THREE ESSAYS IN FINANCIAL ECONOMICS

by

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ABSTRACT

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This dissertation is comprised of three essays in financial economics. Chapter 1 is a critique of a highly influential paper in corporate governance. Chhaochharia and Grinstein (2009) estimate that CEO pay decreases 17% more in firms that were not compliant with the recent NYSE/Nasdaq board independence requirement than in firms that were compliant. We document that 74% of this magnitude is attributable to two outliers out of 865 sample firms. In addition, we find that the compensation committee independence requirement increases CEO total pay, particularly in the presence of effective shareholder monitoring. Our evidence casts doubt on the effectiveness of independent directors in constraining CEO pay as suggested by the managerial power hypothesis.

In chapter 2, we investigate whether the earnings overstatements that led to the Sarbanes-Oxley-Act (SOX) may have been an intended consequence of pay-for-performance. We find that incentives were higher when current shareholders stood

to benefit from overstatements by selling their shares at inflated prices. Incentives also fell in response to the additional costs imposed by SOX, and the decrease is concentrated in firms whose shareholders benefit from overstatements. If overstatements were a symptom of the agency conflict, incentives should have increased around SOX to induce more productive effort as managers voluntarily cut back on overstatements. The empirical evidence thus rejects the view that earnings overstatements prior to 2002 were an unintended consequence of pay-for-performance.

In chapter 3, we explore the relationship between obesity and household credit risk. Obesity is a known health risk factor and carries a social stigma. Its presence provides a potentially informative signal about individuals' choices and preferences. Using NLSY survey data, we estimate that the loan delinquency rate among the obese is 20% higher than among the non-obese after controlling for numerous observable, prohibited, and — to lenders — unobservable credit risk factors. The economic significance of obesity for delinquencies is comparable to that of job displacements. Obesity is particularly informative about future delinquencies among those with low credit risk. In terms of channels, we find that the obesity effect is at least partially mediated through poor health, but is not attributable to individuals' time preferences.

CHAPTER 1

CEO Compensation and Board Structure Revisited

1.1 Introduction

Whether board composition affects executive pay has been the subject of debate for decades. Proponents of the managerial power hypothesis (e.g., Bebchuk, Fried and Walker (2002)) argue that managers' influence over their directors allows them to extract rents, for example, through excessive pay. An implication of the theory is that making boards more independent from management is key to improving corporate governance. The spectacular rise in executive pay over the 1990s has made the managerial power hypothesis, with all its implications, a popular view among politicians, regulators, academics, and the media. In the wake of the accounting scandals that led to the Sarbanes-Oxley Act of 2002, NYSE and Nasdaq revised their listing standards to improve corporate governance. The stock exchanges now require boards to have a majority of independent directors, as well as fully independent nominating, compensation, and auditing committees.

If independent directors are indeed better monitors of CEOs, then according to the managerial power hypothesis CEO pay should decline. To test this prediction, Chhaochharia and Grinstein (2009) (henceforth CG) use firms' compliance status before the rule change to identify the causal effect of board composition on CEO pay. The independence mandate is an exogenous constraint imposed by the stock exchanges and provides a quasi-experimental setting. The main advantage to CG's difference-in-difference approach is that they circumvent the endogeneity problem identified by Hermalin and Weisbach ((1998), (2003)) that has plagued the empirical literature on the effects of board characteristics.

CG find that CEO pay decreases 17% more in noncompliant firms than in compliant firms, which they interpret as the causal effect of improvements in board independence. Their findings are consistent with the managerial power hypothesis, that is, with the view that non-independent directors allow CEOs to extract rents in the form of higher pay.

We reexamine the impact of the new independence mandate on CEO pay using CG's data and methodology. We document that CG's main results are mostly attributable to the decrease in pay for just two CEOs, namely, Steve Jobs at Apple and Kosta Kartsotis at Fossil. We argue that Jobs' and Kartsotis' pay are outliers because they unduly impact the mean estimate of the noncompliance effect and they do not fit the story of the causal effect of board independence on CEO pay. Dropping these two firms from the full sample of 865 firms (i.e., 12 out of 5,190 firm-years) reduces the point estimate of the effect of board independence by 74%, rendering it economically insignificant and statistically indistinguishable from zero even at the 20% significance level. As such, the mean causal effect of board independence on CEO pay as identified by CG is not generalizable to large publicly traded firms.[1]

CG's results for compensation committees are also sensitive to the outliers. Excluding the two outliers uncovers an *increase* in CEO pay in firms whose compensation committees are not fully independent prior to the new listing requirements relative to compliant firms. Moreover, the increase in CEO pay is most pronounced in the presence of stronger shareholder monitoring (i.e., blockholder directors and concentrated institutional ownership). These findings are inconsistent with the view that independent directors prevent managers from extracting rents in the form of excessive

[1]The IRRC definition of independence is stricter than those of the NYSE/Nasdaq. In an attempt to adjust for the discrepancies, CG reclassify former employees as independent if three or more years have passed since termination. While reclassifying former employees, however, CG ignore other IRRC disqualifications of independence, such as business relationships. Therefore, they end up treating business relationships inconsistently: former employees with business ties to the firm are considered independent, while directors with business ties who were not formerly employed are not considered independent. An anonymous referee finds that in all the cases he/she checked, the relationships are immaterial under the NYSE/Nasdaq rules. If we treat all such relationships as immaterial, the number of firms not compliant with the new board independence requirements in CG's sample decreases from 142 to 50. Since the misclassification does not alter our conclusions, we use CG's definition of noncompliance. We plan to investigate this issue in more detail in separate research.

pay.[2]

The remainder of the paper is organized as follows. In Section 1.2, we replicate CG's estimate of the effect of board independence on CEO pay. In Section 1.3, we show that the magnitude of CG's estimate is sensitive to the outliers. In Section 1.4, we argue that the change in CEO pay at the outliers was not caused by the board independence mandate. In Section 1.5, we show that CG's result is fragile even if we allow the effect of independence to vary with the strength of shareholder monitoring prior to the independence requirement. In Section 1.6, we explore the effect of compensation committee independence on CEO pay. Section 1.8 concludes.

1.2 The Effect of Board Independence on CEO Pay: Replication of CG's Estimates

Column 1 of Table 1.1 reproduces CG's main result on board independence from Table II, column 1 of the published paper: the estimated effect of *noncompliant board* $\times$ *after* is -0.192 (with an implied p-value of 2.6%). In columns 2 and 3 we present the results from replicating CG's main result *using their data*.[3] The estimates do not match the published results.

Reconciling the differences requires two modifications of the estimates. First, one would have to combine the estimates from two regressions, namely, the larger point estimate from the regression that controls for CEO tenure (column 2) and the smaller standard error from the regression that excludes CEO tenure (column 3). As some firms have missing data on CEO tenure, controlling for CEO tenure reduces the sample size from 5,190 to 4,956 observations. Using the full sample by dropping tenure from the regression lowers the magnitude of the estimate slightly to -0.173

[2] Our findings are also congruent with Anderson and Bizjak (2003) and Wan (2003) in that director independence has little effect on executive pay.

[3] We also construct our own sample following CG's data requirements (six years of director data from IRRC, six years of CEO pay data, but allowing for missing observations on tenure). Our final sample contains 909 firms (including Apple and Fossil). Our findings become qualitatively stronger when we use our own sample instead of CG's, which suggests that CG's results are also sensitive to sample selection effects. The results are available in Table 1.A.1 in Appendix 1.A to keep the focus of the paper on the effect of outliers.

(p-value of 4.6%). Second, one would have to truncate the estimates after the third decimal place, instead of rounding them to the nearest thousandth.[4]

To minimize the possibility that sample selection is driving our results below (and to avoid contention over which sample to use), we use CG's full sample (i.e., 5,190 observations) as the benchmark for comparison throughout our paper. In doing so, we complement their data by hand-collecting missing observations on CEO tenure from various sources such as companies' websites and proxy statements, Hoover company records, and news and reports from Forbes and Business Week.[5] Column 4 of Table 1.1 contains the replicated results from the full sample, in which missing observations on CEO tenure have been replaced with hand-collected data. Overall, the magnitude and statistical significance of the coefficient of CG's main result remain quantitatively similar to those in the regression without the tenure variable.[6] The coefficient of -0.179 translates into a decline in CEO pay of 16.4% (p-value of 4.1%). Note that after the corrections and additions to CEO tenure observations, the coefficient on tenure becomes statistically significant at conventional levels.

1.3 The Impact of Outliers on CG's Estimates

Fig. 1.1 presents the histogram of the change in CEO pay for noncompliant firms. As only 142 sample firms are noncompliant with the new listing requirements, estimates of the mean effect of noncompliance are particularly susceptible to outliers among the noncompliant firms. CG read the proxy statements for some of the noncomplying firms that had the largest drop in compensation. They find that the decline

[4]To account for serial correlation within firms, CG use clustered standard errors at the firm-period level. The clusters, however, are not nested within firm-level panels. As such, the degrees of freedom should be adjusted using the *dfadj* option in Stata. On average, the adjustment increases the estimates of the standard errors by about 10%, but we omit the adjustment to make our results more comparable to those of CG. Using the placebo technique suggested by Bertrand, Duflo and Mullainathan (2004) yields similar results. Note that we report the within-R^2, which is maximized by the fixed effects estimator, whereas CG reported the overall-R^2.

[5]To allow future replication of our results, Table 1.A.2 in Appendix 1.A presents the year in which the executive was first appointed as the CEO of the company for observations with either missing or incorrect data on CEO tenure in the Execucomp database.

[6]Throughout the paper, our results are robust to using the actual sample that CG used in their main results (4,956 observations) and to excluding the tenure variable completely from the regressions.

in CEO pay at Adobe Systems and Compuware — which rank fourth and fifth in the distribution — appears to be linked to a reevaluation of incentive pay. However, there is no mention of the three firms with the largest decrease in CEO pay: Steve Jobs at Apple, Kosta Kartsotis at Fossil, and Jack Welch at GE. Apple and Fossil are clearly identifiable outliers among the noncompliant firms.[7] We argue that the decrease in CEO pay at Apple and Fossil makes them outliers for two reasons. First, the decrease is very large in magnitude, particularly for Apple. As such, these two firms unduly influence CG's estimate of the effect of board independence on CEO pay. Second, CEO pay at Apple and Fossil is idiosyncratic in nature. In a nutshell, Kartsotis insisted on the decrease himself, and Jobs' pay is erratic and tied to unusual circumstances. In other words, neither the magnitude nor determinants of the change in CEO pay at Fossil and Apple are representative of other firms in CG's sample or are driven by the recent board mandate. We devote Section 1.4 to an in-depth look at the circumstances of Kartsotis' and Jobs' pay cuts, but first explore the sensitivity of CG's main result to these outliers.

In column 5 of Table 1.1, we account for the outliers' excessive influence on CG's mean estimate by excluding Apple and Fossil from the regression. The magnitude of the coefficient on *noncompliant* $\times$ *after* drops from -0.179 to -0.047, or by 74%.[8] CG's main result becomes statistically insignificant even at the 20% level due to the decrease in its magnitude, and despite the large decrease in its standard error (p-value of 23%). Interestingly, excluding the outliers also affects the coefficients and standard errors of all other explanatory variables appreciably, even though the empirical model constrains the coefficients to be equal for compliant and noncompliant firms. In other words, removing Apple and Fossil significantly affects the mean estimates derived from 865 firms. This implies that the relationship between CEO pay and the explanatory

[7] Apple and Fossil constitute outliers based on z-scores exceeding ± 3.3, which corresponds to a probability of less than 0.1% of those values occurring (assuming that the change in $\ln(pay)$ is normally distributed). Oracle is a large positive outlier among the compliant firms. Excluding Oracle from our analyses further strengthens our results.

[8] Our results remain quantitatively similar throughout all our analyses in the paper even if we drop only Apple from the regressions. In this case, the magnitude of the coefficient of CG's main result drops from -0.179 to -0.068 (p-value of 9.8%), or by 62%.

variables at Apple and/or Fossil is fundamentally different from the other sample firms.

An alternative solution to dealing with outliers is to use the least absolute deviations method (i.e., median regression) for estimation, which is less sensitive to extreme observations. The main benefit to using a median regression is that we need not explicitly (and perhaps subjectively) identify outliers. In column 6 of Table 1.1, we present results from the median regression (including Apple and Fossil). Since the inclusion of a large number of variables exponentially increases the time required to obtain quantile regression estimates, we account for firm fixed effects by demeaning all variables and including industry-period dummies instead of industry-year dummies. We estimate bootstrapped standard errors to allow for heteroskedasticity and clustering at the firm-period level (Petersen (2009)). We find that CG's main result is weakened to -0.045 (p-value of 28%). We conclude that the median effect of board independence on CEO pay is economically and statistically insignificant.

Conceptually, if board independence indeed affects CEO compensation decisions, its influence should extend to the remuneration of non-CEO top executives. After all, the same directors who negotiate or approve CEO compensation are also responsible for the compensation of other top executives. If board independence strengthens directors' bargaining position vis-à-vis top executive officers, then we would also expect non-CEO executives' pay to decrease in noncompliant firms. In contrast, if CG's main result is driven mainly by outliers in CEO pay, then the board independence requirement should have no effect on the remuneration of non-CEO top executives. Column 7 presents the results. We include all non-CEO top executives with non-missing pay data in Execucomp in our regression, including those of Apple and Fossil. Again, CG's main result is weakened substantially to -0.031 and remains statistically insignificant at conventional levels (p-value of 28%).[9]

To summarize, we provide strong empirical evidence that CG's main finding on

[9]The results are robust to excluding some outliers in non-CEO pay changes (the influence of any one outlier on the mean estimate is mitigated by the larger number of observations). Our results also remain quantitatively similar if we use the average pay of these non-CEO top executives or just the highest-paid non-CEO top executive in our analysis.

the effect of board independence on CEO pay is fragile. In particular, CG infer the effect of noncompliance for a broad sample of 865 large and publicly traded firms primarily from the change in Steve Jobs' and Kosta Kartsotis' pay. We conclude that the mean effect documented by CG is not representative of the sample firms.

1.4 Do the Outliers Fit the Story?

Panels A and B of Table 1.2 present the various components of the total pay to Kosta Kartsotis and Steve Jobs.[10] Kartsotis earned approximately $255,000 annually in 2000 to 2004, but his pay dropped to nearly zero in 2005. Similarly, Jobs' total pay ranged from $75 million to $600 million per year between 2000 and 2003, and dropped to a symbolic $1 per year in 2004 and 2005.

As the boards of Apple and Fossil did not comprise a majority of independent directors prior to the passage of the board reform, at first sight the substantial drop in their CEOs' pay during the post-reform period is consistent with the claim that the board independence requirement did indeed affect CEO compensation decisions. Alternatively, it might be coincidence that CEO pay dropped in these firms, that is, the drop may be related to factors other than the board independence requirement. To disentangle these two competing hypotheses, we examine the individual pay to Kosta Kartsotis and Steve Jobs during our sample period. The plunge in their pay seems to be driven by CEO/firm-specific factors other than the board independence requirement.

1.4.1 Kosta Kartsotis

Kosta Kartsotis is the brother of Tom Kartsotis — founder of Fossil, former CEO, and chairman of the board in 2005. At the beginning of fiscal year 2005, Kosta and Tom were the firm's largest shareholders, owning about 30% of the firm's shares. Given the Kartsotis' continuing and pervasive influence on the firm — holding the

[10]The information on Fossil and Apple comes from their DEF 14A filings with the SEC (available from ⟨http://www.sec.gov/edgar.shtml⟩). We obtain historical stock prices from CRSP and political contributions from ⟨http://www.opensecrets.org⟩.

positions of founder, chief executive officer, chairman of the board, and largest shareholders between them — it is highly implausible that the pay cut was caused by the director independence mandate.

In fact, it was Kosta Kartsotis himself, rather than the compensation committee or the board as a whole, who proposed that his base salary be cut from $255,000 to $0 in 2005. The voluntary cut was motivated by his concern about Fossil's recent stock price performance. In 2005, the stock price of Fossil dropped by about 16%, compared to a 1% average increase for the industry. The pay cut can be described as symbolic, as Kosta Kartsotis' stake in the company exceeded $200 million in 2005.

Further, even prior to becoming majority-independent, the board followed Kartsotis' recommendation on pay. Kosta Kartsotis was appointed as the CEO of Fossil in October 2000. Between 2001 and 2004, the cumulative return on Fossil's stock was 398%, while that for the industry average was 207%. Despite outperforming his industry peers, and the compensation committee's explicit recognition in the firm's proxy statements for 2002 to 2004 that Kartsotis' pay was below the market median, Kartsotis repeatedly requested that his pay not be raised. His refusal to accept pay increases is contradictory to the claim that the board independence requirement has been important to compensation decisions at Fossil.

1.4.2 Steve Jobs

Steve Jobs went from earning more than $600 million in 2000 to $1 in 2005. What happened at Apple for Steve Jobs to experience such a drastic reduction in pay? Paradoxically, the decline in pay does not reflect a pay cut, but rather temporarily abnormal pay to Jobs in the early sample period from 2000 to 2003. His base salary has remained unchanged at $1 per year since he rejoined the company as interim CEO in September 1997. As Jobs' annual total pay typically consists only of the base salary, his total pay was merely $1 per year in 1998 and 1999, and it returned to $1 for 2004 to 2008.

Jobs' erratic compensation reflects four events. First, in fiscal year 2000, upon accepting the position of permanent CEO at Apple, Jobs was granted options with a Black-Scholes value of $600 million, which was the second largest annual pay ever awarded to a corporate executive in the U.S. at that time. Later that year, Apple's stock price dropped precipitously, rendering the options worthless. Second, in fiscal year 2001, Jobs received a $90 million bonus for his success as interim CEO during fiscal years 1997 to 1999. Third, in 2002, the board decided to grant Jobs additional options valued at $90 million, because the previously granted options no longer tied Jobs' pay to firm performance (these new options carried an exercise price of $18.30). Fourth, in 2003, Jobs voluntarily canceled his outstanding options when Apple's shares traded at around $14.50. The board chose to replace those options with restricted stocks worth $75 million. In 2004 and 2005, Jobs did not receive any additional stocks or options — his pay went back to the token $1 salary he earned in 1998 and 1999.

The firm fixed-effects model used by CG is inadequate to explain the erratic timing and magnitude of Jobs' pay.[11] Specifically, the time-series variation in Jobs' total pay violates the matching principle, i.e., Jobs' pay is typically not timed to match his contributions and services rendered during the period. More importantly, Jobs' recorded pay does not always reflect the decisions of the board in that year, but those of prior years. During the entire period from September 10, 1997 to December 1999, Jobs received total pay of merely $2 for serving as the interim CEO of the company, while the value of Apple's shares more than quadrupled. To reward him for his outstanding achievement during that period, the board granted Jobs a special executive bonus in the form of an aircraft in December 1999. The total cost of the aircraft (including tax benefits) was approximately $90 million and was eventually reported as income to Jobs in 2001 and 2002, because the aircraft was not physically transferred to him until 2001.

[11] To obtain a reliable estimate from the firm fixed-effects model, one requires a stable relationship between time-varying economic factors (e.g., firm sales, firm performance, and tenure) and CEO total pay. In Jobs' case, the relationship is unstable. As such, the time-series variation in his pay is poorly captured by the firm fixed-effects model.

Jobs' $600 million option grant in 2000 also violates the matching principle, because it provided him with multiple years' worth of annual stock options at once.[12] As such, this mega grant is equivalent to early payment for services that Jobs had not yet rendered, but was expected to render in future years.

In addition, Apple's poor stock performance contributed to Jobs' high pay in 2000 to 2003. When Apple's stock price was declining, the board replaced underwater options to maintain incentives. After 2003, when Apple's stock price was rising, no further grants were necessary. This negative relationship between pay and stock performance at Apple contrasts sharply with the empirical relationship found in Table 1.1 — the positive coefficient on stock returns shows that, on average, CEOs' pay increases with prior-year stock returns. Instead, some of Jobs' pay was contingent on prior pay becoming worthless (i.e., new grants were made only because previous grants ended up not costing Apple's shareholders anything).

Despite earning only a $1 salary in 2004 and 2005, Jobs was well compensated for his effort. The market value of his stock holdings increased from $75 million in March 2003 (date of stock grant) to $540 million in September 2005 (end of sample period). Furthermore, the impact of the fluctuations in the value of the stock and option grants on CG's finding is exacerbated by Jobs' token salary of $1 over the sample period (small changes in the dollar value of pay lead to large changes in the log value of pay at low income levels). Overall, the magnitude of CG's result seems to reflect a temporary restructuring of incentive pay at Apple rather than a systematic adjustment to the level of pay in large, publicly traded firms in the U.S.[13]

[12] On March 18, 2008, Jobs gave a deposition to the SEC regarding the option backdating case against two top executives at Apple, in which he described the mega option grant as designed to provide four years' worth of equity upfront. The full text of the deposition is available at ⟨http://images.forbes.com/media/2009/04/24/jobs-deposition.pdf⟩.

[13] The board room dynamics at Apple also illustrate the shortcomings of formal director independence. Jobs personally contributed $50,000 to the Democratic National Committee on November 1, 2000 (a soft money campaign contribution; after his options became worthless), but not to the Republicans. Then, in September 2002, former Vice President Al Gore joined Apple's board and compensation committee as an independent director. Also, from 2003 onward, continuing director Millard Drexler was deemed an independent director, despite his and Jobs' prior interlocking relationship. Until 2002, Jobs was CEO of Apple and served on Gap's board, while Drexler was CEO of Gap and served on Apple's board. Perhaps not coincidentally, Drexler also joined the compensation committee at Apple starting in 2003. Therefore, it is questionable whether the increase in formal board independence made Apple's directors more effective monitors.

To summarize, we find that including the large drops in pay for Kosta Kartsotis and Steve Jobs leads to false inferences about the effect of board independence on CEO pay for most other firms. The changes at Apple and Fossil do not fit the story of board independence causing a drop in CEO pay.

1.5 CEO Compensation, Board Independence, and Shareholder Monitoring

CG contend that effective shareholder monitoring mutes the effect of board independence on CEO pay. We follow CG in allowing the noncompliance effect to vary (i) between firms with and without a non-employee blockholder on the board (as suggested by Core, Holthausen and Larcker (1999)), and (ii) with institutional ownership concentration (as suggested by Hartzell and Starks (2003)).

1.5.1 Blockholder Directors

The data set supplied to us by CG does not contain their measure for the presence of blockholder directors. We follow CG in identifying the presence of director blockholders based on non-employee directors holding 5% or more of their firms' shares in 2002.[14] Column 1 in Panel A of Table 1.3 reproduces CG's main result on block ownership from Table VII, column 1 of the published paper: the estimated effect of *noncompliant board* $\times$ *after* $\times$ *no blockholder* is -0.270 (p-value of less than 0.01%). Column 2 presents the results from replicating CG's main result. Again, we fail to replicate CG's published results, particularly the standard errors. If we reversed the standard errors of the point estimates for noncompliant firms with and without blockholder directors, then we would obtain results reasonably close to CG's. In column 3 we present the replicated results based on the full sample. The magnitude of the coefficient of CG's main result drops slightly to -0.262 (p-value of 1.3%). CG's results

[14]CG offer two conflicting definitions for blockholder directors. On page 254 of the published article, they define a blockholder director as a nonemployee director who owns *5% or more* of the outstanding shares. However, on page 255, CG write that the ownership cutoff is *more than 5%* of the outstanding shares. We identify 35 noncompliant firms with blockholder directors under the first definition, compared to 34 noncompliant firms with blockholder directors under the second definition. The results are insensitive to which definition we use.

indicate that the board independence requirement led to a large decrease in CEO pay in noncompliant firms, but only in the absence of blockholder directors. Their findings suggest that blockholder directors are an effective monitoring substitute for board independence.

As neither Apple nor Fossil have non-employee blockholder directors on their boards in 2002, we investigate the sensitivity of CG's estimates to the outliers in columns 4 to 6. In column 4, we account for the outliers' excessive influence on CG's mean estimate by excluding them from the regression. The magnitude of the coefficient drops to -0.100, or by 62%, but it maintains its statistical significance with a p-value of 2.5%. Contrary to CG's findings, however, we also find a significant increase in CEO pay in noncompliant firms with non-employee blockholder directors (coefficient of 0.111; p-value of 7.7%). This result suggests that the board independence requirement has had the unintended consequence of increasing CEO pay in firms with blockholder directors.[15,16]

We present results from the median regression and from non-CEO top executive pay in columns 5 and 6 as alternative ways to examine the robustness of CG's main result to outliers. Our estimate from the median regression is -0.074.[17] The pay of non-CEO top executives responds even less to the board independence mandate (coefficient of -0.039). Neither estimate is statistically distinguishable from zero at conventional significance levels, indicating that outliers drive CG's published result.

[15] In additional robustness tests, we also control for changes in blockholder presence and institutional concentration, as well as interactions between the substitute monitor classifications prior to the rule change with a dummy identifying the period after the rule change. The results remain similar.

[16] Using our own data set (as opposed to CG's), the noncompliance effect is also positive and highly significant in firms with blockholder directors (coefficient of 0.160). However, the coefficient for noncompliant firms without blockholder directors is of very small magnitude (-0.005) and not distinguishable from zero. The estimation results are available in Table 1.A.1.

[17] The absence of blockholder directors is the only instance in which using industry-year dummies rather than industry-period dummies in a median regression with demeaned data yields an estimate of the effect of noncompliance that is statistically significant at the 10% level (coefficient of -0.079, p-value of 7.2%).

1.5.2 Institutional Ownership Concentration

We follow CG in defining institutional ownership concentration as the sum of shares held by the five largest institutional investors relative to total institutional shareholdings in the firm. Note that CG deviate from Hartzell and Starks (2003) in classifying high and low ownership concentration as belonging to the top and bottom quartile of the distribution in their sample, whereas Hartzell and Starks use concentration as a continuous measure. For the sake of comparability to CG's results, we follow CG's classification into quartiles based on all sample firms' 2002 observations.[18]

Column 1 in Panel B of Table 1.3 reproduces CG's results on institutional ownership concentration from Table VII, column 2 of the published paper. They find that noncompliant firms with low concentration of institutional holdings decrease CEO pay by 21.2% more than compliant firms (coefficient of -0.238; implied p-value of 2.6%). However, CG acknowledge that the decrease in CEO pay in noncompliant firms with low concentration of institutional ownership is not statistically different from the decrease for firms with high concentration, for which they report a statistically insignificant point estimate of -0.176.

Column 2 in Panel B of Table 1.3 displays our replication estimates. The estimates differ greatly from CG's published results, as we find that neither noncompliant firms with high institutional concentration nor noncompliant firms with low institutional concentration decrease CEO pay relative to compliant firms (neither estimate is distinguishable from zero at conventional significance levels).

Why are our estimates so different from CG's published results?[19] We suspect that the difference stems from a discrepancy in CG's research design and their imple-

[18]The data set supplied by CG contains measures of institutional ownership by the top 5 institutions and total institutional ownership, but some observations are missing. We were able to match every firm in CG's data set with institutional ownership data from TFN. To keep the sample consistent throughout the paper, we proceed using our measure of institutional ownership concentration. For each firm, we calculate the average value of institutional concentration over the four quarters in calendar year 2002. The results based on CG's measures are similar. Our findings remain qualitatively identical when we use the continuous measure of institutional ownership concentration.

[19]Without access to CG's coding or clarifications on the implementation, we are unable to replicate their published results (even using CG's data on institutional ownership). Our replication attempts rely on trial and error. The following section is based on the replication that yields estimates closest to the published results.

mentation of it. The research design for institutional ownership concentration, as laid out in their published paper, is problematic. CG allow the effect of noncompliance to differ between firms with high and low institutional ownership concentration (i.e., firms in the top and bottom quartiles), but implicitly constrain the noncompliance effect of firms with institutional concentration in the interquartile range to be zero (i.e., those firms are treated like compliant firms). However, in the implementation CG appear to treat all firms in the bottom three quartiles as having low institutional ownership concentration.

The next two columns present results based on modifications of CG's original methodology. For column 3, we allow the effect of noncompliance to differ across all four quartiles of institutional concentration. As before, we find that noncompliant firms in the top and bottom quartiles of institutional concentration do not decrease CEO pay by more than compliant firms. Since both outliers — Apple and Fossil — belong to the third quartile, it is not surprising that the decrease in CEO pay is concentrated there. The point estimate of -0.804 is unrealistic, as it suggests that board independence in the presence of medium-to-high ownership concentration causes CEO pay to drop by over 55%. For column 4, we redefine low institutional concentration to encompass all noncompliant firms in the bottom three quartiles of the distribution. These results are closest to CG's published results. Note that the low institutional concentration group now includes Apple and Fossil, which drive both the magnitude and significance of CG's result.

As before, we account for the outliers' influence on CG's mean estimate by excluding them from the regression (column 5), using a median regression (column 6), and evaluating the effect of board independence on non-CEO executive pay (column 7). We continue to group the bottom three quartiles into the low concentration category. In all cases, the estimates of the effect of noncompliance on executive pay are economically negligible and statistically indistinguishable from zero.

To summarize, our results indicate that CG's main findings on substitute monitors are not robust and are driven by the decrease in CEO pay at Apple and Fossil. We find no support for the hypothesis that the board independence requirement led to a decrease in CEO pay in noncompliant firms with low (or high) institutional ownership concentration.

1.6 Compensation Committees

Once we account for the effect of outliers, we find that the requirement for a fully independent compensation committee, rather than that for a majority of independent directors on the board, affects the level of CEO pay.

Column 1 of Table 1.4 reproduces CG's main result from Table II, column 2 of the published paper: the estimated effect of *compensation committee noncompliant* $\times$ *after* (hereafter referred to as CC-noncompliance) is -0.014 (p-value of 83%). Column 2 of Table 1.4 contains the estimates from the full sample. The magnitude and statistical significance of the coefficient on CC-noncompliance are nearly zero, which if true suggests that the requirement for independent compensation committees has no influence on CEO compensation.

In column 3, we remove Apple and Fossil from the regression.[20] Without the downward influence of Jobs' huge pay decrease on the estimate, the magnitude of the coefficient increases and turns positive. Specifically, the logarithm of CEO pay increases by an additional 0.069 in firms not compliant with the compensation committee independence rule relative to compliant firms, and the result is statistically significant at the 5% level (p-value of 2.3%). This result indicates that the requirement for compensation committee independence has not only been ineffective at reducing CEO pay, but has had the presumably unintended consequence of raising CEO pay. We obtain a similar estimate, albeit smaller in magnitude (0.046, p-value of 4.3%), from the pay of non-CEO executives (column 4). While using committees instead of boards impacts the noncompliance coefficients dramatically, the standard errors remain similar (compared to columns 5 and 7 of Table 1.1).[21]

[20] Apple's compensation committee did not comply with the new requirements in 2002, but Fossil's did. We continue to remove both firms from the regression for consistency in the presentation of our results. The results are virtually unchanged if we keep Fossil in the sample, as its impact on the estimate for compliant firms is negligible.

[21] Using median regressions, we find no significant effect of compensation committee independence on executive compensation levels throughout this section.

To examine how CEO pay changes in the presence or absence of monitoring substitutes, we follow CG in allowing the CC-noncompliance effect to vary with the presence of blockholder directors and the concentration of institutional investor ownership. In column 5 of Table 1.4, we find that the increase in CEO pay is concentrated in CC-noncompliant firms with blockholder directors. Numerically, CEO pay in those firms increases by an additional 15% (coefficient of 0.136, p-value of 0.4%) when compared to the CC-compliant firms. The effect is economically meaningful and statistically significant at well below the 5% level. We find a similar but again smaller effect for non-CEO executives. Our results in column 6 show that non-CEO pay rises by an additional 8% (p-value of 4.3%) in those firms relative to CC-compliant firms. However, the effect of CC-noncompliance differs only marginally between firms with high and low institutional concentration (columns 7 and 8), but it remains positive. Based on CG's sample, we conclude that CC-noncompliance leads to an increase in CEO pay, regardless of institutional shareholder concentration.[22]

Taken together, our findings indicate that the requirement for compensation committee independence produces a perverse effect on executive remuneration in noncompliant firms, particularly in the presence of blockholder directors.

1.7 Discussion

Here we discuss two plausible explanations of our findings that compensation committee independence leads to an increase in executive pay, and that the increase is concentrated in firms with powerful monitors: (i) the independence mandate forces noncompliant firms to move away from their optimum governance structures; and (ii) in response to the Sarbanes-Oxley Act of 2002, director priorities shift from reigning in CEO pay toward other tasks, especially in noncompliant firms.

[22]We obtain economically and statistically highly significant estimates for compensation committee noncompliance when we use our own data set. The CC-noncompliance effect is concentrated in firms with blockholder directors (coefficient of 0.137) and with high institutional concentration (coefficient of 0.167). On the other hand, the coefficients for firms without blockholder directors and low institutional concentration are of very small magnitude (0.037 and 0.026) and not distinguishable from zero. These results imply that the compensation committee independence requirement has had the unintended consequence of increasing CEO pay, especially in firms with effective shareholder monitoring. The estimation results are available in Table 1.A.1.

1.7.1 Endogenous Board and Committee Composition

The perverse effect of the director independence mandate on CEO pay is consistent with the view that the composition of boards and compensation committees is determined endogenously to fit the needs and circumstances of shareholders, managers, and firms (e.g., Fama and Jensen (1983) argue that boards are an effective solution to agency problems; and Hermalin and Weisbach ((1998), (2003)) emphasize the endogenous nature of boards). To the extent that non-independent directors are better monitors than independent directors, they play an integral part in the makeup of the compensation committee or board. The independence mandate, however, forces noncompliant firms to alter their board and committee structures to suboptimal ones. In other words, the new listing requirements force firms to reduce the influence of potentially more competent and/or powerful monitors (non-independent directors) by replacing them with or adding less competent/powerful monitors (independent directors). The regulation thus tilts the bargaining power towards management and away from the compensation committee in negotiating pay, especially in the presence of substitute monitors such as blockholder directors or institutional ownership concentration (consistent with empirical evidence of endogenously determined ownership structure in Demsetz and Lehn (1985) and Demsetz and Villalonga (2001).[23]

While most of the recent corporate governance literature views boards as endogenously determined institutions, independent directors are typically assigned the role of monitors and non-independent directors are cast as advisors (e.g., Boone, Field, Karpoff and Raheja (2007), Coles, Daniel and Naveen (2008), and Linck, Netter and Yang (2008)). Yet, if the increase in CEO pay in noncompliant firm is any indication of director monitoring, then independent directors appear to be worse monitors than non-independent directors.

One reason non-independent directors may be better monitors is that they typically own more of their companies' shares than independent directors. Their equity

[23]It also applies to firms that warrant control rights, e.g., family firms and founder-managed firms. The director regulation reduces the ability of these owners to exert effective control over their firms' directions.

stakes provide more powerful incentives, and perhaps more power, to monitor management and to prevent excessive managerial compensation. In the 2002 IRRC data, for example, only 3.2% of independent directors, but 35.6% of non-independent directors have voting rights of 1% or more in their firms (with means of 0.2% vs. 2.8%). A second reason for non-independent directors to be better monitors is that they are more familiar with the firm, its market, and even the management team due to their business relationships with the firms (e.g., as suppliers, bankers, and attorneys).[24] The additional information can be leveraged into more accurate performance evaluations, as well as better compensation and retention decisions. Third, as emphasized by Acharya, Myers and Rajan (2008), junior managers with career concerns and the promise of future rents can effectively discipline CEOs, because they can threaten to withhold their productive effort. Thus, non-independent directors may well be more effective monitors and pay negotiators than independent directors.

Our findings and rationalization are very similar to those of Anderson and Bizjak (2003). They argue that CEOs with substantial equity stakes or founders may want to serve on the compensation committee to help design optimal contracts for other key managers. More generally, insiders serving on compensation committees may provide insights into the special social and political aspects of their company that help improve executive compensation decisions. Indeed, Anderson and Bizjak do not find that CEO pay is higher when CEOs or insiders serve on their own compensation committees. To the contrary, the insiders typically own more of their firms' equity and thus have stronger incentives, but tend to earn less. Interestingly, they also find that when founders or their family members leave a compensation committee, CEO pay increases subsequently. Similarly, Core et al. (1999) and Wan (2004) find that CEOs also earn significantly less total pay in firms with a greater representation of current officers sitting on the board of directors.

[24]IRRC considers directors with business relationships as non-independent. The NYSE/Nasdaq independence definition is more lenient, as it disqualifies only directors with material business relationships. Since the materiality of those relationships is not available in IRRC, CG opt not to reclassify non-independent directors as independent.

1.7.2 Shifting Priorities

Our second explanation for the observed increase in CEO pay in noncompliant firms is based on the increase in director responsibilities imposed by the contemporaneous Sarbanes-Oxley Act of 2002. SOX was enacted in response to a string of major corporate accounting scandals with the goal to restore public confidence in financial markets. The provisions of SOX aim to improve the accuracy and integrity of financial reporting, for example by strengthening disclosure and internal controls, enhancing oversight and accountability, and increasing the penalties for misreporting. In addition, the structure of pay as opposed to its level has come under intense scrutiny in an attempt to reduce managerial incentives for misreporting and excessive risk-taking.[25] Since most of the workload post-SOX is expected to be carried out by independent directors, the dramatic increase in director responsibilities forces board members to reprioritize their tasks.

Even after satisfying the new listing standards, previously noncompliant firms have a lower representation of independent directors on their boards. In CG's data, the average fraction of independent directors on compliant boards increases from 72% to 77% from the pre- to post-independence mandate, and from 43% to 58% on noncompliant boards. In addition, noncompliant boards also experience higher director turnover, which means that more of their directors are busy getting to know the firm, its stakeholders, and economic environment. Those boards must also spend more time on recruiting qualified directors, a task that was deemed difficult even before SOX increased the demand for independent directors.[26] As a consequence, the boards and committees in noncompliant firms are likely to be more time-constrained in fulfilling their post-SOX responsibilities than their counterparts in compliant firms.[27]

[25]A number of recent empirical studies document a differential shift in the composition of pay for compliers and noncompliers around the new listing requirements. For example, see Chung (2008), Cohen, Dey and Lys (2007), and Guthrie, Kwon and Sokolowsky (2008).

[26]World at Work and Towers Perrin conducted a survey on outside director pay and practices in 2004 (available from ⟨http://www.worldatwork.org/pub/outside-director-0104.pdf⟩). Of the respondents who indicated that they recruited new directors for 2004, 33% found recruiting more difficult than usual and less than 4% found it easier than usual.

[27]This explanation is consistent with the findings of Fich and Shivdasani (2006), who document that busy outside directors are associated with weak governance.

Linck, Netter and Yang (2009) provide empirical evidence that directors indeed reevaluate their priorities post-SOX. They document that the meeting frequencies of audit and nominating committees rise almost two- and four-fold from 2001 to 2004. The meeting frequency of compensation committees, on the other hand, is almost unchanged. They also show that the percentage of independent directors who serve on all committees — auditing, compensation, and nominating — more than quadruples between 2001 and 2004, from 2.14% to 9.03%. The increased demand on and for directors is reflected in rapidly rising insurance premiums (with the average rising nearly five-fold between 2001 and 2004) and director pay (an increase of 50%).[28]

To substantiate our hypothesis that independent directors in compliant and noncompliant firms experience a differential increase in their responsibilities and workload, we calculate the change in committee membership of independent directors for compliant and noncompliant firms separately. Specifically, in our replicated sample we find that in firms with compliant compensation committees the fraction of independent directors who simultaneously serve on the audit, compensation, and nomination committees increases by 2.68 percentage points on average (or a 42% increase over the 2000 to 2002 mean). In contrast, the increase in noncompliant firms amounts to 6.02 percentage points (an 84% increase). Using the average change in the number of committee memberships per independent director yields similar conclusions.

To put the value of reigning in CEO pay into perspective, we make some back-of-the-envelope comparisons of the costs and benefits of lowering CEO pay relative to other director responsibilities within the monitoring realm. CG estimate that the independence mandate reduces CEO pay by 17.5%, which translates into $362,000 for the typical noncompliant firm in CG's sample. The value drops to $95,000 if we base the calculation on the estimate excluding Apple and Fossil. For our first point of comparison, Taylor (2008) estimates that boards act as if firing a CEO costs them the equivalent of 5.9% of firms' assets ($111 million at the median). To the extent

[28]The caveat to the findings by Linck et al. is that some of these findings are based on small samples or on non-IRRC data. As such, we cannot be sure about the exact magnitudes of the changes in director responsibilities for the firms in the CG sample, but we have no reason to expect that the general trend would be any different for our sample firms.

that boards dislike cutting CEOs' pay as much as firing them it is entirely implausible that boards would — immediately upon becoming formally independent — set out to reduce executives' pay. Second, Karpoff, Lee and Martin (2008b) find that the cost to firms of being caught cooking the books carries a reputational loss worth \$20 million at the median (\$264 million at the mean), in addition to fines and litigation costs. SOX further increases the penalties for corporate wrongdoing and the likelihood that directors are personally implicated. Third, as emphasized by Gabaix and Landier (2008), even small differences in CEO talent are magnified by firm size to impact shareholder wealth significantly. If the selection of a more talented CEO improves a firm's market value by 1%, firm value would increase by \$20 million at the median (\$72 million at the mean).

Given the relatively low importance of reigning in the level of CEO pay, it is plausible that newly independent boards and compensation committees spend relatively less time on negotiating the level of CEO pay. As a result, CEO pay increases in noncompliant firms relative to compliant firms.

1.8 Conclusion

Using corporate governance listing requirements imposed by the U.S. stock exchanges as a quasi-natural experiment to examine whether board structure influences CEO remuneration, Chhaochharia and Grinstein (2009) find that CEO pay decreases by about 17% in firms with noncompliant boards relative to firms with a majority of independent directors.

We reexamine CG's evidence using their data and methodology and find that the results are fragile. Their results are driven by two outlier firms in CEO pay, namely, Apple and Fossil. After excluding these two outlier firms (12 firm-year observations) from the full sample of 865 firms (5,190 firm-year observations), our results indicate that (i) board independence does not affect the level of CEO pay; (ii) compensation committee independence causes CEO pay to increase; and (iii) the increase in CEO pay occurs only in the presence of blockholder directors or high institutional ownership concentration, both of which are considered to be monitoring substitutes. We draw similar conclusions based on median regressions and the change in pay for non-CEO executives. These results are based on CG's sample selection criteria, definitions, and methodology and may not be generalizable.

Taken together, there is little evidence that the board reforms have had any meaningful effect on the level of CEO pay. While it is tempting to reject the managerial power hypothesis, the evidence alternatively calls into question the effectiveness of director independence in corporate governance or the importance of reducing CEO pay.

One plausible explanation for the increase in pay in firms with noncompliant compensation committees is that non-independent directors — perhaps due to more powerful incentives or superior information — have more bargaining power than independent directors, and thus monitor more effectively. It is also consistent with directors shifting their attention away from reigning in CEO pay levels and toward other board responsibilities, especially in previously noncompliant firms. Alternatively, the managerial power hypothesis (e.g., Bebchuk, Fried, and Walker (2002)) may not be a valid representation of the determination of executive pay. Distinguishing between these interpretations remains an open and pertinent research question.

Figure 1.1: Histogram of Changes in CEO Pay for Noncompliant Firms

This figure plots the histogram of the changes in CEO pay around the new stock exchange listing requirements for noncompliant firms.

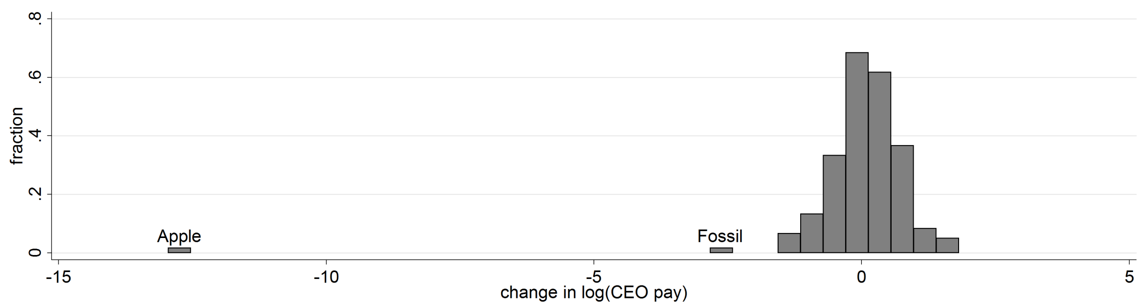


Table 1.1: The Effect of Board Independence on CEO Pay

The results in this table are based on the data used and supplied by CG. The empirical model also follows CG: $ln(CEO\ pay) = a_0 + a_1 \times D(noncompliant\ board\ '02)_i \times D('03\text{–}'05)_t + [controls_{it}] + [FE_i] + [FE_{jt}] + e_{it}$, where CEO pay is total CEO compensation (variable *tdc1* in Execucomp), and *Noncompliant Board* is a binary variable that takes the value of one if the firm did not have a majority of independent directors on the board in 2002 and zero otherwise. A director is defined as an independent director if the director was not an employee of the firm during the previous three years, did not have any family affiliation of the officers of the firm, and did not have any material business transactions with the firm. *before* and *after* are period indicators, taking the value of one if the observation is in the pre-mandate period (2000 to 2002) or post-mandate period (2003 to 2005), and zero otherwise. Controls include: *Sales*, the natural log of company sales (Compustat data item 12); *ROA*, the natural log of one plus net income before extraordinary items (data item 18) scaled by the book value of assets (data item 6) — all measured in $(t-1)$; *RET*, the natural log of one plus the annual stock return (with dividends reinvested), measured in year $(t-1)$; *Tenure*, the natural log of one plus the number of years the CEO served in the firm; and *Tenure (adj)*, CEO tenure, with hand-collected corrections and additions to replace missing *Tenure* observations. *Sales*, *ROA*, and *RET* are interacted with the period indicators before and after. We include firm fixed-effects (FE_i) and industry-year dummies (FE_{jt}) in the regressions. *Industry-year dummies* are based on the Fama and French (1997) 48-industry classification interacted with year dummies. All nominal variables are adjusted for inflation using 2002 as the base year. The numbers in parentheses are heteroskedasticity-robust standard errors, clustered at the firm-period level. ***, **, and * indicate statistical significance at the 10%, 5%, and 1% levels.

	Published and Replicated Results				Effect of Outliers		
	(1) Published Results	(2) Replicated Results	(3) Replicated Results w/o Tenure	(4) Replicated Results w/ Tenure adj.	(5) Excluding Apple & Fossil	(6) Median Regression	(7) Non-CEO Top-Executives
Noncompliance	-0.192**	-0.193**	-0.173**	-0.179**	-0.047	-0.045	-0.031
× after	(0.086)	(0.094)	(0.086)	(0.087)	(0.039)	(0.042)	(0.028)
Sales	0.305***	0.306***	0.319***	0.326***	0.379***	0.356***	0.292***
× before	(0.066)	(0.066)	(0.063)	(0.063)	(0.048)	(0.083)	(0.035)
Sales	0.268***	0.268***	0.280***	0.287***	0.355***	0.416***	0.272***
× after	(0.072)	(0.073)	(0.069)	(0.069)	(0.048)	(0.048)	(0.035)
ROA	0.321	0.322	0.290	0.311	0.164	0.620*	0.209
× before	(0.399)	(0.399)	(0.389)	(0.390)	(0.375)	(0.335)	(0.182)
ROA	0.260*	0.260*	0.268*	0.278*	0.209*	0.172	0.016
× after	(0.150)	(0.150)	(0.149)	(0.152)	(0.120)	(0.155)	(0.088)
RET	0.123***	0.124***	0.117***	0.117***	0.118***	0.183***	0.094***
× before	(0.033)	(0.034)	(0.033)	(0.034)	(0.033)	(0.029)	(0.026)
RET	0.269***	0.270***	0.273***	0.276***	0.302***	0.228***	0.189***
× after	(0.048)	(0.049)	(0.047)	(0.047)	(0.042)	(0.030)	(0.029)
Tenure	-0.034	-0.034					
	(0.022)	(0.023)					
Tenure (adj.)				-0.046**	-0.034*	0.024	
				(0.022)	(0.020)	(0.020)	
# firm-years	5,190	4,956	5,190	5,190	5,178	5,190	22,736
# firms	865	841	865	865	863	865	865
Adj. R^2	0.260	0.103	0.104	0.105	0.124		0.062

Table 1.2: CEO Pay at Fossil and Apple from 2000 to 2005

Notes: (i) *All other compensation* to Kosta Kartsotis refers to the premiums paid by the company on his term life insurance policies. (ii) In December 1999, Jobs was awarded a special executive bonus in the form of an aircraft for serving as the company's interim CEO between September 1997 and December 1999. The total cost of the aircraft was about $90 million. The entire cost of the aircraft was initially reported as a bonus to Jobs in 2000. Later, however, the bonus was reclassified into four different income components to Jobs in 2001 and 2002, because the aircraft was not physically transferred to him until 2001. The reclassification reflects that the purchase of the aircraft involved two payments: approximately $40.5 million in 2001 and approximately $2.7 million in 2002. The company also made two corresponding payments to settle related tax obligations, reported as *All other compensation* of $40.5 million in 2001 and $1.3 million in 2002.

Panel A: Compensation to Kosta Kartsotis at Fossil

Year	Salary	Bonus	Restricted Stock Grants	Option Grants (Black-Scholes)	All Other Compensation	Total Pay
2000	$255,000	$0	$0	$0	$35	$255,035
2001	$255,000	$0	$0	$0	$21	$255,021
2002	$255,000	$0	$0	$0	$17	$255,017
2003	$255,000	$0	$0	$0	$324	$255,324
2004	$255,000	$0	$0	$0	$220	$255,220
2005	$0	$0	$0	$0	$180	$180

Panel B: Compensation to Steve Jobs at Apple

Year	Salary	Bonus	Restricted Stock Grants	Option Grants (Black-Scholes)	All Other Compensation	Total Pay
2000	$1	$0	$0	$600,347,400	$0	$600,347,351
2001	$1	$43,511,534	$0	$0	$40,484,594	$83,996,129
2002	$1	$2,268,698	$0	$89,444,690	$1,302,795	$93,016,179
2003	$1	$0	$74,750,000	$0	$0	$74,750,001
2004	$1	$0	$0	$0	$0	$1
2005	$1	$0	$0	$0	$0	$1

Table 1.3: The Impact of Shareholder Monitoring

To the extent that the presence of monitoring substitutes mutes the effect of board independence, one would expect the decrease in pay to be concentrated in noncomplying firms without monitors. Here we modify the empirical model of Table 1.1 to allow the effect of noncompliance to differ between the presence and absence of substitute monitors. *Blockholder* is a binary variable that equals one if a firm has any non-employee directors who own more than 5% of the company's shares and zero otherwise. *High concentration* is a binary variable that takes the value of one if a firm's institutional ownership concentration falls into the top quartile. The other concentration variables — *upper middle*, *lower middle*, and *low concentration* — are also binary variables indicating the lower three quartiles of institutional concentration. Note that *low concentration* encompasses the bottom quartile in columns 1 to 3, and the bottom three quartiles in columns 4 to 7. Concentration of institutional ownership is the proportion of institutional investor ownership accounted for by the five largest institutional investors in the firm. See Section 1.5.2 for more details. All other variables are defined in Table 1.1. The numbers in parentheses are heteroskedasticity-robust standard errors, clustered at the firm-period level. ***, **, and * indicate statistical significance at the 10%, 5%, and 1% levels.

Panel A: Blockholder Directors

	Published and Replicated Results			Effect of Outliers		
	(1) Published Results	(2) Replicated Results	(3) Replicated Results w/ Tenure (adj)	(4) Excluding Apple & Fossil	(5) Median Regression	(6) Non-CEO Top-Executives
Noncompliance						
× after						
× blockholder	0.054	0.053	0.075	0.111*	0.044	-0.006
	(0.106)	(0.073)	(0.067)	(0.063)	(0.064)	(0.046)
× no blockholder	-0.270***	-0.273**	-0.262**	-0.100**	-0.074	-0.039
	(0.063)	(0.114)	(0.106)	(0.044)	(0.048)	(0.033)
Sales	0.333***	0.314***	0.334***	0.384***	0.357***	0.293***
× before	(0.055)	(0.066)	(0.062)	(0.048)	(0.085)	(0.036)
Sales	0.298***	0.278***	0.296***	0.361***	0.425***	0.273***
× after	(0.056)	(0.072)	(0.067)	(0.048)	(0.048)	(0.035)
ROA	0.285	0.339	0.331	0.178	0.614	0.211
× before	(0.256)	(0.399)	(0.390)	(0.375)	(0.334)	(0.182)
ROA	0.249	0.253*	0.271*	0.204*	0.170	0.015
× after	(0.161)	(0.149)	(0.150)	(0.119)	(0.161)	(0.088)
RET	0.122***	0.123***	0.116***	0.117***	0.181***	0.093***
× before	(0.037)	(0.034)	(0.034)	(0.033)	(0.029)	(0.026)
RET	0.265***	0.273***	0.279***	0.304***	0.229***	0.190***
× after	(0.051)	(0.048)	(0.046)	(0.042)	(0.030)	(0.029)
Tenure	-0.034	-0.036				
	(0.022)	(0.023)				
Tenure (adj.)			-0.048**	-0.035*	0.021	
			(0.022)	(0.020)	(0.020)	
# firm-years	5,190	4,956	5,190	5,178	5,190	22,736
# firms	865	841	865	863	865	865
Adj. R^2	0.280	0.105	0.107	0.125		0.062

Panel B: Institutional Ownership Concentration

	Published and Replicated Results				Effect of Outliers		
	(1) Published Results	(2) Top vs. Bottom Quartiles	(3) All Quartiles	(4) Top vs. Bottom 3 Quartiles	(5) Excluding Apple & Fossil	(6) Median Regression	(7) Non-CEO Top-Executives
Noncompliance							
× after							
× high conc	-0.176	-0.062	-0.092	-0.096	-0.083	-0.046	-0.046
	(0.112)	(0.058)	(0.059)	(0.059)	(0.056)	(0.061)	(0.045)
× upp-mid conc			-0.804**				
			(0.350)				
× low-mid conc			0.133*				
			(0.073)				
× low conc	-0.238**	-0.038	-0.072	-0.224*	-0.027	-0.042	-0.023
	(0.107)	(0.075)	(0.077)	(0.124)	(0.047)	(0.049)	(0.034)
Sales	0.304***	0.312***	0.310***	0.327***	0.379***	0.356***	0.292***
× before	(0.065)	(0.067)	(0.065)	(0.062)	(0.048)	(0.082)	(0.035)
Sales	0.266***	0.278***	0.270***	0.289***	0.354***	0.416***	0.272***
× after	(0.071)	(0.071)	(0.071)	(0.068)	(0.048)	(0.048)	(0.035)
ROA	0.326	0.327	0.325	0.309	0.164	0.620*	0.210
× before	(0.398)	(0.396)	(0.381)	(0.389)	(0.376)	(0.338)	(0.182)
ROA	0.258	0.268*	0.279*	0.280*	0.208*	0.172	0.016
× after	(0.150)	(0.149)	(0.153)	(0.153)	(0.120)	(0.157)	(0.088)
RET	0.123***	0.117***	0.114***	0.116***	0.118***	0.183***	0.094***
× before	(0.036)	(0.034)	(0.033)	(0.033)	(0.033)	(0.029)	(0.026)
RET	0.270***	0.276***	0.274***	0.276***	0.303***	0.227***	0.189***
× after	(0.048)	(0.047)	(0.047)	(0.047)	(0.042)	(0.030)	(0.029)
Tenure	-0.033						
	(0.003)						
Tenure (adj.)		-0.043**	-0.038*	-0.045**	-0.034*	0.024	
		(0.021)	(0.021)	(0.022)	(0.020)	(0.020)	
# firm-years	5,190	5,190	5,190	5,190	5,178	5,190	22,736
# firms	865	865	865	865	863	865	865
Adj. R^2	0.280	0.103	0.115	0.106	0.124		0.062

Table 1.4: The Effect of Compensation Committee Independence on CEO Pay

This table repeats the regressions displayed in Tables 1.1 and 1.3, except that we determine firms' noncompliance status from their compensation committee independence. *Noncompliant* now takes the value of one if the firm did not have a fully independent compensation committee in 2002 and zero otherwise. *Blockholder* and *High institutional concentration* are dummies indicating the presence of substitute monitors prior to the independence mandate (see Table 1.3 for more details). *Low institutional concentration* encompasses firms in the bottom three quartiles. All other variables are defined in Table 1.1. The numbers in parentheses are robust standard errors, clustered at the firm-period level. ***, **, and * indicate statistical significance at the 10%, 5%, and 1% levels.

	Published and Replicated Results		Effect of Outliers		Block Ownership		Instit. Ownership Concentration	
	(1) Published Results	(2) Replicated Results w/ Tenure adj.	(3) Exclude Apple & Fossil	(4) Non-CEO Top-Execs	(5) Exclude Apple & Fossil	(6) Non-CEO Top-Execs	(7) Exclude Apple & Fossil	(8) Non-CEO Top-Execs
Noncompliance	-0.014	-0.000	0.069**	0.046**				
× after	(0.064)	(0.061)	(0.031)	(0.023)				
× blockholder					0.136***	0.080**		
					(0.047)	(0.039)		
× no block					0.049	0.036		
					(0.035)	(0.026)		
× high inst conc							0.073	0.037
							(0.047)	(0.037)
× low inst conc							0.067*	0.049*
							(0.037)	(0.026)
Sales	0.290***	0.310***	0.370***	0.286***	0.372***	0.287***	0.370***	0.287***
× before	(0.068)	(0.064)	(0.048)	(0.035)	(0.048)	(0.035)	(0.048)	(0.035)
Sales	0.259***	0.277***	0.350***	0.269***	0.352***	0.270***	0.350***	0.269***
× after	(0.073)	(0.070)	(0.048)	(0.035)	(0.048)	(0.035)	(0.048)	(0.035)
ROA	0.346	0.331	0.183	0.223	0.178	0.221	0.182	0.224
× before	(0.404)	(0.394)	(0.377)	(0.183)	(0.377)	(0.183)	(0.377)	(0.183)
ROA	0.248*	0.267*	0.199*	0.011	0.198*	0.010	0.199*	0.011
× after	(0.148)	(0.150)	(0.119)	(0.087)	(0.118)	(0.087)	(0.119)	(0.087)
RET	0.124***	0.117***	0.119***	0.094***	0.118***	0.094***	0.119***	0.094***
× before	(0.034)	(0.034)	(0.033)	(0.026)	(0.033)	(0.026)	(0.033)	(0.026)
RET	0.269***	0.276***	0.303***	0.190***	0.303***	0.190***	0.303***	0.190***
× after	(0.048)	(0.047)	(0.042)	(0.029)	(0.042)	(0.029)	(0.042)	(0.029)
Tenure	-0.029							
	(0.024)							
Tenure (adj.)		-0.042**	-0.032		-0.032		-0.032	
		(0.021)	(0.020)		(0.020)		(0.020)	
# firm-years	5180	5190	5178	22,736	5178	22,736	5178	22,736
# firms	865	865	863	865	863	865	863	865
Adj. R^2	0.260	0.103	0.124	0.062	0.125	0.062	0.124	0.062

1.A Appendix

Table 1.A.1: Replication of the Results from Tables 1.1, 1.3, and 1.4 Using Our Data

The results in this table are based on our own sample. In constructing this sample, we follow all of CG's data requirements (six years of director data from IRRC, six years of CEO pay data from Execucomp, but allowing for missing observations on tenure) and definitions. Our final sample contains 909 firms (including Apple and Fossil). All variables are as defined in Tables 1.1, 1.3, and 1.4. Columns 1 to 4 are based on firms' compliance status with the board majority independence requirement in 2002, and columns 5 to 7 are based on compensation committee independence. We differentiate the effect of noncompliance (columns 1, 2, and 5) by the presence of substitute monitors in columns 3, 4, 6, and 7. The numbers in parentheses are heteroskedasticity-robust standard errors, clustered at the firm-period level. ***, **, and * indicate statistical significance at the 10%, 5%, and 1% levels.

	Board				Compensation Committee		
	(1) Baseline All firms	(2) Baseline Excl. A&F	(3) Block Dir Excl. A&F	(4) Inst Conc Excl. A&F	(5) Baseline Excl. A&F	(6) Block Dir Excl. A&F	(7) Inst Conc Excl. A&F
Noncompliance	-0.098	0.037			0.060*		
× after	(0.091)	(0.043)			(0.034)		
× blockholder			0.160***			0.137**	
			(0.062)			(0.057)	
× no blockholder			-0.005			0.037	
			(0.051)			(0.038)	
× high inst conc				0.004			0.167***
				(0.060)			(0.056)
× low inst conc				0.047			0.026
				(0.051)			(0.038)
Sales	0.320***	0.354***	0.357***	0.353***	0.354***	0.355***	0.356***
× before	(0.059)	(0.051)	(0.051)	(0.051)	(0.051)	(0.051)	(0.051)
Sales	0.293***	0.342***	0.346***	0.341***	0.342***	0.344***	0.346***
× after	(0.063)	(0.051)	(0.051)	(0.051)	(0.050)	(0.051)	(0.051)
ROA	0.464	0.374	0.383	0.375	0.378	0.379	0.367
× before	(0.334)	(0.336)	(0.335)	(0.336)	(0.334)	(0.335)	(0.333)
ROA	0.202	0.124	0.122	0.124	0.120	0.119	0.121
× after	(0.135)	(0.106)	(0.105)	(0.106)	(0.105)	(0.105)	(0.105)
RET	0.084***	0.087***	0.086***	0.087***	0.087***	0.087***	0.087***
× before	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)
RET	0.285***	0.318***	0.320***	0.319***	0.319***	0.319***	0.319***
× after	(0.049)	(0.043)	(0.043)	(0.043)	(0.043)	(0.043)	(0.043)
Tenure	-0.016	-0.007	-0.008	-0.007	-0.007	-0.007	-0.007
	(0.018)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
# firm-years	5,318	5,306	5,306	5,306	5,306	5,306	5,306
# firms	909	907	907	907	907	907	907
Adj. R^2	0.096	0.121	0.121	0.121	0.121	0.121	0.121

Table 1.A.2: Data on CEOs' First Year in Office

Company Name	GVKEY	CEO Name	Year
AMERICAN FINANCIAL GROUP INC	8431	Carl Henry Lindner	1959
AMERICREDIT CORP	17197	Clifton H. Morris, Jr.	1988
ANALOGIC CORP	1633	Bernard M. Gordon	1995
ANALOGIC CORP	1633	John W. Wood, Jr.	2003
BEST BUY CO INC	2184	Richard M. Schulze	1983
BIG LOTS INC	12123	Steven S. Fishman	2005
BJ SERVICES CO	22794	J. W. Stewart	1990
BJ'S WHOLESALE CLUB INC	65105	John J. Nugent	1997
CAMBREX CORP	13839	James A. Mack	1995
CASCADE NATURAL GAS CORP	2803	W. Brian Matsuyama	1995
CDW CORP	28320	Michael P. Krasny	1984
CENTRAL PARKING CORP	61404	Monroe J. Carell, Jr.	1980
COCA-COLA ENTERPRISES INC	12756	Lowry F. Kline	2001
COMMERCE BANCORP INC/NJ	16784	Vernon W. Hill, II	1982
COMMERCIAL METALS	3246	Stanley A. Rabin	1979
COMPASS BANCSHARES INC	2849	D. Paul Jones Jr.	1991
CTS CORP	2577	Joseph P. Walker	1988
DATASCOPE CORP	3786	Lawrence Saper	1964
DELL INC	14489	Michael S. Dell	1984
DILLARDS INC	3964	William Dillard II	1998
DOW CHEMICAL	4060	William S. Stavropoulos	1995
DOWNEY FINANCIAL CORP	4065	Daniel D. Rosenthal	1998
DRIL-QUIP INC	65671	Gary D. Smith	1981
DYNEGY INC	25495	Charles L. Watson	1985
EDWARDS (A G) INC	4230	Benjamin F. Edwards III	1983
FIRST AMERICAN CORP/CA	12796	Parker S. Kennedy	1993
FIRST DATA CORP	25157	Henry C. Duques	1989
GTECH HOLDINGS CORP	25807	W. Bruce Turner	2000
HARMAN INTERNATIONAL INDS	12788	Bernard A. Girod	1998
INVESTMENT TECHNOLOGY GP INC	30146	Raymond L. Killian Jr.	1994
JLG INDUSTRIES INC	6207	L. David Black	1991
KELLY SERVICES INC	6379	Terence E. Adderley	1987
LINDSAY CORP	14954	Gary D. Parker	1984
MAF BANCORP INC	20075	Allen H. Koranda	1989
MBIA INC	13561	Joseph W. Brown, Jr.	1999
MDC HOLDINGS INC	6865	Larry A. Mizel	1988
MOLEX INC	7506	Frederick A. Krehbiel	1988
NATIONAL INSTRUMENTS CORP	31607	James J. Truchard	1976
NBTY INC	7798	Scott Rudolph	1994
NORTH FORK BANCORPORATION	15202	John Adam Kanas	1976
OFFICEMAX INC	2290	George J. Harad	1994
O'REILLY AUTOMOTIVE INC	28180	Greg Henslee	2005
PAXAR CORP	8293	Arthur Hershaft	1980
PAYCHEX INC	8402	B. Thomas Golisano	1971
PEDIATRIX MEDICAL GROUP INC	61325	Roger J. Medel	1979
PHOTRONICS INC	13200	Constantine S. Macricostas	1974
PLEXUS CORP	12945	Peter Strandwitz	1979
RAYMOND JAMES FINANCIAL CORP	8898	Thomas A. James	1983
SLM CORP	10121	Albert L. Lord	1997
SPX CORP	5087	John B. Blystone	1995
STARBUCKS CORP	25434	Howard D. Schultz	1985
STURM RUGER & CO INC	10124	William B. Ruger	1949
SUSQUEHANNA BANCSHARES INC	17233	Robert S. Bolinger	1982
SYNOVUS FINANCIAL CORP	13041	James H. Blanchard	1971
TECHNITROL INC	10374	James M. Papada III	1999
TELLABS INC	10420	Michael J. Birck	1975
TEXAS INDUSTRIES INC	10498	Robert D. Rogers	1970
TOLL BROTHERS INC	12395	Robert I. Toll	1967
UNITED BANKSHARES INC/WV	17248	Richard M. Adams	1984
UNITED NATURAL FOODS INC	63927	Michael S. Funk	1999
VITAL SIGNS INC	23088	Terry D. Wall	1972
WATSCO INC	11313	Albert H. Nahmad	1973
WESTAMERICA BANCORPORATION	14253	David L. Payne	1989
ZENITH NATIONAL INSURANCE CP	13597	Stanley R. Zax	1978
ZIONS BANCORPORATION	11687	Harris H. Simmons	1990

CHAPTER 2

Earnings Overstatements: An Intended or Unintended Consequence of Pay-for-Performance?

2.1 Introduction

CEO incentives have been linked to income-increasing accrual choices, earnings reports that systematically exceed analysts' forecasts, earnings restatements, consecutive strings of earnings increases, and securities class action law suits for financial misrepresentation.[1] The purpose of our paper is to shed light on whether such earnings overstatements are an intended or unintended consequence of pay-for-performance.

The theoretical literature on incentive design has long recognized the potential tradeoff between inducing long-term value creation and short-term overstatements. While incentive pay is used to align the interests of managers with those of owners, managers also inflate the stock price to improve their performance evaluation and increase compensation.[2] This tradeoff is based on the premise that managers benefit from increasing firm value in the short term, whereas shareholders care about firm value in the long run.

In reality, however, current shareholders could benefit from short-run overstatements for several reasons. For example, Bushee (2001) emphasizes the short-term value preferences of transient institutional investors, and Shleifer (2004) argues that

[1]For example, see Bergstresser and Philippon (2006), Burns and Kedia (2006), Cheng and Warfield (2005), Denis, Hanouna and Sarin (2006), Efendi, Srivastava and Swanson (2007), Kadan and Yang (2004), Ke (2004), and Peng and Röell (2008). Two notable exceptions are Armstrong, Jagolinzer and Larcker (2010) and Erickson, Hanlon and Maydew (2006). However, the sample period of Armstrong et al. spans pre- and post-SOX years and their results are not robust when restricted to the pre-SOX period; and Erickson et al. base their study on a small number of accounting frauds that likely reflect idiosyncratic managerial expropriation.

[2]The tradeoff between incentives for productive effort and overstatement has been formalized by Crocker and Slemrod (2005), Dye (1988), Goldman and Slezak (2006), Guttman, Kadan and Kandel (2006), and Kwon and Yeo (2009).

shareholders benefit from attracting external finance at lower cost. Some of the theoretical studies on incentive design explicitly recognize the potential for shareholders to use pay-for-performance to induce overstatement by managers. Dye (1988), for example, calls the possibility of shareholders using pay-for-performance to reward managers for overstatements the external demand for earnings management, and Bolton, Scheinkman and Xiong (2006) refer to it as the strong form of their theory. To the best of our knowledge, however, there exists no empirical evidence to distinguish between the views that overstatements are an intended or unintended consequence of incentive pay. To this end, we investigate empirically whether CEO incentives reflect shareholder costs and benefits of overstatements.

To differentiate between these two opposing views on shareholders' underlying preference for earnings overstatements, we develop a novel test based on predictions derived from a principal-agent model linking pay to the costs and benefits of overstatements. We show that when the cost of overstatement increases, the change in the optimal pay-for-performance sensitivity (PPS) for a CEO depends on shareholders' preference for overstatements. Specifically, if shareholders do not value overstatements, optimal incentives strike a balance between inducing productive effort and avoiding overstatements. Since managers overstate less following an increase in the cost of overstatement, shareholders can raise incentives to induce more productive effort. On the other hand, if shareholders value overstatements, optimal incentives fall in response to an increase in the cost of overstatement (like other ordinary goods, the quantity of overstatements demanded is inversely related to its price). Throughout the paper, we refer to such shareholders as myopic to describe their preference for maximizing a firm's market value in the short run as opposed to its fundamental value in the long run.

We provide three pieces of empirical evidence that are consistent with the view that earnings overstatements were an intended consequence of pay-for-performance, at least prior to 2002. Our first approach exploits the increase in CEOs' expected cost of overstatement with the passage of the Sarbanes-Oxley Act of 2002 (SOX) to infer shareholder objectives from observed changes in pay-for-performance sensitivities.[3] Using SOX as an exogenous shock to firms' optimal incentive contracts offers a quasi-experimental setting which allows us to circumvent the typical endogeneity issues plaguing much of empirical research on corporate governance (e.g., see Hermalin and Weisbach (2003) for a literature survey on corporate boards as endogenously determined institutions). In addition, Karpoff, Lee and Martin (2008a) show that managers suffer severe consequences for financial misrepresentation. Consequently, it is reasonable to expect that changes in the cost of overstatements impact managerial incentives. We find that pay-for-performance sensitivity decreases significantly in the fiscal year of and after SOX, but not in other years. In particular, we estimate that incentives fall by about 8% (or about $23,000 per 1% change in firm value at the median and $85,000 at the mean) from before to after SOX. The empirical evidence is consistent with the view that SOX decreased the shareholder demand for overstatements.[4]

Our second approach relies on empirical proxies for shareholder benefits from overstatements (SBO) to substantiate our finding that shareholder objectives are reflected in CEO incentives. Our model makes two predictions in this regard. First, greater benefits from overstatements should lead to higher CEO incentives. Second, CEO incentives should fall by more around SOX in firms whose shareholders benefit more from overstatements. To test these predictions, we use two proxies for shareholder benefits from overstatements: (i) the Kaplan-Zingales (1997) measure of capital constraint (overstatements temporarily reduce the cost of capital)[5]; and (ii) the portfolio

[3]SOX increased the cost to CEOs for overstating earnings by (i) increasing the limits on financial penalties and prison terms for financial misrepresentation; (ii) requiring CEOs to reimburse any incentive based compensation or profit from the sale of stock received within 12 months after the misreporting if there is an accounting restatement as a result of misconduct; (iii) providing an additional $776 million in funding to the Securities and Exchange Commission (SEC) to step up its monitoring and enforcement efforts; and numerous other provisions.

[4]We acknowledge that this first piece of evidence — the decrease in incentive pay after SOX — has been documented previously (e.g., Cohen et al. (2007) and Indjejikian and Matějka (2009)). However, several authors present evidence to the contrary (e.g., Carter, Lynch and Zechman (2009), Jayaraman and Milbourn (2010), and Paligorova (2007)). As such, we contribute an independent assessment of these claims, and more importantly, we offer a novel interpretation of the empirical evidence. See Section 3.2 for further discussion.

[5]Our results are qualitatively robust to the alternative measures of financial constraints proposed by Hadlock and Pierce (2010) and Whited and Wu (2005). See Section 2.4.2.2 for further details.

turnover rate of firms' institutional owners (overstatements increase the return to influential short-term investors). Our choice of proxies for shareholder benefits from overstatements reflects the motivations used in the theoretical literature cited above (i.e., Bolton et al. (2006) and Shleifer (2004)), as well as empirical evidence linking these firm attributes to earnings management (e.g., Bushee (1998) and Linck, Netter and Shu (2010)).

Higher capital constraints and higher portfolio turnover rates are indicative of higher CEO incentives cross-sectionally. Moving from the 25th to the 75th percentile of each measure corresponds to differences in CEO incentives of 36% and 21%. We also find that the decrease in CEO incentives is concentrated in firms whose shareholders are most likely to benefit from overstatements. This difference-in-difference approach implicitly controls for confounding events or changes in market conditions that affect firms with high and low shareholder benefits from overstatements equally. For example, one alternative explanation for the observed decrease in incentives is that shareholders learned from the numerous scandals about the extent of overstatements, which in turn could have lead to the decrease in incentives. However, this alternative story fails to explain why the decrease in incentives would be concentrated in firms whose shareholders stood to gain from overstatements.

Our research complements prior work linking earnings management to firm objectives. Our revealed-preference-approach to uncovering shareholder objectives from changes in optimal incentive pay circumvents the problem of how to identify earnings overstatements. Researchers disagree whether accruals (or which accruals) are good proxies for earnings management and whether discontinuity in the distribution of forecast errors around various earnings benchmarks constitutes evidence of earnings management. Other measures of overstatements, such as shareholder litigation, earnings restatements, and enforcement actions by the SEC suffer from the drawback that only a fraction of overstatements is detected.[6] It is also unclear where to draw

[6] For example, see Ball and Shivakumar (2006), Beneish (2001), Dechow and Dichev (2002), Kothari, Leone and Wasley (2005), and Schipper (1989) on the accruals debate, Durtschi and Easton (2005) on forecast errors, and Burns and Kedia (2006), Dechow, Sloan and Sweeney (1996), Hennes, Leone and Miller (2008), Peng and Röell (2008), and Wang (2006) on restatements, enforcement actions, and litigation.

the line between desired and undesired earnings management, because the cost of overstatement increases with its magnitude. Our research design has the advantage that, unlike the aforementioned studies, we infer shareholders' underlying objectives from observed CEO contracts without relying on a proxy for earnings management.

Despite the different approaches, our findings are consistent with recent accounting studies documenting a decrease in accruals-based earnings management and the frequency of meeting or beating analysts' consensus forecasts, as well as an increase in accounting conservatism around SOX (e.g., Bartov and Cohen (2009), Cohen, Dey and Lys (2008), and Lobo and Zhou (2009)). The extant literature strongly supports our interpretation that the reduction in CEOs' pay-for-performance around SOX reduces their incentives to overstate earnings.[7]

To summarize, our findings and interpretation primarily contribute to our understanding of the tradeoffs inherent in the design of managerial incentives. Given the importance of this topic to managers, investors, and regulators, our goal is to engage these parties in continuing the discussion on incentive pay and the integrity of capital markets.[8] To this end, we provide a novel test of shareholder objectives with implications for corporate governance and public policy. We find that both costs and benefits of overstatements are reflected in CEO contracts through pay for performance. Our results challenge the majority view that overstatements are an undesired side-effect of inducing productive effort. The important implication for corporate governance is that overstatements are not necessarily a symptom of poor oversight. In an effort to improve the quality of financial reporting, recent corporate governance reforms have put great emphasis on board and committee independence to improve directors' ability to act independently from management, and possibly to the detriment of directors' access to information. We emphasize that directors must also want to act as monitors. If the shareholders they represent benefit from

[7]In addition, Bargeron, Lehn and Zutter (2010) and Cohen et al. (2007) show that firms reduce their capital and research and development expenditures. These findings are consistent with our model's prediction that the decrease in CEO incentives also curtails productive effort and/or risk-taking. The extent to which the documented decrease in firm values around SOX is attributable to a change in CEO incentives is beyond the scope of the present paper.

[8]Similar concerns are present in many other fields, such as education and health care.

overstatements, then one cannot expect the directors to be effective at preventing overstatements. We conjecture that the provisions of SOX that increase the expected cost of overstatements have done more to improve the quality of financial reporting than the board composition mandates.

2.2 Other Related Literature

Although our paper primarily focuses on the shareholder costs and benefits of overstatements, using SOX as a quasi-experimental shock to optimal incentives also places it in the ongoing debate on the effects of SOX on the performance sensitivity of managerial compensation. Here we provide a brief sketch of the conflicting empirical evidence on incentive changes around SOX and competing interpretations. Carter et al. (2009), on the one hand, find an increase in the earnings-sensitivity of bonuses following SOX. They argue that the increase reflects firms' willingness to offer greater incentives for productive effort, because SOX constrains managers' flexibility in managing earnings.

In contrast, Indjejikian and Matějka (2009) find that bonuses become less sensitive to financial performance measures in the post-SOX period. The strength of their empirical analysis derives from their survey data: the availability of bonus data for both public and private firms allows them to differentiate between the effects of SOX and a general time trend or contemporaneous events. As such, theirs is one of the most convincing pieces of evidence on the change in incentives around SOX. To guide the interpretation, Indjejikian and Matějka model the optimal contract as balancing benefits from productive effort and costs from misreporting. Based on this tradeoff, they interpret the decrease in incentives to reveal that firms must have experienced an increase in the cost of misreporting that warrants a cutback in misreporting above and beyond the response of CFOs to SOX.

However, it is difficult to reconcile the empirical evidence with some of the explanations for why firms would cut back on incentives when managers already cut back on misreporting themselves. If firms revised their assessment of the power of incentives or investors became more sensitive to misreporting, then one would expect similar responses in both public and private firms.[9] In addition, SOX imposes not only greater risk of detection, but also greater penalties and personal liability on CEOs and CFOs. In the absence of additional evidence for these types of explanations for a decrease in incentive pay, the evidence contradicts the predictions from standard theories on the tradeoff between productive effort and misreporting.

For this reason, we consider the possibility that shareholders benefit from overstatements and use incentive pay to reward managers for overstatements. Implicitly, we also assume that firms and managers experience a proportional increase in their costs of misreporting. In light of the emphasis of SOX on personal responsibility and liability for managers, we believe this to be a reasonable, even conservative assumption. Taken together, these two differences lead us to infer that shareholder used pay-for-performance to reward managers for overstating earnings prior to SOX. Our cross-sectional results on shareholder benefits from overstatements corroborate our view. In addition, our study differs from Carter et al. and Indjejikian and Matějka in a number of other dimensions, e.g., sample period, coverage of executives, and most importantly the measure of incentives. We do not only study the performance sensitivity of bonus pay, but also include the incentive effects from stocks and options. Our more comprehensive incentive measure is better suited for drawing inferences about the nature of incentive pay.

Several other contemporaneous working papers also address this topic using broader measures of incentive pay. For example, Cohen et al. (2007) find that incentives drop around SOX.[10] In contrast to us, they attribute the decrease in incentives to public pressure to rein in executive compensation. While we cannot completely refute this argument, we are skeptical that public pressure or the realization that perhaps incentives had grown out of control adequately explain the data. For one, Core, Guay

[9]On the other hand, private firms might have different governance structures that allow them respond differently. In this case, however, private firms would not make a good control group for studying the effect of SOX on incentive pay at public firms.

[10]Mirroring the disagreement about the direction of the change in the performance sensitivity of bonus pay, there exists contradictory evidence on the effect of SOX on incentive pay. Jayaraman and Milbourn (2010), for example, argue that SOX led to increase in incentives, which they interpret analogously to Carter et al.. However, the result appears to be driven by their research design (they estimate the SOX effect after controlling for year effects, but not for firm-fixed effects).

and Larcker (2008) find no consistent evidence that CEO compensation responds to negative media attention. Second, we find that the drop in incentives occurs in fiscal years 2002 and 2003, and does not correspond to the growing number of accounting restatements that ultimately led to the passage of SOX (see Section 2.4.2.1 for further details). Finally, and perhaps most importantly, the alternative explanations cannot account for the fact that the decrease in incentives is concentrated among firms whose shareholders stood to gain from overstatements prior to SOX.

2.3 Theory and Hypothesis Development

In this section, we develop a principal-agent model of optimal contracts. It formalizes how optimal incentive contracts respond to an exogenous increase in the cost of overstatements.[11] The theory provides a framework that allows us to infer shareholder objectives from the observed changes in CEO pay around SOX, which we investigate empirically in Section 2.4.2.1. The model's additional testable predictions provide the basis for our empirical work in Sections 2.4.2.2–2.4.2.4.

The key point in the theoretical model is the potential tradeoff — depending on shareholders' preference for overstatements — between inducing overstatement and inducing productive effort through pay for performance. Few models have captured this trade-off because most of them look at either overstatements or productive effort, but not both. For example, Fischer and Verrecchia (2004) and Stein (1989) do not consider the agent's productive effort or the optimal contract. Gibbons and Murphy (1992) and Holmström (1999) do not consider the agent's overstatement. And several models that capture this tradeoff do not consider different objectives of the principal. While Crocker and Slemrod (2005) and Kwon and Yeo (2009) do not model shareholder benefits from overstatements, Bolton et al. (2006), Dye (1988), and Goldman and Slezak (2006) do not offer empirically testable implications to distinguish between principals that discourage and principals that encourage overstatements.

[11] Our model is one formalization of the hypotheses and we acknowledge that there could be alternative, more complex ways of modeling these effects. An informal summary of the empirical hypotheses with a brief explanation of the intuition behind them is provided in Section 2.3.4.

2.3.1 Set-up

We consider a firm with one principal (e.g., shareholders represented by a board) and one agent (e.g., a CEO).[12] The agent exerts productive effort (a) to increase a firm's underlying fundamental value, $y = a + \epsilon_a$, where ϵ_a follows a normal distribution $N(0, \sigma_a^2)$. As in Kwon and Yeo (2009), we allow the agent to overstate the fundamental value by m. Neither the principal nor the market observes the fundamental value (y) or overstatement (m). However, the market can discount the reported value by its expectation on overstatement (m^e). Then, a firm's *market* performance (e.g., stock price), denoted by $\tilde{y}$, is determined by $\tilde{y} = y + m + \epsilon_m - m^e$, where ϵ_m is random noise following a normal distribution $N(0, \epsilon_m^2)$.[13]

As in Bolton et al. (2006), we assume that investors have heterogenous beliefs on the agent's overstatement, and that the firm's market value is determined by the most optimistic investor (or the smallest expected overstatement). In other words, investors who value the firm's shares most highly hold the long positions. More specifically, let us denote an investor i's expectation on the agent's overstatement by m_i^e, where m_i is distributed over $[\underline{m}, \overline{m}]$, and $\overline{m} > \underline{m} > 0$. We assume that $E[m_i^e] = m^*$, where m^* is the agent's equilibrium overstatement level. Therefore, investors' expectations are rational on average. However, Bolton et al. (2006) show that if short selling is costly, the market price is determined by the most optimistic belief, $m_i^e = \underline{m}$. In this case, the market's expectation is $m^e = \underline{m}$.[14]

Let us define θ such that $m^e = \underline{m} = \theta m^*$. Note that $0 < \theta < 1$, since $E[m_i^e] = m^* > \underline{m} > 0$, and the market underestimates the extent of overstatement. If market uncertainty increases and investors' beliefs are more dispersed (holding the mean constant), then $\underline{m}$ (θ) becomes smaller and the market will underestimate the extent

[12] Throughout the paper, we ignore the possible agency problem between the shareholders and the board. Allowing such agency problem in this model would be an interesting topic for future research.

[13] In this paper, we do not consider the agent's incentive to understate performance to smooth income, for example. If there is such an incentive, we can regard m as the overstatement above and beyond the understated performance.

[14] For example, D'Avolio (2002), Geczy, Musto and Reed (2002), and Jones and Lamont (2002) provide empirical evidence that it is costly to short sell stocks.

of overstatement by more. Thus, we can interpret θ as a measure of mispricing in the market.

The agent's wage (w) is contingent on the firm's market value.[15] In the spirit of Holmström and Milgrom (1992), we assume a linear contract, where $w = s + \beta\tilde{y}$.[16]

The principal is risk-neutral, and the agent is risk-averse. The agent's utility function is given by $U(w, a, m) = -\exp^{-r(w-\frac{1}{2}a^2-\frac{k}{2}m^2)}$, where $\frac{1}{2}a^2$ is the cost of productive effort and $\frac{k}{2}m^2$ captures the cost of overstatement. More specifically, we assume that the probability of getting caught overstating (q) is proportional to the size of overstatement, i.e., $q = \rho_1 m$. Once caught, the punishment for overstatement (P) is also proportional to the size of overstatement, i.e., $P = \rho_2 m$. Let $\rho = \rho_1\rho_2$, and the expected punishment amounts to $qP = \rho m^2$. The punishment is shared between the agent (η) and the principal ($1-\eta$). Then, the expected punishment of overstatement to the agent is $\eta qP = \eta\rho m^2 = \frac{k}{2}m^2$, where $k = 2\eta\rho$. Note that the marginal cost of overstatement to the CEO ($= km$) is increasing in m. Similarly, the expected cost of overstatement to the principal is $(1-\eta)qP = (1-\eta)\rho m^2 = \frac{c}{2}m^2$, where $c = 2(1-\eta)\rho$. Recall that SOX has increased both funding to the SEC to increase enforcement, as well as penalties for CEOs (e.g., through required reimbursement of incentive payments from overstatement). Thus, SOX has increased both ρ_1 and ρ_2, or k and c. We normalize the agent's reservation utility to -1, and assume that the principal has all the bargaining power.

The timing of the game is as follows. First, the principal and the agent sign a binding wage contract. Then, the agent chooses productive effort (a). After the fundamental value (y) is realized, the agent chooses his overstatement level (m). The market discounts the reported value and determines the market value of the firm ($\tilde{y}$).

[15]This assumption reflects the usual stock- and option-based compensation packages for CEOs. Technically, we assume that the agent's reported performance is not verifiable. For example, the agent may only know the probability distribution of the true performance, and can only report the mean of the distribution. Then, the agent is unlikely to become liable for the report. Technically, this assumption allows us to avoid the revelation mechanism, as discussed in Crocker and Slemrod (2005) and Dye (1988). Under a revelation mechanism, there is no tradeoff between inducing overstatements and productive effort.

[16]For recent attempts to characterize general non-linear contracts, see Crocker and Slemrod (2005) and Hemmer, Kim and Verrecchia (2000).

The agent then gets paid based on the initial contract.

2.3.2 Overstatement and Effort

We solve the model by backward induction. Given the contract and the market's expectation, we first characterize the agent's incentive constraints for overstatement (m) and productive effort (a).

Overstatement Given fundamental value ($y = a + \epsilon_a$), the agent solves the following maximization problem to determine the optimal level of overstatement:

$$\begin{aligned} & \max_m \; E\left[-\exp\left(-r(s+\beta\tilde{y}-\frac{1}{2}a^2-\frac{k}{2}m^2)\right)\right] \\ \iff & \max_m s+\beta(y+m-m^e)-\frac{1}{2}a^2-\frac{k}{2}m^2-\frac{r}{2}\beta^2\sigma_m^2. \end{aligned}$$

From the first order condition, we obtain the optimal level of overstatement

$$m^*(y) = \frac{\beta}{k}. \tag{2.1}$$

Since the agent's overstatement level does not depend on the reported value, it is rational for the market to discount the reported value by a constant. Therefore, in this simple equilibrium, the agent can take the market expectation (m^e) as given.[17]

Effort Given the agent's optimal overstatement rule in (1), the agent's optimal choice of effort solves the following optimization problem:

$$\begin{aligned} & \max_a \; s+\beta E\Big[a+\epsilon_a+m^*(y)+\epsilon_m-m^e\Big]-\frac{1}{2}a^2-\frac{k}{2}m^*(y)^2-\frac{r}{2}\beta^2(\sigma_a^2+\sigma_m^2) \\ = & \max_a \; s+\beta\left(a+(1-\theta)\frac{\beta}{k}\right)-\frac{1}{2}a^2-\frac{\beta^2}{2k}-\frac{r}{2}\beta^2(\sigma_a^2+\sigma_m^2). \end{aligned}$$

[17]Kwon and Yeo (2009) show that there is another, more complex equilibrium where market expectation is a strictly increasing function of reported performance. Such an equilibrium becomes quickly untractable in this paper, but the qualitative results of this paper should hold in that equilibrium too.

When the agent decides on his effort level, both ϵ and ϵ_m are still random variables. The first order condition yields

$$a^* = \beta. \tag{2.2}$$

Not surprisingly, if β increases, the agent exerts more productive effort. But from Eq. (1), the agent will also overstate the fundamental value by more, which presents a potential trade-off to the principal.

The agent's participation constraint must also be binding. That is,

$$\begin{aligned} E\left[-\exp^{-r(w-\frac{1}{2}a^2-\frac{k}{2}m^2)}\right] &= -1 \\ &\Updownarrow \\ s+\beta\left(a+(1-\theta)\frac{\beta}{k}\right)-\frac{1}{2}a^2-\frac{\beta^2}{2k}-\frac{r}{2}\beta^2(\sigma_a^2+\sigma_m^2) &= 0. \end{aligned} \tag{2.3}$$

2.3.3 The Optimal Contract

We model two opposing views on shareholder objectives: maximization of either the market value or fundamental value of the firm. To encompass both views, we assume that the principal maximizes the weighted average of market performance and fundamental performance of the firm. We introduce λ to capture the weight the principal places on her firm's market value instead of its fundamental value. The principal's optimization problem is thus given by

$$\max_{s,\beta} \; E\left[\lambda\tilde{y}+(1-\lambda)y-w-\frac{c}{2}m^2\right] = a+\lambda(m-m^e)-\Big(s+\beta(a+m-m^e)\Big)-\frac{c}{2}m^2,$$

subject to the incentive constraints (1) and (2), and the participation constraint (3).

Substituting (1), (2), and (3) into the principal's objective function yields

$$\begin{aligned} \max_{\beta} \; \beta+\lambda(1-\theta)\frac{\beta}{k}-\Bigg[\; - \; & \left(\beta\Big(\beta+(1-\theta)\frac{\beta}{k}\Big)-\frac{\beta^2}{2}-\frac{\beta^2}{2k}-\frac{r}{2}\beta^2(\sigma_a^2+\sigma_m^2)\right) \\ + \; & \beta\Big(\beta+(1-\theta)\frac{\beta}{k}\Big)\Bigg]-\frac{c}{2}\Big(\frac{\beta}{k}\Big)^2. \end{aligned}$$

The first order condition is

$$1 + \lambda\left(\frac{1-\theta}{k}\right) - \left(1 + \frac{1}{k} + r(\sigma_a^2 + \sigma_m^2)\right)\beta - \frac{c}{k^2}\beta = 0. \tag{2.4}$$

This first order condition reveals the trade-off in choosing the optimal pay-for-performance sensitivity (PPS), β. The marginal benefits of raising β include the increased productive effort and the returns from the agent's overstatement, $\lambda\left(\frac{1-\theta}{k}\right)$. The marginal costs of raising β include the increased cost of productive effort, overstatement, and risk-premium, as well as the increased cost from overstatement to the principal.

The optimal PPS, β^*, is given by

$$\begin{aligned} \beta^* &= \frac{1 + \lambda\left(\frac{1-\theta}{k}\right)}{1 + \frac{c+k}{k^2} + r(\sigma_a^2 + \sigma_m^2)} \\ &= \frac{1 + \lambda\left(\frac{1-\theta}{2\eta\rho}\right)}{1 + \frac{1}{2\eta^2\rho} + r(\sigma_a^2 + \sigma_m^2)}. \end{aligned} \tag{2.5}$$

We are interested in how optimal PPS changes in response to an exogenous increase in the cost of overstatement, ρ. The following proposition states that optimal PPS can either increase or decrease depending on the principal's degree of myopia (λ) and market uncertainty (θ).

Proposition 2.1.

$\frac{\partial \beta^*}{\partial \rho} < 0$ *if and only if* $\lambda > \frac{1}{\eta(1-\theta)(1+r(\sigma_a^2+\sigma_m^2))}$.

Proof. See Appendix 2.A.1. □

Proposition (2.1) states that an increase in the cost of overstatement (ρ) will *decrease* optimal PPS only if the principal is sufficiently myopic (i.e., λ is sufficiently large). However, if the principal cares about the fundamental value of the firm, optimal PPS will increase. Note that, because $\lambda < 1$, if market uncertainty is sufficiently small (i.e., θ is sufficiently large and overstatements are ineffective) or if the agent bears insufficient costs of overstatement (i.e., η is sufficiently small), then optimal PPS will increase in response to a rising cost of overstatement, regardless of the degree of shareholder myopia.

Intuitively, suppose that the principal does not want the agent to overstate performance, because there is no payoff from overstating (high θ), the principal bears excessive costs (low η), or the principal does not care about market value (low λ). To induce the agent's productive effort, the principal still has to provide positive PPS and induce overstatements. In this case, if the agent's cost of overstatement increases, the agent will reduce overstatements voluntarily, and the principal can raise PPS to induce more productive effort with less overstatement.

However, if the principal wants the agent to overstate, because overstating yields high returns (low θ) and because the principal cares about the market value of the firm (high λ), the principal will provide high PPS to induce large overstatements. In this case, as the cost of overstatement increases, it becomes more costly for the principal to induce the agent to inflate the market value of the firm. Thus, the principal would have to reduce PPS.

These results are significant, as they show that we can potentially distinguish between shareholder objectives of maximizing firms' market values and fundamental values. More specifically, when there is an exogenous increase in the cost of overstatement, if the firm increases PPS, it implies either that the firm cares more about the fundamental value or that the returns from overstatement are negligible. However, if the firm decreases PPS, it would be an indication that the firm focuses relatively more on the market value, and not the fundamental value of the firm.

To the extent that we can find empirical measures of λ, we can test the model's predictions directly (i.e., without inferring shareholder objectives). In particular, the model predicts:

Proposition 2.2.

(i) $\frac{\partial \beta^*}{\partial \lambda} > 0$.

(ii) $\frac{\partial^2 \beta^*}{\partial \rho \partial \lambda} < 0$.

Proof. See Appendix 2.A.1. □

When the principal focuses more on the market value, instead of the fundamental value, the principal wishes to encourage more overstatement by providing larger incentives. Thus, as λ increases, optimal incentives increase too.

However, exactly when the principal cares more about the market value (i.e., λ is large), the effect of the increased cost of overstatement (ρ) becomes even bigger. In other words, when the cost of overstatement increases, optimal incentives in firms that focus relatively more on market value will decrease by more (or increase by less) compared to firms that focus relatively more on fundamental value.

2.3.4 Empirical Hypotheses and Strategy

Our empirical analysis consists of three parts. First, we utilize Proposition (2.1) to infer shareholder objectives from observed changes in CEO incentives around SOX:

Hypothesis 2.1. According to Proposition (2.1), an observed decrease in CEOs' pay-for-performance sensitivity in response to SOX is consistent with market value maximization, but inconsistent with maximization of fundamental value (i.e., shareholders must benefit from overstatements). On the other hand, if CEO incentives increase in response to SOX, overstatements are either ineffective or too costly, and/or shareholders do not value gains from overstatements.

Intuitively, shareholders who do not value overstatements are constrained in offering their CEO higher incentives, because higher incentives lead to costly overstatements. An increase in the cost of overstatement induces the CEO to reduce overstatements for any given level of incentives, and the shareholders' constraint loosens — they can now raise incentives to induce more productive effort. On the other hand, if shareholders value overstatements, an increase in the cost of overstatements leads to fewer/smaller overstatements desired by them (the quantity demanded decreases as the price rises, reflecting a downward sloping demand curve for overstatements), which in turn lowers CEO incentives to overstate (and, by extension, to exert productive effort).

The Sarbanes-Oxley Act of 2002 provides a quasi-experimental increase in the cost of overstatements that allows us to assess the model's predictions. We argue that SOX increased the cost to the agent for overstating earnings directly by increasing CEOs' personal exposure to liability (e.g., through higher expected penalties) and indirectly by making financial misrepresentation more difficult (e.g., through more auditor oversight and independence).

Specifically, SOX requires CEOs to reimburse any incentive based compensation or profit from the sale of stock received within 12 months after the misreporting if there is an accounting restatement as a result of misconduct (section *304*). SOX also grants the SEC power to permanently bar fraudulent executives from serving as officers or directors in the future (*1105*). Maximum criminal penalties for fraud under the Securities and Exchange Act of 1934 are increased to $5 million and 20 years of prison (*1106*), and maximum prison terms increase to 25 years for securities fraud and up to 20 years for mail and wire fraud (*807* and *903*). In addition, SOX requires CEOs to personally certify the correctness and completeness of the financial statement (*302*), as well as to disclose any significant deficiencies and changes in internal controls over financial misrepresentation (*404*). According to Bainbridge (2007), the purpose of these certifications is to prevent CEOs from hiding behind the veil of ignorance. SOX also institutes stiff penalties for noncompliance with the certification requirements; they are punishable with up to $5 million in fines and 20 years in prison (*906*).

Furthermore, the SEC is apportioned an additional $776 million of funding for fiscal year 2003, of which $201 million are intended for higher staff compensation and at least 200 new hires (*601*). To better protect investors, SOX mandates the SEC to review each firm's disclosures at least once every three years (*408*). SOX also makes it more difficult to misrepresent a firm's financial situation by creating the Public Company Accounting Oversight Board (title I); requiring auditor independence (title II); improving the quality of audit committees through independence (*301*) and financial expertise (*407*); and providing explicit protection of whistleblowers (*806* and *1107*).

The second and third parts of our empirical analysis are tests of Proposition (2.2). These tests are independent from inferred shareholder objectives based on Proposition (2.1). The model parameter λ captures the weight shareholders assign to the market value as opposed to the fundamental value of the firm. While λ is not directly observable, we can proxy for λ using measures of shareholder benefits from overstatements. Our two measures are (i) the Kaplan-Zingales measure of capital constraint (KZ-score) and (ii) the turnover rate in the portfolios of institutional shareholders (IT-score). Our choice of these measures is motivated by recent theoretical contributions on overstatements in contract design and empirical evidence on earnings overstatements. We discuss these measures in more detail in Section 2.4.2.2.

The model makes the following testable predictions about the relationship between CEO incentives and shareholder benefits:

Hypothesis 2.2. According to Proposition (2.2) (i), higher shareholder benefits from overstatements are reflected in higher CEO incentives.

Hypothesis 2.3. According to Proposition (2.2) (ii), higher shareholder benefits from overstatement are reflected in a larger decrease in CEO incentives around SOX.

The intuition for Hypothesis (2.2) is straightforward: The more shareholders benefit from overstatements, the more they are willing to pay their CEO to achieve them. Hypothesis (2.3) is a combination of Hypotheses (2.1) and (2.2). The effect of the increase in the cost of overstatements is more pronounced the more shareholders benefit from overstatements.

To test Hypothesis (2.2), we link cross-sectional variation in the proxies for shareholder benefits to CEO incentives. In addition, using a difference-in-difference approach around SOX, we test if incentives fall by more in firms exposed to high pre-SOX shareholder benefits. This approach allows us to rule out alternative explanations of the decrease in CEO incentives that affect shareholders with high and low benefits from overstatements equally.

2.4 Empirical Analysis

2.4.1 Sample Description

Our sample covers over 850 large publicly traded firms with fiscal years 1999–2005. We require annual data on CEO incentives (from Execucomp) and firm characteristics (from Compustat). To avoid entry and exit effects, we only keep firms with CEO incentive data for all seven years of the sample. However, our results are qualitatively unchanged if we relax this restriction. Our findings are also robust to excluding firms with missing control variables and to excluding financial firms and utilities. Definitions of all variables are provided in Table 2.1 (Appendix 2.A.2 describes the calculation of PPS in more detail). To mitigate the effect of outliers, we winsorize all variables at the top and bottom percentile (the results are qualitatively similar if we do not winsorize, but the mean estimates tend to increase). Table 2.2, Panel A, displays the means of all variables for each fiscal year. Panel B provides further summary statistics for the pooled cross-section. All nominal values are expressed in December 2006 dollars (using the BLS CPI for all urban consumers – current series).

2.4.2 Results

2.4.2.1 Timing of SOX and Changes in CEO Incentives

To infer shareholder objectives from Hypothesis (2.1), we need to determine how CEO incentives change with the passage of SOX in 2002. Whether firms had sufficient time to react to SOX in the fiscal year of its passage is a priori uncertain. Therefore, we treat fiscal year 2002 as the transition year (event year $t = 0$). Initially, we consider fiscal years 1999-2001 as the pre-SOX period ($-3 \leq t \leq -1$) and fiscal years 2003-2005 as the post-SOX period ($1 \leq t \leq 3$).[18] To study changes in CEO incentives

[18] SOX was passed in July 2002 in response to the large corporate scandals in the preceding year (e.g., Enron, Tyco, Worldcom). We assume that fiscal year 2003 falls into the post-SOX period, as its begin date falls between June 2002 and May 2003. To the extent that the expected cost of overstatements increased prior to the adoption of SOX (e.g., through anticipated regulatory changes or higher scrutiny by investors and enforcement agencies), incentive effects can already be visible in earlier years.

around SOX, we estimate the regression

$$incentives_{it} = \sum_{t=-2}^{+3} \delta_t D_t + \alpha_0 + \sum_{j=1}^{k} \alpha_j X_{jit} + \upsilon_i + \epsilon_{it} \ , \tag{2.4}$$

where t denotes the number of years before or after SOX, i denotes firms, and j denotes control variables. δ_{-2}–δ_{+3} are the coefficients of interest. D_t are year dummies, X_{jit} includes standard control variables used in the literature on executive compensation, namely market value of equity, stock price volatility, market-to-book ratio, and leverage as measures of firm characteristics; return on assets, firms' total shareholder returns, and market returns as performance controls; as well as CEO tenure, CEO turnover, and CEO option exercises.[19] υ_i are firm fixed effects. We estimate heteroskedasticity-robust standard errors, clustered at the firm level to address serial correlation concerns.

We set $D_t = 1$ for all fiscal years in or after event year t, and equal to zero otherwise. That is, D_t is not the usual year dummy which captures the *cumulative* change from the base year (in our case 1999). Instead, we define it to capture the *marginal* change from the prior year. This definition allows us to use the t-test for significant difference from zero to determine if incentives fall or rise from their level in the previous year. To the extent that incentives adjust slowly (i.e., over several years), one has to add the coefficients for $t \geq 0$ to obtain the full impact of SOX on the level of incentives.

Column 1 in Table 2.3 shows the results for CEOs' pay-for-performance sensitivity as the dependent variable. Following Core and Guay (2002), we define PPS as the dollar change in executives' stock- and option holdings for a hypothetical one percent change in firm value. In column 2, the dependent variable is the PPS-ratio, an alternative measure of incentives (as used in Bergstresser and Philippon (2006) and Cornett, Marcus and Tehranian (2008)). It scales PPS by the sum of PPS, salary, and bonus. The PPS-ratio measures the importance of incentive pay that is directly tied

[19] Controlling for R&D and cash constraints as predictors of option usage does not materially affect our estimates.

to the stock price relative to CEOs' total compensation. It also implicitly controls for changes in the level of CEO pay, because the denominator captures the bulk of annual CEO pay.

We make the following three observations. First, we observe that PPS and the PPS-ratio fall in fiscal years 2002 and 2003 by a statistically significant amount, but not in other years: the adjustment begins immediately in the transition year and is completed by the following year. The empirical evidence thus suggests that firms adjust CEO incentives in response to SOX. Second, the economic magnitude of the adjustment is significant. We find that log(PPS) falls by a combined 0.232 over 2002 and 2003, which translates into an average drop in PPS of about 20.7% (or about \$59,000 per 1% change in firm value at the median and \$222,000 at the mean).[20] Similarly, we estimate that the PPS-ratio falls by 5.0 percentage points around SOX, or by about 18.7% from its average pre-SOX level. Third, the adjustment seems permanent in the sense that it is not reversed in fiscal years 2004 and 2005. While we estimate that log(PPS) increases in 2004 by 0.042 from the previous year, the magnitude of the increase is not sufficient to offset the earlier decrease.

Note that our identification strategy assumes that incentives were optimal before SOX and adjust to new optimal levels after SOX. Alternatively, one could argue that pre-SOX incentives were excessive, and that the decrease in incentives merely reflects a market correction. While we cannot completely rule out the possibility that the decrease in incentives can be explained by this learning story, the timing of the incentive changes coincide strongly with SOX and not with the revelation of the accounting frauds that actually led to SOX. For example, according to a report by the General Accounting Office ((2002)), the fraction of listed companies issuing an accounting restatement steadily increased from 0.89% in 1998 to 2.47% in 2001, which represents an annualized growth rate of over 29%. Note that the largest increase occurred from 1998 to 1999, when the fraction of restating firms jumped from 1.02% to 1.73%.

[20]We calculate the percentage change as $exp(-0.232) - 1 = 20.7\%$. We calculate the dollar change by multiplying the percentage change with the mean and median values of PPS of the sample firms before SOX.

Incentive Levels vs. Flow

One potential drawback to our incentive measure PPS is that it may not only reflect optimal contracting considerations, but also CEOs' timing of option exercises and stock sales. For example, if CEOs choose to unwind their holdings of exercisable options following SOX, then we could mistakenly attribute the decrease in PPS to shareholders' preference for maximizing market values. We provide three arguments against this alternative explanation. First, as is evident from Table 2.2, Panel A, the option exercise ratio drops sharply in 2003. Fewer exercised options translate into higher PPS. Second, we include the option exercise ratio as a control variable in our regressions. As expected, its effect on PPS is negative. Third, we use the equity grant ratio as an alternative incentive measure that is arguably less affected by CEOs' choices and market conditions. The equity grant ratio captures the fraction of annual pay in the form of stock and option grants, which are more performance sensitive than salary, bonus, and other pay. Contrary to PPS which measures the stock of incentives, the equity ratio indicates the performance sensitivity of the flow of pay. The results are presented in column (iii) and are consistent with the results for PPS. The fall in the level of incentives is mirrored in the composition of the flow of incentive pay.

Pre-SOX vs. Post-SOX Period

While estimating year dummies sheds light on when the changes in CEO incentives take hold, the year dummies are not well-suited for interacting with proxies for shareholder benefits from overstatements, which we do in parts 2 and 3 of our empirical analysis. Thus, for ease of interpretation and comparison of incentive levels between the pre- and post-SOX periods, we re-estimate Eq. (2.4), but replace the year dummies with one post-SOX dummy. When using the post-SOX dummy, we cluster standard errors by firm-periods to address serial correlation concerns and to account for the fact that SOX affected the firms simultaneously.[21]

[21] While the computationally more flexible, but more time-intensive two-way clustering procedures recommended by Petersen (2009) (*cluster2*) and Cameron, Gelbach and Miller (2011) (*cgmreg*) in-

Fig. 2.1 offers a graphical representation of the change in CEO incentives around SOX. It plots the kernel density estimates of average residual CEO incentives for the pre- and post-SOX periods. We obtain residual CEO incentives from regressing CEO incentives on its known economic determinants used in estimating q. (2.4), but without time effects. For each firm, we then average the residuals over the pre- and post-SOX years.

Table 2.4 displays the estimation results. For the first two columns, we define fiscal years 2002 and later as the post-SOX period, because CEO incentives start falling in fiscal year 2002. As a robustness check, we define all fiscal years beginning on or after August 1, 2002 as post-SOX years, as SOX was signed into law on July 30, 2002. The change in the definition of post-SOX affects a large number of firm-years. 840 observations of fiscal year 2002 and 61 observations of fiscal year 2003 are considered post-SOX in columns 1 and 2, but pre-SOX in columns 3 and 4. The results, however, are very similar across the definitions of post-SOX. We estimate that, on average, log(PPS) falls by 0.082–0.083 and the log(PPS-ratio) by 0.117–0.128 from before to after SOX. The reason that the definition of post-SOX does not significantly affect the results is that CEO incentive levels in 2002 lie in between those of earlier and later years. Shifting fiscal year 2002 observations from the post- to the pre-SOX period raises the averages in both periods, but leaves the difference largely unaffected.

In untabulated robustness checks, we estimate variations of Eq. (2.4) for different event windows ($\pm$ 1, 2, or 3 years around SOX, including and excluding 2002). While our estimates of the magnitude of the decrease in CEO incentives vary depending on the size of the event window, the results are qualitatively unchanged. Since our theory only makes directional predictions about CEO incentives, and not their magnitude, the choice of the event window is largely inconsequential. Appendix 2.A.3 contains further robustness tests dealing with influential observations, methodology, and measurement of CEO incentives, including a detailed discussion of bonus pay.

crease the standard errors somewhat, the coefficient of interest remains statistically highly significant with a p-value of 0.1% or less.

To summarize, we find CEO incentive levels decrease in response to SOX by an economically large and statistically highly significant amount, which is inconsistent with the hypothesis that shareholders discourage overstatements, but consistent with the alternative view that shareholders benefit from overstatements.

2.4.2.2 CEO Incentives and Shareholder Benefits from Overstatements

We now take a completely independent approach to identify shareholder objectives. In the previous section, we inferred shareholder objectives from the change in CEO incentives in response to an increase in the cost of overstatements. Here we proxy for unobservable shareholder objectives and test if they are reflected in CEO incentives as predicted by the model. As stated in Hypothesis (2.2), we expect firms whose shareholders benefit from overstatements to provide more incentives. To test this prediction, we build on the following regression equation linking CEO incentives and shareholder benefits:

$$\mathit{incentives}_{it} = \psi_1 \; SBO_{it} + \psi_2 \; D(t \geq 0)_t + \alpha_0 + \sum_{j=1}^{k} \alpha_j X_{jit} + \upsilon_i + \epsilon_{it} \;, \tag{2.5}$$

where ψ_1 is the coefficient of interest and SBO_{it} is the generic label for our proxies for shareholder benefits from overstatements. As before, $D(t \geq 0)_t$ is the post-SOX dummy and X_{jit} includes control variables: market value of equity, stock price volatility, market-to-book ratio, leverage, firms' total shareholder returns, market returns, CEO tenure, CEO turnover, and CEO option exercises.

Our two measures of shareholder benefits to proxy for shareholder objectives are based on the motivations behind the recent contributions by Bolton et al. (2006) and Shleifer (2004): (i) the Kaplan-Zingales measure of capital constraint (KZ-score); and (ii) the turnover rate in the portfolios of institutional shareholders (IT-score).

The KZ-score (as estimated by Lamont, Polk and Saá-Réquejo (2001) and used, for example, in Bergman and Jenter (2007) and Malmendier and Tate (2005)) captures firms' need to access external capital markets. Financially constrained firms benefit from overstating performance, as it helps them reduce the cost of external financing.

For example, Guthrie and Sokolowsky (2010), Rangan (1998), and Teoh, Welch and Wong (1998a, 1998b) provide empirical evidence of earnings overstatements around IPOs and SEOs, and DeFond and Jiambalvo (1994) document overstatements to avoid debt-covenant violations. More recently, Linck et al. (2010) find that discretionary accruals are significantly higher in financially constrained than unconstrained firms prior to investment. Their evidence suggests that managers use earnings management to ease financial constraints, gain access to external funds, and invest.[22]

The IT-score captures internal pressure from firms' investors. Institutional shareholders with higher portfolio turnover rates are more likely to value short-term performance (see Carhart (1997) and Gaspar, Massa and Matos (2005) for applications). Indeed, Bushee (2001) shows that transient institutional owners overweigh current earnings in valuing firms; and Bushee (1998) finds that in the presence of transient institutional investors managers are more likely to engage in real earnings management to avoid to reverse an earnings decline. Also, there is mounting evidence that institutional shareholders actively influence management. Brav, Jiang, Partnoy and Thomas (2008), Carleton, Nelson and Weisbach (1998), and Becht, Franks, Mayer and Rossi (2009), and provide detailed studies of shareholder activism for a sample of US hedge funds, by TIAA-CREF in the US, and the Hermes Fund in the UK.

A difficulty in estimating Eq. (2.5) is that CEO incentives may reflect variation in shareholder benefits either over time and/or across firms. The fixed effects estimator, however, utilizes only within-firm variation and the between estimator uses only cross-sectional variation. Applying the random effects estimator to Eq. (2.5) constrains the within-effect to equal the between-effect. Yet, there is no reason to expect that the

[22] In robustness checks, we also use the Hadlock-Pierce-index (HP) and Whited-Wu-index (WW) of financial constraints. However, in our research setting, the KZ-index is more desirable for two reasons. First, the main difference between the KZ- and the WW-indexes is that the WW-index includes firm size (as measured by total assets; size is also the major determinant of the HP-index) as an indicator of financial constraint. However, firm size directly affects PPS: small firms are more financially constrained according to HP and WW, but also offer lower PPS (e.g., because the marginal returns of CEO effort are smaller). The size-PPS relationship is of lesser concern in estimating Eq. (2.7), because the direct effect of firm size on PPS is canceled out through the difference-in-difference approach. Second, the KZ-index is more highly correlated with actual debt and equity issuances in our sample. We posit that it is this access to capital markets that provides shareholders with benefits from overstatements.

difference in CEO incentives between two firms reflecting a one unit difference in SBO is equal to the change in CEO incentives within a firm for a one unit increase in SBO. Furthermore, our SBO measures exhibit greater variation in across firms than within firms.

To allow the between-firm effects to differ from the within-firm effects, we decompose every right hand side variable from Eq. (2.5) into a firm-fixed component (the average value for each firm — denoted by $\varnothing$) and the firm-change component (the period-to-period fluctuations around the firm average — denoted by Δ), as explained in Gould (2001):

$$\begin{aligned} incentives_{it} &= \psi_1^{\varnothing}\ SBO_i^{\varnothing} + \psi_2^{\varnothing}\ D(t \geq 0)_i^{\varnothing} + \sum_{j=1}^{k} \alpha_j^{\varnothing} X_{ji}^{\varnothing} \\ &+ \psi_1^{\Delta}\ SBO_{it}^{\Delta} + \psi_2^{\Delta}\ D(t \geq 0)_t^{\Delta} + \sum_{j=1}^{k} \alpha_j^{\Delta} X_{jit}^{\Delta} + \upsilon_i + \epsilon_{it}\ . \end{aligned} \tag{2.6}$$

To account for the increase in the cost of overstatements from SOX, we allow the effect of the shareholder benefit measures to vary from before to after SOX by interacting them with pre- and post-SOX dummies. We estimate the regression using the random-effects estimator. $\varnothing$-coefficients equal the coefficients that would be estimated using the between estimator; the Δ-coefficients equal the coefficients that would be estimated using the fixed-effects estimator. $D(t \geq 0)_i^{\varnothing}$ gets dropped from the regression, because it does not vary between firms (due to our requirement of no entry into and exit from the sample). Again, we estimate heteroskedasticity-robust standard errors and account for clustering at the firm-period level.

We run four versions of regression (2.6): two measures for CEO incentives (log(PPS) and log(PPS-ratio)) times two measures of shareholder benefits. The results are displayed in Table 2.5. In all four cases, we obtain a positive and statistically significant estimate of the effect of shareholder benefits on CEO incentives in the cross-section before SOX. We also find that the cross-sectional link between shareholder benefits and CEO incentives weakens after SOX. The p-value for Δ_{sox} confirms our conjecture that $\psi_1^{\varnothing}$ is indeed smaller after SOX than before SOX.[23]

To compare the economic magnitudes across the different measures of shareholder benefits from overstatements, we evaluate the percentage difference in expected CEO incentives for moving from the 25th to the 75th percentile in the pooled cross-sectional distribution of the SBO measures. The interquartile ranges ($\Delta_{iq}SBO$) are 1.06 for the KZ-score and 0.15 for the IT-score. The percentage change in CEO incentives is then given by $exp(\widehat{\psi_1^{\varnothing}} \times \Delta_{iq}SBO)$. We obtain KZ- and IT-effects of 36% and 21% on PPS, which translate into differences between \$220,000–\$382,000 per 1% increase in firm value at the mean of pre-SOX PPS, and \$58,000–\$101,000 at the median of pre-SOX PPS.

Our findings on the cross-sectional relationships between proxies for SBO and PPS are consistent with those of contemporaneous work on executive compensation: Wang (2008) finds that CEO pay-for-performance sensitivities are higher in financially constrained firms than in unconstrained firms; and Shin (2008) documents that short-term institutional ownership is associated with higher option compensation.[24]

To summarize, we show that cross-sectional variation in shareholder benefits from overstatements is reflected in cross-sectional variation in CEO incentives. We also document that the cross-sectional link between shareholder benefits and CEO incentives is stronger before SOX than after SOX. These findings suggest that shareholder objectives vary cross-sectionally with shareholder benefits from overstatements.

[23] In contrast to the strong results in the cross-section, we find a weaker relationship between within-firm variation in the KZ-score and CEO incentives, and no link for IT-score. The within-firm variation comes from only 3 years in the pre-SOX period, and 4 years in the post-SOX period, but not from across the periods (we do this in the next section). Therefore, this finding is not surprising, given the limited number of observations per firm over time and the between- and within-variation in the SBO-scores mentioned previously.

[24] Dikolli, Kulp and Sedatole (2009) also find that bonuses, which capture only a fraction of total incentive pay, are more sensitive to stock returns than earnings and equity grants are larger when transient institutional ownership is high. However, the authors interpret these findings as evidence that CEO incentive contracts are designed to offset myopia.

2.4.2.3 The Change in CEO Incentives around SOX: The Effect of Shareholder Benefits from Overstatements

In the preceding section we show that our measures of shareholder benefits of overstatements are consistent with the model's prediction about the effect of shareholder myopia λ in the cross-section. In this section we go one step further and test if, within firms, incentives also fall by more around SOX in firms with high shareholder benefits, as stated in Hypothesis (2.3). To that end, we run the regression

$$\begin{aligned} \textit{incentives}_{it} &= \phi_1 \; SBO_{it} + \phi_2 \; D(t \geq 0)_t \times D(SBO|t < 0)_i + \phi_3 \; D(t \geq 0)_t \\ &+ \alpha_0 + \sum_{j=1}^{k} \alpha_j X_{jit} + \upsilon_i + \epsilon_{it} \; , \end{aligned} \tag{2.7}$$

where $D(t \geq 0)_t$ is a dummy set to one for fiscal years 2002–2005 and $D(SBO|t < 0)_i$ is a dummy that indicates high shareholder benefits from overstatements in the period before SOX. In particular, for the time-varying KZ- and IT-scores, we average the score over the three-year pre-SOX period for each firm. We consider the upper half of the distribution to have high SBO ($D(SBO|t < 0)_i = 1$). While separating the SBO groups at the median is coarse, it is transparent and easily interpretable.[25] Thus, ϕ_2 is the coefficient of interest. A negative estimate of ϕ_2 would indicate that incentives fall by more in firms with high shareholder benefits from overstatements before SOX.[26] To control for the possibility that the within-firm change in incentives is driven by the within-firm change in shareholder benefits from overstatements over time, we also include the time-varying continuous measure of shareholder benefits in the regression. X_{jit} contains the same standard determinants of CEO incentives as regression (2.4). As before, υ_i are firm fixed effects. We estimate heteroskedasticity-robust standard errors, clustered by firm-period.

[25] The results remain qualitatively unchanged if we use continuous pre-SOX averages of the proxies for shareholder benefits instead of their dummy versions, or consider only the top 15% of KZ-scores as financially constrained.

[26] Here we allow the average within-firm response of CEO incentives to SOX to vary cross-sectionally. The fixed effects estimator identifies ϕ_2, because the time-invariant shareholder benefits variable is interacted with the time-varying post-SOX dummy.

The results are displayed in Table 2.6. The coefficients are directly comparable across SBO measures for the same measure of incentives, because the interaction term uses a dummy for SBO. Our estimates are remarkably similar across the different specifications. Specifically, we find that log(PPS) falls by 0.121–0.164 more in firms with high pre-SOX shareholder benefits than in firms with low pre-SOX shareholder benefits. Translating these estimates into dollar figures yields an additional decrease in PPS for high SBO firms between \$122,000 and \$162,000 at the mean level of pre-SOX PPS, and between \$32,000 and \$43,000 at the median level of PPS. The results for the PPS-ratio are similar. All interaction terms are significant at the 1% confidence level or better.

It is worth noting that, when measuring CEO incentives with log(PPS), the coefficient for the post-SOX dummy loses its statistical significance and much of its economic magnitude compared to the specifications in Table 2.4. This finding suggests that the decrease in stock- and option-based incentives around SOX is fully concentrated in firms with high benefits from overstatements prior to SOX: only firms with high shareholder benefits from overstatements value market performance. When measuring CEO incentives with the log(PPS-ratio), however, the post-SOX dummy remains negative with sizable magnitude in all regressions. This finding suggests that all firms — with and without benefits from overstatements — increase the relative importance of salary and bonus pay around SOX.

The evidence in Table 2.6 is arguably stronger than the evidence presented in Table 2.4. While significant changes in PPS coincide with SOX, the estimated SOX effect potentially reflects other events or changes in market conditions. The results in Table 2.6 implicitly control for such confounding effects, because we compare the change in PPS around SOX between firms with high and low shareholder benefits from overstatements. Through this difference-in-difference approach we are able to rule out alternative explanations that affect the two groups equally. For example, heterogeneity in investors' beliefs about the extent of overstatements may have decreased with the revelation of more and more accounting scandals. Growing media and social pressure on restraining skyrocketing CEO pay, especially in the form of stock options, may have also led to the change in the structure of CEO compensation.

2.4.2.4 Contemporaneous Changes in NYSE/Nasdaq Listing Requirements

Contemporaneous to SOX, NYSE and Nasdaq were in the process of revising their listing requirements. The goal of these reforms was to improve the quality of corporate governance by increasing the independence of corporate boards and their committees. In particular, the new listing requirements on the NYSE and Nasdaq require each board to have a majority of independent directors, as well as fully independent compensation and audit committees. The new NYSE and Nasdaq rules became effective with a company's first annual meeting occurring after January 15, 2004, but no later than October 31, 2004. For the majority of firms, the new requirements became binding for fiscal year 2003 reports.

We use board data provided by Riskmetrics to determine firms' compliance status. We match the Riskmetrics observation to the fiscal year into which the board meeting date falls. We classify boards as compliant or non-compliant based on their board independence in fiscal year 2002, the year prior to the rule change. Following Chhaochharia and Grinstein (2009), we reclassify directors as independent when their employment relationship terminated three or more years ago to reconcile the differences in how Riskmetrics and the NYSE/Nasdaq listing standards define independence. Of our 857 sample firms, we classify 138 as non-compliant, and lack board data for 77.

The new listing requirements had a noticeable impact on board independence. The change in board independence is evident in Table 2.2, Panel A. Firms that were failing the new director independence standards in the year prior to those rules going into effect, improved their governance drastically over the following years. In the non-compliant firms, only 42% of directors were independent before the new rules, but independence increased by 10 percentage points within one year and by 20 percentage points by 2005. On the other hand, firms that already met the requirements show an increase of only 3 percentage points from 2002 to 2005. The fraction of compliant boards in our sample jumps from 82% in 2002 to 93% in 2004.

We allow the effect of SOX on CEO incentives to differ between compliant and non-compliant firms by estimating regression (2.4) separately for compliers and non-compliers. In addition, we add various measures of board characteristics that might either affect CEO pay and incentives (e.g., board ownership, tenure and age of directors) or vary systematically around SOX (e.g., board size, board independence, and the number of directorships of board members) as control variables. Table 2.7 displays the results. CEO incentives decrease in compliant firms, which suggests that even independent boards emphasize market values over fundamental values. Thus, we should not expect independent boards to be effective monitors of overstatements.

The economic magnitude of the change in log(PPS) is three times larger for non-compliers than compliers, but not for incentives measured as log(PPS-ratio). The difference between the SOX effects, however, is not statistically significant for log(PPS) with a p-value of 29.9%.

Note that the estimate of the decrease in PPS around SOX for compliant firms isolates the effect of shareholder myopia from changes in board independence. In contrast, the estimate for non-compliant firms captures both myopia and changes in board independence. Under the assumption that the effect of myopia is the same for compliant and non-compliant firms, our estimates suggest that board independence leads to an economically significant decrease in CEO incentives (at least if measured as log(PPS)). There are at least two possible explanations for this finding. First, the decrease in CEO incentives in non-compliant firms is consistent with the view that oversight and incentive pay are substitutes (Holmström 1979). The large decrease could thus reflect not just the change in the cost of overstatement, but also the improvement in the quality of corporate governance. Second, as suggested by Bertrand and Mullainathan (2001), non-independent boards may not have been setting or enforcing optimal incentive contracts. Therefore, the large decrease in CEO incentives could also be attributable to a regime shift from managerial skimming to optimal contracting. That compliant and non-compliant boards differ in the allocation of power between managers and shareholder becomes evident when one compares board ownership. The mean pre-SOX ownership of compliant boards is only 6.4%, but 20.0% in non-compliant firms. Ownership by independent directors, however, is much smaller in magnitude and about equal at 1.1% in compliant firms and 1.2% in non-compliant firms.

Our model does not contain a parameter for board independence and thus does not offer predictions about the effect of independence (and its interaction with SBO) on CEO incentives. In light of the alternative views on the role of board independence, we simply replicate the empirical tests of Hypotheses (2.2) and (2.3) for the subsample of firms in compliance with the new board independence requirement in fiscal year 2002 and control for the various board characteristics mentioned previously. The results remain qualitatively, and in most cases even quantitatively, unchanged. We conclude that our findings are not attributable to the contemporaneous changes in board characteristics.

2.5 Conclusion

Recent corporate governance reforms and proposals have put great emphasis on improving board independence and shareholder empowerment. The view behind these reforms is that shareholder voice is an important building block to improving corporate decision making and the quality of financial reporting. In contrast, opponents cite stockholder power — and their demand for increasingly higher returns — as a major reason for firms' focus on short-term performance. However, empirical evidence on shareholder preferences for overstatements is scant. By inferring shareholder objectives from observed changes in pay-for-performance sensitivities around SOX, we are able to differentiate between these two opposing views.[27]

[27] We do not speak to the efficiency of overstatements. On the one hand, earnings overstatements can distort investment decisions. If firms appear more profitable than they are, managers invest in insufficiently profitable projects to mimic investment and employment of truly profitable firms (as documented in Kedia and Philippon (2009), for example). On the other hand, Shleifer and Vishny (1990) argue that short-term arbitrage being cheaper than long-term arbitrage leads to firms focusing on short-term assets to avoid prolonged underpricing. That is, firms may avoid long-term investments with positive net present values, because of fear of underpricing. Therefore,

Our two approaches — inferring shareholder objectives and proxying for shareholder objectives — yield results that are consistent with each other. We find that firms of large public companies in the U.S. respond to the increase in the cost of earnings overstatements imposed by the Sarbanes-Oxley Act of 2002 by reducing CEO incentives. Using two proxies for shareholder benefits from overstatements (capital constraints and portfolio turnover rates of institutional owners), we document a positive relationship between shareholder benefits and CEO incentives. We also find that the decrease in CEO incentives is concentrated in firms whose shareholders benefit from overstatements. These results indicate that CEO incentives reflect shareholders' benefits and CEOs' costs from overstatements. Our empirical evidence thus casts doubt on overstatements being an unintended consequence of inducing productive effort, at least prior to SOX.

After the wave of corporate scandals, much emphasis has been placed on board independence to curb and prevent corporate fraud. Yet, our results hold even if we restrict the sample to firms that were compliant with the board independence requirements instituted by the stock exchanges in 2003. Thus, shareholder myopia — and not just lack of board independence — could have been responsible for performance overstatements. We conclude that corporate boards as representatives of shareholders face a conflict of interest in their role as monitors of overstatements, and thus cannot be expected to effectively prevent overstatements.

contracts that encourage CEOs to avoid underpricing by inflating earnings could in fact alleviate underinvestment in long-term assets.

Figure 2.1: Residual CEO Incentives before and after SOX

This figure captures the change in CEO incentives around SOX. It plots the kernel density estimates of average residual CEO incentives for the pre- and post-SOX periods. We obtain residual CEO incentives from regressing CEO incentives on its known economic determinants used in estimating Eq. (2.4), but without time effects. For each firm, we then average the residuals over the pre- and post-SOX years.

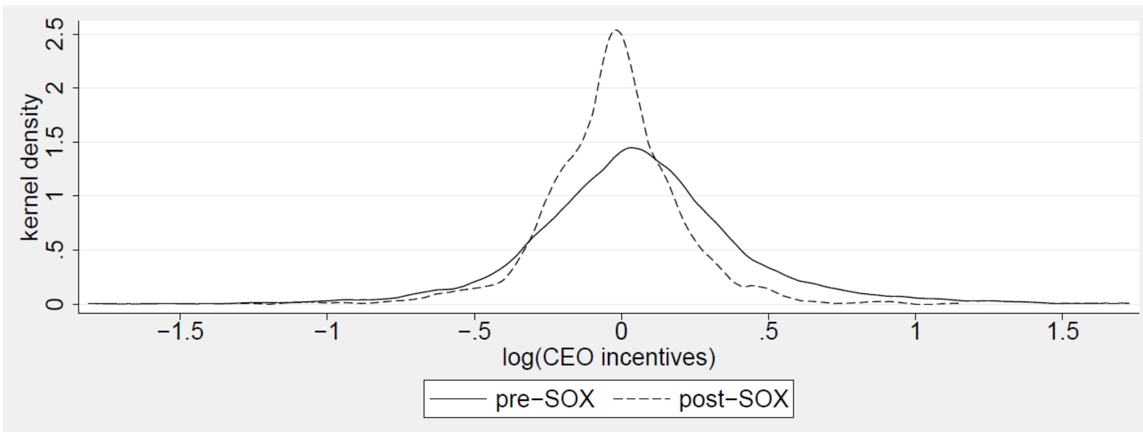


Table 2.1: Definition of Variables

variable	unit	transformation	definition
PPS	thsd. $	log	Δ in $ value of CEOs' equity and option holdings from a 1% increase in share price; inflation adjusted; see Core and Guay (2002) for details
PPS-ratio	fraction	log	PPS scaled by sum of PPS, salary, and bonus
equity-ratio	fraction		value of option and stock grants scaled by annual pay; ([blk_valu]+[rstkgrnt])/[tdc1] from Execucomp
market value	mill. $	log	$[199] * [25]$ from Compustat; inflation adjusted
stock price volatility		log	[bs_volat] from Execucomp
market-to-book ratio	multiple	log	$([6] - [60] - [74] + (199 * 25))/[6]$ from Compustat
leverage	fraction		$([9] + [34])/[6]$ from Compustat
return on assets	fraction	log(1+roa)	[18]/[6] from Compustat
shareholder return	fraction	log(1+ret)	calculated from monthly [ret] from CRSP; inflation adjusted
market return	fraction	log(1+ret)	calculated from monthly value-weighted market return incl. dividends [vwretd] from CRSP; inflation adjusted
CEO tenure	years	log	based on [becameceo] from Execucomp
option exercise ratio	fraction		fraction of exercisable options exercised; [opt_exer_val]/([opt_exer_val]+[opt_unex_exer_est_val]) from Execucomp
CEO turnover	dummy		$= 1$ if Δ_t[co_per_rol]$\neq 0$ from Execucomp
board size	count	log	number of directors on the board from IRRC
board independence	fraction		fraction of independent directors on the board from IRRC
board ownership	fraction		fraction of voting rights held by independent directors from IRRC
board tenure	years	log	average tenure of directors from IRRC
board age	years	log	average age of directors from IRRC
board busyness	count		average number of board seats held by directors from IRRC
KZ-score	index		based on Kaplan and Zingales (1997); inputs are scaled by [6] (lagged) $= -1.002 * [308] - 39.368 * [127] - 1.315 * [1] + 3.139*$[lev]$+0.283*$[mb] high likelihood of cap. constraints $\Rightarrow$ high benefits from overstatements
IT-score	fraction		based on Gaspar et al. (2005); annualized high institutional turnover rate $\Rightarrow$ high benefits from overstatements

Table 2.2: Summary Statistics on CEO and Firm Characteristics

Our sample covers large publicly traded firms with fiscal years 1999–2005. We require annual data on CEO incentives (from Execucomp) and firm characteristics (from Compustat). To avoid entry and exit effects, we only keep firms with CEO incentive data for all seven years of the sample. However, our results are qualitatively unchanged if we relax this restriction. Table 2.2, Panel A, displays the means of all variables (winsorized at the 1st and 99th percentiles) for each fiscal year. We consider fiscal years 1999–2001 as pre-SOX and fiscal years 2003–2005 as post-SOX. It is a priori unclear how fiscal year 2002 is affected by SOX, so we treat it as a transition year. Panel B provides further summary statistics for the pooled cross-section.

Panel A: Means by Fiscal Year

	1999	2000	2001	2002	2003	2004	2005
PPS ($ thsd.)	1178	1223	976	752	921	984	969
PPS-ratio	0.29	0.29	0.28	0.23	0.25	0.25	0.24
equity-ratio	0.43	0.44	0.48	0.45	0.41	0.42	0.42
market value ($ mill.)	8688	9518	8125	6707	8098	8732	8960
return volatility	0.40	0.46	0.48	0.50	0.48	0.45	0.39
market-to-book ratio	2.38	2.22	1.95	1.63	1.88	1.90	1.87
leverage	0.24	0.24	0.25	0.24	0.23	0.22	0.22
return on assets	0.05	0.05	0.02	0.02	0.03	0.04	0.04
shareholder return	0.24	0.19	0.06	-0.12	0.41	0.17	0.07
market return	0.22	-0.09	-0.15	-0.22	0.27	0.10	0.05
CEO tenure	8.49	8.35	8.01	8.13	8.08	8.46	8.16
option exercise ratio	0.13	0.15	0.15	0.16	0.11	0.16	0.17
CEO turnover	0.11	0.13	0.13	0.10	0.11	0.10	0.13
KZ-score	0.98	0.97	0.79	0.62	0.69	0.62	0.57
IT-score	0.61	0.61	0.50	0.45	0.49	0.45	0.51
compliant boards	0.77	0.79	0.80	0.82	0.88	0.93	0.95
board independence - compliers	0.70	0.72	0.73	0.75	0.76	0.77	0.78
board independence - noncompliers	0.43	0.44	0.44	0.42	0.52	0.60	0.62
board size	9.94	9.72	9.54	9.58	9.53	9.58	9.51
board ownership (indep.)	0.01	0.01	0.01	0.01	0.01	0.01	0.01
board tenure	9.73	9.72	9.67	9.73	9.84	9.82	9.89
board age	59.02	59.02	58.94	59.22	59.57	59.88	60.13
board busyness	1.76	1.69	1.67	1.60	1.59	1.57	1.54

Panel B: Summary Statistics for the Pooled Cross-Section

	25th %ile	50th %ile	75th %ile	mean	st dev	# obs
PPS ($ thsd.)	102	294	790	1001	2417	6153
PPS-ratio	0.09	0.18	0.34	0.26	0.24	6153
equity-ratio	0.19	0.46	0.67	0.43	0.29	6106
market value ($ mill.)	650	1786	6209	8404	20920	6152
return volatility	0.30	0.39	0.55	0.45	0.21	5913
market-to-book ratio	1.12	1.45	2.19	1.97	1.45	6150
leverage	0.08	0.23	0.35	0.24	0.18	6137
return on assets	0.01	0.04	0.08	0.04	0.09	6152
shareholder return	-0.16	0.07	0.32	0.15	0.54	6131
market return	-0.14	0.04	0.22	0.03	0.18	6146
CEO tenure	3.00	6.00	11.00	8.24	7.37	5891
option exercise ratio	0.00	0.00	0.19	0.15	0.26	6152
CEO turnover	0.00	0.00	0.00	0.12	0.32	6054
KZ-score	0.16	0.65	1.22	0.74	1.03	5721
IT-score	0.43	0.50	0.58	0.52	0.13	6153
board independence	0.60	0.73	0.82	0.70	0.16	5509
board size	8.00	9.00	11.00	9.63	2.68	5509
board ownership (indep.)	0.00	0.00	0.00	0.01	0.04	5509
board tenure	7.13	9.29	11.88	9.77	3.74	5509
board age	57.14	59.63	61.88	59.40	3.76	5509
board busyness	1.20	1.50	1.93	1.63	0.54	5509

Table 2.3: The Change in Incentives Around SOX — Year Dummies

In this table, we document that CEO incentives decrease around SOX, which was signed into law on 7/25/2002. We define 2002 as the transition year, as SOX falls into fiscal year 2002 for most companies. Fiscal years 1999–2001 are considered pre-SOX and fiscal years 2003–2005 are considered post-SOX. The year dummies are defined to capture the *marginal* effect of each year on the level of incentives and incentive pay (i.e., each year dummy captures the change from the previous year). Our measures of the level of CEO incentives are the dollar change in CEOs' stock and option holdings from a hypothetical 1% increase in firm value (PPS) in column (i); and the fraction of income derived from PPS relative to the sum of PPS, salary, and bonus (PPS-ratio) in column (ii). As a robustness check, we also look at the fraction of stock and option grants of total pay (equity-ratio) in column (iii), which captures the flow of incentives. Two-sided p-values — based on heteroskedasticity robust standard errors clustered at the firm level — are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% confidence levels.

	log(PPS)	log(PPS-ratio)	equity-ratio
2000 (pre-SOX)	0.114***	0.048	0.034*
	(0.009)	(0.246)	(0.082)
2001 (pre-SOX)	0.031	0.089***	0.046***
	(0.228)	(0.000)	(0.000)
2002 (transition year)	-0.044**	-0.110***	-0.046***
	(0.049)	(0.000)	(0.000)
2003 (post-SOX)	-0.188***	-0.097*	-0.053**
	(0.001)	(0.075)	(0.046)
2004 (post-SOX)	0.042	-0.033	-0.001
	(0.100)	(0.166)	(0.926)
2005 (post-SOX)	0.013	-0.016	-0.009
	(0.645)	(0.513)	(0.395)
market value (log)	0.901***	0.465***	0.063***
	(0.000)	(0.000)	(0.000)
return volatility (log)	0.183*	0.109	-0.021
	(0.074)	(0.242)	(0.441)
market-to-book ratio (log)	0.232**	0.298***	0.027
	(0.039)	(0.000)	(0.250)
leverage	-0.120	-0.065	0.010
	(0.347)	(0.589)	(0.829)
return on assets (log)	0.097	-0.307*	-0.073
	(0.536)	(0.050)	(0.173)
shareholder return (log)	0.188***	0.058***	-0.049***
	(0.000)	(0.008)	(0.000)
market return (log)	0.282***	0.224**	0.050
	(0.010)	(0.030)	(0.329)
CEO tenure (log)	0.441***	0.276***	-0.054***
	(0.000)	(0.000)	(0.000)
option exercise ratio	-0.174***	-0.132***	0.029**
	(0.000)	(0.000)	(0.038)
CEO turnover (dummy)	0.073*	0.076**	0.017
	(0.069)	(0.041)	(0.257)
# of observations	5,549	5,549	5,511
# of firms	857	857	856
within-R^2	0.540	0.325	0.069

Table 2.4: The Change in Incentives Around SOX — Post-SOX Dummy

In this table, we simplify our regressions from Table 2.3 by replacing the year dummies with a single dummy variable to differentiate between pre- and post-SOX years. We use this specification for ease of interpretation of our subsequent results. In the first two columns, we define fiscal years 2002 and later to be post-SOX. We choose to count fiscal year 2002 toward post-SOX, because the downward adjustment in CEO incentives becomes evident in fiscal year 2002, as shown in Table 2.3. In columns 3 and 4, we document that our finding is robust to an alternative definition of the post-SOX period. There, the post-SOX period includes all fiscal years that begin on or after 8/1/2002 (i.e., the first month after the Sarbanes-Oxley Act was signed into law on 7/25/2002). Two-sided p-values — based on heteroskedasticity robust standard errors clustered at the firm-period level — are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% confidence levels.

	fiscal year $\geq$ 2002		fiscal year begins $\geq$ 8/1/2002	
	log(PPS)	log(PPS-ratio)	log(PPS)	log(PPS-ratio)
post-SOX (dummy)	-0.082***	-0.117***	-0.083***	-0.128***
	(0.000)	(0.000)	(0.000)	(0.000)
market value (log)	0.901***	0.464***	0.905***	0.473***
	(0.000)	(0.000)	(0.000)	(0.000)
return volatility (log)	0.217***	0.209***	0.194***	0.175***
	(0.001)	(0.000)	(0.003)	(0.003)
market-to-book ratio (log)	0.234***	0.289***	0.247***	0.301***
	(0.007)	(0.000)	(0.004)	(0.000)
leverage	-0.121	-0.063	-0.105	-0.045
	(0.247)	(0.529)	(0.314)	(0.649)
return on assets (log)	0.091	-0.360***	0.092	-0.361***
	(0.493)	(0.008)	(0.488)	(0.007)
shareholder return (log)	0.188***	0.065***	0.188***	0.067***
	(0.000)	(0.001)	(0.000)	(0.001)
market return (log)	-0.065*	0.010	0.023	0.152***
	(0.059)	(0.757)	(0.635)	(0.001)
CEO tenure (log)	0.441***	0.276***	0.441***	0.276***
	(0.000)	(0.000)	(0.000)	(0.000)
option exercise ratio	-0.175***	-0.134***	-0.175***	-0.134***
	(0.000)	(0.000)	(0.000)	(0.000)
CEO turnover (dummy)	0.073*	0.071**	0.078**	0.078**
	(0.051)	(0.038)	(0.030)	(0.019)
# of observations	5,549	5,549	5,549	5,549
# of firms	857	857	857	857
within-R^2	0.539	0.321	0.538	0.319

Table 2.5: The Link between CEO Incentives and Shareholder Benefits from Overstatements

Our model predicts that greater shareholder benefits from overstatements lead to higher CEO incentives. This table presents empirical evidence linking shareholder benefits to incentives. We use two continuous proxies of shareholder benefits: (i) the Kaplan-Zingales measure of capital constraint; and (ii) the portfolio turnover rate of firms' institutional owners. We employ generalized random effects regressions to allow the between-firm effect of each right hand side variable to differ from its within-firm effect. The regressions include all the previous control variables, including the post-SOX dummy. Two-sided p-values — based on heteroskedasticity robust standard errors clustered at the firm-period level — are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% confidence levels. p-values for Δ_{sox} provide the confidence level for rejecting the null hypothesis that the link between shareholder benefits from overstatements and CEO incentives is stronger after SOX than before SOX.

SBO measure	KZ capital constraints		institutional investor horizon	
	log(PPS)	log(PPS-ratio)	log(PPS)	log(PPS-ratio)
Between Effects: Utilizing Variation Between Firms				
SBO-score × pre-SOX	0.288***	0.236***	1.248**	0.931**
	(0.000)	(0.000)	(0.025)	(0.027)
SBO-score × post-SOX	0.239***	0.188***	0.288	0.097
	(0.002)	(0.000)	(0.580)	(0.804)
p-value for Δ_{sox}	0.037	0.007	0.000	0.000
Within Effects: Utilizing Variation Within Firms Over Time				
SBO-score × pre-SOX	0.066***	0.008	-0.003	-0.200
	(0.004)	(0.689)	(0.983)	(0.132)
SBO-score × post-SOX	0.045*	0.029	0.080	-0.134
	(0.091)	(0.242)	(0.596)	(0.368)
p-value for Δ_{sox}	0.251	0.762	0.665	0.632
# of observations	5,217	5,217	5,549	5,549
# of firms	813	813	857	857
overall-R^2	0.645	0.443	0.641	0.428

Table 2.6: The Change in Incentives around SOX: The Effect of Shareholder Benefits from Overstatements

In this table we test the model's prediction that around SOX incentives will fall by more in firms with higher benefits from overstatements before SOX. *post-SOX* equals one for fiscal years 2002–2005, and zero otherwise. *pre-SOX SBO-dummy* equals one if the mean value of the KZ/IT-scores over the pre-SOX period falls in the upper half of the distribution, and zero otherwise. We also control for the variation in the KZ/IT-scores over time, as well as all other previous controls. Two-sided p-values — based on heteroskedasticity robust standard errors clustered at the firm-period level — are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% confidence levels.

SBO measure	KZ capital constraints		institutional investor horizon	
	log(PPS)	log(PPS-ratio)	log(PPS)	log(PPS-ratio)
post-SOX	-0.121***	-0.079***	-0.164***	-0.100***
× pre-SOX SBO-dummy	(0.000)	(0.004)	(0.000)	(0.000)
SBO-score	0.049***	0.013	-0.064	-0.217**
	(0.008)	(0.433)	(0.497)	(0.026)
post-SOX (dummy)	-0.019	-0.081***	-0.010	-0.089***
	(0.414)	(0.000)	(0.679)	(0.000)
market value (log)	0.882***	0.451***	0.905***	0.464***
	(0.000)	(0.000)	(0.000)	(0.000)
return volatility (log)	0.252***	0.228***	0.206***	0.202***
	(0.000)	(0.000)	(0.002)	(0.001)
market-to-book ratio (log)	0.202**	0.292***	0.218**	0.297***
	(0.023)	(0.000)	(0.013)	(0.000)
leverage	-0.297**	-0.062	-0.100	-0.036
	(0.015)	(0.601)	(0.339)	(0.720)
return on assets (log)	0.133	-0.345**	0.090	-0.358***
	(0.329)	(0.011)	(0.501)	(0.009)
shareholder return (log)	0.182***	0.064***	0.186***	0.068***
	(0.000)	(0.002)	(0.000)	(0.001)
market return (log)	-0.054	0.014	-0.055	0.030
	(0.131)	(0.698)	(0.115)	(0.376)
CEO tenure (log)	0.452***	0.286***	0.450***	0.282***
	(0.000)	(0.000)	(0.000)	(0.000)
option exercise ratio	-0.172***	-0.139***	-0.179***	-0.138***
	(0.000)	(0.000)	(0.000)	(0.000)
CEO turnover (dummy)	0.083**	0.087**	0.072*	0.076**
	(0.030)	(0.015)	(0.055)	(0.027)
# of observations	5,096	5,096	5,431	5,431
# of firms	779	779	823	823
within-R^2	0.546	0.327	0.549	0.329

Table 2.7: The Change in Incentives Around SOX: Controlling for Changes in Board Characteristics

This table replicates the tests reported in columns 1 and 2 of Table 2.4, except that we run the regressions separately for firms whose boards of directors were compliant and non-compliant with the new NYSE/NASDAQ listing requirements for board independence and control for various board characteristics. We determine compliance status in fiscal year 2002, which for most firms is the year preceding the announcement of the new governance standards. CEO incentives decreased even in compliant firms, although by a smaller magnitude than in non-compliant firms, indicating that our results are fully attributable to the contemporaneous changes in governance. Two-sided p-values — based on heteroskedasticity robust standard errors clustered at the firm-period level — are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% confidence levels.

	log(PPS)		log(PPS-ratio)	
	compliant	non-compliant	compliant	non-compliant
post-SOX (dummy)	-0.047**	-0.133	-0.108***	-0.099**
	(0.015)	(0.105)	(0.000)	(0.048)
market value (log)	0.936***	0.817***	0.516***	0.185**
	(0.000)	(0.000)	(0.000)	(0.042)
return volatility (log)	0.124**	0.594**	0.211***	0.255*
	(0.029)	(0.027)	(0.000)	(0.078)
market-to-book ratio (log)	0.233***	-0.098	0.262***	0.307**
	(0.000)	(0.847)	(0.000)	(0.034)
leverage	-0.080	-0.308	-0.086	-0.016
	(0.526)	(0.339)	(0.485)	(0.950)
return on assets (log)	-0.046	0.475	-0.800***	-0.012
	(0.806)	(0.349)	(0.000)	(0.971)
shareholder return (log)	0.231***	0.191*	0.061**	0.067
	(0.000)	(0.057)	(0.013)	(0.228)
market return (log)	-0.057	-0.037	0.017	0.033
	(0.108)	(0.709)	(0.644)	(0.724)
CEO tenure (log)	0.406***	0.674***	0.263***	0.363***
	(0.000)	(0.000)	(0.000)	(0.000)
option exercise ratio	-0.185***	-0.294**	-0.143***	-0.181*
	(0.000)	(0.039)	(0.000)	(0.090)
CEO turnover (dummy)	0.033	0.240*	0.052	0.081
	(0.437)	(0.059)	(0.201)	(0.415)
board size (log)	-0.301***	0.155	-0.236***	-0.234
	(0.000)	(0.515)	(0.001)	(0.162)
board independence	0.156	-0.161	0.098	-0.144
	(0.161)	(0.564)	(0.347)	(0.392)
board ownership (indep.)	0.326	-0.841	1.048	-0.248
	(0.592)	(0.210)	(0.194)	(0.657)
board tenure (log)	0.029	0.206	0.070	0.057
	(0.681)	(0.355)	(0.317)	(0.703)
board age (log)	-1.203**	0.093	-0.773*	0.316
	(0.012)	(0.928)	(0.099)	(0.626)
board busyness	-0.065	0.224	-0.051	0.128
	(0.120)	(0.133)	(0.167)	(0.174)
# of observations	4,056	802	4,056	802
# of firms	642	138	642	138
within-R^2	0.565	0.421	0.311	0.314

Table 2.8: The Link between CEO Incentives and Shareholder Benefits from Overstatements: Controlling for Changes in Board Characteristics

This table replicates the tests reported in Table 2.5, except that we restrict the sample to firms whose boards of directors were compliant with the new NYSE/NASDAQ listing requirements for board independence and control for various board characteristics (as displayed in Table 2.7). This restriction ensures that our results are not driven by the contemporaneous changes in governance. The regressions include all the previous control variables, including the post-SOX dummy. Two-sided p-values — based on heteroskedasticity robust standard errors clustered at the firm-period level — are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% confidence levels. p-values for Δ_{sox} provide the confidence level for rejecting the null hypothesis that the link between shareholder benefits from overstatements and CEO incentives has strengthened around SOX.

SBO measure	KZ capital constraints		institutional investor horizon	
	log(PPS)	log(PPS-ratio)	log(PPS)	log(PPS-ratio)
Between Effects: Utilizing Variation Between Firms				
SBO-score × pre-SOX	0.242***	0.193***	1.390**	1.172**
	(0.000)	(0.000)	(0.048)	(0.018)
SBO-score × post-SOX	0.202***	0.139***	0.852	0.494
	(0.002)	(0.003)	(0.189)	(0.305)
p-value for Δ_{sox}	0.076	0.012	0.022	0.002
Within Effects: Utilizing Variation Within Firms Over Time				
SBO-score × pre-SOX	0.062**	0.024	0.080	-0.263*
	(0.035)	(0.362)	(0.574)	(0.084)
SBO-score × post-SOX	0.060*	0.033	0.175	0.043
	(0.079)	(0.317)	(0.373)	(0.820)
p-value for Δ_{sox}	0.476	0.595	0.656	0.900
# of observations	3,777	3,777	4,056	4,056
# of firms	601	601	642	642
overall-R^2	0.667	0.461	0.668	0.451

Table 2.9: The Impact of Shareholder Benefits from Overstatements on the Change in Incentives around SOX: Controlling for Changes in Board Characteristics

This table replicates the tests reported in Table 2.6, except that we restrict the sample to firms whose boards of directors were compliant with the new NYSE/NASDAQ listing requirements for board independence and control for various board characteristics (as displayed in Table 2.7). This restriction ensures that our results are not driven by the contemporaneous changes in governance. *post-SOX* equals one for fiscal years 2002 – 2005, and zero otherwise. *pre-SOX KZ/IT-dummy* equals one if the mean value of the KZ/IT-scores over the pre-SOX period falls in the upper half of the distribution, and zero otherwise. We also control for the variation in the KZ/IT-scores over time, as well as all the previous control variables. Two-sided p-values — based on heteroskedasticity robust standard errors clustered at the firm-period level — are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% confidence levels.

SBO measure	KZ capital constraints		institutional investor horizon	
	log(PPS)	log(PPS-ratio)	log(PPS)	log(PPS-ratio)
post-SOX	-0.061*	-0.063**	-0.104***	-0.056*
× pre-SOX SBO-dummy	(0.063)	(0.037)	(0.002)	(0.068)
SBO-score	0.057**	0.025	0.030	-0.146
	(0.017)	(0.245)	(0.794)	(0.217)
post-SOX (dummy)	-0.002	-0.073***	0.001	-0.097***
	(0.933)	(0.001)	(0.978)	(0.000)
# of observations	3,710	3,710	3,988	3,988
# of firms	582	582	622	622
within-R^2	0.568	0.314	0.572	0.317

2.A Appendix

2.A.1 Proofs

Proof of Proposition (2.1)

From (2.5),

$$\begin{aligned} \frac{\partial \beta^*}{\partial \rho} \quad &= \quad \frac{2\eta^2\Big(1-\lambda\eta(1-\theta)\big(1+r(\sigma_a^2+\sigma_m^2)\big)\Big)}{\Big(1+2\eta^2\rho\big(1+r(\sigma_a^2+\sigma_m^2)\big)\Big)^2} \geq 0 \qquad (2.A.1)\\ \iff \quad & \lambda \leq \frac{1}{\eta(1-\theta)(1+r(\sigma_a^2+\sigma_m^2))}. \end{aligned}$$

Since $\lambda \leq 1$, $\frac{\partial \beta^*}{\partial \rho}$ is always non-negative if $\frac{1}{(1-\theta)(1+r(\sigma_a^2+\sigma_m^2))} \geq 1$, that is, if $\theta \geq \frac{\eta r(\sigma_a^2+\sigma_m^2)-1}{\eta r(\sigma_a^2+\sigma_m^2)}$.

If $\theta < \frac{\eta r(\sigma_a^2+\sigma_m^2)-1}{\eta r(\sigma_a^2+\sigma_m^2)}$, however, $\frac{\partial \beta^*}{\partial \rho} < 0$ if and only if $\lambda > \frac{1}{\eta(1-\theta)(1+r(\sigma_a^2+\sigma_m^2))}$.

Proof of Proposition (2.2)

(i) From (2.5), it is straightforward to show that

$$\frac{\partial \beta^*}{\partial \lambda} = \frac{\frac{1-\theta}{2\eta\rho}}{1+\frac{1}{2\eta^2\rho}+r(\sigma_a^2+\sigma_m^2)} > 0. \qquad (2.A.2)$$

(ii) From (2.A.1),

$$\frac{\partial^2 \beta^*}{\partial \lambda \partial \rho} = -\frac{2\eta^3(1-\theta)\big(1+r(\sigma_a^2+\sigma_m^2)\big)}{\Big(1+2\eta^2\rho\big(1+r(\sigma_a^2+\sigma_m^2)\big)\Big)^2} < 0. \qquad (2.A.3)$$

2.A.2 Details on Calculating PPS

We construct the incentive measure following Core and Guay (2002). In particular, we compute the dollar change in executives' stock- and option holdings for a hypothetical one percent change in firm value (we call this variable pay-for-performance sensitivity [PPS]). We separately calculate PPS for newly granted options, previously granted exercisable and unexercisable options, and stock holdings. Measuring PPS requires six inputs: the risk-free rate, stock price volatility, dividend yield, time to maturity, stock price, and number of options granted or held. All variables except for the risk-free rate can be obtained from Execucomp, either directly (e.g., dividend yield and volatility, stock price) or indirectly (time-to-maturity, number of options held).

Following the Execucomp convention in calculating option grant values, we winsorize volatility and dividend yields within each fiscal year. The largest and smallest values are least likely to be good representations of expectations about their future values. We replace missing values of the 3-year average dividend yield (*bs_yield*) with current dividend yields, missing values for volatility (*bs_volat*) with the Execucomp sample mean, and missing values for exercise price (*expric*) with either the market price (*mktpric*) or the average of the fiscal-year-end closing price (*prccf*) and the closing price discounted by total shareholder returns that year (*trs1yr*). We also observe that firms who make only one grant to an executive within a fiscal year often only report the total number of options granted (*soptgrnt*), but not the number of options in that grant (*numsecur*). We estimate maturity to be the difference between exercise date and grant date. Missing values are assumed to be 10 years. Some maturities are computed to be 0 years, so we replace those with 1 year. We also value the options at the end of the fiscal year, not at the time of the grant to make all values comparable and current at fiscal year end. Finally, we weight the individual grants' deltas by the grant values to each executive within each year to compute PPS from new option grants for each executive-firm-year.

Estimating the inputs for previous grants is harder. Information on the characteristics of past option grants is not available. For example, the number and value of unexercisable options are available, but we do not know the composition of the unexercisable options from previous grants. Similarly, for exercisable options, we do not know which previously granted options were exercised by the executives and which ones were kept in the portfolio. However, Core and Guay's main contribution lies in showing that imputing the missing characteristics yields a very close approximation to hand-collected, full-information option portfolios. Unfortunately, the documentation in Core and Guay does not allow us to replicate their imputation strategy directly. We encounter a number of problems. For example, the reported value of (un)exercisable options pertains only to in-the-money options, but the number of (un)exercisable options also includes out-of-the-money options. Furthermore, adjusting the value and number of unexercisable options for current year option grants imply that about half of our observations would end up with negative values. We assume that the reported number of unexercisable options held includes newly granted options, unless the number of options granted exceeds the holdings. Similar to our approach for newly granted options, we estimate the exercise price for previously granted options by appropriately discounting the adjusted fiscal-year end stock price by total shareholder returns (*trs3yr*). The maturity of unexercisable options is assumed to be one year less than the maturity of any option grant in the previous year, or 9 years if no options were granted in the previous year. The maturity of exercisable options is assumed to be 3 years less than that of unexercisable options.

2.A.3 Robustness Checks

Representativeness of the Mean Effect

To ensure that our results are representative of the typical firm in the sample instead of being driven by large changes in a few firms, we also estimate median regressions. The results are presented in Table 2.A.1. We purge firm fixed effects by demeaning all variables.[28] The estimated median change in incentives from before

[28]First-differencing instead of demeaning does not materially affect the results.

to after SOX is almost identical to the mean effect. We conclude that the change in CEO incentives is pervasive and representative of the typical firm in our sample.

Bonus Pay

Our measures of CEO incentives emphasize CEOs' wealth gains from stock and option holdings. In practice, however, other forms of pay, such as bonuses, are also tied to firm performance and can thus provide incentives for overstatements. Our first measure of the level of CEO incentives — log(PPS) — completely ignores CEOs' bonus compensation. Although our second measure of CEO incentives — log(PPS-ratio) — includes bonuses, it assumes that bonuses provide CEOs with fewer incentives to overstate performance than stock- and option holdings. To rule out the possibility that CEO incentives shifted from PPS to bonus pay around SOX without affecting the link between total CEO pay and firm performance, we take an alternative approach offered in the prior literature on CEO pay to estimate how the performance-sensitivity of CEO pay has changed around SOX. We regress bonus pay and total CEO pay on two measures of firm performance: return on assets and firm stock returns. We also interact the performance measures with the post-SOX dummy to allow for changes in the performance sensitivity of CEO pay:

$$\begin{aligned} pay_{it} &= \tau_1 \; performance_{it} + \tau_2 \; D(t \geq 0)_t \times performance_{it} + \tau_3 \; D(t \geq 0)_t \\ &+ \alpha_0 + \sum_{j=1}^{k} \alpha_j X_{jit} + \upsilon_i + \epsilon_{it} \; , \end{aligned} \tag{2.A.4}$$

where $D(t \geq 0)_t$ is a dummy set to one for fiscal years 2002–2005. The interaction term captures whether the link between pay and performance has strengthened or weakened from before to after SOX. Again, we estimate heteroskedasticity-robust standard errors, clustered at the firm-period level.

The results are displayed in Table 2.A.2. In column 1, we use bonus pay as the dependent variable. In column 2, we use total CEO pay as the dependent variable, which includes the flow of compensation (such as salary, bonus, stock and option grants), as well as changes in the value of CEOs' stock and option holdings. We use the dollar value of bonus and total pay (in $ mill.) instead of their logarithmic values, because the dollar amounts are zero or negative in a non-negligible fraction of observations. To alleviate the concern that outliers severely affect the magnitude of our estimates, we winsorize the pay and performance measures at the top and bottom percentile.

The result for bonus pay confirms that incentive pay has in fact shifted from stocks and options toward bonus pay. We estimate that bonus pay has increased by $166,000 around SOX on average. Furthermore, bonus pay does increase with return on assets (accounting performance) and with firm stock returns (market performance). Most interestingly, however, is the finding that the accounting-performance sensitivity decreases around SOX, while the market-performance sensitivity of bonus pay increases. This shift towards bonus pay and its increasing market-performance sensitivity suggest that our earlier results based on log(PPS) overstate the true decrease in incentives.

Turning to total pay, we find that it primarily responds to firms' market performance. The economic magnitude of its performance sensitivity swamps the wealth effects from bonus pay.[29] More importantly, the performance-sensitivity of total pay decreases sharply around SOX by almost half. We conclude that the declining performance sensitivity of stock and option holdings outweighs the increasing weight placed on bonus pay and its increasing market-performance sensitivity.

[29] As CEO pay is highly skewed, estimated mean effects are not representative of the typical firm. Using median regressions reduces the magnitude of the estimates by factors ranging from 2 to 4, but the qualitative findings do not change.

Table 2.A.1: The Change in Incentives Around SOX — Median Regression

In Tables 2.3 and 2.4, we report results from firm-fixed-effects regressions that estimate the mean change in CEO incentives from before to after SOX. To ensure that our results are representative of the typical firm in the sample (instead of being driven by large changes in a few firms), we also estimate median regressions. We purge firm fixed effects by demeaning all variables. In columns 1 and 2, the post-SOX period includes fiscal years 2002 and later. In columns 3 and 4, the post-SOX period includes all fiscal years that begin on or after 8/1/2002. Two-sided p-values are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% confidence levels.

	fiscal year $\geq$ 2002		fiscal year begins $\geq$ 8/1/2002	
	log(PPS)	log(PPS-ratio)	log(PPS)	log(PPS-ratio)
post-SOX (dummy)	-0.080***	-0.110***	-0.084***	-0.120***
	(0.000)	(0.000)	(0.000)	(0.000)
market value (log)	0.879***	0.424***	0.875***	0.426***
	(0.000)	(0.000)	(0.000)	(0.000)
return volatility (log)	0.064**	0.155***	0.028	0.110***
	(0.034)	(0.000)	(0.420)	(0.007)
market-to-book ratio (log)	0.293***	0.270***	0.319***	0.297***
	(0.000)	(0.000)	(0.000)	(0.000)
leverage	-0.032	-0.067	-0.044	-0.046
	(0.605)	(0.412)	(0.529)	(0.577)
return on assets (log)	0.124*	-0.353***	0.144*	-0.345***
	(0.098)	(0.000)	(0.091)	(0.001)
shareholder return (log)	0.136***	0.054***	0.131***	0.057***
	(0.000)	(0.003)	(0.000)	(0.002)
market return (log)	-0.029	0.033	0.057	0.166***
	(0.286)	(0.365)	(0.121)	(0.000)
CEO tenure (log)	0.382***	0.256***	0.382***	0.248***
	(0.000)	(0.000)	(0.000)	(0.000)
option exercise ratio	-0.132***	-0.110***	-0.137***	-0.121***
	(0.000)	(0.000)	(0.000)	(0.000)
CEO turnover (dummy)	0.007	0.030	0.020	0.041
	(0.742)	(0.248)	(0.368)	(0.123)
# of observations	5,549	5,549	5,549	5,549
# of firms	857	857	857	857
Pseudo-R^2	0.389	0.189	0.388	0.187

Table 2.A.2: The Changing Link between CEO Pay and Firm Performance

Our first measure of the level of CEO incentives — log(PPS) — has the potential drawback that it does not include CEOs' bonus compensation, which can also be tied to firm performance. Although our second measure of CEO incentives — log(PPS-ratio) — does include bonuses, it assumes that bonuses provide CEOs with fewer incentives to overstate performance than stock- and option holdings. To rule out the possibility that CEO incentives shifted from PPS to bonus pay around SOX without affecting the link between total CEO pay and firm performance, we take an alternative approach offered in the prior literature on CEO pay. To this end, we regress CEO pay (in $ mill.) on two measures of firm performance: return on assets and firm stock returns. We also interact the performance measures with the post-SOX dummy to allow for changes in the performance sensitivity of CEO pay. In column 1, we only consider bonuses. In column 2, we consider total CEO pay, which includes both the flow of compensation (e.g., stock and option grants, salary, and bonus) as well as changes in the value of CEOs' stock- and option holdings. Two-sided p-values — based on heteroskedasticity robust standard errors clustered at the firm-period level — are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% confidence levels.

	Bonus Pay	Total Pay
post-SOX (dummy)	0.166***	0.119
	(0.000)	(0.937)
market value (log)	0.338***	2.785
	(0.000)	(0.402)
return volatility (log)	-0.232***	-15.829***
	(0.003)	(0.001)
market-to-book ratio (log)	-0.262***	23.970***
	(0.000)	(0.000)
leverage	-0.060	25.601**
	(0.606)	(0.013)
return on assets (log)	1.035***	-27.218
	(0.000)	(0.280)
return on assets (log) × post-SOX	-0.504**	-4.136
	(0.015)	(0.853)
shareholder return (log)	0.133***	67.780***
	(0.000)	(0.000)
shareholder return (log) × post-SOX	0.135***	-31.631***
	(0.005)	(0.000)
market return (log)	-0.072	21.243***
	(0.445)	(0.007)
market return (log) × post-SOX	-0.027	-4.330
	(0.819)	(0.646)
CEO tenure (log)	0.030	4.360***
	(0.294)	(0.001)
option exercise ratio	0.055	-0.628
	(0.194)	(0.810)
CEO turnover (dummy)	-0.031	3.576*
	(0.489)	(0.081)
# of observations	5,549	5,361
# of firms	857	857
within-R^2	0.104	0.229

CHAPTER 3

Obesity, Health Costs, and Credit Risk

3.1 Motivation

While most of the costs of obesity are borne by the obese through the impact on their personal health, a significant amount accrues to the public (Bhattacharya and Sood (2011)). Due to the growing prevalence of obesity in the population, obesity is increasingly being considered as a factor in economic interchange.

For example, businesses use obesity as the basis for differential treatment of customers and employees. Airlines and movie theaters require large people to purchase two seats; the obese tend to earn lower wages; and legal cases provide anecdotal evidence of consideration of body weight in hiring and promotion decisions. In the majority of cases, the justification for differential treatment is that obesity imposes higher costs. When a large person takes up two seats, airlines and movie theaters cannot sell the extra seat to another customer. Even when a large person fits into one seat, airlines incur higher fuel costs and landing fees. When obese employees suffer more injuries or illnesses than their non-obese coworkers, employers experience a productivity loss and incur higher costs for their health plans. To offset the higher cost of serving or employing the obese, businesses have incentives to charge higher prices, offer lower wages, or avoid hiring obese workers (e.g., Kirkland (2008), Bhattacharya and Bundorf (2009), and Lundborg, Nystedt and Rooth (2009)).[1]

[1] The weight gain in the U.S. population during the 1990s cost the airlines an estimated 350 million gallons of additional jet fuel in 2000 (Dannenberg, Burton and Jackson (2004)). A Duke University Medical Center analysis found that obese workers filed twice the number of workers'

With public attitude toward obesity ranging from viewing it as a stigmatized difference to being a character flaw, U.S. law and public policy have struggled with how to respond to the statistical discrimination against the obese in the markets. Sometimes obesity is viewed as an affliction, and sometimes as a personal choice. In 2002, the IRS recognized obesity as a medical condition, allowing tax deductions for certain medical expenses. Yet, in 2005, the House of Representatives passed the *Personal Responsibility in Food Consumption Act* to protect the fast food industry from legal liability (although the bill did not pass the Senate vote). In 2006, changes to the Health Insurance Portability and Accountability Act (HIPAA) paved the way for group health plans to charge lower premiums to the non-obese. In response, the State Employees' Insurance Board of Alabama, for example, approved a policy under which obese employees will have to pay an additional $25 per month in health insurance beginning in 2011 if they do not make sufficient progress toward lowering their BMI. There has also been renewed interest in levying a tax on fatty foods and sugary drinks.

Given the increasing relevance of obesity for public and business policies, and the controversy surrounding them, it is worth investigating the stakes. The purpose of our research is to shed light on the magnitude and nature of the relationship between obesity and relevant economic outcomes, specifically credit risk. Obesity is primarily the result of excessive caloric intake (Cutler, Glaeser and Shapiro (2003)). It is a known health risk factor and carries a social stigma. Its presence, despite being preventable, thus provides a potentially informative signal from individuals' past choices about their preferences or future choices. Based on these premises, we first explore whether obesity does in fact have predictive power for consumer delinquencies

compensation claims, had seven times higher medical costs from those claims and lost 13 times more days of work from work injury or work illness than did nonobese workers (Østbye, Dement and Krause (2007)). According to Thompson, Edelsberg, Kinsey and Oster (1998), already in 1994 the cost of obesity to U.S. businesses amounted to $2.4 billion for paid sick leave, $2.6 billion for life and disability insurance, and $7.7 billion for health insurance. See Hammond and Levine (2010) for a recent and thorough review of the economic consequences of obesity.

and bankruptcies. Second, we investigate the channels through which obesity affects credit risk.[2]

Our empirical analysis utilizes 2004 and 2008 survey data from the 1979 cohort of the National Longitudinal Survey of Youth (NLSY79), which is a nationally representative sample of the U.S. population born between 1957 and 1964. We estimate that the obese have a 3.8 percentage point higher likelihood of becoming delinquent over the following four years (this constitutes a 20.5% increase relative to the sample delinquency rate of 18.5%), after controlling for credit risk relevant factors that are observable to lenders and permissable under federal regulations, such as respondents' income and net wealth, debt capacity, credit histories, and income instability.[3] The magnitude is comparable to 62%, 83%, and 58% of the impact of unemployment spells, marriage dissolutions, and disability shocks conditional on those trigger events occurring. Taking into account the frequency with which they occur, the total incidence of obesity on delinquencies is on par with that of unemployment spells, triple the effect of marriage dissolutions, and twice as important as disability shocks.

We conduct numerous robustness tests of our main result in terms of measurement and estimation, and consider several alternative interpretations. We show that delinquencies are indicators of serious financial distress, as opposed to forgotten bills while on vacation. Our result holds for alternative measures of financial distress (bankruptcies and maxing out credit cards). We also address the possibility that obesity simply proxies for omitted variables that are credit-risk-relevant and available to lenders. In a subsample of respondents who have applied for credit, we show that obesity is conditionally uncorrelated with lenders' credit decisions, but has strong predictive power

[2]In light of the substantial medical expenditures attributable to obesity, we are interested in the relationship between obesity and personal financial outcomes. Also, the high rate of personal bankruptcies has drawn much attention from academics, policy makers, and the public. The rate has more than quadrupled between 1980 and 2005, culminating in the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA). While BAPCPA resulted in a steep drop-off in personal bankruptcies, the bankruptcy rate has again reached its pre-2005 level.

[3]Delinquency is defined as having completely missed a payment or been at least 2 months late in paying any bills in the previous 5 years.

for subsequent delinquencies. We conjecture that obesity does not proxy for credit relevant information that is used by lenders, but is unobservable to us. Furthermore, the result is not driven by observable attributes that are prohibited under the Equal Credit Opportunity Act (ECOA), such as ethnicity, gender, and marital status. It also does not reflect credit risk factors or signals that are unobservable to lenders, such as cognitive abilities (as measured by the AFQT score), one's youth BMI, or parental influence on respondents' preferences or decision making quality (controlled for by including sibling fixed effects). Conditional on observable and permissible credit risk factors, these variables are uncorrelated with obesity.

The obesity effect is present in different time periods (e.g., before the financial crisis) and is robust to more flexible specifications of the credit risk model. Semiparametric estimates show that the delinquency rate rises almost monotonically over most of the BMI range (although we do not have sufficient observations in the underweight category). Propensity scoring is highly effective at making the obese and non-obese comparable along observable characteristics, reveals common support over almost the entire range of the propensity score, and yields estimates similar to the credit risk benchmark.

Next, we explore cross-sectional heterogeneity in the ability of obesity to predict delinquencies. It is particularly informative when obesity is least likely, e.g., among high income earners, the wealthy, and those with low credit risk. These findings are consistent with (i) obesity being more informative when it has not yet lead to financial distress in the past; (ii) obesity being more informative when it reflects individuals' choices rather than their economic environment or financial situations; and (iii) obesity having higher predictive power among individuals with higher human capital and correspondingly greater incentives to invest in health, e.g., in the form of medical expenditures.

Finally, we investigate two plausible mechanisms through which obesity affects delinquencies. First, we find that the obesity effect is at least partially mediated through health. Including measures of physical well-being reduces the obesity effect by about 30%.[4] Second, we find no support for the idea that the obesity effect reflects individuals' time or risk preferences. Conditional on economic factors, plausible preference parameters such as impatience and myopia are weakly related to BMI or obesity. While measures of time-preferences help predict delinquency, their inclusion does not absorb any of the obesity effect. With much of the obesity effect on delinquencies left unexplained, we also cannot rule out that obesity proxies for other unobserved socioeconomic factors.

Our paper proceeds with a review of some related literature in Section 3.2, followed by a description of the methodology (Section 3.3) and the data (Section 3.4). Section 3.5 shows that obesity predicts delinquencies after controlling for observable and permissible credit risk factors, as well as prohibited or to lenders unobservable characteristics. We assesses the economic magnitude of our finding in Section 3.6, and investigate cross-sectional heterogeneity in the predictive power of obesity in Section 3.7. Section 3.8 analyzes the potential channels through which obesity impacts delinquencies. Section 3.9 concludes. All robustness tests and further discussions of potential methodological issues have been relegated to the Appendix.

3.2 Related Literature

While our paper adds a new dimension in evaluating the costs of obesity, it specifically contributes to the rapidly growing literature on household finance and the determinants of credit risk.

In an attempt to rein in risky lending, the Financial Services Authority (FSA) of the UK proposed in October 2009 "to require all lenders to assess the level of a consumer's expenditure in determining the affordability of a mortgage product"

[4]Unfortunately, NLSY data does not permit us to cleanly identify health shocks, or to link those to increased expenditures or loss of income.

(Judge 2009). It was suggested that such an affordability test would assess borrowers' spending on shoes, clothes, childcare, alcohol, and smoking. However, differentiating between needs and wants in borrowers' spending is difficult, and such spending measures are easily manipulable by the borrowers and costly to verify for the lenders.[5] Nevertheless, the proposal raises an intriguing question: What additional predictors of loan defaults are not currently priced, but could be incorporated into loans?

Current credit risk models — such as the one behind the FICO score — primarily focus on credit capacity (i.e., income and wealth relative to debt or credit limits) to assess borrowers' ability and willingness to repay, as well as borrowers' payment histories to identify different risk types. One area in which the current models fall short is the assessment of *future* changes in borrowers' ability to repay. Illiquidity is the key driver of defaults, and often stems from the loss of employment, medical expenditures, or marital dissolutions.[6,7]

Only recently have academics begun to shift their attention towards predicting such trigger events or identifying the relatively risky borrowers. For example, self-employed individuals are more likely to experience a drop in income than employees of large corporations. Workers in the construction industry have riskier jobs than those working for utility companies, and tenured professors have more stable income perspectives than untenured professors. Drewianka (2010) investigates such system-

[5]We do recognize that misaligned incentives play a critical role in risky lending practices (e.g., Keys, Mukherjee, Seru and Vig (2010)). This paper, however, is about the information available to lenders, regardless of whether they utilize it.

[6]With the recent fall in real estate values, negative equity has also become a driver of defaults.

[7]Keys (2010) investigates the consequences of these trigger events on households' credit market outcomes. He finds strong support for for job displacement causing long-lasting repercussions. His results for marital dissolutions and health shocks are somewhat more tenuous. While bankruptcy filings are higher in the years after a divorce, the bankruptcy rate begins to rise in the year prior to the divorce. He interprets this finding as money problems influencing the probability of getting divorced, but his findings are equally consistent with partners experiencing cash flow shocks in anticipation of the divorce (e.g., separate housing, legal costs, etc.). Keys also notes that the effect of health shocks is imprecisely estimated, mostly because they are noisily proxied for with the onset of disability that lasts at least two consecutive surveys. While he finds a significant increase in the bankruptcy rate from before to after the onset of disability, those individuals who become disabled also have higher bankruptcy rates 3 to 4 years prior to the disability.

atic variation in income instability across workers, and its determinants could then be incorporated into credit risk models to assess future income risk. Guvenen and Smith (2010) point to large cross-sectional variation in income growth. They are able to elicit individuals' private information about future income prospects from their consumption choices. Using credit card account data from a Mexican retail chain, Vissing-Jorgensen (2011) is able to predict future default losses from consumers' purchases. Specifically, she finds that people who spend more on luxuries cause higher losses, which is consistent with the view that consumer spending reveals information about the borrowers' time preferences or self-control problems.[8,9]

These recent advances complement a rapidly growing literature on the effect of individuals' preferences and decision making processes on financial outcomes. For example, Agarwal and Mazumder (2010) show that households with lower cognitive abilities are more likely to make costly mistakes in financial decisions. Lusardi and Tufano (2009) find that individuals with low financial literacy amass excessive debt. Gatherwood (2011) links financial literacy and impatience to higher delinquency rates. While these studies validate theories of preferences and decision making, and even offer new insights into how to improve the quality of financial decision making, the underlying traits of intelligence, financial literacy, impatience, or self-control problems are not directly observable. As such, they are of limited use in assessing future credit risk.[10]

[8] Anecdotal evidence suggests that lenders do take expected earnings changes into consideration. A recent article in the New York Times tells the story of a woman who was denied credit after the lender received an automated response from her email account that she would be on maternity leave (Bernard (2010)). While it is prohibited to discriminate against pregnant women or base loan decisions on the number of children in the household, the lender was legally allowed to take the expected loss of income during maternity leave into account when making the loan decision. This story did have a happy ending, as the woman's employer continued to pay her. See Duhigg (2009) for an account of how a credit card company already utilizes its knowledge of consumers' purchases.

[9] We are aware of only one provider of credit scores that factor in income stability (ScoreLogix, founded in 2003).

[10] Our study also adds to the recent work on the relationship between obesity or health and wealth. Zagorsky (2004) documents that weight gain negatively impacts wealth. Poterba, Venti and Wise (2010) find a large correlation between poor health and asset accumulation for retirement. Münster, Rüger, Ochsmann, Letzel and Toschke (2009) find that obesity is more prevalent among the over-

3.3 Methodology

The goal of this paper is to assess whether obesity captures credit risk relevant information that is otherwise unobservable to or nonverifiable by the lender at the time of extending the loan. We do not argue for a causal relationship between obesity and future delinquencies — for our purposes, it suffices to show that the correlation between obesity and future delinquencies is not captured by information typically found on credit applications.

The basic idea is that a borrower's financial liquidity is represented by a latent random variable

$$Y^* = X'\beta + \varepsilon, \tag{3.1}$$

where $\varepsilon \sim N(0, \sigma^2)$ and X captures financial resources and obligations (e.g., income, wealth, and debt). If liquidity falls below zero, the borrower becomes delinquent, an observable event we denote as Y. That is,

$$Y = \mathbf{1}_{\{Y^*<0\}} = \begin{cases} 1 & \text{if } Y^* < 0 \Leftrightarrow \varepsilon < -X'\beta, \\ 0 & \text{otherwise.} \end{cases} \tag{3.2}$$

This yields the familiar probit model

$$\Pr(Y = 1 \mid X) = 1 - \Phi(X'\beta). \tag{3.3}$$

In estimating this basic credit risk model we regress future delinquencies (Y_{t+1}) on a number of variables intended to capture the credit risk relevant information set

indebted, suggesting a link from financial distress to weight gain. However, while finding a link from debt to deteriorating physical and mental health, Keese and Schmitz (2010) find no impact of over-indebtedness on obesity.

available to lenders (X_t).[11,12] Those credit risk relevant variables are called characteristics, one of which is obesity. We seek to understand the association between obesity and future delinquency.

As is standard in credit risk modeling, we adopt a flexible specification for all characteristics by creating a number of attributes for each characteristic. Each attribute is a dummy indicating whether an observation falls into a particular range of the underlying characteristic. Jointly, the attributes cover the range of each characteristic. Admittedly, there is no perfect way to determine the optimal number and spacing of attributes. Some credit risk modelers reduce the arbitrariness by iteratively parsing the characteristic to maximize a pre-specified objective function (e.g., maximizing adjusted R^2) subject to pre-specified constraints (e.g., monotonicity requirements). This process is not entirely non-arbitrary. For example, it still depends on the order in which characteristics are parsed and how the parsing proceeds. Should the modeler split each characteristic into 2 splices, then 3 splices (i.e., thus move the boundaries of the attributes with each round of parsing)? Or should the modeler keep previous parsings and continue parsing each subgroup?

The obvious constraint to the number of attributes is the size of the sample available to the credit risk modeler. We determine attributes rather arbitrarily (although we check whether our main result is robust to various alternative specifications). For example, from a continuously measured characteristic we create six attributes — five attributes corresponding to the quintiles of the characteristic and an additional attribute for missing responses.[13]

[11]The Board of Governors of the Federal Reserve System provides an excellent overview about credit risk scoring in its Report to the Congress on Credit Scoring and Its Effects on the Availability and Affordability of Credit (BGFRS 2007). This section draws heavily on that document.

[12]We estimate probit models. Probit, logit, and linear probability model results do not materially differ from each other.

[13]Dropping observations with missing data on controls reduces the sample by about one third and yields a larger estimate of the obesity effect.

DiNardo, Garlick and Stange (2010) criticize the underlying monotonicity assumption behind the obesity-outcome relationship and the parsimonious specifications of control variables typically employed in academic research on obesity for failing to capture important non-linearities and heterogeneity. Our research design mitigates these concerns through the creation of attributes, which yield a flexible specification for the controls. While we keep our main analysis simple by comparing outcomes between the obese and non-obese, we also carefully examine the effect of weight on delinquency over the entire range of BMIs in Section 3.A.1.3.

ECOA makes it unlawful for lenders to discriminate against credit applicants with protected personal or demographic characteristics. The prohibition is far reaching, from discouraging applications to differential loan pricing. Under the Federal Reserves Regulation B, which implements ECOA, protected characteristics include race, ethnicity, gender, marital status, religion and to a limited extent age. Furthermore, Regulation FF does not allow lenders to use medical information in credit eligibility or pricing decisions. Therefore, our baseline credit risk model excludes the prohibited characteristics, but we later investigate if obesity is simply a proxy for the prohibited characteristics.

3.4 The Data

Ideally, our analysis would link individual loan performance to borrowers' obesity, while controlling for information that is available to the lender at the time the loan contract is signed. Unfortunately, the ideal data is currently not available; it would require the cooperation of a lender in collecting borrowers' weight and height without influencing the loan decision and tracking loan performance over several years.

We use individuals' survey responses from the 1979 youth cohort of the National Longitudinal Survey of Youth (NLSY79), which is administered by the Bureau of Labor Statistics. The NLSY79 is a nationally representative survey that began in

1979, covering 12,686 individuals born between 1957 and 1964. We primarily utilize data from the 2004–2008 biennial interviews, which still cover about 60% of the original sample.[14]

Designed to follow life-time experiences of a representative cross-sectional sample of the population, the focus of the NLSY79 has been on labor market outcomes. As such, this data set has been used extensively in labor economics, but rarely in financial economics. The 2004 and 2008 interviews, however, include questions on respondents' loan delinquency status. Both surveys ask respondents about having been delinquent on any debt payment over the last five years, as well as prior bankruptcies and credit card usage.[15] Combined with detailed data on respondents' income, wealth, debt, employment status, education, family background, the NLSY79 allows us to use borrowers' 2004 obesity status to predict 2008 delinquencies, while controlling for numerous factors that affect both. In addition, survey questions on respondents' health status, as well as risk and time preferences allow us to examine various mechanisms that potentially link obesity to delinquency.[16]

So why do we not predict bankruptcies rather than delinquencies? In light of the relative infrequency of bankruptcies, our sample of 6,995 observations is too small to yield sufficient statistical power. To put this in perspective, typical credit risk models contain hundreds of characteristics and are estimated over millions of observations.[17] Turning to delinquencies improves the statistical power, because they occur more fre-

[14]The annualized attrition rate in the NLSY79, adjusted for the discontinuation of the two subsamples, is only 1%. The drop in participation is primarily driven by the discontinuation of the military subsample (1,079 participants) after the 1984 interview and the discontinuation of the subsample of economically disadvantaged, nonblack/non-Hispanic respondents (1,643 participants) after the 1990 interview. The loss of these two subsamples does not affect the representativeness of the survey of this age cohort in the U.S. population, because those two subgroups were intentionally oversampled.

[15]The exact question is: "In the last 5 years, have you completely missed a payment or been at least 2 months late in paying any of your bills?"

[16]The usual disclaimer about surveys applies to our research as well. The answers to the interview questions are self-reported. As such, they may reflect biases (e.g., underreporting of one's weight) or mistakes (e.g., a misunderstanding of the interview question). In addition, answers to questions that gauge respondents' risk and time preferences reflect beliefs, not actual economic choices.

[17]There are 7,661 respondents in the 2004 survey, 7,156 of which have information on future delinquency. Missing BMI eliminates another 161 respondents.

quently than defaults as people catch up with their late payments. One immediate concern then is whether delinquencies capture serious financial distress or whether delinquencies are inconsequential late water bills, perhaps because someone had forgotten to make a payment while on vacation or after having ones credit card replaced. To this end, we tabulate delinquencies vs. bankruptcies in Table 3.1. In the 2008 survey, 9.2% of delinquent respondents declared bankruptcy between 2004 and 2008. In the 2004 survey, 11.8% of delinquent respondents filed for bankruptcy between 2000 and 2004. For comparison, the bankruptcy rates among the non-delinquent respondents were 2.7% in 2008 and 2.5% in 2004. That is, delinquency almost quadruples bankruptcy risk, which indicates that delinquencies are an indicator of serious financial distress.[18]

NLSY data allow us to construct an estimate of each respondent's body mass index (BMI) as the ratio of respondents' self-reported weight (converted to kilograms) and height (converted to meters) squared. Height is reported in years 1981, 1982, 1985, 2006, and 2008. We use the average of the observations from 1985, 2006, and 2008 as our measure of adult height to reduce measurement error (we discard the observations from 1981 and 1982, because respondents may not yet have reached their adult heights at that time). Weight is reported more frequently (in 1981, 1982, 1985, 1986, 1988–90, and 1992–2008 interviews), because it fluctuates more over time. To calculate BMI in 2004, we use the weight reported in the 2004 survey. Following the convention set forth by the World Health Organization (WHO), we classify a respondent as obese if his/her BMI is 30 or greater. We also use the more narrow classification into categories of underweight (BMI < 18.5), normal weight ($18.5 \leq$ BMI < 25), overweight ($25 \leq$ BMI < 30), class I obese ($30 \leq$ BMI < 35), and class II/III obese (BMI ≥ 35). Fig. 3.1 displays the distribution of BMI in the U.S. population in their

[18]In undisclosed results, we find that delinquencies are also associated with a high probability of declining wealth. Also see Section 3.A.1.1 for robustness tests in which we utilize bankruptcies and maxed out credit cards.

early 40s and its classification. 32% of the cohort are of normal weight, 39% are overweight, and 28% are classified as obese.[19,20]

The NLSY79 data are obtained from a complex survey design. As such, the participants do not represent a random sample of the population (e.g., they are geographically clustered to minimize interviewers' travel times between participants and are subject to self-selection, because only those respondents who chose to complete the initial interview became NLSY79 cohort members). In all of our analyses, we use the sample weights provided in the NLSY (which also adjust for non-interviews) to obtain estimates that are representative of the U.S. population born between 1957 and 1964.[21,22]

3.5 Obesity Is a Delinquency Risk Factor

Fig. 3.2 shows that the incidence of delinquency rises steadily across the weight categories. Relative to the population of normal weight who have a delinquency rate of 14.5%, the overweight have a 3 percentage point higher incidence of delinquency. The difference in delinquency rates relative to respondents of normal weight is 8 percentage points for the obese (class I) and 14 percentage points for the severely obese (classes II/III). In light of evidence that being underweight also constitutes a health risk factor, it is somewhat surprising to find that the underweight have

[19]Information on BMI in 2004 is missing for 2% of the cohort. Our results are virtually unchanged if we use those respondents' BMI from earlier or later years.

[20]One drawback to the NLSY data is that delinquencies occur at the household level, but BMI is available only for the respondent. However, Abrevaya and Tang (2010) find that a spouse's BMI is the most significant predictor of BMI after controlling for individual socioeconomic and behavioral characteristics. Using MEPS data (specifically, longitudinal panel 9 covering years 2004 and 2005), we also find that conditional on observing a non-obese respondent of the same age as the NLSY cohort, the probability of another household member being obese is 28%. Conditional on observing an obese respondent, the probability is 53%.

[21]As access to the detailed geographical information on respondents' residence is restricted, we cluster standard errors by the intersection of geographic region and whether the respondent lives in an urban or rural location, SMSA, or city to approximate the use of stratification and sampling units in the survey design.

[22]Appendix 3.A.2 contains a discussion of measurement error in our key variables.

the lowest delinquency rate of all (but less than 1% of the population falls into this category, which makes it a very imprecise estimate).[23,24]

As is evident from Table 3.2, however, the observable characteristics of the obese represent a systematically higher credit risk than those of the non-obese. The obese have lower income and wealth, and are more highly leveraged. Their credit histories are substantially worse: they are more likely to have been delinquent or bankrupt in the past, or to have been denied additional credit.[25] The obese are also less educated. We also find that obesity is more prevalent among Hispanics and Blacks and that it is negatively correlated with respondents' age-adjusted AFQT-score ranking (used in prior literature as a measure of general ability). The only mitigating factors are that the obese tend to have more stable incomes (measured as the ratio of average total net family income over years 1996 to 2004 over its standard deviation) and are less likely self-employed. The biggest concern we face is to differentiate the effect of obesity from the effect of other risk factors.[26]

In the following section, we provide evidence that obesity has an effect on delinquencies above and beyond that captured by other measures of credit risk, and that the obesity effect is not driven by any of the prohibited characteristics (such as ethnicity or gender) or borrower attributes that are not observable to the lender (e.g., cognitive ability and common family background). Column 1 of Table 3.3 provides our starting point. Without any control variables, the marginal effect of obesity on delinquency risk is 8.1 percentage points, with a standard error of 1.3 percentage

[23]The results are almost identical if we restrict the sample to the 5,301 respondents who were indebted in 2004, through mortgages, home loans, car loans, student loans, credit card debt, or debt owed to other businesses, people or institutions (such as doctors, lawyers, and hospitals). Delinquency need not be restricted to borrowers, because even respondents without debt in 2004 can borrow subsequently.

[24]Fig. 3.A.1 in the Appendix provides the corresponding graph for bankruptcies.

[25]We use the individual credit history components in the regressions, but combine them into a single adverse credit history dummy variable for this table.

[26]*Unemployment* identifies unemployment spells since the last interview (it does not mean currently unemployed). As it pertains to a longer time period, the number is larger than the unemployment rate in 2004.

points. Note that the delinquency rate in the population is 18.5%, so the potential contribution of obesity is large in magnitude.

3.5.1 Permissible and Observable Credit Risk Factors

More Than Income, Wealth, and Debt

Column 2 of Table 3.3 shows the impact of including various controls — taken from the 2004 survey — for income, wealth, and debt on the obesity effect.[27] Income, wealth, and debt are primary credit risk factors and have been linked to obesity in numerous prior studies. For example, Lundborg et al. (2009) document an 18% earnings penalty for obesity among 450,000 Swedish men; Zagorsky (2004) finds that Americans with BMIs in the normal range have about twice the net worth of the obese; and Münster et al. (2009) show that obesity is more prevalent among the over-indebted in Germany.

As expected, being in a higher income or wealth quintile is associated with a lower risk of delinquency (see Table 3.A.1). Also, delinquencies tend to rise with the debt-to-asset ratio. However, we find that the debt-to-income ratio is not related to future delinquencies, which is consistent with the results of Foote, Gerardi, Goette and Willen (2009). We acknowledge that the irrelevance of the debt-to-income ratio may be driven by measurement error. However, neither using an estimate of the annual debt payment in lieu of debt nor average reported household income to smooth out temporal variation materially alters our finding. Even after controlling for wealth, income, and debt ratios, the average marginal effect of obesity remains economically

[27]The appendix contains three additional tables (Tables 3.A.1, 3.A.2, and 3.A.3), in which we sequentially introduce every control variable. The details provided there allow the interested reader to assess the relative importance of the various credit risk factors and their relation to obesity. Here we provide only a brief summary of the main findings.

significant at 5.1 percentage points (with a p-value of 0.1%).[28,29]

More Than Credit History

Credit histories play a crucial role in assessing credit risk, because they — at least partially — reveal information about borrowers' types that is otherwise unobservable. Of course, not all prior derogatory accounts are the borrowers' fault; they can happen to the best risk types due to bad luck, e.g., the closing of a major local employer or the health repercussions from a car accident. However, to the extent that borrowers are of inherently different risk types, bad types will on average have worse credit histories than good types.

If, as we posit, obesity is related to credit risk, we would expect obesity and future delinquencies to be correlated with the incidence of prior bankruptcies and delinquencies. And since the 2004 and 2008 interviews ask respondents about bankruptcies and delinquencies, we can either control for or condition on such past derogatories when predicting 2008 delinquency status. In addition, in 2004 the NLSY asked participants whether they had applied for a loan in the last 5 years or since the last bankruptcy; and whether the application was denied or approved. An application for a loan indicates a borrowers' need or desire to borrow (e.g., due to liquidity constraints or for consumption smoothing). More importantly, credit denials also reflect lenders' assessments of the applicants' credit risk.

As expected, we find that credit risk is serially correlated, i.e., prior delinquencies and bankruptcies positively predict future delinquencies (see Table 3.A.2). This find-

[28]Using average income instead of 2004 income to reduce measurement error further strengthens the obesity effect.

[29]The estimate of the obesity effect on delinquencies is highly robust to increasing the flexibility of the specification (why quintiles?); the marginal effect is 0.050 when we create deciles instead of quintiles (see column 6 in Table 3.A.1) and 0.052 when we include dummies for each $5,000 increment in income, $10,000 increment in wealth, and 5% increment in the debt-income and debt-asset ratios (unreported; available from the authors upon request). In this last specification, we lose about 23% of the sample, because certain attributes perfectly predict delinquency. Further tests indicate that the obesity effect is also highly robust to polynomial specifications (as an alternative to the creation of attributes) and various interactions between the income, wealth, and debt characteristics.

ing is consistent with the view (i) that individual fixed effects are important credit risk factors or (ii) that financial distress can have long-lasting repercussions (e.g., analogous to a poverty trap). On average, having been delinquent translates into a 26.0 percentage point higher likelihood of becoming delinquent again. The average marginal effect of prior bankruptcy is lower, but economically still highly significant at 7.3 percentage points. The fact that a household has applied for credit and was accepted does not help predict future delinquencies, either positively or negatively. However, delinquencies are estimated to be 16.3 percentage points higher when credit was denied, and 11.0 percentage points higher for credit applications withheld because of a low chance of approval.

The explanatory power of credit history and lenders' information about future credit risk is evident in column 3 of Table 3.3, which shows that adding these three increases the Pseudo-R^2 from 0.009 to 0.119. Despite the economic and statistical significance of the proxies for credit history and the differential prevalence among the obese and non-obese, their impact on the coefficient on obesity is relatively modest.

More Than Income Risk and Labor Market Indicators

As earnings from labor typically constitute the largest fraction of income, we now turn to assessing the relationship of earnings risk with obesity and delinquency. Credit applications routinely ask potential borrowers about their current employment situation and how long they have been with the employer. The primary objective is to verify borrowers' claims about their current income and to ascertain that the borrower can be expected to continue earning this income. Until recently, however, credit risk models have not systematically attempted to assess cross-sectional differences in applicants' income instability.

Our most direct measure is the income instability coefficient (the ratio of 1996–2004 average income over its standard deviation. We further select a number of variables based on the growing strand of literature in labor economics that investigates cross-sectional heterogeneity in earnings instability. Drewianka (2010) provides a recent and detailed study based on data from the PSID on this topic. He finds that education, self-employment, and workers' occupations and industries affect earnings stability. Age and ethnicity matter as well, but regulations prohibit their use in credit risk models.[30] Therefore, we exclude them from our analysis in this section (we will explore their impact on the obesity effect in Section 3.5.2). We further add respondents' tenure with their employer and whether they have experienced an unemployment spell or been out of the labor force since the last the last interview in 2002.

We find that greater income instability is associated with a higher delinquency rate (see Table 3.A.3). Respondents in the highest quintile are 6.3 percentage points more likely to become delinquent than respondents in the lowest quintile. Greater educational attainment translates into a lower delinquency rate, e.g., an advanced degree by 2.8 percentage points. The greater earnings volatility associated with self-employment manifests itself in a more than 6.5 percentage point increase in the expected delinquency rate. The marginal effects of tenure, unemployment, and labor force participation are of economically smaller magnitudes and not statistically significant at conventional significance levels. More importantly, our estimate of the effect of obesity on delinquency is relatively insensitive to controlling for labor market indicators and income instability.

Benchmark Model

In column 5 of Table 3.3, we add all of the credit risk factors into the regression (for a total of 96 attributes). This specification is our benchmark result. At 3.8 percentage points, the average marginal effect of obesity on delinquency remains economically

[30] In contrast, Bostic (1997) fails to find racial differences in earnings volatility in an attempt to explain racial differences in mortgage application denial rates.

large (20.5% of the total delinquency rate) and sufficiently precisely estimated (a standard error of only 1.2 percentage points). Obesity appears to contain credit-risk-relevant information above and beyond income and wealth; debt-to-income and debt-to-asset ratios; prior bankruptcies, delinquencies, and credit decisions; income risk and labor market indicators. The caveat remains that we are missing some important information that is available to lenders, like data on the number of credit accounts, credit limits, and utilization. Those measures may also be correlated with obesity, for the same reasons that we expect obesity to proxy for credit risk in the first place.

Does Obesity Proxy for Omitted Credit Risk Factors?

We have included numerous regressors to ensure that obesity does not simply capture observable credit-risk-relevant information that is otherwise available to lenders. Yet, the possibility remains that the NLSY lacks relevant information that is available to lenders, but not us (such as credit scores, borrowing capacity, and utilization). This raises the question whether our results extend to those omitted variables as well.

We have already incorporated lenders' information contained in credit decisions on loan applications into our control variables. Here, we investigate its properties in more detail. First, we test whether obesity also predicts credit denials. If obesity does in fact predict denials after controlling for the other credit risk factors, then it would likely be proxying for omitted variables — and cast serious doubt on our interpretation of obesity risk. In a second step, one would then have to test whether the informational overlap between obesity and denial is the same as the overlap between obesity and delinquency.

Column 1 of Table 3.4 shows the results of the first test. The sample is restricted to respondents who in the 2004 survey said they had applied for credit. The credit decision (*Denial*) is now the dependent variable (equal to one if the loan application was rejected). We include the full set of observable and permissible credit risk factors as controls, with the exception of the credit decision itself (obviously). The coefficient on obesity is 2.5 percentage points, with a standard error of 2.0 percentage points (with a p-value of over 20%). We cannot reject the hypothesis that obesity is not reflected in credit decision.[31]

When we regress delinquency on obesity (and the controls), the coefficient on obesity is large and significant (4.8 percentage points in column 2). Controlling for the credit decision (column 3) and conditioning on no denying the credit application (column 4) does not take away from the obesity effect on delinquency. The results indicate that the credit decisions contain no informational overlap with obesity. In other words, obesity appears to be unrelated to the additional credit-risk-relevant information apparently utilized by lenders.

In addition, as noted by Vissing-Jorgensen (2011), the statistical fit of predictive default regressions is typically modest. Gross and Souleles (2002), for example, obtain Pseudo-R^2s of about 14% in dynamic probit regressions predicting delinquency and 13% predicting bankruptcies after controlling for account age, credit utilization, internal and external credit scores, and local economic conditions (many of their variables were collected by the credit card issuers themselves). Vissing-Jorgensen obtains Pseudo-R^2s of about 10% in predicting loss rates with information on account age, loan amounts, downpayments, interest rates, loan terms, credit limits, repayment histories, credit scores, demographics, and store fixed effects. The statistical fit of our model compares favorably to that of other credit risk models in the literature, which suggests a limited potential role of additional covariates for explaining the obesity-delinquency link that we document here.

These observations increase our confidence that obesity would survive the inclusion of additional control variables that are not available in the NLSY.

[31] The marginal effect of obesity turns even negative if we control for its propensity score instead of including the covariates separately.

3.5.2 Personal Characteristics that Are Prohibited or Unobservable to Lenders

One important concern about using obesity in assessing credit risk is that it merely proxies for factors that are known to be correlated with credit risk, but are by law prohibited from being used in credit decisions. For example, it is well-known that obesity is more prevalent among Blacks and Hispanics (e.g., CDC (2009)), and their default rates are higher (e.g., see Martin and Hill (2000) on car loan performance and Pope and Sydnor (2008) for evidence from peer-to-peer lending). In columns 1–3 of Table 3.5, we individually introduce indicator variables for ethnicity, gender, and marital status as control variables (in addition to all the controls used in the benchmark regression).[32] Even controlling for these prohibited characteristics does not affect our estimate of the obesity coefficient in any meaningful way. Some of the factors, however, are correlated with delinquency. Relative to Whites, Blacks are more likely to be delinquent by 4.1 percentage points, suggesting that ethnicity proxies for unobserved socioeconomic factors. We also find that women are more likely to be delinquent than men, by about 2.6 percentage points. While those never married, separated, divorced, or widowed are more likely to be delinquent than married borrowers, the effect is relatively small in magnitude (1.3 and 1.4 percentage points) and not statistically significant at conventional levels.

Lundborg et al. (2009) fully attribute the negative relationship between obesity and earnings among 450,000 Swedish men to differences in cognitive and non-cognitive skills and fitness between the obese and non-obese. They conclude that employers utilize obesity to statistically discriminate against employees of lower expected productivity. Furthermore, Agarwal and Mazumder (2010) document a strong association between cognitive skills and the quality of household financial decision-making

[32] BMIs also tend to increase with age up until the mid 50s or early 60s (e.g., Baum and Ruhm (2009), DiNardo et al. (2010)). Yet, the age profile does not play an important role in our study, because the NLSY participants' age range is limited (an interquartile range of only 5 years).

among members of the U.S. military. In column 4 of Table 3.5, we control for early educational attainment/innate ability as captured by respondents' age-adjusted score on the Armed Forces Qualifying Test (AFQT). Delinquency risk does decrease with higher AFQT scores. For example, those with AFQT scores in the top quintile are 3 percentage points less likely to be delinquent than those in the bottom quintile (p-value of 2.6%). Despite that AFQT scores are higher for the non-obese than the obese, including them in the regression does not affect the relationship between obesity and delinquencies.

Persico, Postlewaite and Silverman (2004) show that the positive association between adult height and earnings can be fully traced back to youth height. Height advantages during the teen years can affect self-esteem or social dominance which in turn may translate into better economic outcomes during adulthood (e.g., through participation in club sports) as argued by Persico et al. or simply proxy for better cognitive ability as argued by Case and Paxson (2008). To test whether obesity risk can be traced back to the teen years, we include attributes for youth BMI quintiles in column 2 of Table 3.5.[33] We find that the youth BMI attributes absorb none of the conditional association between obesity and delinquency.

In column 5, we control for all prohibited or unobservable characteristics simultaneously. Taken together, the factors that are prohibited or not observable to the lenders do not impact the correlation between obesity and delinquency.[34]

Finally, it is possible that parental influence manifests itself both in obesity and in a higher probability of financial distress in adulthood. For example, Baum and Ruhm (2009) show that socioeconomic status during childhood is strongly related to adult BMIs. To see whether differences in early life conditions can explain the link

[33]We include youth BMI in the regression instead of obesity, because less than 5% of the respondents in our sample were obese during their teen years. Using teen height also does not affect our results.

[34]In unreported robustness tests, we also interact ethnicity, gender, and marital status. The results are quantitatively similar.

between obesity and delinquencies during adulthood, we restrict our sample to NLSY siblings and include sibling fixed effects in the regression. The fixed effects absorb variation in siblings' shared environments while growing up together.[35] The data reveal substantial variation in obesity outcomes even among siblings. Of the 6,995 respondents, 3,453 have siblings in our sample. Respondents with siblings originate from 1,499 unique households, 596 of which have at least one obese and one non-obese respondent. The results are displayed in column 7 of Table 3.5. The marginal effect of obesity on delinquencies remains large and significant (coefficient of 5.0 percentage points, standard error of 2.4 percentage points).[36]

3.5.3 Description of Robustness Tests Available in the Appendix

We conduct numerous tests to assess the robustness and generalizability of our benchmark result. For the interested reader, those results are reported in the Appendix. Here we only provide a synopsis.

In Section 3.A.1.1, we document robustness in three dimensions. First, our results extend to alternative measures of financial distress, namely bankruptcies and maxing out credit cards. Second, we show that credit risk tends to rise across the BMI categories, i.e., the result is not driven by our simplification of comparing delinquency rates between the obese and non-obese. Third, we obtain similar results utilizing the survey waves from before the financial crisis.

In Section 3.A.1.2, we take a different approach to comparing the obese and non-obese. We use the covariates from Sections 3.5.1 and 3.5.2 to obtain a propensity score of obesity, with which we make the two groups comparable along their observable characteristics (see Fig. 3.A.3 for a depiction of the comparability of the obese and

[35] After identifying eligible households, all household members born between 1957 and 1964 were asked to participate in the NLSY. About half of all NLSY respondents in our sample come from households with multiple respondents. We implement the sibling regression as a linear probability model with sibling fixed effects.

[36] There are some siblings with varying ethnicity. The obesity effect is insensitive to the inclusion or exclusion of ethnicity in the regression.

non-obese before and after propensity scoring). We find that propensity scoring further strengthens our estimate of the effect of obesity on delinquency.

Finally, in Section 3.A.1.3, we estimate the relationship between BMI and delinquency semiparametrically (all covariates are collapsed into a propensity score, which is treated parametrically). Fig. 3.A.4 plots the predicted probability of delinquency for an average individual at any given level of BMI. We find that the delinquency risk increases over most of the BMI range, but most drastically between BMIs of 30 and 37.

3.6 How Costly is the Obesity Effect on Delinquencies?

3.6.1 Comparing Obesity Risk to Trigger Events

Income or expenditure shocks that interfere with debt payments are called trigger events. The three key trigger events are job displacements, marriage dissolutions (due to separation, divorce, or death of a spouse), and health shocks. One way to assess the economic magnitude of obesity risk is to compare it to the marginal effects of known trigger events. Unlike obesity, a state variable, these trigger events represent shocks to respondents' economic and financial well-being.[37]

Table 3.6 presents the results. Each regression controls for the full set of credit risk characteristics used in the benchmark regression (column 5 in Table 3.3). Based on the estimates shown in column 4, those who become unemployed between 2004 and 2008 are 6.1 percentage points more likely to become delinquent than those who do not experience unemployment. Similarly, those whose marriages dissolve are 4.6 percentage points more likely to become delinquent. Disability increases the probability of delinquency by 6.5 percentage points. At face value, the relative impact

[37] We identify health shocks as the onset of job limitations in 2006 or 2008, i.e., we count only those cases in which the respondent did not have health limitations in 2004. To identify disability shocks, researchers typically use the stricter definition of the onset of a job limitation that lasts at least two consecutive survey waves. Our results are quantitatively similar if we employ the stricter definition.

of obesity on delinquency is 62% of the unemployment effect, 83% of the marriage loss effect, and 58% of the disability shock.

However, as indicated in column 4, obesity occurs more frequently in the population than the other three trigger events. In 2004, 27.7% of the population born between 1957 and 1964 was obese, but only 7.4% saw their marriage dissolve over the subsequent four years. After adjusting for the relevance of each credit risk in the population, the incidence of obesity on delinquencies ($= 0.038 \times 0.277 = 1.05\%$) is almost on par with that of unemployment (1.08%), triple the magnitude of the impact of marriage dissolutions (0.34%), and double the impact of disability (0.49%).[38]

3.6.2 Back-of-the-envelope Calculation of an Obesity Risk Premium

By how much would the obesity risk affect interest rates if it were priced? To answer this question, let us consider the following example, which features representative inputs as of 2004. Assume that lenders are risk-neutral, expect a rate of return of 6%, and incur losses of 20% in the event of a default. Let the probability of a default across all outstanding consumer loans be 5% and the amount of consumer debt owed by the obese be proportional to their prevalence in the population (30%).

First, we calculate the conditional probabilities of default for obese and non-obese borrowers by equating the default probabilities, weighted by their prevalence in the population, to the unconditional default rate:

$$0.3 \times \Pr(D|O) + 0.7 \times \Pr(D|\overline{O}) = 5\%. \tag{3.4}$$

[38] That is, we calculate the contribution of each trigger event to the overall delinquency rate in the population. Denote delinquency as D, obesity as O, and non-obesity as $\overline{O}$. Then $\Pr(D) = \Pr(D|O)\Pr(O) + \Pr(D|\overline{O})\Pr(\overline{O}) = \Pr(D|O)\Pr(O) + \Pr(D|\overline{O})\big(1 - \Pr(O)\big) = \Pr(D|\overline{O}) + \big[\Pr(D|O) - \Pr(D|\overline{O})\big]\Pr(O)$. Multiplying the estimated marginal effect of obesity $\Pr(D|O) - \Pr(D|\overline{O})$ by its prevalence $\Pr(O)$ yields the difference between the observed delinquency rate in the population and the rate that would prevail if all respondents were non-obese.

Assuming that the obese are 20% more likely to default than the non-obese (this is our best estimate based on their relative delinquency rates as predicted from the model in column 5 of Table 3.3), we find that $\Pr(D|\overline{O}) = 4.72\%$ and $\Pr(D|O) = 5.66\%$.

Second, for the lenders to earn an expected return of 6%, their required returns on loans that do not default (R_o for the obese and $R_{\overline{o}}$ for the non-obese) must offset the loss they incur on defaults:

$$(1 - 4.72\%) \times R_{\overline{o}} + 4.72\% \times (-20\%) = 6\% \tag{3.5}$$

$$(1 - 5.66\%) \times R_o + 5.66\% \times (-20\%) = 6\%. \tag{3.6}$$

Solving for R yields $R_{\overline{o}} = 7.29\%$ and $R_o = 7.56\%$, or an obesity risk premium of $7.56\% - 7.29\% = 0.27\%$.

While our data do not allow us to estimate the effect of obesity on delinquency for individual loan types, the following example illustrates the financial impact of a quarter point increase in the interest rate for a typical mortgage in 2004. On a 30-year fixed rate mortgage of $200,000 with a base rate of 6%, the risk premium would increase the monthly payments from $1,199 to $1,235 (or $432 annually). This amount is more than double the average difference in out-of-pocket medical expenditures between the obese and non-obese.

3.7 Cross-sectional Variation in the Informativeness of Obesity

How does the predictive power of observed obesity vary cross-sectionally? The answer to this question is of interest for many reasons, including substantiating our interpretation of the result, assessing its robustness across subsamples, and finetuning our understanding of when conditioning on obesity is most valuable.

At first glance, if obesity proxies for omitted socioeconomic factors that are related to delinquency (perhaps a lingering concern to some readers), then we would expect it to be most informative in the economic environment that gives rise to both obesity and delinquency (e.g., among those with low income and wealth).

On the other hand, there are several plausible reasons to expect obesity to be particularly informative when it is *least* likely. First, if obesity is indeed informative about the future as we posit in the preceding sections, then the information must also be revealed over time. In other words, higher risk translates into more frequent bad realizations over time, revealing itself to the lenders. Therefore, we expect the predictive power of obesity for future delinquencies to be lower for borrowers with poor credit histories. Second, obesity is more prevalent among individuals with lower incomes and wealth, which are correlated with the value of human capital and individuals' willingness to invest in it, e.g., in the form of health. Third, we expect obesity to be a more informative signal when it reflects borrowers' choices rather than their economic circumstances (e.g., budget constraints limit the choice set).

We explore the cross-sectional variation across a number of variables that capture the explanations above: the obesity propensity score, a credit risk score, race, and gender (see Table 3.7); and income, wealth, credit history, and debt types (secured vs. unsecured) (see Table 3.A.6 in the Appendix).

For more information on the obesity propensity score, see Section 3.A.1.2. The credit risk score comes from regressing 2004 delinquencies on 2000 covariates without an obesity indicator. The estimated coefficients are then multiplied by the 2004 values of the covariates to obtain a forward-looking assessment of credit risk (with the caveat that this credit risk score does not contain information on credit applications or past delinquencies, as those are not available in the 2000 survey). From both scores, we create quintile attributes and interact them with obesity. Additional information on the range of the scores and the prevalence of obesity within each quintile is provided in columns 2, 3, 5, and 6.

As shown in columns 1 and 4 of Panel A, obesity has the highest marginal effect on delinquency when obesity is least prevalent and when credit risk is lowest, and the obesity effect is statistically different from zero only in the lowest two quintiles for both scores. The magnitude of the heterogeneity is quite large: the difference in marginal effects between the top and bottom quintiles is $8.0 - 1.8 = 6.2$ percentage points for the obesity propensity score and $8.7 - 3.3 = 5.4$ percentage points for the credit risk score. The relative magnitude — compared to the average delinquency rate in each quintile — even larger. For example, the 2008 delinquency rate in the bottom quintile of the obesity propensity score is just 9.4%, whereas it is 31.7% in the top quintile. Also, as shown in the Appendix, the obesity effect is strongest among the top 40% of the income distribution and the top 20% of the wealth distribution. Among respondents without prior bankruptcies, delinquencies, and credit denials, the obese are 4.5 percentage points more likely to become delinquent. Among those with damaged credit history, the marginal effect of obesity is only 2.7 percentage points (not statistically different from zero).[39]

It is also of interest whether the obesity effect holds across various demographic groups. Controlling for gender and ethnicity in Table 3.5 only ensures that the obesity effect is not driven by differences in obesity and delinquency across gender and race. Here we investigate whether the intensity of the obesity effect varies across these demographic groups. To this end, we stratify the sample and run separate regressions for each subpopulation. The results are shown in Panel B. The obesity effect is evident among Hispanic and White and female and male respondents, but not among Blacks. One potential reason for not finding an obesity effect among Blacks include greater measurement error in BMI (Burkhauser and Cawley (2008)).

[39]To account for the uncertainty in estimated — rather than observed — propensity scores, we obtain bootstrapped estimates of the standard errors on the obesity coefficients shown in column 1 of Tables 3.7 and 3.A.5. Based on 100 replications, they are almost identical to those reported.

3.8 What Are the Mechanisms Through Which Obesity Affects Delinquencies?

3.8.1 Health Costs

Previous Evidence

The first plausible explanation for a link between obesity and delinquency is that the obese tend to incur higher health-related costs than the non-obese, and these cash flow shocks impede borrowers' ability to meet their debt obligations. Being a leading health risk factor, obesity is associated with increased risks of heart disease, diabetes, cancer, breathing problems, arthritis, depression, premature death, and many other health consequences.[40]

Finkelstein, Trogdon, Cohen and Dietz (2009) estimate that medical per-adult-capita spending is $1,429 (or 41.5%) higher for the obese than non-obese. In an earlier study, Finkelstein, Fiebelkorn and Wang (2003) found that about 14% of total medical spending attributable to obesity came out-of-pocket. Assuming that the out-of-pocket contribution has not changed between 1998 and 2006, the additional out-of-pocket medical spending due to obesity amounts to approximately $200 per adult capita. However, the incidence of medical spending is highly skewed, with a large fraction of the population incurring zero medical expenditures in a given year. We expect that the difference in average medical expenditures between the obese and non-obese also translates into a differential probability of experiencing a significant cash-flow shock relative to income or wealth.

Health-related costs do not only come in the form of higher out-of-pocket medical expenditures. Obesity-related health issues can jeopardize job security and affect loan delinquencies through the loss of income. Numerous studies have shown that

[40] Further information available at ⟨`http://www.surgeongeneral.gov/topics/obesity`⟩. See DiNardo et al. (2010) for a contrarian view.

the obese miss more days at work due to illness or injury, or that their productivity is lower because they do go to work ill or injured.[41] For example, Tucker and Friedman (1998) find in a sample of over 10,000 employees in the U.S. that the obese are 74% more likely to experience high levels of absenteeism than their normal weight weight coworkers. We conjecture that a wage-growth penalty or higher likelihood of job separation for the obese resulting from poorer job performance would adversely affect their ability to meet their debt obligations in the future.[42]

Himmelstein, Thorne, Warren and Woolhandler (2009)'s findings attest to the importance of health for financial outcomes. In 2007, they surveyed a random sample of bankruptcy filers in the U.S. about the causes of their financial difficulties. They find that income shortfalls or medical bills due to illness contributed to 62.1% of all bankruptcies. Most of the debtors were well-educated and middle class. The average out-of-pocket medical expense of the bankrupt was $18,000. Common diagnoses included multiple sclerosis, diabetes, injuries, stroke, mental illnesses, and heart disease, all of which are more prevalent and cause more complications among the obese.

Results

Ideally, we would like to observe health shocks or medical expenditures incurred by the households. We could then test whether the additional delinquencies among the obese are due to the realization of such adverse events. Unfortunately, the NLSY does not contain good information on health shocks.[43] Our approach to assess the extent to which the effect of obesity on delinquencies is potentially channeled through

[41] See Hammond and Levine (2010) and Wolf (2002) for a more detailed and comprehensive review of the evidence.

[42] There is also evidence of a wage-penalty for the obese (e.g., see Cawley (2004) and Han, Norton and Stearns (2009)). While lower wages decrease a borrower's potential to absorb spending shocks, the lower wages are observed at the time the loans are made. Therefore, we would not expect obesity to provide additional information about future delinquency risk after controlling for income.

[43] The only available measure is based on job limitations, which we have already discussed in Section 3.6. After controlling for credit risk factors, obesity weakly predicts the onset of job limitations in 2006 and 2008 (obesity is a stronger predictor of existing job limitations). Predicted probabilities of new job limitations have no effect on delinquencies.

health costs follows Persico et al. (2004). Paraphrasing them, to the extent that health outcomes reflect individual choices, a decrease in the coefficient on obesity would suggest that part of the effect of obesity on delinquency is channeled through health. We introduce several health outcome measures to the benchmark credit risk model and test whether the obesity-delinquency effect is sensitive to the inclusion of the health measures.

Our primary measure of health is a self-reported assessment of the respondents' health after they turn 40 years old. In 2004, respondents' ages range from 39 to 48. That is, we have data on most respondents' health status, although for some it is somewhat dated.[44] Table 3.8, column 1, shows the results. Including respondents' self-assessed health reduces the marginal effect of obesity on the delinquency rate by an economically meaningful amount, from 3.8 percentage points in the benchmark model to 2.6 percentage points, or by -31.6% (the difference in the obesity effect with and without controlling for health is statistically significant at a p-value of less than 0.1%). In other words, about one third of the obesity effect on delinquencies appears to be channeled through poor health. The NLSY further provides physical component summary scores, which summarize NLSY participants' responses to 12 questions on their physical health (the scores are highly correlated with self-assessed health). Unreported results show that being in the bottom quintile of physical health is associated with a significantly higher incidence of delinquencies when compared to the credit risk of borrowers in the upper 80% of the distribution. The inclusion of physical health scores also lowers the marginal effect of obesity to 3.0 percentage points (a decrease of 21.1%).[45]

[44] In unreported specifications, we attempt to account for this potential problem by controlling for the number of years since the health assessment. We also conduct the analysis with the pre-financial crisis data, for which we lack some other controls. The impact of controlling for self-reported health on the marginal effect of obesity remains quantitatively similar.

[45] The NLSY data also contain a long list of illnesses and mental health scores. Including indicator variables for the various health conditions yields results similar to those of the physical component score; while mental health affects credit risk, it does not impact the obesity effect.

We conclude that the effect of obesity on delinquencies is at least partially mediated through health outcomes. Since the onset of job limitations does not appear to explain the effect of obesity on delinquency, we conjecture that the obesity effect is primarily channeled through higher expenditures rather than income shocks and/or that the obesity effect reflects longer-term conditions rather than temporary shocks. The nature of the relationship between health outcomes, obesity, and delinquencies raises further issues. If one subscribes to the view that weight is a choice variable, then conditioning loan decisions on obesity would force the obese to internalize the costs of their caloric intake choices. On the other hand, if one believes that weight is not a choice variable, then obesity would merely proxy for health outcomes. In this case, conditioning loan decisions on obesity would penalize borrowers for non-preventable poor health outcomes. The matter is further complicated by the role of external factors that affect whether weight is a choice variable (e.g., community characteristics and budget constraints can effectively eliminate choices).

3.8.2 Time Preferences

Previous Evidence

The second plausible explanation for a link between obesity and delinquency is that obesity reflects time- or risk-preferences. Because obesity is the result of excess caloric intake over an extended period of time, it can contain valuable information about individuals' underlying preferences or characteristics underlying their choices.

Courtemanche and McAlvanah (2011) provide an in-depth assessment of the link between time preferences and BMI. Using NLSY79 data, they find that both rational intertemporal tradeoffs and time inconsistency play a role in weight gain. Other studies provide similar findings. For example, Ikeda, Kang and Ohtake (2010) also document a positive association between BMI and impatience and hyperbolic discounting in a survey of Japanese adults; Scharff (2009) shows that obese dieters display behavior consistent with hyperbolic discounting in the Continuing Survey of Food Intakes by Individuals; and in calibrating a model of caloric intake, Buttet and Dolar (2010) also find that commitment problems play an important role in weight gain.[46,47]

Results

We are aware of three measures of impatience obtained from NLSY data. DellaVigna and Paserman (2005) use factor analysis to construct an aggregate measure of impatience from seven behaviors that indicate impatience.[48] Cadena and Keys (2010) identify impatience from NLSY interviewers' assessments of the respondent as impatient or restless in any of the first five surveys.[49] These two studies link impatience measures from respondents' early surveys to subsequent labor market outcomes (job search and formation of human capital). Courtemanche and McAlvanah (2011), on the other hand, obtain estimates about respondents' underlying time preferences parameters from survey questions about the time value of money and show that these measures of impatience are reflected in respondents' BMIs after controlling for a wide range of economic and demographic factors.

Our primary measure is based on Courtemanche and McAlvanah, because it is most strongly related to BMI/obesity.[50] In 2006, NLSY participants were asked the

[46] For further references as well as evidence on the time-stability of intertemporal preferences of individuals, see citations in Courtemanche and McAlvanah (2011).

[47] In the 2002 Swiss Health Survey, Stutzer (2006) documents that obesity decreases well-being only for those respondents who report limited self-control and concludes that obesity reflects sub-optimal choices. His interpretation is supported by the observation that individuals spend considerable resources trying to lower or maintain their weight. According to Cummings (2003), annual spending on diet products by U.S. consumers alone is estimated to be $40 to $100 billion (or about $200 to $500 per adult capita).

[48] The indicators are labeled NLSY assessment of impatience, having a bank account, use of contraceptives, insurance, health habits (smoking and drinking), and vocational clubs in high school.

[49] It is not clear to us why Cadena and Keys did not incorporate the other indicators from DellaVigna and Paserman. To the best of our knowledge, there exists no comparison between the various impatience measures.

[50] We present results only for the ex-post best measure, and even it fails to absorb any of the obesity effect on delinquency. However, we implement and test all of the previously used measures, variants thereof, as well as our own measures of impatience based on the hypothetical time- and

following question:

> "Suppose you have won a prize of $1,000, which you can claim immediately. However, you can choose to wait one month to claim the prize. If you do wait, you will receive more than $1,000. What is the smallest amount of money in addition to the $1,000 you would have to receive one month from now to convince you to wait rather than claim the prize now?"

The question was then repeated using a one year wait time. As Courtemanche and McAlvanah explain in detail, the implicit annualized discount factors should be equal for time-consistent individuals. Yet, most respondents reveal a greater discount factor for the one-year delay than for the one-month delay, which is consistent with present-biased preferences. Assuming that individuals discount payoffs τ periods in the future by $\beta\delta^{\tau}$, the authors back out estimates of β and δ for each respondent. δ reflects long-run impatience and β captures myopia (lower value correspond to higher discounting and more impatience). We winsorize the β and δ estimates at the top and bottom percentile. Our sample moments closely match those of Courtemanche and McAlvanah.[51] We convert the estimates of β and δ from discount factors into discount rates to match the interpretation of the other measures of impatience (where higher values correspond to more impatience) and because the discount rates are more strongly related to BMI/obesity. The β- and δ-discount rates have means and standard deviations of 0.517 and 0.956 and interquartile ranges of 0.678 and 0.920.[52]

Table 3.8 presents the results for the impatience channel. In columns 2 and 3, we regress obesity on the β- andδ-discount rates (first without controls, then with controls). Similar to Courtemanche and McAlvanah, excess weight reflects both greater

risk-preference questions in the 2006 survey. Almost all measures predict delinquency, but few are positively correlated with obesity (DellaVigna and Paserman's and Cadena and Keys' impatience measures are even negatively correlated with obesity after controlling for credit risk factors). The results are available from the authors upon request.

[51] In our sample, the relationship between β and δ and BMI is weaker.

[52] A potential objection to using information from these 2006 survey questions is reverse causality. Respondents in financial distress are more likely to place greater value on receiving money sooner rather than later. However, the results from alternative impatience measures that pre-date our sample by 20 years yield qualitatively similar estimates without impacting the marginal effect of obesity.

short-term and long-term impatience. The magnitude of the effects is quite modest — the time preference parameters explain only 0.3% of the variation in obesity, and moving from the 25th to the 75th percentile in the discount rate distributions increases the likelihood of being obese by only 2 and 3 percentage points.[53] The majority of the myopia effect on obesity is absorbed by the credit risk factors.

Since about 8.5% of our sample does not have information on discount rates, we re-estimate the benchmark credit risk model for this subsample (column 4). The marginal effect of obesity on delinquency is 4.2 percentage points, moderately higher than for the full sample. Including the β- and δ-discount rates only trivially lowers the obesity effect. Based on the many failed attempts to have measures of impatience absorb some of the obesity effect, we conclude that obesity does not proxy for time-preferences. Interestingly, impatience itself is predictive of delinquencies, above and beyond what is revealed through other economic outcomes.

3.9 Conclusion

3.9.1 Summary of Findings

We estimate that loan delinquency rates among the obese are 20% higher than among the non-obese after controlling for differences in income and wealth; debt-to-income and debt-to-asset ratios; prior bankruptcies, delinquencies, and credit decisions; income risk and labor market indicators. The results are not driven by race, gender, marital status, age, human capital, or youth experiences. We document that despite its power to predict delinquencies, obesity does not help explain credit decisions. This observations suggests that obesity does not merely proxy for omitted credit-risk-relevant variables. Our finding is robust to alternative measures of financial distress and holds in other time periods. Delinquency risk increases over most

[53] Also, the relationship appears to be non-monotonic, but ultimately none of our further attempts of estimating the BMI/obesity-impatience relationship yielded significant impact on the effect of obesity on delinquency.

of the BMI range, but especially between BMIs of 30 and 37. The economic significance of obesity for delinquencies is comparable to that of job displacements, and it is particularly informative among households with low credit risk. We identify poor health as a channel through which obesity is linked to delinquencies. Our findings are consistent with the view that obesity-linked medical expenses or loss of income impede borrowers' ability to meet their financial obligations. However, we find no support for the view that the obesity effect captures borrowers' time preferences.

3.9.2 Discussion

While our findings are intriguing, they must be interpreted with care. Our results are indicative of cross-subsidization in the credit market. Average risk pricing in the credit market amounts to a wealth transfer from the non-obese to the obese. However, we cannot speak to the role of obesity in the allocative efficiency of the credit market. Also, our study is mute about whether BMIs would drop in response to an increase in the cost of being obese born by the obese (e.g., due to actuarial pricing of credit risk). At best, the cross-subsidization constitutes an unintended redistribution of wealth, and possibly an inefficiency in the allocation of capital and personal investment in health. As such, our results should inform the discussions on the desirability of providing legal or regulatory protection to the obese.

We would like to emphasize that we do not claim causality from excess weight to credit risk. Beside the obvious academic distinction, there are important implications for how to think about obesity. Our study does not provide justification for any diet regimens, diet drugs, or surgical weight loss. It is compatible with a number of studies and books that argue that it is not fatness per se, but the obsession with or the underlying factors leading to it, that are harmful (e.g., see Gaesser (1996), Campos (2004), and Oliver (2005)). Also, we do not advocate taste-based discrimination. We do not argue that the obese should pay more for their loans because of their appearance per se, but per se, but point to their higher credit risk on economic grounds.

Regulations in Credit, Labor, and Insurance Markets

In the context of credit markets, U.S. federal regulations restrict the use of medical information in the provision of credit. At this point, however, it is not clear whether obesity would be considered a medical condition under Regulation FF. Even without explicit legal restrictions on the actuarialization of obesity, it often conflicts with the protection of other protected groups under antidiscrimination law. For example, the Equal Credit Opportunity Act (ECOA) prohibits lenders to discriminate against credit applicants with protected personal or demographic characteristics. The prohibition is far reaching, from discouraging applications to differential loan pricing. Under the Federal Reserve's Regulation B, which implements ECOA, protected characteristics include race, ethnicity, gender, marital status, religion and to a limited extent age. For credit score models to comply with antidiscrimination rules, the characteristics included in the model must have sufficient business rationale, yet not disproportionately affect protected populations.[54]

Insurance markets have a longer history of dealing with the issue of obesity, and due to their actuarial nature provide a good comparison for credit markets. While most states prohibit race-based discrimination for medical insurance underwriting, none restricts the use of weight. However, when insurance is offered as an employment benefit, Title VII of the Civil Rights Act mandates equal treatment of all employees. This creates incentives for companies to avoid hiring employees with high health risks. However, rules do change, as evidenced by the changes to the Health Insurance Portability and Accountability Act (HIPAA) in 2006, which opened the door to differential pricing between the obese and non-obese in employers' group health plans.

[54] While our results are not driven by the disproportionate prevalence of obesity among Blacks, the use of obesity in credit modeling may still have a disparate impact on minorities.

Based on the increasing use of weight in the insurance market and its similarities with the credit market, it appears likely that its use would also be permitted in credit markets.

Obesity vs. Race

The conditional correlation between obesity and delinquency that we document here mirrors the relationship between race and delinquency (e.g., as seen in Table 3.5, Martin and Hill (2000), and Charles, Hurst and Stephens (2008)). In both cases, the empirical results establish statistical stereotypes that are based on the logic of actuarial personhood (Kirkland (2008)). The predictions do not apply to any specific person in the population, but to a hypothetic average member of a group.

As Kirkland puts it, the question then is "Whose personhood is subject to actuarialism in the law and whose gets protected from it?" Despite the similarity between race and obesity in predicting credit risk, they are treated differently under U.S. law. To argue that fatness is like race one would have to establish that it

> "has been subject to invidious discrimination, that it is an immutable trait [...], that it causes isolation and exclusion of a group of people from public life and opportunity (as segregation practices did), and that fat people as a group are marked by their fatness in a way that is tempting for government to use to subordinate them [...] As long as being fat is like being a smoker, it will never be like being black" Kirkland (2008).

In addition, antidiscrimination law struggles with how to treat actuarial personhood of obesity due to the absence of a moral underpinning in the classification of people into risk attributes (i.e., discrimination is based on presumably true facts, not on distaste for obesity).

3.9.3 Open Questions

We establish a robust relationship between obesity and credit risk using NLSY data, but limited data quality and availability put a cap on the confidence we can place on the generalizability of this finding. Follow-up research could conduct an out-of-sample evaluation of our empirical model using the Panel Survey of Income Dynamics (PSID) or similar international data. One could also combine detailed loan level data from LPS Applied Analytics with obesity prevalence estimates at the county or ZIP code level based on survey responses from the Behavioral Risk Factor Surveillance System (BRFSS) of the Centers of Disease Control and Prevention (CDC). Furthermore, it would be interesting to expand on whether obesity is being priced (which would suggest that obesity merely captures other observable characteristics).

While our results point to health shocks as the main channel from obesity to delinquency risk, they are far from conclusive. Yet, the distinction between the channels — revelation of borrower behavior vs. future health shocks — is very important. On the one hand, if obesity is informative about the inherent type of the borrower, then an increase in the overall rate of obesity in the population would have no effect on total defaults. On the other hand, if obesity is informative about future cash flow risk, then its rising prevalence should coincide with a rise in credit defaults over time.

Fig. 3.3 depicts the comovement between obesity prevalence and personal bankruptcy rates and is, at first glance, consistent with our interpretation that the obesity effect is at least partially mediated through health shocks.[55] Also note that the bankruptcy rate for businesses, which are not directly affected by the rise in obesity, trends downward over the available sample period. It would be interesting to quantify the extent to which the rise in defaults and bankruptcies over the last decades can be attributed to obesity. However, a detailed examination of the long-term time trends are beyond the scope of this paper, and would need to account for various contemporaneous changes in the U.S. economy over that time period, especially the increase in household indebtedness.

[55]The sharp decline in personal bankruptcy rates in 2005 coincides with the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA).

Given that body weight provides information that is relevant for economic outcomes in the labor and credit markets, it is of great interest to explore its significance in other settings. Note that obesity or weight gain need not be negative indicators in all settings, and the outcomes need not reflect the same channels to which we have appealed here. Negative stress could cause weight gain or loss as people respond differently (in which case it is the magnitude of the change in BMI that would be relevant), and obesity may signal the amount of effort devoted to and focus on a task rather than oneself. We recognize the difficulty of establishing causality in this line of inquiry, but — in light of its importance — hope that future research will take on this challenge.

Figure 3.1: Distribution and Categorization of BMI

We compute the body mass index (BMI) from NLSY respondents' self-reported height and weights. As our measure of adult height, we use the average of the heights reported in 1985, 2006, and 2008. The classification of BMIs into under/normal/overweight and obese categories reflects 2004 WHO standards. Data source: NLSY79.

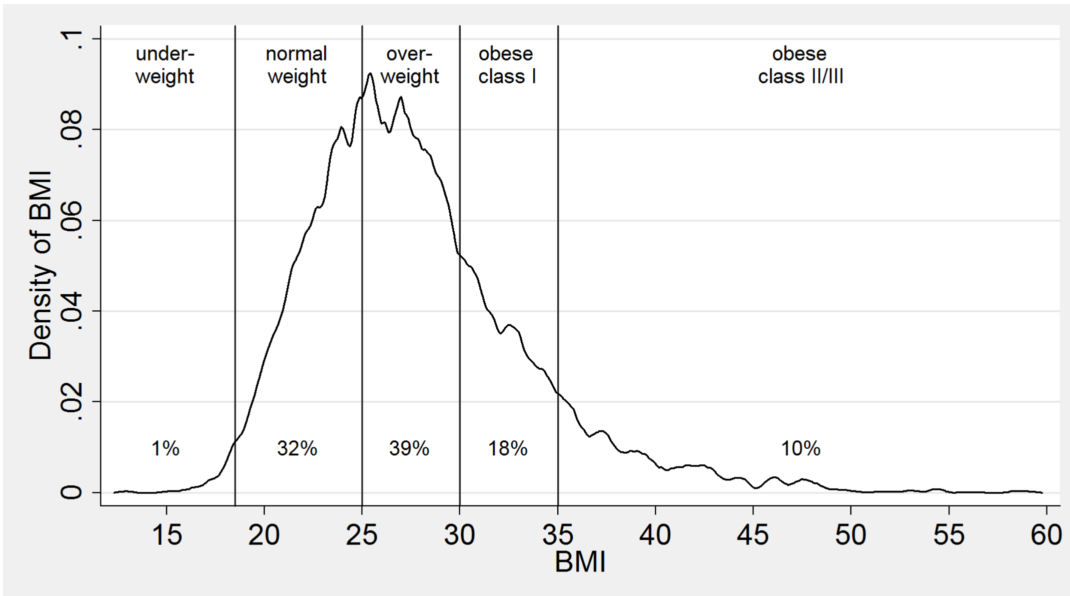


Figure 3.2: Delinquency Rates Across BMI Categories
This graph displays the delinquency rate across the BMI categories (brackets denote the 95% confidence interval). Delinquency is defined as having completely missed a payment or having been late by at least 2 months on any bill over the last 5 years. Data source: NLSY79.

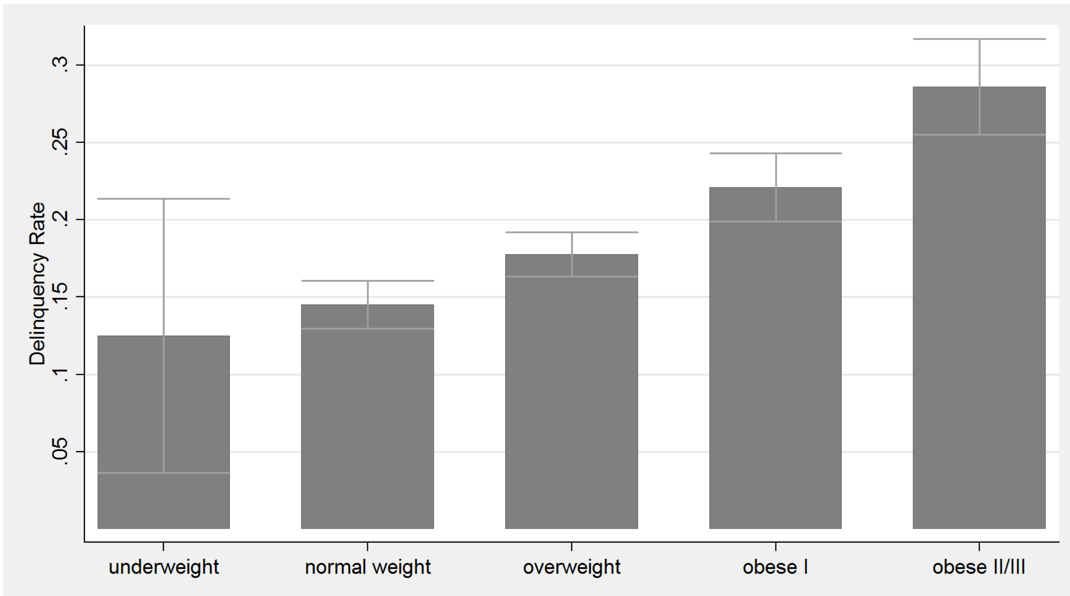


Figure 3.3: National Obesity and Bankruptcy Filing Trends

This graph plots long-term trends in the prevalence of obesity among adults and bankruptcy rates in the U.S. Obesity prevalence is based on NHES/NHANES estimates available through the World Health Organization's Global Database on Body Mass Index. We calculate personal bankruptcy rates as the number of bankruptcies per 1,000 households and business bankruptcy rates as the number of bankruptcies per 1,000 employers. The drop in personal bankruptcy filings in 2005 coincides with the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA). Data sources: WHO, ABI, US Census, SBA.

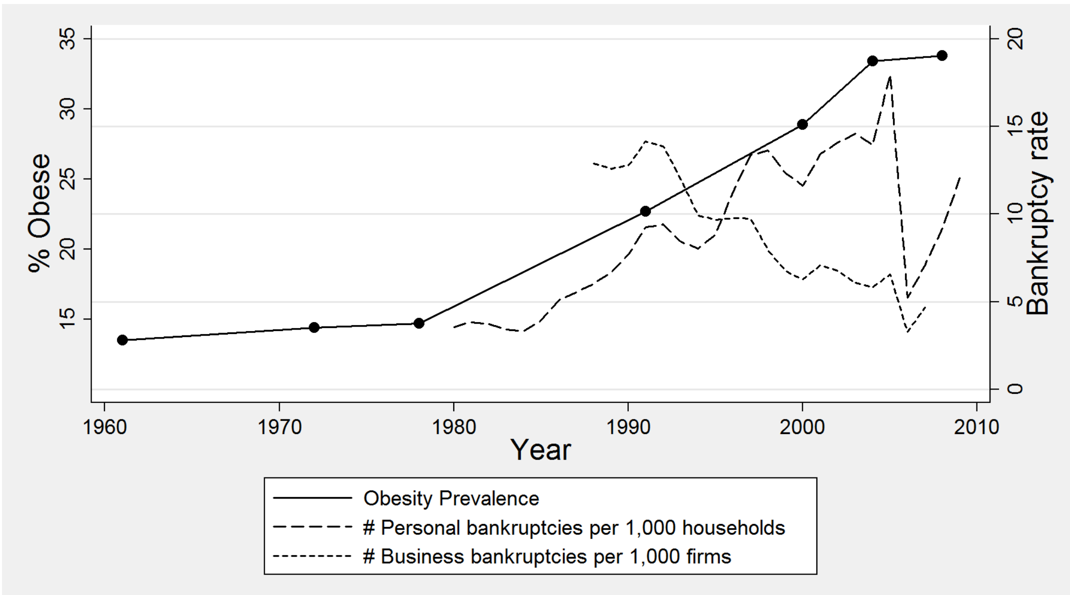


Table 3.1: Delinquencies Are an Indicator of Serious Financial Distress

What do delinquencies measure: financial distress or forgotten bills? This table shows that bankruptcy risk is about four times as high when households are delinquent than when they are not. In other words, delinquencies are an indicator of serious financial distress.

	Bankruptcy rate	
Survey year	2004	2008
	(1)	(2)
Non-delinquent	2.51	2.67
Delinquent	11.78	9.17
Population average	4.20	3.87

Table 3.2: Summary Statistics, By Obesity

This table captures that the obese and non-obese systematically differ in their observable credit-risk relevant, socioeconomic, and demographic characteristics. The top and bottom 1 percent of debt-to-income and debt-to-asset ratios have been winsorized. Poor credit history encompasses prior bankruptcy filings, delinquencies, and rejected applications for credit. *** denote statistically significant differences in means between the obese and non-obese at the 1% significance level. Observations are weighted using NLSY 2004 sampling weights.

	Non-obese			Obese			
	mean	st dev	obs	mean	st dev	obs	Δ
Measures of credit risk as of 2008							
Delinquent	0.162	0.369	4,803	0.244	0.429	2,192	***
Bankrupt	0.032	0.177	4,794	0.054	0.228	2,189	***
Maxed out credit card	0.082	0.274	4,752	0.127	0.333	2,169	***
Credit-risk relevant characteristics as of 2004							
Income	87,975	86,138	4,197	65,651	53,070	1,932	***
Wealth	305,000	546,000	4,695	170,000	337,000	2,148	***
Debt/income ratio	1.519	2.106	4,075	1.406	2.057	1,896	
Debt/asset ratio	0.481	1.072	4,417	0.651	1.491	2,025	***
Poor credit history	0.291	0.454	4,761	0.407	0.491	2,182	***
Income instability	0.395	0.330	4,555	0.364	0.303	2,094	***
> High school educ	0.318	0.466	4,752	0.205	0.404	2,168	***
Self-employed	0.104	0.306	4,803	0.076	0.266	2,192	***
Job tenure	7.743	6.854	4,149	7.939	6.953	1,856	
Unemployed	0.128	0.335	4,546	0.127	0.333	2,056	
Out of labor force	0.274	0.446	4,591	0.270	0.444	2,091	
Prohibited/unavailable characteristics as of 2004							
White	0.821	0.383	4,803	0.721	0.449	2,192	***
Male	0.513	0.500	4,803	0.514	0.500	2,192	
AFQT ranking	0.605	0.275	4,606	0.542	0.287	2,113	***
Age	43.301	2.337	4,803	43.381	2.255	2,192	

Table 3.3: Marginal Effect of Obesity on Delinquency After Controlling for Credit-Risk-Relevant Variables that Are Observable and Permissible

The table displays marginal effects of obesity on subsequent delinquency, estimated from credit risk probit models. All explanatory variables are from the 2004 survey. For a flexible specification, we create quintile attributes from continuous characteristics. The bottom quintiles are the omitted categories, so the displayed marginal effects are to be interpreted relative to the omitted category. Tables 3.A.1, 3.A.2, and 3.A.3 provide estimated marginal effects of the control variables and show how the marginal effect of obesity changes as we add characteristics one at a time. Heteroskedasticity robust standard errors, clustered by residence typology (region and urban/rural), and accounting for the complex survey design, are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% significance levels. Observations are weighted using NLSY 2004 sampling weights.

	No controls (1)	Income & assets (2)	Credit history (3)	Income risk (4)	All controls (5)
Obese	0.081***	0.051***	0.047***	0.074***	0.038***
	(0.013)	(0.016)	(0.012)	(0.013)	(0.012)
Income	no	**yes**	no	no	**yes**
Wealth	no	**yes**	no	no	**yes**
Debt/income	no	**yes**	no	no	**yes**
Debt/assets	no	**yes**	no	no	**yes**
Delinquent	no	no	**yes**	no	**yes**
Bankrupt	no	no	**yes**	no	**yes**
Recent credit decision	no	no	**yes**	no	**yes**
Income instability	no	no	no	**yes**	**yes**
Education	no	no	no	**yes**	**yes**
Job tenure	no	no	no	**yes**	**yes**
Self-employed	no	no	no	**yes**	**yes**
Unemployed	no	no	no	**yes**	**yes**
Out of labor force	no	no	no	**yes**	**yes**
Industry dummies	no	no	no	**yes**	**yes**
Occupation dummies	no	no	no	**yes**	**yes**
# of observations	6,995	6,995	6,995	6,995	6,995
Pseudo-R^2	0.009	0.075	0.119	0.054	0.162

Table 3.4: Obesity Does Not Enter Credit Decisions, But Predicts Delinquencies

In this table we investigate whether obesity captures information that is available to lenders, but unobservable to us (the researchers). To this end, we constrain the sample to those respondents who in 2004 said that they had applied for credit. On this subsample, we regress the binary variable *Denied* (equals one if credit was denied) on all of the observable and permissible covariates of Table 3.3 column 5 (excluding the credit application indicator). The estimated marginal effect of obesity on the credit decision is displayed in column 1. Then we regress *Delinquent* on obesity and the controls included in column 1. Column 2 provides the baseline estimate for the effect of obesity on delinquency without controlling for lenders' information. We add *Denied* as a control in column 3, and we condition on credit not having been denied in column 4. Heteroskedasticity robust standard errors, clustered by residence typology (region and urban/rural), and accounting for the complex survey design, are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% significance levels. Observations are weighted using NLSY 2004 sampling weights.

Dependent var:	Credit denied	Delinquent		
		Baseline	Control for Denied	Condition on Denied=0
	(1)	(2)	(3)	(4)
Obese	0.025	0.048***	0.046***	0.048***
	(0.020)	(0.016)	(0.016)	(0.017)
Denied			0.087***	
			(0.016)	
Benchmark controls	yes	yes	yes	yes
# of obs	3,007	3,007	3,007	2,241
Pseudo-R^2	0.259	0.191	0.200	0.163

Table 3.5: Obesity Is Not Just a Proxy for Race, Gender, Marital Status, Cognitive Ability, or Parental Influence

The table displays marginal effects of obesity on subsequent delinquency, estimated from credit risk probit models. In columns 1–5, we successively introduce new control variables that are potentially correlated with obesity and credit risk, but are either prohibited by ECOA (ethnicity, gender, and marital status) or are unobservable to the lender (cognitive abilities, as measured by the age-adjusted AFQT-score, and youth BMI in 1981). The explanatory variables are from the 2004 survey (except for the AFQT-score and youth BMI). The excluded base categories are Whites, males, the married, and the bottom quintiles of ability and youth BMI. In column 6, we combine all of the new controls. In column 7, we estimate a linear probability model (OLS) with sibling fixed effects (the sample being restricted to respondents with siblings in the NLSY; there are some siblings with varying ethnicity). Every specification also includes the full set of attributes from Table 3.3, column 5 (coefficients not reported to save space). Heteroskedasticity robust standard errors, clustered by residence typology (region and urban/rural), and accounting for the complex survey design, are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% significance levels. Observations are weighted using NLSY 2004 sampling weights.

	Ethnicity (1)	Gender (2)	Marital status (3)	Cognit ability (4)	Youth BMI (5)	All controls (6)	Sibling FEs (7)
Obese	0.035***	0.038***	0.038***	0.037***	0.038***	0.034***	0.050*
	(0.012)	(0.012)	(0.012)	(0.012)	(0.011)	(0.011)	(0.024)
Hispanic	0.025					0.025	-0.096
	(0.017)					(0.019)	(0.164)
Black	0.041***					0.039***	-0.336*
	(0.010)					(0.010)	(0.184)
Female		0.026*				0.027*	0.020
		(0.015)				(0.016)	(0.019)
Never married			0.013			0.009	-0.006
			(0.016)			(0.015)	(0.021)
Separated, divorced,			0.014			0.012	-0.009
widowed			(0.011)			(0.011)	(0.026)
AFQT Q2				-0.012		-0.004	0.000
				(0.016)		(0.016)	(0.036)
AFQT Q3				-0.008		0.004	0.018
				(0.014)		(0.015)	(0.033)
AFQT Q4				-0.025		-0.010	-0.009
				(0.021)		(0.022)	(0.035)
AFQT Q5				-0.030**		-0.010	0.029
				(0.013)		(0.016)	(0.049)
Youth BMI Q2					0.047***	0.050***	0.043
					(0.011)	(0.011)	(0.028)
Youth BMI Q3					0.006	0.010	-0.028*
					(0.015)	(0.015)	(0.014)
Youth BMI Q4					0.005	0.011	-0.010
					(0.016)	(0.018)	(0.029)
Youth BMI Q5					0.020	0.027	-0.026
					(0.016)	(0.017)	(0.030)
Benchmark controls	yes	yes	yes	yes	yes	yes	yes
# of observations	6,995	6,995	6,995	6,995	6,995	6,995	3,453
Pseudo-R^2	0.164	0.163	0.162	0.163	0.164	0.168	0.154

Table 3.6: Comparing Obesity Risk to the Impact of Trigger Events on Delinquencies

The table displays the marginal effects of obesity and various trigger events on subsequent delinquency rates. In all specifications (columns 1–4), we include the set of controls used in column 5 of Table 3.3. *Unemployment*, *Marital dissolution*, and *Disability* represent negative shocks to the household. They come from the 2006 and 2008 surveys. *Unemployment* and *Marital dissolution* equal one if a respondent experiences the conditions between 2004 and 2008, and zero otherwise. *Disability* only equals one if respondents are not disabled in 2004 *and* claim disability in the 2006 or 2008 surveys. Column 4 displays the relative frequency of occurrence of obesity, unemployment, and marital dissolutions in the population for which the NLSY is representative. Heteroskedasticity robust standard errors, clustered by residence typology (region and urban/rural), and accounting for the complex survey design, are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% significance levels. Observations are weighted using NLSY 2004 sampling weights.

	Estimates				Occurrence rate
	(1)	(2)	(3)	(4)	(5)
Obese	0.039***	0.038***	0.037***	0.038***	27.7%
	(0.012)	(0.012)	(0.012)	(0.012)	
Unemployment	0.065***			0.061***	17.7%
	(0.016)			(0.016)	
Marital dissolution		0.052***		0.046**	7.4%
		(0.020)		(0.021)	
Disability			0.073***	0.065***	7.5%
			(0.021)	(0.021)	
Benchmark controls	yes	yes	yes	yes	
# of observations	6,995	6,995	6,995	6,995	
Pseudo-R^2	0.167	0.164	0.165	0.171	

Table 3.7: Cross-sectional Heterogeneity in the Informativeness of Obesity

The table displays estimates of the marginal effect of obesity on delinquency across the quintile attributes of the obesity propensity score and the credit risk score (Panel A), and across race and gender (Panel B). The obesity propensity score are the predicted values obtained from regressing obesity on all the attributes in Table 3.3. The credit risk score is the predicted delinquency risk (combining estimated coefficients from the regression of 2004 delinquencies on 2000 covariates [excluding obesity] with 2004 covariates). Note that for the credit risk score, data on credit applications and prior delinquencies are not available in 2000, which restricts the credit history controls to just prior bankruptcies. In all specifications, quintile attributes of the obesity propensity score are included. *Range w/in Q* provides the minimum and maximum value of the interacted variable within that particular quintile. *% obese* shows the fraction of the population in each quintile that is obese. Heteroskedasticity robust standard errors, clustered by residence typology (region and urban/rural), and accounting for the complex survey design, are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% significance levels. Observations are weighted using NLSY 2004 sampling weights.

Panel A: Interactions with Credit Risk Factors

	Average marginal effect of obesity over the quintiles of ...					
	Obesity propensity score			Credit risk score		
	marginal effect	range w/in Q	% obese w/in Q	marginal effect	range w/in Q	% obese w/in Q
	(1)	(2)	(3)	(4)	(5)	(6)
Obese						
× Q1	0.080**	0.010–0.174	12.9	0.087**	0.130–0.191	19.7
	(0.036)			(0.038)		
× Q2	0.060**	0.174–0.242	21.4	0.062***	0.192–0.240	30.0
	(0.026)			(0.021)		
× Q3	0.046	0.242–0.299	25.8	0.025	0.241–0.246	32.7
	(0.029)			(0.017)		
× Q4	0.033	0.299–0.370	34.0	0.018	0.246–0.285	28.4
	(0.036)			(0.023)		
× Q5	0.018	0.370–0.732	44.2	0.033	0.285–0.453	27.5
	(0.028)			(0.036)		
BM controls	yes			yes		
# of obs	6,995			6,995		
Pseudo-R^2	0.046			0.076		

Panel B: Effect of Obesity by Race and Gender

	Race			Gender	
	Hispanic	Black	White	Male	Female
	(1)	(2)	(3)	(4)	(5)
Obese	0.048*	0.006	0.043**	0.034***	0.049**
	(0.027)	(0.014)	(0.018)	(0.010)	(0.019)
BM controls	yes	yes	yes	yes	yes
# of obs	1,326	2,142	3,527	3,394	3,601
Pseudo-R^2	0.026	0.019	0.048	0.030	0.058

Table 3.8: Channels of the Obesity Effect: Health and Impatience

This table shows the results on the channels through which obesity affects delinquency. Column 1 displays marginal effects of obesity and self-reported health status on the probability of becoming delinquent in 2008 (we also control for all variables included in column 5 of Table 3.3). Self-reported health status and physical health scores come from the surveys around which the respondents turn 40 years old. The omitted category is *Excellent health*. The remaining columns investigate the impatience channel. Specifically, columns 2 and 3 show the average marginal effects of individuals' β- and δ-discount rates on the probability of being obese (column 2 does not include the controls from the benchmark model). β- and δ-discount rates are obtained from respondents' answers to NLSY survey questions about time preferences. Column 5 displays the results from the benchmark credit risk model augmented by the β- and δ-discount rates (and column 4 serves as a comparison for the obesity effect in the smaller sample due to missing observations for the discount rates). Heteroskedasticity robust standard errors, clustered by residence typology (region and urban/rural), and accounting for the complex survey design, are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% significance levels. Observations are weighted using NLSY 2004 sampling weights.

	Health	Impatience			
Dependent variable:	Delinquent	Obese	Obese	Delinquent	Delinquent
	(1)	(2)	(3)	(4)	(5)
Obese	0.026**			0.042***	0.041***
	(0.013)			(0.011)	(0.011)
Very good health	0.029**				
	(0.012)				
Good health	0.081***				
	(0.015)				
Fair health	0.075***				
	(0.024)				
Poor health	0.111***				
	(0.042)				
β-discount rate		0.022***	0.009		0.010**
		(0.008)	(0.008)		(0.005)
δ-discount rate		0.011***	0.008**		0.006***
		(0.004)	(0.003)		(0.002)
Benchmark controls	yes	no	yes	yes	yes
# of observations	6,995	6,403	6,403	6,403	6,403
Pseudo-R^2	0.170	0.003	0.064	0.167	0.169

3.A Appendix

3.A.1 Robustness Tests

3.A.1.1 Weight Categories, The Financial Crisis, and Other Measures of Financial Distress

In this section, we consider robustness test in several dimensions. First, delinquency is not the only indicator of financial distress in the NLSY. Respondents also declare when they have filed for bankruptcy in the 2004 and 2008 interviews and how many maxed-out credit cards they have in 2008. Finding a similar association between excess weight and other measures of financial distress would further validate our interpretation. Second, it would be interesting and informative to know how delinquency risk varies across the range of BMI. Third, our sample period partially overlaps with the financial crisis. To assess the temporal robustness of our main result, we use 2000 survey data to predict delinquencies reported in 2004.

Results are displayed in Table 3.A.4. In panel A, we use year 2004 covariates to predict 2008 outcomes, and in panel B we use year 2000 covariates to predict 2004 outcomes. Being obese is associated with a 0.9 percentage point greater incidence of bankruptcy and 2.8 percentage point greater incidence of reaching a credit card limit. While statistically insignificant, the 0.9 percentage point impact of obesity on bankruptcies in 2008 is economically large (0.9 percentage points relative to the 3.87% incidence rate amounts to a 23% higher bankruptcy rate). Also, with the exception of the thinly populated underweight category, we find that financial distress risk increases for the most part across the BMI classifications.

One potential drawback to predicting 2008 financial distress is the interference of the financial crisis that began in August 2007. Note that the delinquency rate in our sample rises only modestly from 18.17% in 2004 to 18.49% in 2008, suggesting that delinquencies reported in the 2008 interview do not yet fully reflect the changing economic environment. The 2008 NLSY interviews were conducted between January 2008 and April 2009, with 65% of the observations taken by the end of the first quarter in 2008, and 82% by the end of the second quarter. As is evident from Fig. 3.A.2, national delinquency rates for consumer loans and mortgages began rising in 2006, albeit at a very slow pace. Growth in mortgage delinquencies accelerated in mid 2007, but growth in consumer loan delinquencies and the unemployment rate accelerated only in mid 2008. The NLSY79 data shows a similar pattern. Respondents who answered the survey in the first quarter of 2008 have lower incidences of delinquency and unemployment than respondents who answered the survey later, but the magnitude of the difference is relatively small.

The caveat to using surveys prior to 2004 to predict financial distress in 2004 is the lack of detailed information on assets and debts (e.g., no information on credit card or student loans) and no information on credit histories. Some of our control variables will be measured differently (e.g., debt-to-income and debt-to-asset ratios) or be excluded from the 2000 credit risk model (e.g., credit history). Therefore, the estimated marginal effects of the various BMI categories on delinquencies are not directly comparable between the 2000 and 2004 credit risk models. Nevertheless, the results in panel B are qualitatively similar to those in panel A. We conjecture that the link between obesity and financial distress that we document in the cross-section is stable over time and not driven by the financial crisis.

3.A.1.2 Estimates from Propensity Scoring

The purpose of adding the many factors to our credit risk model was to account for differences between the obese and non-obese, so that we do not mistakenly attribute delinquencies to obesity. An alternative way to achieve this goal is to use propensity scoring. The propensity score is the predicted probability that a respondent is obese based on his/her observed characteristics, which we obtain from a probit regression of obesity on the full set of credit risk attributes. The weights emphasize the comparison of obese and non-obese that are similar in their observable characteristics (see DiNardo, Fortin and Lemieux (1996) for an early application and Nichols ((2007), (2008)) for details on the implementation).[56]

Fig. 3.A.3 displays the distribution of propensity scores for the obese and non-obese before and after reweighting. The upper panel utilizes the observable and permissible characteristics (see Table 3.3) for propensity scoring; the lower panel also includes the factors that are prohibited or unobservable to the lender (see Table 3.5). The left hand panels indicate that the the credit risk factors are strongly correlated with obesity. The right hand panels indicate that the distributions overlap almost perfectly over the entire range of propensity scores after we reweight the observations, which suggests that we have sufficient variation in obesity across the spectrum of observable credit risk factors.

In Table 3.A.5 we display the estimates from regressing delinquency on obesity after propensity scoring. We implement propensity scoring in two ways: including the score as a control variable in the regression (columns 1 and 3) and using the score to reweigh the observations (giving more weight to the more typical observations; columns 2 and 4). Restricting the regressions to the common support is superfluous, as the common support covers almost the entire range of the propensity scores. We find that the likelihood of delinquency among the obese is about 4.0 percentage points higher than among the non-obese, with a standard error of about 1.4 percentage points. This estimate based on propensity scoring is very close to the benchmark estimate of 3.8 percentage points.

[56]Propensity score methods are often considered a valid approach to causal inference, albeit less convincing than experiments, regression discontinuity designs, or instrumental variables. Nevertheless, we caution against the causal interpretation of our results, both due to data constraints and our use of obesity as a proxy for or signal of credit risk. A causal interpretation of the results is not necessary for the objective of our paper; we merely attempt to establish that obesity is incrementally informative about the likelihood of the repayment of debt.

3.A.1.3 Non-monotonic Effects of BMI on Delinquency: Evidence from Semiparametric Estimation

Gronniger (2006) argues that variation in BMI within a broad category need not have a monotonic effect on outcomes like mortality. As such, the optimal level of BMI may not belong to the BMI category with the best average outcome. To gain a better understanding of the relationship between BMI and delinquency across the full range of BMIs, we estimate the relationship between BMI and delinquency semiparametrically. Fig. 3.A.4 plots the predicted probability of delinquency for an average individual at any given level of BMI. All covariates other than BMI are collapsed into a propensity score, which is treated parametrically. We find that the delinquency risk increases over most of the BMI spectrum and most drastically between BMIs of 30 and 37. The evidence suggests that our main result is not qualitatively sensitive to the definition or classification of obesity.

3.A.2 Measurement Error

Our primary variables of interest, obesity and delinquency, rely on survey data and are potentially mismeasured. The following discussion is largely based on Bound, Brown and Mathiowetz (2001). Due to the nature of our data, the measurement error cannot be of the classical form: (i) BMI is the ratio of weight squared and height, which implies that classical measurement error in the inputs would no longer be classical for BMI; (ii) obesity and delinquency are binary variables, and therefore measurement error must be mean reverting. We will therefore focus our discussion of measurement error on the potential consequences of misclassification of obesity and delinquency.

Whereas classical measurement error in continuous dependent variables does not bias the coefficient estimates, misclassification error in the dependent variable causes the estimates to be biased in probit models. Assuming that delinquency is the only mismeasured variable, the marginal effect of obesity on the *observed* delinquency rate will differ from the marginal effect of obesity on the *true* delinquency rate by a factor of $1 - \tau_{01} - \tau_{10}$, where τ_{01} is the probability of unreported delinquencies conditional on actually being delinquent (false negatives) and τ_{10} captures false positives (Hausman, Abrevaya and Scott-Morton (1998)). We can obtain a rough estimate of the misclassification probability by comparing the bankruptcy rate reported by NLSY respondents to the national bankruptcy rate based on court filings. Measured over years 2004 to 2008, the bankruptcy rate among NLSY respondents is 26% lower than the national rate. Assuming that classification error stems from underreporting only, the marginal effect of obesity on the observed delinquency rate is 74% of the marginal effect on the true delinquency rate.[57]

Turning to misclassification in obesity, let us assume that the measurement error is non-differential (i.e., conditional on true obesity, the error is independent of delinquency). Based on Aigner (1973), Bound et al. (2001) show that the bias factor is

$$1 - \frac{\pi_{01}\pi}{\pi_{01}\pi + (1-\pi_{10})(1-\pi)} - \frac{\pi_{10}(1-\pi)}{\pi_{10}(1-\pi) + (1-\pi_{01})\pi}, \tag{3.A.1}$$

where π is the true prevalence of obesity, π_{01} is the probability of false negatives, and π_{10} is the probability of false positives. The estimated coefficient on obesity will be biased towards zero, but — for sufficiently high degrees of misclassification — can lead to a sign reversal on the estimated coefficient (i.e., the factor would turn negative).

To quantify the potential downward bias in the obesity estimate, we obtain estimates of the various probabilities from Grabner (2009), who compares the extent of bias between self-reported and measured height and weight and the effect on BMI and

[57] Additional bias may arise from inconsistent estimation of the coefficients. Implementing the solution proposed by Hausman et al. (1998) — explicitly allowing for misclassification in the likelihood function — yields an estimate of the marginal effect of obesity on delinquency that is about 35% higher than the benchmark estimate. The Stata routine is available at ⟨http://www.utexas.edu/cola/depts/economics/faculty/ja8294?tab=139⟩.

obesity across various data sets. Survey respondents tend to overstate their height and underreport their weight, leading primarily to false negatives in the obesity classification. According to Grabner's Fig. 1.2.a, average measured BMI in NHANES is about 1.5 units higher than average self-reported BMI in NHIS and BRFSS. Adding the difference to each respondent's reported BMI in NLSY raises the prevalence of obesity in our sample from 27.7% to 37.3%. With $\pi_{01} = 9.6/37.3$ as the misclassification rate and $\pi = 37.3\%$ as the true obesity prevalence, we obtain from eq. (3.A.1) a factor of 0.87. That is, the amount of misclassification inherent in obesity suggests that our estimate of the obesity effect is downward biased by about 13%. Alternatively, Grabner's Fig. 1.2.b suggests a true obesity rate of about 35% (based on measured NHANES data), and a misclassification rate of 28.6% (underreporting of obesity by 10 percentage points in NHIS/BRFSS data). These assumptions also yield an estimated downward bias of 13%.[58,59,60]

Looking at measurement error in obesity and delinquency independently suggests that the true relationship between the variables is stronger than what we capture in the benchmark specification. However, in theory systematic joint misreporting of weight, height, and delinquency could induce a positive correlation between obesity and delinquency. Suppose that in the true state of the world the obese and non-obese are equally likely to be delinquent. Yet, individuals who are self-conscious and

[58]The heading to Fig. 1.2.b states that it depicts class I obesity rates, but in private correspondence Grabner has confirmed that it reflects the overall obesity rate.

[59]A second concern is that BMI does not distinguish between fat and muscle mass or bone structure, which leads to substantial measurement error. Utilizing results from Burkhauser and Cawley (2008), we estimate bias factors of 0.5 (based on classification errors and prevalence without the treshold adjustment) and 0.4 (with the threshold adjustment).

[60]Sometimes, researchers attempt to correct for misreporting bias with regression-based adjustments, calibrated on the difference between reported and measured height and weight data from NHANES. While BMI and obesity prevalence estimates are affected substantially, the relationship between BMI or obesity and various outcomes appears to be insensitive to the self-reporting bias. For example, Lakdawalla and Philipson (2002) and Zagorsky (2005) report that adjustments to BMIs calculated from NLSY data do not substantively alter their results. More recently, Grabner (2009) concludes that self-reported BMIs from other datasets (such as BRFSS and NHIS) are valid sources for BMI trends and associations despite their bias, but cautions against adjusting self-reported data based on NHANES calibrated corrections due to significant differences in self-reports across the data sets.

insecure are more likely to understate BMI *and* delinquency, as both are perceived negatively in society. Thus, compared to the true state of the world, misreporting would lead to more non-obese respondents and a lower delinquency rate among them in the data. However, correlated misclassification cannot match the magnitude of our benchmark estimate under reasonable assumptions.

Finally, we acknowledge the possibility that using classically mismeasured values in place of true values in control variables (e.g., income and wealth) only partially controls for the confounding effects of the correctly measured variables on the estimate of the effect of obesity on delinquency (Bound et al. (2001)).

In conclusion, there are several reasons to believe that the estimated effect of obesity on delinquency is biased downward, and a few reasons for why the effect might be biased upward. Taken together, it is difficult to assess the relative magnitudes of the potential biases inherent in the data, but there is no indication that the obesity effect is fully attributable to measurement error.

3.A.3 Additional Figures

Figure 3.A.1: Bankruptcy Rates Across BMI Categories
This graph displays the bankruptcy rate across the BMI categories (brackets denote the 95% confidence interval). Bankruptcy refers to bankruptcies declared between 2004 and 2008. Data source: NLSY79.

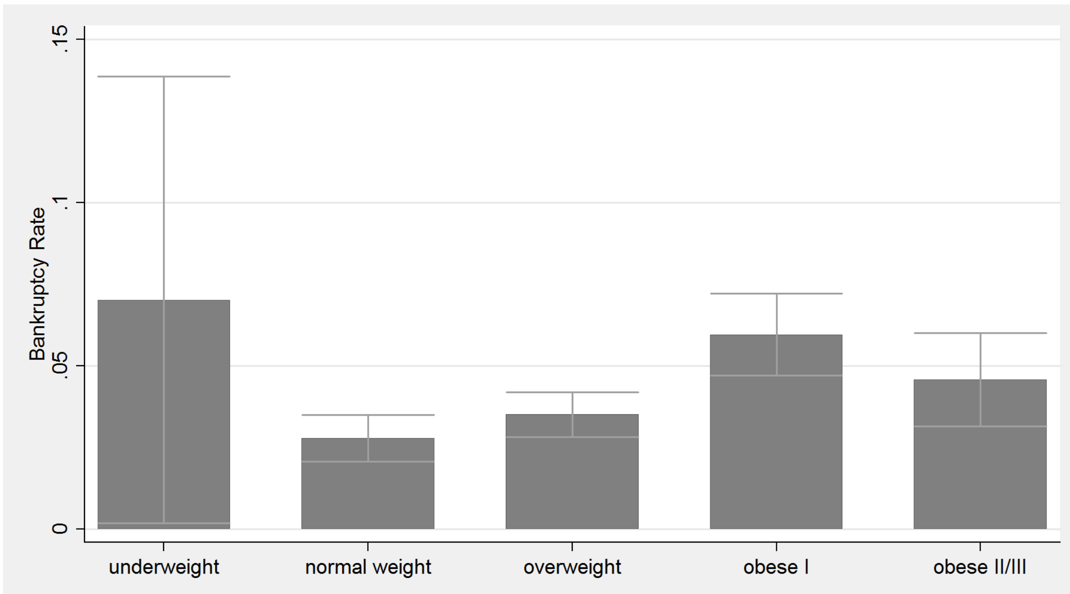


Figure 3.A.2: Economic Environment Before and During the Sample Period

To gauge the impact of the financial crisis on the delinquency rate reported in the 2008 NLSY interview, we plot the unemployment rate and the residential real estate loan and consumer loan delinquency rates over time. The 2008 NLSY interviews were conducted between January 2008 and April 2009, with 65% of the observations taken by the end of Q1 2008, and 82% by Q2. Data sources: BLS and Federal Reserve Bank.

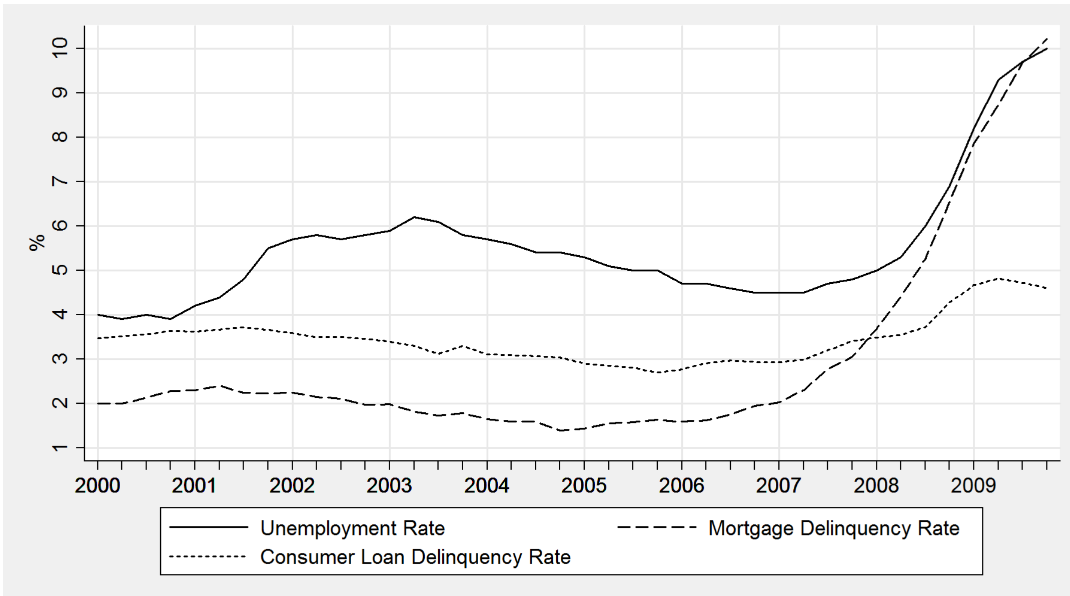


Figure 3.A.3: Comparability of Obesity Propensity Scores Before and After Reweighting

Numerous observable predictors of obesity are also known credit risk factors. These graphs illustrate the comparability of obese and non-obese respondents. The figures on the left display the kernel density estimates of the probability density functions of unadjusted propensity scores (i.e., predicted probabilities that individuals are obese) for the obese and non-obese. The figures on the right display propensity-score-reweighted densities (i.e., giving more weight to observations that are representative of the population average). The upper panel is based on the permissible and observable characteristics (Table 3.3, column 5). The lower panel also includes the additional covariates from Table 3.5 (e.g., race and gender). Data source: NLSY79.

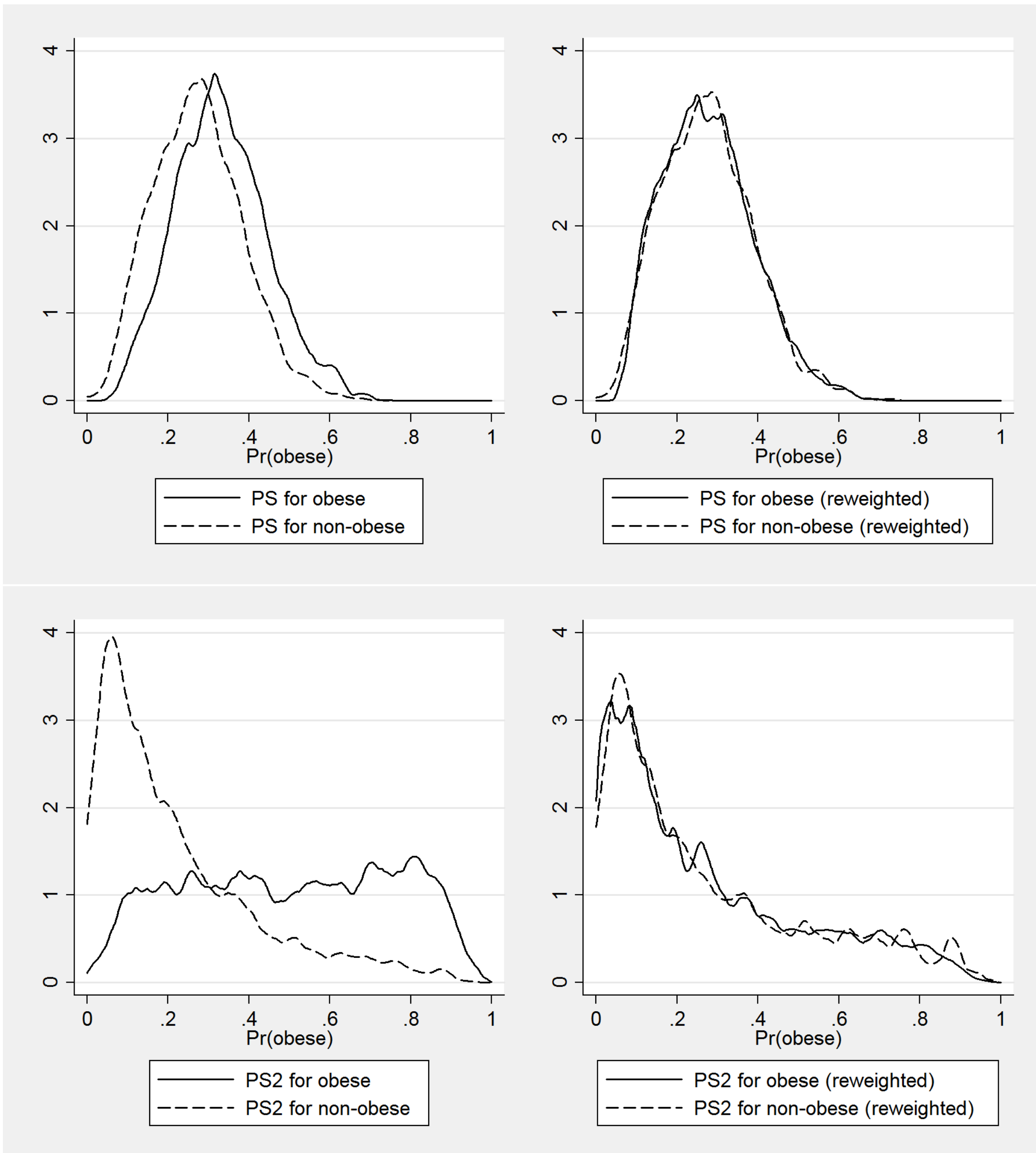
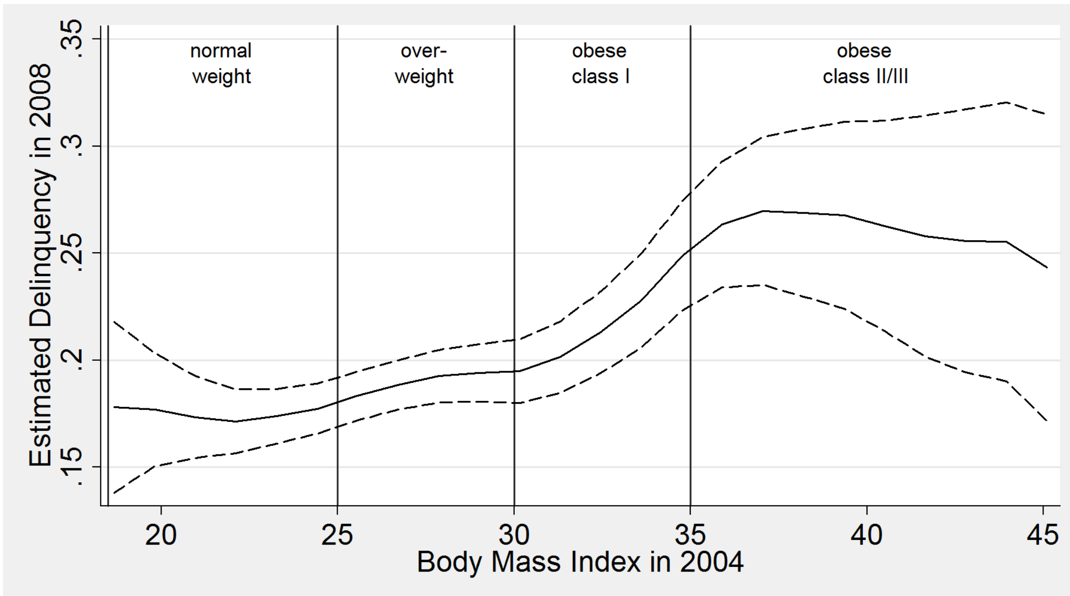


Figure 3.A.4: Average Rate of Delinquency Across BMIs — Local Linear Estimates
The plot represents the predicted probability of delinquency for an average individual at a given BMI according to semiparametric regression results (kernel bandwidth = 2, pilot bandwidth = 3). All covariates other than BMI (including all typical observable credit risk factors, but excluding those prohibited from inclusion in credit risk models) were collapsed into a propensity score, which was then treated parametrically. Data source: NLSY79.



3.A.4 Additional Tables

Table 3.A.1: Marginal Effect of Obesity on Delinquency After Controlling for Income, Wealth, and Debt Capacity

The table displays marginal effects of obesity on delinquency, estimated from credit risk probit models. All explanatory variables are from the 2004 survey; the dependent variable *Delinquent* comes from the 2008 survey. In columns 1–4, we individually introduce the income, wealth, debt characteristics as control variables. To achieve a flexible specification, each characteristic is represented by 6 attributes (5 attributes for the quintiles of the distribution and one attribute for missing responses). The first quintile represents the base category; estimates for the missing category are suppressed for brevity. In column 5 we control for all attributes simultaneously. In column 6, we increase the number of attributes by creating deciles. Heteroskedasticity robust standard errors, clustered by residence typology (region and urban/rural), and accounting for the complex survey design, are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% significance levels. Observations are weighted using NLSY 2004 sampling weights.

	Quintile attributes					Deciles
	(1)	(2)	(3)	(4)	(5)	(6)
Obese	0.066***	0.057***	0.082***	0.070***	0.051***	0.050***
	(0.015)	(0.015)	(0.014)	(0.015)	(0.016)	(0.016)
Income Q2	-0.068***				-0.055***	
	(0.021)				(0.020)	
Income Q3	-0.110***				-0.064***	
	(0.019)				(0.021)	
Income Q4	-0.150***				-0.083***	
	(0.011)				(0.017)	
Income Q5	-0.204***				-0.117***	
	(0.022)				(0.021)	
Wealth Q2		-0.070***			-0.073***	
		(0.024)			(0.025)	
Wealth Q3		-0.176***			-0.158***	
		(0.018)			(0.023)	
Wealth Q4		-0.196***			-0.165***	
		(0.017)			(0.022)	
Wealth Q5		-0.241***			-0.198***	
		(0.019)			(0.028)	
Debt/income Q2			0.024		0.040	
			(0.020)		(0.027)	
Debt/income Q3			-0.019		0.037	
			(0.017)		(0.028)	
Debt/income Q4			-0.021		0.034	
			(0.017)		(0.029)	
Debt/income Q5			0.027		0.043*	
			(0.026)		(0.025)	
Debt/assets Q2				-0.027	0.037	
				(0.021)	(0.023)	
Debt/assets Q3				-0.022	0.039**	
				(0.014)	(0.018)	
Debt/assets Q4				0.010	0.043*	
				(0.018)	(0.025)	
Debt/assets Q5				0.148***	0.073***	
				(0.017)	(0.022)	
# of observations	6,995	6,995	6,995	6,995	6,995	6,995
Pseudo-R^2	0.039	0.0615	0.012	0.036	0.075	0.083

Table 3.A.2: Marginal Effect of Obesity on Delinquency After Controlling for Credit History (and Income, Wealth, and Debt Capacity)

The table displays marginal effects from credit risk probit models, delinquency reported in 2008 being the dependent variable. All explanatory variables are from the 2004 survey. In all specifications, we include the set of controls used in column 5 of Table 3.A.1, but suppress their average marginal effects to save space. For the credit attributes, the omitted category is *Did not apply for credit.* Heteroskedasticity robust standard errors, clustered by residence typology (region and urban/rural), and accounting for the complex survey design, are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% significance levels. Observations are weighted using NLSY 2004 sampling weights.

	(1)	(2)	(3)	(4)
Obese	0.037***	0.049***	0.045***	0.035**
	(0.014)	(0.016)	(0.015)	(0.014)
Delinquent	0.260***			0.232***
	(0.014)			(0.013)
Bankrupt		0.073**		0.006
		(0.031)		(0.024)
Credit approved			-0.014	-0.012
			(0.011)	(0.012)
Credit denied			0.163***	0.091***
			(0.020)	(0.019)
Credit expected			0.110***	0.043***
to be denied			(0.022)	(0.016)
Controls from Table 3.A.1	yes	yes	yes	yes
# of observations	6,995	6,995	6,995	6,995
Pseudo-R^2	0.137	0.077	0.096	0.144

Table 3.A.3: Marginal Effect of Obesity on Delinquency After Controlling for Employment Factors (and Income, Wealth, Debt Capacity, and Credit History)

The table displays marginal effects from credit risk probit models, delinquency reported in 2008 being the dependent variable. All explanatory variables are from the 2004 survey, with the exception of *Income instability coefficient* (based on survey years 1996–2004). In all specifications, we include the set of controls used in column 4 of Table 3.A.2. The omitted category for education attainment is *Did not complete high school*. Industry and occupation dummies are based on 4-digit Census codes and follow the classification in the NLSY codebook. Heteroskedasticity robust standard errors, clustered by residence typology (region and urban/rural), and accounting for the complex survey design, are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% significance levels. Observations are weighted using NLSY 2004 sampling weights.

	(1)	(2)	(3)	(4)	(5)	(6)
Obese	0.034***	0.038***	0.035**	0.036***	0.038***	0.038***
	(0.012)	(0.014)	(0.014)	(0.014)	(0.013)	(0.012)
Income instability Q2		0.022*				0.017
		(0.012)				(0.012)
Income instability Q3		0.014				0.008
		(0.014)				(0.014)
Income instability Q4		0.038*				0.030
		(0.020)				(0.019)
Income instability Q5		0.063***				0.048***
		(0.018)				(0.017)
High school degree			-0.013			-0.010
			(0.017)			(0.016)
Some college			-0.008			-0.010
			(0.017)			(0.020)
College degree			-0.028			-0.033
			(0.021)			(0.025)
Advanced degree			-0.028**			-0.040***
			(0.013)			(0.013)
1yr $<$ job tenure $\leq$ 2yr				0.016		0.015
				(0.017)		(0.018)
2yr $<$ job tenure $\leq$ 3yr				0.015		0.018
				(0.022)		(0.026)
3yr $<$ job tenure				-0.009		0.000
				(0.010)		(0.013)
Self-employed					0.065***	0.040
					(0.021)	(0.027)
Unemployed					0.027	0.028
					(0.019)	(0.019)
Out of labor force					0.015	0.001
					(0.010)	(0.012)
Industry dummies	yes	no	no	no	no	yes
Occupation dummies	yes	no	no	no	no	yes
Controls from Table 3.A.1	yes	yes	yes	yes	yes	yes
Controls from Table 3.A.2	yes	yes	yes	yes	yes	yes
# of observations	6,995	6,995	6,995	6,995	6,995	6,995
Pseudo-R^2	0.156	0.148	0.146	0.146	0.149	0.162

Table 3.A.4: Marginal Effects of Excess Weight on Financial Distress

The table displays marginal effects of obesity on subsequent financial distress, estimated from credit risk probit models. The dependent variables are dummies indicating delinquency (columns 1 and 4), bankruptcy (columns 2 and 5), or maxing out a credit card (columns 3 and 6). In all specifications of Panel A, we include the set of controls used in column 5 of Table 3.3. In Panel A, the measures of financial distress are obtained from the 2008 survey and the explanatory variables from the 2004 survey. In Panel B, the financial distress measures are taken from the 2004 survey, and the explanatory variables from 2000. Note that the regressions in Panel B control for fewer credit history variables, as they are not available in the 2000 survey (we only have information on prior bankruptcies). Heteroskedasticity robust standard errors, clustered by residence typology (region and urban/rural), and accounting for the complex survey design, are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% significance levels. Observations are weighted using NLSY 2004 sampling weights.

Panel A: 2004 Obesity → 2008 Financial Distress

	Coarse Obesity Classification			Finer Classification of BMI		
	Delinquent (n=1,482)	Bankrupt (n=275)	Maxed out (n=784)	Delinquent (n=1,482)	Bankrupt (n=275)	Maxed out (n=784)
	(1)	(2)	(3)	(4)	(5)	(6)
Obese	0.038***	0.009	0.028**			
	(0.012)	(0.006)	(0.014)			
Underweight				-0.064**	0.036	0.016
				(0.029)	(0.043)	(0.040)
Overweight				0.028*	0.000	0.018**
				(0.017)	(0.005)	(0.007)
Obese I				0.040***	0.013	0.032*
				(0.015)	(0.008)	(0.017)
Obese II/III				0.075***	0.004	0.052**
				(0.016)	(0.008)	(0.021)
BM controls	yes	yes	yes	yes	yes	yes
# of obs	6,995	6,787	6,853	6,995	6,787	6,853
Pseudo-R^2	0.162	0.193	0.093	0.165	0.194	0.095

Panel B: 2000 Obesity → 2004 Financial Distress

	Coarse Obesity Classification		Finer Classification of BMI	
	Delinquent (n=1,440)	Bankrupt (n=315)	Delinquent (n=1,440)	Bankrupt (n=315)
	(1)	(2)	(4)	(5)
Obese	0.062***	0.019***		
	(0.009)	(0.006)		
Underweight			0.066	0.025
			(0.054)	(0.018)
Overweight			-0.005	0.004
			(0.007)	(0.006)
Obese I			0.055***	0.022***
			(0.018)	(0.006)
Obese II/III			0.071***	0.018*
			(0.020)	(0.010)
BM controls	yes	yes	yes	yes
# of obs	6,958	7,019	6,958	7,019
Pseudo-R^2	0.081	0.171	0.082	0.172

Table 3.A.5: Marginal Effect of Obesity on Delinquency After Propensity Scoring

The table displays estimates of the marginal effect of obesity on delinquency after propensity scoring. In columns 1 and 3 we include the propensity scores as a control in the regression of 2008 delinquencies on 2004 obesity. In columns 2 and 4 we use the propensity scores to reweigh the observations. In either case, restricting the sample to the common support has no impact on the obesity coefficient. Note that R^2 is much lower than in the comparable specifications in column 5 of Table 3.3 and column 6 of Table 3.5, because the propensity score discards the information contained in those covariates that explains variation in delinquencies, but is not correlated with obesity. Heteroskedasticity robust standard errors, clustered by residence typology (region and urban/rural), and accounting for the complex survey design, are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% significance levels. Observations are weighted using NLSY 2004 sampling weights.

	Score estimated from covariates in Table 3.3, column 5		Score estimated from covariates in Table 3.5, column 6	
	control	reweigh	control	reweigh
	(1)	(2)	(3)	(4)
Obese	0.041***	0.040***	0.040***	0.032**
	(0.013)	(0.014)	(0.014)	(0.015)
BM controls	yes	yes	yes	yes
Other controls	no	no	yes	yes
# of obs	6,995	6,995	6,995	6,995
Pseudo-R^2	0.045	0.002	0.017	0.001
Observed prob	0.185	0.185	0.185	0.185
Predicted prob	0.185	0.188	0.185	0.190

Table 3.A.6: Additional Evidence on Cross-sectional Heterogeneity in the Informativeness of Obesity

The table displays estimates of the marginal effect of obesity on delinquency across income and wealth quintiles (Panel A) and conditional on credit history and across holdings of secured and unsecured debt (Panel B). All specifications control include quintile attributes of the obesity propensity score. We run separate regressions for good and poor credit histories. Poor credit history encompasses any of the adverse histories (delinquency, bankruptcy, or credit denial) as well as the cases in which respondents expected to be denied. For debt types, we interact obesity with debt type. Heteroskedasticity robust standard errors, clustered by residence typology (region and urban/rural), and accounting for the complex survey design, are in parentheses. ***, **, and * denote significant differences from zero at the 1%, 5%, and 10% significance levels. Observations are weighted using NLSY 2004 sampling weights.

Panel A: Interactions with Income and Wealth

	Average marginal effect of obesity over the quintiles of . . .					
	Income			Wealth		
	marginal effect	range w/in Q ($ thsd.)	% obese w/in Q	marginal effect	range w/in Q ($ thsd.)	% obese w/in Q
	(1)	(2)	(3)	(4)	(5)	(6)
Obese						
× Q1	0.041	0–29	34.0	0.035	-926–8	34.7
	(0.029)			(0.026)		
× Q2	0.044	29–53	29.2	0.016	9–67	32.2
	(0.028)			(0.022)		
× Q3	-0.005	53–76	32.1	0.032**	67–167	31.1
	(0.028)			(0.016)		
× Q4	0.073***	76–111	25.0	-0.009	167–354	24.1
	(0.026)			(0.027)		
× Q5	0.118***	111–443	18.0	0.182***	354–2720	16.8
	(0.037)			(0.053)		
BM controls	yes			yes		
# of obs	6,995			6,995		
Pseudo-R^2	0.060			0.078		

Panel B: Effect of Obesity by Credit History and Debt Type

	Credit history		Debt type
	Good	Poor	
	(1)	(2)	(3)
Obese	0.045***	0.027	
	(0.013)	(0.030)	
Obese			
× no debt			0.033
(19.9%)			(0.027)
× secured debt only			0.035**
(52.9%)			(0.014)
× unsecured debt only			-0.013
(7.1%)			(0.049)
× sec & unsec debt			0.075***
(20.1%)			(0.025)
BM controls	yes	yes	yes
# of observations	4,675	2,264	6,995
Pseudo-R^2	0.016	0.008	0.069

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