The Economics of Fraudulent Accounting*

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Abstract

We argue that earnings management and fraudulent accounting have important economic consequences. In a model where the costs of earnings management are endogenous, we show that in equilibrium, low productivity firms hire and invest too much in order to pool with high productivity firms. This behavior distorts the allocation of economic resources in the economy. We test the predictions of the model using firm-level data. We show that during periods of suspicious accounting, firms hire and invest excessively, while managers exercise options. When the misreporting is detected, firms shed labor and capital and productivity improves. Our firm-level results hold both before and after the market crash of 2000. In the aggregate, our model provides a novel explanation for periods of jobless and investment-less growth.

JEL codes: D2, E3, G3

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Introduction

Fraudulent accounting by management has been costly for shareholders. The market adjusted return over the three-days surrounding the announcement of a restatement to financial statements is associated with an average return of –10% (see GAO (2002)). Though the losses to shareholders are large and apparent, the impact of fraudulent accounting on the wider economy is not well understood. It is not well known, for instance, whether earnings management lowers economic efficiency or whether it simply redistributes income from shareholders to insiders. In this paper, we examine the potential economic consequences of fraudulent accounting, with a particular focus on the dynamics of employment and investment.

The dramatic case of Enron's restatement illustrates this effect. On November 8, 2001, Enron announced that it would restate its earnings for the period 1997 through 2001. This restatement recorded a $1.2 billion reduction to shareholders equity. The stock price of Enron declined from more than $30 to less than $1 between October 16, 2001 and November 28, 2001. Accompanying these large losses was a dramatic pattern of growth and demise. During the period when Enron was misreporting, it grew faster than any other firm in the industry. The book value of Enron's assets nearly tripled, from $23.5 billion in 1997 to $65.5 billion in 2000. Tobin's Q increased from 1.32 to 1.8 over this period. At its peak, Enron employed more than 20,000 employees worldwide. This period of misreporting was also characterized by substantial stock sales by Enron insiders see **Figure 1**). After its restatement Enron shrank rapidly. Today, about 500 employees remain and Enron's creditors expect to receive about one-fifth of the estimated $63 billion they are owed.

In this paper, we report that Enron's story is typical – if somewhat extreme – of the dynamic of employment and investment around periods of fraudulent accounting. We also show that the joint dynamics of misreporting, insider's trades, employment and investment can be explained by a simple model of multi-dimensional signaling.

We study the problem of managers who privately observe the true productivity of their companies, and who make hiring and investment decisions. Managers who want to hide the low productivity of their firms must not only manage earnings, but also hire and invest as if productivity was high. It would not be sufficient to merely misreport performance. In equilibrium, firms with low true productivity hire and invest excessively, distorting the al-

location of resources in the economy. Prior and concurrent theoretical work (see Narayanan (1985), Stein (1989), Guttman, Kadan, and Kandel (2004), Goldman and Slezak (2003) and Povel, Singh, and Winton (2004)) has not emphasized these implications. In our model, real costs of manipulation arise endogenously because earnings management distorts the hiring and investment decisions of firms.

In our model, managers want to report high profits because of the opportunity to engage in insider trading. Managers do not have a direct preference for investing, but the requirements of signalling compels them to act in a consistent manner. The essential point that emerges is a general one: in any signaling equilibrium hiring and investment must be consistent with reported profits. In alternative interpretations, such as managerial optimism or empire building, managers might have a primary desire for hiring and investment, but it is the same requirement of consistency that lead them to manipulate earnings. In both cases, earnings manipulation is a necessary condition for overinvestment.

We use a sample of firms that restate their earnings between January 1997 and June 2002 to test the predictions of our model. We first look at insider trading. In the model, earnings management boost stock prices, allowing managers to make profitable trades, and managers with larger stock and option holdings are more likely to engage in earnings management. Recently, Beneish and Vargus (2002), Bergstresser and Philippon (2006), Burns and Kedia (2004), Bartov and Mohanram (2004) and Roulstone (2005) have confirmed these predictions. Similarly, our data shows that, during the misreported period, CEOs exercise a significantly higher fraction of their exercisable options than the CEOs of comparable firms.

We then focus on the dynamics of employment and investment. We find that, during the periods when they misreport, firms hire and invest more than comparable firms matched on age, industry and initial size. Hiring and investment are significantly lower after the restated period. The use of a control group ensures that our results are not explained by industry-age-size specific exogenous factors. In other words, the results do not simply reflect a bubble affecting young firms in the computer industry. Our results continue to hold if we restrict our sample to the restatements announced before the market crash of 2000 and also hold in industries with below median growth rates. It is therefore unlikely that earnings management is simply a side effect of exogenous over valuation.

When testing for the implications of the model, the question is not whether earnings

management causes overinvestment (in our model, both earnings management and overinvestment are caused by disappointing true productivity) but rather whether overinvestment would have been possible if firms had reported the true numbers. In other words: would Enron have been able to hire and invest the way it did if Enron had published its true profit numbers? Our results suggest that the answer is no, for two sets of reasons. First, our empirical results clearly indicate that overinvestment is not random, but mimics the investment of firms with similar market value growth prior to the misreporting. Also, the distortions in investment and hiring are related to the extent of manipulation. This validates our main theoretical point about the requirement for consistency, and documents for the first time that misreporting is accompanied by distortions in the firm's hiring and investment. Second, if earnings management was not necessary, why did managers engage in it at significant personal costs? Clearly, they must have believed that showing high profits was required. Moreover, market value drops by more than 10% on average when restatements are announced (GAO (2002)). Had managers published the true numbers, the market value of their companies would have been substantially lower, which would have made it much harder for them to hire and invest a lot.

In their review of the earnings management literature, Healy and Wahlen (1999) argue that "prior research has focused almost exclusively on understanding whether earnings management exists and why." They also point to a crucial question that the academic research has left unanswered: What is the effect of earnings management on the allocation of resources? Our paper addresses this issue and is the first to show that earnings management can explain periods of jobless and investment-less growth.

The paper is organized as follows. Section 1 presents the model. Section 2 presents our firm level data. Section 3 briefly considers insider trading. Section 4 examines the dynamics of employment and investment for fraudulent firms. It contains various subsections with robustness checks and a discussion of alternative interpretations of our results. Section 5 focuses on the dynamics of non-restating firms. Section 6 concludes.

1 Model

We now present a model of earnings manipulation. We show that real inefficiencies arise from the *interaction* of endogenous hiring and investment decisions with the opportunity to manipulate earnings.

1.1 Description of the Model

Technology

The model has two periods, $t = 1, 2$, and a large number of firms whose true profits x_t depend on their true productivity θ and on the amount of labor they hire n_t. The productivity θ is the same in the two periods. For simplicity, we use a Leontief production function and we assume that labor is the only factor of production, supplied at price $w < 1$. Profits are given by

$$x_t = \min(n_t, \theta) - w n_t \ .$$

Half of the firms have a low productivity $\theta = \theta^L$, and half of the firms have a high productivity, $\theta = \theta^H$, with $\theta^H > \theta^L > 0$.[1]. The first best level of employment is simply $n^*(\theta) = \theta$ and the first best true profits are

$$x^*(\theta) = \theta(1 - w) \ .$$

Information

To study earnings manipulation, we assume that the true productivity of the firm is observed only by the manager, and that reported profits can be manipulated. More precisely, investors observe only employment n_t and reported profits y_t, which are equal to true profits plus discretionary accruals, a_t. The investors cannot observe θ nor x_t directly. The risk free rate is normalized to 0, and accruals always have a zero net present value. Hence,

$$\begin{aligned} y_1 &= x_1 + a \ , \\ y_2 &= x_2 - a \ . \end{aligned}$$

[1] The Leontief technology makes the formula easier to read, but the results generalize to any production function that is super-modular in (n, θ). An example is when managers influence the productivity of their companies and output is $y = \theta f(n)$ for some increasing function $f(.)$. A case that would not deliver the same result is $y = \theta + f(n)$ because it makes optimal employment independent of the type of the manager. The evidence supports the super-modular case, since for instance, managers of large companies are paid more than managers of small companies.

Trading and Punishment for Manipulation

Each firm has one share, and all earnings are paid out as dividends. Hence, each stock holder receives y_t in period t. Managers know x, and they own $\alpha \in (0,1)$ shares that they must sell at the end of the first period. If they manage their earnings, managers are caught and punished with some probability. Let γ be the expected punishment. In section 1.3, we extend the model to allow for endogenous trading. Goldman and Slezak (2003) show how to endogenize α in a model with unobserved managerial effort.

1.2 Equilibrium

In our model, the managers privately observe the productivity of their company. For half of them, the news is good, while the other half discovers a low productivity and can be tempted to hide it by managing earnings. Let λ be the fraction of managers of unproductive firms who manipulate (strategy m) and $1-\lambda$ the fraction who report honestly. Let $\hat{\lambda}$ be the market belief about λ. Let us assume for now that the managers of productive firms report honestly. We will return to this issue below. Since n is observable, firms who manipulate must hire just like good ones, therefore

$$n^m = \theta^H,$$

and the associated true profits are

$$x^m = \theta^L - \theta^H w \ .$$

Note that $x^m < x_L^*$ because of the excessive hiring. Discretionary accruals have to make up not only for the fundamental difference in quality $x_H^* - x_L^* = \left(\theta^H - \theta^L\right)(1-w)$, but also for the inefficient allocation of resources $x_L^* - x^m = \left(\theta^H - \theta^L\right) w$. Thus,

$$a = x_H^* - x^m = \theta^H - \theta^L \ .$$

Assuming efficient financial markets, the market value of the firm, as a function of its current earnings, is

$$V\left(y_1, \hat{\lambda}\right) = E\left[y_2 \mid y_1, \hat{\lambda}\right] = \left\{ \begin{array}{c} V_L = x^*\left(\theta^L\right) \text{ if } y_1 < x_L^* \\ V_H\left(\hat{\lambda}\right) \text{ if } y_1 \geq x_H^* \end{array} \right\} , \tag{1}$$

where

$$V_H\left(\hat{\lambda}\right) = \frac{x_H^* + \hat{\lambda} x_L^*}{1+\hat{\lambda}} - \frac{\hat{\lambda}}{1+\hat{\lambda}} a \ .$$

The expected utilities of managers of unproductive firms under strategies o and m are

$$U^* = \alpha V_L \ ; \ U^m = \alpha V_H\left(\hat{\lambda}\right) - \gamma \ .$$

Definition 1 *An equilibrium is a market belief* $\hat{\lambda}$ *such that managers choose* $\max\left(U^*, U^m\right)$ *and* $\lambda = \hat{\lambda}$.

Proposition 1 *There exists a unique equilibrium where the managers of productive firms report truthfully. This equilibrium is partially pooling. The fraction* $\hat{\lambda}$ *of managers of unproductive firms who manipulate increases with the number of shares owned by the managers* α*, with the difference between productive and unproductive firms* $\theta^H - \theta^L$*, and it decreases with the cost of manipulation* γ.

Proof. The equilibrium condition for $0 < \lambda < 1$ is

$$U^* = U^m \Leftrightarrow V_H\left(\hat{\lambda}\right) = V_L + \frac{\gamma}{\alpha} \ ,$$

which leads to

$$\frac{1+\hat{\lambda}}{1-w-\hat{\lambda}} = \frac{\alpha}{\gamma}\left(\theta^H - \theta^L\right) \ . \tag{2}$$

This shows that $\hat{\lambda}$ is unique, and is increasing in α and in $\theta^H - \theta^L$ and decreasing in γ. Note that this implies that $\lambda = 1$ is not an equilibrium. On the other hand $\lambda = 0$ is an equilibrium if $\gamma > \alpha\left(1-w\right)\left(\theta^H - \theta^L\right)$, because the punishment are strong enough to deter manipulation. Finally, it is clear from (1) that managers of productive firms strictly prefer to report truthfully. ■

1.3 Discussion of the Model

In this section, we discuss three issues: the existence of separating equilibria, the impact of endogenous insider trading, and the revelation of information over time. We conclude by presenting the predictions of the model that we are going to test empirically.

Separating equilibria

In the previous section, we have considered only those equilibria in which productive managers report truthfully. With the assumptions that we have made, there are no other equilibria. We know from game theory that separation can happen if there exists an observable action which is relatively more costly for the mimicking type than for the mimicked type, as in Spence (1974). But in our setup, it is not possible for the productive managers to separate because the punishment function γ is a step function: the expected punishment does not increase with the size of the manipulation. Hence, it is not more costly for the unproductive type to report any $y_1 \geq x_H^*$ than it would be for the productive type. This result relies on the assumptions that we have made, since we know that in general, the set of equilibria in general depends on the details of the information structure,[2] and on the functional form for the punishment technology γ.[3] However, one clear result in the empirical literature on earnings management is that stock prices react strongly to announcements of earnings restatements. Therefore, pooling does occur in the real world. Let us emphasize here that we do not pretend to show theoretically that pooling is likely to occur, but rather, that we focus on pooling equilibria because they seem to be empirically relevant.

Endogenous trading

Another important assumption we have made is that managers are exogenously required to sell their stocks. Obviously, the managers of productive firms would like to wait until all uncertainty is resolved to avoid selling their shares at a discount. It is straightforward to extend the model to allow for endogenous trading. Suppose that a fraction δ of managers are hit by liquidity shocks and have to trade. The remaining managers decide to trade or not, based on their private information. Managers who are not hit by a liquidity shock consume at the end of period 2. It is easy to see that managers of productive firms do not trade unless they have to, and that managers who have manipulated always trade. Productive managers are better off waiting since they would have to sell below the market price. Unproductive managers who manipulated their earnings at $t = 1$ are better off trading since their manipulation will be found out at time $t = 2$. Therefore, the value of

[2] See Guttman, Kadan, and Kandel (2004)

[3] It is possible to construct equilibria where good managers separate from bad managers if the probability of detection increases quickly enough with the amount of manipulation.

the firm conditional on reporting high profits and trading at date 1, is

$$V_H\left(\hat{\lambda}, trade\right) = \frac{\delta x_H^* + \hat{\lambda} x_L^*}{\delta + \hat{\lambda}} - \frac{\hat{\lambda}}{\delta + \hat{\lambda}} a \ ,$$

while $V_H\left(notrade\right) = x_H^*$. The equilibrium condition $V_H\left(\hat{\lambda}, trade\right) = V_L + \frac{\gamma}{\alpha}$ leads to

$$\frac{\delta + \hat{\lambda}}{\left(1 - w\right)\delta - \hat{\lambda}} = \frac{\alpha}{\gamma}\left(\theta^H - \theta^L\right) \ . \tag{3}$$

Comparing this equation to equation (2) in section 1.2, we see that endogenous trading reduces the incentives to manipulate because of the price impact. In particular, the equilibrium level of manipulation always satisfies $\hat{\lambda} < \left(1 - w\right)\delta$, so manipulation must disappear when δ goes to zero. Like in the noise trading literature, a higher δ induces more insider trading by decreasing the price impact. In mapping the model to the data, it is important to keep in mind that in reality, there is no final period where the true value of the firm is perfectly revealed. It is clear that managers must liquidate their positions at some point during their lifetime, and as long as some uncertainty remains, our qualitative results will continue to hold.

Multiple periods

Finally, there is the issue of what would happen in a multiperiod framework. With an infinite horizon, our model would become equivalent to a Ponzi scheme. The stock of accruals would grow over time as managers continuously borrow against the future in order to keep posting high earnings. We have assumed that managers use accruals to manipulate the perceived value of their firm, but accruals are clearly not the only way to do so (see for instance Bergstresser, Desai, and Rauh (2006)). However, irrespective of the technical details, it is clear that the manipulation, while likely to be effective in the short-run, cannot go on forever. This intuition is supported in the data: earnings are typically restated 7 quarters after the beginning of the suspicious period.

Empirical predictions

In the remaining of the paper, we use firm level data to test the empirical predictions of the model. Bergstresser and Philippon (2006) and Burns and Kedia (2004), among others, have already confirmed the comparative statics with respect to α. The straightforward extension

of our model to endogenous trading, discussed above, is consistent with the evidence in Roulstone (2005), that insider purchases are higher before the release of good news, and lower before the release of bad news. In the empirical analysis below, we confirm these prior results by showing higher exercises during periods when earnings are being manipulated. The other empirical predictions of our model that have not been tested in the literature are summarized below:

1. Firms managing earnings hire and invest more than predicted by their technology. Hence, one would expect to see these firms shrink after they are exposed.

2. As firms manage earnings to be valued like successful firms, the excessive hiring and investment is not random but consistent with that of mimicked successful firms.

3. The magnitude of earning manipulations should be related to the observed distortions in hiring and investment.

4. Earning manipulation decreases with γ, the expected costs of manipulation.[4]

We first describe the data. Next, we briefly discuss insider trading, an area where much work has already been done. We then turn to the dynamics of hiring and investment, that we are the first to investigate. We then explore the other empirical implications of the model before concluding with a discussion of alternate explanations for our findings.

2 Data

To capture alleged fraudulent accounting, we use the list of firms that restated their earnings in the late 1990s. This list was compiled by the General Accounting Office (GAO) in 2002 (GAO (2002)). The GAO "identified 919 financial restatements by 845 public companies from January 1, 1997 to June 30, 2002, that involved accounting irregularities resulting in material misstatements of financial results." These financial restatements occur when a company, either voluntarily or prompted by its auditors or regulators, revises public financial information that was previously reported.[5] Six-hundred-forty-five of these companies

[4] Piotroski and Roulstone (2005) provide evidence on the impact of potential legal liability costs on insider trading decisions.

[5] These announcements exclude stock splits, changes in accounting principles, and other restatements that were not made to correct mistakes in the application of accounting standards.

were publicly traded. The distribution of announcements per year shows a clear upward trend (see **Table 1**). The number of identified restatements rose from 92 in 1997 to 225 in 2001. "The proportion of listed companies on NYSE, Amex and NASDAQ identified as restating their financial reports tripled from 0.89% in 1997 to 2.5% in 2001. From January 1997 through June 2002, about 10% of all listed companies announced at least one restatement."(GAO (2002)). Moreover, later restatements involved larger firms: the average market capitalization of restating companies quadrupled between 1997 and 2002, from $500 million to $4 billion, while the average size of listed companies increased only about 60% over the same period.

The GAO also reports the reasons for the restatements. Errors in revenue recognitions account for roughly 40% of the cases while those due to improper cost accounting explain 16%. Issues with loans, like write-offs, reserves, and bad loans account for 14% of the cases. Issues with assets and inventories, like goodwill, write downs, and valuation account for another 9% of restatements. The remaining 20% of cases are linked to R&D, M&A, securities (Enron for instance), reclassifications of debt payments and related party transactions. Only 16% of the restatements can be formally attributed to external parties' actions like the SEC or independent auditors. Further, many firms do not mention in their reports the real reason for their restatements, unless they are somehow forced to do so (see GAO for details). Restatements are not fully anticipated by the market; the market-adjusted return over the three trading days surrounding the initial announcement is -10%. For the 575 restatements for which six months of data were available around the announcement, the six month abnormal holding period return was -18%.

We match the GAO data to COMPUSTAT through company name. Out of the 645 publicly traded companies, 560 firms were covered by COMPUSTAT. For 539 firms, we were able to obtain the beginning and end dates of the restated period, in addition to the date on which the restatement was announced. The restated period or the fraudulent period is the period for which the financial data was eventually restated. This restated period, over which the fraud was allegedly committed, lasts for an average of five quarters (see **Table 1**). It takes an average of two quarters from the end of the restated period to the announcement of the restatement. We also collected data on the size of the restatement.[6] We were able

[6] Size of restatement is the average annual impact of the restatement on net income.

to obtain this information for 396 firms. The average ratio of restated earnings over lagged sales is -6% and 80% of the restatements are negative, i.e., involve negative revisions to reported net income. This variable is winsorized so that the maximum is no more than +1 and the minimum no less than -1.

Table 1 also displays the summary statistics for the other variables of interest. The growth rates reported are the 1-year log differences and have been winsorized so that the maximum is no more than +1 and the minimum no less than -1. To capture hiring decisions we calculate the growth rate of the number of employees (COMPUSTAT Data Item 29), which, for non-restating COMPUSTAT firms over the period 1991 to 2003, was 4%. To capture investment decisions, we look at the growth rate of property plant and equipment (COMPUSTAT Data Item 8). The average growth in property, plant, and equipment for non-restating COMPUSTAT firms was 7% per year. The second measure of investment activity that we examine is the ratio of capital expenditures (COMPUSTAT Data Item 30) to property plant and equipment. According to these three measures, restating firms grew slightly faster that non-restating firms over the whole sample, but the differences are very small relative to the standard deviations of these variables.

The unconditional dynamics of restating and non-restating firms are also remarkably similar with respect to the growth rate of market values and sales. We use sales per employee to measure labor productivity. The growth rate of sales per employee is 5% for both restating and non-restating firms. We also compute a measure of total factor productivity growth (henceforth, TFP) by estimating the shares of labor and capital for each industry, at the two digit SIC level.[7] To capture insider trading activity, we get data on CEO option exercises from EXECUCOMP. Option exercises are captured by the ratio of the value realized from option exercises normalized by the total value realizable from options. The total value

[7] Sales are deflated using the GDP deflator, while PP&E are deflated using the non-residential investment deflator. For firm i in industry j at time t, with deflated sales s_{it}, number of employees n_{it} and deflated PP&E k_{it}, we define the growth of TFP as

$$dTFP = d\log s_{it} - \alpha_j d\log n_{it} - (1-\alpha_j)\, d\log k_{it} \ ,$$

where α_j is the industry-mean ratio of compensation of employees over operating income + compensation of employees. Needless to say, there are issues with measuring productivity in COMPUSTAT. We do not have firm specific price deflators and we do not have good measures of inventories or intermediate inputs. Nonetheless, when we average across the firms in the sample, we find that this measure yields a good estimate of aggregate TFP growth computed from the NIPA. In the rest of the paper, we will always report the results for both TFP and labor productivity (deflated sales per employee).

realizable from options is the sum of the value realized from option exercises and the value of exercisable options. We find no difference in the unconditional value of this ratio between restating and non-restating firms. Overall, **Table 1** shows that the unconditional dynamics of restating and non-restating firms are quite similar. We show below that the conditional dynamics are remarkably different.

3 Insider Trading

Since there already exists a large literature on earnings management and insider trading (see for instance Bartov and Mohanram (2004) or Roulstone (2005)), and since our main focus is on the investment-employment dynamics, we examine only briefly the prediction that insider trading is higher during the restated period.

Data on option exercises of CEOs is obtained from EXECUCOMP. Our sample consist of all the firms in EXECUCOMP with non-missing value for the variables of interest and with at least one year of data in the before, during and after period. We estimate

$$y_{it} = \beta^{before} 1_{t<\tau(i)} + \beta^{during} 1_{t\in\tau(i)} + \beta^{after} 1_{t>\tau(i)} + \phi_t + \gamma x_{it-1} + u_{it} \ .$$

In these regressions, y_{it} is the ratio of the number of options exercised to total options exercisable. We also examine the ratio of the value realized from option exercises over the total value realizable from options and find the results are similar. $\tau(i)$ is the restated period for firm i, and $\tau(i) = \varnothing$ for firms that do not restate. The RHS variables include time dummies ϕ_t as well as some control variables $x_{i,t-1}$. We control for industry patterns by including average option exercises for all firms in the same two-digit SIC with data on EXECUCOMP for that year. We also control for the number of options outstanding, for past stock performance and lagged Tobin's Q..[8] **Table 2** shows that the β^{during} is positive and significant implying that the CEOs of restating firms exercised relatively more options than the CEOs of comparable firms in their industry during the period in which they were misreporting. The magnitude of this effect is stable across these specifications, between 5% and 6%. This is economically large given that the unconditional mean of y is 18% (see **Table 1**). The higher exercises by CEOs during misreported periods relative to control firms, and

[8] Options outstanding control for the need to diversify firm risk. Executives with large option holdings and large option grants are likely to have higher exercises. Ofek and Yermack (2000).

relative to exercises in the prior period indicate that managerial optimism is unlikely to explain the observed dynamic of employment and investment that is documented below.[9] We now move on to the unexplored area of employment and investment dynamics.

4 Earnings Restatements and Firm Dynamics

4.1 Main Result

We want to compare the dynamics of hiring and investment for restating firms around the restated period. We first create a control group of non-restating firms that are matched in age, industry and initial size. For every restating firm, we choose all non-restating firms that appear in COMPUSTAT in the same year as the restating firm, or in 1991 for the firms already present at the beginning of our sample. We then select non-restating firms that operate in the same industry (defined as two-digit SIC code), and that are in the same initial book asset quintile. We exclude observations in government, health and education sectors and firms which have less than three observations for asset and sales growth over this time period. We adjust the variables of interest by subtracting the mean of this control group.

$$\hat{g}_{it} = g_{it} - \bar{g}_{C(i)t} \ , \tag{4}$$

where $C(i)$ is the control group for firm i.

Figure 2 plots the mean adjusted growth rates, as in equation (4), for four key variables: total market value, number of employees, PP&E and TFP. All these variables are constructed with the data as reported by the firm in real time, and do not include the effects of the restatements. The horizontal axis measures time in years relative to the restated period, which is time 0 by definition. Time +1 is one year after the end of the restated period, and time -1 is one year before the beginning of the restated period. Note that the length of the restated period varies across firms, so time 0 may include more than one year of data for some firms. Also note that 97% of the restatements are announced either at time 0 or at time 1. The figure shows that the market value of restating firms grew at a faster rate than

[9] These results are broadly consistent with the evidence in existing literature, though a recent paper by Agrawal and Cooper (2006) find no evidence of abnormal equity sales in the misreported period. As we model exercises at the executive level and control for his portofio holdings, rather than model exercises for executives as a group, we are more likely to capture abnormal exercises.

that of the control group before the restated period, at the same rate during the restated period, and more slowly afterwards.

A similar picture emerges with respect to growth in PP&E and the number of employees. On the other hand, productivity is flat. One must keep in mind, however, that the sales were probably over-stated, so that true productivity probably increased.

We now turn to more formal econometric tests to substantiate this evidence. Consider the econometric equation

$$\hat{g}_{it} = \beta^{before} 1_{\tau(i)-2 \leq t < \tau(i)} + \beta^{during} 1_{t \in \tau(i)} + \beta^{after} 1_{\tau(i) < t \leq \tau(i)+2} + u_{it} \ , \tag{5}$$

where $\tau(i)$ is the restated period for firm i. A positive estimated coefficient β^{during} implies that the restating firms grew faster than comparable firms in their industry during the period in which they were misreporting. The coefficients β^{before} and β^{after} show if they grew differently before and after the suspicious period. In these comparisons, the null hypothesis is that $\beta = 0$. We can also compare β over time to see if the dynamics of restating firms changed significantly around the restated period. In this case, the null hypothesis is that $\beta^{before} = \beta^{during}$, for instance.

The results are presented in **Table 3.1**. The growth of employment in fraudulent firms is 4.1% higher during the fraudulent period. Consistent with **prediction 1**, we find that the growth of employment is significantly lower after the restatement. A similar dynamic is seen with investment activity. The growth rate of investment, i.e., PP&E is about 4.4% higher during the restated period and 5.6% lower after the restated period. The same pattern is seen when we examine capital expenditures normalized by PP&E. McNichols and Stubeen (2006) also report similar higher investment during class periods by firms under litigation for accounting manipulation. It appears that restating firms were growing rapidly in the years prior to the restated period. These firms most likely misreported in order to continue portraying themselves as high growth firms. This is reinforced by the dynamics of market value. The growth in market value was 7.5% higher before the restatement and 6.7% lower afterwards, and it was the same during the restated period, suggesting that the firms did not surprise the market during this period.[10]

[10] A similar picture emerges when we examine analyst forecasts obtained from IBES for 408 restating

The null hypothesis that β^{during} is the same as β^{after} can be rejected at less than 1% level for all the variables except productivity. We can safely conclude that growth rates of assets, employees, capital and market values were higher during the restated periods than after, as predicted by the model. Interestingly, the growth rates of TFP and labor productivity are not significantly different across firms and over time. As the period after the restatement is not associated with lower productivity, it is unlikely that restatements were the result of negative TFP shocks. Moreover, as the sales in the restated period were inflated by fraudulent accounting for a large fraction of firms, the true productivity probably increased after the restatement.

4.2 Subsample Analysis

Here we examine the robustness of our results in various subsamples.

We first ask whether our results are driven by the steep market decline in the Spring of 2000.[11] To do so, we estimate our econometric equation (5) on the sample of firms that announced a restatement prior to the first quarter of 2000. As seen in the bottom half of **Table 3.1** we find similar results in this subsample. We conclude that our results are not driven by the market crash of 2000.

We next split our sample according to industry growth rates before or after the restatements. The industry growth rate is defined as the average growth in market value of firms in that industry. The idea is similar in spirit to our first robustness check, but it is broader in scope. Instead of asking whether our results are driven by a particular aggregate shock (i.e. Spring 2000), we test the sensitivity of our results to business conditions in general. There are two ways to split the sample: based on industry growth after the restatements (**Table 3.2**), or based on industry growth before the restatements (**Table 3.3**).

In **Table 3.2**, we sort the restatements based on ex-post industry performance. This allows us to ask whether our results are driven by restatements that take place in industries experiencing large negative shocks. This test is closely linked to the one in table 3.1, but it uses industry variation, and could therefore be more powerful. We find that our results

firms. For the two years prior to the restated period, in 62% of quarters restating firms beat analyst forecast by an average of 18 cents. During the restated period, a similar fraction of quarters (59%) were associated with exceeding analyst forecasts though the mean forecast error was only 6 cents. This is indicative of firms managing earnings to just beat analyst forecasts and continue to portray themselves as growth firms.

[11] We are grateful to a referee for suggesting this analysis.

hold in both subsamples. In fact, statistical significance is even higher when restatements are *not* accompanied by negative industry shocks.

In **Table 3.3**, we sort the restatements based on industry performance in the two years prior to misreporting. This test is different from the other two. Based on our model, we would expect the results to be stronger in fast growing industries, since the incentives to manipulate are stronger when P/E ratios are higher. **Table 3.3** confirms this prediction. The results continue to hold in industries with low growth rates, but they are stronger in industries with high growth rates.

4.3 Mimicking strategy

Although the growth rate of employment and investment for misreporting firms is higher than the average growth of age-size-industry matched control firms, it is not arbitrary. **Prediction 2** says that the earnings-employment-investment dynamics of restating firms should be consistent with that observed in "mimicked" firms. To test this prediction, we select from all the industry-age-size matched control firms those that also had similar growth in market value (within 70% to 130% of the restating firm) in the years prior to the misreporting. **Table 4** shows that the growth rates of employment and investment of restating firms were similar to those of mimicked firms in the years prior to, as well as, during the misreported period. As the z-statistics indicate, it would not have been possible for an econometrician to tell apart manipulating from mimicked firms prior to the announcements. Both mimicked and manipulating firms grew faster than their industry averages by similar amounts along all observable dimensions. As predicted by the requirement of consistency, the hiring and overinvestment in misreported years is benchmarked to that of mimicked firms with similar market value growth.

We now turn to the sub-sample of firms for which we were able to collect information on the size of the restatement in order to test **prediction 3**. As we model the behavior of low productivity firms misreporting to pool with high productivity firms, we restrict the sample to firms that announce income decreasing restatements. We investigate whether larger restatements were associated with larger growth in employment and investment during the misreported years and subsequent larger drops in employment and investment upon the announcement of the restatement. **Table 5** shows that this is indeed the case. Both the

excessive employment and investment relative to control during periods of earning management, as well as, the subsequent shrinkage of employment and investment upon discovery is related to the magnitude of earnings management. On the other hand, we do not see much effect of the magnitude of restatement on sales growth and productivity. This relationship between the magnitude of the earnings management and the degree of distortions in employment and investment clearly indicate that the two are related, and that it is unlikely that a similar dynamic of employment and investment could happen without earnings manipulation.

4.4 Predicting Restatements: the Role of Governance

We now attempt to test **prediction 4** that higher values of γ decrease the incidence of fraud. It is difficult to find a quantitative measure of γ, the expected penalties from restating, let alone one that is available for a large sample of firms. Presumably, managers of well governed firms are more likely to be discovered in case of earnings manipulation. Thus, we would expect these managers to face a higher γ. Recent work by Agrawal and Chadha (2005) finds that board characteristics, especially the level of financial expertise of the audit committee, affects the propensity to misreport. We complement their work by using another proxy of good governance, proposed by Gompers, Ishii, and Metrick (2003). As good governance is likely to increase γ, we would expect a negative relation between governance and earnings manipulation. We test this prediction by running predictive logit regression in the cross-section of firms present in our sample in 2002

$$P\left(restat_{i,02}\right) = F\left(\gamma' X_{i,96} + \alpha_{I(i)}\right) \ ,$$

where $restat_{i,02}$ is a dummy variable for any restatement by firm i between 1997 and 2002, $F\left(.\right)$ is the logistic function, $\alpha_{I(i)}$ is a set of 2-digits industry dummies, and $X_{i,96}$ includes age, assets and Tobin's Q in 1996, as well as governance variables measured in 1995.[12] The governance variables come from the Institutional Investor Research Center (IRRC). IRRC follows 24 governance provisions that appear beneficial to management, and which may be harmful to shareholders. Gompers, Ishii, and Metrick (2003) have used all 24 provisions to construct an index of bad governance, and have shown that the index is negatively correlated

[12] The first year before the beginning of our sample where the data is available.

with Tobin's Q. Recently, Bebchuk, Cohen, and Ferrell (2004) have argued that staggered boards, limits to shareholder bylaw amendments, super-majority requirements, poison pills and golden parachutes account for most of the correlation.

In our data set, 770 firms have IRRC data available in 1995, and 99 of them restated. **Table 6** shows that firms with poor governance in 1995 were more likely to restate between 1997 and 2002 than comparable firms in the same industry. Among the individual provisions, we find that classified boards are significant. Of course, we do not infer causality from these reduced form regressions, as we cannot rule out omitted variable bias. It is possible that good firms, or honest managers, would be more likely to choose good governance provisions, and at the same time would be less likely to commit frauds. Nonetheless, these results show that there is information in the governance provisions studied by Gompers, Ishii, and Metrick (2003). The timing of our regression also rules out the issue of reverse causality to the extent that the sample of accounting problems examine here could not be anticipated in 1995.

4.5 Alternate Interpretations

The empirical facts that we have documented are all consistent with the model of section 1. In this section, we will specify three alternate interpretations and discuss why they are not consistent with the overall evidence.

First, one might argue that earnings restatement does not reflect genuine manipulations, but rather excessive optimism by some managers. This view is inconsistent with the explicit selection criteria of the GAO sample, that of including only those restatements that involve accounting irregularities resulting in material misstatements. Nonetheless, for the sake of the argument, let us assume that some managers are overly optimistic and that this leads them to misstate their financial results. This might explain the relatively higher growth rates before the restatement, and relatively lower growth rates afterwards. However, this optimistic interpretation is inconsistent with the fact that managers exercise more options during the misreported period.

Though the data shows that it is unlikely that managerial optimism explains the observed hiring and investment dynamic, the presence of managerial optimism does not refute our central point. Optimism generates a preference for hiring and investment, and con-

sistency requirement for the signaling equilibrium creates a secondary demand for high numbers and earnings manipulation. Even with managerial optimism, overinvestment and hiring are accompanied by earnings manipulation, which is our basic point.

Second, one could argue that all firms are ex-ante identical, and that firms with negative productivity shocks are forced to restate, while other firms with positive shocks do not. We have shown, however, that measured productivity growth does not decrease following the restatements. As the measured productivity during the misreported period is likely overstated, true productivity probably increased after the restatement. Moreover, these shocks are not randomly distributed among firms, since restating firms were growing faster than their industry peers during the restated period, and since their managers were selling more than the usual amount of shares.

Lastly, one might argue that exogenous over-valuation is responsible for the higher growth of employment and investment and earnings manipulation merely a side effect of this over-valuation. The issue here is not whether manipulating firms were over-valued, but whether the same dynamics of investment and employment would have happened without manipulation. Several reasons lead us to conclude that the dynamics would have been very different if earnings manipulation had not been possible.

First, we match our restating firms to a control group of firms in the same industry, and with similar characteristics. If the exogenous over-valuation is industry-age-size specific it should also affect the control group and be irrelevant to us. For instance, our results cannot be explained simply by a bubble affecting young firms in the computer industry. As a similar hiring and investment dynamic is observed for restatements announced prior to Spring 2000 and also in industries with below median growth rates, the results are not driven by market level or industry level euphoria.

Second, if earnings management was not important, why should managers have engaged in this activity at significant personal costs to them? The personal costs to the manager in the form of reduced opportunities in the labor market are substantial as documented by two recent papers (Desai, Hogan, and Wilkins (2005), Karpoff, Lee, and Martin (2005)). Clearly these managers believed that earnings manipulation was effective, otherwise they would have abstained from it.

Third, we have quantitative evidence on the effect of the manipulation on the value of

the firm. Event studies reported in GAO (2002) show that stock prices drop by an average of 10% on the day that the restatement is announced. In other words, had the market known before that the reported earnings were not correct, the valuation of the firm would have been 10% lower, on average. This is exactly the number that we report in **Figure 2**: a negative 10% growth in market value over the year following the end of the restated period.

We conclude that both market participants and managers believed that firm value was significantly affected by earnings manipulation. This speaks directly to the link between manipulation and the dynamics of firm value, and therefore investment and employment. These dynamics would not have been the same if the firm had published the true numbers throughout the period.

However, while manipulation is a necessary condition, it is not a sufficient one: unobserved technology shocks are also necessary to create room for asymmetric information in the first place. Our findings therefore support the following interpretation, which is simply one way to rephrase the model of section 1. Some firms, previously successful, discover that their potential growth has slowed down. To avoid, or at least to delay, the expected drop in market value, the managers engage in earnings manipulations and continue hiring and investing as before, while at the same time selling their stocks. It is a decline in growth opportunities that leads to earnings management, but it is earnings management that allows the misallocation of resources.[13]

5 Aggregate Employment Growth

We now investigate the aggregate impact of earnings manipulations. The evidence presented here goes beyond the predictions of the model of section 1, but it is relevant for two reasons: first, to show that the mechanisms that we have emphasized do not wash in the aggregate, and second, to stimulate future research.

[13] In our setup, the manager is motivated by the desire to sell his stocks at a high price. Of course, one could write a similar model where the manager is motivated by the desire to keep his job. The implications would be similar.

5.1 Restating Firms

A clear picture of the raw data can be obtained by looking at the dynamics of firms that announced a restatement in 2000 (111 firms) and 2001(120 firms). The number of people employed in these 231 restating firms over the period 1997 to 2002 is displayed in **Figure 3**. The left panel of the figure compares the 231 restating firms to aggregate non-farm payrolls obtained from the Bureau of Labor Statistics (BLS). Employment in restating firms went up by 0.5 million (+25%) between 1997 and 1999, and down by 0.6 million between 2000 and 2002. Over the same period, non-farm payrolls went up by 6.7% and then down by 1.5%. The relative increases and decreases in employment for restating firms are clearly much larger than for the economy as a whole. A potential concern in this analysis is that some firms drop out of the sample after the announcement of the restatement, sometimes due to delisting, sometimes due to bankruptcy.

In the left panel, we implicitly assign zero employees to firms that drop out. For instance, complete data for Enron is available only until 2000. To the extent that some firms drop out of the sample, but, unlike Enron, continue operating, the left panel may overestimate the true dynamics. To address this issue, we construct a constant sample of firms for which we have complete data over this period. This constant sample comprises 74 firms that restate in 2000 and 96 firms that restate in 2001. The right panel of **Figure 3** compares the employment in these restating firms to a constant sample of non-restating firms in COMPUSTAT. Restating firms grew more rapidly than non-restating firms from 1997 to 1999 and declined much faster afterwards. The right panel also gives a sense of the coverage of our data set: a bit less than a third of total non-farm payrolls.[14] Clearly, the truth lies somewhere in between the left panel and the right panel. If most restating firms are like Enron, then the left panel is the better approximation. If most restating firms continue operating with a reduced, but still significant, number of employees, then the right panel is more appropriate.

[14] But a much larger share of output (more than 1/2), since only large firms with relatively high labor productivity are included.

5.2 Dynamics of Non Restating Firms

The dynamics of restating firms, that make them grow faster than comparable firms in the restated period and slower afterwards, is also likely to impact other non-restating firms in their industry. Some non-restating firms surely engaged in earnings management, but probably to a lesser extent than firms that eventually had to restate. In this case, our control group is not valid and our results under-estimate the true impact of earnings management. Moreover, investors may draw negative inferences about all firms that belong to an industry where many accounting frauds have been revealed, even if most of the firms were actually honest. This suggests that the announcement of a restatement could have negative implications for other, non-restating, firms in the industry. On the other hand, there are equilibrium reasons to expect that non-restating firms might actually benefit from the announcements of restatements by their competitors. If they did not themselves manage their earnings, and if investors do not become suspicious of them, non-restating firms should expand in response to the negative shocks affecting other firms in their industry. They should try to steal market shares from the restating firms, and hire some of the laid-off workers.

We investigate the impact of restatements on non-restating firms by creating a panel of industries at the 2-digit SIC level using only the non-restating firms.[15] For each variable of interest, we take the mean across non-restating firms in a particular year and industry as our LHS variable, $\bar{g}_{jt}$. We then estimate

$$\bar{g}_{jt} = \beta \bar{R}_{j,t} + \phi_t + \phi_j + u_{jt} \ , \ t = 1991..2003 \ , \tag{6}$$

where $\bar{R}_{j,t}$ is the fraction of firms in industry j that restated up to time t. We also include year and industry dummies. We estimate this for all the relevant variables studied earlier. The results are in **Table 7**. Non-restating firms grow more slowly when they belong to an industry that had a lot of announced restatements in the preceding years. Interestingly, sales per employee and TFP grow significantly faster following a wave of restatements. In other words, fraudulent industries are characterized by high labor productivity growth together with negative employment and investment growth, even for firms that did not have to restate their earnings. The fact that sales per employee and TFP increase is not

[15] At least between January 1997 and June 2002. It is possible that some of these firms will restate after June 2002.

consistent with the interpretation of restatements simply as negative productivity shocks.

The potential impact of these industry dynamics on overall employment is large. To get a sense of the magnitudes involved, we can obtain the predicted drop in employment by multiplying the estimated β in the above regression with the average number of restating firms $\bar{R}_t$ across all industries. **Figure 4** plots the employment growth predicted by the evolution of the average $\bar{R}_t$, and compares it to the actual employment growth between 1995 and 2002. Note, however, that general equilibrium effects mean that our coefficient from the cross-section of industries will over-estimate the true impact on aggregate employment growth. The cross-sectional estimate is obtained for given factor prices (labor, capital, intermediate inputs). In the aggregate, a drop in labor demand, for instance, would drive down the wage, and mitigate the actual drop in employment.[16]

6 Conclusion

Earnings management is accompanied by distortions in the allocation of resources in the economy. When hiring and investment decisions are observable, managers of firms with low productivity not only manage earnings but also hire and invest too much in order to mimic good managers. When they are caught and forced to restate, their firms shrink quickly. We find strong support for these theoretical predictions in firm level data. Restating firms grow at significantly higher rates during the periods where they misreport, relative to firms matched on age, size, and industry. Growth in restating firms is significantly slower than growth in matched firms in the years after the restatement.

In light of the overall evidence, we would argue that earnings manipulations can cause an amplification of business cycles and a misallocation of resources. The publicly traded firms that restated their earnings in 2000 and 2001 lost between 250,000 and 600,000 jobs between 2000 and 2002. Some of these jobs would have been lost in any case, but earnings management allowed the manipulating firms to grow faster than their peers in the years prior to the restatements, thereby increasing the cyclical pattern. The misallocation is temporary at the firm level, since it is not possible to conceal the true profitability of a business forever,

[16] Earnings management is a particular case of imperfect governance. Philippon (2004) studies imperfect governance in a standard real business cycle model, calibrated to the US economy, and finds that imperfect governance amplifies business cycle dynamics.

but there is a permanent misallocation in the aggregate, since some firms are manipulating in any given period. Moreover, we find that other, non-restating firms did not expand their employment and investment to compensate for the losses of the restating firms. In fact, in industries with a high incidence of restatements, non-restating firms also exhibit slow growth in investment and employment, together with strong productivity growth.

The dynamics of hiring and investment in restating firms breaks the link between productivity and factor demands. The period after the restatement is characterized by strong productivity growth, while labor and capital demands fall. Earnings management can impact aggregate dynamics through two opposing channels. On the one hand, the inefficient allocation of resources among firms, created by earnings management, tends to reduce aggregate activity. On the other hand, greater hiring and investment by misreporting firms tend to increase aggregate activity. Thus, waves of earnings restatements can be followed by periods of jobless growth and low investment. A full understanding of the macroeconomic implications is a task for future research.

Appendix

A A Brief Review of the Literature on Earnings Management

In this section, we discuss previous research on earnings management, following Dechow, Kothari, and Watts (1998). Economists write models about cash flows, but in practice, investors look at earnings. Why? Because earnings forecast future cash flows. Consider a firm, and assume that sales follow a random walk

$$s_t = s_{t-1} + \varepsilon_t \ .$$

Earnings (assuming a constant profit rate π) are

$$e_t = \pi s_t \ ,$$

and we assume that accounts receivable (rec_t) and payable (pay_t) are constant fractions of sales and total costs

$$rec_t = \alpha s_t \ , \text{and } pay_t = \beta \left(1 - \pi\right) s_t \ .$$

In this simplified setup, cash flows are simply given by

$$\begin{aligned} c_t &\equiv e_t + \Delta pay_t - \Delta rec_t \\ &= \pi s_t + \left[\beta \left(1 - \pi\right) - \alpha\right] \varepsilon_t \ , \end{aligned}$$

so we see that

$$E_t \left[c_{t+1}\right] = \pi s_t = e_t \ .$$

To forecast future cash flows, and therefore to compute the value of the firm, we start with earnings. The value of the firm at the end of period t is

$$V_t = \frac{e_t}{r} \ ,$$

where r is the risk-adjusted discount rate. Dechow, Kothari, and Watts (1998) expand this model to take into account other important features of accruals, such as depreciation, and show that, empirically, accruals are indeed the better predictors of future cash flows (See Barth, Cram, and Nelson (2001)).

What we would like the reader to take away from this brief discussion is that earnings forecast cash flows and that, to a first order, investors are right to focus on earnings when assessing the value of a firm. The problem, however, is that earnings can be manipulated. For instance, accruals, defined in our example as $\Delta rec_t - \Delta pay_t$, cannot be verified. Investors need to trust a manager who claims high earnings coming from large future receivables. Unfortunately, there are documented cases of earnings management. The evidence of earnings management is manifest in the existence of restatements, as well as, shareholder litigation based on the accusations of earnings management.

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Table 1 : Descriptive Statistics

	Variable	Obs	Mean	Std. Dev.	Min	Max
Non Restating Firms	Value Realized / Exercisable Value	12393	0.18	0.29	0	1
	Book Assets ($)	97560	3869	28938	0.20	1264032
	Age (years)	97560	12.76	12.18	1	53
	Market Value (growth rate)	79649	0.05	0.42	-1	1
	Sales (growth rate)	97560	0.11	0.36	-1	1
	Number of Employees (growth rate)	81133	0.04	0.30	-1	1
	Prop. Plant & Equip. (growth rate)	92949	0.09	0.37	-1	1
	Cap. Exp./ PPE	83625	0.32	0.30	-0.52	1
	Total Factor Productivity (growth rate)	78445	0.04	0.30	-2	2
	Sales per Employee (growth rate)	81133	0.05	0.32	-2	2
Restating Firms	Value Realized / Exercisable Value	1358	0.18	0.29	0	1
	Book Assets ($)	5565	3319	22319	0.25	705983
	Age (years)	5565	14.62	13.82	1	53
	Market Value (growth rate)	5039	0.06	0.44	-1	1
	Sales (growth rate)	5565	0.12	0.35	-1	1
	Number of Employees (growth rate)	5019	0.06	0.32	-1	1
	Prop. Plant & Equip. (growth rate)	5397	0.10	0.39	-1	1
	Cap. Exp./ PPE	5036	0.36	0.30	-0.08	1
	Total Factor Productivity (growth rate)	4895	0.04	0.29	-2	1.83
	Sales per Employee (growth rate)	5019	0.05	0.30	-2	2
	Reported Length of Restated Period (quarters)	539	4.70	3.71	1	20
	Delay between End of Restated Period and Announcement (quarters)	539	2.21	2.19	0	22
	Restated Earnings over Lagged Sales	396	-0.06	0.20	-1	1
	Distribution of Restatement Announcements by Year	year	Freq.	Percent		
		1997	63	11.01		
		1998	65	11.36		
		1999	114	19.93		
		2000	123	21.50		
		2001	138	24.13		
		2002	69	12.06		
		Total	572	100		

Note: Value Realized / Exercisable Value is (value realized from options exercised) / (value realized from options exercised + value of exercisable options) from EXECUCOMP. Age is current year minus first year the firm appears in COMPUSTAT. Sample period is 1991-2003.

Table 2 : Insider Trading

The table displays the results from a tobit estimation of exercises by top executives. Before is a dummy for years preceding the restated period. During is a dummy for restated years. After is a dummy for years following the restated period. All regressions include year fixed effects. The sample period is 1991-2003. Restating firms were included if they had at data available for at least one year before, during and after misreporting. The control is matched on two digit SIC and size. Coefficients are in bold; t-statistics are below the coefficients.

Dependent Variable	Number of Options Exercised over Total Exercisable Options			Value Realized from Options Exercised over Value of Exercisable Options
	(i)	(ii)	(iii)	(iv)
Before	**0.0176**	**0.0175**	**0.0197**	**0.0166**
	1.19	1.18	1.36	0.91
During	**0.0510**	**0.0503**	**0.0655**	**0.0669**
	2.08	2.05	2.72	2.19
After	**-0.0565**	**-0.0543**	**-0.0231**	**-0.0170**
	-2.26	-2.17	-0.94	-0.54
Average industry exercises		**0.890**	**1.675**	**0.887**
		1.66	3.16	1.63
Options outstanding			**0.002**	**0.003**
			3.33	3.46
Past year returns			**0.0003**	**0.0003**
			4	3.05
Tobin Q			**0.0324**	**0.0389**
			12.93	12.33
Pseudo R^2	0.01	0.01	0.0413	0.03
N	6029	6029	5949	5949

Table 3.1 : Adjusted Dynamics of Restating Firms

The dependent variables are relative to the mean of a control group, matched by size, age, and industry. Before and Afer are dummies for the 2-year periods before and after the restated period. During is a dummy for restated years. Coefficients are in bold, t-statistics are below the coefficients. Standard errors are robust and corrected for firm level clustering.

Complete Sample: 1991-2004

Dependent Variable	Growth of Market Value	Growth of Sales	Growth of Employees	Growth of Prop. Plant & Equip.	Cap. Exp./ PPE	Growth of TFP	Growth of Sales per Employee
Before	**0.075**	**0.069**	**0.059**	**0.058**	**0.043**	**0.004**	**0.009**
	5.46	6.51	5.3	4.76	4.26	0.44	1.06
During	**-0.003**	**0.034**	**0.041**	**0.044**	**0.043**	**-0.007**	**-0.002**
	-0.2	2.86	3.48	3.33	4.11	-0.66	-0.24
After	**-0.067**	**-0.045**	**-0.044**	**-0.056**	**-0.023**	**0.004**	**0.002**
	-5.48	-3.85	-4.41	-4.66	-2.66	0.44	0.2
R^2	0.02	0.025	0.026	0.023	0.022	0	0
N	2745	2976	2656	2886	2669	2588	2656
				p-values			
Before=During	*0.001*	*0.03*	*0.2*	*0.43*	*0.93*	*0.56*	*0.53*
During=After	*0.001*	*0*	*0*	*0*	*0*	*0.55*	*0.98*

Pre Market Crash Sample: Restatements Announced Before Spring 2000

Before	**0.078**	**0.061**	**0.059**	**0.047**	**0.057**	**-0.008**	**-0.002**
	3.48	3.55	3.31	2.26	3.17	-0.65	-0.18
During	**-0.006**	**0.027**	**0.042**	**0.053**	**0.069**	**-0.017**	**-0.008**
	-0.22	1.3	2.05	2.42	3.9	-1.09	-0.53
After	**-0.143**	**-0.079**	**-0.065**	**-0.092**	**-0.04**	**0.004**	**-0.007**
	-7.17	-3.79	-3.79	-4.58	-2.62	0.26	-0.4
R^2	0.048	0.03	0.03	0.03	0.039	0.001	0
N	1105	1215	1065	1190	1095	1048	1065
				p-values			
During=After	*0.0001*	*0.0001*	*0*	*0*	*0*	*0.36*	*0.95*

Table 3.2 : Adjusted Dynamics of Restating Firms

The dependent variables are relative to the mean of a control group, matched by size, age, and industry. Before and Afer are dummies for the 2-year periods before and after the restated period. During is a dummy for restated years. Coefficients are in bold, t-statistics are below the coefficients. Standard errors are robust and corrected for firm level clustering.

Dependent Variable	Growth of Market Value	Growth of Sales	Growth of Employees	Growth of Prop. Plant & Equip.	Cap. Exp./ PPE	Growth of TFP	Growth of Sales per Employee
Sample of Industries with Below Average Market Value Growth in the During/After Period							
Before	**0.082**	**0.078**	**0.077**	**0.07**	**0.054**	**0.002**	**0.005**
	3.72	5.02	4.93	4.16	3.86	0.22	0.45
During	**0.002**	**0.029**	**0.04**	**0.041**	**0.043**	**-0.005**	**-0.001**
	0.09	1.58	2.35	2.15	2.87	-0.34	-0.08
After	**-0.042**	**-0.034**	**-0.04**	**-0.056**	**-0.019**	**0.01**	**0.004**
	-2.46	-2.02	-2.78	-3.18	-1.56	0.79	0.3
R^2	0.015	0.024	0.032	0.025	0.025	0.001	0
N	1366	1500	1374	1498	1472	1372	1374
				p-values			
During=After	*0.1376*	*0.0077*	*0.0003*	*0.0001*	*0.0002*	*0.48*	*0.81*
Sample of Industries with Above Average Market Value Growth in the During/After Period							
Before	**0.071**	**0.054**	**0.038**	**0.039**	**0.029**	**0.002**	**0.01**
	4.17	3.74	2.33	2.21	1.89	0.19	0.76
During	**-0.007**	**0.039**	**0.041**	**0.046**	**0.044**	**-0.008**	**-0.003**
	-0.38	2.46	2.56	2.55	2.94	-0.6	-0.26
After	**-0.094**	**-0.058**	**-0.048**	**-0.055**	**-0.03**	**-0.003**	**0**
	-5.5	-3.61	-3.59	-3.57	-2.39	-0.22	-0.02
R^2	0.033	0.027	0.021	0.019	0.02	0	0
N	1359	1451	1260	1363	1174	1194	1260
				p-values			
During=After	*0.0013*	*0*	*0*	*0*	*0*	*0.80*	*0.88*

Table 3.3 : Adjusted Dynamics of Restating Firms

The dependent variables are relative to the mean of a control group, matched by size, age, and industry. Before and Afer are dummies for the 2-year periods before and after the restated period. During is a dummy for restated years. Coefficients are in bold, t-statistics are below the coefficients. Standard errors are robust and corrected for firm level clustering.

Sample of Industries with Below Average Market Value Growth in the Before Period

Dependent Variable	Growth of Market Value	Growth of Sales	Growth of Employees	Growth of Prop. Plant & Equip.	Cap. Exp./ PPE	Growth of TFP	Growth of Sales per Employee
Before	**0.051**	**0.059**	**0.059**	**0.057**	**0.039**	**-0.004**	**-0.001**
	2.59	4.03	3.96	3.42	2.94	-0.39	-0.11
During	**-0.01**	**0.003**	**0.024**	**0.043**	**0.035**	**-0.02**	**-0.01**
	-0.45	0.18	1.34	2.27	2.36	-1.47	-0.73
After	**-0.056**	**-0.034**	**-0.036**	**-0.045**	**-0.02**	**0.006**	**0.004**
	-3.28	-2.18	-2.63	-2.83	-1.59	0.46	0.26
R^2	0.011	0.017	0.021	0.02	0.017	0.002	0
N	1335	1445	1314	1433	1380	1302	1314
				p-values			
During=After	*0.1327*	*0.1015*	*0.0055*	*0.0002*	*0.0009*	*0.22*	*0.53*

Sample of Industries with Above Average Market Value Growth in the Before Period

Dependent Variable	Growth of Market Value	Growth of Sales	Growth of Employees	Growth of Prop. Plant & Equip.	Cap. Exp./ PPE	Growth of TFP	Growth of Sales per Employee
Before	**0.1**	**0.079**	**0.059**	**0.058**	**0.049**	**0.013**	**0.02**
	5.27	5.19	3.52	3.31	3.08	1.03	1.64
During	**-0.008**	**0.058**	**0.054**	**0.046**	**0.043**	**0.007**	**0.005**
	-0.4	3.52	3.3	2.45	2.83	0.5	0.35
After	**-0.077**	**-0.039**	**-0.039**	**-0.059**	**-0.026**	**0.015**	**0.008**
	-4.36	-2.39	-2.66	-3.33	-1.95	1.19	0.68
R^2	0.036	0.037	0.029	0.025	0.026	0.002	0.002
N	1298	1391	1219	1321	1169	1171	1219
				p-values			
During=After	*0.0097*	*0*	*0*	*0*	*0*	*0.70*	*0.86*

Table 4: Comparison of Restating and Mimicked Firms.

This table reports the median values for restating firms and mimicked firms. The mimicked firms were matched on age-size-industry, as well as, on pre-fraud market value growth rate. The growth rate in market value of mimicked firms in the pre-fraud years was within 70-130% of the restating firm. The pre-fraud years include the five years prior to misreporting. The Z-statistics tests for the difference in the medians of the two groups.

	Pre Fraud Years					Fraud Years				
	Market Value (growth rate)	Number of Employees (growth rate)	Prop. Plant & Equip. (growth rate)	Cap. Exp/ PPE	Sales per employee (growth rate)	Market Value (growth rate)	Number of Employees (growth rate)	Prop. Plant & Equip. (growth rate)	Cap. Exp/ PPE	Sales per employee (growth rate)
Mimicked Firms	0.2321	0.1127	0.1635	0.3185	0.0634	-0.023	0.0813	0.1323	0.2983	0.0419
Restating Firms	0.2413	0.1141	0.1713	0.3105	0.0488	-0.0878	0.0737	0.1247	0.306	0.0515
Z statistic	0.5878	0.2198	0.2657	0.4212	1.1706	1.9464	0.5027	0.3869	0.546	0.7653
N	328	328	328	328	328	328	328	328	328	328

Table 5: Adjusted Dynamics and Magnitude of Earnings Management

The regressions use only restating firms and the dependent variables are relative to the mean of a control group, matched by size, age, and industry. Restated earnings/ sales is the absolute value of the average magnitude of the restatement to sales for all income decreasing restatements. For panel b, the growth rates are from end of year prior to the restatement announcement to end of year of the announcement. Coefficients are in bold, t-statistics re below the coefficients.

	Market Value (growth Rate)	Sales (growth rate)	Employees (growth rate)	Prop. Plant & Equip (growth rate)	Cap. Exp. / PPE (level)	Sales per Employee (growth rate)
	Panel A: During Misreporting Years					
Restated Earnings /Sales	**0.1459**	**0.0582**	**0.0475**	**0.0075**	**0.048**	**0.0525**
	11.71	0.78	2.11	1.03	11.5	0.92
Constant	**-0.1081**	**0.0096**	**0.0386**	**0.0473**	**0.0185**	**-0.029**
	-2.65	0.37	1.7	1.76	1.46	-1.31
R^2	0.0643	0.019	0.0232	0.0004	0.0783	0.0254
N	369	384	360	375	350	360
	Panel B: Around Announcement of Restatement					
Restated Earnings /Sales	**-0.161**	**-0.062**	**-0.195**	**-0.287**	**-0.221**	**0.112**
	-2.08	-0.66	-2.33	-2.95	-3.55	1.28
Constant	**-0.055**	**-0.04**	**-0.028**	**-0.032**	**-0.015**	**-0.009**
	-3.74	-2.18	-1.72	-1.67	-1.15	-0.54
R^2	0.013	0.001	0.016	0.025	0.039	0.005
N	337	349	338	340	315	338

Table 6 : Predicting Restatement using Corporate Governance

Logit Models estimated in one cross-section in 2002 by pseudo maximum likelihood, with robust standard errors. Governance is measure in 1995 using IRRC, Q age and assets are measured in 1996, and restatements happen between 1997 and 2002. Coefficients are in bold, t-statistics are below the coefficients. Out of the 770 firms in the sample, 99 have a restatement.

	Dependent Variable is Dummy for Restatement between 1997 and 2002.					
Independent Variables, all measured in 1998	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Bebchuck et al. index	**0.267**					
	2.95					
Gompers et al. index		**0.158**				
		3.45				
Classified Board			**0.616**			
			2.33			
Poison Pills				**0.437**		
				1.74		
Limits to Amend Corporate Charter					**0.886**	
					1.51	
Golden Parachute						**0.298**
						1.26
Log Tobin's Q	**0.535**	**0.555**	**0.474**	**0.452**	**0.447**	**0.44**
	1.99	2.07	1.79	1.73	1.71	1.68
Log Age	**0.272**	**0.246**	**0.263**	**0.245**	**0.245**	**0.259**
	2.9	2.58	2.8	2.71	2.68	2.81
Log Assets	**0.334**	**0.297**	**0.405**	**0.416**	**0.428**	**0.381**
	0.61	0.52	0.75	0.78	0.81	0.71
N	770	770	770	770	770	770

Table 7 : Industry Dynamics of Non-Restating Firms

Panel of industries created at the 2-digit SIC level from COMPUSTAT. Only firms that do not restate are included. Dependent variables are industry means. Sample period 1991-2003. Coefficients in bold, t-statistics below coefficients. All regressions include industry fixed effects and year fixed effects.

Dependent Variable	Market Value (growth rate)	Sales (growth rate)	Number of Employees (growth rate)	Prop. Plant & Equip. (growth rate)	Cap. Exp./ PPE	TFP (growth rate)	Sales per Employee (growth rate)
	ols	*ols*	*ols*	*ols*	*ols*	*ols*	*ols*
Average Number of Restatements in Industry in Previous Years	**-0.394**	**-0.315**	**-0.467**	**-0.428**	**-0.167**	**0.202**	**0.232**
	-2.57	-3.07	-4.26	-3.72	-2.04	2.02	2.31
Year & Industry Fixed Effects	yes	yes	yes	yes	yes	yes	yes
N	796	796	796	796	796	796	796
R^2	0.316	0.244	0.157	0.288	0.35	0.069	0.067

Figure 1: Insider Trading at Enron

Bars are shares sold, in millions on the left axis, line is average transaction price, in dollar on the right axis. Source: Thomson Financials.

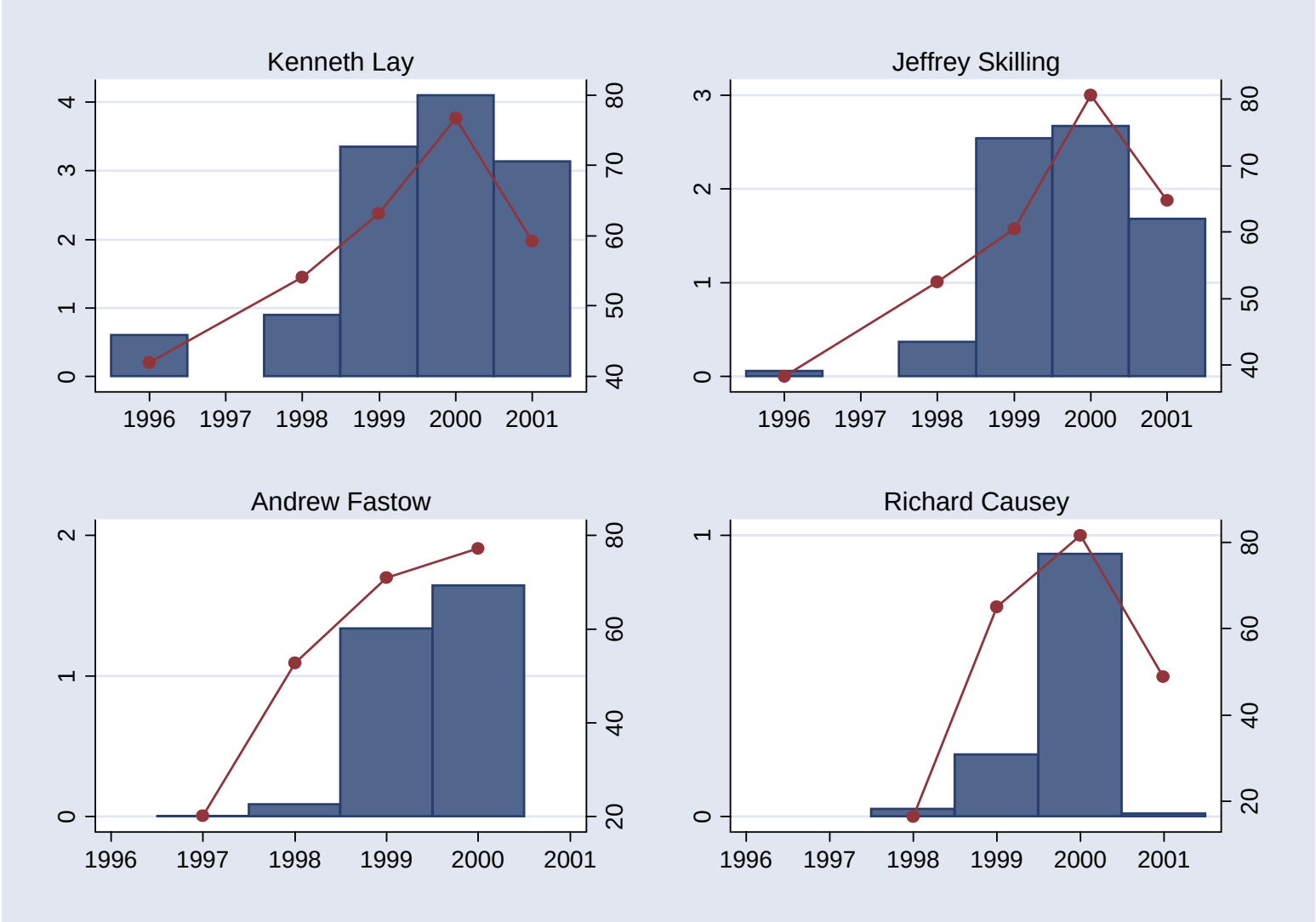

Figure 2: Dynamics of Firms Restating Earnings

Growth rates are relative to a control group of firms matched on size, age and industry.

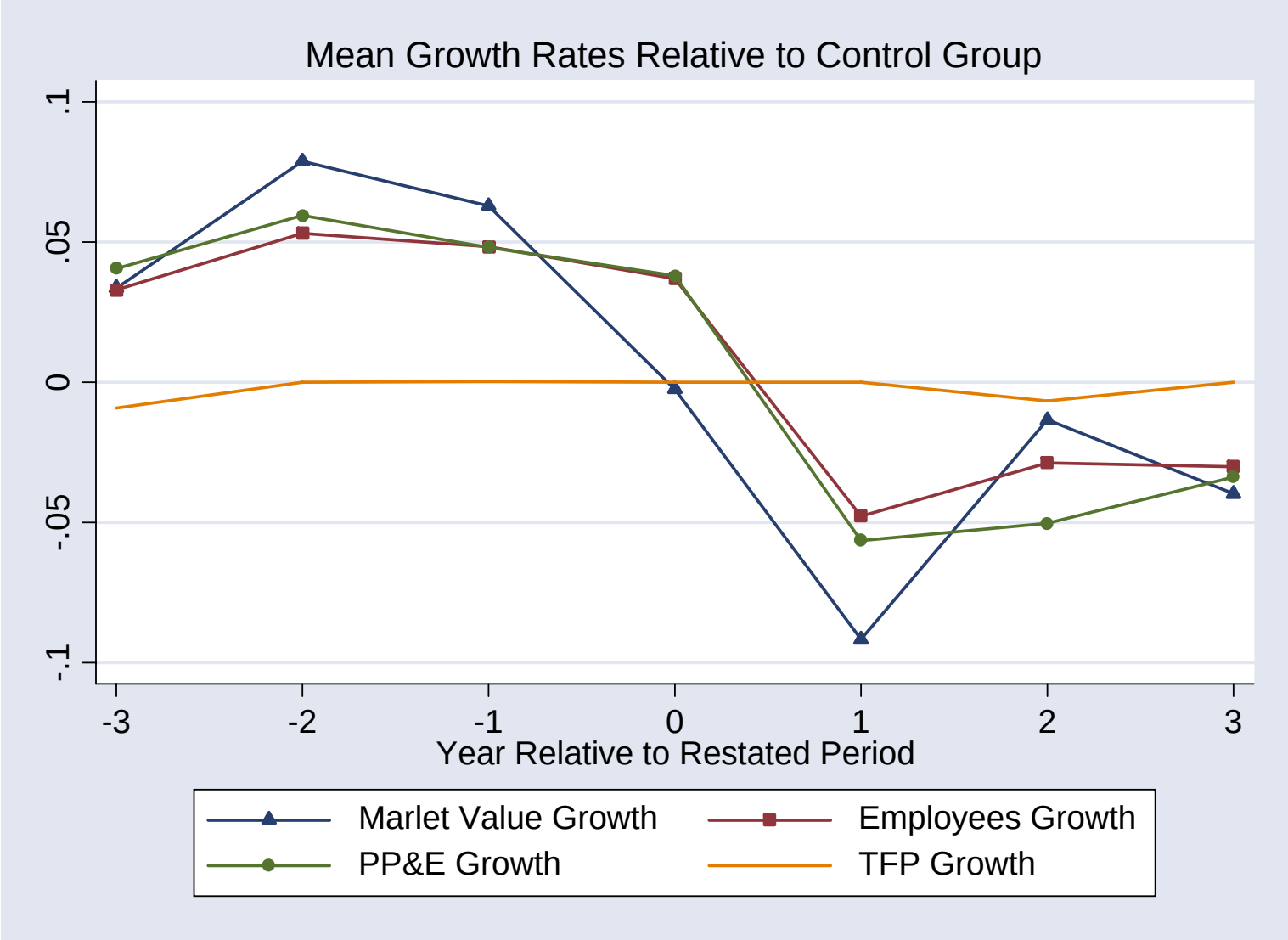

Figure 3: Employment Dynamics of Firms Announcing Restatements in 2000 or 2001

Number of employees, in millions.

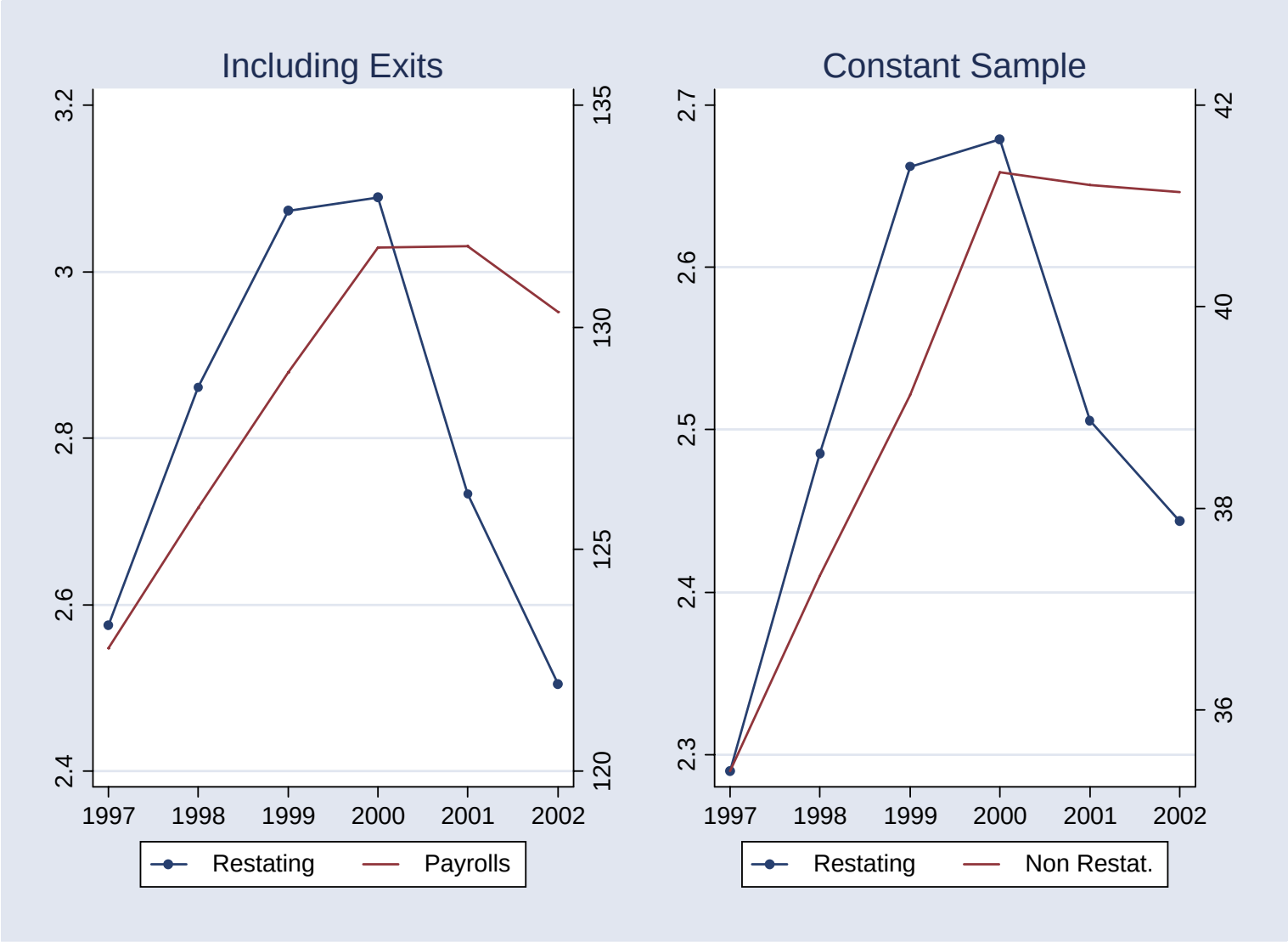

Figure 4: Employment Growth Predicted by Lagged Restatements