FIRM LIFE CYCLE AND DETECTION OF ACCRUAL-BASED EARNINGS MANIPULATION

BY

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ABSTRACT

The identification of estimation samples is important for estimating discretionary accruals using an accrual model. This study proposes an alternative identification of estimation samples using a firm's life cycle (i.e., life cycle-based estimation samples). Analyses using U.S. and international data show that when detecting accrual manipulation, life cycle-based estimation samples outperform industry-based or size-based estimation samples in sample retention, specification, and detection power. Improved detection power by life cycle-based estimation samples is also evident in the AAERs sample. Lastly, I reexamine Dechow, Richardson, and Tuna (DRT) (2003) and Teoh, Wong, and Rao (TWR) (1998) applying life cycle-based estimation samples. I find that life cycle-based estimation samples change the inferences from DRT by improving test power and mitigate misspecification in TWR. Collectively, the current study provides empirical evidence that supports the use of life cycle-based estimation samples over other existing estimation samples.

Keywords: firm life cycle; estimation samples; discretionary accruals; earnings management

To my family

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I. INTRODUCTION

An extensive body of accounting literature (Kothari 2001; Dechow et al. 2010) uses discretionary accruals as a proxy for accrual-based earnings manipulation. Empirically, discretionary accruals are estimated using an accrual model within certain *estimation samples* where normal accrual generating processes are assumed to be homogeneous.[1] While the specification of widely-used accrual models (i.e., variants of the Jones model and the Dechow-Dichev model) has been questioned and evaluated (Dechow et al. 1995; Kothari et al. 2005; Stubben 2010), there is very limited research on the identification of estimation samples. The identification of estimation samples is important because superior estimation samples enable the accrual model to adequately capture normal accrual generating processes and thus allow separating normal and discretionary accruals with precision and sufficient power (Dopuch et al. 2012). Relying on theories on accruals (e.g., Sloan 1996; Fairfield et al. 2003) and firm life cycle (e.g., Liu 2006; Dickinson 2011), this study proposes an alternative identification of estimation samples using a firm's life cycle. I investigate whether using life cycle-based estimation samples improves the detection of accrual manipulation.

Traditionally, researchers estimate an accrual model using estimation samples defined as all firms in the same industry, assuming that industry-based estimation samples have homogeneous accrual generating processes. Yet, several studies (Dopuch et al. 2012; Owens et al. 2013) show that the normal accrual generating processes within an industry are not as homogeneous as are implicitly assumed in the literature. Recently, Ecker et al. (2013) point out that industry-based estimation samples impose substantial sample attrition, and suggest forming estimation samples by similar firm size. Size-based estimation samples resolve the problem of sample attrition and yield comparable levels of specification and power of the tests

[1] Researchers model normal (non-discretionary) accruals as a function of firm characteristics (e.g. change in cash sales and PP&E). Normal accrual generating processes refer to the activities that give rise to accruals associated with such firm characteristics.

for detecting accrual manipulation to industry-based estimation samples. It is, however, theoretically unclear how firm size is able to capture homogeneity in normal accrual generating processes.

Estimation samples based on a firm's life cycle are potentially superior to other estimation samples for the following three reasons. First, normal accrual generating processes are expected to be relatively homogeneous within each life cycle stage because firms within the same life cycle stage have a similar size of investment in both short-term and long-term operating assets and share a similar level of profitability and credit policy (Dickinson 2011). Second, a firm's life cycle is defined in a way that allows an ample number of firms in each of the five life cycle stages (Introduction, Growth, Mature, Shake-out, and Decline).[2] Therefore, unlike industry-based estimation samples, life cycle-based estimation samples do not cause substantial sample attrition. Third, similar firm characteristics (e.g., size and profitability) shared by firms in the same life cycle stage are potentially correlated omitted variables in the accrual model and therefore, estimating the accrual model by life cycle can mitigate the misspecification problem. Statistically, the increased number of observations and homogeneous estimation samples could lead to tests with higher power and better goodness of fit.

To test different identifications of estimation samples, I employ the most popular accrual model, the modified Jones model. Estimation of the modified Jones model shows that the model estimates (i.e., the coefficients on change in cash sales and PP&E) and detection power for accrual manipulation vary systematically across life cycle stages as predicted by

[2] A firm's life cycle stage is determined by the patterns of cash flows from operating, investing, and financing activities (Dickinson 2011). When estimating an accrual model, the dependent variable is total accruals and researchers require data for earnings before extraordinary items (*ibc*) and cash flows from operating activities (*oancf*) to calculate total accruals (=*ibc*-*oancf*). Non-missing values for these variables are the typical minimum requirements for estimating an accrual model. When cash flows from operating activities (*oancf*) are reported, both cash flows from investing activities (*ivncf*) and cash flows from financing activities (*fincf*) are also reported in Compustat. Accordingly, no/little additional information is required for life cycle-based estimation samples beyond the data for estimating an accrual model.

existing theories of life cycle (e.g., Spence 1977, 1979). First, the coefficient of change in cash sales is significantly more positive for introduction firms, consistent with firms in this stage investing heavily in working capital while having operating cash *out*flows. For firms in the growth and mature stages, the coefficient of change in cash sales gradually decreases because of the increase in operating cash flows. Second, the coefficient of PP&E is significantly more negative for growth firms, suggesting that growth firms have a newer and larger scale of investment in PP&E. Third, an adjusted R^2 and detection power for accrual manipulation in simulations are highest among growth and mature firms and lowest among introduction and decline firms, consistent with firms in the early or late life cycle stage being more heterogeneous from each other. Fourth, detection power is significantly higher for firms whose life cycle stages remain unchanged for two or three consecutive years, indicating that life cycle transitions capture significant changes in accrual generating processes.

I then compare the performance of life cycle-based estimation samples to that of industry-based and size-based estimation samples. The simulation results for a large U.S. sample provide three insights. One, the modified Jones model is well-specified regardless of which identification of estimation samples is used. Two, both life cycle-based and size-based estimation samples outperform industry-based estimation samples in detecting seeded accrual manipulation and mitigating sample loss. Three, matching a firm-year from each life cycle stage with its industry or size peers does not provide much improvement in the detection of accrual manipulation compared to when estimation samples are formed by firm life cycle alone.

Smaller samples face the problem of substantial sample attrition and require higher levels of homogeneity in normal accrual generating processes within estimation samples. To further compare the performance of different estimation sample identifications in smaller samples, I utilize two settings: randomly selected small U.S. samples (N=500 to 15,000), and international samples (Compustat Global). I document two advantages of using life cycle-based estimation samples in these settings. First, choosing life cycle-based estimation samples mitigates the sample attrition to a significant extent. For example, when 30 or more observations are required for each industry and year, 52.43% international observations are excluded from the sample while life cycle-based estimation samples reduce this percentage to 12.97%. Second, in both small U.S. samples and international samples, the detection power of life cycle-based estimation samples is significantly higher than that of industry-based or size-based estimation samples, confirming the efficacy of life cycle-based estimation samples when sample size is relatively small. Supplemental tests suggest that these simulation results are not sensitive to alternative accrual models and/or different approaches to seed manipulation.

Similar to detection power, specification is also an important concern in earnings management studies. Prior studies (e.g., Dechow et al. 1995) conclude all models reject the null hypothesis of no earnings management at rates exceeding the specified test levels when applied to samples of firms with extreme performance. Given similar levels of firm performance (in multiple dimensions of performance metrics) in the same life cycle stage, a correlated omitted variables problem may be alleviated by life cycle-based grouping. Consequently, life cycle-based estimation samples are less likely to yield misspecified tests in extreme performance samples. Consistent with this prediction, I find that using life cycle-based estimation samples mitigates misspecification for firms with extreme operating performance (i.e., extreme operating cash flows (*OCF*) and extreme return on assets (*ROA*)).

Next, I investigate which estimation samples generate the highest detection power for *actual* accrual manipulation. I use the cases in the Accounting and Auditing Enforcement Releases (AAERs) sample as a proxy for actual accrual manipulation. The AAERs database primarily includes enforcement actions against firms in which the SEC alleges that earnings manipulation has taken place (Dechow et al. 2011). Results show that detection power for actual accrual manipulation is 46% (36%) higher when the accrual model is estimated by life cycle and year than by industry and year (by size and year). These findings reflect that when firms are grouped with homogeneous peers, there is a higher chance of detecting accrual manipulation.

Lastly, I reexamine Dechow, Richardson, and Tuna (DRT) (2003) and Teoh, Wong, and Rao (TWR) (1998) applying life cycle-based estimation samples to each study. DRT hypothesize that boosting discretionary accruals to avoid reporting a loss is the reason for the kink in the earnings distribution (i.e., too few firms report small losses, too many firms report small profits). However, in their study, accrual models estimated within each industry and year generate discretionary accruals of small profit firms that are statistically *indifferent* from those of small loss firms. Using life cycle-based estimation samples, I find significantly larger discretionary accruals for small profit firms than for small loss firms consistent with the kink being caused by earnings management. Also, TWR find evidence that IPO firms report high earnings during the IPO by reporting discretionary accruals (estimated using industry-based estimation samples) aggressively. However, IPO firms are generally high performance companies. If firm performance is a potentially omitted correlated variable in the accrual model, misspecification is expected to be relatively high in the IPO setting. Using life cycle-based estimation samples, I find that discretionary accruals at the time of IPO are significantly lower than the ones reported in TWR. Collectively, these results indicate that life cycle-based estimation samples not only improve detection power but also mitigate misspecification.

This study contributes to the accruals literature (Kothari 2001; Dechow et al. 2010) by proposing a theoretically-grounded alternative identification of estimation samples for the accrual model estimation. From a practical perspective, life cycle-based estimation samples impose little to no sample loss and yield higher detection power in samples of varying number of observations, benefitting studies using small samples and international data in particular. Life cycle-based estimation samples also benefit earnings management studies that use extreme performance samples (e.g., Teoh et al. 1998; Shivakumar 2000; Ball and Shivakumar 2008) by mitigating misspecification. Lastly, this study complements studies on identifying economically-related peer firms (Dichev et al. 2013; Lee et al. 2014) and extends the firm life cycle literature (Spence 1977, 1979, 1981; Anthony and Ramesh 1992; Dickinson 2011). Prior research on identifying peer firms has been largely centered on refining the industry classification (Lee et al. 2014). In addition, studies on firm life cycle (e.g., Anthony and Ramesh 1992) have focused on the role of life cycle in valuation settings. This study identifies peer firms based on life cycle and demonstrates the advantages of using these peers in the context of accruals.

Section II provides related literature on accrual models and estimation samples, and Section III presents theoretical relations between life cycle and accruals. Sample, descriptive data, and model estimation are described in Section IV. Section V presents simulation procedures and results for U.S. samples, international samples, extreme performance samples, and the AAER sample. Section VI reexamines Dechow, Richardson, and Tuna (2003) and Teoh, Wong, and Rao (1998) using life cycle-based estimation samples. Section VII presents supplemental analyses and Section VIII concludes.

II. RELATED LITERATURE

2.1. Accrual Models

Jones-type accrual models have been the most popular in the literature. Jones (1991) is the first study that considers residuals from the expectation model for total accruals as estimated discretionary accruals. In the Jones model, change in sales and PP&E are included as two economic drivers that determine normal accruals. Accordingly, regression residuals, which are assumed to be orthogonal to these economic drivers, could represent discretionary accruals. Dechow et al. (1995) replace change in sales in the Jones model with change in *cash* sales ($\Delta SALES_{it}$-ΔREC_{it}) (see Equation (1)). This modified version of Jones model (known as the modified Jones model) is intended to capture revenue manipulation that occurs through the misstatement of accounts receivable. Based on its popularity in the literature (Collins et al. 2012), the current study employs the modified Jones model to compare the performance of life cycle-based estimation samples to that of other alternative estimation samples. In Equation (1), an error term (ε_{it}) represents discretionary accruals and the remaining terms (predicted value of regression) represent normal accruals.

$$TA_{it}/Asset_{it-1}=\alpha+\beta_1[1/Asset_{it-1}]+\beta_2[(\Delta SALES_{it}-\Delta REC_{it})/Asset_{it-1}]+\beta_3[PPE_{it}/Asset_{it-1}]+\varepsilon_{it} \quad (1)$$

where TA_{it} is total accruals for firm *i* in year *t*, $\Delta SALES_{it}$-ΔREC_{it} is change in cash sales for firm *i* in year *t*, PPE_{it} is net property, plant, and equipment for firm *i* in year *t*, $1/Asset_{it-1}$ is an intercept scaled by total assets for firm *i* in year *t-1*, and ε_{it} is an error term for firm *i* in year *t*.

Accrual models, in general, suffer from measurement errors and correlated omitted variables, which lead to Type I and Type II errors. Type I errors (also known as *mis*specification) are defined as rejection of a true null hypothesis of no earnings management. Dechow et al. (1995, p. 193) conclude that all accrual models appear well-specified when applied to a random sample, but all models reject the null hypothesis of no earnings management at rates exceeding the specified test levels when applied to samples of firms with extreme performance. To mitigate Type I errors, Kothari et al. (2005) develop performance-matched accrual models. In the matching procedures, they first identify a firm (i.e., control firm) with the closest level of return on assets to that of the sample firm within each industry and year and then deduct the control firm's discretionary accruals estimated using either Jones or modified Jones model from those of the sample firm.[3] While the performance matching procedure mitigates Type I errors, it is known to augment Type II errors. Type II errors (=1-Power) are defined as failure to reject a false null hypothesis of no earnings management.

In an attempt to reduce both Type I and Type II errors, researchers often modify their choice of economic drivers that are included in accrual models (e.g., Dechow et al. 1995; Dechow and Dichev 2002; Francis et al. 2005). For example, Dechow and Dichev (2002) include past, present, and future cash flows as determinants of working capital accruals based on an accounting relation between accruals and cash flows.[4] Also, several studies (e.g., McNichols and Wilson 1988; Stubben 2010; Choudhary et al. 2013) focus on specific accruals to mitigate measurement errors caused by including all other accruals and irrelevant economic drivers to accruals of a researcher's choice. In particular, Stubben (2010) shows improvement in detecting accrual manipulation by modeling a single earnings component (revenue) and the related accrual (accounts receivable). Furthermore, more recent studies improve the specification and power of tests for detecting accrual manipulation by incorporating accrual

[3] Kothari et al. (2005) model the level of discretionary accruals (i.e., discretionary accruals adjusted for a performance-matched firm's discretionary accruals). Rather than modeling the level of discretionary accruals, this study focuses on how to form estimation samples when estimating an existing accrual model. For example, in combining life cycle-based estimation samples with the performance-matched accrual model, researchers could identify a performance-matched control firm within each life cycle stage (rather than within each industry).

[4] The accounting system provides for accruals, i.e., temporary adjustments that shift the recognition of cash flows over time (Dechow and Dichev 2002). Accruals refer to exchanges of goods/services for which cash has not yet been exchanged and deferrals refer to exchanges of cash for which goods/services have not yet been exchanged. The terminology "accruals" used in this study includes both accruals and deferrals. To illustrate, suppose an introduction firm purchases large quantities of inventory with cash, anticipating increasing product demand. Because costs of goods sold are generally recorded when goods are sold, a firm's inventory purchases are considered accruals. Precisely, inventory purchases are deferrals.

reversals into the test model (Dechow et al. 2012) or by correcting for heteroscedasticity of discretionary accruals within pre-specified estimation samples (Chang et al. 2014).

Although much emphasis in prior literature has been placed on the specification of accrual models, very little attention has been paid to how estimation samples for accrual models are identified. This study focuses on which *estimation samples* perform best in lowering both Type I and Type II errors when detecting accrual manipulation.

2.2. Estimation Samples

When estimating accrual models, the identification of estimation samples is an important issue because it significantly influences the specification and power of tests for detecting accrual manipulation as well as the extent of sample attrition (Ecker et al. 2013). Estimation samples used in prior literature on accrual models include (1) estimation samples by each firm, (2) estimation samples based on industry membership, and (3) estimation samples based on similar firm size.

Accrual models can be estimated at the firm level under the assumption that a firm has a stable normal accrual generating process over time (Jones 1991). Estimation samples by each firm allow cross-sectional variation in parameter estimates but impose strict data requirements for a firm to be included for analysis, resulting in substantial sample attrition (DeFond and Jiambalvo 1994; Subramanyam 1996). To avoid this issue, researchers commonly estimate accrual models cross-sectionally.[5] Milder data requirements in cross-sectional estimation likely mitigate potential survivorship bias and lead to higher estimate precision.

Most researchers select an industry classification scheme from several variants (e.g., SIC, NAICS, GICS, and Fama-French 12 industry) to delineate industrial activities depending

[5] Collins et al. (2012) note that 17 of 22 studies referenced in Dechow et al. (2010) estimate accrual models cross-sectionally and only 4 studies employ time-series firm-specific estimation.

on the context of their study.[6] Estimation samples based on industry membership assume that firms in the same industry have similar normal accrual generating processes. To some extent, accrual generating processes could be captured by industry membership because firms in the same industry share similar operations and apply similar accounting rules (Bartov et al. 2000). For instance, firms in the financial industry generate accruals (e.g., loan loss reserves) largely from their financial assets/liabilities that are measured at fair value. On the contrary, for manufacturing firms, most accruals arise from PP&E, which are reported at historical cost on the balance sheet.

Nevertheless, recent studies (Dopuch et al. 2012; Owens et al. 2013) suggest that the normal accrual generating processes within an industry are not homogeneous due to firms' varying business strategies and differing sensitivity to business shocks. In addition, *industry*-based estimation samples have three inherent shortcomings. First, because many firms currently compete in multiple industries and their product offerings are quite diverse, it is often challenging to identify a firm's primary industry (Dickinson 2011). For instance, General Electric (GE) operates through four divisions, such as Energy (inactive 2013), Technology Infrastructure, Capital Finance, and Consumer and Industrial. Due to ambiguity in determining GE's primary industry, data vendors, such as Compustat, assign a non-classifiable SIC code (9997) to GE.[7] Second, imposing requirements for estimating a regression model (e.g., 10 or more observations per *covariate*) leads to substantial sample attrition. Specifically, in the Compustat Global database (1988-2009), the average number of firms per industry is 3.5 (SIC 2), 1.8 (SIC 3), and 1.6 (SIC 4) (Ecker et al. 2013). Lastly, two firms with different business

[6] The SIC and NAICS systems are based on a production-based framework for delineating industries, which was developed by governmental agencies (i.e., Federal Census Bureau). On the other hand, the Fama-French industry classifications were developed by financial academics (Fama and French 1997), aiming to form industry groups that are more likely to share common risk characteristics. Lastly, the GICS structure was the result of collaboration between MSCI and S&P. In assigning firms to certain industries, S&P and MSCI analysts are guided by information from annual reports and financial statements, as well as investment research reports and other industry information. See Bhojraj et al. (2003) for discussion about these industry classification schemes.

[7] Other examples include Simens AG, ABB Ltd, and Seaboard Corporation.

environments could still belong to the same industry (Dopuch et al. 2012). For example, the same two-digit SIC code (35) is assigned to makers of heavy equipment for the oil and gas industry, producers of video games, manufacturers of lawn mowers, and makers of personal computers (Bernard and Skinner 1996).

To address limitations imposed by industry-based estimation samples, Ecker et al. (2013) propose identifying estimation samples based on similarity in firm size. The authors find that estimation samples based on similar firm size perform at least as well as estimation samples based on industry membership in detecting accrual manipulation for both U.S. data and non-U.S. data. The use of size-based estimation samples is especially beneficial to international studies because it mitigates sample attrition. However, despite these advantages of size-based estimation samples, it is not obvious through which mechanisms firm size captures homogeneity in accrual generating processes. Moreover, it is possible that even when firm size captures homogeneity in accrual generating processes, smaller samples have firms that differ in size to a greater extent. Relying on theories on accruals (e.g., Sloan 1996; Fairfield et al. 2003) and firm life cycle (e.g., Liu 2006; Dickinson 2011), this study proposes using life cycle-based estimation samples as more theoretically-grounded alternative estimation samples.

III. THEORETICAL BACKGROUND

In this section, I provide theoretical background on how normal accrual generating processes are likely to be homogeneous within each life cycle stage. Normal accrual generating processes are represented by the association between total accruals and its determinants. In the modified Jones model, the determinants of total accruals include change in cash sales and PP&E. I discuss how the association between total accruals and its determinants vary across life cycle stages below.

3.1. Life Cycle and Normal Accrual Generating Processes

Normal accrual generating processes likely vary across life cycle stages because firms under different life cycle stages have different production capacity, investment opportunity sets, and risk (Park and Chen 2006). For example, consider a firm under two different life cycle stages (e.g., a growth firm and a decline firm). Suppose also that these firms are identical in all respects except their production capacity. For any given shock to sales (e.g., a large increase in sales), a growth firm can generate accruals by further increasing inventory because it has gradually increased its production capacity. A decline firm, however, cannot generate accruals as much as a growth firm because it generally liquidates assets and does not have enough production capacity to accommodate the shock to sales.

Following Dickinson (2011), I classify each firm-year into one of the five life cycle stages (Introduction, Growth, Mature, Shake-out, and Decline) based on the patterns of cash flows from operating, investing, and financing activities.[8] Prior to Dickinson (2011), studies rely on a composite of economic characteristics, such as dividend payout, sales growth, and

[8] The signs (+/−) of cash flows from operating, investing, and financing activities generate the following eight combinations. Dickinson (2011) collapses these eight combinations into five life cycle stages: Introduction, Growth, Mature, Shake-out, and Decline.

Cash Flows	Introduction	Growth	Mature	Shake-out			Decline	
Operating	−	+	+	−	+	+	−	−
Investing	−	−	−	−	+	+	+	+
Financing	+	+	−	−	+	−	+	−

age, and conduct portfolio sorts to draw distinctions between life cycle stages. These sorting methods, however, require an ex ante assumption about the underlying distribution of life cycle membership (i.e., uniform distribution). Dickinson's classification based on the patterns of cash flows reflects the result of firm performance and the allocation of resources, as opposed to an ad hoc assignment.

Firms in the introduction stage (where an innovation is first produced) incur cash *out*flows from operating and investing activities and *in*flows from financing activities because those firms make early large investment funded by external stakeholders (Jovanovic 1982; Spence 1977, 1979; Jensen 1986). Their heavy investment is necessary for developing, introducing, and marketing a new product (Spence 1977, 1979). As a result, investment in operating assets significantly increases firms' working capital accruals (e.g., inventory) in the introduction stage (Liu 2006). Together with operating cash *out*flows, heavy investment in working capital makes the coefficient on change in cash sales larger in the introduction stage. Furthermore, in this stage, firms gradually accumulate fixed assets, but their carrying amount and depreciation expense are small compared to the amounts in the growth and mature stages (Dickinson 2011). Accordingly, the absolute value of the coefficient on PP&E is likely to be smaller in the introduction stage than in the other stages that carry a large amount of depreciation expense.

Firms in the growth stage (where the number of producers increases dramatically) have similar cash flow patterns to introduction firms except for cash *in*flows from operating activities (Spence 1977, 1979; Dickinson 2011). To deal with extremely intense competition, growth firms tend to expand inventory capacity and sell their products on credit, resulting in the increase in working capital accruals (e.g., inventory and accounts receivable) (Dechow et al. 1998; Bushman et al. 2012; Liu 2006). Although growth firms experience the increase in working capital accruals related to sales growth, high operating cash *in*flows could result in the smaller coefficient on change in cash sales in the growth stage than in other stages. Using cash *in*flows generated by operating activities and funded by external stakeholders, these growth firms further expand production capacity (e.g., PP&E) to accommodate increasing customer demand. Dickinson (2011) documents that PP&E (as a main component of total assets) and the corresponding depreciation expense are maximized in this stage. Hence, the absolute value of the coefficient of PP&E is also expected to be maximized in the growth stage.

Firms in the mature stage (where the number of producers reaches a maximum) have operating efficiency through increased knowledge of operations, resulting in cash *in*flows from operating activities (Spence 1977, 1979; Wernerfelt 1985). Mature firms, however, incur cash *out*flows from investing and financing activities due to obsolescence in investment previously made and distribution of excess funds, respectively (Jovanovic 1982; Jensen 1986). Also, mature firms focus on maintaining the market share and current profitability rather than making new investment. Their business strategy therefore aims to improve production processes (e.g., quality control) and to minimize manufacturing costs (Spence 1981; Wernerfelt 1985). However, mature firms are characterized as having a lower level of working capital accruals because they cut investment in short-term operating assets while generating high operating cash *in*flows (Fairfield et al. 2003; Liu 2006). Therefore, the coefficient on change in cash sales could be relatively smaller in the mature stage than in other stages. Furthermore, because mature firms also reduce investment in long-term operating assets (i.e., PP&E) and thus depreciation expense gradually decreases, the absolute value of the coefficient on PP&E may decrease in the mature stage compared to other life cycle stages, especially, the growth stage.

In the shake-out stage, the number of producers begins to decline (Gort and Klepper 1982). Because theory is silent about cash flows for shake-out firms, firms are by default classified as shake-out firms if the cash flow patterns do not fall into one of the other theoretically defined stages (Dickinson 2011) (see footnote 8). Moreover, facing declining profitability, shake-out firms either make new investment to rejuvenate the business or begin downsizing the company. Hence, the magnitudes of the coefficients on change in cash sales and PP&E are indeterminable from either cash flows or investment patterns in the shake-out stage.

Lastly, firms in the decline stage (where there is essentially a zero net entry) experience cash *out*flows from operating activities and cash *in*flows from investing activities due to declining growth rates and liquidation of assets, respectively (Wernerfelt 1985). Furthermore, those firms have positive or negative cash flows from financing activities depending on whether debts are repaid or renegotiated. Despite low investment in working capital, operating cash *out*flows for decline firms make the coefficient on change in cash sales larger in the decline stage than in the other stages with operating cash *in*flows. To cope with negative operating cash flows and low profitability, decline firms engage in liquidation. Liquidation activities (e.g., selling off PP&E, downsizing and undertaking restructuring) involve significant accounting treatments. For example, liquidating firms adjust their book value to reflect liquidation value to avoid assets being overstated or liabilities being understated (Dechow and Ge 2006). Therefore, a small amount of fixed assets and depreciation expense are expected in this stage and the absolute value of the coefficient of PP&E in the decline stage is likely to be smaller than that in other stages (Francis et al. 1996; Liu 2006; Dickinson 2011).

As described, normal accrual generating processes are expected to vary with a firm's life cycle. The following section provides the framework for how life cycle-based estimation samples improve the detection of accrual manipulation.

3.2. Advantages of Estimation Samples by Life Cycle

Researchers use the following linear framework (McNichols and Wilson 1988) in the detection of accrual manipulation.

$$DAP_{it}=\alpha+\beta PART_{it}+\mu_{it}+\varepsilon_{it} \quad (2)$$

where DAP_{it} is a discretionary accrual proxy, $PART_{it}$ is a dummy variable partitioning observations into two groups for which earnings management predictions are specified by the researcher, μ_{it} is other relevant variables influencing discretionary accruals and measurement errors, and ε_{it} is an error term that is independently and identically normally distributed.

Because researchers cannot easily identify other relevant factors influencing discretionary accruals (μ_{it}), the detection model for accrual manipulation typically estimated by the researcher can be represented as

$$DAP_{it}=\hat{a}+\hat{b}PART_{it}+e_{it} \quad (3)$$

By regressing DAP_{it} on $PART_{it}$, researchers essentially aim to (1) explain the variation in DAP_{it} by the variation in $PART_{it}$ and (2) perform a hypothesis test concerning whether the null hypothesis of no earnings management is rejected. The first objective is shown in the R^2 and the second objective is shown in the t-statistic.

$$R^2 = 1 - \frac{\sum_{t=1}^{m}\sum_{i=1}^{n} e_{it}^2}{\sum_{t=1}^{m}\sum_{i=1}^{n}(DAP_{it} - \overline{DAP_{it}})^2} \quad (4)$$

where R^2 is the coefficient of determination (a measure of goodness of fit), e_{it} is an error term, and $\overline{DAP_{it}}$ is the mean for a discretionary accrual proxy.

$$t = \frac{\hat{b}}{SE(\hat{b})} = \frac{\hat{b}\sqrt{(n-1)}s_{PART}}{s_{\varepsilon}} \quad (5)$$

where $\hat{b}$ is the coefficient on $PART_{it}$, n is the total number of observations, s_{ε} is the standard error of the regression, and s_{PART} is the sample standard deviation of $PART_{it}$.

In this framework, life cycle-based estimation samples provide three advantages. First, when estimation samples based on a firm's life cycle are homogeneous, the standard deviation of estimated discretionary accruals becomes smaller. Consequently, life cycle-based estimation samples decrease the denominator in Equation (4) and in turn increase R^2. Second, holding $\hat{b}$ constant, the power of the t-test for earnings management is increasing in the total number of observations (n). Because a firm's life cycle is defined in a way that allows an ample number of firms in each life cycle stage, life cycle-based estimation samples mitigate sample attrition (i.e., an increase in n), increasing the t-statistic in Equation (5). Third, firms in the same life cycle share similar firm characteristics and those firm characteristics are potentially omitted correlated variables in the accrual model. For instance, if other omitted variables (μ_{it}) are positively correlated with $PART_{it}$, then the estimated coefficient on $PART_{it}$ will be biased away from zero (i.e., an increase in Type I errors) (Dechow et al. 1995). Grouping firms by similar firm characteristics likely mitigates this possibility. In addition, by controlling for omitted correlated variables, the standard deviation of the combined impact of other determinants of discretionary accruals becomes smaller (i.e., a decrease in s_ε) and the t-statistic in Equation (5) increases. In this study, I investigate these advantages of life cycle-based estimation samples by performing simulation procedures using the above framework.

IV. SAMPLE, DESCRIPTIVE DATA, AND MODEL ESTIMATION

4.1. Sample

I begin with Compustat firms for the years 1988-2012. The sample period begins in 1988 because cash flows from operating activities become available with the disclosure requirement under Statement of Financial Accounting Standards (SFAS) No. 95. I measure total accruals by subtracting cash flows from operating activities (*oancf*) from earnings before extraordinary items (*ibc*) (i.e., cash flow statement approach).[9] All the input variables in the modified Jones model (i.e., total accruals, change in cash sales and PP&E) are deflated by lagged total assets and are winsorized at 1% and 99%.

Consistent with prior research (e.g., Stubben 2010; Ecker et al. 2013), samples are restricted to non-regulated industries (i.e., non-utilities and non-financial industries) because accruals of regulated industries differ from those of other industries. Next, I exclude firm-year observations with missing values for the input variables in the modified Jones model and observations with the absolute value of total accruals scaled by total assets exceeding one to avoid the influence of outliers (Kothari et al. 2005). Lastly, I require each subgroup, formed by either industry membership, or by life cycle, or by similar size, to have 31 firm-year observations (one event firm-year and 30 non-event firm-years). Discretionary accruals are the regression model-based residual estimates, and a commonly-used rule of thumb in the statistics literature to obtain reasonable power is to use at least 10 observations per covariate (Harrell 2001; Babyak 2004). The modified Jones model has three covariates, including a scaled intercept ($=1/Asset_{t-1}$) and thus I require 30 observations (excluding an event firm-year) for each subsample when estimating accrual models.[10] These screens result in data comprising

[9] The balance sheet approach to measure total accruals involves large measurement errors and as a result, reduces the power in detecting accrual manipulation (Hribar and Collins 2002).

[10] Kothari et al. (2005) suggest that researchers include a scaled intercept in the estimation for several reasons. First, it provides an additional control for heteroscedasticity not alleviated by using assets as the deflator. Second,

134,944 firm-year observations.[11] The total sample consists of 25,965 introduction firms, 37,180 growth firms, 48,426 mature firms, 12,303 shake-out firms, and 11,070 decline firms.

4.2. Descriptive Statistics

Table 2 reports descriptive statistics of key variables by each life cycle stage. The mean total accruals (*TA*) are −0.070, −0.077, −0.083, −0.071, and −0.061 of lagged total assets in the introduction, growth, mature, shake-out, and decline stage, respectively. The negative total accruals suggest that the magnitude of depreciation expense dominates that of working capital accruals. Variation in accruals aside from depreciation expense is manifest in working capital accruals (*WCA*). Consistent with Liu (2006), working capital accruals (*WCA*) are positive in the introduction and the growth stages (0.058; 0.026) and are negative in the mature, shake-out, and decline stages (−0.005; −0.014; −0.002). Table 2 also reports that change in sales (*⊿SALES*) is the highest for growth firms, followed by introduction firms. Similar to change in sales (*⊿SALES*), change in *cash* sales (*⊿SALES−⊿REC*) is highest for growth firms, confirming that life cycle classification based on the patterns of cash flows appropriately identifies the growth stage. The mean of change in cash sales (*⊿SALES−⊿REC*) is positive (negative) in the introduction, growth, mature, and shake-out stage (decline stage), indicating growing (diminishing) cash sales in these stages.

The mean PP&E (*PPE*) also varies across life cycle stages. For example, PP&E (*PPE*) is largest in the growth stage and smallest in the decline stage. Because depreciation expense is an increasing function of PP&E, depreciation expense (*Depr*) is higher in the introduction, growth, and mature stages (0.064, 0.065, and 0.055, respectively) than in the shake-out and decline stages (0.044 and 0.045, respectively). The mean operating cash flows, *OCF*, is negative in the introduction (−0.384) and declining stages (−0.253), and is maximized in the

it mitigates problems stemming from an omitted size (scale) variable. Third, discretionary accrual measures based on models without a scaled intercept are less symmetric, making power of the test comparisons less clear-cut.

[11] Industry-based estimation samples impose a 7% sample loss even by the minimum requirements for estimating the accrual model, resulting in 126,690 firm-years.

mature stage (0.138). Moreover, profitability measured as return on assets (*ROA*) varies across life cycle stages, with the mature stage having the highest *ROA* (0.056) and the introduction stage having the lowest *ROA* (−0.457). Lastly, lagged total assets ($Asset_{t-1}$) are the highest in the growth (1754.77) and mature (2971.50) stages and lowest in the introduction (235.25) stage, indicating that firm size, as measured by lagged total assets, reaches its maximum (minimum) when the firm is in the growth and mature stages (introduction stage). In sum, significant F-statistics reflect that the mean of these key variables differs significantly from one another across life cycle stages.

4.3. Model Estimation by Each Life Cycle Stage

Table 3 Panel A reports that the coefficients (both magnitude and signs) in the modified Jones model vary systematically across life cycle stages. Reported coefficients reflect the association between economic drivers (change in sales, and PP&E) and total accruals across life cycle stages, respectively. Panel A shows that the coefficients are mostly significant, implying that homogeneous estimation samples lead to the estimation of the accrual model with precision and sufficient power (Dopuch et al. 2012).

First, the intercept varies across life cycle stages. Although the intercept is negative in all stages, it is the most negative for mature firms (−0.073) consistent with the lowest total accruals in the mature stage (see Table 2). Also, the least negative intercept (−0.052) in the growth stage reflects heavy investment in working capital made by growth firms (Dechow et al. 1998; Bushman et al. 2012; Liu 2006). Second, the coefficient on $1/Asset_{t-1}$ reflects the association between firm size (measured by lagged assets) and total accruals. The negative coefficients on $1/Asset_{t-1}$ across all stages indicate that total accruals increase as firm size increases. Specifically, the absolute value of the coefficient on $1/Asset_{t-1}$ is larger in the growth, mature, and shake-out stages.

Third, the coefficient on change in cash sales (*ΔSALES−ΔREC*) differs in magnitude across life cycle stages. The magnitude of the coefficient is largest (0.104) for introduction firms consistent with operating cash *out*flows and heavy investment in working capital. It is smallest (0.020; 0.030) for growth and mature firms largely due to the increase in operating cash flows. The coefficients on *ΔSALES−ΔREC* are large in the shake-out and decline stages (0.054; 0.041) consistent with firms in these stages suffering from low/negative operating cash flows.

Fourth, the coefficient on PP&E (*PPE*) differs in both magnitude and signs across life cycle stages. Consistent with high PP&E and depreciation expense in the growth life cycle stage (see Table 2), the absolute value of the coefficient on *PPE* (−0.065) is largest for growth firms. Moreover, the coefficient on *PPE* (−0.054) has the second largest absolute value in the introduction stage, potentially due to accelerated depreciation by introduction firms. In contrast, the absolute value is smaller for shake-out and decline firms (0.005; −0.010), implying that these firms liquidate their assets and thus report a small amount of depreciation expense. Lastly, the R^2 and adjusted R^2 are higher in the growth (0.231; 0.142) and mature (0.246; 0.159) stages than in the shake-out (0.208; 0.117) and decline (0.200; 0.108) stages. The highest concentration of firms and goodness of fit in the growth and mature stages suggest that higher detection power is expected for firms in these stages.[12]

Table 3 Panel B presents descriptive statistics of estimated discretionary accruals. By construction, discretionary accruals are close to zero in all life cycle stages. The standard deviation of discretionary accruals is lower for growth (0.105) and mature (0.108) firms and higher for introduction (0.238) and decline (0.185) firms. Given that the R^2 and adjusted R^2 are also higher for growth and mature firms, normal accrual generating processes in the growth and mature stages can be considered more homogeneous than those in the other stages.

[12] Detection power by each life cycle stage is examined in Section 5.2.

Conversely, the higher standard deviation in the introduction and decline stages indicates that firms in the early or late life cycle stage are more heterogeneous from each other.

Table 3 Panel C reports Wald χ^2-statistics, computed for testing whether coefficients are statistically different across life cycle stages. Significant χ^2-statistics indicate that the coefficient on change in cash sales (*⊿SALES−⊿REC*) or on PP&E (*PPE*) in one life cycle stage is statistically different from the coefficient in another stage. Panel C shows that the coefficients on *⊿SALES−⊿REC* and *PPE* vary significantly from the introduction stage to the growth, mature, shake-out, or decline stage, from the growth to the mature, shake-out, or decline stage, and from the mature to the shake-out stage (and vice versa). For example, the coefficient on change in cash sales for introduction firms (0.104) is significantly different from that for growth firms (0.020) (χ^2=278.37). However, neither the coefficient on change in cash sales or the coefficient on PP&E is significantly different between shake-out and decline firms (χ^2 = 1.81 and 1.19). In summary, significant variation in the coefficients across life cycle stages reflects homogeneity in normal accrual generating processes within each life cycle stage.

V. SIMULATION PROCEDURES AND RESULTS

5.1. Simulation Procedures

In this section, I perform simulation procedures similar to Ecker et al. (2013) to evaluate the specification and power of tests for detecting accrual manipulation by different identifications of estimation samples.[13] The specification test captures Type I errors under a true null hypothesis of no earnings management and thus, I do not induce earnings manipulation (i.e., 0% manipulation). For the power test, I seed earnings manipulation of known magnitudes because the test is based on the presumption that a null hypothesis of no earnings management is false. Therefore, lower rejection rates (2-8%) are preferred in the specification test, and higher rejection rates are directly interpreted as higher detection power in the power test.[14] The following simulation procedures (Steps (1)-(5)) are repeated 100 times.

(1) I select 500 event firm-years and match each event firm-year with 30 peer firm-years. Accordingly, each subsample consists of 31 observations, one event firm-year and 30 peer firm-years. The number of observations remains constant throughout the samples to equalize the power of tests, which is affected by sample size (Ecker et al. 2013). The selection criterion for 30 peer firm-years varies across different identifications of estimation samples. For industry-based estimation samples, those peer firm-years are randomly selected from the same industry (2-digit SIC code) and year as the event firm-year. For size-based estimation samples, 30 peer firm-years are

[13] There are three differences between the simulation procedures used by Ecker et al. (2013) and those used by the current study. First, Ecker et al. measure total accruals using the balance sheet approach while this study employs the cash flow statement approach (i.e., measures total accruals by subtracting cash flows from operating activities (*oancf*) from earnings before extraordinary items (*ibc*)). Second, Ecker et al. assume expense manipulation while the current study assumes 50% revenue manipulation following Kothari et al. (2005). Third, Ecker et al. require 11 observations (one event firm-year and 10 non-event firm-year) for each subsample while this study requires 31 observations (one event firm-year and 30 non-event firm-year). The statistics literature recommends researchers use at least 10 or more observations *per covariate* in each regression to obtain precise parameter estimates (Harrell 2001; Babyak 2004). Because the modified Jones model has three covariates (including $1/Asset_{t-1}$), I require 30 observations (non-event firm-years) for each regression.

[14] The 95% confidence interval for the rejection rate of 5% ranges from 2% to 8%. If the actual rejection rate falls below (above) 2% (8%), the test is misspecified as it rejects too infrequently (frequently), and is biased in favor of (against) the null hypothesis (Kothari et al. 2005).

defined as observations that are closest in size (lagged total assets) to the event firm-year in the same year. For life cycle-based estimation samples, peer firm-years are randomly selected from the same life cycle and year as the event firm-year.

(2) I set a partitioning variable PART to 1 for each event firm-year and to 0 for 30 peer firm-years.

(3) For each event firm-year with PART set to 1, I artificially induce accrual manipulation of known magnitudes (i.e., 0-20% of total assets with an increment of 2%) to total accruals.[15] Assuming that a part of accrual manipulation occurs through revenue manipulation, I also introduce the corresponding revenue manipulation (i.e., 0-10% of total assets with an increment of 1%) to change in cash sales of the event firm-year (i.e., 50% revenue manipulation).

(4) In each sample, I estimate the modified accrual model using 30 peer firm-year observations and apply the estimated coefficients to calculate discretionary accruals.

(5) I then regress the estimated discretionary accruals proxy (*DAP*) on *PART* (i.e., $DAP_{it}=\hat{a}+\hat{b}PART_{it}+e_{it}$). In the specification and power tests, I record the percentage of 500 samples where the null hypothesis of non-positive discretionary accruals is rejected at the 5% significance level of one-tailed tests.[16] For the power tests, rejection rates directly reflect detection power.

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[15] Some may argue that assuming 2-20% of total assets as seeded accrual manipulation is unrealistic, although this assumption is consistent with prior literature (Kothari et al. 2005; Ecker et al. 2013). However, when the number of observations (e.g., N=31) in each cross-sectional subgroup is small, it is not easy to detect even large magnitudes of accrual manipulation. When the number of observations increases, detection power naturally improves because the standard deviation of the mean discretionary accruals becomes smaller. Thus, large magnitudes of accrual manipulation in this study are to show detection power in the context where the power is *ex ante* considerably low. In addition, Wu et al. (2010) report a standard deviation (SD) of discretionary accruals (scaled by assets) of 0.10 in a broad sample spanning 1970-2007. Similarly, Klein (2002) reports a corresponding SD of 0.19 in 692 U.S. firm-years during 1992-1993. That is, approximately 68% (32%) of discretionary accruals are less (greater) than one standard deviation (0.10; 0.19) away from zero, providing the support for the range of accrual manipulation in this study.

[16] This study focuses on upward earnings management (i.e., the null hypothesis of non-positive discretionary accruals) because upward earnings management is far more common than downward earnings management (Kinney and Martin 1994; Dechow et al. 2011).

Steps (1)-(5) describe 1 out of 100 iterations. Consequently, the resulting entire sample is comprised of 50,000 event firm-years, each matched with 30 peer firm-years.[17] When testing the specification and power in smaller samples, I randomly select 500, 1000, 2000, 5,000, 10,000 and 15,000 observations from the population (N=134,944) before Steps (1)-(5). I then follow the same procedures (100 iterations for Steps (1)-(5)) with one exception: In Step (1), I select 50 event firm-years (instead of 500 event firm-years) to avoid using the same firm-year as either an event firm-year or a non-event firm-year multiple times.

5.2. Simulation Results-Sample by Each Life Cycle Stage

In this section, I investigate whether the specification and power of tests for detecting accrual manipulation vary across life cycle stages. The detection of accrual manipulation could differ across life cycle stages because the ability of a firm's life cycle to capture homogeneity in normal accrual generating processes changes across life cycle stages. Table 4 presents the simulation results by each life cycle stage under the null hypothesis of non-positive discretionary accruals. For test specification, I consider rejection rates ranging from 2 to 8% to be an indication of well-specified tests. Panel A of Table 4 shows that when there is no manipulation (i.e., column of 0% manipulation), life cycle-based estimation samples generate well-specified tests in each life cycle stage with rejection rates ranging from 5.24 to 7.32%. Panel A also reports that detection power is highest among growth and mature firms and lowest among introduction and decline firms, consistent with firms in the early or late life cycle stage being more heterogeneous from each other. Also, the gap in detection power between growth/mature firms and introduction/shake-out/decline firms becomes larger as the manipulation level increases. For instance, at 2% manipulation, all life cycle stages show

[17] Following Ecker et al. (2013), I analyze detection rates at the subsample level (50,000 subsamples each consisting of one event firm-year and 30 non-event firms-years instead of 100 samples each consisting of 500 event firm-years and 15,000 non-event firm-years). At the sample level, detection rates will approach 100% even at small seeded discretionary accruals levels quickly for all models because firm-specific idiosyncrasies are averaged out.

comparable levels of rejection rates while at 12% manipulation, the growth and mature stages show significantly higher rejection rates (42.22; 46.76) than the introduction, shake-out and decline stages (20.22; 21.61; 15.15).

Life cycle-based identification of estimation samples offers finer partitions where such partitions are determined by life cycle persistence. Panel B of Table 4 presents the results of the specification and power tests by life cycle-based estimation samples when samples are restricted to firms whose life cycle stage remains unchanged for 2 or 3 consecutive years. For comparison, the simulation results on life cycle-based estimation in a large U.S. sample (N=134,944) are also provided (Life Cycle-Year). A sample of two-year persistence includes firms that remain in their initial life cycle stage in year t+1. Similarly, a sample of three-year persistence includes firms that remain in their initial life cycle stage in year t+1 and t+2. When samples are restricted to firms with the same life cycle for 2 (3) consecutive years, detection power improves by 14% (26%) on average compared to when there is no sample restriction by life cycle persistence.[18] For example, at 10% seeded manipulation, the rejection rate increases from 27.80% to 32.59% (37.31%) by restricting to samples with two-year (three-year) life cycle persistence. This improvement in detection power suggests that life cycle transitions indeed capture significant changes in normal accrual generating processes. Overall, results by each life cycle stage and on life cycle persistence show that detection power varies across life cycle stages, consistent with different degrees of homogeneity in normal accrual generating processes in each life cycle stage.

5.3. Simulation Results-Large U.S. Sample

In this section, I compare the performance of life cycle-based estimation samples to that of industry-based, size-based or two other alternative estimation samples. I define two other

[18] To gauge the average improvement in detection power, I first calculate the improvement in detection power (divided by the original rejection rate) at each manipulation level and then take the average of all the improvements across 2-20% manipulation levels.

alternative estimation samples with respect to how peer firm-years are selected: (1) "Whole Sample" estimation samples whereby peer firm-years are selected from the entire population and (2) "Year" estimation samples whereby peer firm-years are selected from the same fiscal year as the event firm-year.

Table 5 presents the simulation results for a large U.S. sample on the test specification and power by different identifications of estimation samples under the null hypothesis of non-positive discretionary accruals. Panel A of Table 5 reports that, regardless of which identification of estimation samples is used, the modified Jones model yields rejection rates ranging from 5.22 to 6.61% when no accrual manipulation is seeded. That is, all identifications of estimation samples yield well-specified tests for detecting accrual manipulation in a large U.S. sample. Panel A also reports the detection power of different estimation samples. First, two other alternative estimation samples ("Whole Sample" and "Year") generate considerably lower detection power than industry-based, size-based, and life cycle-based estimation samples at 6-20% manipulation. These results indicate that the use of finer cross-sectional partitions (e.g., by industry) is more beneficial in the detection of accrual manipulation. Second, at 2-6% manipulation, industry-based, size-based, and life cycle-based estimation samples yield comparable levels of detection power for accrual-based earnings manipulation. However, at larger magnitudes of manipulation (8-20%), life cycle-based and size-based estimation samples outperform industry-based estimation samples in detection power. For example, at 12% manipulation, the modified Jones model estimated by life cycle and year (by size and year) yields the rejection rate of 34.94% (34.91%) while the same model estimated by industry and year generates 28.70% rejection rate (i.e., approximately 20% improvement in detection power). Last, Panel A of Table 5 shows that there is a small difference between selecting peer firms based on an absolute cut-off, such as quintiles (i.e., absolute peer group approach) and selecting the event firm's 30 closest-in-size neighbors (i.e., relative peer group approach) while the latter still performs better than the former.[19]

I further investigate whether combining life cycle-based identification of estimation samples with other identifications can improve detection power. For this analysis, I select an event firm-year from each life cycle stage and match the event firm-year with its peers from the same industry or similar size groups.[20] Panel B of Table 5 reports the simulation results on these combined identifications of estimation samples. When the event firm-year is defined based on its life cycle stage, selecting peers from the same industry and year generates (1) a great degree of misspecification (Type I error) at 0% manipulation for introduction and decline firms, (2) significantly lower detection power for growth and mature firms, and (3) moderate improvement in detection power for shake-out firms compared to when estimation samples are identified by life cycle alone (see also Table 4 Panel A). The results for size peers in Panel C of Table 5 are similar. Overall, the combined identifications do not provide much improvement in the detection of accrual manipulation relative to a stand-alone identification of estimation samples by life cycle.

5.4. Simulation Results-Small U.S. Sample

Smaller samples face the problem of substantial sample attrition and require higher levels of homogeneity in accrual generating processes within estimation samples. Due to these unique characteristics of smaller samples, the results for a large sample may not hold in smaller samples.

To investigate the specification and power by different identifications of estimation samples in smaller samples, I randomly select 500, 1,000, 2,000, 5,000, 10,000 and 15,000

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[19] Ecker et al. (2013) emphasize two disadvantages of the absolute approach compared to the relative peer group approach. First, the absolute approach does not ensure symmetry in the selection of peer firms. Second, the absolute peer group approach requires the full cross-section of firms to determine the initial partitions.

[20] When selecting industry peers or size peers of each event firm-year, such peers are not necessarily from the same life cycle stage as the event firm-year.

observations from a large U.S. sample (N=134,944) as a representation of smaller samples. I begin with 500 observations because life cycle-based estimation samples (without year classification) impose no sample loss in sample size of 500 and above. Also, life cycle-based estimation samples (with year classification) no longer suffer from sample attrition in sample size of 15,000 and above. Thus, I estimate the modified Jones model by life cycle (without year classification) when sample size is between 500 and 15,000, and estimate the model by life cycle and year when sample size is 15,000 firm-year observations and above. The choice of life cycle (without year classification) or life cycle and year classification based on the sample size enables comparison between life cycle-based estimation samples and size-based estimation samples, free from sample attrition issues.[21] I also include two other alternative estimation samples ("Whole Sample" and "Year") because these estimation samples are considered as substitutes for industry-based, size-based, and life cycle-based estimation samples especially when sample size is small because of their "less strict data availability requirements" for a firm to be included for analysis.[22]

Table 6 reports that in smaller samples, all estimation samples generate well-specified tests for detecting accrual manipulation with rejection rates ranging from 4.36% to 7.96%. Also, Table 6 shows that rejection rates from "Whole Sample" and "Year" estimation samples are significantly lower than those from industry-based, size-based, and life cycle-based estimation samples. For instance, when sample size is 15,000, life cycle-based estimation samples

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[21] Estimation samples by similar size and year (where similar size peers are defined as 30 closest-in-size neighbors) impose no sample loss if it satisfies the number of observations in each year required for estimating the accrual model. Also, estimation samples by life cycle (without year classification) impose no sample loss in smaller samples. Thus, it is unclear which one (estimation samples by size vs. estimation samples by life cycle or estimation samples by size and year vs. estimation samples by life cycle) is an apples to apples comparison in smaller samples. However, if estimation samples by life cycle (in sample size of 500 to 15,000) outperform both (1) estimation samples by size and (2) estimation samples by size and year in detection power, life cycle-based estimation samples can be considered superior to size-based estimation samples.

[22] For "Year" estimation samples, a sample size of 500 observations is excluded from the analysis because the number of observations is less than 31 firm-years for each fiscal year. Note that, in this study, the sample period spans 25 years (from 1988 to 2012). Therefore, at least 775 (=31×25 years) observations are required for "Year" estimation samples.

improve detection power by approximately 31% (29%) on average compared to “Whole Sample” (“Year”) estimation samples. Life cycle-based estimation samples are also superior to industry-based estimation samples in sample retention and detection power. Industry-based estimation samples impose sample attrition in all smaller samples and generate at least 5% lower rejection rates than life cycle-based estimation samples at 10-20% manipulation. Similarly, life cycle-based estimation samples outperform both size and size-year estimation samples in detection power, consistent with smaller samples having firms that differ in size to a greater extent. More specifically, when sample size is 1,000, life cycle-based estimation samples generate an 11.54% rejection rate while size (size-year) estimation samples yield an 8.62% (9.88%) rejection rate at 4% manipulation. The gap in detection power between life cycle-based and size-based estimation samples increases when the seeded manipulation increases. For example, at 12% seeded manipulation, the rejection rate by life cycle-based estimation samples is 36.82% while the rejection rate by size-based estimation samples is only 27.74% when sample size is 1,000. Similar patterns are observed in other smaller samples. Hence, the advantages of using life cycle-based estimation samples over other available estimation samples (especially, size-based estimation samples) are maximized in smaller samples.

5.5. Simulation Results-International Sample

In this section, I evaluate sample attrition issues in international settings, using Compustat Global. I then select a comprehensive list of countries of varying sample sizes and perform simulation procedures within each country to compare the performance of different estimation samples.

5.5.1. Sample

To evaluate sample attrition issues in international settings, I begin with all Compustat Global firms (359,599 obs.; 109 countries) for the years 1988-2012. Minimum data requirements to calculate the input variables in the modified Jones model result in 242,733 firm-year observations and 107 countries. The statistics literature (Harrell 2001; Babyak 2004) suggests that 10 or more observations per covariate are required for each regression and thus 30 observations are necessary for estimating the modified Jones model. When researchers require 30 or more observations for each industry and year in Compustat Global, 52.43% of firm-year observations and approximately 77% of countries are excluded from the sample. Even when a more generous requirement (10 or more observations) is imposed, industry-based estimation samples suffer from significant sample attrition (i.e., 28.52% of reduction in firm-year observations; 62 countries (out of 107) are removed from the sample). On the other hand, if 30 or more observations are required for each life cycle and year or size quintile and year, only approximately 12% of firm-year observations are eliminated from the sample and 20 *more* countries can remain in the sample relative to when the same requirement is imposed for each industry and year. Furthermore, estimation samples comprised of closest-in-size neighbors in the same year result in the least sample attrition because such samples only require sufficient observations for each year. Overall, life cycle-based estimation samples mitigate the problem of sample attrition to a significant extent.

As another test of the efficacy of different identifications of estimation samples in smaller samples, I use international settings. I first select a comprehensive list of countries of varying sample sizes.[23] The same sample filtering procedures in Section 4.1 are employed for each country-level analysis. For countries with multiple currencies, I only include observations whose values are stated in the country's main currency. Also, I require 31 observations (one

[23] Specifically, I investigate the following 22 countries/territories (country/territory name; currency): Australia (AUS; AUD), Bermuda (BMU; HKD), Brazil (BRA; BRL), Cayman Islands (CYM; HKD), Chile (CHL; CLP), China (CHN; CNY), Denmark (DEU; EUR), France (FRA; EUR), India (IND; INR), Japan (JPN; JPY), Korea (KOR; KRW), Malaysia (MYS; MYR), Norway (NOR; NOK), Pakistan (PAK; PKR), Philippines (PHL; PHP), Poland (POL; PLN), Russia (RUS; RUB), Singapore (SGP; SGD), Sweden (SWE; SEK), Taiwan (TWN; TWD), Thailand (THA; THB), and United Kingdom (GBR; GBP). Due to limited space, I present the simulation results for eight countries (Australia, Brazil, China, India, Philippines, Poland, Russia, and Singapore) only. Inferences based on the results for the remaining countries are similar to those reported in the paper.

event firm-year and 30 peer firm-years) for each of the estimation samples and this requirement leads to different sample sizes across three estimation samples. Although different estimation samples impose different sample sizes, the number of observations (31 observations) remains constant throughout each subgroup to equalize the power of tests. The simulation procedures described in Section 5.1 are used with one exception: I select 50 event firm-years (instead of 500 event firm-years) to avoid using the same firm-year as either an event firm-year or a non-event firm-year.

5.5.2. Simulation Results-International Sample

Table 8 reports the simulation results for eight countries (i.e., Australia, Brazil, China, India, Philippines, Poland, Russia, and Singapore) on the specification and power under the null hypothesis of non-positive discretionary accruals. At 0% manipulation, rejection rates are within a range of 2-8% (i.e., a threshold for well-specified tests) with exceptions in two countries. Specifically, industry-based estimation samples yield misspecified tests with rejection rates of 9.12% and 9.54% for Poland and Russia, respectively. To compare the performance of different estimation samples aside from sample attrition issues, I estimate the modified Jones model either by life cycle (without year classification) or by life cycle and year depending on sample size.

Table 8 also presents varying degrees of detection power across different identifications of estimation samples. Detection power for accrual manipulation improves when the modified Jones model is estimated by life cycle (or life cycle and year). For instance, at 6% manipulation, life cycle-based estimation samples generate the rejection rate of 19.62% while industry-based and size-based estimation samples produce 13.18-14.98% rejection rates in Brazil. Similarly, at 12% manipulation, life cycle-based estimation samples improve detection power by 33% (35%; 49%) compared to industry (size; size-year)-based estimation samples in Philippines. Similar patterns are observed in other countries. Moreover, improvement in detection power is maximized when the level of seeded manipulation increases. In particular, when the largest manipulation level is assumed, life cycle-based estimation samples yield 15.76% (10.68%; 8.76%) higher rejection than industry (size; size-year)-based estimation samples in Poland. As in smaller U.S. samples, size-based estimation samples show only a moderate degree of improvement in detection power compared to industry-based estimation samples. In international settings, the number of observations available for the analysis is relatively small and thereby smaller samples have firms that are significantly different in firm size, leading to the underperformance of size-based estimation samples. Lastly, detection power varies across countries. Specifically, detection power in China is twice as high as detection power in Australia at 4-20% manipulation levels. A large difference in detection power among different countries indicates that normal accrual generating processes captured by the modified Jones model also vary across countries, potentially due to institutional features that differ in each country. Overall, analysis using international data provides additional support for the use of life cycle-based estimation samples over industry-based or size-based estimation samples when sample size is small.

5.6. Simulation Results-Extreme Operating Performance Sample

Prior studies (Dechow et al. 1995; Kothari et al. 2005; Stubben 2010; Dechow et al. 2012) document that accrual models are significantly misspecified for extreme performance samples. That is, in extreme performance samples, accruals are highly likely to be classified as discretionary when they represent fundamental performance (i.e., Type I errors). Given that firms in the same life cycle exhibit comparable levels of firm characteristics (e.g., operating cash flows (*OCF*) and return on assets (*ROA*)) and have similar earnings manipulation incentives, life cycle-based estimation is expected to reduce misspecification in extreme performance samples.

Table 9 presents the specification of the modified Jones model estimated by industry and year, by size and year, or by life cycle and year in extreme operating performance samples. The simulation procedures remain unchanged except that an event firm-year is randomly selected from extreme performance samples (i.e., highest (lowest) quintile of operating cash flows/return on assets). Panel A of Table 9 reports that industry-based and size-based estimation samples generate rejection rates of 19.22 and 13.22%, respectively, for firms with low operating cash flows (*OCF*) while life cycle-based estimation samples reduce the rejection rate to 8.54%. When operating cash flows (*OCF*) are high, size-based estimation samples yield slightly lower rejection of 1.88% than the specified test levels (i.e., 2-8%). Both industry-based and life cycle-based estimation samples generate well-specified tests for firms with high operating cash flows (*OCF*).[24] Panel B of Table 9 presents the simulation results for samples with high/low return on assets (*ROA*). When return on assets (*ROA*) is low, industry-based estimation samples generate the rejection rate of 9.08% whereas life cycle-based and size-based estimation samples yield well-specified tests (4.42 and 3.02%, respectively) for detecting accrual manipulation. Panel B also shows that all three estimation samples, however, generate misspecified tests when an event firm-year is selected from the highest quintile of *ROA*. Together, these findings suggest that life cycle-based estimation mitigates misspecification (Type I error) in extreme operating performance samples.

5.7. Simulation Results-AAER Sample

5.7.1. Sample

In this section, I compare the power of industry-based, size-based, and life cycle-based estimation samples in detecting *actual* earnings manipulation. The U.S. Securities and Exchange Commission (SEC) has published details of financial reporting related enforcement

[24] Type I and Type II errors are trade-offs. Hence, too low rejection rates (low Type I errors) are problematic because such rates imply low power (high Type II errors) in the detection of accrual manipulation.

actions in a series of Accounting and Auditing Enforcement Releases (AAERs) since 1982.[25] The AAERs database primarily includes enforcement actions against firms in which the SEC alleges that earnings manipulation has taken place (Dechow et al. 2011). Therefore, I employ those enforcement actions in the AAERs database as a sample of actual earnings manipulation. Among many other advantages, a primary advantage of using this database is that researchers do not need to assume the magnitude and channels of earnings manipulation because the SEC identifies a group of economically significant manipulations from various sources (Dechow et al. 2011). Accordingly, the use of the AAERs sample can avoid potential biases induced by researchers' individual earnings manipulation classification schemes (Dechow et al. 2011). The AAERs, however, do not capture earnings management occurring within GAAP because the SEC mainly pursues accounting misstatement cases involving GAAP violations.[26]

I use several filters to classify the SEC's enforcement actions as earnings manipulation firm-years. I begin with 1,105 enforcement actions. First, I restrict analysis to the AAERs where actions are brought against firms pursuant to Section 13(a) of the Securities Exchange Act of 1934.[27] This restriction excludes enforcement actions against firms with wrongdoing unrelated to financial misstatements (e.g., bribes). Second, I require each investigation to be related to annual filings because I use annual data to estimate discretionary accruals, a proxy for earnings manipulation. Third, I require firms to be a respondent in any of the regulatory proceedings associated with the enforcement action. By doing so, I exclude enforcement actions brought against auditors or employees. In addition, I exclude enforcement actions if they occur in regulated industries or in relation to firms headquartered outside North America.

[25] The 2012 version of the AAERs database is kindly provided by Gerald S. Martin.

[26] Firms can manipulate earnings within the rules of GAAP in many ways. For example, some firms offset one-time gains from big asset sales with restructuring charges (to keep earnings from rising so high that they can't be topped the following year) or time asset purchases and sales to produce gains when needed (Smith et al. 1994).

[27] Section 13(a) requires issuers whose securities are registered with the SEC to file reports as required by the SEC's rules and regulations. The financial statements contained in the filings are required to comply with Regulation S-X, which in turn requires conformity with Generally Accepted Accounting Principles (GAAP).

The above filtering procedures generate 586 enforcement actions, in which firms are alleged to manage earnings. Lastly, I convert 586 enforcement actions into earnings manipulation firm-years. The final AAERs sample consists of 928 earnings manipulation firm-years.[28]

5.7.2. Simulation Results-Accounting and Auditing Enforcement Releases (AAERs) Sample

To evaluate detection power for actual accrual manipulation by different identifications of estimation samples, I employ the simulation procedures described in Section 5.1. In the simulations, I select 50 event firm-years from 928 observations in the AAERs sample and match each event firm-year with its 30 peers defined by the same industry and year, or by similar size in the same year, or by the same life cycle and year. For the rest of the procedures, I follow Step (2), (4), and (5) in Section 5.1 and repeat these Steps 100 times.[29]

Panel A of Table 10 reports the results for detection power by industry-based, size-based, and life cycle-based estimation samples. Because a null hypothesis of no earnings management is false in the AAERs sample, high (low) rejection rates can be directly interpreted as high (low) detection power. Panel A shows that while industry-based and size-based estimation samples generate comparable levels of rejection rates (13% and 14%), life cycle-based estimation samples yield the highest rejection rate, 19%. In other words, detection power for actual accrual manipulation is 46% (36%) higher when the accrual model is estimated by life cycle and year than by industry and year (by size and year), reflecting that life cycle-based estimation samples provide better predictions for actual earnings manipulation compared to existing estimation samples.

I further investigate how the detection power of life cycle-based estimation samples varies across life cycle stages. Panel B of Table 10 presents the simulation results by each life

[28] Some enforcement actions occur over multiple fiscal periods. For example, Sunbeam Corporation was alleged to employ improper earnings management techniques (e.g., "cookie jar" reserves) for the last quarter of 1996 until June 1998. Due to the enforcement actions whose violation periods span multiple fiscal years, 586 enforcement actions are converted to 928 firm-years.

[29] For the AAERs analysis, annual Compustat data are pulled using the *DATAFMT=STD* flag. The use of this flag is to ensure that the original "as reported" and unrestated data are employed.

cycle stage. Among 928 earnings manipulation firm-years, 230 (339, 230, 70, and 59) firm-years belong to the introduction stage (growth, mature, shake-out, and decline stage, respectively). The distribution of AAERs firm-years across life cycle stages is different from that of a large U.S. sample (N=134,944). Specifically, growth firms make up the largest portion of AAERs firm-year observations (36.53%), followed by introduction and mature firms (24.78%). Panel B of Table 10 also shows that detection power is highest for introduction firms. The mean total accruals (*TA*) and working capital accruals (*WCA*) indicate that introduction firms in the AAERs sample have extremely higher levels of *TA* (0.017) and *WCA* (0.123) than those in a large U.S. sample. Therefore, higher detection power for introduction firms implies that these firms engage in larger magnitude of manipulation when they *actually* manipulate earnings.[30]

Finally, I investigate whether combining life cycle-based identification of estimation samples with other identifications improves detection power. I select an event firm-year from each life cycle stage and match it with its industry peers (i.e., the same industry and year) or size peers (i.e., closest-in-size neighbors in the same year). Panel C of Table 10 presents the results for these combined identifications. While industry-based estimation samples improve detection power for earnings manipulation by decline firms, life cycle-based estimation samples alone generate higher detection power for firms in the other stages. Collectively, Table 10 provides evidence for the advantages of using life cycle-based estimation samples over existing estimation samples in detecting *actual* earnings manipulation.

[30] The AAERs database (provided by Gerald S. Martin) does not include the amount of earnings (or other income statement items) that is manipulated by a company. Nevertheless, Panel B of Table 10 provides a general idea of the magnitude of manipulation by the AAER sample companies in each life cycle stage.

VI. REEXAMINATION OF PRIOR EARNINGS MANAGEMENT STUDIES

6.1. Reexamination of Dechow, Richardson, and Tuna (2003)

Dechow, Richardson, and Tuna (DRT) (2003) examine whether boosting discretionary accruals to avoid reporting a loss is the reason for the kink in the earnings distribution (i.e., too few firms report small losses, too many firms report small profits). DRT find that discretionary accruals of small profit firms are statistically *indifferent* from those of small loss firms, inconsistent with the kink being caused by earnings management. They however caution that their lack of findings could be attributed to low power (i.e., the accrual models lack power to detect earnings management). Therefore, I investigate whether life cycle-based estimation samples change the inferences from DRT. In doing so, I first replicate sections of DRT and then estimate the accrual models; namely, the lagged discretionary accrual model and forward-looking discretionary accrual model, by life cycle and year.[31] DRT estimate these accrual models by industry (2 digit SIC) and year, requiring 10 or more observations for each industry and year.

Panel A of Table 11 presents the replication results for DRT. Consistent with their findings, discretionary accruals using the lagged discretionary accrual model (forward-looking discretionary accrual model) for small profit firms are 0.022 (0.024) and insignificantly different from 0.018 (0.017) reported for the small loss group (p=0.173; p=0.332). Panel B of Table 11 reports the results by using life cycle-based estimation samples. The number of observations (N) doubles in Panel B because regression requirements (10 or more observations) are more easily met for life cycle-based estimation samples than for industry-based estimation samples. Compared to industry-based estimation samples, I find that life cycle-based

[31] Both the lagged discretionary accrual model and forward-looking discretionary accrual model are "adjusted" modified Jones models. Specifically, both models make an adjustment for the expected increase in credit sales. In addition, the lagged discretionary accrual model includes the lagged value of total accruals in the modified Jones model to capture predictable accruals. Finally, the forward-looking discretionary accrual model further adds future sales growth to the lagged discretionary accrual model.

estimation samples generate lower discretionary accruals (0.014; 0.015) for small loss firms under both models, increasing the difference in discretionary accruals between small profit firms and small loss firms (p=0.029; p=0.052). The significant difference in discretionary accruals indicates that the kink in the earnings distribution is caused by earnings management. To test whether the results (Table 11 Panel B) are influenced only by observations that are added due to milder data requirements in life cycle-based estimation, I conduct the same analyses using the restrictive data in Panel A. In Panel C, I show that using the restrictive data does not change the results, i.e., each accrual model estimated by life cycle and year generates lower discretionary accruals for small loss firms (p=0.037; p=0.050). In sum, these results imply that DRT's lack of findings is indeed driven by the low power of the tests and an increase in the number of observations is one of the contributors to the improvement in test power.

6.2. Reexamination of Teoh, Wong, and Rao (1998)

Teoh, Wong, and Rao (TWR) (1998) examine whether initial public offering (IPO) firms have high positive issue-year earnings and discretionary accruals. Using industry-based estimation samples, TWR find evidence that IPO firms report high earnings during the IPO by reporting discretionary accruals aggressively. To gauge the extent to which their results are driven by the misspecification of accrual models in the IPO setting, I estimate discretionary accruals for a sample of firms making initial public offers using life cycle-based estimation samples.

I first replicate TWR using their sample period (1980-1990). I estimate the modified Jones model by industry (2 digit SIC) and year, and the replication results are consistent with the findings of TWR. Specifically, the replication results indicate that IPO firms report significantly higher discretionary accruals (Mean=10.846% of lagged assets) at the time of the IPO (Year 0) and Year 1 and lower discretionary accruals in subsequent years (Year 3, 4, 5, and 6). I then estimate the same accrual model using industry-based estimation samples but changing the sample period to 1988-2012.[32] Consistent with higher discretionary accruals at Year 0 and 1 in TWR, I find that discretionary accruals are significantly higher at Year 0 and 1 for firms making initial public offers between 1988 and 2012 (Table 12 Panel B).

Lastly, I estimate the modified Jones model using the sample period 1988-2012 and life cycle-based estimation samples to investigate whether life cycle-based estimation samples mitigate misspecification in the IPO setting. Table 12 Panel C indicates that the mean discretionary accruals (2.765% in Year 0 and 1.425% of lagged assets in Year 1) by life cycle-based estimation samples are significantly lower than those by industry-based estimation samples (4.718% in Year 0 and 2.253% of lagged assets in Year 1). In sum, these findings indicate that life cycle-based estimation samples not only increase power of the test but also attenuate accrual model misspecification.

[32] The life cycle measure used in this study is based on cash flow information, which becomes available after 1987. Because I compare the performance of life cycle-based estimation samples to that of industry-based estimation samples, I use the sample period from 1988 to 2012 in Table 12 Panel B rather than 1980-1990 in TWR. Also, I employ the balance sheet approach to measure total accruals across Table 12 Panel A, B, and C to alleviate the concern that the results arise from different measurements (balance sheet approach vs. cash flow statement approach) of total accruals.

VII. SUPPLEMENTAL ANALYSES

7.1. Alternative Accrual Models

This study compares different estimation samples using the modified Jones model. To investigate whether the tenor of results remain unchanged when using other accrual models, I employ two additional models, the Jones model and Dechow-Dichev model (hereafter, DD model). The Jones model includes change in sales and PP&E as the determinants of total accruals while the DD model includes past, present, and future operating cash flows as the determinants of working capital accruals. Both models are originally estimated using time-series data, but due to strict data requirements imposed by firm-specific estimation, researchers commonly estimate these accrual models cross-sectionally. I estimate both models cross-sectionally using life cycle-based estimation samples and compare the performance of life cycle-based estimation samples to that of other existing estimation samples. In particular, the DD model is of interest because both a life cycle measure and the DD model utilize cash flow information. Untabulated results indicate that the Jones model provides almost equivalent detection power to the modified Jones model. I also find that the DD model estimated by the life cycle-based method generates a marginally higher rejection rate (8.17%) at 0% manipulation, falling into a range of misspecification (less than 2% or greater than 8%). Other than slight misspecification at 0% manipulation, life cycle-based estimation samples provide higher detection power at 2-20% manipulation than industry-based or size-based estimation samples when the DD model is used. These findings suggest that the superiority of life cycle-based estimation samples does not depend on different specifications of accrual models.

7.2. Expense Manipulation

In simulations, I add discretionary accruals to total accruals and change in cash sales assuming revenue manipulation. On the other hand, Ecker et al. (2013) employ an "expense manipulation" approach whereby they add between 2 and 20% of total assets to the event firm's ratio of total accruals to lagged assets and do not adjust other variables such as sales or total assets. To investigate whether an "expense manipulation" approach changes the inferences from the current study, I repeat the simulation procedures assuming expense manipulation. Unreported results indicate that when an "expense manipulation" approach is employed, life cycle-based estimation samples outperform industry-based estimation samples in detecting accrual manipulation in a large U.S. sample and are superior to both industry-based and size-based estimation samples in smaller U.S. samples.

7.3. IPO Firms (Extreme Performance Sample Cases)

In Section 5.6, I find that life cycle-based estimation samples mitigate misspecification in extreme performance samples. Misspecification (Type I error) is expected to be relatively high in the IPO setting because IPO firms are generally high performance companies. To investigate whether using life cycle-based estimation samples mitigates misspecification in the IPO setting, I randomly select event firm-years from a pool of IPO firm-years and match each event firm-year with its industry peers or life cycle peers. Unreported results show that industry-based estimation samples generate a 16.19% rejection rate while life cycle-based estimation samples reduce this percentage to rejection rate of 11.75% at 0% manipulation. These results suggest that life cycle-based estimation samples mitigate the possibility of falsely rejecting the null hypothesis of no earnings management.

7.4. Two-way Sorting

In this study, I show that combined estimation samples do not provide much improvement in detecting accrual-based earnings manipulation compared to when life cycle-based estimation samples are used alone (see Section 5.3). For this analysis, I randomly select event firm-years from each life cycle stage and match each event firm-year with its industry peers or size peers. Another way to construct combined estimation samples is to employ two-way sorting. I sort firm-years on life cycle and on industry independently and then combine these two sorts. For instance, if two firms are in the same industry and life cycle, those firms are considered estimation peers. Untabulated results indicate that two-way sorts increase detection power by approximately 14% on average. However, this sorting approach no longer provides an advantage of using life cycle-based estimation samples, i.e., a higher degree of sample retention. Some estimation samples, such as industry-based estimation samples, lead to significant sample losses, and combined estimation samples by two-way sorts exacerbate this problem. In summary, these supplemental analyses support the use of life cycle-based estimation samples in various research settings.

VIII. CONCLUSION

This study proposes an alternative identification of estimation samples using a firm's life cycle. The detection of accrual manipulation relies on the ability of estimation samples to capture homogeneity in normal accrual generating processes. Estimation of the modified Jones model by life cycle suggests that normal accrual generating processes vary with a firm's life cycle, implying that such processes are homogeneous within each life cycle stage. Detection power for accrual manipulation also varies across life cycle stages, with the growth and mature stages having the highest detection power. Furthermore, analyses using simulated U.S. and international data show that when detecting accrual manipulation, life cycle-based estimation samples are superior to industry-based or size-based estimation samples in sample retention, specification, and detection power. These advantages of life cycle-based estimation samples are maximized in smaller samples including international settings. Life cycle-based estimation samples also outperform other existing estimation samples in detecting actual accrual manipulation. Lastly, I reexamine Dechow, Richardson, and Tuna (2003) and Teoh, Wong, and Rao (1998) applying life cycle-based estimation samples. I provide evidence that life cycle-based estimation samples change the inferences from DRT by improving test power and mitigate misspecification in TWR. These findings suggest the need for reexamining other prior earnings management studies using life cycle-based estimation samples.

The scope of the current study is limited to accrual-based earnings manipulation. However, if normal *cash flow* generating processes are homogenous within each life cycle stage, the detection of real activities manipulation can also be improved with life cycle-based estimation samples. Future research can address whether and how models for detecting real activities manipulation (Roychowdhury 2006) generate a more accurate estimate of normal cash flows and improve the detection of real activities manipulation when such models are estimated by life cycle. Taken together, earnings management studies can be significantly improved with theoretically identified estimation samples: life cycle-based estimation samples.

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TABLES

Table 1 Sample Selection for Simulation using U.S. Sample (1988-2012)

Selection Criteria	No. of Firm-Years
U.S. Compustat firm-years	263,829
Firm-years in non-regulated industries (i.e., excluding firm-years with SIC 4900-4999 or 6000-6999)	183,443
With data for calculating total accruals, change in cash sales, and PP&E	141,106
With the absolute value of total accruals scaled by total assets not exceeding one	134,944
Life Cycle Stage	**No. of Firm-Years**
Introduction	25,965
Growth	37,180
Mature	48,426
Shake-out	12,303
Decline	11,070
Total	**134,944**

Table 1 reports the sample restrictions imposed by requirements to have the necessary data to compute the input variables in the modified Jones model along with the number of firm-year observations by each life cycle stage.

Table 2 Descriptive Statistics of Key Variables by Each Life Cycle Stage

(N=134,944)

	N	*TA* Mean (Std.dev)	*WCA* Mean (Std. dev)	*⊿SALES* Mean (Std.dev)	*⊿SALES−⊿REC* Mean (Std.dev)
Introduction	25,965	−0.070 (0.256)	0.058 (0.207)	0.227 (0.580)	0.165 (0.499)
Growth	37,180	−0.077 (0.133)	0.026 (0.115)	0.272 (0.453)	0.225 (0.398)
Mature	48,426	−0.083 (0.101)	−0.005 (0.082)	0.099 (0.316)	0.092 (0.288)
Shake-out	12,303	−0.071 (0.144)	−0.014 (0.124)	0.013 (0.366)	0.016 (0.332)
Decline	11,070	−0.061 (0.214)	−0.002 (0.163)	−0.016 (0.398)	−0.016 (0.364)
F-stat		56.71	1152.50	1882.54	1463.91

	PPE Mean (Std.dev)	*Depr* Mean (Std.dev)	*OCF* Mean (Std.dev)	*ROA* Mean (Std.dev)	*$Asset_{t-1}$* Mean (Std.dev)
Introduction	0.279 (0.305)	0.064 (0.161)	−0.384 (0.863)	−0.457 (1.087)	235.25 (1419.79)
Growth	0.425 (0.349)	0.065 (0.055)	0.132 (0.147)	0.055 (0.176)	1754.77 (5529.78)
Mature	0.330 (0.245)	0.055 (0.077)	0.138 (0.117)	0.056 (0.174)	2971.50 (7585.76)
Shake-out	0.216 (0.209)	0.044 (0.040)	0.046 (0.156)	−0.018 (0.225)	1582.16 (5330.64)
Decline	0.159 (0.192)	0.045 (0.045)	−0.253 (0.408)	−0.304 (0.522)	363.60 (2210.24)
F-stat	2735.25	216.82	9040.42	5347.81	1181.70

Table 2 presents descriptive statistics for key variables by each life cycle stage. All variables are deflated by *$Asset_{t-1}$* (lagged total assets (*at*)). Definitions for each variable are as follows:
TA=total accruals=earnings before extraordinary items (*ibc*)−cash flows from operations (*oancf*); *WCA*=working capital accruals=change in current assets (*act*)−change in current liabilities (*lct*)−change in cash (*che*)+change in short-term debt (*dlc*); *⊿SALES*=change in sales (*sale*); *⊿SALES−⊿REC*= change in cash sales=change in sales (*sale*)−change in accounts receivable (*rect*); *PPE*=net property, plant, and equipment (*ppent*); *Depr*=depreciation expense (*dp*); *OCF*=operating cash flows (*oancf*); *ROA*=return on assets=net income (*ni*)/total assets (*at*). Table 2 also reports F-tests of whether the mean of each variable differs across life cycle stages.

Table 3 Model Estimation by Each Life Cycle Stage

Panel A: Model Estimation by Each Life Cycle Stage

	Introduction	Growth	Mature	Shake-out	Decline
	Mean (t-stat)	Mean (t-stat)	Mean (t-stat)	Mean (t-stat)	Mean (t-stat)
Intercept	−0.070 (−32.86)	−0.052 (−46.29)	−0.073 (−93.86)	−0.069 (−37.05)	−0.057 (−21.29)
$1/Asset_{t-1}$	−0.006 (−7.14)	−0.046 (−12.52)	−0.037 (−16.46)	−0.049 (−10.89)	−0.013 (−6.13)
$\Delta SALES - \Delta REC$	0.104 (33.20)	0.020 (11.56)	0.030 (18.67)	0.054 (14.02)	0.041 (7.36)
PPE	−0.054 (−10.53)	−0.065 (−33.55)	−0.033 (−17.87)	0.005 (0.75)	−0.010 (−0.94)
R^2	0.225	0.231	0.246	0.208	0.200
Adjusted R^2	0.136	0.142	0.159	0.117	0.108
N	25,965	37,180	48,426	12,303	11,070

Panel B: Estimated Discretionary Accruals by Each Life Cycle Stage

Life Cycle Stage	Mean	Std.dev	Lower 25%	Median	Upper 25%
Introduction	0.005	0.238	−0.131	0.041	0.152
Growth	−0.000	0.105	−0.044	0.008	0.062
Mature	−0.002	0.108	−0.032	0.014	0.049
Shake-out	−0.003	0.125	−0.067	0.014	0.072
Decline	0.023	0.185	−0.064	0.029	0.125

Table 3 (Cont'd)

Panel C: Tests of Coefficient Differences across Life Cycle Stages

Life Cycle		*Intercept*		*1/Asset$_{t-1}$*		*ΔSALES−ΔREC*		*PPE*	
LC_a	LC_b	LC_a−LC_b	χ^2	LC_a−LC_b	χ^2	LC_a−LC_b	χ^2	LC_a−LC	χ^2
Introduction	Growth	−0.018	52.22***	0.040	20.41***	0.084	278.37***	0.011	3.22*
Introduction	Mature	0.003	1.27	0.031	12.96***	0.074	208.00***	−0.021	11.10***
Introduction	Shake-out	−0.001	0.12	0.043	22.10***	0.049	45.53***	−0.058	45.49***
Introduction	Decline	−0.013	14.93***	0.007	2.59	0.063	50.72***	−0.044	11.19***
Growth	Mature	0.021	175.15***	−0.009	0.56	−0.010	5.18**	−0.032	110.02***
Growth	Shake-out	0.017	51.16***	0.003	0.06	−0.035	25.88***	−0.070	105.51***
Growth	Decline	0.005	2.35	−0.033	12.95***	−0.021	6.42**	−0.055	21.20***
Mature	Shake-out	−0.004	2.69	0.012	0.98	−0.025	12.88***	−0.038	31.38***
Mature	Decline	−0.016	31.20***	−0.024	7.24***	−0.011	1.80	−0.023	3.79*
Shake-out	Decline	−0.012	14.52***	−0.036	1.81	0.013	1.81	0.015	1.19

Table 3 Panel A presents the coefficients of the modified Jones model and associated t-stats by each life cycle stage along with R^2 and adjusted R^2. Panel B presents the mean and standard deviation of estimated discretionary accruals. Panel C reports Wald χ^2-statistics, computed for tests of whether coefficients are statistically different across life cycle stages. *, **, and *** denote statistical significance levels of 0.10, 0.05, and 0.01, respectively (two-tailed). Descriptive statistics for R^2, adjusted R^2, and estimated discretionary accruals are based on 50,000 regressions for estimating the parameters of the modified Jones model by life cycle.

Table 4 Simulation Results-Specification/Power by Each Life Cycle Stage

Panel A: Specification/Power by Each Life Cycle Stage

Life Cycle Stage	N	0	2	4	6	8	10	12	14	16	18	20
											Seeded manipulation (%)	
Introduction	25,965	6.63	8.32	10.31	12.40	14.82	17.42	20.22	23.21	26.33	29.56	32.98
Growth	37,180	5.27	7.78	11.56	17.08	24.28	32.76	42.22	51.34	60.13	67.81	74.37
Mature	48,426	5.24	7.80	11.93	18.22	26.64	36.63	46.76	56.57	65.05	72.32	78.10
Shake-out	12,303	5.31	6.59	8.28	10.61	13.58	17.21	21.61	26.72	32.23	37.98	43.87
Decline	11,070	7.32	8.23	9.27	10.43	11.83	13.34	15.15	17.00	19.24	21.63	24.20

Panel B: Life Cycle Persistence

Persistence	N	0	2	4	6	8	10	12	14	16	18	20
											Seeded manipulation (%)	
Life Cycle-Year	134,944	5.64	7.70	10.92	15.46	21.11	27.80	34.94	41.98	48.53	54.58	59.76
2 Year-Persistence	83,807	5.79	8.30	12.13	17.68	24.56	32.59	40.88	48.55	55.44	61.28	66.31
3 Year-Persistence	53,481	5.79	8.73	13.29	19.74	28.00	37.31	46.31	54.54	61.37	67.08	71.71

Table 4 Panel A reports simulation results by each life cycle stage. Panel A presents the percentage of 50,000 sub-samples where the null hypothesis of non-positive discretionary accruals is rejected at the 0.05 level of one-tailed tests. Each sub-sample consists of one event firm-year and 30 peers from the same life cycle and year as the event firm-year. A partitioning variable, *PART* is set to 1 for the event firm-year and to 0 for its 30 peer firm-years. For each event firm-year, accrual manipulation (0-20% of total assets) is added. Using 30 peer firm-years, discretionary accruals (DAP_{it}) are estimated from the modified Jones model. DAP_{it} is then regressed on *PART* and the frequency of *PART*>0 with statistical significance (0.05 level of one-tailed tests) is counted. For example, when a firm in the mature stage manipulates accruals by 4% of total assets, the probability that a false null hypothesis of no earnings management is rejected is 11.93%. Panel B presents the simulation results when there is no sample restriction by life cycle persistence (Life Cycle-Year) and when samples are restricted to firms with the same life cycle for two (2 Year-Persistence) or three consecutive years (3 Year-Persistence). Except for this sample restriction, all the simulation procedures are the same as those for Panel A.

Table 5 Comparison of Estimation Samples

Simulation Results-Specification/Power (Large U.S. Sample)

Panel A: Stand-alone Identification of Estimation Samples

		Seeded manipulation (%)										
Estimation Samples	N	0	2	4	6	8	10	12	14	16	18	20
Whole Sample	134,944	6.47	7.91	9.81	12.19	15.17	18.79	23.37	28.49	34.24	40.30	46.52
Year	134,944	6.35	7.93	9.88	12.41	15.71	19.79	24.53	29.89	36.04	42.08	48.59
Industry (2digit)-Year	125,690	6.61	8.37	10.79	14.11	18.23	23.22	28.70	34.59	40.72	46.79	52.85
Size (quintile)-Year	134,944	5.55	7.84	11.34	15.89	21.64	27.90	34.30	40.43	46.35	51.89	56.87
Size (close30)-Year	134,944	5.22	7.48	11.04	15.73	21.85	28.27	34.91	41.33	47.27	53.14	58.16
Life Cycle-Year	134,944	5.64	7.70	10.92	15.46	21.11	27.80	34.94	41.98	48.53	54.58	59.76

Panel B: Combined Identifications of Estimation Samples (Life Cycle with Industry)

		Seeded manipulation (%)										
Event Firm-Year	Peers	0	2	4	6	8	10	12	14	16	18	20
Introduction	Same industry (2digit) and year	15.42	18.44	22.24	26.22	30.18	34.50	39.04	43.50	47.82	51.82	55.12
Growth		3.76	5.76	8.58	13.00	18.60	25.68	32.76	39.80	46.92	54.02	60.20
Mature		2.08	3.14	4.48	6.82	10.54	16.06	22.30	29.42	36.56	44.74	52.22
Shake-out		6.66	8.16	10.72	13.12	16.64	21.00	25.52	30.82	36.48	41.76	46.92
Decline		14.00	16.38	18.28	20.54	23.36	25.96	29.08	32.70	35.84	39.78	43.60

Panel C: Combined Identifications of Estimation Samples (Life Cycle with Size)

		Seeded manipulation (%)										
Event Firm-Year	Peers	0	2	4	6	8	10	12	14	16	18	20
Introduction	Similar size (closest-in-size neighbors) and year	12.16	15.46	19.00	22.58	26.60	31.24	36.12	40.76	44.92	48.44	52.04
Growth		3.90	6.54	11.30	17.52	25.78	34.38	42.90	50.26	57.46	63.62	69.12
Mature		2.14	3.44	6.34	11.22	17.82	24.84	32.70	39.66	46.96	53.44	59.16
Shake-out		5.14	7.50	10.16	14.16	18.88	23.66	28.68	33.68	39.34	44.70	48.38
Decline		10.04	11.72	14.06	16.96	19.70	22.80	25.64	28.58	31.94	35.22	38.32

Table 5 reports the simulation results on a large U.S. sample. Panel A presents the percentage of 50,000 sub-samples where the null hypothesis of non-positive discretionary accruals is rejected at the 0.05 level of one-tailed tests. Each sub-sample consists of one-event firm-year and 30 peers defined by each of the estimation samples. Specifically, 30 peers are selected from the same industry (2 digit SIC) and year, the same size quintile and year, closest-in-size neighbors in the same year, or the same life cycle and year. I define two other alternative estimation samples with respect to how peer firms are selected: (1) “Whole Sample” estimation samples whereby peer firm-years are selected from the entire population and (2) “Year” estimation samples whereby peer firm-years are selected from the same fiscal year as the event firm-year. A partitioning variable, *PART* is set to 1 for the event firm-year and to 0 for its 30 peer firm-years. For each event firm-year, accrual manipulation (0-20% of total assets) is added. Using 30 peer firm-years, discretionary accruals (DAP_{it}) are estimated from the modified Jones model. DAP_{it} is then regressed on *PART* and the frequency of *PART*>0 with statistical significance (0.05 level of one-tailed tests) is counted. For example, when a firm manipulates accruals by 6% of total assets, the probability that a false null hypothesis of no earnings management is rejected is 15.46% by life cycle-based estimation samples. Panel B (Panel C) presents the simulation results when life cycle-based estimation samples are combined with industry-based (size-based) estimation samples. In the simulations, an event firm-year is classified by its life cycle stage and then is matched with its industry peers (size peers). The remaining simulation procedures are unchanged.

Table 6 Comparison of Estimation Samples

Simulation Results-Specification/Power (Small U.S. Sample)

											Seeded manipulation (%)		
Estimation Samples		N	0	2	4	6	8	10	12	14	16	18	20
Whole Sample	500 (obs.)	500	6.06	7.28	8.72	10.58	13.34	16.88	20.82	24.98	29.66	35.06	40.50
Industry (1digit)		471	5.14	6.62	9.14	11.20	14.26	17.52	21.36	26.58	31.80	39.10	45.66
Industry (FF12)		425	4.44	5.70	7.86	10.00	12.78	16.56	20.82	25.10	29.14	33.80	39.22
Size		500	6.18	7.96	9.80	14.02	20.48	26.82	34.24	42.54	49.28	55.52	60.74
Life Cycle		500	5.24	7.04	10.06	16.18	23.32	31.88	40.68	48.14	54.48	59.46	64.16
Whole Sample	1,000	1,000	6.00	7.58	9.52	12.02	14.74	18.10	22.16	27.28	32.40	38.70	44.78
Year		1,000	6.68	8.20	9.94	12.22	15.44	18.98	23.12	27.52	32.68	38.76	45.02
Industry (1digit)		995	7.96	9.58	11.32	14.04	17.56	21.98	26.74	32.72	39.10	45.82	52.48
Industry (FF12)		976	6.72	8.18	10.60	12.88	15.92	20.92	26.80	33.28	39.32	45.36	52.70
Size		1,000	4.80	6.28	8.62	12.40	18.80	25.58	31.78	37.44	43.48	50.28	55.62
Size-Year		972	5.44	7.68	9.88	13.16	17.88	21.98	27.74	33.76	41.20	48.20	55.02
Life Cycle		1,000	5.82	8.28	11.54	16.50	22.36	29.38	36.82	44.88	51.58	57.80	63.76
Whole Sample	2,000	2,000	6.46	8.06	9.94	12.50	15.70	19.64	24.24	29.30	34.70	40.40	46.76
Year		2,000	5.96	7.76	10.00	12.50	15.30	19.30	24.02	29.52	34.96	41.00	47.32
Industry (1digit)		1,983	6.16	7.78	10.06	12.98	16.46	21.04	26.30	32.24	38.70	44.84	50.98
Industry (FF12)		2,000	7.18	8.58	10.56	13.28	17.26	22.60	27.88	33.54	39.82	45.78	51.44
Size		2,000	5.28	7.12	11.08	15.12	20.76	28.34	35.96	42.28	48.10	54.62	60.02
Size-Year		2,000	4.94	6.70	8.90	12.46	16.92	22.98	28.94	36.68	43.10	51.14	57.76
Life Cycle		2,000	7.74	9.60	12.24	16.76	23.50	31.02	38.48	45.44	52.08	57.48	61.20

Table 6 (Cont'd)

			Seeded manipulation (%)										
Estimation Samples		N	0	2	4	6	8	10	12	14	16	18	20
Whole Sample	5,000	5,000	6.38	8.06	9.66	12.02	15.06	18.44	23.16	27.90	33.38	39.06	45.74
Year		5,000	7.02	8.38	10.44	13.12	16.56	20.02	24.26	29.68	34.88	40.64	47.02
Industry (1digit)		4,997	6.14	7.70	9.88	12.60	16.54	20.76	25.34	31.74	37.60	44.18	50.62
Industry (FF12)		5,000	6.00	7.78	9.70	12.10	15.60	20.00	25.18	30.66	36.84	43.52	50.08
Size		5,000	6.02	8.20	11.34	15.46	20.30	25.96	32.00	37.96	44.56	49.68	55.30
Size-Year		5,000	5.16	6.82	9.60	14.14	20.20	26.42	32.86	39.56	46.64	52.54	58.30
Life Cycle		5,000	4.90	6.74	9.40	14.06	21.24	27.60	35.28	43.22	50.50	56.94	61.86
Whole Sample	10,000	10,000	6.28	7.76	9.44	11.66	14.76	18.10	22.50	27.64	32.64	39.06	44.80
Year		10,000	6.88	8.74	10.76	13.24	16.36	20.42	25.64	30.38	36.40	42.58	48.80
Industry (1digit)		9,982	6.02	7.66	10.46	13.78	17.74	22.52	27.46	34.00	39.72	45.64	52.26
Industry (FF12)		10,000	6.20	8.10	9.94	13.08	16.80	21.18	26.18	32.00	38.34	44.38	50.96
Size		10,000	4.36	6.56	9.72	14.82	20.46	26.74	34.28	41.30	47.46	53.04	57.90
Size-Year		10,000	4.62	6.72	9.88	15.00	20.70	27.44	35.06	41.22	47.60	52.56	58.54
Life Cycle		10,000	6.06	8.30	11.14	15.58	21.46	28.36	35.36	43.04	49.72	56.52	62.28
Whole Sample	15,000	15,000	6.42	7.84	9.94	12.24	15.46	19.34	23.72	28.36	34.18	40.32	46.56
Year		15,000	6.46	7.94	9.68	12.30	15.52	19.56	24.08	29.14	34.82	41.36	47.70
Industry (1digit)		14,985	6.30	7.72	9.40	11.80	15.10	18.98	24.04	29.76	35.66	42.26	48.40
Industry (FF12)		15,000	5.08	6.38	8.16	10.70	13.60	18.22	22.92	28.78	35.22	41.76	48.76
Size		15,000	5.68	7.86	11.00	14.90	19.82	25.96	32.34	37.82	43.68	49.58	55.64
Size-Year		15,000	5.34	7.30	10.24	15.36	20.90	27.42	34.20	40.84	47.02	53.30	58.04
Life Cycle		15,000	6.04	7.84	10.56	14.82	20.44	26.84	35.22	42.18	49.30	55.78	61.36

Table 6 reports the simulation results on smaller U.S. samples. For smaller samples, a certain number of observations (500, 1,000, 2,000, 5,000, 10,000 and 15,000 obs.) are randomly selected from the entire population. This table presents the percentage of 5,000 sub-samples where the null hypothesis of non-positive discretionary accruals is rejected at the 0.05 level of one-tailed tests. Each sub-sample consists of one event firm-year and 30 peers defined by each of the estimation samples. Specifically, 30 peers are selected from the same industry (i.e., 1 digit SIC or Fama-French 12 industry), closest-in-size neighbors (in the same year), or the same life cycle. I define two other alternative estimation samples with respect to how peer firms are selected: (1) "Whole Sample" estimation samples whereby peer firm-years are selected from the entire population and (2) "Year" estimation samples whereby peer firm-years are selected from the same fiscal year as the event firm-year. A partitioning variable, *PART* is set to 1 for the event firm-year and to 0 for its 30 peer firm-years. For each event firm-year, accrual manipulation (0-20% of total assets) is added. Using 30 peer firm-years, discretionary accruals (DAP_{it}) are estimated from the modified Jones model. DAP_{it} is then regressed on *PART* and the frequency of *PART*>0 with statistical significance (0.05 level of one-tailed tests) is counted. For example, when a firm manipulates accruals by 6% of total assets, the probability that a false null hypothesis of no earnings management is rejected is 14.06% by life cycle-based estimation samples in a random sample of 5,000 observations.

Table 7 Sample Attrition in International Sample (1988-2012)

Requirement	No. of Obs.	No. of Countries	% of Reduction in Obs.
Compustat Global	359,599	109	
Require minimum requirements for accrual model estimation	242,733	107	
Require 10 (30) or more obs. for each country, industry, and year.	173,498 (115,478)	45 (25)	28.52% (52.43%)
Require 10 (30) or more obs. for each country, life cycle, and year.	232,314 (211,250)	69 (46)	4.29% (12.97%)
Require 10 (30) or more obs. for each country, size rank, and year.	233,535 (213,569)	70 (44)	3.79% (12.01%)
Require 10 (30) or more obs. for each country, size (close 30), and year.	240,700 (235,437)	79 (55)	0.84% (3.01%)

Table 7 reports sample attrition by different identifications of estimation samples in international settings (i.e., Compustat Global).

Table 8 Comparison of Estimation Samples

Simulation Results-Specification/Power (International Sample)

			Seeded manipulation (%)										
Country	Estimation Samples	N	0	2	4	6	8	10	12	14	16	18	20
Australia 17,186 (obs.)	Industry (1digit)	17,171	7.38	8.58	10.18	11.78	14.08	16.74	20.12	23.48	27.26	31.54	35.86
	Size (close30)	17,186	5.36	6.82	9.00	11.94	15.16	19.10	23.14	27.90	31.66	36.20	40.38
	Size (close30)-Year	17,181	5.96	7.20	9.18	12.20	15.54	18.80	22.98	27.20	31.52	36.24	40.98
	Life cycle	17,186	7.38	9.58	11.72	14.58	18.08	22.34	27.04	32.54	37.08	41.58	46.66
	Life cycle-Year	16,826	5.16	6.90	9.50	12.56	16.38	20.98	26.56	32.04	37.42	42.30	47.16
Brazil	Industry (1digit)	2,589	6.94	9.02	11.48	14.82	19.42	24.36	30.04	37.20	44.74	51.94	58.96
2,594	Size (close30)	2,594	5.56	7.34	9.74	13.18	17.90	23.66	29.70	37.44	45.84	51.94	59.62
	Size (close30)-Year	2,594	5.96	8.00	11.40	14.98	19.28	24.68	31.28	38.34	45.38	52.56	59.42
	Life cycle	2,594	7.50	10.72	14.68	19.62	26.00	33.34	41.06	49.18	56.76	64.32	70.18
China	Industry (1digit)	22,533	7.32	10.36	13.86	19.64	26.16	33.86	42.48	51.02	59.84	67.64	74.48
22,533	Size (close30)	22,533	7.28	10.42	14.02	19.68	26.70	34.80	44.10	52.34	60.40	67.72	73.62
	Size (close30)-Year	22,524	6.76	9.80	14.50	20.92	28.46	37.42	46.74	54.86	62.72	70.52	76.30
	Life cycle	22,533	5.02	9.60	15.52	23.18	33.04	43.98	54.12	64.20	73.84	81.86	87.10
	Life cycle-Year	22,364	6.64	11.40	18.08	26.98	36.40	47.46	57.22	65.88	72.72	78.44	82.80
India	Industry (1digit)	24,907	8.00	9.62	12.50	15.42	19.24	23.56	28.74	34.42	40.48	47.16	54.10
24,907	Size (close30)	24,907	8.00	10.14	12.36	15.32	19.52	23.92	28.98	35.02	41.16	48.78	55.08
	Size (close30)-Year	24,905	7.60	9.96	12.78	16.24	20.58	25.72	31.86	37.94	44.44	51.14	56.92
	Life cycle	24,907	6.86	9.62	13.60	18.72	26.62	35.38	44.60	52.78	59.64	65.20	69.70
	Life cycle-Year	24,758	7.72	10.50	14.28	20.08	27.50	35.74	44.18	52.60	59.78	65.78	71.26

Table 8 (Cont'd)

Country	Estimation Samples	N	Seeded manipulation (%) 0	2	4	6	8	10	12	14	16	18	20
Philippines	Industry (1digit)	1,847	7.50	9.42	11.92	15.46	19.30	23.46	29.00	35.14	40.80	46.58	52.34
1,847	Size (close30)	1,847	7.06	8.84	10.98	14.18	18.40	23.14	28.64	34.80	41.38	47.82	53.76
	Size (close30)-Year	1,825	6.24	8.04	10.52	13.62	17.36	21.06	25.96	32.66	38.40	44.26	49.98
	Life cycle	1,847	5.86	8.18	11.48	16.78	23.62	31.02	38.68	45.88	52.26	58.46	63.76
Poland	Industry (1digit)	3,089	9.12	10.62	12.42	15.26	18.58	22.10	26.16	30.44	35.68	41.00	46.04
3,112	Size (close30)	3,112	7.08	9.30	12.12	15.32	19.78	24.68	29.60	34.94	39.96	45.34	51.12
	Size (close30)-Year	3,093	6.74	8.96	11.60	15.04	18.96	23.22	29.16	34.70	40.82	46.70	53.04
	Life cycle	3,094	6.38	8.66	11.96	16.38	21.84	28.34	35.28	42.60	49.76	56.40	61.80
Russia	Industry (1digit)	728	9.54	10.84	12.58	15.24	19.16	23.56	28.94	35.02	41.52	48.32	54.96
746	Size (close30)	746	6.38	7.82	10.42	12.76	16.78	21.84	29.14	35.38	41.62	47.04	52.96
	Size (close30)-Year	687	8.22	10.62	13.36	16.52	20.14	24.38	28.92	35.02	42.08	48.48	55.80
	Life cycle	713	7.16	9.40	12.18	15.50	21.80	28.82	36.76	47.58	55.48	63.02	69.42
Singapore	Industry (1digit)	5,844	7.88	10.24	12.54	15.92	19.98	25.00	30.52	37.00	44.06	51.54	58.52
5,845	Size (close30)	5,845	7.00	9.16	11.78	15.54	20.06	25.54	31.72	38.70	45.96	52.56	59.34
	Size (close30)-Year	5,821	7.32	9.92	12.74	16.56	21.92	27.26	33.00	39.36	46.32	53.10	60.28
	Life cycle	5,845	6.04	8.50	11.82	16.34	22.84	30.28	37.18	45.24	52.80	59.12	65.86

Table 8 reports the simulation results on eight countries (within country analysis): Australia, Brazil, China, India, Philippines, Poland, Russia, and Singapore (alphabetical order). This table presents the percentage of 5,000 sub-samples where the null hypothesis of non-positive discretionary accruals is rejected at the 0.05 level of one-tailed tests. Each sub-sample consists of one event firm-year and 30 peers. Specifically, 30 peers are selected from the same industry (i.e., 1 digit SIC code), closest-in-size neighbors (in the same year), or the same life cycle (and year). A partitioning variable, *PART* is set to 1 for the event firm-year and to 0 for its 30 peer firm-years. For each event firm-year, accrual manipulation (0-20% of total assets) is added. Using 30 peer firm-years, discretionary accruals (DAP_{it}) are estimated from the modified Jones model. DAP_{it} is then regressed on *PART* and the frequency of *PART*>0 with statistical significance (0.05 level of one-tailed tests) is counted. For example, when a firm in Singapore manipulates accruals by 6% of total assets, the probability that a false null hypothesis of no earnings management is rejected is 16.34% by life cycle-based estimation samples.

Table 9 Comparison of Estimation Samples

Simulation Results-Specification (Extreme Performance Sample)

Panel A: Extreme Operating Cash Flows (*OCF*)

	Lowest Quintile	Highest Quintile
Industry (2digit)-Year	19.22%	2.94%
Size (close30)-Year	13.22%	1.88%
Life Cycle-Year	8.54%	5.08%

Panel B: Extreme Earnings deflated by total assets (*ROA*)

	Lowest Quintile	Highest Quintile
Industry (2digit)-Year	9.08%	12.62%
Size (close30)-Year	3.02%	12.74%
Life Cycle-Year	4.42%	13.56%

Table 9 reports the simulation results on extreme operating performance sample. Panel A presents the percentage of 50,000 sub-samples where the null hypothesis of non-positive discretionary accruals is rejected at the 0.05 level of one-tailed tests. Each sub-sample consists of one event firm-year and 30 peers defined by each of the estimation samples. An event firm-year is selected from highest (lowest) quintile of operating cash flows/return on assets. 30 peers are selected from the same industry (2 digit SIC) and year, closest-in-size neighbors in the same year, or the same life cycle and year. A partitioning variable, *PART* is set to 1 for the event firm-year and to 0 for its 30 peer firm-years. Using 30 peer firm-years, discretionary accruals (DAP_{it}) are estimated from the modified Jones model. DAP_{it} is then regressed on *PART* and the frequency of *PART*>0 with statistical significance (0.05 level of one-tailed tests) is counted. For example, when a firm does *not* manipulate accruals, the probability that a true null hypothesis of no earnings management is rejected is 8.54% by life cycle-based estimation samples in samples with low operating cash flows (*OCF*).

Table 10 Power Tests for Actual Earnings Manipulation (AAERs Sample)

Panel A:Comparison of Estimation Samples

Estimation Samples	Rejection Rate
Industry-Year	13%
Size-Year	14%
Life cycle-Year	19%

Panel B: Estimation by Each Life Cycle

	N		*TA*		*WCA*		
Life Cycle Stage	AAER	Large U.S.	AAER	Large U.S.	AAER	Large U.S.	Rejection Rate
Introduction	230	25,965	0.017	−0.070	0.123	0.058	28%
Growth	339	37,180	−0.070	−0.077	0.029	0.026	11%
Mature	230	48,426	−0.076	−0.083	−0.018	−0.005	14%
Shake-out	70	12,303	−0.089	−0.071	−0.031	−0.014	6%
Decline	59	11,070	−0.070	−0.061	−0.014	−0.002	4%
Total	928	134,944					

Panel C: Combined Identifications of Estimation Samples

Life Cycle	Event	Life Cycle with Industry	Life Cycle with Size
Introduction	230	24%	21%
Growth	339	5%	7%
Mature	230	3%	6%
Shake-out	70	3%	4%
Decline	59	8%	3%
Total	928		

Table 10 Panel A reports the simulation results for the AAERs data. This table presents the percentage of 5,000 sub-samples where the null hypothesis of non-positive discretionary accruals is rejected at the 0.05 level of one-tailed tests. Each sub-sample consists of one event firm-year from the AAERs sample and 30 peers defined by each of the estimation samples. Specifically, 30 peers are selected from the same industry (2 digit SIC) and year, closest-in-size neighbors in the same year, or the same life cycle and year. A partitioning variable, *PART* is set to 1 for the event firm-year and to 0 for its 30 peer firm-years. Using 30 peer firm-years, discretionary accruals (DAP_{it}) are estimated from the modified Jones model. DAP_{it} is then regressed on *PART* and the frequency of *PART*>0 with statistical significance (0.05 level of one-tailed tests) is counted. Panel B reports the simulation results by each life cycle stage. An event firm-year is selected from the AAERs sample and matched with 30 peers from the same life cycle and year as the event firm-year. For example, when a firm in the AAER sample manipulates accruals, the probability that a false null hypothesis of no earnings management is rejected is 19% by life cycle-based estimation samples. Panel B also reports the mean of working capital accruals (*WCA*) and total accruals (*TA*) (deflated by $Asset_{t-1}$ (lagged total assets)) by each life cycle stage in the AAER sample and a large U.S. sample (N=134,944). Panel C presents the simulation results when life cycle-based estimation samples are combined with industry-based (size-based) estimation samples. In simulations, an event firm-year is classified by its life cycle and then is matched with its industry peers (size peers). The remaining simulation procedures are unchanged.

Table 11 Reexamination of Dechow, Richardson, and Tuna (2003)

Panel A: Discretionary accruals estimated using industry-based estimation samples (Replication of Dechow, Richardson, and Tuna 2003)

	Small Profit Firms		Small Loss Firms			
	N	Mean	N	Mean	Test Statistic	p-value
Lagged Discretionary Accruals	908	0.024	444	0.017	−1.365	0.173
Fwd. Look Discretionary Accruals	908	0.022	444	0.018	−0.971	0.332

Panel B: Discretionary accruals estimated using life cycle-based estimation samples (Non-restricted Sample)

	Small Profit Firms		Small Loss Firms			
	N	Mean	N	Mean	Test Statistic	p-value
Lagged Discretionary Accruals	1,836	0.022	895	0.014	−2.184	0.029
Fwd. Look Discretionary Accruals	1,836	0.022	895	0.015	−1.944	0.052

Panel C: Discretionary accruals estimated using life cycle-based estimation samples (Restricted Sample)

	Small Profit Firms		Small Loss Firms			
	N	Mean	N	Mean	Test Statistic	p-value
Lagged Discretionary Accruals	908	0.026	444	0.016	−2.089	0.037
Fwd. Look Discretionary Accruals	908	0.026	444	0.017	−1.959	0.050

Table 11 Panel A presents the replication results for Dechow, Richardson, and Tuna (2003). Discretionary accruals are obtained from estimating the lagged discretionary accrual model and forward-looking discretionary accrual model within each industry (2 digit SIC) and year. Differences in discretionary accruals between small profit firms and small loss firms are tested using two-sample t-tests. Panel B reports discretionary accruals from estimating the same accrual models within each life cycle and year. Panel C reports discretionary accruals by life cycle-based estimation samples using the restrictive data in Panel A.

Table 12 Reexamination of Teoh, Wong, and Rao (1998)

Panel A: Discretionary accruals estimated using industry-based estimation samples during the sample period 1980-1990 (Replication of Teoh, Wong, and Rao 1998)

Year	0	1	2	3	4	5	6
Mean	10.846	3.740	0.765	−0.122	−1.380	−1.794	−0.842
t-stat	24.533	7.504	0.987	−0.714	−2.591	−2.585	−1.250
Obs.	1,554	1,365	1,154	990	732	467	363

Panel B: Discretionary accruals estimated using industry-based estimation samples during the sample period 1988-2012

Year	0	1	2	3	4	5	6
Mean	4.718	2.253	0.740	0.126	0.281	−0.336	0.043
t-stat	15.569	7.067	1.937	−0.047	0.410	−1.360	−0.265
Obs.	5,249	5,093	4,769	4,293	3,954	3,711	3,298

Panel C: Discretionary accruals estimated using life cycle-based estimation samples during the sample period 1988-2012

Year	0	1	2	3	4	5	6
Mean	2.765	1.425	0.059	−0.516	−0.242	−0.659	−0.318
t-stat	8.812	4.337	−0.075	−1.774	−0.922	−2.040	−1.038
Obs.	5,286	5,126	4,798	4,315	3,975	3,732	3,315

Table 12 Panel A presents the replication results for Teoh, Wong, and Rao (1998). Discretionary accruals are obtained from estimating the modified Jones model within each industry (2 digit SIC) and year for the years 1980-1990. The mean discretionary accruals for a sample of firms making initial public offers (IPOs) are presented as a percentage of lagged total assets. Differences in discretionary accruals between IPO firms and all other firms are tested using two-sample t-tests. Panel B reports discretionary accruals from estimating the same accrual model using industry-based estimation samples for the years 1988-2012. Panel C reports discretionary accruals by life cycle-based estimation samples for the years 1988-2012.