Correlation in corporate defaults: Contagion or conditional independence? *

David Lando †
Mads Stenbo Nielsen

Abstract

We revisit a test for conditional independence in intensity models of default proposed by Das, Duffie, Kapadia, and Saita (2007) (DDKS). Based on a sample of US corporate defaults, they reject the conditional independence assumption but also observe that the test is a joint test of the specification of the default intensity of individual firms and the assumption of conditional independence. We show that using a different specification of the default intensity, and using the same test as DDKS, we cannot reject the assumption of conditional independence for default histories recorded by Moody's in the period from 1982 to 2006. We also show, that the test proposed by DDKS is not able to detect all violations of conditional independence. Specifically, the tests will not capture contagion effects which are spread through the explanatory variables ('covariates') used as conditioning variables in the Cox regression and which determine the default intensities of individual firms. We therefore perform different tests to see if firm-specific variables, i.e quick ratios and distance-to-default, are affected by defaults. We find no influence from defaults on Quick ratios, but some influence on distance-to-default. This suggests, that violations of conditional independence do indeed arise from balance sheet effects.

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†Both authors are at the Department of Finance, Copenhagen Business School, Solbjerg Plads 3, DK-2000 Frederiksberg, Denmark (e-mail: dl.fi@cbs.dk, msn.fi@cbs.dk).

1 Introduction

There is ample evidence that corporate defaults are correlated. Direct evidence can be found in several empirical studies which document a large time variation in default frequencies and link this variation to, among other variables, business cycle indicators. Examples of this include Nickell, Perraudin, and Varotto (2000), Shumway (2001), Duffie, Saita, and Wang (2007), and many others. Since such indicators simultaneously affect the default probabilities of many firms, their variation induces correlation between default events just as variation of common factors in asset return models induce correlation between returns.

We can also find indirect evidence that defaults are correlated by looking at market prices of traded securities. For example, credit default swap premia have significant common movements and prices of tranches of Collateralized Debt Obligations can only be reasonably explained if one assumes a significant amount of default clustering. Of course, market prices of these securities reflect not only the physical probabilities of defaults but also contain an adjustment for risk. Still, it is fair to assume that the price patterns we observe for CDO tranches can at least partially be attributed to correlated default risk.

How to best model the correlation effects is less clear. The most tractable way from an analytical standpoint is to work under a conditional independence assumption, in which a common factor structure induces covariation between the default times of different firms. Conditionally on the evolution of the common factors, defaults are independent. This is a setting in which default dependence is captured by business cycle related variables. The conditional independence structure is analyzed among other places in Jarrow, Lando, and Yu (2005), and it is applied to CDO modeling for example in Duffie and Gârleanu (2001).

A more direct way of inducing dependence between default times is to assume that there is contagion, i.e. that the actual default event of one firm either directly triggers the default of other firms or causes their default probabilities to increase[1]. Some examples of contagion models include Davis and Lo (2001), Jarrow and Yu (2001), Azipour and Giesecke (2008a) and Azipour and Giesecke (2008b). This type of contagion is clearly relevant when firms belong to the same corporate family, for example through parent/subsidiary relationships, see for example Emery and Cantor (2005). The question is whether this type of contagion is present even for firms which do not belong to the same corporate family. Note that our focus in this paper is not on 'informational' contagion in prices on equity, corporate bonds or credit default swap premia as studied for example by Collin-Dufresne, Goldstein, and Helwege (2003) and Jorion and Zhang (2008). Rather, we focus on methods for testing for conditional independence in actual defaults.

In a recent paper Das, Duffie, Kapadia, and Saita (2007) (DDKS) test whether default events in an intensity-based setting can reasonably be modeled as conditionally independent. The basic ingredient of their approach is to transform the time scale using the sum of the default intensities estimated for individual firms and then test whether defaults on this transformed time scale

[1] It is also conceivable that defaults could cause the default probabilities of competing firms to *decrease* which can also be captured by the model specifications we consider .

behave as a standard Poisson process. Based on a time series of U.S. corporate defaults, most of which are obtained from Moody's default database, and ranging from 1979 to 2004 they strongly reject that defaults are conditionally independent. However, as pointed out by the authors, this test is really a joint test of the specification of the default intensities of the individual firms *and* the assumption of conditional independence. There are at least two reasons why the second possible source of rejection - the specification of the intensities - deserves closer scrutiny.

First, when looking through the default histories in Moody's default database, it is almost impossible to locate any examples where the brief description of what caused a firm to default mentions other firms outside the corporate family. The vast majority of cases list reasons such as too much leverage, failing sales in declining markets, and lawsuits - effects that are typically captured through either firm specific explanatory variables or market wide conditions. Indeed looking at the points in time where the defaults seem to cluster more than what can be explained by the aggregate intensity in the DDKS specification, we find that none of the default stories contains any instances of contagion from other firms in the sample. This rules out at least the direct 'domino effect' explanation for clustering of defaults and also raises doubts that earlier defaults in the sample have any effect.

Second, as argued by DDKS, 'default intensity correlation accounts for a large fraction, but not all, of the default correlation´ [page 98]. The fact that a large fraction is explained in their specification makes it likely that the inclusion of extra explanatory variables can indeed explain the full dependence structure. DDKS do investigate whether inclusion of additional explanatory variables affects their conclusion but find no evidence among the variables they consider.

The first contribution of our paper is to show that a different specification of the intensity will indeed make us unable to reject the conditional independence assumption. That is, using our specification of the intensity there is no excess default clustering. Using the sample of firms listed in Moody's default database, we show that specifying the explanatory variables as in DDKS, we reject the assumption of conditional independence but using our specification, we are not able to reject using a large variety of tests. In essence, our change in specification consists in replacing a measure of the short rate with a measure of steepness of the term structure, adding industrial production (a variable also examined in DDKS) and adding the following three firm specific variables: quick ratio, short-to-long debt and the book value of assets. We will discuss this choice of the explanatory variables below.

The fact that we are unable to reject the conditional independence assumption when following the procedure of DDKS and using an appropriate set of covariates could lead us to conclude, that there are no detectable contagion effects in the data. This conclusion is premature, however. Our second contribution is to show that when contagion takes place through firm covariates (as opposed to contagion by domino effects), this will not be detected by the test procedure followed in DDKS (and in the first part of our paper). If it is the case that the default of one firm causes, say, the book value of assets of another firm to fall, and this increases the intensity of default of the other firm, then as long as the book asset value is an explanatory variable in the Cox regressions, we will not detect this as a contagion effect. While it is possible to state and prove a rigorous theorem explaining this, we consider it much more illustrative to set up a very simple example which clearly illustrates the idea. In fact, we set up the simplest structure rich enough to illustrate a contagion effect which occurs through explanatory variables but which is not detected by the test procedure based on a time transformation.

Our final contribution is to analyze contagion effects, both direct and through explanatory variables, and using both likelihood tests based on Hawkes processes and regression analysis. Hawkes processes, or self-exciting processes, are a class of counting processes which allow intensities to depend on the timing of previous events. For a recent application to corporate defaults and risk premia in CDO markets, see Azipour and Giesecke (2008a) and Azipour and Giesecke (2008b). When we use firm specific variables in the Cox regressions, the Hawkes specification does not add any explanatory power. If, however, we believe that contagion is channeled through firm specific covariates, then we should not condition on these variables in the Cox regression before testing for contagion. If we only condition on macro-economic variables and look for contagion by checking through a Hawkes specification whether downgrade intensities increase following a default, then we do detect a contagion effect. Since this effect may be due to rating agency behavior, we also perform regression tests to check for contagion through the firm-specific variables distance-to-default and the quick ratio to be defined below.

The outline of the paper is as follows: We start in Section 2 by briefly recalling the method of DDKS and define conditional independence. In section 3 we describe the data that we are using, and we give examples of the kind of default histories that we have access to in the Moody's data. In section 4 we set up the Cox regressions for estimating the default intensities of the individual firms and in section 5 we perform various tests for conditional independence using aggregate intensities. In particular, we show that with our specification we cannot reject the assumption of conditional independence but using the DDKS specification, this is not the case. We also consider a method for testing for contagion using a Hawkes process alternative. In section 6, we set up the simplest possible example of a specification in which there is contagion through explanatory variables. In this example, there is clearly not conditional independence but we will not be able to reject the tests for conditional independence if we use the time transformation based on the aggregate intensity. This example motivates our extended testing for conditional independence in which we first look for contagion effects through ratings which are used as a one-dimensional proxy for the firm-specific explanatory variables. We then perform regression tests to see if defaults affect levels of distance-to-default and the quick ratio. Section 7 concludes.

2 The conditional independence assumption

The conditional independence assumption is meant to capture a setup in which probabilities of default of individual firms are affected by exogenous 'background variables'. The variables are exogenous in the sense that they are not affected by actual defaults of firms. A helpful illustration from medical science could be pollution in a city and onsets of asthma attacks among its citizens. When the level of pollution is high, there are more asthma attacks and hence onsets of these attacks are correlated. However, conditioning on the level of pollution the onsets are independent (assuming that asthma is non-contagious). Also, asthma attacks do not affect the level of pollution. For an example with more relevance to default modeling, it is possible that increasing oil prices will cause more firms who use oil as an input in their production to default, but that the defaults will have no effect on oil prices, and conditionally on the level of oil prices defaults are uncorrelated. In models of stock returns, conditional independence is often assumed in factor models where the residual returns, i.e. the part that is not explained by the factors, are independent across firms.

Unfortunately, the fairly intuitive notion of conditional independence of defaults is somewhat technical to describe rigorously. In the appendix, we give a definition which is equivalent to but perhaps a bit more intuitive than the one used in DDKS. However, to understand the test procedure of DDKS, we do not need to actually go into this technical analysis. Instead, we proceed to repeat the idea behind the test procedure used in DDKS. The test procedure proposed in DDKS runs as follows: First, estimate individual firm intensities using Cox regressions. Then compute the sum of these intensities which under the assumption of conditional independence is equal to the aggregate default intensity. Now, transform time using the aggregate intensity and check whether aggregate defaults in the new time scale are a unit rate Poisson process. The tests then consist of checking this property using different procedures.

Formally, the default of a single debt-issuing firm i is described by the default time τ_i, and we assume that the default time can be modeled through its stochastic intensity λ_i. If the firm is alive at time t, then the intensity at time t for firm i satisfies

$$\lambda_i(t) = \lim_{\Delta t \to 0} \frac{P(t < \tau_i \leq t + \Delta t \,|\, \tau_i \geq t)}{\Delta t},$$

i.e. the probability of default within a small time period Δt after t is close to $\lambda_i(t)\Delta t$. In the intensity setting, modeling the probability of default for firm i reduces to modeling its default intensity λ_{it}. We will specify later how the intensity depends on explanatory variables, but we suppress this dependence for now. Note that specifying the individual intensities of the firms when they are alive does not describe the simultaneous distribution of the default times. If, for example, two firms always default at the same time, and this time has an exponential distribution with mean $\frac{1}{\alpha}$, then firm i would have a default intensity equal to $\alpha 1_{(\tau_i > t)}$. At the other extreme, the firms would have the same intensity if they default at independent times with the same exponential distribution. In this case, the probability of a simultaneous default is zero. This is also true under much weaker conditions than independence and we need to refer to this property in what follows. Thus, default times are said to be *orthogonal* if $P(\tau_i = \tau_j) = 0$ whenever $i \neq j$. The cumulative number of defaults among n firms is defined as

$$N(t) = \sum_{i=1}^{n} 1_{(\tau_i \leq t)} \qquad t \geq 0$$

and if the default times are orthogonal, this cumulative default process has intensity

$$\lambda(t) = \sum_{i=1}^{n} \lambda_i(t) 1_{(\tau_i \geq t)} \qquad t \geq 0$$

and the compensator of the cumulative default process is then the integral of the intensity

$$\Lambda(t) = \int_0^t \lambda(s)ds \qquad t \geq 0.$$

If we time-change the cumulative default process by the compensator, it follows from Meyer (1971)[2] that the cumulative default process becomes a unit rate Poisson process, i.e. the time-scaled process

$$J(t) = N\big(\Lambda^{-1}(t)\big) \qquad t \geq 0$$

is then a unit Poisson process with jump times $V_i = \Lambda(\tau_{(i)})$, where $0 \leq \tau_{(1)} \leq \tau_{(2)} \leq \ldots$ denotes the ordered default times. A consequence of this is that $V_1, V_2 - V_1, \ldots$ are independent exponentially distributed variables and for any $c > 0$, the binned jump times

$$Z_j = \sum_{i=1}^{n} 1_{]c(j-1),cj]}(V_i)$$

will be independent Poisson$(c)-$distributed variables. In summary, if default times are orthogonal, we can transform the time scale of the cumulative default process to obtain a unit rate Poisson process and we can then use standard properties of this process (exponential waiting times between jumps, number of jumps in a given interval is Poisson distributed with mean equal to the length of the interval) for testing. But note, however, that the conditional independence assumption is not needed to have orthogonality of the default times. If defaults of firms cause intensities of other firms to rise (but never cause an immediate default) then we have orthogonality but not conditional independence. This means that the Poisson property of the transformed process can hold even in cases where there is not conditional independence. In section 6, we provide the simplest possible example in which there is contagion in the model but the transformation test will not capture this. Had we been able to reject the DDKS tests for all specifications of default intensities, then this insight would not have changed much, since a rejection is certainly enough to rule out conditional independence. But since we are able to specify intensity processes which make us unable to reject the tests for conditional independence proposed in DDKS, the insight becomes very important. Before specifying the default intensities, we describe the data and consider some representative default stories.

[2] The result is usually ascribed to Meyer (1971) with a multitude of successive variations and extensions in e.g. Papangelou (1972), Brémaud (1972), Aalen and Hoem (1978), Cocozza and Yor (1980), Brown and Nair (1988) and Kallenberg (1990). See also Aalen and Hoem (1978) for a brief historical review.

3 Data and the default explanations

Our empirical analysis is based on corporate default data from Moody's Default Risk Service Database (DRSD), which essentially covers the period since 1970. However, the material is sparse until 1982, which we therefore choose as the beginning of our sample period. Other default studies have used the same data supplemented with additional defaults from other sources, see e.g. Li and Zhao (2006), DDKS, Davydenko (2007), and Le (2007).[3] We have chosen to rely only on the data in the Moody's database since these all have explanatory notes associated with each default allowing us to both screen the default histories for traces of contagion and for parent/subsidiary relationships. It also has the advantage of giving us an unambiguous definition of what constitutes a default event[4].

Thus our estimation will comprise all U.S. industrial firms with a debt issue registered in Moody's DRSD, and for which we are able to obtain accompanying stock market data from CRSP and accounting information from CompuStat. This leaves us for the period January 1982 to December 2005 with a total of 2,557 firms comprising 370 defaults, with an average of 1,142 and a minimum of 1,007 firms in the model at any time throughout the sample period, all of which have at least 6 months of available data.

The suspicion that default contagion is not obviously present in the data came from an inspection of all default explanations in the DRSD database. It is illustrative to consider an example of a typical explanation of a default event (our emphasis added).

> ***Heartland Wireless Communications, Inc.***, *based in Plano, Texas, develops, owns and operates wireless cable television systems and channel rights in small to mid-size markets in the central United States. Although the company has experienced strong revenue growth since its inception, posting \$78.8 million in revenues for 1997 compared to \$2.2 million in its first full operating year (1994), substantial start-up capital costs and* **an aggressive expansion strategy** *pursued by management resulted in consecutive operating losses and built up significant amounts of debt. Heartland Wireless incurred a net loss of \$134.6 million for 1997, compared to a net*

[3] Le (2007) includes defaults registered in the CompuStat database, which he notes in some instances implies that a registered default does not correspond to an actual default, but merely reflects the timing of a stock delisting event. To resolve a similar difficulty, in the case where the actual default date is known but delisting occurs prior to default, Davydenko (2007) applies an extrapolation technique to infer values for the necessary stock market variables at the actual default date, although inspection of the default data in Moody's DRSD reveals that this occasionally leads to extended periods of time, where inference can only be based on imputed data.

[4] We consider as a default any of the following events classified in Moody's DRSD: "Chapter 7", "Chapter 11", "Distressed exchange", "Grace period default", "Missed interest payment", "Missed principal payment", "Missed principal and interest payments", "Prepackaged Chapter 11", and "Suspension of payments". In particular, we do not correct the timing of a "Distressed exchange", which in the DRSD is registered as the time of completion of the exchange, although as suggested by Davydenko (2007), it would probably be more appropriate to instead collect separate information on the announcement date of the exchange.

> *loss of $61.1 million a year earlier. The technological limitations of Heartland's major product (MMDS - multichannel multipoint distribution service - has a limited number of channels it can disseminate), an* **inability to achieve sufficient subscriber levels***, and* **intense competition** *from traditional hard-wire cable television firms have applied additional pressure to the company's financial position. Mounting debt service costs and the need for additional capital induced the company to hire Wasserstein Perella & Co., an investment banking firm, to analyze all available options to finance the company's business plan and service its existing debt. In consultation with its financial advisor, Heartland Wireless announced that it would not be making interest payment due April 15, 1998 on its 13% senior notes due 4/15/2003.*

It is clear in this explanation that there is no trace of contagion. What might a contagion story have looked like in the data? We have two examples. The first concerns the famous Penn Central default - often mentioned as a contagious default event.

> *On June 21, 1970, the Penn Central declared bankruptcy and sought bankruptcy protection. As a result,* **the PC was relieved of its obligation to pay fees to various Northeastern railroads—the Lehigh Valley included***—for the use of their railcars and other operations. Conversely,* **the other railroads' obligations to pay those fees to the Penn Central were not waived**. *This* **imbalance in payments** *would prove fatal to the financially frail Lehigh Valley, and it declared bankruptcy three days after the Penn Central, on June 24, 1970.*

The source of this default history is Wikipedia and if we look in Moody's database, we learn that Penn Central was in fact a majority shareholder in Lehigh Valley, and hence they belonged to same corporate family by Moody's definition. Since we exclude defaults within the same corporate family which occur less than a month apart, this event would not have been in our data, even if we had extended back to 1970.

As an example of a default event description that does include contagion, consider the following description.

> *Town & Country, a manufacturer of fine jewelry, suffered in the early 1990s from deteriorating results and a large debt load pursuant to two debt-financed acquisitions in 1988. The default of large jewelry retailer and major Town & Country customer Zale Corp. in late 1991 seriously worsened the already difficult financial situation, which eventually led Town & Country to omit an interest payment on its debt in June 1992.*

While this does contain traces of contagion, Zale Corp is not part of our data set, and hence this story can not have contributed to a default clustering in the data. In general, we could not find any verbal accounts of contagion in the default explanations given by Moody's in the database but we were still able to reject the hypothesis of conditional independence using the DDKS specification (see below). This led us to suspect that a different specification of the intensity could lead to a non-rejection. We now discuss this alternative specification.

4 Model specification

Since testing for conditional independence involves transforming the time by a cumulative intensity, which is the sum of default intensities estimated for each firm separately, we first need to specify a model for each firm's default intensity. The critical exercise here is to determine the firm specific and macro variables which are significant explanatory variables in the Cox regressions used to specify the intensity. In the specification of individual default intensities we employ a selection of four macro economic variables collected from CRSP and the U.S. Federal Reserve Board:

- 1-year return on the S&P500 index
- 3-month U.S. treasury rate
- 1-year percentage change in U.S. industrial production, calculated from monthly data on the gross value of final products and nonindustrial supplies (seasonally adjusted)
- Spread between the 10-year and 1-year treasury rate

and five firm-specific variables collected from CRSP and CompuStat:

- 1-year equity return
- 1-year "Distance to Default"
- Quick ratio, calculated as the sum of cash, short-term investments and total receivables divided by current liabilities
- Percentage short term debt, calculated as debt in current liabilities divided by the sum of debt in current liabilities and long-term debt
- Book asset value (log).

Table 1 shows descriptive statistics for the variables to guide the interpretation of the regression coefficients obtained below. We also show average levels of the covariates for defaulting vs. non-defaulting firms.

For all balance sheet variables we substitute, if quarterly data are missing, with the latest yearly observation, and for the calculation of the "Distance to Default" measure we follow the iterative approach described in Duffie, Saita, and Wang (2007). Moreover, to comply with the mathematical foundations of our model, we require that the value of $\lambda_i(t)$ is known prior to time t, a phenomenon referred to as "predictability" in the technical literature, such that e.g. as a proxy for the book value of assets on, say January 1st, we use the number reported for December of the previous year[5]. Finally, in order to correct for observations of multiple defaults caused by parent-subsidiary relations, we disregard all consecutive default events that occur within a 1-month horizon of any previously registered default ascribed to the same parent company[6].

The doubly stochastic assumption now implies that the log (partial) likelihood function takes the form (see Andersen, Ørnulf Borgan, Gill, and Keiding (1992))[7]

$$\log L(\beta) = \sum_{i=1}^{n} \int_0^T \left(\beta_W' W_t + \beta_X' X_{it}\right) dN_i(t) - \sum_{i=1}^{n} \int_0^T R_{it} e^{\beta_W' W_t + \beta_X' X_{it}} 1_{(\tau_i \geq t)} dt \tag{1}$$

where T is the terminal time point of the estimation and n the total number of firms. W_t is a vector containing the covariates that are common to all firms whereas X_{it} contains firm-specific variables. R_{it} is an indicator which is 1 if firm i is at risk of defaulting at time t and zero otherwise and $N_i(t)$ is the one-jump process which jumps to 1 if firm i defaults at time t. We can then apply standard maximum likelihood techniques to draw inference about $\beta = (\beta_W, \beta_X)$. Table 2 reports estimates and asymptotic standard errors from two different intensity specifications: Model I which is the model analyzed in DDKS, and Model II which is an extension that incorporates a wider selection of variables. The signs of the various $\beta-$coefficients are largely as expected and consistent with the findings of DDKS (see Duffie, Saita, and Wang (2007) for parameter estimates) except that we find the role of the short-term default-free interest rate to be insignificant. Table 2 also reveals how both model I and II, somewhat surprisingly but consistent with for example Figlewski, Frydman, and Liang (2006) and Duffie, Saita, and Wang (2007), show a positive dependence of default intensities on the yearly return on the S&P500 stock index. [8]

[Table 2 about here]

[5]The issue of delayed public disclosure leads Carling, Jacobson, Lindé, and Roszbach (2007) to argue that it is more appropriate to use lagged values for both macro economic and accounting variables, although it is not clear exactly how to choose an appropriate lag length. Similarly, Koopman and Lucas (2005) suggest that macro economic variables could be lagged in order to improve causality of the model, arguing that to the extent that default events are consequences of (and thus lagged wrt.) macroeconomic fluctuations, they will appear with a certain time lag which should be corrected for. However, they also demonstrate how estimation results may be highly vulnerable to the choice of lag length.

[6]Davydenko (2007) similarly chooses to disregard all subsequent defaults within a 2-year period, which may be a more appropriate horizon. However, our shorter horizon should make it harder to specify intensities consistent with an assumption of conditional independence.

[7]We work under the usual assumption of independent filtering by assuming that the various filtering mechanisms we employ: left truncation for all firms operating on January 1st 1982 (beginning of the estimation period), (temporary) withdrawal of firms in case of lacking covariates, and right censoring of all firms operating on December 31st 2005 (end of the estimation period) do not alter the likelihood function. For thorough discussions of these issues see Andersen, Borgan, Gill and Keiding (1992) and Martinussen and Scheike (2006).

[8]Duffie, Saita, and Wang (2007) suggest that this may in part reflect business cycle effects as well as be a consequence of correlation with the idiosyncratic stock returns, and perhaps also with other variables.

Figure 1 shows monthly defaults along with the estimated cumulative default intensities for both models. Clearly, the estimated default intensities are different, but the graph also shows that it is difficult from visual inspection to tell which model gives the better fit.

We have examined the influence of additional economy-wide factors besides those appearing in Model I and II through proxies for the U.S. unemployment rate, the wages of U.S. production workers, the U.S. consumer price index, the U.S. gross domestic product in both real and nominal terms, the price of crude oil, and the spread between Moody's Aaa- and Baa-rated corporate bonds, but without finding any significant effects. In a similar fashion, we have looked at a variety of alternative indicators of financial soundness at the firm-specific level including some of the empirical default predictors proposed by Altman (1968) and Zmijewski (1984), but likewise without finding support for further expansion of the set of explanatory variables.

Ideally, we should also take specific account of debt issue characteristics such as the time of issuance, maturity, face value, coupon payments including possible step up-clauses etc. given the empirical evidence presented in Davydenko (2007) who demonstrates the influence of this type of information on the probability of default, and it could also be of importance to allow for specific industry effects given the variation in default rates across industries documented by Li and Zhao (2006). However, the lack of available debt issue information and the limited number of defaults unfortunately prevents us from performing either type of analysis on the current data set. Working with larger data sets and performing out-of-sample tests would naturally lead us to include more variables but as we will see in the next section our specification is rich enough to capture the correlation in the data.

5 Testing for conditional independence and contagion

Having estimated the default intensities of each firm, we now use the time-change technique to test whether the default arrivals of firms can be thought of as conditionally independent given the cumulative intensity. We follow DDKS by transforming the time scale using the cumulative intensity and test whether on the new time scale the default arrivals are a unit rate Poisson process. We also propose and test an extended version of the default intensity which explicitly models the possibility of contagion through a Hawkes process specification.

5.1 The time change test

To test whether the default arrivals on the transformed time scale are a unit rate Poisson process, we use the properties of a (unit rate) Poisson process that the number of arrivals in a time interval is Poisson distributed with a mean equal to the length of the time interval and that arrivals in disjoint time intervals are independent. Thus, if we split up the entire time period into intervals in each of which the cumulative intensity increases by an integer c, then the number of arrivals in each of these intervals are independent and Poisson distributed with mean c. We follow DDKS and refer to c as the bin size, since it reflects the expected number of defaults in each time interval. The larger c is, the smaller is the total number k of time intervals (and hence Poisson variables) that we get, thereby weakening the power of our statistical tests. On the other hand, by increasing c we can hope to get a clearer picture of the presence of heavy tails representing excess clustering of defaults. We use the same test statistics as those of DDKS, i.e. the Fisher Dispersion (FD) and the upper tail statistics (UT1, UT2)[9], and supplement with further tests detailed in Karlis and Xekalaki (2000). Since we only have a limited number of observations and some of the asymptotic distributions of the test statistics require a much larger amount of data (see Karlis and Xekalaki (2000)), we calculate instead for each statistic the $p-$value under the null hypothesis from a history of 100.000 simulated test statistics to improve accuracy.

The tests of the Poisson distribution listed above tend to concentrate on whether the univariate distribution of recorded defaults for a given bin size is Poisson. They therefore ignore the time series aspects. If default contagion takes place with a time lag, it is conceivable that bins with many defaults tend to be followed by bins with many defaults and vice versa. To account for this possibility we use (as an alternative to the regression test in DDKS) the additional test statistics SC1 and SC2.

The Fisher and upper tail tests are outlined in DDKS, so we only describe the remaining statistics, which we define through the following acronyms:

$$
\begin{aligned}
\text{BD} &= \frac{1}{\overline{Z}\sqrt{2(k-1)}}\sum_{j=1}^{k}\left(Z_j-\overline{Z}\right)^2-\sqrt{\frac{k-1}{2}} \\
\text{CVM} &= \frac{1}{k}\sum_{i=0}^{\infty}V_i^2 \qquad \text{with} \qquad V_i=\sum_{s=0}^{i}(|\{j \mid Z_j=s\}|-\text{Expected}_s) \\
\text{KK} &= \sqrt{k}\frac{\phi_k(t)-\exp\left(\overline{Z}(t-1)\right)}{\exp\left(\overline{Z}(t^2-1)\right)-\exp\left(2\overline{Z}(t-1)\right)\left(1+\overline{Z}(t-1)^2\right)} \text{ with } \phi_k(t)=\frac{1}{k}\sum_{j=1}^{k}t^{Z_j} \\
\text{NPA} &= \frac{1}{k^3\overline{Z}^{1.45}}\left(\sum_{i,j,l,m=1}^{k}Z_i(Z_i-Z_j-1)Z_l(Z_l-Z_m-1)1_{(Z_i+Z_j=Z_l+Z_m)}\right) \\
\text{SC1} &= \frac{1}{k-1}\sum_{j=1}^{k-1}\left(Z_jZ_{j+1}-c^2\right)^2 \\
\text{SC2} &= \frac{1}{k-1}\sum_{j=1}^{k-1}\left(Z_j-c\right)\left(Z_{j+1}-c\right)
\end{aligned}
$$

All of these tests rely on descriptions of the Poisson distribution obtained from point probabilities, moments and characteristic functions. The results of the tests are reported in Table 3. Model I refers as before to the intensity specification used in DDKS and Model II to our intensity specification. All except one test is rejected at the 5% level using the Model II specification, whereas a number of tests are rejected for the Model I specification - predominantly

[9]We correct for the apparent misprint in DDKS in the description of the upper tail median statistic by comparing the simulated median statistics to the sample *median* (instead of the sample mean). However, this implies that the median statistic by construction only will be efficient for large bin sizes.

for the large bin sizes. For bin size 8, for example, Figure 2 shows that the Model II specification has a less pronounced heaviness in the right tail of the distribution and Figure 3 shows that it also is better at eliminating serial dependence. Hence we conclude, that using our specification of the firm default intensities, we are not able to reject that the time transformed process is Poisson.

It is interesting to note that there is a deviation from the Poisson property which is not detected by the test. In Figure 4 we have plotted the distribution of default events by calendar day and we note that most defaults occur on calendar days 1 and 15. This is consistent with the frequent use of these days for coupon payments on corporate bonds. It is not enough, however, to affect our test results since the defaults are spread out over a 24-year period and thus we do not see any large default clusters on any particular calendar day. Subsequent work by Kramer and Löffler (2008) indicates that an improvement in fit can be obtained by explicitly modeling this effect as through a baseline intensity.

5.2 A contagion alternative

All of the tests performed above rely on transforming the time using the estimated intensities. We now perform a different, likelihood-based test which does not rely on the time-transformation. We use an extended model which explicitly includes a contagion effect in the intensity specification. To be specific, following Hawkes (1971b) and Hawkes (1971a), we use an intensity of the form[10]

$$\lambda^c_{it} = R_{it}\left(e^{\beta'_W W_t+\beta'_X X_{it}} + \int_0^t (\alpha_0+\alpha_1 Y_s)e^{-\alpha_2(t-s)}dN_s + \delta\right) \qquad t\geq 0$$

where Y_s denotes the log book asset value of the firm defaulting at time s. The idea behind the specification is to allow the default of a firm to influence all other intensities. The immediate effect is modeled as an affine function of Y thus allowing for larger firms to have a higher impact on the individual default intensities. The exponential function makes the default impact decay exponentially with time at a rate. The log (partial) likelihood function follows from this expression by standard arguments (Rubin (1972), Ogata and Akaike (1982), Andersen, Ørnulf Borgan, Gill, and Keiding (1992))

$$\begin{aligned}\log L(\alpha,\beta) = \quad & \sum_{i=1}^n \int_0^T \log\left(e^{\beta'_W W_t+\beta'_X X_{it}} + \int_0^t (\alpha_0+\alpha_1 Y_s)e^{-\alpha_2(t-s)}dN_s + \delta\right) dN_{it} \\ & -\sum_{i=1}^n \int_0^T R_{it}\left(e^{\beta'_W W_t+\beta'_X X_{it}} + \int_0^t (\alpha_0+\alpha_1 Y_s)e^{-\alpha_2(t-s)}dN_s + \delta\right) 1_{(\tau_i\geq t)}dt\end{aligned}$$

and we can apply maximum likelihood inference as before[11]. Note that α_2 may be taken as a measure of the horizon of influence of a default on the overall default proneness of remaining firms (Hawkes (1971b)).

[10] See Kwiecinski and Szekli (1996) for alternative specifications.

[11] Ogata (1978) gives sufficient conditions to ensure consistency and asymptotic normality of the estimators under an additional assumption of stationarity, and Ozaki (1979) presents simulation results that support numerical feasibility of maximum likelihood estimation for self-exciting processes.

Note that the Hawkes specification is used to supplement the Cox regression specifications used in Models I and II. Since Model I caused a rejection of the Poisson property, it is possible that this is caused by a contagion effect which the Hawkes specification might capture. However, as shown in Table 4, there is no explanatory power added by this specification. Even if the Model II specification did not reject the Poisson property of the time-transformed cumulative default process, we use the Hawkes specification as a robustness check. As shown in Table 5, we find no significance of this addition in the contagion related parameters. We do find a significant effect of adding a constant term to the default intensities. There is a 'floor' on all default intensities of 3.5 basis points arising from the constant term δ. This term may be capturing a small misspecification of the proportional hazard regression or of the functional form of the hazard function. The functional form (using the exponential function of a linear function of the covariates) forces intensities to be very small when default covariates are in very 'safe territory' far from values held by risky firms. It is possible that even if true intensities are not as small for safe firms as shown in the proportional hazard regression, this deviation is not penalized heavily in the likelihood function and therefore does not affect our time-change test. However, if we allow a constant term in the regression, it does show up as significant, but very small.

6 Contagion through covariates

We have shown that with a different specification of the explanatory variables in the hazard regressions, we are not able to reject the hypothesis of conditional independence using this specification but on the same sample we reject using the DDKS specification. It is thus tempting to conclude that contagion effects are eliminated as long as we specify our covariates carefully. There are, however, possibilities of contagion effects which are not captured by the tests performed here and in DDKS. In essence, the time transformation of the intensity may not capture contagion effects which occur through the covariates. That is, if the default of firm A causes (say) the leverage of firm B to rise, and subsequently the increased leverage ratio contributes to the default of firm B, then we will not see this as a contagious default effect since the tests we are performing are conditioning on the evolution of the covariates. The increased leverage will cause the default intensity of firm B to rise, and therefore this will not be seen as a contagion effect violating conditional independence. A full test of contagion should address these 'weak' contagion effects as well. In this section we first give a basic illustration of the problem we are addressing using the simplest possible example which is rich enough to capture the effect. We then set up tests for contagion using rating as a proxy for quality of covariates and looking at covariates directly.

6.1 Contagion through covariates - an illustration

It is possible using the language of filtrations to give a rigorous definitions of what we are trying to capture, but we believe that the example below is more useful as a reference for the discussion and gives a much clearer illustration of the main point.

Consider a collection of firms whose default risk is entirely determined by their rating which can be either A or B. Firms with rating A have a default intensity of 0.001 and firms in rating class B have a default intensity of 0.01. Assume that there is a 'basic' migration intensity of 0.1 from A to B and the same intensity from B to A. In addition to this 'basic' migration, there is a contagion effect in ratings in the following sense: Every time a firm defaults from rating class B, it implies that 1% of the A-rated firms are instantaneously downgraded into B. No A or B-rated firm is thrown directly into default because of the default of another firm, but some downgrades from A to B are due to a contagion effect from the defaults of B-rated firms. If we simulate a sample of firms that follow these dynamics, we subsequently estimate the default intensities of all firms as a function of rating and finally we transform the time of default arrivals by the cumulative intensities of all firms, then we do not see a violation of the conditional independence assumption. Yet it is clear that this setup has contagion through the (only) covariate, namely the rating of the firms.

We performed a simulation study based on 1000 firms initially rated A and 1000 firms initially rated B, and we ran the experiment for 24 years. The estimated default intensities from class A and B were very close to the actual intensities (0.01 and 0.001). The estimated transition intensity from A to B was 0.123, but this estimate is not used for computing the test statistics in the procedure followed above and in DDKS. We then performed all of the Poisson distribution tests for the same bin sizes that we did for our data set in the previous section. Not a single test rejects the Poisson distribution assumption. In summary, conditioning on firm specific covariates and testing for conditional independence using the cumulative intensities may not reveal contagion through the covariates. We now address a way of testing for such a contagion effect.

6.2 Testing for contagion through covariates

As we have just learned from our simulation experiment, it is perfectly possible that there are contagion effects in the data in the sense that observed defaults affect the firm specific variables X_{it}. As explained above and in the appendix, the Cox regression conditioning on these variables will not detect this source of contagion. We now wish to address this issue of "contagion through covariates" more closely. It is difficult to test for each covariate whether it is affected by defaults of other firms. We therefore choose to use rating changes as a proxy for changes in firm-specific covariates. For our total sample of 2,557 firms over the period 1982 to 2005, we therefore consider all changes in the rating of their publicly issued debt as recorded in Moody's DRS database. Specifically, we investigate whether defaults cause an increase in the aggregate number of rating downgrades.

To ensure a reasonable comparison of ratings across firms, abstracting from differences caused by special features of the individual debt contracts, we use the Estimated Senior Rating (ESR) as a measure of the overall default risk of the firm. For firms without an ESR, we complement the ESR data by instead using either an issuer rating if available, or alternatively a corporate family rating, in compliance with the guidelines set up by Moody's for the calculation of ESR[12]. This procedure reduces the total set of firms in our data set from 2,557 to 2,503 of which the 2,434 have an ESR and the remaining 69 firms a comparable, inferred rating.

We define the aggregate downgrade intensity for the firms as

$$\eta_t = \sum_{i=1}^{n} R_{it} 1_{(\tau_i \geq t)} \left(e^{\tilde{\beta}_W' W_t} + \int_0^t (\tilde{\alpha}_0 + \tilde{\alpha}_1 Y_s) e^{-\tilde{\alpha}_2 (t-s)} dN_s + \tilde{\delta} \right) \qquad t \geq 0$$

with W_t representing various macro variables to account for changes in rating intensities caused by business cycle variations and with R_{it}, Y_t and N_t as previously defined. We thus allow for the same type of "contagion mechanism" from observed defaults to the intensity of (future) rating transitions as we studied in section 5.2. Note, however, that we only allow for defaults to affect the future downgrade intensity whereas non-default downgrades do not cause a Hawkes effect. As shown in Table 6, we find a strongly significant effect in that defaults cause the downgrade intensity to increase. We also find that defaults of larger firms have a larger effect on the downgrade intensity. The decay rate is close to 2 which means that the effect tapers off to roughly 1/8 after one year. In Figure 5 we show downgrade occurrences (scaled) and the default events. However, the strong significance of these tests may be difficult to attribute to contagion effects. The problem is that when we measure contagion through the ratings we may really be capturing the reactions of rating agencies to corporate defaults. These reactions could potentially reflect revisions of rating policies or extra scrutiny in light of a recent default. This extra scrutiny could lead to updating of the rating agency's measurement of critical firm characteristics and this in turn cause downgrades. As such, the measurement of contagion would be consistent with contagion taking place through updating of latent variables. However, our main focus is on whether actual, measurable key ratios are affected by economy-wide defaults. We therefore turn to conducting such tests.

6.3 Effects through quick ratios and distance-to-default

In this section we carry out simple regression tests to see if the average levels of distance-to-default and quick ratios are affected by corporate defaults. Specifically, we test whether changes in quick ratios and distance-to-default react to the number of defaults occurring in a preceding time window of variable length. We control for economy-wide variables that were significant in our Cox regressions.

As a representative example, we consider the following regression

$$\begin{aligned}\Delta(\text{1-year "Distance to Default"})_t &= \eta_0 + \eta_1(\text{1-year S\&P500 return})_t + \eta_2(\text{Industrial production})_t \\ &\quad + \eta_3(\text{Treasury term spread})_t + \eta_4(\text{Defaults in } k \text{ mths.})_t\end{aligned}$$

based on monthly observations. Δ(1-year "Distance to Default")$_t$ is the change from t to $t+1$

[12]Hamilton, David T. (2005). Moody's Senior Ratings Algorithm and Estimated Senior Ratings, *Moody's Investors Service.*

in the cross-sectional median across all firms at risk, and (Defaults in k mths.)$_t$ is the aggregate number of observed defaults within the last k months prior to t.

We choose the k-month time windows to be of length 1, 3, 6, 12 and 24 months. We consider both the quick ratio and distance-to-default. For these variables we consider both the median, the 10% quantile and the 90% quantile of the variables. Note that since a low quick ratio and a low distance-to-default both are indicators of high default risk, the 10% quantile represents riskier firms. The median level tests for whether there is an effect of default on the level of the variables overall whereas the quantiles are meant to capture effects that affect the tails - either the more risky firms or the safer firms. Ideally, we would want to look in specific sectors as well, but our data set is too thin for this purpose.

We are unable to find any effects from the number of defaults to quick ratios. As illustrated in Tables 7-9 the quick ratio seems unaffected by any information related to the number of default regardless of which quantile we consider and regardless of the width of the default window. This is not true for the distance-to-default. As shown in tables 10-12, we find some significant coefficients for some choices of the length of the default window. We find that the number of defaults in the prior 6-month period and the prior 12-month default window affect the changes in distance-to-default. There is no effect on the shorter horizons, and only for the 90% quantile do we see an effect from 24-month default.

We are reluctant at this stage to interpret these results as an unambiguous sign of contagion. We need to have better data on the specific financial interactions between firms and sectors to explain why the 6-month and 12-month windows turn out to be significant. This is a topic for future research.

7 Conclusion

In this paper we re-investigate the time-change method used by DDKS for testing whether company defaults in the US can be viewed as conditionally independent. As noted by the authors, their test is a joint test of the specification of the individual firms' default intensities and the assumption of conditional independence. While they reject this joint hypothesis, we show (on a slightly smaller data set) that if we use a different specification of the firms' default intensities we cannot reject the assumption of conditional independence. To show that this is not due to a lack of power, we show that we do reject in most tests using the specification used in DDKS.

The time-change test is based on testing a Poisson property of a time-transformed process of aggregate defaults. Our second contribution is to show that the Poisson property may be satisfied even if defaults are not conditionally independent. Thus, the fact that we cannot reject the Poisson property need not be indicative of conditional independence. The reason for this is that the test procedure proposed in DDKS may not capture contagion through covariates and needs to be adjusted if one wants to rule out such contagion effects. That is, if the default of one company leads to a change in covariates (i.e. the explanatory variables used in the Cox regression specification of the default intensities) of other companies, then this is a contagion effect which is not captured by the time transformation. We provide an illustrative example which gives the central idea and we point to a central result in point process theory that explains this fact.

To test for the possibility of contagion through covariates, we first use rating as a summary statistic for the credit quality of the firms. The idea is that if a default of one firm significantly affects another firm's credit quality by changing explanatory variables, then this is reflected in the ratings. We therefore test whether downgrade intensities are significantly affected by defaults. We find a significant contagion effect. Since this may be an effect caused the behavior of rating agencies rather than actual default intensity changes alone, we also perform regression tests to see if quick ratios and distance-to-default are affected by the occurrence of defaults after controlling for macroeconomic variables. We find no effects on quick ratios but possibly an effect on distance-to-default. Our conclusion is that the focus on contagion should be put on balance sheet effects.

Appendix

This appendix contains a mathematical definition of conditional independence and recalls a general result that explains the time-transformation method which turns a counting process into a unit rate Poisson process. All relevant measure-theoretic concepts used can be found in Brémaud (1981).

Consider a fixed probability space $(\Omega, \mathcal{F}, P)$ and assume it is rich enough to support all variables and processes defined below. A doubly stochastic process is defined as follows.

Definition A.1 *Consider a simple, non-explosive, $(\mathcal{F}_t)_{t\geq 0}-$adapted point process $N = (N_t)_{t\geq 0}$, a non-negative stochastic process $\lambda = (\lambda_t)_{t\geq 0}$, and a sub-$\sigma-$algebra $\mathcal{G}$. If*

(i) λ_t *is $\mathcal{G}-$measurable for all $t \geq 0$*

(ii) $\int_0^t \lambda_s ds < \infty$ *a.s. for all $t \geq 0$*

(iii) $P\big(N_t - N_s = k \mid \mathcal{F}_s \vee \mathcal{G}\big) = \frac{1}{k!}\left(\int_s^t \lambda_u du\right)^k \exp\left(-\int_s^t \lambda_u du\right) \quad a.s.$
for all $k \in \mathbb{N}_0,\ 0 \leq s \leq t$

then N is a $\big((\mathcal{F}_t)_{t\geq 0}, \mathcal{G}\big)-$doubly stochastic Poisson process with intensity λ.

This expresses the doubly stochastic assumption for a univariate point process with particular emphasis on the information set $\mathcal{G}$ emanating from the intensity process[13]. The generalization of this concept to a multivariate process involves a requirement of conditional independence between the individual filtrations.

Definition A.2 *Consider a finite collection of simple, non-explosive point processes $N_1, \ldots, N_n$ and a sub-$\sigma-$algebra $\mathcal{G}$. If*

(i) *N_i is a $\big((\mathcal{F}_{it})_{t\geq 0}, \mathcal{G}\big)-$doubly stochastic Poisson process with intensity λ_i for $i = 1, \ldots, n$*

(ii) *$N_1, \ldots, N_n$ are conditionally independent given $\mathcal{G}$ in the sense that*

$$P\Big(\bigcap_{i=1}^n F_i \,\Big|\, \mathcal{G}\Big) = \prod_{i=1}^n P(F_i \,|\, \mathcal{G}) \quad a.s.$$

for all $F_1 \in \bigvee_{t\geq 0} \mathcal{F}_{1t}, \ldots, F_n \in \bigvee_{t\geq 0} \mathcal{F}_{nt}$

then the processes $N_1, \ldots, N_n$ are jointly $\Big(\big((\mathcal{F}_{it})_{t\geq 0}\big)_{i=1,\ldots,n}, \mathcal{G}\Big)-$doubly stochastic.

[13] The contents of definition A.1 coincides with the standard definition in the literature albeit the formulation differs slightly, since we wish to emphasize, more directly, the information that pertains to the intensity λ. Thus, a $\big((\mathcal{F}_t)_{t\geq 0}, \mathcal{G}\big)-$doubly stochastic Poisson process N in terms of definition A.1 corresponds to a $(\mathcal{F}_t \vee \mathcal{G})_{t\geq 0}-$doubly stochastic Poisson process in Brémaud (1981).

The important thing to note here is that the information set $\mathcal{G}$ containing all information about the intensity λ_i for the i'th process N_i is *the same for all* i, i.e. there may be some "global" information contained in the set $\mathcal{G}$ that can affect all processes N_i through covariations in the intensities λ_i. But note also that any such "cross-sectional" information must necessarily be stored in $\mathcal{G}$, since conditionally on $\mathcal{G}$ the point processes are independent and hence cannot affect each other. In particular, for our application, the default of firm i as represented by the first jump of N_i cannot influence the default probabilities of any other firms. We think of $\mathcal{G}$ as containing processes which are exogenous to the default events, i.e. they influence but are not influenced by actual default events. $\mathcal{G}$ may contain macroeconomic factors, such as the gross domestic product, the term structure etc. that affect all firms at once, and it may contain exogenous variables which affect industries or even single firms. The important requirement is that there is no information on actual default times of individual firms. With the above definitions in place, we can now state the essential property for jointly doubly stochastic processes that connects the individual intensities λ_i to an aggregate intensity.

Proposition A.3 *Assume $N_1, \ldots, N_n$ are jointly $\left(\left((\mathcal{F}_{it})_{t\geq 0}\right)_{i=1,\ldots,n}, \mathcal{G}\right)-$doubly stochastic and let $\mathcal{F}_t = \bigvee_{i=1}^n \mathcal{F}_{it}, t \geq 0$. Then*

(i) *N_i is a $\left((\mathcal{F}_t)_{t\geq 0}, \mathcal{G}\right)-$doubly stochastic Poisson process with intensity λ_i, $i = 1, \ldots, n$*

(ii) *$\sum_{i=1}^n N_i$ is a $\left((\mathcal{F}_t)_{t\geq 0}, \mathcal{G}\right)-$doubly stochastic Poisson process with intensity $\sum_{i=1}^n \lambda_i$.*

Proof. Definition A.2 readily implies

$$\begin{aligned} & P\big(N_{it} - N_{is} = k \,\big|\, \mathcal{F}_s \vee \mathcal{G}\big) \\ &= P\big(N_{it} - N_{is} = k \,\big|\, \mathcal{F}_{is} \vee \mathcal{G}\big) \\ &= \frac{1}{k!}\left(-\int_s^t \lambda_{iu} du\right)^k \exp\left(-\int_s^t \lambda_{iu} du\right) \quad a.s. \qquad k \in \mathbb{N}_0, 0 \leq s \leq t \end{aligned}$$

for fixed i which proves (i). Moreover, an argument similar to Revuz and Yor (1999) proposition 12.1.5 shows that since the N_i are conditionally independent Poisson processes given $\mathcal{G}$, they have no simultaneous jumps. Hence the aggregate process $N_t = \sum_{i=1}^n N_{it}$ is again a simple, non-explosive point process, and the conditional independence assumption in definition A.2 (ii) together with the convolution property of the Poisson distribution thus ensures that

$$\begin{aligned} & P\big(N_t - N_s = k \,\big|\, \mathcal{F}_s \vee \mathcal{G}\big) \\ &= \frac{1}{k!}\left(\int_s^t \sum_{i=1}^n \lambda_{iu} du\right)^k \exp\left(-\int_s^t \sum_{i=1}^n \lambda_{iu} du\right) \quad a.s. \qquad k \in \mathbb{N}_0,\ 0 \leq s \leq t. \end{aligned}$$

□

In the particular case, where we only consider the first jump

$$\tau_i = \inf\{t \geq 0 \,|\, N_{it} = 1\}$$

of every N_i process, the corresponding intensity reduces to $\left(\lambda_{it}1_{(\tau_i \geq t)}\right)_{t\geq 0}$, and hence the aggregate process $N_t = \sum_{i=1}^n 1_{(\tau_i \leq t)}$ has intensity $\sum_{i=1}^n \lambda_{it}1_{(\tau_i \geq t)}$.

By standard arguments one may now show that our definition A.2, which leads to the result in proposition A.3, is in fact equivalent to the slightly more abstract definition of doubly stochastic processes given in DDKS, which essentially states that the processes $N_1, \ldots, N_n$ are jointly doubly stochastic if

$$(N_{1,t+\xi})_{t\geq 0}, \ldots, (N_{n,t+\xi})_{t\geq 0}$$

are conditionally independent for every finite stopping time ξ. A proof of the mathematical equivalence is available upon request.

It is important to note that the method of transforming into a Poisson process applies to a much broader class of processes than those arising from addition of conditionally independent point processes. The following general result is due to Meyer (1971) but see also Brown and Nair (1988) for a simpler proof.

Theorem A.4 *Consider a multivariate point process* $(N_1, \ldots, N_n)$ *and assume that every coordinate process* N_i *has a continuous compensator* Λ_i *satisfying* $\lim_{t\to\infty} \Lambda_i(t) = \infty$. *Then*

$$\left(\sum_{j\in\mathbb{N}} 1_{(\Lambda_1(\tau_{1,j})\leq t)}\right)_{t\geq 0}, \ldots, \left(\sum_{j\in\mathbb{N}} 1_{(\Lambda_n(\tau_{n,j})\leq t)}\right)_{t\geq 0}$$

are independent unit Poisson processes where $\tau_{i,j}$ *denotes the j'th jump of* N_i.

Remark 1 The assumption that $(N_1, \ldots, N_n)$ be a multivariate point process merely amounts to requiring that no two coordinate processes N_i and N_j have simultaneous jumps, which in particular is satisfied if $N_1, \ldots, N_n$ are jointly doubly stochastic as follows from proposition A.3. ■

If in particular all the coordinate point processes N_i have intensities, i.e.

$$\Lambda_i(t) = \int_0^t \lambda_{i,s} ds$$

for some intensity process $(\lambda_{i,s})_{s\geq 0}$, then

$$\sum_{j\in\mathbb{N}} 1_{(\Lambda_1(\tau_{1,j})\leq t)} = N_{i,\Lambda_i^{-1}(t)}$$

which corresponds to the situation analyzed in this paper. But note that Meyer's result applies even in the case where compensators are affected by jumps in other processes, and hence the time transformation result applies to this much broader class outside the class of conditionally independent processes.

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Table 1. Descriptive statistics for covariates

The table reports empirical averages and standard deviations (in parenthesis) for the explanatory variables used in the Cox regressions.

Macro variables:						
1-year S&P500 return	0.110	(0.164)				
3-month treasury rate	5.469	(2.671)				
Industrial production	0.027	(0.029)				
Treasury term spread	1.371	(0.955)				
Firm specific variables:						
	Defaulting firms		*Non-def. firms*		*All firms*	
1-year equity return	0.044	(0.497)	0.119	(0.526)	0.109	(0.523)
1-year "Distance to Default"	0.612	(1.356)	2.063	(2.854)	1.867	(2.746)
Quick ratio	0.507	(6.237)	0.682	(3.091)	0.658	(3.677)
Short-to-long term debt	0.057	(0.154)	0.094	(0.185)	0.089	(0.181)
Book asset value (log)	1.835	(2.882)	3.170	(3.582)	2.990	(3.526)

[14] Calculations are based on the likelihood ratio test statistic and its asymptotic distribution. However, the (asymptotically equivalent) Wald and score test statistics yield similar conclusions thus indicating a limited finite sample bias in the results.

Table 2. Parameter estimates (doubly stochastic models)

The macro variables entering the models are the 1-year return on the S&P500 index, the level of the 3-month U.S. treasury yield, the 1-year percentage change in U.S. industrial production, and the spread between the 10-year and 1-year U.S. treasury yields. The firm specific variables are the 1-year stock return, the 1-year distance to default, the quick ratio, short-term debt as a percentage of total debt, and (log) book value of assets. Asymptotic standard errors are reported in parenthesis and statistical significance is indicated at 5% (*), 1% (**), and 0.1% (***) levels, respectively[14].

	Model I	Model II
Macro variables:		
Constant	-3.735 ***	-3.480 ***
	(0.179)	(0.299)
1-year S&P500 return	1.566 ***	1.886 ***
	(0.318)	(0.353)
3-month treasury rate	-0.040	
	(0.024)	
Industrial production		-5.723 **
		(1.956)
Treasury term spread		0.209 ***
		(0.055)
Firm specific variables:		
1-year equity return	-3.131 ***	-3.151 ***
	(0.202)	(0.213)
1-year "Distance to Default"	-0.841 ***	-0.794 ***
	(0.039)	(0.043)
Quick ratio		-0.263 ***
		(0.085)
Short-to-long term debt		0.651 ***
		(0.177)
Book asset value (log)		-0.095 **
		(0.031)

Table 3. Binned data tests (doubly stochastic models)

The table reports $p-$values for tests of the fit of the transformed default data to the Poisson distribution for bin sizes $c = 1, 2, 4, 6, 8, 10$, based on the intensity specification $\lambda_{it} = R_{it}e^{\beta'_W W_t + \beta'_X X_{it}}$ estimated in table 2. The employed test statistics are: Fisher dispersion (FD), Upper tail mean (UT1), Upper tail median (UT2), Böhning dispersion (BD), Cramer von Mises (CVM), Kocherlakota-Kocherlakota with parameter $t = 0.9$ (KK), Nakamura-Perez-Abreu (NPA), and the serial correlation statistics (SC1 and SC2). $p-$values (rounded to 3rd decimal) are calculated with 2-sided alternatives for the BD, KK, and SC statistics and 1-sided otherwise following Karlis and Xekalaki (2000), and statistical significance is indicated at 5% (*), 1% (**), and 0.1% (***) levels.

	FD	UT1	UT2	BD	CVM	KK	NPA	SC1	SC2
Model I									
Bin size 1	0.326	0.440	1.000	0.558	0.819	0.548	0.570	0.038 *	0.003 **
Bin size 2	0.110	0.476	1.000	0.156	0.439	0.152	0.090	0.047 *	0.002 **
Bin size 4	0.021 *	0.211	0.972	0.023 *	0.435	0.028 *	0.052	0.011 *	0.001 ***
Bin size 6	0.013 *	0.062	0.136	0.012 *	0.195	0.012 *	0.019 *	0.004 **	0.000 ***
Bin size 8	0.002 **	0.012 *	0.260	0.001 **	0.117	0.002 **	0.001 **	0.003 **	0.000 ***
Bin size 10	0.004 **	0.013 *	0.025 *	0.004 **	0.063	0.006 **	0.000 ***	0.002 **	0.000 ***
Model II									
Bin size 1	0.349	0.302	1.000	0.616	0.847	0.570	0.479	0.225	0.412
Bin size 2	0.324	0.428	0.728	0.582	0.993	0.558	0.981	0.289	0.226
Bin size 4	0.377	0.447	0.972	0.625	0.629	0.768	0.246	0.392	0.209
Bin size 6	0.406	0.501	0.734	0.696	0.853	0.787	0.712	0.722	0.277
Bin size 8	0.113	0.233	0.257	0.154	0.639	0.192	0.191	0.170	0.181
Bin size 10	0.037 *	0.120	0.566	0.051	0.746	0.112	0.111	0.177	0.135

Table 4. Parameter estimates (contagion models)

The explanatory variables in the table are the same as appearing in table 2. Asymptotic standard errors are reported in parenthesis and statistical significance is indicated at 5% (*), 1% (**), and 0.1% (***) levels, respectively[14].

	Model I				
	Without level ($\delta = 0$)		With level ($\delta \neq 0$)		
Macro variables:					
Constant	-3.840 ***	-3.840 ***	-3.845 ***	-3.845 ***	-3.845 ***
	(0.193)	(0.192)	(0.191)	(0.191)	(0.191)
1-year S&P500 return	1.607 ***	1.607 ***	1.604 ***	1.604 ***	1.604 ***
	(0.322)	(0.322)	(0.322)	(0.321)	(0.321)
3-month treasury rate	-0.038	-0.038	-0.039	-0.039	-0.039
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
Industrial production					
Treasury term spread					
Firm specific variables:					
1-year equity return	-3.252 ***	-3.252 ***	-3.270 ***	-3.270 ***	-3.269 ***
	(0.222)	(0.221)	(0.218)	(0.218)	(0.218)
1-year "Distance to Default"	-0.858 ***	-0.858 ***	-0.856 ***	-0.856 ***	-0.856 ***
	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)
Quick ratio					
Short-to-long term debt					
Book asset value (log)					
Contagion effects:					
Constant (α_0)	$1.2{\cdot}10^{-5}$	$1.3{\cdot}10^{-5}$ *	$2.3{\cdot}10^{-14}$	$2.3{\cdot}10^{-14}$	
	$(8.3{\cdot}10^{-5})$	$(1.7{\cdot}10^{-5})$	$(5.1{\cdot}10^{-5})$	$(1.1{\cdot}10^{-9})$	
Firm size (α_1)	$2.2{\cdot}10^{-14}$		$2.6{\cdot}10^{-14}$		
	$(1.3{\cdot}10^{-5})$		$(4.6{\cdot}10^{-6})$		
Decay rate (α_2)	0.902	0.905	0.982	1.001	
	(1.069)	(1.051)	(—)	(—)	
Level (δ)			$2.1{\cdot}10^{-4}$	$2.1{\cdot}10^{-4}$	$2.1{\cdot}10^{-4}$ **
			$(3.3{\cdot}10^{-4})$	$(1.2{\cdot}10^{-4})$	$(1.2{\cdot}10^{-4})$

Table 5. Parameter estimates (contagion models)

The explanatory variables in the table are the same as appearing in table 2. Asymptotic standard errors are reported in parenthesis and statistical significance is indicated at 5% (*), 1% (**), and 0.1% (***) levels, respectively[14].

	Model II				
	Without level ($\delta = 0$)		With level ($\delta \neq 0$)		
Macro variables:					
Constant	-3.077 ***	-3.077 ***	-3.086 ***	-3.085 ***	-3.099 ***
	(0.318)	(0.318)	(0.318)	(0.318)	(0.317)
1-year S&P500 return	1.833 ***	1.833 ***	1.832 ***	1.832 ***	1.830 ***
	(0.361)	(0.361)	(0.361)	(0.361)	(0.361)
3-month treasury rate					
Industrial production	-5.833 **	-5.832 **	-5.834 **	-5.838 **	-5.874 **
	(2.008)	(2.008)	(2.008)	(2.008)	(2.007)
Treasury term spread	0.207 ***	0.207 ***	0.207 ***	0.207 ***	0.208 ***
	(0.056)	(0.056)	(0.056)	(0.056)	(0.056)
Firm specific variables:					
1-year equity return	-3.236 ***	-3.236 ***	-3.244 ***	-3.244 ***	-3.254 ***
	(0.237)	(0.236)	(0.236)	(0.237)	(0.234)
1-year "Distance to Default"	-0.799 ***	-0.799 ***	-0.797 ***	-0.798 ***	-0.796 ***
	(0.046)	(0.046)	(0.046)	(0.046)	(0.046)
Quick ratio	-0.765 ***	-0.765 ***	-0.764 ***	-0.765 ***	-0.762 ***
	(0.130)	(0.130)	(0.131)	(0.131)	(0.132)
Short-to-long term debt	0.389 *	0.389 *	0.390 *	0.390 *	0.392 *
	(0.182)	(0.182)	(0.183)	(0.183)	(0.183)
Book asset value (log)	-0.103 **	-0.103 **	-0.102 **	-0.102 **	-0.101 **
	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)
Contagion effects:					
Constant (α_0)	$2.1 \cdot 10^{-5}$	$2.1 \cdot 10^{-5}$ ***	$1.5 \cdot 10^{-6}$	$1.4 \cdot 10^{-5}$	
	($8.7 \cdot 10^{-5}$)	($2.1 \cdot 10^{-5}$)	($1.1 \cdot 10^{-4}$)	($2.5 \cdot 10^{-5}$)	
Firm size (α_1)	$2.4 \cdot 10^{-14}$		$1.7 \cdot 10^{-6}$		
	($1.3 \cdot 10^{-5}$)		($1.4 \cdot 10^{-5}$)		
Decay rate (α_2)	0.868 *	0.869 *	0.737	0.803	
	(0.837)	(0.813)	(1.063)	(0.894)	
Level (δ)			$1.2 \cdot 10^{-4}$	$8.9 \cdot 10^{-5}$	$3.5 \cdot 10^{-4}$ ***
			($3.9 \cdot 10^{-4}$)	($2.9 \cdot 10^{-4}$)	($1.4 \cdot 10^{-4}$)

Table 6. Parameter estimates for aggregate rating downgrade intensity

The explanatory variables in the table are the same as appearing in table 2. Asymptotic standard errors are reported in parenthesis and statistical significance is indicated at 5% (*), 1% (**), and 0.1% (***) levels, respectively[14].

Macro variables:			
Constant	-3.031 ***	-3.030 ***	-3.030 ***
	(0.307)	(0.444)	(0.130)
1-year S&P500 return	-0.887 *	-0.890 *	-0.887 *
	(0.468)	(0.524)	(0.387)
Industrial production	-4.864 *	-5.429 *	-4.868 *
	(2.760)	(3.941)	(2.169)
Treasury term spread	-0.270 ***	-0.298 ***	-0.270 ***
	(0.135)	(0.206)	(0.087)
Contagion effects:			
Constant ($\tilde{\alpha}_0$)	$2.5 \cdot 10^{-14}$		
	(0.003)		
Firm size ($\tilde{\alpha}_1$)	0.001 ***	0.001 ***	0.001 ***
	($4.4 \cdot 10^{-4}$)	($1.6 \cdot 10^{-4}$)	($1.7 \cdot 10^{-4}$)
Decay rate ($\tilde{\alpha}_2$)	2.007 ***	1.874 ***	2.006 ***
	(0.269)	(0.235)	(0.257)
Level ($\tilde{\delta}$)	$1.2 \cdot 10^{-10}$	$1.2 \cdot 10^{-8}$	
	(0.013)	(0.019)	

Table 7. Effect of previous defaults on changes in quick ratio (median)

The table reports estimation results for the time series regression

$$\Delta(\text{Quick ratio})_t = \eta_0 + \eta_1(\text{1-year S\&P500 return})_t + \eta_2(\text{Industrial production})_t + \eta_3(\text{Treasury term spread})_t + \eta_4(\text{Defaults in } k \text{ mths.})_t$$

based on monthly observations. $\Delta(\text{Quick ratio})_t$ is the change from t to $t+1$ in the cross-sectional median across all firms at risk, and $(\text{Defaults in } k \text{ mths.})_t$ is the aggregate number of observed defaults within the last k months prior to t. Asymptotic standard errors are Newey-West-corrected, and statistical significance is indicated at 10% (*), 5% (**), and 1% (***) levels.

Constant	-0.002 *	-0.002	-0.001	-0.004	-0.004 *
1-year S&P500 return	0.006 *	0.005	0.004	0.007 *	0.005
Industrial production	-0.039 **	-0.039 *	-0.050 **	-0.021	-0.015
Treasury term spread	0.002 ***	0.002 ***	0.002 ***	0.002 ***	0.002 **
Defaults in 1 mth.	$1.7 \cdot 10^{-5}$				
Defaults in 3 mths.		$4.7 \cdot 10^{-5}$			
Defaults in 6 mths.			$-8.1 \cdot 10^{-5}$		
Defaults in 12 mths.				$6.0 \cdot 10^{-5}$	
Defaults in 24 mths.					$5.3 \cdot 10^{-5}$
R^2	0.041	0.038	0.038	0.036	0.033
Obs.	287	285	282	276	264

Table 8. Effect of previous defaults on changes in quick ratio (10% quantile)

The table reports estimation results for the time series regression

$$\Delta(\text{Quick ratio})_t = \eta_0 + \eta_1(\text{1-year S\&P500 return})_t + \eta_2(\text{Industrial production})_t + \eta_3(\text{Treasury term spread})_t + \eta_4(\text{Defaults in } k \text{ mths.})_t$$

based on monthly observations. Δ(Quick ratio)$_t$ is the change from t to $t+1$ in the cross-sectional 10% quantile across all firms at risk, and (Defaults in k mths.)$_t$ is the aggregate number of observed defaults within the last k months prior to t. Asymptotic standard errors are Newey-West-corrected, and statistical significance is indicated at 10% (*), 5% (**), and 1% (***) levels.

Constant	-0.001	-0.001	0.000	-0.001	-0.001
1-year S&P500 return	0.001	0.001	-0.001	0.001	-0.001
Industrial production	-0.013	-0.015	-0.028	-0.012	-0.033
Treasury term spread	0.001	0.001	0.001	0.001	0.001
Defaults in 1 mth.	$2.1 \cdot 10^{-4}$				
Defaults in 3 mths.		$4.8 \cdot 10^{-5}$			
Defaults in 6 mths.			$-7.8 \cdot 10^{-5}$		
Defaults in 12 mths.				$2.6 \cdot 10^{-5}$	
Defaults in 24 mths.					$1.6 \cdot 10^{-5}$
R^2	0.008	0.007	0.008	0.007	0.013
Obs.	287	285	282	276	264

Table 9. Effect of previous defaults on changes in quick ratio (90% quantile)

The table reports estimation results for the time series regression

$$\Delta(\text{Quick ratio})_t = \eta_0 + \eta_1(\text{1-year S\&P500 return})_t + \eta_2(\text{Industrial production})_t + \eta_3(\text{Treasury term spread})_t + \eta_4(\text{Defaults in } k \text{ mths.})_t$$

based on monthly observations. $\Delta(\text{Quick ratio})_t$ is the change from t to $t+1$ in the cross-sectional 90% quantile across all firms at risk, and $(\text{Defaults in } k \text{ mths.})_t$ is the aggregate number of observed defaults within the last k months prior to t. Asymptotic standard errors are Newey-West-corrected, and statistical significance is indicated at 10% (*), 5% (**), and 1% (***) levels.

Constant	-0.000	-0.005	-0.001	0.001	-0.004
1-year S&P500 return	0.056 ***	0.061 ***	0.060 ***	0.059 ***	0.056 **
Industrial production	-0.228 **	-0.194	-0.221 *	-0.223 *	-0.161
Treasury term spread	0.000	0.001	0.001	0.001	0.001
Defaults in 1 mth.	$1.2 \cdot 10^{-3}$				
Defaults in 3 mths.		$9.4 \cdot 10^{-4}$			
Defaults in 6 mths.			$2.0 \cdot 10^{-5}$		
Defaults in 12 mths.				$-1.7 \cdot 10^{-4}$	
Defaults in 24 mths.					$1.3 \cdot 10^{-5}$
R^2	0.033	0.036	0.035	0.034	0.024
Obs.	287	285	282	276	264

Table 10. Effect of previous defaults on changes in 1-year "Distance to Default" (median)

The table reports estimation results for the time series regression

$$\Delta(\text{1-year "Distance to Default"})_t = \eta_0 + \eta_1(\text{1-year S\&P500 return})_t + \eta_2(\text{Industrial production})_t + \eta_3(\text{Treasury term spread})_t + \eta_4(\text{Defaults in } k \text{ mths.})_t$$

based on monthly observations. Δ(1-year "Distance to Default")$_t$ is the change from t to $t+1$ in the cross-sectional median across all firms at risk, and (Defaults in k mths.)$_t$ is the aggregate number of observed defaults within the last k months prior to t. Asymptotic standard errors are Newey-West-corrected, and statistical significance is indicated at 10% (*), 5% (**), and 1% (***) levels.

Constant	-0.006	0.033	0.070 ***	0.075 **	0.045
1-year S&P500 return	-0.036	-0.055	-0.083	-0.090	-0.092
Industrial production	-0.577 **	-0.784 ***	-1.080 ***	-1.074 ***	-0.726 **
Treasury term spread	0.009	0.006	0.005	0.007	0.008
Defaults in 1 mth.	0.004				
Defaults in 3 mths.		-0.002			
Defaults in 6 mths.			-0.004 **		
Defaults in 12 mths.				-0.003 **	
Defaults in 24 mths.					-0.001
R^2	0.036	0.040	0.056	0.049	0.034
Obs.	287	285	282	276	264

Table 11. Effect of previous defaults on changes in 1-year "Distance to Default" (10% quantile)

The table reports estimation results for the time series regression

$$\Delta(\text{1-year "Distance to Default"})_t = \eta_0 + \eta_1(\text{1-year S\&P500 return})_t + \eta_2(\text{Industrial production})_t + \eta_3(\text{Treasury term spread})_t + \eta_4(\text{Defaults in } k \text{ mths.})_t$$

based on monthly observations. Δ(1-year "Distance to Default")$_t$ is the change from t to $t+1$ in the cross-sectional 10% quantile across all firms at risk, and (Defaults in k mths.)$_t$ is the aggregate number of observed defaults within the last k months prior to t. Asymptotic standard errors are Newey-West-corrected, and statistical significance is indicated at 10% (*), 5% (**), and 1% (***) levels.

Constant	0.001	0.004	0.034 **	0.035	0.013
1-year S&P500 return	-0.021	-0.025	-0.048	-0.052	-0.052
Industrial production	-0.461 ***	-0.485 ***	-0.720 ***	-0.696 ***	-0.433 **
Treasury term spread	0.013 **	0.012 **	0.010 *	0.011 **	0.012 **
Defaults in 1 mth.	-0.001				
Defaults in 3 mths.		-0.000			
Defaults in 6 mths.			-0.003 **		
Defaults in 12 mths.				-0.002 *	
Defaults in 24 mths.					-0.000
R^2	0.057	0.057	0.075	0.066	0.050
Obs.	287	285	282	276	264

Table 12. Effect of previous defaults on changes in 1-year "Distance to Default" (90% quantile)

The table reports estimation results for the time series regression

$$\Delta(\text{1-year "Distance to Default"})_t = \eta_0 + \eta_1(\text{1-year S\&P500 return})_t + \eta_2(\text{Industrial production})_t + \eta_3(\text{Treasury term spread})_t + \eta_4(\text{Defaults in } k \text{ mths.})_t$$

based on monthly observations. Δ(1-year "Distance to Default")$_t$ is the change from t to $t+1$ in the cross-sectional 90% quantile across all firms at risk, and (Defaults in k mths.)$_t$ is the aggregate number of observed defaults within the last k months prior to t. Asymptotic standard errors are Newey-West-corrected, and statistical significance is indicated at 10% (*), 5% (**), and 1% (***) levels.

Constant	0.013	0.053 **	0.085 ***	0.104 ***	0.087 **
1-year S&P500 return	-0.034	-0.064	-0.085	-0.104 *	-0.120
Industrial production	-0.709 **	-1.008 ***	-1.264 ***	-1.363 ***	-1.053 ***
Treasury term spread	0.003	-0.001	-0.002	0.000	0.003
Defaults in 1 mth.	0.009				
Defaults in 3 mths.		-0.002			
Defaults in 6 mths.			-0.004 **		
Defaults in 12 mths.				-0.003 **	
Defaults in 24 mths.					-0.001 **
R^2	0.032	0.035	0.042	0.042	0.032
Obs.	287	285	282	276	264

Aggregate default intensity 1982-2005

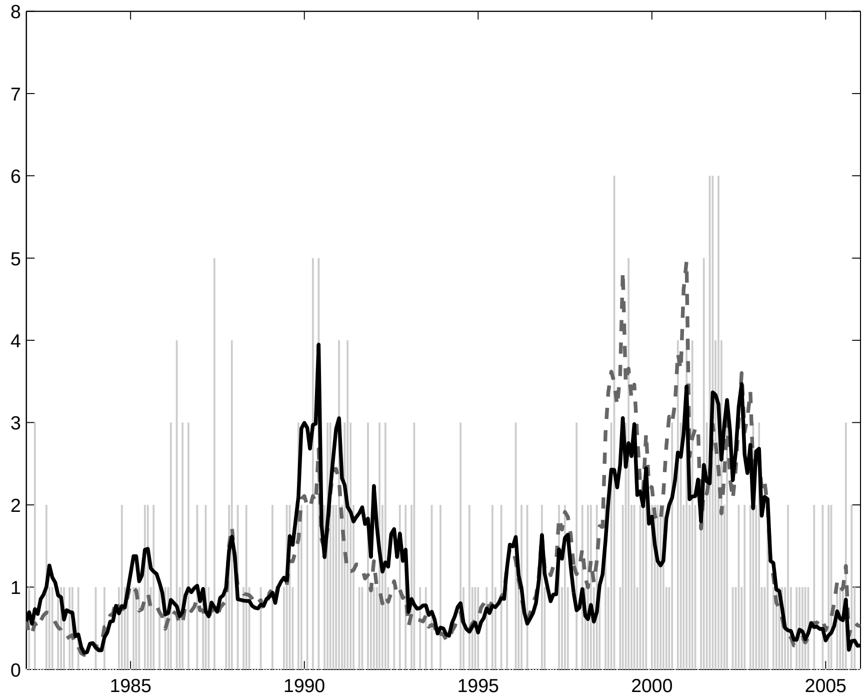

Figure 1. Monthly number of U.S. industrial defaults recorded in Moody's DRSD in the period 1982-2005 and estimated default intensities for the simple (Model I, dashed) and the expanded (Model II, solid) model.

Distribution of binned data Z_j $(c = 8)$

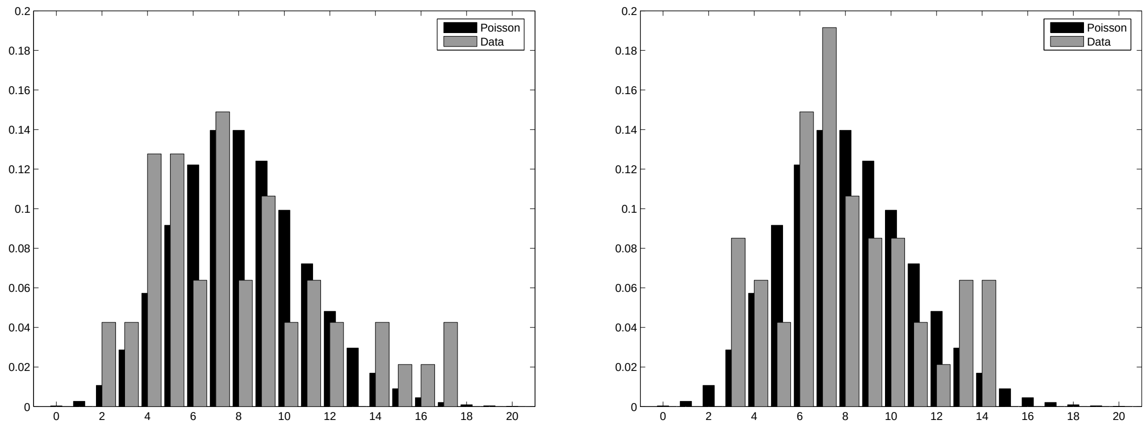

Figure 2. Empirical distribution for $c = 8$ of the binned data Z_j (gray) for the simple (Model I, left) and the expanded (Model II, right) model against their theoretical counterpart (black).

Sequence of binned data Z_j $(c = 8)$

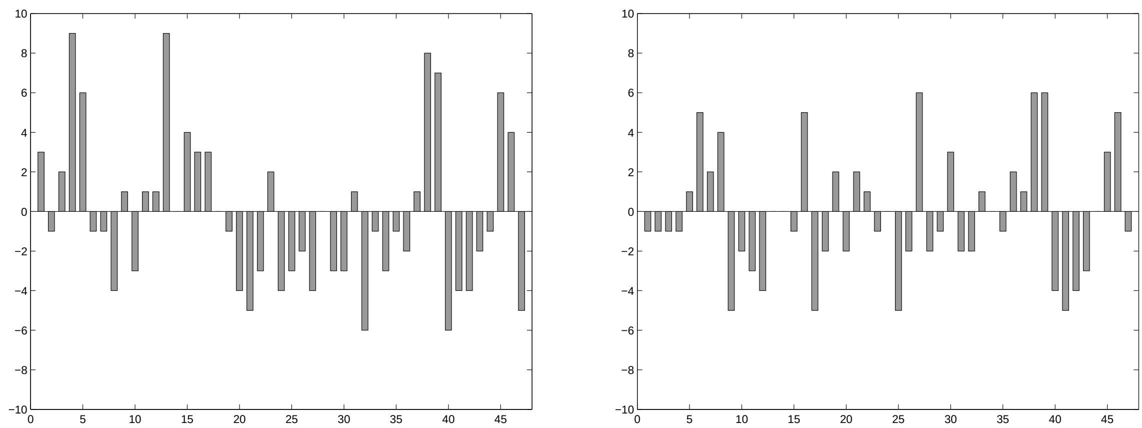

Figure 3. The sequence of binned, centered data $Z_j - c$ for $c = 8$ for the simple (Model I, left) and the expanded (Model II, right) model.

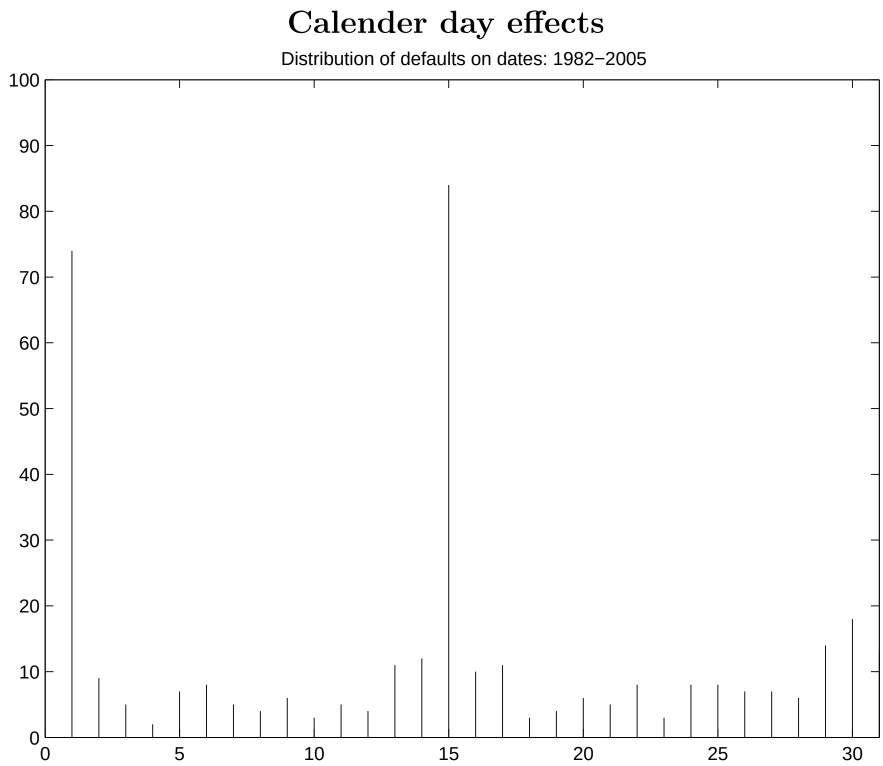

Figure 4. The distribution of U.S. industrial defaults 1982-2005 on calendar day.

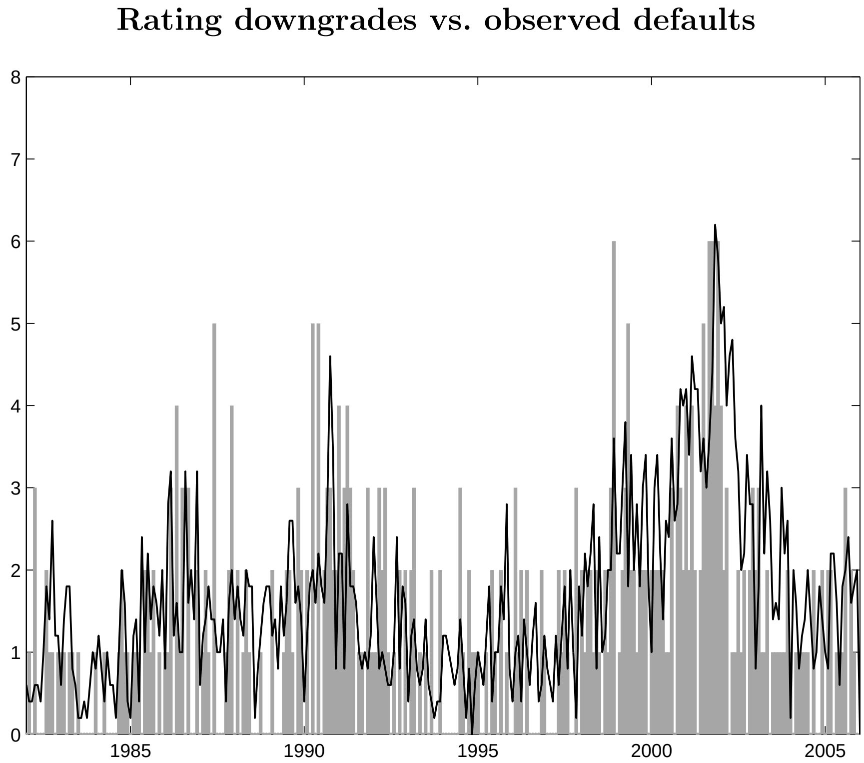

Figure 5. Monthly number of registered U.S. industrial defaults and (scaled) number of rating downgrades among Moody's rated U.S. industrial firms (solid line) for the period 1982-2005.