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# Investment-Cash Flow Sensitivity – A Focus on the Panel-Data Econometrics Involved

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**Abstract**

I revisit Fazzari et al. (1988) seminal paper on the investment-cash flow sensitivity as a measure of financing constraints and augment their approach with the findings from recent papers. I find that the investment-cash flow sensitivity has decreased and mostly disappeared over time, in line with recent literature. This finding is robust to alternative specifications and a number of robustness checks. I contribute to the literature by explicitly analyzing the strict-exogeneity assumption of the fixed-effects and first-differences estimators in empirical practice. In this setting, strict exogeneity does not hold and the violation can cause substantial inconsistencies.

*Keywords:* Investment-cash flow sensitivity; Capital market imperfections; Strict exogeneity; Panel data

## 1. Introduction

Many empirical business investment models have relied and still generally rely on the assumption that there is a "representative firm." This firm responds to prices set in centralized security markets. For this firm, only its cost of capital and investment demand affect investment spending. Its financial structure, for example, is irrelevant to investment, given that internal and external finance are perfect substitutes. So, generally, when capital markets are perfect, this firm's investment decisions are independent of its financial condition. However, an alternative research stream has been focusing on the view that there is a wedge between the internal and external cost of finance – a firm is then said to be financially constrained (Fazzari et al. (1988) (FHP), p. 141). This definition provides a useful framework to differentiate firms according to the extent to which they are financially constrained (Kaplan and Zingales (1997), p. 172). (A firm is considered more financially constrained as the gap between its cost of internal and external funds widens.)

Following this research, a firm's investment may also depend on financial factors, such as the availability of internal finance and access to new debt or equity finance. For example, a firm's internal cash flow may affect investment spending because of a financing hierarchy, in which internal funds have a cost advantage over external funds. Thus, a firm's sensitivity of (physical) investment to cash flow has often been used in the empirical literature as a measure of capital constraints. The underlying logic is that if firms could easily access external capital, there would be no need to alter investment due to shocks to cash flow. As a result, there would be no significant relation between cash flow and investment (Gatchev et al. (2010)).

Conventional representative firm models, in which the financial structure is irrelevant to the investment decision, are likely to be suitable for mature firms, whose prospects are well known. However, for other firms, financial factors might – even today – matter in the sense that, especially in the short run, internal and external funds are imperfect substitutes. For these firms, the availability of internally generated funds may have an effect on investment decisions (Fazzari et al. (1988), p. 142). A main foundation for such a capital market imperfection is the presence of information asymmetries. This makes it very costly for providers of external finance to evaluate the quality of firms' investment opportunities. Theoretical arguments that support this view draw heavily from the "lemons" problem first considered by Akerlof (1970). Akerlof argues that some sellers with inside information about the quality of an asset or a security may be unwilling to accept the terms offered by a less-informed buyer. This can cause the breakdown of the market; at least, this can lead to the sale of an asset at a price that is lower than it would be if all parties had full information. This argument can be applied to the example of new share issues, as suggested by Myers and Majluf (1984), in cases in which managers have inside information. If this information is favorable, it may happen that management, acting in the interest of existing shareholders, does not issue new shares, as

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these would be underpriced. Knowing about this asymmetric information problem, investors might generally interpret management's decision to issue new shares as a signal that shares are overpriced. In this case, given the adverse selection problem, new equity finance can only be obtained at a premium (Devereux and Schiantarelli (1990), p. 282).

Agency costs might also give ground to capital constraints. Conflicts between shareholders and debtholders can lead to agency costs of debt. When a company is partly financed with debt, Jensen and Meckling (1976) suggest that stockholders have an incentive to engage in projects that would otherwise be too risky, thereby increasing the probability of financial distress. If the project is successful, the payoff to the firm owners is large. However, if unsuccessful, the limited liability provision of debt contracts stipulates that it is the creditors who bear most of the costs. On top of that, Myers (1977) suggests that in these cases, in which a firm is partly debt financed, it may underinvest in the sense that it forgoes projects with a positive net present value. Provided that potential creditors understand the incentives of stockholders and incorporate the risk of bankruptcy in loan negotiations, the company owners ultimately bear the consequences of these agency problems in terms of a higher cost of debt (Devereux and Schiantarelli (1990), p. 280).

According to Jensen and Meckling (1976), the possible divergence of interest between managers and outside shareholders can give rise to agency costs of equity. In case of a separation between ownership and control, managers are encouraged to use a greater than optimal amount of the firm's resources in the form of perquisites. Though such activities can be monitored by outside shareholders, doing so is costly. Ultimately, it is again the owners who have to bear the costs in terms of a reduced price that prospective outside shareholders are willing to pay for a stake in the firm. Marris (1964) growth model is also based on the divergence of interest between managers and outside shareholders. Marris shows that managers may wish to increase the growth rate of their company beyond the level that maximizes shareholder wealth. They do so while they try to maintain the company's share price at a sufficiently high level to avoid a takeover by outsiders, who would then be likely to dismiss the managers.

Finally, capital market imperfections causing capital constraints can exist due to transaction costs. For example, investment-banking fees must be paid when raising external finance (Duesenberry (1958)). Implications arise for the study of macroeconomic investment fluctuations and the impact of public policy on capital spending in the case that capital market imperfections still exist. These capital market imperfections lead to binding financial constraints on investment. Financial constraints in capital markets could then magnify the macroeconomic effect of shocks to cash flow or cash stock on aggregate investment. With regards to tax policy, in case of frictionless financial markets, only the marginal tax rate on returns from a new project matter. However, for firms that face imperfect markets for external finance, the amount of earnings devoted to taxes – and therefore the average tax rate on returns from existing projects – matters for investment, possibly along with incentive effects of marginal tax rates (Fazzari et al. (1988), pp. 184–186).

Several recent studies have shown that the investment-cash flow sensitivity has decreased over time. There are several reasons to suspect why this is the case. One important reason is the substantial development in US equity markets over the last decades. For example, in 1971, young firms got access to a much more efficient stock exchange when Nasdaq was created. This is underlined by a steep increase in public equity finance use by young firms suggesting that stock issues may have become a closer substitute for internal finance. The second reason is closely related to that. It is the sharp increase in the fraction of publicly traded firms that consistently present negative cash flow figures. When cash flow is particularly low, these firms often make heavy use of public equity to expand investment. Hence, a failure to account for external finance in investment-cash flow regressions can result in a downward omitted variable bias in the estimated cash-flow coefficient. For example, cash flow goes down, causing external finance to go up, which then finances investment. A third reason is the change in the composition of total investment. Research and development (R& D) intensity has risen strongly for the typical manufacturing firm, while the absolute and relative importance of physical investment has deteriorated. Given that most investment-cash flow studies have so far focused on physical investment, its declining relative importance has potentially led to a decline in the conventionally measured investment-cash flow sensitivity (Brown and Petersen (2009)).

In this research paper, I revisit the very early literature on investment-cash flow sensitivity as a measure of financing constraints and see what has changed. Concretely, I use Fazzari et al. (1988) partially as a guidance and comparison. I augment this with the findings from influential, recent papers in the literature (e.g., regarding the changing composition of investment). Inter alia and naturally, this implies that I investigate whether the sensitivity of investment to cash flow has reduced over time.

Throughout the paper, I put a great focus on the econometrics involved (e.g., careful selection of the estimation method). Noteworthy, this is one of the first papers to explicitly acknowledge and analyze the strict exogeneity assumption of the fixed-effects and first- differences estimator in empirical practice. Apart from the typical notion of contemporaneous exogeneity, which only requires a lack of contemporaneous correlation between the error term and the explanatory variables, strict exogeneity requires the absence of feedback from the dependent variable to future values of the independent variable. So far, the overwhelming majority of panel-data finance literature does not explicitly account for the strict exogeneity. To overcome this weakness, I partially follow the approach in Grieser and Hadlock (2015). They show not only that it is highly likely that the strict exogeneity assumption is quite commonly violated in practice, but also that, in many instances, the possible magnitude of the inference errors is substantial. Given that I pay explicit attention to this assumption and that practical, empirical evidence is still rare, credibility of my general results is increased, plus I can offer additional evidence on the topic of strict exogeneity in empirical practice.

I test the following hypotheses: a) in the investment-cash flow regression, the coefficient on cash flow is still positive on average, b) the coefficient is still significantly larger for companies a priori assigned into the more financially constrained sub-sample, c) the investment-cash flow sensitivity has decreased over time, d) fixed effects (FE) is the most appropriate estimation method for this research question, e) the strict exogeneity assumption is generally violated, and f) there is evidence that the inconsistency caused by violation of strict exogeneity can be substantial. My main findings indicate that the investment-cash flow sensitivity has decreased and (mostly) disappeared over time. This finding is robust to alternative specifications and a number of robustness checks. Also, the usage of different estimation methods does not significantly alter the main findings, while FE seems to be the most suitable estimation method; however, the choice of the estimation method can have a substantial effect on the estimation results. Furthermore, in this setting, strict exogeneity does not seem to hold and the inconsistency caused by this can be substantial.

The rest of the paper is organized as follows. Section 2 scans the relevant literature, divided into chapters on the influential early papers, more recently written papers, and papers written on common application errors of estimation methods on panel data. Section 3 describes the data and the classification scheme applied to the data. Section 4 presents the models used in this thesis, specified as market-to-book and sales accelerator models of investment. Section 5 covers the relevant estimation methods employed, as well as explanations of the strict exogeneity test and the impact of a violation of the strict exogeneity assumption. Section 6 addresses the topic of standard errors, including a discussion of appropriate clustering. Section 7 presents the results, compares these to Fazzari et al. (1988), analyses differences of estimates based on the respective estimation methods, draws inferences based on the strict exogeneity tests, and explores whether the general findings hold in a number of robustness checks. Section 8 concludes, discusses this paper's contributions and drawbacks, and outlines possibilities for future research.

## 2. Literature Review

### 2.1. Early Investment Literature

In the post-war period, investment research, especially the work of Meyer and Kuh (1957), examined the importance of financial considerations in business investment decisions. In general, established by the "debt inflation" school, financial effects on various facets of real economic activity received much attention. However, starting in the 1960s, research mostly isolated real firm decisions from purely financial factors. The theoretical basis was laid by Modigliani and Miller (1958) who demonstrated that, under certain conditions, financial structure and financial policy are irrelevant for real investment. According to them, a firm's financial structure does not affect its market value in perfect capital markets. If the assumptions put forward in their work are satisfied, real firm decisions, motivated by the maximization of shareholder value, are independent of financial factors such as a firm's liquidity, debt leverage, and dividend payments.

Fazzari et al. (1988) relate the traditional study of financial effects on investment to a recent literature on capital market imperfections by studying investment behavior in groups of firms with different financial characteristics. Their article was a starting point for a number of studies that show that investment is more sensitive to cash flow for firms that are a priori classified to be more financially constrained. FHP's findings are based on a sample that comprises manufacturing firms with positive sales growth from 1969–1984. The size of the cash flow coefficients is generally large, ranging from approximately 0.2 for the unconstrained sub-sample of firms to 0.6 for the constrained sub-sample. They test for the accelerator, neoclassical, and $q$ models of investment, with $q$ being the main specification. A simple cash flow model stresses only the cost of capital side. This is of crucial importance, however, given that it may capture the wedge between the internal and external costs of finance and given that internal finance in the form of retained earnings generates the majority of the net funds for firms in all size categories.

The simple accelerator model stresses the demand for the capital side of the investment decision. The neoclassical theory incorporates the principle of the accelerator model by making investment a function of output and lagged capital stock. However, it differs from the accelerator model by additionally making investment dependent on product price and idiosyncratic cost of capital. For the purposes of this paper, it is sufficient to use – besides a $q$ measure – the accelerator model, as both theories, accelerator and neoclassic, make today's investment a function of today's output. The $q$ theory of investment incorporates the basic assumptions and conditions of the neoclassical model. Under these, differences in $q$ across firms reflect differences in desired capital stocks relative to actual capital stocks. Thereby, these differences should explain differences in investment, without actually having to measure the cost of capital of individual firms. Fazzari et al. (1988) invoke transaction costs, tax advantages, agency problems, costs of financial distress, and (especially) asymmetric information as reasons for internal and external finance not being perfect substitutes in practice. They argue that if the cost of capital differs by the source of funds, the availability of finance will likely have an effect on the investment practice of some firms. In financing hierarchy models, the availability of internal funds allows firms to undertake desirable investment projects without the need to resort to high-cost external finance.

Devereux and Schiantarelli (1990) have written an influential paper whose findings are in line with those presented in Fazzari et al. (1988). They provide empirical evidence on the impact of financial factors like cash flow, debt, and stock measures of liquidity on the investment decisions of 720 U.K.

manufacturing firms. These firms are split by size, age, and type of industry (growing or declining) – as proxies for the degree of financing constraints – over the period 1969–1986. This classification scheme is different to those employed in most of the other early papers in the field, such as Fazzari et al. (1988), Gertler and Hubbard (1988), and Hoshi et al. (1991), which use "broad" proxies for financing constraints like the dividend-payout ratio. Apart from that, while the approach in Devereux and Schiantarelli (1990) is very similar to the one in Fazzari et al. (1988), the explanatory variables are introduced via a different, more comprehensive extension of the $q$ model of investment. The model explicitly includes a term representing agency costs. This agency cost function is expected to vary for firms in different age and size classes and in different industries. The model also includes lagged values of the dependent variable and of each regressor to allow for the possibility of an innovation error that follows a first-order autoregressive process. The model is – contrary to the common approach in the literature – estimated in first differences to allow for firm-specific, time-invariant effects and an instrumental variables procedure is used to allow for endogeneity of the regressors. (Endogeneity can arise because current cash flow, debt, current assets, $Q$ , and investment may all be simultaneously determined.)

Like in Fazzari et al. (1988), the econometric results indicate that financial factors, principally in the form of lagged cash flow, have an independent effect on investment. The size of the effect is, however, smaller than in Fazzari et al. (1988), ranging from around 0.05–0.25. Cash flow has a (slightly) higher coefficient in the small, young firm sub- sample than in the small, mature firm sub-sample, as one expects when the market learns to evaluate investment opportunities better with time. Moreover, as outlined in Titman and Wessels (1988), smaller firms regularly tend to be less diversified, to display greater earnings volatility, and to be more prone to bankruptcy. In contrast to that, since size may proxy for (a diversified) ownership structure, in which agency problems can be more pronounced, there is some ambiguity in assessing the effect of size on agency cost. Hence, the authors use these agency costs arising from large firms' diversified ownership base to explain the magnitude of the impact of cash flow on investment, which is larger for large firms than for small firms.

Hoshi et al. (1991) is another well-known study that interprets a greater investment-cash flow sensitivity of firms, which are a priori considered to be more likely to face a larger wedge between the internal and external cost of funds, as evidence that these firms are indeed financially constrained. The authors work with a panel data sub-set of Japanese manufacturing firms listed on the Tokyo Stock Exchange between 1965 and 1986. These firms are divided on the basis whether they belong to a keiretsu and, thus, to a large extent whether they have a main-bank relationship. This scheme is based on the theories by Myers and Majluf (1984), who suggest a positive role for a main-bank relationship in reducing informational asymmetries and, thus, in alleviating financing constraints. Stressing these theories, the authors interpret their findings, namely that Japanese firms with an exclusive bank relationship have a lower investment-cash flow sensitivity, as evidence that a main-bank relationship reduces financial constraints. However, this interpretation is questioned by the theory of Sharpe (1990), among others. Sharpe argues that banks can exploit an exclusive main-bank relationship and make client firms more constrained by charging them a higher cost of capital. The finding in Hoshi et al. (1991) that the financially strongest Japanese firms subsequently broke their bank relationship is consistent with this interpretation. This theoretical ambiguity is not unique to this paper.

Blanchard et al. (1994) analyze what firms do with cash windfalls, which do not change their investment opportunity set, that is, their marginal Tobin's $q$. The authors' sample is comprised of eleven firms with such windfalls in the form of a won or settled lawsuit during 1980–1986. This sample includes firms without attractive investment opportunities. Nevertheless, the managers of these firms choose to keep the cash windfall in the firm rather than distribute it to the shareholder. If anything, they typically borrow more after the windfall. Like in Fazzari et al. (1988), this evidence is broadly inconsistent with the perfect capital markets model; rather it supports the agency model of managerial behavior, in which managers try to ensure the long-term survival and independence of the firms with themselves as the commander-in-chief.

In Lamont (1997), data from the 1986 oil price decrease are used to examine physical investment of oil companies' non-oil subsidiaries. The 1986 oil shock, during which oil prices fell by 50 percent, is argued to be an unambiguously exogenous shock to any individual firm. Lamont identifies a group of firms that have corporate segments in the oil extraction industry and in non-oil industries and tests whether a decrease in cash/collateral decreases investment and whether the finance costs of different parts of the same corporation are interdependent. Similar to Blanchard et al. (1994), the profitability of investment opportunities is not impacted. This is because marginal $q$ in corporate segments in the non-oil industries is uncorrelated with marginal $q$ in the oil extraction industry. Results in Lamont (1997) support the hypotheses: oil companies significantly reduced their non-oil investment compared to the median industry investment. Though the sample size is fairly small, the results appear to be moderately robust. This is interpreted as external capital markets being imperfect (i.e., financial slack matters for investment) and as internal capital markets allocating capital within firms (i.e., different parts of the firm are interdependent).

Kaplan and Zingales (1997) is the first influential study to oppose the findings in Fazzari et al. (1988) and in the follow-up literature. Kaplan and Zingales are agnostic on what source of capital market imperfection causes financing constraints. Unlike Blanchard et al. (1994), the authors' goal is to understand the effects capital market imperfections have on investment. For that, they investigate the relationship between financing constraints and investment-cash flow sensitivities by analyzing the 49 low-dividend-firms identified in

Fazzari et al. (1988). Their approach is to examine each of the 49 firms' annual reports for each sample year, and read the management's discussion of liquidity that describes the firm's future needs of funds and sources it plans to use to meet those needs. Kaplan and Zingales integrate this information with quantitative data and public news in order to retrieve a comprehensive picture of the availability of funds for each firm, as well as each firm's demand for funds. Based on that, they rank the extent to which the sample firms are financially constrained each year. According to that, in only 15 percent of firm-years there is some question as to a firm's ability to access internal or external funds to finance investment. They present theoretical and empirical evidence that a greater sensitivity of investment to cash flow is not a reliable measure of the differential cost between internal and external finance. This evidence holds for the entire sample period, sub-periods, and individual years.

Cleary (1999) confirms the findings in Kaplan and Zingales (1997) using, with 1,080 firms, a much larger sample over the period 1991–1994. In fact, in his doctoral thesis, he finds that investment-cash flow sensitivities are actually inversely related to constraints – the most constrained firms have the lowest sensitivities and the least constrained firms have the highest sensitivities. However, the size of the sensitivities is only around 0.1–0.2, while Kaplan and Zingales report estimates of around 0.3–0.6, potentially indicating that sensitivities have decreased over time. In the early papers in this literature stream, it is fairly standard to sort firms according to within-sample characteristics, such as in Fazzari et al. (1988) and Hoshi et al. (1991). As this approach has received a number of criticisms, Kaplan and Zingales (1997), as well as Cleary (1999), address this concern by defining a company's financial status in a way that reflects only past (not future) information.

## 2.2. Recent Investement Literature

Despite – and probably also because of – controversy in the investment-cash flow literature, many studies still analyze and use investment-cash flow sensitivity. **?** use data from 1985–2001 to examine the investment-cash flow sensitivity of US manufacturing firms in relation to five factors that they associate with capital market imperfections. They are one of the first to explicitly analyze and find a steady decline in the estimated sensitivity over time. The overall evidence suggests that the sensitivity of investment to cash flow decreases with factors that reduce capital market imperfections. This implies that the sensitivity of investments to the availability of internal funds cannot be explained solely as an artifact of measurement error – which has often been used in the more recent literature to explain a positive sensitivity, especially related to the *M/B* measure employed.

A paper by Brown and Petersen (2009) follows **?**, but lays a greater emphasis on the impact of the changing composition of investment and of developments in equity markets on the sensitivity. Brown and Petersen cover the period 1970–2006, split into three sub- periods. They split firms into young and mature. The standard OLS fixed effects model used in the investment-cash flow literature is employed, though the paper's main results are based on dynamic investment regressions using general methods of moments (GMM), where cash flow and other financial variables are treated as endogenous. Sensitivity of investment to cash flow largely disappears for physical investment, remains relatively strong for R& D, and substantially declines for total investment. The GMM regressions that control for negative cash flow and include measures of external finance show a decline in the sensitivity of at least 70 percent over 1970–1981 and 1994–2006, largely explained by the decrease in importance of tangible investment relative to total investment. The empirical strategy in Brown and Petersen (2009) is motivated by a number of papers that criticize conventional investment-cash flow regressions, especially when these do not control for the potential endogeneity of cash flow and when the possible importance of external finance is neglected.

Criticism of the methodology in the standard investment-cash flow literature is also the motivation of a study written by Gatchev et al. (2010), who develop a dynamic multi- equation model where firms make financing and investment decisions simultaneously, subject to the constraint that sources must equal uses of cash. They argue that static models of financial decisions – as mostly employed in the literature – produce inconsistent coefficient estimates, and that models that do not acknowledge the interdependence among decision variables produce inefficient estimates. The authors work with annual data, which exclude financial institutions and utilities, spanning the period 1950–2007. When they use a standard single-equation approach, the coefficient on cash flow has a size of 0.47 – as substantial as in the papers by Fazzari et al. (1988) and Kaplan and Zingales (1997). However, using their system-of-equations model, estimates are in many specifications indistinguishable from zero. This difference in coefficient estimates basically stems from the inclusion of lagged capital expenditures (CapEx) in the system-of-equations model. This suggests that failing to account for persistence can lead to biased results. The system-of-equations approach examines capital constraints comprehensively by allowing indirect (investment-cash flow) and direct (financing-cash flow) effects to be studied simultaneously. Compared to the static single-equation methodology, the multivariate model produces substantially smaller estimates of the investment-cash flow sensitivity and makes clear that the sensitivity to cash flow of financing dominates over investment. This makes clear that firms absorb cash-flow fluctuations mainly by altering net debt – and not by changing real assets. That is, they decrease leverage and basically do not invest. In addition to that, unlike the static single-equation studies that find that firms underinvest given cash-flow shortfalls, Gatchev, Pulvino, and Tarhan conclude that firms maintain investment by borrowing.

Chen and Chen (2012) try to settle the debate on the interpretation of investment-cash flow sensitivity as a measure of financial constraints. They find that the investment-cash flow sensitivity has declined and disappeared, even during the 2007–2009 credit crunch. The results are robust to considerations of R& D and cash reserves, and across groups of firms. Though the information content in cash flow regarding investment opportunities has declined, measurement error in Tobin's $q$ cannot completely explain the patterns in the sensitivity. Decline and disappearance can neither be explained by changes in sample composition, corporate governance, or market power. The authors show that the investment-cash flow sensitivity is about 0.3 in the 1960s. Since 1997, it has been below 0.03. It has disappeared in manufacturing, as well as non-manufacturing firms. These findings are robust to alternative model specifications.

Like in Blanchard et al. (1994) and Lamont (1997), Andrén and Jankensgård (2015) use an exogenous shock as a basis for their study. They are the first to bring evidence from some kind of natural experiment in which there was an unexpected, substantial, and persistent decrease in the cost of external financing: the sudden abundance of liquidity in the oil and gas industry in the mid 2000s. This abundance was triggered by high oil prices and an eased access to external financing, and the authors look at its influence on the investment-cash flow relationship. For that, they use a balanced sample of 78 firms, rendering 612 firm-year observations. Firm size is the splitting criterion for classifying firms as constrained or unconstrained. By carrying out regressions of investment on Tobin's $q$ and financial variables (cash flow, cash, and leverage), the authors use the standard methodology in the literature. They find that, for financially constrained firms, the investment-cash flow sensitivity decreased in the abundance period (2005–2008), suggesting that the financial constraints became less binding. Instead, for financially unconstrained firms, the sensitivity increases over time, suggesting that this relationship is driven by agency problems related to free cash flow. Hence, this paper's results are partly at odds with findings in other recent papers, such as Brown and Petersen (2009), who find that the investment-cash flow sensitivity has decreased over time due to capital market improvements. Andrén and Jankensgard verify the differential role of cash flow to investment across systematically different types of firms, even in a recent period (2000–2008).

Chowdhury et al. (2016) try to mitigate some of the conceptual and methodological problems brought up in the investment-cash flow literature by using a research design that relates changes in the sensitivity to changes in information asymmetries. The bid-ask spread surrounding the implementation of the Sarbanes-Oxley (SOX) Act (2002) and the deregulation of firms in the transportation, telecommunication, and petroleum and natural gas industries (end of 1970s) serves as a measure of information asymmetry. The authors base their paper on the idea in Cleary et al. (2007) that two firms, ceteris paribus, may face differently severe problems of information asymmetry. Cleary et al. (2007) predict that the investment-cash flow sensitivity is unambiguously higher the greater the asymmetry of information. Chowdhury et al. (2016) find that information asymmetry decreases following SOX and that there is a corresponding decrease in the sensitivity, pre- to post- SOX. Greater decreases in information asymmetry following SOX are associated with greater decreases in the sensitivity of investment to cash flow. They also detect an increase in information asymmetry with a corresponding increase in the sensitivity following deregulation.

Other relatively recent studies examine the presence of financial market frictions. For that, they analyze the connection between changes in the values of pledgeable assets and financing. These tests generally produce evidence that financially weak firms face difficulties in raising funds through equity or debt markets. For example, Rauh (2006) finds that CapEx decline with mandatory contributions to defined benefit pension plans. Also, Almeida and Campello (2007) find that sensitivity of investment to cash flow increases – in the case of financially constrained firms – in the tangibility of assets. Hence, they conclude that financing frictions influence investment decisions. Another influential example showing that the topic of financing constraints is broadened beyond investment-cash flow sensitivities is the forthcoming paper in The Accounting Review by Linck et al. (2013). They hypothesize that a financially constrained firm with valuable projects can use discretionary accruals to credibly signal positive prospects, thus easing the possibility to raise capital to make the investments. To test the hypothesis, the authors use panel data for 1987–2009. They find, inter alia, that financially constrained firms with good investment opportunities have significantly higher discretionary accruals prior to investment compared to their unconstrained counterparts. Their results support evidence that the use of discretionary accruals can help promising firms suffering from financing constraints to ease those constraints and increase firm value.

### 2.3. Application Errors of Estimation Methods on Panel Data

A couple of important papers have recently been written on common application errors in the finance literature of the fixed-effects estimator on panel data (Grieser and Hadlock (2015), p. 1). For example, Gormley and Matsa (2014) discuss the limitations of two widely used approaches in finance research: demeaning the dependent variable with respect to the group (e.g., industry-adjusting) and adding the mean of the group's dependent variable as a control. Both methods produce inconsistent estimates, thereby distorting inference. As an alternative, the FE estimator is consistent and should therefore be used.

Petersen (2009) examines the different methods used in the literature to work around the problem that, in panel data sets, the residuals may be correlated across firms or across time. Thompson (2011) writes that it is common practice, when estimating finance panel regressions, to adjust the standard errors for correlation either across firms, or across time. These procedures are only valid if the residuals are not correlated across time and firms. Thompson shows that it is easy to calculate standard errors that are robust to simultaneous correlation along two dimensions, such as firms and time. Both Petersen (2009) and Thompson (2011) highlight that researchers do not use the appropriate standard errors to adjust for the types of error variance and covariance structures that are common in finance settings. This recent literature does not emphasize the strict exogeneity assumption that must hold for the FE estimator or its cousin, the first-difference (FD) estimator, in order to have a chance to consistently estimate the coefficients of interest. It is already a cursory consideration of the variables used in finance research that suggests that this assumption will often be violated. Many of the relevant dependent variables to financial economists are likely to be related to the subsequent evolution of the key explanatory variables. In Strebulaev et al. (2012), as well as in other papers, dynamic theoretical models posit exactly this feedback that will lead to violation of the assumption.

The work by Wintoki et al. (2012) is the first exception in that it acknowledges the strict exogeneity issue in a panel data finance model that does not include a lagged dependent variable. (As the inclusion of a lagged dependent variable in any panel data analysis structurally violates strict exogeneity in the underlying model, there is no need to test for it anymore.) Wintoki, Linck, and Netter highlight the importance of strict exogeneity in the setting of the effect of board structure on firm performance. They question prior work on this issue that relies on (probably) inconsistent FE or FD estimators when the authors reject the validity of the strict exogeneity assumption. A paper by Grieser and Hadlock (2015) is similar in many ways to Wintoki et al. (2012). However, by examining the strict exogeneity assumption in a set of canonical panel-data regression models selected from the existing finance literature, the paper does not restrict attention to one specific research context, but highlights that this issue applies to a large set of empirical models in finance. The work by Grieser and Hadlock (2015) serves as a basis and guidance for the analysis of the strict exogeneity assumption in this thesis. The authors use the entire universe of available Compustat data (excluding financials and utilities) from 1965–2012. They search through every issue of the Journal of Finance, Journal of Financial Economics, and Review of Financial Studies over the period 2006–2013. They categorize each paper that features an empirical model with unit-level (e.g., firm, bank, person) fixed effects rather than solely time (e.g., year, quarter) effects into traditional FE estimates (most often used), traditional FD estimates, or dynamic panel GMM estimates. Each categorized panel data study is then assigned into a broad set of mutually exclusive categories based on the employed dependent variables (inter alia, leverage) and corresponding relevant independent variables (inter alia, return on assets). That way, the authors work out the five dependent variables that are used most frequently in the literature.

Based on Wooldridge (2010), for every model created, it is tested whether the strict exogeneity assumption holds and explored whether failures in the assumption are probable to lead to substantial inconsistencies in the common estimators. In the paper, it becomes evident that the strict exogeneity assumption is quite frequently violated; it can even be rejected in virtually all of the canonical regression models considered by the authors when large samples are used. In addition to that, the inconsistency caused by the violation can have a significant effect on economic inferences in finance settings. The problem of the inconsistency in the FE estimator is known to be around $1/T$, where $T$ denotes the number of time periods. (The inconsistency in the FD estimator does not depend on $T$.) $1/T$ requires the presence of stable, i.e., time-invariant, fixed effects. Demonstrating that in common finance panel settings unit-level fixed effects seem to change over time, the authors show that it appears unlikely that the $1/T$ results will solve the problem – provided that a large number of time periods is actually available. Also, differences between the FE and FD estimates are regularly in the order of 50 percent or higher. Using these differences to gauge the possible magnitude of inference errors, a substantial economic impact of basing inferences on inconsistent estimates is not unlikely. Based on the evidence presented, Grieser and Hadlock (2015) indicate that simple FE or FD panel data estimators are in many cases not the correct tools to use in settings that include the presence of unit-level fixed effects – and they offer a serious challenge to empirical finance research.

Overall, this literature review makes clear, among other things, that a) the investment- cash flow sensitivity has decreased over time, b) a number of papers criticize the interpretation of the sensitivity as a measure of financial constraints (e.g., an increase in sensitivity cannot be seen as an increase in financing constraints), c) a number of recent papers criticize conventional investment-cash flow regressions (e.g., for neglecting the possibility of impact of external finance), d) the topic of financing constraints and investment-cash flow sensitivity constitutes one of the largest literature streams in corporate finance and is still a vivid research field, and e) testing for violation of the strict exogeneity assumption is of importance. This paper adds value to a line of research that is of ongoing interest. It basically uses the structure in Fazzari et al. (1988) – the parent of all papers on this topic –, analyzes what has changed (e.g., decrease in sensitivity over time), puts a greater focus on the econometrics involved (e.g., analyses of estimation methods, strict exogeneity assumption, and standard errors), and adds some of the more important findings in the recent literature to the thesis (e.g., changing composition of investment, impact of external finance).

## 3. Sample

The sample in this study consists of US industrial firms between 1990 and 2015. Data is obtained from Compustat. The sample period covers two times of crises and three waves of corporate investment. The first crisis happened after the burst of the dot-com bubble, which climaxed in 2000. The second crisis was the global financial crisis of 2007–2008. The first wave of corporate investment was related to the dot-com splurge of 1997–2001. Cash was poured into building cell-phone networks and the Internet's backbone. From 2003–2010, there was an emerging-market frenzy, with Western firms investing about $ 2 trillion in factories and other facilities in places like China and India. Finally, also driven by insatiable Chinese demand, there was a craze for commodities over 2005–2013. Global energy and metals firms spent $ 6 trillion digging in the Australian outback and drilling for oil in North Dakota and deep beneath Brazil's costal waters (The Economist (2016)). Data in this sample are annual and include companies with calendar-year end unequal to fiscal-year end. (This will slightly impact year-fixed effects.) Analyses are based on an unbalanced panel. This might come with the disadvantage that there is a non-random sample in advanced time periods, as, for example, a lack of investment took companies out of business (Wooldridge (2013), p. 491). Balancing the panel, however, may introduce sample bias in that firms with certain characteristics are more likely to enter or exit the sample, such as survivorship bias (Andrén and Jankensgård (2015), p. 206). Given the pros and cons, the robustness section (7.3) includes an analysis based on a balanced panel. The definition of the empirical variables follows in chapter 4.

Robust regression is an alternative to ordinary least squares (OLS) regression when data is contaminated with outliers or influential observations and it can be used for the purpose of detecting influential observations. The idea of robust regression is to weight the observations differently based on how well behaved these observations are. Hence, it is a form of weighted and reweighted least squares regression (Li (1985)). In Stata, the programming language used for this thesis, a version of robust regression first runs OLS regression, gets the Cook's distance – a measure that combines the information of leverage and residual of the observation – for each observation, and then drops any observation with Cook's distance greater than one. In the following iteration process, the most influential points are dropped, and then cases with large absolute residuals are down-weighted (Verardi and Croux (2009)). Figure 1 shows the leverage versus the squared residuals of observations, labeled with the entity names (GVKEY) and serving as the basis for the computation of Cook's distance. I drop the observations with Cook's distance greater one. (In figure 1, these are observations on entities 109522 and 165743.) When the two observations are excluded, the estimation results of robust regression and OLS regression are very close together. Also, for OLS, standard errors are lower, F-statistics higher, adjusted R-squared higher, and root mean squared error lower – and thereby summary statistics more closely resemble those of the robust regression. (Results are not shown in a table; the corresponding Stata commands can be found in the appendix.) The impact of outliers can be substantial. In Fazzari et al. (1988), for example, eliminating or down-weighting high-growth firm years reduces the estimated investment-cash flow sensitivity of the entire low dividend-payout sample to 0.2–0.25. This is effectively identical to the estimate of their unconstrained, low-retention sample. These results suggest that FHP's overall findings are at least partially impacted by extreme observations, given that high-payout firms are less likely to experience such extreme growth rates (Kaplan and Zingales (1997), p. 206).

Biased findings based on differential outliers in sub-samples are not restricted to Fazzari et al. (1988). These differential outliers are one reason why "broad" classification schemes are generally no longer without controversy. Any splitting criterion that sorts firms into sub-samples with differential outliers in growth rates – this holds, for example, for splits on size and age as well –, so that certain groups grow faster, may be biased toward finding a difference in coefficients on cash flow. This bias may partially explain the large body of evidence (in early literature) finding a higher investment-cash flow sensitivity in fast growing companies, which tend to be classified as financially constrained (Kaplan and Zingales (1997), p. 206). In another criticism, Cleary et al. (2007) find that two otherwise identical firms may face differently severe problems of information asymmetry – implying that there might be substantial heterogeneity regarding financing constraints in a single sub-sample that is based on these broad classifications. Their model predicts that the sensitivity of investment to cash flow is unambiguously higher the greater the asymmetry of information.

This idea serves as a basis for research designs in a number of recent studies, such as Chowdhury et al. (2016), using measures of information asymmetry as proxies for capital market imperfections / financing constraints instead of the broad splits mentioned above. Anyway, applying the research design in Fazzari et al. (1988), I use industrial firm data to analyze differences in investment in firms classified according to their dividend- payout practices. Payout practices should reveal little about investment if the cost disadvantage of external finance is small. Firms will, in this case, use external funds to smooth investment when internal finance fluctuates, independent of their dividend policy. If the cost disadvantage is significant, however, investment should be driven by fluctuations in cash flow for firms that retain and invest most of their income, indicating that they do not have a low-cost source of investment finance (Fazzari et al. (1988), pp. 157– 158). Thus, observed payout practices may provide a useful a priori criterion for identifying firms that are likely to face relatively high costs of external finance.

The classification scheme applied in this paper divides firms into three groups based on dividend payouts. Also, the sample period is divided into two sub-sample periods: period 1 covers the years 1990–2002; period 2 covers 2003–2015. Class-1 firms have a ratio of dividends to net income of less than 0.1 for at least 16 years over the total sample period. In order to be classified as a class-1 firm in either period 1, or period 2, a firm needs to have a dividend-net income ratio of less than 0.1 in at least eight years, respectively. Class-2 firms have a dividend-income ratio of less than 0.3, but more than 0.1, for at least 15 years over the total sample period (seven years for periods 1 and 2, respectively). I use 15 and seven years due to the small number of observations in the second class. The third class includes all other firms. A firm's income can be abnormally low in a particular year. Because of the resulting outlier in the dividend- income ratio, this approach is more robust than classifying firms according

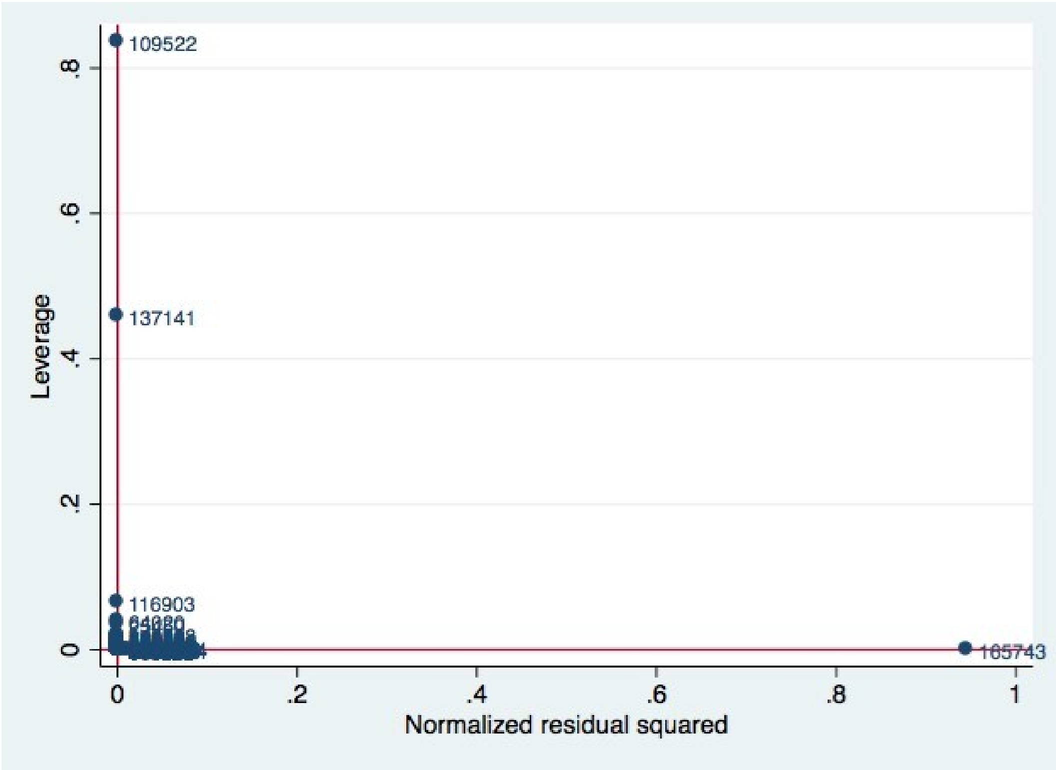


**Figure 1:** Leverage vs. Squared Residuals of Observations, 1990–2015

to their average payout ratio. Also, the classification scheme with at least 7–8 ratio observations in the respective time periods does not come at the cost of too many lost observations, as most firm-year payout observations are available over the entire (sub-)sample period. On top of that, the classification scheme implies that many firms that went out of business over the sample period are naturally excluded, because they will not have that many payout observations – impeding the impact of attrition bias. Structural interpretation of the coefficients should be seen with caution. As is standard in the literature, it is the differential results across sub-samples that inferences are based on.

Summary statistics of the three payout-classes are presented in table 1. These statistics are based on the sample that excludes the two observations with Cook's distance greater one. Clearly, the lowest number of observations is available for class-2 firms. This is why the sub-sample includes firms with payout ratios of up to 0.3 – in contrast to Fazzari et al. (1988), whose class-2 firms are in the payout ratio range of 0.1–0.2. With 4.3, the average number of years with positive dividends in class 1 is obviously the lowest, given that class-2 and class-3 firms pay dividends in almost every year over the sample period. Median sales growth figures are inconspicuous across classes, while their standard deviations are very high for classes 1 and 3. Cash flow (0.2) is insufficient to stem physical investment for class-1 firms (0.94). Both variables are scaled by capital. Again, standard deviations are very high for classes 1 and 3. Average capital stock values across classes are much higher than median values, accounting for the number of high-capital-stock observations. Average capital stock growth over the sample period is highest in classes 1 and 2, with some companies in class 2 experiencing very high growth in the capital stock, while median capital stocks grow strongest in class 1. Finally, average $M/B$ ratios are higher than median $M/B$ ratios. Average $M/B$ is highest in class 1, driven by some high-growth/high-potential companies. Median $M/B$ is highest in class 3 and lowest in class 1. Section 7.1 includes a comparison of descriptive statistics to those in Fazzari et al. (1988).

## 4. Models

I examine two of the broad empirical specifications that encompass the most common approaches of constructing models' investment demand side. First (and more important), these are models based on a market-to-book, $M/B$, ratio – though usually called $q$ models of investment in the literature – that emphasize market valuations of the firm's assets as the determinant of investment. Derived from an adjustment cost technology, investment is determined according to

$$I_{it}/K_{it-1} = \beta_o + \beta_1 (M/B)_{it} + u_{it'} \quad (1)$$

where $I_{it}/K_{it-1}$ represents investment in property, plant, and equipment (CapEx) for firm $i$ during period $t$, normalized by a firm's capital stock at the beginning of the period.

**Table 1:** Summary Statistics: Sample of Firms, 1990- 2015

Source: author's calculations based on samples selected from the Compustat database. See text.
a. Firms with dividend-income ratios of less than 0.1 for at least 16 years.
b. Firms with dividend- income ratios greater than 0.1 but less than 0.3 for at least 15 years.
c. All other firms.
d. In millions of dollars

| Statistic | Category of firm | | |
|---|---|---|---|
| | Class 1[a] | Class 2[b] | Class 3[c] |
| Number of observations | 51,111 | 2,763 | 27,788 |
| Median payout ratio | 0 | 0.21 | 0.58 |
| Average number of years with positive dividends | 42798 | 42790 | 42847 |
| Median sales growth | 8.4% | 8.2% | 0,05 |
| Standard deviation sales growth | 8,000.2% | 14.9% | 4,900.2% |
| Median investment-capital ratio | 0.94 | 0.24 | 0.17 |
| Standard deviation investment-capital ratio | 42824 | 0.2 | 0.6 |
| Median cash flow-capital ratio | 0.2 | 0.49 | 0.19 |
| Standard deviation cash flow-capital ratio | 217.5 | 42795 | 21 |
| Average capital stock, 1990[d] | 154 | 929.8 | 2,457.2 |
| Median capital stock, 1990[d] | 42890 | 148.8 | 556.2 |
| Average capital stock, 2015[d] | 566.3 | 4,721.9 | 4,516 |
| Median capital stock, 2015[d] | 45.2 | 672.7 | 1,440.2 |
| Average M/B ratio | 42737 | 42767 | 42887 |
| Median M/B ratio | 0.3 | 0.6 | 0.7 |

$\beta_o$ is the normal value for $I$ / $K$ for the $i$th firm when $M/B$ is zero. $M/B$ is calculated as number of common shares outstanding, multiplied with the annual closing stock price, plus the book value of current and long-term liabilities; the sum is divided by the book value of total assets. These variables are preferred over Compustat's total debt (DT) and market capitalization (MKVALT) variables, as for the two latter variables observations are unavailable prior to 1998. It is common practice to assume equivalence of the liabilities' market and book value. $u_{it}$ is an error term. Some skeptical authors point out that Tobin's $q$ is difficult to measure and has strict conditions under which it is sufficient to assess how much the firm should invest – this is why it is called $M/B$ in this paper. Hayashi (1982) has derived these conditions. There are at least two problems in measuring $q$ that might affect the econometric results for liquidity: first, $q$ may not reflect market fundamentals when the stock market is excessively volatile. Second, there might be measurement error in the replacement capital stock in $q$ (Hoshi et al. (1991), p. 43). Hence, it should not be surprising that in investment regression equations that include (flow or stock of) liquidity and $q$, both variables are significant. (Either $q$ is mismeasured, or liquidity constraints are important.) However, the intuition of the $M/B$ model is that, absent considerations of taxes or capital market imperfections, a value-maximizing firm will keep investing as long as the shadow value of an additional unit of capital, marginal $M/B$, exceeds unity. In equilibrium, the value of an extra unit of capital is just its replacement cost, so that marginal $M/B$ is unity. The theoretical advantage of this framework in modeling the effects of internal finance on investment (see below) is that $M/B$ supposedly controls for the market's evaluation of the firm's investment opportunities (Fazzari et al. (1988), p. 165). This is important, as internal finance also proxies for other unobservable determinants of investment, in particular the profitability of investment. High liquidity signals that the firm has done well and is likely to continue doing well. Thus, more liquid firms have better investment opportunities; it is not surprising that they tend to invest more (Hoshi et al. (1991), p. 36). Again, there is theoretical ambiguity, this time on the economic interpretation of a high cash stock. Almeida et al. (2004) claim that constrained firms hoard cash to protect against future downturns. (They call this the cash flow sensitivity of cash.) Following this reasoning, high liquidity may be a signal for poor performance, contrary to what Hoshi, Kashyap, and Scharfstein (Hoshi et al. (1991), p. 36) outline.

4.1. *M/B* Specification

In this paper, investment is often determined using the $M/B$ model, including cash flow and firm and year fixed effects, for each of the three payout-classes. That is,

$$I_{it}/K_{it-1} = \beta_o + \beta_1(M/B)_{it} + \beta_2(CF_{it}/K_{it-1}) + \alpha_1 + \lambda_t + u_{it'} \tag{2}$$

where $CF_{it}/K_{it-1}$ is net income before depreciation, less dividend payments, deflated by beginning-of-period capital.

$\alpha_i$ is a firm dummy to remove firm-specific effects. $\lambda_i$ is a year dummy to weed out macro shocks. (A detailed analysis of estimation methods can be found in chapter 5.) The argumentation for subtracting dividends in the cash flow number follows the reasoning of Lintner (1956). Lintner shows that dividend-smoothing behavior is widespread, based on three important observations concerning dividend policies: first, managers are primarily concerned with the stability of dividends. They believe that the market puts a premium on firms with a stable dividend policy. Second, earnings are the most important determinant of any change in dividends. Most companies appear to have a target payout ratio. If there is a sudden unexpected increase in earnings, firms adjust their dividends slowly. Also, firms are very reluctant to cut dividends. Third, management sets their dividend policy first. Other policies are then adjusted, taking dividend policy as given. For example, if investment opportunities are abundant and the firm has insufficient internal funds, it would resort to outside funds instead of cutting dividends. Hence, subtracting dividends is meaningful, as dividends seem to have a higher priority use of cash flow than investment. Anyway, the robustness section includes a test on the effects of cash flow defined without subtracting dividends.

Theories discussed in Fazzari et al. (1988) imply that for firms that face asymmetric information problems in capital markets, the supply of investment finance is not perfectly elastic. This is in line with Myers and Majluf (1984), who stress that if managers are better informed than investors about a firm's prospects, the firm's risky securities will sometimes be underpriced, thereby raising the cost of external finance. It is not only information problems, but also, as described above, incentive problems that lead managers to prefer financing investment with internal funds. Jensen and Meckling (1976) argue that these incentive problems raise the cost of external finance. Outside financing dilutes management's ownership stake, thereby exacerbating incentive problems that arise when managers control the firm but do not own it. So, regardless of the true economic process at the foundation of investment demand, i.e., no matter whether it is a $q$ model, an accelerator model or any other model, the supply of low-cost finance, and therefore the level of internal cash flow, enters the investment equation of firms for which internal and external finance are not perfect substitutes. This is confirmed by a large body of theoretical work that shows that (flow and stock measures of) liquidity should be an important determinant of investment when there are information problems in the capital markets. Basically all models that posit some sort of information problem in the capital market predict that more liquid firms should invest more. Models also predict that liquidity is irrelevant when there are no information problems (Hoshi et al. (1991), pp. 33–34).

In a first alternative specification of the model in equation 2, I include an analysis of models with cash flow lags and lagged $M/B$ ratios, respectively. The rationale for this analysis, apart from the possibility to compare the results to FHP (1988, table 6), is that it can give greater insights on the question whether cash flow contains news about investment opportunities. Effects of lagged coefficients on cash flow could well reflect shortcomings in the empirical performance of the $M/B$ ratio. For example, in FHP (1988, pp. 31–32), $Q$ is only half as large (0.001 vs. 0.002) when their model includes cash flow lags – a finding that supports this interpretation. Another rationale is that cash flow could have explanatory power in a time-to-build context, which could explain why a contemporaneous coefficient on cash flow lacks significance. In a second alternative specification, the effect of stock measures of a firm's internal liquidity is analyzed with the model

$$I_{it}/K_{it-1} = \beta_o + \beta_1 (M/B)_{it} + \beta_2 (Cash/K)_{it-1} + \alpha_i + \lambda_t + u_{it'} \tag{3}$$

where $(Cash/k)_{it-1}$ is the stock of liquidity, defined as cash and short-term securities, i.e., securities the firm describes as readily convertible into cash, normalized by the firm's capital stock in the beginning of the year. The stock of liquidity is measured at the beginning of the period to measure the stock of liquid assets the firm has when it decides on investment at the beginning of the period. This approach follows Kaplan and Zingales (1997) and Kashyap et al. (1994). Stock measures of a firm's internal liquidity, just like flow measures, might have an effect on investment for firms that must pay a premium for external funds; cash and marketable securities provide a low-cost source of investment finance for these firms. A financial cushion through accumulated liquid resources may reduce the sensitivity of investment to cash flow fluctuations for such firms. Hence, one might expect to observe a positive effect of stock measures of liquidity for the high-retention firms, whose investment is especially sensitive to fluctuations in cash flow. The motivation for this test is analogous to thoughts on precautionary saving. Managers should accumulate a stock of liquid assets when the operational cash flow is high if they know that there is a wedge in the costs of internal vis-à-vis external finance. That liquidity can help to smooth investment over downturns, so that firms impede the necessity to obtain potentially costly capital from external sources. In addition to that, the financial cushion provides collateral for new debt. Covenants may constrain a firm's ability to use stocks of liquidity. Therefore, when financially constrained firms experience a higher cash stock, they might be able to increase CapEx. Financially unconstrained firms – likely those in Class 3 that pay out a substantial portion of their earnings in the form of dividends – are not expected to experience a significant impact of stock measures of liquidity on investment. If these firms' retained earnings are lower than their desired level of investment, they should be able to easily raise low-cost external finance.

A potential further reason for the importance of specifying a model with cash stock as a key explanatory variable is that it is less likely than cash flow to indicate much about the profitability of new investments (Fazzari et al. (1988), pp. 179–181). However, Almeida et al. (2004) outline that high liquidity may also be a signal for poor performance. Kaplan and Zingales (Kaplan and Zingales (1997), pp. 202–203) argue that, in theory, there should be no difference between cash flow and cash stock: it does not matter whether an extra dollar enters the firm this period (cash flow) or whether it was present in the firm in the beginning of the year (cash stock). For this reason, they estimate a regression that measures liquidity as the sum of cash flow and cash stock. As Fazzari et al. (1988) outline, there is a theoretical basis for a significant isolated effect of cash stock on investment, so that I incorporate Kaplan and Zingales (1997) approach and estimate a regression adding the flow of cash to the stock of cash.

### 4.2. Sales Accelerator Specification

The second empirical specification encompassing a common approach in constructing a model's investment demand side is the acceleration principle. I specify the following model, in line with FHP (1988, table 7):

$$\begin{aligned} I_{it}/K_{it-1} = \beta_o + \beta_1(CF_{it}/K_{it-1}) + \beta_2(S_{it}/K_{it-1}) + \\ \beta_3(S_{it-1}/K_{it-2}) + \beta_4(S_{it-2}/K_{it-3}) + \\ \beta_5(S_{it-3}/K_{it-4}) + \alpha_i + \lambda_t + u_{it}, \end{aligned} \tag{4}$$

where $S_{it}/K_{it-1}$ is defined as a firm's sales, deflated by beginning-of-period net property, plant, and equipment. The model also includes three lags of this ratio. In the robustness chapter 7.3, I test whether production, that is sales plus change in finished goods inventories, instead of only sales, leads to different results. In a sales accelerator model, fluctuations in sales or output motivate changes in capital spending.

Many successful empirical investment models are based on the traditional acceleration principle, despite a lack of a compelling theory behind it (Hoshi et al. (1991), p. 35). For example, Schiantarelli and Georgoutsos (1987) have shown that when firms have monopoly power, lagged production should be related to current investment. Incorporating this sales model in the analysis is also helpful, as one possible explanation for the effects of cash flow variables is that internal finance is correlated with sales (omitted variable bias). As it is typical that both sales and cash flow have significant effects in an investment equation, the question arises whether the cash flow variable should be interpreted as a signal of the profitability of investment not captured in the sales formulation; or does significance mean that cash flow represents low-cost investment supply for firms that face a wedge between the costs of internal and external finance? Incorporating the $M/B$ ratio variable in the regression equation (in an additional model) might help to resolve the question, as it should be more adequate in capturing the prospective profitability of investment than lags of past profits, given that it is based on asset prices determined in forward-looking markets (Fazzari et al. (1988), pp. 173– 175). However, both empirical specifications of a model's investment demand side, $M/B$ and sales accelerator – and including the alternative specifications –, are imperfect attempts to control for effects that are difficult to observe. Structural interpretation of the coefficients should be seen with caution. This is why, inter alia, Fazzari et al. (1988) draw conclusions based on estimated differences across the three retention classes, Hoshi et al. (1991) make inferences based on the differences in the effects of liquidity across the two subsets of Japanese firms, and Kaplan and Zingales (1997) rest conclusions on the differences across firms with various degrees of financing constraints.

## 5. Estimation Methods

This paper pays special attention to the various estimation methods that are used for standard corporate finance regression equations with panel data, such as the equations handled here. In this chapter, these estimation methods are introduced and discussed. Chapter 7.2 handles the estimation specific results. A population multiple linear regression model usually takes the following form:

$$y = \beta_o + \beta_1 x_1 + \beta_2 x_2 + ... + \beta_k x_k + u. \tag{5}$$

The key assumption for this general multiple regression model is $E(u|x_1, x_2, ..., x_k) = 0$. It requires that all factors in the unobserved error term be uncorrelated with the explanatory variables. It also means that one has correctly accounted for the relationships between the dependent and independent variables. Any problem that causes $u$ to be correlated with any of the explanatory variables causes the assumption to fail (Stock and Watson (2015), pp. 236–238).

### 5.1. Ordinary Least Squares

OLS is a main estimation method. An exemplary estimated OLS equation, in the case of two independent variables, looks as follows:

$$\hat{y} = \hat{\beta}_o + \hat{\beta}_1 x_1 + \hat{\beta}_2 x_2. \tag{6}$$

OLS chooses the estimates that minimize the sum of squared residuals. That is, given n observations on $y, x_1$, and $x_2$, the argument that minimizes $\sum_{i=1}^{n}(y_i - \hat{\beta}_o - \hat{\beta}_1 x_{i1} - \hat{\beta}_2 x_{i2})^2$. The estimates $\hat{\beta}_1$ and $\hat{\beta}_2$ have partial effect, or ceteris paribus, interpretations. Hence, holding all other independent variables constant, the coefficient on $x_1$ measures the change in $\hat{y}$ due to a one-unit increase in $x_1$ (Wooldridge (2013), pp. 72–76). Under the first four Gauss-Markov assumptions, OLS is unbiased. When a homoskedasticity assumption, i.e., a constant error variance assumption, is added, OLS has an important efficiency property; the estimator is then the best linear unbiased estimator (BLUE), where best is defined as being the most efficient. Adding an irrelevant variable to an equation generally increases the variances of the remaining OLS estimators because of multicollinearity (Wooldridge (2013), p. 105).

### 5.2. Fixed Effects

The combined entity and time FE regression method, which is the main method used in this paper, eliminates omitted variable bias arising both from unobserved variables that are constant over time and from unobserved variables that are constant across entities. A model with a single explanatory variable has the following form:

$$y_{it} = \beta_1 x_{it} + \alpha_i + \lambda_t + u_{it}, \tag{7}$$

where $\alpha_1$ is the unobserved effect, which is constant over time and disappears in the within-transformation. $\lambda_t$ denotes a full set of year dummies, which can be left out from the following transformation. For each $i$, equation 7 is averaged over time:

$$\bar{y}_i = \beta_1 \bar{x}_i + \alpha_1 + \bar{u}_1. \tag{8}$$

Then, equation 8 is subtracted from equation 7:

$$y_{it} - \bar{y}_i = \beta_1 (x_{it} - \bar{x}_i + u_{it} - \bar{u}_i. \tag{9}$$

Accordingly, in time-demeaned data, the unobserved effect, $\alpha_i$, has disappeared:

$$\tilde{y}_{it} = \beta_1 \tilde{x}_{it} + \tilde{u}_{it}. \tag{10}$$

The pooled OLS estimator that is based on time-demeaned variables is called the FE estimator. It allows for arbitrary correlation between the unobserved effect and the explanatory variables in any period. Because of this transformation, any explanatory variable that is constant over time gets swept away. Hence, variables, such as a firm's distance from the airport, cannot be included in the regression equation (Wooldridge (2013), p. 484–485). Regression software typically does not use the dummy variable model formulation (not presented here), as it is tedious if the number of entities is large (Stock and Watson (2015), p. 403). The number of entity-specific intercepts required would be too large. Naturally, time demeaning is potentially problematic when the key explanatory variables do not vary much over time. But even with sufficient time-varying explanatory variables, the FE estimator can be subject to biases – just like the first differences (FD) estimator (see below). For example, strict exogeneity is a critical assumption. If it does not hold, the estimator is inconsistent. Fortunately, unlike the FD estimator, if each $x_{itj}$ is uncorrelated with $u_{it}$, but the strict exogeneity assumption is otherwise violated – e.g., when the lagged dependent variable is included among the regressors –, the FE estimator's bias tends to zero at the rate of *1/T* in theory (Wooldridge (2013), pp. 473–491). In a multiple regression with panel data, if the FE regression assumptions hold, the FE estimator is consistent and asymptotically normally distributed when $n$ is large (Stock and Watson (2015), p. 406). The assumptions (see table 2) extend the least squares assumptions to panel data.

### 5.2.1. Strict Exogeneity Test

The theoretical part on the strict exogeneity assumption is put as an FE sub-chapter, because the strict exogeneity tests are mainly based on the FE transformation, given that this is the main estimation method for linear unobserved effects panel data models employed in this paper, in the investment-cash flow literature, and in the papers published in the three leading finance journals (Grieser and Hadlock (2015), p. 9). However, I also conduct strict exogeneity tests based on the FD transformation. With an unobserved effect, the strict exogeneity assumption can have the following form:

$$E(y_{it}|x_{i1}, x_{i2}, ..., x_{iT}, \alpha_i) = E(y_{it}|x_{it}, \alpha_i) = x_{it}\beta + \alpha_i. \tag{11}$$

Once controlled for $x_{it}$ and $\alpha_i, x_{is}$ has no partial effect on $y_{it}$ for $s \;/t$. This assumption is more reasonable than the one without the unobserved effect. The natural assumption is that, once it is controlled for contemporaneous inputs and the unobserved effect, explanatory variables in other years do not affect the dependent variable during the current year. In every year, the explanatory variables generally depend on the unobserved effect. So, it is likely that some partial correlation between the dependent variable in year $t$ and explanatory variables in other years will exist if it is not controlled for the unobserved effect (Wooldridge (2010), pp. 252–254).

Following the approach by Grieser and Hadlock (2015), I conduct strict exogeneity tests that follow the procedure outlined by Wooldridge (2010). The strict exogeneity tests add one-period-ahead future values of the independent variable to the regression model and test whether the associated coefficient is zero, as should be the case if strict exogeneity holds. (In my case, I use a CapEx lag (dependent variable) and cash flow lag (independent variable), and augment the equation with contemporaneous cash flow.) Hence, evidence of a non-zero one-period-ahead coefficient is taken as evidence against the strict exogeneity assumption. Giving theoretical ground for the violation of the assumption in this equation, economic intuition suggests that (physical) investment today translates into a future cash inflow stream – this is the nature of decision making based on a project's expected net present value. The FE investment equation that is subject to the Wooldridge (2010) test takes the following form:

$$\begin{aligned} I_{it-1}/K_{it-2} = \beta_o + \beta_1(CF_{it}/K_{it-1}) + \beta_2(CF_{it-1}/K_{it-2}) + \\ \alpha_i + \lambda_t + u_{it}. \end{aligned} \tag{12}$$

The test is for a model in which the dependent variable is a linear function of the firm fixed effect, year dummies, and the selected explanatory variable. Apart from the test on equation 12, a test for a model in which all of the independent variables are included (that is, cash flow, as well as *M/B*) is conducted as well. Both tests are for the total sample period. Given the lack of general significance of the traditional investment-cash flow sensitivity (see results section), the strict exogeneity test is conducted again with a regression of total investment on cash flow, as results for this equation are relatively significant (see robustness section). This is meaningful, because a lack of contemporaneous correlation of cash flow and CapEx (i.e., no violation of simple exogeneity) makes the finding of a violation of the strict exogeneity assumption unlikely, undermining the relevance of strict exogeneity tests in the setting of this thesis. For this equation, I

**Table 2:** Fixed Effects Regression Assumptions

Source: Wooldridge (2013), pp. 509–510.

| | |
|---|---|
| Assumption FE.1 | For each $i$, the model is $y_{it} = \beta_1 x_{it1} + \ldots + \beta_k x_{itk} + \alpha_i + u_{it}, t = 1, T$, where the $\beta_j$ are the parameters to estimate and $\alpha_i$ is the unobserved effect. |
| Assumption FE.2 | The sample is randomly chosen from the cross section. |
| Assumption FE.3 | No perfect multicollinearity is present. In addition to that, at least for some entities, each explanatory variable changes over time. |
| Assumption FE.4 | Given all $t$ values of $xi$ and the unobserved effect, the error term has conditional mean zero. This implies that there is no omitted variable bias. This assumption is violated if current $u_i$ is correlated with past, present, or future values of $x_i$. |
| Assumption FE.5 | The errors are homoskedastic. |
| Assumption FE.6 | Conditional on all explanatory variables and $\alpha_i$, the idiosyncratic errors are uncorrelated. |
| Assumption FE.7 | The variables are i.i.d. across entities for $i = 1, \ldots, n$. This assumption holds if entities are selected by random sampling from the population. |

conduct tests on periods 1 and 2 as well. I follow the procedure outlined in this paragraph for tests on the FD transformation. The first-differenced equation is augmented by the standard variable of contemporaneous cash flow, scaled by the capital stock.

### 5.2.2. Magnitudes of Errors if no Strict Exogeneity

Economic theory also suggests that strict exogeneity should be violated in many other standard panel finance settings. Hence, general inconsistency of the FE and FD estimates in these settings is a concern. However, if the inconsistency is small – as many researchers assumed prior to Grieser and Hadlock (2015) –, inferences regarding the magnitude of a coefficient or whether it is significantly different from zero may be at least approximately valid. As Wooldridge (2010) notes, substantial differences between FE and FD estimators are often an indication of a violation of strict exogeneity – and suggest that the problem caused by inconsistency is one of a large magnitude. To gauge the possible magnitude of inference errors, I analyze the relative variation in the FE and FD estimates based on an unbalanced, as well as on a balanced panel. Under strict exogeneity, these two estimates asymptotically converge to the same true underlying parameter value. If the assumption is violated, both estimators have different probability limits, neither of which is the true parameter value of interest (Grieser and Hadlock (2015), p. 3).

As a researcher, a reason for working with FE, though knowing that its estimator might be inconsistent, is that he could hope for a *1/T* save. The FE and FD estimators are both inconsistent when strict exogeneity fails. However, the degree of inconsistency of the FE estimator may be smaller in a long panel than the inconsistency in the FD estimator. This is because the FE estimator effectively differences variables from their means while the FD estimator takes differences from adjacent periods. Feedback effects are intuitively more influential when directly comparing adjacent periods. In fact, this notion is formally captured in that the inconsistency of the FE estimator is in the order of *1/T*, while the inconsistency of the FD estimator is independent of *T*. This is why researchers, when a long panel is available, hope that the FE coefficients are relatively informative. For this to be justified, a firm's fixed effect for the dependent variable of interest needs to be stable over the entire sample period. But, for example, due to occasional changes to an average firm's management, shareholder base, and capital, economic intuition makes obvious that the assumption of a stable unit-level fixed effect over a long sample period may not hold. In fact, Grieser and Hadlock (2015) do not find support for the stability of the underlying unit-level fixed effects coefficients.

### 5.3. First Differences

A third main estimation method employed is first differencing. After first differencing the data, a full equation looks like

$$\Delta y_{it} = \alpha_o + \alpha_3 d3_t + \alpha_4 d4_t + \ldots + \alpha_T d3_T + \beta_1 \Delta x_{it1} + \ldots + \beta_k \Delta x_{itk} + \Delta u_{it}, \tag{13}$$

where there are *T-1* time periods on each unit $i.d3_t, d4_t$, etc. denote year dummies. $\Delta y_{it}$ is equal to $y_{it}$ minus $y_{it-1}$. The total number of observations is *N(T-1)*. As there is nothing to subtract from *t=1*, there is no differenced equation for *t=1*. By allowing an intercept to be included, a differenced equation for *t=2* must be excluded. The reason I include a dummy variable for each time period is to account for the secular changes that are not being modeled. This is meaningful when *T* is small relative to N. Provided that the observations have been properly organized and the differencing has been carefully done, equation 13 is simple to estimate by pooled OLS. Arranged chronologically, the first *T* records are for the first cross-sectional observations, the second *T* records are for the second cross-sectional observations, arranged chronologically, and so forth. Just like with the FE method, there are potential problems with first differencing when the key explanatory variables do not vary much over time. The strict exogeneity assumption is even more critical for the FD method. If this assumption is violated, the bias in the FD estimator does not depend on T, that is, having more time periods does not reduce the inconsistency in the estimator. Also, employing the FD estimator can be worse than pooled OLS if at least one of the explanatory variables is subject to measurement error (Wooldridge (2013), pp. 473–491). When the disturbances follow a random walk, the FD estimator is more efficient than the FE estimator. The FE estimator, however, is more efficient when the errors are serially uncorrelated (Wooldridge (2010), p. 284). In many applications, the unobserved factors that change over time are serially correlated. Anyway, in practice, the FE estimator is used more frequently than the FD estimator. This is because the unobserved effects model is typically stated with serially uncorrelated idiosyncratic errors (Wooldridge (2013), p. 490). Under assumptions FD.1–FD.6, which are quite similar to the FE assumptions, the FD estimator of the $\beta_j$ is the BLUE, conditional on the explanatory variables (table 3). FD.7 is needed in order to have a normal distribution of the FD estimators. Without this assumption, it can be relied on the asymptotic approximations.

### 5.4. Random Effects

In using FE or FD, the goal is to eliminate $\alpha_i$, because the effect presumably is correlated with one or more of the $x_{itj}$. A transformation to eliminate $\alpha_i$ is redundant (and inefficient) when one thinks that $\alpha_i$ is uncorrelated with each explanatory variable in all time periods. So, when $Cov(x_{itj}, \alpha_i) = 0$, the standard unobserved effects model of equation 7 becomes a random effects (RE) model; if correlation is assumed, FE or FD should be used. (The RE estimation method is the fourth and last method used/analyzed in this paper.) In cases in which the unobserved effect is thought to be uncorrelated with the explanatory variables, the $\beta_j$ can be consistently estimated using a single cross section, as there is no need to control for the unobserved effect in order to avoid omitted variable bias. Though there is no need for panel data, a single cross section disregards much useful information in the other time periods. Under the RE assumptions, employing a pooled OLS procedure instead of RE produces consistent estimators of the $\beta_j$ as well; but it allows for positive serial correlation in the composite error term. The composite error term is defined as $v_{it} = a_i + u_{it}$. So, the pooled OLS procedure ignores a key feature of the model: because $a_i$ is in the error term in each time period, the $v_{it}$ are autocorrelated. Generalized least squares (GLS) can be used to solve the serial correlation in the errors. The transformed GLS equation turns out to be

$$\begin{aligned} y_{it} - \theta \bar{y}_i = \beta_o(1-\theta) + \beta_1(x_{it1} - \theta \bar{x}_{i1}) \quad + \ldots + \\ \beta_k(x_{itk} - \theta \bar{x}_{ik}) + (v_{it} - \theta \bar{v}_i), \end{aligned} \tag{14}$$

where, again, the overbar denotes time averages. The transformed GLS equation involves quasi-demeaned data. The difference to time-demeaned data is that the RE transformation subtracts a fraction, $\theta$, of the time averages. This fraction depends on $\sigma_u^2, \sigma_a^2$, and the number of time periods, $T$. The FE estimator, as usual, subtracts the time averages from the corresponding variable. The fraction, $\theta$, equals $1 - Corr(v_{it}, V_{is})$. Pooled OLS is obtained when $\theta = 0$. FE is obtained when $\theta = 1$. Whereas, in practice, the estimator is never zero or one, the RE estimates will be close to the pooled OLS estimates when $\sigma_a^2$ is small relative to the error variance, which implies that the unobserved effect is relatively unimportant. It is more common for the unobserved effect's variance to be large relative to $\sigma_u^2$ in which case $\hat{\theta}$ will be closer to unity. The GLS estimator, which uses $\theta$, is called RE estimator and is simply the pooled OLS estimator of equation 14, in which the errors are no longer autocorrelated. RE allows for explanatory variables that are constant over time. This clearly is an advantage over FE and FD. These time-constant right-hand-side variables can be included in the RE method because only a fraction of the time averages is subtracted, given that RE assumes that the unobserved effect is uncorrelated with these variables. However, in many applications, the whole reason for using panel data is to allow the unobserved effect to be correlated with the explanatory variables. Therefore, situations when RE is preferred over FE/FD are scarce. This is though FE and FD are not necessarily more suitable than RE when there is a time-varying key policy variable. It is just that it takes, for example, a natural experiment setting to achieve $Cov(x_{itj}, \alpha_i) = 0$. It is much more likely that the regressors themselves are outcomes of choice processes. Compared to pooled OLS, RE is preferred, as it is generally more efficient. Comparing efficiency of RE to FE, the RE estimator is more efficient for coefficients on time varying-explanatory variables. But FE is not meant to be efficient under the RE assumptions, which can be seen in table 4. There is a tradeoff between robustness and efficiency, as FE is just intended to be robust to correlation between $\alpha_i$ and the $x_{itj}$. Naturally, the RE assumptions include the requirement that $\alpha_i$ is independent of all explanatory variables in all time periods. On top of that, the ideal RE assumptions include most of the FE assumptions. Under these, the estimator is consistent (not unbiased) and asymptotically normally distributed (Wooldridge (2013), pp. 490–496). Assumption FE.3 is omitted, because time-constant explanatory variables are allowed (Wooldridge (2013), p. 510).

## 6. Standard Errors

The asymptotic 95 percent confidence interval is $\hat{\beta} \pm 1.96 \times se$, where $se$ stands for standard error. Hypothesis testing is typically based on the Wald t-statistic, defined as $w = (\hat{\beta} - \beta_o)/se$. It is clear that both $\hat{\beta}$ and $se$ are critical ingredients for statistical inference, so that obtaining accurate standard errors is of fundamental importance (Cameron and Miller (2015), pp. 4–5). In panel data, the regression error can be correlated over time within an entity. This correlation does not introduce bias into the fixed effects

**Table 3:** First Differences Regression Assumptions

Source: Wooldridge (2013), pp. 482–483.

| | |
|---|---|
| Assumption FD.1 | For each $i$, the model is $y_{it} = \beta_1 x_{it1} + \ldots + \beta_k x_{itk} + \alpha_i + u_{it}, t = 1, T$, where the $\beta_j$ are the parameters to estimate and $\alpha_i$ is the unobserved effect. |
| Assumption FD.2 | The sample that is being worked with is randomly chosen from the cross section. |
| Assumption FD.3 | No perfect multicollinearity is present. In addition to that, at least for some entities, each explanatory variable changes over time. |
| Assumption FD.4 | Given all $t$ values of $x_i$ and the unobserved effect, the error term has conditional mean zero. This implies that there is no omitted variable bias. This assumption is violated if current $u_i$ is correlated with past, present, or future values of $x_i$. |
| Assumption FD.5 | The differenced errors are homoskedastic. |
| Assumption FD.6 | The differenced errors follow a random walk; the $u_{it}$ are serially uncorrelated. |
| Assumption FD.7 | Conditional on the explanatory variables, the $\Delta u_{it}$ are i.i.d. normal variables. |

**Table 4:** Random Effects Regression Assumptions

Source: Wooldridge (2013), pp. 510–511.

| | |
|---|---|
| Assumption RE.1 | There is no perfect multicollinearity. |
| Assumption RE.2 | Given all explanatory variables, the expected value of $\alpha_i$ is constant. This is the key distinction between RE and FE, as it rules out $Corr(x_{itj}, a_i) \neq 0$. |
| Assumption RE.3 | $a_i$ is homoskedastic. |

estimator, but it affects the variance of the fixed effects estimator and, thus, it affects how one computes standard errors, just like with heteroskedasticity (Stock and Watson (2015), pp. 411–413). It is not unusual to have applications where standard errors that control for within-cluster correlation are several times larger than default standard errors that ignore such correlation (see robustness section below for an example). A failure to control for within-cluster correlation can lead to these misleadingly small standard errors, and, consequently, misleadingly narrow confidence intervals, large t-statistics, and low p-values. The need to control for within-cluster correlation increases not only with an increase in the size of within-cluster error correlation, but also with the size of within-cluster correlation of regressors and with the number of observations within a cluster. The above means that, in OLS estimation, there can be a great loss of efficiency if errors are correlated within cluster rather than completely uncorrelated. If the model systematically overpredicts (or underpredicts) the regressors in a given firm (or industry, depending on the cluster), the error term is positively correlated with the firm/industry. In this case, default OLS standard errors will be biased downwards and, thus, greatly overstate estimator precision (Cameron and Miller (2015), pp. 6–7). Standard errors for FE regressions are cluster-robust standard errors, which are robust not only to the described autocorrelation within an entity, but also to heteroskedasticity (Stock and Watson (2015), p. 411). For first differences, it must be assumed that $\Delta u_{it}$ is serially uncorrelated for the usual standard errors and test statistics to be valid. Only when $u_{it}$ follows a random walk will $\Delta u_{it}$ be serially uncorrelated (Wooldridge (2013), pp. 469–470). A regression model with limited or no control for within-cluster error correlation is estimated to control for clustered errors, as the cluster-robust standard errors are obtained post-estimation. Rogers (1993) incorporated this method in Stata. Instead of requiring specification of a model for within-cluster error correlation, these cluster-robust standard errors require the additional assumption that the number of clusters, rather than just the number of observations, goes to infinity (Cameron and Miller (2015), p. 5).

There are two principles that give guidance to determine what to cluster over: first, whenever there is reason to believe that both the regressors and the errors might be correlated within cluster, one should think about clustering defined in a broad enough way to account for that clustering. Second, $\hat{V}_{cluster}[\hat{\beta}]$ is the group average, a term that gets closer to $V[\hat{\beta}]$ only as the number of groups becomes large. ($\hat{V}_{cluster}$ is the standard estimator for the variance of the cluster.) This means that, if one defines very large clusters, so that there are very few clusters to average over, the resulting $\hat{V}_{cluster}[\hat{\beta}]$ can be a very poor estimate of $V[\hat{\beta}]$. Just like when comparing the estimation methods FE and RE in chapter 5.4, these two principles illustrate the bias-variance tradeoff that is common in many estimation problems: larger and fewer clusters have less bias but more variability. There is neither a general solution to this tradeoff, nor a formal test of the level at which to cluster. The consensus is to be conservative and avoid bias, and use bigger and more aggregate clusters when possible. In practice, one should start clustering at a narrow level, which is then being broadened until there is relatively little change in the standard errors (Cameron and Miller (2015), p. 21). This procedure is being followed in section 7.3, and serves as a basis for the decision to cluster over firms in this paper. There are several settings where one may not need to use cluster-robust standard errors at all. In any of these cases, it is always possible to still obtain these errors and compare them with the default standard errors. The appreciable differences revealed in the robustness section serve as a general reason for choosing cluster-robust standard errors. The above-mentioned cluster-robust standard error assumption, which implies that the number of clusters should go to infinity, is another argument in favor of clustering at firm level. As outlined in chapter 5, Wooldridge (Wooldridge (2010), p. 256) writes that for pooled OLS, the composite errors will be autocorrelated due to the presence of $a_i$ in each time period. Hence, inference using pooled OLS requires robust test statistics.

## 7. Results

Table 5 presents estimates of the $M/B$ investment model, including cash flow, for each of the three retention-classes. The equations were estimated with fixed firm and year effects. Results are presented for three time periods, 1990–2002 (period 1), 2003–2015 (period 2), and 1990–2015 (total sample period). The striking result is that it is only the coefficient on cash flow for group 2 that is strongly statistically and slightly economically significant; I would rather expect significance for class-1 firms. The coefficient amounts to 0.088 for period 1, 0.033 for period 2, and 0.04 for the total sample period, and is always significant at least at the five-percent level. Economically, this means that an increase in the cash flow-capital ratio by one increases the investment-capital ratio by 0.088 (period 1). An increase by one is equivalent to increasing the cash flow, scaled by capital, of the median class-2 firm by just over 200 percent. An effect on investment-capital of 0.088 is equivalent to 37 percent of the median class-2 firm's investment-capital ratio (see table 1). Hence, for the median class-2 firm over 1990–2002, increasing the cash flow-capital ratio by 200 percent translates into an increase in the investment-capital ratio of 37 percent.

Sticky dividends, that is, a reluctance to cut dividends, cannot be the driver of this finding, because the signaling effect is incorporated in the definition of cash flow, in which dividends are already subtracted. Also, the signaling effect should then lead to cash flow significance for the third-class firms as well. The underlying rationale for a potential impact is that, in the case of small cost differentials between internal and external finance, mature firms with substantial payouts might reduce investment instead of cutting dividends when cash flow falls (Fazzari et al. (1988), p. 183).

Agency costs of internal finance, that is, potential "managerial waste" on less productive investments, could be a driver of the effect – just like irrational or overly risk-averse managers, who choose to rely primarily on operational cash flow to invest despite the possibility to get low-cost external funds (Kaplan and Zingales (1997), p. 173; Andrén and Jankensgård (2015)). But again, this point should then hold for class-3 firms as well – but for these firms, the coefficient on cash flow is statistically and economically insignificant in any sample period examined. As outlined in Cleary et al. (2007), two otherwise identical firms may face severe differences in asymmetric information problems. The authors conclude that a model that captures information asymmetries (bid-ask spreads) is more adequate to proxy for financing constraints than a broad proxy, such as dividend payouts. Though this potentially explains the findings presented here, it stands in contrast to the vast literature using these broad proxies to classify firms according to their degree of financing constraints. Still, dividend payouts specifically might no longer serve as an adequate proxy for financing constraints. However, the class-2 coefficient on cash flow is insignificant when sales variables are included in the model (see table 8). This suggests that the apparent correlation between cash flow and investment in these (more mature) firms may be due to the omission of output terms important in reconciling the difference between marginal and average $M/B$ . Still, this should then hold for the third class as well.

The general interpretation of a lack of significance of cash flow in explaining investment across the three payout-classes is that there is only a small (if at all) cost disadvantage of external finance, so that payout ratios reveal nothing about investment: firms will use external funds to smooth investment when internal finance fluctuates. This finding is in line with the growing literature showing a decrease (often disappearance) of the investment-cash flow sensitivity (inter alia, Chen and Chen (2012)). No present financing constraints – measured by the investment-cash flow sensitivity – across classes could be, as described in chapter 1, driven by a) the downward omitted variable bias that might occur due to a failure to account for external finance in the regression, b) a deterioration in the relative importance of tangible investment, and/or c) an increased and cheaper usage of public equity markets by young firms (Brown and Petersen (2009)).

A different aspect is described in Kaplan and Zingales (Kaplan and Zingales (1997), p. 205). They outline that when deflating variables by net property, plant, and equipment, estimates are only consistent when there is no growth, among other things. So, if investment and cash flow grow at a rate similar to the rate of sales growth, part of the two variables' co-movement may be due to a scale factor. As this effect biases the investment-cash flow sensitivity upwards, firms with higher annual growth rates may drive the results. Neither the median sales growth of class-1 firms (8.4 percent), nor that of class-2 firms (8.2 percent) is abnormally high, such as the 18 percent growth rate in the sample of Kaplan and Zingales (1997). Unlike in Fazzari et al. (1988), outliers (see the high standard deviation of sales growth for classes

**Table 5:** Effects of Cash Flow and M/B on Investment, Various Periods, 1990-2015[a]

Source: author's estimates of equation 2 based on a sample of firm data from Compustat database. See text.
a. Property, plant and equipment and K is beginning-of-period capital stock. The equations were estimated using fixed firm and year effects (not reported). The constant is not reported. Cluster-robust standard errors appear in parentheses. *** p<0.01, ** p<0.05, * p<0.l.
b. Independent variables are defined as follows: $(CF/K)_{it}$ is the cash flow-capital ratio, defined as the sum of net income and depreciation, les s dividend payments, deflated by beginning-of-period capital. $(M/B_{it}$ is the market-to-book ratio. It is calculated as the common stock, multiplied with the annual closing stock price, plus current and long term liabilities; the sum is divided by total assets.

| Independent variable and summary statistic[b] | Class 1 | Class 2 | Class 3 |
|---|---|---|---|
| | | 1990-2002 | |
| $(CF/K)_{it}$ | -0.011* | 0.088*** | 0.036 |
| | (0.007) | (0.016) | (0.044) |
| $(M/B)_{it}$ | 0.009 | 0.016*** | -0.000 |
| | (0.011) | (0.003) | (0.003) |
| Within $R^2$ | 0.0124 | 0.425 | 0.0790 |
| | | 2003-2015 | |
| $(CF/K)_{it}$ | 0.022 | 0.033** | 0.000 |
| | (0.020) | (0.014) | (0.000) |
| $(M/B)_{it}$ | 0.051 | 0.002** | 0.009*** |
| | (0.041) | (0.001) | (0.003) |
| Within $R^2$ | 0.0140 | 0.0852 | 0.008 |
| | | 1990-2015 | |
| $(CF/K)_{it}$ | -0.007 | 0.040*** | -0.003 |
| | (0.008) | (0.015) | (0.003) |
| $(M/B)_{it}$ | -0.003 | 0.007* | 0.005*** |
| | (0.009) | (0.004) | (0.002) |
| Within $R^2$ | 0.004 | 0.104 | 0.016 |

1 and 3) do not seem to bias the sensitivity upwards, given the lack of significance in the cash flow coefficients. Apart from that, a lack of significance of class 1 firms' coefficient on cash flow might be because class-1 firms include companies that are in financial distress. For these, it is plausible that there are low investment-cash flow sensitivities. For example, creditors might force an insolvent firm to use additional cash flow to repay debt rather than for CapEx. Necessarily, this reduces the investment-cash flow sensitivity (Kaplan and Zingales (1997), p. 208).

It is unexpected that there is no (general small) sensitivity of investment to cash flow for the vast majority of firms, as this sensitivity would be easy to justify. There is a wedge between internal and external costs of finance for all firms as long as some transaction costs are involved (Kaplan and Zingales (1997), p. 173). Findings might be interpreted in that transaction costs might simply be too low by now to establish a sensitivity of investment to cash flow. Finally, it is intuitive that the cash flow effect on investment of class-2 firms is stronger in period 1 (0.088) than in period 2 (0.033) and stronger than over the total sample period (0.04). Asymmetric information should reduce with the degree to which the market understands the companies and its projects, that is, with the time the companies are in existence.

There is no clear-cut pattern in the coefficients on $M/B$ . The coefficient on $M/B$ is statistically significant for class-2 across all periods. For class 1, the coefficient is economically significant (0.051) in the period 2003–2015. For class 3, it is statistically significant in the second period, as well as for the entire sample period. These findings are not surprising, as in most empirical studies, the size of the coefficient on $M/B$ is small. Also, in cases, in which there is a premium for issuing new shares, there is a possibility that the firm finds itself at a point of discontinuity where all profits are retained, no dividends are paid, and the firm's future prospects are not good enough to induce it to issue new shares. In that case, $M/B$ does not matter, while cash flow should matter (Devereux and Schiantarelli (1990), p. 293). This argument is based on the adverse selection argument in Myers and Majluf (1984).

In addition to that, as addressed in section 4, the market-to-book ratio may not reflect market fundamentals during times when the stock market is excessively volatile. In this case, one would expect that during times of potential speculative bubbles or fads in the stock market over the sample period (dot-com bubble, financial crisis), the coefficients on $M/B$ and cash flow should be different, compared with more stable periods. In particular, one may expect that $M/B$ matters less relative to cash flow during these extreme times (Blanchard et al. (1993)). Obviously, even ex-post, it is difficult to identify unambiguously when bubbles/fads caused stock prices to be a poor reflection of fundamentals. A glance at the within R-squared numbers underlines that the regressions are driven by the cash flow coefficients. Within R-squared is, with 0.425, high for class 2 in the first period (in which the cash flow coefficient is the highest as well). For period 2 and the total sample period, within R-squared figures for class 2 are noteworthy as well. For classes 1 and 3 – with the small exception of class 3, first period, due to a cash flow coefficient of 0.036 – within R-squared is not substantially different from zero.

Table 6 reports estimates of alternative specifications in order to test the previous result's robustness and to check whether cash flow lags have a stronger effect on investment, for example, in a time-to-build context. Results are reported for both periods, 1990–2002 and 2003–2015, for each of the three payout-classes. The first model in table 6 includes two cash flow lags. For classes 1 and 3 over both time periods, coefficients on these lags are statistically and economically insignificant. The coefficient on contemporaneous cash flow of class 2 is robust to the incorporation of the lags. This implies that collinearity among cash flow variables does not impact the coefficient on $CF_{it}/K_{it-1}$. Lagged cash flow does neither have an economically significant impact on investment for class-2 firms. Especially since the data used are annual, to the extent that the differences in the cash flow effects across classes reflect the impact of financial constraints on investment, one would expect that these differences are most evident in the coefficient on current cash flow. This is based on Andrew and Blanchard (1986), who compute a present value series of marginal profits in a $M/B$ model of investment under various assumptions and find that it is only the first of three lags of quarterly profits that is statistically different from zero. This time period falls within the contemporaneous annual observation, that is, $CF_{it}/K_{it-1}$. Thus, an effect of coefficients on lagged cash flow might well reflect shortcomings in $M/B$ 's empirical performance. But coefficients on $M/B$ do not change with the incorporation of cash flow lags, so that lagged cash flow does not seem to capture news about investment opportunities. The second model includes a lagged market-to-book ratio variable. Some rejections of versions of the$q$ theory result from a significant lagged $M/B$ coefficient explaining investment. Adding lagged $M/B$ does not change the estimation results of $M/B$ for class 1 with regards to statistical significance. In the second class, the coefficient on $M/B$ is no longer significant, whereas lagged $M/B$ is significant at the ten percent level in the second period. In the third class, both coefficients on $M/B$ are now significant; the period-2 coefficient on lagged $M/B$ is significant as well. The pattern of the cash flow coefficients is virtually identical.

The first model in table 7 presents the estimation results of equation 3. It is an augmented $M/B$ equation similar to equation 2, but includes the cash stock variable. Results are reported for 1990–2015. Clearly, the coefficients on the cash stock are statistically and economically insignificant across the three classes. Thus, the coefficients on the other variables – as well as within R-squared – are indistinguishable from the estimation results in table 5. This implies the following: a) the stock of liquidity does not function as a financial cushion to smooth investment and to reduce the sensitivity of investment to cash flow; b) a cushion does not seem to provide collateral for new debt, which could be used for CapEx; c) cash stock does not proxy for longer lags of cash flow – but lags of cash flow were insignificant already; d) as expected, cash stock does not reveal any news about the profitability of investments. Results might also be impacted by an unclear relation of cash holdings to financing constraints, as outlined in Hadlock and Pierce (2010). To the extent that the significant coefficient on cash flow for the second class is driven by data issues or omitted accelerator effects of sales, clear-cut insignificance of cash stock is a windfall for the hypothesis that there is really no significant cost disadvantage for external finance for any of the three payout-classes. The second model in table 7 includes the sum of the flow of cash and the stock of cash instead of two separate variables. As cash stock does not capture any information relevant for explaining the variation in CapEx, the sum of cash flow and cash stock is less helpful in explaining investment than cash flow itself. This leads to a lower statistical and economic significance of the coefficient on the sum relative to the coefficient on cash flow and, hence, a lower R-squared.

Table 8 presents estimated equations for the three payout-classes that include cash flow and current and lagged values of sales. Two equations are reported: one that includes only cash flow augmented by sales variables (see equation 4 above), and a second one that adds $M/B$ to that equation 4. In both estimations, it is striking that none of the coefficients on cash flow is significant, while the contemporaneous effect of sales is approximately equally significant in economic terms across the three classes and statistically significant in the second class. Therefore, the effects of cash flow in the $M/B$ model can indeed be explained by the correlation of cash flow and sales – and, hence, by an omitted variable bias. Fluctuations in sales seem to motivate changes in capital spending; cash flow does not seem to represent an additional supply of low-cost investment finance for firms that have to pay a premium for external funds. Findings are not impacted when the equation is augmented by $M/B$ .

### 7.1. Comparison to Fazzari et al. (1988)

Beginning with Fazzari et al. (1988), a comprehensive empirical literature has documented that a positive and significant relationship between cash flow and investment exists, holding investment opportunities constant (Andrén and Jankensgård (2015), p. 204). In addition to that, almost all of the earlier studies support the Fazzari et al. (1988) findings that financial constraints positively affect the sensitivity of investment to cash flow (Gatchev et al. (2010), p. 729). Though this financing constraints view has been challenged and the interpretation of the investment-cash flow sensitivity

**Table 6:** Effects of Cash Flow and M/B on Investment: Alternative Specifications, Various Periods, 1990-2015[a]

Source: same like table 5.
a. The dependent variable is the investment-capital ratio $(I/K)_{it}$. All independent variables are as defined in table 5, note b. Equations are estimated with fixed firm and year effects (not reported). The constant is not reported. Cluster-robust standard errors appear in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

| Independent | Class 1 | | Class 2 | | Class 3 | |
|---|---|---|---|---|---|---|
| and summary statistic | 1990-2002 | 2003-2015 | 1990-2002 | 2003-2015 | 1990-2002 | 2003-2015 |
| | | | Model with cash flow lags | | | |
| $(CF/K)_{it}$ | -0.014 | 0.028 | 0.089*** | 0.027*** | -0.001** | -0.000 |
| | (0.009) | (0.027) | (0.017) | (0.010) | (0.001) | (0.000) |
| $(CF/K)_{it-1}$ | -0.004 | -0.005 | -0.010 | 0.030*** | -0.005 | 0.001 |
| | (0.005) | (0.004) | (0.011) | (0.009) | (0.006) | (0.001) |
| $(CF/K)_{it-2}$ | -0.000 | -0.001 | 0.029*** | 0.003 | 0.016 | -0.000 |
| | (0.001) | (0.002) | (0.007) | (0.006) | (0.014) | (0.000) |
| $(M/B)_{it}$ | 0.000 | 0.058 | 0.015*** | 0.002* | 0.004** | 0.006 |
| | (0.006) | (0.044) | (0.002) | (0.001) | (0.002) | (0.004) |
| Within $R^2$ | 0.031 | 0.021 | 0.472 | 0.120 | 0.007 | 0.009 |
| | | | Model including lagged market-to-book ratio | | | |
| $(CF/K)_{it}$ | -0.011 | 0.029 | 0.088*** | 0.032** | -0.000 | 0.000 |
| | (0.007) | (0.021) | (0.017) | (0.014) | (0.001) | (0.000) |
| $(M/B)_{it}$ | 0.017 | 0.052 | 0.017*** | 0.001 | 0.005** | 0.002** |
| | (0.016) | (0.041) | (0.004) | (0.001) | (0.002) | (0.001) |
| $(M/B)_{it-1}$ | -0.014 | 0.025 | 0.002 | 0.003* | -0.000 | 0.008*** |
| | (0.013) | (0.047) | (0.004) | (0.002) | (0.001) | (0.003) |
| Within $R^2$ | 0.013 | 0.024 | 0.444 | 0.087 | 0.003 | 0.010 |

**Table 7:** Effects of Cash Flow, Cash Stock, and M/B on Investment, 1990 - 2015[a]

Source: first model is based on the author's estimates of equation 3. Second model is based on a variant of that model. Both models are based on a sample of firm data from Cornpustat database. See text.
a. The dependent variable is the investment-capital ratio $(I/K)_{it}$ where I is investment in property, plant and equipment and K is beginning-of-period capital stock. Equations are estimated with fixed firm, and year effects (not reported). The constant is not reported. Cluster-robust standard errors appear in parentheses. *** p<0.01, ** p<0.05, * p<0. l.
b. Cash is defined as beginning-of-period cash and short-term securities. All other independent variables are as defined in table 5, note b.

| Independent variable and summary statistic[b] | Class 1 | Class 2 | Class 3 |
|---|---|---|---|
| | Model including cash and short-term securities variable | | |
| $(CF/K)_{it}$ | -0.006 | 0.039*** | -0.003 |
| | (0.009) | (0.015) | (0.003) |
| $(Cash/K)_{it-1}$ | 0.004 | 0.001 | 0.001 |
| | (0.004) | (0.001) | (0.001) |
| $(M/B)_{it}$ | -0.001 | 0.007* | 0.004** |
| | (0.010) | (0.004) | (0.002) |
| Within $R^2$ | 0.006 | 0.105 | 0.019 |
| | Model including sum of cash flow and stock variable | | |
| $(CF_{it} + Cash_{it-1})/K_{it}$ | 0.002 | 0.003** | -0.000 |
| | (0.004) | (0.001) | (0.001) |
| $(M/B)_{it}$ | 0.005 | 0.008** | 0.004*** |
| | (0.008) | (0.004) | (0.002) |
| Within $R^2$ | 0.002 | 0.077 | 0.005 |

**Table 8:** Effects of Cash Flow, Safes, and M/B on Investment, 1990- 2015[a]

Source: first model is based on the author's estimates of equation 4. Second model is based on equation 4 and includes $(M/B)_{it}$ Both models are based on a sample of firm data from Compustat database. See text.
a. The dependent variable is the investment-capital ratio $(I/K)_{it}$ where I is investment in property, plant and equipment and K is beginning-of-period capital stock. Equations are estimated with fixed firm, and year effects (not reported). The constant is not reported. Cluster-robust standard errors appear in parentheses. *** p<0.01, ** p<0.05, * p<0.l.
b. $(S/K)_{it}$ is the ratio of sales, S, to the beginning-of-period capital stock. All other independent variables are as defined in table 5, note b.

| Independent variable and summary statistic[b] | Class 1 | Class 2 | Class 3 |
|---|---|---|---|
| | Model with sales-capital ratio | | |
| $(CF/K)_{it}$ | -0.017 | -0.016 | 0.001 |
| | (0.017) | (0.015) | (0.001) |
| $(S/K)_{it}$ | 0.014 | 0.012** | 0.004 |
| | (0.008) | (0.006) | (0.005) |
| $(S/K)_{it-1}$ | -0.002 | 0.004 | -0.001 |
| | (0.002) | (0.002) | (0.003) |
| $(S/K)_{it-2}$ | -0.001 | -0.001 | 0.000 |
| | (0.001) | (0.002) | (0.001) |
| $(S/K)_{it-3}$ | -0.001 | -0.003 | 0.000 |
| | (0.001) | (0.004) | (0.000) |
| Within $R^2$ | 0.020 | 0.164 | 0.028 |
| | Model with sales-capital ratio and M/B | | |
| (CF/K);, | -0.017 | -0.016 | 0.001 |
| | (0.017) | (0.015) | (0.002) |
| $(S/K)_{it}$ | 0.013 | 0.012** | 0.011 |
| | (0.008) | (0.006) | (0.007) |
| $(S/K)_{it-1}$ | -0.001 | 0.004 | -0.007 |
| | (0.002) | (0.002) | (0.007) |
| $(S/K)_{it-2}$ | -0.001 | -0.001 | -0.011** |
| | (0.001) | (0.002) | (0.005) |
| $(S/K)_{it-3}$ | -0.001 | -0.003 | 0.031** |
| | (0.001) | (0.004) | (0.014) |
| $(M/B)_{it}$ | -0.006 | 0.009** | 0.004** |
| | (0.016) | (0.004) | (0.002) |
| Within $R^2$ | 0.019 | 0.167 | 0.127 |

continues to generate controversy, it is sensible to compare the findings of this paper with the ones by the parent of all papers in this literature (Fazzari et al. (1988), p. 695).

While my approach is relatively close to that in Fazzari et al. (1988), there could still be significant differences in estimation results, inter alia, due to the following: first, Fazzari et al. (1988) use Value Line data, while I work with data from Compustat. Second, many variables are defined differently. For example, they use three $Q$ measures (Tobin's q, tax-adjusted $Q$ with no dividends paid, tax-adjusted $Q$ with dividends paid), while I work with the simple $M/B$ measure. Also, FHP (Fazzari et al. (1988), pp. 193–194) employ a replacement value of the capital stock, estimated from book values using a method similar to that of Salinger and Summers (1983), in contrast to the book value of net property, plant, and equipment in this paper. The robustness section (7.3) includes estimations based on cash flow defined without subtracting dividends, like in Fazzari et al. (1988). Third, as outlined above, it is plausible that financially distressed firms exhibit low investment-cash flow sensitivities. In fact, FHP (Fazzari et al. (1988), p. 158) intended to eliminate distressed firms because they explicitly excluded firms with overall negative real sales growth from their sample. Though their results do not change substantially by including firms with negative sales growth in the sample, the section on robustness includes model estimations that only incorporate firm-year observations with positive sales growth. Fourth, data in FHP (Fazzari et al. (1988), p. 191) are uninterrupted from 1970–1984, while data in this paper are unbalanced over 26 years from 1990–2015. Section 7.3 includes estimations using a balanced panel as well. Fifth, estimations in this paper work with cluster-robust standard errors, clustering over firms. Fazzari et al. (1988) could not yet do so.

Substantial differences in sample statistics, among other things because of the differences stated above, could impact the findings. Naturally, differences in descriptive statistics also exist because of mainly comparing mean values (FHP) with median values (this paper). The median is preferred over the mean, given that extreme values substantially impact the summary statistics when these are based on mean values. This paper is much more comprehensive with regards to firm-year observations for each class. Apart from that, many differences relate to the supposedly capital constrained class of firms, that is, class 1. For the vast majority of these firms, in FHP (Fazzari et al. (1988), p. 159), the sales growth is higher (13.7 percent vs. 8.4 percent), investment-capital ratio is lower (0.26 vs. 0.94), and standard deviation of the investment-capital ratio is lower (0.17 vs. 30.3). Regarding the cash flow-capital ratio, averages of the FHP sample are higher for the first class (0.3 vs. 0.2) and lower for the second class (0.26 vs. 0.49) compared to the sample in this paper, while standard deviations of that ratio are lower for all classes in FHP, especially the first class (0.2 vs. 217.5) and third class (0.06 vs. 21).

Four tables that summarize estimation results are compared. The first estimation output is based on standard equation 2, which augments the *M/B* investment model with cash flow (see Fazzari et al. (1988), table 4). FHP find that cash flow is significant in explaining CapEx across the three classes, and its coefficient is significantly higher for the high-retention sample. This paper presents findings (table 5) showing only statistical significance of the coefficient on cash flow for the second class, translating into the single specification in which a substantial portion of the variance in investment is explained (0.425, for the first period). Both papers see the deterioration of the cash flow effect over time, having its root in lower asymmetric information.

The second regression results compared are based on the basic equation 2, including either cash flow lags, or a lagged market-to-book ratio. FHP (1988, table 6) show that collinearity among cash flow variables reduces the impact of the current cash flow variable in all classes when its lags are included; the pattern, however, remains clear. The mostly significant cash flow lags might well reflect shortcomings in the empirical performance of Q, which is lower when lags of cash flow are included. In contrast to that, this paper (table 6) finds that contemporaneous cash flow is robust to augmentation with its lags, while the coefficient on *M/B* does not change in all three classes. Both papers demonstrate that the pattern of the cash flow coefficients across classes for both time periods is virtually identical when the lagged investment demand variable is included.

The third comparison is based on the investment-cash stock model (equation 3), to which the cash flow variable is added. Again, the contrast in results is obvious. FHP (1988, table 10) find that the stock liquidity has a significant effect on investment for the low-payout firms. The strong results of the cash stock variable, which, in contrast to the flow of liquidity, do not indicate much about the profitability of new investment, are interpreted as strong evidence for the imperfect substitutability of internal and external finance at the margin. However, I find (table 7) that cash stock is statistically and economically insignificant across the three classes – a windfall for the hypothesis that there is no cost disadvantage of external finance.

The last comparison uses estimations of equation 4, the sales accelerator equation. FHP (1988, table 7) report that most of the sales terms are statistically significant individually, with the effect of contemporaneous sales clearly being the strongest; cash flow coefficients decline in all three classes when sales variables are added to the equation – anyway, the cash flow pattern remains robust. When adding $Q$ to that, the pattern is still existent. Table 8 in this paper makes clear that no coefficient on cash flow is significant any longer. Instead, it is the correlation between cash flow and sales that leads to significance of cash flow in class 2 in table 5. The effect of contemporaneous sales is approximately equally significant in economic terms across the three classes and statistically significant in the second class. The findings hold for the estimation that includes *M/B* as well. The results indicate that cash flow does not seem to represent an additional supply of low-cost investment finance for those firms that must pay a premium for external capital.

### 7.2. Estimation Specifics

Table 9 presents the regression results of the basic equation 2, employing the ordinary least squares, fixed effects, random effects, and first differences estimation methods. Results are reported for the entire sample period. I find that across all tests, the pattern remains: robust statistical and small economic significance of the coefficient on cash flow is only present in the second class. Still, in class 2, there are differences in the magnitudes of the cash flow coefficient depending on the estimation method used. The magnitudes of the coefficients vary from 0.02–0.057. I also find that, in any payout class, the coefficient on *M/B* strongly depends on the employed estimation method. For example, using OLS in class 3, the coefficient on *M/B* is -0.006 (significant at the five-percent level), while it is 0.006 (significant at the one-percent level) using FE. Findings are approximately in line with those in FHP (1988, table 5), who find that across all estimation methods, differences in cash flow effects between class 1 and class 3 remain remarkably consistent, while there is clear variation in the magnitudes of *M/B* . Looking at the class-2 coefficient on cash flow estimated by the RE technique (0.032) in this paper, it gets clear that it is a bit closer to the FE estimate (0.04) than to the OLS estimate (0.02). Thus, the unobserved effect, $\alpha_i$, tends to be relatively important, which is quite common in practice (Wooldridge (2013), p. 494).

As expected, table 9 also reveals that FE and FD estimates might differ in ways that go beyond a sampling error, so that a potential violation of the strict exogeneity assumption should be considered. In absolute terms, estimation differences are negligible (0.04 vs. 0.057). However, differences are around 43 percent. If $u_{it}$ is correlated with $x_{is}$ for any $t$ and $s$, FE and FD generally have different probability limits. Simultaneity, or any other standard endogeneity problem, generally causes

**Table 9:** Effects of Cash Flow and M/B on Investment: Consideration of Measurement Error, 1990-2015[a]

Source: same like table 5.
a. The dependent variable is the investment-capital ratio $(I/K)_{it}$. All independent variables are as defined in table 5, note b. The constant is not reported. Cluster-robust standard errors appear in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
b. Estimation includes year effects. All variables expressed as first differences. Estimation includes year effects.

| Independent variable and summary statistic | Ordinary least squares | Fixed effects[b] | Random effects | First difference[c] |
|---|---|---|---|---|
| | | Class 1 | | |
| $(CF/K)_{it}$ | -0.008 | -0.007 | -0.008 | -0.024 |
| | (0.009) | (0.008) | (0.008) | (0.017) |
| $(M/B)_{it}$ | -0.004 | -0.003 | -0.004 | 0.008 |
| | (0.011) | (0.009) | (0.010) | (0.021) |
| $R^2$ | 0.003 | 0.004 | 0.0043 | 0.0123 |
| | | Class 2 | | |
| $(CF/K)_{it}$ | 0.020* | 0.040*** | 0.032*** | 0.057** |
| | (0.011) | (0.015) | (0.012) | (0.024) |
| $(M/B)_{it}$ | 0.001 | 0.007* | 0.004 | 0.013** |
| | (0.002) | (0.004) | (0.003) | (0.005) |
| $R^2$ 0.022 | | 0.104 | 0.0777 | 0.0387 |
| | | Class 3 | | |
| $(CF/K)_{it}$ | -0.003 | -0.003 | -0.003 | -0.004 |
| | (0.004) | (0.003) | (0.003) | (0.004) |
| $(M/B)_{it}$ | -0.006** | 0.005*** | -0.005* | 0.006*** |
| | (0.003) | (0.002) | (0.002) | (0.001) |
| $R^2$ | 0.015 | 0.016 | 0.0184 | 0.0194 |

contemporaneous correlation, so that FE and FD are inconsistent and have different probability limits. On top of that, correlation between $u_{it}$ and $x_{is}$ for $s \not{t}$ causes both, FD and FE, to be inconsistent. For example, it is possible that the true effect of cash flow on physical investment is zero. But, if lagged CapEx causes current cash flow, this would mean that high lagged CapEx feeds back to high current cash flow. Hence, the correlation between delta investment and delta cash flow is negative and will yield a spurious negative-estimated cash flow coefficient. However, the opposite seems to be true, given that the cash flow coefficient estimated by first differencing is more positive than the cash flow coefficient estimated by fixed effects (Wooldridge (2010), pp. 284–285).

7.2.1. Choice of Estimation Method

In the investment-cash flow sensitivity literature, it is standard to work with the fixed effects estimation method (Brown and Petersen (2009), p. 972). However, given the focus on econometrics in this paper, several tests are conducted to evaluate the appropriateness of the various estimation methods. Table 10 presents the results of these tests. The Chow test is a test of whether the coefficients estimated over one group of the data are equal to the coefficients estimated over another (Wooldridge (2013), p. 453). In this case, two Chow tests are conducted. For the first test, the first group of the data is the sub-sample of observations prior to the dot-com bubble, while the second group is the sub-sample after the dot-com bubble. That is, the structural break is expected to have occurred right during the crisis. For the second test, the break date is right during the financial crisis of 2007–2008 (first group ex ante, second group ex post). Diagnostic statistics are quite similar. Both tests reject the null hypothesis of no structural change in the explanatory variables at the five-percent significance level. This does not come as a surprise, given that regression estimation results are different in the respective sample periods (see tables above, for example, table 5).

The next test is an F-test that serves to assess whether the null hypothesis that firm effects are jointly zero can be rejected. As the null hypothesis cannot be rejected, no firm fixed effects are necessary – implying that the pooled OLS method is sufficient to estimate equation 2. In contrast to that, the two F-tests assessing the need of time effects reject the null hypothesis that the time effects are jointly zero. That is, no matter whether the classification scheme is applied (splitting the sample into the three payout-classes), time fixed-effects are needed. In empirical research, it is still fairly common to use FE and RE, and then formally test for statistically significant differences in the coefficients on the time-varying explanatory variables. Concretely, quite frequently, the idea is that one uses RE unless the Hausman test (Hausman (1978)) indicates that it is more appropriate to use FE (Wooldridge (2013), p. 496). The Hausman

(1978) test is based on the difference between the RE and FE estimates. Since FE is consistent when $\alpha_i$ and $x_{itj}$ are correlated, but RE is inconsistent, a statistically significant difference is interpreted as evidence against the assumption RE.2, i.e., $Corr(x_{itj}, a_i) = 0$. Hence, the key consideration in choosing between an RE and FE approach is whether $\alpha_i$ and $x_{itj}$ are correlated.

When conducting the Hausman test, one should be aware of two caveats. First, assumption FE.4, that is, strict exogeneity, is maintained under the null and the alternative. For any $s$ and $t$, correlation between $u_{it}$ and $x_{is}$ causes both, FE and RE, to be inconsistent – and generally their probability limits will differ. The Hausman test is of no importance when this assumption is violated. (This underlines the importance of the strict exogeneity chapters (5.2.1, 5.2.2, and 7.2.2) in this paper.) Second, implementation of the test assumes that assumption RE.3, i.e., homoskedasticity, holds under the null hypothesis. This assumption is not being tested by the Hausman statistic. Violation of assumption RE.3 causes the usual Hausman test to have a non-standard limiting distribution, which means that the resulting test could have an asymptotic size that is larger or smaller than the nominal size (Wooldridge (2010), pp. 288–289). As table 10 reveals, the Hausman test rejects the null hypothesis of an unsystematic difference in the coefficients at the one-percent level. Hence, the FE approach is preferred over the RE approach. In practice, a failure to reject means either that the RE and FE estimates are sufficiently close so that it does not matter which method is used, or practically significant differences cannot be concluded to be statistically significant, because the sampling variation in the FE estimates is too large. In the latter case, one is left to wonder whether information in the data are sufficient in order to provide precise estimates of the coefficients (Wooldridge (2013), p. 496). Finally, the Breusch-Pagan Lagrange multiplier test is used to decide between RE and simple OLS regression. It is a test for heteroskedasticity across entities where the squared OLS residuals are regressed on the explanatory variables in equation 2 (Wooldridge (2013), p. 277). The test rejects the null hypothesis that the variances across entities are zero (homoskedastic) at the five-percent level. This implies that there are significant differences across firms – and, hence, there is a panel effect. All in all, tests make clear that FE is preferred over RE (Hausman), RE is preferred over OLS (Breusch-Pagan), but OLS is preferred over FE (F). Time fixed effects should be included. Given these unclear test results, economic intuition suggesting the use of FE, and the literature employing FE, firm and year FE is the preferred estimation method in this paper.

### 7.2.2. Strict Exogeneity

As shown in section 7.2, when estimations are based on the unbalanced panel, differences in cash flow estimates between FE and FD are in the order of almost 50 percent. On top of that, when working with a balanced panel, differences in estimates even increase. For example, in class 1, the FE cash flow estimate is 0.075 (significant at ten percent level) vs. FD estimate of 0.144 (significant at one percent level); in class 2, coefficient on cash flow based on FE is 0.121 (significant at five percent level), while the FD estimate is 0.064 and statistically insignificant. This can be seen as further evidence in line with Grieser and Hadlock (2015). (The results based on the balanced panel are not reported in any table; the corresponding Stata commands can be found in the appendix.)

Table 11 presents the results of strict exogeneity tests based on the FE transformation. As is standard in this thesis and following other tests for strict exogeneity, test statistics are calculated with clustered standard errors at the firm level to allow for arbitrary autocorrelation and heteroskedasticity. When the test is for a model with cash flow as the only selected explanatory variable, i.e., equation 12, magnitude and significance (but not signs) of the coefficients on the one-period-ahead cash-flow variable are approximately in line with the lagged cash-flow coefficients across payout classes. For example, the coefficient on the cash-flow lag in class 2 is 0.066 (one percent significance) and its one-step-ahead counterpart has a magnitude of -0.04 (five percent significance). This indicates violation of strict exogeneity. The same is true when the equation is augmented with $M/B$ and its first lag. Results on the cash flow coefficients are basically unchanged. The one-step-ahead $M/B$ is insignificant in every payout class; however, this is not a surprise, as its first lag is economically insignificant as well. Still, controlling for investment demand today, it makes intuitive sense that physical investment does feed back to tomorrow's investment demand. Exemplarily, due to promising investment opportunities today (high $M/B$ ), capital is used to buy a new machine (high CapEx). This new machine might cause better/more investment opportunities tomorrow, even when controlling for today's investment demand. This is because this machine could, inter alia, differentiate the company from its competitors, so that it allows the company to take on projects it could not do before. The test for the model with total investment as the dependent variable does underline that the strict exogeneity assumption is not maintained. Clear significance of contemporaneous cash flow for class-2 firms is evidence for that. I use the total investment model for tests over the time periods 1990–2002 and 2003–2015 as well. (Test results are not reported in a table; Stata commands can be found in the appendix, however.) The strict exogeneity assumption does seem to hold over the smaller time periods 1 and 2. Contemporaneous cash flow coefficients are statistically insignificant at the five-percent level. However, given the smaller sample sizes involved in these tests, the p-values are generally somewhat higher than for the larger samples. Grieser and Hadlock (2015) experience the same issue.

Table 12 is similar to table 11, except for the test results being based on the FD transformation. The test on the first (pure) cash flow model indicates that the coefficient on contemporaneous cash flow is not as significant as the coefficient on the first-differenced cash-flow variable (if at all). Again, the test for the full model, i.e., including $M/B$ and its first difference, does not impact effects of contemporaneous cash

**Table 10:** Various Tests on Estimation Methods, 1990- 2015

Source: same like table 5.
a. Tests make use of equation 2.

| Test[a] | H0 | Diagnostic statistics | Probability | Notes |
|---|---|---|---|---|
| Chow | No structural change | F = 3.04 | 0.028 | Dot-com as break |
| Chow | No structuralchange | F = 3.17 | 0.023 | Financial crisis as break |
| F | Firm effects jointly zero | F = 0.193 | >0.1 | |
| F | Firm effects jointly zero | F = 3.49 | 0.000 | With classification scheme |
| F | Firm effectsjointly zero | F = 1.52 | 0.05 | Without classification scheme |
| Hausman | Unsystematic difference in coefficients | Chi-square = 34.11 | 0.000 | |
| Breusch-Pagan LM | Homoskedasticity across entities | Chi-square = 2.9 | 0.044 | |

**Table 11:** Tests for Strict Exogeneity in FE: Various Specifications, 1990-2013[a]

Source: the three models are based on the author's estimates of variations of equation 12.
a. The dependent variable is the first lag of the ratio of physical investment (total investment in the third model) to the beginning-of-period stock of assets (capital in the third model). Equations are estimated with fixed firm and year effects (not reported). The constant is not reported. Cluster-robust standard errors appear in parentheses. *** p<0.01, ** p<0.05, * p<0. l.
b. $(CF/K)_{it-1}$ is the first lag of the ratio of cash flow, CF, to the beginning-of-period capital stock, K. $(M/B)_{it-1}$ is the first lag of the market-to-book ratio. $(CF/A)_{it}$ is the ratio of cash flow, CF, to the beginning-of-period asset stock, A. $(CF/A)_{it-1}$ is the first lag of the ratio of cash flow, CF, to the beginning-of-period asset stock, A. $(M/B)_{it}$ is defined like in table 5, note b.

| Independent variable and summary statistic[b] | Class 1 | Class 2 | Class 3 |
|---|---|---|---|
| | Test on key explanatory variable | | |
| $(CF/K)_{it}$ | 0.010* | -0.040** | 0.001 |
| | (0.006) | (0.017) | (0.001) |
| $(CF/K)_{it-1}$ | -0.018* | 0.066*** | -0.003 |
| | (0.011) | (0.023) | (0.003) |
| Within $R^2$ | 0.012 | 0.126 | 0.016 |
| | Test on standard full model | | |
| $(CF/K)_{it}$ | 0.010* | -0.039** | 0.001 |
| | (0.006) | (0.017) | (0.001) |
| $(CF/K)_{it-1}$ | -0.017 | 0.066*** | -0.003 |
| | (0.011) | (0.023) | (0.004) |
| $(M/B)_{it}$ | -0.009 | 0.001 | -0.001 |
| | (0.009) | (0.003) | (0.002) |
| $(M/B)_{it-1}$ | 0.045 | 0.006 | 0.005*** |
| | (0.029) | (0.005) | (0.002) |
| Within $R^2$ | 0.011 | 0.126 | 0.018 |
| | Test on model with total Investment as dependent variable | | |
| $(CF/A)_{it}$ | 0.030 | 0.132*** | 0.080** |
| | (0.026) | (0.035) | (0.040) |
| $(CF/A)_{it-1}$ | -0.285** | 0.185*** | 0.230* |
| | (0.140) | (0.059) | (0.120) |
| Within $R^2$ | 0.271 | 0.311 | 0.121 |

flow and its first difference. Like with the FE transformation, there does not seem to be feedback from the dependent variable to the contemporaneous *M/B* variable. However, again, the delta *M/B* effect is very small as well. Surprisingly, the test for the total investment model leads to very high coefficients on cash flow in class 1; these coefficients are not statistically significant though. When I test the total investment model for strict exogeneity in the sub-periods 1 and 2, contemporaneous cash flow coefficients are close to their first-difference counterparts, including clearly significant coefficients (again, results are not reported in a table; however, the corresponding Stata commands can be found in the appendix). Still, given the general lack of significance of the results of most variables presented in this table, it is difficult – more difficult than for the FE transformation – to draw inferences regarding the violation of strict exogeneity.

### 7.3. Robustness

Brown and Petersen (2009) have shown that a decline in the relative importance of physical investment has led to deterioration in the conventionally measured investment-cash flow sensitivity. However, because R&D intensity has risen strongly, they find that the sensitivity of investment to cash flow remains relatively strong for investment into R&D. The total investment-cash flow sensitivity has declined, but is still significant. The authors argue that this broad investment measure is less subject to the problem of changing composition of investment expenditures than CapEx and might, thus, be a more promising measure of the investment-cash flow sensitivity and thereby a better proxy for financing constraints. This is why I estimate variations of equation 2 regarding the type of investments undertaken and the scaling variable. The investment and cash flow variables are scaled by total assets in order to have a common scale factor for all regressions in this robustness check (and in some of the following). Results of these estimations are presented in table 13 for the entire sample period.

In the first model, the dependent variable is R&D expenditure. A sensitivity of R&D investment-cash flow is basically not present. One could argue that this is because of a potential measurement error in the key variables, especially in cash flow. This is because R&D is expensed and, as a consequence, cash flow measures are net of R&D expenditures (Brown and Petersen (2009), p. 975). But, when a gross cash flow measure, that is, cash flow with R&D added back, is employed instead of the "standard" cash flow measure, estimation results are basically unchanged. This lack of significance is in line with the findings of some of the recently written papers in the investment-cash flow literature, such as Chen and Chen (2012). They find that the sensitivity has declined and disappeared, even during the 2007–2009 credit crunch – and their results are robust to considerations of R&D.

In the second model, total investment is regressed on cash flow and the market-to-book ratio. The class-1 cash flow coefficient is, with -0.304, economically significant, but statistically significant only at the ten-percent level; cash flow is strongly significant for class-2 firms. Compared with that, when physical investment is the dependent variable (third model), the cash flow pattern resembles that of the second estimation, but is less pronounced.

This third model serves as a robustness check to the findings presented in table 5, as well as a comparison to the first two models discussed above. Estimation results are different to those with normalizations by capital stock in that the class-1 cash flow coefficient is significantly negative, while the second-class cash flow coefficient is more positive. When using the sales accelerator specification instead of $M/B$ to capture the investment demand side, magnitudes of the coefficients on cash flow across the payout classes generally increase in all three equations. The only exception is the total investment equation, in which the class-2 coefficient on cash flow is no longer significant. (These results are not reported in any table; the corresponding Stata commands can be found in the appendix.)

Cornell and Shapiro (1988) have shown that R&D-intensive firms use little debt, inter alia, due to the poor collateral value of R&D and the fact that debt finance can lead to financial distress, which is especially severe for R&D-intensive companies. As a consequence, R&D investment by young firms, especially those with low or negative cash flows, may be heavily dependent on the availability of public equity finance. In addition to that, as explained above, a failure to control for external finance in the investment equation can lead to a downward omitted variable bias due to the correlation of external finance with cash flow, especially for those firms that make heavy use of stock issues. Therefore, I check the robustness of this paper's findings by estimating equation 2, augmented by two external finance variables. These two external finance variables cover the equity and the debt side. Also, given the substantial theoretical importance of external (equity) finance for R&D-intensive firms, the R&D and total investment models presented above are augmented by the external finance variables as well.

Findings of the estimations over the total sample period are presented in table 14. The equation with R&D investment as the dependent variable does not undergo a significant change in the cash flow coefficients when the model includes external finance variables. It is against the outlined economic theory that public equity finance does not play a role for class-1 firms. Instead, long-term debt issuance is clearly relevant in explaining R&D investment for this subsample of firms. The downward omitted variable bias does not seem to be present in the data. When total investment is regressed on cash flow, $M/B$ , and external finance, the second-class cash flow coefficient is less significant in explaining total investment than without external finance, but the pattern remains. Stock issuance does play a significant role, especially for class-2 firms. Its impact on cash flow is ambiguous, as the coefficient on cash flow is lower in the second class, while it is higher in the third class. For the third model and compared with table 5, the cash flow pattern is quite robust to the augmentation by external finance, while external finance generally has power in explaining variations in CapEx.

One purpose of this paper is to use the approach in Fazzari et al. (1988) and analyze how their findings have changed using the more current set of data (and methodology, e.g., clustering). A potential impact factor for the deviation in results from those in Fazzari et al. (1988) could be different definitions of key variables, as outlined above. In order to see whether findings are robust to different definitions of some variables that more closely resemble those used in Fazzari et al. (1988), sales accelerator and cash flow are defined differently. Sales accelerator, that is, sales plus finished

**Table 12:** Tests for Strict Exogeneity in FD: Various Specifications, 1990-2015[a]

Source: the three models are based on the author's estimates of variations of equation 12.
a. The dependent variable is the first difference of the ratio of physical investment (total investment in the third model) to the beginning-of-period stock of assets (capital in the third model ). Estimation includes year effects. The constant is not reported. Clusterrobust standard errors appear in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
b. Δ denotes first-differenced variables. All independent variables are as defined in table 11, note b.

| Independent variable and summary statistic[b] | Class 1 | Class 2 | Class 3 |
|---|---|---|---|
| | Test on key explanatory variable | | |
| $\Delta(CF/K)_{it}$ | -0.028* | 0.054** | -0.004 |
| | (0.016) | (0.024) | (0.004) |
| $(CF/K)_{it}$ | 0.006 | 0.005* | 0.000 |
| | (0.010) | (0.003) | (0.000) |
| Within $R^2$ | 0.016 | 0.056 | 0.02 |
| | Test on standard full model | | |
| $\Delta(CF/K)_{it}$ | -0.027 | 0.055** | -0.004 |
| | (0.017) | (0.024) | (0.005) |
| $(CF/K)_{it}$ | 0.005 | 0.005* | 0.000 |
| | (0.010) | (0.003) | (0.000) |
| $\Delta(M/B)_{it}$ | 0.021 | 0.010* | 0.006** |
| | (0.034) | (0.005) | (0.003) |
| $(M/B)_{it}$ | -0.016 | 0.001 | 0.000 |
| | (0.032) | (0.000) | (0.000) |
| Within $R^2$ | 0.015 | 0.059 | 0.023 |
| | Test on model with total investment as dependent variable | | |
| $\Delta(CF/A)_{it}$ | 0.973 | 0.020 | -0.007 |
| | (0.662) | (0.042) | (0.057) |
| $(CF/A)_{it}$ | -0.950 | 0.074* | -0.020 |
| | (0.674) | (0.039) | (0.080) |
| Within $R^2$ | 0.227 | 0.036 | 0.017 |

goods inventory, is employed as an independent variable instead of sales. Cash flow is defined without subtracting dividends – assuming that sticky dividends play a smaller role. In both specifications, results are basically indistinguishable from those with the "standard" set of variables, undermining the possibility that the different findings are, among other things, due to a different set of variables.

Another robustness check refers to clustering, which is relatively new, so that Fazzari et al. (1988) could not make use of it yet. When employing default standard errors instead of the cluster-robust version, the failure to control for within-cluster correlation leads to misleadingly small standard errors, narrow confidence intervals, large t-statistics, and low p-values. When working with cluster-robust standard errors instead, it does not matter too much what to cluster over. This is because there is basically no difference in the standard errors when they are clustered over firms compared with clustering over industries. As explained above, all outputs presented in this paper are based on standard errors that are clustered over firms. A different issue is financial distress. Financially distressed firms may experience lower investment-cash flow sensitivities. Coefficients are basically unchanged when all firm-year observations are excluded in which sales growth was not positive. Thus, findings are robust to trying to eliminate distressed firms (or firm-year observations during which companies were struggling). This is in line with Fazzari et al. (1988) who only work with companies that had grown their sales, but find that results do not change significantly when negative-sales-growth firms are included. Excluding some distressed firms this way is complementary to balancing the panel, which omits some financially distressed firms as well. (The results presented in this paragraph are not reported in any table; the corresponding Stata commands can be found in the appendix.)

A final issue is the potential discrepancy due to working with an unbalanced panel, as compared to working with a balanced panel, such as in Fazzari et al. (1988) among others. The mechanics of FE estimation with an unbalanced and a balanced panel are basically the same. If $T_i$ is the number of time periods for cross-sectional unit i, one simply uses these $T_i$ observations in doing the time-demeaning. Like in the balanced panel case, one degree of freedom is lost for every cross-sectional observation due to time-demeaning. However, the reason for an unbalanced panel, among other

**Table 13:** Effects of Cash Flow and M/B on Physical, R&D, and Total Investment, 1990-2015[a]

Source: the three models are based on the author's estimates of variations of equation 2.
a. The dependent variable is the ratio of some form of investment to the beginning-of period stock of assets. It is investments in R&D in the first mode l, investments in physical assets plus R&D in the second mode l, and investments in physical assets in the third model. Equations are estimated with fixed firm and year effects (not reported). The constant is not reported. Cluster-robust standard errors appear in parentheses. *** p<0.01,** p<0.05, * p<0.l.
b. $(CF/A)_{it}$ is the ratio of cash flow, CF, to the beginning-of-period asset stock. $(M/B)_{it}$ is defined in table 5, note b.

| Independent variable and summary statistic[b] | Class 1 | Class 2 | Class 3 |
|---|---|---|---|
| | Model with R&D investment as dependent variable | | |
| $(CF/A)_{it}$ | -0.038* | 0.059 | -0.097 |
| | (0.022) | (0.045) | (0.061) |
| $(M/B)_{it}$ | 0.114 | -0.000 | -0.000 |
| | (0.077) | (0.001) | (0.000) |
| Within $R^2$ | 0.336 | 0.095 | 0.178 |
| | Model with total investment as dependent variable | | |
| $(CF/A)_{it}$ | -0.304* | 0.229*** | -0.032 |
| | (0.171) | (0.068) | (0.094) |
| $(M/B)_{it}$ | -0.074 | 0.001 | 0.001 |
| | (0.151) | (0.002) | (0.001) |
| Within $R^2$ | 0.278 | 0.311 | 0.118 |
| | Model with physical investment as dependent variable | | |
| $(CF/A)_{it}$ | -0.163** | 0.198*** | -0.020 |
| | (0.083) | (0.041) | (0.028) |
| $(M/B)_{it}$ | -0.107** | 0.003* | 0.002*** |
| | (0.051) | (0.001) | (0.001) |
| Within $R^2$ | 0.145 | 0.283 | 0.054 |

things, can be that data in advanced years do no longer include those firms that have gone out of business or merged with other firms. This can be problematic if the reason a firm leaves the sample (attrition) is correlated with the idiosyncratic error, i.e., those unobserved factors that change over time and affect investment spending. A resulting non-random sample can cause biased estimators. Nevertheless, fixed effects analysis is useful here, as it allows attrition to be correlated with $\alpha_i$, the unobserved effect (Wooldridge (2013), p. 491). Also, attrition bias should not be a substantial issue here, given that the classification scheme requires companies to have firm-year observations, i.e., to be active, over the majority of the sample period.

The classification scheme might also (partially) impede an effect of balancing the panel on the firms' degree of asymmetric information (and thereby investment-cash flow sensitivities): as firms mature and more observations of project realizations and balance sheets become available, asymmetric information problems should become less severe. It is striking that when comparing descriptive statistics between the balanced and the unbalanced panel, the number of observations is much lower (approximately factor of ten), there are much lower standard deviations of sales growth (approximately factor of 50 for class 1), investment-capital ratio, and cash flow-capital ratio, and there is less of a difference between mean and median values of capital stock and $M/B$ . These differences are likely to impact the estimation results, which are presented in table 15, and serve as an important test to see whether the very high standard deviations of key variables in the unbalanced case harm the credibility of the estimation results. Table 15 is just like table 5, except that the estimation is based on a balanced panel. Interestingly, the cash flow coefficient on class 1 is significant over 2003–2015, while economic theory would suggest it is more significant over 1990–2002 (asymmetric information). The balanced panel might show significant cash flow effects of the low-payout firms because financially distressed firms that went out of business or merged during the sample period were entirely omitted from the estimation – and were not omitted due to the classification scheme already. The class-2 coefficient on cash flow is more significant over 1990–2015 than in the unbalanced case. For the third class, the cash flow coefficient is still not economically significant. When different estimation methods are employed (just like in table 9), the cash flow pattern generally remains: a small effect in class 1, a more significant effect in class 2, and no effect in class 3. Only the FD estimation is slightly different in that the class-1 cash flow effect is more pronounced (0.144, significance at the one-percent level), while the class-2 coefficient is insignificant. In addition, just like in the case with an unbal-

**Table 14:** Effects of Cash Flow, M/B, and Externat Finance on Physical, R&D, and Total Investment, 1990-2015[a]

Source: the three models are based on the author's estimates of variations of equation 2.
a. The dependent variable is the ratio of some form of investment to the beginning-of period stock of assets (capital in the third model). It is investments in R&D in the first model, investments in physical assets plus R&D in the second model, and investments in physical assets in the third model. Equations are estimated with fixed firm and year effects (not reported). The constant is not reported. Cluster-robust standard errors appear in parentheses. *** p<0.01, ** p< 0.05, * p<0.l.
b. $(CF/A)_{it}$ is the ratio of cash flow, CF, to the beginning-of-period asset stock, A. $(CST/A)_{it}$ is the ratio of commons hare s iss ued, CST, to beginning-of-period total assets. $(LTD/A)_{it}$ is defined as the measure of the issuance of long-term debt, LTD, scaled by total assets at the beginning of the period. All other independent variables are as defined in table 5, note b.

| Independent variable and summary statistic[b] | Class 1 | Class 2 | Class 3 |
|---|---|---|---|
| | Model with R&D investment as dependent variable | | |
| $(CF/A)_{it}$ | -0.032* | 0.017 | -0.010 |
| | (0.019) | (0.054) | (0.023) |
| $(CST/A)_{it}$ | -0.000 | 0.110 | 0.007*** |
| | (0.000) | (0.072) | (0.001) |
| $(LTD/A)_{it}$ | 0.228*** | 0.028* | -0.000** |
| | (0.077) | (0.015) | (0.000) |
| $(M/B)_{it}$ | 0.122 | -0.000 | 0.000 |
| | (0.075) | (0.001) | (0.000) |
| Within $R^2$ | 0.393 | 0.180 | 0.506 |
| | Model with total investment as dependent variable | | |
| $(CF/A)_{it}$ | -0.296* | 0.142* | 0.095* |
| | (0.171) | (0.072) | (0.056) |
| $(CST/A)_{it}$ | 0.000 | 0.367*** | 0.011*** |
| | (0.000) | (0.096) | (0.002) |
| $(LTD/A)_{it}$ | 0.171 | 0.039* | 0.000 |
| | (0.332) | (0.021) | (0.001) |
| $(M/B)_{it}$ | -0.075 | 0.004** | 0.002** |
| | (0.152) | (0.002) | (0.001) |
| Within $R^2$ | 0.280 | 0.387 | 0.250 |
| | Model with physical investment as dependent variable | | |
| $(CF/K)_{it}$ | -0.001 | 0.024** | -0.002** |
| | (0.007) | (0.011) | (0.001) |
| $(CST/A)_{it}$ | 0.000 | 0.224** | 0.004*** |
| | (0.000) | (0.100) | (0.001) |
| $(LTD/A)_{it}$ | 0.176*** | 0.002*** | -0.002*** |
| | (0.047) | (0.001) | (0.000) |
| $(M/B)_{it}$ | -0.001 | 0.011* | 0.008*** |
| | (0.010) | (0.006) | (0.002) |
| Within $R^2$ | 0.609 | 0.167 | 0.608 |

anced panel, when working with the acceleration principle to construct a model's investment demand side, cash flow coefficients are no longer significant in any payout class. This also means that, when working with the unbalanced panel, the high standard deviations of key variables (e.g., cash flow-capital ratio) do not substantially impact the estimation results. (The descriptive statistics, the results of the different estimation methods, and the results of the sales accelerator specification are not reported; the corresponding Stata commands can be found in the appendix.)

## 8. Conclusion

The sample in this study consists of annual data of US industrial firms between 1990 and 2015. I use Fazzari et al. (1988) as a guidance and comparison and analyze what has changed. I augment their research design with the findings from influential, other papers in the literature. In addition to that, throughout this paper, I focus on the econometrics involved, making this paper one of the first empirical finance papers to explicitly acknowledge and analyze the strict exogeneity assumption. The literature demonstrates that the investment-cash flow sensitivity has decreased over

**Table 15:** Effects of Cash Flow and M/B on Investment, Various Periods, Balanced Panel, 1990- 2015[a]

Source: author's estimates of equation 2 based on a sample of firm data from Compustat database. See text.
a. The dependent variable is the investment-capital ratio $(I/K)_{it}$ where I is investment in property, plant and equipment and K is beginning-of-period capital stock. All independent variables are as defined in table 5, note b. The equations were estimated using fixed firm and year effects (not reported). The constant is not reported. Cluster robust standard errors appear in parentheses. *** p<0.01, ** p<0.05, * p<0. l.

| Independent variable and summary statistic | Class 1 | Class 2 | Class 3 |
|---|---|---|---|
| | | 1990-2002 | |
| $(CF/K)_{it}$ | 0.001* | 0.044** | -0.001*** |
| | (0.001) | (0.017) | (0.000) |
| $(M/B)_{it}$ | 0.004 | 0.015*** | 0.004 |
| | (0.009) | (0.005) | (0.003) |
| Within $R^2$ | 0.032 | 0.147 | 0.223 |
| | | 2003- 2015 | |
| $(CF/K)_{it}$ | 0.100*** | 0.014 | 0.010 |
| | (0.016) | (0.038) | (0.009) |
| $(M/B)_{it}$ | 0.038 | 0.002 | -0.009*** |
| | (0.047) | (0.003) | (0.003) |
| Within $R^2$ | 0.274 | 0.075 | 0.029 |
| | | 1990-2015 | |
| $(CF/K)_{it}$ | 0.075* | 0.121** | -0.001*** |
| | (0.038) | (0.056) | (0.000) |
| $(M/B)_{it}$ | 0.028 | 0.006 | -0.001 |
| | (0.024) | (0.007) | (0.003) |
| Within $R^2$ | 0.175 | 0.214 | 0.195 |

time. This literature is also not without criticism. For example, some researchers criticize the conventional regression equations used, while others doubt that these equations serve as adequate measures of financing constraints. Given that much remains unsolved, the topic of financing constraints and investment-cash flow is still a vivid research field – and one of the largest in corporate finance. It is recently also being broadened. Finally, the literature section shows that testing for the violation of the strict exogeneity assumption in empirical panel data finance research is of critical importance.

Revisiting the hypotheses a, b, and c, this paper presents evidence that the investment-cash flow sensitivity has decreased and (mostly) disappeared over time. For that, I use two of the broad empirical specifications that encompass the most common approaches of constructing models' investment demand side: models based on the *M/B* ratio, as well as models based on the sales accelerator. The estimation results basically do not change, no matter whether the *M/B* model is augmented with cash flow, with cash flow lags and lagged *M/B* ratios on top of that, or with cash stock in addition to cash flow. The sales accelerator model consists of three lags of sales, augmented with cash flow. An alternative specification adds *M/B* to that. When the sales accelerator specification is used instead of *M/B* , there is no significant cash flow coefficient in any class. This suggests that the previous significance in class 2 is due to correlation of cash flow and sales. The results are also robust to employing different estimation methods, namely OLS, FE, RE, and FD. Depending on which of the estimation methods is used, magnitudes of the cash flow coefficient in equation 2 vary from 0.02–0.057. While the differences in absolute terms are negligible, the differences in, for example, FE and FD estimates are in the order of nearly 50 percent. Given this difference – which is even more pronounced when estimations are based on a balanced panel instead of an unbalanced panel –, I suspect that estimation differences are driven by the violation of the strict exogeneity assumption. (This is confirmed in explicit strict exogeneity tests, so that hypothesis e is not rejected.) The magnitude of the difference also suggests that the inconsistency caused by the violation of the assumption can be substantial (hypothesis f). Coefficients on *M/B* strongly depend on the choice of the estimation method.

In order to choose the optimal estimation method for the purposes of this research, I conduct several tests. Their results are ambiguous. FE is preferred over RE (Hausman test), RE is preferred over OLS (Breusch-Pagan test), but OLS is preferred over FE (F test). However, given that economic intuition suggests the use of FE and given that the literature mostly employs FE, firm and year FE is the preferred estimation method in this paper (hypothesis d). My results are unchanged in substance when the following robustness checks are conducted: consideration of a changing composition of investment, consideration of the impact of external finance, different definitions of variables, various forms of clustering, exclusion of firm-years with negative sales growth, and estimations based on a balanced panel. Hence, evidence on a lack of significance of the sensitivity of investment to cash flow stands in stark contrast to the findings in . However, my evidence is in line with many recent papers (e.g., Chen and Chen (2012)).

This paper contributes to the literature by being an additional piece of evidence that the investment-cash flow sensitivity has decreased over time (and disappeared) – robust to a number of alternative specifications and robustness checks. In addition to that, it presents further evidence that the strict exogeneity assumption is quite commonly violated in empirical panel data finance research and that this violation can cause a substantial distortion in results.

This paper offers me a great gain in knowledge in empirical finance research, panel data estimation methods, and the use of Stata – and, hence, serves as a good preparation for my doctoral studies. However, the conclusions drawn in this paper do not come without caveats. Estimations are mainly based on an unbalanced panel. Descriptive statistics show very large standard deviations in sales growth, investment-capital ratio, and cash flow-capital ratio, especially for class-1 firms. This is worrying, as outliers can undermine the credibility of estimation results. However, I drop the two observations with a Cook's distance greater one. Then, estimation results of OLS are very close to those of a robust regression. Furthermore, descriptive statistics of the payout classes show much lower standard deviations in the case of a balanced panel. Estimation results based on a balanced panel are approximately in line with the ones based on an unbalanced panel. This should indicate that the presented evidence is not impacted by outlier problems.

The same arguments can be applied against the criticism on employing a splitting criterion that sorts firms into subsamples with differential outliers in growth rates, which can lead to finding a difference in the coefficients on cash flow. Therefore, dividend payouts as a broad splitting criterion are no longer without controversy. In line with that is that some recent papers use more direct proxies for capital market imperfections and financial constraints. For example, Chowdhury et al. (2016) use the bid-ask spread measure of information asymmetry. This measure is generally accepted in the market microstructure literature and might improve upon the broad proxy of dividend payout rates used in this paper. Another drawback of this paper is the theoretical ambiguity regarding the economic interpretations of some variables presented throughout this study. For example, a firm's high cash stock can either signal that the firm has done well and is likely to continue doing well (thus, more liquid firms have better investment opportunities), or it means that a constrained firm hoards cash to protect against future downturns. This would then be a signal for poor performance.

With regards to the outline for future research, I can say that there is still room for research in areas in which financial constraints possibly play a role. Exemplarily, further research can be conducted on the efficiency of internal capital markets, the effect of agency on firm policies, and the influence of managerial characteristics. There is ongoing interest in the study of the relationship between investment and cash flow because of the importance of firm and aggregate investment and because of continuing innovation regarding alternative investment-cash flow regression equations, measures of financing constraints, usage of exogenous shocks, etc. These innovations can shed additional light on the issue of financing constraints. Also, research can try to work around the theoretical ambiguity of variables. Finally, econometrics can play a larger role, for example, by using dynamic instead of static models.

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