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Victor Jarosiewicz
Bryant University, vjarosiewicz@bryant.edu

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Victor Esteban Jarosiewicz[*†]
Robert J. Trulaske, Jr. College of Business, University of Missouri-Columbia
Columbia, Missouri, U.S.A.

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[*] Correspondence to: Victor Esteban Jarosiewicz, Robert J. Trulaske, Jr. College of Business, University of Missouri-Columbia, Columbia, Missouri 65211, U.S.A. Email: jarosiewiczv@missouri.edu
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CEO Compensation after Harvester Director Departure

I examine the effect of post-IPO board member departure on CEO compensation. Theory predicts that a reduction in board monitoring should results in an increase in CEO pay. Using a novel dataset and empirical setting, I find that the effect is found only after the last 'harvester director' departs, who represents the interests of a Venture Capital or Private Equity fund that has backed the firm pre-IPO, and who has incentives to provide the strongest monitoring. Moreover, equity compensation does not increase, contrary to theory. This paper sheds light on important dynamics between board composition, monitoring and CEO compensation.

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1. INTRODUCTION

While much attention has been paid to venture-backed initial public offerings (IPOs), almost none has been given to those firms after the IPO directors depart. However, agency theory and prior research suggest that critical corporate policies, such as CEO compensation, are most affected by large, sharp changes in governance. It is during the years after IPO that one such sharp change occurs – the departure of "harvester" directors (i.e., directors representing Venture Capital funds (VCs) - and Private Equity Groups (PEGs)) from the board, and in studying that change I can test whether the change in governance really has a causal effect on policies such as CEO compensation.

CEO compensation is sensitive to active monitoring by board members (Cronqvist & Fahlenbrach, 2009). When directors representing outside funds join a board, monitoring intensifies and reduces CEO compensation. However, not all settings are the same: for example, when PEGs take public firms private, they increase CEO compensation (but also require CEOs to purchase a substantial stake in the firm) (Cronqvist & Fahlenbrach, 2013).

Beatty & Zajac (1994) examined CEO compensation for IPO firms, using a sample of IPOs from 1984. Their sample was strictly cross-sectional, and in their conclusions they suggested that "another avenue for future research would involve assessing how and why compensation contracts and corporate governance mechanisms change over time." However, in the more than twenty years since that article, research has not yet directly examined what happens to CEO compensation when harvesters depart after the IPO (Berry, Fields & Wilkins (2006) being the only notable exception; however, they found "no discernable pattern over time in incentive-based pay"), though theory predicts that the sharp change in board composition and decline in monitoring intensity should lead to increased CEO compensation. I examine the effect

on CEO compensation of the departure of IPO directors, in particular harvester directors, who have served on the board since the firm's IPO, to test such predictions from agency theory in this scarcely studied setting.

1.1 Research Question and Motivation

Agency theory predicts that board monitoring activities, including setting CEO compensation, vary by the relative power of founders and external financiers. The departure after IPO of "harvester" directors – powerful, active monitors able and motivated to constrain the CEO – has not been studied extensively as a setting. But, like other settings with sharp changes in board composition, it offers a means to testing predictions of the relationship between governance and CEO pay. However, it is also distinct - with firm and governance dynamics that are unique from other previously studied settings, such as in go-private leveraged buy-outs (LBOs), or in pre-IPO VC-funded start-ups.

What effect does the departure of directors (in particular "harvester" (external VC- and PE- fund) directors) who have served on the board since the firm's IPO have on CEO compensation, and how does it differ across departing directors? The contribution I seek to make to the corporate governance and CEO compensation literature is three-fold: First, I re-examine previously hypothesized governance-compensation relationships using in a novel empirical setting (IPO firms around and after harvester departure) that provides an opportunity to confirm and to contrast against existing empirical results. Second, using a treatment effect analysis, I find partial support for a causal relationship between changes in governance and changes in CEO pay; something which has proven to be difficult to resolve in prior research. Third, I draw attention to and expand our knowledge of a phenomenon (last harvester departure) and dataset that has the potential to deepen our understanding of the effects of external VC and PEG funding on firms sometimes long after they have become publicly traded.

1.2 The Complicated Relationship between IPO Director Departure and CEO Compensation

I construct a panel of firm-years from Emerging Growth Company (EGC) firms, following their IPO. I obtain the departure years of the directors on the board at IPO. I examine the relationship between the presence, and then the departure, of those IPO directors and CEO compensation. While I find statistically and economically significant relationships, I do not yet have a causal claim.

Director departure is not a random event, but is endogenously determined by the directors themselves. Hermalin & Weisbach (2003) discuss endogeneity extensively in their survey paper on endogeneity and boards of directors. I address the endogeneity by using matched samples to replicate a randomized experiment as closely as possible. I use a difference-in-differences method to reduce selection bias and also to isolate a treatment effect. From the original sample, I create a sub-sample of "harvester" IPOs that have experienced the departure of the last harvester director from the board (the treatment) matched with other, similar "harvester" IPOs that have yet to have the last harvester depart. In doing so, I minimize the effect of selection, as all firms are "harvester" IPOs, with similar selection by "harvesters" for IPO, and all such firms are plausibly similar in the likelihood of last harvester departure, making assignment to treated or control group as good as random.

In the difference-in-differences matched-sample regressions I find that departure of the last harvester has a negative marginal effect on equity compensation, but not direct (cash) compensation, in the year of departure. However, joint *F*-tests of the main and interaction terms of the difference-in-difference effect are only significant for direct compensation.

I also find that departure of the last harvester does not have a significant effect on a change in compensation from the years before last harvester departure to the years after the last

harvester departure. However, as with the comparison with the actual year of last harvester departure, joint *F*-tests of the main and interaction terms of the difference-in-difference effect before-and-after departure are only significant for direct compensation. Taken together, it appears that departure of the last harvester has an immediate, but not lasting, effect on equity compensation, and possibly a lasting effect on direct compensation.

One potentially confounding effect is change in compensation due to CEO turnover. I find both in the unmatched sample regressions and in the matched sample difference-in-differences regressions that CEO turnover has an immediate effect on (increases equity, decreases direct) compensation, but it only appears to have a lasting positive effect post-CEO turnover on direct compensation, and it varies according to how many CEOs turnover within the sample period.

These findings are not entirely consistent with agency theory. In particular, the lasting increase in direct compensation is the only finding consistent with predictions based on agency theory. The findings contribute to our understanding of the effects upon entrepreneurial firms when the last "harvesters" depart post-IPO, and contribute to a better understanding of changes on a board during a time in a public firm's existence that is relatively understudied.

2. THEORETICAL GROUNDING AND HYPOTHESES

2.1 CEO Compensation

CEO compensation is sensitive to active monitoring by board members (Cronqvist & Fahlenbrach, 2009); when directors representing outside funds join a board, monitoring intensifies and affects CEO compensation. They also find significant heterogeneity across different blockholders. "Executive compensation policies are systematically related to the particular large shareholder present in a firm. Adding blockholder fixed effects to a model that

already controls for important firm variation improves the model fit, and statistical tests reject the null hypothesis that all blockholder effects are zero for most policies. The effects are mainly concentrated in blockholder categories such as … private equity firms…."

Kaplan & Strömberg (2001), in their study of optimal contracting by venture capitalists, find that VCs typically include restrictive terms such as non-compete agreements on founders, and accordingly CEOs of entrepreneurial firms would want to be paid more than otherwise to compensate for the restrictions. They also document that increased responsibilities, such as CEO-chair duality, also increase CEO compensation in such firms.

Leslie & Oyer (2008) document that "PE-owned companies provide higher managerial incentives to their top management: CEOs have almost twice as much equity, approximately 10% lower salary, and more variable cash compensation than their counterparts at comparable public corporations." While it is tempting to directly compare PEG-owned firms taken public to first-time IPO firms with VC and PEG backing, it is important to distinguish between them. "Private equity (PE) firms buy companies, fix them, and then sell them." (Leslie & Oyer, 2008: 1) "A very important aspect of the equity programs is that managers are required to contribute capital—managers purchase the equity with their own funds. One interviewee explained that equity sharing is less about compensation of managers than it is about investment by managers." (Leslie & Oyer, 2008: 4) EGC IPOs are not about fixing broken firms to restore profitability and growth, and their founders and CEOs already own substantial portions of the equity. The comparison is limited to the notion of aligning managers' with major outside shareholders' financial interests.

Beatty & Zajac (1994) concluded that "the findings support our basic contingency arguments: that firms' use of executive compensation contracts to address managerial incentive

problems is hampered by risk-bearing concerns stemming from the risk-aversion of top managers and that firms seek to address this problem, which is greatest for the riskiest firms, by structuring their boards of directors to ensure sufficient monitoring of managerial behavior." That is, incentive based compensation has limits to its effectiveness, but the addition of strong monitors (i.e., harvester directors) substitutes for and mitigates this problem. Given this situation, it would be natural to expect compensation, particularly equity compensation, to increase after the departure of harvester directors from a board. Indeed, Ofek & Yermack (2000) note "boards' compensation committees routinely cite the goal of increasing managerial ownership as the rationale for equity-based pay."

Cho & Shen (2007) "examine the changes in executive compensation at the top management team (TMT) level following an environmental shift, and find that a dramatic environmental change [airline deregulation] that heightens managerial discretion leads to greater pay level and performance sensitivity of TMT compensation; and the greater the magnitude of turnover among TMT members following the environmental shift, the greater the compensation change." While their empirical setting focuses on an exogenous environmental change rather than an endogenous change in governance, their theoretical perspective is of increased managerial discretion, which would certainly be the case in the situation where harvester directors finally depart from the board after IPO.

2.2 Hypothesis Development

2.2.1 IPO Director Departure and Changes in CEO Compensation

Departures of a director from a firm's board signal that something is changing in its governance. Director departure from a healthy firm does affect governance (Campbell & Frye, 2009), just as it does when things go wrong at the firm (Fahlenbrach, Low, & Stulz, 2010). Moreover, IPO directors are unique: they are not randomly chosen from among all directors;

they are specifically chosen to signal a firm's quality at IPO, and to provide essential guidance and resources to the newly public firm (Kor & Misangyi, 2008; Pollock, Chen, Jackson, & Hambrick, 2010). Current work supports the notion that IPO director departure also sends a positive signal because directors tend to depart from the board when the firm performs well (Jarosiewicz & Lee, 2017).

Positive firm performance tends to increase CEO compensation (Bertrand & Mullainathan, 2001), so if IPO director departure tends to send a positive signal about firm performance, then I would expect IPO director departure to also be associated with increases in CEO compensation. Also, as IPO directors depart from the board, withdrawing their guidance and provision of resources, the CEO may face additional duties and responsibilities, which may require additional compensation.

Hypothesis 1: IPO director departure is associated with an increase in the firm's CEO compensation.

2.2.2 Harvester IPO Director Departure and Changes in CEO Compensation

Outside directors are seen as better monitors than inside directors. Blockholder investors who sit on the board are the strongest monitors. Adding one blockholder director to the board has the greatest effect on CEO compensation (Bertrand & Mullainathan, 2001).

Hypothesis 2: The increase in CEO compensation is stronger when harvester IPO directors depart than when non-harvester IPO directors depart.

Blockholder investors who sit on the board are the strongest monitors. Therefore, even when some harvester directors depart, so long as at least one harvester remains on the board to protect their investment and help guide the firm, they will continue to constrain CEO compensation. Only after the departure of the final harvester director does the balance of power

truly shift to the CEO, who can then renegotiate their compensation more favorably with a board now lacking the strongest monitors.

Hypothesis 3a: The increase in CEO compensation is weaker when harvester IPO directors depart (while other harvester directors remain on the board) than when the last harvester IPO director departs.

Hypothesis 3b: The increase in CEO compensation is greatest when the last harvester IPO director departs.

3. RESEARCH METHODS

3.1 Empirical Design

I test my hypotheses using a sample of IPO firms to observe the effect of departure of IPO directors on CEO compensation. The sample is focused on the outsider directors of young, entrepreneurial firms that have gone public. These firms often have external financial backing prior to the IPO, and consequently have harvester directors included on their IPO boards. I obtain year of departure for the outsider directors and annual CEO compensation data. To control for known alternative factors that also affect CEO compensation, I also obtain data related to firm size, growth, profitability, industry, and year of CEO turnover (when those occur).

With that data, I first examine univariate changes in CEO compensation. Because my strongest prediction is regarding last harvester departure, I focus on changes in CEO compensation before and after those years.

Next, I follow Cronqvist & Fahlenbrach (2009) and regress CEO compensation against the presence of harvester IPO directors to estimate the average effects when such harvester IPO directors – who represent large, active blockholder investors – are included on the board. In particular, in Table 3, I estimate models using equations (2) and (3) from their analysis.

Also, using fixed effects regressions (like Cronqvist & Fahlenbrach (2009), who in turn follow Bertrand & Schoar (2003)), I estimate the impact of individual heterogeneity among non-harvester and harvester IPO directors upon CEO compensation. In particular, in Table 4, I estimate models using equation (1) from their analysis, which includes year and firm fixed effects, along with fixed effects for each individual (individual IPO directors in my dataset, contrasted with individual blockholders in their dataset).

Finally, to test my hypotheses, I regress annual CEO compensation on indicators of departures of non-harvester and harvester IPO directors, including an indicator for the departure of the last harvester IPO director.

To control for the endogeneity of departure and control for selection bias, following Chhaochharia & Grinstein (2009), I then estimate a treatment effect using a difference-in-differences model. I create a matched sample of firms having external financial backing prior to the IPO and harvester directors included on their IPO boards. The treated firms experience the departure of the last harvester from the board, while the matched control firms do not, during the sample period. Following the difference-in-difference methodology in Chhaochharia & Grinstein (2009), I estimate the pre-vs-post treatment effect of director departure on CEO compensation over the sample period, in order to test whether the effect persists over the sample period. Finally, I re-estimate the regressions of annual CEO compensation, but using the matched sample difference-in-differences model, to estimate a treatment effect of director departure on CEO compensation just in the year of departure.

3.1.1 Data Sources

I build upon the research by Kenney et al. (2012), which is the first published work directly addressing EGC IPOs, covering all EGC IPOs between 1996 and 2010 in the United

States. I combined their dataset[1] with firms' financial information drawn from Jay Ritter's IPO dataset[2] and from the Compustat database available in WRDS.[3] I obtained director information from Martin Kenney and Don Patton's outsider (i.e., non-executive) director dataset for the same EGC IPOs. For CEO compensation (and CEO turnover data), I used both BoardEx and ExecuComp (from WRDS) datasets to obtain coverage as complete as possible of the dependent variables.

For the starting and ending dates of IPO directors' tenure on a firm's board, I used directors' employment history from BoardEx. BoardEx has both announcement dates and event dates (average and median differences between announcement and event dates are 23 days and 0 days, respectively) for company announcements[4], while its employment history for directors reports effective dates of joining and departure from boards.[5] However, the Announcements file does not cover all Employment History events (i.e., not all director join dates and departure dates are recorded as announcements), so I choose to rely on the Employment History file, recognizing that the join and departure dates are effective dates, not announcement dates.

[1] Martin Kenney and Don Patton's database includes board of director information on all Emerging Growth Company initial public offerings (IPOs) on American stock exchanges and filed with the Securities and Exchange Commission (SEC) from January 1996 through December 2010. A description of the database and contact information for Don Patton for requesting a copy of the database are found at: http://hcd.ucdavis.edu/faculty/webpages/kenney/misc/IPO_database.htm.

[2] Jay Ritter's database contains a broad range of information, including post-IPO stock performance, on all U.S. listed IPOs from 1975-present. Substantially all of the data is available across his data files at his website: http://bear.warrington.ufl.edu/ritter/ipodata.htm. A non-public copy of an aggregate file was used for this analysis.

[3] Wharton Research Database Services.

[4] The BoardEx Company Announcements file contains both an announcement date and an effective date. The average difference between announcement date and effective date is 23 days, while the median difference is 0 (both dates are the same). The BoardEx Director Employment History file only contains the effective dates.

[5] The 2013 BoardEx Glossary indicates that employment-related variables, such as "Time (Yrs) on Board," are based on the date of joining the Board and the date of leaving the board. However, if the join date is not available, then the assumed date is the start of the financial year when the director is first quoted in the annual report. Likewise, if the date of leaving is not known then the assumed date is the end of the financial year when the director is last quoted in the annual report.

3.1.2 Dependent Variables

BoardEx[6] defines Direct Compensation as the sum of salary and cash bonus. BoardEx defines Equity Linked Compensation as the sum of the value of shares awarded, the value of LTIP awarded, and the estimated value of options awarded[7]. BoardEx defines Total Compensation as the sum of Direct Compensation and Equity Linked Compensation. ExecuComp's ANNCOMP[8] dataset defines Total Compensation and Direct Compensation similarly to BoardEx, and so I calculate Equity Compensation from ANNCOMP data as the difference between the other two variables.

ANNCOMP has a CEO flag, which facilitates identifying CEO compensation. However, in working with and merging BoardEx and ANNCOMP data I discovered that it contains false negatives (for some executives, the observations do not have the flag set to CEO, even though I confirm that they are CEO observations). To correct that, I search within the executive role variable in ANNCOMP[9], and I also merge the ANNCOMP observations with the BoardEx

[6] The BoardEx files I rely upon contain person-firm-year observations for directors of publicly traded firms, as well as of private firms and of non-profit organizations. The dataset coverage is sparse before 1999, and my version's coverage ends shortly after 2013. The dataset covers a broad range of public firms, including many small and young firms; however, its coverage greatly increases over time. Prior literature has noted that years prior to 2003 have a marked decline in coverage, particularly among smaller firms. I have not made any adjustments for this type of selection effect, though I feel it is important to point out, given the size of my final sample (only 77 treated firms and 77 matched control firms) due to loss of observations with missing data. Among the EGC IPO firms I study, I have no prior reason to believe that those that are included in the BoardEx data are substantially different from those that are not, other than the size effect previously observed in early years of the dataset. Consistent with prior literature, I include proxies for firm size as controls in my analyses, which should help partial out its impact, as the observations remaining in my sample still exhibit significant variation as far as firm size.

[7] BoardEx defines the estimated value of options awarded as: "a prediction of the value of the options awarded during the period based on the latest closing stock price using an Generalised [sic] Black Scholes option pricing model. Volatility is measured using a 100-day historic volatility. The Option Expiry Date uses the Country average if Expiry Date is unavailable."

[8] The ANNCOMP dataset I rely upon contains person-firm-year observations for the top managers of publicly traded firms, as reported in the management section of annual reports filed with the SEC. The dataset coverage begins in 1992, and my version's coverage ends shortly after 2013. The dataset mainly covers S&P 1500 firms, and thus does not extensively cover the EGC IPO firms I study.

[9] I search for "CEO" and its variants in the role description. I also screen out other phrases, such as "assistant to the CEO" and others, which would create false positives. In any fiscal year where I obtain more than one CEO observation in the compensation dataset, I first cross-reference the name and role in BoardEx. In a few instances when this does not resolve the problem, I use CapitalIQ to access the firm's top management team (TMT) profile, and if necessary, access the underlying SEC filings directly.

observations (often, one dataset has information for CEO-firm-years that are missing from the other). I identify CEOs, their compensation and turnover dates in BoardEx from their roles in their director profiles and in their employment history. CEO compensation exhibits a number of outliers, so I winsorize the variables at the 2.5% level in each tail (by comparison, Cronqvist & Fahlenbrach (2009) winsorize at the 1% level in each tail).

3.1.3 Independent Variables

"Departure" is an indicator variable coded as 1 if the IPO director departs from a firm's board within five years since the firm's IPO (based on the departure date from BoardEx), else 0.

As mentioned earlier, "Departure" is endogenous, so I use it as a dependent variable for propensity score matching.

"Harvester" is an indicator variable coded as 1 if the IPO director represents a VC or PEG fund that provided external capital to the firm (and must therefore "harvest" their investment), else 0. The Kenney-Patton dataset only flags IPO directors as "VC" or not. However, the Ritter dataset flags IPOs as VC-backed, PEG-backed, or not backed. Therefore, I flag any harvester as a "PEG director" if they only appear at PEG-backed IPOs, while any IPO director only appearing at VC-backed or both at VC-backed and PEG-backed IPOs I flag as a "VC director".[10]

Chhaochharia & Grinstein (2009) document that CEO compensation differs if the CEO is replaced than if the same CEO remains in a sample period. Accordingly, I include a "new CEO" turnover identifier in the fiscal year when turnover occurs. Going further, multiple or frequent CEO turnover might have a different effect than a single change in CEOs, so I also generate a

[10] While an individual could be simultaneously, or sequentially, affiliated with a VC firm and a PEG fund, those business models are very distinct and I expect such a situation to be an outlier. However, my identification scheme would misclassify such individuals as being only VC-affiliated.

"Post n^{th} CEO change" variable (effectively, a counter or categorical variable) that indicates how many times the CEO has changed in the sample period.

I mainly use the same control variables as in Cronqvist & Fahlenbrach (2009), namely lagged Tobin's Q and lagged natural log of total assets, and where I follow Chhaochharia & Grinstein (2009), I use the same control variables as they do, namely, natural log of sales ("*Sales*") and the natural log of one plus ROA ("*ROA*").

The pool of EGC firms covers IPOs from 1996 to 2010 (15 years of IPOs), and IPO director departures from 1999 to 2013 (matching the availability of director departure data from BoardEx). The final sample is substantially reduced due to lack of availability of data, covering 303 EGC IPOs with observations from 1999 to 2013, resulting in 2,483 firm-year observations.

3.2 Sample Construction with Matching Methods

3.2.1 Matched Sample

Following Cronqvist & Fahlenbrach (2013), I use a matched sample analysis, to facilitate a difference-in-differences analysis, such as those used in Cronqvist & Fahlenbrach (2009) and Leslie & Oyer (2008). I created a 1-to-1 matched sample of harvester-backed IPOs. All firms in the matched sample were chosen from the initial EGC IPO sample. All firms in the sample have harvesters on the board, but matched control firms have not yet experienced the last harvester departure. 80 "Treated" firms in the original sample experienced the departure of the last harvester from their board during the sample period, but 3 treated firms were unmatched by 2-digit SIC code and were excluded from the matched sample. Each of the remaining 77 treated firms (representing 726 firm-year observations) was matched with a control firm by 2-digit SIC code and the condition that the matched firm-years had to include the same fiscal year as the fiscal year in which the last harvester departed from a treated firm's board (e.g., for a treated firm with a last harvester departure in 2008, the matched control firm's firm-years had to include

2008). The first 43 firms I was also able to match on IPO year (to control for years since IPO, a proxy for time-based pressure for harvester directors to finish harvesting and depart) and on CEO turnover. I matched another 29 firms only on CEO turnover without regard to match by IPO year. I matched the final 5 firms without regard to IPO year or CEO turnover. In cases where a treated firm was initially matched with multiple control firms, I chose the best match by minimizing the difference in ROA in the fiscal year that coincided with the last harvester departure year. For each firm, I included all firm-years with available data. The final combined sample contained 1,482 firm-year observations.

3.2.2 Summary Statistics

Table 1 presents summary statistics and pairwise correlations among the variables under study. Equity compensation dwarfs direct compensation, and is the driver of total compensation (also noted by the 0.986 correlation between equity compensation and total compensation). The coefficient of variation (standard deviation divided by the mean) in equity compensation is much larger than in direct compensation as well, meaning it is much more varied than direct compensation. The mean natural log of total assets of 5.795 corresponds to an average firm size of approximately 330 million dollars, confirming that the EGC IPO based sample is much smaller than the typical, large public companies in the S&P 500, while the lagged Tobin's Q ratio mean of 3.373 indicates that these EGC firms are truly growth firms, well above the typical Tobin's Q ratio of closer to 2 for large public companies in the S&P 500.

Insert Table 1 about here

3.3 Estimating the Effect of IPO Director Departure on Changes in CEO Compensation

3.3.1 Univariate Results

Table 2 presents a univariate analysis of CEO compensation around last harvester director departure. The observations are split between those before the last harvester departure and those after the last harvester departure, and also between firms with no CEO change and those with (one or more) CEO changes.

In the observations without a CEO change, none of the components of CEO compensation show any significant difference from before to after the departure of the last harvester director. The same is true in the observations with CEO changes. There is, however, a significant difference between firms with and without a change in CEO in the equity compensation (and thus, total compensation) before the departure of the last harvester director, which disappears after the departure of the last harvester director. At first blush, then, the departure of the last harvester director does not appear to significantly affect any part of CEO compensation, while the alternative explanation of CEO turnover does have some effect.

Insert Table 2 about here

3.3.2 Examining Effects of Harvesters and their Heterogeneity upon CEO Compensation

Table 3 follows from Cronqvist & Fahlenbrach (2009) Table 3, Columns 3 and 6. Its purpose is to test whether the presence of "harvester" directors affects CEO compensation (Models (1), (3) and (5)), and whether VC directors have a different effect from PEG directors within the harvester category (Models (2), (4) and (6)). The coefficients for harvester are statistically and economically significant for direct, equity and total compensation, and *F*-tests strongly reject that VC and PEG director coefficients are equal to each other for any of the

compensation variables. Interestingly, the coefficients on equity and total compensation are strongly positive for the marginal effect of VC directors but strongly negative for the marginal effect of PEG directors, opposite that in prior literature.

Similar to results from Cronqvist & Fahlenbrach (2009) Table 3, lagged Tobin's Q is not significant, while lagged log of total assets is strongly significant and positive, indicating that while variance in growth does not appear to affect compensation, size most definitely does.

Insert Table 3 about here

Table 3 follows from Cronqvist & Fahlenbrach (2009) Table 4, Panel C. Its purpose is to test whether the heterogeneity of individual "non-harvester" and individual "harvester" directors (grouped by VC and PEG affiliation) differs in the effect upon CEO compensation. *F*-tests strongly reject no heterogeneity in fixed effects among non-harvester directors. On the other hand, *F*-tests cannot reject no heterogeneity in fixed effects among VC directors for any component of CEO compensation, but they can for PEG directors, as far as equity (and thus, total) compensation. Strictly for comparison, when the natural log of CEO compensation is considered instead, the opposite is true as far as VC and PEG director heterogeneity is concerned.[11]

[11] Table 4 has an additional restriction: there are more individual directors than firm-year observations in the full sample, resulting in insufficient degrees of freedom when including all individual fixed effects. So, to produce Table 4, I only include non-harvesters that are present at least at two or more firms in the sample (this is actually consistent with what Cronqvist & Fahlenbrach do, as they follow Bertrand & Schoar's moving dummy variable methodology that requires multi-firm individuals for model identification). I do not similarly restrict the VC and PEG directors in my sample. Theoretically, harvesters are much more influential than non-harvesters, so by only including non-harvesters at two or more firms I oversample those non-harvesters that are selected by firms as being more valuable and influential, similar to harvesters. Empirically, given the much smaller pool of VC and PEG directors, I choose to maximize the information retained in the Table 4 sample, even though it might lead to some sampling / selection bias. Identification of harvester fixed effects is achieved because they are not perfectly collinear with firm and year fixed effects (i.e., they are present more than one year in a firm, but not for all the years that firm appears in the sample, so they are distinct from time-invariant firm fixed effects covering all the years in the sample, and they are

Interestingly, the median fixed effect declines for equity and total compensation, but increases for direct compensation, when harvester directors are included alongside non-harvester directors. Consistent with the findings in Table 3, this is again contrary to previous findings in the literature.

Insert Table 4 about here

3.3.3 Regressions of CEO Compensation on IPO Director Departure

Table 5 tests several hypotheses regarding the effect of IPO director departures on CEO compensation, while controlling for size, growth, and CEO turnover. Model (1) tests whether harvester and non-harvester director departures affect compensation equally. Model (2) tests whether last harvester departure and other than last harvester departures affect compensation equally. Model (3) examines whether CEO turnover affects CEO compensation. Model (4) examines whether the last harvester departure effect is robust to inclusion of a CEO turnover effect. Model (5) again tests whether last harvester departure and other than last harvester departures affect compensation equally, while controlling for non-harvester departures and for CEO turnover. Models (6) through (10) repeat the analysis for direct compensation.[12]

Insert Table 5 about here

distinct from cross-sectionally invariant year fixed effects, as they are only in one or a few firms in the cross-sectional sample of each firm year). It is possible that the sampling / selection bias is at least partly responsible for the strongly significant *F*-tests on non-harvester directors and also for the lack of strong significance on the harvester directors.

[12] Untabulated regressions of Total compensation (the sum of Direct and Equity compensation) result in identical patterns of signs and significance as with Equity compensation Models (1) to (5), with only minor increases in estimated coefficients.

Model (1) shows that only the harvester departure coefficient on equity compensation is statistically significant, but negative, though an *F*-test fails to reject that the coefficients are equal at conventional statistical levels (*p*-value 0.125). Hypothesis 1 does not distinguish between harvester and non-harvester IPO directors, but this finding suggests that such a distinction is crucial, so given the non-significance of non-harvester departures, I do not believe Hypothesis 1 is strongly supported. However, this finding directly contradicts Hypothesis 2, that CEO compensation increases more when harvester directors depart than when non-harvester directors depart. Model (2) shows that only the last harvester departure coefficient on equity compensation is statistically significant, but negative, and an *F*-test rejects that the coefficients are equal (*p*-value 0.079). While the statistical significance and relative magnitudes of the effects support Hypotheses 3a and Hypotheses 3b, the negative sign directly contradicts the predicted increase in CEO compensation.

Model (3) indicates that CEO turnover affects CEO compensation, which was expected, and will be used as a control in examining director departure effects in the presence of this potential alternative explanation. Model (4) examines does confirm that the last harvester departure effect is robust to inclusion of a CEO turnover effect, and Model (5) again confirms that the last harvester departure affect compensation more strongly than other than last harvester departures, while controlling for non-harvester departures and for CEO turnover. Unfortunately, the sign remains the opposite of the predicted increase in compensation.

Models (6) through (10) generally find the same results for director departure effect on direct compensation, but that CEO turnover has a statistically significant and negative effect on direct compensation.[13]

3.3.4 Matched Sample Summary Statistics

Table 6 presents summary statistics and pairwise correlations among the variables in the only-harvester-backed-IPO matched sample. The sample statistics and correlations for the variables carried over from the main sample remain mostly unchanged. However, direct compensation decreases slightly and equity compensation increases slightly from the main sample, and statistics related to harvester departure increase naturally due to the restriction to a harvester-backed-IPO-only sample.

Insert Table 6 about here

3.3.5 Last Harvester Departure Treatment Effect on Post-vs-Pre Departure Levels of Compensation

Following Chhaochharia & Grinstein (2009) ("CG"), I estimate a treatment effect for the departure of the last harvester director on pre-vs-post departure levels of CEO compensation. However, they do not use a matched sample, thus industry-year fixed effects are appropriate in their analysis. My sample matches by industry in all matched firms, so while year effects are still relevant, industry effects are less so.[14]

[13] Untabulated regressions of Total compensation (the sum of Direct and Equity compensation) result in identical patterns of signs and significance as with Equity compensation Models (1) to (5), with only minor increases in estimated coefficients.

[14] Untabulated regressions including industry-year fixed effects indicate qualitatively similar results, though standard errors increase. Notably, the only coefficients with changes in significance are that POST 1st CEO change becomes significant in Models (8) and (10). Otherwise, all margins and margins contrasts tests maintain the same (in-)significance as in the tabulated results.

Model (1) most closely resembles CG, including the same control variables (*Sales* and *ROA*, as they define them) interacted with the pre-vs-post period indicator variable. Model (1) includes an estimated effect for tenure, which is insignificant (β=-0.018, s.e.=0.012, *p*-val 0.128) and is not carried forward into other models. Model (2) differs only from Model (1) by not including tenure. Model (3) differs from Model (2) only in using year fixed effects instead of industry-year fixed effects. Thus, Models (1)-(2) include industry-year and firm fixed effects, while Models (3)-(10) include year and firm fixed effects.

Models (1)-(7) follow the CG regression specification, which excludes estimation of main effects for interaction terms, whereas Models (8)-(10) include them. No explanation is found in the original article for their exclusion, but they potentially act as omitted variables in estimating the true effect of the interacted indicator variables, so they are included in the final three models used to re-examine the hypotheses.

Models (1)-(4) follow CG dependent variable specification of log of compensation, whereas Models (5)-(10) revert to the untransformed dependent variables, consistent with the rest of the analysis. Models (1)-(7) follow CG regression specification, interacting pre-vs-post period dummy with controls. However, as the Assets coefficients are not different in Models (5)-(7), and Tobins Q coefficients are not different in Model (6), those interactions are removed in Models (8)-(10), respectively. Models (1)-(7) are estimated using the *REGHDFE* command in Stata 14.2; Models (8)-(10) estimated using the *REGRESS* command in Stata 14.2. I do this because the inclusion of the main effect indicator variable "treated" results in an unreliable and nonsensical coefficient using *REGHDFE* (standard error magnitude of 10^8), while using *REGRESS* results in a reliable and reasonable standard error estimate.

Standard errors are clustered by firm-years for Models (1)-(7) following the CG specification, and clustered by firm for Models (8)-(10). Clustering by firm-year treats separate firm-years as independent, while clustering by firm considers the intertemporal correlation across firm-years for each firm. Untabulated regressions clustering by firm-year indicate qualitatively similar results, with smaller standard errors, as expected.

While models (1) through (7) are presented to show a natural progression, one change at a time, from the original CG specifications to those most directly applicable to the hypotheses, I focus here on discussion of models (8) through (10), which directly test the proposed hypotheses.

The coefficients of the interaction between treatment and post-vs-pre last harvester departure are not significant for any CEO compensation component. However, a more appropriate test is whether the average marginal effects are significantly different between the treated and untreated observations. Unfortunately, Stata is unable to directly compute a test of the contrast of the average marginal effects. Instead, I present an *F*-test of the combined main and interaction term coefficients that together determine the difference between treated and untreated departure treatment effects. The *F*-test is only statistically significant for the departure treatment effect on direct compensation, and the sum of the coefficients is positive. This lends some (limited) support to the hypothesis that CEO compensation increases after the departure of the last harvester director. Moreover, the nature of the analysis suggests that this increase is a treatment effect rather than a selection effect.

Insert Table 7 about here

3.3.6 Last Harvester Departure Treatment Effect on Year of Departure Compensation

Table 8, using the difference-in-differences matched sample framework, re-examines the hypotheses regarding the effect of last harvester departure on CEO compensation in the year of departure, while controlling for size, growth, and CEO turnover. Models (1)-(3) present year of last harvester departure-vs-other years difference in compensation, differenced between treated and control firms.

The sample is the same matched sample as I used for the Table 7 difference-in-differences treatment effect regressions. The models replicate the analysis from Table 5, with the addition of an interaction for "treated" in order to compute difference-in-differences between treated and matched control firms. "Treated" firms experienced the departure of the last harvester from their board during the sample period, while matched control firms have not.

Insert Table 8 about here

Model (1) shows that the marginal effect from last harvester departure on equity compensation is statistically significant, but negative. Model (2) shows that the marginal effect from last harvester departure on direct compensation is not statistically significant, but that the treated indicator is strongly significant and positive. Model (3) shows that the marginal effect from last harvester departure on total compensation is statistically significant, but negative.

Again, a more appropriate test is whether the average marginal effects are significantly different between the treated and untreated observations. Unfortunately, Stata is again unable to directly compute a test of the contrast of the average marginal effects. Instead, I present an *F*-test of the combined main and interaction term coefficients that together determine the difference between treated and untreated departure treatment effects. The *F*-test is only statistically

significant for the departure treatment effect on direct compensation, and the sum of the coefficients is positive. This again lends some (limited) support to the hypothesis that CEO compensation increases after the departure of the last harvester director. Moreover, the nature of the analysis suggests that this increase is a treatment effect rather than a selection effect.

4. DISCUSSION AND CONCLUSIONS

4.1 Harvester IPO Directors Matter

Univariate results are inconclusive as to changes in CEO compensation after the departure of the last harvester director, but they do support that CEO turnover might potentially explain changes in CEO compensation. However, I find that the presence of harvester directors (particularly, VC harvester directors) does appear to have a positive effect on equity, and therefore, total CEO compensation, but a negative effect on direct CEO compensation. Curiously, there is significant individual heterogeneity in the effect on CEO compensation among IPO directors. Accounting for non-harvester and harvester director heterogeneity improves the adjusted R^2 of models for direct, equity and total compensation, with the median fixed effect on equity and total compensation clearly appearing to be different from zero. When I consider IPO director departures, I find that only the last harvester director departure has a statistically significant effect on equity and on direct compensation.

4.2 Last Harvester Director Departure Treatment Effect

When I take into account the endogeneity of departure and isolate a treatment effect separate from the selection effect potentially present in the main sample, I again find that the departure of the last harvester director has a significant treatment effect on CEO compensation, when contrasting harvester funded firms with last harvester departure against matched harvester funded firms without last harvester departure. The marginal effect (i.e., the interaction term of

last harvester departure and of treated firm) of this treatment is significant for equity compensation (and therefore total compensation) in the year of departure, where it contradicts the hypothesized relationship. However, the *F*-test of the overall treatment effect (i.e., the sum of the terms constituting the difference-in-differences) is only significant for direct compensation, both in the year of last harvester departure and in the change in compensation from years before that departure to years after the departure. Here, however, the sum of the coefficients is positive, consistent with the hypothesized relationship.

IPO directors appear to affect CEO compensation, but the hypotheses are only partially supported. Being a harvester director or not, and being a VC or PEG harvester director, affects CEO compensation, and even individual differences within those groups appear to have an effect on CEO compensation. Among IPO directors, only the departure of the last harvester director appears to have a treatment effect, but even then it only agrees with the theoretically motivated hypothesis with regard to direct compensation, but not with regard to equity compensation.

4.3 Unresolved Matters

Beatty & Zajac (1994) find "that the levels of monitoring observed are inversely related to the levels of managerial incentives used," which is contrary to my findings, which suggest instead that a decline in monitoring intensity (the departure of the last harvester director) is associated with a decline in equity compensation. On the other hand, Guthrie, Sokolowsky, & Wan (2012), in their critique of Chhaochharia & Grinstein (2009), find the following: "our results indicate that (i) board independence does not affect the level of CEO pay; (ii) compensation committee independence causes CEO pay to increase; and (iii) the increase in CEO pay occurs only in the presence of blockholder directors or high institutional ownership concentration, both of which are considered to be monitoring substitutes. These results imply that the compensation committee independence requirement has had the unintended consequence

of increasing CEO pay, especially in firms with effective shareholder monitoring…. These empirical findings stand in sharp contrast to the prediction of the managerial power hypothesis that director independence effectively curbs rent extraction in the form of excessive CEO pay."

My dataset is only a convenience sample, and any inferences are limited by differences between it and the overall population of EGC IPO firms (and the larger set of all IPO firms). Out of 1,700 EGC IPO firms from 1996 through 2010, I only have 303 firms in my main estimation sample, and 154 firms in my final matched sample. Even among the firms in my sample, I am missing a number of CEO-firm-year observations, as well as annual information regarding the IPO directors. It is possible that I have false negatives, meaning that some IPO directors and CEOs have departed within the sample but their departure dates are not properly captured in either the BoardEx or ANNCOMP datasets. This would underestimate the effect of director departure and the control for CEO turnover, respectively. Likewise, I have firm-years with missing CEO compensation data. These concerns can only be alleviated by hand-collecting the missing data. Furthermore, my dataset ends in 2013, which truncates and omits many potential IPO director exits after 2013, which become more relevant particularly for those IPOs near the 2010 end of the range of years in my sample.

Before last harvester IPO director departure, are CEOs paid more than their industry peers? While the departure of arguably the strongest and most likely director to push to constrain CEO compensation does not lead to a plausible explanation of why compensation should decline, it at least provides room for the board to make an adjustment based on a comparative logic.

Cronqvist & Fahlenbrach (2009) examine a number of different types of blockholders and their varying effects on CEO compensation. I only examine two: VC-firms and PEG funds. Also, I only examine them if they are represented on the board at IPO, and I only hypothesize

regarding the effect of their departure after IPO. One other aspect that I have not controlled for is a potential substitution effect, where harvester departures from the board are replaced by other blockholders that have been found to be active and to have an effect on CEO compensation.

Chhaochharia & Grinstein (2009) examine the effect on CEO compensation of several other monitoring mechanisms, namely concentration of institutional holdings, exogenous regulatory shocks (Sarbanes-Oxley) to director independence, corporate governance quality (as measured by Institutional Shareholder Services). They also examine the effect of accounting conventions (i.e., the change in expensing of stock options due to FAS123 and its successors) on stock options as part of CEO compensation. My work has not focused on or controlled for these alternative explanations and factors, though they are at least potentially present in my dataset as well.

Berry et al. (2006) examine CEO compensation in a panel of IPO firms up to 11 years after IPO, and they differentiate their sample among firms that are acquired, delisted or surviving. My work has not focused on these differences, though they potentially could explain both one motivation for IPO director, and particularly harvester director, departure and also changes in CEO compensation.

I examine direct and equity-linked CEO compensation. However, direct compensation includes both salary and cash bonuses, which have different theoretical motivations and have been shown empirically to react differently to changing conditions in a firm's environment. Likewise, equity-linked compensation is an aggregate dollar amount representing very different forms of compensation, including stock, restricted stock, stock options, phantom stock, LTIPs and even the cash value of retirement benefits. Prior literature has examined each of these components of CEO compensation in finer detail, both theoretically and empirically. It is

possible the predictions and hypotheses concerning director departure, particularly last harvester director departure, may differ substantially with regard to each of these sub-components of CEO compensation, and that a similarly detailed examination, using my empirical setting, might uncover varied patterns of effects and of changes in such compensation.

Lastly, at least one other alternative explanation grounded in optimal contracting literature is that, particularly in externally funded IPOs, the observed changes in CEO compensation are contractually set as part of the negotiations between the entrepreneurs and the external financiers. In that case, what is observed is caused indirectly due to those prior contractual terms (or their expiration). I have not examined the CEO employment agreements and external funding agreements to systematically disprove, or find support for, that alternative explanation. What is clear from the current analysis is that the treatment effect appears to be isolated to coincide with the departure of the last harvester IPO director. Given the departure of multiple harvester directors from some firms, this would require either that such a contingency be required by all the harvester directors, or at least no later than the last investment round with new harvester directors, or that the determinants of such a contingency also determine which harvester director would be last to depart.

Moreover, prior literature has shown that not all investments by harvester firms (VCs and PEGs) correspond to a harvester director joining the board. I do not examine the effect on CEO compensation of such non-board-participating harvester firms completely harvesting their investments, even though prior literature finds evidence of the parallel situation that harvester directors do not depart from boards until they also completely harvest their investments, which is the setting that I do examine. Finding a similar change in CEO compensation in situations of complete harvest without harvester director participation could strengthen the argument for the

alternative explanation, as while such harvesters still actively influence CEOs and management, they obviously do not have board participation and cannot directly influence compensation practices in the same way as harvester directors, but potentially rely more on initial bargaining and contracting.

In this initial effort to re-examine previously hypothesized governance-compensation relationships using the novel empirical setting of "harvester" IPO director departures, I find partial support for a plausibly causal relationship between changes in governance and changes in CEO pay. However, I find just as much room for future empirical and theoretical improvements in understanding this governance-CEO compensation relationship.

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6. TABLES

Table 1. Descriptive Statistics and Correlations of Main (Unmatched) Sample

	Mean	Std. Dev.	1	2	3	4	5	6	7	8
1. *Direct Compensation ($MM)*	0.560	0.319								
2. *Equity Compensation ($MM)*	2.583	4.102	0.172**							
3. *Total Compensation ($MM)*	3.121	3.948	0.267**	0.986**						
4. *Non-Harvester Departure this yr (1/0)*	0.151	0.401	-0.014	-0.022	-0.020					
5. *Not Last Harvester Departure this yr (1/0)*	0.039	0.194	-0.023	0.028	0.022	0.002				
6. *Last Harvester Departure this yr (1/0)*	0.035	0.184	-0.044*	-0.041*	-0.044*	0.043*	-0.038†			
7. *CEO Turnover this yr (1/0)*	0.125	0.331	-0.078**	0.109**	0.101**	0.044*	-0.026	-0.023		
8. *Lag LN Assets (LN of MM)*	5.795	1.243	0.341**	0.294**	0.341**	-0.011	-0.014	-0.040*	0.049*	
9. *Lag Tobin's Q (ratio)*	3.373	5.575	-0.088**	0.167**	0.137**	-0.026	0.008	-0.013	0.025	-0.103**

Statistics for main regression sample used for Table 1 through Table 5. N=2,483, except for CEO Turnover this year (N=2,194).
†, *, ** Denote significance at the 10%, 5%, and 1%, levels, respectively.

CEO compensation as reported in BoardEx or ExecuComp (in millions of dollars). Direct compensation includes salary plus cash bonus. Equity compensation is "equity-linked" compensation, as reported, which includes stock, options, LTIPS and other equity-based components of compensation. Total compensation is the sum of direct and equity compensation. Where two of three variables are reported, the third is calculated from the two reported values. If two variables are missing, that observation is excluded.

Non-Harvester Departure this year is an indicator for the departure during the fiscal year of an IPO director (but not affiliated with a VC or PEG fund), an IPO director being on the board of directors as of the IPO of the firm.

Not Last Harvester Departure this year is an indicator for the departure during the fiscal year of an IPO director affiliated with a VC or PEG fund (a "Harvester" director), but not the last such "Harvester" to depart from the board after the IPO.

Last Harvester Departure this year is an indicator for the departure during the fiscal year of an IPO director affiliated with a VC or PEG fund (a "Harvester" director), where the departure is of the last such "Harvester" to depart from the board after the IPO.

CEO Turnover this year is an indicator for a new CEO during the fiscal year (i.e., the CEO listed at fiscal year-end is not the same as the one listed in the prior fiscal year-end. Strictly speaking, any interim CEOs starting and departing in between fiscal year ends are not captured in this data generation process).

Lag LN Assets is the lagged value of the natural log of total assets.

Lag Tobin's Q is the lagged value of the Tobin's Q ratio of the market value of assets to book value of assets (calculated as the market value of equity plus book value of debt divided by the book value of equity plus the book value of debt).

Table 2. Univariate Analysis of CEO Compensation[1] Around Last Harvester Departure

	Pre Last Harvester Departure		Post Last Harvester Departure		Pre-vs-Post 2-sample Tests	
	Mean	Median	Mean	Median	*t*-stat	*p*-value
No CEO change in firm sample	Obs=850		Obs=109			
Direct Compensation	0.554	0.483	0.563	0.481	0.61	0.545
Equity Compensation	1.879	0.938	2.204	0.953	-0.90	0.370
Total Compensation	2.440	1.529	2.699	1.387	-0.75	0.452
CEO change in firm sample	Obs=1195		Obs=329			
Direct Compensation	0.566	0.498	0.560	0.500	0.32	0.750
Equity Compensation	3.018	1.164	2.949	1.839	0.27	0.785
Total Compensation	3.539	1.786	3.506	2.314	0.13	0.894
No CEO change vs. CEO change 2-sample Tests	*t*-stat	*p*-value	*t*-stat	*p*-value		
Direct Compensation	-0.83	0.409	-0.77	0.439		
Equity Compensation	-6.61	<0.001	-1.84	0.068		
Total Compensation	-6.60	<0.001	-2.09	0.038		

Means shown rounded from actual, unrounded values; their related t-statistics and p-values are from the unrounded estimates. Statistics for main regression sample used for Table 1 through Table 5, N=2,483.

[1] CEO compensation as reported in BoardEx or ExecuComp (in millions of dollars). Direct compensation includes salary plus cash bonus. Equity compensation is "equity-linked" compensation, as reported, which includes stock, options, LTIPS and other equity-based components of compensation. Total compensation is the sum of direct and equity compensation. Where two of three variables are reported, the third is calculated from the two reported values. If two variables are missing, that observation is excluded.

Table 3. Average Harvester Effects on CEO Compensation

DV=CEO Compensation[1]	(1) Total	(2) Total	(3) Direct	(4) Direct	(5) Equity	(6) Equity
Harvester indicator						
VC/PEG director	0.986**		-0.296***		1.409***	
	(0.467)		(0.028)		(0.509)	
Harvester category						
VC director		0.986**		-0.296***		1.409***
		(0.467)		(0.028)		(0.509)
PEG director		-1.123**		-0.120***		-0.832
		(0.545)		(0.032)		(0.591)
Control variables						
Lagged Tobin's Q	0.025	0.025	-0.000	-0.000	0.034	0.034
	(0.041)	(0.041)	(0.002)	(0.002)	(0.044)	(0.044)
Lagged ln Total Assets	0.830***	0.830***	0.060***	0.060***	0.787***	0.787***
	(0.205)	(0.205)	(0.012)	(0.012)	(0.222)	(0.222)
Observations	2,483	2,483	2,483	2,483	2,483	2,483
Adj. R^2	0.356	0.356	0.535	0.535	0.320	0.320
F-Tests:						
VC director = PEG director		*F*-stat 169.78 *p*-val <0.001		*F*-stat 196.71 *p*-val <0.001		*F*-stat 188.89 *p*-val <0.001

This table follows Cronqvist & Fahlenbrach (2009) "Large shareholders and corporate policies" Table 3, Columns 3 and 6. Its purpose is to test whether the presence of "Harvester" directors affects CEO compensation, and whether VC directors have a different effect from PEG directors within the Harvester category.

[1] CEO compensation as reported in BoardEx or ExecuComp (in millions of dollars). Direct compensation includes salary plus cash bonus. Equity compensation is "equity-linked" compensation, as reported, which includes stock, options, LTIPS and other equity-based components of compensation. Total compensation is the sum of direct and equity compensation. Where two of three variables are reported, the third is calculated from the two reported values. If two variables are missing, that observation is excluded.

Standard errors, clustered by firm, shown in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 4. Director Fixed Effects and CEO Compensation

	F-tests on fixed effects for						*N*	*Adj*	*Median*
	non-harvesters		*VCs*		*PEGs*		*Obs.*	R^2	*FE*
A. Actual Compensation									
Total Compensation							2,483	.356	
Total Compensation	3.46***	(<.0001, 16)					2,483	.368	1.295
Total Compensation	3.42***	(<.0001, 16)	1.00	(0.478, 38)	3.94***	(0.008, 3)	2,483	.370	0.399
Direct Compensation							2,483	.535	
Direct Compensation	2.94***	(<.0001, 16)					2,483	.541	-0.001
Direct Compensation	2.99***	(<.0001, 16)	1.27	(0.123, 38)	1.09	(0.353, 3)	2,483	.544	0.010
Equity Compensation							2,483	.320	
Equity Compensation	2.75***	(<.0001, 16)					2,483	.329	1.261
Equity Compensation	2.74***	(<.0001, 16)	0.93	(0.596, 38)	4.83***	(0.002, 3)	2,483	.332	0.586
B. Natural Log of Actual Compensation									
Total Compensation							2,483	.436	
Total Compensation	3.62***	(<.0001, 16)					2,483	.447	0.298
Total Compensation	3.63***	(<.0001, 16)	1.54**	(0.019, 38)	1.36	(0.253, 3)	2,483	.453	0.212
Direct Compensation							2,483	.540	
Direct Compensation	3.29***	(<.0001, 16)					2,483	.548	0.028
Direct Compensation	3.34***	(<.0001, 16)	1.28	(0.122, 38)	0.18	(0.908, 3)	2,483	.549	0.060
Equity Compensation							2,483	.345	
Equity Compensation	2.03***	(<.0001, 16)					2,483	.350	0.689
Equity Compensation	2.02***	(<.0001, 16)	1.35*	(0.078, 38)	1.67	(0.172, 3)	2,483	.355	0.684

This table follows Cronqvist & Fahlenbrach (2009) "Large shareholders and corporate policies" Table 4, Panel C. Its purpose is to test whether the heterogeneity of individual "Non-Harvester" and individual "Harvester" directors (grouped by VC and PEG affiliation) differs in the effect upon CEO compensation.

Each regression includes lagged Tobin's Q and lagged natural log of total assets as time-varying firm-level controls, as well as fiscal year and firm fixed effects.

Reported are the *F*-tests for the joint significance of the non-harvester director fixed effects, VC director fixed effects, and PEG director fixed effects. For each *F*-test, I report the value of the *F*-statistic, the *p*-value, and the number of constraints. I also include the median estimated fixed effects.

Standard errors clustered by firm. *** p<0.01, ** p<0.05, * p<0.1

Table 4 has an additional restriction: there are more individual directors than firm-year observations in the full sample, resulting in insufficient degrees of freedom when including all individual fixed effects. So, to produce Table 4, I only include non-harvesters that are present at least at two or more firms in the sample (this is actually consistent with what Cronqvist & Fahlenbrach do, as they follow Bertrand & Schoar's moving dummy variable methodology that requires multi-firm individuals for model identification). I do not similarly restrict the VC and PEG directors in my sample. Theoretically, harvesters are much more influential than non-harvesters, so only including non-harvesters at two or more firms I oversample those non-harvesters that are selected by firms as being more valuable and influential, similar to harvesters. Empirically, given the much smaller pool of VC and PEG directors, I choose to maximize the information retained in the Table 4 sample, even though it might lead to some sampling / selection bias. Identification of harvester fixed effects is achieved because they are not perfectly collinear with firm and year fixed effects (i.e., they are present more than one year in a firm, but not for all the years that firm appears in the sample, so they are distinct from time-invariant firm fixed effects covering all the years in the sample, and they are distinct from cross-sectionally invariant year fixed effects, as they are only in one or a few firms in the cross-sectional sample of each firm year). It is possible that the sampling / selection bias is at least partly responsible for the strongly significant *F*-tests on non-harvester directors and also for the lack of strong significance on the harvester directors.

Table 5. Regressions of CEO Compensation on One Year Prior IPO Director Departure and CEO Turnover

DV = CEO Compensation:	(1) Equity	(2) Equity	(3) Equity	(4) Equity	(5) Equity	(6) Direct	(7) Direct	(8) Direct	(9) Direct	(10) Direct
(A) Non-harvester departures last year	0.136 (0.234)				0.111 (0.228)	-0.003 (0.012)				-0.000 (0.012)
(B) Harvester departures last year	-0.199 (0.297)					0.017 (0.018)				
(C) Not last Harvester departures last year		0.567 (0.557)			0.560 (0.546)		0.047 (0.029)			0.048 (0.030)
(D) Last Harvester departure last year		-1.041*** (0.272)		-1.040*** (0.263)	-1.003*** (0.269)		-0.020 (0.022)		-0.028 (0.021)	-0.025 (0.021)
(E) CEO turnover this year			0.933*** (0.329)	0.918*** (0.330)	0.913*** (0.327)			-0.095*** (0.022)	-0.095*** (0.022)	-0.095*** (0.022)
Control variables										
Lagged Tobin's Q	0.045 (0.053)	0.045 (0.053)	0.045 (0.055)	0.044 (0.055)	0.046 (0.055)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)
Lagged ln Total Assets	0.847*** (0.250)	0.826*** (0.248)	0.846*** (0.251)	0.825*** (0.250)	0.825*** (0.251)	0.067*** (0.014)	0.067*** (0.014)	0.068*** (0.014)	0.067*** (0.014)	0.067*** (0.014)
Observations	2,162	2,162	2,162	2,162	2,162	2,162	2,162	2,162	2,162	2,162
Adj. R^2	0.345	0.348	0.351	0.353	0.353	0.538	0.539	0.547	0.547	0.547
F-tests:										
(A) = (B)	*F*-stat 0.70 *p*-val 0.403					*F*-stat 0.82 *p*-val 0.367				
(C) = (D)		*F*-stat 7.64*** *p*-val 0.006			*F*-stat 7.60*** *p*-val 0.006		*F*-stat 4.44** *p*-val 0.036			*F*-stat 4.98** *p*-val 0.026

This table tests several hypotheses regarding the effect of IPO director departures on CEO compensation, while controlling for size, growth, and CEO turnover. Models (1) tests whether harvester and non-harvester director departures affect compensation equally. Model (2) tests whether last harvester departure and other than last harvester departures affect compensation equally. Model (3) examines whether CEO turnover affects CEO compensation. Model (4) examines whether the last harvester departure effect is robust to inclusion of a CEO turnover effect. Model (5) again tests whether last harvester departure and other than last harvester departures affect compensation equally, while controlling for non-harvester departures and for CEO turnover. Models (6) through (10) repeat the analysis for direct compensation.

Standard errors, clustered by firm, shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

[1]CEO compensation as reported in BoardEx or ExecuComp (in millions of dollars). Direct compensation includes salary plus cash bonus. Equity compensation is "equity-linked" compensation, as reported, which includes stock, options, LTIPS and other equity-based components of compensation.

Untabulated regressions of Total compensation (the sum of Direct and Equity compensation) result in identical patterns of signs and significance as with Equity compensation Models (1) to (5) above, with only minor increases in estimated coefficients.

Table 6. Descriptive Statistics and Correlations of Matched Sample

	Mean	Std. Dev.	1	2	3	4	5	6	7	8	9	10	11	12
1. *Direct Compensation ($MM)*	0.504	0.260												
2. *Equity Compensation ($MM)*	2.836	4.335	0.247**											
3. *Total Compensation ($MM)*	3.276	4.114	0.314**	0.993**										
4. *Post Last Harvester Departure (1/0)*	0.457	0.498	0.145**	0.027	0.042									
5. *POST CEO change (1 .. 5)*	0.693	0.950	0.033	0.104**	0.114**	0.252**								
6. *Non-Harvester Departure this yr (1/0)*	0.126	0.372	-0.043	-0.035	-0.039	-0.059*	-0.025							
7. *Not Last Harvester Departure this yr (1/0)*	0.052	0.223	-0.053*	-0.026	-0.032	-0.139**	-0.064*	0.032						
8. *Last Harvester Departure this yr (1/0)*	0.108	0.311	-0.034	-0.068**	-0.069**	-0.319**	-0.058*	0.048†	-0.041					
9. *CEO Turnover this yr (1/0)*	0.133	0.340	-0.096**	0.098**	0.094**	0.086**	0.427**	0.045	-0.008	-0.056*				
10. *LN Sales (LN of MM)*	5.147	1.568	0.226**	0.251**	0.269**	0.192**	0.254**	-0.043	-0.052*	-0.033	0.043			
11. *LN ROA (LN of ratio)*	0.015	0.295	0.029	0.074**	0.076**	0.056*	0.088**	-0.043	-0.029	-0.038	-0.006	0.526**		
12. *Lag LN Assets (LN of MM)*	5.742	1.306	0.330**	0.331**	0.356**	0.203**	0.242**	-0.055*	-0.006	-0.020	0.054*	0.713**	0.215**	
13. *Lag Tobin's Q (ratio)*	3.530	6.687	-0.032	0.202**	0.186**	-0.147**	-0.087**	-0.013	-0.007	-0.045†	0.009	0.014	-0.038	-0.063*

Statistics for difference-in-differences regressions matched sample used for Tables 6 through Table 8. N=1,416, except for LN Sales (N=1,403), LN ROA (N=1,387), and Lag Tobin's Q (N=1,338).
†, *, ** Denote significance at the 10%, 5%, and 1%, levels, respectively.

POST Last Harvester Departure this year is an indicator for the years including, and after, the departure during the fiscal year of an IPO director affiliated with a VC or PEG fund (a "Harvester" director), where the departure is of the last such "Harvester" to depart from the board after the IPO.

POST CEO Change is a categorical variable indicating the number of changes in CEO that have happened in the sample since IPO up through that fiscal year. For example, a firm with three distinct CEOs will have firm-year observations with POST CEO Change equal either to 0, 1, or 2 to indicate the two changes in CEO. In the sample, the maximum number of CEO changes for any given firm is 5 (i.e., in the sample, at least one firm had 6 different CEOs).

LN Sales is the natural log of revenues (reported in millions).

LN ROA is the natural log of 1 plus ROA, with ROA calculated as EBITDA divided by total assets.

Table 7. Last Harvester Departure Treatment Effect on Post-vs-Pre Departure Levels of Compensation[1]

	DV = CEO Compensation[2]:	(1) ln(Total)	(2) ln(Total)[3]	(3) ln(Total)[4]	(4) ln(Total)	(5) Total	(6) Direct	(7) Equity	(8) Total	(9) Direct	(10) Equity
					Panel A. Difference-in-Differences Regression Estimates						
β_1	Treated								0.884	0.194***	0.650
									(0.822)	(0.058)	(0.850)
β_2	POST last Harvester departure								-0.533	-0.019	-0.485
									(0.368)	(0.021)	(0.392)
β_3	Treated*POST last Harvester departure	0.017	-0.047	-0.018	-0.102	-0.612	0.022	-0.732	-0.510	0.030	-0.649
		(0.164)	(0.127)	(0.126)	(0.129)	(0.447)	(0.027)	(0.474)	(0.569)	(0.031)	(0.603)
	POST 1st CEO change	0.246	0.234**	0.136	0.153	0.525	-0.032	0.651	0.545	-0.032	0.668
		(0.183)	(0.117)	(0.117)	(0.120)	(0.415)	(0.025)	(0.445)	(0.510)	(0.033)	(0.537)
	POST 2nd CEO change	-0.164	0.129	-0.036	0.026	0.080	-0.101***	0.148	0.092	-0.102**	0.159
		(0.284)	(0.170)	(0.166)	(0.175)	(0.594)	(0.035)	(0.622)	(0.658)	(0.042)	(0.667)
	POST 3rd CEO change	-0.031	-0.068	-0.292	-0.050	0.193	-0.164***	0.430	0.215	-0.166***	0.450
		(0.353)	(0.213)	(0.206)	(0.224)	(0.781)	(0.041)	(0.820)	(0.886)	(0.052)	(0.914)
	POST 4th CEO change	-0.631	-0.152	-0.332	-0.157	-1.132	-0.144**	-0.880	-1.133	-0.147**	-0.879
		(0.460)	(0.273)	(0.260)	(0.280)	(0.945)	(0.057)	(1.000)	(1.170)	(0.071)	(1.194)
	POST 5th CEO change		-0.284	-0.273	-0.121	-1.400	-0.163	-1.253	-1.539*	-0.165***	-1.371*
			(0.280)	(0.245)	(0.261)	(1.744)	(0.113)	(1.852)	(0.784)	(0.042)	(0.822)
(A)	Sales*PRE last Harvester departure	0.013	0.146***	0.194***							
		(0.057)	(0.055)	(0.051)							
(B)	Sales*POST last Harvester departure	-0.000	0.177***	0.220***							
		(0.058)	(0.057)	(0.052)							
(C)	ROA*PRE last Harvester departure	0.059	0.065	-0.080							
		(0.192)	(0.187)	(0.178)							
(D)	ROA*POST last Harvester departure	0.062	0.073	0.138							
		(0.166)	(0.188)	(0.209)							
(E)	Lag Tobin's Q*PRE last Harvester departure				0.002	0.032	0.001	0.038	0.032		0.038
					(0.010)	(0.042)	(0.002)	(0.046)	(.047)		(.051)
(F)	Lag Tobin's Q*POST last Harvester departure				0.071**	0.369***	0.003	0.400***	0.419***		0.442***
					(0.028)	(0.099)	(0.006)	(0.106)	(0.127)		(0.137)
	Lag Tobin's Q									0.001	
										(0.002)	
(G)	Lag LN Assets*PRE last Harvester departure				0.259***	0.877***	0.046***	0.881***			
					(0.059)	(0.200)	(0.011)	(0.218)			
(H)	Lag LN Assets*POST last Harvester departure				0.271***	0.836***	0.044***	0.840***			
					(0.062)	(0.211)	(0.011)	(0.230)			
	Lag LN Assets								0.854***	0.045***	0.860***
									(0.249)	(0.013)	(0.265)
	Observations	866	1,365	1,377	1,338	1,338	1,338	1,338	1,338	1,338	1,338
	Adj. R^2	0.509	0.476	0.448	0.434	0.367	0.487	0.337	0.367	0.488	0.338

Table 7. Last Harvester Departure Treatment Effect on Post-vs-Pre Departure Levels of Compensation[1]

DV = CEO Compensation[2]:	(1) ln(Total)	(2) ln(Total)[3]	(3) ln(Total)[4]	(4) ln(Total)	(5) Total	(6) Direct	(7) Equity	(8) Total	(9) Direct	(10) Equity
Panel B. Tests of Coefficients and of Margins Contrasts										
Margins Tests: (E) = (F)				$\chi 2$ 5.63**	$\chi 2$ 10.63***	$\chi 2$ 0.13	$\chi 2$ 10.73***	*F*-stat 8.90***		*F*-stat 8.28***
				p-val 0.018	*p*-val 0.001	*p*-val 0.721	*p*-val 0.001	*p*-val 0.003		*p*-val 0.005
Margins Tests: (G) = (H)				$\chi 2$ 0.50	$\chi 2$ 0.49	$\chi 2$ 0.36	$\chi 2$ 0.43			
				p-val 0.479	*p*-val 0.483	*p*-val 0.549	*p*-val 0.510			
Test[5] (β_1+ β_2+ β_3)|treated=0								*F*-stat 0.26	*F*-stat 24.58***	*F*-stat 0.00
vs. (β_1+ β_2+ β_3)|treated=1								*p*-val 0.612	*p*-val <0.001	*p*-val 0.999

[1] Difference-in-differences regressions, using a 1-to-1 matched sample of harvester-backed IPOs (harvesters being directors representing VC and PEG funds that invested in the IPOs and needing to "harvest" their investment post-IPO), of a pre-vs-post last harvester exit period difference in compensation, differenced between treated and control firms. All firms in the matched sample were chosen from the initial EGC IPO sample used in Tables 1 through 5. All firms in the sample have harvesters on the board, but matched control firms have not yet experienced the last harvester departure. 80 "Treated" firms in the original sample experienced the departure of the last harvester from their board during the sample period, but 3 treated firms were unmatched by 2-digit SIC code and were excluded from the matched sample. Each of the remaining 77 treated firms (representing 726 firm-year observations) was matched with a control firm by 2-digit SIC code and the condition that the matched firm-years had to include the same fiscal year as the fiscal year in which the last harvester departed from a treated firm's board (e.g., for a treated firm with a last harvester departure in 2008, the matched control firm's firm-years had to include 2008). 43 firms were also matched on IPO year and on CEO turnover. Another 29 firms were also matched only on CEO turnover without regard to match by IPO year. The final 5 firms were matched without regard to IPO year or CEO turnover. In cases where a treated firm was initially matched with multiple control firms, the best match was chosen by minimizing the difference in ROA in the fiscal year that coincided with the last harvester departure year. For each firm, all firm-years with available data were included. The final combined sample contained 1,482 firm-year observations. Sample size varies by Model due to missing information for that Model's variables.

[2] CEO compensation as reported in BoardEx or ExecuComp (in millions of dollars). Direct compensation includes salary plus cash bonus. Equity compensation is "equity-linked" compensation, as reported, which includes stock, options, LTIPS and other equity-based components of compensation. Total is the sum of Direct plus Equity compensation.

[3] Model (2) differs only from Model (1) by not including tenure.
[4] Model (3) differs from Model (2) only in using year fixed effects instead of industry-year fixed effects.
Models (1)-(2) include industry-year and firm fixed effects, while Models (3)-(10) include year and firm fixed effects. Chhaochharia & Grinstein (2009) do not use a matched sample, thus industry-year fixed effects are appropriate. My sample matches by industry in all matched firms, so while year effects are still relevant, industry effects are less so. Untabulated regressions including industry-year fixed effects indicate qualitatively similar results, though standard errors increase. Notably, the only coefficients with changes in significance are that POST 1st CEO change becomes significant in Models (8) and (10). Otherwise, all margins and margins contrasts tests maintain the same (in-)significance as in the tabulated results.
Model (1) includes an estimated effect for tenure, which is insignificant (β=-0.018, s.e.=0.012, *p*-val 0.128) and is not carried forward into other models.
Models (1)-(7) follow the Chhaochharia & Grinstein (2009) regression specification, which excludes estimation of main effects for interaction terms, whereas Models (8)-(10) include them.
Models (1)-(4) follow the Chhaochharia & Grinstein (2009) DV specification of log of compensation, whereas Models (5)-(10) revert to the untransformed DVs, consistent with the rest of the analysis.
Models (1)-(7) follow the Chhaochharia & Grinstein (2009) regression specification, interacting pre-vs-post period dummy with controls. However, as the Assets coefficients are not different in Models (5)-(7), and Tobins Q coefficients are not different in Model (6), those interactions are removed in Models (8)-(10), respectively.
Models (1)-(7) estimated using *REGHDFE* command in Stata 14.2; Models (8)-(10) estimated using *REGRESS* command in Stata 14.2.

Sales is the natural log of firm revenues. *ROA* is the natural log of 1 plus the ratio of EBITDA divided by total assets. *Lag LN Assets* is the natural log of lagged total assets.

[5] Contrast of margins is unable to estimate test of contrasts. As a rudimentary substitute, I present an *F*-test of the main and interaction coefficients. Untabulated margins tests of (A) = (B) and of (C) = (D) (i.e., the Chhaochharia & Grinstein (2009) control variables) reject that the PRE-vs-POST estimated coefficients are equal for Sales but not for ROA. The Chhaochharia & Grinstein (2009) control variables are replaced after Model (3), so results of these tests are omitted from the table.

Standard errors in parentheses: clustered by firm-years for Models (1)-(7) following the Chhaochharia & Grinstein (2009) specification, and clustered by firm for Models (8)-(10). Clustering by firm-year treats separate firm-years as independent, while clustering by firm considers the intertemporal correlation across firm-years for each firm. Untabulated regressions firm for Models (8)-(10) clustering by firm-year indicate qualitatively similar results, with smaller standard errors, as expected.
*** p<0.01, ** p<0.05, * p<0.1

Table 8. Last Harvester Departure Treatment Effect on Year After Departure Compensation[1]

	DV = CEO Compensation:	(1) Equity	(2) Direct	(3) Total
β_1	Treated	0.696 (0.548)	0.148*** (0.028)	0.905* (0.527)
	Non-harvester departures last year	-0.402 (0.264)	-0.011 (0.018)	-0.402 (0.258)
	Not last Harvester departures last year	0.194 (0.655)	0.015 (0.042)	0.135 (0.598)
β_2	Last Harvester departure last year	0.008 (0.234)	-0.035 (0.022)	-0.007 (0.226)
β_3	Treated* Last Harvester departure last year	-0.898** (0.386)	0.012 (0.027)	-0.867** (0.373)
	CEO turnover this year	1.050** (0.451)	-0.089*** (0.022)	0.875** (0.414)
Control variables				
	Lagged Tobin's Q	0.043 (0.051)	0.001 (0.002)	0.038 (0.047)
	Lagged ln Total Assets	0.937*** (0.278)	0.047*** (0.013)	0.933*** (0.259)
	Observations	1,338	1,338	1,338
	Adj. R^2	0.340	0.494	0.368
	Test[2] $(\beta_1+\beta_2+\beta_3)$|treated=0 vs. $(\beta_1+\beta_2+\beta_3)$|treated=1	*F*-stat 0.11 *p*-val 0.740	*F*-stat 19.06*** *p*-val <0.001	*F*-stat 0.00 *p*-val 0.946

This table, using a difference-in-differences matched sample framework, re-examines the hypothesis regarding the effect of last Harvester departure on CEO compensation, while controlling for size, growth, and CEO turnover. Models (1)-(3) present year of last harvester departure-vs-other years difference in compensation, differenced between treated and control firms.

[1] The sample is the same matched sample as I used for the Table 7 difference-in-differences treatment effect regressions. The Models replicate the analysis from Table 5, with the addition of an interaction for Treated in order to compute difference-in-differences between treated and matched control firms. "Treated" firms experienced the departure of the last harvester from their board during the sample period, while matched control firms have not.

[2] Contrast of margins is unable to estimate test of contrasts. As a rudimentary substitute, I present an *F*-test of the main and interaction coefficients.

Standard errors, clustered by firm, shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1