
α_4	Correlation of firm's industry return and	3.833	3.851
	potential peer's industry return	(0.000)	(0.000)
		[0.757]	[0.762]
	Sales and performance measures:		
α_5	Log peer sales – Log firm sales when Firm	-0.891	-0.889
	sales < Peer sales, = 0 otherwise.	(0.000)	(0.000)
		[-0.176]	[-0.176]
α_6	Log peer sales – Log firm sales when Firm	1.708	1.652
	sales > Peer sales, = 0 otherwise.	(0.000)	(0.000)
		[0.337]	[0.327]
α_7	Peer ROA – Firm ROA when Firm ROA <	-0.020	-0.021
	Peer ROA, = 0 otherwise.	(0.000)	(0.000)
		[-0.004]	[-0.004]
α_8	Peer ROA – Firm ROA when Firm ROA >	0.023	0.021
	Peer ROA, = 0 otherwise.	(0.000)	(0.000)
		[0.004]	[0.004]
α_9	Peer MTB – Firm MTB when Firm MTB	-0.050	-0.035
	< Peer MTB = 0, otherwise.	(0.002)	(0.032)
		[-0.010]	[-0.007]
α_{10}	Peer MTB – Firm MTB when Firm MTB	0.700	0.705
	> Peer MTB = 0, otherwise.	(0.000)	(0.000)
		[0.138]	[0.140]

Table 4 (continued)

		Dependent variable is one if a potential peer is chosen as a peer by the sample firm and zero otherwise	
		(1)	(2)
	Customer or supplier relation:		
α_{11}	Fraction of output (in dollars) that firm's industry sells to potential peer's industry	0.114 (0.394) [0.023]	0.110 (0.414) [0.022]
α_{12}	Fraction of input (in dollars) that firm's industry buys from potential peer's industry	0.109 (0.444) [0.021]	0.057 (0.692) [0.011]
	Executive transfers:		
α_{13}	Fraction of external hires for CEO positions over the last 5 years that firm's industry made from potential peer's industry.	0.967 (0.000) [0.191]	0.964 (0.000) [0.191]
α_{14}	Fraction of executive talent loss that potential peer's industry hired from firm's industry for CEO positions over last 5 years.	0.721 (0.000) [0.142]	0.736 (0.000) [0.146]
	S&P 500 firms:		
α_{15}	Dummy equal to one if both firm and peer are S&P 500 firms	1.255 (0.000) [0.248]	1.185 (0.000) [0.235]
α_{16}	Dummy equal to one if both firm and peer are not S&P 500 firms	-0.341 (0.000) [-0.067]	-0.321 (0.000) [-0.064]
	Credit market characteristics:		
α_{17}	Dummy equal to one if both firm and peer have the same credit rating	0.317 (0.000) [0.063]	0.313 (0.000) [0.062]
	Product and market diversification:		
α_{18}	Dummy equal to one if both firm and peer have multiple business segments	0.307 (0.000) [0.061]	0.300 (0.000) [0.059]
α_{19}	Dummy equal to one if both firm and peer have only one business segment	0.173 (0.000) [0.034]	0.175 (0.000) [0.035]
α_{20}	Dummy equal to one if both firm and peer are geographically diversified	0.665 (0.000) [0.131]	0.666 (0.000) [0.132]
α_{21}	Dummy equal to one if both firm and peer have only one geographical location	0.176 (0.000) [0.035]	0.180 (0.000) [0.036]
	Fixed effects	Yes	Yes
	Number of observations	707*2677	707*2677
	Number of event occurs	11570	11570
	McFadden's pseudo R^2	0.395	0.397

[a] Each sample firm is excluded from its own set of potential peers.

[b] Marginal effects are computed at the following values of covariates: dummies 3, 15, 17, 18, and 20 set to one; dummies 16, 19, and 21 set to zero; all distance variables (1–2, 5–10) set to zero; the remaining variables (4, 11–14) set at the respective means.

Table 5
Peer group pay target percentiles

Summary statistics of peer group pay target percentiles along with a logit regression of firm characteristics associated with pay targets above the median. Panels A and B report summary statistics of the target percentiles. Panel C reports results from the logit regression. Target percentiles are hand-collected from proxy statements. When a target range is specified, the middle of the range is used. For 229 firms that do not explicitly specify targets, the median is used. For the logit regression, the dependent variable is one if the pay target is above the median and zero otherwise. The independent variables include the log of sales revenue in the 2005 fiscal year, a dummy equal to one for S&P 500 firms, a dummy equal to one if the firm reports more than one business segment, and a dummy equal to one if the firm reports more than one geographical location. For the logit regressions, the sample consists of 707 firms that reported peer groups in their 2006 proxy statement where we have full data on sales and compensation for all peer firms. ***, **, And * represent differences at the 1%, 5%, and 10% levels. *T*-Test is used. For the logistic regressions, *p*-values are reported in parentheses and marginal effects in brackets.[a]

Panel A: Number of firms with target at median or above

	Number of observations	Firms where target is assumed to be median	Firms specifying a range	Firms specifying a precise target	Firms specifying a target equal to median
S&P 500	259	64	34	161	110
Non-S&P 500	448	165	69	214	158

Panel B: Target percentiles

	Number of observations	Mean	Fraction of firms with target higher than median	Fraction of firms with target lower than median	Min	Max
S&P 500	259	0.562***, [b]	0.320	0.000	0.5	0.9
Non-S&P 500	448	0.547***, [b]	0.268	0.004	0.375	0.9
p-Value for difference between S&P 500 and non-S&P 500 firms		(0.040)	(0.143)	(0.158)		

Panel C: Logit regression

	Intercept	Log sales revenue 2005	Dummy = 1 for S&P 500 firms	Dummy = 1 for Multiple business segments	Dummy = 1 for Multiple geographic segments	Pseudo R^2
	-1.057	0.011	0.236	-0.136	0.100	0.003
	(0.041)	(0.879)	(0.303)	(0.451)	(0.561)	
		[0.002]	[0.048]	[-0.028]	[0.020]	

[a] Marginal effects are calculated as the partial derivatives of the event probability at the covariate means.
[b] Means are compared to 0.5.

Table 6

Summary statistics on peer firms.

Summary statistics for firm size, performance, and compensation for firms and their reported compensation peers. Panel A (B) reports medians across S&P 500 (non-S&P 500) sample firms. Firm size is measured by sales revenue and firm performance is ROA. Compensation includes all forms of pay such as salary and bonus, options, restricted stock awards, etc. The Wilcoxon signed rank test is used in testing the differences in column 3. ***, **, And * indicate significance at 1%, 5%, and 10% confidence levels. Data are for 2005 fiscal year and consist of 707 firms that reported peer groups in their 2006 proxy statement where we have full data on sales and compensation for all peer firms.

Panel A: S&P 500 (259 firms)

	Sample firm (1)	Peer group median (2)	Peer group median minus sample firm (3)
Sales (log)	8.977	9.000	-0.016
Sales ($ millions)	7919	8104	-92
ROA (%)	9.216	10.366	0.121
Total compensation (log)	8.955	8.976	-0.035
Total compensation ($ 000s)	7748	7914	-161**
Pay mix (salary & bonus/total compensation)	0.344	0.345	0.010

Panel B: Non-S&P 500 (448 firms)

	Sample firm (1)	Peer group median (2)	Peer group median minus sample firm (3)
Sales (log)	6.907	7.235	0.221***
Sales ($ millions)	999	1388	172***
ROA (%)	7.774	9.016	0.164
Total compensation (log)	7.836	8.043	0.153***
Total compensation ($ 000s)	2530	3113	365***
Pay mix (salary & bonus/total compensation)	0.497	0.449	-0.044***

Table 7

Propensity score matching peers and real peer group comparison.

Comparison of characteristics between the real peer group target (median) peers and the propensity score matched (PSM) peer group target (median) peers. The coefficient estimates from the logit regression specification 1 in Table 3 are used to estimate the predicted probability (propensity score) for every potential peer. For each real peer group, a PSM peer group is formed by selecting potential peers that have the closest propensity score to the individual peers in the real peer group. Matching is done without replacement. Target peer(s) are defined for each peer group based on total compensation and the target pay percentile specified by the sample firms.[a] Medians across sample firms are reported. For test statistics the Wilcoxon signed rank test is used. ***, **, And * represent differences at the 1%, 5%, and 10% levels, respectively.

	S&P 500		Non-S&P 500	
	Actual median peers minus PSM median peers	Actual target peers minus PSM target peers	Actual median peers minus PSM median peers	Actual target peers minus PSM target peers
Sales (log)	0.079	0.079**	0.000	0.005
Sales ($ millions)	591***	529***	0	2
ROA (%)	0.000	0.000	0.000	0.000
Total compensation (log)	0.080***	0.059***	0.007	0.004
Total compensation ($ 000s)	536***	414***	24*	15

[a] Depending on peer group size and target percentile specified, some peer groups have two target(median) peers. In this case, the mean of the two is used.

Table 8

Corporate governance and peer group selection.

The effect of corporate governance on the peer group target pay percentile, size bias, and compensation bias. Panel A reports the results for S&P 500 firms and Panel B for non-S&P 500 firms. The size bias is the difference in log sales between the peer group median and the sample firm. Compensation bias is the difference in log total compensation between the target peer (median peer) and the propensity score matched target (median) peer. Three governance attributes are analyzed: CEO tenure, the fraction of the board hired after the CEO, and the GIM index. The sample consists of 707 firms that reported peer groups in their 2006 proxy statement where we have full data on sales and compensation for all peer firms. Values for all the governance variables are partitioned above and below median values within the sample of 707 firms reporting peers. Median values are then reported for each subsample. The Wilcoxon rank-sum test is used to compare the two subsamples. ***, **, And * represent differences at the 1%, 5%, and 10% levels, respectively.

Panel A: S&P 500

	CEO tenure		Fraction of board hired after CEO		GIM index	
	Above median 129 observations	Below median 127 observations	Above median 124 observations	Below median 125 observations	Above median 120 observations	Below median 133 observations
Fraction of firms with target higher than median	0.333	0.315	0.371	0.272*	0.292	0.346
Log of peer group median size minus log firm size	0.025	-0.032	-0.021	-0.012	0.001	-0.032
Log of real target peer compensation minus log of PSM target peer compensation	0.069	0.040	0.085	0.023	0.024	0.081
Log of real median peer compensation minus log of PSM median peer compensation	0.080	0.081	0.126	0.031	0.043	0.098

Panel B: Non-S&P 500

	CEO tenure		Fraction of board hired after CEO		GIM index	
	Above median 213 observations	Below median 213 observations	Above median 184 observations	Below median 167 observations	Above median 173 observations	Below median 211 observations
Fraction of firms with target higher than median	0.319	0.221**	0.283	0.246	0.220	0.318**
Log of peer group median size minus log firm size	0.203	0.221	0.276	0.191*	0.239	0.192
Log of real target peer compensation minus log of PSM target peer compensation	0.003	0	0.028	-0.009	0.049	0
Log of real median peer compensation minus log of PSM median peer compensation	0	0.005	0.026	0	0.039	0

Table 9

Peer groups and pay changes.

The effect of peer group compensation on changes in pay. The dependent variable in all specifications is the change in the log of total compensation from 2005 to 2006. Independent variables include a measure of the distance in the firm's pay from peer group benchmarks. Two different benchmarks—the naïve and the actual—are used. The naïve benchmark is the median pay of a peer group that consists of all firms in the same Fama-French industry that have sales revenue between 0.5 and 2.0 times that of the sample firm. All firms that have compensation available—ExecuComp firms and hand-collected firms—are used in construction of the naïve peer groups. The actual benchmark is constructed as target pay percentile of the actual peer group reported in the proxy statement. Other independent variables include the lagged log of sales revenue, change in the log of sales revenue, ROA, prior year ROA, stock return, prior year stock return, stock return volatility, and lagged log total compensation. *P*-Values are reported in parentheses.

	Change in log(Total compensation)				
	Whole sample	Whole sample	Whole sample	S&P 500	Non-S&P 500
	(1)	(2)	(3)	(4)	(5)
Intercept	1.474 (0.000)	1.184 (0.000)	0.874 (0.008)	1.549 (0.018)	1.600 (0.000)
Difference in log compensation between *Naïve* peer group and the firm	0.225 (0.000)		0.099 (0.095)		
Difference in log compensation between *Actual* peer group and the firm		0.306 (0.000)	0.279 (0.000)	0.344 (0.000)	0.267 (0.000)
Log of sales revenue$_{2005}$	0.154 (0.000)	0.155 (0.000)	0.122 (0.000)	0.109 (0.017)	0.144 (0.000)
Change in log sales revenue	0.439 (0.006)	0.346 (0.025)	0.333 (0.030)	0.957 (0.004)	0.062 (0.690)
ROA$_{2006}$	0.004 (0.585)	0.004 (0.566)	0.006 (0.398)	-0.013 (0.161)	0.015 (0.077)
ROA$_{2005}$	-0.005 (0.516)	-0.007 (0.332)	-0.007 (0.278)	0.015 (0.180)	-0.018 (0.015)
Total shareholder return$_{2006}$	0.002 (0.114)	0.002 (0.071)	0.002 (0.077)	0.003 (0.108)	0.002 (0.155)
Total shareholder return$_{2005}$	0.001 (0.158)	0.001 (0.141)	0.001 (0.188)	-0.001 (0.674)	0.002 (0.046)
Stock price volatility	-0.050 (0.494)	-0.083 (0.279)	-0.081 (0.285)	-0.221 (0.489)	-0.036 (0.643)
Log of total compensation$_{2005}$	-0.317 (0.000)	-0.290 (0.000)	-0.220 (0.000)	-0.284 (0.005)	-0.334 (0.000)
Adjusted *R*-squared	0.336	0.360	0.362	0.337	0.406
Number of observations	697	707	697	259	448

Table 10

Changes in peer group characteristics.

Analysis of changes in peer group characteristics between 2006 and 2007. Firms are sorted into quartiles based on the frequency of changes in the composition of the peer group between years. Panel A reports the fraction of peer groups that remain the same between 2006 and 2007, broken down by quartiles. The first quartile represents the group with the greatest change in peer group composition between years while the fourth quartile represents the group with the least change. In Panels B and C, Group 1 represents the quartile with the greatest number of replacements or additions to the peer group between years. Group 2 represents the remaining three quartiles. Panel B (C) reports the differences and changes in differences of the peer group median sales revenue (total compensation) and that of the sample firm. Medians are reported in Panels B and C. ***, **, And * represent differences at the 1%, 5%, and 10% levels (Wilcoxon signed rank test is used). *p*-Values are in parentheses (Wilcoxon two-sample test comparing two group medians).

Panel A: Fraction of the peer group that remains the same from 2006 to 2007

Mean fraction of similar peers between 06 and 07				
0 – 100 Percentile	0 – 25 Percentile	25 – 50 Percentile	50 – 75 Percentile	75 – 100 Percentile
0.810 (651 observations)	0.511	0.814	0.923	0.998

Panel B: Median of differences and change in differences between peer group median sales revenue and sample firm sales revenue (log)

Group	Obs	Median of sales difference in 2006	Median of sales difference in 2007	Median of change in sales differences
1	165	0.158***	0.140***	-0.024
2	486	0.114***	0.122***	-0.001
p-Value		(0.159)	(0.792)	(0.270)

Panel C: Median of differences and change in differences between peer group median total compensation and sample firm total compensation (log)

Group	Obs	Median of compensation difference in 06	Median of compensation difference in 07	Median of change in compensation differences
	165	0.141**	0.131*	-0.043
	486	0.041*	0.026*	0.006
p-Value		(0.264)	(0.407)	(0.536)

CHAPTER 3

DIVIDEND YIELDS AND STOCK RETURNS: EVIDENCE FROM AN ECONOMY WITHOUT TAXES

3.1 Introduction

A long standing question in financial economics is whether investor level taxes are reflected in asset prices. One view is that the tax payments on dividends and capital gains are capitalized into stock prices resulting in a negative relationship between equity valuations and the tax burden imposed on investors. Under this view an increase in the tax burden lowers equity valuations resulting in higher before-tax expected returns that compensate investors for the additional taxes paid on their holdings. A competing view, however, is that the effect of taxes on equity prices is negligible. Miller and Scholes (1978) argue that investment taxes can largely be avoided in perfect capital markets, thus making the marginal investor tax exempt. Alternatively, the effect of taxes on equity prices and returns may also be small if investors sort themselves into clienteles such that tax exempt agents hold high tax securities and taxable agents hold low tax securities.

Recently, Sialm (2009) provides both time-series and cross-sectional evidence consistent with the tax capitalization hypothesis using data on the variation in the tax burden on U.S. equity securities. Indeed, Sialm's estimates suggest that taxes are completely capitalized into asset prices in the sense that a 1% increase in the tax burden is associated with approximately a 1% increase in the before-tax rate of return. There are, however, reasons to question these findings. A large fraction of the variation in the tax burden variable used by Sialm comes from variation in the dividend yield, and prior studies on U.S. data (e.g., Naranjo, Nimalendren, and Ryngaert, 1998) find that dividend yields are positively related to returns in a manner similar to the tax burden. More importantly, Naranjo et al. (1998) argue that the magnitude of the effect of the dividend yield on returns is too large to be consistent with tax effects, and instead suggest that the relationship between dividend yields and returns is likely the result of omitted risk factors or other characteristics related to stock returns. The fact that dividend yields (which are a key input to the tax measure in Sialm) are highly correlated with the tax burden makes it difficult to disentangle tax effects from other possible explanations for the correlation between returns and the tax burden variable.

In this paper we propose a different approach. The Hong Kong Special Administrative Region does not levy tax on either dividend income or capital gains and thus provides a unique economic setting in which to examine the tax capitalization hypothesis. We document a robust positive relation between the dividend yield and stock returns in the Hong Kong market that is very similar in magnitude to the effect documented by Sialm in the U.S. market. In cross-sectional tests, the coefficient estimates associated with the effect of the dividend yield on stock returns are 1.266 and 1.262 when returns are risk-adjusted using CAPM and Fama-French three-factor model, respectively.[26] These estimates indicate that a one percent difference in dividend yields is associated with slightly higher than one percent difference in risk-adjusted return. In addition to the cross-sectional results, we also provide time-series evidence. In the Hong

[26] When stocks are classified into 11 dividend yield portfolios.

Kong market, there is a robust negative relation between the aggregate dividend yield and aggregate equity valuations. In other words, during times when the dividend yield is high (low), aggregate equity values tend to be low (high). Similar to the findings from the cross-sectional tests, the effect of the dividend yield on aggregate valuations in Hong Kong is comparable to that of the effect of the tax yield in the U.S. documented in Sialm (2009). In the case of Hong Kong, however, the relationship between dividend yields and returns (valuations) cannot be due to tax effects as there are no investor level taxes on dividends or capital gains.

Consistent with the views expressed in Naranjo et al. (1998), our paper suggests that there are nontax reasons that cause the relationship between dividend yields and returns. Our findings do not necessarily invalidate the Brennan (1970) after-tax CAPM, nor do they completely rule out taxes as one of the drivers of the dividend yield effect. Our contribution is to illustrate the difficulty of conducting a powerful test of the tax capitalization hypothesis in practice and to urge caution in interpreting the dividend yield effect as evidence in support of the tax capitalization hypothesis.

The remainder of the paper is organized as follows. Section 3.2 briefly reviews the related literature. Section 3.3 discusses important features of Hong Kong taxation for the issue being studied. Section 3.4 presents cross-sectional evidence. Section 3.5 presents time-series evidence and section 3.6 concludes the paper.

3.2 Literature review

There is an extensive literature examining the relationship between taxes and equity prices.[27] In the U.S., dividend income has typically been taxed at a higher rate than capital gains income. Based on this observation, Brennan (1970) derives an after tax version of the capital asset pricing model in which the risk adjusted returns on stocks are positively related to the stock's dividend yield. In Brennan's model, the extra return earned on stocks with high dividend yields offset the increased taxes that investors must pay to hold these assets. Black and Scholes (1974) test the Brennan model using data on stocks listed on the New York Stock Exchange between 1926 and 1966. They proxy for the expected dividend yield in the current year, they use the ratio between dividends paid during the prior year and the stock price at the end of the previous year. Black and Scholes (1974) find no relationship between their measure of the expected dividend yield and risk-adjusted stock returns, which is inconsistent with the Brennan model. Miller and Scholes (1978) argue that one should not expect to find a large effect of taxes on asset prices, because the marginal investor is likely to be tax exempt. In contrast to this view, Litzenberger and Ramaswamy (1979) employ a different measure of the dividend yield and find strong evidence in favor of the tax hypothesis. Litzenberger and Ramaswamy classify dividend paying stocks as having a positive dividend yield only during months in which the stock goes ex-dividend. In the ex-dividend months the expected dividend yield is calculated as D_t/P_{t-1} (the announced dividend divided to the price at the beginning of the month). In months other than ex-dividend months, stocks are classified as having zero dividend yield. Using this measure of the dividend yield, they find a positive relationship

[27]Detailed reviews of the literature are given in Allen and Michaely (2003) and Kalay and Lemmon (2008).

between dividend yields and risk adjusted returns that they interpret as being consistent with tax effects.

Eades, Hess, and Kim (1994) and Kalay and Michaely (2000) provide a reconciliation of the conflicting results in Black and Scholes (1974) and Litzenberger and Ramaswamy (1979) results. Both set of authors find that stocks that pay dividends exhibit higher returns during the ex-dividend month, but do not have higher returns in other months. More importantly, the high returns in ex-dividend months are unrelated to the magnitude of the dividend yield. As a result of using a short-term dividend yield definition, the experiment in Litzenberger and Ramaswamy (1979) uncovers time-series variation in returns rather than cross-sectional return variation as a function of the dividend yield. This pattern in returns is not in favor of the tax hypothesis.

More recently, Naranjo, Nimalendran and Ryngaert (1998) use a measure of the long-term dividend yield in a carefully selected sample of NYSE stocks. Naranjo et al. (1998) find a significant positive relationship between dividend yields and returns. As stated in their paper, however, the relation is difficult to be explained by the tax hypothesis. First, the coefficient estimates on the dividend yield variables are too large to be consistent with tax effects. Second, the relationship between dividend yields and returns is not present in large firms. Finally, they find no evidence that the magnitude of the effect varies across different tax regimes.

In contrast, Sialm (2009) directly computes a measure of the total tax burden imposed on investors from both capital gains and dividend income. Using a long sample period, Sialm provides both time-series and cross-sectional evidence of a robust negative relation between his measure of the effective tax yield and equity prices in the U.S.

market. Based on this evidence Sialm concludes in favor of the tax capitalization hypothesis.

Overall, the evidence of whether taxes affect asset prices is mixed. It is difficult to construct a definitive test of the tax hypothesis because of the possibility that the dividend yield might proxy for omitted risk factors or other characteristics related to returns that are unrelated to tax effects. Sialm attempts to measure the tax burden on securities directly. Interpreting the results remains difficult however, as one of the primary inputs to the computation of the tax burden is the dividend yield. We provide new evidence on whether taxes affect equity valuations using data from Hong Kong, an economy where there is no tax disadvantage of dividend income relative to capital gains. If taxes are the sole driver of the dividend yield effect then there should be no relation between dividend yields and returns in Hong Kong data. The most closely related paper to ours is Lim (1996) who uses Hong Kong data from October 1983 to December 1991 to investigate the tax hypothesis. The coefficient estimate on the dividend yield variable is not significantly different from zero, which supports the tax hypothesis. Lim's results are difficult to interpret however. First, the sample period is very short, which limits the power of the test. Second, Lim includes in the sample only stocks that go ex-dividend in any particular month and make dividend announcement in the prior month, and thus one can only interpret Lim's result as documenting that there is no relation between dividend yields and returns during ex-dividend months. Moreover, the short-term dividend yield definition employed by Lim is inconsistent with the long-term measures of the dividend yield that are used in most other recent studies. Finally, it is expensive to buy and sell stocks in Hong Kong market. Investors have to pay stamp duty of 0.15% of the stock price on each stock transaction (Frank and Jagannathan, 1998). This suggests that investors in Hong Kong stocks care about long-term returns, and that to test the tax hypothesis one needs to investigate returns across longer holding periods, not just the ex-dividend month.

3.3 Taxation in Hong Kong

Hong Kong has one of the simplest tax regimes among the developed economies. It adopts a territorial-source principle of taxation; i.e. only Hong Kong sourced incomes are taxed. Furthermore, only specified types of incomes are taxed – namely, profits, salaries and properties: profits from trade or business are subject to a profits tax; income from employment is subject to a salaries tax; and income from property is subject to a property tax.[28]

Profits arising from the sale of capital assets are excluded from assessable profits. In addition, dividends received from a corporation that is subject to the Hong Kong Profits Tax are tax-exempt. Thus, investors who invest in Hong Kong common stocks face neither taxes on capital gain nor taxes on dividend income. The works of Frank and Jagannathan (1998) and Lim (1996) are among the prior research that exploits this unique feature of Hong Kong market.

Although there is no capital-gains tax, it should be noted that if financial assets are acquired for the purpose of short-term profit-taking then the gain on disposal of such assets is taxable. The fact that some investors might be taxed on stock price appreciation makes capital gains less tax-desirable than dividend income in Hong Kong. In this case,

[28] http://www.ird.gov.hk/eng/pdf/tax_guide_e.pdf (A brief guide to taxes administered by the inland revenue department) and http://www.ird.gov.hk/eng/pdf/e_dipn42.pdf (Departmental interpretation and practice notes, No. 42, profit tax, part A, taxation of financial instruments).

the tax capitalization hypothesis predicts that the relationship between dividend yields and stock returns would be negative, which is opposite to our finding. This strengthens our conclusion that there are omitted factors that drive the positive yield-return relationship.

3.4 Cross-sectional evidence

3.4.1 Data and summary statistics

Our sample consists of all Hong Kong stocks covered by DataStream.[29] Only the securities that have the type 'EQ' (equity) are included in the sample. The sample starts in January 1973, the first available month, and ends in December 2005. We download monthly data on prices, dividends, returns, volume, and shares outstanding.

Turnover is defined as number of shares traded divided by the number of share outstanding. Monthly turnover is the sum of the daily turnover of all trading days in the months. Firms that have one or more of the following are excluded: average monthly turnover less than 0.73% (approximately the 5th percentile), percentage of no-trade days greater than 50%, six or greater continuous months of no-trade. These screens are used to ensure that our results are not driven by small, illiquid stocks. Nevertheless, the results are similar when we don't exclude any firms or employ other reasonable screens to exclude illiquid stocks.

The stock return for a given month is calculated based on the Return Index. As defined by DataStream, the Return Index shows the growth in value of a stock assuming

[29] See Ince and Porter (2006) for short overview of DataStream. Ince and Porter (2006) show that Datastream constituent lists do not include all dead securities. Thus we use the DataStream Navigator Search interface. Our search criteria are: market is Hong Kong; Exchange is Hong Kong; price currency is Hong Kong dollar. There are 1238 securities that satisfy these criteria. If we do not require the last criteria, there are 1244 securities.

that dividends are reinvested in the same stock. Thus return during month t can be computed as ($RI_{i,t}/RI_{i,(t-1)} - 1$), where $RI_{i,t}$ and $RI_{i,(t-1)}$ are the levels of the Return Index at the end of month t and t-1. Returns are winzorized at 1st and 99th percentiles to ensure the results are not driven by outliers. We report results using returns in denominated U.S. dollars rather than returns in Hong Kong dollar. Excess returns are defined as the difference between realized returns and returns on 1-month US Treasury-bills.[30]

Datastream provides a measure of the dividend yield for each security. According to Datastream, the dividend yield is calculated as the ratio of anticipated amounts of dividends during the next twelve months to current stock prices. For the Hong Kong market Datastream computes the anticipated dividend amount as the amount of dividends paid during the prior twelve months excluding special and one-off dividends.[31] This measure of the dividend yield is similar to the definition of the long-term dividend yield used by Keim (1985) Naranjo, Nimalendren, and Ryngaert (1998). This definition of the dividend yield differs slightly from the dividend yield defined in Sialm (2009). The Datastream dividend yield is computed by scaling dividends by the current stock price, whereas the dividend yield used by Sialm scales by the one-year lagged stock price. Following Sialm, we adjust the Datastream dividend yield using the current and lagged one-year stock price level to create a measure of the dividend yield that is identical to that used by Sialm. We winsorize the adjusted dividend yield at 99th percentile.

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[30] Note that the returns in U.S. dollar and Hong Kong dollars are nearly identical because the Hong Kong dollar is pegged to the U.S. dollar. To convert monthly returns to U.S. dollars we use the ratio between the current and prior month exchange rates. If the exchange rate was completely fixed then the ratio would be equal to one for every month and returns in both currencies would be identical. Over the period from January 1973 to December 2005, this exchange rate ratio has mean of 0.9995 and standard deviation of 0.0126.

[31] We confirmed the methodology used to calculate the dividend yield for Hong Kong securities directly with Datastream client support.

A final issue that arises is that DataStream continues reporting the last valid data for inactive firms even after they become inactive. For example, if a company became inactive in November 2005 and the last valid data are for October 2005 then DataStream keeps reporting the October 2005 data for all months afterward. Following the recommendation of Ince and Porter (2006) we remove all the padded monthly zero-return series at the end of the sample. Using this procedure, it is possible that a few observations that have valid zero returns are unintentionally deleted, but any omissions appear minor. Our final sample consists of 837 firms with 109,316 firm-month observations.

Figure 1 and Table 11 display the number of stocks in the sample at end of June of each year. It can be seen that the number of firms has increased significantly over the sample period. The number of firms increases from 42 in June 1974 to 638 in June 2005. From 1988 to 1989, the number of firms almost doubled from 93 to 172. The numbers of stocks that have positive and zero dividend yields are also reported. In June 1974, only 1 out of 42 firms did not pay dividends. In June 2005, 236 out of 638 firms did not pay dividends. Figure 2 plots the aggregate dividend yield trend in the Hong Kong market during the sample period, where dividend yields are calculated in June of each year. The figure shows that there is considerable variation in the average dividend yield over time. The mean dividend yield is highest at 7.13% in 1985 and lowest at 2.77% in 2000.

3.4.2 Methodology and empirical results

The basis for tests of tax effects on equity returns is the version of the capital asset pricing model (CAPM) developed by Brennan (1970). This model relates the expected return on a stock to the stock's systematic or market risk and to the stock's dividend yield as follows:

$$E(r_{i,t} - r_{F,t}) = a_1 + a_2\beta_{i,t} + a_3(dy_{i,t} - r_{F,t}) + \varepsilon_{i,t} \qquad (1)$$

where E is the expectation operator, $r_{i,t}$ is the rate of return on stock i during period t, $\beta_{i,t}$ is its systematic risk as captured by the market beta of the stock, $d_{i,t}$ is the expected dividend yield, and $r_{F,t}$ is the risk-free rate. In the original model $a_3 = (T_d\text{-}T_g)/(1\text{-}T_g)$ where T_d and T_g are the tax rates on dividend income and capital gains of the representative investor. Coefficient a_3 can be interpreted as investors' tax disadvantage of receiving dividend income relative to capital gain income. Equation (1) predicts stocks that have higher dividend yields should provide higher before-tax risk-adjusted returns to compensate investors for the tax disadvantage of dividend income. In the case where capital gains taxes can be easily deferred (T_g = 0), the coefficient a_3 should equal to the tax rate of the marginal investor on dividend income.[32] Given that both T_d and T_g are equal to zero in Hong Kong, there should be no relationship between dividend yields and risk-adjusted stock returns in our data. Alternatively, finding a positive dividend yield coefficient in an economy where neither dividend income nor capital gains are taxed suggests there are nontax reasons that drive the relation between returns and dividend yields.

In section 3.4.2.1 we analyze the abnormal returns of different portfolios ranked by lagged dividend yield. In section 3.4.2.2 we quantify the magnitude of dividend yield effect using regression analysis. Section 3.4.2.3 reports some robustness tests.

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[32] Historically, in the U.S. 1 > T_d > T_g , thus a_3 is a positive number and is less than one.

3.4.2.1 Dividend-yield portfolios. In June of each year in the sample, stocks are classified into portfolios based on their lagged dividend yield. We use two different classifications, six portfolios and 11 portfolios. For each classification, the first portfolio contains all zero yield stocks in that year. The remaining stocks are then allocated according to their dividend yields such that each of the other portfolios contains roughly equal numbers of stocks. Both Naranjo et al. (1998) and Sialm (2009) also examine portfolios sorted on both dividend yield and firm size. The small number of stocks at the beginning of our sample period prevents us from sorting stocks based on both dividend yield and size.

For each portfolio, we calculate value-weighted returns and the value-weighted dividend yield of the portfolio. Returns are computed on a monthly basis and following Sialm (2009), the portfolio dividend yields are updated annually. Table 12 columns 1 and 2 report the average dividend yield in the portfolio formation year and in the following year after portfolio formation. As seen in the table, although there is evidence of mean reversion in dividend yields, the yields are nonetheless quite persistent and the portfolio rankings based on their dividend yields are maintained in the post-formation period.

To investigate the relationship between abnormal returns and dividend yields it is important to adequately control for differences in risk. One possible reason that dividend yields might be related to stock returns is that the dividend yield proxies for loadings on some omitted risk factors. For example, Fama and French (1992, 1993) find that a single market factor is insufficient to describe the cross-section of stock returns in the U.S. They propose a three-factor model that includes factors associated with firm size and the book-to-market ratio, and show that this three-factor model does a good job at capturing the cross-section of stock returns. Ang, Hodrick, Xing and Zhang (2009), use size and book-to-market-factors to control for risk in other countries, including Hong Kong. Following Naranjo et al. (1998) and Sialm (2009), we report results using risk-adjusted returns based on both the CAPM and the three-factor model.[33] Specifically we calculate abnormal returns for each portfolio as follows:[34]

$$\alpha_{k,t} = (r_{k,t} - r_{F,t}) - \beta_{k,T}^{M}(r_{M,t} - r_{F,t}) - \beta_{k,T}^{SMB}(r_{S,t} - r_{B,t}) - \beta_{k,T}^{HML}(r_{H,t} - r_{L,t}) \qquad (2)$$

Where $(r_{k,t} - r_{F,t})$, $(r_{M,t} - r_{F,t})$, $(r_{S,t} - r_{B,t})$, $(r_{H,t} - r_{L,t})$ are the values of excess return on the k^{th} dividend yield portfolio, the market factor, the size and book-to-market factors, respectively. The portfolio factor loadings $\beta_{k,T}^{M}$, $\beta_{k,T}^{SMB}$, $\beta_{k,T}^{HML}$ are allowed to differ across each nonoverlapping five-year period in the sample. The factor loadings are estimated for each portfolio separately in each five-year period using following time-series regression.[35]

$$r_{k,t} - r_{F,t} = \alpha_{k,T} + \beta_{k,T}^{M}(r_{M,t} - r_{F,t}) + \beta_{k,T}^{SMB}(r_{S,t} - r_{B,t}) + \beta_{k,T}^{HML}(r_{H,t} - r_{L,t}) + \varepsilon_{k,t} \qquad (3)$$

Table 12 columns 3 and 4 report means abnormal returns for each dividend yield portfolio, based on the CAPM and three-factor adjusted returns, respectively. Figure 3 graphically displays the results for the 11 dividend yield portfolios based on the three-factor adjusted returns. There is a nearly monotonic relationship between dividend yields and abnormal returns across portfolios. Focusing on the three-factor adjusted returns for the 11 dividend yield portfolios in column 4 of panel A, the difference in abnormal

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[33] We thank Andrew Ang, Robert Hodrick, Yuhang Xing and Xiaoyan Zhang for kindly sharing Hong Kong Fama-French factors. For more details on factors' calculation see Ang, Hodrick, Xing and Zhang (2009)

[34] CAPM model includes only market factor. Fama-French model include market, size and book-to-market factors. To save the space, we will write equation for full Fama-French model.

[35] Five nonoverlapping periods (1980-1985, 1986-1990, 1991-1995, 1996-2000 and 2001-2005) are used to allow the factor loadings to change overtime.

returns between the highest yield portfolio and the zero-yield portfolio is 1.5% per month (p-value < 0.01). By way of comparison, Naranjo et al (1998) report a difference in returns between the highest yield decile and the zero-yield portfolio of approximately 0.5% per month. Part of the large difference in returns is driven by the very low abnormal returns on the zero-yield portfolio, a finding also documented by Naranjo et al (1998). To be sure that the results we document are not driven by the zero-yield portfolio, we also report the difference in returns between the highest and lowest yield decile portfolios. Based on the three-factor adjusted returns this difference is 1.0% per month (p-value < 0.01). Panel B of the table repeats the analysis for the six portfolio classification. Consistent with the result in panel A, there is a strong dividend yield effect in returns. Moreover, this effect is economically large, over 12% per year on an annualized basis. This effect is difficult to reconcile with the tax capitalization hypothesis, given that both capital gains and dividend yields are untaxed in Hong Kong.

3.4.2.2 Regression analysis. The previous section suggests there exists a positive relation between risk-adjusted returns and dividend yields. To directly quantify the magnitude of the yield effect we use a regression approach similar to that employed by Sialm (2009). Specifically we estimate the following regression:

$$r_{k,t} - r_{F,t} = \alpha + \beta_{k,T}^{M}(r_{M,t} - r_{F,t}) + \beta_{k,T}^{SMB}(r_{S,t} - r_{B,t}) + \beta_{k,T}^{HML}(r_{H,t} - r_{L,t}) + \gamma dy_{k,t} + \varepsilon_{k,t} \quad (4)$$

where $dy_{k,t}$ is the expected monthly dividend yield of portfolio *k* at month *t*. The factor loadings $\beta_{k,T}^{M}$, $\beta_{k,T}^{SMB}$, $\beta_{k,T}^{HML}$ are estimated for each portfolio and are allowed to differ in each 5-year period. The coefficient on the dividend yield is of special interest. Under the tax capitalization hypothesis, the coefficient estimate should equal the difference in the tax rates on dividend income and capital gains. In the case of Hong Kong, the tax capitalization hypothesis predicts that the coefficient estimate on the dividend yield should be zero. Alternatively, finding a nonzero coefficient estimate on the dividend yield indicates that there are nontax reasons that drive the yield-return relation.

Table 13 reports the results. Focusing on the three-factor adjusted returns, the coefficient estimates on the dividend yield are all positive and significantly different from zero. Based on the 11 portfolio classification, the coefficient estimates range from 1.26 (p-value < 0.01) when the zero-yield portfolio is included to 0.94 (p-value < 0.01) when the zero-yield portfolio is excluded.[36] Economically, the coefficient estimates indicate that a one percent increase in dividend yields is associated with an approximately one percent increase in risk-adjusted returns. Under the tax capitalization hypothesis, this would require a difference in dividend and capital gains tax rates of 100%.

3.4.2.3 Robustness tests. To assess the robustness of our results Table 14 reports the coefficient estimates on the dividend yield variable from regression equation (4) above for subsamples segmented by time period and firm size. To examine the yield effect in different time periods, we divide our sample of 306 months (July 1980 to December 2005) into two equal subperiods. Table 14, panel A reports the coefficient estimates on the dividend yield for the two subperiods. Based on the three-factor model, the coefficient estimate on the dividend yield is 0.83 (p-value < 0.10) in the first subperiod, and 1.54 (p-value < 0.01) in the second subperiod. The lower statistical significance in the first subperiod is likely driven by the much smaller number of stocks in this time period. In both periods, however, the yield effect is economically large.

[36]Clustered standard errors by time are calculated. See Petersen (2009) for the issue of standard errors in panel data regression.

Naranjo et al. (1998) find that the dividend yield effect in their sample is absent in the largest quartile of NYSE firms. To examine size effects in our sample, we divide our sample into small and large firms based on the market capitalization at the end of June of each sample year. Firms are included in the large-firm (small-firm) subsample if the firm's size is greater (smaller) than median size in that sample year. Within each subsample, we further divide stocks into 11 dividend yield portfolios as described earlier. Table 14, panel B reports the coefficient estimates on the dividend yield for the large-firm and small-firm subsamples. The dividend yield coefficients are of similar magnitude and are statistically significant for both size groups. The magnitude of the yield effect is slightly larger for the small-firm subsample, but the difference is economically small. Overall, the dividend yield effect appears to be a robust phenomenon. The consistency of the effect in an economy where dividends and capital gains are not taxed provides additional evidence that is difficult to reconcile with the tax capitalization hypothesis.

3.5 Time-series evidence

The previous section shows that there is strong positive relation between the dividend yield and risk-adjusted stock returns in the cross-section in a no-tax environment. In this section we provide additional evidence regarding the tax capitalization hypothesis by examining the time-series relationship between the aggregate dividend yield and aggregate equity valuations. The methodology closely parallels Sialm (2009), who shows that aggregate equity valuations are lower at times when the overall tax burden on shares is higher. Under the tax capitalization hypothesis, we expect to find no relationship between the aggregate dividend yield and equity valuations in Hong Kong.

3.5.1 Data and variables

To measure equity valuations we use the Datastream Hong Kong Total Market Index to present Hong Kong aggregate market. This index includes approximately 130 equity securities and covers at least 75% of the market capitalization.[37] While the data for cross-sectional analysis are at monthly frequency, the data for time-series analysis are at annual frequency. We employ two measures of aggregate equity valuation, the price-earnings ratio and the total return on the index. Datastream defines the Price-Earnings ratio as the ratio between current price to current earning. To follow Sialm (2009) who defines the price-earnings ratio as the current price to earnings in the subsequent year, we calculate the price-earnings ratio as Datastream's subsequent year Price-Earnings ratio adjusted to reflect the change in the index price level over the year. In addition to the aggregate dividend yield, we also control for a number of macroeconomic variables, including the six-month interest rate, inflation, and GDP growth.

Table 15 reports summary statistics of the macroeconomic variables. The Hong Kong price-earnings ratio, divided by 100, has a mean of 0.146, similar to 0.144 for the aggregate U.S. data reported in Sialm (2009). The index's dividend yield has a mean of 4.2%, compared to 4.5% for the U.S. data. During the sample period, the index has an average return of 18.6% per year.

The Hong Kong stock market data begins in 1973. However, the 6-month deposit rate series begins only in 1985. As an alternative measure of the interest rate, we use the U.S. 6-month CD rate. The interest rates in the two countries should be highly correlated

[37] Datastream Global Equity Indices, User Guide, Issue 5

because the Hong Kong dollar is pegged to the U.S. dollar. Indeed, the correlation between these two series is 0.81 for the overlapping period. The inflation rate is calculated based on the composite CPI that begins in 1982. The website of Hong Kong Census and Statistics Department also provides CPI series A, B and C in addition to the composite CPI.[38] These series represent the CPI for households with low, medium and high expenditures, respectively. The series A, B and C begin in 1975. Thus, as an alternative measure of inflation measure, we also use inflation rate based on CPI series B.[39] Our most comprehensive sample, thus covers the 32 year period from 1975-2008.

3.5.2 Methodology and empirical results

Following Sialm (2009), we estimate the following regression to quantify the relation between the aggregate dividend yield and the valuation level:

$$val_t = \beta_0 + \beta_1 dy_t + \beta_2 r_t + \beta_3 \pi_t + \beta_4 g_t + \beta_5 t + \varepsilon_t \qquad (5)$$

We use the aggregate price-earnings ratio as dependent variable val_t to proxy for aggregate equity valuation. The independent variables include annual dividend yield dy_t, short term interest rate r_t, inflation rate π_t, GDP per capita growth rate g_t, and time trend t that takes value 1 for the year of 1973.

The tax capitalization hypothesis predicts high tax burden leads to low equity valuation. In case of Hong Kong, the tax capitalization hypothesis implies that dividend yield has no effect on equity valuation. Alternatively, finding negative relationship of the dividend yield with valuation indicates that there are nontax reasons that drive the yield-valuation relations.

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[38] http://www.censtatd.gov.hk/

[39] All macroeconomic variables are from Datastream except CPI (B) that is from Census and Statistics Department. Inflation rates calculated from Composite CPI and CPI (B) have correlation of 0.996.

Table 16, columns 1-3 report regression results. Dividend yield has a significant negative effect on equity valuation. In an univariate regression the coefficient on dividend yield is -1.39, with control variables added the effect decreases to -1.06. At the aggregate level, the dividend effect in Hong Kong is smaller than that reported in Sialm (2009) for the US, where the coefficients are -3.75 and -3.15. In unreported results, we find the dividend yield effect is robust when price-to-dividend ratio is used as proxy for valuation level.

The tax capitalization hypothesis predicts high tax burden results in not only low equity valuation but also in high equity return. Columns 4-6 report the results where we estimate equation (5) with index return as dependent variable. The coefficient of dividend yield is 12.06 in a univariate regression and 17.16 when control variables included. Sialm (2009) reports a coefficient of 4.67 and 5.16 on the effective tax yield. Overall, our time-series analysis provides additional evidence that tax capitalization cannot be a sole drive of yield-return relation.

3.6 Conclusion

A long standing question in financial economics is whether investor level taxes are reflected in asset prices. One view is that the tax payments on dividends and capital gains are capitalized into stock prices resulting in a positive relationship between equity returns and the tax burden imposed on investors. This tax hypothesis is supported by the theoretical work in Brennan (1970), the CAPM with tax, and empirically verified by well-documented positive relationship between dividend yield and stock return in the US data.

However, some evidence exists that challenges the tax hypothesis. The documented dividend yield effect is too large to be solely attributed to tax and the effect does not vary across different tax regimes. In addition, the effect is not present in large firms. This evidence leads to a second view on the dividend yield effect. There are omitted factors or variables that cause the relationship between dividend yields and returns. In this case, the yield-return relationship is just a spurious one as dividend yield also is affected by these omitted factors or variables.

In this paper we propose a novel approach. The Hong Kong Special Administrative Region does not levy tax on either dividend income or capital gains and thus provides a unique economic setting in which to examine the tax capitalization hypothesis. We document a robust positive relation between the dividend yield and stock returns in the Hong Kong market that is very similar in magnitude to the effect documented in the U.S. market. In addition to the cross-sectional results, we also provide time-series evidence. In the Hong Kong market, there is a robust negative relation between the aggregate dividend yield and aggregate equity valuations. In other words, during times when the dividend yield is high (low), aggregate equity values tend to be low (high).

Consistent with the second view, our paper suggests that there are nontax reasons that cause the relationship between dividend yields and returns. Our findings do not necessarily invalidate the Brennan (1970) after-tax CAPM, nor do they completely rule out taxes as one of the drivers of the dividend yield effect. Our contribution is to illustrate the difficulty of conducting a powerful test of the tax capitalization hypothesis in practice and to urge caution in interpreting the dividend yield effect as evidence in support of the tax capitalization hypothesis.

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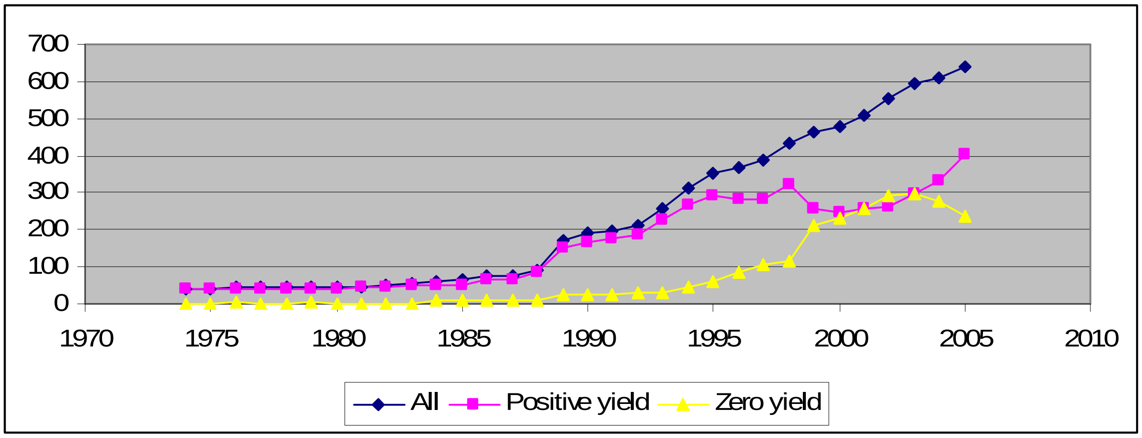

Figure 1
Number of stocks in the sample.

Hong Kong data obtained from DataStream for period from January 1973 to December 2005[40]. Only security type EQ is included. Firms that have one or more of the following are excluded: average monthly turnover (shares traded divided to shares outstanding) less than 0.73% (5th percentile of turnover distribution), percentage of no-trade days greater than 50%, six or greater continuous months of no-trade. Firm-month observations that have one or more of the following are excluded: missing market capitalization, missing price, missing unadjusted price, missing return index, return index equal zero, missing dividend yield. Dividend yield is winsorized at 99th percentile, return is winsorized at the 1st and 99th percentile. Padded monthly zero-return series at the end of the sample are deleted (see text for discussion). The figure shows the number of all firms, of positive dividend yield and of zero dividend yield firms at end of June each year.

[40] Dividend yield is defined as prior-year paid dividend divided to price one year ago. Thus, even thought our data start from Jan 1973, the first time portfolios are forms is June 1974. For analysis of dividend yield and abnormal return, we limit the sample to July 2000 to December 2005, the period that Fama-French factors are available.

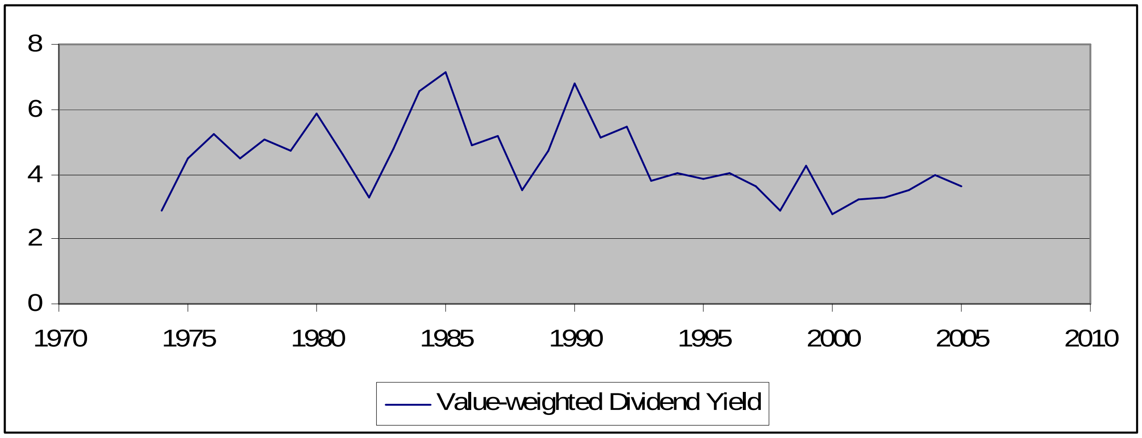

Figure 2
Hong Kong market value-weighted dividend yield trend.

Market value-weighted annual dividend yield is calculated annually in June.

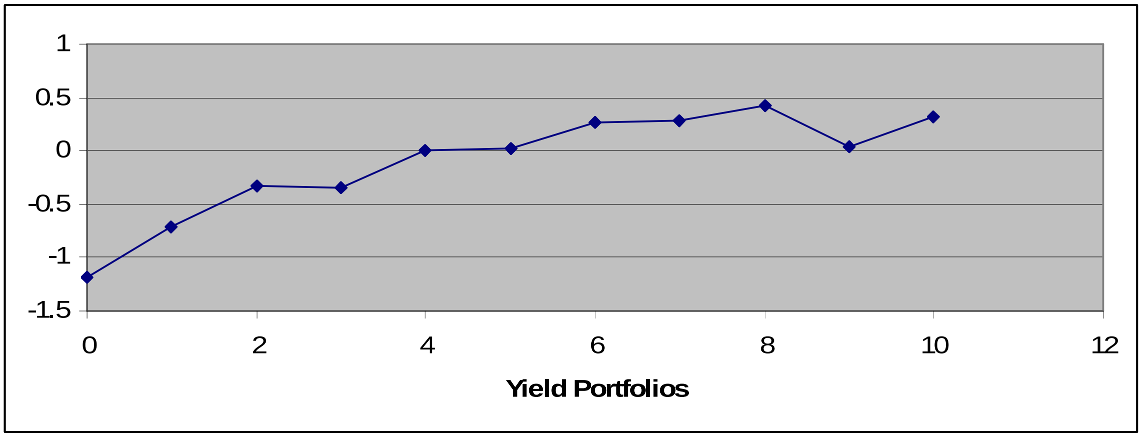

Figure 3
Fama-French model abnormal returns.

Figure presents means of abnormal returns of 11 dividend yield portfolios. Portfolios are formed annually end of June, one zero yield portfolio and 10 positive yield deciles. Abnormal returns are calculated monthly for the whole period 7/1980-12/2005 as following:

$$\alpha_{k,t} = (r_{k,t} - r_{F,t}) - \beta_{k,T}^{M}(r_{M,t} - r_{F,t}) - \beta_{k,T}^{SMB}(r_{S,t} - r_{B,t}) - \beta_{k,T}^{HML}(r_{H,t} - r_{L,t})$$

Where $(r_{k,t} - r_{F,t})$, $(r_{M,t} - r_{F,t})$, $(r_{S,t} - r_{B,t})$, $(r_{H,t} - r_{L,t})$ are values of k portfolio's excess return and of Fama-French three factors realized during month t[41]. $\beta_{k,T}^{M}$, $\beta_{k,T}^{SMB}$, $\beta_{k,T}^{HML}$ are the factor loadings estimated for each portfolio-period k-T separately using following equation[42].

$$r_{k,t} - r_{F,t} = \alpha_{k,T} + \beta_{k,T}^{M}(r_{M,t} - r_{F,t}) + \beta_{k,T}^{SMB}(r_{S,t} - r_{B,t}) + \beta_{k,T}^{HML}(r_{H,t} - r_{L,t}) + \varepsilon_{k,t}$$

[41] We thank Andrew Ang, Robert Hodrick, Yuhang Xing and Xiaoyan Zhang for kindly sharing Hong Kong Fama-French factors. For more details see Ang et al. (2009).

[42] Five nonoverlapping periods (1980-1985, 1986-1990, 1991-1995, 1996-2000 and 2001-2005) are used to allow the factor loadings to change overtime. Thus T takes values from 1 to 5. For the case of 11 dividend yield portfolios, for examples, the regression equation is estimated 55 times separately for each portfolio-period combination.

Table 11

Number of stocks in the sample.

Hong Kong data obtained from DataStream for period from January 1973 to December 2005. Only security type EQ is included. Firms that have one or more of the following are excluded: average monthly turnover (shares traded divided to shares outstanding) less than 0.73% (5-percentile of turnover cut-off), percentage of no-trade days greater than 50%, six or greater continuous months of no-trade. Firm-month observations that have one or more of the following are excluded: missing market capitalization, missing price, missing unadjusted price, missing return index, return index equal zero, missing dividend yield. Dividend yield is winsorized at 99^{th}-percentile, return is winsorized at the 1^{st}- and 99^{th}-percentile. Padded monthly zero-return series at the end of the sample are deleted (see text for discussion). The table reports the number of all firms, of positive dividend yield and of zero dividend yield firms at end of June each year.

Year	Number of all stocks	Number of positive yield stocks	Number of zero yield stocks	Year	Number of all stocks	Number of positive yield stocks	Number of zero yield stocks
1974	42	41	1	1990	189	166	23
1975	42	41	1	1991	197	174	23
1976	43	40	3	1992	214	184	30
1977	43	41	2	1993	257	225	32
1978	43	41	2	1994	311	266	45
1979	43	40	3	1995	354	293	61
1980	43	41	2	1996	367	283	84
1981	45	43	2	1997	389	283	106
1982	49	47	2	1998	434	320	114
1983	53	51	2	1999	465	256	209
1984	59	49	10	2000	478	246	232
1985	64	52	12	2001	510	255	255
1986	74	64	10	2002	553	262	291
1987	78	67	11	2003	593	298	295
1988	93	84	9	2004	608	331	277
1989	172	149	23	2005	638	402	236

Table 12
Dividend yield portfolios

Table reports means of annual dividend yield and monthly abnormal returns. Portfolios are formed annually end of June. Dividend yield is calculated annually in June[43]. Abnormal returns are calculated monthly for the whole period 7/1980-12/2005 as following[44]:

$$\alpha_{k,t} = (r_{k,t} - r_{F,t}) - \beta^{M}_{k,T}(r_{M,t} - r_{F,t}) - \beta^{SMB}_{k,T}(r_{S,t} - r_{B,t}) - \beta^{HML}_{k,T}(r_{H,t} - r_{L,t})$$

Where $(r_{k,t} - r_{F,t})$, $(r_{M,t} - r_{F,t})$, $(r_{S,t} - r_{B,t})$, $(r_{H,t} - r_{L,t})$ are values of *k* portfolio's excess return and of Fama-French three factors realized during month *t*. $\beta^{M}_{k,T}$, $\beta^{SMB}_{k,T}$, $\beta^{HML}_{k,T}$ are the factor loadings estimated for each portfolio-period *k-T* separately using following regressions.

$$r_{k,t} - r_{F,t} = \alpha_{k,T} + \beta^{M}_{k,T}(r_{M,t} - r_{F,t}) + \beta^{SMB}_{k,T}(r_{S,t} - r_{B,t}) + \beta^{HML}_{k,T}(r_{H,t} - r_{L,t}) + \varepsilon_{k,t}$$

Returns are value-weighted and in US dollars. Values are in percent and standard errors are in parentheses. *, **, and *** denote significance at the 10, 5 and 1 percent level, respectively.

Panel A: 11 Dividend yield portfolios

	Annual Dividend Yields		Abnormal returns	
	Prior Year	Subsequent Year	CAPM	Fama-French
	(1)	(2)	(3)	(4)
No Dividend Portfolio	0	0.971	-0.931*	-1.183**
			(0.543)	(0.472)
Lowest Yield Decile	1.397	2.258	-0.692**	-0.717***
			(0.302)	(0.276)
Decile 2	2.491	3.038	-0.336	-0.320
			(0.236)	(0.228)
Decile 3	3.219	3.522	-0.288	-0.344*
			(0.210)	(0.202)
Decile 4	3.835	3.834	-0.012	0.010
			(0.178)	(0.168)
Decile 5	4.457	4.312	0.124	0.015
			(0.216)	(0.198)
Decile 6	5.140	5.078	0.516**	0.274
			(0.231)	(0.214)
Decile 7	5.944	5.507	0.319	0.280
			(0.210)	(0.204)
Decile 8	6.988	6.299	0.497**	0.428**
			(0.217)	(0.211)
Decile 9	8.456	6.887	0.140	0.030
			(0.293)	(0.266)
Highest Yield Decile	12.106	8.526	0.377	0.319
			(0.299)	(0.269)
Highest Yield Decile Minus		7.554***	1.308**	1.501***
No Dividend Portfolio		(0.463)	(0.559)	(0.522)
Highest Minus Lowest			1.069***	1.035***
Yield Decile Portfolios			(0.374)	(0.360)

[43] For 'Prior Year' years 1980-2004 are used, for 'Subsequent Year' years 1981-2005 are used
[44] The equations are written for Fama-French model. Both CAPM and Fama-French model are used to adjust risk.

Table 12 (continued)

Panel B: 6 Dividend yield portfolios

	Annual Dividend Yields		Abnormal returns	
	Prior Year	Subsequent Year	CAPM	Fama-French
No Dividend Portfolio	0	0.971	-0.931*	-1.183**
			(0.543)	(0.472)
Lowest Yield Quintile	2.093	2.758	-0.565***	-0.513***
			(0.204)	(0.186)
Quintile 2	3.575	3.733	-0.128	-0.122
			(0.146)	(0.138)
Quintile 3	4.801	4.710	0.203	0.043
			(0.172)	(0.156)
Quintile 4	6.343	5.806	0.444**	0.422**
			(0.176)	(0.169)
Highest Yield Quintile	9.918	7.467	0.142	0.075
			(0.251)	(0.228)
Highest Yield Quintile Minus No Dividend Portfolio		6.495***	1.073*	1.258**
		(0.377)	(0.558)	(0.507)
Highest Minus Lowest Yield Quintile Portfolios			0.707**	0.588**
			(0.280)	(0.268)

Table 13

Dividend yield effect regressions

The table reports the dividend yield coefficient γ of the following regressions:

$$r_{k,t} - r_{F,t} = \alpha + \beta_{k,T}^{M}(r_{M,t} - r_{F,t}) + \beta_{k,T}^{SMB}(r_{S,t} - r_{B,t}) + \beta_{k,T}^{HML}(r_{H,t} - r_{L,t}) + \gamma dy_{k,t} + \varepsilon_{k,t}$$

Where $(r_{k,t} - r_{F,t})$, $(r_{M,t} - r_{F,t})$, $(r_{S,t} - r_{B,t})$, $(r_{H,t} - r_{L,t})$ are values of k portfolio's excess return and of Fama-French three factors realized during month t. $dy_{k,t}$ is expected dividend yield of portfolio k at month t. Two different portfolio formations are used: one zero yield portfolio and 10 positive yield deciles; one zero yield and 5 positive yield quintiles. $\beta_{k,T}^{M}$, $\beta_{k,T}^{SMB}$, $\beta_{k,T}^{HML}$ are estimated for each portfolio and are allowed to differ in each period[45]. Returns are value-weighted and in US dollars. The standard errors take into account clustering by month and are reported in parentheses. *, **, and *** denote significance at the 10, 5 and 1 percent level, respectively.

	CAPM	Fama-French
11 Dividend Yield Portfolios	1.266***	1.262***
	(0.344)	(0.321)
10 Positive Yield Deciles only	1.042***	0.938***
	(0.350)	(0.344)
6 Dividend Yield Portfolios	1.266***	1.338***
	(0.495)	(0.435)
5 Positive Yield Quintiles only	0.826**	0.683*
	(0.419)	(0.411)

[45] This regression equation is fitted only one time. For 11 portfolio case, for example, this regression estimates 167 coefficients: one alpha; 55 on market factor, 55 on SMB factor, 55 on HML factor, one on dividend yield.

Table 14

Dividend yield effect regressions: Subperiod and Subsample evidence

The table reports the dividend yield coefficient γ of the regressions in described in table 13. 11 dividend yield portfolio classification is used. Panel A divides the sample's 306 months into two equal-length periods. In panel B, subsamples are formed annually at the end of June. Small-firm (big-firm) subsample includes firms that below (above) median by market capitalization. Within each subsample, stocks are classified into 11 dividend yield portfolios as usual. Returns are value-weighted and in US dollars. The standard errors take into account clustering by month and are reported in parentheses. *, **, and *** denote significance at the 10, 5 and 1 percent level, respectively.

Panel A: Subperiods

	CAPM	Fama-French
7/1980-3/1993	0.836	0.828*
	(0.522)	(0.493)
4/1993-12/2005	1.590***	1.539***
	(0.506)	(0.459)

Panel B: Subsamples

	CAPM	Fama-French
Small-firm subsample	2.064***	1.957***
	(0.508)	(0.434)
Big-firm subsample	1.458***	1.276***
	(0.410)	(0.394)

Table 15

Summary Statistics of Macroeconomic Variables

All series are from Datastream, except (7) is from Hong Kong Census and Statistics Department. Data are at annual frequency and covers 36 years from 1973 to 2008. Missing data at the beginning years causes some series have fewer observations. Series (1)-(4) are for the Hong Kong Total Market Index. Dividend yield is dividend paid over the current year divided by index's price level at the end of the prior year. Price-Earnings (Price-Dividend) ratios are price level at the end of the current year divided by earnings (dividend) in the subsequent year. Index return is based on price level at the ends of the current year and of subsequent year. Inflation is calculated from composite CPI. Inflation (B) is calculated from CPI (B) that is CPI for households that have medium expenditure.[46] Last column reports correlation with Dividend Yield.

		Number of obs	Mean	Std. Dev.	Min	Max	Corr
(1)	Price-Earnings Ratio (Divided by 100)	35	0.128	0.050	0.084	0.367	-0.553
(2)	Price-Dividend Ratio (Divided by 100)	35	0.262	0.087	0.161	0.577	-0.603
(3)	Dividend Yield	35	0.042	0.011	0.017	0.062	1
(4)	Index Return	35	0.186	0.282	-0.657	0.696	0.553
(5)	Hong Kong 6-month Deposit Rate	24	0.047	0.026	0.002	0.104	0.232
(6)	Hong Kong Inflation	27	0.047	0.049	-0.039	0.113	0.644
(7)	Hong Kong Inflation (B)	33	0.055	0.052	-0.047	0.151	0.680
(8)	Hong Kong GDP per Capita Growth rate	36	0.103	0.085	-0.061	0.258	0.632
(9)	US 6-month CD rate	36	0.066	0.034	0.012	0.157	0.437

[46] See http://www.censtatd.gov.hk/

Table 16

Aggregate Dividend Yield and Aggregate Valuation Level

$$val_t = \beta_0 + \beta_1 dy_t + \beta_2 r_t + \beta_3 \pi_t + \beta_4 g_t + \beta_5 t + \varepsilon_t$$

The dependent variable val_t is either Price-Earnings ratio or Market Index return[47]. The independent variables include: expected dividend yield dy_t; short-term interest rate r_t; inflation rate π_t; GDP per capita growth rate g_t; and time trend t that takes value of 1 for year 1973. Price-Earnings, dividend yield and return are for Datastream Hong Kong Total Index. Data are at annual frequency and covers period of 1973-2008. The Newey-West standard errors are calculated with four-year lags and reported in parentheses. *, **, and *** denote significance at the 10, 5 and 1 percent level, respectively.

	Price-Earnings Ratio (Divided by 100)			Index return		
	(1)	(2)	(3)	(4)	(5)	(6)
Dividend Yield	-1.39**	-1.31*	-1.06**	12.06***	12.29*	17.16***
	(0.63)	(0.66)	(0.49)	(2.19)	(6.78)	(4.50)
Hong Kong 6-month Deposit Rate		0.11			-2.75	
		(0.27)			(1.81)	
US 6-month CD rate			0.01			1.11
			(0.18)			(1.68)
Hong Kong Inflation		-0.37**			1.36*	
		(0.15)			(0.71)	
Hong Kong Inflation (B)			-0.36***			-0.35
			(0.10)			(1.02)
Hong Kong GDP per Capita Growth rate		-0.07	0.01		-0.73	-0.10
		(0.07)	(0.09)		(0.53)	(0.78)
Time trend		0.00	0.00		0.00	0.01*
		(0.00)	(0.00)		(0.01)	(0.01)
Constant	0.18***	0.23***	0.20***	-0.29***	-0.25	-0.75**
	(0.03)	(0.03)	(0.03)	0.10	(0.46)	(0.27)
Obs	34	23	32	34	23	32
Adjusted R-square	0.28	0.59	0.52	0.29	0.14	0.18

[47] Valuation ratio is persistent. It is important to test for unit roots. Dickey-Fuller tests for unit roots in the price-earnings ratio can be rejected at one percent level. A regression of the difference in the price-earnings ratio on the corresponding lagged values have coefficients of -0.795 with standard errors of 0.09.

CHAPTER 4

CONCLUSION

In this dissertation, I have considered the effects of two corporate policies on shareholder wealth. First, I examined the effect of dividend policy. Firms can choose to follow either high- or low-dividend policies. I investigate whether paying high dividends in an economy such as the U.S., where tax on dividend income is higher than tax on capital gains, results in higher stock required rate of return to compensate investors for higher tax burden. Higher required rate of return, in turn, lowers equity valuation and decreases shareholder wealth. This view is suggested by the theoretical work in Brennan (1970), the CAPM with tax, and empirically verified by the positive relationship between dividend yield and stock return in the U.S. data.

Still, there is a second view on the dividend-yield effect. The positive relationship might be spurious, as dividend yield can be a proxy for omitted factors or variables. I document a robust dividend-yield effect in the Hong Kong market, where neither dividend income nor capital gains are taxed. My result is consistent with the second view, that there are unknown factors that affect both stock required rate of return and dividend policy. In this case, paying high dividends might be a part of an optimal corporate policy, and thus does not necessarily decrease shareholder wealth.

Second, I examine the effect of compensation policy. Specifically, I consider whether the practice of using peer groups in setting Chief Executive Officers' (CEO) compensation results in inflated pay, and thus transfers shareholder wealth to the CEOs. Many critics contend that the use of compensation peer groups has resulted in pay that cannot be justified based on economic fundamentals. Still others argue that peer groups are an efficient way for the board of directors to determine the competitive pay level that is necessary to both retain and motivate top executives. We examine the use of compensation peer groups using the mandated disclosure of peers that began in 2006.

We document evidence to support both sides of the debate, and find that the wealth transfer to CEOs is relatively small. We find that, on average, peers are chosen largely based on economic factors that reflect the managerial labor market in which the firms compete. Compensation peer groups contain firms that are in the same industry, are similar in size and scope, and reflect other commonalities related to labor-market factors. Nevertheless, we find that firms appear to exercise significant discretion in choosing peer firms. We show that when firms deviate from the economic model of peer choice, they tend to pick larger firms and firms with higher CEO pay. These biases in peer-group selection are more evident in smaller, less visible firms, where management arguably has more discretion in selecting the peer group. Despite the evidence of peer-group biases that we find, boards appear to only partially adjust pay in response to differences in compensation between the comparison group and the CEO, suggesting that boards exercise discretion that mitigates the effects of peer-group bias on pay increases. Finally, we also document preliminary evidence that increased disclosure has reduced the biases in peer-group choice.