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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, bad managers hire and invest too much in order to pool with the good managers. This behavior distorts the allocation of economic resources among firms. We test the predictions of the model using new historical and firm-level data. First, we show that periods of high stock market valuations are systematically followed by large increases in reported frauds. We then show that during periods of suspicious accounting, firms hire and invest excessively, while insiders exercise options and sell stocks. When the misreporting is detected, firms shed labor and capital and productivity improves. In the aggregate, our model seems able to account for periods of jobless and investment-less growth.

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Introduction

Fraudulent accounting by management is 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 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 economic consequences of fraudulent accounting, with a particular focus on the dynamics of employment and investment.

The dramatic case of Enron's restatement illustrates our point. 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. The period of misreporting was characterized by substantial stock sales by Enron insiders (see **Figure 1**). Less well known, but equally important, is the fact that during this same period Enron grew faster than any other firm in its 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. At its peak, Enron employed more than 20,000 employees worldwide. 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 is a typical – if somewhat extreme – example of fraudulent accounting in periods of high financial valuations. We also show that the joint dynamics of misreporting, insiders' trades, employment and investment can be explained by a simple model of multi-dimensional signalling.

We study the problem of a manager whose productivity is private information, and who makes observable hiring and investment decisions. Bad managers who want to hide their poor quality must not only manage their earnings, but also hire and invest like good managers. It is not sufficient to merely misreport performance. In equilibrium, the bad managers hire and invest excessively, distorting the allocation of resources among firms.

Prior and concurrent theoretical work has not emphasized these implications.[1] In our model, real costs of manipulation arise endogenously because earnings management distorts the hiring and investment decisions of the firms.

Our main contribution is then to test the predictions of the model using two newly collected data sets. The first prediction is that the incidence of fraudulent accounting will be higher when price-earnings ratios are high.[2] We use historical data on actions by the Securities and Exchange Commission (SEC), from 1936 to 2003, to capture the incidence of fraudulent accounting. We find that periods with high price-earnings ratios are associated with significant increases in the number of civil injunctive actions and administrative proceedings by the SEC.

Testing the second set of predictions requires firm level data, and we use a sample of firms that have restated their earnings between January 1997 and June 2002. In the model, earnings management boosts 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 (2002), Burns and Kedia (2004), Bartov and Mohanram (2004) and Roulstone (2005) have confirmed these predictions. Similarly, our data shows that, during the misreported periods, 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 where 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. There is no counter-factual experiment that would allow us to learn what would have happened if these companies had not been able to misreport their earnings, but our theory suggests that the firms managed their earnings during this period in order to produce the profit numbers consistent with their rapid growth. Our theory also explains why these firms shrank quickly after the restatements were announced.

Other explanations cannot account for the joint dynamics of firm value, hiring, invest-

[1] See Narayanan (1985), Stein (1989), Guttman, Kadan, and Kandel (2004), Goldman and Slezak (2003), Povel, Singh, and Winton (2004), Bolton, Scheinkman, and Xiong (2003), Friebel and Guriev (2005).

[2] This prediction depends on which parameter drives the variation of P/E. We will argue that the prediction is likely to hold in historical time series, and we discuss why it may not hold in a cross section of countries or firms.

ment, and insider trading. First, the measured relative productivity growth of restating firms stays constant before, during and after the restated period. This suggests that restatements are not simply driven by unobserved negative technology shocks. Second, the pattern of insider exercises suggests that managerial optimism does not explain the observed dynamics of investment and employment. Third, Desai, Hogan, and Wilkins (2005) and Karpoff, Lee, and Martin (2005) have recently shown that earnings management is costly for managers, who would not have engaged in such activities unless they believed that earnings management had a causal effect on firm value. Finally, our results are qualitatively and quantitatively consistent with event studies of the impact of restatements on firm value.

The macroeconomic consequences are potentially large. The publicly traded firms that restated their earnings in 2000 and 2001 created between 350,000 and 500,000 jobs between 1997 and 2000, and destroyed between 250,000 and 600,000 jobs between 2000 and 2002. 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.

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."[3] 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? To the best of our knowledge, our paper is the first to provide evidence on this issue. Our paper is also 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 examines, with historical data, the relationship between stock market valuation and the incidence of fraudulent accounting. Section 3 uses firm level data to examine the dynamics of employment and investment for fraudulent firms, and the link between restatements and corporate governance. Section 4 discusses the interpretation of the results and the extent to which we can conclude that there is a causal link between earnings manipulation and employment

[3] See for instance Sloan (1996) and Teoh, Welch, and Wong (1998).

dynamics. Section 5 focuses on the dynamics of non-restating firms. Section 6 concludes.

1 Model

We now present a model of earnings manipulation. We first describe the case where the underlying cash flows are exogenous. This case has been the main focus of the literature so far, but it is not particularly useful for thinking about the real effects of fraudulent accounting. We then show that real inefficiencies arise from the *interaction* of endogenous hiring and investment decisions with the opportunity to manipulate earnings.

1.1 Exogenous Fundamental Earnings

The model has two periods, $t = 1, 2$, and a large number of firms whose fundamental earnings x depend on the quality of their managers. There are two types of managers. Half are bad, $x = x_L$, and half are good, $x = x_H$. We assume that both x_L and x_H are strictly positive. The type of a manager is known only to the manager. Absent any manipulation, earnings are x at $t = 1$, and ϕx at $t = 2$. Realized earnings in periods t, y_t, are equal to fundamental earnings plus discretionary accruals, a_t. The risk free rate is normalized to 0, and accruals always have a zero net present value. Hence,

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

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, and we let γ be the expected punishment. The appendix shows how to extend the model to allow for endogenous trading. Goldman and Slezak (2003) show how to endogenize α in a model with unobserved managerial effort.

Let λ be the fraction of bad managers who manipulate (strategy m) and $1-\lambda$ the fraction of bad managers who report honestly (strategy o). Let $\hat{\lambda}$ be the market belief about λ. We focus on equilibria where good managers report honestly. The set of equilibria depends in general on the details of the information structure,[4] and on the functional form for the

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

punishment technology γ.[5] In this respect, our setup is special, but it is not arbitrary. One clear result in the literature on earnings management is that stock prices react strongly to announcements of earnings restatements. Therefore, pooling does occur in the real world. We do not pretend to show theoretically that pooling should be expected, but rather, we focus on pooling equilibria because they appear empirically relevant.

Note that, without manipulation, ϕ would be the price-earnings ratio. In our model, ϕ captures the rational expectation of earnings growth.[6] Assuming efficient financial markets, the actual market value of the firm, as a function of its current earnings, is

$$V\left(y_1, \hat{\lambda}\right) = E\left[y_2 \mid y_1\right] = \left\{ \begin{array}{c} V_L = \phi x_L \text{ if } y_1 = x_L \\ V_H\left(\hat{\lambda}\right) \text{ if } y_1 = x_H \end{array} \right\},$$

where

$$V_H\left(\hat{\lambda}\right) = \phi \frac{x_H + \hat{\lambda} x_L}{1+\hat{\lambda}} - \frac{\hat{\lambda}}{1+\hat{\lambda}} a,$$

and

$$a = x_H - x_L \; .$$

The expected utilities of managers under strategies o and m are

$$U^o = \alpha V_L \; ; \; U^m = \alpha V_H - \gamma \; .$$

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

Proposition 1 *The partially pooling equilibrium is characterized by:*

- *If $\gamma \geq \alpha\phi\left(x_H - x_L\right)$, no earnings are manipulated*
- *If $\gamma \leq \frac{1}{2}\alpha\left(\phi - 1\right)\left(x_H - x_L\right)$, all the bad managers manipulate*
- *If $\frac{1}{2}\alpha\left(\phi-1\right)\left(x_H - x_L\right) < \gamma < \alpha\phi\left(x_H - x_L\right)$, then the solution is interior and the fraction of bad managers who manipulate satisfies*

$$\frac{1+\hat{\lambda}}{\phi - \hat{\lambda}} = \frac{\alpha}{\gamma}\left(x_H - x_L\right) \; . \tag{1}$$

[5] It is possible to construct equilibria where good managers separate from bad managers if the probability of detection increases with the amount of manipulation. Piotroski and Roulstone (2005) provide evidence on the impact of potential legal liability costs on insider trading decisions.

[6] Other factors driving the P/E ratio could be market risk aversion, or irrational beliefs about earnings growth. Our results do not depend on where ϕ is coming from, and we do not need to take a stand on the long standing debate about time variation in P/E.

Proof. Under condition 1, $\hat{\lambda} = 0$ is not an equilibrium since $V_H(0) > V_L + \frac{\gamma}{\alpha}$. Under condition 1, $\hat{\lambda} = 1$ is not an equilibrium either, since $V_H(1) < V_L + \frac{\gamma}{\alpha}$. Otherwise, equation (1) comes from the indifference condition $U^o = U^m$. ■

Equation (1) shows that, in equilibrium, earnings management increases with the amount of stocks owned by the manager α, with the undistorted price earnings ratio ϕ, and with the difference between the fundamental values of good and bad managers. The appendix describes the equilibrium when we allow for endogenous trading decisions, together with liquidity shocks. It is straightforward to show that insider trades forecast earnings growth, and that the price impact reduces the equilibrium amount of earnings management.

1.2 Endogenous Factor Demand

Our main focus in this paper is on the allocation of resources among firms. To study this question, we extend the model to incorporate managerial production decisions. By assumption, when fundamental earnings are exogenous, earnings manipulation does not effect the efficiency of the economy. To overcome this problem, the literature has typically introduced *ad-hoc* costs of manipulation. Here we show that this assumption is unnecessary. Inefficiencies arise automatically when the hiring and investment decisions are endogenous – and observable – because the need to mimic the good types distorts all the observable actions of the bad types.

Suppose that the production technology is Leontief with scale θ, which is private information of the manager. Assume for simplicity that labor is the only factor of production, supplied at price w. Profits are given by

$$x = \min(n, \theta) - wn \ .$$

The critical assumption here is that good managers should optimally hire more than bad managers – an assumption that seems plausible. The Leontief technology makes the formula easier to read, but the results generalize to any production function that is super-modular in (n, θ).[7] Assume that $w < 1$ and that $\theta \in \{1, 1 + \Delta\}$ for bad and good managers,

[7] 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.

respectively. The first best level of employment is simply

$$n^*(\theta) = \theta = \left\{ \begin{array}{c} 1 \text{ for bad managers} \\ 1+\Delta \text{ for good managers} \end{array} \right\} ,$$

but since n is observable, bad managers who manipulate must hire just like good ones, therefore

$$n^m = 1 + \Delta \ .$$

So we have the following true profits:

$$\begin{aligned} x_H^* &= (1-w)(1+\Delta) \ , \\ x_L^* &= 1-w \ , \\ x_L^m &= 1-w(1+\Delta) \ . \end{aligned}$$

Discretionary accruals have to make up not only for the fundamental difference in quality $\Delta(1-w)$, but also for the inefficient allocation of resources Δw :

$$a = x_H^* - x_L^m = \Delta \ .$$

Making n observable creates misallocations and real costs. Unlike previous models, we do not need to assume that manipulating accruals is costly in and of itself. The market value of a firm reporting high earnings is

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

and the equilibrium condition

$$U^o = U^m \Leftrightarrow V_H = V_L + \frac{\gamma}{\alpha}$$

leads to

$$\frac{1+\hat{\lambda}}{\phi(1-w) - \hat{\lambda}} = \frac{\alpha \Delta}{\gamma} \ .$$

Proposition 2 *The fraction $\hat{\lambda}$ of managers who manipulate their earnings increases with the undistorted price-earnings ratio ϕ, and with the number of shares owned by managers α, and decreases with the cost of manipulation γ.*

Bergstresser and Philippon (2002) and Burns and Kedia (2004), among others, have already confirmed the comparative statics with respect to α. The straightforward extension – presented in the appendix – of our model to endogenous trading 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 will focus on the predictions of the model that have not been tested in the literature so far:

1. If time variation in the P/E ratio is driven by changes in ϕ, then violations of accounting rules are more likely when price-earnings ratios are high. We have shown that $\hat{\lambda}$ increases with ϕ. However, ϕ is not the actual P/E ratio, it is the P/E ratio of the undistorted economy. To give empirical content to this prediction, the appendix shows that the observed P/E ratio is always strictly increasing in ϕ, as long as x_L is strictly positive.

2. Firms managing earnings hire and invest like successful firms. A direct implication of this prediction is that fraudulent firms end up larger than predicted by their technology. Hence, one would expect to see these firms shrink after they are exposed.

2 Historical Evidence from SEC Actions

We now use historical data to examine the relation between P/E ratios and the incidence of fraud. Our maintained hypothesis in this section is that ϕ is the driving force behind changes in the P/E ratio over time. The model of section 1 makes it clear that some parameters create a positive correlation between P/E ratios and accounting manipulation, while other parameters create a negative correlation. Shocks to variables such as the punishment for cheating γ create a negative relation between incidence of fraud and P/E ratios. Indeed, the law and finance literature shows that γ and P/E ratios are negatively related across countries.[8] On the other hand, we expect ϕ to be more relevant in US time series, both because we believe that the other parameters of the model are unlikely to change very much from one year to the next and because many economic shocks can lead to variation in ϕ over time: discount rate shocks, shocks to expected aggregate growth as in Bansal

[8]See La Porta, Lopez-de Silanes, Shleifer, and Vishny (1998), La Porta, Lopez-de Silanes, Shleifer, and Vishny (2002) and Shleifer and Wolfenzon (2002).

and Yaron (2002), time variation in risk-aversion as in Campbell and Cochrane (1999), and bubbles as in Scheinkman and Xiong (2003) or Allen, Morris, and Shin (2005). Therefore, we look at the historical time series as if it was generated predominantly by changes in ϕ, and we expect to find a positive correlation between P/E ratios and accounting fraud. This prediction is shared by essentially all models of earning management. The evidence that we present here is therefore relevant for a large class of models. In the next section, we will investigate the particular predictions of our model using firm level data on hiring and investment.

We use the annual reports of the Securities and Exchange Commission (SEC), from 1936 to 2003, to capture the incidence of fraudulent accounting. The SEC classifies its actions into several categories. We use the longest available series, which combines civil injunctive actions and administrative proceedings initiated in each year (ACT_t). Civil injunctive actions usually involve securities fraud. Administrative proceedings involve allegations that a firm or individual has violated GAAP or that an individual has caused a firm or other individuals to act unlawfully. To control for the increase in the number of publicly traded companies in the US , we adjust the number of SEC actions using the regression

$$ACT_t = \alpha + \beta N_t^{Public\ Firms} + \varepsilon_t^{SEC} ,$$

where $N_t^{Public\ Firms}$is the number of publicly listed firms obtained from CRSP.

We obtain historical data on P/E from the web site of Robert Shiller.[9] **Figure 2** plots the adjusted number of SEC actions ε_t^{SEC} and the two-year average P/E ratio $\frac{1}{2}\left(\frac{P_t}{E_t} + \frac{P_{t-1}}{E_{t-1}}\right)$ from 1936 to 2003. The two series are positively correlated at medium run frequencies. To confirm the visual impression, we estimate the linear regression

$$\varepsilon_t^{SEC} = \underset{s.e.\ =\ 1.03}{7.46} \times \frac{1}{2}\left(\frac{P_t}{E_t} + \frac{P_{t-1}}{E_{t-1}}\right) + u_t,\ R^2 = 44\%$$

As predicted by the model, there is a tight correlation between SEC actions and market valuations in historical data. This shows that the link between market valuations and fraudulent activity is not exclusively an experience of the late 1990s. The historical evidence is also consistent with recent work emphasizing the strategic complementarity in the release of accounting information, like Hertzberg (2005b), or changes in ethical behavior, like Shleifer (2004).

[9] http://www.econ.yale.edu/~shiller/

An important issue here is that SEC actions are endogenous. An increase in the number of SEC actions can come from an increase in frauds, an increase in scrutiny, or both. One would naturally expect the SEC to increase its investigations when frauds go up. In this case, the number of SEC actions would overestimate the true increase in fraud. However, this does not change our qualitative interpretation of the evidence. It simply means that we may need to scale down our estimated elasticity of fraud cases to market valuations.[10] On the other hand, there are good reasons to believe that the number of reported SEC cases might underestimate the true incidence of fraud. The SEC has limited resources and cannot expand very quickly. Therefore, in the short run, we would expect the detection technology to exhibit decreasing returns to scale. Consequently, the number of SEC actions would increase less than one for one with the number of frauds.

3 Firm Level Evidence from the 1990s

In this section, we use firm level data to test the other predictions of the model. Firm level data allow for more direct tests of the hypothesis presented above. 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. Finally, we show that companies with good governance – a possible proxy for γ – are less likely to engage in earnings manipulations. We conclude this section by a discussion of alternate explanations for our findings.

3.1 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

[10] This is similar, for instance, to the issues of unobserved labor effort and capacity utilization in the business cycle literature.

information that was previously reported.[11] Six-hundred-forty-five of these companies 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." 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. It is useful to keep in mind that 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

[11] 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.

of the restatement. We also collected data on the size of the restatement.[12] We were able 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 73). 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.[13] To capture insider trading activity, we get data on CEO option exercises

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

[13]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).

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 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 now show that the conditional dynamics are remarkably different.

3.2 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. Since EXECUCOMP covers only S&P 1500 firms, only 140 restating firms have available data on value of option exercises in EXECUCOMP. We estimate

$$y_{it} = \beta^{before} 1_{t<\tau(i)} + \beta^{during} 1_{t\in\tau(i)} + \beta^{after} 1_{t>\tau(i)} + \phi_j + \phi_t + \gamma x_{it-1} + u_{it} \ , \ t = 1991..2003 \ ,$$

In these regressions, y_{it} is the ratio of the value realized from option exercises over the total value realizable from options, and $\tau(i)$ is the restated period for firm i, and $\tau(i) = \varnothing$ for firms that do not restate. The RHS variables include industry and time dummies ϕ_j and ϕ_t as well as some control variables $x_{i,t-1}$ discussed below. A positive estimated coefficient β^{during} implies 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.[14] The coefficients β^{before} and β^{after} show if they did the same 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 $\beta^{before} = \beta^{during}$. **Table 2** shows that β^{during} is significantly greater than zero and also significantly greater

[14] Ofek and Yermack (2000), looking at US executives, document that nearly all executive stock option exercises are followed by share sales.

than β^{before}. This result holds if we control for the lagged growth rate of the stock price, and lagged Tobin's Q, and the magnitude of the 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**). All these results are consistent with the evidence discussed earlier. We refer the reader to the existing literature for robustness checks and extensions, and we move on the the unexplored area of employment and investment dynamics.

3.3 Earnings Restatements and the Dynamics of Employment and Investment

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 substracting the mean of this control group.

$$\hat{g}_{it} = g_{it} - \bar{g}_{C(i)t} \tag{2}$$

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

Figure 3 plots the mean adjusted growth rates, as in equation (2), 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 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.

$$\hat{g}_{it} = \beta^{before} 1_{t<\tau(i)} + \beta^{during} 1_{t\in\tau(i)} + \beta^{after} 1_{t>\tau(i)} + u_{it} \ , \ t = 1991..2003 \ ,$$

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**. The growth of employment in fraudulent firms is 3.7% higher during the fraudulent period. As predicted by the model 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 5% higher during the restated period and 6% lower after the restated period. The same pattern is seen when we examine capital expenditures normalized by PP&E. 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 also what the dynamics of market value suggest. The growth in market value was 5.2% higher before the restatement and 4% lower afterwards, and it was the same during the restated period, suggesting that the firms did not surprise the market during this period.[15]

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

[15] A similar picture emerges when we examine analyst forecasts obtained from IBES for 408 restating 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 peirod, 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.

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.

We now turn to the sub-sample of firms for which we were able to collect information on the size of the restatement. We investigate whether larger restatements were associated with larger drops in employment and investment. **Table 4** shows that this is indeed the case (remember that restatements are negative numbers). On the other hand, we do not see much effect on sales, and restatements have the opposite effect on productivity, although it is not significant.

3.4 Predicting Restatements: the Role of Governance

We now attempt to test the prediction that higher values of γ decrease the incidence of fraud. It is difficult to find a quantitative measure of γ, let alone one that is available for a large sample of firms. We will rely on recent research by Gompers, Ishii, and Metrick (2003) on the role of corporate governance. Good governance can increase γ either because it makes it easier to detect manipulations, or because it increases the range of punishments available to shareholders. In both cases, we would expect a negative relation between governance and manipulation.[16] We test this prediction by running predictive logit regressions in the cross-section of firms present in our sample in 2002

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

where $restat_{i,02}$ is a dummy variable for any restatement by firm i between 1997 and 2002, $F(.)$ 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.[17] 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

[16] Note that variation in governance across firms, like investor protection across countries discussed earlier, creates a negative correlation between fraud and P/E ratios.

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

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 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 5** 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 want to infer causality from these reduced form regressions. 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 accounting scandals of the late bubble period were not anticipated in 1995. On the other hand, the issue that we cannot address is omitted variable bias, since one could imagine 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.

3.5 Alternate Interpretations

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

First, one might argue that restatements do not reflect genuine manipulations, but rather excessive optimism by some managers. This view is refuted by the 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, that this leads them to misstate their financial results, and that GAO experts are not able to distinguish these honest mistakes from intentional manipulations. 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 sell the stock of their companies during the misreported period.

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 are not. We have shown, however, that measured productivity growth does not decrease following the restatements. Thus, true productivity probably increases since measured productivity was likely overstated during the suspicious period. Moreover, these shocks could not have been 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 earnings manipulation is a side effect of exogenous 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 was industry-age-size specific, it would also affect the control group, and would be irrelevant for us. For instance, our results cannot be explained simply by a bubble affecting young firms in the computer industry.

Second, if earnings management was not important, why would managers have engaged in this dangerous activity? The personal costs to the managers – in the form of reduced opportunities in the labor market – are substantial, as documented recently by Desai, Hogan, and Wilkins (2005) and Karpoff, Lee, and Martin (2005). Clearly, the managers believed that earnings manipulation was effective, otherwise they would have abstained from it. This argument applies not only to the decision whether to manipulate or not, but also to the decision of how much to manipulate, since **Table 4** shows that the size of the drop in employment and investment is related to the extent of the manipulation.

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 restatements are 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 3**: a negative 10% growth in market value over the year following the end of the restated period.

We conclude that managers believed that firm value was significantly affected by earnings manipulation, and that the quantitative impact was very much in line with the numbers that we report. This speaks directly to the causal link between manipulation and the dynamics of firm value, and therefore of investment and employment. These dynamics would not have been the same if the firms 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 causes the misallocation of resources.

4 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 out in the aggregate, and second, to stimulate future research.

4.1 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 4**. 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 4** 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.[18] 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.

4.2 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 an-

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

nouncements 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.[19] 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

$$\bar{g}_{jt} = \beta \bar{R}_{j,t} + \phi_t + \phi_j + u_{jt} \ , \ t = 1991..2003 \ , \tag{3}$$

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 6**. 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 consistent with the interpretation of restatements simply as negative productivity shocks. The evidence is consistent with models that emphasize the strategic interactions between firms in the release of information (Hertzberg (2005a)).

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 5** 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

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

down the wage, and mitigate the actual drop in employment (see Philippon (2004) for a general equilibrium model).

5 Conclusion

Earnings management distorts the allocation of resources in the economy, especially in periods of high financial valuations. When hiring and investment decisions are observable, bad managers 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 historical and 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.

The dynamics of hiring and investment in restating firms break the link between productivity and factor demands. The period after the restatement is characterized by strong productivity growth, while labor and capital demands fall. Thus, waves of earnings restatements can be followed by periods of jobless growth and low investment. 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. Earnings management is a particular case of imperfect governance. It 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. 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

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

In this simplified setup, cash flows are simply given by

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

so we see that

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

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.

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.

1. Firms avoid negative numbers. There is a higher-than-expected frequency of firms with slightly positive earnings changes. Burgstahler and Dichev (1997), and see Guttman, Kadan, and Kandel (2004) for a model, and Durtschi and Easton (2005) for discussion.

2. Who manipulates earnings? Bergstresser and Philippon (2002) and Burns and Kedia (2004) show that managers with many stock options are more likely to engage in earnings management.

3. Accruals are mispriced. Sloan (1996) documents the presence of negative excess returns after large positive accruals. In fact, excess returns follow high accruals *that coincide with insider selling the stock*, as shown by Beneish and Vargus (2002). Richardson, Sloan, Soliman, and Tuna (2005) provide further evidence on which types of accruals are actually mispriced.

B Extension of the Model to Endogenous Trading Decisions

In this section, we briefly show how the model extends to the case where trading is an endogenous decision for some managers. A fraction δ of managers are hit by liquidity shocks and have to trade. The remaining $1-\delta$ 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.

Claim *Good managers do not trade unless they have to, and bad managers who have manipulated always trade.*

The proof of the claim is straightforward. Good managers are better off waiting since they would have to sell below the market price. Bad managers who manipulated their earnings at $t=1$ are better off trading since their manipulation will be found out at time $t=2$.

$$\begin{aligned} V_H\left(\hat{\lambda}, trade\right) &= \phi\frac{\hat{\lambda}x_L+\delta x_H}{\delta+\hat{\lambda}}-\frac{\hat{\lambda}}{\delta+\hat{\lambda}}a \ , \\ V_H\left(notrade\right) &= \phi x_H \ , \end{aligned}$$

The equilibrium condition becomes

$$\frac{\delta+\hat{\lambda}}{\delta\phi-\hat{\lambda}}=\frac{\alpha}{\gamma}\left(x_H-x_L\right) \ .$$

Comparing this formula to the one in section 1, we can see that endogenous trading reduces the incentives to manipulate because of the price impact. Like in the noise trading literature, a higher δ induces more insider trading by decreasing the price impact.

C Observed Price Earnings Ratios

In this section of the appendix, we show that the actual P/E ratio – with earnings management – increases with ϕ, the P/E ratio of the benchmark economy without earnings management. This is not trivial because earnings are inflated and prices are lower in the distorted economy. In the mixed strategy equilibrium of section 1, we have $V^H - V^L = \frac{\gamma}{\alpha}$ and $V^L = \phi x_L$. The aggregate market valuation is therefore

$$\bar{V} = (1+\lambda) V^H + (1-\lambda) V^L = 2\phi x_L + (1+\lambda)\frac{\gamma}{\alpha} ,$$

and aggregate reported earnings are

$$\bar{y} = x^H + x^L + \lambda \left(x^H - x^L\right) .$$

The equilibrium condition $(1+\lambda)\frac{\gamma}{\alpha} = \left(x^H - x^L\right)(\phi - \lambda)$ leads to

$$\lambda = \frac{\left(x^H - x^L\right)\alpha\phi - \gamma}{\left(x^H - x^L\right)\alpha + \gamma} .$$

Therefore, the observed log-P/E ratio is

$$\log(P/E) = \log\left(\left(x^H + x^L\right)\phi - \lambda\left(x^H - x^L\right)\right) - \log\left(x^H + x^L + \lambda\left(x^H - x^L\right)\right) .$$

We are interested in the link between the actual log-P/E ratio and the benchmark log-P/E ratio. We use λ' to denote the derivative of λ with respect to ϕ, and we get

$$\begin{aligned} \frac{d\log(P/E)}{d\phi} &= \frac{\left(x^H + x^L\right) - \lambda'\left(x^H - x^L\right)}{\left(x^H + x^L\right)\phi - \lambda\left(x^H - x^L\right)} - \frac{\lambda'\left(x^H - x^L\right)}{x^H + x^L + \lambda\left(x^H - x^L\right)} \\ &= \frac{\left(x^H + x^L\right)\left[x^H + x^L + \left(x^H - x^L\right)\left(\lambda - \lambda'(1+\phi)\right)\right]}{\left[\left(x^H + x^L\right)\phi - \lambda\left(x^H - x^L\right)\right]\left[x^H + x^L + \lambda\left(x^H - x^L\right)\right]} , \end{aligned}$$

Moreover

$$\lambda - \lambda'(1+\phi) = \frac{\left(x^H - x^L\right)\alpha\phi - \gamma - \left(x^H - x^L\right)\alpha(1+\phi)}{\left(x^H - x^L\right)\alpha + \gamma} = -1$$

thus

$$\frac{d\log(P/E)}{d\phi} = \frac{2x^L\left(x^H + x^L\right)}{\left[\left(x^H + x^L\right)\phi - \lambda\left(x^H - x^L\right)\right]\left[x^H + x^L + \lambda\left(x^H - x^L\right)\right]}$$

which shows that the observed P/E ratio increases with the undistorted P/E ratio ϕ as long as $x^L > 0$.

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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 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

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 and industry (2-digit SIC) fixed effects. The sample period is 1991-2003. Coefficients are in bold; t-statistics are below the coefficients. Standard errors are robust and corrected for firm level clustering.

Dependent Variable	Value Realized from Options Exercised over Value of Exercisable Options		
	(i)	(ii)	(iii)
Before	**0.001**	**0.005**	**-0.001**
	0.04	0.23	-0.07
During	**0.053**	**0.063**	**0.056**
	2.17	2.11	2.06
After	**-0.006**	**0.009**	**0.004**
	-0.23	0.32	0.15
Lagged Growth Rate of Stock Price		**0.025**	
		2.62	
Lagged Market Value			**0.063**
			6.19
Lagged Book Value			**-0.06**
			-5.73
R2	0.107	0.128	0.124
N	9436	6464	7878
pvalue (Before=During)	*0.0317*	*0.0484*	*0.0337*

Table 3 : Adjusted Dynamics of Restating Firms

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. 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. Sample period is 1991-2003. Coefficients are in bold, t-statistics are below the coefficients. Standard errors are robust and corrected for firm level clustering.

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)
Method	*ols, difference from matched firms*	*ols, difference from matched firms*	*ols, difference from matched firms*	*ols, difference from matched firms*	*ols, difference from matched firms*	*ols, difference from matched firms*	*ols, difference from matched firms*
Before	**0.052**	**0.041**	**0.039**	**0.043**	**0.034**	**-0.001**	**0.001**
	7.57	6.6	6.36	5.99	4.51	-0.13	0.24
During	**-0.002**	**0.028**	**0.037**	**0.038**	**0.034**	**-0.005**	**-0.005**
	-0.17	2.47	3.3	3.06	3.58	-0.55	-0.48
After	**-0.04**	**-0.035**	**-0.035**	**-0.055**	**-0.017**	**0.007**	**0.003**
	-4.92	-4.14	-4.66	-6.13	-2.64	1.08	0.41
R2	0.014	0.015	0.019	0.02	0.016	0	0
N	5039	5565	5019	5397	5036	4895	5019
pvalue (Before=During)	*0.0014*	*0.2373*	*0.892*	*0.9793*	*0.1944*	*0.6677*	*0.5897*
pvalue (During=After)	*0.0074*	*0*	*0*	*0*	*0*	*0.3038*	*0.4938*
pvalue (Before=After)	*0*	*0*	*0*	*0*	*0*	*0.3527*	*0.7863*

Table 4 : Adjusted Dynamics around Announcement of Restatement

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. Coefficients are in bold, t-statistics are below the coefficients.

Effect of Size of Restatement

Dependent Variable	Market Value (log change)	Sales (log change)	Number of Employees (log change)	Prop. Plant & Equip. (log change)	Cap. Exp./ PPE	TFP (log change)	Sales per Employee (log change)
	end of year of announcement - end of previous year	end of year of announcement - end of previous year	end of year of announcement - end of previous year	end of year of announcement - end of previous year	end of year of announcement	end of year of announcement - end of previous year	end of year of announcement - end of previous year
Restated Earnings over Lagged Sales	**0.161**	**0.062**	**0.195**	**0.287**	**0.221**	**-0.141**	**-0.112**
	2.08	0.66	2.33	2.95	3.55	-1.61	-1.28
Constant	**-0.055**	**-0.04**	**-0.028**	**-0.032**	**-0.015**	**-0.013**	**-0.009**
	-3.74	-2.18	-1.72	-1.67	-1.15	-0.74	-0.54
R2	0.013	0.001	0.016	0.025	0.039	0.008	0.005
N	337	349	338	340	315	331	338

Table 5 : 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 6 : 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
R2	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.

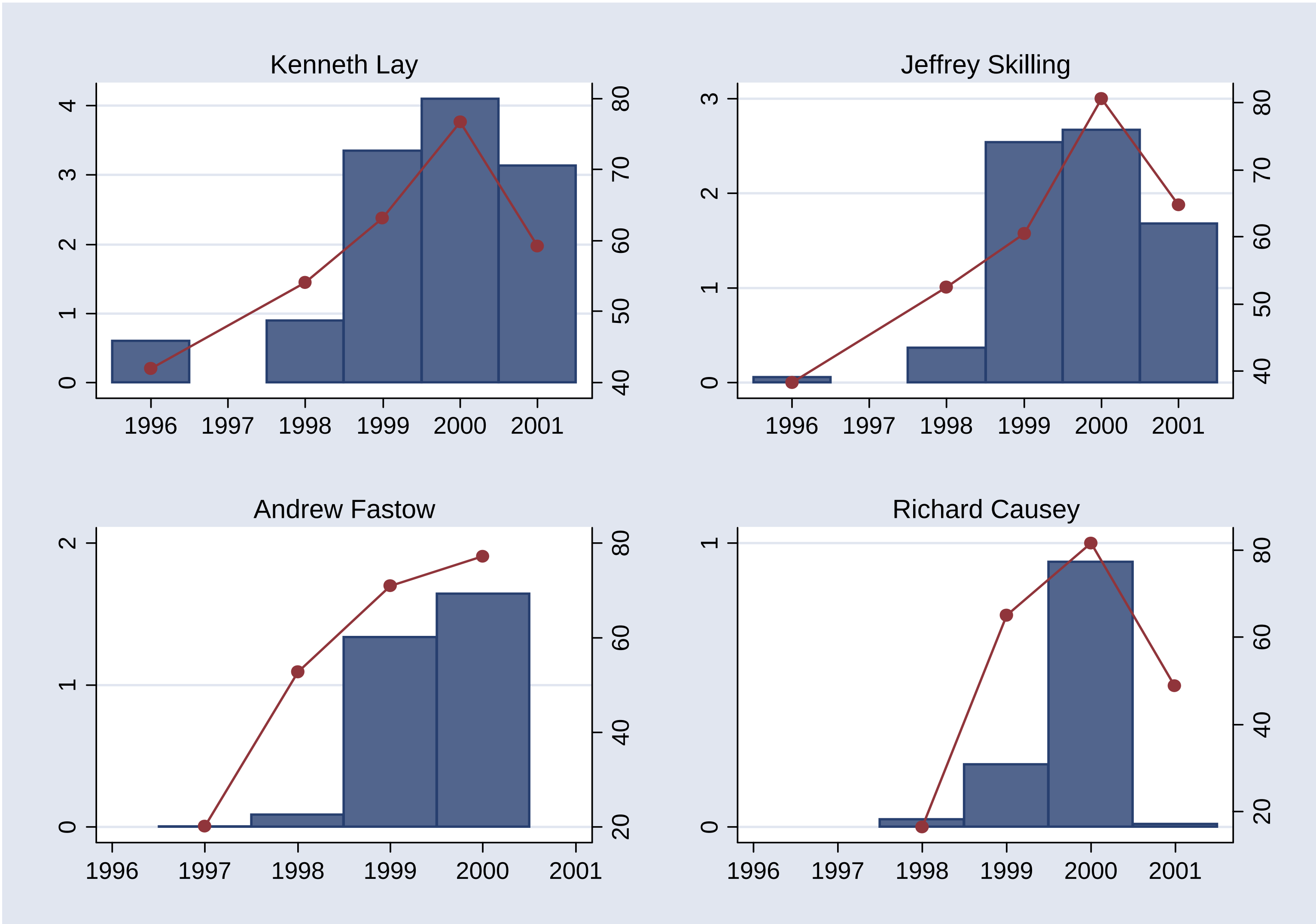

Figure 2: P/E Ratio and SEC Actions

P/E ratios are from Shiller. SEC actions come from the annual reports of the SEC and include civil injunctive actions and administrative proceedings. The number of SEC actions is first regressed on the number of publicly traded companies, from CRSP. The figure shows the residual from this preliminary regression

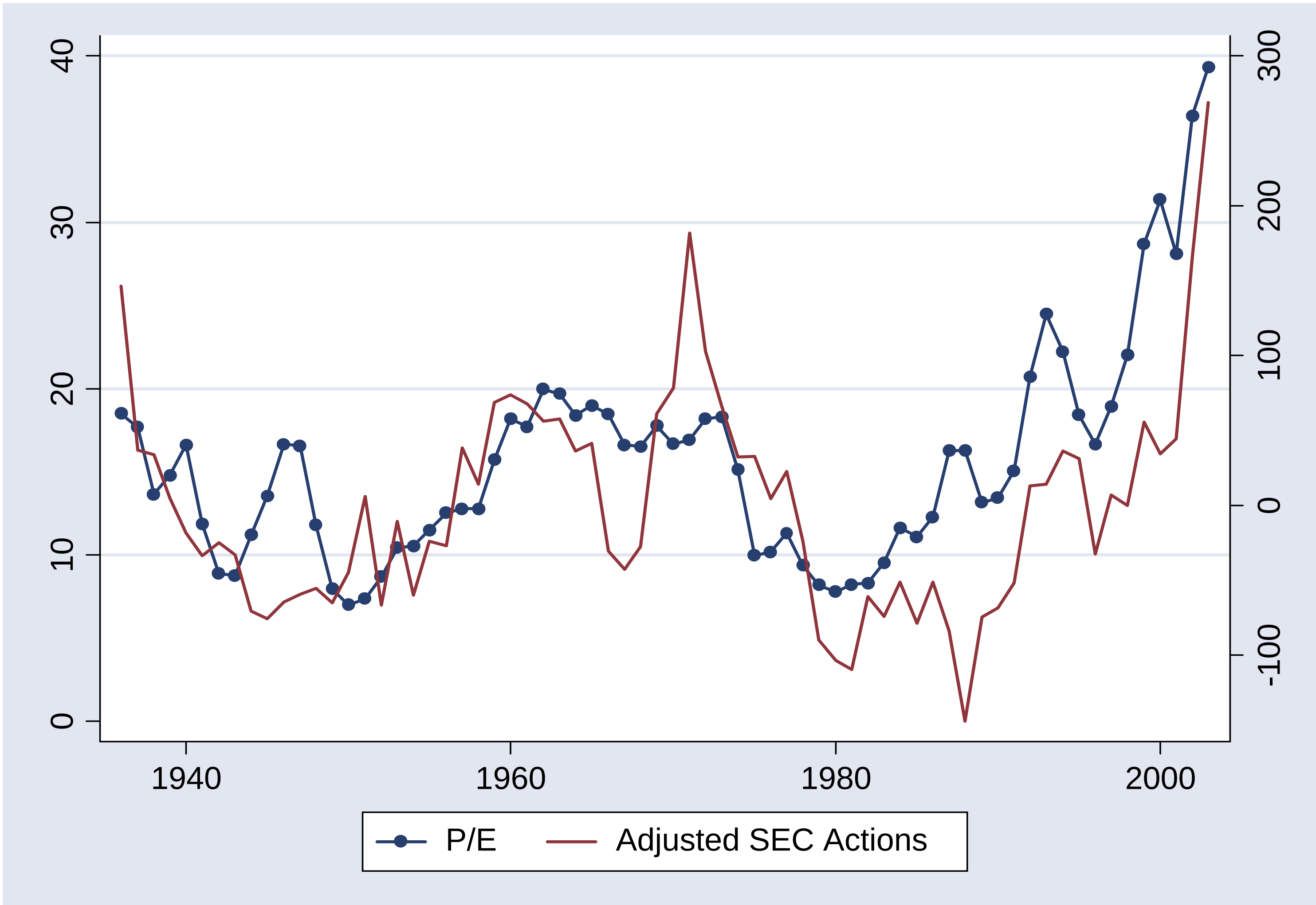

Figure 3: Dynamics of Firms Restating Earnings

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

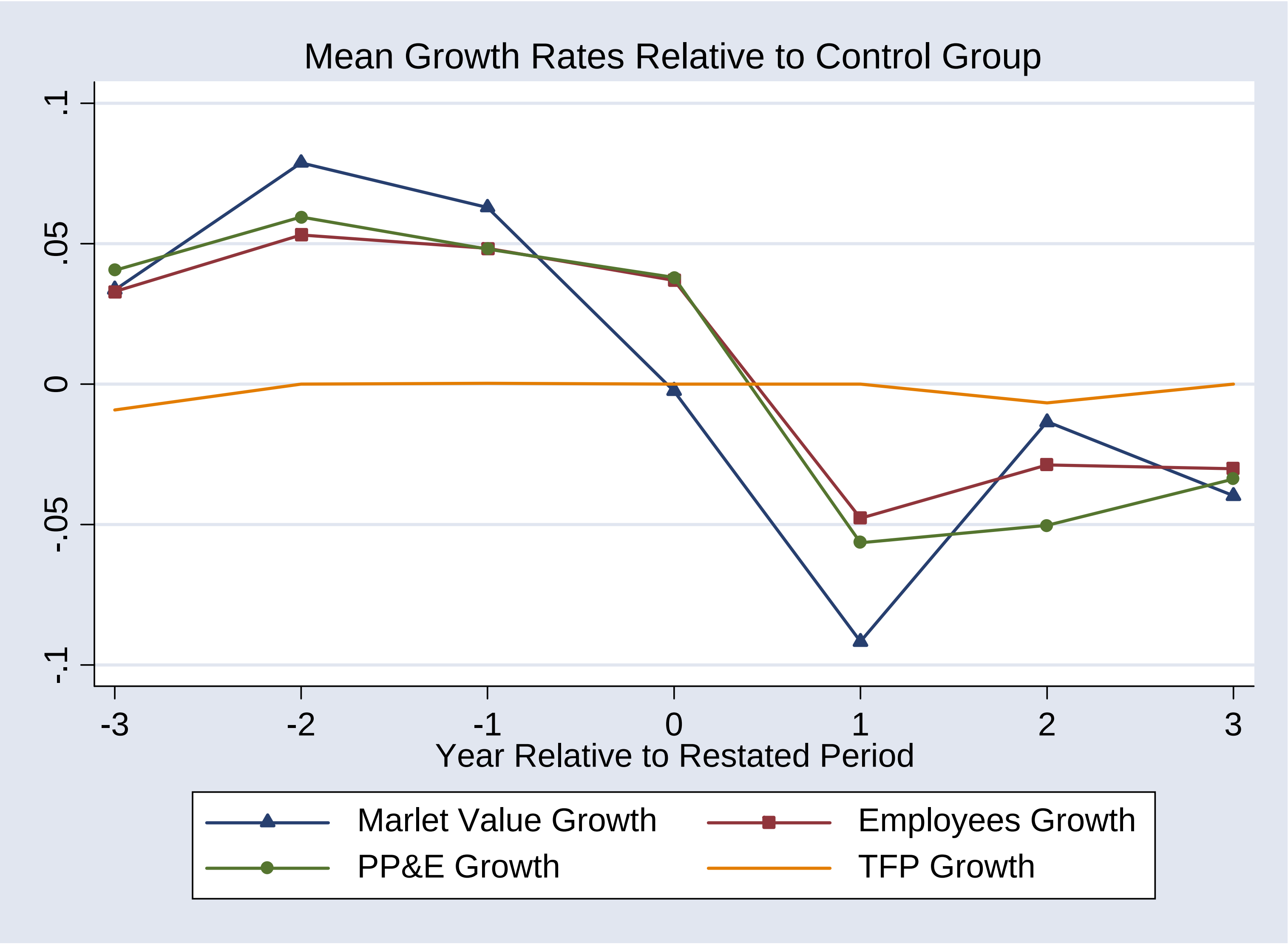

Figure 4: Employment Dynamics of Firms Announcing Restatements in 2000 or 2001

Number of employees, in millions.

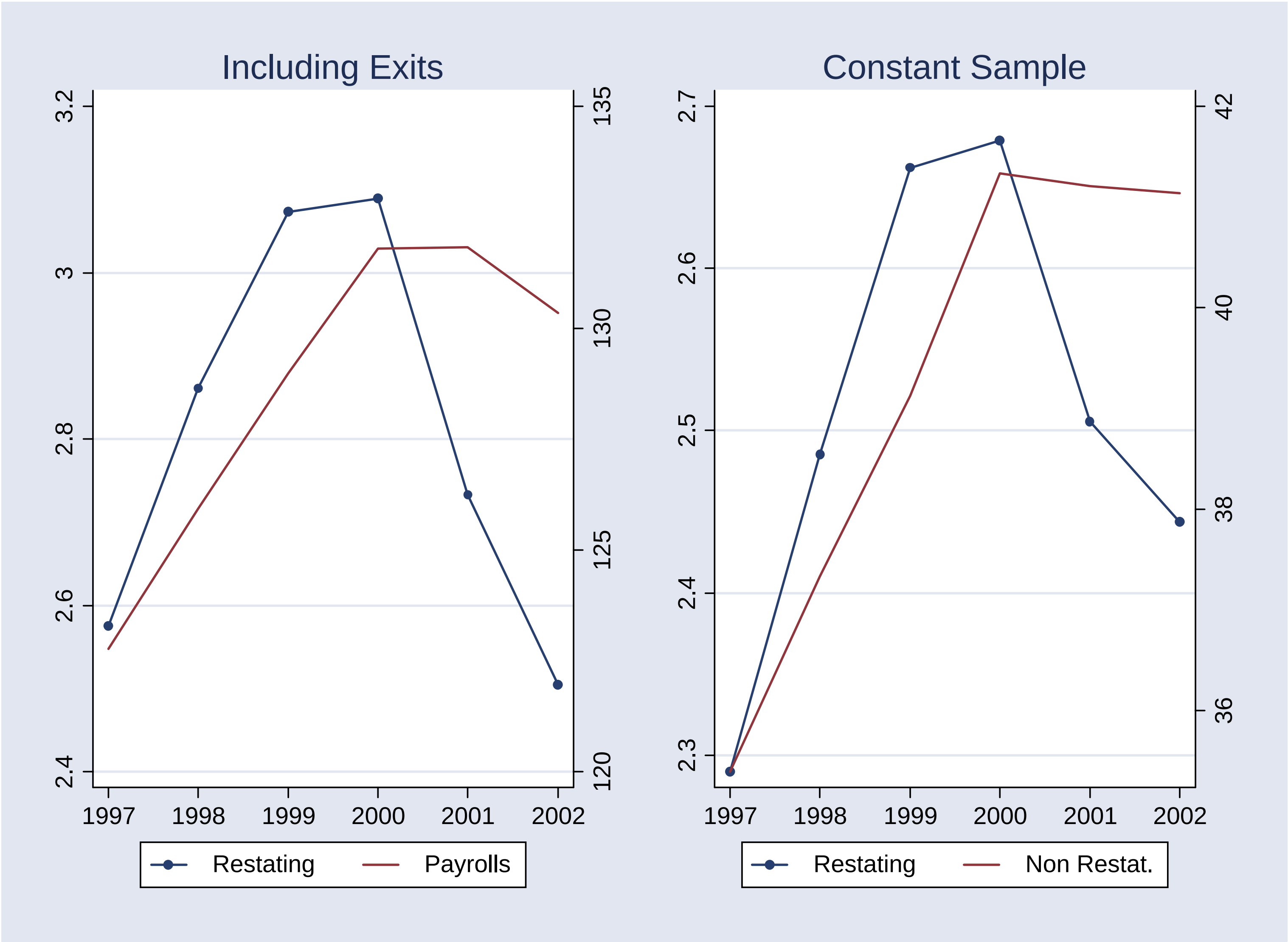

Figure 5: Employment Growth Predicted by Lagged Restatements