Empirical Operations Management—
Three Essays

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DISSERTATION ACCEPTANCE CERTIFICATE

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The undersigned, approved by the Chairman of the Doctoral Programs, have examined a dissertation entitled

Empirical Operations Management – Three Essays

1) Is Inventory's Fiscal Year End Effect Caused by Sales Timing? A Test Using a Natural Experiment from Germany
2) Inventory Signals
3) Inventory and the Stock Market

presented by **Richard Kum-Yew Lai**

candidate for the degree of Doctor of Business Administration and hereby certify that it is worthy of acceptance.

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Professor Ananth Raman (Chair) Richard Kum-Yew Lai

Empirical Operations Management—
Three Essays

ABSTRACT

This dissertation comprises three papers on how firms really manage operations. In particular, I focus on the management of inventory. I also focus on how operations management affects and is affected by managers' financial incentives.

In the first paper, I first observe a fiscal-year end (FYE) effect in which firm-level inventory exhibits large and regular dips at the end of the firm's fiscal year. A classical story is that firm inventory is tied to the calendar year, which reflects fundamental industry demand. So it is peculiar that inventory is also tied to the fiscal year, which is an accounting artifact. In the paper, I show empirically that the FYE effect is due to sales timing, in which managers' private benefits lead them to pull some post-FYE sales into the FYE. To test for sales timing, I employ a novel natural experiment based on Germany's tax code change in 2000, when some firms change their FYEs in a way that is plausibly exogenous to inventory patterns. I report evidence consistent with sales timing that is not explained by alternative hypotheses. I conclude by posing intriguing implications arising from the existence of the FYE effect and the finding that sales timing is a cause.

In the second paper called Inventory Signals, I consider how operational competence—such as inventory management competence—translates to market value, when firms cannot credibly communicate their competence to the stock market. When the stock market sees a high-inventory firm, it cannot tell whether the inventory is due to incompetence or to a strategy to enhance fill rate. Based on this incomplete information, she has to decide how to value the firm. Based on the investor's decision algorithm, high-competence firms that might otherwise pursue a high-inventory high-fill-rate strategy face the decision of whether to carry less inventory, so as to signal competence to the investor. What holds in equilibrium? I show conditions for separating and pooling perfect Bayesian equilibria. I also provide empirical evidence consistent with three predictions of this theory that inventory has a signaling role. The theory has implications for firms, such as how to strategically communicate to the market, reward managers, or even whether to go public and be subject to market pressures.

While the first paper considers inventory management within firm and year, the second studies that across firms and between years. The second paper also

considers managerial benefits arising not from sales incentives, but from short-term considerations of their firms' stock prices.

These first two papers consider a world into which actors—firms and the stock market—are rational. The third paper, called Inventory and the Stock Market, is motivated by the growing body of evidence that the stock market can temporarily mis-value firms. I report evidence that the market's "behavioral" component explains firms' inventory as much as its "rational" component. I further test three possibilities for how the behavioral component works. The first is a financing channel. When the market over-values firms, firms can get cheaper financing and increase inventory. The second is dissipation. When the market over- or under-values firms, firms are less disciplined and let inventories rise. The third is catering. When the market discounts high-inventory firms, firms decrease inventory, and vice versa. I report evidence that weakly supports financing, rejects dissipation and strongly supports catering. The findings suggest that we need to find new ways of calculating the cost of capital for operations models. They could begin to form the basis of a more empirically accurate account of how inventory decisions are affected by financial markets.

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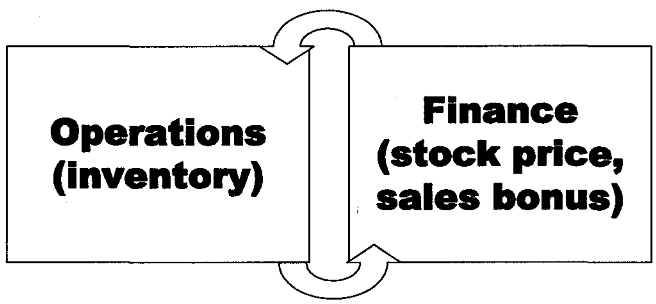
ACKNOWLEDGMENT

I thank my advisors—Ananth Raman (chair), Vishal Gaur, Dale Jorgenson, Josh Lerner—for their guidance, wisdom, and friendship. This work is for my parents, Elizabeth, and Jessica.

Chapter 1 Introduction

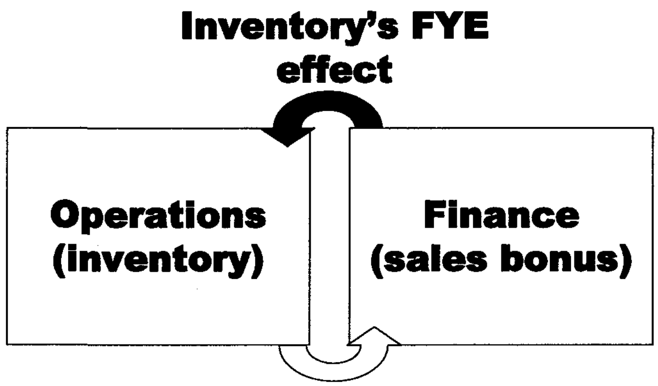
In the three papers in this dissertation, I examine how firms really manage operations. This is an enormous research agenda, so in this dissertation, I focus on how operations management affects and is affected by financial considerations such as stock price and sales bonuses. And of the different aspects of operations management, I focus on the management of inventory:



With these foci, the claims I make in this dissertation can be sharper, although they will also be narrower. However, I believe at least some of the material could be generalized. I describe these in the conclusion of this chapter.

1. Inventory's Fiscal Year End Effect

In the first paper, I first observe a fiscal-year end (FYE) effect in which firm-level inventory exhibits large and regular dips at the end of the firm's fiscal year. A classical story is that firm inventory is tied to the calendar year, which reflects fundamental industry demand. So it is peculiar that inventory is also tied to the fiscal year, which is an accounting artifact. In the paper, I show empirically that the FYE effect is due to sales timing, in which managers' private benefits lead them to pull some post-FYE sales into the FYE.

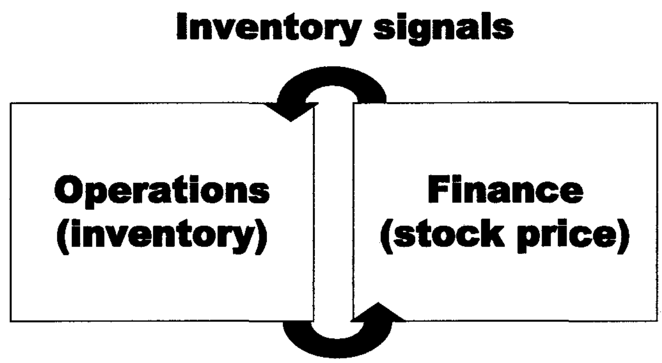


To test for sales timing, I employ a novel natural experiment based on Germany's tax code change in 2000, when some firms change their FYEs in a way that is plausibly exogenous to inventory patterns. I report evidence consistent with sales timing that is not explained by alternative hypotheses. I conclude by posing intriguing implications arising from the existence of the FYE effect and the finding that sales timing is a cause.

2. Inventory Signals

In the second paper called "Inventory Signals," I consider how operational competence translates into market value, when firms cannot credibly communicate their competence to an investor? I consider the example of inventory and fill rates.

When the investor sees a high-inventory firm, she cannot tell whether the inventory is due to incompetence or a strategy to enhance fill rate. Based on this incomplete information, she has to decide how to value the firm. Based on the investor's decision algorithm, high-competence firms that might otherwise pursue a high-inventory high-fill-rate strategy face the decision of whether to carry less inventory, so as to signal competence to the investor. What holds in equilibrium?

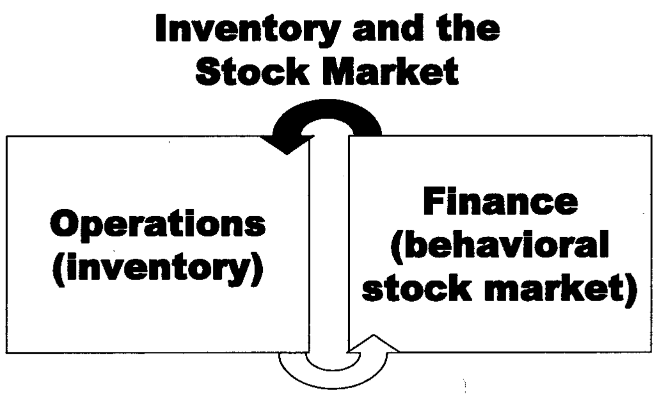


I show conditions for separating and pooling perfect Bayesian equilibria. I also provide empirical evidence consistent with three predictions of this theory that inventory has a signaling role. The theory has implications for firms, such as how to strategically communicate to the market, reward managers, or even whether to go public and be subject to market pressures.

While the first paper considers inventory management within firm and year, the second studies that across firms and between years. The second paper also considers managerial benefits arising not from sales incentives, but from short-term considerations of their firms' stock price.

3. Inventory and the Stock Market

The first two papers consider a world into which actors—firms and the stock market—are rational. The third paper, called "Inventory and the Stock Market," is motivated by the growing body of evidence that the stock market can temporarily mis-value firms. I report evidence that the market's "behavioral" component explains firms' inventory as much as its "rational" component.



I further test three possibilities for how the behavioral component works. The first is a financing channel. When the market over-values firms, firms can get cheaper financing and increase inventory. The second is dissipation. When the market mis-values firms, firms are less disciplined and let inventories rise. The third is catering. When the market discounts high-inventory firms, firms decrease inventory, and vice versa. I report evidence that weakly supports financing, rejects dissipation and strongly supports catering. The findings suggest that we need to find new ways of calculating the cost of capital for operations models. They could begin to form the basis of a more empirically accurate account of how inventory decisions are affected by financial markets.

4. Implications

Here, I summarize the implications of the three papers, as well as implications that these papers have for more general future work.

4.1 Implications from the Papers: Operations Management from an Investor's Perspective

Taken together, the three papers suggest that financial considerations could significantly affect inventory decisions, and conversely, inventory decisions could also be "managed" by firms in a way to maximize sales bonus payouts and stock price.

These findings have important implications. For example, if the inventory FYE effect is due to unintended mis-coordination between sales and production/purchasing, then it seems that—conditional on sales timing—improving coordination can improve firm value.

However, the major implications might be less for managers and more for investors. This is because what we find—sales timing in inventory's FYE effect, agency in inventory signals, catering in inventory management in the face of behavioral stock markets—all suggest that managers are already acting rationally and optimally in the face of *their* incentives. However, the findings also suggest that firm value might be compromised, at the expense of investors.

This brings us to the topic of "operations management from the perspective of an investor."[1] Traditionally, research in operations management is focused on the manager. The manager is different from the investor (or her proxy, such as an analyst or a fund manager) in at least two ways: the manager has more information and more decision rights. The findings in this dissertation suggest that an investor should be at least aware of how operational decisions affect and are affected by managers' *financial considerations*.

For example, before investing in a firm, a private equity investor might want to understand the extent to which managers under-stock inventory in order to signal competence to the stock market. After investing, she could reconfigure some parameters (such as lengthening the manager's horizon) to minimize under-stocking and improve firm value. Some of these changes may be so radical that they may even require changing the nature of ownership—such as from publicly-listed to private-owned—in order to be implemented.

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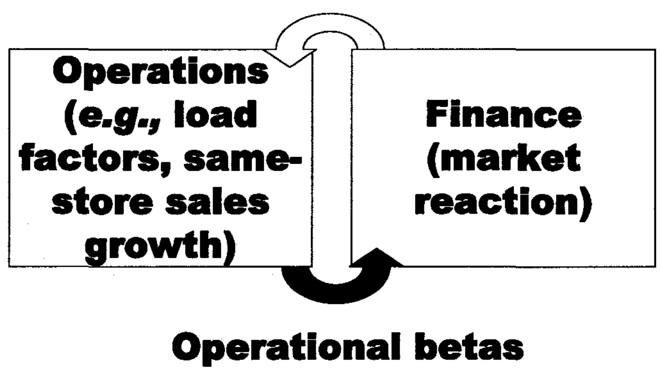
[1] This perspective borrows heavily from Professors Ananth Raman and Vishal Gaur. Any error in this exposition is mine.

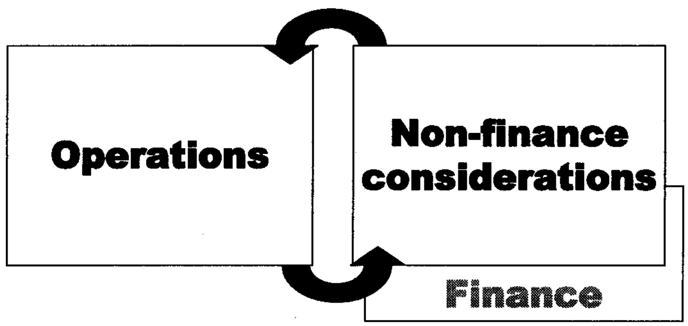
4.2 Implications for Future Research

There are three intriguing avenues for future research, some of which I am now pursuing.

The first is to consider operational variables other than inventory. Indeed, some of the characteristics of inventory I consider here—such as its opaqueness to investors (Chapter 3, Inventory Signals)—also feature in other operational parameters like research investments. Still others might be amenable to partial equilibrium research. For example, in continuing work, I seek to understand how investors react to operational information such as airlines' load factors, wireless companies' subscriber churn, retailers' same-store sales growth, or banks' loan efficiency ratios:



A second avenue is to consider how operational decisions affect or are affected by *non-financial* considerations. For example, Lai (2005a) compares rational versus behavioral origins of the bullwhip effect in a Spanish supermarket, Lai (2005b) documents how various operational decisions are tied to managers' personal styles, and Lai (2006) reports the degree to which geographic considerations enter operational decisions. Taken together, these suggest that growing evidence that real operational management needs to be explained by factors beyond the economic considerations directly impacting operational problems.



Finally, the method of a natural experiment (see Chapter 2, Inventory's Fiscal Year End Effect)—although new in empirical operations management—could be a very persuasive paradigm for future work. More generally, I believe it is helpful to introduce techniques developed in other social sciences such as strategic management, economics, and sociology, into the empirical repertoire of the empiricist in operations management. For example, Lai, et al. (2008), in examining whether information technology (IT) reduces inventory, uses managers' college majors as an instrumental variable to more sharply address endogeneity arising from joint determination of IT investments and inventory decisions by omitted variables.

5. References

Lai, R. *Bullwhip in a Spanish Shop, Harvard NOM Working Paper No. 06-06*. Boston, MA: Harvard Business School, 2005a

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Chapter 1 Is Inventory's Fiscal Year End Effect Caused by Sales Timing? A Test Using a Natural Experiment from Germany*

We first document that firms have lower inventory at fiscal year end (FYE) than at other times of the year. We then produce evidence that sales timing—in which executives' private benefits lead them to pull some post-FYE sales into the FYE—is one cause of the "FYE effect." Finally, we estimate the financial impact of the FYE effect on firm value. We conclude with a discussion of limitations, next steps, and some intriguing implications.

1. Introduction

Much of the empirical research on inventory management (*e.g.*, Rajagopalan and Malhotra (2001); Chen, et al. (2005); Gaur, et al. (2005)) has focused on *between-year* inventory patterns. There is also some literature (*e.g.*, Nerlove, et al. (1993)) on *within-year* inventory patterns, which examines how inventory is tied to the calendar year, reflecting fundamental demand. In this paper, we consider the phenomenon in which inventory is also tied to the fiscal year, an accounting artifact.

Figure 1 depicts RadioShack's finished goods inventory levels over time. Before 1992, inventory dips in the second calendar quarters. The dips are sizable, about $1

* This is a revised version of a paper entitled "Inventory's Fiscal Year End Effect," which received Honorable Mention for the 2007 INFORMS M&SOM Student Paper Competition and is a finalist for the 2008 POMS Supply Chain College Student Paper Competition (results to be revealed in May 2008). It has also been released as Harvard Business School Technology & Operations Management Unit (TOM) Research Paper No. 08-86. I am most grateful for encouragement and feedback from my advisors: Ananth Raman (chair), Vishal Gaur, Dale Jorgenson, and Josh Lerner. I also benefited tremendously with feedback from Phillip Berger, Dennis Campbell, Nicole Dehoratius, Mihir Desai, Marshall Fisher, Lee Fleming, Rob Huckman, Andy King, Deishin Lee, Tom Malone, Paul Oyer, Dan Snow, Mike Toffel, Anita Tucker, Noel Watson, Zeynep Ton, and participants in seminars at Boston College, Chicago, Carnegia-Mellon, GeorgiaTech, HBS, Pittsburgh Katz, Northwestern Kellogg, MIT Operations Management, MIT Information Systems, NYU Stern, Penn State, Stanford, USC, and Wharton. All errors are mine.

billion in inventory, valued at cost of goods. One explanation for this (*e.g.*, Stevenson (1999), pg. 485) is that inventory varies with demand seasonality in the calendar year. Perhaps in those second quarters, RadioShack's demand peaks, depleting inventory to its trough.

In 1992, RadioShack changed its fiscal year to end not in the second calendar quarter, but the fourth. Figure 1 shows that after that, inventory is lowest in the new fiscal year ends. We shall call the phenomenon in which inventory is, *ceteris paribus*, lower at fiscal year end than at other times, the "fiscal year end (FYE) effect." In section 2, we estimate the FYE effect more rigorously and show that it is not limited to RadioShack. In a panel of all listed U.S. manufacturers, wholesalers, and retailers, inventory is 10% lower at FYE. In 2006, this inventory dip is about $47 billion in valued at cost of goods sold. Therefore, inventory's FYE effect is large and pervasive.

This leads to a natural question: what might cause the effect? While we know of no study that directly answers this question, previous research (*e.g.*, Oyer (1998); Steenburgh (2004); Larkin (2006)) suggests the main hypothesis of our paper: that inventory's FYE effect is caused by <u>sales timing</u>, in which executives' private benefits lead them to pull some next-quarter sales into the FYE. This is not a perfunctory hypothesis. For one, inventory is determined not just by sales, but also by operational decisions such as purchasing and production, and by accounting decisions such as inventory write-offs[1]. So sales timing does not necessarily lead to inventory's FYE effect. Further, inventory's FYE effect could be explained by other confounding hypotheses, which we describe in section 3. We are agnostic about whether these confounding hypotheses also true. Instead, we only ask if sales timing is a cause. We focus on sales timing because there are *a priori* reasons to believe that it harms firm value. This is suggested by the popular literature on channel stuffing[2]. It is also suggested by the research literature, in which greater earnings from increased sales at FYE might not be compensated by lower earnings after FYE (Oyer (1998) shows this at the industry level; we will confirm this at the firm level), and variations in inventory might lower profitability when profits are a convex function of inventory (Karush (1957)).

In section 4, we provide evidence for sales timing that is not explained by the confounding hypotheses. We exploit a novel natural experiment based on the Germany's tax code change in 2000, when some firms change their FYE in a way that is plausibly exogenous to inventory patterns. Using a panel dataset of German firms

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[1] For example, it is plausible that production goes up enough that inventory does not decrease, so sales timing does not necessarily lead to inventory's FYE effect. But production might also not increase enough with sales—perhaps for incentive reasons such as those in Porteus and Whang (1991) or Lai (2006)—so that sales timing causes inventory to deplete. In short, sales does not have a mechanical relationship with inventory.

[2] In an egregious example, the Securities and Exchange Commission (SEC) alleges that Bristol-Myers Squibb cut prices to induce its distributors to take on $1.3 billion of inventory at 2001 FYE. This inflated FYE revenues by 7% so that its executives could make sales targets. This practice, also called channel *stuffing*, is so pervasive that it has gotten many labels: "loading" in SEC (2006) vs. Virbac, "gallon pushing" in SEC (2005) vs. Coca-Cola, "floor sweeping" in SEC (2003) vs. ClearOne, and "pull forwards" in SEC (2004b) vs. K-mart.

hand-coded from primary sources and from CapitalIQ, we find that firms that changed their FYE have lower inventory in both their old and new fourth fiscal quarters. This result is robust to corrections for possible treatment selection using the Heckit procedure and propensity scoring in a differences-in-difference framework. We will explain why this is clean evidence for sales timing that is not consistent with the confounding explanations.

In section 5, we provide further evidence for sales timing as a cause of inventory's FYE effect by directly examining mediators and moderators. For example, we find that the link from FYE to lower inventory is mediated by lower margins and higher sales, and not by reduced production. The FYE effect is stronger for firms that pay higher bonuses and sell durable goods, and weaker for firms under scrutiny, such as those who face federal class action suits. All these are consistent with sales timing and are not explained by the other hypotheses.

In section 6, we consider financial implications. We find that 1 percentage point lower inventory at FYE is associated with 1.7% lower valuation in industry-adjusted Tobin's q. This elasticity arises from two sources: lower gross profits and higher costs. Gross profits are net 5% lower than if there were no FYE effect, because firms sell more but at lower margins at FYE. Costs are higher because inventory fluctuations result in higher holding costs and sales fluctuations in higher capacities.

In section 7, we discuss the limitations of our study and suggest next steps. For example, we have focused only on the importance of the sales timing and not that of the confounding hypotheses; we merely provide evidence for sales timing not explained by others, not evidence for the absence of others. We conclude with implications for research and practice.

To summarize, what is new in this study is that it:

1. Identifies inventory's FYE effect, as related to but distinct from previous phenomenon studied (please see figure 2);
2. Provides the first empirical evidence that the effect is pervasive and substantive;
3. Provides the first empirical evidence that sales timing is a cause of the effect, evidence that is not explained by possibly confounding hypotheses;
4. Introduces the natural experiment as a paradigm in empirical operations management;
5. Presents the first empirical estimation of the valuation impact of the effect.

The rest of the paper provides details of the above.

2. Motivation: the Pervasive and Substantive FYE Effect

We motivate this paper by showing that the FYE effect is pervasive and economically significant. Here, we seek only to *identify* the size of the FYE effect. We are not concerned with causality, which we address in the rest of this paper.

We use a panel of all 2,512 U.S. manufacturers, wholesalers, and retailers (NAICS codes 31 through 48) in COMPUSTAT, from 1984 through 2006. We omit observations that are economically insignificant, with missing or negative sales or cost of goods sold (COGS). Table 1, panel (a), shows the variation of the firm-quarter observations by FYE; this variation allows identification of the FYE effect. Panel (b) shows the summary statistics.

Our empirical model is a straightforward reduced form specification:

$$v_{it} = \sum_{f=1}^{4} \iota_{it}^{f} \phi^{f} + \sum_{c=1}^{4} \iota_{it}^{c} \chi^{c} + \gamma_{y} + \kappa_{i} + \eta_{it}, \tag{A}$$

where v_{it} is inventory adjusted with LIFO reserve, measured as finished goods inventory divided by quarterly COGS, of firm i in quarter t, each of which is indexed with a fiscal quarter label f and a calendar quarter label c. Different types of inventory—raw materials, work-in-progress—have different dynamics; we focus on finished goods inventory because it has the highest economic value. We also measure inventory without scaling by COGS and scaling by total assets, and obtain the similar results (see the next section on measuring inventory). ϕ^{f} is the effect on inventory of being in fiscal quarter f, and χ^{c}, of being in calendar quarter c; ι's are indicator variables. γ_{y} and κ_{i} are calendar year (indexed by y) and firm fixed effects, and η_{it} is assumed to be white noise. κ_{i} accounts for time-invariant firm characteristics, such as industry. The de-meaned model is as follows, with Δ the de-mean operator:

$$\Delta v_{it} = \sum_{f=1}^{4} \Delta \iota_{it}^{f} \phi^{f} + \sum_{c=1}^{4} \Delta \iota_{it}^{c} \chi^{c} + \Delta \gamma_{y} + \Delta \eta_{it}. \tag{B}$$

In Table 2, we first report in model (1) estimates of calendar quarter effects without fiscal quarters. As with all estimations in this paper, these are obtained with Huber-White robust standard errors in case inventory varies differently by fiscal quarter, and clustered around firms to account for potential within-firm correlation. It is also in log form, so we interpret the estimate as 11.3% lower inventory in the fourth calendar quarter, χ^4, and a little higher in the quarters before and after—*i.e.*, we use χ^2 as the base. In model (2), we see a large FYE effect: inventory is 10.3% lower in the fourth fiscal quarter, ϕ^4. As expected, ϕ^3 is statistically weak. Interestingly, ϕ^1 is positive, a point we address in the next section. We also note that calendar quarter effects are much diminished, with just 5.1% for χ^4, suggesting that the FYE effect might be even more substantive than the calendar effect.

2.1 Measuring Inventory

There are many views on how "inventory" should be measured. Which ones to use? That depends on the question, which (for us) is: "what does it mean to hold 'lower' inventory?":

1. ***End-of-quarter inventory***. This measure has the merit of being parsimonious.
2. ***End-of-quarter inventory scaled by quarterly COGS*** (cost of goods sold). This is the view in operations management—*e.g.*, Gaur, et al. (2005). Inventory supports demand, so if one observation has a lower unscaled inventory level than another but is associated with a much lower sales level,

we would not conclude that this former observation holds less inventory. In this view, proper comparison of inventory requires absolute levels to be scaled by COGS. The baseline version of this measure scales inventory by contemporaneous COGS, but we also use different variants, scaling by the average of contemporaneous and next-period COGS and by the average of contemporaneous, previous-period, and next-period COGS.

3. ***End-of-quarter inventory scaled by end-of-quarter total assets.*** This view is often associated with the accounting literature—*e.g.*, Roychowdhury (2006). It is analogous to the previous one, but the idea here is that inventory is working capital, so it is comparable only as part of total assets.

Similar Results. It turns out that empirically, the different measures produce qualitatively similar results. For example, Table 2, model (2) shows that inventory is 10.3% lower at FYE, when we measure inventory as absolute inventory scaled by COGS. The corresponding estimates are 5.9% for inventory measured as a absolute level and 3.4% for inventory measured as absolute level scaled by total assets. The former is about US$21.8 billion in inventory dollars and the latter, US$18.2 billion in asset dollars, using our U.S. dataset.

Implications of Scaling by Exogenous Variable. It might appear that scaling by COGS creates a measure that becomes mechanically tied to COGS[3]. This turns out to

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[3] When we test COGS as a mediator, one of the two estimations (see the section on mediator tests) includes COGS as an additional covariate in specification (A), with inventory as dependent

be immaterial:

- ***Conceptually,*** all three measures are functions of several exogenous variables. To see this, recall that:

  $$\textit{Absolute inventory}_t = \textit{Absolute inventory}_{t-1} + \textit{COGS}_t - \textit{Production}_t - \textit{Writeoffs}_t,$$

  Dividing the above by COGS does not make it any more or less tied to COGS.

- ***Empirically,*** the scaled measures are not highly correlated with COGS. The correlations with COGS (the signs are irrelevant) are:

  - Absolute inventory: 0.838
  - Scaling by COGS: -0.014
  - Scaling by assets: -0.006

  If anything, the absolute inventory measure has a higher correlation with COGS.

- ***Econometrically,*** that the measures are correlated with COGS is a *requirement* for identification. What is needed is that the dependent variable is neither orthogonal to nor collinear with the covariate of interest (see Wooldridge (2002)), which is true for all measures.

Finally, we reiterate that all the estimations with inventory do not say much about why inventory is higher or lower—*i.e.,* which exogenous variables (sales, production, or writeoffs) are driving inventory down. This is why we undertake explicit tests of

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variable.

various mediators to see the pathways with which FYE leads to low inventory.

2.2 Robustness Tests

For robustness, we consider the possibility that demand seasonality might vary by industry. In models (3) and (4), we show calendar quarters interacting with two extremes of NAICS classification, at 2 digits and all 6 digits. The main result holds: inventory is lower by about 10% in the fourth fiscal quarter.

Thus, RadioShack is not an isolated case. Inventory is about 10% lower in the fourth fiscal quarter, about $47 billion based on average quarterly values in 2006 in our dataset of U.S. firms. This raises the question: why?

3. Literature Review and Hypotheses Development

We organize previous research by the hypotheses that might explain the FYE effect. In figure 3, we summarize these hypotheses and their predictions, which are of three types:

1. Effects of how fiscal quarters affect inventory levels—*e.g.*, low in some fiscal quarter;
2. Mediators through which fiscal quarters affect inventory—*e.g.*, fourth fiscal quarters have lower inventory, via higher sales. Mediators are sometimes called channels of influence, mechanisms, pathways, or intervening variables;
3. Moderators that affect the strength of the link from fiscal quarter to inventory—*e.g.*, fourth fiscal quarters have especially lower inventory for the

sub-sample of firms with stronger bonus incentives to reach sales targets. Moderators are sometimes called cross-sectional predictions or interaction effects.

3.1 Baseline Hypothesis: Sales Timing

With sales timing, executives' private benefits lead them to pull some next-quarter sales into fourth fiscal quarters, depleting inventory in a way that is not compensated by increased production or purchasing. There is past research on why and how this happens:

Why? There are at least five motivations for sales timing. One has to do with sales bonuses. Joseph and Kalwani (1998) find that 95% of 215 senior and sales executives surveyed are rewarded on bonuses, largely structured as non-linear functions of sales levels determined at FYE. Such bonus structures could lead executives to time sales so as to make targets. This phenomenon has been studied as the hockey stick effect in Chen (2005) and Sohoni, et al. (2005) and as push contracts in Lariviere and Porteus (2001) and Taylor (2006).

Another motivation is the time value of bonuses (Jensen and Murphy (1990)). Bonuses associated with a sale right after the current fiscal year might be paid at the end of the next fiscal year, so there is incentive to book that sale before the current fiscal year ends.

Third, the equity market is more sensitive to financial figures in the fourth fiscal quarter than other quarters (Collins, et al. (1984), Mendenhall, et al. (1988)). If executives avoid drops in their firms' equity prices—perhaps because of their equity interests (Jensen and Murphy (1990))—then they also have incentives to time sales into the fourth fiscal quarter.

Fourth, executives are more likely to resign just after getting their year-end bonuses (Blakemore, et al. (1987)), so there is incentive to time sales into the current fiscal year if the bonus associated with a next-year sale after the resignation is discounted or forfeited.

Finally, executives might be motivated to reach sales goals even if these are not associated with explicit bonuses. There might be career concerns (*e.g.*, Holmstrom (1999)) or simply psychological motivation (*e.g.*, Locke and Latham (2002)).

How? Just how does sales timing cause inventory to be lower at FYE? This requires: (1) sales to be higher and (2) production not replenish inventory at the higher rate. On the former, Oyer (1998) shows that, at the industry level, sales are 2.6% higher in FYE. But how do firms enhance sales? One possibility, as Oyer (1998) also shows at the industry level, is that firms cut prices by 1.6% at FYE. His finding is supported by Nevo and Wolfram (2002), Larkin (2006), Roychowdhury (2006), and Chapman and Steenburgh (2007), as well as the large trade promotion literature, such as Krishnan, et al. (2004). Oyer (1998) and Chapman and Steenburgh (2007) also show that timing is especially prevalent in durable goods industries, since it is harder to get customers to take perishable inventory.

It has also been suggested that production might not replenish inventory to compensate for the higher sales. One reason is that the sales department is not necessarily aligned with production. For example, Porteus and Whang (1991) point out that a sales department might want higher sales, but a production department is incentivized to keep inventory low to reduce holding costs. Second, even where incentives are aligned, production often relies on demand forecasts provided by the sales department. If these forecasts are used to set sales targets, and given the sales department's incentive to meet sales targets, its forecasts—especially those at FYE—are often sandbagged (see Davis and Mentzer (2007) for a review). Working with these low-balled forecasts, production might not be able to produce or buy enough to maintain inventory levels. Third, the kind of bonus incentives described as motivations for sales timing is much less prevalent among production functions, in the U.S., the U.K., or Australia (Heywood, et al. (1997)). Using Australian establishment data, Drago and Heywood (1995) report that bonuses apply to just 1.1% of the workforce.

Predictions. Taken together, the above imply specific predictions (recall figure 3):

- Effects. **P1a:** *Inventory is lower at FYE.* This follows from our discussion of how sales timing leads to lower inventory at FYE.
- Mediators. **P1b, P1c:** *FYE leads to lower inventory via lower margin and higher sales.* This also follows directly from how sales timing works.

- Moderators. The following are from our discussion of why firms want to, can, and are sometimes prevented from sales timing.
  - **P1d** *FYE effects (P1a) are stronger when executive pay has a higher bonus portion.*
  - **P1e:** *FYE effects are stronger for firms in durable good industries.*
  - **P1f:** *FYE effects are stronger for firms under less scrutiny.* Such scrutiny might be by auditors or regulators, for example.

Post-FYE Effect. We now turn to the observation from the U.S. dataset in the previous section, in which inventory is abnormally *high* in first fiscal quarters. In sales timing, customers might stockpile that goods pushed to them at FYE that demand is dampened in the next quarter. A firm also has less visibility on inventory "stuffed" down the supply chain, since there is likely to be stuffing by competitors, too (Armony and Plambeck (2005)). Finally, returns might also be more likely. In the more egregious cases, firms might even provide favorable terms to customers for returns, a practice called round-tripping—*e.g.*, the SEC (2004a) alleges that Bristol-Myers Squibb provides such guarantees, so that $35 million in inventory was returned to the company right after FYE in 2001.

These suggest the following additional predictions (figure 3 again):

- Effects. **P1g:** *Inventory is higher post-FYE.*
- Mediators. **P1h:** *Post-FYE leads to higher inventory via lower sales.*
- Moderators. The moderators for FYE effect also apply to the post-FYE

effect.

For clarity, we use "FYE effect" to mean lower inventory at FYE and "post-FYE effect" to mean higher inventory in the period right after FYE.

3.2 Confounding Hypotheses

Our point in this section is that the confounding hypotheses have predictions that are sufficiently different than those of sales timing that we can empirically identify sales timing. To reiterate, sales timing and these confounding hypotheses might not be mutually exclusive. Our goal is only to ascertain whether sales timing is a cause of inventory's FYE effect, and not to necessarily rule out the confounding hypotheses. We explain our focus on sales timing at the end of this section.

Sales effort hypothesis. Even with the motivations for sales timing just described, executives could simply exert more effort to generate sales at FYE, without having to pull in sales that might naturally occur in the next quarter (Basu, et al. (1985), Kocabiyikoglu and Popescu (2007)). There is also empirical literature supporting a sales effort story. However, much of these investigate sales effort averaged over time, and not specifically at FYE—*e.g.*, Bratkovich and Steele (1989) and Lazear (2000). An important exception is Steenburgh (2004), who finds that—for an office products manufacturer—"lump-sum bonuses primarily motivate salespeople to work harder" (pg. 1).

The sales effort story predicts that (figure 3 again):

- Effects. **P2a:** *Inventory is lower at FYE.* As an alternative explanation, sales effort has this same prediction as sales timing;
- Mediators. **P2b:** *FYE leads to lower inventory via higher sales.* However, unlike sales timing, there is no prediction of lowering margins to enhance sales;
- Moderators. **P2c**: *FYE effects (P2a) are stronger when executive pay has a higher bonus portion.* However, while sales timing is easier for firms pushing durable goods down the supply chain, there is no such prediction here in the sales effort story. Nor is there any prediction about firms under greater scrutiny.

Stock taking hypothesis. In this story, FYE is associated with activities that mechanically lead to lower inventory. For one, FYE audits are often when firms write off inventory, so that could explain lower reported inventory at FYE. Importantly, FYE audits are mandated by accounting guidelines while interim quarter audits are often not: "even when regulations require a review of interim earnings by auditors, the review can be done at the time of the annual audit" Basu, et al. (2001), pg. 4). The write-offs at FYE could be larger those at interim quarters because FYE audits are more conservative (Basu, et al. (2001)) and the write-offs in the interim quarters tend to be unreported and get booked only at FYE, in a process often called "settling up" (AICPA (1973)).

One other way in which inventory might be lower at FYE is that firms often produce less, to simplify the "sight audit" of inventory.

Stock taking then produces the following predictions:

- Effects. **P3a**: *Inventory is lower at FYE.* Again, this is why stock taking is a possible explanation for the FYE effect.
- Mediators. **P3b, P3c**: *FYE leads to lower inventory via higher write-offs and lower production.* This is a prediction not from sales timing or sales effort.
- Moderators. We find no consensus on how the FYE effect under stock taking would be moderated, so we make no explicit prediction. For example, there seems to be no clear prediction for the sales bonus moderator. And unlike sales timing, the FYE effect could be *weaker* for firms in durable goods if write offs are greater for perishable goods. It is also *weaker* if with more scrutiny; firms more diligently write off in the interim quarters rather than "settle up" only at FYE.

FYE setting hypothesis. This is a story of endogeneity, in which firms set their FYE to when inventory is the lowest. One motivation for this is that equity markets assign higher valuations to firms with low or decreasing inventory—*e.g.*, Thomas and Zhang (2002), Lai (2006). Also, the equity market is more sensitive to figures disclosed at FYE than at other times (Collins, et al. (1984), Mendenhall, et al. (1988)), and if executives have interests in their firms' equity prices, then firms have incentives to set the FYE to when inventory is lowest or have decreased the most. The same mechanics applies not only to low inventory, but to high sales, so if high sales are linked to low inventory as described above for sales timing, we again have FYE setting.

Firms might also "use a fiscal period to attempt to measure performance at a time when they have concluded most operating activities" (Stickney and Weil (2000), pg. 102), which could be the time when inventory is most depleted. It is also easier to conduct inventory audits and more resources are freed up to close the year's accounting books.

The FYE setting hypothesis has one only prediction:

- Effects. **P4a:** *Inventory is not lower at FYE, after accounting for endogeneity.* This is different than the earlier predictions.

There are still other possible hypotheses, but these are either variants of the above or can be easily ruled out; we discuss these in Appendix A.

We conclude this section by pointing out that of all the hypotheses, sales timing seems to have the most deleterious impact on firm valuation. Sales timing involves margin discounting, more returns, and shifting of sales that could damage brand equity. Sales effort, on the other hand, might even be a positive, if the extra effort produces incremental revenues that outweigh the bonus payment. Stock taking seems neutral, because lower inventory just more accurately reflects the true inventory level. FYE setting also seems neutral, since it involves just an accounting choice in setting the FYE. To reiterate the point made in the introduction, it is for these reasons that we focus on sales timing.

We now turn to tests of the predictions. In figure 3, we summarize all key findings in the rightmost column. The evidence is consistent only with sales timing, and is not consistently explained by the confounding hypotheses.

4. Test of "Effects" Predictions Using a German Natural Experiment

We first test for sales timing by investigating the "effects" predictions.

4.1 A Natural Experiment

The biggest empirical challenge is to rule out endogeneity in FYE setting. To be sure, our U.S. results in table 2 already address endogeneity to a degree, and it is one of the arguments used in Oyer (1998). The idea is that if endogeneity is present, then finer controls of calendar quarters, using interactions with finer industry classifications, should reduce the significance of the FYE effect. This is not the case in models (3) and (4) in table 2. Still, this evidence is indirect. Another approach is to see if the FYE effect is still significant after firms change their FYEs, as in RadioShack. The identifying assumption is that demand patterns do not change *as quickly as* the FYE changed, over one year. In regressions using specification (A) but only the 32 firms in the U.S. dataset that changed their FYE, we find that inventory is 15.4% (p=0.000) lower at the old and new FYEs. Unfortunately, from Factiva news reports on the circumstances surrounding FYE changes, we find that the changes are often made to synchronize FYEs after mergers and acquisitions of companies, so there might be confounding changes in product lines that affect inventory patterns.

We find a unique natural experiment that can more cleanly rule out FYE setting. In 2000, Germany reduced its corporate tax rate from 40% to 25%. Consider a hypothetical firm with FYE in the middle of the calendar year, as in figure 4. The law stipulates that firms pay the tax for the full fiscal year at the rate at the start of that year. This is unlike many tax reforms, such as those in the U.S. The left panel shows the firm if it does not change its FYE. It pays 40% for two fiscal years (indicated by the horizontal full black lines), and 25% thereafter. The right panel shows the firm if it changes its FYE in 2000 to end in calendar 2000. It pays the lower 25% for the full 2001 year, capturing the tax savings shown. Thus, some German firms set their new FYE to end with the calendar year. This new FYE setting is *not* exogenous to factors such as taxable income or the cost of FYE change. It is, however, plausibly exogenous to inventory patterns, our dependent variable.

4.2 Data

We start with a panel dataset of all 661 German manufacturers, wholesalers, and retailers in CapitalIQ. The data is spotty on quarterly inventory data, which we hand-code from primary sources—annual and quarterly reports, direct communications with the firms. Among the 76 firms whose FYE are not already at the end of the calendar year, 19 change their FYE. The rest have not changed presumably because the tax benefit is smaller than the cost of the change. We summarize the data in table 3.

One complication is that we obtain only total inventory, not finished goods inventory. In the U.S. data, the correlation coefficient between finished goods and total inventory is 0.78 when measured in absolute levels, and 0.94 when measured as quarters of COGS. So this issue may not be material, but our results are subject to this qualification.

4.3 Empirical Approach

Our empirical model is a modification of specification (A), with an interaction indicator that is coded 1 for observations after the 2000 tax change and 0 otherwise:

$$\text{(C)} \qquad v_{it} = \sum_{f=1}^{4} \iota_{it}^{f}\phi^{f} + \sum_{f=1}^{4}\left[\iota_{it}^{f}\phi^{f} \times \mathrm{I}\left(after2000\right)\right] + \mathrm{I}\left(after2000\right) + \sum_{c=1}^{4} \iota_{it}^{c}\chi^{c} + \gamma_{y} + \kappa_{i} + \eta_{it}.$$

We are interested in ϕ^{4} and ϕ^{1} (the latter for the post-FYE effect). Further, we expect that the interaction terms are zero—*i.e.*, ϕ^{4} and ϕ^{1} effects are maintained over the FYE change. For firms that change their FYE (and for firms that already have their FYE at calendar year end), the interaction terms pick up the post-change updating of *both* ϕ's and χ's. But in practice, these interactions are likely to be just ϕ updating. First, we have no *a priori* reason to believe that the χ update is anything but zero. Second, in a direct estimation of (C) without the ϕ's and with the χ's interacted with I(*after2000*), we find that the χ update is indeed statistically indistinguishable from zero (p=0.56). Third, if we find the interaction terms to be statistically indistinguishable from zero (as is the case, see below), it is unlikely that the ϕ update is not zero and is exactly cancelled by an opposite χ update.

To be conservative, in this as well as subsequent estimations reported here, we measure ϕ^4 and ϕ^1 using $\phi^2 = \phi^3$ as a baseline, so the resulting ϕ^4 and ϕ^1 estimates can be interpreted as deviations from both bases, rather than just from ϕ^2, as in the previous U.S. estimation. We also undertake estimations with just ϕ^2 as a base and the results are similar. Further, the ϕ^3 estimate is always economically or statistically insignificant, or both.

Given the small sample size, we report results using first differences (FD) rather than fixed effects (FE) estimation because the bias of FD estimators is independent of sample size, while that for FE vanishes at the reciprocal of sample size. Also, FE estimators are more sensitive to non-normality of the disturbance term (Wooldridge (2002)). For robustness, we also execute FE estimation, which produces similar results and are unreported.

The small sample size means that our test has low power, but that suits our empirical objective since it only biases us against finding sales timing as a cause even when it is one. Another issue is potential sample selection bias. We attend to that after the results below.

4.4 Results

In Table 4, panel (a), we report our FD estimates under specification (A). The

FYE effect of 21.5% is maintained over the FYE change.[4] This is not explained by the FYE setting hypothesis. We also note that, as in the U.S., there is a post-FYE effect that is not explained by the sales effort and stock taking hypotheses (**P2a** and **P3a**). Taken together, these results are consistent only with sales timing (**P1a** and **P1g**). We also note that the interaction terms are not economically and statistically significant, as predicted. Given the small sample, the *p* value for the entire specification is unsurprisingly high.

We are concerned that firms might have anticipated the tax change, and conversely, inventory pattern changes might have a lag after the change. But our estimates are robust to various pre-change and post-change windows, with the former at various end years (1998 through 2000), and the latter, various start years (2000 through 2002).

4.5 Treatment Selection Bias

The small sample size may involve treatment (*i.e.*, whether a firm changes its FYE) selection bias. Heckman (1979) notes that there are two types of potential bias: selection on unobservables and on observables. Specification (C) is a treatment equation, and conceptually, there is also a selection equation that models how firms select to change their FYE. Selection on unobservables arises from correlation

[4] That the German effect is larger than the U.S. one is not germane to our empirics, but is consistent with the large "law and finance" literature (*e.g.*, La Porta, et al. (1998)) suggesting that the German commercial code provides weaker corporate governance than common law in the U.S.

between the disturbances in the two equations, biasing our estimation. Selection on observables arises from omitting selection variables in the treatment equation.

Following Heckman (1979), we address selection on unobservables with a Heckit procedure. Since we have panel data, we modify his first stage estimation to use a population-averaged probit model for the selection of observations—see Kyriazidou (1997):

(D) $$\zeta_{it}^{*} = \pi_{it} + \omega_i + \mu_{it},$$

where ζ^*_{it} is a latent continuous dependent variable representing the selection, π_{it} represents covariates that could explain the selection, ω_i accounts for unobserved firm fixed effects, and μ_{it} is white noise. We then define a dichotomous dependent variable:

(E) $$\zeta_{it} = \begin{cases} 1 & \text{if } \zeta_{it}^{*} \geq 0 \\ 0 & \text{otherwise.} \end{cases}$$

We include several covariates in π_{it}. The first is taxes saved, calculated as in the blue area in figure 4, based on firms' reported taxable profits. The second is the counter-balancing cost of changing FYE, using log COGS as a proxy. The third is an indicator for whether a firm already has its FYE at calendar year end, in which case there is no need to change FYE. For the first stage, we run specification (D) with ζ_{it} on all observations.

The second-stage regression is standard, and includes the inverse Mill's ratio as an additional covariate in the treatment equation. We obtain a Chi-square of 79 (p=0.000) in a test for exclusion restriction. The result is in table 4, panel (a), model (2). It is almost identical to our FD estimates. The inverse Mill's ratio is insignificant, suggesting that there is not a large bias in the first place.

To address treatment on observables, we use the propensity scoring method proposed by Rosenbaum and Rubin (1983). The method is designed for only one treatment at a time, so for us, it is more natural now to test for the ϕ^4 and ϕ^1 effects separately. We do that in a differences-in-differences framework. Consider ϕ^4. We ask whether after 2000, the fourth *calendar* quarter (*i.e.*, also the fourth *fiscal* quarter then) has lower-than-annual-average inventory than the fourth calendar quarter before 2000 the "first difference":

(F)
$$F_{it} = \begin{cases} F_{it}(0) & \text{if } C_{it} = 0 \text{ (no change)}, \\ F_{it}(1) & \text{if } C_{it} = 1 \text{ (change)}. \end{cases}$$

F_{it} equals how far inventory at the fourth calendar quarter is below the annual mean, for firm i in calendar quarter t. C_{it} is whether the firm changes its FYE—*i.e.*, the treatment. We want the average treatment effect (ATE):

(G)
$$\tau_{it} = E\left[F_{it}(1) - F_{it}(0)\right].$$

To address selection bias, we compare the first difference for treated firms against that for untreated firms—hence, the "second difference." The difficulty, of course, is that we observe $F_{it}(1)$ only if firm i in the treated group and $F_{it}(0)$ only if it is in the untreated group.

The propensity scoring method develops a score as the probability of selecting into the treated group, conditional on some observable matching covariates M_{it}. We use as covariates the three used in the Heckit procedure, plus firm fixed effects. In robustness tests, we build the propensity score using just year 2000 data (no firm fixed effects, but focused at the year of change) and we obtain similar results

Heckman, et al. (1998) note that the method performs well under three conditions:

1. *Ignorability.* Selection is ignorable conditional on the matching covariates:

(H) $$C_{it} \perp \left(F_{it}(1), F_{it}(0)\right) \middle| M_{it} ;$$

2. *Common support.* The intuition is that we require the probability distribution of the matching covariates to be bounded away from zero for the treated observations, on the range of values taken by the untreated observations:

(I) $$\exists \varepsilon > 0 : \varepsilon < \Pr\left(C_{it} = 1 \middle| M_{it} = m_{it}\right) < 1 - \varepsilon \text{, for all } m_{it} \text{ in the support of } M_{it} ;$$

3. *Heterogeneous distributions.* The propensity score distribution of the treated is skewed toward higher values than that of the untreated.

We have addressed ignorability with our Heckit procedure (and find no significant bias from that). To address the last two conditions, we implement *k*th-neighbor matching with caliper restrictions, so as to contain matching within specific ranges.

In table 4, panel (b), we see that inventory is 19.8% lower at FYE and 16.9% higher post-FYE. The estimates are robust to calipers from 0.001 through 0.5, and *k*th-neighbors from 1 through 20. We also report (not in the table) that for ϕ^4, the treatment effect on just the treated (called ATT) and untreated (ATU) are similar, at -0.193 and -0.206 respectively. For ϕ^1, these are both 0.169. These similarities suggest that the treated are representative and endogeneity is probably not a big issue, as is the case with the U.S. dataset.

Taken together, the above provide a clean test that sales timing is a cause of the FYE effect, and at the magnitudes estimated, a sizeable cause at that.

5. Tests of Mediators and Moderators

We now turn to tests of mediators and moderators (figure 3 again). We report results using the U.S. dataset because it has higher power, it appears that endogeneity is not significant, and the German dataset has limited variables. Wherever variables are available in the German data (COGS, margin, and production as mediators), we run the estimations on it and find qualitatively the same results.

5.1 Mediators

We describe our empirical approach, data, and results.

Empirical approach. We follow Baron and Kenny (1986) and test each mediator by running two estimations: regressing the mediator on specification (A) covariates and estimating specification (A) with the mediator as an additional covariate. From these estimations, we construct a Z statistic to summarize the presence of mediation. We construct three versions of Z proposed—Sobel, Goodman, and Aroian—and obtain the same significance on each. Here, we report the Aroian Z because MacKinnon and Warsi (1995) show that it does not assume that the multiple of the standard errors from the two estimations are vanishingly small and it performs best in Monte Carlo studies.

Data. We use COGS as a measure of sales, to separate sales from gross margin, our other mediator. Gross margin is defined as one minus COGS divided by revenues. Production is defined as change in inventory plus COGS. We do not have inventory write-off data, but only general write-offs, so that write-off results must be viewed with caution.

Results. In Table 5, panel (a), we report the results of our tests. The evidence is that the FYE effect on lower inventory is mediated by higher sales, as predicted by sales timing and sales effort, but not by the other two hypotheses. The FYE effect is also mediated by lower gross margin and the post-FYE effect, by lower sales. These are both predicted by sales timing (**P1c** and **P1h**) and not by any of the confounding hypotheses, not even by sales effort. Finally, we find weak or no evidence of mediation via write-offs or lower production (**P4b, P4c**). This is not only supportive of sales timing, but it is also not supportive of stock taking.

5.2 Moderators

In figure 3, we have predictions for three moderators: bonuses, durability, and scrutiny.

Empirical approach. The bonus test checks if the FYE effect is stronger for firms whose executives have higher bonus components. There are two standard ways to check this: interact the fiscal quarter effects in specification (A) with a bonus measure, or estimate specification (A) using sub-samples with high and low bonuses and see if the FYE effect is stronger in the high-bonus sub-sample. We do both but report results from the latter, which does not assume that high- and low-bonus firms have the same covariate estimates or the same distribution in their disturbances (Brame, et al. (1998)). It is less information-efficient but with our large U.S. dataset, the estimation will not be too noisy.

The durability test checks if the FYE effect is stronger for firms in durable goods industries. The mechanics is the same as for the bonus test.

For even sharper results, we report here a test that interacts the bonus *and* durability sub-samples, instead of tests with them as univariate partitions of sub-samples (which we do, with stronger results than reported here).

Finally, we test the "scrutiny" moderator with an event study and estimate if a firm's FYE effect is weakened after it faces a federal class action suit. We employ this different empirical approach because we are concerned about reverse causality: a strong FYE effect might lead to greater scrutiny. Reverse causality is less likely in the durability test, and in the bonus test, if it happens, it works in our favor since a strong FYE effect might lead to compensating *lower* bonus as a component of total compensation.

Data. We construct a concordance of our U.S. dataset with the three moderators. We obtain firm-year bonus data from ExecuComp. ExecuComp provides bonus data at the executive-year level, so we construct our firm-year bonus measure as the median executive bonus (as a percent of the executive's total compensation) each year. We include only executives with an explicit sales or marketing function. Given that ExecuComp captures bonuses for only the top few executives (up to 15, median is 5 executives), this is only a proxy for sales bonuses company-wide, so our result is subject to this caveat. We tag each observation in our U.S. dataset with an indicator of whether the firm is in a durable good industry, as defined by the U.S. Census. The Census defines a good as durable if its life expectancy is three years or more. Finally, we obtain federal class action suits from the Stanford Securities Class Action Clearinghouse. We include all 39 suits in all years available (1996-06) that have at least one of these words: sale*, revenue*, inventor*, bonus*.

Result. In Table 5, panel (b), we report the results of FYE and post-FYE effects in four sub-samples constructed by dividing firm-quarter observations using the median bonus and durability. The top-left sub-sample, with higher bonus and in durable goods, has the strongest effects, and bottom-right has the weakest. We compare the effects across every pair of sub-samples using standard errors as in

Brame, et al. (1998) and Paternoster, et al. (1998), and the differences are as predicted. For example, the top-left of the FYE matrix is significantly different (p=0.08) from the bottom-right. It is also different (p=0.09) than the bottom-left, which is less significantly different (p=0.12) than the top-right. The difference along the bonus dimension for the FYE effect is consistent with sales timing (**P1d** in figure 3) and sales effort (**P2c**), but is not explained by confounding hypotheses. The difference along the durability dimension and the results for post-FYE effects fit only the sales timing predictions (**P1d** and **P1e**), and is not explained by others, not even by sales effort.

For robustness, we divided the bonus dimension into not just two fractiles, but three or four. The results are qualitatively the same and are not reported here.

In Table 5, panel (c), we show the results of interactions of the fiscal year effects with an indicator for whether observations are before or after the suit. The results are robust to various definitions of before- and after-windows, as well as to whether we include the year of the suit in either window, or neither. In models (1) and (2), we show two examples where the before window is years (-19,-2)—where 0 represents the year of the suit—and the after windows are (0,1) and (0,5).

In model (1), we estimate these firms have a staggering 33.1% lower inventory at FYE before the suit. After the suit, this inventory dip is reduced by 17.6 percentage points. It is this that is consistent with the scrutiny prediction in sales timing (**P1f** in figure 3), and is not explained by other hypotheses. Also, one interpretation is that the 17.6% represents the explanatory power of the confounding hypotheses, and the difference (33.1 – 17.6 = 15.5) represents that of sales timing. That would suggest that sales timing is at least as important as other explanations. The estimates in model (2), with a longer after-window, are similar. We do not find significant post-FYE effects. This might be due to noise with the small number of observations.

6. Financial Implication of the FYE Effect

We first show evidence that FYE and post-FYE effects are associated with lower firm value. These effects might be picking up broader characteristics like governance or could be proxies for other characteristics like operational competence, so we run firm fixed effects regressions that partial out time-invariant characteristics. Since these characteristics might also change over time, we next dig deeper to directly check that lower valuation is due to lower gross profits and higher costs. Lower gross profits could arise if price discounting is not compensated by higher sales. Higher costs might arise if inventory fluctuations lead to higher inventory holding costs, and sales fluctuations to higher capacity investments.

6.1 Fiscal Quarter Effects and Valuation

For the reasons before, we report results using the U.S. dataset. Our estimations using the German data produce qualitatively the same results and are not reported here.

In first-stage regressions, we estimate firm-specific FYE and post-FYE effects using specification (A) without the firm effects term. In the second stage, we see if firms with stronger effects have lower valuations. We prefer that the latter include firm fixed effects to account for unobserved firm heterogeneity, so we run our first-stage regressions on periods of the dataset to obtain a time series of effects.

In the second stage, we follow standard q regressions by Shin and Stulz (2000). We use Tobin's q, a standard measure of valuation, and regress it on the first stage FYE and post-FYE effects, with firm fixed effects and the log of total assets. The industry-adjusted q is the sum of total assets and market capitalization, less common equity and deferred taxes, as a deviation from the industry median q. We use the NAICS 3-digit industry classification.

In Table 6, panel (a), we see that 1 percentage point in FYE effect (lower inventory) is associated with 1.74% lower valuation. The post-FYE elasticity of valuation is -3.56, signed as expected. Here, the first-stage regressions use the dataset divided into 4 periods, but the result is robust to dividing into 5 through 10 periods. We also use lagged FYE effects to address possible reverse causality and obtain similar results. The result is robust to Fama and MacBeth (1973) estimation in the second stage, which accounts for serial correlation.

The higher elasticity associated with post-FYE effects is intriguing. We conjecture, but leave for future research, that this is consistent with two forces: (1) a sign effect, in which the equity market is more concerned about inventory peaks at post-FYE than troughs at FYE, because the former are due to lower sales and the latter to higher sales (achieved with lower margins); and (2) an uncertainty effect, in which inventory peaks are unambiguously bad (due to sales timing) but troughs might be good or bad (part of it due to sales timing, part to any or all of the confounding hypotheses).

Although the elasticities are large, we make no claim about whether shareholders suffer. For example, shareholders may not have suffered if they have priced the lower valuation at the time of contracting—*i.e.,* when they buy into the firms' shares (Christie and Zimmerman (1994)). Nevertheless, the valuation elasticities suggest opportunities for *improving* firm valuation, which we discuss in the concluding section.

6.2 Plausible Explanations of q Regressions: Gross Profits and Costs

Why would the FYE and post-FYE effects lower valuations? We suggest two reasons:

- ***Lower gross profits.*** Table 6, panel (b) shows how much sales (COGS) and gross margin—mediators from the previous section—vary at fiscal quarters. At FYE, sales are 4.9% higher and margins 3% lower; gross profits are 0.5% higher, although that is not statistically distinguishable from zero. But post-FYE, sales are 4.8% lower while margins are not any higher; gross profits are 5.0% lower. The net 5% loss in gross profits paints a picture of sales timing being injurious to the firm's long-term valuation, and is consistent with the literature on managerial short-termism—*e.g.*, Lai (2006).

In unreported estimations, we explicitly test gross profit as a mediator in the previous q regression and find that it does play that role—*e.g.*, regressing log gross profit on fiscal quarter effects produce the predicted signs (both negative) and including log gross profit into the q regression eliminates the significance of fiscal quarter effects in panel (a).

- ***Higher costs.*** This could arise from two other factors:
  - *Inventory fluctuations increase Inventory holding costs.* If cost is a convex function of inventory, then varying inventory implies higher cost. Our findings suggest that firms are not factoring sales timing into inventory management in post-FYE, resulting in higher inventories;
  - *Sales fluctuations increase capacity investments.* Since we find sales timing as a cause, sales vary, too. That in turn means capacity investments might be more than if there were no FYE effect.

We are not ready to estimate the financial impact of the above, but it seems reasonable that, with the FYE effect on the order of 10%, the financial impact could be significant.

7. Discussion and Conclusion

This study has several limitations and paths for future research. First, we have not considered the relative importance of sales timing versus the confounding hypotheses. The evidence is that sales timing is an important cause, but we have not really rigorously examined the relative importance among hypotheses. Second, it would be interesting to consider interactions between hypotheses. For example, are sales timing and sales effort complements or substitutes? Third, we have not rigorously considered inventory carrying costs and capacity investments. A more rigorous treatment of these and welfare effects in general is needed. Finally, it would be intriguing to test the FYE effect on other operational measures, such as capacity utilization in service industries, or R&D spending.

We conclude with some example implications:

- *Research.* One implication for empirical work is that structural estimations of inventory levels with covariates that correlate with fiscal quarters should include the latter, or suffer omitted variable bias. For example, if one wants to use seasonality factors to forecast demand, the estimation of these factors should partial out fiscal effects. Separately, it is also desirable to enhance models of inventory management to account for sales timing in particular and agency issues in general, not just between firms and their suppliers, but also between firms and their shareholders.
- *Practice.* These implications for practice are more for shareholders than executives, since we find that the FYE effect is caused by sales timing, an agency behavior by executives themselves. For shareholders, we may divide the implications into two types:

- *Screening firms for investing*. With the large elasticity of valuation to FYE effects, private equity investors might find it attractive to acquire firms with high FYE effects and reduce these after acquisition. Conversely, public markets investors might consider low FYE-effect firms if there is a market flight to quality. While agency behavior in general is difficult to observe for outsiders, inventory and sales levels are publicly released data, and with appropriate empirics, FYE effects can be estimated. Furthermore, one can rely on mediator and moderator effects (margins, product durability) to ascertain the magnitude of inventory's FYE effect.
- *Improving valuation after investing*. One of our more interesting findings is that firms do not appear to account for sales timing in their inventory management post-FYE. Correcting this seems to be a candidate for improvement that could lead to lower inventory holding costs. One could also run through the why's and how's for sales timing in section 3.1 to design a program to improve valuation. For example, one of the why's is that both firm and equity market overweight financial disclosures at FYE; this could be mitigated with more emphasis on interim reporting. Another example might be to increase scrutiny, perhaps with independent board members and rotating auditors. Interestingly, our q regressions—which account for industry effects—suggest that it is possible to have lower FYE effects and higher valuation even if competitors do not.

This study extends previous research—on sales seasonality, inventory seasonality, and sales FYE effects—to inventory's FYE effect. We find that sales timing is an important cause and measure the financial implications. We hope others will build upon these findings to develop a more complete picture of how firms actually manage inventory.

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Figure 1 RadioShack Inventory Over Time

The vertical axis is RadioShack level in finished goods inventory, measured in quarters of cost-of-goods sold (COGS). The squares show inventory at the last quarter of *fiscal* years and the circles, at other quarters. Until 1991, the fiscal year ends in the second *calendar* quarter. After that, it changes to end in the fourth *calendar* quarter.

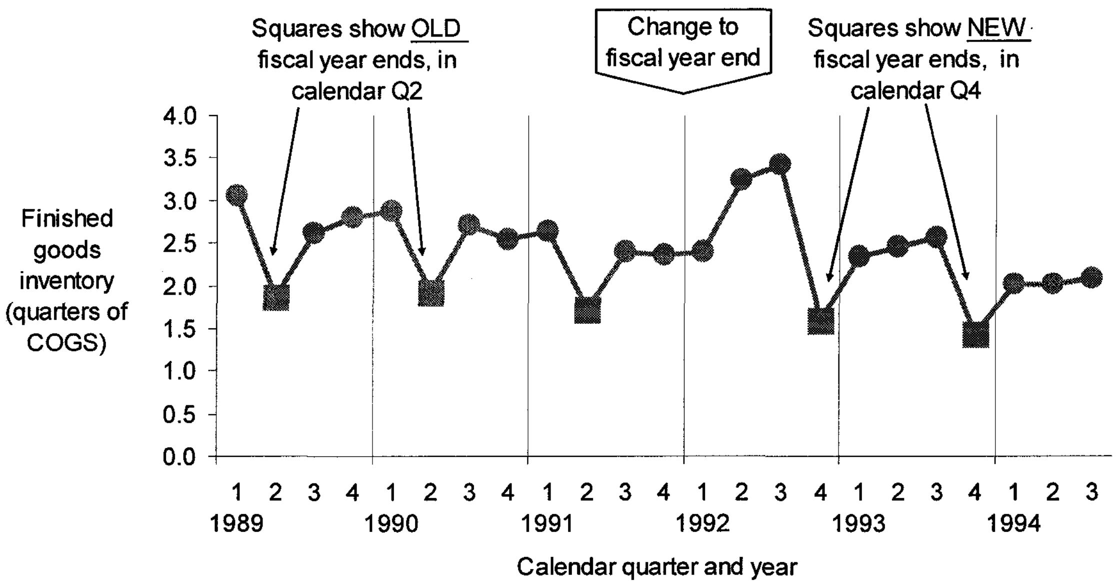

Figure 2 Inventory's Fiscal Year End (FYE) Effect in the Context of Current Research

Citations in cells are example studies, but are not meant to suggest number of studies. There are considerably more studies on calendar year than fiscal year effects.

	Calendar year effects ("seasonality")	**Fiscal year effects**
Sales	Fisher and Raman (1996) Bitran and Mondschein (1997) Fisher, et al. (2006) Taylor (2006)	Oyer (1998) Nevo and Wolfram (2002) Steenburgh (2004) Larkin (2006)
Inventory	Nerlove, et al. (1993) Rajagopalan and Malhotra (2001) Gaur, et al. (2005) Netessine and Roumiantsev (2007)	*Current study*

Figure 3 Hypotheses, their Predictions, and Findings

"FYE" is fiscal year end, econometrically implemented as the last quarter of the fiscal year; "post-FYE" means the quarter after that. A dash means no prediction.

		Baseline hypothesis	Confounding hypotheses			
		Sales timing	**Sales effort**	**Stock taking**	**FYE setting**	**Findings**
Effects	*FYE inventory is*	**P1a.** Lower	**P2a.** Lower	**P3a.** Lower	**P4a.** Not lower+	Lower
	Post-FYE inventory is	**P1g.** Higher	-	-	-	Higher
Mediators	*FYE leads to lower inventory via*	**P1b.** Higher sales **P1c.** Lower margins	**P2b.** Higher sales	**P3b.** Higher write-offs **P3c.** Less production	-	Higher sales Lower margins
	Post-FYE leads to higher inventory via	**P1h.** Lower sales	-	-	-	Lower sales
Moderators	*FYE and post-FYE effect stronger for firms with*	**P1d.** Higher bonus % **P1e.** Durable goods **P1f.** Less scrutiny	**P2c.** Higher bonus %	-	-	Higher bonus % Durable goods Less scrutiny

+The idea here is that observed lower inventory at FYE is a result of poor econometrics. After properly controlling for endogeneity, inventory is not lower at FYE.

Figure 4 German Tax Change

In 2000, Germany reduced its corporate tax rate from 40% to 25%. Consider a hypothetical firm that starts its fiscal year in the middle of the calendar year. The laws stipulate that firms pay the tax rate for the full fiscal year, depending on rate at the start of that year. The left panel shows the firm if it does not change its fiscal year end (FYE), so it pays 40% for two fiscal years (indicated by the horizontal full black lines), and then pays 25% thereafter. The right panel shows the firm if it changes its FYE in 2000 to end in calendar 2000. Therefore, it pays the lower 25% for the next full 2001 year, capturing the tax savings in blue.

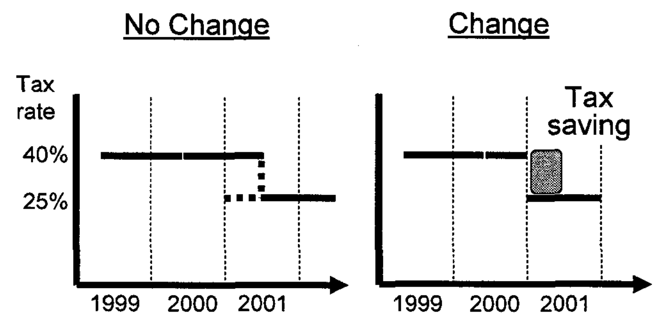

Table 10 Summary Statistics (U.S. Dataset)

The data is an amalgam from the COMPUSTAT quarterly financials tape, COMPUSTAT annual financials tape, Standard & Poor's ExecuComp database, US Census data on whether an industry sells durable goods, and the Stanford Class Action Suit Clearinghouse. Each observation is a firm-quarter, for all 2,512 U.S. manufacturers, wholesalers, and retailers (NAICS codes 31 through 48) in COMPUSTAT. The data is for 1984 through 2006. All monetary amounts are in US$ millions, unless otherwise indicated.

(a) Variation of fiscal year ends among firm-quarters

FYE*	No. of firm-quarters	%
1	8,544	29.8
2	2,367	8.3
3	3,434	12.0
4	14,318	50.0
Total	28,663	100.0

* The FYE is in one of four *calendar* quarters.

(b) Firm-quarter observations

Variable	Obs	Median	Std. Dev.	Min	Max
Calendar year	28,663	2005	5.60	1984	2006
Inventory (qtrs of COGS)	28,663	0.92	12.16	0.0004	955
-raw mat	13,642	0.30	4.03	0.00	410
-WIP	13,673	0.11	2.45	0.00	235
-fin gds	28,663	0.58	10.75	0.0002	929
COGS (US$ mil)	28,663	71.62	2728.77	0.001	109657
Sales net (US$ mil)	28,663	115.07	3453.23	0.001	126477
Gross margin	28,663	0.33	29.94	0.00	1.0
Production (US$ mil)	27,414	73.77	2441.02	-601.13	68838
Write-offs (US$ mil)	28,663	0.00	36.48	-3800.00	105
Bonus (median executive, % of total comp.)	9,112	0.30	0.22	0.00	0.90
In durable goods industry (indicator)	28,585	0.63+	0.48	0	1
Face class action suit (indicator)	28,663	0.09+	0.28	0	1

+ These are means, not medians, which are more meaningful for indicator variables.

Table 2 Identifying the FYE Effect (U.S. Dataset)

We estimate the reduced form:

$$v_{it} = \sum_{f=1}^{4} \iota_{it}^{f} \phi^{f} + \sum_{c=1}^{4} \iota_{it}^{c} \chi^{c} + \gamma_{t} + \kappa_{i} + \eta_{it}$$

where v_{it} is the inventory level, measured as inventory divided by quarterly COGS, of firm i in fiscal quarter f, calendar quarter c, and calendar year t. ϕ^f is the effect on inventory of being in fiscal quarter f, and ι's are indicator variables. χ^c is the effect on inventory of being in calendar quarter c. γ_y and κ_i are calendar year (indexed by y) and firm fixed effects, and η_{it} is assumed to be white noise. The base fiscal quarter is ϕ^2 and the base calendar quarter is χ^2. Estimation is done in log form and clustered around firms. Figures in brackets are Huber-White sandwich robust standard errors.

Variable	(1)	(2)	(3)	(4)
Fiscal quarters				
ϕ^4		-.103***	-.094***	-.098***
		(.015)	(.014)	(.015)
Before (ϕ^3)		.025*	.025*	.026*
		(.013)	(.014)	(.014)
After (ϕ^1)		.049***	.055***	.054***
		(.013)	(.013)	(.013)
Calendar quarters				
χ^4	-.113***	-.051***		
	'(.013)	(.015)	×	×
Before (χ^3)	.039***	.027**	2-digit	6-digit
	'(.009)	(.013)	NAICS	NAICS
After (χ^1)	.027***	-0.001		
	'(.010)	(.014)		
Calendar yr effects	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y
N	28,663	28,663	28,663	28,663
F	6.5	11.9	10.2	11.4
p	.000	.000	.000	.000

*** = significant at the 1% level, ** at the 5%, * at the 10% level.

Table 3 Summary Statistics (German Dataset)

This panel dataset of 661 German firms is hand-coded from primary sources—annual and quarterly interim reports, direct communications with the firms—and from CapitalIQ. Tax savings is calculated as in the blue area in figure 4, based on firms' reported taxable profits and an assumed corporate tax of 40%.

Variable	Obs	Median	Std. Dev.	Min	Max
Calendar yr	8,944	2,003	1.99	1,997	2,005
I(not December)	8,944	0.16+	0.36	0	1
I(change FYE)	8,944	0.05+	0.21	0	1
Inventory (qtrs of COGS)	8,944	0.75	10.51	0	385
Tax savings (E mil)	8,944	0.00	64	-38	865

+ These are means, not medians, which are more meaningful for indicator variables.

Table 4 Test of Effects Predictions: Natural Experiment from Germany

The dataset consists of German firms summarized in the previous table.

(a) First Difference and Heckit Estimations

The dependent variable is log inventory. The model is:

$$v_{it} = \sum_{f=1}^{4} \iota_{it}^{f} \phi^{f} + \sum_{f=1}^{4} \left[\iota_{it}^{f} \phi^{f} \times \mathrm{I}(after2000) \right] + \mathrm{I}(after2000) + \sum_{c=1}^{4} \iota_{it}^{c} \chi^{c} + \gamma_{y} + \kappa_{i} + \eta_{it}{}'$$

where the notation is as in the U.S. dataset (see table 2). I(*after*2000) is an indicator for whether a firm-quarter observation is after year 2000, the year of the German tax reform. Estimation is done in log form and clustered around firms. Figures in brackets are Huber-White sandwich robust standard errors.

Variable	(1) First Difference	(2) Heckit
ϕ^4	**-.215***	**-.215***
	(.113)	(.114)
ϕ^1	**.147****	**.147****
	(.065)	(.065)
I(After)	.001	.001
	(.001)	(.001)
$\phi^4 \times$ I(After)	.000	.000
	(.001)	(.001)
$\phi^1 \times$ I(After)	.000	.000
	(.001)	(.001)
Inverse Mill's ratio		.017
		(.029)
Calendar qtr effects	Y	Y
Calendar yr effects	Y	Y
Firm fixed effects	Y	Y
First stage probit unrestricted log likelihood		-166.8
N	262	262
F	1.3	1.2
p	.291	.364

(b) Average Treatment Effect (ATE) Estimates using Propensity Scores

The dependent variable is log inventory. We test for the ϕ^4 and ϕ^1 effects separately, using a differences-in-differences framework. For the ϕ^4 effect, we calculate for each firm-year the deviation of fourth calendar quarter inventory from annual average. Then we consider how different post-change deviations are from pre-change ones, comparing this difference for treated versus that for untreated firms.

Variable	(1)	(2)
ϕ^4	**-.198***	
ϕ^1		**.169***
Firm fixed effects	Y	Y
Caliper	.25	.25
*k*th-neighbors	20	20
N (on and off support)	82	76
-Treated on common support	52	55
-Untreated on common support	29	21
Selection model		
-Log likelihood	-50.2	-43.6

*** = significant at the 1% level, ** at the 5%, * at the 10% level.

Table 5 Tests of Mediators and Moderators Predictions (U.S. Dataset)

(a) Mediators

We use the U.S. dataset for these estimations. The predictions refer to those in figure 3. The Aroian Z is:

$$Aroian\ Z = \frac{a \times b}{\sqrt{b^2 s_a^2 + a^2 s_b^2 + s_a^2 s_b^2}}$$

where a is the estimate of the fiscal quarter effect in a regression of the mediator on specification (A) covariates and b is the estimate of the mediator when it is included in specification (A) as an additional covariate. The s's are standard errors of these estimates.

Prediction	Relationship	Mediator	Aroian Z	p-value
P1b, P2b	ϕ^4 to lower inventory	Higher sales (COGS)	7.92	.000
P1b	ϕ^4 to lower inventory	Lower gross margin	5.79	.000
P1g	ϕ^1 to higher inventory	Lower sales (COGS)	2.21	.000
P4b	ϕ^4 to lower inventory	Higher write-offs	1.91	.057
	ϕ^4 to lower inventory	Less production	1.22	.222

(b) Moderators: Bonus × Durability

The dependent variable is log inventory; we run specification (A) on four sub-samples of the U.S. dataset, partitioned by firms' bonus component paid to their executives (as a percent of total compensation) and durability of goods sold by the firms. The bonus data is from ExecuComp and durability data from the U.S. Census. Estimation is done in log form and clustered around firms. Figures in brackets are Huber-White sandwich robust standard errors. Total N for all four cells is 19,556.

	FYE effect (ϕ^4)		*Post-FYE effect* (ϕ^1)	
	Durable	**Not**	**Durable**	**Not**
Above median bonus	-.145*** (.027)	-.135*** (.022)	.051*** (.015)	.033** (.014)
Below	-.112*** (.024)	-.108*** (.027)	.031* (.019)	.037* (.021)

(c) Moderator: Scrutiny

The dependent variable is log inventory; we run specification (A) on the U.S. dataset, combined with federal class action suits obtained from the Stanford Clearing House. I(afterSuit) is an indicator for whether an observation is after the suit has been filed, so the comparison is for fiscal quarter effects in the before- versus after-window. Estimation is done in log form and clustered around firms. Figures in brackets are Huber-White sandwich robust standard errors.

	Windows (0=year of suit)	**(1)**	**(2)**
	Before	*(-19,2)*	*(-19,2)*
Variable	*After*	*(0,1)*	*(0,5)*
ϕ^4		**-.331 (.083)*** **	**-.311 (.086)*** **
ϕ^1		-.056 (.067)	-.05 (.066)
I(suit)		.033 (.185)	.011 (.158)
ϕ^4 × I(afterSuit)		**.176 (.082)** **	**.166 (.087)** **
ϕ^1 × I(afterSuit)		.01 (.062)	.049 (.055)
Calendar qtr effects × NAICS 3 digits		Y	Y
Calendar yr effects		Y	Y
Firm fixed effects		Y	Y
N		1,082	1,826
P		.000	.000

*** = significant at the 1% level, ** at the 5%, * at the 10% level.

Table 6 Financial Implication of the FYE Effect

Estimation is done in log form and clustered around firms. Figures in brackets are Huber-White sandwich robust standard errors.

(a)—Association with Tobin's q

The dependant variable is industry-adjusted q, as a deviation from the industry median q. We use the NAICS 3-digit industry classification. ϕ^4 and ϕ^1 are firm-specific effects in time series obtained from OLS first-stage regressions, using 4-year periods. In this version reported here, only effects significant at the 10% level or better are treated as non-zero. The result is robust to treating all estimates as non-zero, or at the 5% level.

Variable	Industry-adjusted q
ϕ^4 effects	**1.74 (.996)***
ϕ^1 effects	**-3.56 (1.59)****
Log total assets	-.274 (.175)
Firm fixed effects	Y
N	302
F	2.32
p	.077

(b)—Association with Sales, Gross Margin, Gross Profit

The dependent variables are in the column headings.

Variable	(1) Sales (COGS)	(2) Gross margin	(3) Gross profit
ϕ^4	**.049 (.011)*****	**-.030 (.013)****	**.005 (.833)**
ϕ^1	**-.048 (.008)*****	**-.005 (.007)**	**-.050 (.013)*****
Calendar qtr effects	Y	Y	Y
Calendar yr effects	Y	Y	Y
Firm fixed effects	Y	Y	Y
N	28,403	27,497	27,497
F	36.5	1.5	30.7
p	.000	.041	.000

*** = significant at the 1% level, ** at the 5%, * at the 10% level

Appendix A—Further Hypotheses

There are three possible hypotheses beyond those in the main text. These can be thought of as variants of the ones in the text, so to keep the paper brief, we do not listed them there.

One is a variant of sales timing, in which firms cut margins but the effect is not within-customer—moving sales from post-FYE into the FYE quarter—but across—*i.e.* taking share from competitors. In this version, there is no prediction of higher post-FYE inventory, mediation of post-FYE inventory via sales, or moderation by being in durable goods. As our results show, there is evidence for all these, so that is not explained by this story.

The second variant is the idea of strategic customers, in which the FYE effect might be induced by customers seeking deals. This most likely happens only if sellers have the incentive for sales timing, so this is really another way to look at sales timing.

A third possible hypothesis is that the FYE effect for manufacturers might be really due to sales timing by downstream retailers who end their fiscal years at the same time as the manufacturers. It turns out that manufacturers and retailers generally have different FYEs. While manufacturers generally end their FYEs in the last calendar quarter, retailers do so in the first, a result of a 1933 recommendation by the U.S. National Retailers Federation. For example, in table 1, panel (a), among the 29.8% percent of observations with FYE in the first calendar quarter, 4.5% are manufacturers and 25.1% are retailers. Conversely, among the 50% of observations with FYE in the last calendar quarter, 32.3% are retailers and 16.3% are manufacturers (the small difference is made up by wholesalers).

Chapter 2 Inventory Signals*

How does operational competence translate into market value, when firms cannot credibly communicate their competence to an investor? I consider the example of inventory and fill rates. When the investor sees a high-inventory firm, she cannot tell whether the inventory is due to incompetence or a strategy to enhance fill rate. Based on this incomplete information, she has to decide how to value the firm. Based on the investor's decision algorithm, high-competence firms that might otherwise pursue a high-inventory high-fill-rate strategy face the decision of whether to carry less inventory, so as to signal competence to the investor. What holds in equilibrium? I show conditions for separating and pooling perfect Bayesian equilibria. I also provide empirical evidence consistent with three predictions of this theory of inventory signals. First, the investor rewards what she can *observe*: lower-inventory firms have higher valuation, all else being equal. Second, the investor punishes firms when their inventory is *revealed* to be bad, such as when it is being written off. The drop in valuation is more than the size of the write-off, suggesting that such revelations have informational value. Third, in industries where equilibria are more likely to be separating than pooling, the inventory-to-valuation link is sharper. This theory that firms signal their competence with inventory levels has practical implications for how firms might strategically communicate to the investor, reward managers, or even whether to go public and be subject to investor pressures.

1. Introduction

How does the market value firms' operational investments? In many settings, the payoffs from these investments are not only uncertain, but also have long time

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lags. For example, Ford and Mazda's joint investment in flexible manufacturing capability is expected to produce savings "over the next decade" (*Automotive Intelligence News* 2003, cited in, Chod and Rudi (2005)). Linking investments to payoffs is especially hard for investors and analysts, because of information asymmetry: the latter do not have even the vague measures that insiders could examine. At the same time, there are good reasons why some firms may sustain high levels of investment. For example, these could be in anticipation of future sales. Yet, the investor may believe that firms also have bad reasons to over-invest. Firms' managers may have private benefits from investments (*e.g.*, Jensen and Meckling (1976)). Or they over-invest because they are incompetent or over-confident (Malmendier and Tate (2005)). With uncertain and lagged payoffs, information asymmetry, and possibly bad investment decisions, firms cannot credibly communicate the true worth of the investments to the investor. Conversely, the investor has to make some guesses about the valuation of these investments. Knowing this, firms have incentives to invest less to get higher valuations, and knowing that, the investor values firms' investments differently than if firms do not manage them down. What holds in equilibrium?

I describe equilibria in the setting of inventory, which can be described as an investment to prevent stockouts (as in the traditional newsvendor model) or enable sales (inventory at shop windows can prompt sales; Ferdows, et al. (2004)). When the investor sees a high-inventory firm, she cannot tell whether the inventory reflects incompetence or an investment to enhance fill rate. Competent firms decide whether to signal their competence to the investor by holding less inventory than they might otherwise, thus distinguishing themselves from incompetent firms who cannot lower inventory as easily. For these competent firms, signaling gets better valuation in the short term, but is costly in the long term if the first-best strategy is to maintain higher fill rates and higher inventory. The decision to signal hinges on this balance between short-term benefits and long-term costs. I describe conditions for which competent firms signal, so that a separating perfect Bayesian equilibrium (PBE) obtains, and when they do not, so that a pooling PBE is observed. In short, this is a theory that inventory has a signaling role.

I also report empirical evidence for this theory using a panel dataset of firms drawn from the merged CRSP-COMPUSTAT tapes, IRRC, I/B/E/S, ExecuComp, and First Call. Specifically, I test three predictions of the theory, regarding the existence of an incentive to signal (high-inventory firms do not get better valuations), short-termism (firms care about short-term valuation), and information asymmetry (firms cannot credibly communicate the value of their investments to the investor).

To test the first—an incentive to signal—I run fixed effects regressions of valuation on inventory. If inventory and valuation are positively correlated, then that falsifies the prediction that high-inventory firms suffer. But I find that it is low-inventory firms that get better valuations, consistent with an incentive to signal. The difference is economically significant. For example, the difference in Tobin's q

between the lowest- and highest-inventory firms is 0.94, which is on the same order as the q mean (0.40) and standard deviation (1.65). I handle endogeneity issues with the use of a variety of lag structures. The estimations are also robust to different measures of valuation (*e.g.*, buy-and-hold returns) and inventory (*e.g.*, total vs. finished goods inventory, scaling). Valuation is also estimated with a number of controls, such as those variables consistent with the story that the investor is simply assigning higher valuations to better firms, which tend to carry less inventory. Nevertheless, this result alone could have alternative explanations. For example, the higher valuation accorded to low-inventory firms could be the result not of signaling in rational investor, but the outcome of an inefficient investor. However, none of the alternative explanations involve short-termism and information asymmetry, so tests of these can be used to rule out the non-signaling explanations.

To test short-termism, I investigate the model's prediction that shorter-term industries have more separation. In one test of this mechanism, I check if industries in which the average executive has more stock and options holdings (interpreted as more short-term) exhibit greater sensitivity of valuation to inventory (more separation). The result holds up. It is also economically significant. One standard deviation in stock holdings changes the data-centered sensitivity by 54% of its standard error.

To test information asymmetry, I investigate the model's prediction that when asymmetry is suddenly reduced, such as when firms are forced to write off bad inventory, they suffer a drop in valuation more than what the size of the write-off itself implies. In other words, a write-off announcement has informational content about the investment competence of the firm. The model further predicts that drops in valuation should be larger in industries with more pooling. With more pooling, more competent firms get mixed up with incompetent ones, so that the pooled valuation is predicted to be higher. Firms that are revealed to be incompetent will suffer bigger falls from this higher pooled valuation. I report empirical evidence consistent with this, using event analyses that take care of confounding informational effects of write-off announcements, possible leakage before the announcements, the fact that firms tend to bunch up bad announcements, *etc.* I find that post-announcement valuations are only 78% of those predicted if there were no signaling.

Summing up, the evidence from all three tests is consistent with a signaling story.

This paper contributes to the literature that ties operational decisions to investor performance. One stream of the literature studies investor reaction to announcements. For example, Klassen and McLaughlin (1996) report that investors react positively to announcements of environmental management awards. Corbett and de Groote (2000) find likewise, for ISO 9000 certification. Hendricks and Singhal (2005a) look at reactions to supply chain disruptions and Hendricks and Singhal (1997), at awards for total quality management.

Another stream of research takes a different empirical approach. Instead of event studies, authors use panel datasets to look at correlations between operations and valuation. For example, Chen, et al. (2005) find that lower-inventory firms have better stock returns, except those with the lowest inventory levels. Netessine and Roumiantsev (2006) examine whether it is low or responsive inventory levels that contribute to superior financials such as earnings and returns on assets. Raman, et al. (2005) document a fund manager who claims to be able to detect if firms are incompetent, so that he could short them before their write-off announcements trigger large drops in valuation.

A third research stream includes theoretical work linking finance and operations. Examples include the work of Berling and Rosling (2005) on the impact of financial uncertainty on inventory policies, Buzacott and Zhang (2004) on an inventory model supported by asset-based financing, and Caldentey and Haugh (2006) on jointly optimizing firms' operational and financial hedging strategies.

This paper is also related to a fourth stream of work that looks at strategic interactions and conflicts of incentives. For example, Afeche (2005) looks at the strategic interaction between service firms, although I look at the interaction between firms and the investor. Conflicts of incentives among agents in a supply chain are surveyed in Cachon and Zipkin (1999), the review in Tsay, et al. (1999), and the special issue in Chen and Zenios (2005). The closest works related to the model in this paper are those of Ackoff (1967) and Porteus and Whang (1991). They highlight the conflict of incentives between a marketing department, which is keen to use higher inventory levels to avoid stock-outs, and a purchasing department, which is keen to have lower inventory levels to keep holding costs down. The latter paper also develops an internal futures market as an incentive-compatible solution to the problem. Others, beginning with Monahan (1984), work out pricing discounts that can induce purchasing managers to order quantities that are more optimal. Still other examples include Deng and Elmaghraby (2003), who study how buyers can use tournaments in sourcing, when they can observe only noisy signals about suppliers. Li, et al. (2005) also consider the case of information asymmetry along a supply chain. They study how asymmetry is correlated with different types of supply chain contracts. Zlobin, et al. (2003) look at information asymmetry at the retail level, and consider how moral hazard affects the financing of dental care.

While all these papers are related to this paper empirically and theoretically, they do not analyze the signaling role of inventory for publicly-traded firms, the subject of this paper. In particular, none considers an equilibrium framework, in which investors and managers make valuation and inventory decisions respectively, based on what each knows about how the other would decide.

In summary, this paper's contribution is in articulating and empirically testing a richer theory of one possible interaction between inventory and valuation decisions. It also places the interaction mechanism (signaling) on micro-economic foundations. While the setting in this paper is in inventory, it would be intriguing to see the extent to which the story here could be generalized to other operational investments with the same type of lags. For example, Boyer (1999) reports that investments in administrative, design, and manufacturing technologies enhance financial performance only after a lag. Sterman, et al. (1997) find that total quality management programs lower productivity growth in the short term even though they improve cost positions in the long term. Bharadwaj, et al. (1999) document that information technology investments provide benefits with a lag. Many more examples abound. Reducing queue length may improve customer loyalty, but only later. Longer service times at a call center now may reduce rework, again in the future (Gans, et al. (2003)). Maintaining machines now can stretch their life times, but over a long period. Training employees now may pay off in terms of higher productivity much later.

Finally, the way in which operational investments interact with the investor have practical implications for firms, such as how to strategically communicate to the investor, reward managers, or even whether to go public and be subject to investor pressures. I discuss these in the conclusion.

2. The Signaling Role of Inventory—A Simple Model

The model I describe builds on general work in signaling that begins with Ross (1973) and Spence (1973). More specifically, my model is associated with those in corporate finance, especially models of managerial myopia and career concerns. Examples of these are Holmstrom and Ricart i Costa (1986) for labor markets, Fudenberg and Tirole (1986) in predatory pricing, and Stein (1988) and Stein (1989) in acquisitions. These pioneering "myopia models" spawn a very large literature, both in theory and empirics, ranging from banking, managerial incentives, product-investor competition, capital structure, accounting, and marketing—examples of more recent work are those by Chemmanur and Ravid (1999), Prendergast (1999), Rotemberg and Scharfstein (1990), Fluck (1998), and Srivastava, et al. (1998). None, however, has considered the operations management setting.

I describe the model in the setting of inventory and fill rates, adapted from Stein (1988). Among practitioners, inventory is often considered a central issue in operations management. It "plays a key role in the logistical behavior of virtually all manufacturing systems" (Hopp and Spearman (2000), pg. 48). Victor Fung, Chairman of Li & Fung, remarks that "as far as I'm concerned, inventory is the root of all evil." (Magretta 1998) Managers also treat inventory as an important signal to the investor and a yardstick for comparison with other firms. For example, Steve Jobs declares in a 1999 analyst briefing that "last quarter, we ended with less than a day of inventory—15 hours. As a matter of fact, we've beat Dell now for the last four quarters." (Sheffi (2005), pg. 226). These points—that inventory is an important managerial concern, especially in view of managing it with a view to the investor—are also reinforced in conversations with retailers (CORE (2005), CORE (2006)) and investment executives (Raman, et al. (2005)).

As this is primarily an empirical paper, I shall use graphical and simple algebraic descriptions of the model. I also make some simplifying assumptions, such as having just two types of firms.

Figure 1, panel (a), depicts the model. There are two types of firms: competent (which I label *C*) and incompetent (*N*). Firms adopt inventory positions (horizontal axis) to achieve desired fill rates (vertical axis), as shown by the curves.[1] There is no discounting over time. All agents are risk-neutral.

In panel (a), both *C* and *N* firms adopt positions along the curves, which can be interpreted as strategic possibility frontiers. *C* firms are able to achieve any fill rate with less inventory than *N* firms can. In addition, *N* firms are defined so that they cannot go below a certain inventory level (the x-intercept in the figure). Along the frontiers, firms have exogenous long-term optimal positions. These positions, interpreted as first-best, might arise from competitive positioning (*e.g.*, Porter (1980)) or resource endowments (*e.g.*, Penrose (1959)). Firms that move away from these first-best positions incur costs.[2]

The fair valuations of *C* and *N* firms are x_C and x_N, with $x_C > x_N$. In the long term, an efficient investor will assign to these firms these fair valuations. However, in the

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[1] I do not mean that fulfilling the optimal fill rate is the only role of inventory. Other roles could be to smooth production or to take advantage of forward buying (see Arrow, et al. (1951)). I interpret achieving the desired fill rate as a proxy for what might be a collection of reasons for holding inventory. The model only needs the long-term optimum of this collection (fill rate here) to be imperfectly observable to the investor.

[2] As an example of these costs, Fisher (1997) notes that there needs to be a fit between product type (functional versus innovative) and supply chain configuration (physically-efficient versus investor-responsive). Deviations from first-best positions would be costly. Deviations could also incur a loss of complementarity with other parts of the firms (*e.g.*, Milgrom and Roberts (1995)).

short term, the investor has to estimate valuations, since the investor cannot observe whether a firm is on the upper frontier (*C* firm) or lower (*N* firm), but she can see only the inventory position on the horizontal axis. Specifically, the investor has to deduce the valuation for observable high-inventory versus low-inventory firms. Given this, C firms whose first-best positions are high-inventory-high-fill rate (to the right of the vertical dashed line) worry about the investor mistaking them for *N* firms. Therefore, they might consider shifting southwest along the frontier, from their first-best position (high-inventory-high fill rate) to one of low-inventory-low-fill rate. Such deviations from first-best (signaling) reduce these C firms' true long-term valuations xc by a cost, denoted rc. Firms use this calculus, balancing short-term benefits and long-term costs, to decide if they should deviate (*i.e.,* signal).

In the model, the tradeoff between the short- and long-term is captured in a premium m that firms place on their short-term valuation. But why does short-termism exist (*i.e.,* m>0) and what determines its degree (*i.e.,* the size of m)? Short-termism might arise, for example, because firms' managers are concerned about their short-term reputation in the job market (*e.g.,* Holmstrom and Ricart i Costa (1986)). Such firms signal using short-term observables such as lean inventory or reduced investments in customer service, at the expense of longer-term performance. Firms might also need to raise funding in the stock market, so lower inventory levels provide them with better valuation for this short-term purpose (*e.g.,* Grinblatt and Titman (1989)). Stein (1988) and Stein (1989) offer other reasons.

Managers might want to sell off their shares in their firms in the near term, so they have to ensure that their firms are not under-valued during the period. Managers fear losing their jobs if buyout raiders take over their firms, which is likely if the firms have high inventory and are under-valued; shareholders of the firms might also be forced to tender their shares for the under-valued price.[3]

It is common knowledge among firms and the investor that a fraction f of the high-inventory firms is competent. For simplicity, my analysis aggregates a firm's managers and its shareholders as one party. I discuss the implications of divergent interests and incentives between managers and shareholders in the conclusion. In this paper, signals are sent by firms to only the investor, but the story can be generalized to the extent that other capital markets rely on firms' equity valuations—*e.g.*, the debt investor uses equity valuation in collateral assessments. Although the model rules out methods of signaling other than through inventory levels, I do not mean that other signals are not useful. It does mean that signaling through inventory is "relevant at the margin" (Stein (1988), pg. 65).

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[3] The parameter m can also be interpreted probabilistically. In the takeover example of Stein (1988), for example, raiders incur some cost c of checking out target firms and if they were to takeover these firms and turn them around, the benefits v come with distribution $F(v)$. Therefore, the probability that v exceeds c is $1 - F(c)$, which is my m. As another example, much of the analyst industry is predicated on the proposition that analysts can get better information on measures such as fill rates, and do a better job of assessing the true value of firms. Raman, et al. (2005) report that Berman Capital purports to do just that. In general, c could be interpreted as the cost for reducing the degree of information asymmetry. The cost c could also be interpreted as a public policy parameter. When regulators and accounting standards require more disclosure of information, they effectively reduce c. Laws for or against firing management or takeovers can affect c. To simplify our analysis, and without loss of generality (see Stein (1988), for example), I skip F, v, and c and use the deterministic weight m.

I now describe the perfect Bayesian equilibria (PBE). Under PBE's, firms choose their inventory level given the investor's beliefs, which are in turn fulfilled by the equilibrium path. The PBE's should satisfy the intuitive criterion of Cho and Kreps (1987) off the equilibrium path.

Proposition 1 – A separating PBE satisfying Cho-Kreps exists, for some parameter values.

Figure 1, panel (b), illustrates this. Suppose it is common knowledge that the proportion of C firms that might separate is g. In the figure, this is the portion of C firms to the right of the vertical divider. To show proposition 1, I start with the observation that in a PBE in which C firms always signal, the investor has beliefs with Bayesian updating as follows: (1) if she observes that a firm has high inventory, she is sure that it is an N firm, and (2) if she observes that a firm has low inventory, she is sure that it is a C firm. In the former case, the investor values the firm simply at x_N. In the latter case, she values the firm according to the proportion of C firms that signal, $(1 - g).x_C + g.(1 - r_C)x_C$. The investor values a separating high-inventory firm at $(1 - r_C).x_C$ because she is not fooled about the cost of signaling. This follows the logic in signal jamming models such as those in Fudenberg and Tirole (1986) and Holmstrom (1999).

How do firms' actions fulfill these beliefs? By definition, N firms cannot signal. If a C firm signals, its true long-term value declines to $(1 - r_C).x_C$. In the short term, it gets pooled with C firms that do not need to signal. It puts weight m on this short-term (over-) valuation and (1-m) on its long-term valuation. If it does not signal, it gets x_C in the long term and x_N in the short-term. Given these, C firms that face signaling decisions signal when:

$$(1)\quad \begin{aligned} m.[(1-g).x_C + g.(1-r_C)x_C] + (1-m)(1-r_C)x_C &\geq m.x_N + (1-m).x_C\,, \quad \text{or} \\ m &\geq r_C.x_C \,/\, [(1-g).r_C.x_C + x_C - x_N]\,. \end{aligned}$$

The above is an expression for the break-even value of m in a separating PBE, which I denote as m_s. For $m > m_s$, the pressure to reduce inventory is so high that such C firms become myopic, so that their second-best points are at a lower fill rate than their first-best positions.

An interesting result from the above is that, in the short-term, C firms with low fill rates (left of the vertical in panel (b)) get mixed up with the other C firms that separate. To the extent that this leads to inefficiencies among the former group of C firms, the welfare effect of signaling could be larger.

Proposition 2 – A pooling PBE satisfying Cho-Kreps exists, for some parameter values.

Figure 1, panel (c) illustrates this. There is one pooling PBE in which both C and N firms do not signal. The case in which both signal is ruled out since, by definition, N firms cannot signal. In the pooling PBE, the investor has the following Bayesian updating process: (1) if she observes that a firm has high inventory, she concludes that it has an *ex ante* probability of being a C firm, (2) if she observes a firm has low inventory, she concludes that it is a C firm. The latter is the only out-of-equilibrium belief that can sustain a pooling equilibrium. Pooling is sustained if, for C firms facing signaling decisions (recalling that f is the fraction of high-inventory firms that are competent):

$$\begin{aligned} m.x_C + (1-m)(1-r_C)x_C &\leq m[f.x_C + (1-f).x_N] + [1-m]x_C, \quad \text{or} \\ m &\leq r_C.x_C / [(1-f).(x_C - x_N) + r_C.x_C]. \end{aligned} \tag{2}$$

Denote the break-even m as m_p. Pooling obtains when $m < m_p$. Depending on various values of f and g, it is easy to see that m_p could be greater than, equal, or less than m_s. Specifically, m_s is less than m_p if:

$$f < g.r_C.x_C / (x_C - x_N).$$

The various parameters, such as m, f, and x_C, are useful in determining the comparative statics, as follows. I confine this short discussion to the key conditions for inventory to have a signaling role. Each condition maps to a comparative static—a prediction—that could be empirically tested.

- *Incentive for signaling.* The model requires that the investor rewards competence. Therefore, my testable hypothesis is:

  H1: Lower-inventory firms are associated with weakly[4] better valuations.

- *Short-termism.* In the model, this is the m parameter. It is because of short-termism that firms want to signal, to get better short-term valuation. The testable

[4] The simple two-type model described does not necessarily predict a monotonic relationship between inventory levels and valuation. Indeed, in Figure 1, panel (c), all low-inventory firms have x_C and all high-inventory firms have $f.x_C + (1-f).x_N$. Therefore, the relationship might look like a step function. On the other hand, with continuous types, the relationship is monotonic. In reality, the situation is likely to be somewhere between these extremes.

hypothesis is:

H2: Shorter-term industries are associated with more separation,

where I will later describe I might measure "short-termism" in industries.

- *Information asymmetry.* This means that the investor cannot tell if inventory is used to enhance fill rate or is the result of incompetence, while firms know (or think they do). What happens when asymmetry is reduced? Inventory write-off announcements by firms have informational value—firms reveal themselves as incompetent—so the hypothesis is:

  H3a: Write-off announcements are accompanied by valuation drops that are larger than just the write-off amounts.

  Another hypothesis arises from the idea that pooling happens in various degrees. The greater is f and the larger is x_C compared with x_N, the higher is the pooled valuation compared with x_N, and consequently:

  H3b: Valuation drops accompanying write-off announcements are more severe if there is more pooling.

3. Empirical Tests

I test the four hypotheses just described. I also describe alternative explanations of the results, and how these are ruled out.

3.1. Signaling Incentive (H1): Lower-inventory firms are associated with weakly better valuations?

I use the following specification:

$$(3)\quad VALUATION_{f,t+1} = \beta_0 + \sum_{lag=0}^{l} \beta_{lag}.VALUATION_{f,t\text{-}lag} + \beta_1.INVENTORY_{ft} + FIRM\text{-}EFFECTS_f + YEAR\text{-}EFFECTS_t + \mathbf{W}_{ft}\gamma_{ft} + \varepsilon_{ft},$$

and the test is whether β_1 is non-negative as predicted. $VALUATION_{ft}$ is some suitable measure of the value of firm f at time t, $INVENTORY_{ft}$ is a suitably scaled level of inventory (and for robustness, is measured in many ways using inventory of various types, such as work-in-progress, finished goods), *FIRM-EFFECTS* and *YEAR-EFFECTS* are unobserved firm and year fixed effects, $\mathbf{W}_{ft}$ a vector of relevant controls, and ε_{ft} is assumed to be white noise. Specifically for $\mathbf{W}_{ft}$, I follow the more recent practice for q regressions, especially Gompers, et al. (2003), and include in it the log of assets and the log of firm age (Shin and Stulz, 2000), an indicator that is 1 if the firm is in the S&P 500 (Morck and Yang, 2001), and the governance index created by Gompers, et al. (2003). Because sales are potentially correlated with operations, I include the log of net sales as a control, too. To minimize endogeneity, I lag the right-hand-side, and include l lagged dependent variables. I report estimations with 0 and 3 lags, but the results are robust to other lag structures.

The data are obtained from a number of sources. From CRSP and COMPUSTAT, I obtain financial profiles of firms for years between 1950 and 2003. From IRRC, I obtain the governance index G. From Professor French's website, I obtain the factors for returns regressions. I then link all firm-year observations from these sources. I include only observations from manufacturing and the retail or wholesale sectors, since inventory is harder to interpret for other sectors. To rid the data of outliers, I winsorize values at 1% and 99%. Analyses without these two exclusions produce the same qualitative results. The estimation sample is summarized in Table 1. Because there are so few observations with governance index values, I report regressions without using these as regressors. In unreported regressions with governance indices, I obtain qualitatively similar results. Another concern is that observations dating back to the earlier years might be systematically different (*e.g.*, fewer, more likely to be measured with error). In unreported regressions, I regress with sub-samples excluding earlier data (1980- and 1990-2003) and obtain similar results. In these sub-samples, I also use a Heckman sample selection correction that exploits the availability of earlier data, and again obtain the same qualitative results.

Table 2 reports estimates using four models: retail versus manufacturing and with zero versus three lags. In this baseline model, I measure *INVENTORY* using inventory/sales, following the literature (*e.g.*, Gaur, et al. (2005)). I measure *VALUATION* using Tobin's q, following the literature on firm valuation since Demsetz and Lehn (1985) and Morck, et al. (1988). I also follow the method in Gompers, et al. (2003), and use the industry-adjusted median q, which is the firm's q minus the industry-mean, where I use the two-digit SIC classification for industry classification. In unreported regressions, I use the Fama and French (1997) forty-eight industries as classification and obtain the same results.

In all models, I would reject the signaling incentive prediction if β_1 the coefficient on *INVENTORY*, is positively signed. The table shows that signaling incentive cannot be rejected. Indeed, the coefficient is negative and is modestly statistically and economically significant. In model (1), for example, the difference in predicted q between the lowest and highest inventory firms is 0.94, which is on the same order as the q mean (0.40) and standard deviation (1.65). This is consistent with an incentive to signal. As expected, the results are more significant for retail industries, where inventory is a greater determinant of valuation (Gaur, et al. (2005)).

The estimation is robust to different measures, controls, and estimation methods:

1. *VALUATION*. Instead of q, I also measure valuation with buy-and-hold returns. The specifications for buy-and-hold return regressions follow those in Gompers, et al. (2003), in which I regress on *INVENTORY* as well as the usual Fama-French factors (*SML, HML, UMD*; please see next section).
2. *INVENTORY*. Instead of inventory divided by sales, I also use inventory divided by cost of goods sold, assets, and lagged values of these denominators. Further, I employ finer measures of inventory, at the level of materials, work-in-progress, and finished goods. I also add LIFO (last-in-first-out) reserves to inventory, so that all firms are put on an equivalent FIFO (first-in first-out) basis.
3. Control variables. Apart from the governance index, I use an indicator for whether the firm has undergone an acquisition or merger in any year prior to that of the observation, indicators for the identity of the auditor of the firm,

indicators for different audit opinions (classified into unaudited, unqualified, qualified, disclaimer or no opinion, unqualified with explanatory language, and adverse opinion), and inventory valuation methods (no inventory; FIFO; LIFO; "specific identification'; average cost; retail method; standard cost; current or replacement cost; not reported). I also include the regressors for inventory turns in Gaur, et al. (1999): capital intensity, gross margin, and sales surprise.

4. Estimation methods: Besides dealing with potential heteroscadasticity using Huber-White robust standard errors, I manage potential correlation with clustering. In fixed effects estimations, I test if random effects might be more appropriate, with Hausman tests. In return regressions, I use the standard Fama and MacBeth (1973) framework. Finally, I model the innovations with an AR(1) process to account for potential serial correlation in the disturbances.

Estimations with the above variations do not qualitatively change the findings and are not reported here. In short, the evidence is consistent with hypothesis H1.

3.2. Short-termism (H2): Shorter-term industries are associated with more separation?

I measure short-termism with some variables standard in the corporate finance literature (*e.g.*, Core, et al. (1999), Ritter and Welch (2002)). Specifically, I consider short-term holdings of stocks and options and long-term incentive plans. The data is from ExecuComp, which has data on the top 5 executives in each firm. I calculate the following for the average executive in each industry-year: (1) for stock holdings, the dollar value of stock holdings or the percent of company stock held, (2) for options, the value realized from options exercised in the next period, and (3) for long-term incentives, the amount paid based on firm performance over at least one year (usually three years). All are scaled by total compensation, including stock and options granted. For robustness, I use a variety of other measures, such as the value of options granted (rather than exercised) valued with the Black-Scholes formula, or the value of in-the-money options exercised and that unexercised. I also scale with total compensation excluding options, and total excluding both stocks and options. These produce similar results and are not reported here. These variables are shown as *SHORT-TERMISM* in the following specification, indexed by *s*, while industry is indexed by *i*:

$$(4)\ VALUATION_{f,i,t+1} = \beta_0 + \sum_{lag=0}^{l} \beta_{lag}.VALUATION_{f,i,t\text{-}lag} + \beta_1.INVENTORY_{fit} +$$
$$\sum_{s\in\{short-termism\}} [\beta_{s1}.INVENTORY_{fit}.SHORT\text{-}TERMISM_{sit} + \beta_{s2}.SHORT\text{-}TERMISM_{sit}]$$
$$+ FIRM\text{-}EFFECTS_f + \mathrm{I}(YEAR_{f,it}) + \mathbf{W}_{ft}\gamma_{ft} + \varepsilon_{ft}\,.$$

The prediction is that β_{s1} is negative for stock and options holdings and non-negative for long-term compensation. Table 3 shows the results. For retail, shown in model (1), the signs are as predicted and are statistically significant. They are also economically significant. For stock holdings, the coefficient of -1.973 could be compared with the -4.987 coefficient on *INVENTORY* alone; this would be 1.973/4.987=39.6%. For manufacturing, in model (2), the result is murkier. This is because I do not have sufficient number of observations on executive compensation. However, the signs on the interactions are all as predicted. Further, the only significant coefficient, on the interaction of *INVENTORY* and long-term compensation, is non-negative as predicted. In model (3), I use a different definition of stock holdings—the percent of company stock held by the average executive in the industry-year. As before, all the interaction variables are correctly signed. Only one coefficient, the interaction of *INVENTORY* with options, is significant, and it is also signed as predicted.

Taken together, I interpret these findings as consistent with hypothesis H2.

3.3. Information Asymmetry (H3a and H3b): Write-off announcements and valuation drops

Although there is much work in the accounting literature that tests the impact of write-offs. However, most investigate write-offs of capital investments in general, rather than inventory in particular. There are two exceptions. The first, by Francis, et al. (1996), documents investor reaction to inventory write-offs. They conduct an event analysis based on write-off announcements reported in *PR Newswire* between 1989 and 1992. Although inventory write-offs are not the focus of their paper, they do report a 31.7% drop in excess return over days -1 and 0. For our purpose, however, their result is less informative for three reasons. First, their analysis predates recent innovations in event analysis, such as the use of Fama-French factors and industry controls. Second, they do not consider the magnitude of the write-off, only whether an announcement is made. Finally, their test makes no prediction about differences in reaction between separating and pooling PBEs.

The second noteworthy work, by Hendricks and Singhal (2003; 2005; 2005), looks at, among other things, the impact of announcements of production and shipping delays on firms' investor returns. They classify these delays by cause (*e.g.*, customer-induced) and consequences (*e.g.*, quality problems). They find that returns drop by an order of 10% in the days -1 and 0 event period. Their study is therefore related to our study in that such delays could be due to overage in inventory. As the focus of their study is not on inventory, their classifications of cause or consequence are not specific to inventory levels or quality. Another difference is in methodology. I control for potential confounding informational effects in announcements, such as simultaneous announcements of earnings forecasts. I also test for reaction magnitudes conditioning on write-off amount.

To test H3a (write-off announcements are accompanied by valuation drops that are larger than just the write-off amounts?), I check if the investor reaction is *bigger* than what the write-off amount implies if there were no signaling. To test H3b (valuation drops accompanying write-off announcements are more severe if there is more pooling?), I check if investor reaction is more negative for industries that have more pooling.

I use event analyses, but need to strip the announcements of confounding informational effects. I am concerned about what might potentially confound the link between write-offs and investor reaction: (1) an event analysis is really a joint test of market efficiency (does the investor react quickly?) and the null hypotheses of interest (H3a or H3b), (2) write-off decisions could be discretionary, (3) even if write-offs are not discretionary, decisions on the timing of announcements could be, and (4) even if both types of decisions are not discretionary, announcements of write-offs are often made at the same time as earnings announcements, so the reaction may be wrongly attributed to write-offs.

Fortunately, the first issue of joint tests does not affect the test of H3a, since I am only interested in one side of the test. Only if I find a valuation drop *not* disproportionately larger than the write-off amount would I be concerned about insufficient evidence to reject H3a. Otherwise, I will have evidence to not reject H3a, and that evidence is rid of the confounding issue about market efficiency. The situation for testing H3b is harder. But it is plausible to assume that issues of market efficiency do not affect industries conditional on the degree of pooling. If so, then the test for H3b can also be cleared of the confounding issue of market efficiency.

The next two issues have largely been addressed in the accounting literature. The consensus is that firms do manipulate the timing of disclosures, but this does not have significant effects on investor reactions. The main reasons are that manipulation is limited due to litigation risks (*e.g.,* Skinner (1994), Barth, et al.

(2001)) and it is factored into investors' behavior *ex ante* (*e.g.,* Kothari, et al. (2005)). Further, I can directly address the issue of discretionary write-offs by exploiting an institutional detail. In March 1995, the accounting standards board issues SFAS No. 12 that provides less discretion on write-off decisions. Although the note focuses on long-lived assets, inventory write-offs after that ought to be also less (but obviously not totally) discretionary. Therefore, I estimate the regressions here using a sub-sample after that date, correcting for truncation. The results are similar to the ones using the full sample, so I do not report them here.

To tackle the last issue, I construct a sample rid of confounding news. I first obtain all 133,122 footnotes from First Call, and after manual inspection of the footnotes, decide to screen for those with the word "invento" (for inventory, inventories, *etc.*) and one of the following words in the footnote: "reserve," adjacent "mark" and "down," "charge," "obsol" (for obsolete, obsolescence, etc.), "write" (for write-offs, write-downs, etc.), "loss." To ensure quality of the data, the items screened out and retained are manually inspected to ensure proper exclusion. This removes one that is also associated with "facility closure," another with "restructuring," and a third with "product recall."

To eliminate the confounding impact of simultaneous earnings announcements, I restrict the sample to announcements in which there are no earnings surprises. In the results reported, I define "no surprise" as when the analyst mean consensus of expected earnings, from I/B/E/S, is within 5% of actual earnings. Other thresholds, at

0%, 1%, and 10%, do not change the qualitative results and are unreported. I also check that there are no confounding acquisitions or stock splits. If there is more than one footnote in a year, I remove all but the earliest of these. Table 4 shows the summary statistics of the sample footnotes.

An important consideration is whether the culling of the announcements leads to sample selection bias. I use a Heckman correction procedure, with a selection model as follows:

$$SELECTED_{ft} = f[WRITE\text{-}OFF_{ft}, MKTCAP_{ft}, FPE_{ft}, \mathrm{I}(PERIODICITY_{ft}), \mathrm{I}(SIC_{ft})],$$

where the regressors are the write-off amount, investor capitalization, fiscal end-date of the announcement, indicators for periodicity (*e.g.*, quarterly or annually) and the two-digit SIC code.

Next, I measure abnormal investor reaction—*i.e.*, that stripped of the usual explanatory factors such as risk. The factors that are partialled out are those proposed by Fama and French (1993) and Carhart (1997):

$$R_{it} - R_{ft} = \alpha_t + \beta_t(R_{mt} - R_{ft}) + s_t SMB_t + h_t HML_t + u_t UMD_t + \varepsilon_{it},$$

where R_{it} is the return for the ith stock at time t, R_{ft} the risk-free return, R_{mt} the investor return, SMB_t the small-medium-large factor, HML_t the high-medium-low factor, and UMD_t the momentum factor. As before, the data are from CRSP-COMPUSTAT and Professor French. I use a monthly frequency and an estimation window of 6 months. Estimations using daily data and other estimation windows, as well as other estimation models using CRSP-indexed value- and equal-weighted models, all produce the same qualitative results.

I calculate two versions of what the drop might be if there were no signaling. In a less conservative version, I impute the "bad" inventory dollar-for-dollar as the reduction in market value of the firm. In a more conservative version, I impute the write-off amount as an earnings drop. In this version, the investor assumes that the write-off will be an annual hit on the earnings of the firm. To translate this annual hit into investor return, I use the previous-month price-earnings ratio. This takes care of the worry that the drop in valuation is really about the investor's worry that there are more write-offs to come. In this paper, I report only the latter, more conservative version. Not surprisingly, the less conservative version yields stronger evidence for signaling.

In Table 5, panel (a), we report the results of the test. Each announcement is a dot, comparing its actual and non-signaling post-announcement stock prices, or equivalently (since there are no stock splits), investor valuation. Specifically, the ratio is 0.78, and is statistically significant. The t-statistic is -2.67 and the p-value is 0.0006. In other words, the actual post-announcement valuation is only 78% of what the write-off amount implies, even if the latter is aggressively considered to be an annual hit thereafter.

This result could be subject to a competing explanation that there is a "torpedo" effect (Skinner and Sloan (2002)), in which bad announcements are severely punished for growth firms which under-deliver on analyst expectations. To rule this out, I remove all firms in "growth" industries (biotechnology, drugs), I obtain the same qualitative results. The average ratio of actual to non-signaling is 0.79, the t-statistic for a test against unity is -2.06 and the p-value is 0.025.

The results of the test of hypothesis H3b is in panel (b). I use the sensitivity of q to inventory as an observable indication of whether firms tend to pool. In industries with more pooling (sensitivity is below the median), the model predicts that the post-announcement stock price is lower than that for separating ones. The result shows that firms in "pooling" industries suffer a lower valuation, at 0.64 of the valuation imputed by the write-off amount, compared with 0.77 for "separating" industries. Because of the small number of observations, the t statistic of the difference is not high. The evidence is directionally consistent with hypothesis H3b.

To wrap up, I report the results of the Heckman correction procedure to check that there is no sample selection bias in constructing the announcements dataset. The results still stand; indeed the key results are stronger. For example, the mean ratio of actual to non-signaling post-announcement stock price is lower, at 0.76, compared to the uncorrected result of 0.78.

4. Discussion and Conclusion

I propose that in a world with signaling incentives, short-termism, and information asymmetry, inventory has a signaling role. Firms and investors understand this, resulting in separating or pooling equilibria. This is one channel in which inventory translates into investor valuation. I document empirical evidence that is consistent with this signaling story. As discussed in the introduction, it would be intriguing to investigate the extent to which this theory of how inventory translates into investor valuation might be generalizable to other types of operational investments that have the same properties of signaling incentives, short-termism, and information asymmetry.

One must bear in mind that there would be situations in which the above story would *not* apply. For example, some firms like Neiman Marcus might be able to credibly communicate a high-responsiveness position to the investor, and maintain a high-inventory position.[5] More generally, firms could use repeated interactions with investors to build a reputation for high responsiveness, along the lines of Plambeck and Taylor (2006) for repeated interactions between firms. Still other examples might include firms that are covered by institutional investors, who might be more savvy about firms' strategies than retail investors (Gompers and Metrick (2001)). Of course, some of these could be interpreted as "exceptions that prove the rule." They do not apply precisely because certain conditions—information asymmetry in the examples just listed—are absent.

Conversely, and more speculatively, the theory might be a parsimonious

[5] I thank Walter Salmon for suggesting this example.

explanation for a range of disparate, observed phenomena. First, it already explains how write-off announcements might lead to bigger drops in market value than what the write-off amounts alone might suggest. Second, the theory potentially explains why some high-responsiveness firms, from the Ritz Carlton and Coutts (the private bank) to Brooks Brothers and Neiman Marcus, are or have to be privately-held, at least for long periods of their history.[6] In the financial services industry, according to Forrester Research (Beasty (2005), "whether it's banks, brokerages, or insurers, the privately owned institutions always do better at these [customer advocacy] rankings." Third, the theory could provide an additional explanation for why stock-outs might be pervasive (*e.g.*, Gruen, et al. (2002), Verbeke, et al. (1998)) even among *competent* firms[7]. If technological advance and investments are proxies for competence, it seems that increases in competence have not increase fill rate much. In 1968, *Progressive Grocer* reports that 20% of shoppers face stock-outs. About forty years of technological advances later, roughly "a third of the consumers entering a store are [still] looking for a specific item but fail to buy because they cannot find it" (see Wharton@Work (2002)). Fourth, cross-sectional analysis by Gruen, et al. (2002) reveal that the fill rate for Europe, the U.S., and other parts of the world are about

[6] As an example, the Ritz-Carlton was in private hands for much of its history since the late 1800's, from Edward Wyner and Gerlad Blakely to William Johnson. It was bought by Marriott International in 1995. Marriott, of course, is also a closely held firm (source: Ritz-Carlton corporate website). Neiman Marcus was taken private by the Texas Pacific Group and Warburg Pincus LLC in October 2005. So was Brooks Brothers, by Claudio Del Vecchio.

[7] A competing explanation, for example, is that product variety has increased (*e.g.* Gupta and Srinivasan (1998), Randall and Ulrich (2001)).

the same, despite their different competence levels. Although there could be other explanations, this situation is consistent with the view that competence alone is not a strong predictor of high fill rates. It would be a natural extension to confirm the implication of the theory advanced here with international data, where there is variation in the degrees in the incentive to signal, short-termism, and information asymmetry.

Yet another natural direction is to look at how signaling in the way described here might be applicable not with capital markets, but with others in the supply chain (*e.g.*, Iyer, et al. (2005)). One might also investigate the signaling phenomenon over time. For example, during the take-over wave of the 1980s, we expect that firms are more myopic and tend to signal their competence with lower inventory, even among competent firms pursuing high fill-rate strategies. At these times, short-term valuation could be used as currency for acquisitions or defense against takeovers.

Finally, the model has been worked out as if the firm is a monolithic, aligned entity, without agency problems between managers and shareholders. Suppose managers are keen to not only increase share price for shareholders, but also their private benefits related to inventory—*e.g.*, they might be motivated by fiscal year end sales bonuses to over-sell, as in Lai (2007). Agency theory does not seem to have a clear prediction of how the theory presented here might be modified. This could also be an interesting area for further research (see Kocabiyikoglu and Popescu

(2005) for a recent study, involving different contracts between shareholders and managers).

What is the implication of all this for firms? Any answer must obviously be set in the context of the firms' other priorities. All things being equal, one set of implications is about how to better manage valuation. Specifically, how can firms credibly communicate the motives for high fill-rate (and more generally, high responsiveness) strategies? When should firms volunteer more information, to reduce information asymmetry? When should they reduce short-termism say, by de-emphasizing short-term performance measures based on stock price?

Another set of implications is about getting publicly listed and be subject to pressures of the investor. The theory brings to light how going public might affect operational decisions.

Finally, there might be implications for policy makers. An important consideration is what the social welfare considerations are, and whether, for example, inventory disclosures ought to have the kind of details (*e.g.*, aging records) like Basel requirements for loan portfolios in banks.

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Figure 1 Model in Pictures

There are two types of firms, *C* for competent and *N* for incompetent. *f* is the proportion of *C* firms among high-inventory ones (right of the vertical dashed line), and *g* is the proportion of competent firms that might separate.

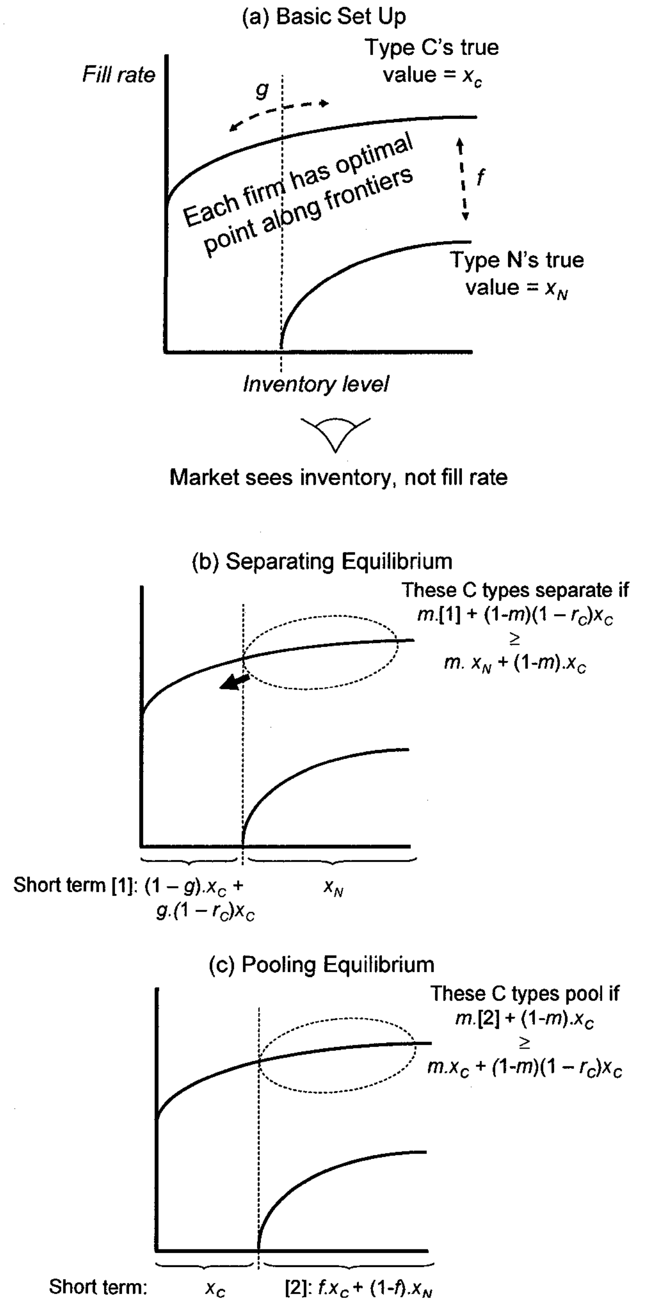


Table 1 Summary Statistics of Sample for Valuation Analysis

The data is from the merged CRSP-COMPUSTAT tapes, IRRC, I/B/E/S, and ExecuComp. Only observations from manufacturing and the retail or wholesale sectors are included. The data is also winsorized at 1% and 99%. The data is for firms from 1950 through 2003.

	Obs	Mean	Std. Dev.
Year	57,485	1,984.9	12.4
Net sales ($M)	57,485	1,453.6	3,770.1
q	57,485	4.3	6.7
Op Income before depr ($M)	55,870	173.3	858.7
Inventory - total ($M)	56,056	161.2	617.8
Inventory – materials ($M)	26,377	45.7	236.0
Inventory – work in prog ($M)	23,862	16.9	76.9
Inventory – finished goods ($M)	25,290	62.3	214.4
Inventory – LIFO reserves ($M)	40,820	22.0	159.0
Age	57,485	11.5	10.3
Investor cap ($M)	56,069	1,291.8	8,091.7
Receivables ($M)	55,746	162.1	880.7
Payables ($M)	50,581	130.3	677.3
Plant, property, equipment ($M)	54,853	82.9	414.9
Working cap ($M)	55,624	123.4	503.4
S&P 500	57,485	0.1	0.3
G index	2,137	9.2	2.8
Acquisitions	57,485	0.1	0.3

Table 2 H1: Lower-inventory firms are associated with weakly better valuations?

The specification is:

$$VALUATION_{f,t+1} = \beta_0 + \sum_{lag=0}^{l} \beta_{lag}.VALUATION_{f,t\text{-}lag} + \beta_1.INVENTORY_{ft} + FIRM\text{-}EFFECTS_f + YEAR\text{-}EFFECTS_t + \mathbf{W}_{ft}\gamma_{ft} + \varepsilon_{ft},$$

where $VALUATION_{ft}$ is some suitable measure of the value of firm f at time t, $INVENTORY_{ft}$ is scaled by sales, *FIRM-EFFECTS* and *YEAR-EFFECTS* are unobserved firm and year fixed effects, $\mathbf{W}_{ft}$ a vector of relevant controls, and ε_{ft} is assumed to be white noise. The dependent variable, *VALUATION*, is measured using the industry-adjusted median q, which is the firm q minus industry-mean, where I use the 2-digit SIC code for industry classification. *INVENTORY* is measured using inventory/sales. $\mathbf{W}_{ft}$, includes log assets, log firm age, an indicator that is 1 if the firm is in the S&P 500, and log net sales. All estimations are done with lagged right-hand-side variables, firm and year fixed effects, with Huber-White robust standard errors and clustered around firms. These estimations are also robust to other performance measures, other controls, different ways of industry classification (please see text).
*** Significant at the 1% level, ** at 5%, * at 10%. Figures in brackets are standard errors.

	Retail/wholesale	Manufacturing	Retail/wholesale	Manufacturing
	(1)	(2)	(3)	(4)
Inventory (scaled by sales)	-1.170 (.340)***	-.847 (.390)**	-.561 (.247)**	-.604 (.314)*
Log assets	-.292 (.076)***	-.369 (.068)***	-.157 (.039)***	-.183 (.052)***
Log firm age	-.462 (.057)***	-.474 (.052)***	-.136 (.054)**	-.219 (.052)***
Log sales	.018 (.067)	.010 (.055)	.012 (.027)	-.060 (.046)
S&P 500	-1.222 (.157)***	-.707 (.247)***	.242 (.121)**	-.387 (.109)***
Constant	2.048 (.320)***	1.750 (.148)***	.458 (.314)	1.059 (.117)***
Lagged dependant variables	0	0	3	3
N	18198	26619	14079	21071
Adj. R squared	39.2%	48.2%	54.4%	60.3%
p-value	.0000	.0000	.0000	.0000

Table 3 H2: Shorter-term industries are associated with more separation?

The specification is:

$$VALUATION_{f,i,t+1} = \beta_0 + \sum_{lag=0}^{l} \beta_{lag}.VALUATION_{f,i,t-lag} + \beta_1.INVENTORY_{fit} +$$

$$\sum_{s\in\{short-termism\}} [\beta_{s1}.INVENTORY_{fit}.SHORT\text{-}TERMISM_{sit} + \beta_{s2}.SHORT\text{-}TERMISM_{sit}] +$$

$$FIRM\text{-}EFFECTS_f + YEAR\text{-}EFFECTS_t + \mathbf{W_{ft}\gamma_{ft}} + \varepsilon_{ft},$$

where $VALUATION_{fit}$ is some suitable measure of the value of firm *f* in industry *i* at time *t*; $INVENTORY_{ft}$ is scaled by sales; $SHORT\text{-}TERMISM_{sit}$ is measure *s* of how short-term are firms in industry *i*; *FIRM-EFFECTS* and *YEAR-EFFECTS* are unobserved industry, firm, and year fixed effects; $\mathbf{W_{ft}}$ a vector of relevant controls, and ε_{ft} is assumed to be white noise. The dependent variable, *VALUATION*, is measured *using the industry-adjusted median q, which is the firm q minus industry-mean, where I use the 2-digit SIC* code for industry classification. *INVENTORY* is measured using inventory/sales. *SHORT-TERMISM* is measured with three variables: stock holdings, options holdings, and long-term compensation. In models (1) and (2), these are the averages per executive in the industry-year in stock holdings, the value realized from options exercised, and the amount paid based on firm performance over at least one year. In model (3), as a variation, stock holdings are measured as the percent of company stock held. All *SHORT-TERMISM* variables are scaled by the executive's total compensation, including stocks and options. $\mathbf{W_{ft}}$, includes log assets, log firm age, an indicator that is 1 if the firm is in the S&P 500, and log net sales. All estimations are done with firm and year fixed effects, with Huber-White robust standard errors and clustered around firms. These estimations are also robust to other performance measures, other controls, different ways of industry classification (please see text).
*** Significant at the 1% level, ** at 5%, * at 10%. Figures in brackets are standard errors.

	Retail	Manufacturing	Manufacturing
	(1)	(2)	(3)
Inventory	-4.987 (1.509)***	.981 (1.571)	-2.268 (3.096)
Inventory x stock holdings	-1.973 (.664)***	-.103 (.686)	
Inventory x % company stock held			-.171 (.475)
Inventory x options exercised	-1.671 (.646)**	-.255 (.632)	-1.274 (.557)**
Inventory x long-term compensation	.020 (.198)	.780 (.396)**	.231 (.406)

Table 4 Summary Statistics of Sample for Event Analysis

The data is matched from First Call (footnotes), I/B/E/S (estimated and actual earnings), and CRSP-COMPUSTAT (financials). In this sample, footnotes exclude those with earnings surprises (mean analyst estimates exceeds 5% of actual earnings per share) and confounding announcements (*e.g.*, restructuring, recalls, facility closures).

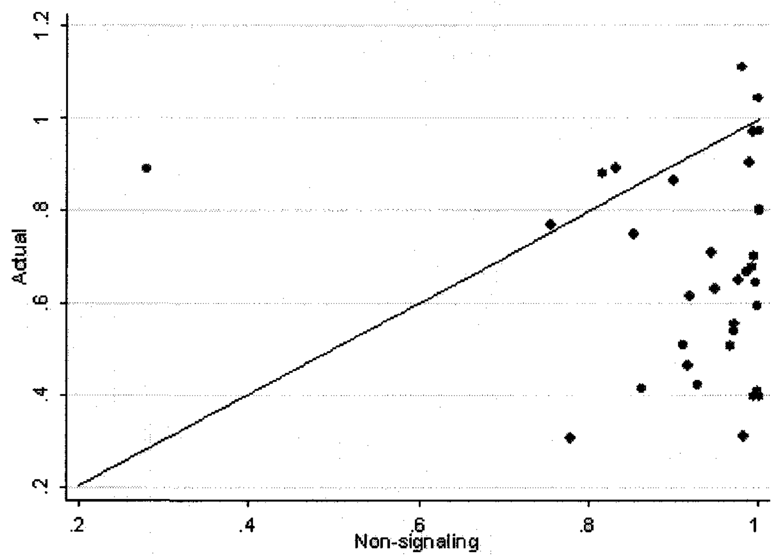
	Firm	Announced	Write-down $mil	Industry
1	Advanced Neuromodulation System	13-Aug-97	0.03	MEDICAL SUPPLIES
2	Alaris Medical Systems Inc	17-Nov-93	0.11	DRUGS
3	Alberto-Culver Co	31-Jul-95	0.02	COSMETICS
4	DIMON Inc	24-Aug-98	0.11	TOBACCO
5	Angeion Corp	19-Jan-99	0.08	MEDICAL SUPPLIES
6	Anheuser-Busch Companies Inc	11-Oct-96	0.09	BEVERAGES
7	Beverly Enterprises	20-Feb-96	0.27	HOSPITALS
8	Bioject Medical Technologies In	25-Jun-99	0.01	MEDICAL SUPPLIES
9	Brothers Gourmet Coffees Inc	18-Aug-98	0.08	FOOD PROCESSORS
10	Cantel Medical Corp	25-Mar-03	0.90	MEDICAL SUPPLIES
11	Cavalier Homes Inc	27-Jul-00	0.40	LEISURE PRODUCTS
12	Cell Tech International Inc	13-Nov-98	0.90	MEDICAL SUPPLIES
13	Chiquita Brands International I	11-Oct-94	1.10	FOOD PROCESSORS
14	Endologix Inc	30-Jan-98	2.00	MEDICAL SUPPLIES
15	Enpath Medical Inc	22-Jul-03	0.03	MEDICAL SUPPLIES
16	Exide Technologies	30-Jan-01	0.18	AUTO PART MFG
17	Falcon Products Inc	3-Sep-98	0.31	HOME FURNISHINGS
18	First Alert Inc	28-Nov-95	0.08	HOME FURNISHINGS
19	Fortune Brands Inc	14-Sep-93	0.09	HOME PRODUCTS
20	Galaxy Nutritional Foods Inc	29-Jun-00	0.90	FOOD PROCESSORS
21	Gish Biomedical Inc	15-Nov-99	0.06	MEDICAL SUPPLIES
22	GTECH Holdings Corp	9-Mar-95	1.06	LEISURE PRODUCTS
23	Innovative Clinical Solutions I	14-Sep-99	1.49	SERVICES TO MEDICAL PROF
24	Interferon Sciences Inc	15-Apr-98	0.55	BIOTECHNOLOGY
25	International Comfort Products	15-Aug-95	0.06	EAFE APPLIANCES
26	Vista Medical Technologies Inc	28-Jan-99	0.04	MEDICAL SUPPLIES
27	Knape & Vogt Manufacturing Co	1-Sep-98	0.13	HOME FURNISHINGS
28	LaserSight Inc	30-Mar-01	0.20	MEDICAL SUPPLIES
29	Laserscope Inc	22-Oct-96	0.37	MEDICAL SUPPLIES
30	William Lyon Homes	21-Aug-92	0.84	HOME BUILDING
31	McClain Industries Inc	21-May-01	0.70	AUTO PART MFG
32	Meridian Bioscience Inc	14-Nov-01	0.08	DRUGS
33	Isolyser Company Inc	13-Nov-97	0.33	MEDICAL SUPPLIES
34	Nam Tai Electronics Inc	30-Jul-01	0.67	HOME FURNISHINGS
35	Nanogen Inc	29-Oct-03	0.04	BIOTECHNOLOGY
36	Northland Cranberries Inc	22-May-00	27.00	FOOD PROCESSORS
37	Oca Inc	19-Mar-03	4.20	HOSPITALS
38	OPTA FOOD INGREDIENTS INC	25-Oct-01	0.07	FOOD PROCESSORS
39	Optical Sensors Inc	5-Nov-97	0.50	MEDICAL SUPPLIES
40	Physiometrix Inc	1-Nov-01	0.37	MEDICAL SUPPLIES
41	Pilgrim's Pride Corp	9-Mar-00	0.09	FOOD PROCESSORS
42	Polaroid Corp	9-Jun-98	0.51	LEISURE TIMES
43	Premium Brands Inc	11-Apr-02	1.60	FOOD PROCESSORS
44	RCS INVESTIMENTI S.p.A.	3-Aug-98	0.57	CLOTHING
45	Revlon Inc	8-Oct-99	280.00	COSMETICS
46	Royal Grip Inc	8-Aug-95	0.13	LEISURE PRODUCTS
47	Sicor Inc	14-Aug-97	2.60	DRUGS
48	JM Smucker Co (The)	17-Feb-00	0.11	FOOD PROCESSORS
49	Synthetech Inc	12-Nov-02	0.06	BIOTECHNOLOGY
50	TL Administration Corp	29-Nov-00	16.00	FOOD PROCESSORS
51	Trans Max Technologies Inc	12-Jul-01	0.16	LEISURE TIMES
52	Vans Inc	28-May-02	2.40	CLOTHING
53	Vivus Inc	15-Oct-98	0.50	MEDICAL SUPPLIES
54	Wyeth	18-Oct-99	0.07	DRUGS
55	Zymetx Inc	13-Oct-00	0.90	BIOTECHNOLOGY

Table 5 H3a and H3b: Write-off announcements

Panel (a) – H3a: Write-off announcements are accompanied by valuation drops that are larger than just the write-off amounts

The axes show actual (vertical) versus imputed (horizontal) drop in share price in the month of an inventory write-off announcement, in fractional terms - *e.g.,* 0.8 means 20% drop. Inventory write-offs obtained from First Call footnotes, culled using a set of phrases (see paper). Footnotes that might be confounded with earnings surprises and other confounding events (e.g., product recalls) are excluded. The remaining inventory write-off announcements are used in an event analysis using a Fama-French-Carhart four factor model (SML, HML, UMD) at the monthly frequency, using data from Ken French and the merged CRSP-COMPUSTAT tapes. The estimation window is 6 months. The actual drop is calculated from the intercept of the predicted excess-return regression. The imputed drop is calculated by conservatively attributing the inventory write-off fully to earnings drop, and using the previous-month earnings-price ratio to calculated the drop in share price.

Actual / non-signaling average ratio = .78 (test against unity: *p*-value .0006, *t*-statistic -2.67, *N*=34)



(b) – H3b: Valuation drops accompanying write-off announcements are more severe if there is more pooling

For each industry, the *q* sensitivity to inventory (per the earlier specification) is estimated. The announcements are then assigned and ranked by their industries' sensitivity. Announcements above the median sensitivity are interpreted to be in industries in which firms are more likely to separate. The means below are mean ratios of actual to non-signaling drops in stock price, calculated for announcements in separating and pooling industries.

	N	Mean	S.E.	*t*
More separating	18	.77	.16	
More pooling	19	.64	.07	
Difference		.13	.17	.74

Chapter 3 Inventory and the Stock Market*

How does the stock market affect inventory decisions? The "efficient markets" view is that low stock price means poor fundamentals, a higher cost of capital, and lower inventory. Normatively, firms should obtain their cost of capital from an efficient markets model of stock prices. My study is motivated by the growing body of evidence that the stock market is *not* efficient and can temporarily mis-value firms. I report evidence that the market's "behavioral" component explains firms' inventory as much as its "fundamentals" component. I further test three possibilities for how the behavioral component works. The first is a *financing* channel. When the market over-values firms, firms can get cheaper financing and increase inventory. The second is *dissipation*. When the market mis-values firms, firms are less disciplined and let inventories rise. The third is *catering*. When the market discounts high-inventory firms, firms decrease inventory, and vice versa. I report evidence that weakly supports financing, rejects dissipation and strongly supports catering. The findings suggest that we need to find new ways of calculating the cost of capital for operations models. They could begin to form the basis of a more empirically accurate account of how inventory decisions are affected by financial markets.

1. Introduction

How does the stock market affect inventory decisions? One view, associated with Tobin (1969), is the "efficient markets" view. It assumes that stock prices accurately reflect investment "fundamentals"—the opportunities and risks, and therefore the marginal cost of capital. This view has positive and normative implications. The positive one is its description of how stock price correlates with

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inventory: low stock price means poor fundamentals, a higher cost of capital, and lower inventory. The normative implication is that a firm can obtain its cost of capital from an efficient markets model of stock prices (see Stein (1996)). This is the cost of capital that the efficient markets view prescribes, to be plugged into foundational formulations in operations management such as the EOQ (economic order quantity) and news-vendor solutions.

My study is motivated by the growing body of evidence that the stock market is *not* efficient. In the inefficient markets view, the stock market can temporarily over- or under-value firms, even if this mis-valuation works itself out of stock prices over time. In the last decade, a vast number of empirical studies interpret their findings as evidence that markets are inefficient. Finance theory has also begun to show that it takes very lax assumptions for inefficiency to obtain. For example, inefficient markets can hold even if transactions are costless. Baker, et al. (2004), Barberis and Thaler (2003), and Shleifer (2000) summarize the theory and evidence. In the operations management literature, some studies now also report abnormal stock prices (e.g., Chen, et al. (2005)).

If true, this inefficient markets view could dramatically revise the positive and normative implications described in the first paragraph. Normatively, "it is no longer obvious that one should set hurdle rates using [the efficient market models]" (Stein (1996), pg. 431), such as CAPM (capital asset pricing model) or linear multifactor models like Fama and French (1993). The positive story of how stock prices correlate with inventory also needs to be revised, since firms' inventory decisions might be different with mis-valuation.

In this paper, I empirically test *whether* the stock market's "behavioral" component affects inventory. Second, I test *how* this happens.

To investigate the first, I test a null "fundamentals only" hypothesis, that the stock market has no impact on firms' inventory levels other than through fundamentals. To investigate the second, I consider three possible but not mutually exclusive channels through which inefficient markets might affect inventory. One possibility is a *financing channel.* For financially constrained firms, over-valuation allows them to raise financing and increase inventory to optimal levels (e.g., Stein (1996) and Baker, et al. (2003) describe similar stories for capital expenditure and dividend policies). Another possibility is a *dissipation channel,* based on the idea of shirking in principal-agent models (e.g., Baker (1992)). When the market mis-values firms, firms become less disciplined and let inventories rise. The third is a *catering channel.* When the market discounts high-inventory firms, firms decrease their inventory. When the market places a premium on inventory, firms increase their inventory (e.g., see Baker and Wurgler (2004) for catering to a dividend premium).

To test the "fundamentals only" hypothesis, my baseline model regresses inventory on behavioral and fundamental components of the stock market. The data is from the merged COMPUSTAT-CRSP tapes, ExecuComp, I/B/E/S, CDA, *The Wall Street Journal,* and a variety of other sources detailed later. I take care to address the econometric challenges in the estimation. For example, to measure unobservable variables like the behavioral component, I use a variety of measures, as in Baker and Wurgler (2004) and Polk and Sapienza (2004). Some example measures of the behavioral component are CAPM and Fama-French alphas, earnings accruals, and the closed-end fund discount. I also instrument some of these variables and use fixed effects. Another econometric challenge is potential endogeneity, which I handle with a range of lag structures. In the baseline model and all its variants, I find that the behavioral component is an important explanation for inventory, after controlling for fundamentals. A typical univariate regression of inventory on the behavioral component (measured using CAPM alpha) has an R-squared of about 24.4%. The coefficient on the component, even after partialling out fundamentals, is statistically and economically significant. One standard deviation change in the behavioral component correlates with a 10% standard deviation change in inventory. This compares well with the economic significance of the fundamentals (measured using cash flow growth), where a standard deviation change correlates with a 28% standard deviation change in inventory.

I report some evidence consistent with the financing channel but not with the dissipation channel, and strong evidence of catering. In regressions of inventory on fundamentals and the three channels, the latter account for an adjusted R-squared of 64.7%, but the bulk of this (63.7%) is by the catering channel.

These results contribute to a deeper understanding of the interaction between operations management and finance. For example, they could begin to form the basis of a more empirically accurate model of how inventory decisions are made (e.g., Netessine and Roumiantsev (2005)). This in turn might go some distance in addressing the concern "often made [about inventory models, that] any resemblances between the models constructed and reality are purely coincidental" (Whitin (1952)).

The findings can also open up new avenues of research. For example, a natural question is whether the welfare implication of catering is positive for managers, shareholders, firms, and society. With regard to the last, the resulting misallocation of resources over time is less severe than misallocation in a cross-section of firms. As pointed out by Morck, et al. (1990), misallocation in a cross-section could have much more serious consequences. For example, all department stores might be simultaneously under- or over-stock.

Finally, the findings can have important practical implications. At the macroeconomic level, Blinder and Maccini (1991) document that inventory changes account for 87% of the total peak-to-trough movement in GNP. Clearly, an understanding of how and why inventory changes is important for management of the economy. At the level of the firm, if catering is mostly a loss for shareholders, for example, it could be an important and not-yet-well-understood consideration for managerial compensation and other aspects of firm management.

2. Inventory and the Stock Market

The question of whether and how the stock market affects inventory has a parallel in the macroeconomics and corporate finance literature. However, there, the dependent variable is corporate investments rather than inventory. The literature divides into two camps. On one side, Tobin (1969) initiates a literature that relates investments to q, a summary statistic for the stock market's information about the firm's fundamental investment costs and opportunities. Market sentiment does not play a role. On the other side, theorists as early as Keynes (1936) argue that stock prices have a behavioral component so that they diverge from fundamental information about investments.

The modern incarnation of Keynes' idea is an inefficient market. It has three components. First, there are noise traders who hold beliefs ("sentiment") that cannot be rationally justified. Second, these traders' activities do not cancel out. Third, there are limits to arbitraging away the uncancelled activities. (Following the literature, I use the terms "inefficient market," "irrational market," "behavioral market," "sentiment," and "mis-valuation" interchangeably.)

Early empirical work does not have a consensus. For example, Barro (1990) argues that the stock market "dramatically out-performs" fundamental variables such as q and cash flow, in explaining investment levels. On the other hand, Blanchard, et al. (1990) state that "market valuation appears to play a limited role, given fundamentals, in the determination of investment decisions." Morck, et al.

(1990) conclude with a hedge, that "the market may not be a complete sideshow, but nor is it very central."

Recent work, such as those by Baker, et al. (2003) and Polk and Sapienza (2004), produce more persuasive evidence in favor of the inefficient markets view by investigating why inefficient markets might affect investments. Specifically, they test the channels through which the stock market might influence investments. This is the approach I take in this paper.

The first possible channel is financing. The idea is that some firms are financially constrained, so they are not able to carry the optimal amount of inventory. If the market is inefficient, temporary over-valuation of these firms allows them to raise funding at a lower cost of capital (see Brainard and Tobin (1968), Fischer and Merton (1984) for similar arguments for investments). This lower cost of capital could come directly from the lower cost of equity issuance, or indirectly by the lower cost of debt with expanded debt capacity or reduced overhang. This view is empirically distinguishable from the other channels I discuss later. Specifically, the financing hypothesis predicts that, in a cross-section, only constrained firms increase inventory; while unconstrained firms may also take advantage of over-valuation to obtain financing, this financing is unlikely to increase inventory, which is *ex ante* optimal.

Another possible channel is dissipation. The idea is that during periods of mis-valuation (either over- or under-valuation), firms let inventories rise unnecessarily, dissipating value that is otherwise captured with more efficient operations. The story is consistent with principal-agent models such as Baker (1992). In these models, effort by the agent (managers) to keep inventory optimally low is costly and inherently unobservable to the principal (shareholders). Thus, the agent's effort is only weakly linked to reward. With inefficient markets, the effort-reward link is further weakened. First, reward often depends on valuation, whether explicitly or implicitly. Valuation now has an exogenous behavioral component, adding more noise to the effort-reward link. Second, the principal reduces monitoring of inventory levels when mis-valuation encourages other priorities, such as equity issuance when there is over-valuation (hence the interaction with the financing channel) or investor communications when there is under-valuation. The dissipation channel is also empirically distinguishable from the other channel in two ways. First, in cross-sections, the dissipation channel should be stronger for weakly governed firms. Second, the financing channel predicts that inventories will be overly high during over-valuation and overly low during under-valuation, while dissipation is predicted to occur in both times of over-valuation and under-valuation.

The dissipation channel might seem to be related to the free cash-flow hypothesis of Jensen (1986), in which firms left with too much cash dissipate them with empire-building. However, the motivating factor there is excess cash rather than mis-valuation. It is also important to clarify that the dissipation channel does not suggest that there is no dissipation without mis-valuation. Instead, it predicts that dissipation increases with mis-valuation, and particularly so in weakly governed firms.

The third channel is catering. Managers cater to the interest of the stock market, even if this catering is at the expense of long-term shareholder value. Managers cater because of short-term interests, as pointed out by Stein (1988). For example, their compensation might be a function of short-term stock price. They might also need to sell off their shares in the firm periodically, so they ensure that the firm is not under-valued. Or they might keep the firm's short-term value high to avoid its being taken-over by buyout firms that might fire them. Or they might want to ensure that their reputation and worth in the executive market is high, as in the career-concerns models of Narayanan (1985) and Holmstrom (1999).

While the financing and dissipation channels depend on stock market mis-valuation in general, the catering channel depends on a specific kind of mis-valuation based on inventory. When there is an "inventory discount," managers cater to the market by reducing their inventories. Conversely, when there is an inventory premium, managers increase inventories. The inventory discount/premium is analogous to similar phenomena studied in other areas of market inefficiency, such as the small firm premium (Roll (1983)), close-end mutual fund premium (Boudreaux (1973), Lee, et al. (1991)), or the dividend discount/premium (e.g., Baker and Wurgler (2004)). This dependence on an inventory discount (if it exists) empirically distinguishes the catering channel from the other two. Another point of distinction is that the catering channel plays a bigger role in firms that are more short-term, such as those whose executives' compensation disproportionately emphasizes stocks and options. A particularly strong cross-sectional test is whether we see catering in the current period stronger among firms whose executives exercise their options in the next period.

Where does the inventory discount come from? It might arise because it is difficult to read what high inventory really means (e.g., Lai (2005)). It could mean operational incompetence and sagging sales, or it could mean a high-responsiveness strategy and good prospective sales. Further, at any one time, the beliefs of investors, especially noisy traders, tend to herd (e.g., Scharfstein and Stein (1990)). Therefore, we might see an inventory discount one time and a premium at another. Aghion and Stein (2005) provide a formal model of how this can happen.

A concluding clarification: it might seem that the inefficient market arguments above require that firms or managers to be smarter than the market to detect mis-valuation, and to ignore market signals. It is a standard result in the literature that only a less restrictive interpretation is needed (e.g., Stein (1996)). Specifically, firms could use a Bayesian combination of their private information and the market's valuation. In the catering case, firms "may just cater to, or even be forced by proxy vote to meet, extreme investor demands in general, and mis-pricing is merely a symptom of extreme investor demand. In this interpretation, managers are not knowingly outwitting the market. Their decisions will still generate return predictability, but they are not explicitly designed to capture mis-pricing." (Baker and Wurgler (2004), pg. 1155).

3. Testing the "Fundamentals Only" Hypothesis

The empirical strategy is to first test the "fundamentals only" hypothesis: whether the stock market's behavioral component (if any) affects inventory. If the "fundamentals only" hypothesis is rejected (which I will show), the second part of the empirics is to test the channels with which the behavioral component affects inventory.

The key challenges are the identification of the unobservable behavioral component, as well addressing potential endogeneity, since inventory and the right-hand-side variables can be simultaneously determined or there might be reverse causality. I address these in this section.

3.1. Data

The data is from COMPUSTAT-CRSP, ExecuComp, I/B/E/S, CDA, *The Wall Street Journal*, and a variety of other sources detailed below. I update all COMPUSTAT-CRSP data with restated values—e.g., for sales, assets, cost of goods sold. To be included in the dataset, I follow the practice in the literature (e.g., Morck, et al. (1990), Polk and Sapienza (2004), Gompers, et al. (2003), Baker, et al. (2003)): observations cannot be involved in acquisitions or mergers, the market-to-book and q must be between 0.1 and 100, sales, assets, capital expenditures, income before earnings and interest, common dividends, common equity must all have non-negative values, and outliers are dealt with by winsorizing at the 1% and 99% percentiles. The summary statistics are in Table 1.

3.2. Model

The theory that the stock market affects inventory only through fundamentals maps into an empirical specification in a straightforward way. The null is that β_2 below vanishes:

$$(1) \qquad INVENTORY_{it} = \beta_0 + \beta_1.FUNDAMENTALS_{it} + \beta_2.BEHAVIORAL_{i,t-1} + \text{firm effects} + \text{year effects} + \varepsilon_{it} \,,$$

where i and t index firms and years, and ε is white. I use this specification in two baseline models. In the first, I use differences with OLS (ordinary least squares) to make the results comparable with Morck, et al. (1990), a major paper in the field. In the second, I use levels with fixed effects, comparable with the more recent literature such as Polk and Sapienza (2004).

Except for *BEHAVIORAL*, equation (1) is essentially the standard q-theoretic investment equation (e.g., Summers (1981)). *BEHAVIORAL* is lagged while *FUNDAMENTALS* is contemporaneous because I am interested in whether the former affects *INVENTORY* beyond its ability to predict the latter. It is possible, however, that lagging *BEHAVIORAL* might provide *FUNDAMENTALS* with an informational advantage. But including contemporaneous measures of *BEHAVIORAL* (unreported) do not change the results, a finding consistent with

Poterba (1990).

When using differences, I measure *INVENTORY* using growth in inventory value, analogous to the growth in capital expenditure in Morck, et al. (1990). When using fixed effects, I measure *INVENTORY* by scaling inventory value with property, plant, and equipment, as in Polk and Sapienza (2004). For further robustness, I also use the adjusted inventory in Gaur, et al. (2005).[1]

I also measure *FUNDAMENTALS* using different variables. In the differences model, these are net sales growth and cash flow growth, following Morck, et al. (1990). These measure expected opportunities and the cost of capital, respectively. A very different measure of *FUNDAMENTALS* is to use the q variable, following Polk and Sapienza (2004). This has the additional advantage that q could capture parts of *BEHAVIORAL* (see Abel and Blanchard (1986)), so that the estimated coefficient of *BEHAVIORAL* is likely to understate its importance. However, the standard measure of observable q itself introduces the well-known problem that it is a poor proxy for marginal q. Fortunately, this is generally resolved in the literature as not an important measurement problem empirically (see, for example, Abel and Blanchard (1986)).

I measure *BEHAVIORAL* using several variables. One is abnormal stock returns.

[1] Gaur, et al. (2005) regress inventory turn (defined as cost of goods sold over inventory) on gross margin, capital intensity, and sales surprise. To facilitate comparison with other measures here, I use the inverse of inventory turn. Also, sales surprise is a market factor likely to be confounded with *BEHAVIORAL*, so I drop it in the baseline regression. In any case, adding it does not qualitatively change the results.

Following Morck, et al. (1990), I employ alphas under a CAPM (capital asset pricing model) model, using annual market and risk-free returns from Professor French's website.[2] CAPM alpha has the advantage that it could understate the effect of *BEHAVIORAL* on *INVENTORY*, because if part of the CAPM alpha is really compensation for risks, that part is a fundamental parameter (being expected, not abnormal, return).

The second measure uses the more recent model by Fama and French (1993) for abnormal returns, based on *SML* ("small minus large"), *HML* ("high book-to-market minus low"), and *UMD* ("up momentum versus down") factors, also obtained from Professor French.

The third directly measures "sentiment." I use the close-end fund discount, which has the well-known characteristic that stock price often differs from net asset value in such funds, because unsophisticated investors hold different beliefs than others. This difference is generally regarded as a measure of the market's behavioral component (e.g., Zweig (1973), Long, et al. (1990), Lee, et al. (1991)). The literature is still ambivalent about the source of this component (e.g., whether it reflects changing risk tolerance or growth expectations, see Baker and Wurgler (2004)), but my goal is to measure it rather than to achieve the more ambitious objective of studying it *per se*. Following Baker and Wurgler (2004), I obtain the value-weighted

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[2] The data is at http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html, for which I am grateful. Last accessed July 21, 2005.

closed-end fund discount from Neal and Wheatley (1998) (for years 1962 through 1993), CDA (1994 through 1998), and *The Wall Street Journal* end-of-year issues (1999 through 2000).

The fourth measure of *BEHAVIORAL* follows Polk and Sapienza (2004). I use discretionary accruals. Sloan (1996) and Teoh, et al. (1998) find that such accruals lead to lower stock returns, which can be interpreted as over-valuation. This argument relies on investors not being sophisticated enough to see through the manipulation of accruals, a fact well-documented in the accounting literature (e.g., Maines and Hand (1996)). There are many models for measuring discretionary accruals, but as Dechow, et al. (1995) show, the main models are about equally accurate, although the Jones (1991) model has the best statistical power. Therefore, I measure discretionary accruals as total accruals less normal accruals, following Jones (1991) and Teoh, et al. (1998). Total accruals are defined as:

$$ACCR_{i,t}=(\Delta[CurrentAssets_{i,t}+Cash_{i,t}]-\Delta[CurrentLiabilities_{i,t}-LongTermDebt_{i,t}])/$$

$$TotalAssets_{i,t-1}.$$

For each firm, I then derive its non-discretionary accruals by first running a cross-section regression using the firm's four-digit SIC code peers (i.e., all but itself):

$$ACCR_{i,t} = \theta_0 + \theta_1.(1/TotalAssets_{i,t-1}) + \theta_2.(\Delta sales_{i,t}/TotalAssets_{i,t-1}) +$$

$$\theta_3.(PlantPropertyEqpt_{i,t}/\ TotalAssets_{i,t-1}) + \varepsilon_{i,t}.$$

Using the predicted coefficients $\hat{\theta}$ from above, the firm's non-discretionary accruals are:

$$NONDIS\text{-}ACCR_{i,t} = \hat{\theta}_0 + \hat{\theta}_1.(1/TotalAssets_{i,t-1}) +$$

$$\hat{\theta}_2.(\Delta sales_{i,t} - \Delta accountsReceivable_{i,t})/TotalAssets_{,t-1} +$$

$$\hat{\theta}_3.(PlantPropertyEqpt_{i,t}/TotalAssets_{i,t-1}).$$

Discretionary accruals are then defined as the difference between total and non-discretionary accruals. One advantage of using discretionary accruals is that it could understate the effect of *BEHAVIORAL*. As Chan, et al. (2001) document, firms with high discretionary accruals tend have low cash flows (a fundamental parameter).

The different measures of *BEHAVIORAL* have two important properties. First, they enhance robustness because they work through different mechanisms in mis-valuation. For example, the closed-end fund discount operates through differences in clientele segments while discretionary accruals work through information distortion. Second, they are all linked to cross-sectional patterns in returns that are not well-explained by standard asset pricing models.

3.3. Results

Table 2 shows the baseline results, in models (1) through (4). These replicate the first two models in Morck, et al. (1990) and those in Polk and Sapienza (2004). Like them, I find that *BEHAVIORAL* is an important driver of inventory decisions, with both a substantial *R*-squared (24.4%, comparable to the 20.8% in Morck, et al. (1990)) and positive significant sign. Model (2) partials out *FUNDAMENTALS*, and the resulting coefficient on *BEHAVIORAL* drops substantially, but is still very statistically significant. Models (3) and (4) obtain the same, despite using different estimation methods (fixed effects) and different measures of *INVENTORY*, *BEHAVIORAL*, and *FUNDAMENTALS*. In each, the *R*-squared is significant, and *BEHAVIORAL* is once again significant and signed as predicted.

The economic significance of *BEHAVIORAL* varies. For the closed-end fund discount measure of *BEHAVIORAL*, a one standard deviation change in *BEHAVIORAL* leads to a 10% standard deviation change in *INVENTORY*, while the CAPM alpha measure produces only a 3% standard deviation change in *INVENTORY*. The important point is that these are still comparable with those of *FUNDAMENTALS*. For example, a standard deviation change in cash flow growth correlates with a 28% standard deviation change in *INVENTORY*, while for lagged q, it is just 6%.

3.4. Robustness Checks

There are some other empirical issues to address:

1. *Choice of horizon, in the difference estimations*. Morck, et al. (1990) use three-year horizons to "capture delayed changes in investment." I expect that inventories, unlike their capital expenditures, are adjustable much faster over time. Also, a longer horizon has the disadvantage that the specification is more susceptible to endogeneity. For example, in q theory, the desired level of capital stock might not follow a deterministic trend over longer periods. In any case, I regress over both one- and three-year horizons and the results are qualitatively unchanged.

2. *Additional controls.* Like Morck, et al. (1990), I use industry dummies and find that the results (unreported) are qualitatively unchanged. Furthermore, I use year dummies (reported here), whose inclusion means a more stringent test because they understate the effect of *BEHAVIORAL* if they pick up time variations of the aggregate stock market. I also cluster regressions around firms, to minimize serial correlation.

3. *Discretized dependant variables.* One problem with *INVENTORY* is that it could be measured with error. Following Morck, et al. (1990), one way to address this is to discretize it, using a dummy which is set to 1 if the change in inventory exceeds a threshold and 0 otherwise. Importantly, this also weakens the contending interpretation that small inventory changes are not the result of conscious firm policy, but involuntary changes. Model (5) shows the result of a logistic regression using a discrete version of *INVENTORY,* in which the dummy is 1 if Δinventory is more than 1.2 and 0 otherwise. The results are qualitative unchanged from the earlier models, as are those (unreported) using different thresholds for creating the dummy variable.

4. *Lag structures.* Lags help minimize simultaneity issues. I use zero to four lags in the specifications. In this paper, I report results from estimations using no lags for OLS regressions and two lags for fixed effect regressions; other specifications give qualitatively similar results, unless otherwise stated.

5. *Reversed causality and simultaneity.* Reversed causality is handled by lagging

BEHAVIORAL. It is still possible that the measures of *FUNDAMENTALS* such as sales and cash flow are simultaneously determined with the stock returns used to measure *BEHAVIORAL*. Suppose a good past return increases inventories because of lower costs of financing, but increased inventories improve sales because of better availability. Therefore, measuring *FUNDAMENTALS* with sales would pick up some effect of the influence of returns on inventories. First, this only understates the effect of *BEHAVIORAL*, thus strengthening the finding. Second, in Model (6), I use all the previous measures of *BEHAVIORAL* as instrument variables for the Fama-French alpha measure, in a two-stage least squares estimation. The *p*-value of an over-identifying restriction test is 0.03, suggesting that the 2SLS is valid. In any case, the instrumented result is qualitatively unchanged. Third, simultaneity seems unlikely from model (2), which shows that doubling sales is associated with an inventory increase of 49.3%. The mean sales-to-inventory ratio is 27.7. Therefore, most of the sales increase is unlikely to come from inventory increase.

6. *Truncation and possible sample selection bias.* One of the usual problems with using COMPUSTAT-CRSP data is truncation bias, because some firms are not documented in the earlier years. To deal with this, I use a Heckman sample selection correction. The first-stage correction model for firm-year observations is:

$$SELECTED = f(MKTCAP, S\&P500, ASSETS),$$

where *MKTCAP* is market capitalization, *S&P500* is whether the firm is ever in the S&P 500, and *ASSETS* is total assets. Model (7) shows that the result is qualitatively the same. The inverse Mill's ratio is marginally significant at 7.9%. A possible reason that the bias is small is that the long period of coverage overwhelms the shorter period of truncation. To simplify the exposition in the rest of the paper, I present results without the Heckman correction, after checking that the corrected results are qualitatively unchanged. There is also possible survivorship bias: the dataset might contain surviving firms that are different from those dropped. For this, I create a sub-sample that truncates five years out of the beginning and end of my dataset. Again, the result is qualitatively unchanged so I do not report this to save space.

4. Testing the Channels

The data for these tests are the same as that for the previous section, except for a few sources that I describe below.

4.1. The Financing Channel

I add variables measuring the volume of past equity and debt issuance:

$$(2) \qquad INVENTORY_{it} = \beta_0 + \beta_1.\ FUNDAMENTALS_{it} + \beta_2.BEHAVIORAL_{it-1} + \gamma_1.EQUITY_{i,t-1} + \gamma_2.DEBT_{i,t-1} + \text{firm effects} + \text{year effects} + \varepsilon_{it}.$$

I should see that the γ coefficients are positively signed and β_2 drops in economic significance. I measure *EQUITY* using two methods. One, following Morck, et al. (1990), divides common equity by beginning-of-year market capitalization, and a more sophisticated way, by Daniel and Titman (2003), considers equity issuance, employee stock and pension plans, repurchases, and dividends. Specifically, the latter, which I will call DT equity, can be interpreted as the (log of the) number of shares one would have at time t for every share one owns at $t-\tau$, had one reinvested all cash distributions back into the stock. It is defined as:

$$DT\ equity = log\ (M_t/M_{t-\tau}) - r(t-\tau,t)\ .$$

M_t is the per share value at t and $r(t-\tau,t)$ is the log stock return from $t-\tau$ to t, in turn defined as:

$$r(t-\tau,t) = \sum_{s=t-\tau+1}^{t} log[(M_s.f_s + D_s)/M_{s-1}]\ ,$$

where f_s is the price adjustment factor from s-1 to s that accounts for splits and rights issues, and D_s is the per-share cash distribution paid at time s.

I measure *DEBT* using book debt. *EQUITY* and *DEBT* are interpreted as specialized measures of *BEHAVIORAL* that account for the financial channel which with *BEHAVIORAL* might influence *INVENTORY*. For example, Daniel and Titman (2003) document how their measure of equity issuance predicts subsequent low stock returns, suggesting current over-valuation.

Empirically, data on these are often inaccurate, so Morck, et al. (1990) suggest using indicator variables, where *EQUITY* is 1 if the change in equity is over 5% and *DEBT* is 1 if the change in debt is over 10%. This provides a less stringent test. As will be seen, the financing channel is not well supported, so using this method allows me to see if the hypothesis is indeed weak even when given benefit of the doubt.

The financing channel makes another prediction that I can use as a still more robust test: financially-constrained (or small, as a proxy) firms should be even more susceptible to the effect of the stock market. To test this, I augment the specification in (2) to:

$$
\begin{aligned}
INVENTORY_{it} = \beta_0 + \left(\text{ as before }\right) & + \gamma_1.EQUITY_{i,t-1} + \gamma_2.DEBT_{i,t-1} + \\
& \delta_1.EQUITY_{i,t-1} \times FINANCIALLY\text{-}CONSTRAINED_{it} + \\
& \delta_2.DEBT_{i,t-1} \times FINANCIALLY\text{-}CONSTRAINED_{it} + \\
& \text{firm effects} + \text{year effects} + \varepsilon_{it}.
\end{aligned}
$$

Positively-signed δ coefficients would be consistent with the financing channel.

A measure of *FINANCIALLY-CONSTRAINED* would include standard corporate finance parameters such as firm size, firm age, leverage, cash balance, cash flow, cash volatility, and investment opportunities. Kaplan and Zingales (1997) incorporate these in an index. The advantage of this KZ index is that it is transparent and, having been built from scratch for a different purpose, is unlikely to be biased for my purpose.

4.2. The Dissipation Channel

The dissipation channel predicts a quadratic relationship in *BEHAVIORAL*:

$$INVENTORY_{it} = \beta_0 + \beta_1.\ FUNDAMENTALS_{it} + \beta_2.BEHAVIORAL_{i,t-1} + \beta_3.BEHAVIORAL^2_{i,t-1} + \text{firm effects} + \text{year effects} + \varepsilon_{it}.$$

Unlike the financing channel, the dissipation channel is motivated by an agency problem between manager and shareholder. Therefore, it also predicts that in a cross section, firms with bigger agency problems have more dissipation. I test for this by modifying specification (1) as follows, where G is a governance index from I/B/E/S calculated by Gompers, et al. (2003):

$$\begin{aligned} INVENTORY_{it} = \beta_0 + \beta_1.\ FUNDAMENTALS_{it} + {} & \\ & \beta_2.BEHAVIORAL_{i,t-1} + \beta_3.BEHAVIORAL^2_{i,t-1} + \\ & \zeta_1\ G_{it} + \\ & \zeta_2.BEHAVIORAL_{i,t-1} \times G_{it} + \zeta_3.BEHAVIORAL^2_{i,t-1} \times G_{it} + \\ & \text{firm effects} + \text{year effects} + \varepsilon_{it}. \end{aligned}$$

A positively signed ζ_2 and negatively signed ζ_3 are consistent with the dissipation channel.

4.3. The Catering Channel

The biggest empirical challenge to testing the catering channel is in measuring the inventory discount. I measure the discount in many different ways to minimize measurement problems. Structurally, each measure divides the dataset into low- and high-inventory firm-year observations and the inventory discount is the difference by some measure (say the mean market-to-book ratio) between these two sub-samples. For the choice of dividing into low- and high-inventory observations, I use a variety of criteria: (1) inventory value *levels*, (2) inventory value *changes*, (3) inventory value divided by property-plant-equipment value, (4) inventory/PPE changes, (5) inventory/sales based on the adjusted-inventory-turn specification in Gaur, et al. (2005), and (6) inventory/sales changes. For the choice of measuring the difference between the low- and high-sub-samples, I use the log of the difference in the market-to-book (MTB) ratios and the *future* one-year, two-year, three-year, and cumulative three-year stock returns (please see Baker and Wurgler (2004) for a similar set-up for calculating the dividend premium). For each of these, I employ still finer variations, using means versus medians, and using equal-weighted versus market cap-weighted measures. The inventory discount is positive when the MTB for low-inventory firms exceed that for high-inventory firms. It is negative when measured using future returns, since future returns are low when current valuations (MTB) are too high and vice versa. We can also think of the difference in future returns as an inventory *premium*.

Figure 1 shows two measures of the inventory discount. It appears that the discount is positive for most of the late 1970s and the 1980s, turns negative in the 1990s, and begins its journey back to neutrality in the early 2000s. Another striking figure of the picture is that the two measures are reassuringly correlated. Table 3 shows the correlation coefficients among a few of these measures. It also reports

Dickey-Fuller tests for unit roots, with and without time trends and lags. These tests are consistent with expectation that these series are covariance stationary. For example, the discount cannot grow indefinitely.

The catering channel predicts that the coefficient on *INVENTORY-DISCOUNT* is negative. It also predicts that firms with shorter-term horizons are more sensitive to the discount. I use six pieces of executive-level information from I/B/E/S, consolidated into six firm-level indices. The larger the value of an index, the shorter-term is the orientation. These indices are: (1) the number of executives granted options or shares[3], (2) percent of options granted to employees, (3) value of unexercised exercisable options held by the average executive, (4) value realized from options for the average executive, exercised at t+1, (5) percentage of company stock held by employees, and (6) value of restricted stock holdings by the average executive. I then compare the inventory discount sensitivity in the top and bottom quartiles ranked by short-termism.

4.4. Results

To save space, I do not report the results of regressions on each individual channel. In these regressions, I also conduct cross-sectional tests. For example, the data shows that shorter term firms tend to cater more. Details of these are at the author's website.

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[3] In I/B/E/S, "executives" mean top management officers as defined by the firm, usually taken to mean vice president and above, while "employees" mean all staff, full-time or otherwise.

In Table 4, I report summary results with all channels together. Model (1) uses the differencing specification and model (2) uses the fixed effects specification. In (1), both financing and catering channels are statistically significant, but in (2), only the catering channel is significant. I interpret this as evidence that the dissipation channel is not present, and the financing channel as at best weakly present. The catering channel, however, is statistically significant in a robust way. It is also as economically significant as the fundamentals, even with the large number of regressors. For example, one standard deviation of the catering coefficient is associated with 3% standard deviation of the *INVENTORY*, comparable to the 5% for q, a proxy for *FUNDAMENTALS*.

In Figure 2, I show the progressive contributions of the three channels, with increasingly restricted models. The baseline specification is that for model (1) in Table 4. Other specifications show the same conclusions. The directions of the arrows show what is being restricted, with the left-pointing ones representing restrictions on catering, upward ones for dissipation, and right-pointing ones for financing. The p-values of the restrictions are show on the arrows. For example, going from the bottom-most full specification to northwest represents a restriction on catering. The low p-value of .0003 says that catering is significantly different from zero. Further, the drop in adjusted R-squared from .647 to .603 is much larger than those going north or northeast. This shows that catering is an important explanation for *INVENTORY*. Indeed, all the north-pointing arrows have high p-

values, suggesting that dissipation is statistically indistinguishable from zero in both unrestricted and restricted models. Also, the catering-only box at upper right has a fairly high adjusted R-squared of .637, consistent again with the story that catering does most of the explaining of inventory.

4.5. Robustness and Alternative Interpretations

I repeat the robustness checks and controls in the previous "fundamentals only" test for the channels. The result, unreported here, is still that the catering channel is most important.

Might there be alternatives to the catering story? First, I emphasize that the financing, dissipation, and catering channels are neither exhaustive nor exclusive. For example, Morck, et al. (1990) describe another hypothesis, that the stock market impacts capital expenditures via an information channel. Under this hypothesis, the stock market affects expenditures because it provides investment information useful to firms. However, even Morck, et al. (1990) dismiss this hypothesis as a lame strawman that does not need testing, because it is difficult to imagine that the stock market has better information than insiders in firms do.

Another set of alternative interpretations can also be ruled out because of the empirics done. These have to do with explanations based on time-varying investment opportunities, financial constraints, or contracting problems such as agency and governance. These are ruled out by the tests of the "fundamentals only" hypothesis and the financing and dissipation channels, respectively. Consider, for example, the alternative story that the catering channel arise not from inefficient markets but from sales opportunities. However, since sales opportunities are part of fundamentals, I have already controlled for them in the "fundamentals only" test. Further, this story of sales opportunities is not consistent with the results obtained when catering to inventory premium is measured using *future* returns.

A more intriguing critique of the set of hypotheses is that it could be that markets are efficient and it is firms and managers who are irrational. Originally developed by Roll (1986) in the context of takeovers, the idea is that managers, even if acting in the interest of shareholders, genuinely believe (inaccurately) that their firms are undervalued. Such overconfident managers might load up on too much inventory (thinking that sales will come) or too little (thinking that they can handle the same amount of sales with less inventory). I do not test this hypothesis for several reasons. First, managers' overconfidence is much less observable (but see Malmeindier and Tate (2002)). Second, while the psychological basis of some aspects of investor sentiment (e.g., herding) has time variations and is widely documented (e.g., Baker, et al. (2004), Barberis, et al. (1998)), it is harder to think of the psychological basis of overconfidence having the same time variation. Third, the investor sentiment paradigm is comparatively more studied and accepted. Finally, the overconfidence interpretation and the market inefficiency interpretation can be discriminated by examining future returns. If markets were efficient and managers overconfident, future returns would not be especially low because efficiency guarantees there is no current over-valuation. In the results section, I report tests that use future returns as measures of *BEHAVIORAL*. The result is consistent only with a market inefficiency paradigm. Nevertheless, both the overconfidence and market inefficiency paradigms could simultaneously hold even if just the latter is observably dominant. This is a promising avenue for future research.

Finally, I have to consider if changes in inventory, unlike changes in capital expenditures studied in macroeconomics and finance, might actually not be the result of policy decisions by firms. Instead, they could be involuntary changes that are outcomes of changes to say, sales. What this means is that the relationship between dependant and independent variables might be hardwired. Three arguments count against this interpretation. First, I measure sales prospects, not sales. It is harder to imagine any involuntary change of inventory due to changes in sales prospects. Second, I measure inventory changes at yearly frequencies. While it is plausible, even likely, that inventory changes might not be policy decisions at shorter frequencies, it is much harder to say that firms do not review their inventory levels at yearly intervals when, for example, they and their auditors review financial statements. Even non-actions could be viewed as policy decisions at these times. The third counter-argument is that in the data, the volatility of inventory is high. For example, a ratio of standard deviation to mean can be computed for inventory change for each firm. This ratio has a mean of 34%. Similarly, a ratio of standard deviation to mean can be computed for inventory/PPE. This has a mean of 51%.

Such high ratios suggest that it is likely that firms have an active hand in their inventory policy.

5. Conclusion

I find that the stock market does influence inventory decisions and provide evidence that this influence is likely to have taken effect via a catering channel, perhaps also by a financing channel, but not by a dissipation channel. If true, this result has important theoretical and practical implications.

The theoretical implications are that inventory models can no longer rely on rational models to derive costs of capital. Further, studies on the stock market and inventory cannot assume a straightforward link from the former to the latter via some cost of capital argument. Finally, more accurate models might be obtained by accounting for inefficient market parameters (e.g., Netessine and Roumiantsev (2005)).

The practical implication is that inventory levels might have been optimized with a short-term view, rather than for the long-term interest of the firm. One natural line of future work is to further confirm this empirically, for example, by investigating long-term returns within a Fama and Macbeth (1973) framework. Another line of future research is to investigate the source of mis-valuation in general, and the inventory discount in particular. For example, it is possible that investors sometimes treat high inventory as a signal of operational incompetence but at other times, treat high inventory as a signal of expected growth.

In a broader sense, this paper points to interaction between financial markets and operations management. I have chosen to study a specific case, between the stock market and inventory decisions. It could be a profitable research agenda to investigate other aspects of this broader interaction.

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Figure 1 – The Inventory Discount

The vertical axis is a normalized scale obtained by subtracting the raw discount by the mean over the entire time series, and dividing that by the standard deviation of the time series. The lighter line is the discount calculated as the difference in mean market-to-book value between the lowest- and highest-inventory quartiles classified by inventory value. The darker line shows the negative of the discount calculated as the mean future three-year stock return of the lowest- and highest-inventory quartiles, also classified by inventory value.

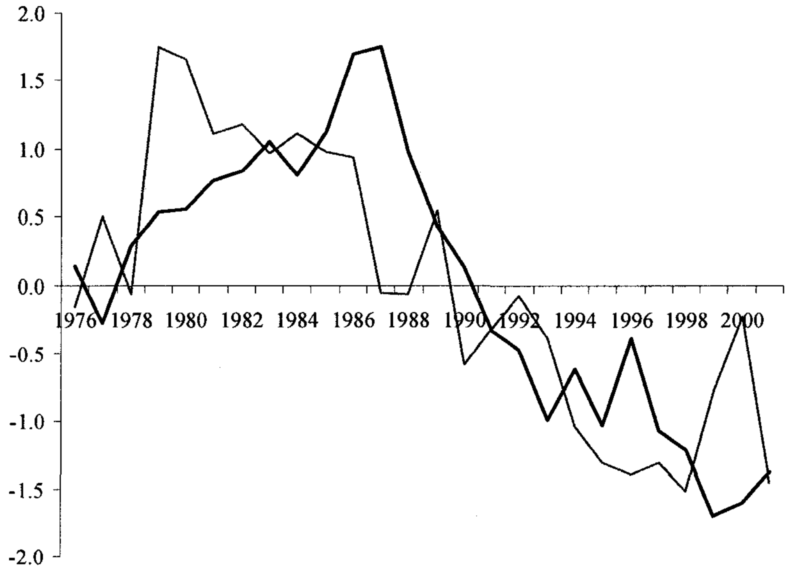

Figure 2 – Contributions by the Three Channels

The bottom-most box has the complete specification is as in Table 4.

$$INVENTORY_{it} = \beta_0 + \beta_1.\ FUNDAMENTALS_{it} + \gamma_1.EQUITY_{i,t-1} + \gamma_2.DEBT_{i,t-1} + \beta_2.BEHAVIORAL_{i,t-1} + \beta_3.BEHAVIORAL^2_{i,t-1} + \zeta_1\ G_{it} + \zeta_2.BEHAVIORAL^2_{i,t-1} \times G_{it} + \eta.INVENTORY\text{-}DISCOUNT_{it} + \varepsilon_{it}\ .$$

The higher boxes place restrictions on the financing, catering, and dissipation variables.

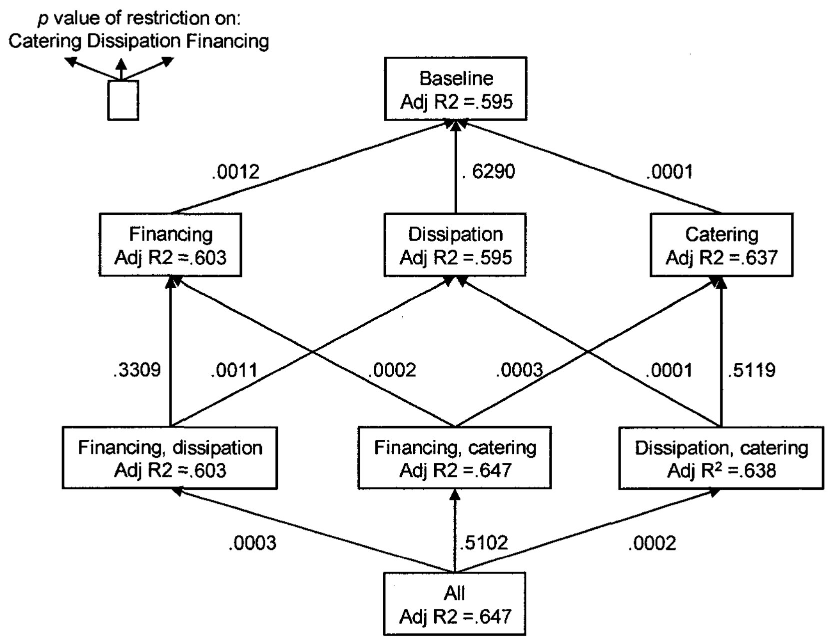

Table 1 - Summary Statistics

The data is from COMPUSTAT-CRSP, I/B/E/S, and ExecuComp, winsorized at 1% and 99%. Observations cannot be involved in acquisitions or mergers, the market-to-book and Q must be between 0.1 and 100, sales, assets, capital expenditures, income before earnings and interest, common dividends, common equity must all be non-negative.

	N	Mean	SD	Min.	Max
Year	97929	1989.7	8.5	1962	2003
Market cap	97917	885.8	6630.1	0.0	508329.5
Book equity	97806	406.1	2388.1	-4174.0	186066.5
Market-to-book ratio	97917	3.0	5.9	0.0	99.4
Return (%)	71057	3.2	29.1	0.0	4474.1
Δcash flow	97917	4.4	5.9	-12.2	12.9
Δsales	97917	1.7	1.1	0.2	3.3
Δinventory	97917	2.1	1.2	0.0	3.4
Alpha CAPM	71057	1.8	29.0	-2.4	4471.8
Alpha Fama French	71057	1.7	29.0	-4.6	4469.3
Inventory_t / PPE_{t-1}	97929	8.7	10.9	0.0	24.1
Discretionary accruals	4512	-40.5	2808.0	-188518.8	5081.5
Δdebt	97929	3.7	4.2	-1.0	8.6
Δequity	97929	1.2	1.8	-0.8	4.0
KZ index	97929	-1.3	15.7	-137.4	6.3
q	97917	1.7	2.3	0.0	85.4
G index	3435	8.8	2.8	1.0	17.0
Restricted stock holdings by average executive ($)	8908	411.5	7436.4	0.0	655968.8
Unexercised exercisable options held by average executive ($)	8908	2114.2	9080.8	0.0	556283.0
$ realized from options for average executive, exercised	6571	600.6	2970.6	0.0	121427.3
% company stock held by employees	5515	5.6	8.6	0.0	64.2
% options granted to employees	10006	19.5	20.7	0.0	342.5
Number of executives granted options or shares	10006	5.5	2.0	1.0	12.0

Table 2 – Testing the "Fundamentals Only" Hypothesis

INVENTORY, BEHAVIORAL, and *FUNDAMENTALS* are measured using a variety of variables in the models below. The specification is of the form:

$$INVENTORY_{it} = \beta_0 + \beta_1.FUNDAMENTALS_{it} + \beta_2.BEHAVIORAL_{i,t-1} + firm\ effects + year\ effects + \varepsilon_{it}\ ,$$

where i and t index firms and years. Models (1) and (2) use OLS on changes, (3) and (4) use firm fixed effects, (5) logistic, (6) uses two-stage least squares with instrumental variables, and (7) a Heckman correction. Accruals are discretionary ones, obtained by subtracting from total accruals the discretionary portion. Totals are:

$$ACCR_{i,t} = (\ \Delta[CurrentAssets_{i,t} + Cash_{i,t}] - \Delta[CurrentLiabilities_{i,t} - LongTermDebt_{i,t}]\ )\ /\ TotalAssets_{i,t-1}.$$

The discretionary portion is:

$$NONDIS\text{-}ACCR_{i,t} = \hat{\theta}_0 + \hat{\theta}_1.(1/TotalAssets_{,t-1}) + \hat{\theta}_2.(\Delta sales_{i,t} - \Delta accountsReceivable_{i,t})/TotalAssets_{,t-1} + \hat{\theta}_3.(PlantPropertyEqpt_{i,t}/TotalAssets_{,t-1})\ ,$$

where the $\hat{\theta}$'s are obtained from firm-by-firm regressions using four-digit SIC code peers (i.e., all but itself):

$$ACCR_{i,t} = \theta_0 + \theta_1.(1/TotalAssets_{,t-1}) + \theta_2.(\Delta sales_{i,t}/TotalAssets_{,t-1}) + \theta_3.(PlantPropertyEqpt_{i,t}/\ TotalAssets_{,t-1}) + \varepsilon_{i,t}.$$

Most data is from COMPUSTAT-CRSP and I/B/E/S, winsorized at 1% and 99%. The closed-end fund discount is from Neal and Wheatley (1998) (for years 1962 through 1993), CDA (1994 through 1998), and *The Wall Street Journal* end-of-year issues (1999 through 2000). Fama-French factors are SML, HML, and MOM, from Professor French's website. The Heckman correction in model (7) uses the following selection model:

$$SELECTED = f\,(MKTCAP,\ S\&P500,\ ASSETS)\ ,$$

where *MKTCAP* is market capitalization, *S&P500* is whether the firm is ever in the S&P 500, and *ASSETS* is total assets. Observations cannot be involved in acquisitions or mergers, the market-to-book and Q must be between 0.1 and 100, sales, assets, capital expenditures, income before earnings and interest, common dividends, common equity must all have non-negative values. Estimations are done with heteroskedastic-robust standard errors (in brackets below) and clustered around firms to minimize serial correlation.

INVENTORY	(1) Δinventory	(2) Δinventory	(3) Inventory/PPE	(4) Inventory/PPE	(5) Inventory dummy	(6) Δinventory	(7) Δinventory
BEHAVIORAL							
CAPM $alpha_{t-1}$	.0350	.0020			.0075		.004
	(.0004)	(.0003)			(.0007)		(.000)
$Accruals_{t-1}$			.019				
			(.006)				
Closed-end fund $discount_{t-1}$				-.010			
				(.006)			
Fama-French $alpha_{t-1}$ instrumented using all three above						.0013	
						(.0004)	
FUNDAMENTALS							
ΔCash flow		.058			.110	.046	.049
		(.002)			(.004)	(.003)	(.005)
ΔSales		.493			1.380	.404	.715
		(.010)			(.027)	(.012)	(.028)
q_{t-1}			.035	.026			
			(.015)	(.013)			
Inverse Mill's ratio							-.543
							(.309)
Firm fixed effects			Yes	Yes			
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	.244	.513	.838	.837	.294	.082	41257 (Wald)
N	97,917	97,917	71,387	66,199	95,321	66,199	97,917

Table 3 – Statistics for Some Measures of Inventory Discount

The measures of inventory discount are based on the difference in the second column (e.g., future return at time t+3, or from t+1 to t+3, or market to book, MTB) of the low vs. high quartiles of firm-year observations, sorted by the criteria in the third column. The data is from COMPUSTAT-CRSP.

			Dickey-Fuller test of unit roots		Correlation coefficients						
		Low vs. high criteria	No lag, no trend	5 lags, trend	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1)	$Return_{t\ to\ t+3}$	Inventory	-3.12 (.025)	-2.58 (.289)	1.00						
(2)	$Return_{t\ to\ t+2}$	Inventory	-3.20 (.020)	-1.57 (.804)	0.41	1.00					
(3)	$Return_{t+1\ to\ 3}$	Inventory	-0.56 (.880)	-2.60 (.279)	0.81	0.76	1.00				
(4)	$Return_{t\ to\ t+1}$	Δ(Inventory/ PPE)	-4.83 (.000)	-2.20 (.493)	-0.14	0.16	0.15	1.00			
(5)	$Return_{t+1\ to\ 3}$	Δ(Inventory/ PPE)	-2.80 (.059)	-2.68 (.244)	0.23	0.33	0.32	0.45	1.00		
(6)	MTB	Inventory	-1.43 (.565)	-2.45 (.356)	-0.64	-0.66	-0.72	-0.15	-0.38	1.00	
(7)	MTB	Δinventory	-2.13 (.234)	-2.47 (.341)	-0.15	-0.30	-0.28	-0.15	-0.29	0.59	1.00

Table 4 – Comparison of All Hypotheses

The specification is:

$$INVENTORY_{it} = \beta_0 + \beta_1. FUNDAMENTALS_{it} + \gamma_1.EQUITY_{i,t-1} + \gamma_2.DEBT_{i,t-1} + \delta_1.EQUITY_{i,t-1} \times FINANCIALLY\text{-}CONSTRAINED_{it} + \delta_2.DEBT_{i,t-1} \times FINANCIALLY\text{-}CONSTRAINED_{it} + \beta_2.BEHAVIORAL_{i,t-1} + \beta_3.BEHAVIORAL^2_{i,t-1} + \zeta_1 G_{it} + \zeta_2.BEHAVIORAL^2_{i,t-1} \times G_{it} + \eta.INVENTORY\text{-}DISCOUNT_{it} + \varepsilon_{it}.$$

Model (1) uses OLS on changes, and measures *BEHAVIORAL* with alpha CAPM and *FUNDAMENTALS* with sales and cash flow growth. Model (2) uses fixed effects, and measures *BEHAVIORAL* with CAPM alpha and *FUNDAMENTALS* with lagged q. Estimations are done with heteroskedastic-robust standard errors and clustered around firms to minimize serial correlation.

INVENTORY	(1) Δinventory	(2) Inventory/PPE
FINANCING		
Δdebt × KZ	.007 (.005)	
Δequity × KZ	.017 (.004)	
Debt × KZ		.00003 (.00008)
Daniel-Titman (DT) equity × KZ		-.00054 (.00267)
DISSIPATION		
G index × BEHAVIORAL	-.006 (.006)	.027 (.032)
G index × BEHAVIORAL squared	.0002 (.0001)	-.001 (.001)
CATERING – inventory discount/premium based on:		
t+1 to t+3 return differences of low vs. high inventory quartiles	.089 (.024)	
t+1 to t+3 return differences of low vs. high Δ(inventory/PPE) quartiles		.215 (.138)
Firm fixed effects		Yes
Adjusted R-squared	.647	.885
N	2,550	1,920

Appendix—Tests of Individual Channels

Table A1 shows the test for the financing channel. Model (1) is a baseline to follow the specification in Morck, et al. (1990). All the coefficients are signed as predicted, and are significant. Firms that issue 1% more new debt show 6.6% more inventory growth, on average and controlling for fundamentals. This is higher but of the same order of magnitude as the 1.75% Morck, et al. (1990) obtain for growth in capital expenditures. Similarly, firms that increase their shares by 1% show 2.5% more inventory growth. Again, this is comparable to the 1.6% obtained by Morck, et al. (1990). Comparing this model (1) with model (1) in Table 2, we can see that *FINANCING* reduces the impact of *BEHAVIORAL* slightly, although the latter is still significant. I interpret this as weak support for the financing channel. Model (2) shows the same, using discretized versions of Δdebt and Δequity following Morck, et al. (1990), in which they set the debt dummy to 1 if the change is more than 20% and the equity dummy to 1 if more than 10%. The results are qualitatively unchanged.

Model (3) shows the delineation of the financing effect by the degree of financial constraint. As expected, constrained firms (high KZ index) have lower inventory levels. More interesting, the interaction of the financing channel in overvaluation (as measured by changes in debt and equity issues, after partialling out *FUNDAMENTALS*) with the KZ index is positive and significant. This again supports the financing channel: the more constrained a firm, the more it leverages misevaluation to obtain easier financing for inventory. Unreported robustness checks with other measures of *BEHAVIORAL* such as

Fama-French alphas and accruals produce the same result, although the effect of *FINANCIALLY-CONSTRAINED* is much reduced with accruals. I interpret this as accruals picking up financial-constraints, so using accruals is less interesting as a measure of *BEHAVIORAL* here.

Model (4) uses the closed-end fund discount as a measure of *BEHAVIORAL* and DT equity. It produces negative coefficients for the *FINANCING* variables. These are the opposite of what is predicted, although their low economic significance (e.g., one standard deviation change in DT equity produces only 0.6% standard deviation change in *INVENTORY*) might be interpreted as negligible impact. The interactions with the KZ index obtain the predicted positive signs on the interaction terms. The economic significance is low.

It is possible that firms have different financing technologies. For example, if *INVENTORY* is concave in *EQUITY* or *DEBT*, and financially-constrained firms tend to also have low *EQUITY* or *DEBT*, then I would observe that these firms have a higher inventory sensitivity to finance. To take care of this, I use quadratic formulations of *EQUITY* and *DEBT*, and it turns out (in unreported regressions) that this is not a concern. Overall, I conclude that I cannot reject the financing channel, although the evidence for it is weak.

In Table A2, I report the results of testing the dissipation channel. In Model (1), I report a predicted quadratic relationship between *INVENTORY* and *BEHAVIORAL*, which is statistically and economically significant. In Model (2), I conduct a further test to see if the quadratic relationship is ameliorated with stronger governance. Unfortunately, the small number of observations with a G index does not produce a statistically valid estimation. Nevertheless, the G index is negatively signed and significant, consistent with the view that stronger governance reduces inventory, controlling for other effects. In Model 3, I estimate with a fixed-effects specification using the closed-end fund discount measure of *BEHAVIORAL*. The dissipation effect is not evident, as the only significant coefficient on *BEHAVIORAL* is on the linear term. Model (4) attempts to use the G index, and again, due to the small sample size, I could not arrive at a reasonable estimation. Overall, it appears that the dissipation effect is only very weakly supported, if at all.

In Table A3, I report the test for the catering channel. Panel (a) shows the influence of *INVENTORY-DISCOUNT* and panel (b) shows how this influence is different for firms with more short-term orientation (S) versus others (L, for long-term orientation), measured in various ways. In panel (a), the first five models are estimated using inventory growth as the measure of *INVENTORY* and five different measures of *INVENTORY-DISCOUNT* (other measures described earlier are unreported but achieve the same qualitative result). As predicted, the measures using differences in future returns (which can be thought of as inventory premium) are positively signed, while the last using the difference in market-to-book between high- and low-inventory firms shows the predicted negative sign. All coefficients are significant. For illustration, I show a model based on inventory/PPE as a measure of *INVENTORY*. The result is qualitatively similar. Panel (b) shows just the coefficients for *INVENTORY-DISCOUNT*, from estimations done with specifications like those of model (6) in panel (a). Each of the six sub-panels is for some measure of short-term orientation of the management. For example, sub-panel (1) classifies firm-years by the number of executives in that firm-year that hold options on the firm's stock. The top quartile of these firm-years is considered short-term and the bottom quartile long-term. As predicted, coefficients on *INVENTORY-DISCOUNT* is almost always more sensitive for short-term oriented firm-years, while they are mostly lower or statistically indistinguishable from zero for long-term oriented firm-years. A particularly strong cross-sectional test is model (4), which shows catering in the current period stronger among firms whose executives exercise their options in the next period.

Table A1 – Testing the Financing Channel

Models (1) and (2) use the following specification:

$$INVENTORY_{it} = \beta_0 + \beta_1.\ FUNDAMENTALS_{it} + \beta_2.BEHAVIORAL_{it-1} + \gamma_1.EQUITY_{i,t-1} + \gamma_2.DEBT_{i,t-1} + \textit{firm effects} + \textit{year effects} + \varepsilon_{it}.,$$

while models (3) and (4) add cross-sectional financial constraints:

$$INVENTORY_{it} = \beta_0 + [\ \textit{as before}\ ] + \gamma_1.EQUITY_{i,t-1} + \gamma_2.DEBT_{i,t-1} + \delta_1.EQUITY_{i,t-1} \times FINANCIALLY\text{-}CONSTRAINED_{it} + \delta_2.DEBT_{i,t-1} \times FINANCIALLY\text{-}CONSTRAINED_{it} + \textit{firm effects} + \textit{year effects} + \varepsilon_{it}..$$

Models (1) through (3) use OLS on changes and (4), firm fixed effects. The data is from COMPUSTAT-CRSP and I/B/E/S, winsorized at 1% and 99%. The closed-end fund discount is from Neal and Wheatley (1998) (1962 through 1993), CDA (1994 through 1998), and *The Wall Street Journal* (1999 through 2000). The discretized versions of Δdebt is set to 1 if Δdebt is more than 20%; likewise for equity if more than 10%. The measure of DT equity is $\log(M_t/M_{t-\tau}) - r(t-\tau,t)$, where M_t is the per share value at t and $r(t-\tau,t)$ is the log stock return from $t-\tau$ to t, in turn defined as $r(t-\tau,t) = \sum_{s=t-\tau+1}^{t} \log[(M_s.f_s + D_s)/M_{s-1}]$, where f_s is the price adjustment factor from $s-1$ to s that accounts for splits and rights issues, and D_s is the per-share cash distribution paid at time s. The KZ index is -1.001909*[(Income before extraordinary items + Depreciation & amortization)/PPE] + 0.2826389*[(Assets + Market capitalization – Common equity – Deferred taxes) / Assets] + 3.139193*[(Long-term debt + Debt in current liabilities) / (Long-term debt + Debt in current liabilities + Stockholders' equity)] -39.3678*[(Common dividends + Preferred dividends) / PPE]-1.314759*[Cash & short-term investments / PPE]. Observations cannot be involved in acquisitions or mergers, the market-to-book and Q must be between 0.1 and 100, sales, assets, capital expenditures, income before earnings and interest, common dividends, common equity must be non-negative. Estimations are with heteroskedastic-robust standard errors (in brackets) and clustered around firms to minimize serial correlation. Model (4) has 2 lags.

INVENTORY	(1) Δinventory	(2) Δinventory	(3) Δinventory	(4) Inventory/PPE
BEHAVIORAL				
CAPM alpha$_{t-1}$	.0010 (.0003)	.0020 (.0003)	.0014 (.0003)	
Closed-end fund discount$_{t-1}$				-.009 (.006)
FUNDAMENTALS				
ΔCash flow	.048 (.002)	.055 (.002)	.024 (.002)	
ΔSales	.332 (.011)	.404 (.010)	.297 (.000)	
q_{t-1}				.019 (.012)
FINANCING				
Δdebt	.066 (003)		.033 (.003)	
Δequity	.025 (.006)		.034 (.006)	
Δdebt dummy		.417 (.014)		
Δequity dummy		.018 (.008)		
Debt				-.0001 (.0001)
Daniel-Titman (DT) equity				-.044 (.009)
Kaplan-Zingales (KZ) index			-.007 (.001)	-.016 (.004)
Δdebt × KZ index			.011 (.001)	
Δequity × KZ index			.022 (.001)	
Debt × KZ index				.0004 (.0001)
DT equity × KZ index				.0029 (.0006)
Firm fixed effects				Yes
Year effects	Yes	Yes	Yes	Yes
Adj R-squared	.543	.534	.565	.840
N	97,917	97,917	97,917	66,199

Table A2 – Testing the Dissipation Channel

The specification for models (1) and (3) is:

$$INVENTORY_t = \beta_0 + \beta_1.\ FUNDAMENTALS_t + \beta_2.BEHAVIORAL_{t-1} + \beta_3.BEHAVIORAL^2_{t-1} + \varepsilon_t.$$

That for models (2) and (4) is:

$$INVENTORY_t = \beta_0 + \beta_1.\ FUNDAMENTALS_t + \beta_2.BEHAVIORAL_{t-1} + \beta_3.BEHAVIORAL^2_{t-1} + \zeta_1\ G_t + \zeta_2.BEHAVIORAL^2_{t-1} \times G_t + \varepsilon_t.$$

Models (1) and (2) use OLS on changes and (3) and (5), firm fixed effects. Most data is from COMPUSTAT-CRSP and I/B/E/S, winsorized at 1% and 99%. The closed-end fund discount is from Neal and Wheatley (1998) (for years 1962 through 1993), CDA (1994 through 1998), and *The Wall Street Journal* end-of-year issues (1999 through 2000). The governance index G is from Gompers, et al. (2003), obtained from I/B/E/S. Observations cannot be involved in acquisitions or mergers, the market-to-book and Q must be between 0.1 and 100, sales, assets, capital expenditures, income before earnings and interest, common dividends, common equity must all have non-negative values. Estimations are done with heteroskedastic-robust standard errors (in brackets below) and clustered around firms to minimize serial correlation. Models (3) and (4) have two lags.

INVENTORY	(1) Δinventory	(2) Δinventory	(3) Inventory/PPE	(4) Inventory/PPE
BEHAVIORAL				
CAPM $alpha_{t-1}$	-.035	.045		
	(.004)	(.067)		
CAPM $alpha^2_{t-1}$	.0012	-.001		
	(.0001)	(.002)		
Closed-end fund $discount_{t-1}$			.027	.057
			(.016)	(.210)
Closed-end fund $discount^2_{t-1}$			.000007	-.012
			(.00057)	(.018)
FUNDAMENTALS				
ΔCash flow	.058	.043		
	(.002)	(.009)		
ΔSales	.490	.642		
	(.010)	(.048)		
q_{t-1}			.026	-.119
			(.013)	(.209)
GOVERNANCE				
G index		-.020		.018
		(.009)		(.102)
G index × BEHAVIORAL		-.006		-.021
		(.007)		(.025)
G index × $BEHAVIORAL^2$		.0002		.003
		(.0002)		(.002)
Firm fixed effects			Yes	Yes
Year effects	Yes	Yes	Yes	Yes
Adj R-squared	.514	.584	.837	.884
N	97,917	3,435	66,203	1,920

Table A3 – Testing the Catering Channel

Panel (a) shows a regression of *INVENTORY* on various measures of *BEHAVIORAL, FUNDAMENTALS,* and *INVENTORY-DISCOUNT*. The measures of inventory discount are based on the difference (such as the future return at time t+3 or market to book, MTB) of the low vs. high quartiles of firm-year observations, sorted by certain criteria (such as inventory, or Δinventory/PPE). Panel (b) shows just the coefficients for *INVENTORY-DISCOUNT*, from estimations done with specifications like those of models (9) and (10) in panel (a). Each of the six sub-panels is for some measure of short-term orientation of the management. For example, sub-panel (1) classifies firm-years by the number of executives in that firm-year that hold options on the firm's stock. The top quartile of these firm-years is considered short-term and the bottom quartile long-term. Most data is from COMPUSTAT-CRSP and I/B/E/S, winsorized at 1% and 99%. The closed-end fund discount is from Neal and Wheatley (1998) (for years 1962 through 1993), CDA (1994 through 1998), and *The Wall Street Journal* end-of-year issues (1999 through 2000). Fama-French factors are SML, HML, and MOM, from Professor French's website. The Heckman correction in model (7) uses the following selection model:

$$SELECTED = f(MKTCAP, S\&P500, ASSETS),$$

where *MKTCAP* is market capitalization, *S&P500* is whether the firm is ever in the S&P 500, and *ASSETS* is total assets. Observations cannot be involved in acquisitions or mergers, the market-to-book and Q must be between 0.1 and 100, sales, assets, capital expenditures, income before earnings and interest, common dividends, common equity must all have non-negative values. Estimations are done with heteroskedastic-robust standard errors (in brackets below) and clustered around firms to minimize serial correlation.

Panel (a)

	(1)	(2)	(3)	(4)	(5)	(6)
INVENTORY	Δinventory	Δinventory	Δinventory	Δinventory	Δinventory	Inventory/PPE
BEHAVIORAL						
CAPM alpha$_{t-1}$	.002 (.000)	.002 (.000)	.002 (.000)	.002 (.000)	.002 (.000)	
Closed-end fund discount$_{t-1}$						-.030 (.008)
FUNDAMENTALS						
ΔCash flow	.058 (.002)	.058 (.002)	.058 (.002)	.058 (.002)	.057 (.002)	
ΔSales	.500 (.010)	.500 (.010)	.500 (.010)	.495 (.010)	.495 (.010)	
q_{t-1}						.026 (.013)
INVENTORY-PREMIUM based on						
t+3 return differences of by inventory quartiles	.045 (.012)					
t+2 return differences of by inventory quartiles		.072 (.013)				
t+1 to t+3 return differences of by inventory quartiles			.034 (.006)			.150 (.047)
t+1 to t+3 return differences of Δinventory/PPE quartiles				.024 (.006)		
INVENTORY-DISCOUNT based on						
MTB differences of inventory quartiles					-.192 (.014)	
Firm fixed effects						Yes
Adj *R*-squared	.507	.507	.507	.502	.513	.837
N	88,722	88,722	88,722	85,932	97,917	66,199

Panel (b)

Orientation	(1) Number of executives granted options or shares				(2) % options granted to employees				(3) Unexercised exercisable options held by average executive ($)			
	Short	Long	Short	Long	Short	Long	Short	Long	Short	Long	Short	Long
INVENTORY-PREMIUM:												
t+1 to t+3 return differences of			.061	.055			.064	.042			.064	.013
Δinventory/PPE quartiles			(.013)	(.044)			(.013)	(.048)			(.013)	(.069)
INVENTORY-DISCOUNT												
MTB differences of inventory	-.108	.012			-.120	-.024			-.118	.165		
quartiles	(.031)	(.153)			(.031)	(.190)			(.031)	(.228)		

Orientation	(4) $ realized from options for average executive, exercised at t+1				(5) % Company stock held by employees				(6) Restricted stock holdings by average executive ($)			
	Short	Long	Short	Long	Short	Long	Short	Long	Short	Long	Short	Long
INVENTORY-PREMIUM:												
t+1 to t+3 return differences of			.064	-.072			.059	.005			.064	.065
Δinventory/PPE quartiles			(.013)	(.061)			(.013)	(.060)			(.013)	(.040)
INVENTORY-DISCOUNT												
MTB differences of inventory	-.109	.313			-.097	-.145			-.122	.069		
quartiles	(.031)	(.206)			(.030)	(.175)			(.031)	(.128)		