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Real Assets and Capital Structure

Murillo Campello and Erasmo Giambona*

Abstract

We characterize the relation between asset structure and capital structure by exploiting variation in the salability of corporate assets. To establish this link, we distinguish across different assets in firms' balance sheets (machinery, land, and buildings) and use an instrumental approach that incorporates market conditions for those assets. We also use a natural experiment driving differential increases in the supply of real estate assets across the United States: The Defense Base Closure and Realignment Act of 1990. Consistent with a supply-side view of capital structure, we find that asset redeployability is a main driver of leverage when credit frictions are high.

I. Introduction

Theory suggests that contract incompleteness and limited enforceability reduce a firm's access to external finance (Hart and Moore (1994), Holmstrom and Tirole (1997)). In the presence of such frictions, assets that are tangible are more desirable from the perspective of creditors because they are easier to repossess in bankruptcy states ("verifiable by the courts"). Tangible assets, however, often lose value when sold under distress (see evidence in Acharya, Bharath, and Srinivasan (2007)). These losses imply that only those tangible assets that can be easily redeployed should sustain high debt capacity. Differently put, tangible assets should facilitate corporate borrowing only to the extent that they are liquid. While this distinction is intuitively clear, it is rarely articulated in capital structure tests.

*Campello, campello@cornell.edu, Johnson Graduate School of Management, Cornell University, 369 Sage Hall, Ithaca, NY 14853 and National Bureau of Economic Research; Giambona, e.giambona@uva.nl, Business School, University of Amsterdam, Roetersstraat 11, Amsterdam 1018 WB, Netherlands. We are thankful for comments from Malcolm Baker, Antonio Galvao, Wenlian Gao, Alessandro Gavazza, John Graham, Ulrich Hege, Paul Malatesta (the editor), Ralf Meisenzahl, Antonio Mello, Lilian Ng, Hernan Ortiz-Molina, José Luis Peydró-Alcalde, Gordon Phillips, Petya Platikanov, Amit Seru, David Thesmar, Philip Valta, Qinghai Wang, Xueping Wu, and Josef Zechner. We are especially thankful to Michael Faulkender (the referee) for many insightful comments. We also thank seminar participants at the 2010 Brazilian Finance Society Meetings, Catholic University of Milan, Cornell University, European Business School, 2011 Financial Intermediation Research Society Conference, 2011 Financial Management Association European Conference, HEC, 2010 International Conference on Panel Data, Maastricht University, University of Amsterdam, University of New South Wales, and University of Wisconsin at Madison. An earlier version of this paper was circulated with the title "Capital Structure and the Redeployability of Tangible Assets." Ping Liu provided excellent research assistance. We are responsible for any remaining errors.

This paper characterizes the relation between asset tangibility and capital structure by exploiting variation in the supply and demand for corporate assets. Assets that are less firm-specific should allow for higher debt capacity because they are easier to resell, for example, to other firms in the same industry (Shleifer and Vishny (1992)). Assets whose usage responds to supply and demand shifts in their secondary markets are also more likely to be redeployable (see Gavazza (2011)). Using these insights, we decompose the measure of asset tangibility commonly used in capital structure tests (property, plant, and equipment (PP&E)) into its main components. We then assess variation in redeployability across each of those components by way of an instrumental variables (IV) approach that uses variation in asset salability in secondary markets. Our study reports new findings on the relation between asset tangibility and capital structure, identifying when and how tangibility affects leverage. Consistent with the view that tangibility facilitates access to credit, we show that the *redeployability* of tangible assets is an important driver of leverage for firms that are more likely to face credit frictions, especially during periods of tight credit in the economy.

Our analysis proceeds in several steps. Our first, basic step is to replicate standard capital structure tests using our data to study the relation between the common proxy for asset tangibility (the ratio of PP&E to total assets) and firm leverage. We then examine the economic relevance of different components of tangibility. This examination is new to the literature and entails breaking down tangible assets into their identifiable parts, which include land and buildings, machinery and equipment, and other miscellaneous assets. Notably, we evaluate the importance of these categories using variation coming from the redeployability of the underlying assets. We do so via an IV approach that identifies the component (or "margin") of tangibility that responds to shifts in liquidity and salability proxies.

Our base tests employ a number of instruments. The first set of instruments speaks to the liquidity of land and buildings owned by firms. This set contains proxies for the supply and demand conditions in the real estate markets where firms operate, including proxies for local real estate operators, the local disposal of real estate assets by the federal government (the largest real estate "supplier" in the United States), as well as the pricing and volatility of local rental rates. A second set of instruments relates to liquidity in the market for machinery and equipment. These include proxies for the volume of transactions of second-hand machinery and equipment in the industries in which our sample firms operate (e.g., Schlingemann, Stulz, and Walkling (2002), Campello (2006)). The list of instruments also includes information on industrial workforce, which affects capital/labor ratios and the demand for fixed assets (MacKay and Phillips (2005), Garmaise (2008)). Sources of data for our instruments range from standard Compustat to the Savings & Loan (SNL) Financial real estate database, to authors' filings of information request under the Freedom of Information Act.

We supplement our tests with evidence from a natural experiment: The Defense Base Closure and Realignment Act of 1990 (DBCRA). The DBCRA mandated the closure and disposition of a large number of military bases and supporting facilities across the United States. This created a supply shock of more than 100,000 acres of land and thousands of buildings suitable for redevelopment

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into office parks and industrial zones. This event is unique in that it generated an influx of corporate-type assets that was not caused by changes in local economic circumstances, but by a nationwide sale of properties that became redundant with the end of the Cold War.[1] The shock affected local commercial real estate markets in nonhomogeneous ways. We take a careful look at this innovation to study how firms adjusted their holdings of land and buildings and whether these adjustments had an effect on their debt capacity. We do so via a difference-in-differences matching estimation approach designed to ensure that firms under examination ("treated" and "controls") are similar except for the extent to which their local real estate markets were affected by the disposition of military bases under the DBCRA.

Our evidence shows that tangible assets drive capital structure *only to the extent* that they are redeployable. Put differently, only the component of asset tangibility that responds to salability ("marketable tangibility") exerts explanatory power over corporate leverage. In addition, across the various categories of tangible assets, we find that land and buildings (arguably, the least firm-specific fixed assets) have the *most* explanatory power over leverage ratios. At the same time, assets that are more directly linked to firm-specific production processes, such as machines and equipment, have only a small explanatory power over leverage. The results we report are new to the literature and are consistent with the argument that frictions such as contract incompleteness and limited enforceability (frictions that are alleviated via access to liquid collateral) have first-order effects on corporate leverage.

To further characterize our inferences about corporate assets and leverage, we contrast firms that are more likely to face credit frictions (small, unrated, and low-dividend-payout firms) with firms that are less likely to face such frictions (large, rated, and high-payout firms). We find that the redeployability–leverage relation is pronounced across the set of credit-constrained firms (firms for which collateral recourse is particularly important in the borrowing process). For unconstrained firms, in contrast, redeployability is an irrelevant driver of leverage. To be concrete, our small-firm estimates imply that a 1-interquartile range (IQR) change in asset redeployability is associated with a 39% increase in market leverage. This is equivalent to a shift in leverage from its mean of 22% to about 31%. For large firms, however, asset redeployability has no effect on capital structure. These cross-sectional contrasts are consistent with the logic of the financing friction argument: Variation in asset redeployability only affects the borrowing capacity of those firms that are likely to be financially constrained.

Macroeconomics research suggests that the extent to which credit frictions bind and affect firm behavior is often a function of the state of the economy (e.g., Bernanke and Gertler (1995)). This observation points to time-series variation that can be used to further identify the redeployability–leverage channel that we propose. Following Kashyap and Stein (2000), we employ a two-step estimator that builds on this intuition and find that the role of redeployability

[1]As we detail below, decisions about base closings and dispositions under the DBCRA were governed by a strict "national security first" doctrine. Sales of real estate facilities under the act were fairly orthogonal to local economic conditions.

in alleviating financing frictions is heightened during periods of tight credit. We estimate, for example, that a 100-basis-point (bp) increase in the Fed funds rate (a proxy for credit tightening) leads to a 42% increase in the sensitivity of leverage to asset redeployability. Consistent with the supply-side view of capital structure, our macro tests suggest that asset redeployability increases debt capacity by ameliorating credit frictions.

It is important that we put our findings in context with the recent literature. Faulkender and Petersen (2006) find that firms with credit ratings (a broad proxy for access to the public debt markets) have higher leverage. Our paper complements Faulkender and Petersen's results in that we explore different sources of data variation in providing evidence of a supply-side view of capital structure. Notably, we find that the economic effect of redeployability on leverage might be as large as that of credit ratings, suggesting that supply-side determinants of capital structure might be even stronger than previously thought. The substantive contribution of our study is that we identify and explore a well-characterized channel through which features of financial contracting (liquidity of collateral recourse) affect credit supply and corporate leverage.

We also experiment with Lemmon, Roberts, and Zender's (2008) leverage model to check whether our inferences about asset tangibility pass those authors' "fixed-effects stress tests." Lemmon et al. show that traditional determinants of leverage become largely irrelevant once the econometrician accounts for time-invariant firm effects. Like those authors, we find that regression coefficients of traditional leverage drivers become mostly insignificant after accounting for firm effects.[2] However, our findings point to a different pattern with respect to our tangibility proxies. Relative to the baseline ordinary least squares (OLS) model of Lemmon et al., the effect of land and buildings on leverage increases by a factor of almost 3 in firm-fixed effects IV estimations. Our findings suggest that while within-firm variation in the traditional determinants of leverage has generally limited ability in explaining variation in leverage, land and buildings seem to play a key role in explaining variation in leverage not only in the cross section but also within the firm in the time series. Our inferences also survive the inclusion of "initial leverage" in the regression specification (also following Lemmon et al.). These experiments highlight the robustness of the redeployability–leverage channel we propose. We conjecture that the estimation performance of other traditional leverage determinants might also improve upon better empirical characterization.

Our paper adds to current research on capital structure by considering credit supply-side frictions as determinants of leverage. A few other papers have explored related ideas. Benmelech (2009) uses variation in the width of track gauges of 19th century railroads to measure asset salability. Empirically, he finds that railroad companies that used more liquid track gauges were able to raise debt with longer maturities, but not necessarily have higher leverage. Using data from the airline industry, Benmelech and Bergman (2009) find that debt tranches secured

[2]Notably, the overarching theme of our analysis is to investigate capital structure dispersion across firms. While we use standard regression analysis to get at this question, we need to make sure our findings are robust to unobserved time-invariant firm heterogeneity. As such, our methods emphasize the use of fixed-effects models.

by more liquid collateral pay lower interest rates and sustain higher loan-to-value ratios.[3] Ortiz-Molina and Phillips (2013) find that asset liquidity lowers the implied cost of capital. The authors, however, do not examine the relation between liquidity and leverage. Chaney, Sraer, and Thesmar (2012) use data on corporate holdings of land to show that shocks to the value of real estate affect a firm's ability to invest (see also Gan (2007)). Lemmon and Roberts (2010) use the 1989 collapse of the junk bond market to study the effect of a credit supply shock on bond issuers. The authors do not find an effect of credit supply on leverage. Our paper contributes to this literature by providing systematic evidence (across firms, time, and industries) of first-order effects of credit supply on firm leverage. Our analysis pins down a well-defined channel (the redeployability of tangible assets) in identifying how credit frictions affect capital structure.[4]

The remainder of the paper is organized as follows: The next section describes the data and compares our sample to those of standard capital structure studies. Section III presents our central results on the effect of asset tangibility (and its various components) on capital structure. Section IV contrasts results across sample partitions where firms are likely to face different degrees of financing frictions. It also contrasts our findings across times of tight and easy credit in the economy. Section V compares the impact of asset tangibility with that of other leverage determinants discussed in recent studies. Section VI concludes the paper.

II. Base Analysis

A. Sampling and Variable Construction

Our sample consists of active and inactive firms from Compustat with main operations in the United States from 1984 through 1996. We focus on that time window because one of our goals is to gauge the relative importance of the different components of firms' PP&E, and Compustat does not report that decomposition in other years. The raw sample includes all firms for which we can gather information on the different components of PP&E except financial, lease, real estate investment trust (REIT) and real estate-related, nonprofit, and governmental firms. We exclude firm-years for which the value of total assets or net sales is less than $1 million. We further exclude firm-years observing an increase in size or sales of more than 100%, or for which market-to-book ratios are greater than 10. Similarly, we exclude firms involved in major restructurings, bankruptcy, or merger activities.

We combine Compustat with several other data sources. We do this in order to implement an IV approach that deals with the endogeneity of asset tangibility. We model the endogeneity of tangibility as a function of industry characteristics,

[3]Relatedly, Benmelech, Garmaise, and Moskowitz (2005) find a positive relation between the liquidation value of commercial real estate and the size of mortgage contracts.

[4]In contemporary work, Rampini and Viswanathan (2013) report evidence of positive correlation between fixed assets (PP&E) and leverage. In contrast to our study, however, those authors neither look at the redeployability of tangible assets, differentiate between different types of tangible assets, nor account for the endogeneity of tangibility.

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real estate market conditions, and the liquidity of the secondary market for machinery and equipment, among others. To streamline the discussion, we dedicate the remainder of this section to describing sample statistics, variable construction, and regression models that are commonly found in the literature. We discuss our instruments in detail in the next section.

The basic left-hand-side variable of the models we estimate is market leverage. Following the literature, *MarketLeverage* is the ratio of total debt (Compustat items *dltt* + *dlc*) to market value of total assets, or *at* – *ceq* + (*prcc_f* × *cshpri*). In every estimation performed, we also look at book values of debt, where we compute *BookLeverage* as the ratio of total debt to book value of total assets (*at*). The drivers of leverage that we examine are also standard, coming from an intersection of papers written on the topic over the last two decades.[5] *Size* is the natural logarithm of the market value of total assets (measured in millions of 1996 constant dollars). *Profitability* is the ratio of income before interest, taxes, depreciation, and amortization (*oibdp*) to book value of total assets. *Q* is the ratio of market value of total assets to book value of total assets. *EarningsVolatility* is the ratio of the standard deviation of income before interest, taxes, depreciation, and amortization to total book assets, computed from 4-year windows of consecutive firm observations. *MarginalTaxRate* is Graham's (2000) marginal tax rate, available from John Graham's Web site (https://faculty.fuqua.duke.edu/∼jgraham/). *RatingDummy* is a dummy variable that takes a value of 1 if the firm has either a bond rating (*splticrm*) or a commercial paper rating (*spsticrm*), and 0 otherwise.

Our focus is on asset tangibility and its components. We denote the standard measure of asset tangibility by *OverallTangibility*, which is defined as the ratio of total tangible assets (*ppent*, or "PP&E") to book value of total assets. *Land&Building* is the ratio of net book value of land and building (*ppenli* + *ppenb*) to the book value of total assets. *Machinery&Equipment* is the ratio of net book value of machinery and equipment (*ppenme*) to book value of total assets. *OtherTangibles* is the ratio of plant and equipment in progress and miscellaneous tangible assets (*ppenc* + *ppeno*) to book value of total assets.

B. Descriptive Statistics

Table 1 presents the descriptive statistics of our data. Our sampling methods and variable construction approaches are similar to those used in existing capital structure studies and, not surprisingly, the associated descriptive statistics mimic those of existing papers. Faulkender and Petersen (2006), for example, report average market and book leverage of, respectively, 19.9% and 26.1%. This is very similar to the corresponding averages of 20.2% and 25.6% that we find for our sample. Similarly, the average *OverallTangibility* of 35.6% that we report is comparable to the average of 34% reported in the Lemmon et al. (2008) and

[5]The literature we follow in our variable selection process includes Barclay and Smith (1995), Rajan and Zingales (1995), Graham (2000), Baker and Wurgler (2002), Frank and Goyal (2003), Korajczyk and Levy (2003), Faulkender and Petersen (2006), Flannery and Rangan (2006), and Lemmon et al. (2008).

Frank and Goyal (2003) studies, or the 33.1% figure reported by Faulkender and Petersen.

TABLE 1

Sample Descriptive Statistics

Table 1 reports summary statistics for the main variables used in the paper's empirical estimations. All firm-level data, with the exception of the marginal tax rate, are obtained from Compustat industrial tapes over the sample period 1984–1996. The sample includes all firms except financial, lease, REIT and real estate-related, nonprofit, and governmental firms. *MarketLeverage* is the ratio of total debt (Compustat items *dltt* + *dlc*) to market value of total assets, or (*at* − *ceq* + *prcc_f* × *cshpri*). *BookLeverage* is the ratio of total debt to book value of total assets (*at*). *OverallTangibility* is the ratio of total tangible assets (*ppent*) to book value of total assets. *Land&Building* is the ratio of net book value of land and building (*ppenli* + *ppenb*) to the book value of total assets. *Machinery&Equipment* is the ratio of net book value of machinery and equipment (*ppenme*) to book value of total assets. *OtherTangibles* is the ratio of plant and equipment in progress and miscellaneous tangible assets (*ppenc* + *ppeno*) to book value of total assets. *Size* is the natural logarithm of the market value of total assets (measured in millions of 1996 dollars using the Producer Price Index (PPI) published by the U.S. Department of Labor as the deflator). *Profitability* is the ratio of earnings before interest, taxes, depreciation, and amortization (*oibdp*) to book value of total assets. *Q* is the ratio of market value of total assets to book value of total assets. *EarningsVolatility* is the ratio of the standard deviation of earnings before interest, taxes, depreciation, and amortization using 4 years of consecutive observations to the average book value of total assets estimated over the same time horizon. *MarginalTaxRate* is Graham's (2000) marginal tax rate. *RatingDummy* is a dummy variable that takes a value of 1 if the firm has either a bond rating (*splticrm*) or a commercial paper rating (*spsticrm*), and 0 otherwise.

	Sample Statistics					
Variables	Mean	Median	Std. Dev.	25th Pct.	75th Pct.	No. of Obs.
MarketLeverage	0.202	0.163	0.175	0.056	0.307	10,128
BookLeverage	0.256	0.227	0.222	0.095	0.367	10,128
OverallTangibility (*PP&E*)	0.356	0.327	0.175	0.244	0.452	10,015
Land&Building	0.118	0.103	0.113	0.035	0.162	10,015
Machinery&Equipment	0.189	0.161	0.129	0.104	0.237	10,015
OtherTangibles	0.015	0.000	0.043	0.000	0.014	10,014
Size	5.038	4.860	1.945	3.620	6.253	10,128
Profitability	0.107	0.133	0.169	0.068	0.187	10,128
Q	1.621	1.298	1.054	1.026	1.808	10,128
EarningsVolatility	0.091	0.067	0.089	0.042	0.110	10,078
MarginalTaxRate	0.321	0.340	0.104	0.298	0.360	10,128
RatingDummy	0.164	0.000	0.370	0.000	0.000	10,128

A novel feature of our study is the decomposition of asset tangibility. Table 1 reports that *Land&Building* and *Machinery&Equipment* are both key components of *OverallTangibility*. These items are also quite relevant in terms of the total asset base of the firms in Compustat. The mean (median) ratio of *Land&Building* to total assets is equal to 11.8% (10.3%). For *Machinery&Equipment* the mean (median) ratio is 18.9% (16.1%). In contrast, *OtherTangibles* accounts for only 1.5% of total assets.

C. Standard Leverage Regressions

We verify that our sample is representative of previous capital structure studies by running standard leverage regressions for both the 1984–1996 window (which we use due to data availability) and a larger 1971–2006 window (for comparability with other papers). Similar to previous studies, we estimate a benchmark regression model for *Leverage* (either market or book values) of the following form:

$$(1) \quad Leverage_{i,t} = c + \alpha OverallTangibility_{i,t} + \beta \mathbf{X}_{i,t} + \sum_i Firm_i + \sum_t Year_t + \varepsilon_{i,t},$$

where the index i denotes a firm, t denotes a year, c is a constant, and $\mathbf{X}$ is a matrix containing the standard control variables just described (*Size*, *Q*, *Profitability*, etc.). *Firm* and *Year* absorb firm- and time-specific effects, respectively. Our current focus is on the importance and robustness of the coefficients returned for *OverallTangibility*. We will use these estimates as a benchmark for the tests conducted later in the paper.[6] All of our regressions are estimated with heteroskedasticity-consistent errors clustered by firm (Petersen (2009)).

The results are reported in Table 2. The standard leverage regression (equation (1)) is estimated four times, considering different combinations of leverage definitions (*MarketLeverage* vs. *BookLeverage*) and sample periods (1984–1996 vs. 1971–2006). For our purposes, the key finding from Table 2 is that the coefficients returned for *OverallTangibility* are of similar magnitudes across the 1984–1996 and 1971–2006 windows. The coefficients are also similar to those reported in prior studies. For the *MarketLeverage* model, we find that the coefficient on *OverallTangibility* is 0.212 in the 1984–1996 baseline sample, compared to 0.220 in the 1971–2006 extended sample.[7] These estimates are economically and statistically indistinguishable from each other. Inferences are similar for the *BookLeverage* model. The magnitudes of the coefficients associated with the other regressors are also generally similar across samples.[8]

TABLE 2

Standard Leverage Regressions

Table 2 reports regression results for OLS with firm-fixed effects estimations of the restricted model (equation (1) in the text) for both our sample and an extended Compustat sample (for 1971–2006). Estimations also include year dummy variables. All firm-level data are from Compustat industrial tapes. Refer to Table 1 for detailed variable definitions. The sample includes all firms except financial, lease, REIT and real estate-related, nonprofit, and governmental firms. Standard errors reported in parentheses are based on heteroskedastic-consistent errors adjusted for clustering across observations of a given firm (Petersen (2009)). ***, **, and * indicate statistical significance at the 1%, 5%, and 10% (2-tail) test levels, respectively.

	Market Leverage		Book Leverage	
Independent Variables	1984–1996	1971–2006	1984–1996	1971–2006
OverallTangibility	0.212***	0.220***	0.231***	0.245***
	(0.028)	(0.010)	(0.038)	(0.014)
Size	0.005	0.018***	0.016	0.021***
	(0.006)	(0.002)	(0.014)	(0.003)
Profitability	−0.115***	−0.141***	−0.121***	−0.179***
	(0.019)	(0.007)	(0.039)	(0.014)
Q	−0.048***	−0.044***	−0.013*	−0.001
	(0.004)	(0.001)	(0.007)	(0.003)
EarningsVolatility	−0.028	−0.009	−0.203	0.004
	(0.064)	(0.016)	(0.279)	(0.030)
MarginalTaxRate	−0.169***	−0.189***	−0.218***	−0.224***
	(0.026)	(0.010)	(0.035)	(0.015)
RatingDummy	0.042***	0.039***	0.068***	0.059***
	(0.008)	(0.003)	(0.013)	(0.004)
No. of obs.	9,748	97,154	9,748	97,154
Adj. R^2	0.213	0.203	0.090	0.085

[6] Our inferences are the same whether or not we lag the right-hand-side variables of equation (1).

[7] In the capital structure literature, coefficients for *OverallTangibility* range from 0.18 in Frank and Goyal (2003) to 0.32 in Rajan and Zingales (1995).

[8] To avoid repetition, we discuss the coefficients of the other regressors in the tests of the next section.

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III. Main Results

We now examine whether redeployability of a firm's assets is a first-order determinant of observed dispersion in capital structure. We do so by partitioning the commonplace measure of asset tangibility (PP&E over assets, which we call *OverallTangibility*) into its identifiable components from Compustat (*Land&Building*, *Machinery&Equipment*, and *OtherTangibles*). We then adopt an IV approach that considers redeployability across different classes of tangible assets. Finally, we further characterize the redeployability–leverage channel using a natural experiment approach.

A. The Components of Asset Tangibility

The estimation of equation (1) (the standard leverage model) restricts the coefficients on the different components of asset tangibility to a single estimate. We refer to that equation as the "restricted model." In this section, we reestimate equation (1) under different econometric approaches. More importantly, we also allow different components of asset tangibility to attract individual coefficients. We call this alternative model the "unrestricted model." The unrestricted tangibility model of leverage can be written as

$$(2) \quad Leverage_{i,t} = c + \alpha_1 Land\&Building_{i,t} + \alpha_2 Machinery\&Equipment_{i,t} + \alpha_3 OtherTangibles_{i,t} + \beta \mathbf{X}_{i,t} + \sum_i Firm_i + \sum_t Year_t + \varepsilon_{i,t},$$

where *Leverage*, c, and $\mathbf{X}$ are defined similarly to equation (1), with *Firm* and *Year* absorbing firm- and time-specific effects, respectively.

The standard approach to the estimation of equations such as equations (1) and (2) is the OLS model. One should be concerned, however, with the potential for empirical biases in this estimation. While the tangibility of a firm's assets (the type and mix of assets it uses) might be independently determined by the line of business it operates, one can argue that the firm ultimately makes decisions about the proportion of inputs employed in its production process (e.g., different levels and combinations of land, machinery, labor, and intangibles), making observed asset tangibility an *endogenous* variable. This may bias the estimates of equations (1) and (2) under OLS.

It is difficult to argue away the biases that arise from OLS estimations in this context. A reverse-causality story, for example, could yield a positive association between leverage and tangibility if the firm raises debt to acquire tangible assets. Alternatively, an omitted variable story could be told in which good firm fundamentals may lead to *both* higher use of external financing (in the form of debt) and higher fixed asset acquisition. Our tests, in turn, look for variation coming from the redeployability of different types of assets under an IV approach that is helpful in dealing with potential endogeneity between leverage and tangibility. We supplement these tests using a unique natural experiment.

B. An Instrumental Approach

Much of the analysis in this paper focuses on inferences based on IV approaches to modeling the relation between a firm's leverage and the various components of its tangible assets.[9] The issue of endogeneity of tangibility has not been previously addressed in the empirical capital structure literature. This task is challenging due to the heterogeneity that is imbedded in the traditional measure of tangibility, which includes assets as diverse as vacant land and machines in progress. Econometrically, this implies finding valid instruments for each identifiable type of tangible assets. We experiment with multiple sets of instruments, which we describe in turn. Admittedly, any IV approach is subject to some degree of skepticism. Beyond standard checks of instrument validity and relevance, we make sure that our results do not hinge on any particular instrument choice and are robust to the exclusion of individual instruments. The tests we propose are useful and robust to a number of concerns associated with leverage estimations that use asset tangibility as an input.

1. Sets of Instruments

Our analysis shows that land and buildings are major components of tangible assets, and our first set of instruments contains drivers of supply and demand conditions in the real estate markets where firms operate.

Existing research shows that corporate demand for real estate increases with the volatility of rental rates (Rosen, Rosen, and Holtz-Eakin (1984), Ben-Shahar (1998), and Sinai and Souleles (2005)). This happens because real estate ownership provides insurance against fluctuations in rental costs. Parallel to the insurance rationale, rental cost volatility is unlikely to be a first-order driver of firm capital structure choices, other than through its impact on the demand for real estate facilities. These observations suggest that rental cost volatility can function as an instrument in the relation between firms' holdings of commercial real estate and their leverage. We proxy for the volatility of rental costs in local real estate markets with the average income volatility of commercial real estate lessors operating in the firm's headquarters state. The data used to compute this proxy are taken from SNL–Datasource. We expect this time-varying instrument (denoted *RentalVolatility*) to attract a positive sign in the first stage of our IV estimations.[10]

Land economics research also shows that firms operating in areas where buildings and production facilities are not readily available from the market hold more of those facilities in their balance sheets (see Malpezzi and Green (1996), Ortalo-Magne and Rady (2002)). This evidence is consistent with theoretical work from the search literature, where parties hold longer onto assets with illiquid

[9]For completeness and comparability, however, we also report results from standard OLS models.

[10]One concern is whether firms' major facilities and headquarters are located in the same real estate market. Denis, Denis, and Keven (2002) find that 70% of nonfinancial firms in Compustat conduct their entire operations within one geographical area (largely, the same state). Gao, Ng, and Wang (2008) find that among firms with operations residing outside of the headquarters' state, the median firm has operations in only one additional state (often a neighboring state). While relatively scant, the available evidence suggests that the bulk of operating facilities of most firms (headquarters and major plants) are located together in the same state, consistent with our identification strategy.

secondary markets since selling and repurchasing these assets too often entails high trading costs (see Diamond (1982), Duffie, Gârleanu, and Pedersen (2005)).[11] Existing evidence also suggests that the presence of REITs in a local real estate market is indicative of the liquidity of the market for commercial properties used by firms. Indeed, REITs were introduced with the Real Estate Act of 1960 to enhance the liquidity of commercial real estate assets, and various studies show that REITs increase the supply of real estate in local markets (see Chan, Erickson, and Wang (2003), Geltner, Miller, Clayton, and Eichholtz (2007)).[12] We proxy for the depth of the supply of local commercial real estate facilities using the log of the number of REITs operating in a firm's state (denoted *LogSuppliers*). Since firms are expected to hold less real estate assets when local real estate markets are more liquid, we expect this instrument to enter our estimations with a negative sign.

To supplement our set of real estate market instruments, we include a proxy for the sale of real estate by the federal government (*GovernmentDisposal*). The federal government is the largest real estate "supplier" in the United States, and disposals of land and buildings by the government (which can be massive at times) are known to impact local commercial real estate markets.[13] The Federal Properties Disposition Act of 1949 regulates the process of disposal and management of U.S. government properties. The purpose of the act is to restrain federal spending, and one can argue that the federal government's need to dispose of land is plausibly exogenous to economic circumstances of local real estate markets and firms operating in those markets. Building on extant research, one can conjecture that firms operating in state-years where the government disposes of real estate assets will hold less land and buildings in their balance sheets due to the lower price volatility (Sinai and Souleles (2005)) and higher availability (Ortalo-Magne and Rady (2002)) of those assets. We obtain state-year panel data on U.S. dealings in real estate assets by filing a request under the Freedom of Information Act.[14]

Our second set of instruments looks at the market for machinery and equipment. Our first instrument in this set considers the liquidity of machinery and equipment within the industry in which firms operate. Firms operating in industries with an active secondary market for their equipment will be more likely to carry those assets at a lower cost in their balance sheets (Almeida and Campello (2007)). In particular, since those assets can be easily found in secondary markets, they need not be built (custom made) for the firm. Instead, they can be bought as used goods and integrated in the firm's production process at a lower user cost (see Gavazza (2011)). Following Schlingemann et al. (2002), we use the 4-digit Standard Industrial Classification (SIC) industry-year ratios of sales of PP&E

[11]Related work by Gavazza (2011) on real asset markets is discussed in further detail later.

[12]REITs hold property portfolios that are both highly focused on a specific property segment (e.g., office buildings or industrial facilities) and geographically concentrated.

[13]Land ownership by the federal government varies greatly across states. In the northeastern states of New York and Connecticut, for example, the U.S. government owns less than 1% of total state-land acreage, while land ownership is as high as 44% in California and 52% in Oregon. The origin of this variation dates back to the Northwest Territory Ordinance of 1787, by which new states (beyond the original 13 colonies) were obligated to transfer massive amounts of land to the fledgling U.S. government.

[14]These data are compiled by the Real Property Disposal Division (General Services Administration (GSA)) under the U.S. Department of Commerce.

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to the sum of sales of PP&E and capital expenditures (Compustat items *sppe*/(*sppe* + *capx*)) as a proxy for the liquidity of machinery and equipment in the industry in which a firm operates (see also Sibilkov (2009)). This proxy is denoted *IndustryResale*.

Prior literature also shows that manufacture structure (machinery and equipment) and labor configuration are correlated decisions (see MacKay and Phillips (2005), Garmaise (2008)). Following Garmaise, we use the 4-digit SIC industry-year ratios of the number of employees scaled by total assets as an additional instrument for fixed capital. The idea is that while different firms may use different levels of capital and labor in their production process, depending on considerations such as capital vintage and utilization, one might expect these two quantities to move together along the investment expansion path. We use industry-level measures of that relation (*IndustryLabor*) to capture variation that is not part of the individual firm's policy set.[15]

Before moving to the estimation of leverage models (the focus of the analysis), it is important that we assess the quality of our instruments. To streamline the flow and provide detail, we dedicate a separate Appendix to a full-fledged discussion of the battery of tests we perform in order to assess the properties of our instruments. Tests for *instrument relevance* and *instrument validity* lend strong support to our empirical implementation. Perhaps more important, the economic priors we utilize to select our instruments are confirmed in the first-stage regressions reported in Table A1 in the Appendix.

2. Core Business, Real Estate Markets, and Assets Holdings: The Case of Wal-Mart

Our identification strategy is centered on the acquisition process of corporate properties. To illustrate the economic rationale of this process in relation to our identification strategy, we provide an example using a firm from our Compustat sample: Wal-Mart. Our focus is on how rental volatility works in governing the decision to own or lease corporate properties that are linked to that firm's core business.

Wal-Mart's strategy toward real estate assets includes a combination of owned and leased properties. Wal-Mart Realty Department (one of the largest worldwide) plays a key role in identifying, managing, and maintaining properties that are strategic to the growth objectives of the firm, while maintaining economic efficiency. The interlink between core-retail business and property-selection process is summarized in the following statement from the firm's Web site: "As *Fortune's* #1 retailer, we're responsible for building thoughtfully, leaving no waste for landfills, as well as economically, so we can pass on the savings of smart, simple construction to the consumer."

As of 2011, the firm owned 3,883 and leased 596 properties across the United States. The language in the annual report is unequivocal on how the lease

[15]Our results could be biased if the employees-to-assets ratio varies across industries in a way that is correlated with leverage. To assess this concern, we check whether the employees-to-assets ratio is correctly excluded from the second-stage leverage regressions. Our examination suggests that the exclusion restrictions are met in the data.

arrangements are structured. These contracts are linked to conditions in the rental market via a "rental escalation clause," which states that the firm might opt for owning real properties in those markets that have experienced an increase in the rental rate. To understand this asset management policy in more detail, we analyze the firm behavior with respect to its real-property portfolio in a period characterized by a significant change in the market for rental properties. We focus on 1992 (one of our sample years). In that year, rental volatility increased, on average, by 70% in the locations where the firm operated: from 10% in 1991 to 17% in 1992. Following this increase, balance sheet data show that the firm's holdings of land and buildings increased by almost 37% (from 19% of total assets in 1991 to 26% in 1992). Additional evidence based on rental expenses from the firm income statement is also consistent with a decreased reliance on leased properties. Rental costs decrease by 47%: from 17% of sales in 1991 to 8% in 1992.

3. The Restricted Tangibility Model

Second-stage coefficients for the restricted model (which includes only *OverallTangibility*) are reported in Table 3. We first discuss the statistical properties of these estimates (economic magnitudes are discussed shortly).

TABLE 3

Second-Stage Regression Estimates: Restricted Model

Table 3 reports second-stage regression results for fixed effects instrumental variables estimations of the restricted model (equation (1) in the text). Estimations also include year dummy variables. The figures in square brackets under the standard errors represent the percentage changes [%] in leverage relative to its sample mean as each continuous regressor increases from the 25th to the 75th percentile, while all other regressors are kept at their sample mean. The exception is *RatingDummy*, for which we report the raw regression coefficient. For example, as *OverallTangibility* increases from its 25th to its 75th percentile, market leverage increases by 0.066, which is a 32.42% increase relative to the sample mean leverage of 0.202. All firm-level data are from Compustat industrial tapes. Refer to Table 1 for detailed variable definitions. The sample includes all firms except financial, lease, REIT and real estate-related, nonprofit, and governmental firms. Standard errors reported in parentheses are based on heteroskedastic-consistent errors adjusted for clustering across observations of a given firm (Petersen (2009)). ***, **, and * indicate statistical significance at the 1%, 5%, and 10% (2-tail) test levels, respectively.

Independent Variables	Market Leverage	Book Leverage
OverallTangibility	0.321***	0.260***
	(0.082)	(0.101)
	[32.42%]	[20.76%]
Size	0.004	0.015
	(0.006)	(0.014)
	[5.11%]	[15.39%]
Profitability	−0.108***	−0.110***
	(0.020)	(0.039)
	[−6.23%]	[−5.03%]
Q	−0.046***	−0.014**
	(0.004)	(0.007)
	[−17.43%]	[−4.15%]
EarningsVolatility	−0.026	−0.215
	(0.065)	(0.284)
	[−0.87%]	[−5.61%]
MarginalTaxRate	−0.156***	−0.209***
	(0.026)	(0.036)
	[−4.24%]	[−4.50%]
RatingDummy	0.045***	0.071***
	(0.009)	(0.013)
	[0.05]	[0.07]
No. of obs.	8,887	8,887
Adj. R^2	0.205	0.088

We start by noting that *OverallTangibility* enters both the *MarketLeverage* and *BookLeverage* regressions with a positive, highly statistically significant coefficient. Turning to the control variables, they also attract the expected signs. *Size* enters the leverage regressions with a positive sign, although statistically weak. *Profitability* has a strong negative effect on leverage, a result that is commonly associated with Myers's (1984) pecking order story. The coefficient on *Q* obtains the expected negative sign, a finding often seen as consistent with the argument that firms with significant growth opportunities use less debt to avoid underinvestment (see also Myers (1977), Hart (1993)). Cash flow volatility may increase the cost of financial distress. Accordingly, *EarningsVolatility* enters the leverage regressions with the expected negative sign, although statistically insignificant. Firms with a high marginal tax rate should increase leverage to shield their tax burden. Contrary to this prediction, *MarginalTaxRate* enters the leverage regressions with a negative coefficient, a finding that is similarly reported by Faulkender and Petersen (2006). Consistent with Faulkender and Petersen's argument that firms with access to the public debt market are less opaque and can borrow more, we find that our bond market access indicator (*RatingDummy*) enters all regressions with a positively significant coefficient.

The economic effects of the regressors of the leverage model are reported in square brackets in Table 3. These effects are displayed in terms of percentage change in leverage relative to its sample mean as each regressor increases from the 25th to the 75th percentile (1-IQR change), while all other variables are kept at their sample mean. The existing literature pays little attention to the relative economic importance of the various forces driving observed capital structure, focusing instead on their statistical significance. This makes our exercise particularly interesting. At the same time, we are cautious about the interpretation of these results since the estimates are derived from reduced-form-type equations.

Despite concerns about the precision of estimates from standard capital structure regressions, the results in Table 3 highlight the importance of asset redeployability as a driver of leverage. The results in the table suggest that *OverallTangibility* is a main determinant of *MarketLeverage*. For example, a 1-IQR change in *OverallTangibility* leads *MarketLeverage* to increase by 0.066, which is a 32.4% increase relative to the sample mean leverage of 0.202. In this regression, the coefficient for *Q* also implies a sizable effect, but it is only half of that of tangibility under the experimental design we consider.[16] Other important variables such as *Size* and *Profitability* have more limited economic impact on *MarketLeverage*. *OverallTangibility* is also a first-order driver of *BookLeverage*. Since our estimates are similar to those of many other papers in the capital structure literature, our findings highlight the relatively low degree of attention researchers have paid to the economic role of asset tangibility as a driver of leverage.

Because *OverallTangibility* (or PP&E over assets) is a coarse collection of different types of assets, it is important that we do a better job of identifying the

[16]We also considered experiments where we perturb the variable of interest with shifts measured in terms of standard deviations. Because some variables are highly skewed (e.g., *Q*), this purely parametric approach could lead us to conclude that those variables have disproportionately larger economic effects. As it turns out, however, our conclusions also hold when we consider standard deviation shifts in our experimental design.

connections between tangible asset structure and capital structure. This is the goal of the next set of tests.

4. The Unrestricted Tangibility Model

Our approach allows for the fact that corporate assets differ in their degree of redeployability. This dimension has not been examined in the existing empirical literature. We are able to do so by decomposing the standard measure of asset tangibility (*OverallTangibility*) into various components: *Land&Building*, *Machinery&Equipment*, and *OtherTangibles*. With this decomposition, we can reestimate the models of Table 3 and then assess the significance of individual components of a firm's tangible assets.

The results from our asset decomposition analysis are in Table 4, where we report estimates of economic significance. One can readily recover the original regression coefficients from the estimations in Table 4 with the use of Table 1. For example, the original slope coefficient for *Land&Building* is 0.207 in the OLS model, which can be backed out by multiplying 13.0% from Table 4 by the average leverage of 0.202, divided by the IQR of 0.127 from Table 1. The tabulated regression coefficients are also available from the authors.

TABLE 4

Economic Significance (Unrestricted Model): Interquartile Change Effects

Table 4 reports regression results for ordinary least squares (OLS), firm effects least squares (FE), and fixed effects instrumental variables (IV) estimations of the unrestricted model (equation (2) in the text). Estimations also include year dummy variables. Results are displayed in terms of percentage changes in leverage relative to the sample mean as each continuous regressor increases from the 25th to the 75th percentile, while all other regressors are kept at their mean. The exception is the *RatingDummy*, for which we report the raw regression coefficient multiplied by 100. All firm-level data are from Compustat industrial tapes. Refer to Table 1 for detailed variable definitions. The sample includes all firms except financial, lease, REIT and real estate-related, nonprofit, and governmental firms. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% (2-tail) test levels, respectively.

	% Change in Response to IQR Shift					
	Market Leverage			Book Leverage		
	OLS	FE	IV	OLS	FE	IV
Independent Variables	1	2	3	4	5	6
Land&Building	13.00***	19.24***	27.65***	13.05***	17.26***	19.85**
Machinery&Equipment	12.07***	9.10***	9.43	11.99***	5.51*	1.68
OtherTangibles	0.55	1.04**	2.68**	0.56	0.79**	0.95
Size	−11.53***	6.04***	4.05	−6.13**	15.29	15.64
Profitability	−6.54***	−7.04***	−6.61***	−8.03***	−5.91***	−5.44***
Q	−22.94***	−18.36***	−16.97***	−3.53*	−3.80*	−3.80*
EarningsVolatility	−5.30**	−0.87	−0.91	−2.72*	−5.38	−5.58
MarginalTaxRate	−6.58***	−5.14***	−4.33***	−7.68***	−5.21***	−4.57***
RatingDummy	6.35***	4.01***	4.21***	9.49***	6.65***	6.75***
No. of obs.	9,748	9,748	8,887	9,748	9,748	8,887
Adj. R^2	0.231	0.213	0.203	0.102	0.089	0.086

To characterize the role played by redeployability, we present estimates that are obtained from ordinary least squares (OLS), least squares with fixed effects (FE), and instrumental variables with fixed effects (IV). Focusing on the IV specification, *Land&Building* appears as the most important determinant of leverage (either book- or market-based measures). At the same time, *Machinery&Equipment* is far less relevant. In the *MarketLeverage* model (under column 3), a 1-IQR shift in *Land&Building* is associated with an increase of 27.7% in the

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firm's leverage. This is almost twice as high as the effect of *Q* (which is 17.0%) and multiple times larger than any other traditional determinant of leverage. These contrasts are even sharper in the *BookLeverage* specification. In that model (column 6), a 1-IQR change in *Land&Building* causes leverage to increase by 19.9%. This is about six times the effect of traditional drivers of capital structure, such as *Profitability* and *Q*. The only regressor in the *BookLeverage* model that has comparable economic magnitude is *Size*, which is not statistically significant.[17]

We note that while our redeployability-leverage argument is mainly cross-sectional in nature, the fixed-effects estimator tends to capture the time-series effects of the relation of interest. Our main motivation in the choice of such an estimator is to ensure that our results are robust to unobserved, time-invariant heterogeneity. Given the focus of our analysis, it is important to isolate the relative effect of the cross-sectional variation in *Land&Building* on leverage. With this in mind, we reestimate our FE-IV models in Table 4 by way of IV without firm-fixed effects. This estimation shows that the coefficient on *Land&Building* is large in nominal terms in the IV specification without firm-fixed effects and compares with the estimate from the IV specification with fixed effects that we report. For instance, in the market leverage regression, the coefficient on *Land&Building* is equal to 0.44 in the FE-IV estimation and 0.39 in the IV specification without firm-fixed effects. This implies that a 1% increase in *Land&Building* generates an increase in leverage of about 40% that increases with both the FE-IV estimator and the simple IV estimator without firm-fixed effects, suggesting that *Land&Building* is an important determinant of leverage both within-firm over time as well as across firms.

Summing up our results, for either definition of leverage (market or book leverage) and under alternative estimation approaches (OLS, FE, or IV), we find evidence pointing to land and buildings (presumably, the least firm-specific, most redeployable assets) as a first-order driver of leverage. Estimates for the other components of tangibility imply smaller, weaker effects. Importantly, as highlighted in the comparisons between the IV model and the other least-square-based approaches, it is the component of land and buildings that responds to redeployability in secondary markets that explains the observed dispersion in corporate leverage. Differently put, our results show that tangible assets enable firms to sustain higher borrowing capacity, but *only to the extent* that those assets are redeployable. This evidence is new to the literature and is consistent with theories suggesting that contracting frictions such as limited enforceability condition firms' borrowing on their ability to offer collateral with high liquidation value.

C. Evidence from a Natural Experiment: Military Base Closings in the Post-Cold War Era

Our proposed redeployability–leverage channel can be shown to operate in the data via an alternative identification strategy. In this section, we isolate changes

[17]The results we report are robust to the inclusion of operating leases in our models. In particular, our conclusions remain unchanged if we capitalize operating leases as in Rampini and Viswanathan (2013) and add this value to leverage and tangibility in our regressions.

in capital structure that are caused by exogenous shifts in the supply of real estate assets. Our tests build on the intuition used above regarding the sale of real estate by the federal government. However, we now focus on a surrogate natural experiment: the sale of military bases and supporting real estate facilities by the U.S. government following the end of the Cold War.

1. Institutional Setting

Following the fall of the Iron Curtain, the U.S. military initiated the largest disarmament program in its history.[18] One of the steps of the program was the disposition of military installations across the country. This event is unique in that it created a shock to the supply of corporate-type assets that was not caused by changes in local economic fundamentals, but rather by a massive sale of assets that became superfluous with the end of the Cold War. It is estimated that these dispositions generated an influx of more than 100,000 acres of land and thousands of buildings suitable for redevelopment into office parks and industrial zones (Murphy (2003)). In the greater Chicago area, for example, the closure of Glenview Naval Air Station released 1,000 acres of real estate for development. The closure of Joliet Army Arsenal freed up an additional 2,000 acres of land and facilities, which were later scheduled to house the largest industrial park in metro Chicago. We take a careful look at this innovation to commercial real estate markets to study how firms adjusted their holdings of land and buildings and whether these adjustments had an effect on their debt capacity.

Before proceeding, we stress that simply exploring an event of this nature may not be sufficient to establish a causal link between a firm's asset structure and its leverage. Our identification strategy could be compromised if the government's selection of disposable bases was driven by the value of the properties they occupy. This could be problematic for our purposes because that value is generally correlated with the conditions of the local economies and, arguably, these conditions could also affect the growth opportunities and leverage decisions of local firms. To isolate the link between tangible assets and leverage, we need the government's selection of bases suitable for closure to be independent of the economic fundamentals of their location. To achieve identification, we exploit the procedures for base closure and liquidation that were established by the Defense Base Closure and Realignment Act (H.R. 101-665). As we discuss below, our test strategy works because decisions about base closures under the act were governed by a strict "national security first" doctrine, rather than by local economic circumstances. To ensure the robustness of our findings, however, we use a test approach that accounts for potential deviations from that principle.

2. Data and Experimental Design

Data on U.S. dealings in real estate assets are compiled by the General Services Administration (GSA), under the Department of Commerce. To identify transactions involving military installations, we intersect the GSA database

[18]The U.S. Department of State reports that military spending was cut by over 20% from 1986 to 1994 (see also U.S. Arms Control and Disarmament Agency (1995)).

with data from the Base Closure Division of the Pentagon. Consistent with the security-first doctrine, the data show that the DBCRA led to a significant number of base closures in areas that are not considered "strategic regions of the country," as defined by the Office of Economic Adjustment, under the Department of Defense.[19] Closure activity was particularly pronounced in the Midwest, and our tests focus on the neighboring states of Illinois, Indiana, Iowa, Kentucky, Missouri, and Wisconsin, which we call the "experimental region." As we discuss below, this choice is made to mitigate the influence of confounding factors in our analysis, as these states' economies are fairly comparable.

Illinois was the state with the largest number of base dispositions in the experimental region following the DBCRA (21 large dispositions). By comparison, there were no disposition activities in Kentucky and Wisconsin, and only small activities in Indiana, Iowa, and Missouri. The data show that the intensity of disposition activities was associated with the larger presence of military installations in Illinois relative to the other states in the region. This helps with our identification strategy, as it suggests that the government's decision to focus on Illinois was primarily driven by considerations about the large military footprint in that state in 1990. Indeed, the DBCRA states in its "procedures for closure recommendations" (Section 2903) that the Secretary of Defense *must not* consider the economic circumstances of communities affected by base closures when deliberating on closure decisions.[20]

Our basic prediction is that firms headquartered in Illinois ("treated firms") should reduce their holdings of land and buildings following the DBCRA relative to similar firms in the other experimental states (Indiana, Iowa, Kentucky, Missouri, and Wisconsin). To wit, theoretical work suggests that firms have long-term strategic plans and these plans include the future utilization of real estate assets (see, e.g., Gavazza (2011)). When firms operate in areas where the supply of commercial real assets is large, they have less need to hoard those assets in their own balance sheets, since the assets can be more promptly purchased from the open market when needed. In our test setting, we hypothesize that when more real estate assets come to the market due to the DBCRA, firms in the affected areas will have a lower need to hoard those assets in their balance sheets and will respond by disposing of some of those assets.[21] Our proposed redeployability–leverage channel would imply that this exogenous downward adjustment in real estate holdings should lead to a decline in leverage for firms in Illinois.

Naturally, differences between treated and untreated firms in our cross-state comparisons could be confounded by other sources of heterogeneity. We address this issue using a difference-in-differences matching estimator approach. To be precise, we pair up treated firms with a subsample of "matched controls" that

[19]Strategic regions are the coastal states, those bordering Mexico, and some areas on the border with Canada.

[20]For each base closure decision, one can find recommendations of the Secretary of Defense on a Web page supported by the Base Closure Division of the Pentagon (www.hqda.army.mil/acsimweb/brac/).

[21]We note that this increase in supply might make it relatively more difficult for firms to sell real assets. This countervailing effect could reduce the extent of disposition activities in the data, with the result of dampening our estimates. We thank the referee for pointing out this issue.

are extracted from the population of nontreated firms (firms in other Midwestern states). Firms in the counterfactual set are identified as the closest matches in terms of size, profitability, Tobin's *Q*, earnings volatility, marginal tax rate, and credit rating. Treated–control matches are also confined to the same 2-digit SIC categories (we exclude any observations from the defense industry). The purpose of the approach is to ensure that treated firms are comparable to controls on multiple observable dimensions, with the only difference being the extent to which the local real estate markets were affected by the disposition of military bases. Our final sample consists of 97 firms for which data necessary to build our matching variables are available in Compustat.

The DBCRA became law (P.L. 101-510) in fiscal-year 1991. We measure outcome variables in changes (from 1991 to 1992) because variables in levels of the treated and control firms could be different prior to the treatment (the DBCRA), and those differences could carry over after the treatment, biasing our inferences. We perform tests of the average effect of the treatment on the treated (ATT) using the Abadie and Imbens (2006) estimator, as implemented by Abadie, Drukker, Herr, and Imbens (2004). The Abadie-Imbens estimator produces "exact" matches on categorical variables such as credit ratings and industry. Naturally, the matches on continuous variables will not be exact. The procedure recognizes this difficulty and applies a bias correction to the estimates of interest. The estimator conveniently produces heteroskedastic-robust standard errors.

3. Results

Difference-in-differences matching estimation results for our experiment are reported in Table 5. Panel A reports that firms headquartered in Illinois reduced corporate holdings of *Land&Building* by 0.006 (from 0.139 to 0.133) between 1991 and 1992. In economic terms, this represents a decline of 4.06% in their holdings of real estate assets. By comparison, their control matches increased corporate holdings of *Land&Building* by 1.61% over the same time period. The simple difference-in-differences estimation is equal to 5.67% and is statistically significant at the 1% level. The estimate increases to 8.82% when we use the Abadie-Imbens (2006) bias-corrected estimator; it is also highly statistically significant.

The results in Panel A suggest that the DBCRA had a significant impact on corporate holdings of real estate assets. It is important to note that the changes reported are not explained by a drop in the value of real estate held by firms, but rather by actual sales of properties in their portfolios. Indeed, accounting figures for *Land&Building* in Compustat are recorded at historical book values following U.S. generally accepted accounting principles (GAAP). These figures do not reflect changes in market valuation. We further check the accuracy of our inferences by gathering information on the actual sales of real estate properties by companies in Illinois. Data from companies' 10-Ks confirm that their real estate holdings decline between 1991 and 1992 by way of active real estate sales activities.

Having documented the effect of the DBCRA on corporate holdings of land and buildings, we next analyze the implications of the experiment for corporate leverage. Difference-in-differences estimation results for market and book leverage measures are reported in Panels B and C of Table 5. Panel B shows that treated

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TABLE 5

Change in Corporate Holdings of *Land&Building* and *Leverage* for Treated and Control Firms Following the DBCRA

Table 5 reports the average change in corporate holdings of *Land&Building* and *Leverage* (market and book) from 1991 to 1992. The figures in square brackets represent the percentage changes [%] in land and buildings and leverage from 1991 to 1992. For example, treated firms' *Land&Building* declines by 0.006 from 0.139 in 1991 to 0.133 in 1992, which is a 4.06% decline relative to the average value in 1991. Treated firms are those headquartered in Illinois. Untreated firms are those headquartered in the neighboring states of Indiana, Iowa, Kentucky, Missouri, and Wisconsin. Control firms are a subsample of the untreated firms selected as the closest match based on size, profitability, Tobin's Q, earnings volatility, marginal tax rate, credit rating, and industry. There are 39 treated firms and 58 untreated firms. All firm-level data are from Compustat industrial tapes. The government disposition data are obtained from the General Services Administration, under the U.S. Department of Commerce. These data are released under the Freedom of Information Act. ATT is the Abadie-Imbens (2006) bias-corrected average treatment effect matching estimator for the treated firms. Standard errors reported in parentheses are based on heteroskedastic-consistent estimations. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% (2-tail) test levels, respectively.

	Treated versus Control Firms			
	Treated Firms	Control Firms	Treated − Control Firms	Matching Estimator (ATT)
Panel A. Land&Building				
Average change	−0.006 [−4.06%]	0.002 [1.61%]	−0.008 [−5.67%] (2.07)***	−0.012 [−8.82%] (2.96)***
Panel B. MarketLeverage				
Average change	−0.036 [−16.55%]	−0.002 [−1.04%]	−0.034 [−15.51%] (5.15)***	−0.039 [−17.83%] (9.03)**
Panel C. BookLeverage				
Average change	−0.031 [−12.38%]	<−0.001 [−0.02%]	−0.031 [−12.36%] (3.81)**	−0.037 [−15.15%] (6.86)**

firms reduced *MarketLeverage* by 0.036 (from 0.216 to 0.180) between 1991 and 1992. This corresponds to a proportional drop of 16.55% in treated firms' leverage. By comparison, control firms decrease their leverage by only 1.04% over the same time period. The simple difference-in-differences estimate for *MarketLeverage* is 15.51%. The difference-in-differences estimate increases to 17.83% after employing the Abadie-Imbens (2006) method. Panel C shows a very similar pattern for *BookLeverage*.

We note that while *Land&Building* falls from 0.139 to 0.133 (a change equivalent to 0.6% of assets) for treated firms as a result of the act-induced supply shock, the decline in leverage was sizably larger, ranging from 3.6% (from 0.216 to 0.180) for *MarketLeverage* to 3.1% for *BookLeverage* (Table 5, column 1, Panels A–C). To understand the larger economic effect on leverage, it is helpful to recognize that the increase in the supply of commercial real estate in the experimental region works through two different channels. First, it reduces a firm's need to hoard real assets, causing the reduction in corporate holdings of land and buildings that we observe in the data. Second, it might affect the market value of commercial properties. According to U.S. GAAP, firms are required to report their corporate holdings of *Land&Building* only at book value (firms do not report property-acquisition prices in their annual reports: "Item 2 – Properties"). As a result, we can observe dispositions of corporate land and buildings, but it is not possible to obtain an accurate measure of the change in the value of these same

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properties. Although we cannot measure market value changes at the firm level, we note that corporate-type real estate assets lost about 3% of their value in the Midwest (our experimental region) in the second half of 1991 (source: National Council of Real Estate Investment Fiduciaries (NCREIF)). Firms responded to this overall change in their collateral capacity by reducing net debt by 2.0% and increasing net equity by 1.5%, where these leverage adjustments could have been dictated, for instance, by covenants in place. Overall, these combined figures imply a leverage adjustment of roughly −3.5%, which compares to the −3.6% to −3.1% leverage adjustment (book and market) documented in Table 5.

The results in Table 5 indicate that local supply shocks to real state assets lead firms to hold less of those assets, which in turn leads to a reduction in their leverage. These results are new and reveal a strong connection between firm collateral and financial decisions by tracing out the redeployability–leverage channel in a precisely identified setting.

4. Robustness Tests

Note that our DBCRA-based estimations could still be biased if our matching variables do not adequately capture heterogeneity in local business fundamentals and firm characteristics. In particular, one could be concerned that the closure of military installations might cause a recession in local economies, and this effect could make local firms demand less debt. We address this concern with a battery of robustness checks.

In a first test, we analyze the implications of base closures for corporate holdings of machinery and equipment. If the disposition of military installations causes treated firms to cut holdings of real estate by way of a local recession channel, we should observe a similar effect on treated firms' other production inputs as well, including machinery and equipment. At a more basic level, the local economy recession story should also lead to a decline in treated firms' sales.

Difference-in-differences test results comparing the average change in machinery and equipment holdings as well as sales for treated and control firms are reported in Table 6. Panel A shows that *Machinery&Equipment* declined for treated firms by 0.99% between 1991 and 1992 (from 0.185 to 0.183). By comparison, control firms experienced a decline of 0.93%. The Abadie-Imbens (2006) estimator points to a difference of −0.30% across the two groups of firms. These estimates are both economically and statistically insignificant. The evidence in Panel B leads to very similar conclusions. Sales increased by 1.52% for treated firms between 1991 and 1992. By comparison, sales increased by 2.34% for control firms. The difference-in-differences estimation based on the Abadie-Imbens method is statistically insignificant. Results in Table 6 cast doubt on the argument that a local recession channel associated with the disposition of military installations underlies our leverage results.

In a second test, we control directly for the economic conditions of the local economies by excluding from our sample those firms that sell goods and services primarily in the markets where their headquarters are located. In other words, we focus on firms for which cash flows are largely disassociated from local economic conditions, and thus largely unaffected by the disposition of military installations in their locations. The data necessary to perform this test are obtained from

TABLE 6

Change in Corporate Holdings of *Machinery&Equipment* and *Sales* for Treated and Control Firms Following the DBCRA

Table 6 reports the average change in corporate holdings of *Machinery&Equipment* and *Sales* (market and book) from 1991 to 1992. The figures in square brackets represent the percentage changes [%] in machinery and equipments and sales from 1991 to 1992. Treated firms are those headquartered in Illinois. Untreated firms are those headquartered in the neighboring states of Indiana, Iowa, Kentucky, Missouri, and Wisconsin. Control firms are a subsample of the untreated firms selected as the closest match based on size, profitability, Tobin's Q, earnings volatility, marginal tax rate, credit rating, and industry. There are 39 treated firms and 58 untreated firms. All firm-level data are from Compustat industrial tapes. The government disposition data are obtained from the General Services Administration, under the U.S. Department of Commerce. These data are released under the Freedom of Information Act. ATT is the Abadie-Imbens (2006) bias-corrected average treatment effect matching estimator for the treated firms. Standard errors reported in parentheses are based on heteroskedastic-consistent estimations.

	Treated versus Control Firms			
	Treated Firms	Control Firms	Treated − Control Firms	Matching Estimator (ATT)
Panel A. Machinery&Equipment				
Average change	−0.002 [−0.99%]	−0.002 [−0.93%]	<−0.001 [−0.05%] (1.92)	−0.001 [−0.30%] (3.09)
Panel B. Sales				
Average change	<0.001 [1.52%]	<0.001 [2.34%]	<−0.001 [−0.82%] (1.53)	<−0.001 [−2.10%] (2.50)

the Directory of Corporate Affiliations (DCA) database. This database contains information on firms' sales distribution across different regions. Using this information, we exclude treated firms that sell more than 25% of their products in their headquarters' state.

Table 7 reports difference-in-differences estimation results for corporate holdings of land and buildings as well as leverage using this subset of firms. Panel A shows that the Abadie-Imbens (2006) estimator for *Land&Building* is economically large, equal to −9.71%, and statistically significant at the 1% level. Results for *MarketLeverage* are reported in Panel B. *MarketLeverage* declines by 22.53% (from 0.216 to 0.167) for treated firms relative to the control group following the DBCRA. Panel C documents a very similar pattern for *BookLeverage*. The results in Table 7 mimic those of the base analysis of Table 5, suggesting that local economic conditions do not explain our characterization of the redeployability–leverage channel.

5. Further Falsification Tests

Finally, we perform a set of falsification tests in checking the internal logic of our findings.[22] First, we simulate the implementation of the DBCRA test *as if* the act took place in 1989. Once again assigning firms in Illinois to the treatment group, we find no changes in corporate holdings of real estate assets and leverage between 1989 and 1990. We repeat the same placebo tests for 1988 and find similar patterns. These results suggest that our base test results are not simply

[22]The tabulations are omitted in the interest of brevity but are readily available from the authors.

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TABLE 7

Change in Corporate Holdings of *Land&Building* and *Leverage* for Treated and Control Firms Following the DBCRA: National-Sale Firms

Table 7 reports the average change in corporate holdings of *Land&Building* and *Leverage* (market and book) from 1991 to 1992. The figures in square brackets represent the percentage changes [%] in land and buildings and leverage from 1991 to 1992. Treated firms are those headquartered in Illinois, but with less than 25% of their sales in the headquarters' state. Untreated firms are those headquartered in the neighboring states of Indiana, Iowa, Kentucky, Missouri, and Wisconsin. Control firms are a subsample of the untreated firms selected as the closest match based on size, profitability, Tobin's Q, earnings volatility, marginal tax rate, credit rating, and industry. There are 32 treated firms and 58 untreated firms. All firm-level data are from Compustat industrial tapes. The government disposition data are obtained from the General Services Administration, under the U.S. Department of Commerce. These data are released under the Freedom of Information Act. ATT is the Abadie-Imbens (2006) bias-corrected average treatment effect matching estimator for the treated firms. Standard errors reported in parentheses are based on heteroskedastic-consistent estimations. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% (2-tail) test levels, respectively.

	Treated versus Control Firms			
	Treated Firms	Control Firms	Treated − Control Firms	Matching Estimator (ATT)
Panel A. Land&Building				
Average change	−0.007 [−4.88%]	0.002 [1.47%]	−0.009 [−6.35%] (2.26)***	−0.013 [−9.71%] (3.02)***
Panel B. MarketLeverage				
Average change	−0.040 [−18.32%]	−0.002 [−1.07%]	−0.038 [−17.25%] (5.27)***	−0.049 [−22.53%] (8.39)***
Panel C. BookLeverage				
Average change	−0.037 [−15.09%]	< 0.001 [0.07%]	−0.037 [−15.16%] (4.02)***	−0.050 [−20.36%] (6.58)***

capturing differential trends in leverage across firms in Illinois vis-à-vis those in other states in the region. Second, we look for a state with a comparable amount of base closings also in the Midwest (a "false counterfactual"). Analysis of the GSA database reveals that Ohio observed 10 dispositions activities of military installations following the DBCRA. Using Ohio as a control match, we should find less pronounced effects associated with our Illinois-based treatment assignment. This is exactly what we find in the data. These additional tests make it harder to argue that confounding effects (and not the experimental treatment we designed) can explain the results in Table 5.

IV. Credit Frictions and Macroeconomic Movements

The evidence thus far is consistent with the argument that the redeployability of tangible assets affects leverage ratios because it relaxes financing frictions (provides liquid collateral to creditors). Taking this argument to its next logical steps, in this section we first contrast firms that are more likely to face financing frictions (for which collateral should be particularly important in raising debt finance) with firms that are less likely to face those problems. In a second set of experiments, we examine whether asset redeployability becomes a stronger driver of leverage in times when financing frictions are likely to be heightened, such as periods of aggregate credit contractions.

A. Cross-Sectional Variation in Credit Constraints and Leverage

We investigate whether asset tangibility is a particularly important driver of leverage for firms that are more likely to face financing constraints. The first step in this analysis is to sort firms into "financially constrained" and "financially unconstrained" categories. The literature offers a number of plausible approaches to this sorting and we consider three alternative schemes:

- *Scheme 1.* We rank firms based on their asset size over the sample period and assign to the financially constrained (unconstrained) group those firms in the bottom (top) 3 deciles of the size distribution. The rankings are performed on an annual basis. This approach resembles that of Gilchrist and Himmelberg (1995), who also distinguish between groups of financially constrained and unconstrained firms on the basis of size. Fama and French (2002) and Frank and Goyal (2003) also associate firm size with the degree of external financing frictions. The argument for size as a good observable measure of financing constraints is that small firms are typically young, less well known, and thus more vulnerable to credit imperfections.

- *Scheme 2.* We retrieve data on firms' bond ratings and classify those firms without a rating for their public debt as financially constrained. Given that unconstrained firms may choose not to use debt financing and hence not obtain a debt rating, we only assign to the constrained subsample those firm-years that both lack a rating and report positive long-term debt (see Faulkender and Petersen (2006)).[23] Financially unconstrained firms are those whose bonds have been rated. Related approaches for characterizing financing constraints are used by Gilchrist and Himmelberg (1995) and Almeida, Campello, and Weisbach (2004).

- *Scheme 3.* In every year over the sample period, we rank firms based on their payout ratio and assign to the financially constrained (unconstrained) group those firms in the bottom (top) 3 deciles of the annual payout distribution. We compute the payout ratio as the ratio of total distributions (dividends and repurchases) to operating income. The intuition that financially constrained firms have significantly lower payout ratios follows early work by Fazzari, Hubbard, and Petersen (1988). In the capital structure literature, Fama and French (2002) use payout ratios as a measure of difficulties firms face in assessing the financial markets.

Table 8 reports second-stage IV estimation results for our three credit friction partition schemes. For ease of exposition and comparability, we report estimates for *OverallTangibility*, *Land&Building*, *Machinery&Equipment*, and *OtherTangibles* in terms of their marginal economic effects.

For the three subsamples of constrained firms (small, unrated, and low-dividend-payout firms), *Land&Building* appears as the main driver of capital structure. Panel A, for example, shows that a 1-IQR change in *Land&Building*

[23] Firms with no bond rating and no debt are excluded, but our results are not affected if we treat these firms as either constrained or unconstrained.

TABLE 8

Low/High Credit Market Frictions: Interquartile Change Effects

Table 8 reports second-stage regression results for fixed effects instrumental variables estimations of the unrestricted model (equation (2) in the text). Estimations also include control variables and year dummy variables (omitted). Results are displayed in terms of percentage changes in leverage relative to the sample mean as each continuous regressor increases from the 25th to the 75th percentile, while all other regressors are kept at their mean. The exception is the *RatingDummy*, for which we report the raw regression coefficient. All firm-level data are from Compustat industrial tapes. Refer to Table 1 for detailed variable definitions. The sample includes all firms except financial, lease, REIT and real estate-related, nonprofit, and governmental firms. Small (Large) Firms are firms in the bottom (top) 3 deciles of the annual sample size distribution. Unrated (Rated) Firms are firms without (with) a debt rating and positive leverage. Low (High) DivPayout firms are firms in the bottom (top) 3 deciles of the annual sample payout distribution. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% (2-tail) test levels, respectively.

	% Change in Response to IQR Shift						
		Size		Ratings		DivPayout	
	Full Sample	Small Firms	Large Firms	Unrated Firms	Rated Firms	Low DivPayout	High DivPayout
	1	2	3	4	5	6	7
Panel A. Market Leverage							
Land&Building	27.65***	38.66***	0.83	39.32***	−6.26	31.55***	−27.92
Machinery&Equipment	9.43	12.04	−4.45	12.49*	1.57	12.98	2.86
OtherTangibles	2.68**	3.69	−3.21	2.50**	−4.07	1.35***	−10.57*
Panel B. Book Leverage							
Land&Building	19.85**	27.87**	−21.43	28.54***	−13.20	24.52*	−35.11
Machinery&Equipment	1.68	3.10	−8.21	1.92	0.70	0.37	−5.55
OtherTangibles	0.95	−1.40	−1.18	0.89	2.00	0.61	−9.67

is associated with a 39% increase in *MarketLeverage* for the small-firm partition. This is equivalent to a shift in market leverage from its mean of about 22% to nearly 31%. Other categories of tangible assets (*Machinery&Equipment* and *OtherTangibles*), in contrast, allow for less debt financing (their economic effect is smaller and statistically insignificant). Alternative leverage determinants (untabulated estimates) also have small economic effects when compared to *Land&Building*. For example, within the same small-firm partition, a 1-IQR change in *Q* is associated with a 12% change in *MarketLeverage*. A similar experiment using *Size* yields a change of 11% in *MarketLeverage*. Notably, the economic effects of both *Q* and *Size* are less than one-third of the effect of *Land&Building*. We reach very similar conclusions when we examine the other constrained firm partitions, as shown in the results for unrated and low-dividend-payout firms (columns 4 and 6, respectively).

In contrast to the previously mentioned findings, asset tangibility does not affect leverage across unconstrained firms (large, rated, and high-payout firms). The tangibility proxies enter the market leverage regressions with generally negative, statistically insignificant coefficients. These contrasting results imply that *only constrained firms* have their capital structures explained by credit supply-side considerations (creditworthiness based on redeployable collateral).

Panel B of Table 8 reports regressions featuring *BookLeverage* as the dependent variable. In these regressions, *Land&Building* more sharply dominates other categories of asset tangibility (*Machinery&Equipment* is now always small and insignificant) as well as competing drivers of leverage (the economic effects of *Q*, *Size*, and *Profitability* are also much smaller). For the small-firm partition, for example, a 1-IQR change in *Land&Building* causes *BookLeverage* to increase by

28% from its mean, compared to an effect of only 3% for *Machinery&Equipment* and 1% for *Q*. One reaches similar conclusions by examining the unrated and low-payout firm partitions.

Our finding that financially unconstrained firms have lower coefficients for *Land&Building* could be questioned if large, rated, and high-payout firms were more likely to have corporate properties dispersed across states. In particular, if this dispersion remains unaccounted for, it could create concerns about the impact of mismeasurement in the regressions performed in Table 8. Arguably, we could introduce attenuation biases that differentially affect firms in financially unconstrained partitions.

To investigate this issue, we resort to the Securities and Exchange Commission's (SEC's) Edgar database (http://www.sec.gov/edgar/searchedgar/companysearch.html) to retrieve the annual reports of each of the firms in our unconstrained partitions. Looking at the 1996 reports, we collect from "Item 2 – Properties" detailed information on each property on a firm's balance sheet, its location (state), ownership status (owned or rented), and size (square feet).[24] Using property-level information, we construct a weighted measure of the headquarters-specific instruments that accounts for the geographic dispersion of corporate properties (owned properties only). Consider, for example, the measurement of *RentalVolatility* for a hypothetical firm. If the firm has 80% of its properties in NY (headquarters), 15% in MA, and 5% in CT, rather than using the rental volatility of NY, we use a measure of rental volatility that is 80% the rental volatility in NY, plus 15% the rental volatility in MA, plus 5% the rental volatility in CT. We then use this "dispersion-weighted" version of *RentalVolatility* as an instrument in our regressions. We follow a similar approach for *LogSuppliers* and *Government-Disposal*. Using these geo-weighted versions of our instruments, we replicate the tests performed in Table 8 (and also those in Table 9 for robustness). While we omit this table for brevity, our findings confirm the results based on headquarters-specific instruments reported earlier. We also use a version of our geo-weighted instruments that directly accounts for property size, and again we are able to replicate the patterns documented in Tables 8 and 9.[25]

It is worthwhile discussing our results in a broader context. The estimates in Table 8 suggest that *Land&Building* is the most important driver of leverage, with its effect concentrated among firms that are likely to face greater credit frictions

[24]Online filings in the Edgar database only started in 1996, the last year of our window. Notably, evidence suggests that firms generally do not significantly change their holdings of corporate properties, at least in terms of their geographic locations. Pirinsky and Wang (2006) find that of the 5,000 firms that they consider in their analysis, only 118 relocated during the sample period 1992–1997 (which partially overlaps with our sample period).

[25]All of these results are readily available. As an additional check, we replicated the estimations in Table 8 using a simple OLS-FE specification test. One advantage of this approach is that it does not depend on the geographic relevance of the instruments. Our findings are qualitatively similar under this approach. In particular, we find that the effect of *Land&Building* on leverage is economically and statistically significant for all three financially constrained partitions. The effect of *Land&Building* on leverage is, in contrast, small and insignificant for financially unconstrained partitions. We also experiment with various combinations of our instruments, trying to minimize the inclusion of those that could impart greater concerns with geographical dispersion. Doing this also renders results that are similar to those in Tables 8 and 9.

TABLE 9

Macroeconomic Effects: The Impact of Land and Buildings on Leverage during Credit Contractions

The dependent variable is the annual series of the estimated coefficients on *Land&Building* from the fixed effects instrumental variable regression with market leverage (equation (3) in the text). In Panel A, the dependent variable is regressed on the 3 lags of the *FedFunds* or *Libor* (only sum of coefficients tabulated). In Panel B, the dependent variable is regressed on the 3 lags of the *FedFunds* (only sum of coefficients tabulated) and *GDP* (omitted). In Panel C, the dependent variable is regressed on the 3 lags of the *FedFunds* (only sum of coefficients tabulated), *GDP* (omitted), and *ConsumerExpenditures* (omitted). All regressions include a constant and a trend variable (omitted). The sample includes all firms except financial, lease, REIT and real estate-related, nonprofit, and governmental firms. Newey-West (1987) consistent standard errors with 4 lags and robust to heteroskedasticity are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% (2-tail) test levels, respectively.

		Size		Ratings		DivPayout	
ΔCredit	Full Sample	Small Firms	Large Firms	Unrated Firms	Rated Firms	Low DivPayout	High DivPayout
Panel A. Univariate							
FedFunds	0.187*	0.402*	0.051	0.251**	0.056	0.195*	0.125**
	(0.080)	(0.165)	(0.027)	(0.063)	(0.109)	(0.079)	(0.031)
Libor	0.122**	0.190**	0.036	0.151***	0.079	0.135***	0.059**
	(0.028)	(0.059)	(0.019)	(0.023)	(0.039)	(0.024)	(0.015)
Panel B. Bivariate							
FedFunds	0.182*	0.392**	0.052	0.201**	0.135	0.240**	0.064
	(0.066)	(0.119)	(0.026)	(0.061)	(0.061)	(0.046)	(0.065)
Libor	0.140**	0.247**	0.046	0.173***	0.084	0.155**	0.064*
	(0.040)	(0.062)	(0.020)	(0.061)	(0.060)	(0.037)	(0.024)
Panel C. Multivariate							
FedFunds	0.161*	0.364*	0.047	0.182*	0.119	0.234**	0.075
	(0.047)	(0.123)	(0.038)	(0.047)	(0.053)	(0.046)	(0.076)
Libor	0.121*	0.265*	0.102	0.172**	−0.004	0.146*	0.156***
	(0.032)	(0.073)	(0.038)	(0.038)	(0.063)	(0.043)	(0.014)

(firms that are small, unrated, and pay low dividends). These results are interesting in their own right and also add context to tests commonly conducted in the capital structure literature. In particular, standard capital structure models tend to consider asset tangibility as a "general driver" of leverage, presumably affecting all companies in a homogeneous fashion (Rajan and Zingales (1995) is a classical example). Our evidence suggests, instead, that the channel through which asset tangibility affects leverage might be concentrated within particular categories of firms (e.g., small and unrated firms).[26]

The estimates in Table 8 also imply that the types of tangible assets that are less suitable to resolving financing frictions (e.g., machinery and equipment) are also economically and statistically less relevant in explaining leverage. These results are consistent with the notion that the effect of asset tangibility on capital structure operates through its ability to ameliorate contracting frictions between lenders and borrowers: Tangible assets allow for more credit conditional on their redeployability.

[26]We took the additional step of running standard leverage regressions (similar to equation (1)) across partitions of small and large firms, as well as rated and unrated firms. Our simple OLS-FE estimations of equation (1) show that the traditional proxy for asset tangibility (the ratio of PP&E over total assets, which is labeled *OverallTangibility* in our paper) is only significant across small and unrated firm partitions. This basic check might invite more careful conceptualization of models that are meant to be all-encompassing in describing corporate leverage using asset tangibility as an input.

B. Macroeconomic Movements and Leverage

We now focus on the role of asset tangibility in explaining capital structure when credit conditions shift as a result of macroeconomic shocks. According to Bernanke and Gertler (1995), examining firm financing patterns over the business cycle is important because during those times credit frictions become more acute (e.g., agency problems are heightened). During contractions, tangibility should more significantly affect the availability of credit for firms that are most affected by financing constraints. If, as we argue, tangible assets are first-order drivers of leverage because they ease borrowing through a collateral channel, then the redeployability–leverage relation should strengthen during credit contractions. We test this hypothesis in turn.

A number of empirical studies have used economy-wide shocks to study firms' leverage decisions (e.g., Korajczyk and Levy (2003)), liquidity management (Almeida et al. (2004)), and inventory behavior (Carpenter, Fazzari, and Hubbard (1994)). While these papers have not examined the role of tangible assets in driving capital structure over the business cycle, we build on their approach to examine this association. Here, we follow the two-step procedure used in Kashyap and Stein (2000). The Kashyap-Stein two-step approach essentially provides validation for micro-level relations (in our case, between corporate asset structure and capital structure) using plausibly exogenous macroeconomic variation.

The first step of the procedure consists of estimating the baseline regression model (equation (2)) every year for our sample period. From the sequence of cross-sectional regressions, we collect the coefficients returned for *Land&Building* (i.e., α_1) and "stack" them into the vector Ψ_t, which is then used as the left-hand-side variable in the following (second-stage) time-series regression:

$$(3) \qquad \Psi_t = \eta + \sum_{j=1}^{3} \phi_j \Delta Credit_{t-j} + \rho Trend_t + u_t.$$

The term $\Delta Credit$ represents innovations to the supply of credit, which is proxied by changes in the Fed funds rate (*FedFunds*). We replicate our estimations using the London Interbank Offered Rate (LIBOR) (*Libor*). Relative to the Fed funds rate, LIBOR allows us to assess the effect of credit tightening while controlling for possible variation in credit risk over time or across firm types. The impact of shocks to credit supply on the sensitivity of *MarketLeverage* to *Land&Building* is gauged from the sum of the coefficients ϕs on the lags of *FedFunds* (*Libor*). A time trend (*Trend*) is included to capture secular changes in capital structure. To control for changes in the demand for credit, in multivariate versions of equation (3) we include the log of gross domestic product (GDP) and the log of consumer expenditures.[27] These regressions are estimated with Newey-West (1987) heteroskedasticity-consistent standard errors.

The results from the second-step estimation are reported in Table 9. The estimates in the table suggest that the role of land and buildings as a first-order driver of leverage becomes noticeably more important during credit contractions. Using

[27] These series are obtained from the Bureau of Labor Statistics.

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the univariate model from the full sample as an example (Panel A), the positive estimate for *FedFunds* (i.e., the sum of the coefficients for the 3 lags of the Fed funds) implies that the coefficient on *Land&Building* increases by 0.187 when the Fed funds rate increases by 100 bp. This is a significant shift given that the *Land&Building* coefficient equals 0.442 in the first-stage IV. By comparison, the coefficient on *Libor* is 0.122, which is somewhat smaller but still very sizable relative to the coefficient associated with the *FedFunds*. Notably, this smaller effect suggests that the Fed funds rate might overestimate the effect of *Land&Building* in ameliorating credit tightening if the econometrician does not take into account variation in credit risk, which in our context is achieved with the use of the LIBOR rate. The estimates in Panels B and C show that our conclusions hold steady after we control for shifts in the demand for credit using GDP (Panel B) and both GDP and consumer expenditures (Panel C).

The results in Table 9 also show that the increased sensitivity of *MarketLeverage* to *Land&Building* is especially strong for firms in the high financing friction partitions. In particular, the coefficient on *FedFunds* is positive and highly statistically significant for the small, unrated, and low-payout firms. In contrast, the same macroeconomic variable attracts coefficients that are very small in magnitude and generally statistically insignificant for unconstrained firms.

The recent subprime crisis has spurred a debate on whether the Fed funds rate is an adequate indicator for the amount of credit available in the economy. Evidence suggests that the ability of the Fed to implement effective countercyclical monetary policies is significantly diminished when market rates are close to 0 (e.g., Iwata and Wu (2006)). This should not be a strong concern for our analysis, because the Fed funds rate was far from 0 during our sample period, averaging around 6.5%. Nevertheless, we reestimate equation (3) using a measure of changes in lending standards from the Loan Officer Opinion Survey that is now commonly used in monetary economics research (e.g., Lown and Morgan (2006)).[28] All the findings reported in Table 9 are robust to this alternative measure of credit contraction. To be specific, we find that the sensitivity of *Leverage* to *Land&Building* increases significantly following a tightening of the *lending standards*, but mainly for firms in the high-financing friction partitions.

The results of this section suggest that asset redeployability facilitates borrowing by firms that are likely to be credit constrained (small, unrated, and low-payout firms) during times when credit constraints bind the most (monetary tightenings). In all, they substantiate the argument that credit supply effects play a key role in the time-series and cross-sectional variation of corporate leverage ratios, especially for firms that are likely to face credit imperfections.

V. Comparisons with Recent Studies

Our analysis thus far uses standard leverage models to facilitate comparisons with the broader capital structure literature. Our arguments, however, are

[28]The Loan Officer Opinion Survey data are available on the Federal Reserve Board Web page (http://www.federalreserve.gov/boarddocs/snloansurvey/) starting from 1997. We are grateful to Donald Morgan from the Federal Reserve Bank of New York for making the earlier data available to us.

not model specific, and our results should hold under specifications used in papers that are closely related to ours. We experiment with this idea in turn. We first build on Faulkender and Petersen's (2006) credit-supply study, introducing our asset tangibility decomposition into their empirical model. Within their framework, we assess the economic effect of asset tangibility. We then consider Lemmon et al.'s (2008) leverage model. Lemmon et al. find that traditional drivers of leverage become virtually irrelevant when one accounts for firm-specific, time-invariant effects. We subject our tangibility proxies to a similar experiment, using those authors' approach.

A. Asset Tangibility and Credit Ratings

Faulkender and Petersen (2006) hypothesize that access to the public debt markets mitigates credit rationing, allowing firms to increase their borrowings. Using credit ratings as a proxy for access to those markets, the authors find a significant impact of ratings on leverage. In particular, estimates in Table 5 of their paper show that a ratings dummy increases a firm's market leverage ratio by 0.051 (see column 3). Relative to the average ratio of 0.222 that the authors report in their Table 1, this corresponds to an increase in leverage of 22.9%. The authors report that leverage increases range from 0.057 to 0.063 in IV models that tackle the endogeneity of ratings (see their Table 8). These numbers correspond to an increase in leverage in the order of 25.7%–28.4% relative to the sample average leverage.

We use our sample to replicate the tests of Faulkender and Petersen ((2006), Tab. 4). In columns 1 and 2 of Table 10, we report OLS and IV results for our restricted leverage model. In column 3, we report IV results for the unrestricted model. Notably, the results reported in Table 10 are very similar to those in Faulkender and Petersen. Focusing on the ratings dummy (their key variable), column 3 shows that access to the public debt market increases leverage by 0.045. Relative to the average of 0.203, this corresponds to a 22.3% increase in leverage relative to the sample mean, which very closely resembles the 22.9% estimate of Faulkender and Petersen's paper.

Once we replicate Faulkender and Petersen's (2006) findings, our main task is to gauge the relative importance of our measures of tangibility. Table 10 reports, in square brackets, the percentage change in leverage relative to its sample mean as each variable increases from the 10th to the 90th percentile while all the other variables are kept at their mean.[29] The only exception is the ratings dummy, which should be interpreted as the percentage change in leverage relative to its sample mean for firms with a credit rating relative to those without one.

The estimates of Table 10 imply that asset tangibility remains as a key driver of leverage even under Faulkender and Petersen's (2006) specification. One finds, for example, that as *Land&Building* increases from the 10th to the 90th percentile, leverage increases by 0.106. Relative to the sample mean of 0.203, this

[29]We use the 10th–90th percentile change for continuous variables in the tests of this section to more closely mimic the impact of a dummy variable (similar to Faulkender and Petersen's (2006) credit rating dummy variable).

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TABLE 10

Asset Tangibility and Credit Ratings

Table 10 reports results from replicating the basic regression model in Faulkender and Petersen (2006) for our sample based on OLS and fixed effects instrumental variable estimations (IV-FE) for both our restricted and unrestricted models. Estimations also include year dummy variables. The dependent variable is market leverage. We follow Faulkender and Petersen in defining variables and model specifications, but in column 3 we use our *Land&Building*, *Machinery&Equipment*, and *OtherTangibles* instead of the traditional tangibility proxy. All firm-level data are from Compustat industrial tapes. The sample includes all firms except financial, lease, REIT and real estate-related, nonprofit, and governmental firms. Standard errors reported in parentheses are based on heteroskedastic-consistent errors adjusted for clustering across observations of a given firm (Petersen (2009)). To resemble closely the impact of a dummy variable, the figures in square brackets under the standard errors represent the percentage changes in leverage relative to the sample mean as each continuous regressor increases from the 10th to the 90th percentiles, while all other regressors are kept at their sample mean. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% (2-tail) test levels, respectively.

	Restricted Model		Unrestricted Model
	OLS	IV-FE	IV-FE
Independent Variables	1	2	3
OverallTangibility	0.190*** (0.024) [40.85%]	0.271*** (0.069) [58.25%]	
Land&Building			0.423*** (0.130) [52.04%]
Machinery&Equipment			0.198** (0.097) [26.63%]
OtherTangibles			0.384** (0.184) [7.78%]
Firm_has_a_debt_rating	0.067*** (0.010) [33.24%]	0.047*** (0.009) [23.21%]	0.045*** (0.009) [22.26%]
ln(*MarketAssets*)	−0.007*** (0.002) [−18.28%]	0.002 (0.006) [4.77%]	0.000 (0.006) [1.07%]
ln(1 + *FirmAge*)	−0.007 (0.007) [−2.81%]	0.063** (0.025) [25.62%]	0.057** (0.025) [22.98%]
Market-to-Book	−0.064*** (0.003) [−56.65%]	−0.048*** (0.004) [−42.59%]	−0.046*** (0.004) [−41.23%]
R&D/Sales	−0.080*** (0.025) [−3.66%]	−0.019 (0.026) [−0.86%]	−0.017 (0.026) [−0.78%]
Advertising/Sales	−0.133* (0.076) [−2.83%]	−0.200 (0.167) [−4.24%]	−0.185 (0.169) [−3.93%]
Profits/Sales	−0.026* (0.014) [−3.02%]	−0.008 (0.010) [−0.88%]	−0.007 (0.010) [−0.80%]
MarginalTaxRate	−0.296*** (0.036) [−42.21%]	−0.218*** (0.026) [−31.12%]	−0.220*** (0.026) [−31.29%]
No. of obs.	8,719	8,719	8,719
Adj. R^2	0.236	0.201	0.195

corresponds to a 52.0% increase in leverage. This is more than twice as large as the increase associated with the rating dummy, which is 22.3%. We draw similar inferences for the more standard measure of asset tangibility, *OverallTangibility* (see column 2). This is an interesting finding, since both our main arguments and Faulkender and Petersen's central theory revolve around supply-side determinants of capital structure. The more substantive contribution of our findings is that, rather than using a broadly defined measure of access to credit (ratings), we identify a specific channel through which creditworthiness affects capital structure. Our results add to those of Faulkender and Petersen in further characterizing the supply-side determinants of observed leverage dispersion.

B. Asset Tangibility and Firm Effects in Leverage Regressions

Lemmon et al. (2008) show that most of the empirical variation in corporate leverage can be explained by unobserved, time-invariant firm effects. On this basis, the authors argue that capital structure models estimated via OLS might overestimate the marginal effects of the traditional determinants of leverage. Consistent with this argument, they report that coefficients of traditional leverage determinants drop on average by about 60% after accounting for firm-fixed effects. Their paper gives a "dim picture" (p. 1605) of existing models' ability to explain capital structure.

We replicate the tests reported in Table V of Lemmon et al. (2008) using our sample. The results are given in Table 11. Comparing OLS estimates (columns 1 and 3) with those of the firm-fixed effects IV specifications (columns 2 and 4), we find a clear pattern of decline in the size of the coefficients attracted by traditional determinants of leverage, similar to the pattern reported by Lemmon et al.[30] However, our findings point to a different pattern with respect to our tangibility proxies. For *OverallTangibility*, a comparison of results across columns 1 and 2 shows an *increase* in the magnitude of the coefficient from 0.164 to 0.260. In economic terms, this implies that a 1-standard-deviation increase in *OverallTangibility* makes leverage increase by 21.2% from its mean, compared to 13.4% in the OLS specification.[31] Remarkably, we find a much sharper increase if we compare the coefficient estimates for *Land&Building* across columns 3 and 4 (unrestricted model). In this case, the tangibility coefficient increases by a factor of almost 3 (from 0.171 in the OLS to 0.437 in the IV specification).[32]

We also compare the economic effects of *Land&Building* and "initial leverage" (the firm's leverage at the time it first appears in Compustat). We do so replicating Table II (full model) of Lemmon et al. (2008).[33] This is an interesting comparison, since Lemmon et al. argue that initial leverage is one of the key predictors of capital structure. In this test, we emulate the impact of firm-fixed effects by subtracting firm-centered averages for all variables except initial leverage (which is fixed within firm). Results are reported in column 5 of Table 11. Our estimates imply that a 1-standard-deviation increase in initial leverage causes

[30] As in Lemmon et al. (2008), one exception to this pattern is the estimate for *Log(Sales)*.

[31] For comparability with Lemmon et al. (2008), in this section we assess the economic significance of our estimates using 1-standard-deviation shifts.

[32] These results are not surprising in the context of fixed-effects econometrics. Overall, our findings suggest that while within-firm variation in the traditional determinants of leverage has generally limited ability in explaining variation in leverage, land and buildings seem to play an important role in explaining variation in leverage not only in the cross section but also within the firm in the time series.

[33] For comparability, in Table 11 we rely on a model specification that adheres closely to that of Lemmon et al. (2008). Because of missing information in Compustat related to additional variables used by Lemmon et al. and due to the use of lagged explanatory variables, the number of observations for the tests reported in Table 11 is lower than the number of observations displayed in some of the previous tables in our paper.

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TABLE 11

Asset Tangibility and Firm-Fixed Effects

Table 11 reports results from replicating Table V in Lemmon et al. (2008) for our sample based on OLS and fixed effects instrumental variable estimations (IV-FE) for both our restricted and unrestricted models. Estimations also include year dummy variables. The dependent variable is market leverage. We follow Lemmon et al. in defining variables and model specifications, but in columns 3–5 we use our *Land&Building*, *Machinery&Equipment*, and *OtherTangibles* instead of the traditional tangibility proxy. All firm-level data are from Compustat industrial tapes. The sample includes all firms except financial, lease, REIT and real estate-related, nonprofit, and governmental firms. Standard errors reported in parentheses are based on heteroskedastic-consistent errors adjusted for clustering across observations of a given firm (Petersen (2009)). The figures in square brackets under the standard errors represent the percentage changes in leverage relative to the sample mean as each continuous regressor increases by 1 standard deviation, while all other regressors are kept at their sample mean. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% (2-tail) test levels, respectively.

	Restricted Model		Unrestricted Model		
	OLS	IV-FE	OLS	IV-FE	IV-FE
Independent Variables	1	2	3	4	5
OverallTangibility	0.164*** (0.030) [13.38%]	0.260** (0.104) [21.21%]			
Land&Building			0.171*** (0.044) [9.46%]	0.437** (0.185) [24.12%]	0.559*** (0.172) [30.85%]
Machinery&Equipment			0.136*** (0.044) [8.03%]	0.127 (0.145) [7.49%]	−0.090 (0.159) [−5.29%]
OtherTangibles			0.152 (0.094) [2.70%]	0.690*** (0.231) [12.20%]	0.587** (0.244) [10.39%]
InitialLeverage					0.482*** (0.033) [36.13%]
Log(Sales)	0.003 (0.003) [2.93%]	0.026*** (0.008) [23.85%]	0.004 (0.003) [3.49%]	0.026*** (0.008) [23.65%]	0.041*** (0.009) [37.91%]
Market-to-Book	−0.059*** (0.004) [−26.14%]	−0.026*** (0.003) [−11.29%]	−0.058*** (0.004) [−25.70%]	−0.025*** (0.004) [−10.94%]	−0.025*** (0.004) [−11.14%]
Profitability	−0.058** (0.027) [−5.71%]	−0.036* (0.021) [−3.58%]	−0.058** (0.027) [−5.76%]	−0.037* (0.022) [−3.67%]	−0.048** (0.024) [−4.72%]
IndusMedLev	0.224*** (0.042) [10.88%]	0.045* (0.027) [2.19%]	0.235*** (0.042) [11.38%]	0.051* (0.028) [2.49%]	0.044 (0.030) [2.12%]
CashFlowVol	−0.121* (0.072) [−4.08%]	0.053 (0.074) [1.79%]	−0.109 (0.074) [−3.70%]	0.058 (0.074) [1.97%]	0.085 (0.079) [2.88%]
DividendPayer	−0.078*** (0.010) [−37.56%]	−0.012 (0.008) [−5.87%]	−0.083*** (0.010) [−39.94%]	−0.011 (0.008) [−5.52%]	−0.015* (0.008) [−3.51%]
No. of obs.	6,073	6,073	6,073	6,073	6,073
Adj. R^2	0.219	0.107	0.213	0.103	0.105

leverage to increase by 0.07. Relative to our sample mean, this change corresponds to an increase of about 36%. This result is consistent with the evidence in Lemmon et al., who report in Table II (column 6) of their paper that a 1-standard-deviation increase in initial leverage causes leverage to increase by 0.07. More importantly, a comparison of the results across columns 4 and 5 shows that the coefficient of *Land&Building* becomes stronger in the model with initial leverage. As it turns out, the impact of *Land&Building* is sizable and comparable to the impact of initial leverage. In particular, we find that a 1-standard-deviation increase in *Land&Building* causes leverage to increase by about 0.06. Relative to the sample mean, this figure implies an increase in leverage of 31%.

The tests in this section show that, unlike traditional determinants of leverage, our measures of asset tangibility *strengthen* after one controls for firm idiosyncratic characteristics, such as initial leverage and standard fixed effects. Simply put, they pass the "firm-effects stress tests" proposed by Lemmon et al. (2008). These results highlight the importance and robustness of the redeployability–leverage channel we propose. More generally, they imply that one potential problem with traditional leverage determinants is that their proxies might be too crude. Our findings provide a brighter picture of leverage models in suggesting that the statistical properties of other traditional leverage determinants might also improve upon better empirical characterization.

VI. Concluding Remarks

Understanding the role of collateral in borrowing is important because of its implications for corporate financing. In the presence of contracting frictions, assets that are tangible are more desirable from the point of view of creditors, because they are easier to repossess in bankruptcy states. Tangible assets, however, often lose value in liquidation. It is thus unclear whether and how they affect a firm's debt capacity.

The results of this paper suggest that the redeployable component of tangible assets drives observed leverage ratios. Furthermore, across the various categories of tangible assets, it is land and buildings (presumably, the least firm-specific assets) that have the most explanatory power over leverage. The evidence we present implies that financing frictions are key determinants of capital structure. While prior literature has considered the notion that these credit imperfections are potentially relevant, we show that they have first-order effects on leverage.

Our analysis sheds additional light on the effect of credit market imperfections on leverage by comparing firms that are more likely to face financing frictions (small, unrated, and low-dividend firms) and firms that are less likely to face those frictions (large, rated, and high-payout firms). We find that our redeployability–leverage results are pronounced across the first set of firms. In contrast, for unconstrained firms, redeployability does not explain leverage. These firm-type contrasts are consistent with the financing friction argument: Variation in asset redeployability only affects the credit access of those firms that are credit constrained. Further tests show that redeployability eases borrowing the most when the supply of credit is tightened.

Our paper identifies a well-defined channel (the redeployability of tangible assets) to characterize the impact of credit frictions on leverage. We believe future research should more carefully consider trade-offs between credit constraints, credit supply, and firms' demand for debt financing. It should do so emphasizing concrete aspects (and frictions) of real-world financial contracts. More generally, this strategy can be useful for research focusing on the interplay between access to collateral, corporate financing, and investment. The importance of connections between access to collateral and corporate policy, for example, came to the forefront of the economic debate during the recent financial crisis. One could argue

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that more work on this topic can be useful for researchers as well as economic policymakers.

Appendix. Instrument Quality Assessment

It is important that we verify the validity and relevance of our proposed instruments. Test statistics that speak to these properties are reported in Table A1. The table presents the slope coefficients returned from four different first-stage regressions featuring, alternatively, *OverallTangibility*, *Land&Building*, *Machinery&Equipment*, and *OtherTangibles* as the endogenous variable. The instruments considered deliver results that agree with our priors. For example, proxies for rental volatility and the supply of rentable real estate in a firm's location load, respectively, positively and negatively on the firm's propensity to acquire land and buildings. Likewise, liquidity in the market for machinery and equipment leads firms to carry less of those assets in their balance sheets, while the ratio of employees to assets is positively associated with the demand for capital. Some of the instruments we include based on our priors prove to have somewhat low (individual) explanatory power, nonetheless. It is thus important that we carefully examine the statistical relevance of our instrumental set.

TABLE A1

First Stage of IV Regressions

Table A1 reports the first stage of instrumental variable regressions. For the Restricted Model the dependent variable is *OverallTangibility*. For the Unrestricted Model the dependent variables are *Land&Building*, *Machinery&Equipment*, and *OtherTangibles*. We only tabulate coefficients on excluded instruments in the interest of space. Estimations also include firm- and year-fixed effects. All firm-level data are from Compustat industrial tapes. Instrumental variables are obtained from several sources and are described in detail in the text. The sample includes all firms except financial, lease, REIT and real estate-related, nonprofit, and governmental firms. Standard errors reported in parentheses are based on heteroskedastic-consistent errors adjusted for clustering within firm. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% (2-tail) test levels, respectively.

	Restricted Model	Unrestricted Model		
Excluded Instruments	*Overall Tangibility*	*Land & Building*	*Machinery & Equipment*	*Other Tangibles*
Panel A. Real Estate Markets				
RentalVolatility	0.019***	0.039**	0.160**	−0.001
	(0.005)	(0.017)	(0.065)	(0.006)
LogSuppliers	−0.013***	−0.006**	−0.006*	−0.001
	(0.005)	(0.003)	(0.003)	(0.001)
GovernmentDisposal	0.002	−0.003*	0.003*	0.001
	(0.003)	(0.002)	(0.002)	(0.001)
Panel B. Machinery&Equipment Market				
IndustryResale	−0.039***	−0.008	−0.019**	−0.001
	(0.011)	(0.006)	(0.007)	(0.003)
IndustryLabor	0.484**	0.124	0.408***	−0.009
	(0.230)	(0.165)	(0.143)	(0.049)
No. of obs.	8,887	8,887	8,887	8,887
Shea's partial R^2 (excluded instruments)	0.054	0.057	0.083	0.071
Standard F-test (excluded instruments)	23.28***	10.08***	16.47***	5.19***
Kleibergen-Paap's statistic	23.28		10.59	
Hansen's J-statistic: p-value	0.19		0.57	

The first instrument relevance test statistic we consider is Shea's partial R^2 (Shea (1997)). Shea's R^2 measures the overall relevance of the instruments for the case of multiple endogenous variables. Table A1 reports that the Shea's R^2s associated with our instruments are relatively large for panel tests of the type we conduct, in the range of 5.7%–8.3%. The simple partial R^2s are, respectively, 6.7% for the *Land&Building* model and 8.6%

for *Machinery&Equipment*. Baum, Schaffer, and Stillman (2003) recommend as a rule of thumb that if the Shea's partial R^2 and the simple partial R^2 are of similar magnitude, then one can infer that instruments used in the identification have adequate explanatory power. Our instruments perform well under that metric.

We also conduct first-stage exclusion F-tests for our set of instruments, and the associated p-values for those tests are all much lower than 1% (confirming the explanatory power of our instruments). One potential concern with the first-stage F-test in the case of multiple endogenous regressors is that it might have associated low p-values for all first-stage regressions even if only one instrument is valid (see Stock and Yogo (2005)). To address this issue, we conduct the *Kleibergen-Paap* test for weak identification (Kleibergen and Paap (2006)). In the case of multiple endogenous variables, this is a test of the maximal IV bias that is possibly caused by weak instruments. For the unrestricted model, the *Kleibergen-Paap F*-test statistic is 10.6. Since the corresponding Stock and Yogo critical value for a maximal IV bias of 10% is 9.4, the maximal bias of our IV estimations will be below 10%. Following Stock and Yogo, for further robustness, we reestimate our models using the limited information maximum likelihood (LIML) estimator and the Fuller's modified LIML estimator, which are both robust to weak instruments. Our results are invariant to the use of maximum likelihood estimators. In all, these various checks collectively suggest that our results are robust to concerns about weak instruments.

Finally, we examine the validity of the exclusion restrictions associated with our instruments. This helps address concerns about whether our instruments belong in the leverage (second-stage) equation. We do this using Hansen's (1982) J-test statistic for overidentifying restrictions. The p-values associated with Hansen's test statistic are reported in the last row of Table A1. The high p-values reported in the table imply the acceptance of the null hypothesis that the identification restrictions that justify the instruments chosen are met in the data. Specifically, these reported statistics suggest that we do not reject the joint null hypothesis that our instruments are uncorrelated with the error term in the leverage regression and the model is well specified.

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