

Concentration of Control Rights in Leveraged Loan Syndicates: Online Appendix

Mitchell Berlin, Greg Nini, and Edison G. Yu*

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* Berlin (mitchell.berlin@phil.frb.org) is from the Federal Reserve Bank of Philadelphia; Nini (gpn26@drexel.edu) is from LeBow College of Business, Drexel University; and Yu (edison.yu@phil.frb.org) is from the Federal Reserve Bank of Philadelphia. The views expressed in this appendix are our own, and not necessarily those of the Federal Reserve Bank of Philadelphia or the Federal Reserve System.

This is the Online Appendix to accompany “Concentration of Control Rights in Leveraged Loan Syndicates” by Mitchell Berlin, Greg Nini, and Edison Yu. It includes material that we deem as supplementary to the primary analysis included in the main document.

A. Institutional vs. Non-Institutional Deals

Table OA.1 offers a univariate comparison of institutional borrowers with other borrowers based on the full deal sample, which includes observations in columns (1) and (3) of Table 1 in the main paper. Limiting the sample to deals with a loan contract produces a nearly identical comparison.

Table OA.1: Comparison of Deals with and without an Institutional Tranche

	Deals with Institutional Tranche (N=1,149)	Deals without Institutional Tranche (N=634)	p-value for difference
Borrower assets (\$ millions)	4,791	2,122	0%
Borrower OIBDP / assets	0.12	0.11	4%
Borrower debt / assets	0.54	0.36	0%
Borrower debt / OIBDP	5.32	4.27	0%
Borrower unrated	0.27	0.72	0%

Note: This table reports summary statistics for the set of leveraged loan deals, split on whether the deal contains an institutional tranche. The data are compiled from Compustat as of the fiscal quarter following the issuance date of the loan. “OIBDP” stands for operating income before depreciation and is measured over 4 quarters.

B. Accounting for Potential Selection on Unobservables

One concern with the evidence in Table 5 in the main paper is that a common, unobservable factor may be explaining both the presence of an institutional tranche and split control rights in the same deal. We address the possibility formally using the insights and methodology of Altonji et al. (2005).

We modify the model in (1) to account for the potential endogeneity of the decision to include an institutional tranche. The model becomes a bivariate probit:

$$Split_{it} = 1 (\alpha \cdot Institutional_{it} + X'_{it}\beta^S + \varepsilon_{it}^S > 0), \quad (3)$$

$$Institutional_{it} = 1 (X'_{it}\beta^I + \varepsilon_{it}^I > 0),$$

$$\begin{bmatrix} \varepsilon^I \\ \varepsilon^S \end{bmatrix} \sim N \left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix} \right),$$

where the model for *Split* is unchanged from (1), and *Institutional* is modeled as a probit function of the observable factors X and a random variable ε^I . This model allows for the endogeneity of *Institutional* through the correlation ρ , which captures any common unobserved factors that affect both the likelihood that a deal has split control rights and an institutional tranche. A large positive value of ρ would result in a positive bias on the estimate of α in model (1), which has an implicit assumption that $\rho = 0$. Altonji et al. (2005) suggest several strategies for estimating the degree of bias that might be present in an estimate of α in the following model due to the failure to observe ρ . Although α can be identified in model (3) using the parametric assumption on the distribution of residuals, we are hesitant to rely exclusively on the assumption that the error terms are jointly normally distributed.

The first suggestion from Altonji et al. (2005) is to estimate (3) assuming different levels of residual correlation ρ and examining how our inferences might change. Table OA.2 reports the result from this exercise by showing the estimates of β^S , β^I , and α for three different levels of ρ : 0.2, 0.4, and 0.6. For this analysis, we examine only the period 2011-2014.

The estimates in Table OA.2 show that a higher level of ρ does indeed result in a lower estimate for α and the estimated marginal effect of an institutional tranche. However, the inference that an institutional tranche leads to a large and statistically significant increase in the probability of split control rights remains true for even a very high degree of correlation among unobserved factors. Even with a correlation of 0.6 among the unobserved factors, the estimated marginal effect remains a statistically significant 14.3%. The robustness of the inference reflects the statistically strong relationship between the presence of an institutional tranche and split control rights; the omitted factors would need to be very important determinants of both *Split* and *Institutional* to fully account for the estimate of α .

In addition to examining the sensitivity of $\hat{\alpha}$ to alternative assumptions for ρ , Altonji et al.

Table OA.2: Split Control Rights and Institutional Tranches, Bivariate Probit 2011-2014

	Correlation = 0.2.		Correlation = 0.4		Correlation = 0.6.	
	Split (1)	Institutional (2)	Split (3)	Institutional (4)	Split (5)	Institutional (6)
Institutional Tranche	1.551*** (0.323)		1.188*** (0.313)		0.777*** (0.293)	
Ln(Assets)	-0.023 (0.103)	0.467*** (0.102)	0.013 (0.101)	0.460*** (0.101)	0.052 (0.098)	0.448*** (0.099)
Debt / Assets	-0.090 (0.840)	1.819** (0.719)	0.040 (0.826)	1.803** (0.714)	0.182 (0.801)	1.774** (0.704)
Debt / OIBDP	0.137 (0.098)	0.093 (0.074)	0.136 (0.096)	0.095 (0.074)	0.133 (0.092)	0.099 (0.074)
OIBDP / Assets	4.731 (4.489)	0.140 (3.248)	4.481 (4.401)	0.304 (3.238)	4.114 (4.244)	0.555 (3.223)
M.E. of Institutional Tranche	0.268*** (0.048)		0.210*** (0.049)		0.143*** (0.049)	
Year-Quarter FE	Yes		Yes		Yes	
Industry FE	Yes		Yes		Yes	
Rating FE	Yes		Yes		Yes	
N	356		356		356	

Note: This table displays coefficient estimates of the bivariate probit model presented in model (3) where the residual correlation is constrained to be 0.2, 0.4, and 0.6. The dependent variables are indicators that a deal has split control rights and contains an institutional tranche. The split control rights indicator is determined by reading the governing credit agreements, and the institutional indicator is from DealScan. The borrower data are compiled from Compustat as of the fiscal quarter following the issuance date of the loan. “OIBDP” stands for operating income before depreciation and is measured over 4 quarters. “M.E. of Institutional Tranche” reports the marginal effect of a discrete change in Institutional Tranche from 0 to 1, computed with control variables at their sample means. “Year-Quarter FE” refers to fixed effects for the calendar quarter the deal was originated, and “Industry FE” refers to the Fama-French 30 industry classification based on the SIC code. The sample includes only deals issued during 2011-2014. Standard errors are reported in parentheses.

* p < 0.10, ** p < 0.05, *** p < 0.01

(2005) suggest making the assumption that the correlation between the unobservable factors, ρ , is equal to the correlation between observable factors influencing *Split* and *Institutional*, which are given by $X'\beta^S$ and $X'\beta^I$ in model (3). Altonji et al. (2005) show that this assumption can be implemented by setting $\rho = \frac{\text{Cov}(X'\beta^S, X'\beta^I)}{\text{Var}(X'\beta^S)}$.¹ This is a parametric assumption that formalizes the idea that the degree of correlation based on observable factors can be used as a guide to assess the potential for unobserved correlation, and Altonji et al. (2005) suggest that a conservative assumption is that the correlations are equal. In our setting, several observable factors are significantly correlated with the likelihood that a deal has an institutional tranche, particularly the borrower's size, credit rating status, and leverage. These factors, however, have a weak relationship with the probability of split control rights, which suggests that there is little correlation of observables in the data. Moreover, the fact that including these factors in Table 5 hardly changes the estimate of α foreshadows that permitting similar correlation of unobservable factors will have little impact on the estimate of α .

Additionally, if there is an instrumental variable that affects the likelihood that a deal has an institutional tranche but does not directly affect the probability a deal has split control rights, α is identified through the exclusion restriction. The modified model becomes:

$$\begin{aligned} Split_{it} &= 1 (\alpha \cdot Institutional_{it} + X'_{it}\beta^S + \varepsilon_{it}^S > 0), \\ Institutional_{it} &= 1 (X'_{it}\beta^I + \gamma Z + \varepsilon_{it}^I > 0), \\ \begin{bmatrix} \varepsilon^I \\ \varepsilon^S \end{bmatrix} &\sim N \left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix} \right), \end{aligned}$$

where Z is the instrumental variable that is assumed to not directly affect *Split*.

Table OA.3 provides coefficient estimates of the bivariate probit model under the two alternative identifying assumptions. Columns (1) through (4) implement the instrumental variables strategy, in which we use an indicator that the borrower issued an institutional loan prior to the year

¹This condition follows considering the linear projection of the determinants of *Institutional*, $I^* \equiv X'\beta^I + \varepsilon_{it}^I$, onto the observable and unobservable factors that determine *Split*, which are $X'_{it}\beta^S$ and ε_{it}^S . The coefficients in the linear projection are $\frac{\text{Cov}(I^*, X'\beta^S)}{\text{Var}(X'\beta^S)}$ and $\frac{\text{Cov}(I^*, \varepsilon^S)}{\text{Var}(\varepsilon^S)}$, and the assumption that selection on observables is the same as selection on unobservables is formalized by assuming that the coefficients are equal. Based on model (3), $\text{Var}(\varepsilon^S) = 1$, $\text{Cov}(I^*, \varepsilon^S) = \rho$, and $\text{Cov}(I^*, X'\beta^S) = \text{Cov}(X'\beta^I, X'\beta^S)$. Substituting yields $\rho = \frac{\text{Cov}(X'\beta^S, X'\beta^I)}{\text{Var}(X'\beta^S)}$.

2011 as the instrument. Since there was very little issuance during 2008-2010, this amounts to an indicator that the borrower issued an institutional loan during the early surge in institutional issuance from 2005-2007. We conjecture that the prior experience with the institutional market will facilitate subsequent issuance but should not have a direct effect on the structure of subsequent loan contracts. Columns (1) and (2) use no additional control variables, and columns (3) and (4) add the same set of controls used in Table 5.

As shown in the second row of columns (2) and (4), the previous institutional tranche indicator is positive and statistically significant, although the instrument is fairly weak when controlling for other borrower characteristics. Under the assumption that the previous institutional indicator does not directly affect the probability of split control rights, the correlation of residuals is estimated to be significantly above the 0 value assumed in Table 5. Nevertheless, the top row in columns (1) and (3) shows that the estimate of α remains positive and statistically significant, though the estimate is less precise than in Table 5. The estimated marginal effects of a deal having an institutional tranche are close to the magnitudes estimated in Table OA.2 under the assumption of a residual correlation of 0.4.

Columns (5) and (6) in Table OA.3 show the estimates under the assumption that the residual correlation is equal to that of the observable factors affecting *Institutional* and *Split*. The estimated correlation is 0.134, which results in a large, positive estimate of α and an estimated marginal effect close to the magnitudes estimated in Table OA.2 under the assumption of a residual correlation of 0.2.

A final suggestion from Altonji et al. (2005) is to assess the degree of bias that might be present in the estimate of α in model (1) due to the assumption that $\rho = 0$. Failing to account for non-zero correlation will result in an inconsistent estimate of α if $\rho > 0$, which leads to the standard violation of conditional mean independence of the errors: $E(\varepsilon_{it}^S | Institutional_{it} = 1) - E(\varepsilon_{it}^S | Institutional_{it} = 0) > 0$. We refer to this term as the degree of selection based on unobservable factors: $S_{uo} \equiv E(\varepsilon_{it}^S | Institutional_{it} = 1) - E(\varepsilon_{it}^S | Institutional_{it} = 0)$.² Altonji et al. (2005) show that:

$$\text{plim } \hat{\alpha} \simeq \alpha + S_{uo} \frac{\text{Var}(Institutional)}{\text{Var}(\widetilde{Institutional})}, \quad (4)$$

²We borrow the notation from Mavisakalyan and Meinecke (2016).

Table OA.3: Split Control Rights and Institutional Tranches, Bivariate Probit 2011-2014

	Instrumental Variable				Equal Residual Correlation	
	Split (1)	Institutional (2)	Split (3)	Institutional (4)	Split (5)	Institutional (6)
Institutional Tranche	0.966* (0.402)		1.193* (0.581)		1.662*** (0.309)	
Previous Institutional Tranche		0.953*** (0.133)		0.388* (0.179)		
Ln(Assets)			0.014 (0.150)	0.404*** (0.108)	-0.034 (0.062)	0.472*** (0.093)
Debt / Assets			0.050 (0.920)	1.575** (0.731)	-0.130 (0.608)	1.831* (0.958)
Debt / OIBDP			0.134 (0.096)	0.095 (0.075)	0.137* (0.081)	0.090 (0.088)
OIBDP / Assets			4.390 (4.514)	-0.086 (3.320)	4.790 (2.982)	-0.028 (3.264)
Residual Correlation	0.352		0.411		0.134	
M.E. of Institutional Tranche	0.240** (0.111)		0.211** (0.093)		0.286*** (0.051)	
Year-Quarter FE	Yes		Yes		Yes	
Industry FE	Yes		Yes		Yes	
Rating FE	Yes		Yes		Yes	
N	388		356		356	

Note: This table displays coefficient estimates of the bivariate probit model presented in model (3) using either an instrumental variable (columns 1-4) or an assumption of equal selection on unobservables and observables (columns 5 and 6). The dependent variables are indicators that a deal has split control rights and contains an institutional tranche. “OIBDP” stands for operating income before depreciation. M.E. of Institutional Tranche reports the marginal effect of a discrete change in Institutional Tranche from 0 to 1, computed with control variables at their sample means. Year-Quarter FE refers to fixed effects for the calendar quarter the deal was originated, and Industry FE refers to the Fama-French 30 industry classification based on the SIC code. The sample includes only deals issued during 2011-2014. Standard errors are reported in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

where $\widetilde{Institutional}$ is the residual from a regression of $Institutional$ on the observable variables X . The term $\frac{Var(\widetilde{Institutional})}{Var(Institutional)}$ is larger than 1 and provides a measure of how well the observable factors X explain the likelihood that a deal has an institutional tranche. In our sample during the years 2011-2014, this term is estimated to be 1.403, reflecting the strong correlation between the presence of an institutional tranche and borrower size, leverage, and credit rating.³

The important insight from Altonji et al. (2005) is that we can estimate the degree of bias necessary for the positive estimate of α in column (4) of Table 5 to be entirely due to selection based on unobservable factors. Under the hypothesis that $\alpha = 0$, the degree of selection on unobservables can be estimated by $\hat{S}_{uo}^{max} = \frac{\hat{\alpha}}{1.403}$, which is 1.338 based on the estimate $\hat{\alpha} = 1.878$ in Table 5. This number can be interpreted as the difference in the expected value of the unobservable factors explaining split control rights, $E(\varepsilon_{it}^S)$, for deals with and without an institutional tranche, under the assumption that $\alpha = 0$.

We can gauge the size of \hat{S}_{uo}^{max} by comparing it with the amount of selection based on observable factors, X , which can also be estimated under the hypothesis that $\alpha = 0$. Assuming $\alpha = 0$, the parameters in (3) can be estimated as a standard probit model, and the degree of selection based on observables is given by:

$$S_o = \frac{\hat{E}(X'\beta^S | Institutional_{it} = 1) - \hat{E}(X'\beta^S | Institutional_{it} = 0)}{\hat{Var}(X'\beta^S)},$$

where $X'\beta^S$ are the estimated latent thresholds that determine the probability of split control, and \hat{E} , \hat{Var} denote the sample average mean and variance.⁴ In our sample, the average estimated threshold for deals with an institutional tranche is -0.282 , and the average estimated threshold for deals without an institutional tranche is -1.005 , with the difference being driven by positive coefficient estimates on firm size and leverage. Compared with an overall variance of 1.024, this generates an estimated degree of selection on observables of $\hat{S}_o = 0.706$.

The two estimates \hat{S}_o and \hat{S}_{uo}^{max} can be compared to provide a sense of how large the degree of selection based on unobservable factors would have to be, relative to the degree of selection based on observable factors, for the estimate of α to be fully explained by unobserved selection. In our sample, the estimated ratio is $\frac{\hat{S}_{uo}^{max}}{\hat{S}_o} = \frac{1.338}{0.706} = 1.895$. This estimate means that the degree

³The R^2 of the OLS regression of $Institutional$ on the control variables in Table 5 is 0.475.

⁴There is no corresponding denominator in the equation for S_{uo} because $Var(\varepsilon^S)$ is set to 1.

of selection based on unobservables would need to be roughly 1.9 times as large as the degree of selection on observables to fully explain the large positive estimate of α in Table 5. In our opinion, it is unlikely that factors so important in explaining the nature of covenants in loan agreements would be unfamiliar to us as researchers and excluded from standard databases.⁵

Based on the collective evidence, we conclude that there is a large, positive relationship between a deal containing an institutional tranche and the use of split control rights, which can at most be only partially attributed to unobserved factors. The estimated marginal effect of a loan having an institutional tranche is only somewhat sensitive to our different estimation strategies and consistently above 14%. The robustness of the conclusion reflects two features of the data: (1) the statistical relationship between the presence of an institutional tranche and split control rights is very strong, so any unobserved factors would need to be very important, and (2) accounting for observable factors that affect the likelihood that a deal has an institutional tranche has no impact on the estimated value of α .

C. Measures of Negotiation Frictions

We begin with our sample of leveraged loan deals with available data in Compustat and restrict attention to loans issued prior to 2009. For each borrowing firm, we further limit the sample to the most recently issued loan, which means that we have one loan per firm. We find at least one quarter of data for 760 different firms.

We then search all 10-Q and 10-K filings subsequent to the issuance of the loan for evidence of a covenant violation. We look for the occurrence of a “new violation,” which is defined as a reported violation following 4 quarters of no reported violations. This helps ensure that we are identifying a violation related to the sample deal rather than a prior loan. We search through the end of 2010, which is prior to the period when split control rights became more common.

We find a new violation for 15% of our sample deals. Comparing institutional and noninstitutional deals issued prior to 2009, we find that 12.8% of institutional deals reported a violation, and 18.7% of noninstitutional deals reported a violation. We can confidently reject the hypothesis that institutional deals experienced violations at a higher rate during this period.

For each of these 114 incidents of violations, we search SEC filings around the date of the

⁵Altonji et al. (2005) also view such a ratio as implausible in their setting.

violation to determine the resolution of the violation. In the vast majority of cases – 97 cases in our sample – the legal resolution of the violation involves an amendment to the loan. Of the 17 cases in which we do not find an amendment, 6 of the firms filed for Chapter 11 bankruptcy protection, 6 received a new loan in lieu of an amendment, 3 firms repaid the loan balance without issuing a new loan, and 2 returned to compliance without needing an amendment. We exclude the two firms that returned to compliance to generate the final sample of 112 firms examined in Table 7.

Among the reported violations, 56% are for institutional deals, which reflects the higher share of institutional deals in the sample (about two-thirds of deals issued prior to 2009 were institutional) and the lower rate of reported violations.

For each of the violations, we record whether the firm received a waiver prior to resolving the violation. If the waiver is not accompanied by an amendment to the loan, we continue searching for an ultimate resolution. In 18% of our sample, the firm received a waiver prior to resolving the violation. For an example of a waiver without resolution, Chiquita Brands International reported in 10-Q for the period ending September 30, 2006:

On October 5, 2006, the company obtained a temporary waiver from compliance, for the period ended September 30, 2006, with certain financial covenants in the CBL Facility, with which the company otherwise would not have been in compliance. The temporary waiver was effective through December 15, 2006. On November 8, 2006, the company obtained a permanent amendment to the CBL Facility to cure the covenant violations that would have otherwise occurred when the temporary waiver expired. The amendment revised certain covenant calculations relating to financial ratios for leverage and fixed charge coverage, established new levels for compliance with those covenants to provide additional financial flexibility, and includes interest rates on the Revolving Credit Facility of LIBOR plus a margin ranging from 1.25% to 3.00%, and on the Term Loans of LIBOR plus a margin ranging from 2.00% to 3.00%, in each case depending on the company's consolidated leverage ratio. Initially, the interest rates on both the Revolving Credit Facility and the Term Loans will be LIBOR plus 3.00%.

Chiquita Brands disclosed the waiver letter in an 8-K filing on October 5, 2006. In exchange for the lenders temporarily waiving the requirement that the borrower comply with the financial covenants, Chiquita Brands agreed to tight restrictions on new borrowing, investment, and cash distributions during the waiver period. Such waivers do not provide a permanent solution to the covenant violation, so in these cases we continue searching for a resolution via amendment, repayment of the loan, or bankruptcy.

In the 97 cases for which we find an amendment, we read the amendment and code the following

information: the date of the amendment, the amendment fee charged to the borrower, and changes to the interest rate spread on the term loan and revolver. For 4 amendments, we know that the borrower paid a fee but cannot determine the exact amount, and so exclude these observations from fee-related statistics. With this information, we compute several measures to quantify the outcome of the violation: (i) the frequency of waivers, (ii) the cost of amending the loan paid by the borrower, and (iii) the changes of interest rate at the amendment.

D. Examples of Loan Contract Provisions

This section provides an example of the contract language used in the amend and extend and refinancing provisions examined in Section 5.5.4. The language helps make clear that the provisions are designed to facilitate renegotiation by allowing a subset of lenders to agree to change the existing terms of the loan.

Amend and Extend

For an example of an amend and extend provision, J. Crew Group's March 5, 2014 credit agreement contained the following provision:

SECTION 2.14. Extensions of Loans. (a) Notwithstanding anything to the contrary in this Agreement, pursuant to one or more offers (each, an "Extension Offer") made from time to time by the Borrower to all Lenders of Loans with a like Maturity Date on a pro rata basis (based on the aggregate outstanding principal amount of the respective Loans with the same Maturity Date) and on the same terms to each such Lender, the Borrower may from time to time with the consent of any Lender that shall have accepted such offer extend the maturity date of any Loans and otherwise modify the terms of such Loans of such Lender pursuant to the terms of the relevant Extension Offer (including, without limitation, by increasing the interest rate or fees payable in respect of such Loans and/or modifying the amortization schedule in respect of such Loans) (each, an "Extension", and each group of Loans as so extended, as well as the original Loans not so extended, being a "tranche"; any Extended Loans shall constitute a separate tranche of Loans from the tranche of Loans from which they were converted), so long as the following terms are satisfied: (i) no Default shall exist at the time the notice in respect of an Extension Offer is delivered to the Lenders, ..., (ii) except as to interest rates, fees, amortization, final maturity date, premium, required prepayment dates and participation in prepayments (which shall, subject to immediately succeeding clauses (iii), (iv) and (v), be determined by the Borrower and set forth in the relevant Extension Offer), the Loans of any Lender (an "Extending Lender") extended pursuant to any Extension ("Extended Loans") shall have the same terms as the tranche of Loans subject to such Extension Offer ..., (iii) the final maturity date of any Extended Loans shall be no earlier than the

then Latest Maturity Date at the time of extension ..., (iv) the Weighted Average Life to Maturity of any Extended Loans shall be no shorter than the remaining Weighted Average Life to Maturity of the Loans extended thereby, (v) any Extended Loans may participate on a pro rata basis or on a less than pro rata basis (but not on a greater than pro rata basis ... in any voluntary or mandatory prepayments hereunder, ..., (vi) if the aggregate principal amount of Loans (calculated on the face amount thereof) in respect of which Lenders shall have accepted the relevant Extension Offer shall exceed the maximum aggregate principal amount of Loans offered to be extended by the Borrower pursuant to such Extension Offer, then the Loans of such Lenders shall be extended ratably up to such maximum amount based on the respective principal amounts ..., (vii) all documentation in respect of such Extension shall be consistent with the foregoing, (viii) any applicable Minimum Extension Condition shall be satisfied unless waived by the Borrower and (ix) the interest rate margin applicable to any Extended Loans will be determined by the Borrower and the lenders providing such Extended Loans.

Refinancing Facility

As an example, the February 19, 2014 credit agreement for Diamond Foods, Inc. included the following refinancing provision:

2.14 Specified Refinancing Debt. (a) The Borrower may, from time to time, and subject to the consent of the Administrative Agent, add one or more new term loan facilities to this Agreement (“Specified Refinancing Debt”) pursuant to procedures reasonably specified by the Administrative Agent and reasonably acceptable to the Borrower, to refinance all or any portion of the Loans or New Term Loans of any tranche then outstanding under this Agreement pursuant to a Refinancing Amendment; provided that such Specified Refinancing Debt: (i) shall rank pari passu in right of payment with the other Loans and New Term Loans hereunder; (ii) will not be Guaranteed by any Person that is not a Guarantor; (iii) will be unsecured or secured by the Collateral on an equal and ratable basis with the Obligations (or on a second-lien basis pursuant to intercreditor arrangements reasonably satisfactory to the Administrative Agent); (iv) will have such pricing and optional prepayment terms as may be agreed by the Borrower and the applicable Lenders thereof; (v) will have a maturity date that is not prior to the date that is 91 days after the scheduled maturity date of, and will have a Weighted Average Life to Maturity that is not shorter than 91 days longer than the Weighted Average Life to Maturity of, the Loans or New Term Loans being refinanced (provided that, notwithstanding the foregoing, if such Specified Refinancing Debt is secured by the Collateral on an equal and ratable basis with the Obligations, it will have a maturity date that is not prior to the maturity date of, and will have a Weighted Average Life to Maturity that is not shorter than the Weighted Average Life to Maturity of, the Loans or New Term Loans being refinanced); (vi) subject to clauses (iv) and (v) above, will have terms and conditions that are substantially identical to, or less favorable to the Lenders providing such Specified Refinancing Debt than, the terms and conditions of the Loans or New Term Loans being refinanced; and (vii) the Net Cash Proceeds of such Specified Refinancing Debt shall be applied, substantially concurrently with the incurrence thereof, to the pro rata prepayment of outstanding Loans or New Term Loans being so refinanced

pursuant to Section 2.03; provided, however, that such Specified Refinancing Debt shall not have a principal amount (or accreted value) greater than the Loans or New Term Loans being refinanced (excluding any such principal issued or incurred to provide funds for the payment of accrued interest, fees, discounts, premiums or expenses payable in connection with the relevant prepayment).

E. Replicating the Main Results with DealScan

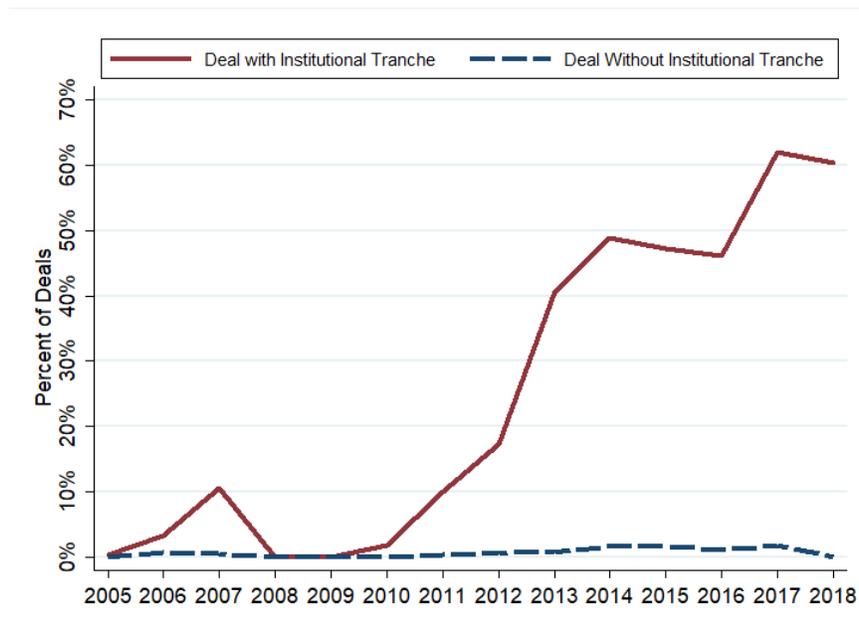
To corroborate our results and facilitate replication, we replicate our main results using data exclusively from DealScan. We use the full sample of leveraged loan deals as constructed in Table 1, except that we extend the sample through the second quarter of 2018. A deal remains defined as the set of facilities with the same facility start date. For this analysis, we do not search for a previously issued revolver but instead assume that every borrower has a revolving line of credit. Moreover, we assume that the revolver has financial maintenance covenants. Based on these assumptions, we define a deal as having split control rights as the presence of a term loan designated by DealScan as covenant-lite, based only on the market segment data.

Figure OA.1 replicates Figure 3 using the larger sample and the alternative definition of split control rights. Consistent with the fact that DealScan fails to identify many covenant-lite term loans, the frequency of split control rights is lower in this sample than in the contract sample during the 2011-2014 period. However, the time series pattern is very similar. Split control rights rose very mildly prior to the crisis and then very sharply during 2011-2014, exclusively for deals with an institutional tranche.

Figure OA.1 also shows that the frequency of split control rights has further increased in the years since 2014. Based on the DealScan sample, the fraction of institutional deals with split control rights increased by about 10 percentage points in 2017 and 2018 relative to 2014, which represents a 20% increase relative to a baseline of 50%. Given that interest rates have risen steadily over this period, Figure OA.1 provides some additional evidence that split control rights is not a contractual feature driven solely by extraordinarily low interest rates.

We next replicate the bivariate probit regression results in Tables 5 and OA.2, again using the larger sample and the alternative definition of split control rights. Here, we examine deals issued over the longer period from 2011-2018, but we separately examine the periods 2011-2014 and 2015-2018 to provide a comparison with the main paper. The sample is restricted to deals

Figure OA.1: Split Control Rights and Institutional Investors, DealScan Sample



Note: The figure shows the annual proportion of leveraged loan deals that contain split control rights, based solely on DealScan data. The red solid line is for deals with an institutional tranche, and the blue dashed line is for deals without an institutional tranche.

with Compustat data, but the sample for the 2011-2014 period is much larger than in Tables 5 and OA.2.

For the period 2011-2014, the results generated using data from the loan contracts can be closely replicated using only data from DealScan. For example, assuming a zero correlation between residuals, the estimated marginal effect of *Institutional* changes from 0.347 in Table 5 to 0.274 in Table OA.4, with the fall likely reflecting the measurement error in DealScan. Nevertheless, the large positive relationship is robust to a high degree of correlation of residuals in model (3). Across the various assumptions for the correlation of residuals in Table OA.4, the estimated marginal effect remains significantly positive and only slightly smaller than those estimated in Table OA.2 using data drawn directly from loan contracts.

Panel B in Table OA.4 shows that this conclusion is even stronger during the more recent 2015-2018 period. For each of the assumptions about the degree of correlation among the residuals, the estimated marginal effect is larger during the later period than the earlier period.

Table OA.4: Split Control Rights and Institutional Tranches, Bivariate Probit 2011-2014, DealScan Sample

	Correlation = 0.0.		Correlation = 0.2		Correlation = 0.4		Correlation = 0.6.	
	Split (1)	Institutional (2)	Split (1)	Institutional (2)	Split (3)	Institutional (4)	Split (5)	Institutional (6)
Panel A. Years 2011-2014 (N=680)								
Institutional Tranche	1.735*** (0.284)		1.398*** (0.285)		1.020*** (0.281)		0.590* (0.269)	
Ln(Assets)	0.114** (0.051)	0.316*** (0.068)	0.136** (0.052)	0.313*** (0.068)	0.156*** (0.052)	0.310*** (0.068)	0.175*** (0.052)	0.304*** (0.068)
Debt / Assets	-0.345 (0.556)	1.945*** (0.484)	-0.183 (0.559)	1.973*** (0.488)	-0.012 (0.553)	2.004*** (0.492)	0.172 (0.540)	2.044*** (0.498)
Debt / OIBDP	0.148** (0.063)	-0.019 (0.042)	0.143** (0.063)	-0.020 (0.041)	0.134** (0.062)	-0.022 (0.040)	0.123* (0.060)	-0.024 (0.040)
OIBDP / Assets	4.431* (2.003)	-2.855* (1.420)	4.096* (1.980)	-2.911* (1.426)	3.662 (1.925)	-2.964* (1.434)	3.108 (1.837)	-3.014* (1.444)
M.E. of Institutional Tranche	0.274*** (0.039)		0.221*** (0.040)		0.165*** (0.041)		0.100** (0.043)	
Panel B. Years 2015-2018 (N=810)								
Institutional Tranche	2.244*** (0.115)		1.872*** (0.113)		1.442*** (0.108)		0.940*** (0.100)	
Ln(Assets)	0.157*** (0.038)	0.303*** (0.060)	0.182*** (0.039)	0.298*** (0.059)	0.206*** (0.039)	0.291*** (0.057)	0.230*** (0.039)	0.282*** (0.055)
Debt / Assets	0.234 (0.405)	1.338*** (0.408)	0.343 (0.410)	1.328*** (0.410)	0.459 (0.410)	1.306*** (0.413)	0.587 (0.405)	1.269*** (0.416)
Debt / OIBDP	0.030 (0.068)	0.082 (0.051)	0.038 (0.067)	0.084 (0.051)	0.045 (0.066)	0.086* (0.051)	0.052 (0.063)	0.089* (0.051)
OIBDP / Assets	-0.256 (2.045)	1.442 (1.774)	-0.101 (1.997)	1.471 (1.771)	0.072 (1.913)	1.503 (1.766)	0.264 (1.788)	1.539 (1.753)
M.E. of Institutional Tranche	0.494*** (0.021)		0.431*** (0.021)		0.350*** (0.022)		0.243*** (0.023)	

Note: This table displays coefficient estimates from the bivariate probit model presented in model (3), where the residual correlation is constrained to be 0.0, 0.2, 0.4, and 0.6. The dependent variables are indicators that a deal has split control rights and contains an institutional tranche, based solely on data from DealScan. “OIBDP” stands for operating income before depreciation. “M.E. of Institutional Tranche” reports the marginal effect of a discrete change in Institutional Tranche from 0 to 1, computed with control variables at their sample means. Each specification includes fixed effects for the calendar quarter the deal was originated, fixed effects for the Fama-French 30 industry classification of the borrower’s SIC code, and fixed effects for the borrower’s credit rating. The sample in Panel A includes all deals in DealScan issued between 2011 and 2014, and Panel B includes deals issued between 2015 and 2018:Q2. Standard errors are reported in parentheses.

* p < 0.10, ** p < 0.05, *** p < 0.01

E. Recent Deals

To ensure that covenant-lite remains best interpreted as split control rights, we examine a set of 10 randomly chosen deals issued during 2017. Using the full sample of deals merged with Compustat data, we select 10 deals that DealScan denotes as having a covenant-lite term loan. For each of these deals, we find the underlying credit agreement in EDGAR and confirm that the term loan does not have financial covenants. For three of the deals, the credit agreement covers only a term loan, but we are able to find a pre-existing revolving loan for each of the borrowers. For each of the 10 deals, we verify that the revolving loan has financial covenants, so in each case the loan has split control rights. Table OA.5 lists each of the deals we examine along with a link to the underlying credit agreement covering the term loan.

Table OA.5: Recent Deals

Borrower	CIK Code	Date of Loan Agreement	Deal Includes Revolver	Split Control		Link to Agreement
				Revolver	Rights	
Clean Harbors Inc	106040	20170630	N	Y		/822818/000082281817000024/ex443_creditagreement.htm
Arch Coal Inc	1037676	20170307	N	Y		/1037676/000110465917014979/a17-7785_1ex10d1.htm
Ferro Corp	35214	20170214	Y	Y		/35214/000119312517048028/d267937dex101.htm
Terex Corp	97216	20170131	Y	Y		/97216/000139843217000024/exh10_01.htm
Sinclair Broadcast Group	912752	20170103	Y	Y		/912752/000091275217000002/sinclair-thirdamendmenttoc.htm
Scientific Games Corp	750004	20170214	Y	Y		/750004/000114420417008829/v459394_ex10-1.htm
Minerals Technologies	891014	20170214	Y	Y		/891014/000095015717000257/ex10-1.htm
Zebra Technologies Corp	877212	20170726	Y	Y		/877212/000087721217000026/a101zebracreditagreement.htm
Jeld-Wen Inc	1674335	20170307	N	Y		/1674335/000119312517073723/d342319dex101.htm
Calpine Corp	916457	20170328	Y	Y		/1584509/000119312517101789/d367212dex101.htm

Note: This table shows a set of 10 randomly chosen leveraged loans that DealScan reports as having a covenant-lite term loan. The column “Deal Includes Revolver” denotes whether the loan agreement also covers a revolving loan. In the cases where the agreement does not cover a revolving loan, we confirm that the borrower has a revolving loan. The column “Split Control Rights” denotes whether the deal has split control rights. The “Link to Agreement” provides a portion of the url to see the underlying agreement, using the prefix <https://www.sec.gov/Archives/edgar/data>.