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The Effect of Forced Refocusing on the Value
of Diversified Firms

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Abstract

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Key Words: Diversification, divestiture, refocusing, internal capital market, business groups, Chinese economy, tunneling

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The Effect of Forced Refocusing on the Value of Diversified Firms

1. Introduction

This paper studies how investors responded when Chinese regulators required large, publicly traded companies to divest their non-core hotel and real estate assets in 2010. Because all firms with non-core assets in these industries were required to refocus, this episode allows estimates of diversification's effect on value that are free from the selection problems that have complicated causal inference in the existing literature (see Maksimovic and Phillips (2013) for a recent survey). We find that stock prices rose on average in response to the forced divestitures, suggesting that investors viewed diversification as a value-destroying strategy for those firms. We also calculate the implied diversification discount for each firm using the excess value method (Lang and Stulz, 1994; Berger and Ofek, 1995), and find that it is a poor predictor of the market response, raising further questions about the validity of that construct.

Corporate diversification is a central topic in economics and finance research, in part because most large companies are diversified and a huge amount of capital is allocated through the internal capital markets of these firms. Diversified companies are the rule in most industrialized economies and diversified business groups dominate most emerging economies (Khanna and Yafeh, 2007). The net effect of diversified businesses on economic performance remains a source of controversy, with some theory and evidence suggesting that they add value through winner-picking and coinsurance while other theory and evidence suggesting that they destroy value through internal politics, investment distortion, and tunneling.¹

A large empirical literature has accumulated that attempts to understand the value consequences of corporate diversification, most of it employing either the "excess value/diversification discount" method pioneered by Lang and Stulz (1994) and Berger and Ofek (1995), or announcement return methods.² Both methods are now understood to have significant limits in their ability to measure the causal effect of diversification on value. The excess value, which compares the value of a diversified

¹ On winner picking, see Williamson (1975; chapters 8-9), Stein (1997), Matsusaka and Nanda (2002), and Maksimovic and Phillips (2002). On internal inefficiencies, see Scharfstein and Stein (2001) and Ozbas (2005). In emerging economies, diversified groups are believed to use within-group transactions to overcome deficiencies in external market due unreliable contract enforcement, weak corporate governance, limited information disclosure, ineffective regulations and enforcement, and so forth. See Morck et al. (2005) and Khanna and Yafeh (2007) for surveys.

² A partial list of articles using the diversification discount method would include Berger and Ofek (1995), Servaes (1996), Lins and Servaes (1999, 2002), and Hoechle et al. (2012). Event study methods have been used to examine refocusing divestitures (e.g. John and Ofek (1995), Berger and Ofek (1999), and Dittmar and Shivdasani (2003)) as well as diversifying acquisitions (see Akbulut and Matsusaka (2010) for an overview).

firm to the weighted median value of undiversified firms in the same industry, can be interpreted as a causal effect of diversification only if the assets of the diversified firms (treatment group) would be identical in value to those of the comparison undiversified firms (control group) if the diversified firms were broken up. Yet both theory and evidence cast doubt on this assumption, and give ample reason to believe that the assets of diversified firms are inherently less productive than the assets of undiversified firms, creating a downward bias in the excess value.³ The announcement return, which measures the market's reaction to changes in diversification (through an acquisition, divestiture, or refocusing program) relative to firms that do not make an announcement, provides an unbiased estimate of the value effect for firms that make an announcement. However, the sample of firms that make an announcement is self-selected; to the extent that managers seek to increase firm value, the sample of firms making diversification announcements will be biased toward those that stand to benefit from the change.

An ideal experiment for estimating the effect of diversification would involve a random sample of firms exogenously shedding their unrelated assets ("refocusing") and then observing how their value changes compared to a control group that does not refocus. Implementing a field experiment along those lines is hard to imagine, but the unexpected issuance of two orders by China's State-owned Assets Supervision and Administration Commission (SASAC) in 2010 provides two quasi-experiments that share key features of the ideal experiment. SASAC's order required *all* central government controlled firms that were not primarily in the hospitality or real estate businesses to divest their hotel and real estate assets. Our research strategy is to estimate the abnormal stock price return of the targeted firms around the announcement date. SASAC did not force refocusing on a random sample of firms, but under plausible assumptions, event study methods – using firms that were not required to refocus as the control group – provide an estimate of the effect of refocusing on value without the self-selection problem in previous event studies, and without the biased control group problem present in excess value studies. Our main finding is that companies forced to refocus experienced an abnormal return of 1 to 2 percent during the announcement period, suggesting that diversification was a value-destroying strategy for these firms. The abnormal return for firms with large forced divestitures was 2.9 to 4.0 percent on average. The finding of a positive announcement return from forced refocusing is robust to a variety of alternative specifications and controls.

³ Theories predicting that diversified firms have different potential values and opportunities include Gort et al. (1985), Matsusaka (2001), and Maksimovic and Phillips (2002). For evidence on the predictability of diversification, see Gort et al. (1985), Hariharan and Montgomery (1991), Campa and Kedia (2002), and Villalonga (2004a).

Our second set of results uses the episode to evaluate the construct validity of the excess value/diversification discount measure. While validity of this construct has been questioned by many (Matsusaka, 2001; Campa and Kedia, 2002; Graham et al., 2002; Maksimovic and Phillips, 2002; Villalonga, 2004a, 2004b; Custodio, 2014), researchers continue to use it as a proxy for value destruction from diversification (e.g., Hoechle et al. (2012)). We are able to provide a direct assessment of the extent to which the excess value measure captures the potential value gain from refocusing by comparing a company's actual value change when forced to refocus with its estimated excess value. Our main finding in this regard is that the correlation between the diversification discount implied by the excess value measure and the actual change in value associated with forced refocusing is a modest 0.13, reinforcing previous arguments that excess value may be a weak proxy for the actual value consequences of diversification.

Our third set of results explores the cross-sectional variation in abnormal returns in order to shed light on what factors influence the value of diversification. Theory suggests that a diversified company's ability to create or destroy value ultimately stems from the control rights of headquarters to transfer resources from one unit to another. On the positive side, internal allocation may be able to move resources from low to high value uses more efficiently than external markets due to better information and the ability to avoid costly external finance (Williamson, 1975; Stein, 1997; Matsusaka and Nanda, 2002), and timely resource transfers can be used to buffer group firms from unexpected shocks (Jia et al., 2013). On the negative side, internal capital markets may be susceptible to investment distortions arising from internal politics and strategic use of information (Scharfstein and Stein, 2000; Ozbas, 2005), and in business groups, internal transfers may be used to expropriate minority shareholders by diverting corporate resources to entities tied to the controlling shareholder, often referred to as "tunneling" (Morck and Yeung, 2003; Khanna and Yafek, 2007).

To assess the explanatory power of these theories, we consider several variables linked to potential resource distortions and the volume of internal transfers. The literature on business groups suggests that the incentive for tunneling is greatest when the controlling shareholder has only a small claim on the firm's cash flow (e.g. Claessens et al. 2002). We find that abnormal returns are most positive for firms that have a controlling shareholder with limited cash flow rights, suggesting that investors' perception of value destruction in these firms stems in part from fear over the possibility of tunneling. However, we are unable establish a link between announcement returns and actual internal transactions, where internal transactions are measured by the amount of intercorporate related-party transactions (loans, loan guarantees, and asset and goods sales between controlled units) and by "other

receivables,” which includes intercorporate loans that have not been repaid. In short, our evidence suggests that investors’ concern with value destruction in these firms is connected to fear of tunneling, but we cannot detect the form in which that tunneling is taking place.

Our study is intended to contribute to two literatures that should be closely connected but so far have tended to proceed along separate paths. The literature on corporate diversification has focused on corporations with fully owned subsidiaries or divisions, primarily in the United States, while the literature on business groups has focused on groups of companies in emerging economies that are legally separate entities but jointly controlled through interlocking ownership. The economic theories used to explain these organizations are similar, revolving around potential advantages of internal versus external resource allocation, and the dangers of misallocation from giving control rights to a headquarters unit. A central issue in both literatures is whether bringing diverse business operations together under the control of a headquarters unit is value enhancing or value destroying. Our estimates provide some of the most direct evidence that diversification was value destroying for the particular set of firms that we study. Both literatures are also interested in whether headquarters enhances efficiency by using internal allocation to improve on external markets, or reduces efficiency by misallocating or expropriating internal resources. Our evidence on this point is more tentative but suggests that investors disliked diversification in our sample firms because of a fear of tunneling by controlling shareholders.

Although our study provides what we believe is some of the most direct evidence on the value consequences of corporate diversification to date, our quasi-experiment comes with some limitations. First, the number of directly impacted firms is not large – 27 with hotel assets and 23 with real estate assets – meaning that the analysis at times is akin to a case study. Second, while the forced refocusing was exogenous from the perspective of the firms, it was not random; only firms with assets in the hotel and real estate industries were targeted. Thus, while our study provides relatively clean estimates of the value consequences of diversification for firms in these industries, the lessons may or may not extend to other industries. A third limitation is that the study is focused on Chinese firms with significant government ownership. Because capital markets are less efficient in China than more developed economies, internal capital allocation may be more important and play a different role in China than elsewhere (although our evidence tends to run against this idea by suggesting that internal capital markets are less efficient), and the economic logic of state-controlled firms may be different than firms that have no state ownership. Again, these are potential limits on external validity. We do not believe our study raises more or fewer concerns than other studies in terms of external validity, and we believe

that Chinese companies and companies with government ownership are important to study in their own right – but as we learn more about variations across countries, it seems wise to keep in mind that any time we use data from one country, the results may or may not extend to others.

2. The Announcements and Institutional Context

On January 25, 2010, the Enterprise Reform Bureau of China's State-owned Assets Supervision and Administration Commission ordered a restructuring of hotel operations among central government-owned enterprises (CSOEs; *zhongyang qiye* in Chinese). Specifically, any CSOE that was not primarily based in the tourism or hospitality business was instructed to exit the hotel industry (and also to divest their restaurants) within three to five years.⁴ On March 18, 2010, a similar directive was issued calling for companies to shed their real estate assets if their primary business was not real estate. For the purposes of identification, the best case is if the SASAC orders were motivated by something orthogonal to the value consequences for the individual firms. The policy making process in China is somewhat opaque so the intended purpose of the reforms was not stated publicly, but many analysts believed that one goal of the real estate divestitures was to cool off the country's booming real estate market. It was believed that CSOEs outside real estate industries were bidding on property, and driving up prices.⁵ Some media outlets speculated that the hotel divestitures were aimed at improving the performance of underperforming hotels, but others suspected that the government agencies might have had undisclosed interests in the divestment.⁶

The forced refocusing announcements occurred in an institutional context that avoids some of the more common problems with event studies. One concern when conducting event studies is the possibility that the announcement was anticipated by investors. In China important regulatory changes are typically announced without prior discussion among interested groups so the announcement is the first real information to reach the market. This contrasts with the United States where policy announcements are preceded by public debate and lobbying. In the case of our two events, media coverage indicates that both came as surprises to investors, and market participants expressed

⁴ Note that implementation is ongoing at the time of writing, so we are unable to study the implementation itself.

⁵ For example, a story in *China Daily* began: "In a move to curb soaring property prices, the state-assets watchdog on Thursday told major state-owned enterprises whose core business is not real estate to quit the market." (Hu, 2010).

⁶ For example, a story in *CaixinOnline* reported, "Sources close to SASAC said the move is aimed at reorganizing and integrating state-owned hotels, transferring them to companies that can manage them more efficiently. . . . Some industry experts were puzzled by the order, and wonder whether government agencies have unannounced interests in the divestments." (Wang et al., 2010).

puzzlement at the rationale for the policies. While the government had expressed an interest in separating primary and secondary businesses in CSOs for almost a decade, there does not appear to have been any advance warning about these particular orders or that these particular industries would be targeted. For more formal evidence on whether the announcements were anticipated, we estimated the cumulative abnormal return over the five weeks preceding each announcement; the mean was -0.04 percent for the hotel announcement and 0.03 percent for the real estate announcement.

Another concern when conducting event studies is the possibility that the announcement reveals or signals other information than the change in diversification itself. For example, in an event study that examines the return from diversifying acquisitions, the acquisition announcement may include other information that independently influences the return, such as the means of payment (e.g. that the firm plans to issue stock) or that the acquirer has fewer internal investment opportunities than previously believed (Matsusaka, 2001). Because we study refocusing that is forced and not connected to any firm-specific information, the main problems with event studies do not arise in our context. One issue that might be of concern is that the announcement changed expectations about the level of government action going forward. None of the media reports we reviewed interpreted the announcements as signals of a broader change in regulatory intensity, so we have no reason to believe that the event returns capture anything other than the anticipated effect of the divestitures. To put this on an empirical footing, below we also explore the return to government-owned firms that were not affected by the announcement; there is at best weak evidence of an abnormal price movement among these firms, and in any case not enough to account for the main effects.

A third concern with event studies is whether the announcement is credible. Enforcement, or implementation, can be erratic in China, so investors may doubt that the orders will be implemented (Calomiris et al., 2010). As it turns out, although the pace of sales has been slow, the targeted companies have been selling off their assets in the three years since the initial announcement, so the announcements appear to have been credible.⁷ Moreover, if investors entirely doubted that the orders would be followed, then the abnormal return would be zero, so this concern tends to bias the effects toward zero.⁸

⁷ For hotels, see “SOEs check out of hotel business under SASAC rule,” in China.org.cn (October 19, 2013). For real estate, see “More SOEs exit real estate market,” in CaixinOnline (February 23, 2011) (Xu, 2011).

⁸ As a rough check on this, we estimated abnormal returns on April 13, 2010 when several influential financial media outlets pointed out that some firms were continuing to acquire land despite having submitted a divestiture plan, and suggesting that the program might be implemented more slowly than was originally thought. We found announcement returns that were in the opposite direction from those associated with the original announcement, suggesting that the initial announcement was deemed credible at the time.

The divestiture order impacted both publicly traded and nontraded firms. Our study focuses on the subset of publicly traded firms for which daily stock price data are available. Each of the publicly traded firms in our sample is part of a business group that is ultimately controlled by the central government. Specifically, at the head of each group is a nontraded company fully owned by the central government; this “parent” firm owns a controlling interest in the shares of the publicly traded company that is being forced to divest (or owns a controlling interest in one or more firms that themselves have a controlling interest in the publicly traded company). This raises the question of how similar the behavior of our sample firms is likely to be compared to firms that are not ultimately controlled by the government. We cannot answer the question in this study, but we note that the publicly traded firms in our sample should not be seen as equivalent to “government firms.” Because the publicly traded firms in our sample issue stock that is traded on a financial market, they return their profits to investors through dividends, and shareholders have the right to seek distribution of dividends and share repurchases. Chinese law also gives shareholders significant influence in the governance of these companies: each firm has a board of directors elected by shareholders, and the board has the right to hire and fire the managers. Shareholders also have the right to request or convene general meetings of shareholders, the right to make proposals, and the right to revoke decisions of the board, among other things.⁹ Of course, the government holds the most shares, so these rights do not wall off the government from corporate decisions. However, as a practical matter, the government does not appear to be involved in the strategic or operational decisions of these firms, and the firms appear to be guided primarily by a conventional desire to produce profit.¹⁰ Thus, while it must matter in some situations that the government has a controlling interest, there is no obvious reason to believe that the diversification strategies of these firms are more or less motivated by profit considerations than other firms.

3. Methods and Data

To calculate abnormal announcement returns, for each firm we estimated the parameters of a Fama-French three-factor model over a window running from 150 to 30 trading days before the announcement. We then calculated the abnormal return for each day, and summed them over an event

⁹ Chinese corporate law has changed often and significantly over the last two decades. For an overview of Chinese company law at about the time of the announcement, see Gu (2010) and OECD (2011). On the government’s role in corporate decision making, see SASAC’s *Interim Measures for the Supervision and Administration of State-Owned Assets of the Enterprises*, issued in 2003.

¹⁰ Indeed, the fact that refocusing was forced by an order from a regulatory agency suggests that the government does not directly oversee operational or strategic decisions through its controlling ownership interest.

window to arrive at the cumulative abnormal return (CAR) associated with the announcement.¹¹ Daily stock price information was taken from the GTA Database, part of which is available through Wharton Research Data Services. Daily Fama-French factors were provided by RESSET, a Chinese financial data vendor, headquartered in Beijing.¹² CARs are winsorized at the 1st and 99th percentiles. We obtained conventional firm-level financial information (assets, capital expenditure, return on assets, cash flow, market-to-book) and ownership data from the GTA Database.

Segment data from 2009 were hand-collected from annual reports. Some of the segments we study are organized as independent companies and others as divisions. China's Ministry of Finance, which sets accounting rules in China, requires all firms to provide segment sales information in their annual reports. The Ministry of Finance rules require a firm to break out segment information if (i) a segment's sales constitute more than 10 percent of the company's total sales; (ii) a segment's absolute profit constitutes more than 10 percent of the firm's total profit; or (iii) a segment's assets constitute more than 10 percent of the total assets of the firm.¹³ We collected segment sales information for all central government-controlled listed firms, a total of 234 after dropping financial firms.¹⁴ In our sample, 41 percent of firms have two segments, 23 percent have three segments, 17 percent have four segments, 10 percent have five segments, 6 percent have six segments, and 1 percent have seven segments (this is similar to the distribution in Lang and Stulz (1994) for American firms.)

We calculate the excess value for each listed firm following the method in Berger and Ofek (1995). Excess value is defined as the ratio of a diversified firm's market value to its imputed market value, minus one. The potential value of a company's segments if they were independent firms is assumed to be equal to the median market-to-sales multiple of single-segment firms in the industry.¹⁵ A diversified firm's imputed value is the sales-weighted imputed value of its segments. Excess values are winsorized at the 1st and 99th percentiles, and three observations with values above 1.0 are restated as 1.0. Note that we work with excess values directly rather than logarithms because a logarithmic

¹¹ For the hotel event, we use January 26, 2010 as day 0 because the announcement appears to have been made late in the afternoon of January 25 and was not discussed in the media until January 26. For the real estate event, we use March 18, 2010 as day 0.

¹² Corporate debt is rare in China, so we can focus on stock price movement as a measure of firm value.

¹³ Most firms provide segment sales information even if a segment does not exceed the 10 percent threshold: 85 percent of sample firms report sales information for segments that produce less than 10 percent of total sale, and the smallest segment accounts for 0.14 percent of total sales of the listed firm.

¹⁴ Because segments are often organized as independent subsidiaries of the listed firm, related party transaction regulations require the listed firm to provide financial information on the subsidiary. We use this information to verify the accuracy of the segment information collected from annual reports.

¹⁵ We lack information to calculate an asset-weighted measure of excess value.

transformation is a good approximation only in the vicinity of 1.0, and many observations are not close to 1.0. Results are similar with log values.

To capture the intensity of internal capital markets, we obtained related party transaction (RPT) data from GTA's Related Party Transaction Database. We classify related party transactions into two types: intercorporate loans and loan guarantees, and intercorporate goods and service transactions.¹⁶ Since we are interested in the overall activity level of the internal market, we sum the transactions of each type; that is, if the parent firm sells \$100 of goods to a subsidiary, and the subsidiary sells \$100 of goods to the parent firm, the total trading volume is \$200. As another potential measure of tunneling, following Jiang et al. (2010), we also collected accounting information on "other receivables" for each listed company from this database, specifically, the value of loans outstanding at the end of each year that had been provided by the listed firm to its controller.

Table 1 reports summary statistics. The table distinguishes firms that were forced to divest hotel assets, firms that were forced to divest real estate assets, and CSOEs that were not forced to divest hotel or real estate assets.

4. Findings

A. Response to Announcement of Forced Refocusing

Table 2 reports cumulative abnormal returns (CARs) associated with the forced divestiture announcements. The first row (1) reports CARs for all 50 firms that were ordered to divest, that is, combining the 23 firms that were required to divest hotel assets and the 27 firms that were required to divest real estate assets. The mean CAR over the $[-1, 1]$ event window was 1.0 percent, a number that is significantly different from zero at the 5 percent level. The CAR is larger for longer windows, growing to 2.0 percent over the $[-1, 7]$ event window, a number that is different from zero at the 1 percent level of significance. The pattern suggests that information took several days to diffuse throughout investors.¹⁷ The standard errors are too large to statistically confirm this pattern, but in light of uncertainty about the correct size of the event window, we present results for multiple event windows throughout. The median abnormal return was 0.5 percent over the $[-1, 1]$ window, statistically distinguishable from zero

¹⁶ An inter-corporate loan guarantee differs from an inter-corporate loan in that the former is a contingent liability for the focal firm, that is, it is only a liability for the guarantor when the guaranteed defaults on the loan.

¹⁷ We searched for other news in the event windows that might explain an upward pattern but did not find an obvious candidate. Due to the underdeveloped nature of financial information intermediaries (such as analysts and business media) in China, it seems possible that investors may have required several days to fully understand the implications of the government orders. For example, it may have taken some time to assess the likelihood of implementation.

at the 5 percent level. The median rises to 2.2 percent in the $[-1,7]$ window, a number that is different from zero at the 5 percent level. Sixty-four percent of abnormal returns were positive during the $[-1,1]$ window, a number that can be distinguished from 50 percent at the 5 percent level. All of these numbers indicate that investors expected the companies to be worth more refocused than with their existing diversified lines of business. That is, the market viewed diversification by these companies as value-destroying.

Firms that are forced to divest but whose hotel or real estate holdings are small are likely to experience little or no market reaction to the forced refocusing announcement. To provide cleaner estimates of the effect of refocusing, row (2) of Table 2 reports CARs for companies whose hotel or real estate holdings generated more than 4 percent of the company's sales. The CAR in this subsample had a mean of 2.9 percent over the $[-1,1]$ window, rising to 4.0 percent over the $[-1,6]$ window. The median was 2.0 percent over the $[-1,1]$ window and 3.2 percent over the $[-1,6]$ window. All of these numbers are different from zero at conventional levels of statistical significance. These findings reinforce the notion that the market response was linked to the value consequence of refocusing.

Rows (3) and (4) of Table 2 report the returns separately for companies required to divest hotel assets and companies required to divest real estate assets. The mean and median are positive in all cases, and similar in magnitude, and the percentage of positive returns ranges from 56 to 65 percent. The market's response to forced refocusing was more positive in the hotel than real estate industries, but qualitatively similar and returns for the two events cannot be distinguished from each other statistically.

As discussed above, one concern in interpreting the market response is the possibility that the announcement signaled other information about future government action with regard to state-controlled enterprises. For example, if the announcement signaled heightened scrutiny of CSOEs and an increased emphasis on profits in the future, then investors might have responded positively for reasons unrelated to refocusing. We found nothing in the media reports surrounding the announcements that would suggest this possibility is more than hypothetical, but the last row (5) of Table 2 attempts to assess it by reporting the CARs for companies controlled by the central government that were not required to divest hotel or real estate assets. Over the $[-1,1]$ window, the mean CAR was 0.3 percent and the median was 0.2 percent; only the median is different from zero at conventional levels of significance. Over the $[-1,6]$ window, the mean CAR was 0.0 percent and the median was -0.7 percent; neither value is different from zero at conventional levels of significance. The percent positive was 53 percent over the $[-1,1]$ window and 44.6 percent over the $[-1,6]$ window; the latter different from 50

percent at the 5 percent level.¹⁸ Overall, there is not much evidence that the market interpreted the forced refocusing announcements as auguring something positive for state-controlled firms in general.

Table 3 reports regressions using the full sample of companies (both forced and not forced) in which the dependent variable is the CAR. The dimensions of the event window are indicated at the top of each column. This parametric specification allows us to control for additional factors that might influence the announcement return. In addition to the forced refocusing variable of primary interest, the regressions include five firm characteristic variables: assets, market to book, earnings, cash, and investment.¹⁹ Inclusion of these variables has advantages and disadvantages. By controlling for other effects that might determine the announcement return, they potentially allow more precise estimates of the refocusing effect. On the other hand, some of the control variables are known to be influenced by diversification – for example, diversified firms hold less cash (Duchin, 2010) – so the control variables may end up capturing and stripping out some of the refocusing effect. Whether the benefits outweigh the costs of this specification are unclear, but we present the regressions to flesh out the picture. The regressions also include a dummy for the real estate event and 2-digit industry-specific fixed effects. By allowing returns to vary for impacted versus non-impacted companies, the regressions allow for the possibility of a CSOE-specific factor in returns, something that is not allowed in our baseline asset pricing model.

The first four regressions in Table 3 use the full sample over various windows. The first two regressions allow for a refocusing effect with a dummy variable equal to one for firms that were forced to refocus. The coefficient on the dummy is positive and statistically different from zero at the 10 percent level in both regressions, and the magnitudes are similar to what appeared in Table 2. In the third and fourth columns, the refocusing dummy is replaced with a variable representing the size of the divestiture: the percentage of sales that had to be divested. The advantage of this variable is that it allows the refocusing effect to vary with the size of the divestiture, which we would expect.²⁰ The

¹⁸ The difference in means between forced and non-forced firms is not statistically significant over the [-1,1] window ($p = .113$), and is significant over the [-1,6] window ($p = .020$).

¹⁹ We also explored, for all regressions reported in the paper, inclusion of a variable equal to the amount of the firm owned by the government. Because this variable was statistically insignificant in all regressions and had no effect on the coefficients of interest, we did not include it in the final regressions.

²⁰ A third specification would include both the dummy variable and the size of the divestiture. Such a specification would be appropriate if, for example, there was a fixed cost associated with filing a divestiture plan, no matter how small. Formally, the specification in the third and fourth columns assumes that the refocusing effect is continuous at zero, while the dummy + size specification allows for a discontinuity. In unreported regressions, we found little evidence for a discontinuity at zero.

coefficient on the size of the divestiture is positive and statistically significant in both regressions, further suggesting that the positive market reaction is not spurious.

The last four columns of Table 3 report separate regressions for the hotel and real estate events. The coefficients on the size of the divestiture are positive and statistically distinguishable from zero in all four regressions. The market's positive reaction to forced refocusing is a characteristic of both the hotel and real estate event.

Tables 2 and 3 provide relatively clean evidence that forced refocusing creates value for our sample firms. One concern we have not yet addressed is the possibility that impacted firms are different from nonimpacted firms in a way that influences the announcement returns (because we are using a quasi-experiment that does not have randomized assignment to treatment or control groups). This concern is mitigated by our identification coming not only from the comparison between impacted versus nonimpacted firms, but also from comparisons within impacted firms that have differential exposure to policy shock. In addition, as suggested by Table 1, the impacted and nonimpacted firms are generally comparable along observable dimensions.

Another issue is that investors might have expected the impacted firms to divest assets at fire sale prices. The multiyear window within which firms were allowed to refocus substantially relieves the pressure to conduct fire sales. Also, note that investors would have reacted negatively if firms were expected to make fire sales, that is, the possibility of fire sales biases the announcement return in a negative direction, against the pattern we find.

B. Announcement Returns and the Excess Value Construct

Over the last two decades, much of the literature on corporate diversification has revolved around an empirical construct called the "excess value" or "diversification discount" that is intended to measure the effect of diversification on a firm's value. The excess value is calculated as the difference between a diversified firm's observed market value and a weighted average of the value of matched nondiversified firms in the same industries as the diversified firm's divisions. Typically, studies have found an average excess value for diversified firms in the range of -10 to -15 percent, which taken at face value implies that diversified firms would be worth 10-15 percent more if they refocused. However, controversy surrounds the excess value construct, particularly concerning whether the matched nondiversified firms are an appropriate benchmark for the divisions of diversified firms (or, in the language of causal analysis, whether a diversified firm's divisions and matching nondiversified firms have the same potential values). Theory predicts that diversified and nondiversified firms are likely to have different capabilities and

opportunities (Matsusaka, 2001; Maksimovic and Phillips, 2002), and empirical research shows that they differ on observables (Campa and Kedia, 2002; Graham et al., 2002; Villalonga, 2004a; Custodio, 2014). Some attempts to control for selection and choose more suitable counterfactuals have produced estimates of no diversification discount or a diversification premium. One recent survey (Maksimovic and Phillips, 2007) concluded, “Much of the conglomerate discount can be explained by sample selection. Firms that choose to diversify, or to stay diversified or to be acquired by diversifiers inherently differ from single-segment firms.” Yet the excess value construct continues to be used by researchers, and interpreted as an estimate of the causal effect of diversification on value (for example, Hoechle et al., 2012).

The quasi-experiment we study offers a rare opportunity to directly assess the validity of the excess value construct by comparing the actual value consequence of refocusing with the implied value consequence from the excess value construct. Our basic question is: how well does the excess value predict the actual value change following the announcement?²¹

To begin, Panel A of Table 4 summarizes the excess value of firms in our sample that were forced to refocus. The mean estimated excess value among firms that were forced to refocus was -8.8 percent, and the median was -21.8 percent. These numbers are comparable to findings of other studies in the literature, and under the received interpretation, imply that forcing these firms to divest their unrelated assets will increase their value by 8.8 percent on average. We know from Table 2 that the mean increase in value upon the refocusing announcement was only 1 to 2 percent. Therefore, at first blush, the excess value measure appears to overstate the true diversification discount for the sample firms by a factor of four to nine.

Figure 1 summarizes the basic information in a different way, by plotting each company’s estimated excess value against its abnormal return from the refocusing announcement. To establish a reference point, suppose that that the divestitures were expected to take place immediately, that after divestiture the refocused firms would be completely undiversified, and that the forced refocusing was expected to be implemented with certainty. Under these admittedly strong assumptions, if the excess

²¹ Two previous studies report some evidence along the same lines, but only for firms that chose to refocus. Berger and Ofek (1999, Table 9) regress the abnormal return associated with a refocusing announcement on the excess value for a sample of 67-103 American corporations. They find a negative coefficient from -0.099 to -0.135 on the excess value, depending on the precise measure. Interpretation is complicated by a large number of explanatory variables in the regression, many of which may impact the excess value. Dittmar and Shivdasani (2003, Table III) find that when diversified firms divest a unit, and reduce from two segments to one segment, the mean (median) estimated diversification discount falls from 23 (32) percent to 14 (6) percent. If the excess value accurately measures the true discount, then the excess value should be zero after refocusing to a single segment.

value accurately represents the effect of diversification on value, then the return associated with the refocusing announcement should be equal to (minus) the excess value, that is, the data should be aligned along the negatively sloping 45-degree line.²² Panel A plots the comparison using the [-1,1] event window and Panel B plots the comparison using the [-1,6] event window. Neither figure display a pattern at all close to the negative 45-degree line.

Panel B of Table 4 provides a parametric version of Figure 1. Each column in the panel reports coefficient estimates from a regression of the abnormal return on the excess value. Again, if the “strong assumptions” listed above hold and the excess value accurately captures the value consequences of diversification, the regression would have an intercept of zero and the coefficient on excess value would be -1.000.²³ The regression in column (1), which is based on all firms subject to the divestiture order and uses the [-1,1] event window, has an intercept of 1.04, significantly different from zero at the 5 percent level. The coefficient on excess value is 0.002, the incorrect sign, quantitatively negligible, and not distinguishable from noise. The R^2 indicates that variation in the excess value can explain 0.3 percent of the variation in the abnormal return. Column (2) reports the same regression using the [-1,6] event window with similar findings: the coefficient on excess value is 0.018, again the wrong sign, and quantitatively and statistically insignificant. Columns (3) and (4) report the regressions separately for companies forced to divest hotel and real estate assets. The patterns are essentially the same: the coefficients on excess value are tiny (0.029 for hotel firms and -0.016 for real estate firms) and the R^2 s remain small.

The lack of a connection between the announcement return and excess value could be because the “strong assumptions” do not hold, or because the excess value does not measure the value of diversification. We next consider the strong assumptions, beginning with the possibility of delayed implementation. The SASAC order required divestiture plans to be submitted within a matter of months, but did not provide a hard deadline by which the assets actually had to be divested (although the media reported that the divestiture plans were expected to be concluded within three to five years). To the extent that divestiture was delayed, the value consequences would be discounted, and the abnormal return would be lower than the true diversification discount. To get a rough sense of whether this can

²² This discussion assumes that investors are able to correctly value the consequences of forced refocusing. This may be a strong assumption, but it applies equally to the excess value measure and the abnormal return measure. That is, both estimates rely on the validity of the assessments of market participants; if those assessments are mistaken then neither construct is a reliable measure of the effect of diversification on value.

²³ This is an approximation: since the CAR percentage is calculated against a base equal to the diversified value while the excess value is calculated against a base equal to the imputed undiversified value, the coefficient will not be exactly equal to one.

account for the observed patterns, suppose that divestitures were not expected to occur for five years and suppose the discount rate was 5 percent. Then the relation between abnormal returns would be $CAR = 0.78 \times EV$. The coefficients in panel B of Table 3 are two orders of magnitude smaller than 0.78. Also, if it was largely a matter of delay, we would still expect to see a correlation between announcement returns and excess values in the cross-section, although muted. In short, it seems unlikely that the possibility of delay can account for the negligible connection between announcement returns and excess value.

The second strong assumption is that the companies would be completely undiversified after the divestiture. Although the targeted companies were required to divest unrelated hotel and real estate assets, they were not required to divest unrelated assets in any other industries. For firms that operate in more than two industries, the excess value is intended to capture the value of the assets if *all* of the unrelated segments were divested. The abnormal return from partial refocusing will be smaller than the overall diversification discount. One way to assess whether partial refocusing can explain the weak connection between the announcement return and excess value is to study firms that began with exactly two segments. In principle, those firms should be completely undiversified after divesting (because of measurement error in segments, this is only an approximation). Figure 2 plots the connection between excess value and abnormal returns for the subsample of firms that begin with exactly two segments. The basic pattern is essentially the same, indicating that partial refocusing is probably not the culprit.

Another way to get a sense of the potential importance of partial divestitures is to allow the relation between abnormal returns and excess value to depend on the fraction of assets that are divested. If the excess value construct captures the value consequences of diversification, we expect a stronger relation between abnormal returns and excess value for those companies whose forced divestitures are a larger fraction of assets. Panel C of Table 4 reports regressions in which excess value is interacted with the percentage of sales that are to be divested. For all firms in the sample, columns (1) and (2) shows that adding the interaction term provides almost no additional explanatory power. Neither the coefficient on excess value nor the interaction term is distinguishable from zero, both are small in magnitude, and the R^2 is almost unchanged. For firms divesting hotel assets, column (3) reveals a statistically significant coefficient on the interaction term, and a healthy R^2 , but the coefficient has the wrong sign. The coefficients in column (4) that focuses on firms divesting real estate assets remain small and statistically insignificant. Allowing the relation between the abnormal return and excess value to depend on the percentage of assets divested adds little explanatory power (or produces a pattern

opposite from what would be expected if excess value accurately measured the effect of diversification on firm value).²⁴ The weak link between the actual market reaction and the excess value construct is difficult to explain as a consequence of partial refocusing.

The third strong assumption is that the announcement was credible, that is, investors expected the refocusing to happen. In fact, many divestitures have happened since the announcement, but compliance is not 100 percent yet, and a reasonable investor might have had some doubt at time of the announcement. In order to equalize the mean excess value and the mean announcement return, the expected probability of compliance had to have been between $\frac{1}{8.8} = .11$ and $\frac{2}{8.8} = .23$, which seems unreasonably low based on the tenor of media stories at the time. Moreover, even if expected compliance was less than 100 percent and therefore the announcement return was systematically smaller than the excess value, we would still expect to see a cross-sectional correlation between excess value and the announcement return, which we do not see.

Taking all of these considerations together, the strong assumptions do not seem to be sufficiently wrong to account for the weak connection between excess value and announcement returns. Perhaps the simplest interpretation of the evidence is that the excess value construct does not capture very well the value consequences of corporate diversification for the companies in our study. This suggests that selection bias – the assets of diversified firms are not equivalent in value to the assets of the nondiversified firms used as benchmarks – is a significant problem, and gives additional reason to suspect the validity of the excess value construct as a measure of the value consequence of diversification.

C. Cross-Sectional Variation in Announcement Returns

Our third and final set of results attempt to identify why investors approved of the forced divestitures, guided by prominent theories that have been offered to explain diversification. The theory of the firm following Grossman and Hart (1986) calls attention to control rights; it suggests that the core difference between a diversified firm and a set of stand-alone firms is that the headquarters of the diversified firm has control rights that would otherwise be dispersed among the individual firms. On the positive side, giving control rights to headquarters can increase value by allowing winner-picking (Stein, 1997; Matsusaka and Nanda, 2002), coinsurance (Khanna and Yafeh, 2005; Jia et al., 2013), or more efficient financing (Gertner et al., 1994). On the negative side, which seems more relevant given the evidence

²⁴ All of the regressions in Table 4 are substantively similar with industry fixed effects and firm control variables.

reported so far, giving headquarters control rights can allow resources to be tunneled from the company's businesses to entities affiliated with the controlling shareholder (Johnson et al., 2000; Morck and Yeung, 2003) and may lead to inefficient cross-subsidization within the group (Scharfstein and Stein, 2000; Ozbas, 2005).

To assess the importance of tunneling, we begin with a variable that is commonly used in the literature to capture the controller's incentive to tunnel: the fraction of cash flow rights held by the company that ultimately controls the company that is forced to refocus (Claessens et al., 2002; Lemmon and Lins, 2003; Jiang et al., 2010). This variable, which ranges from .06 to .71, represents the wedge between control and cash flow rights. As cash flow rights decline, the controller's incentive to capture resources through intercorporate transfers rather than dividends rises. If investors are concerned about tunneling, the forced refocusing return should be decreasing in the controller's cash flow rights.

Table 5 reports regressions that allow the announcement return to vary with the controller's ownership share. Each column in each panel reports select coefficient estimates from a regression in which the dependent variable is the CAR over various windows. The panel A regressions do not include the battery of control variables because of the concerns expressed above that they might be absorbing refocusing effects, while the panel B regressions include (but do not report) the control variables; as it turns out, the findings are similar with or without the control variables. All regressions include the percentage of sales that the target is forced to divest. Regressions (1), (2), (7), and (8) introduce the controller's cash flow rights, defined as the fraction of the targeted firm's cash flow that is legally owned by the ultimate parent. The key variable is the interaction between the size of the divestiture and the controller's cash flow rights. In all four regressions, the coefficient is negative and distinguishable from zero at the 5 percent level or better of statistical significance. Investors are more favorable toward forced refocusing when the controller's cash flow rights are small, suggesting that the positive overall return may be connected to fears of tunneling. To interpret the magnitudes, the coefficients in regression (1) imply that a 10 percent divestiture produces a 1.5 ($= 1.00 \times (.36 - .71) - 0.53 \times (.36 - .71) \times 10$) percent higher return if the controller's cash flow rights are 0.36 (the median) than if cash flow rights are 0.71 (the maximum).

The coefficients on the controller's cash flow rights show that investors disliked diversified groups in which there was an incentive for tunneling. We next explore whether the CAR can be tied to direct evidence of tunneling. The regressions in columns (3), (4), (9), and (10) include one possible direct measure: the amount of "other receivables" on the firm's balance sheet. Jiang et al. (2010) observe that in Chinese firms, "other receivables" often consist of intercorporate loans that have not been repaid, a

plausible indicator of tunneling from the unit. Jiang et al. (2010) show that firms with high levels of “other receivables” exhibited worse operating performance and were more likely to become distressed during the period 1996-2006. We follow Jiang et al. (2010) and construct the variable OREC by dividing other receivables with respect to the controller by total assets. Note that OREC is a characteristic of the firm that is forced to refocus, not the company to be divested. Thus, a high level of OREC should be seen as an indicator of the tunneling in the group, not as a measure of tunneling that is occurring in the business to be divested. As can be seen, the OREC coefficient is never distinguishable from zero at conventional levels of statistical significance, nor does it have a consistent sign.

The remaining columns in Table 5 include both the controller’s cash flow rights and OREC at the same time to see if this might sharpen the estimates of the OREC coefficients. The results are essentially unchanged: the CAR for forced divestitures is negatively related to the controller’s cash flow rights interacted with being forced to divest, and not reliably connected to OREC. On the face of it, this evidence suggests that unpaid intercorporate loans were not the sort of tunneling that concerned investors. However, there is a caveat: Jiang et al. (2010) note that the central government began requiring repayment of these loans in 2006, and abuses declined thereafter. It is thus possible that OREC does not capture tunneling during our time period.

Another way to measure tunneling is with an array of internal financial transactions. The China Securities Regulatory Commission – the Chinese equivalent to the U.S. Securities and Exchange Commission – requires multibusiness companies to document all transactions between business units in the group, typically referred to as “related party transactions” (RPTs) (see Jia et al. (2013) for a detailed discussion of these transactions and Fisman and Wang (2010) for an overview of basic patterns). We focus on two types of transaction: (i) intercorporate loans and loan guarantees, and (ii) other transactions. “Other transactions” include internal purchases of goods and assets, leases, and the formation of joint ventures. Although “other transactions” involve transfers of real goods, they can have a significant financial component; if the price of a transfer is set above or below the market price, then the transaction redistributes cash from one unit to another. RPTs can be interpreted as a measure of the activity level of internal markets for capital and goods. As Table 1 shows, RPTs are nontrivial for sample firms, averaging 17.8 percent of assets for companies forced to divest hotel assets and 13.7 percent of assets for companies forced to divest real estate assets. Although RPTs provide a detailed record of internal capital transfers, their interpretation is not clear cut: while some studies consider RPTs as *prima facie* evidence of tunneling (e.g. Cheung et al. (2006)), other evidence indicates that these internal transfers are used efficiently to buffer group companies from economic shocks (Jia et al., 2013). The

simple correlation between the controller's cash flow rights and total RPTs is -0.16, suggesting that groups with a suspect controller use more RPTs but the connection is modest. Our approach here is somewhat agnostic: we include the RPT variables in regressions and let the data tell us how investors viewed these transactions.

Table 6 includes regressions that allow the announcement returns to vary with the volume of RPTs. As with the previous table, each column is a regression, and the panels differ in that panel A does not and panel B does include (unreported) control variables. Regressions (1), (2), (5), and (6), which do not include the controller's cash flow rights, provide weak and somewhat inconsistent evidence for a connection between CARs and RPTs. The coefficients on the interaction terms are positive in six of eight cases, but statistically different from zero only for loans over the [-1,6] window. On the face of it, the coefficient of 1.41 on the interaction term in regression (2) implies that investors favored forced refocusing when related party loans and loan guarantees were large. This would be consistent with the view of RPT loans and loan guarantees as a form of tunneling. Regressions (3), (4), (7), and (8) introduce the controller's cash flow rights. Adding this variable does not materially change the RPT coefficients, although they slightly decline in magnitude. The interaction term on the controller's cash flow rights remains negative and different from zero at conventional levels of statistical significance. The main message remains the same: there is some weak evidence that investors dislike RPTs, but the pattern is not robust. The strongest predictor of investor reaction to forced refocusing is the cash flow rights of the controller.

5. Discussion and Conclusion

This paper reports what we believe is the first quasi-experimental evidence on how corporate diversification affects firms value. Unlike research based on the excess value construct, our estimates do not rely on hypotheticals about how the diversified firm would be valued if it was refocused; we directly observe the value consequences of refocusing. Unlike research based on voluntary divestitures and refocusing, our estimates are not restricted to a sample of firms that choose to refocus; in our quasi-experiment, all firms in the target industries were forced to refocus.²⁵

²⁵ A paper close in spirit to ours is Natividad and Rawley (2013). They study the productivity of Peruvian fishing ships after the government banned mackerel fishing, forcing ships that previously fished for both anchovies and mackerel to "refocus" and specialize in anchovies. The paper finds that productivity declined in response to this form of forced refocusing. As the authors note, diversification in their industry operated primarily by providing scope economies, unlike the companies we study in which diversification operates through intercorporate resource transfers.

Three main findings emerge: First, companies that were forced to refocus experienced positive abnormal announcement returns of 1 to 2 percent, with returns of 2.9 to 4 percent for large divestitures. These estimates suggest that investors viewed diversification as a value-destroying strategy in the business groups we study, but the magnitude of the effect is not enormous. Second, there is essentially no connection between the forced refocusing return and a firm's estimated excess value or so-called "diversification discount;" this reinforces a growing body of research questioning the validity of the excess value construct. Third, the market's reaction to forced refocusing was most positive for firms that had a controlling shareholder with limited cash flow rights, suggesting that investors were concerned with the possibility of tunneling.

At first glance, our findings that investors view internal transactions in business groups as value destroying might seem to be at odds with a healthy body of research showing that diversified firms internally allocate some of their funds in a manner that is consistent with economic efficiency; for example, Maksimovic and Phillips (2002) show that investment patterns in American corporations are consistent with value maximizing principles, and Jia et al. (2013) show that Chinese business groups move resources within the group to buffer group members from economic shocks. Our findings are not necessarily inconsistent with the idea that some internal capital allocation follows optimal investment principles, or with the idea that diversified groups serve to bridge gaps in external markets; rather it suggests that internal transfers are also being used for value destroying purposes, and that investors view the downsides as more worrisome than the upsides, at least for the particular firms we study. If we bring together the different findings in the literature, the picture that emerges is that the internal markets of these business groups serve some valuable economic purposes *and at the same time* allow controlling shareholders to capture or dissipate some corporate resources. It may that the literature's focus on whether internal capital markets are efficient or inefficient should be reframed in terms of understanding when internal allocation helps and when it hurts, with the understanding that both efficient and inefficient transactions can exist inside the same company.

While our quasi-experiment allows estimates of the value of diversification that overcome important limitations in the existing literature, the quasi-experiment has its own limitations. The sample contains only 50 firms that are forced to refocus, which restricts the statistical power of some estimates. The firms are all Chinese businesses that are ultimately controlled by the central government. Whether our findings would extend to non-Chinese firms or to firms not controlled by the central government is unknown.

Regardless of whether the results can be generalized beyond the Chinese context, we believe the findings are relevant for understanding business groups in emerging economies, an important economic phenomenon on its own right with the growing importance of China, India, Brazil, and other emerging economies (Khanna and Yafeh, 2007). A healthy debate runs through the literature concerning the economic function of such business groups. One view is that these groups serve to internalize a variety of transactions due to institutional and legal gaps in external markets. Evidence in Allen et al. (2005) and other studies shows that market institutions are underdeveloped in China, creating opportunities for business groups to add value through internal transactions. The contrasting view is that the internal transactions of business groups lend themselves to wealth transfers between controlling and non-controlling shareholders and investment distortions. Our evidence suggests that despite the opportunity for value gains through internal transactions, the Chinese companies in our sample were using their internal markets inefficiently on net, and reducing corporate value as a consequence.

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Figure 1. Excess Value and Announcement Returns

Figure 2. Excess Value and Announcement Returns for Companies with Only Two Segments

Table 1. Summary Statistics

	Companies Forced to Divest Hotels	Companies Forced to Divest Real Estate	Companies Not Forced to Divest
%Divested	1.53 (2.24) [0.68]	9.33 (11.51) [2.55]	...
Assets, log	23.18 (2.10) [23.53]	23.24 (2.11) [23.40]	22.37 (1.63) [22.04]
Market/Book	2.09 (1.30) [1.56]	2.07 (1.34) [1.49]	2.50 (1.25) [2.16]
ROA	0.031 (0.036) [0.031]	0.045 (0.051) [0.031]	0.030 (0.099) [0.030]
Cash/Assets	0.111 (0.124) [0.075]	0.167 (0.136) [0.141]	0.183 (0.154) [0.147]
CAPEX/Assets	0.069 (0.082) [0.048]	0.051 (0.039) [0.039]	0.057 (0.078) [0.040]
Excess Value	-0.035 (0.443) [-0.131]	-0.133 (0.419) [-0.269]	0.002 (0.474) [-0.115]
Controller's CF right	0.374 (0.178) [0.357]	0.385 (0.160) [0.355]	0.392 (0.172) [0.400]
Other Receivables/Assets (OREC)	0.003 (0.007) [0.0001]	0.006 (0.016) [0.0002]	0.005 (0.032) [0.0001]
RPT _{loans}	0.102 (0.160) [0.031]	0.056 (0.133) [0.000]	0.101 (0.153) [0.028]
RPT _{other}	0.076 (0.135) [0.018]	0.081 (0.160) [0.013]	0.160 (0.304) [0.051]
Observations	23	27	217

Note. The main entry is the mean; standard deviation is in round parentheses; and the median is in square brackets. Market/book is based on 22 observations in the forced hotel sample and 207 observations in the not forced sample. CAPEX is based on 216 observations in the not forced sample. RPT variables are expressed as a fraction of assets. All companies are CSOEs.

Table 2. Cumulative Abnormal Return Associated with Announcement of Forced Divestitures

	CAR	N	Mean		Median		% Positive		Min	Max
			Value	t	Value	z	Value	z		
(1) All companies required to divest	[-1,0]	50	0.2	0.4	0.1	0.4	54.0	0.6	-5.2	9.5
	[-1,1]	50	1.0	2.5**	0.5	2.4**	64.0	2.0**	-5.5	11.4
	[-1,2]	50	1.4	2.9***	0.6	2.7***	64.0	2.0**	-3.8	12.7
	[-1,3]	50	1.5	3.2***	1.2	2.6***	66.0	2.3**	-3.4	13.3
	[-1,4]	50	1.6	2.8***	0.9	2.3**	66.0	2.3**	-4.2	18.4
	[-1,5]	50	1.3	2.2**	0.6	1.8*	60.0	1.4	-5.1	15.5
	[-1,6]	50	2.0	2.8***	2.1	2.6***	60.0	1.4	-5.5	23.5
	[-1,7]	50	2.0	2.8***	2.2	2.4**	64.0	2.0**	-5.6	17.5
(2) Companies required to divest assets providing more than 4 percent of sales	[-1,1]	15	2.9	3.2***	2.0	3.1***	86.7	2.8***	-0.6	11.4
	[-1,6]	15	4.0	2.3**	3.2	2.4**	73.3	1.8*	-5.5	23.5
(3) Companies required to divest hotel assets	[-1,1]	23	1.2	1.5	0.4	1.4	65.2	1.5	-5.5	11.4
	[-1,6]	23	2.5	1.9*	2.1	1.8*	65.2	1.5	-5.5	23.5
(4) Companies required to divest real estate assets	[-1,1]	27	0.9	2.4**	0.5	2.0**	63.0	1.3	-3.0	4.5
	[-1,6]	27	1.5	2.2**	0.8	1.9*	55.6	0.6	-4.9	9.6
(5) CSOs not required to divest	[-1,1]	432	0.3	1.6	0.2	1.9*	53.1	1.3	-9.0	11.4
	[-1,6]	431	0.0	0.1	-0.7	1.5	44.6	2.3**	-12.8	23.6

Notes. Cumulative abnormal returns are expressed as percentages. The event date for the forced divestiture announcement was January 26, 2010 for hotel assets and March 18, 2010 for real estate assets. Significance levels are indicated: * = 10 percent, ** = 5 percent, *** = 1 percent.

Table 3. Regressions of CARs on Forced Divestitures

	Full Sample				Hotel Event		Real Estate Event	
	[-1,1]	[-1,6]	[-1,1]	[-1,6]	[-1,1]	[-1,6]	[-1,1]	[-1,6]
Dummy = 1 if forced to divest	0.92 [*] (0.51)	1.70 [*] (0.93)	...	...	...	...	...	...
%Divested	...	...	0.07 [*] (0.04)	0.16 ^{**} (0.08)	1.09 ^{***} (0.21)	1.50 ^{**} (0.76)	0.06 [*] (0.03)	0.13 ^{***} (0.05)
Assets, log	0.08 (0.12)	-0.32 (0.20)	0.10 (0.12)	-0.28 (0.20)	0.25 (0.22)	-0.001 (0.41)	0.06 (0.16)	-0.29 (0.26)
Market/Book	-0.004 (0.17)	-0.65 ^{**} (0.30)	0.00 (0.17)	-0.63 ^{**} (0.26)	-0.16 (0.29)	-0.96 ^{**} (0.44)	0.25 (0.23)	-0.08 (0.37)
ROA	7.82 ^{***} (2.83)	8.10 ^{**} (3.95)	8.03 ^{***} (2.84)	8.42 ^{**} (3.93)	14.68 ^{***} (5.01)	13.57 ^{**} (6.89)	0.90 (2.77)	1.93 (5.25)
Cash/Assets	-0.52 (1.27)	2.39 (2.47)	-0.65 (1.28)	2.18 (2.45)	1.89 (2.47)	7.54 [*] (4.35)	-2.66 ^{**} (1.17)	-2.43 (2.67)
CAPEX/Assets	1.18 (2.28)	3.62 (3.37)	0.90 (2.20)	3.10 (3.24)	0.35 (3.93)	-0.34 (6.17)	1.37 (2.57)	6.91 (4.50)
Dummy = 1 for real estate event	0.10 (0.33)	-0.60 (0.59)	0.06 (0.33)	-0.69 (0.59)	...	...	...	...
Constant	-1.86 (2.92)	8.37 [*] (4.87)	-2.20 (2.95)	7.63 (4.91)	-6.43 (5.39)	0.79 (9.77)	-1.02 (3.79)	6.77 (6.36)
R ²	.120	.088	.120	.090	.289	.245	.211	.213
Observations	465	464	465	464	234	234	231	230

Note. Each column reports a regression in which the dependent variable is the cumulative abnormal return, expressed as a percentage. The size of the event window and sample are indicated at the top of each column. Each regression includes 2-digit industry fixed effects. Standard errors (clustered at the firm level in the full sample) are reported in parentheses beneath the coefficient estimates. Significance levels are indicated: * = 10 percent, ** = 5 percent, *** = 1 percent.

Table 4. Excess Value (Diversification Discount) and Announcement Returns

	All Firms Divesting Assets		Firms Divesting Hotel Assets	Firms Divesting Real Estate Assets
	CAR [-1,1]	CAR [-1,6]	CAR [-1,1]	CAR [-1,1]
	(1)	(2)	(3)	(4)
Panel A				
Excess Value%, Mean	-8.8		-3.5	-13.3
[Median]	[-21.8]		[-13.11]	[-26.9]
Panel B. Regression $CAR[-1,1]\% = a + bEV\% + e$				
Constant	1.04** (0.46)	2.22*** (0.77)	1.51* (0.85)	0.63 (0.40)
Excess Value%	0.002 (0.013)	0.018 (0.021)	0.029 (0.027)	-0.016 (0.010)
R ²	.0003	.015	.053	.085
Panel C. Regression $CAR[-1,1]\% = a + bEV\% + cEV\% \times \%Divested + e$				
Constant	1.04** (0.46)	2.21*** (0.77)	0.61 (0.77)	0.61 (0.41)
Excess Value%	0.002 (0.015)	0.023 (0.025)	-0.032 (0.030)	-0.014 (0.013)
Excess Value% $\times$ %Divested	-0.0001 (0.0018)	-0.0011 (0.0030)	3.821*** (1.217)	-0.039 (0.123)
R ²	.0003	.018	.366	.088
Observations	50	50	23	27

Note. Excess Value is the value of the company minus the value of a sales-weighted portfolio of single-segment firms in matching industries, expressed as a percentage. For the regressions, standard errors are reported in parentheses beneath the coefficient estimates. Significance levels are indicated: * = 10 percent, ** = 5 percent, *** = 1 percent.

Table 5. Regressions of CARs on Controller Cash Right and Other Receivables

	[-1,1]	[-1,6]	[-1,1]	[-1,6]	[1,1]	[-1,6]
Panel A. Regressions without Control Variables						
	(1)	(2)	(3)	(4)	(5)	(6)
%Divested	0.26 ^{***} (0.09)	0.64 ^{***} (0.18)	0.08 [*] (0.04)	0.17 ^{**} (0.08)	0.26 ^{***} (0.09)	0.64 ^{***} (0.19)
Controller's CF right	1.00 (0.83)	0.92 (1.69)	...	...	0.98 (0.83)	0.85 (1.69)
Controller's CF right × %Divested	-0.53 ^{**} (0.22)	-1.38 ^{***} (0.47)	...	...	-0.53 ^{**} (0.22)	-1.37 ^{***} (0.47)
OREC	...	...	-2.73 (1.98)	-4.16 (4.29)	-2.50 (1.95)	-3.95 (4.30)
OREC × %Divested	...	...	1.95 (4.49)	-9.78 (12.17)	2.61 (3.65)	-8.54 (9.54)
R ²	.104	.076	.099	.067	.105	.077
Observations	482	481	482	481	482	481
Panel B. Regressions with Control Variables						
	(7)	(8)	(9)	(10)	(11)	(12)
%Divested	0.26 ^{**} (0.10)	0.58 ^{***} (0.19)	0.07 [*] (0.04)	0.17 ^{**} (0.08)	0.26 ^{**} (0.10)	0.58 ^{***} (0.20)
Controller's CF right	0.81 (0.99)	0.18 (1.60)	...	...	0.81 (1.00)	0.20 (1.61)
Controller's CF right × %Divested	-0.54 ^{**} (0.24)	-1.22 ^{**} (0.50)	...	...	-0.54 ^{**} (0.24)	-1.22 ^{**} (0.50)
OREC	...	...	-1.12 (2.78)	3.12 (4.94)	-0.92 (2.80)	2.97 (5.01)
OREC × %Divested	...	...	1.56 (6.05)	-10.27 (12.75)	2.17 (5.13)	-9.42 (10.53)
R ²	.125	.098	.120	.091	.125	.099
Observations	465	464	465	464	465	464

Note. Each column reports a regression in which the dependent variable is the cumulative abnormal return, expressed as a percentage; the event window is indicated at the top of each column. %Divested is the percentage of sales accounted for by the unit to be divested. Panel A regressions include a dummy for the real estate event and 2-digit industry fixed effects. Panel B regressions include a dummy for the real estate event, 2-digit industry fixed effects, log of assets, market to book, ROA, cash as a fraction of assets, and CAPEX as a fraction of assets. Standard errors clustered at the firm level are reported in parentheses beneath the coefficient estimates. Significance levels are indicated: * = 10 percent, ** = 5 percent, *** = 1 percent.

Table 6. Regressions of CARs on Related Party Transactions (RPTs)

	[-1,1]	[-1,6]	[1,1]	[-1,6]
Panel A. Regressions without Control Variables				
	(1)	(2)	(3)	(4)
%Divested	0.01 (0.04)	0.04 (0.11)	0.14* (0.08)	0.56*** (0.11)
RPT _{loans}	-2.16* (1.11)	0.11 (1.98)	-2.10* (1.11)	0.22 (1.98)
RPT _{loans} × %Divested	0.40 (0.42)	1.41* (0.78)	0.31 (0.40)	1.05 (0.66)
RPT _{other}	-0.29 (0.39)	-0.65 (0.65)	-0.25 (0.39)	-0.58 (0.65)
RPT _{other} × %Divested	0.72 (0.89)	-0.64 (0.85)	0.62 (0.92)	-1.19 (0.93)
Controller's CF right	...	...	0.90 (0.83)	0.67 (1.70)
Controller's CF right × %Divested	...	...	-0.33* (0.18)	-1.34*** (0.33)
R ²	.113	.074	.115	.083
Observations	482	481	482	481
Panel B. Regressions with Control Variables				
	(5)	(6)	(7)	(8)
%Divested	-0.01 (0.04)	0.01 (0.10)	0.13 (0.08)	0.46*** (0.12)
RPT _{loans}	-1.07 (1.14)	1.64 (1.84)	-1.03 (1.15)	1.71 (1.85)
RPT _{loans} × %Divested	0.49 (0.44)	1.58** (0.72)	0.39 (0.41)	1.29** (0.63)
RPT _{other}	-0.52 (0.44)	-0.48 (0.66)	-0.49 (0.44)	-0.48 (0.67)
RPT _{other} × %Divested	0.70 (0.93)	-0.77 (0.84)	0.57 (0.95)	-1.26 (0.91)
Controller's CF right	...	...	0.65 (1.01)	-0.12 (1.61)
Controller's CF right × %Divested	...	...	-0.34* (0.18)	-1.14*** (0.33)
R ²	.130	.102	.132	.108
Observations	465	464	465	464

Note. Each column reports a regression in which the dependent variable is the cumulative abnormal return, expressed as a percentage; the event window is indicated at the top of each column. %Divested is the percentage of sales accounted for by the unit to be divested. Panel A regressions include a dummy for the real estate event and 2-digit industry fixed effects. Panel B regressions include a dummy for the real estate event, 2-digit industry fixed effects, log of assets, market to book, ROA, cash as a fraction of assets, and CAPEX as a fraction of assets. Standard errors clustered at the firm level are reported in parentheses beneath the coefficient estimates. Significance levels are indicated: * = 10 percent, ** = 5 percent, *** = 1 percent