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Corporate Governance and Firm Value

Law Working Paper N° 824/2025
February 2025

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Abstract

Scholars have long debated how the governance structure of a firm affects the value of the firm. Confronted with conflicting theoretical arguments, corporate governance scholars turned to empirical evidence. An approach that has become increasingly popular over the past few decades to assess the effect of governance changes over longer time horizons is to measure the effect of these changes on the firm's Tobin's Q. As we show, Q regressions are theoretically unfounded and can lead to biased results. In a within-firm design, Q regressions are inferior to event studies and calendar time portfolio regressions: they suffer from all of their limitations, but are less good in adjusting for market movements, add noise, and pick up treatment effects unrelated to firm value. In a cross-sectional setting, results derived from standard Q regressions shed little, if any, light on corporate governance controversies.

Keywords: Corporate Governance, Firm Value, Tobin's Q, Q Regressions, Long-Term Event Studies, Calendar Time Portfolio Regression, Exit, State Contingency

JEL Classifications: G30, G32, G34, G38, K22

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INTRODUCTION

Scholars have long debated how the governance structure of a firm affects the value of the firm. The theoretical debate dates to at least 1974, when William Cary analyzed the provisions of Delaware's corporate law and argued that they harmed shareholders.[1] It did not take long for another theorist, Ralph Winter, to disagree. Unlike Cary, Winter did not base his argument on his assessment of specific provisions of Delaware law. Instead, he argued that competition among states engenders a competitive race among states, with each state trying to offer superior laws. Delaware, as the winner of such a race, Winter reasoned, must hence have the best law.[2]

Similar debates among corporate law theorists soon ensued about other aspects of corporate governance. Among the topics analyzed are independent directors, director terms, withhold campaigns, majority voting, empty voting, time-phased voting, proxy access, reimbursement rules in proxy contests, various antitakeover defenses, anti-activist poison pills, staggered boards, lockups, greenmail, one share-one vote rules, controlling shareholders, long-term shareholders, shareholder quality, institutional shareholders, index funds voting rights, hedge fund activism, pension fund activism, director exculpation statutes, exclusive forum bylaws, and the Sarbanes-Oxley Act.

Confronted with conflicting arguments, corporate governance scholars turned to empirical evidence to try to resolve the theoretical debates. An approach that has become increasingly popular over the past few decades to assess the effect of governance changes over longer time horizons[3] is to measure the effect of these changes on Tobin's Q.[4]

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[1] William L. Cary, *Federalism and Corporate Law: Reflections upon Delaware*, 83 YALE L. J. 663, 666 (1974).
[2] Ralph K. Winter, Jr., *State Law, Shareholder Protection, and the Theory of the Corporation*, 6 J. LEGAL STUD. 251, 256 (1977).
[3] Over shorter time horizons, the most common empirical methodology to evaluate corporate governance was the short-term event study. But because of the limitations of short-term event studies, they have been supplemented with, if not replaced by, longer-term studies. The principal reasons for focusing on long term effects is to capture the *actual* effects of the governance change to the extent that these effects have materialized during the period over which returns are included (and are hence impounded in the stock price) rather than merely the market's expectations as to future effects as they stand at the time the governance change is announced. These methodologies thus tackle scenarios in which, for whatever reason, initial market expectations prove to be incorrect or the market is slow to adjust to new information. The flip-side, of course, is that, over the longer time period, many factors other than the treatment of interest will affect stock prices of treated firms, making the identification of the treatment effect less precise. This creates two separate problems. First, the signal-to-noise ratio is much worse for long-run studies (i.e., they have less power), since over the long run noise may swamp any true effect of interest (and if that effect actually takes place in the short run, it may even be detectable through a short-term event study) (See John Y. Campbell, Andrew W. Lo & A. Craig MacKinlay, *The Econometrics of Financial Markets* 175–77 (1997)). Second, long term studies may end up detecting an effect that is not really ascribable to the treatment of interest.
[4] Appendix A lists articles employing *Q* regressions published in the top three finance journals—the Journal of Finance, the Journal of Financial Economics, and the Review of Financial Studies—between 2012 and 2022. Below is a small selection of additional noteworthy contributions: Thomas Stratmann & J. W. Verret, *Does Shareholder Proxy Access Damage Share Value in Small Publicly Traded Companies?*, 64 STAN. L. REV. 1431

We show that *Q* regressions are theoretically unfounded and can lead to biased results.[5] In a within-firm design, *Q* regressions are inferior to long-term event studies and calendar time portfolio regressions (together, "long term return studies"). In a cross-sectional setting, results derived from standard *Q* regressions shed no light on corporate governance controversies. A major implication of our analysis is that, going forward, *Q* regressions, in either setting, should not be performed and should not be published.

In Part I, we briefly explain how *Q* regressions are designed and why they are fundamentally flawed. Part II discusses long term return studies. While these methodologies have some inherent shortcomings, the same shortcomings, or aggravated versions of the same shortcomings, are also present in *Q* regressions. Running *Q* regressions on top of calendar time portfolio regressions or long-term event studies would thus neither add to the robustness nor negate the results of the latter. In Part III, we speculate on how *Q* regressions ever obtained and retained their popularity among financial economists.

(2012) (examining the effect on proxy access); Stuart L. Gillan & Laura T. Starks, *Corporate Governance Proposals and Shareholder Activism: The Role of Institutional Investors*, 57 J. FIN. ECON. 275 (2000) (studying the effect of shareholder proposals); Jonathan M. Karpoff & Paul H. Malatesta, The Wealth Effects of Second-Generation State Takeover Legislation, 25 J. FIN. ECON. 291 (1989) (studying the effect of antitakeover statutes); Jay Cai, Jacqueline L. Garner & Ralph A. Walkling, *A Paper Tiger? An Empirical Analysis of Majority Voting*, 21 J. CORP. FIN. 120 (2013); Stuart Rosenstein & Jeffrey G. Wyatt, Outside Directors, *Board Independence, and Shareholder Wealth*, 26 J. FIN. ECON. 175 (1990); Jerold B. Warner, Ross L. Watts & Karen H. Wruck, *Stock Prices and Top Management Changes*, 20 J. FIN. ECON. 461 (1988); Victoria B. McWilliams & Nilanjan Sen, *Board Monitoring and Antitakeover Amendments*, 32 J. FIN. & QUANTITATIVE ANALYSIS 491 (1997); April Klein, & Emanuel Zur, *Entrepreneurial Shareholder Activism: Hedge Funds and Other Private Investors*, 64 J. FIN. 187 (2009); Alon Brav, Wei Jiang, Frank Partnoy & Randall Thomas, *Hedge Fund Activism, Corporate Governance and Firm Performance*, 63 J. FIN. 1729 (2008); Lucian A. Bebchuk & Alma Cohen, *The Costs of Entrenched Boards*, 78 J. FIN. ECON. 409 (2005); K.J. Martijn Cremers & Simone M. Sepe, *The Shareholder Value of Empowered Boards*, 68 STAN. L. REV. 67 (2016); Yakov Amihud, Markus Schmid & Steven Davidoff Solomon, *Settling the Staggered Board Debate*, 166 U. PA. L. REV. 1475 (2018); Lucian A. Bebchuk, Alon Brav & Wei Jiang, *The Long-Term Effects of Hedge Fund Activism*, 115 COLUM. L. REV. 1085 (2015); Robert Daines, *Does Delaware Law Improve Firm Value?*, 62 J. FIN. ECON. 525 (2001); David Yermack, *Higher Market Valuation of Companies with a Small Board of Directors*, 40 J. FIN. ECON.185 (1996); Charles P. Himmelberg, R. Glenn Hubbard & Darius Palia, *Understanding Determinants of Managerial Ownership and the Link Between Ownership and Performance*, 53 J. FIN. ECON. 353 (1999); Randall Morck, Andrei Shleifer & Robert W. Vishny, *Management Ownership and Market Valuation: An Empirical Analysis*, 20 J. FIN. ECON. 293 (1988); Larry H. P. Lang & René M. Stulz, *Tobin's q, Corporate Diversification, and Firm Performance*, 102 J. POL. ECON. 1248 (1994); John E. Core, Robert W. Holthausen & David F. Larcker, *Corporate Governance, CEO Compensation, and Firm Performance*, 51 J. FIN. ECON. 371 (1999); April Klein, *Firm Performance and Board Committee Structure*, 41 J. L. & ECON. 275 (1998); Paul A. Gompers, Joy L. Ishii & Andrew Metrick, *Corporate Governance and Equity Prices*, 118 Q. J. ECON. 107 (2003); Diane Del Guercio, Laura Seery & Tracie Woidtke, *Do Boards Pay Attention When Institutional Investors "Just Vote No"?*, 90 J. FIN. ECON. 84 (2008); Kenneth R. Ahern & Amy K. Dittmar, *The Changing of the Boards: The Impact on Firm Valuation of Mandated Female Board Representation*, 127 Q. J. ECON. 137 (2012); Hariom Manchiraju & Shivaram Rajgopal, *Does Corporate Social Responsibility (CSR) Create Shareholder Value? Evidence from the Indian Companies Act 2013*, 55 J. ACCT. RSCH. 1257 (2017).

[5] Throughout the paper, we use "*Q* regressions" to refer to analyses where *Q* serves as the dependent variable. In the finance and economics literature, there is a well-established strand of research that instead employs *Q* as a proxy for a firm's investment opportunities, using it as an *independent* variable. *See, e.g.,* Fumio Hayashi, *Tobin's Marginal q and Average q: A Neoclassical Interpretation*, 50 Econometrica 213 (1982); Andrew B. Abel & Janice C. Eberly, *A Unified Model of Investment Under Uncertainty*, 84 Am. Econ. Rev. 1369 (1994); Ryan H. Peters & Lucian A. Taylor, *Intangible Capital and the Investment-q Relation*, 123 J. Fin. Econ. 251 (2017). Our criticisms are not directed at that literature.

I. The Flaws in Q Regressions

Q is a metric developed by James Tobin, a Nobel Prize Laureate in Economics, and is defined as the market value of a company divided the replacement cost of the company's assets.[6] *Q* regressions come in two variants, cross-sectional and within firm. In a cross-sectional study, which does not include firm fixed effects, one regresses the levels of *Q* of treated and untreated firms in a single or in multiple periods against a treatment indicator and various controls. "Within firm" *Q* regressions track changes for several years following a governance change. In a typical "within firm" study, a firm's *Q*[7] is regressed against a treatment indicator, a vector of firm indicators (firm fixed effects), a vector of time (or industry-by-time) indicators, and possibly other control variables. This research design effectively compares the post-treatment changes in the *Q* of treated firms to same-period changes in *Q* in firms that either have not (yet) been treated or have been treated a longer time ago. In either case (cross-sectional or within firm), if the estimate of the coefficient for the treatment indicator is positive, this is taken as evidence that treatment enhanced firm value; if it is negative, it is interpreted as evidence that treatment reduced value.[8]

Using differences in *Q* to assess governance changes has numerous problems. The most fundamental of these problems is that there is no theoretical foundation for using changes in *Q* over the long term to evaluate the effect of treatment on firm value. Treatments that raise firm value can, over the long term, either raise or lower *Q*. In addition, governance changes can affect *Q* for reasons independent of their effect on firm value. A finding that a treatment caused a decrease (or increase) in *Q* over time, or that it failed to do so, thus does not warrant *any* conclusion about the effect of the treatment on firm value.[9] Likewise, the fact that past treatment is associated

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[6] Tobin was principally interested in the macroeconomic significance of *Q* and suggested that a high *Q* would encourage companies to invest more. Tobin's *Q* is also used a measure of a firm's growth opportunities. For an excellent discussion of how *Q* became used as a measure for firm value in finance, see Robert Bartlett & Frank Partnoy, *The Misuse of Tobin's q*, 73 VAND. L. REV. 353 (2020).

[7] In some studies, the dependent variable is the log(*Q*) rather than *Q*. See, e.g., Davidson Heath, Daniele Macciocchi, Roni Michaely & Matthew Ringgenberg, *Do Index Funds Monitor?*, 35 REV. FIN. STUD. 91 (2021). All our criticisms of *Q* regressions apply equally to those studies.

[8] Econometrically, within-firm designs are implemented either by adding firm fixed effects to a regression specification where the outcome variable is measured in levels or by running a first differences regression where the dependent variable is *the change* in the outcome variable of interest over the relevant period (and the explanatory variable of interest is the change in the treatment indicator over that same period). Within-firm designs have a major advantage: they control for any time-invariant firm characteristic that could affect the outcome variable. That is, in a within-firm *Q* regression, one examines directly how a change in a firm's governance characteristics relates to a change in *that firm's* Tobin's *Q*.

[9] Most *Q* regressions simplify the calculation of *Q* by assuming that the firm debt and its preferred stock has a market value equal to their book value and that the replacement cost of its assets is as well equal to the book value. Bartlett and Partnoy critique this use of *Q* for the measurement problems it entails and suggest alternate ways to measure *Q*. Bartlett & Partnoy, *supra* note 6. While we agree with most of their arguments, our criticisms of *Q* go beyond those of Bartlett and Partnoy. Even if we could measure *Q* accurately and address the other

with different levels of *Q* does not indicate that the treatment affected firm value. We first examine the use of *Q* in within-firm studies. We then discuss cross-sectional studies.

A. Treatment and Long-Term Changes in Q

In efficient markets, treatments (or other "shocks") that affect stock prices will have an immediate impact on *Q*. A positive shock, for example, will increase the *Q* numerator generally in expectation that future profits will increase. But a change in expected future profits typically has no instantaneous effect on the *Q* denominator. Over the very short term (say a day), *Q* thus moves in the same direction as the change in firm value induced by treatment.

Q, of course, is not observable on a daily basis and, in any case, *Q* regressions examine the evolution of *Q* over much longer time periods. Over a longer term, however, factors other than returns need to be taken into account. It can be shown (assuming that the market value of debt is equal to its book value)[10] that the change in *Q* that a firm experiences in the period between *t* and τ>*t*, is equal to

$$\Delta Q_{\tau,t} = r_{\tau,t}\frac{E_t}{A_\tau} - \frac{P_{\tau,t}}{A_\tau}Q_t + \frac{CM_{\tau,t}}{A_\tau}(1 - Q_t), \qquad (1)$$

where $r_{\tau,t}$ is the percentage total return (including dividends) experienced by the firm's stock between t and τ, E_t is the firm's market value of equity at *t*, A_τ is the book value of the firm's assets at τ, $P_{\tau,t}$ is the dollar value of the profits realized by the firm between *t* and τ, and $CM_{\tau,t}$ is the net dollar value of the capital market transactions that the firm engages in between *t* and τ (dollars raised by issuing debt and equity minus dollars paid out in dividends, interest repayment, payment of debt principal, and stock repurchases).[11]

Of these three factors, the only factor related to the effect of treatment on firm value in a relevant way is returns. Realized profits, of course, are also related to firm value—in the sense that profitable firms are more valuable—but note that an increase in $\frac{P_{\tau,t}}{A_\tau}$ *lowers* $\Delta Q_{\tau,t}$. In a similar vein, capital market transactions that result in cash inflows mechanically drive *Q* closer to 1—whereas cash outflows mechanically drive *Q* further from 1—, in addition to any direct effect the capital market transaction may

deficiencies levelled against *Q* by Bartlett and Partnoy, *Q* is, for the reasons we discuss, not appropriate to evaluate governance changes.

[10] *Q* regressions in the studies we reviewed always calculate *Q* based on this assumption.

[11] See Appendix B.

have on the value of the debt and equity that were outstanding before the transaction.

Looking at *Q* regressions through the lens of equation (1) thus yields three insights. First, it shows that ΔQ is a noisier measure than returns when what we are after is the effect of treatment on firm value. Empirically, the amount of noise is substantial. In a sample consisting of all CRSP-Compustat firms with *shrcd=11* and spanning fiscal years 2000 to 2020, the R-squared in a regression of one-year changes in *Q* to stock returns over that period was 0.29. Over a three-year period, it was 0.23. An over a five-year period, it was 0.20.

Second, equation (1) shows *Q* regressions will generate biased estimates if treatment affects $\frac{P_{\tau,t}}{A_\tau}$ or $\frac{CM_{\tau,t}}{A_\tau}$.[12] This is not some trivial concern. For one, treatments that affect returns are very likely to affect (at some point) the realization of profits (since higher returns reflect higher expected profits and vice versa), and the respective effects on $\Delta Q_{\tau,t}$ will be in opposite directions. Whether a treatment that increases firm value and profits will raise or lower $\Delta Q_{\tau,t}$ depends, for firms with a pre-treatment level of *Q* exceeds 1 (as is commonly the case)[13], on whether the marginal profits attributable to treatment are realized more slowly or faster than pre-treatment profits. (In the former case, $\Delta Q_{\tau,t}$ will be higher than but for treatment, in the latter, it will be lower.)[14] As a theoretical matter, therefore,

[12] In addition, treated and control firms may differ in their realization of expected profits and the proclivity to engage in capital market transactions for secular reasons. This problem could be mitigated through proper matching or, at a minimum, by confirming that the pre-treatment evolution in *Q* of treatment and control firms followed parallel trends.

[13] In the same sample, approximately 82% of the firm-year observations had a Tobin's *Q* in excess of 1.

[14] As an illustration, consider a firm with $100 in cash. Pre-treatment, the firm planned to invest $100 in a project that yields a payoff of $60 at the end of years 1 and 2. For simplicity, assume that the firm has no debt, pays no dividends, any payoff received is held in cash (or not further invested in a manner that changes *Q*), the replacement value of the investment in each project declines in a straight line over the life of the project, the discount rate is 0%, and markets are efficient (i.e. the firm equity value at all times is equal to the cash holdings at the end of year 2). Table 1, Panel A describes the evolution of *Q* for that project. Assume that a treatment introduced between years 0 and 1 induces firm to substitute alternative X or Y for the initial project. Each alternative would raise firm value to the same extent. But alternative X, which involves back-loaded profits (relative to the initial project) would raise period 1 *Q* while alternative Y, which involves front-loaded profits, would lower it.

Although, as we noted, in the aggregate, there is a positive empirical relationship between total stock returns over a 1-, 3-, and 5-year period and changes in *Q* over that period, such a relationship merely indicates that the average shock experienced by firms during the period, for all shocks and firms combined, has the same directional effect on returns and *Q* from introduction until the end of the period. As explained, whether a treatment that generates positive returns raises *Q* over the corresponding period depends both on the type of treatment and the length of time. Based on the effect of the average shock (that may have occurred any time in a 1-, 3- and 5-year period) until the end of the period, one thus cannot conclude with confidence that a particular treatment at issue resulted in a positive relationship between returns and changes in *Q* over a specific time interval studied.

the effect of a treatment that raises firm value on $\Delta Q_{\tau,t}$ is indeterminate.[15] In addition,[16] many corporate governance treatments more plausibly have material effects on realized profits and capital market transactions[17] than

Table 1: Evolution of Firm *Q*

Panel A			
Year	0	1	2
Project Payoff	0	60	60
Cash (cumulative payoff)	0	60	120
Replacement Value	100	50	0
Equity Value	120	120	120
Asset Value	100	110	120
Q	1.2	1.091	1

Panel B			
Alternative X Payoff	0	60	65
Cash (cumulative payoff)	0	60	125
Equity Value	120	125	125
Asset Value	100	110	125
Q	1.2	1.136	1
Alternative Y Payoff	0	65	60
Cash (cumulative payoff)	0	65	125
Equity Value	120	125	125
Asset Value	100	115	126
Q	1.2	1.086	1
Alternative Z Payoff	0	65	55
Cash (cumulative payoff)	0	65	120
Equity Value	120	120	120
Asset Value	100	115	120
Q	1.2	1.043	1

[15] Our discussion so far has assumed that markets are efficient. But *Q* fares even worse in inefficient markets. If markets are inefficient, stock prices may not accurately reflect the effect of treatment until effects are realized. But, as most firms have a *Q* above 1, a treatment that increases firm value will *only* tend to raise *Q* to the extent that the market (correctly) anticipates *future* profits from treatment that have *not yet* materialized. To the extent that stock prices reflect only realized profits, a treatment that increases firm value lowers *Q* for firms with a *Q* above 1 as such realized profits raise the *Q* numerator (through their effect on returns) and the *Q* denominator (by increasing the book value of assets) by the same amount. If one of the reasons to examine the long-term evolution of *Q* is that short-term market reactions do not accurately reflect the actual impact of treatment, as may be the case in inefficient markets, then the long-term evolution of *Q* is even less likely to correspond to the effect of treatment on firm value.

[16] An additional flaw of *Q* regressions is that they generate estimates for the value effect of a governance change *as a percentage of the Q denominator* rather than as a percentage of their pre-treatment value. *Q* regressions thus present a misleading picture when a treatment has differential effects on high *Q* and low *Q* firms. Consider two firms (A and B), both with a pre-treatment equity value of $100 million, no debt, and asset values of $50 million and $25 million respectively, respectively. A treatment that increases the value of A by 10% and decreases the value of B by 10% – and hence does not affect firm value, on average – would raise the *Q* of A from 2 to 2.2 and lower the *Q* of B from 4 to 3.6, resulting in an average decline in *Q* of 0.1 – or 10% of the *Q* denominator.

[17] If these capital market transactions, in the aggregate, result in net capital inflows ($CM_{\tau,t}>0$), the asset base of a firm grows by more than the profits generated by the firm; if they result in net capital outflows, it grows by less. The direct effect of these capital market transactions on *Q*[17] is to change both its numerator and its denominator by the same amount. Transactions that increase capital hence move *Q* closer to 1 and transactions that reduce capital move *Q* further away from 1. Cf. Phillip H. Dybvig & Mitch Warachka, Tobin's q Does Not Measure

on firm value.[18] For example, many treatments that have been studied (for example, staggered boards, antitakeover statutes, hedge fund activism, and poison pills) fall broadly in the class of measures that change management's exposure to market discipline. Some theorists have posited that increasing exposure to market discipline may increase firm value (by discouraging empire building);[19] others have argued that it decreases firm value (by making management more myopic).[20] But whatever the effect on firm value, if any, increased exposure to market discipline could plausibly affect the timing of profits (by making management pursue more projects with a short-term horizon)[21] and capital market transactions (by inducing firms to return more value to shareholders through dividends or share repurchases).[22]

The potential confounding effect introduced by examining the relation between treatments and changes in *Q*, rather than the relation between treatments and returns, is highlighted by Table 1. In a sample of CRSP-Compustat merged observations involving firms with *shrcd*=11 and spanning fiscal years 2000-2020, $\frac{P_{t,t-1}}{A_t}$ and $\frac{CM_{t,t-1}}{A_t}$ are cross-sectionally correlated with all sorts of governance features and accounting measures. To be sure, the association is not necessarily causal. But the two variables are clearly not random and the fact that they are correlated with so many other covariates places the burden on those who employ *Q* regressions both to show that the governance treatment at issue does not affect $\frac{P_{\tau,t}}{A_\tau}$ and $\frac{CM_{\tau,t}}{A_\tau}$,— and hence that using *Q* regressions merely induces noise, and no systematic biases—and to take extraordinary care that treated and untreated

Firm Performance: Theory, Empirics, and Alternatives, Working Paper (2015). As Dybvig and Warachka have pointed out, the principle that a firm maximizes its value by investing in all projects with a positive net present value (NPV) implies that firms with a high marginal *Q* will both lower their *Q* and increase shareholder value as they acquire capital to invest in additional positive NPV projects. For analytical purposes, we treat the raising of new capital and the investment of such capital as separate transactions. The raising of new capital is a capital market transaction which lowers *Q* if *Q* exceeded 1 and has no direct effect on returns. The investment of capital generally does not affect the *Q* denominator and hence leads to returns and changes in *Q* that have the same sign.

[18] For example, Ahern & Ditmar, *supra* note 4, find that Norway's law requiring a quota of female directors reduced firm *Q*, which they interpret as a reduction in firm value. But they also find that treated firms became more acquisitive. Acquisitions (at the purchase method), unless fully funded by retained earnings, can be decomposed as a capital market transaction (raising the funds for the acquisition) followed by a purchase of the acquisition target. These transactions have no direct effect on firm value but, as explained, would be expected to reduce *Q* of firms with *Q* >1.

[19] Michael C. Jensen, *Agency Costs of Free Cash Flow, Corporate Finance, and Takeovers*, 76 AM. EC. REV. 323, 325-328 (1986). Frank H. Easterbrook & Daniel R. Fischel, *The Proper Role of a Target's Management in Responding to a Tender Offer*, 94 HARV. L. REV. 1161 (1981); Frank H. Easterbrook & Daniel R. Fischel, *Takeover Bids, Defensive Tactics, and Shareholders' Welfare*, 36 BUS. LAW. 1733 (1981).

[20] Martin Lipton, *Corporate Governance in the Age of Finance Corporatism*, 136 U. PA. L. REV. 1, 23-24 (1987); Jeremy Stein, *Takeover Threats and Managerial Myopia*, 96 J. POL. ECON. 61, 62-67 (1988).

[21] *See Id.*

[22] See Jensen, *supra* note 19.

firms do not differ along any of the variables that are correlated with $\frac{P_{\tau,t}}{A_\tau}$ and $\frac{CM_{\tau,t}}{A_\tau}$.

Table 2: How are $\frac{P_{t,t-1}}{A_t}$ and $\frac{CM_{t,t-1}}{A_t}$ associated with other governance or accounting measures?

	$\frac{P_{t,t-1}}{A_t}$	$\frac{CM_{t,t-1}}{A_t}$
Governance features		
Dual Class	8.11	-4.33
Log(firm age)	36.07	-37.98
Delaware incorporated	-17.57	15.35
Nevada incorporated	-4.82	5.13
Staggered board	-1.98	3.81
Board size	1.05	-2.98
Fraction board women	2.94	-2.67
Accounting measures		
Book value of assets	3.71	-3.46
Liabilities to assets	20.44	-16.88
Current liabilities to assets	-0.59	0.49
Current liabilities to current assets	13.00	-12.06
Sales to assets	26.18	-16.98
Tangible assets to assets	17.35	-16.55
Goodwill to assets	17.96	-18.27
Inventory to assets	17.15	-20.57
Operating income to interest	25.42	-14.83
Cost of goods sold to inventory	-1.93	2.88

Note: each of the cells contains the t-statistic for the estimate of β in a univariate regression $y_{it} = \alpha + \beta x_{it-1} + \varepsilon_{it}$, where y_{it} equals the variable at the top of the respective column, and x_{it-1} equals the one-year lag of the variable in the left-most column in the corresponding row. Standard errors are clustered at the firm (*permco*) level. In all regressions except for those where the independent variable corresponds to a board characteristic, the sample comprises all observations from CRSP-Compustat with *shrcd=11* over years 2000-2020, and the independent variable was defined on the basis of CRSP-Compustat or —in the case of *dual class* or *log(firm age)*—CRSP Securities Monthly data. In the case of the regressions where the independent variable consists of *Staggered board*, *Board size*, and *Fraction board women*, the sample spans only the subset of the observations corresponding to firms that were part of the S&P 1500 as of the respective year, and years 2007-2020. Those independent variables were derived from the *ISS Directors US* database, available on WRDS. All financial ratios are winsorized at the extreme 1% tails. For financial ratios where the numerator is a flow variable and the denominator is a stock variable (e.g., *sales to assets*), the denominator is measured as of

the year before the year when the numerator is measured. *Dual Class* is a dummy equal to 1 if and only if *shrcls* is non-empty in CRSP. *Log(firm age)* is the natural logarithm of the difference between the year of the observation and the first year in which the relevant *permco* appears in CRSP with a non-missing stock price. *Delaware [Nevada] incorporated* is a dummy equal to 1 if and only if *incorp* equals Delaware [Nevada] in CRSP Compustat. *Staggered board* is a dummy that equals 1 if and only if the maximum value of the difference between *year term ends* and *year* across all directors corresponding to the firm-year in *ISS Directors US* is 3 or 4. Board size is the number of directors for the relevant firm-year in *ISS Directors US*. *Fraction board women* is the fraction of directors for the relevant firm-year in *ISS Directors US* for which *female* equals Yes. *Book value of assets* and *Assets* are *at*. *Liabilities* is *lt*. *Current liabilities* is *lct*. *Current assets* is *act*. Sales is *sale*. *Tangible assets* is *ppent*. *Goodwil* is *gdwl*. *Inventory* is *invt*. *Operating income* is *oibdp*. *Interest* is *xint*. *Cost of good sold* is *cogs*.

This leads us to the third and most important point. Even if the problems noted before turn out to be relatively minor—there is minimal noise and good reasons to believe that treatment would not affect profits or net capital market transactions—equation (1) highlights that one should never employ *Q* regressions in within-firm studies. To perform within-firm *Q* regressions, one must have a meaningful number of firms whose treatment status changes over the period of analysis as well as information about the share prices for these firms. That means one can directly study how that change in treatment status affected stock returns for those firms. But returns over a given period are a better measure of changes in firm value over that period than changes in *Q* over that period. *The only relevant way* in which changes in *Q* reflect changes in firm value is through returns. That is, *Q* regressions do not look at a different way to assess changes in firm value (such as changes in profitability) that, while less precise than returns, may pick up some facet of firm value than is not reflected in returns. Rather, they start with returns and then transform the return measure in a way that, in the best case, adds noise (by multiplying returns with $\frac{E_t}{A_\tau}$ and then adding $-\frac{P_{\tau,t}}{A_\tau}Q_t + \frac{CM_{\tau,t}}{A_\tau}(1-Q_t)$) and, in the worst case, generates bias (if treatment is correlated with these factors). Thus, just like one would never use a contaminated data set derived from a reasonably clean data set when the clean data set is available, one should never use *Q* regressions to evaluate treatment when returns are observable.[23]

[23] Albeit less common than *Q* regressions, some papers examine the effect of governance changes on ratios other than *Q*, such as the ratio of profits to the book value of assets, commonly known as return on assets or ROA. See, e.g., Roie Hauser, *Busy Directors and Firm Performance: Evidence from Mergers*, 128 J. FIN. ECON. 16 (2018), Nishant Dass, Omesh Kini, Vikram Nanda, Bunyamin Onal & Jun Wang, *Board Expertise: Do Directors from Related Industries Help Bridge the Information Gap?*, 27 REV. FIN. STUD. 1533 (2014), Ronald W. Masulis & Emma Jincheng Zhang, *How Valuable Are Independent Directors? Evidence From External Distractions*, 132 J. FIN. ECON. 226 (2019). These other ratio studies share many of the problems of *Q* regressions. Among others, they are distorted by capital market transactions; and treatments that raise firm value and profits can nevertheless result in reduced return on assets because profits, once realized, increase the denominator of these ratios. Beyond that, return on assets and similar ratios do not even purport to capture effects on firm value that are not reflected

B. The Use of Q in Cross-Sectional Studies

But what about cross-sectional *Q* regressions? In cross-sectional *Q* regressions, only one observation for each firm may be available or—even if the data is structured as a panel—the number of firms that experience changes in treatment status may be small. Given such data constraints, conducting a long-term return study may not be feasible. Despite all their problems, may *Q* regressions be the best one can do and at least better than conducting no study at all?

Our answer is no. The reason harks back to where we started: *Q* regressions lack a proper theoretical foundation. While cross-sectional *Q* regressions do not explicitly measure the effect of treatment on firm value, they do compare treated and untreated firms. The premise of these studies is that treated firms, but for the treatment, would resemble untreated control firms and hence that differences in the outcome variable are attributable to the treatment.

Cross-sectional studies typically provide little or no information of when the treatment occurred. For example, one of the earliest of these studies, by Randall Morck, Andrei Shleifer and Robert Vishny, compares the *Q* of 371 firms with different levels of insider ownership in a single year (1980) and finds that *Q*—the authors' measure of the market valuation of the firm—rises with ownership for low levels of ownership, falls for intermediate levels, and rises again for high levels.[24] Without any information on insider ownership prior to 1980, one must assume that many firms in the data set may have had relatively stable ownership for the preceding 10 or even 20 years.

Assume that, at the point when a treatment was originally assigned, that treatment did, indeed, materially affect shareholder value. How likely is it that such impact would still be detectable in cross-sectional comparisons of *Q* potentially decades after the introduction of treatment?

To answer this question empirically, we identified, for each year *y* between 2000 and 2020, the U.S. publicly traded firms in the CRSP dataset in the top decile of lagged 1-year stock returns for the relevant year. These

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in profits over the period of study. Return on assets thus systematically misvalue treatments that affect profits that fall outside the period of study—and they do so even more if such long-term increased profits come at the expense of reduced shorter-term profits. Return on assets and other ratios based purely on accounting data, however, have one redeeming quality: they do not rely on stock prices. They can thus be calculated for firms where stock price data are not available, say privately-held companies. In addition, to the extent that returns (even over the long term) are not regarded as a fully reliable measure of changes of firm value, other accounting ratio studies can be used, perhaps as a supplement to return studies, to shed light on the effect on treatment on firm value through a metric that contains information not reflected in returns.

[24] Morck, Shleifer & Vishny, *supra* note 4.

"high-return firms of year *y*"[25] had average lagged returns of 159% in year *y* (compared to -2.1% returns for non-high-return firms of year *y*)—an excess return orders of magnitude higher than the excess return most governance changes would generate. For each year *t=y, y+1, y+2,…* we then compared the average *Q* of the high-return firms of year *y* with that of all other U.S. publicly traded firms as of year *t*. Specifically, using annual data spanning the period 2000-2020, we estimated the following cross-sectional event study specification:

$$Tobin'sQ_{it} = \alpha + \sum_{\tau=0}^{20} \lambda_\tau TopDecile_{it\tau} + \gamma_t + \varepsilon_{it}, \qquad (2)$$

where *Tobin's Q* is trimmed at the 1% tails, γ_t is a vector of year fixed effects, and $TopDecile_{it\tau}$ is a dummy equal to 1 if and only if firm *i's* lagged 1-year stock returns in year $t-\tau$ were in the top decile of the distribution of stock returns for that year. Figure 1 describes the results.[26]

Unsurprisingly, during the year when the relevant high-return firms experience the very positive shock (year 0 in the horizontal axis), those firms have an average *Q* that is much higher than the *Q* of the firms that did not experience such a positive shock. However, as time goes by, the difference in average *Q* between high-return firms and non-high-return firms steadily shrinks. By year 5, the average *Q* of high-return firms is statistically and economically indistinguishable from the average *Q* among firms that did not experience such a favorable shock. In other words, in this scenario, "treatment firms" suffered a massive positive shock; and yet, only five years later, cross-sectional comparisons of *Q* offer no evidence of such a shock.

That even massive shocks, on average, have no lasting effect on *Q* is due to the fact that the effect of a treatment that raises firm value on ΔQ is indeterminate. Shocks can affect cash flows in many plausible ways, and the long-term directional effect on *Q* depends on the exact way in which shocks affect cash flows. Without having more precise knowledge of how shocks affect cash flows, therefore, one cannot infer from a cross-sectional difference in *Q* in (say) year t+10 that high-*Q* (low-*Q*) firms have experienced a positive (negative) shock in year t. But this is exactly what cross-sectional studies are trying to do.

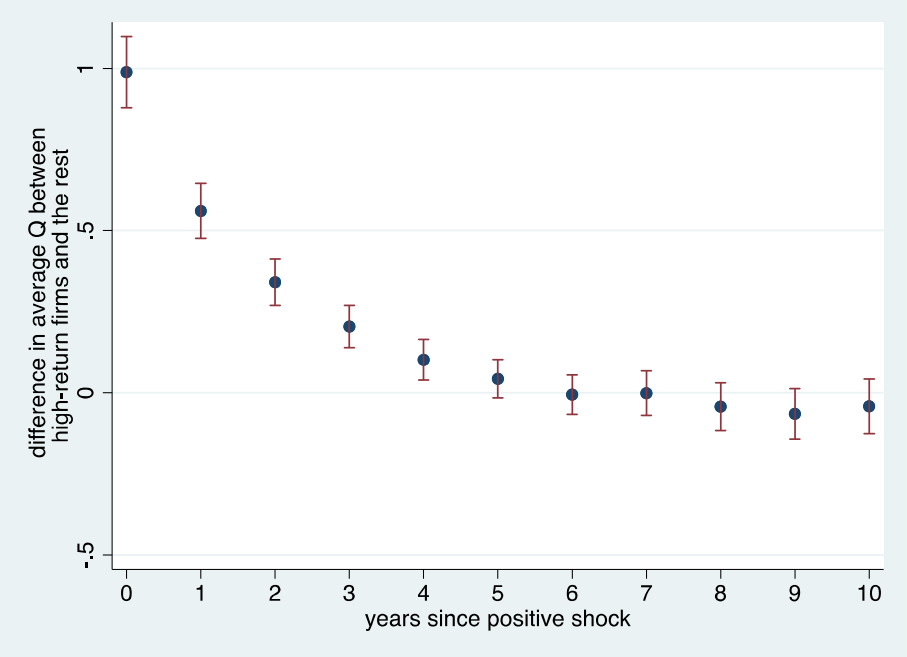
Put differently, given the time between treatment and measurement of *Q* that is likely to have passed in cross-sectional studies, *Q* levels are not just a noisy proxy for the returns from treatment, they are no proxy at all. Thus, even in cross-sectional studies, where it is not possible to observe

[25] We presume that firms experience such a positive stock return because market participants must have believed that those firms' prospects improved.

[26] The estimates corresponding to $\lambda_\tau, \tau \geq 11$ are unreported.

returns directly and long-term return studies are hence infeasible, *Q* regressions should not be employed as the next best alternative.

Figure 1: Long Term Effect of Shock on *Q*



Note: This figure reports the point estimates of the coefficients for $\lambda_\tau \in \{0,1,2,\ldots,10\}$ recovered by estimating the specification described in equation (2) using a sample comprising all firm-year observations in CRSP-Compustat from fiscal years 2000-2020 for which the CRSP variable *shrcd=11*. For each point estimate, the bars reflect 95% confidence bands calculated on the basis of standard errors that are two-way clustered at the firm and industry-by-year level (where industry is measured by 2-digit SIC codes). To mitigate the impact of outliers, *Tobin's Q* is trimmed at the 1% tails.

II. Long-Term Return Studies

In this Part, we briefly explain long-term event studies and calendar time portfolio regressions (collectively, long term return studies), the two other methodologies for examing the long term effect of governance changes on firm value, and identify limitations of these methodologies. We show that *Q* regressions suffer from identical or aggravated versions of these limitations.[27] Despite the limitations of long term return studies,

[27] A theoretical benefit of *Q* regressions over other studies is that *Q* regressions take account of the effect of treatment on shareholders *and creditors* as the *Q* numerator is the sum of the market value of the company's equity and debt. In practice, however, *Q* regressions employ the book value rather than the market value of debt because market values are not easily observable. This effectively assumes that treatment has no effect on debt

running *Q* regressions on top of them would thus neither add to the robustness of or negate their results. Rather, the limitations discussed in this Part present, if anything, additional reasons not to use *Q* regressions.

A. Long-Term Event Studies and Calendar Time Portfolio Regression Methodology

Similar to *Q* regressions, long-term event studies and calendar time portfolio regressions are designed to examine the effect of a treatment over several months or years. Unlike *Q* regressions, however, they do so by directly examing returns.[28]

Longer-term event studies start by calculating for each security *i* undergoing treatment the "buy-and-hold return" from time t_0 to time *T* (the event window, encompassing the time in which firm *i* was treated):

$$BHR_i(t_0,T)=\prod_{t=t0}^{T}(1+R_{it}),$$

where R_{it} is the return experienced by security *i* during period *t* (typically, a month). The methodology then calculates the buy-and-hold *abnormal* return by subtracting from $BHR_i(t_0,T)$ the buy-and-hold return experienced by an appropriate benchmark[29] from t_0 to T, $BHR_{Bi}(t_0,T)$:

$$BHAR_i(t_0,T)= BHR_i(t_0,T)- BHR_{Bi}(t_0,T).$$

The final step is to calculate the average of $BHAR_i(t_0,T)$ across all firms *i* experiencing the event of interest and to evaluate its statistical significance.

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values. See Bartlett & Partnoy, *supra* note 6, at 393. In practice, therefore, *Q* regressions, like return studies, only consider the effect of treatment on shareholders, rather than on broader measures of firm value. Of course, if the effect of treatment on the market value of debt were observable, it could be incorporated into event studies and calendar time portfolio regressions.

[28] This description of the long-run event study and calendar time portfolio regression methodologies follows S. P. Kothari & Jerold B. Warner, *Chapter 1 – Econometrics of Event Studies*, *in* HANDBOOK OF CORPORATE FINANCE: EMPIRICAL CORPORATE FINANCE, 23-24 (B. Espen Eckbo ed., 2007).

[29] For a discussion of how to identify that appropriate benchmark (which generally involves identifying other securities that are similar to security *i* in terms of their exposure to standard risk factors, industry, etc.), see John D. Lyon, Brad M. Barber & Chih-Ling Tsai, *Improved Methods for Tests of Long-Run Abnormal Stock Returns*, 54 J. FIN. 165 (1999). The literature on long run event studies has identified a laundry list of additional issues with the selection of a proper benchmark and developed best practices to handle them. Those issues include (see e.g., *id*. at 165-166): rebalancing bias (in a nutshell: when one uses benchmark portfolios to calculate the "normal" return, monthly rebalancing of that portfolio results in upward-biases in the calculation of the "normal" return); new listing bias (if the benchmark portfolio includes newly listed firms, that can lead to biases because newly listed firms tend to experience systematically different returns); skewness (when one uses a benchmark portfolio to estimate the counterfactual, one may end up with a skewed distribution of abnormal returns, since the return of a treated firm is more right-skewed than the return of the benchmark portfolio); and the problem of non-random treatment (if the firms that are treated are not random but, say, belong to particular industries, then one has to be particularly careful at constructing the control firm/benchmark portfolio). Often, the solution to these problems is to use single firms as opposed to benchmark portfolios to construct the counterfactual.

By comparison, in the calendar time portfolio regressions, one calculates, for each month during the sample period under analysis, the return of a portfolio that consists of firms that have experienced the event of interest at some point over the previous *N* months—where *N* is the number of months over which one wants to estimate the abnormal returns.[30] After calculating that vector of returns R_{pt}—which comprises one observation per calendar month *t* during the sample period[31]—one first subtracts from the portfolio returns during the relevant month the risk-free rate of return R_{ft} during that same month and then regresses that vector of net portfolio returns against a constant, the market return net of the risk-free rate (R_{mt} - R_{ft}) during that month, and typically the return on a set of additional factors (e.g., the Small-minus-Big and High-minus-Low factors developed by Fama and French[32]) during that month:

$$R_{pt} - R_{ft} = \alpha + \beta (R_{mt} - R_{ft}) + \gamma SMB_t + \delta HML_t + \varepsilon_{pt}.$$

The estimate of interest is the one corresponding to the coefficient for the constant (known as "Jensen's alpha"), which captures the portfolio's average monthly abnormal return.

B. Some Problems with Long-Term Return Studies

Extending the time horizon over which the effect of treatment is examined generates problems for long term studies that are not equivalently present in short-term event studies. While these problems should lead us to be cautious in drawing inferences from their results, they present no justification for using *Q* regressions.

1. Exit

Over longer periods of time, firms exit—they are acquired, go bankrupt, or delist for other reasons—and returns for these firms are no longer observable. For example, out of the approximately 4,200 U.S. publicly traded firms tracked by the CRSP database with *shrcd*=11 as December 31, 2009, 8.4% had exited within 1 year, 21.5% within three years, and 30.1% within five years.

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[30] Occasionally, the fact that a firm became subject to an event is not totally unexpected. One can handle the impact of anticipation of treatment on stock returns by adding stocks to the portfolio a number of months before the stocks are actually treated.

[31] Sometimes, the time series data $\{R_{pt}\}$ may have "holes" because no firm experienced an event during the relevant subperiod and thus no return data is available for the portfolio during certain calendar months.

[32] Eugene Fama & Kenneth French, *Common Risk Factors in the Returns on Stock and Bonds*, 33 J. FIN. ECON. 3 (1993).

Exit is often associated with substantial positive returns (in case of acquisition-related exits) or substantial negative returns (in case of financial distress-related exits). As Figure 2 illustrates, the lagged 12-month returns for firm-month observations from 2019 involving acquisition-related exits are substantially higher than the returns for firm-months not involving any exit, which in turn are substantially higher than the returns for firm-months involving distress-related exits. Overall, of all the U.S. firms in the CRSP database that exited during 2019, 71% exited due to being acquired and the rest for distress related reasons.[33] The average lagged 12-month returns among firm-month observations involving exits in connection with acquisitions (distress) were 38 percentage points higher (67 percentage points lower) than the corresponding returns among firm-month observations not involving exits. These differences are economically dramatic and statistically significant.

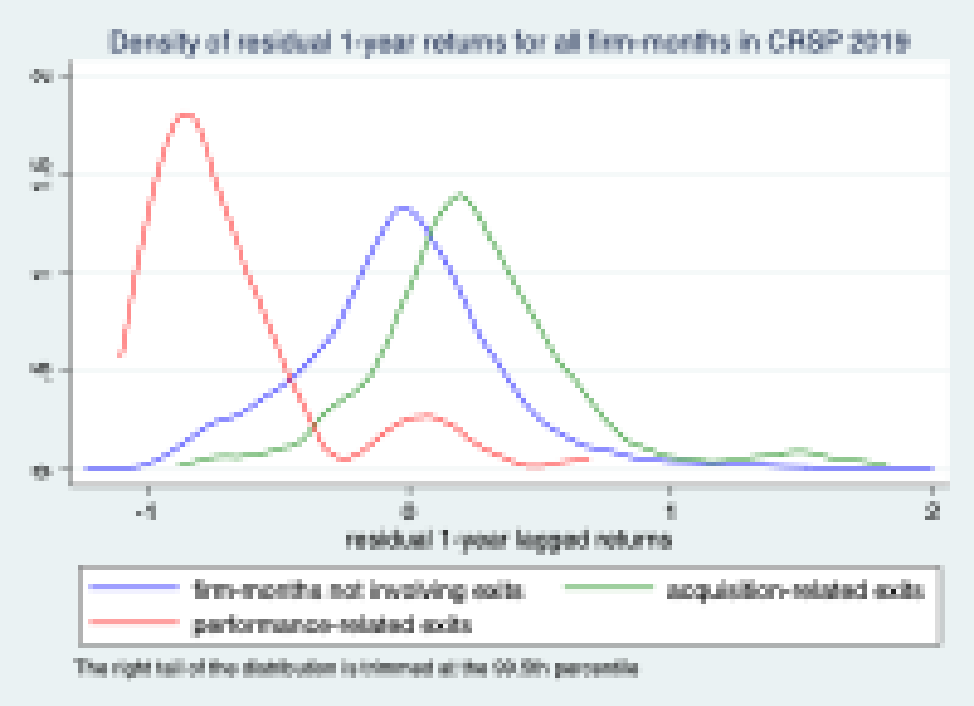
Moreover, treatment can affect the probability of exit.[34] Studies thus cannot just ignore firms that exited and base their assessment of the effects of treatment exclusively on firms that did not exit. But for firms that exit, any post-exit effects on firm value are unobservable. At most, one can follow firms that exited only up to the date of exit. This creates some inherent limitations for both long-term return studies.

But whatever problems exit generates for long-term return studies, they are aggravated for *Q* regressions. The measurement of the *Q* denominator and portions of the *Q* numerator depend on accounting data that are not observable on a daily, or even monthly, basis. Thus, in addition to not being observable post-exit, *Q* may also be impossible to calculate for the period in which exit occurs. *Q* regressions—which typically use data with annual frequency—thus tend to omit observations for firms that exited for the entire fiscal year in which exit took place. Since firms often experience significant returns (and significant changes in *Q*) in the year preceding their exit (see Figure 2), omitting observations from the entire fiscal year of exit can lead to biased estimates of the effect of treatment.

[33] In particular, during 2019, 4.2% (1.7%) of the unique firms in the CRSP dataset with *shrcd=11* left the sample in connection with an acquisition (distress).

[34] See, e.g., Lucian A. Bebchuk, Alon Brav, Wei Jiang & Thomas Keusch, *Dancing with Activists*, 137 J. FIN. ECON. 1, tb. 14 (2020) (finding that firms targeted by activists that settled experienced a significantly high rate of acquisition and going private related exit but not of distressed exit); Nicole M. Boyson, Nickolay Gantchev & Anil Shivdasani, *Activism Mergers*, 126 J. FIN. ECON. 54 (2017) (finding that 22% of firms targeted by activist (treated firms) received merger bid within 2 years of being targeted). See also Ed deHaan, David Larcker & Charles McClure, *Long-term Economic Consequences of Hedge Fund Activist Interventions*, 24 REV. ACCOUNTING STUD. 536 (2019), at tb. 1 (documenting that among companies targeted by hedge fund activists between 1994 and 2011, approximately 19% (6.6%) had been delisted due to merger-related (other) reasons within 24 months).

Figure 2: Lagged 12-Month Returns Depending on Exit



Note: For each *permno*-month observation in the CRSP securities monthly database corresponding to 2019 for which the variable *shrcd* equals 11, we regressed the lagged 12-month total return for the *permno*-month against a vector of month fixed effects. We then calculated the residuals and plotted the distribution of those residuals among three sets of observations—those corresponding to non-exiting *permno*-months, those observations involving exits in connection with mergers, and those involving exits in connection with underperformance (we identified the type of event driving the exit on the basis of the variable *dlstcd*, following the approach of Shumway & Warther (1999)). Hence, for instance, a value of 1 along the horizontal axis corresponds to *permno*-months with a lagged 12-month total that beat the average lagged 12-month return in the sample at large for the same month by 100 percentage points. To prevent outliers from distorting the figure's scale, we trimmed observations with residual returns in excess of 200%—which is above the 99th percentile of the density of residual returns.

2. State Contingency

The reason we study past corporate governance changes is usually either that we are interested in whether these changes were a good idea when they were made or in whether we should make more changes now. But examining how past changes actually worked out is not always a good guide to answering either of these questions.

To see why, we need to explore more deeply how corporate governance changes affect firm (and shareholder) value. Generally, corporate governance changes induce some *changes in behavior* by firms, managers, or other parties, and these behavioral changes in turn affect shareholder value. For example, corporate governance rules may affect the ability of managers to resist unsolicited takeovers, the likelihood that a board would replace the CEO, or the proclivity of managers to pursue short-term rather than long-term profits, invest in risky rather than safe projects, or sacrifice profits to growth.

How these behavioral changes affect shareholder value often depends on the future state of the world in which the firm will operate. For example, the future state—industry growth rates, the degree of competition, interest rates—will have a strong bearing on whether the payoff from short-term projects exceeds the payoff from long-term projects. Put differently, the *actual realized effects* of a governance change are contingent on the future state—just like the actual return of playing a round of roulette is contingent on the number on which the ball comes to rest. Depending on the future state, the actual realized effects of a governance change can turn out to be positive even if the *expected* value of a governance change is negative, and vice versa.

Long term studies examine the effect of a governance change *in the state that prevailed after treatment*. If treatments were randomly distributed across future states, this would not present a problem—over a large number of treatments, the average actual effect of treatment should come close to the expected effect (assuming expectations were correct). However, in real life, treatments are often bunched in time and sometimes on other dimensions. As a result of such bunching, treated firms tend to operate in similar future states—even if treatment is introduced randomly in the cross section.

Consider, for example, studies looking at the effect of state antitakeover statutes. Over 75% of the firms that became subject to these statutes did so in the years 1986 to 1989.[35] At best, studies of state antitakeover statutes that examine, say, the effect of these statutes in the three years following their effective date can tell us whether the statutes turned out to be positive or negative in the set of circumstances that prevailed in the late 1980s and early 1990s.[36]

[35] Emiliano M. Catan & Marcel Kahan, *The Law and Finance of Antitakeover Statutes*, 68 STAN. L. REV. 629, Figure 1 (2016).

[36] In calendar time portfolio regressions, the state contingency problem is ameliorated. Calendar time portfolio regressions collapse individual firm returns into portfolio-level observations before calculating monthly returns. As a result, returns for firms that got treated at the same time—and which may operate in similar future states when the effect of a governance change is measured—contribute less to portfolio returns than returns for firms that are treated when few other firms are treated. This benefit, however, comes at the cost of generating estimates that do not reflect the average returns of treated firms.

3. Calculating Abnormal Returns

In long term studies, the method by which raw returns are adjusted to generate abnormal returns is increasingly important.[37] Using the wrong method for making these adjustments can easily lead to over-rejection of the hypothesis that treatment had no effect and hence to false positives. Hence, as noted by Nobel laureate Eugene Fama over fifty years ago, even if a study finds "abnormal" returns, one cannot tell whether that is because one used the wrong model to predict what "normal" returns would have been or whether that is because, indeed, the event triggered abnormal returns.[38]

Moreover, even if the model for adjusting returns is correct in the abstract, a long term study can lead to biased results if it systematically misestimates the exposure of a company's equity to various risk factors. In event studies, the adjustment to raw returns is typically based on *pre-treatment* characteristics.[39] A firm equity's β estimate, for example, would be based on past correlations between its stock price returns and market returns. The problem with this approach is that treatment may systematically affect the exposure of a company's equity to the risk factors for which returns are adjusted. After all, the premise underlying studies of governance changes is that such changes plausibly affect firm behavior and that such behavioral changes affect shareholder value. But if a governance

[37] Although estimating the "normal returns" adequately is in principle important for both short- and long-run return studies, the results of long-run studies tend to be much more sensitive to the assumptions made by the researcher about the return-generating process. See Kothari & Warner, *supra* note 28, Table 2.

[38] Eugene Fama, *Efficient Capital Market: A Review of Theory and Empirical Work*, 25 J. FIN. 383 (1970).

[39] Calendar time portfolio regressions mitigate this problem by estimating exposure to risk factors over the period of the study, rather than pre-treatment. This is one of the advantages of calendar time portfolio regressions over long-term event studies. Other advantages of the calendar time approach include (a) that it is mechanically easier to implement than long run event studies (since one does not have to bother with identifying a counterfactual firm/portfolio for each of the treated firms); and (b) that since it collapses the data at the portfolio-calendar month level (instead of considering each treated firm as a separate observation) it avoids the risk that cross sectional correlation in the returns of treated firms may require complex adjustments to the test statistics to prevent them from over-rejecting the null hypothesis.

Unlike event studies, however, calendar time portfolio regressions estimate exposure to risk factors for the entire portfolio of treated firms, and not separately for each treated firm. This has two undesired effects. First, if the firms that are exposed to the event of interest have systematically different exposures to the various factors over the sample period (e.g., because treated firms tend to be large early in the sample period and small late in the sample period), that will reduce the goodness of fit of the estimation, and result in higher standard errors for the estimate of alpha (this probably contributes to the relatively low power of calendar time regressions). Second, if the returns on the underlying factors change systematically at the same time as the exposure of the treated firms to those factors is shifting, that may result in artifactual estimates of the coefficient for the alpha.

Moreover, calendar time portfolio regressions, by calculating each period's return separately and then averaging them (after filtering out the portfolio's exposure to various risk factors), implicitly base their return calculations on the assumption that investors "rebalance" their investments after each period (i.e., take out any gains and replenish any losses). Returns so calculated, unlike returns calculated in event studies, do not reflect the returns an actual buy-and-hold investors would earn. As documented by Barber and Lyon, cumulative abnormal returns—whose calculation also entails a rebalancing assumption—provide a biased measure of the buy-and-hold abnormal returns experienced by investors. Brad M. Barber & John D. Lyon, *Detecting Long-Run Abnormal Stock Returns: The Empirical Power and Specification of Test Statistics*, 43 J. Fin. Econ. 341 (1997).

change plausibly affects firm behavior in a manner that affects shareholder value, then it would often be plausible that such a governance change affects firm behavior in a manner that changes the equity's risk profile.[40]

While the approaches for adjusting raw returns in long-term return studies are not perfect, *Q* regressions deal with the problem of how to adjust raw returns by essentially ignoring it. The numerator in *Q* calculations is derived from unadjusted raw stock returns (as well as other confounding factors, as discussed in Part II). Any "adjustment" to these returns is at best implicit in the set of control firms and other control variables included in *Q* regressions. Thus, year fixed effects and industry fixed effects included in *Q* regressions as part of the estimated specification would soak up the average variation in *Q* experienced by firms in the sample that do not become exposed to treatment during the relevant year or that are operating in the same industry. Those half-hearted approaches to adjusting returns are much coarser than the approaches recommended in the methodological literature about long-run event studies.

III. Q regressions: a puzzle

Our analysis so far presents a puzzle. Even if researchers have so far failed to appreciate the shortcomings of *Q*, if one can use a long-term event study or a calendar time regression to assess the long term effects of a governance intervention, why would one instead use a *Q* regression? Hence, one may wonder: why do so many accomplished scholars nonetheless continue to use *Q* regressions with a within-firm design as the method of choice to assess the value implications of governance? We believe this can be explained as follows.

First of all, estimating garden-variety *Q* regressions using annual data is logistically very easy. One does not need to explicitly deal with many of the decisions necessitated by an event study: identifying an event date; defining an asset pricing model/using the proper firms as the benchmark for the treated ones; defining an event window; handling attrition; specifying a proper test statistic.[41]

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[40] Consistent with this, a survey by Kothari and Warner about the event study methodology remarks that "in long-horizon event studies, it is crucial that abnormal-performance measurement be on the basis of post-event, not historical risk estimates […] However, how post-event risk should be estimated is itself a subject of considerable debate." Kothari & Warner, *supra* note 28, at 22.

[41] Until fairly recently, scholars estimating *Q* regressions got away with reporting t-statistics based on White-robust standard errors, which assume that observations in the estimation sample are independent—an assumption that is typically false in contexts involving panel data. See, e.g., Renee Adams & Daniel Ferreira, *Women in the Boardroom and their Impact on Governance and Performance*, 94 J. FIN. ECON. 291, table 9 (2009) (using standard errors adjusted for heteroskedasticity in regressions estimated using a panel spanning 7 years). See also Mitchell A. Petersen, *Estimating Standard Errors in Finance Panel Data: Comparing Approaches*, 22 Rev. Fin. Stud. 435 (2009) (warning about the risks of overrejecting the null hypothesis in empirical corporate finance work involving panel data). To this day, hypothesis testing in connection with *Q* regressions has to catch up to decades-old insights in the long-run event study literature that urge caution about the potential for cross-sectional

Moreover, some of the early studies that used *Q* as a metric of firm value were conducted by very prominent scholars, and went on to become highly influential articles.[42] Those studies tended to focus on cross-sectional designs where there is no obvious alternative to the use of *Q*—a wrong but more understandable approach. Over time, however, scholars started using *Q* as the outcome variable in *within-firm* research designs. Initially, they did so as a mechanical robustness check for the main analyses in the paper, which were still cross-sectional.[43] Eventually, the within-firm specifications became the main workhorse of the empirical analysis.[44] This may have seemed like a natural methodological progression—at worst, a harmless mutation; at best, a substantial improvement in identification. What seems to have gone unnoticed, however, is that once one switches from cross-sectional comparisons to within-firm designs, superior alternatives to *Q* regressions to study the long-term effects of treatments are available: event studies or calendar time portfolio regressions.

Finally, *Q* regressions may more frequently yield (we suspect often spurious) statistically significant results and hence make the paper easier to publish. One reason is that, as we explain in Part I, within-firm *Q* regressions may pick up results generated by a systematic relationship between treatment and realized profits or capital market transactions.

In addition, the methodological literature on event studies shows that the estimation of long-term abnormal returns is extremely sensitive to misspecifications of the asset pricing model and the test statistics. For instance, simulation studies have documented that even modest cross-sectional correlation across treated firms of the abnormal returns under the null hypothesis of no effect—e.g., driven by the fact that treated firms come disproportionately from a given industry, while one's asset pricing model is not sufficiently fine-tuned—can result in systematic over-rejection of the null hypothesis.[45]

This is so well-known in the event study literature that by now any scholar who attempts to approach a long-term study in a half-baked way is likely to receive scathing reviews from editors and referees. Imagine someone is trying to assess the impact of a given governance intervention that in all likelihood cannot impact firm value by more than 5%, and suppose that that person purported to document that the effect of the

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correlation in abnormal stock returns (and how it can result in overrejection of the null hypothesis). See, e.g., Kothari & Warner, *supra* note 28, at 31. On a related point, see Allen Hu & Holger Spamann, *Inference With Cluster Imbalance: The Case of State Corporate Laws*, Harvard Law School John M. Olin Center Discussion Paper No. 1024 (2019), available at SSRN: https://ssrn.com/abstract=3499101.

[42] For example, as of August 2024, Google Scholar reports that Morck, Shleifer & Vishny, *supra* note 4, has been cited 12,420 times.

[43] See, e.g., Yermack, *supra* note 4; Daines, *supra* note 4; Bebchuk et al., *supra* note 4.

[44] See, e.g., Martijn Cremers & Allen Ferrell, *Thirty Years of Shareholder Rights and Firm Value*, 69 J. FIN. 1167 (2014).

[45] See Lyon et al., *supra* note 29, at 188.

intervention can be estimated by looking at stock returns over a period exceeding five years and using a market-adjusted model to derive abnormal returns through an event study.[46] That person would be laughed out of the seminar room.

And yet an analogous approach is par for the course in within-firm *Q* regressions because editors and referees are not as attuned to the fact that exactly the same—and, on top of them, additional—problems arise in connection with *Q* regressions. In fact, until just a few years ago, most scholars using long panels to conduct difference-in-differences analysis of some treatment—think, for example, of the popular approach that exploits the passage of statutes by different states at different points in time to regress the outcome variable of interest against a treatment dummy, firm fixed effects and year fixed effects—may not even have been able to interpret the difference-in-differences estimate in a transparent way.[47]

CONCLUSION

The use of *Q* regressions has become a mainstay in corporate governance research. Yet, *Q* regressions suffer from theoretical and empirical shortcomings. The use of within-firm *Q* regressions is particularly problematic. Instead of using returns as a direct measure for changes in firm value, these regressions modify returns with factors related to the amount and timing of profits and capital market transactions, thereby adding noise and generating potential bias. The continued use of *Q* regressions—even though they are dominated by other approaches—is a cautionary tale about the role of path dependence and the political economy of the publication process.

Cross-sectional *Q* regressions are no less troubling. These studies often rely on the unspoken hope that differences in *Q* levels between treated and untreated firms, sometimes years or even decades after the intervention, reveal something meaningful about governance effects. Yet empirical evidence shows that even massive return shocks—far larger than those

[46] Under the market-adjusted model, the abnormal return for stock *i* during period *t* is calculated as r_{it} - r_{mt} (where r_{it} is the return experienced by stock *i* during period *t*, and r_{mt} is the return experienced by the market during period *t*). This approach for calculating abnormal returns is extremely rudimentary. For example, it neglects that the stock prices of some companies—e.g, those of tech companies—tend to increase (decrease) by a greater percentage for any increase (decrease) in overall market price than those of other companies—e.g., those of consumer staple manufacturers.

[47] As demonstrated by Andrew Goodman-Bacon, when used in settings where treatment is introduced in a staggered way over multiple periods, the regression-based difference-in-differences estimator is a weighted average of all the difference-in-differences estimates one could derive from the data if one focused only on two periods and compared the evolution of the outcome variable experienced by firms for which treatment status changed between those two periods with that experienced by firms for which treatment status did *not* change between those two periods. Andrew Goodman-Bacon, *Difference-in-Differences With Variation in Treatment Timing*, 225 J. ECONOMETRICS 254 (2021) (which also discusses the conditions under which the regression-based difference-in-differences estimate has a causal interpretation in that setting).

likely attributable to governance changes—leave no discernible imprint on *Q* after just a few years. As we argue, a finding in cross-sectional studies that treated and untreated firms differ in their *Q* thus does not warrant any inference about the effect of a treatment on firm value.

The bottom line is clear: *Q* regressions are a methodological dead end. Their continued use not only hinders progress in understanding the value implications of governance changes but risks misleading scholars and policymakers. If the goal is to rigorously assess how governance changes impact firm value, researchers must turn to more robust approaches. Event studies and calendar time portfolio regressions, while not without their own limitations, provide a far clearer lens through which to examine these questions. The time has come to let go of *Q* regressions and embrace methodologies that truly advance our understanding of the impact of corporate governance on firm value.

APPENDIX A

List of corporate governance papers with *Q* regressions published between 2012 and 2022 in the Journal of Finance, Journal of Financial Economics and Review of Financial Studies:

Renée B. Adams, Ali C. Akyol & Patrick Verwijmeren, Director Skill Sets, 130 J. Fin. Econ. 641 (2017)

Renée B. Adams, Vanitha Ragunathan & Robert Tumarkin, Death By Committee? An Analysis of Corporate Board (Sub-) Committees, 141 J. Fin. Econ. 1119 (2021)

Brian Akins, David De Angelis & Maclean Gaulin, Debt Contracting on Management, 75 J. Fin. 2095 (2020)

Rui Albuquerque, Luis Brandão-Marques, Miguel A Ferreira & Pedro Matos, International Corporate Governance Spillovers: Evidence from Cross-Border Mergers and Acquisitions, 32 Rev. Fin. Stud. 738 (2019)

Julian Atanassov, Do Hostile Takeovers Stifle Innovation?, Evidence from Antitakeover Legislation and Corporate Patenting, 68 J. Fin. 1097 (2013)

Bandiera, Oriana, Renata Lemos, Andrea Prat & Raffaella Sadun, Managing the Family Firm: Evidence from CEOs at Work, 31 Rev. Fin. Stud. 1605 (2018)

Suman Banerjee, Mark Humphery-Jenner & Vikram Nanda, Restraining Overconfident CEOs Through Improved Governance: Evidence from the Sarbanes-Oxley Act, 28 Rev. Fin. Stud. 2812 (2015)

Michal Barzuza & David C. Smith, What Happens in Nevada? Self-Selecting into Lax Law, 27 Rev. Fin. Stud. 3593 (2014)

Lucian A. Bebchuk, Alon Brav, Wei Jiang & Thomas Keusch, Dancing with Activists, 137 J. Fin. Econ. 1 (2020)

Lucian A. Bebchuk, Alma Cohen & Charles C.Y. Wang, Learning and the Disappearing Association Between Governance and Returns, 108 J. Fin. Econ. 323 (2013)

Jan Bena, Miguel A. Ferreira, Pedro Matos & Pedro Pires, Are Foreign Investors Locusts? The Long-Term Effects of Foreign Institutional Ownership, 126 J. Fin. Econ. 122 (2017)

Effi Benmelech & Carola Frydman, Military CEOs, 117 J. Fin. Econ. 43 (2015)

Sreedhar T. Bharath, Sudarshan Jayaraman, & Venky Nagar, Exit as Governance: An Empirical Analysis, 68 J. Fin. 2515 (2013)

Ye Cai, Jin Xu & Jun Yang, Paying by Donating: Corporate Donations Affiliated with Independent Directors, 34 Rev. Fin. Stud. 618 (2021).

Jeffrey L Coles, Zhichuan (Frank) Li & Albert Y Wang, Industry Tournament Incentives, 31 Rev. Fin. Stud. 1418 (2018)

Stefano Colonnello, Matthias Efing & Francesca Zucchi, Shareholder Bargaining Power And The Emergence Of Empty Creditors, 134 J. Fin. Econ. 297 (2019)

Ricardo Correa & Ugur Lel, Say on Pay Laws, Executive Compensation, Pay Slice, and Firm Valuation Around the World, 122 J. Fin. Econ. 500 (2016)

Alan D. Crane, Andrew Koch & Sébastien Michenaud, Institutional Investor Cliques and Governance, 133 J. Fin. Econ. 175 (2019)

Martijn Cremers & Allen Ferrell, Thirty Years of Shareholder Rights and Firm Value, 69 J. Fin. 1167 (2014)

K.J. Martijn Cremers, Lubomir P. Litov & Simone M. Sepe, Staggered Boards and Long-Term Firm Value, Revisited, 126 J. Fin. Econ. 422 (2017).

Vicente Cuñat, Mireia Gine & Maria Guadalupe, The Vote Is Cast: The Effect of Corporate Governance on Shareholder Value, 67 J. Fin. 1943 (2012)

Claudia Custodio, Miguel Ferreira & Pedro Matos, Generalists versus Specialists: Lifetime Work Experience and Chief Executive Officer Pay, 108 J. Fin. Econ. 471 (2013)

Amy Dittmar & Ran Duchin, Looking in the Rearview Mirror: The Effect of Managers' Professional Experience on Corporate Financial Policy, 29 Rev. Fin. Stud. 565 (2016)

Ran Duchin, Amir Goldberg & Denis Sosyura, Spillovers Inside Conglomerates: Incentives and Capital, 30 Rev. Fin. Stud. 1696 (2017)

Ran Duchin & Dennis Sosyura, The Politics of Government Investment, 106 J. Fin. Econ. 24 (2012)

Ying Dou, Ronald W. Masulis & Jason Zein, Shareholder Wealth Consequences of Insider Pledging of Company Stock as Collateral for Personal Loans, 32 Rev. Fin. Stud. 4810 (2019)

Kornelia Fabisik, Rüdiger Fahlenbrach, Rene M. Stulz & Jérôme Taillard, Why Are Firms With More Managerial Ownership Worth Less?, 140 J. Fin. Econ. 699 (2021)

Larry Fauver, Mingyi Hung, Xi Li & Alvaro G. Taboada, Board Reforms and Firm Value: Worldwide Evidence, 125 J. Fin. Econ. 120 (2017)

Allen Ferrell, Hao Liang & Luc Renneboog, Socially Responsible Firms, 122 J. Fin. Econ. 585 (2016)

Scott Guernsey, Simone M. Sepe & Matthew Serfling, Blood in the Water: The Value of Antitakeover Provisions During Market Shocks, 143 J. Fin. Econ. 1070 (2022)

Roie Hauser, Busy Directors and Firm Performance: Evidence from Mergers, 128 J. Fin. Econ. 16 (2018)

Davidson Heath, Daniele Macciocchi, Roni Michaely & Matthew Ringgenberg, Do Index Funds Monitor?, 35 Rev. Fin. Stud. 91 (2021)

Larry Fauver, Mingyi Hung, Xi Li & Alvaro G. Taboada, Board Reforms and Firm Value: Worldwide Evidence, 125 J. Fin. Econ. 120 (2017)

David Hirshleifer, Angie Low & Siew Hong Teoh, Are Overconfident CEOs Better Innovators?, 67 J. Fin. 1457 (2012)

William C. Johnson, Jonathan M. Karpoff & Sangho Yi, The Lifecycle Effects of Corporate Takeover Defenses, 35 Rev. Fin. Stud. 2879 (2022)

Nishant Dass, Omesh Kini, Vikram Nanda, Bunyamin Onal & Jun Wang, Board Expertise: Do Directors from Related Industries Help Bridge the Information Gap?, 27 Rev. Fin. Stud. 1533 (2014)

Anzhela Knyazeva, Diana Knyazeva & Ronald W. Masulis, The Supply of Corporate Directors and Board Independence, 26 Rev. Fin. Stud. 1561 (2013)

Kang Jun-Koo, Luo Juan & Na Hyun Seung, Are Institutional Investors With Multiple Blockholdings Effective Monitors?, 128 J. Fin. Econ. 576, 2018

Kai Li, Feng Mai, Rui Shen & Xinyan Yan, Measuring Corporate Culture Using Machine Learning, 34 Rev. Fin. Stud. 3265 (2021)

Chunmei Lin, Massimo Massa & Hong Zhang, Mutual Funds and Information Diffusion: The Role of Country-Level Governance, 27 Rev. Fin. Stud. 3343 (2014)

Claire Liu, Angie Low, Ronald W. Masulis & Le Zhang, Monitoring the Monitor: Distracted Institutional Investors and Board Governance, 33 Rev. Fin. Stud. 4489 (2020).

Ronald W. Masulis & Shawn Mobbs, Independent Director Incentives: Where do Talented Directors Spend their Limited Time and Energy?, 111 J. Fin. Econ. 406 (2014)

Ronald W. Masulis & Emma Jincheng Zhang, How valuable are independent directors? Evidence from external distractions, 132 J. Fin. Econ. 226 (2019)

Vikas Mehrotra, Randall Morck, Jungwoole Shim & Yupana Wiwattanakantang, Adoptive Expectations: Rising Sons in Japanese Family Firms, 108 J. Fin. Econ. 840 (2013)

Leming Lin, Atanas Mihov, Leandro Sanz & Detelina Stoyanovac, Property Rights Institutions, Foreign Investment, And The Valuation Of Multinational Firms, 134 J. Fin. Econ. 214 (2019)

Lasse Heje Pedersen, Shaun Fitzgibbons & Lukasz Pomorskia, Responsible Investing: The ESG-Efficient Frontier, 142 J. Fin. Econ. 572 (2021)

Zigan Wang, Qie Ellie Yin & Luping Yu, Real Effects Of Share Repurchases Legalization On Corporate Behaviors, 140 J. Fin. Econ. 197 (2021)

Stefan Zeume, Bribes and Firm Value, 30 Rev. Fin. Stud. 1457 (2017)

APPENDIX B: DERIVATION OF EQUATION (1)

Equation (1) comes from the following three equations.

(a) The equation that defines Tobin's Q as of *t* as the ratio between the sum of the market value of equity, E_t and the market value of debt, D_t, to the book value of assets, A_t:

$$Q_t = \frac{E_t + D_t}{A_t} \qquad \text{(A1)}$$

(b) The equation that relates the book value of assets as of period *t+1*, A_{t+1}, with the book value of assets as of *t*, A_t, the profits the company obtains during period *t*, P_t, and the net proceeds from capital market transactions the company engages in during period *t*, CM_t:

$$A_{t+1} = A_t + P_t + CM_t. \qquad \text{(A2)}$$

(c) The equation that relates the sum of the values of equity and debt as of *t+t*, $E_{t+1} + D_{t+1}$, with the total shareholder returns (appreciation plus dividends) generated by the stock between *t* and *t+1*, r_t, the market value of equity as of *t*, the net proceeds from capital market transactions the company engages in during period *t*, and the market value of debt as of *t* (assuming that changes in the market value of debt reflect merely incurrence of additional and repayment of existing debt):

$$E_{t+1} + D_{t+1} = E_t (1+r_t) + CM_t + D_t. \qquad \text{(A3)}$$

From (A1), it follows that the change in *Q* between *t* and *t+1* equals

$$\Delta Q_{t+t,t} = \frac{E_{t+1} + D_{t+1}}{A_{t+1}} - Q_t.$$

Using equation (A3) that becomes

$$\Delta Q_{t+1,t} = \frac{E_t(1 + r_t) + CM_t + D_t}{A_{t+1}} - Q_t$$

$$= \frac{E_t r_t + CM_t + D_t + E_t}{A_{t+1}} - Q_t.$$

Using equation (A2), that can be rewritten as

$$\Delta Q_{t+1,t} = \frac{E_t r_t + CM_t + D_t + E_t}{A_{t+1}} - Q_t \frac{A_t + CM_t + P_t}{A_{t+1}}$$

$$= \frac{E_t r_t}{A_{t+1}} + \frac{D_t + E_t}{A_{t+1}} + \frac{CM_t(1 - Q_t)}{A_{t+1}} - Q_t \frac{A_t + P_t}{A_{t+1}}$$

$$= \frac{E_t r_t}{A_{t+1}} + \frac{(D_t + E_t)A_t}{A_t A_{t+1}} + \frac{CM_t(1 - Q_t)}{A_{t+1}} - Q_t \frac{A_t + P_t}{A_{t+1}}$$

$$= \frac{E_t r_t}{A_{t+1}} + \frac{Q_t A_t}{A_{t+1}} + \frac{CM_t(1 - Q_t)}{A_{t+1}} - Q_t \frac{A_t + P_t}{A_{t+1}}$$

$$= r_t \frac{E_t}{A_{t+1}} - \frac{P_t}{A_{t+1}} Q_t + \frac{CM_t}{A_{t+1}}(1 - Q_t).$$

Equation (1) is a generalization of this expression for the case between two potentially non-adjacent periods, t and $\tau > t$.

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