Preliminary! Incomplete Draft!

Measuring Systematic Risk in Recoveries on Defaulted Debt I: Firm-Level Ultimate LGDs

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Several recent empirical papers report evidence of significant systematic variation in recovery rates on defaulted corporate debt, implying that the convenient assumption of independent recovery rates found in most defaultable debt pricing models and portfolio credit risk models is unrealistic. However, such work has used recoveries on individual assets. These are claims on the value of the bankrupt firm at emergence, so systematic variation in such firm value is the most natural source of systematic variation in recoveries. We examine the aggregate recovery at emergence on the debt of each firm in a sample of bankrupt firms. We find mixed evidence of systematic variation. Such evidence is driven largely by experience in a single historical episode and point estimates and statistical significance are sensitive to details of the empirical specification. Our evidence about predictors of recovery suggests that interactions between the default decision and recovery rates may be more complex than existing models imply.

Keywords: credit risk, LGD, recovery rates, bankruptcy, debt default

JEL Codes: G33, G12

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--- Abstract ---

Several recent empirical papers report evidence of significant systematic variation in recovery rates on defaulted corporate debt, implying that the convenient assumption of independent recovery rates found in most defaultable debt pricing models and portfolio credit risk models is unrealistic. However, such work has used recoveries on individual assets. These are claims on the value of the bankrupt firm at emergence, so systematic variation in such firm value is the most natural source of systematic variation in recoveries. We examine the aggregate recovery at emergence on the debt of each firm in a sample of bankrupt firms. We find mixed evidence of systematic variation. Such evidence is driven largely by experience in a single historical episode and point estimates and statistical significance are sensitive to details of the empirical specification. Our evidence about predictors of recovery suggests that interactions between the default decision and recovery rates may be more complex than existing models imply.

The extent of systematic variation in recovery rates on defaulted debt is important to debt pricing and credit risk management. A common intuition is that asset values of bankrupt firms will be lower in bad states of the world and thus recoveries will be lower at the same time that default rates are higher. Moreover, returns on other assets are likely to be low as well. Systematic risk (by definition) cannot be diversified away, and so prices of debt should reflect systematic risk associated with recoveries as well as systematic risk of default. Similarly, credit value-at-risk and other measures of bad-state losses on debt portfolios will be higher if recoveries have a systematic component. The effects are potentially quite important. For example, Hu and Perraudin (2002) suggest that portfolio capital allocations should be twice what is implied by models that assume no systematic variation in recovery rates.

Such common intuition need not be correct. If firms declare bankruptcy when their value drops below an insolvency threshold, as in many models in the spirit of Merton (1974), and if the threshold is the same in all states of the world, then recoveries may be similar across states because the degree of insolvency of bankrupt firms at default will be similar.

A number of recent studies have shed empirical light on the question of systematic risk in recovery rates, including Frye (2000ab, 2003), Hu and Perraudin (2002), Altman, Brady, Resti and Sironi (2003), and Acharya, Bharath and Srinivasan (2004). All find evidence of systematic risk, while disagreeing somewhat about characteristics that influence it. But all examine samples of recoveries on individual debt instruments, whereas intuition about systematic risk is focused on variations in the value of bankrupt firms. The characteristics of instruments vary across the samples used in the existing studies, which complicates interpretation of their results.

Using a new method of measuring recoveries, we contribute new facts about the size and determinants of systematic variation. Our evidence implies that such variation may be material, but the evidence is less robust than previous studies imply. More importantly, the new determinants of recoveries that we uncover suggest that more attention to the interaction between the bankruptcy declaration decision and recovery is likely to be needed before a full understanding of either can be achieved, and that recoveries on individual instruments should be modeled using contingent-claim approaches. We do not claim to settle the question of the size of systematic risk in

recoveries in the current paper, but our evidence does suggest additional research that is likely to be fruitful.

Our methodological suggestion is that recovery should be modeled for each bankrupt firm as a whole, with derivative modeling of individual debt-instrument recoveries. Bankrupt firms are still firms, and the debt liabilities of such firms are still claims on the value of the firms' assets. Such claims may fruitfully be viewed as options both before and after the bankruptcy date, but bankruptcy changes the payoff properties of the claims. Holders of debt of a firm that has not defaulted on or before the maturity date are repaid according to the terms of the debt contract, regardless of the value of the firm's assets. In the United States, for bankrupt firms, the legal rules of absolute priority imply that a pre-bankruptcy debt claim becomes something similar to a collar option once bankruptcy is declared. The lower strike value of the collar is at a firm value just sufficient to satisfy claims of higher priority. Holders of a claim receive nothing if firm value is smaller than this amount, as would be the case for senior unsecured debt in example A in Figure 1. The upper strike is at a firm value sufficient to satisfy all claims of equal or greater priority. Values as large or larger imply that holders of a claim are paid in full (but no more), as would be the case for senior unsecured debt in example B in Figure 1. Previous studies have produced evidence of violations of absolute priority, such as payments to equity or subordinated debtholders even though senior claimants are not paid in full (e.g., Eberhart et al. 1989; Weiss 1990). However, such violations rarely involve a large share of firm value and are not very important for the purposes of this paper because violations within the pool of debt claims on a given firm wash out in our measures.

In a pooled sample of recoveries for many claims on many firms, the individual debt claims will differ in priority (will have different strike prices). Thus, the response of individual-instrument recoveries to variations in the state of the world will differ. If the mix of debt of different priority changes over time, it is difficult to parse variations in average instrument-level LGD into sample composition effects versus systematic variation in the value of bankrupt firm assets. These are potential problems for all previous studies.

We empirically examine firm-level recovery rates, measured for each firm as the dollar-weighted sum of dollar recoveries at emergence from bankruptcy relative to total claims ("ultimate" recoveries, in contrast to the post-default distressed-debt prices that have been used in most previous studies). The sample includes 443 corporate

bankruptcies of U.S. firms filed during the years 1987-2002. In each case, recovery at emergence from bankruptcy is available for each of the firm's debt instruments outstanding at the time of the bankruptcy filing, including loans by bank and nonbank lenders as well as bonds. That is, we know the firm's debt structure and the total recovery on all its debt as well as recoveries on individual debt instruments. The sample includes a large fraction of the bankruptcies and debt instruments present in the samples used in previous studies.

We find mixed evidence about the importance of the systematic component of firm-level recoveries. Roughly, looking across measures and specifications, point estimates imply that firm-level recoveries are about 5 to 10 percentage points worse than the overall sample mean of about 50 percentage points during the high-default-rate years observed in the sample, or about 10 to 20 percent more than the average. But statistical significance is sensitive to how the state of the world is measured and to sample period, and the economic size of systematic variations also is quite sensitive to details of specification. Moreover, almost all of the evidence of systematic variation is driven by behavior in the "bad" years 2000-2003. We find little evidence of systematic variation in other years, including the "bad" years 1989-91, in contrast to previous studies. Worse simple-mean recoveries in 1989-91 are largely due to previously unrecognized sample composition effects. Overall, our results reveal that a portfolio manager must decide how much weight to place on a single historical episode in forming opinions about systematic variation in recovery rates.

Perhaps more important than our estimates of the size of systematic variation are our contributions to understanding of the drivers of systematic risk. Our evidence implies that variation in firm-level recovery is partly predictable. Implications of the evidence presented below include:

- The typical bankrupt firm is deeply insolvent at the time of filing. Only about one-quarter of our sample firms had recovery rates better than 70 percent. Moreover, it is not clear that such insolvency is a surprise. About half of the firms for which we were able to find balance sheet data reported negative accounting equity at the fiscal year-end preceding filing. These facts suggest that first-passage-time and other models of default focused purely on the market value of the firm's assets may be difficult to adapt to simultaneous modeling of default

and recovery. Such simultaneous modeling is likely to be necessary for a satisfactory treatment of default losses.

- The nature of a firm's debt structure is strongly correlated with its firm-level recovery rate. Firms having only bank debt in their capital structure have recoveries that are roughly 30 percentage points better than firms with no bank debt, an enormous effect. Firms with contractually subordinated debt have worse firm-level recoveries than those with none. Banks typically have greater control rights than bondholders as a firm becomes distressed. Such control rights may be an important part of the dynamics of both bankruptcy rates and recovery rates. For example, the typical bankrupt firm may have been economically insolvent for some time prior to filing. In many cases, the timing of filing may be driven by a decision by the firm's bankers to stop financing the firm. Where banks provide a large share of a firm's debt, they are likely to force the firm into bankruptcy while it is still only moderately insolvent, in order to protect their recovery. Where banks provide only a small share of total debt, they may be willing to continue to provide liquidity even if the firm is deeply insolvent because more subordinated layers of debt are likely to bear the losses.
- The mass of observations in the bad tail of the firm-level recovery rate distribution is about twice as large in bad years as in good years, with about 40 percent of bad-year recovery rates at 30 percent or worse, whereas only about 20 percent of firms declaring bankruptcy outside the bad years yield such poor recoveries. An important question is whether the shift is driven by a drop in asset values for all firms going bankrupt in bad years or whether very-bad-LGD firms are selected into bankruptcy at different rates in good and bad years. If the latter, then a better understanding of the bankruptcy decision is crucial to understanding systematic risk in recovery rates.
- The data hint that conditions in the year of emergence matter more for recovery than conditions in the year of bankruptcy, but it is difficult to draw confident conclusions because the years 2000-2003 have a dominant effect on results related to this question.
- "Bubble" firms – internet and telecom firms that declared bankruptcy during 2000-2002 – are an important component of the bad tail of the firm-level recovery rate distribution. Evidence of systematic risk is qualitatively robust to their

elimination from the sample, but the economic size of estimated systematic risk is smaller in their absence.

- We find little evidence that time in bankruptcy affects firm-level recovery rates, especially for undiscounted measures (Covitz, Han, Smith and Wilson (2004) focus on this issue and also find little evidence that time matters). Similarly, whether a bankruptcy is pre-packaged has little effect on firm-level recovery. These results call into question the common presumption that value is destroyed while a firm is in bankruptcy.
- We find little evidence that firm size or pre-bankruptcy financial statement variables predict recovery. Similar to Acharya et al. (2004), apart from public utilities, we find little evidence that the industry of a bankrupt firm is a predictor of recovery rates.

Our findings suggest (at least) three promising avenues for future research. Perhaps most importantly, a better understanding of the dynamics of the bankruptcy decision is needed. Second, the sensitivity of individual debt instrument recovery rates to shifts in the distribution of firm value at emergence might be examined for debt of different seniority as measured by place in debt structure. It would be convenient if exposure to systematic recovery risk varied smoothly with seniority, but our intuition is that the relationship may not be simple. Third, the properties of recovery when measured as the market price of debt (or of firms) just after bankruptcy is declared, as in Frye (2000ab) and Altman et al. (2003), may differ from the properties of ultimate recovery, and the reasons for any such differences are of interest. An understanding of the properties of ultimate recoveries should be helpful to conducting and interpreting results of such research.

This is the first draft of the paper and, though we are confident about the main results, we plan to do more. We plan to study the behavior of individual-instrument ultimate recovery rates in more detail in the next draft. Somewhat more work with the Compustat-matched subsample remains to be done, and more of what has been done remains to be written up. To help motivate the analysis, we plan to include a skeletal model that allows for sample-selection and sample-composition effects at the individual instrument level and at the firm level, as well as for the possibility of systematic risk. We are aware that errors in our OLS regressions, which are the basis for most of the results, are not normal, and that censoring is an issue at the end of the sample. We believe that

use of more appropriate estimation methods, which to date we have only experimented with, will not materially affect results. In future drafts, we will report results of more robustness checks, and we will examine instrument-level results in a bit more detail. We still must write Appendix A, which will describe some details of how the data are produced.

To conform to current risk-management terminology, throughout the remainder of the paper, we analyze loss-given-default (LGD), which is measured as (1-recovery rate). Section 2 describes the data and our measures of LGD. Section 3 provides institutional background that is helpful in evaluating measures and results. Section 4 present simple summary statistics, and Section 5 some results from simple regression models of LGD. Section 6 offers a few preliminary remarks about what we expect to find in examining individual-instrument recoveries. Section 7 is a placeholder for a comparison of our work with that reported in previous papers. Section 8 offers concluding remarks.

2.0 Data and LGD Measures

The primary data are from Standard and Poor's LossStat database (formerly produced by Portfolio Management Data), which tracks ultimate LGDs for each debt instrument outstanding at default for each firm in the database. Ultimate LGDs are payoffs at emergence from bankruptcy, in contrast to the post-default distressed-debt prices used in most of the literature. For example, suppose a firm defaulted and declared bankruptcy on 1 June 1998, that it emerged from bankruptcy exactly one year later, and that the firm's debt on the bankruptcy date consisted of a single bank loan and a single bond issue. Suppose that at emergence, the holders of the loan and bond received a mixture of cash and debt obligations of the emerging firm in compensation for their claims. The database records:

- The market value of such compensation at the time of emergence, separately for each pre-bankruptcy debt instrument.
- The identity and some characteristics of the defaulting firm and of its experience in bankruptcy, such as the court which handled the case;
- Characteristics of each debt instrument, such as original-issue amount, amount outstanding at default, coupon interest rate, whether the instrument is

subordinated or secured, and the actual priority class to which the instrument is assigned by the bankruptcy court.

The database has information for the complete debt structure of each firm. It does not have information about equity or preferred stock claims and their recoveries, nor about accounts-payable or other liabilities (this issue is discussed further below).

Although the database includes defaults and distressed restructurings that did not involve bankruptcy, in this paper we use only data for bankruptcies. Expected outcomes of bankruptcy are likely to influence bargaining and outcomes in non-bankruptcy situations. We wish to understand bankruptcies before attempting to analyze other situations.

Similarly, we wish to better understand ultimate LGDs before analyzing LGDs measured by the price of debt at the time of default. We expect that expectations of ultimate LGD are a primary determinant of such prices. However, liquidity and other factors may also play a role. LossStat does not include prices at default, but it is possible to obtain them for some instruments by matching to other databases.

S&P obtains LossStat data primarily by analyzing SEC filings and bankruptcy court documents. Market values of compensation received at emergence are gathered from a variety of sources (see Appendix A for a discussion). S&P attempts to capture all defaults by firms with more than $50 million of debt outstanding on the date of default, but inclusion in the database is subject to availability of information. The data begin with defaults in 1987, but coverage is more complete in recent years because of the creation of electronic recordkeeping systems at U.S. courts and the SEC. For defaults in the early years of the sample, records may be unobtainable. Moreover, in populating the database in earlier years, S&P focused on obtaining data for defaults involving relatively large amounts of debt. Smaller defaults are more likely to appear if the firm filed its bankruptcy petition with one of the major bankruptcy courts, such as those in New York or Delaware. Almost all the firms are U.S. firms, and most had publicly issued debt or equity outstanding at default.

The release of the database that we use ends in February 2004, but bankruptcies appear in the database only after they are resolved (because only then can ultimate recovery be determined). This raises the possibility of bias: Firms that take a long time to emerge from bankruptcy are more likely to be omitted from our analysis. A common supposition is that the debt of such firms tends to have larger LGDs. In later drafts of

this paper, we will estimate models that include corrections for censoring, but in this draft we simply omit all bankruptcies that began after 2002. We also perform various robustness checks. We believe that censoring is not a major issue for our analysis.

2.1 LGD measures

Our instrument-level measure of LGD is (1-R/D), where R is the total dollar amount of the recovery received by holders of the instrument and D is the total amount owed according to the terms of the debt contract. Our firm-level measure is $(1-\sum R_i/\sum D_i)$, where $\sum R_i$ is the sum of recoveries on all of the firm's debt instruments and D_i is the sum of amounts owed. Thus, firm-level LGD is a weighted average of the LGDs on the firm's individual obligations.

Two matters complicate measurement: Discounting, and the fact that the amount of claims allowed by the bankruptcy court may differ from the face amount outstanding on the bankruptcy date. Throughout the paper, our null hypothesis is one of no systematic risk in the size or timing of recoveries, in which case LGD risk is fully diversifiable and cash flows should be discounted at the risk-free rate. Our preferred measure discounts the recovery at emergence back to the date of bankruptcy using implied zero-coupon U.S. Treasury yields estimated using the method of Fisher, Nychka and Zervos (1994). We also obtain results for an undiscounted measure and for the measure reported in LossStat, which is discounted using pre-default contractual interest rates on each instrument. Such results are not described in detail in this draft because they do not differ much.

The dollar value received at emergence sometimes exceeds the amount of principal outstanding on the bankruptcy filing date. In these cases, if D is measured as the amount outstanding, the undiscounted LGD is negative, sometimes by a considerable amount. One reason that recoveries exceed principal is that accrued interest as of the filing date is an allowable claim and is added to the principal amount outstanding. Another reason is that interest continues to accrue after filing. Such interest is treated as junior to all other debt claims except when the instrument is secured by assets worth more than the amount of principal and pre-petition interest, in which case post-petition accrued interest is treated as secured debt. Ideally we would observe the exact amount of post-petition interest that is treated by the court as a secured claim, but the database does not record this, nor does it record the value of collateral. It only records the total amount of interest accrued between the bankruptcy date and the emergence date.

To address this problem, our preferred measure sets D, the amount of the claim, using a three-part rule: 1) D is principal plus pre-petition accrued interest in cases where the recovery is less than this amount; 2) D is principal plus pre-petition interest plus post-petition interest where the recovery is greater than this sum (yielding a negative LGD), or 3) D is the amount of the recovery if neither 1) nor 2) holds, producing an LGD of zero. Results are qualitatively robust to an alternative that uses 1) for all unsecured debt and 2) for all secured debt. Moreover, most results are robust to using outstanding principal on the bankruptcy date as the value of D.

2.2 Sample characteristics

Table 1 presents some summary statistics for the full sample and for a subsample of bankruptcies for which we were able to obtain Compustat data. The date of Compustat balance-sheet and income-statement variables is the latest fiscal year-end date that precedes the bankruptcy date. Where the fiscal year-end is more than 1.1 years before the bankruptcy date, we eliminate the firm from the Compustat-matched subsample.

As shown in the table, average firm-level LGD is not far from 50 percent regardless of the measure chosen, but individual-firm LGDs range widely, with the best outcome being a *gain* of almost 30 percent of the amount of the claim. (As discussed further below, gains can occur because of fluctuations in the market value of the firm between the time that cash and liabilities of the firm are allocated to claimants and the time of emergence.) Different measures of the amount of claims on the bankruptcy estate do not differ too much on average. The sample includes a wide range of firm sizes, but on average the firms are fairly large, with median total debt a bit below $300 million and median total assets a bit below $400 million. The median firm had approximately a zero net worth at the fiscal year-end before filing (liabilities/assets = 1) and had three debt instruments outstanding. The typical firm spent about a year in bankruptcy.

3.0 Non-debt claimants, subordination, and courts and market prices

Some institutional features of bankruptcy and debt contracting in the U.S. are helpful in interpreting the statistics reported below and in comparing results with those of other studies.

3.1 Other claimants

At emergence from bankruptcy, firm value is allocated not only to holders of pre-bankruptcy debt claims, but also to pay administrative costs of the bankruptcy, to pay taxes, to other claims such as accounts payable, and to repay debtor-in-possession (DIP) loans, if any. DIP loans provide liquidity to the firm while it is in bankruptcy and are repaid at emergence. DIP loans are effectively senior to most pre-petition debt claims.

As noted, our data report only recoveries on debt instruments, so our firm-level LGDs represent a lower-bound estimate of the value of the firm's assets at emergence. In effect, we assume that the sum of non-debt claims experiences the same LGD as that for the sum of debt claims. We check robustness by using the shares of different types of non-debt claims in total liabilities as predictors and find little evidence that they predict our measure of LGD. For example, accounts payable are usually treated as "general unsecured claims" or "senior debt." Other things equal, it would seem that a larger share of accounts payable in total liabilities should reduce firm-level recovery according to our measures, since a larger share of firm value at emergence should be allocated to the accounts payable. But empirically, we find no relationship, nor any relationship with the share of other non-debt liabilities. We take such evidence as supporting our assumption that non-debt LGD is similar to debt LGD.

We lack data on DIP loans, so any systematic variation in the share of firm value at emergence allocated to DIP loans could affect our results. Discussions with market participants lead us to believe that DIP loan drawdowns are usually small relative to pre-petition debt. Often they serve as backstops that give vendors the confidence to deal with the bankrupt firm. However, DIP loans might have a material effect on debtholder LGDs in some cases. We will try to obtain DIP balances at emergence for a random subsample in order to get a sense of their importance.

3.2 Subordination

In the U.S., most subordination is contractual. At issuance, the indenture for a subordinated debt instrument specifies the existing debt instruments to which the new debt is subordinated. The indenture specifies that, at emergence from bankruptcy, holders of the subordinated debt will make side-payments of their recovery to holders of the debt to which theirs is subordinated, up to the point at which the recipients' bankruptcy claims are fully satisfied. Leaving aside the subordination agreement, subordinated debt is just another general unsecured claim, that is, it is "senior unsecured

debt." The subordination agreement is a private contract that is typically enforced and implemented by the bankruptcy court as part of the agreed-upon plan of reorganization, but if the bankruptcy court does not enforce it, separate lawsuits for enforcement of the agreement must be litigated in other courts.

Often accounts payable and other general unsecured claims are not included in the list of debt to which the instrument is subordinated. Thus, in some cases, if the gross recovery received by subordinated debtholders is not exhausted by the contractual side-payments, subordinated debtholders may have LGDs less than 100 percent even if some senior claimants have LGDs greater than zero. This is not a violation of absolute priority. None of these details of contractual subordination are material for our firm-level LGD estimates, but they may be material for instrument-level analyses.

Structural subordination refers to cases where debt is a claim on a holding company and the debt is not guaranteed by subsidiary operating companies. Holding company debtholders are not legal claimants in the operating company bankruptcies and will receive a recovery only if the holding company's equity interest in the subs is worth something at emergence (or if the holding company has other assets). Thus, structurally subordinated debtholders often lose everything or almost everything. Because we are interested in recovery to the firm as a whole, without regard to the structure of its debt, we have identified cases of related-company bankruptcies in LossStat and have combined each set of related entities into a single simulated entity. There are fourteen such cases. Results are qualitatively robust to use of uncombined data.

3.3 Market value at emergence is not known when the plan is made

In U.S. Chapter 11 bankruptcies in which the firm is not liquidated, which are the vast majority in our data, the plan-of-reorganization that is confirmed by the court is typically designed well in advance of the emergence date. The plan specifies amounts of cash as well as equity, debt and other liabilities of the new firm that are to be received by pre-bankruptcy creditors. The amounts are based on estimates of the value of the firm, but such estimates may be incorrect, or value may change between the date the plan is made and the emergence date. Thus, the pattern of recoveries to individual debt instruments can appear to imply a violation of absolute priority when, legally, no such violation occurred. For example, a plan might specify that secured debtholders get cash for the full amount of their claim and senior unsecured debtholders get equity in the new firm. Even though the estimate might be that the senior debtholders will not be paid in

full, if at emergence the equity is worth more than expected, the senior debtholders might have a negative LGD and do better than secured debtholders. More generally, for our preferred measure of LGD, negative firm-level LGDs typically occur because the firm is worth more at emergence than was expected.

4.0 Simple non-parametric measures

In many credit risk-management applications, bad-tail losses are the primary concern. Loss rates in the tail are usually associated with high default rates. Thus, the behavior of average LGDs during periods of high default rates relative to LGDs during normal periods is of primary interest. In asset-pricing applications, again a correlation with default rates is material, but ultimately it is the correlation of credit losses with returns on other assets that matters.

Panel A of Figure 2 plots mean equally-weighted and value-weighted firm-level LGDs by year, and panels B and C plot the S&P all-corporate default rate, real U.S. GDP growth rates, and annual total returns on the S&P 500 equity index, respectively. Note that U.S. recessions as dated by the NBER occurred from July 1990 – March 1991 and from March 2001 – November 2001. Panel D plots the number of bankruptcies in the sample in each year.

An important fact to emphasize is that the number of firm-level bankruptcies per year is not large. In good years, the number is typically below the 30 observations that is the common lower bound from basic statistics for a "large" sample. The numbers are particularly small in 1987-89, so we will de-emphasize experience in those years when making interpretations.

The small numbers are not unique to this study. For example, S&P's (2003) annual default-rate study records 33 defaults of all kinds in 1995, versus 25 in our sample of bankruptcies alone. S&P records 89 defaults of all kinds in 1991, versus 48 bankruptcies in our sample. Most other studies of systematic risk in LGD have used databases with similar numbers of firm-level bankruptcies, but the small numbers have not been so evident because the focus has been on individual debt instruments (as noted previously, the typical large-corporate firm has multiple instruments outstanding). Overall, we should expect inferences about time-varying determinants of LGD to be imperfectly robust across studies.

Returning to Panel A of Figure 2, dollar-weighted mean LGDs by year are a bit more volatile than equally-weighted means, but both measures show somewhat higher average LGDs in what are conventionally thought of as bad years than in good years. An association of average LGDs with the default rates shown in Panel B is evident by inspection, but the relationship is not mechanical. For example, default rates were quite high in 1991 but average LGD was not particularly high for firms declaring bankruptcy in that year, and LGD was high in 1998 even though default rates were only somewhat higher than usual. The association between LGDs and GDP growth or equity returns is even less robust.

Figure 3 is a kernel-smoothed frequency plot of good-year versus bad-year LGDs, where bad episodes are judgmentally defined as 1989-90 and 2000-02 and all other years are "good." The higher means in bad years that are shown in Figure 2 are associated with substantial right-shifts of the distribution in bad years. LGDs near 100 percent are about twice as frequent during bad years as during good years, and the mode is near 70 percent in bad years versus near 50 percent in good years. If we remove from the sample all firms with an LGD of 60 percent or worse, the distributions of remaining LGDs are similar in good and bad years. These facts are consistent with two hypotheses: 1) consistent with conventional intuition, all asset values are worse during bad years, so the distribution of LGD is shifted to the right in bad years (except for those cases already with a loss of 100 percent); or 2) the process by which firms are selected into bankruptcy tends to select a higher proportion of bad-LGD firms in bad years, while not having much effect on less-bad-LGD firms. In the latter view, the population of nonbankrupt firms always includes many that are economically insolvent, and variations in LGD are driven by variations in severity of insolvency of firms that are selected into bankruptcy. Because explicit modeling of default is central to this question, our paper can only provide auxiliary evidence about the relative realism of the two hypotheses. Learning which is more realistic may be central to proper modeling of systematic risk in both default rates and LGD.

As noted previously, the insolvency of many of the bankrupt firms may have occurred long before the bankruptcy filing. Figure 4 displays a histogram of book equity-to-asset ratio values for firms in the Compustat-matched subsample at the fiscal year-end before filing. More than half of the firms were book-value insolvent before filing.

4.1 Firm-level versus individual-firm LGD distributions

Figure 5 reports smoothed frequency distributions of LGDs for the individual debt instruments associated with firms in our full sample and also firm-level LGDs. Viewing the firm-level LGDs as approximations of the firms' asset values at emergence, expressed as a fraction of the debt claims, the picture is a striking illustration of the utility of viewing individual claims as being approximately collar options. The firm-level distribution has a fairly smooth hump shape with mode in the 50 to 60 percentage-point range, but the individual-instrument distribution is quite bimodal. About one-third of instruments lose 10 percent or less of the amount of the claim, and about one-quarter lose 90 percent or more, with another 11 percent losing 80 to 90 percent. In a typical case, where firm assets are sufficient to cover only about half of the aggregate claim, the most senior claims are wholly in the money and are paid in full, whereas subordinated claims are out of the money and recover nothing or almost nothing.[1]

If one wishes to understand systematic variations in bankrupt-firm value and determinants of such variations, performing analysis at the individual-instrument level is at least inconvenient. The non-linearities in individual-claim payoffs are a concern even if they roughly average out. Moreover, the relative frequency with which claims with different levels of seniority appear in the sample has changed over time. Panels A and B of Figure 6 report the share in the number of all instruments outstanding for bankrupt firms in each year for bank debt (which is often secured), senior secured bonds, senior unsecured bonds, and subordinated bonds. The share of bank debt has risen over time while that of subordinated bonds has fallen. Although the labels are not a perfect guide to true seniority (what matters is an instrument's priority relative to other claims), one might expect time variation in average instrument-level LGDs just because the mix has changed. Such variation is especially to be expected in samples that do not include all the outstanding debt instruments of bankrupt firms (such partial coverage characterizes the samples in most previous studies, for example, many omit all or almost all bank debt claims).

[1] 8 percent of bankrupt firms have only a single debt instrument outstanding, and another 10 percent have only a single class of debt as defined by the bankruptcy court. Firm-level and instrument-level LGD will be the same or similar in our measures for these firms. Many of these instruments are in the middle range of the distribution in Figure 5.

5.0 Regression results

We estimate simple ordinary least-squares models of LGD.[2] We use four variables to represent the systematic state of the world: Year dummies, the S&P all-corporate default rate, U.S. annual real GDP growth, and the total annual return on the S&P 500 index. In most specifications, the value for each variable for a given observation is from the year of the bankruptcy filing, but some specifications use the year of emergence. We emphasize the year dummies in describing results because we find them easier to interpret. The omitted year is 1993, selected because the simple mean LGD in that year is not far from the sample mean and the number of bankruptcies for that year is not too small. The dependent variable is our preferred measure of ultimate recovery discounted at the risk-free rate, but results are qualitatively robust to use of undiscounted LGD or other measures.

Other predictors fall into four categories: Debt structure at bankruptcy; characteristics of the bankruptcy process; industry dummies; and, for the Compustat-matched subsample, balance sheet measures.

Results for a base specification appear in the first panel of Table 2. Focusing first on the year-dummy coefficients, and bearing in mind that the bad years in the sample in terms of default rates were 1990-91 and 2000-2002, the evidence of worse LGD in bad years is mixed. Coefficient values are large for 2000-2002, implying an extra 13 to 15 percentage points of LGD in those years. But they are small in 1990-91 and in all other years except 1998. As shown in Figure 2, the simple mean LGD for 1998 was among the largest in the sample, but default rates were moderate, GDP growth rates high, and equity returns quite high. Thus, it appears that substantial volatility in annual averages is possible for idiosyncratic reasons.

Turning to the other predictors in Panel 1, one of the most robust results we find is that the firm's debt structure has economically and statistically significant predictive power for LGDs. On average, a firm with all bank debt has an LGD that is 35 percentage points better than a firm with no bank debt, a huge effect. Firms with all subordinated debt have LGDs that are about 10 percentage points worse than those with none. We speculate that the bank debt is influential because it is usually high in absolute priority

[2] Because LGD is approximately bounded within [0,100], and because there is significant mass at 0 and 100 percent, OLS regression errors are not normally distributed. As of this writing, we have estimated a 2-sided Tobit model for the base-case specification in column 1 of Table 2. Results were quite similar to those using OLS.

and it has pre-bankruptcy control rights in the form of covenants. Bankers may continue lending even to an insolvent firm as long as asset value is sufficient to keep their recovery option in-the-money, but if asset value deteriorates to something close to the upper strike on their collar option, they force the firm into bankruptcy in order to protect their recovery. Because banks begin to suffer losses at higher asset values when they supply a larger share of all debt, they are likely to pull the plug earlier for such firms. That is, they act more forcefully to prevent high firm-level LGDs the more the share of bank debt in total debt. Our ideas about the role of subordinated debt are less clear, but it is possible that this variable acts as a proxy for firms that experienced leveraged buyouts. We will report results of investigations of this possibility in the next draft.

The inclusion of debt-structure control variables is an important reason that systematic variation in LGD is small in Panel 1 of Table 2 whereas it is much larger in the simple means shown in Figure 2. An unusually large share of firms that declared bankruptcy in 1989-91 had little or no bank debt outstanding. (The estimated coefficient on the share of bank debt is similar if bad years are dropped from the sample, so the variable is not a proxy for the 1989-91 episode.)

Another robust result is that creditors of public utilities experience better LGDs. This is the only industry indicator in the base specification partly because there is an obvious reason for the better performance of utilities: They are regulated entities with local monopolies and thus are very likely to have positive operating cash flow in the long run.

We include bankruptcy-process variables in the base specification because we focus more on understanding than on *ex ante* prediction and because some of them may be useful as controls for sample selection. Moreover, as noted below, omitting them does not materially change results. Time in bankruptcy, measured in years, is neither a statistically nor an economically significant predictor of LGD. Although the sign is as expected, with a coefficient near 2 percentage points of LGD per year in bankruptcy, time-driven variations are small relative to total variation.[3] This is somewhat surprising given the usual presumption that asset value destruction is larger the longer the firm is in Chapter 11, but it is consistent with results by Covitz et al. (2004), who focus on this issue.

[3] Only 11 percent of sample firms are in bankruptcy for more than two years. In contrast to Covitz et al. (2004), our sample is not characterized by a trend toward shorter bankruptcies. Mean and median time to emergence was 1.14 and .9 years for 1990 filings and 1.08 and .97 years for 2000 filings.

Pre-packaged bankruptcies also do not have much smaller LGDs than others, with the coefficient on the dummy variable implying about a 4 percentage point improvement. This magnitude is fairly robust across firm-level specifications, but it is never measured precisely. Somewhat surprising to us is that time spent in default on debt before bankruptcy is filed is associated with better LGDs. Our prior was that assets would be destroyed at a rapid rate during the interregnum. The variable may be correlated with the factors that determine when a firm defaults and when it files, so again it appears we need a better understanding of the bankruptcy decision.

We include the identity of the court that supervises the bankruptcy in part because, as noted previously, S&P's data-generating procedures imply there may be some oversampling of bankruptcies at the courts which are reputed to handle many large-corporate bankruptcies, which we understand to be New York and Delaware. Conventional wisdom is that these courts are more efficient, which makes it surprising that cases handled there experience significantly worse firm-level LGDs on average in the absence of sample selection.

Panels 2, 3 and 4 of Table 2 report results when the state of the world is measured by default rates, GDP growth, and equity returns, respectively. GDP growth effects are quite small, but default rates and equity returns are economically materially associated with average LGDs. Given a peak default rate of 3.6 percent and an equally-weighted sample mean of 1.6 percent, the default-rate coefficient of 2.5 implies LGDs about 5 percentage points worse than average in the high-default years in our sample. Given a sample average annual equity return of 12 percent and a worst return of -22 percent (a difference of 30 percentage points), the equity return coefficient implies an average LGD in a bad year about 7 percentage points worse than in the average year. These are smaller effects than are displayed by the year dummies for 2000-2002, but the attenuation arises because LGDs were not particularly high during 1990-91, controlling for other firm characteristics, even though default rates and equity returns were bad in those years.

When all three continuous measures are included in the same regression, as in Panel 5 of Table 2, coefficient magnitudes are not too different, except that the GDP growth coefficient has the wrong sign.

Table 3 displays results of some robustness checks. The first panel collapses year dummies into two bad-episode dummies, one for 1990-91 and one for 2000-2002, which give the same impression as the set of individual-year dummies: Little systematic

variation in the early episode, but a significant effect for 2000-20002. Results are similar if the first episode includes 1989-91.

The second panel includes only variables observable *ex ante,* while the third shows results when only year dummies and the public-utility dummy are included. Both offer more evidence of a systematic effect in 1989-90, but this is also in the period when we would expect court-related sample selection to be stronger.

The adjusted R^2 in Panel 3 of Table 3 is quite low. An examination of the sources of the better predictive power of the base specification relative to the specification in Panel 3, the debt structure variables account for a significant fraction of the variation in firm-level LGDs. If the bankruptcy-process variables were included in Panel 3 of Table 3, the R^2 would rise only to 0.06. An important implication is that systematic effects are not very important in explaining individual LGD variations. This does not imply they are relatively unimportant at the portfolio level, where non-systematic sources of variation tend to wash out, but it does mean that managing portfolio characteristics that have large effects on LGD, such as selecting firms with lots of bank debt in their capital structures, may be more important to the LGD portion of the portfolio's expected return than systematic variation in LGD. More generally, our results show that choosing the seniority of individual debt instruments is not the only means by which an investor can manage expected LGD at the portfolio level.

The fourth panel shows results when fifteen more dummies classifying industry of the firm are included. To save space, only coefficients for those industries with an estimated coefficient larger than 5 appear in the table. Only those for telecom and computer (includes semiconductor and software) are statistically significant. The telecom effect becomes small and insignificant if the years 2000-2002 are omitted from the sample. That is, some industry dummies do not capture general industry effects, but rather time-specific distress. This is one reason we omit most industry dummies from most specifications, because we cannot predict which industries will experience distress in the future, so the dummies might actually hinder understanding by standing in for the industry shocks experienced during the sample period's bad years.

5.1 Is it just the bubble?

As noted previously, the source of much of the evidence of systematic variation of LGD at the firm level is the years 2000-2002. Panels 1-4 of Table 4 mimic Table 2, but observations with either a bankruptcy filing date or an emergence date during 2000-2003

are eliminated. The economic size of the coefficients on the default rate, GDP growth, and equity return variables is reduced and they are no longer statistically significant. For the regressions in Panel 5, we began with the full sample and eliminated 32 firms that we judgmentally classified as "bubble" firms. These included all telecom firms and also firms in other industries that were primarily telecom equipment makers or suppliers, such as certain semiconductor firms, as well as any internet firms (eToys is an example). Relative to the full-sample base case, this reduces the year dummy coefficients for 2000-2002 by about one-third. If we use episode dummies, as in Table 3, the coefficient for the second episode is 6.75, with a p-value of 0.04 (not shown). The coefficient on a default-rate variable in a regression run on this subsample is similar to that shown in Table 4, although that on the equity return variable in a similar regression is about 0.15 (p-value 0.03). Overall, it appears that poor average recoveries during 2000-2002 were driven by more than just bubble firms, but this single historical episode is a crucial driver of any interpretation of this paper's evidence that systematic variation in LGD is economically large.

5.2 Bankruptcy year, or emergence year?

In previous studies, it has been conventional to set the timing of LGD according to the date of default or bankruptcy, perhaps in part because most have used market prices of debt instruments soon after default as the measure of LGD. We follow the same dating convention in most of this paper to promote comparability. However, it may be more appropriate to focus on conditions in the year bankruptcy ends, since ultimate LGD depends on the value of assets in the year of emergence. In Table 5 we take a first step toward examining the relative power of conditions in emergence and bankruptcy years to predict firm-level LGD. In all the regressions reported in Table 5, the usual debt-structure, utility and bankruptcy-process variables are included, but coefficients are not shown in the table to save space. In the first panel we report coefficients on dummies for the year of emergence. These are similar to bankruptcy-year coefficients in Table 2 for 2000-2002 but are a bit larger for the 1990-91 episode. In panels 2-4 we run horse races between continuous measures of economic conditions dated in the year of bankruptcy and in the year of emergence. Year of emergence wins, with even GDP growth being economically significant and with the right sign, although year of bankruptcy is equally powerful in the case of equity return variables. In panel 5 we include both bankruptcy-year and emergence-year dummies. Emergence-year wins again, but interpretation is a

bit difficult because of co-linearity across the two sets of dummies: The bankruptcy-year coefficients usually have negative signs, the R^2 is little affected by adding one set of dummies to the other, and none of the coefficients is statistically significantly different from zero. Such co-linearity is to be expected given that the median firm spends less than a year in bankruptcy.

When we omit 2000-2003 from the sample, again evidence of systematic variation largely disappears, except that the default rate in the emergence year remains economically and statistically significant. On the whole, it appears that conditions at emergence matter more than conditions at bankruptcy, but the robustness of the evidence in favor of material systematic variation in LGD remains in doubt.

5.3 Instrument-level results and the role of seniority in systematic risk

Table 6 presents results for our standard specification run on instrument-level data, for the pool of all instruments and for subsets including only bank debt, only unsubordinated bonds, and only subordinated bonds. Panel 1 displays results for the firm-level base case for convenience of comparison. Panel 2 displays results for all instruments. The mean LGD for the pool of instruments is near 50 percent, similar to the firm-level mean LGD, so the somewhat larger coefficients on the 2000-2002 dummies imply somewhat larger systematic variation than results from the firm-level data. The differences are not huge, but the results in Panel 2 are consistent with the use of instrument-level data imparting some bias to measured systematic variation. Other results are also somewhat sensitive to the change to instrument-level analysis, perhaps because of correlations with the instrument-priority dummies that appear at the bottom of the table. The share of bank debt remains significant but its economic impact is measured as much smaller than at the firm level, and the apparent impact of prepackaging the bankruptcy becomes larger.

Panels 3-5 report results for the different classes of instrument seniority. It is important to note that the label on an instrument is not a perfectly reliable indicator of its place in a firm's debt structure (for example, senior subordinated debt at a firm with only subordinated debt outstanding would be the equivalent of bank debt at a more typical firm). However, the labels are correlated with priority, and are what previous researchers have used to measure relative priority.

Sample-mean instrument-level LGDs are 23, 52 and 78 percent for bank debt, senior bonds, and subordinated bonds, respectively. Focusing on the coefficients on dummies

for 2000-2002, the magnitudes are largest for the senior bonds (around 25), next largest for subordinated bonds (a bit less than 20), and smallest for bank debt (around 10). Comparing these coefficients to sample means, 2000-2002 saw an increase in LGD of roughly half for senior unsecured debt and for bank debt. In contrast, the sample mean LGD for subordinated debt is 78 percent, so the increase was more on the order of one-quarter. All of the impacts are much larger than indicated by either the firm-level analysis or the full-sample instrument-level regression in Panel 2.

We are far from ready to draw firm conclusions from this single exercise, but we do regard the results in Table 6 as preliminary support for our concerns about making inferences about firm-level behavior from evidence obtained using simple linear models run on individual defaulted debt instruments.

5.4 Balance sheet composition

We intend to conduct further investigations of the predictive power of balance sheet and income statement variables, but so far have not found evidence of such power. Firm size, book leverage measured several ways, market-to-book, whether a firm's equity is publicly traded, and the composition of assets and liabilities (such as the share in property, plant and equipment or accounts receivable) all have statistically insignificant coefficients (not shown in tables). As noted previously, the same is true for variables that measure the composition of liabilities, such as the share of accounts payable or "other" liabilities in total liabilities.

6.0 Systematic risk at the instrument level: preliminary remarks

In the next draft, we will shed more light on the distortions in measures of systematic risk that can arise when pools of individual instruments are the basis for estimation. We will also shed light on whether senior instruments are likely to be more or less exposed to systematic risk than subordinated instruments.

6.1 Distortions

At the moment, we plan to focus on simulations. Given firm-value processes that are free of systematic risk, we will examine whether time-variation in the mix of instruments of different seniority in an estimation sample can lead to measured systematic variation even though there is none. We will also examine the effect of omitting most bank debt

and/or some subordinated debt from estimation samples, as has been common in previous studies. We don't know what we'll find.

6.2 Exposure

As noted previously, reasonable people may draw quite different conclusions about the economic importance of systematic risk in LGD from our results. A reader who wishes to maintain a hypothesis of no systematic risk need not worry too much about systematic variation at the individual instrument level. However, a practitioner who concludes systematic variation is material is likely to be quite interested in how variations in firm value will filter through to individual instrument LGDs. Is very senior debt more or less exposed than subordinated debt? This subsection will shed some light on the matter.

Preliminary simulation results indicate that general statements cannot be made about the relative effect of systematic variations in firm value on recoveries to senior versus subordinated instruments. Results depend on the shape of the distribution of individual-instrument recoveries and how that distribution is shifted in bad times. However, under a hypothesis that the empirical distributions shown in Figure 3 are representative, we should be able to offer some insight into relative exposure. Very preliminary results from a single exercise hint that the proportional impact is larger for relatively senior debt than for very junior debt. If this result holds up, it will mean that banks have to worry about systematic risk in LGD more than bondholders.

7.0 Comparison with previous studies

Remains to be written. To summarize, prior work claims strong evidence of material systematic variation, whereas our evidence is much weaker. To the extent we can do exercises similar to those done by others and show exactly why results differ, that would be nice, but differences in datasets may limit our ability to do so. Some of our auxiliary qualitative results can be found in Frye's earlier work, but interpretations are more forceful here because of the focus on firm-level LGDs. Acharya et al worked with the same data that we use for part of their results, but at the instrument level. We have not yet conducted tests of the relative importance of industry and general macroeconomic conditions in the manner of their work.

8.0 Concluding remarks

Standard practice in finance is to view debt as a contingent claim on the value of the assets of a firm. In this paper, we apply the same reasoning to the prioritized claims among debt holders after a firm has entered bankruptcy. In contrast to the existing empirical literature on recoveries, we take seriously this view of individual instruments as collar options on the firm value. Accordingly, we analyze the determinants of firm-level recovery as an essential first step in modeling recoveries at the instrument level. Our evidence that bank debt has a crucial role in firm-level LGD, and that the majority of bankrupt firms were probably known to be insolvent well before the bankruptcy date, suggests that an important next step is a better understanding of the bankruptcy decision itself.

Taken together, previous studies could be read as having settled the question about existence of systematic variation in recovery rates on defaulted corporate debt in the affirmative. The obvious implication would be that attention should turn to satisfactory modeling and prediction of such systematic variation in debt pricing and portfolio credit risk models. We view our evidence as re-opening the empirical question because so much of our evidence of systematic variation rests on the 2000-2002 experience. More importantly, we suggest and implement a method of measuring LGD that conforms more closely to intuition about the determinants of systematic variation, and we present evidence that determinants not previously considered are important.

Appendix A

S&P LossStat Methods of Measuring Value Received at Emergence

Still to be written.

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Table 1. Sample summary statistics

Data are for all bankruptcies in the LossStat database with dates before 2003. Debt claim amounts and assets-of-firm are in millions of dollars. Compustat data are as of the most recent fiscal year-end date preceding the bankruptcy filing date, except that data for year-end dates more than 1.1 years prior to the filing date are eliminated. The LGD measure is our preferred measure that sets LGD to 100 percent when the dollar amount of total recoveries exceeds principal plus prepetition accrued interest but is less than principal plus all accrued interest. Number of instruments in the number of separate debt obligations of the firm at the time bankruptcy is filed.

	Total debt claims on estate			LGD (percent)		Characteristics of Firm			
	Principal alone	Plus prepetition accrued interest	Plus all accrued interest	Discounted	Nominal	Number of instruments	Assets of firm	Liabilities/ assets	Years in bankruptcy
Full Sample									
Mean	570	609	628	53	51	4			1.2
Median	263	285	291	55	53	3			0.9
Minimum	12	12	12	-36	-27	1			0.0
Maximum	10032	10495	10495	100	100	24			5.8
Number of bankruptcies= 446									
Compustat-Matched Subsample									
Mean	547	582	602	54	52	4	1003	1.2	1.1
Median	275	298	303	57	55	3	389	1.0	0.8
Minimum	12	12	12	-28	-27	1	1	0.3	0.1
Maximum	5092	5366	5393	100	100	24	14630	10.7	5.8
Number of bankruptcies= 321									

Table 2. Base-case regressions

The dependent variable is the preferred LGD measure (discounted using risk-free rates). The default rate is the S&P all-corporate default rate, in percent. The GDP growth rate is contemporaneous annual GDP growth, in percent. The S&P 500 return is the total annual return, in percent. The shares of bank, subordinated and secured debt are the fractions of debt outstanding at default of each type of debt. The utility dummy indicates regulated public utilities, such as natural gas delivery companies. Court dummies identify the location of the court that supervised the bankruptcy. The omitted court is "all others."

	(1)		(2)		(3)		(4)		(5)	
Independent	Base case		Default rate		GDP growth		S&P 500 return		All three	
Variable	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value
Intercept	51.61	<.0001	54.21	<.0001	60.93	<.0001	61.06	<.0001	48.27	<.0001
Default rate			**2.52**	0.0239					3.29	0.1037
GDP Growth					**-0.38**	0.6414			1.97	0.1405
S&P 500 return							**-0.23**	0.0007	-0.17	0.0290
Bankruptcy year:										
1987-88	-5.23	0.5380								
1989	**4.82**	0.5574								
1990	**6.65**	0.3185								
1991	**-4.38**	0.4420								
1992	-5.03	0.4161								
1994	2.32	0.7442								
1995	-5.49	0.4223								
1996	-2.82	0.6845								
1997	3.62	0.6295								
1998	16.63	0.0360								
1999	4.09	0.5255								
2000	**13.25**	0.0403								
2001	**13.52**	0.0302								
2002	**15.06**	0.0230								
Share bank debt	**-35.51**	**<.0001**	-33.97	<.0001	-33.49	<.0001	-35.55	<.0001	-35.35	<.0001
Share sub debt	**9.90**	0.0235	7.74	0.0634	7.61	0.0695	8.01	0.0527	8.42	0.0422
Share secured	-2.70	0.5604	-2.95	0.5241	-3.39	0.4667	-2.29	0.6179	-2.52	0.5840
Utility	**-29.91**	0.0010	-33.80	0.0002	-34.80	0.0001	-34.17	0.0001	-33.15	0.0002
Time in bankruptcy	1.95	0.1917	1.61	0.2678	1.62	0.2676	2.07	0.1549	1.90	0.1924
Time in default pre-filing	-4.18	0.1649	-5.61	0.0542	-6.10	0.0370	-5.94	0.0398	-5.22	0.0736
Prepackaged bankruptcy	-4.49	0.1606	-4.33	0.1784	-4.37	0.1768	-4.89	0.1259	-4.97	0.1201
Court dummies:										
California	8.48	0.2483	8.03	0.2780	8.04	0.2799	7.69	0.2951	7.63	0.2987
New York	**16.68**	0.0012	15.89	0.0020	16.04	0.0019	15.25	0.0028	15.12	0.0030
Delaware	**7.00**	0.0789	8.91	0.0238	9.16	0.0211	8.64	0.0272	7.92	0.0445
Illinois	9.47	0.4229	12.92	0.2691	15.94	0.1727	10.79	0.3528	9.61	0.4090
Texas	-8.75	0.1814	-8.28	0.2049	-7.65	0.2440	-7.87	0.2244	-9.02	0.1663
Unknown	5.81	0.1616	0.14	0.9702	-1.37	0.7142	0.95	0.7986	2.72	0.4838
Number observations	443		443		443		443		443	
Adjusted R-squared	0.24		0.22		0.21		0.23		0.23	

Table 3. Robustness

The dependent variable is the preferred LGD measure (discounted using risk-free rates). The episode dummies combine years 1990-91 and 2000-2002, respectively. The shares of bank, subordinated and secured debt are the fractions of debt outstanding at default of each type of debt. The utility dummy indicates regulated public utilities, such as natural gas delivery companies. Court dummies identify the location of the court that supervised the bankruptcy. Industry dummies are based on a judgmental collapsing of industry codes provided by S&P into sixteen categories. Only utility, telecom, computer, airline, food, construction and auto are shown because all others had coefficients smaller than 5 in absolute value. The others include oil & gas, media and publishing, health care, textiles, basic cyclical industrials, hotel gaming and leisure, financials, real estate, and all-other.

	(1)		(2)		(3)		(4)	
Independent	Episode		Ex ante		Time only		Industry	
Variable	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value
Intercept	54.35	<.0001	55.31	<.0001	50.65	<.0001	50.00	<.0001
Episode dummies for yrs:								
1990-1991	**-0.92**	0.7884						
2000-2002	**10.88**	0.0004						
Bankruptcy year:								
1987-88			1.40	0.8687	-0.53	0.9548	-5.20	0.5456
1989			**9.30**	0.2608	**8.69**	0.3376	2.42	0.7712
1990			**11.03**	0.0973	**10.07**	0.1680	5.52	0.4077
1991			-1.63	0.7758	-1.68	0.7894	-5.58	0.3323
1992			-5.00	0.4273	-6.41	0.3538	-6.34	0.3067
1994			4.88	0.4975	1.72	0.8270	0.94	0.8962
1995			-1.80	0.7929	-6.87	0.3518	-5.48	0.4282
1996			-0.40	0.9539	-10.51	0.1642	-2.47	0.7244
1997			7.63	0.3075	1.18	0.8849	0.56	0.9423
1998			17.84	0.0174	10.60	0.1925	12.93	0.1086
1999			5.82	0.3268	0.37	0.9537	1.91	0.7716
2000			15.22	0.0113	**7.81**	0.2300	**10.31**	0.1179
2001			15.82	0.0066	**7.62**	0.2248	**10.12**	0.1146
2002			16.48	0.0073	**8.13**	0.2135	**8.20**	0.2365
Share bank debt	-35.21	<.0001	-31.66	<.0001			-33.53	<.0001
Share sub debt	9.53	0.0231	9.36	0.0336			10.91	0.0178
Share secured	-2.70	0.5571	-5.49	0.2348			-0.78	0.8743
Utility	-32.84	0.0003	-27.84	0.0023	-24.38	0.0147	-28.06	0.0028
Time in bankruptcy	2.05	0.1570					2.18	0.1616
Time in default pre-filing	-5.23	0.0705					-2.95	0.3362
Prepackaged bankruptcy	-4.74	0.1380					-5.49	0.0907
Court dummies:								
California	8.33	0.2564					7.89	0.2886
New York	16.23	0.0015					14.34	0.0063
Delaware	8.07	0.0395					6.20	0.1252
Illinois	10.78	0.3524					16.46	0.1730
Texas	-6.89	0.2878					-7.52	0.2715
Unknown	2.70	0.4841					4.24	0.3125
Selected industry dummies:								
telecom							**14.38**	0.0098
computer							**10.89**	0.0526
airlines							10.43	0.2660
food							-11.70	0.0895
construction							-8.03	0.3346
auto							9.06	0.2216
Number observations	443		443		443		443	
Adjusted R-squared	0.23		0.20		0.03		0.25	

Table 4. One episode is the main source of evidence of systematic variation

The dependent variable is the preferred LGD measure (discounted using risk-free rates). The default rate is the S&P all-corporate default rate, in percent. The GDP growth rate is contemporaneous annual GDP growth, in percent. The S&P 500 return is the total annual return, in percent. The shares of bank, subordinated and secured debt are the fractions of debt outstanding at default of each type of debt. The utility dummy indicates regulated public utilities, such as natural gas delivery companies. Court dummies identify the location of the court that supervised the bankruptcy. The omitted court is "all others."

	(1)		(2)		(3)		(4)		(5)	
Independent	Base case		Default rate		GDP growth		S&P 500 return		No bubble firms	
Variable	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value
Intercept	51.21	<.0001	53.55	<.0001	48.44	<.0001	54.85	<.0001	50.60	<.0001
Default rate			**-1.05**	0.5254						
GDP Growth					**1.31**	0.2257				
S&P 500 return							**-0.10**	0.4174		
Bankruptcy year:										
1987-88	-6.59	0.4490							-5.59	0.5103
1989	**3.98**	0.6351							**4.69**	0.5682
1990	**5.33**	0.4380							**6.37**	0.3389
1991	**-5.02**	0.3866							**-4.38**	0.4404
1992	-4.96	0.4316							-5.21	0.3990
1994	3.28	0.6507							2.67	0.7078
1995	-3.76	0.5936							-5.08	0.4580
1996	-1.48	0.8355							-2.76	0.6900
1997	4.13	0.6043							3.62	0.6294
1998	15.52	0.1002							14.47	0.0836
1999	-1.74	0.8573							3.24	0.6163
2000									**11.21**	0.0930
2001									**8.48**	0.1884
2002									**6.93**	0.3377
Share bank debt	-39.46	<.0001	-39.03	<.0001	-39.18	<.0001	-39.31	<.0001	-34.90	<.0001
Share sub debt	15.33	0.0117	14.72	0.0107	14.95	0.0089	13.33	0.0207	12.35	0.0081
Share secured	-0.83	0.8865	-1.17	0.8378	-1.31	0.8181	-1.05	0.8538	-0.92	0.8485
Utility	-28.82	0.0021	-30.61	0.0010	-30.64	0.0009	-30.85	0.0009	-30.06	0.0010
Time in bankruptcy	1.13	0.5327	0.96	0.5852	0.99	0.5705	0.82	0.6375	2.25	0.1361
Time in default pre-filing	-4.09	0.2207	-4.88	0.1274	-4.62	0.1484	-4.94	0.1229	-4.33	0.1593
Prepackaged bankruptcy	-9.80	0.0250	-10.65	0.0136	-10.89	0.0116	-10.73	0.0129	-4.23	0.2110
Court dummies:										
California	17.05	0.0982	17.76	0.0832	17.26	0.0919	17.70	0.0842	8.44	0.2664
New York	18.75	0.0173	18.56	0.0165	17.94	0.0205	18.33	0.0180	15.27	0.0061
Delaware	6.06	0.3926	8.41	0.2028	7.64	0.2485	8.85	0.1805	6.69	0.1080
Illinois	n.a.								9.49	0.4749
Texas	-9.41	0.4478	-5.63	0.6457	-5.56	0.6479	-7.37	0.5461	-7.39	0.2608
Unknown	6.37	0.2446	5.37	0.3167	5.74	0.2851	5.08	0.3438	4.07	0.3385
Number observations	269		269		269		269		411	
Adjusted R-squared	0.27		0.27		0.27		0.27		0.21	

Table 5. Year of bankruptcy, or year of emergence?

The dependent variable is the preferred LGD measure (discounted using risk-free rates). The default rate is the S&P all-corporate default rate, in percent. The GDP growth rate is contemporaneous annual GDP growth, in percent. The S&P 500 return is the total annual return, in percent. All the usual base specification variables present appear in the regressions but are suppressed in this table to save space.

Independent Variable	(1) Base case Coeff.	p-value	(2) Default rate Coeff.	p-value	(3) GDP growth Coeff.	p-value	(4) S&P 500 return Coeff.	p-value	(5) Both years Coeff.	p-value
Intercept	48.01	<.0001	48.01	<.0001	65.22	<.0001	61.07	<.0001	52.30	<.0001
Default rate-bankrupt yr			**-0.15**	0.9062						
Default rate-emerge yr			**5.15**	0.0002						
GDP Growth-bankrupt yr					0.34	0.6912				
GDP Growth-emerge yr					-2.45	0.0175				
S&P 500 return-bankrupt							**-0.17**	0.0152		
S&P 500 return-emerge							**-0.18**	0.0103		
Emergence year:										
1987-88	-12.60	0.4004							5.40	0.8125
1989	**2.24**	0.8993							**19.03**	0.4111
1990	**12.51**	0.0995							**22.20**	0.1195
1991	**8.81**	0.1777							**11.77**	0.2092
1992	-1.01	0.8504							3.36	0.6091
1994	7.93	0.1910							5.20	0.4957
1995	-11.73	0.0872							-14.82	0.1200
1996	-1.79	0.7866							-3.38	0.7904
1997	2.04	0.7575							0.30	0.9832
1998	7.93	0.2855							4.19	0.8168
1999	8.74	0.2091							9.89	0.6238
2000	**12.57**	0.0436							**16.37**	0.4670
2001	**16.81**	0.0083							**18.07**	0.4718
2002	**18.35**	0.0017							**16.80**	0.5348
2003	**13.17**	0.0411							**10.45**	0.7195
Bankruptcy year:										
1987-88									-23.34	0.2378
1989									-14.13	0.3799
1990									-4.36	0.7130
1991									-10.73	0.2474
1992									-9.87	0.2044
1994									-1.45	0.8588
1995									-4.17	0.6871
1996									-4.20	0.7376
1997									-5.15	0.7378
1998									3.65	0.8408
1999									-13.37	0.5085
2000									-6.17	0.7817
2001									-4.57	0.8525
2002									-0.96	0.9717
Number observations	443		443		443		443		443	
Adjusted R-squared	0.25		0.24		0.22		0.24		0.24	

Table 6. Instrument-level evidence

The dependent variable is the preferred LGD measure (discounted using risk-free rates). It is firm-level LGD in Panel 1 and individual-debt-instrument LGD in all other panels. The shares of bank, subordinated and secured debt are the fractions of debt outstanding at default of each type of debt. The utility dummy indicates regulated public utilities, such as natural gas delivery companies. Court dummies identify the location of the court that supervised the bankruptcy. The omitted court is "all others." Instrument seniority is as stated in LossStat and does not take into account the nature of the firm's debt structure.

	(1)		(2)		(3)		(4)		(5)	
Independent	Base case		Insrument level		Bank debt only		Senior unsecd		Sub debt only	
Variable	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value
Intercept	51.61	<.0001	43.82	<.0001	41.43	<.0001	46.74	<.0001	65.53	<.0001
Bankruptcy year:										
1987-88	-5.23	0.5380	-5.32	0.2066	-14.77	0.0927	-9.39	0.2847	0.51	0.9288
1989	4.82	0.5574	9.31	0.0253	-12.92	0.2018	23.48	0.0023	15.59	0.0048
1990	6.65	0.3185	7.10	0.0524	-4.90	0.5002	9.40	0.1725	14.54	0.0059
1991	-4.38	0.4420	1.58	0.6308	-14.93	0.0294	10.00	0.1064	6.57	0.1565
1992	-5.03	0.4161	1.49	0.6649	-13.12	0.0625	10.33	0.1164	-0.36	0.9425
1994	2.32	0.7442	5.57	0.2362	-10.11	0.2346	16.57	0.0574	9.31	0.2025
1995	-5.49	0.4223	-5.05	0.2229	-16.55	0.0353	-7.64	0.2711	6.26	0.3594
1996	-2.82	0.6845	1.52	0.7127	-6.37	0.3777	8.97	0.2408	-8.64	0.2338
1997	3.62	0.6295	4.96	0.2592	-4.69	0.5342	8.95	0.2694	6.03	0.4395
1998	16.63	0.0360	23.18	<.0001	23.20	0.0058	14.01	0.0831	28.16	0.0003
1999	4.09	0.5255	8.07	0.0324	1.07	0.8766	10.71	0.1145	6.89	0.2747
2000	13.25	0.0403	19.63	<.0001	12.62	0.0519	27.18	0.0001	15.63	0.0073
2001	13.52	0.0302	18.13	<.0001	9.24	0.1451	20.84	0.0012	21.87	<.0001
2002	15.06	0.0230	23.61	<.0001	7.82	0.2289	35.73	<.0001	14.14	0.0367
Share bank debt	-35.51	<.0001	-10.91	0.0029	-9.73	0.1271	-4.78	0.4625	-8.06	0.2230
Share sub debt	9.90	0.0235	-7.42	0.0169	-4.44	0.3536	-38.42	<.0001	0.35	0.9462
Share secured	-2.70	0.5604	18.03	<.0001	31.85	<.0001	14.81	0.0134	2.54	0.6936
Utility	-29.91	0.0010	-29.00	<.0001	-27.09	0.0167	-17.59	0.0161	-49.78	<.0001
Time in bankruptcy	1.95	0.1917	0.48	0.5482	2.85	0.0652	-2.73	0.0782	0.35	0.7598
Time in default pre-filing	-4.18	0.1649	-3.07	0.0721	-4.19	0.2078	-1.56	0.6375	-2.41	0.3305
Prepackaged bankruptcy	-4.49	0.1606	-8.64	<.0001	-4.83	0.1163	-14.35	<.0001	-5.94	0.0482
Court dummies:										
California	8.48	0.2483	5.93	0.2154	-13.78	0.0797	10.74	0.3970	9.30	0.1607
New York	16.68	0.0012	18.42	<.0001	17.16	<.0001	13.79	0.0123	18.08	0.0004
Delaware	7.00	0.0789	7.68	0.0009	5.44	0.1097	9.71	0.0427	9.94	0.0156
Illinois	9.47	0.4229	-2.74	0.5331	16.73	0.0440	-15.26	0.0281	1.15	0.9297
Texas	-8.75	0.1814	-2.49	0.5386	-12.34	0.0486	8.03	0.2936	-5.09	0.4796
Unknown	5.81	0.1616	5.88	0.0179	3.51	0.3742	9.53	0.0794	3.47	0.3913
Instrument priority										
Secured bank debt			-40.53	<.0001	-37.97	<.0001				
Unsecured bank debt			-4.42	0.3177						
Senior secured bonds			-22.38	<.0001			-21.59	<.0001		
Subordinated debt			23.34	<.0001						
Junior subordinated debt			37.81	<.0001					14.53	0.0019
Number observations	443		1902		667		613		620	
Adjusted R-squared	0.24		0.49		0.26		0.36		0.17	

Figure 1. Debt instruments as collar options

In this example, in the case of Firm A, the value of the firm at emergence is below the lower strike value needed to make the General Unsecured Claims class in-the-money, so such claims recover nothing. In the case of Firm B, value is above the upper strike for General Unsecured Claims, so such claimants enjoy a full recovery, and further increases in firm value would not improve their payout.

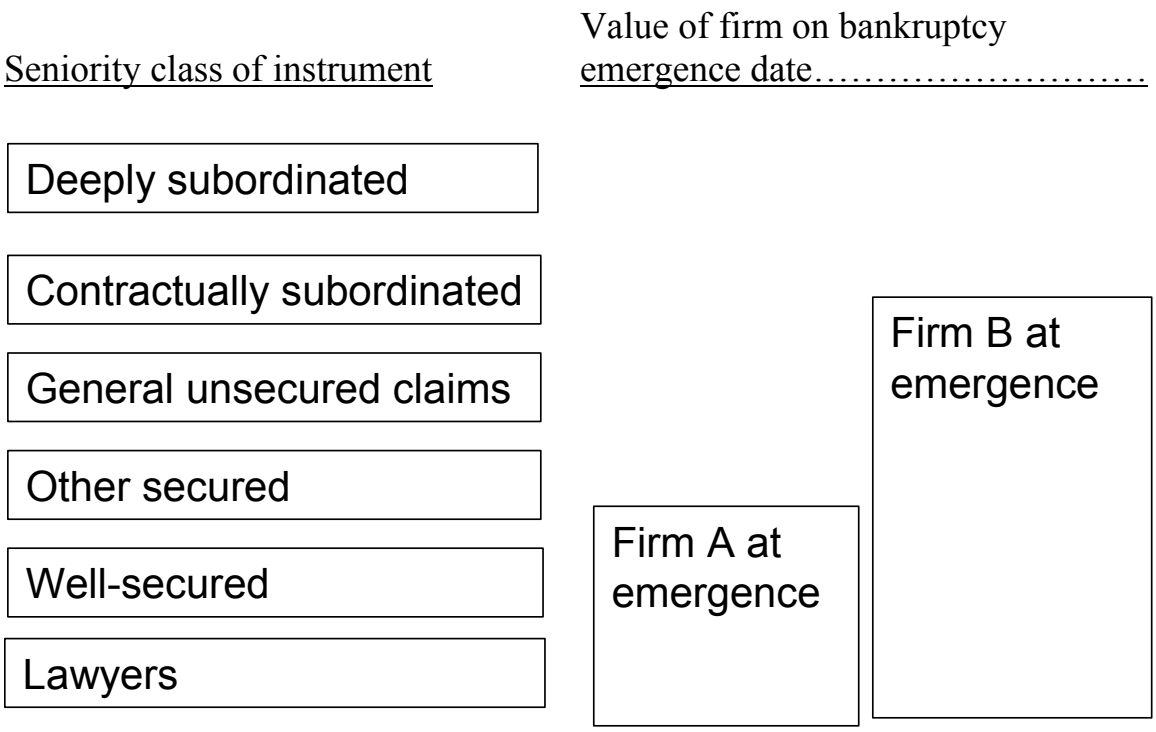
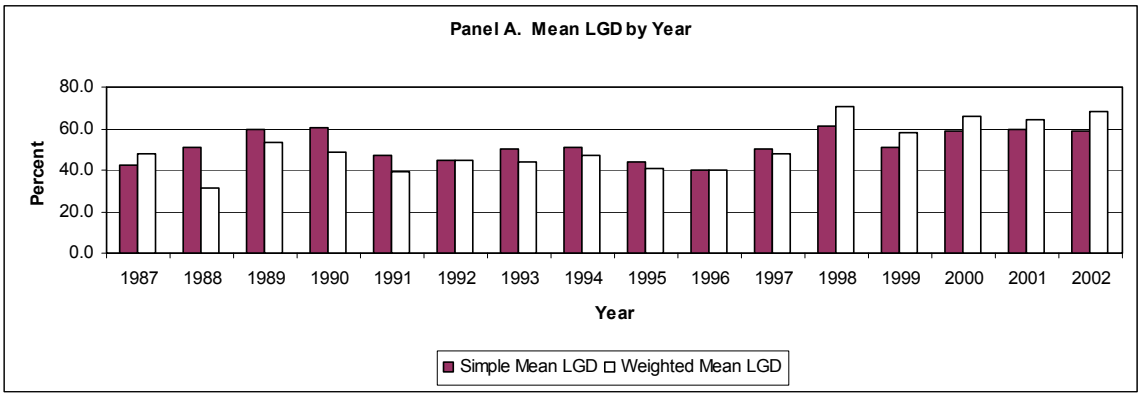
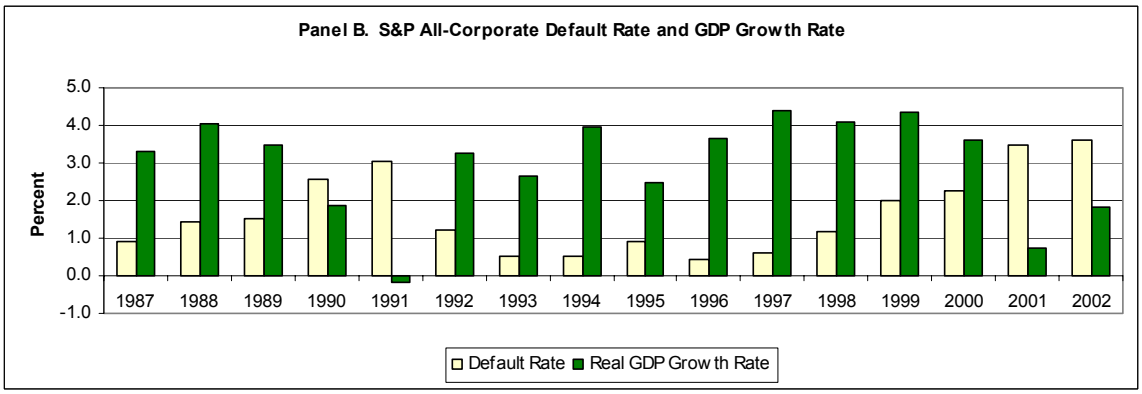
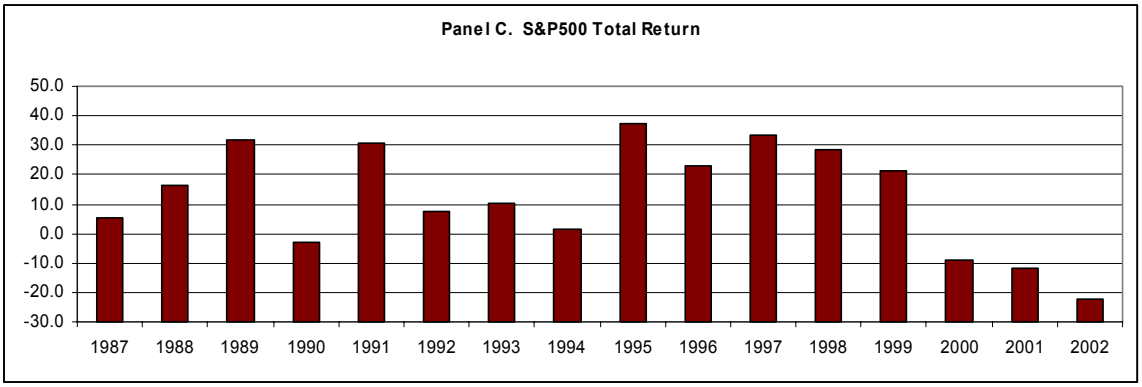


Figure 2.

Weighted-mean LGDs are dollar-weighted using amounts of principal outstanding at bankruptcy.







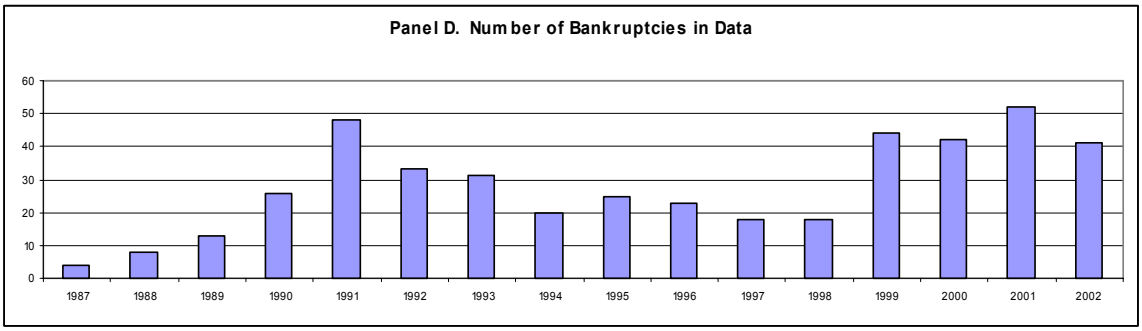


Figure 3. Smoothed incidence of individual LGD rates, separately for good and bad years.

A normal kernel with a bandwidth of 8 percentage points of LGD was used to smooth the frequency distributions. Bad years include 1989-90 and 2000-2002, with each of those episodes shown separately, while good years are all others.

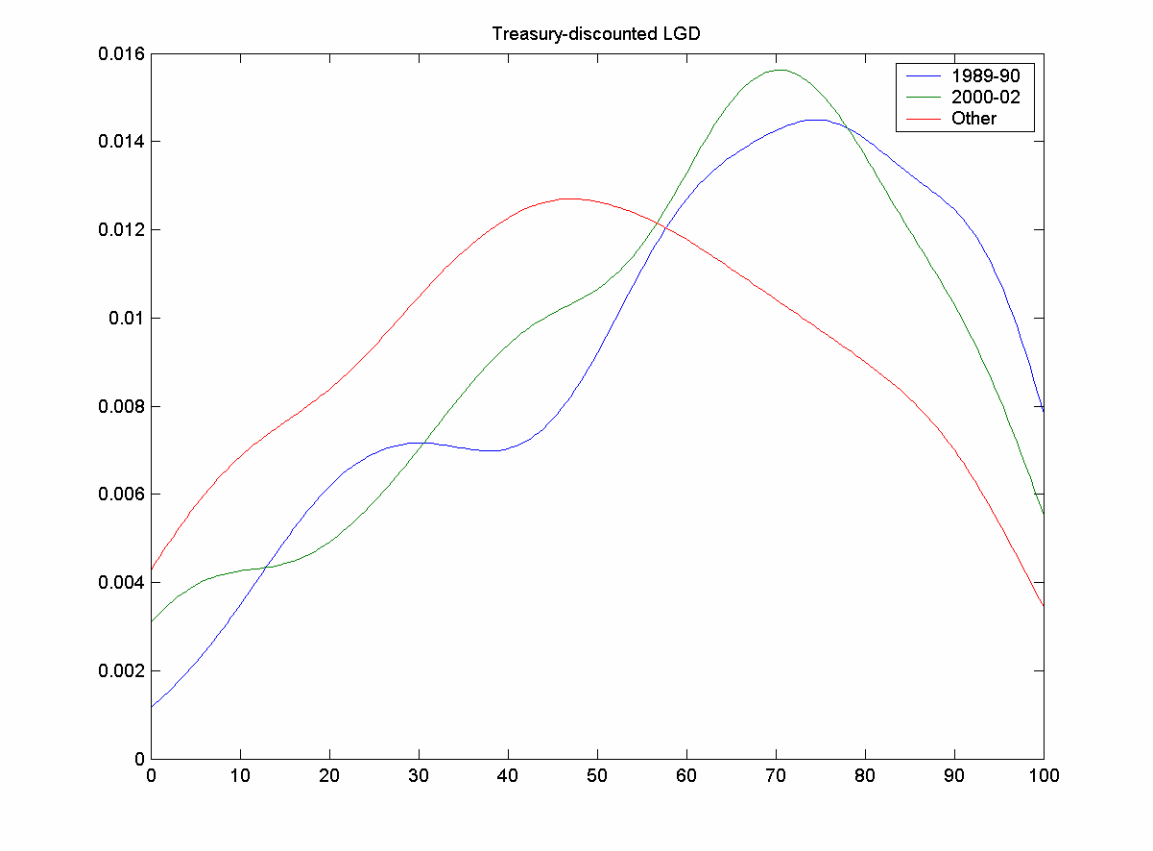


Figure 4. Frequency distribution of book leverage at fiscal year-end prior to bankruptcy.

Leverage is measured as the ratio of book total liabilities to book total assets, expressed as a percentage, and is available only for the Compustat-matched subsample.

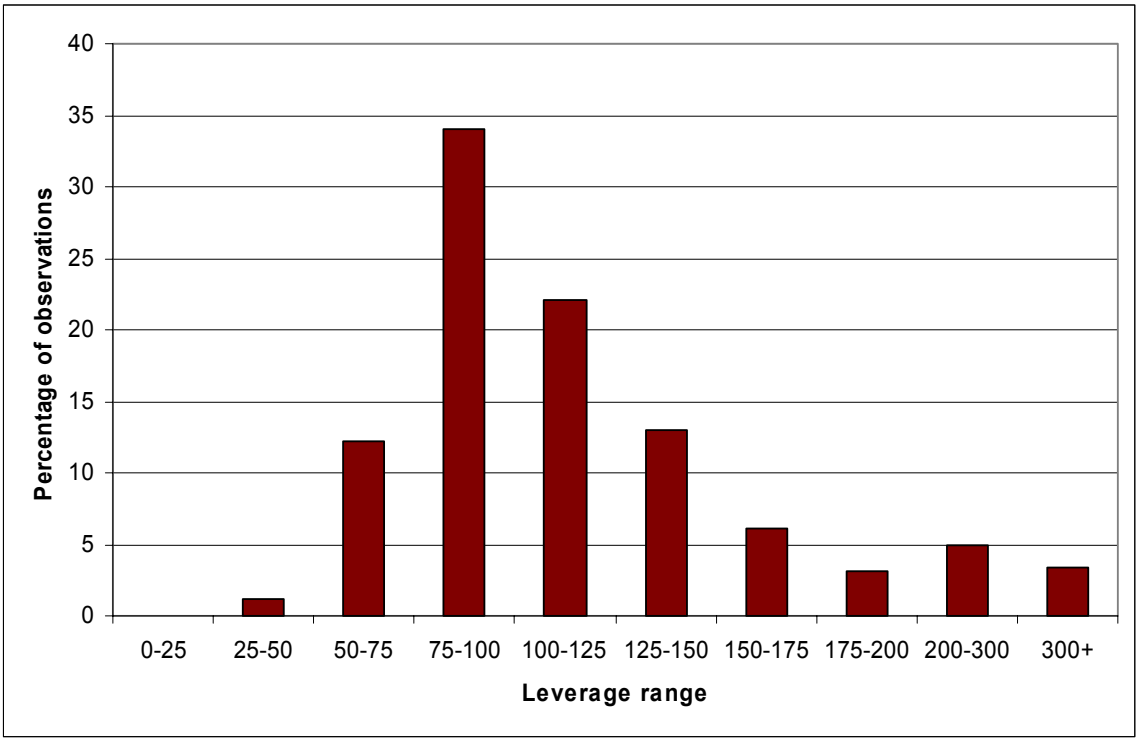


Figure 5. Smoothed frequency distributions of firm-level and individual-debt-instrument-level LGDs.

The estate LGD distribution approximates firm value at emergence, expressed as a percentage of total debt claims. The instrument LGD distribution may be viewed as the distribution of payoffs to collar options written on firm value at emergence. The preferred measure of LGD is used in calculations.

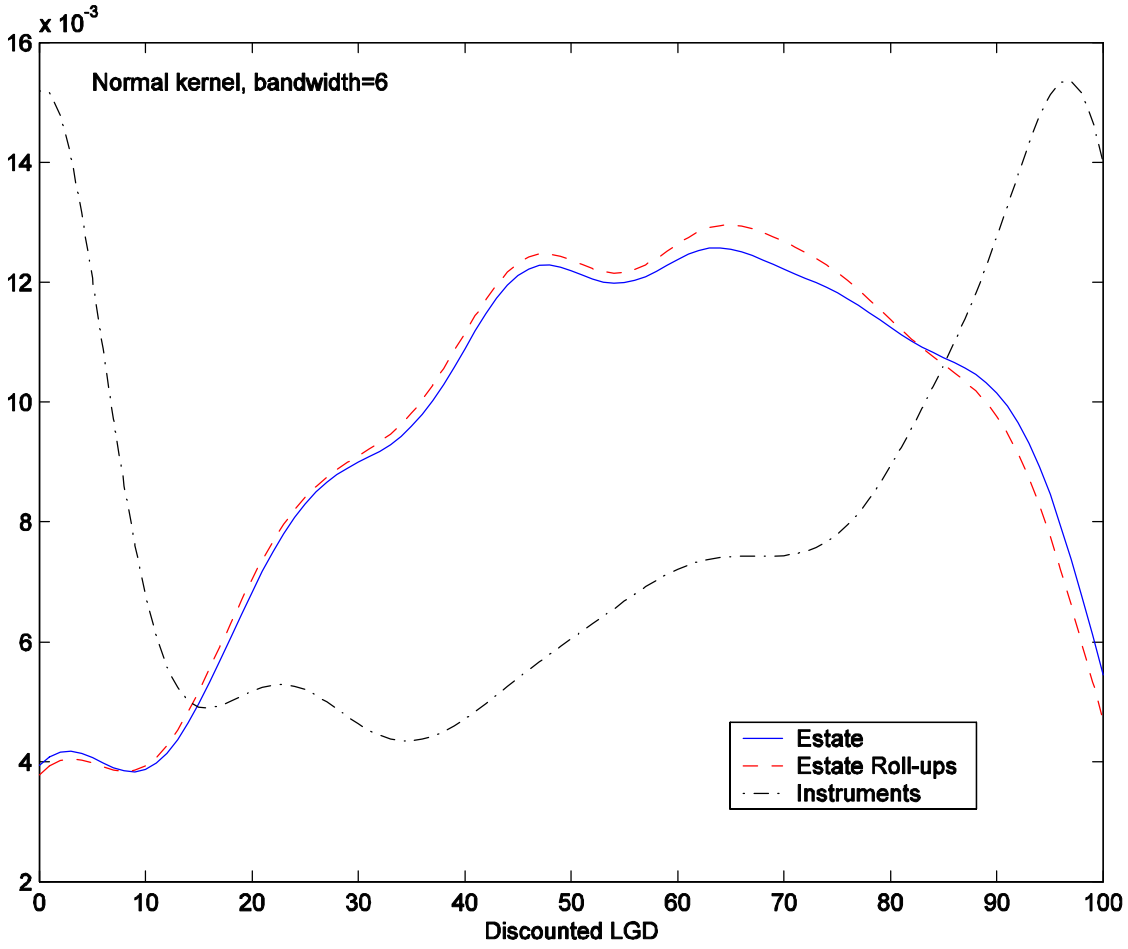
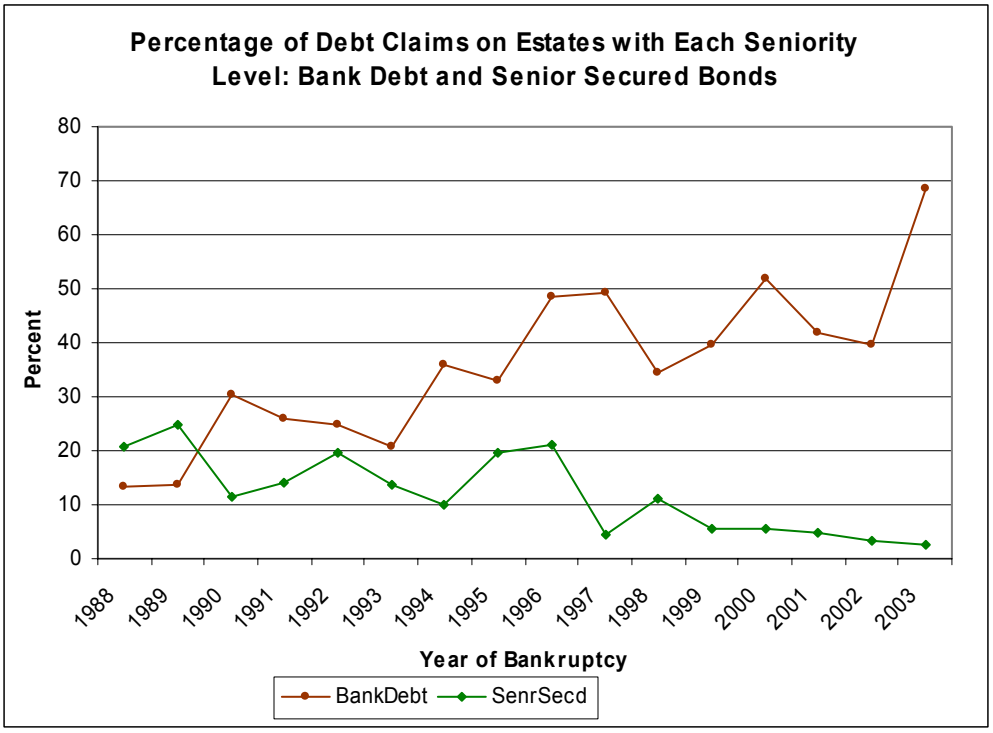


Figure 6. Share of instruments in each category of stated seniority.

Panel A: Bank debt and senior secured bonds



Panel B: Senior unsecured and subordinated bonds