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Social learning and corporate peer effects ☆

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ABSTRACT

We find that firms are more likely to split their stock if their peer firms have recently done so. The effect is comparable to an increase of 40–50% in the share price. Splitting probability is also increasing in the announcement returns of peer splits. These results are consistent with social learning from peers' actions and outcomes. The unique features of the setting and various further tests render alternative explanations unlikely. We find no clear benefit in following successful peer splitters. Firms are sometimes suspected to succumb to imitation, and the effect we show could be a case in point.

1. Introduction

Peer effects are a subject of increasing attention in many areas of economics and finance.[1] Peer influence is interesting as it can create social multiplier effects, whereby a small initial shock can lead to larger changes as individuals are directly influenced by each other's actions. Corporate actions are a potential domain for such peer effects, as anyone having experience with corporate management knows that firms pay close attention to what their peer firms, such as competitors, are doing (see also Porter, 1980). For example, 96% of firms report utilizing peer groups to set executive pay (Bizjak, Lemmon, and Naveen, 2008).

In this paper, we ask whether a company is more likely to execute a stock split after its peer firms have done so. Splits provide a reasonably clean setting for studying corporate peer effects.[2] First, the split decision is unlikely to be related to unobservable fundamentals. While in

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[1] Empirical evidence comes from agriculture (Foster and Rosenzweig, 1995), criminal activity (Glaeser, Sacerdote, and Scheinkman, 1996), labor market (Woittiez and Kapteyn, 1998; Topa, Bayer, and Ross, 2008), use of welfare benefits (Bertrand, Luttmer, and Mullainathan, 2000), individuals' investment choices (Duflo and Saez, 2002; and Hong, Kubik, and Stein, 2004), consumption decisions (Grinblatt, Keloharju, and Ikäheimo, 2008; Cai, Chen, and Fang, 2009), and other domains.

[2] In a similar vein, some prior studies also utilize the setting provided by stock splits to investigate other broader phenomena. Ikenberry and Ramnath (2002) analyze market underreaction and self-selection in corporate news events. Baker, Greenwood, and Wurgler (2009) study firms' catering behavior. Greenwood (2009) focuses on the effect of trading restrictions on stock prices. Green and Hwang (2009) find excess co-movement of similar stocks.

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many domains peer effects can be difficult to identify due to common shocks or unobserved heterogeneity (Manski, 1993), the prospects are much brighter in the case of stock splits. This is because the strongest fundamental driver of the decision to split is the stock price, which can be directly observed. Second, it is very rare that a firm would face a binding constraint preventing it from splitting. Such constraints are relevant with other types of corporate actions, and they are likely to be correlated across firms. Therefore, in this setting, standard panel regressions go a long way in identifying a peer effect, and we are able to rule out alternative explanations with additional analysis.

The main analysis uses a logit regression on a firm-month panel of split activity observations. The dependent variable takes the value of one if a firm has announced a split in a month. The explanatory variable is based on the number of earlier splits by peer firms. To form the peer groups, we employ a new method based on identifying common sell-side analysts between firms. Because of analysts' specialization in certain types of firms, their coverage choices directly reflect informed views on firm relatedness. Conventional industry classifications tend to produce groups that are much too large to effectively identify the set of peers subject to managers' constant attention. For example, Fama and French industries consist of firm groups that are significantly larger than the typical benchmark peer groups used in executive compensation (Faulkender and Yang, 2010; Bizjak, Lemmon, and Nguyen, 2011). More detailed classifications, such as four-digit standard industrial classification (SIC) codes, emphasize the specific nature of firms' product-market operations and might not capture other possible aspects of similarity and relatedness. A particular benefit of our method is that peer identification is based on actual links between firms. A companion paper (Kaustia and Rantala, 2013) shows that the analyst-based method outperforms conventional industry classifications in producing homogenous groups.

To remove the influence of contemporaneous common shocks, we record peer firm split activity during the 12 months prior to the current month. We include control variables related to stock price, market capitalization, past return, and the firm's recent split history. The coefficient on the peer split variable then identifies a peer effect on the propensity to split, under the assumption that other motives for executing a split are perfectly controlled for. This assumption would be violated if there were motives to split that are not captured by these controls or related to peer splits. Time-varying motives to cater to investor demand for low-priced stocks, suggested by Baker, Greenwood, and Wurgler (2009), are one possibility. To deal with this possibility, we include fixed month effects to capture all common time-varying shocks affecting the perceived desirability of a split. The baseline specification clusters standard errors by firm. The data set consists of all NYSE-listed US firms with sufficient data available, and it covers the years 1983–2009.

The main results show that firms are significantly more likely to split when their peers have recently done so. Based on regression coefficients, a peer split dummy has the same effect size as a 45% stock return over the previous year does, clearly an economically significant magnitude. This result is robust in a number of different specifications, including, but not limited to, models with time-varying catering incentives, models addressing general time-varying firm- or industry-specific shocks, fixed effects based on various conventional industry classifications, placebo regressions, and within two subsamples dividing the time period in half. We also address group-specific shocks to benefits of splitting. Although the mechanism by which splits add value is not completely understood, tangible benefits should be associated with higher future market values.[3] Corporate managers also often mention improving stock liquidity as a motive (Baker and Gallagher, 1980). Thus, adding future stock returns and liquidity as peer group-level controls should drive out the effect of the peer split dummy if it was merely proxying for such effects. But this is not what happens. The results from these specifications are similar to the baseline.

A scenario that could undermine this identification strategy is time-varying peer group-specific shocks to unobservable benefits of splitting, i.e., unrelated to future market values and liquidity, common time effects, and other controls, that would cause peer firms to split, but at different times and independent of each other's actions. We address this alternative explanation by instrumenting the peers' splitting activity by a variable that records the percentage of peers trading above their past firm-specific split prices. This aggregates firm-specific information on past nominal prices and split actions in a manner that strongly predicts peer group splits and is sufficiently exogenous for our purposes. In contrast, merely having a high nominal price (i.e., without considering the firms' idiosyncratic split histories) does not predict peer group splits. The instrument does not suffer from a weak instrument problem and satisfies the exclusion restriction of affecting firm *i*'s likelihood of splitting only through its effect on firm *i*'s peers' tendency to split. A significant peer effect comes through in these instrumental variables (IV) regressions as well, giving credence to a causal interpretation of the effect.

Our second set of results concerns the effect of peer firms' split announcement returns on the tendency to split. The benefit of this analysis is that it can provide additional information on the nature of the peer effect shown in the main results. If the nature of social interaction involves observational learning from peers' outcomes, one would expect that firms are particularly likely to follow suit and split when their peers have done so with a favorable impact on their stock price. Consistent with this idea, we find that recent peer splits with positive average announcement returns increase the propensity to split twice as strongly as peer splits with negative average announcement returns do.

The results so far are best characterized by social learning, and they are hard to reconcile with alternative stories based on correlated effects or unobserved

[3] The standard explanations are signaling (Brennan and Copeland, 1988; Asquith, Healy, and Palepu, 1989; Ikenberry, Rankine, and Stice, 1996) and optimal trading range (Lakonishok and Lev, 1987; Angel, 1997). However, Weld, Michaely, Thaler, and Benartzi (2009) present several pieces of evidence against these hypotheses.

heterogeneity. A remaining question is what firms achieve by following peers in splitting their stock. We provide some evidence on this question. First, we find essentially a zero correlation between a firm's split announcement return with the past split announcement returns of its peer firms. Second, we find that firms that split after positive peer announcement returns do not themselves enjoy greater than average returns. So firms behave as if they follow the actions of successful splitters but then fail to reap similar benefits. Finally, we show that firms are even more influenced by earlier splitters' raw announcement returns. That is, the relation is much stronger when we do not adjust for market returns. All these results are consistent with firms mistaking noise for a signal.

Are peer-mimicking stock splits unnecessary? Examples certainly exist of companies with high unit stock prices that seem to be doing just fine. For example, Apple and Google have traded in the $700–$900 range without splitting, and an extreme, yet classical, example is Berkshire Hathaway with a stock price in excess of $200,000. An aggressive interpretation is that stock splits are not only unnecessary, but also could even be harmful because splits do come with nontrivial costs, such as direct administrative costs and increased trading costs to investors (Weld, Michaely, Thaler, and Benartzi, 2009). A more conservative interpretation is that while following peers does not appear to have direct benefits, not doing so could lead to some drawbacks. For example, by not splitting when everyone else is, a firm could lose a portion of retail investor trading volume in the long run. This more conservative interpretation acknowledges the fact that we are not able to observe the long-term effects of a counterfactual scenario when a firm wanted to split after peers but did not. At a minimum, our results suggest that firms process peer information in a biased manner. The fact that peer returns unadjusted for market returns influence behavior more suggests that managers over-attribute an effect (change in market value) to a cause (split), a common bias of intuitive thinking in stochastic environments [see Kahneman (2011) for a review].

This paper links to research in the following areas. First, it is related to an emerging literature on peer effects in corporate decisions. Bouwman (2011) finds that firms with shared directors have similar corporate governance practices. Shue (2013) finds that firms tend to have more similar compensation and acquisition behavior if their chief executive officers were students in the same MBA section. Unlike these papers, our shared analyst method is likely to identify peer firms that are direct competitors and peers in a strategic sense. In our setting, a peer effect is hence more likely due to managers watching what other similar firms do, rather than due to social ties between directors and managers of noncompeting firms. Leary and Roberts (2014) find evidence of peer effects in capital structure decisions among industry peers. Identifying peer effects in corporate finance choices with unobserved fundamental drivers (such as investment opportunities in capital structure decisions) is challenging, despite clever identification strategies, due to the difficulty of teasing out the peer effects from shocks to common fundamentals. Stock splits, meanwhile, offer a clean setting for studying peer effects, as a split is a decision that firms can make at any time, and the split decision is unlikely to be related to unobservable fundamentals.

Finally, and on a more general level, the paper is related to outcome-based social learning, i.e., being affected by others' outcomes in addition to just their actions. Despite its key role in economic theories of social learning (Ellison and Fudenberg, 1995; McFadden and Train, 1996; Persons and Warther, 1997 and others), only a handful of empirical studies, in the fields of agricultural and development economics, address outcome-based social learning (Munshi, 2004; Kremer and Miguel, 2007; Conley and Udry, 2010). In the field of finance, the only prior application is Kaustia and Knüpfer (2012), who find that new investors are more likely to enter the stock market after their neighbors have enjoyed above average portfolio returns. We contribute to this literature by showing outcome-based learning among corporations. In contrast to much more deterministic settings of prior studies, such as agriculture, we show that mimicking peers after observing good peer outcomes might not have clear benefits.

The rest of the paper is organized as follows. Section 2 describes the data and the method for forming the analyst-based peer groups. Section 3 discusses identification issues and econometric methods. Sections 4 and 5 present the empirical results, and Section 6 concludes.

2. Data and analyst-based peer groups

Stock price, stock split, and firm data are from The Center for Research in Security Prices (CRSP) and Compustat, and analyst data are from the Institutional Brokers' Estimate System (I/B/E/S) Detail database. The sample consists of all US-based firms (CRSP share code 10 or 11) listed in the NYSE and covers years 1983–2009. Prior to 1983, the limited availability of analyst data significantly reduces the number of firms for which we can form an analyst-based peer group. Berkshire Hathaway Inc. is excluded from the sample because of its exceptionally high share price. The data for the empirical analysis consist of a monthly panel of firm observations with 324 observation months. Stock splits are defined as events with CRSP distribution code 5523. Reverse splits and stock dividends (events with split ratio less than or equal to 1.25 to 1) are excluded.

The analyst data consist of analysts following the sample firms during the sample years. Individual analysts are identified based on the analyst code in I/B/E/S. The code is normally assigned to individual analysts but can also refer to analyst teams. We exclude codes that are associated with more than 50 different firms in a single year (the excluded year-code combinations account to less than 0.6% of all the analyst observations). An analyst is considered to follow a firm in year t if she has provided any estimates for the firm in year t.

2.1. Common analysts as a measure of firm relatedness

The peer groups that we use are based on the observation that sell-side analysis functions within brokerages are typically organized so that individual analysts cover firms in a specific industry. Statistics on the firms followed by individual analysts provide direct evidence of industry specialization (see, e.g., Mikhail, Walther, and Willis, 2004; Boni and Womack, 2006).

In addition to the industry classification-based evidence, security analyst literature offers potential institutional and incentive-based explanations for analysts' specialization. Large elite brokerage houses employ a large number of individual analysts who cover different industries, and smaller brokerage houses specialize in covering specific industries or types of stock (Hong and Kubik, 2003). Analysts' personal incentives can also contribute to coverage choices that concentrate on a specific industry. One such factor can be that public analyst rankings, such as the *Institutional Investor* All-America Research Team and the *Wall Street Journal* Best on the Street survey, are based on identifying top analysts in different industry sectors. Being selected in the All-America Research Team has a significant effect on analyst compensation (Stickel, 1992; Michaely and Womack, 1999; Hong, Kubik, and Solomon, 2000). Findings of Boni and Womack (2006) suggest that analysts create value in their recommendations mainly through their ability to rank stocks within industries, which indicates that industry-specific human capital can also contribute to the observed industry specialization.

Analysts actively adapt their coverage according to changes in the firm composition of an industry. Das, Guo, and Zhang (2006) report that the number of analysts providing initial coverage for an initial public offering firm is significantly correlated with the number of analysts covering seasoned firms in its industry. Gilson, Healy, Noe, and Palepu (2001) report similar findings for conglomerate breakup. Former conglomerate subsidiaries experience a significant increase in coverage by analysts who cover firms in their industry. These results show that analysts' coverage choices react to changes in industry composition, which is important for the accuracy of the analyst-based peer groups.

A companion paper (Kaustia and Rantala, 2013) shows that the common analyst-based classification method outperforms conventional industry classifications in producing homogenous peer groups based on a number of test variables, such as stock return, beta, firm size, and market-to-book. The method outperforms both broader and more detailed classifications, including classification levels with comparable group size.

2.2. *Method for forming analyst-based peer groups*

Although analysts generally cover firms belonging to the same industry, the fact that two firms are followed by the same analysts might not as such be a sufficient indication of their relatedness. Not all analysts focus on following similar firms and, therefore, unrelated firms sometimes could have common analysts just by chance. For any firm, the probability of sharing an analyst with another random firm is thus increasing in both the number of analysts following it and the number of other firms followed by each of those analysts.

We set a minimum number of analysts for each firm i that it must share with another firm to include this other firm in firm i's peer group. This criterion is calculated based on a simulation taking the number of analysts following firm i in year t, as well as the number of other firms followed by each such analyst as inputs. In the simulation, these analysts following firm i then counterfactually choose the other firms they follow at random, from all NYSE firms covered by analysts in year t. The simulation is repeated one thousand times, and the criterion C for each firm is selected so that the probability of having more than C common analysts by chance is less than 1%. That is, we set C sufficiently large so as not to assign unrelated firms as peers. The simulation is run separately for all firms in each sample year, and the peer groups are updated annually.

The analyst-based peer group method and the analyst data have certain implications on the characteristics of the peer groups. First, the method does not provide peer groups for about 30% of the firms, as some firms do not have sufficient analyst coverage in the I/B/E/S Detail database. As a result of the method, the smallest possible peer criterion is two analysts, so firms with fewer than two analysts can never have a peer group. Second, peer relations are not always mutual, i.e., it is possible that firm A is firm B's peer, but B is not A's peer.[4] Non-mutual peer relations can be intuitively justified in situations in which we are trying to find the closest firm-specific comparables among a group of related firms. For example, suppose that firms A, B, and C are the only firms operating in a specific industry, A and B are large firms, and C is small. A and B are likely to be each others' best comparables because of their similar size. The best comparable for firm C can also be either firm A or firm B from the same industry or another smaller firm from a related industry. In case it is A or B, C would have a non-mutual comparable firm relation with A or B.

2.3. *Statistics on sample firms and characteristics of the analyst-based peer groups*

Table 1 shows statistics on sample firms, sample analysts, and analyst-based peer groups. In an average year, there are 1,501 firms and 2,077 analysts, and over two-thirds of the firms have an analyst-based peer group. The number of analysts is significantly smaller in the 1980s, but over 60% of the firms have an analyst-based peer group in each of the sample years. The size of the peer groups remains relatively stable over the years, although the groups are on average slightly larger during the first half of the sample period. The average annual group size ranges from 14.0 in 1987 to 9.5 in 2002.

[4] For example, suppose that firms A and B have some common analysts and firm A is followed by more analysts than firm B. As a result of the higher analyst coverage, the simulation-based minimum analyst criterion (the number of common analysts required for a peer relation) can be higher for firm A than for firm B. A non-mutual peer relation results if the number of common analysts falls between the minimum criteria for A and B. Suppose that A's analyst criterion is five, B's criterion is three, and the firms have four common analysts. Because B has a lower analyst criterion, A makes it to B's peer group, but B does not make it to A's peer group.

Table 1
Descriptive statistics of the sample and analyst-based peer groups.

This table reports statistics on sample firms, sample analysts, and analyst-based peer groups. The annual number of NYSE firms is based on firms that have Center for Research in Security Prices (CRSP) share code 10 or 11, exchange code 1, and item PRC at the end of the year. The statistics are based on annual observations between 1983 and 2009. The sample analysts are analysts that have provided at least one estimate for a sample firm during the year, and individual analysts are separated based on analyst code item in the Institutional Brokers' Estimate System (I/B/E/S) Detail database. Stock splits are defined as events with CRSP distribution code 5523 and a split ratio higher than 1.25 to 1. The split announcement statistics are based on the declaration date (DCLRDT) item in CRSP. The analyst-based peer groups are formed using a simulated peer criterion. A firm's peer group in year *t* consists of all firms that are followed by at least the criterion number of same analysts in year *t*. The criterion is calculated as the number of analysts a firm shares with another firm with a probability that is smaller than 1% in a simulation in which the firm's analysts choose the other firms they follow randomly among NYSE firms that have analysts. Panels B and C report comparative statistics between the analyst-based peer groups and Fama and French (49), three-digit Standard Industrial Classification (SIC), and six-digit Global Industry Classification Standard (GICS) industry groups. The group size of a firm is calculated based on the number of other firms in the analyst-based peer group or industry group (i.e., it does not include the firm itself), and groups of size zero are not included in the statistics. For the industry classification groups, the size is calculated based on the number of other NYSE firms sharing the same classification code in year *t*. Other statistics include the average number of different classification codes in the peer groups and the average number of peer firms sharing the firm's own classification code. SIC codes are from Compustat (if available) and otherwise from CRSP. GICS codes are from Compustat. The statistics for GICS codes are based on observations from the subperiod for which they are available in Compustat (years 1999–2009).

Panel A: Annual sample statistics

Statistic	Sample NYSE firms	NYSE firms with peers	Analysts per year	Announced splits	Splits by firms with peers	Average peer group size	Median peer group size
Annual average	1,500.8	1,032.7	2,076.6	147.8	82.4	11.8	10.4
Lowest annual value	1,300	848	1,209	4	2	10	9
Highest annual value	1,886	1,278	2,711	354	181	14	13

Panel B: Group size statistics

Statistic	Analyst-based peer groups	Fama and French industries	Three-digit SIC codes	Six-digit GICS codes
Average group size	11.7	54.9	15.8	23.5
5th percentile	1	9	2	2
25th percentile	5	27	5	8
Median	10	44	10	15
75th percentile	16	81	22	34
95th percentile	30	139	48	65
Standard deviation	8.9	36.9	14.9	20.6

Panel C: Distribution of industry codes within analyst-based peer groups

Statistics	Fama and French industries	Three-digit SIC codes	Six-digit GICS codes
Average number of different industry codes within the analyst-based peer group	2.32	3.69	1.74
Average percentage of peers sharing the firm's own industry code	64.80	41.49	72.84

The annual number of split announcements varies significantly over time, ranging from 354 announcements by NYSE firms in 1983 to four in 2009. In an average sample year, 8.0% of the NYSE firms split their stock. The corresponding percentage for firms with an analyst-based peer group is 9.9%, so firms with analyst-based peers are somewhat more likely to split than average NYSE firms. The percentage of splits that are by firms with analyst-based peers is higher in the later years of the sample and ranges from 34% in 1984 to 92% in 2008.

Table 1 also compares the analyst-based peer groups to traditional industry classifications by Fama and French (49), three-digit SIC, and six-digit Global Industry Classification Standard (GICS), the most recent and improved classification method developed by MSCI Inc. and Standard & Poor's in 1999. The average size of an analyst-based peer group is 11.7 firms, and it is 54.9 for the Fama and French industries, 15.8 for three-digit SIC codes, and 23.5 for six-digit GICS-codes. These figures are calculated from all firm-year observations in the sample so that the group size of a firm in year *t* measures the number of peers (i.e., other firms, so excluding the firm itself) in the firm's analyst-based peer group or industry group in year *t*. Groups of size zero are excluded. The interquartile range for size in the analyst-based peer groups is 5–16. Group sizes vary more for the traditional classification systems. The interquartile ranges are 27–81 for the Fama and French industries, 5–22 for the three-digit SIC groups, and 8–34 for six-digit GICS codes. The Bizjak, Lemmon, and Nguyen (2011) study of compensation peer groups finds that firms use relatively small peer groups. Average group size is 16.4. On average, 63% of the firms in a

Table 2
Firms with analyst-based peers compared with all NYSE firms.

This table reports comparative statistics between firms with analyst-based peers and all NYSE firms. The analyst-based peer groups are formed based on a simulated peer criterion. The criterion is calculated as the number of analysts a firm shares with another firm with a probability that is smaller than 1% in a simulation in which the firm's analysts choose the other firms they follow randomly among NYSE firms that have analysts. A firm's peer group in year t consists of all firms that are followed by at least the criterion number of same analysts in year t. Panel A compares firms with analyst-based peers to all NYSE firms based on average and median market capitalization, book equity, and market-to-book. Market capitalization is calculated as Center for Research in Security Prices (CRSP) item PRC at the end of the year times shares outstanding. Shares outstanding are from Compustat (if available) and otherwise from CRSP. Book equity and market-to-book ratio are based on Compustat data and are calculated as in Fama and French (1993). Market capitalization and book equity values are in millions of dollars. Panel B shows the percentage of firms with analyst-based peers in different market capitalization quartiles. All statistics are based on annual observations of firms between 1983 and 2009.

Panel A: Firms with analyst-based peers compared with all NYSE firms based on firm size and market-to-book

Firms	Market capitalization		Book equity		Market-to-book	
	Average	Median	Average	Median	Average	Median
All NYSE firms	4,333	839	1,983	465	2.55	1.63
Firms with analyst-based peers	6,109	1,504	2,696	815	2.76	1.74

Panel B: Percentage of firms with analyst-based peers in different market capitalization quartiles

Quartile 1	Quartile 2	Quartile 3	Quartile 4
27.9	58.0	79.5	93.4

compensation peer group are in the same Fama and French (49) industry. The number of different industry codes represented in the average analyst-based peer group is 2.3 when using Fama and French classifications and is 3.7 with three-digit SIC codes. The six-digit GICS aligns well with the analyst-based groups. In the average analyst-based group, firms come from 1.7 different GICS codes, and the median number is one. On average, 72.8% of a firm's analyst-based peers have the same six-digit GICS code.

Table 2 compares firms with analyst-based peers to all NYSE firms. As expected, the statistics show that firms with analyst-based peers are larger in terms of market capitalization and book equity, and have slightly higher market-to-book ratios. The average market capitalization in the sample is \$4.3 billion for all NYSE firms and \$6.1 billion for firms with analyst-based peers, and the average book equity of firms with analyst-based peer is also 36% higher than the NYSE average.

The larger size of firms with analyst-based peers can be partially attributed to the fact that larger firms have more analyst coverage. In the highest market capitalization quartile, 93% of the NYSE firms have an analyst-based peer group, and the same number for the lowest quartile is 28%. A potential additional implication of the firm size statistics is that analyst-based peers are more similar in terms of firm size compared with the comparable firms suggested by conventional industry classifications.

3. Variable construction and baseline identification

Our empirical analysis focuses on two separate peer effects in stock splits. We study first whether splits by a firm's peers increase its propensity to split and, second, whether peers' split announcement returns are related to a firm's decision to split. We use firm-month panel logit regressions to study both effects. The dependent variable in the regressions is equal to one if a firm has announced a split in month t and zero otherwise. The timing of splits is based on the month of the announcement date (CRSP item DCLRDT). Standard errors in the baseline model are clustered at the firm level to control for within-firm correlation of the error term.

The first explanatory variable of interest is a dummy based on splits announced by peer firms during the previous 12 months. Significant variation exists in splitting activity over time, and firms are ex ante more likely to have splitters in their peer group when the size of the group is large and when the market-level splitting activity is high. For firms with large peer groups, the expected number of splits in the peer group within a certain 12-month period can be larger than one. We address this issue by conditioning the cutoff value for the peer split dummy on the total number of splits in the market. Specifically, we set the peer split dummy equal to one if the average number of splits announced by firm i's analyst-based peers (i.e., total number of splits by firm i's peers divided by the number of firm i's analyst-based peers) during the previous 12 months is higher than the corresponding NYSE average. The NYSE average is calculated as the total number of splits by NYSE firms during the previous 12 months divided by the number of NYSE firms in CRSP during the period. On average, firms whose peer split dummy has value one have 2.4 splits among their peers, and the median number of peer splits is two. In many cases, the potential peer effect captured by the dummy is thus not based on the observation of one, but rather on two separate peer splits in the past 12 months. For robustness, we estimate the results also using a simple version of the peer split dummy that takes the value of one whenever there are any splits among the peers. The results are qualitatively similar.

Control variables used in all the regression specifications are log of stock price (item PRC), NYSE market capitalization percentile (based on market capitalization calculated as item PRC times item SHROUT), and split-adjusted stock return over the past 12 months (change in item PRC adjusted by item CFACPR). These control variables are calculated based on the item values at the end of the previous month. Share price level and its recent development are obvious controls, and the variable for firm size is based on the positive correlation between firm size and share price found by Dyl and Elliott (2006). We also add a dummy for recent splitters, equal to one if the firm made a split during the previous 12 months. This is motivated by the empirical observation that firms rarely execute a series of splits within a short time period.

We control for time effects using two alternative approaches. The baseline model uses month fixed effects, which controls for all time-varying effects in the regressions. Such time effects can capture seasonal variation, longer-term trends related to overall stock market valuations for example, or time varying sentiment and investor demand for low-priced stocks. When using month fixed effects, individual months that do not contain any split announcements must be excluded from the regressions due to lack of variation in the dependent variable. As an alternative approach, we use a specification dubbed "seasonal controls," which consists of year dummies and dummies for the 12 calendar months. This allows obtaining results with a full sample, including months with no splits by any firm. It also allows the inclusion of aggregate time series variables. The calendar month dummies control for possible intra-year variation in splitting activity.

Some specifications use additional control variables. We include a variable to measure firms' incentive to cater to time-varying investor demand for low nominal price stocks, following Baker Greenwood, and Wurgler (2009). This variable is dubbed low-price premium, and it is defined as the log difference in the average market-to-book ratio of low nominal price and high nominal price stocks (stocks below the 30th and above the 70th percentile of prices, respectively). We calculate its values at the monthly level using value-weighted average market-to-book ratios.[5] Because this variable varies only in the time series, we cannot use month fixed effects with it. Instead, we use the seasonal controls approach, as well as another time series variable: the total number of splits during the previous 12 months scaled by dividing with the average monthly number of NYSE firms in CRSP during the period. Because the baseline model with month fixed effects already controls for any aggregate time series variation, including catering incentives, this additional specification is not designed to improve identification of the peer split effect. Rather, it provides an opportunity to learn more about time-varying catering incentives identified by Baker, Greenwood, and Wurgler (2009), while being estimated jointly with the peer effect. As a robustness check for the possibility that the average price level of peer firms is correlated with splitting activity, we also include a specification that has (log) average share price of peers as a control.

The baseline model uses month fixed effects and clusters standard errors by firm. This leads to unbiased standard errors as long as the time effect is fixed (Petersen, 2009). This assumption is violated if the splitting decisions of firms A and B have different sensitivities to a time period-specific shock; for example, if low price-catering motives become stronger for firm A than for firm B in a particular month. However, controlling for variables that affect catering motives (share price, past returns, market cap decile) is likely to go a long way in addressing this dependence. Another potential issue is timeseries dependence due to the construction of the peer split dummy based on past 12 months of observations. For example, consider a shock to splitting activity in one month in a group of firms that causes the dummy value to be one (instead of zero) in 12 consecutive observations. This can cause a time-varying firm specific shock that may lead to inflated *t*-statistics even in our econometric specification. Clustering standard errors simultaneously along time and firm dimensions is robust to both types of effects outlined above (Petersen, 2009). We do not implement this procedure in our baseline model, but unreported results show that two-way clustering has only a small effect on the standard errors.

4. Results for the effect of peer firms' splits on the propensity to split

This section first presents estimates from the baseline model. It then discusses alternative approaches, considers shocks to returns and liquidity, and assesses peer group specific effects in several ways.

4.1. Baseline model

Results from logit regressions measuring the effect of the peer split dummy on the propensity to split are reported in Table 3. The dummy is highly statistically significant and the coefficient values in the full sample regressions are between 0.35 and 0.39, depending on specification. To get a sense of the economic significance of the peer split dummy, it is instructive to compare the effect size with that produced by an increase in the company share price. The marginal effect of the peer split dummy calculated at the means of the independent variables corresponds to an effect of about a 45% stock return over the previous 12 months. Alternatively, one can look at the direct effect that the stock price level has on the probability of splitting and compare it with the peer split dummy. We do this by running a logit regression explaining splits by a set of dummy variables indicating the stock price in \$5 intervals, starting from \$15 to \$20, going up to \$60, and including a dummy for prices above \$60, as well

[5] The monthly low-price premium is based on market-to-book ratios calculated as book assets (Compustat item 6) minus book equity plus market equity all divided by book assets. Market equity is price times shares outstanding. Price is from CRSP, and shares outstanding are from Compustat (if available) or CRSP. Book equity is stockholders' equity (216) [or first available of common equity (60) plus preferred stock par value (130) or book assets (6) minus liabilities (181)] minus preferred stock liquidating value (10) [or first available of redemption value (56) or par value (130)] plus balance sheet deferred taxes and investment tax credit (35) if available and minus post-retirement assets (330) if available. Market values and share price breakpoints for low-priced and high-priced stocks are updated monthly. Book values are updated annually at the end of the year, so that the new book value at the end of year *t* is based on the fiscal year ending in year *t*.

Table 3
Peer firms' splits and the propensity to split.

Monthly panel logit regressions studying the relation between stock splits and recent splits by peer firms. The dependent variable is equal to one if a firm has split its stock in month t. Stock splits are defined as in Table 1. Peer Split Dummy takes the value of one if the number of splits by analyst-based peers during the previous 12 months is higher than the average number of splits by NYSE firms during the same period. Other variables are the logarithm of price Log(Price) [based on Center for Research in Security Prices (CRSP) item PRC], Market Capitalization Percentile (based on market capitalizations of NYSE firms calculated as item PRC times item SHROUT at the end of the previous month), Past 12 Month Return (stock return calculated as change in item PRC adjusted by item CFACPR), Recent Splitter Dummy, which is equal to one if the firm has announced a split during the previous 12 months, Low-Price Premium, which is the log difference in value-weighted average market-to-book ratios of low-priced and high-priced stocks (stocks below 30th and above 70th percentile of NYSE prices), Number of Recent NYSE Splits, which is the number of splits by NYSE firms announced within the last 12 months divided by the average monthly number of NYSE firms in CRSP during the period, and Log(Average Share Price of Peers), which is calculated as the logarithm of average share price of analyst-based peer firms at the end of the previous month. The table also reports results from subsample regressions for periods 1983–1996 and 1997–2009. The regressions include either month fixed effects or seasonal controls (year dummies and dummies for the 12 calendar months). z-Statistics based on standard errors clustered at the firm level are reported in brackets below the coefficients.

	Full sample				1983–1996	1997–2009
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Peer Split Dummy	0.352	0.353	0.355	0.385	0.332	0.358
	[7.08]	[7.15]	[7.16]	[7.66]	[5.47]	[4.19]
Log(Price)	1.875	1.870	1.870	1.913	1.953	1.78
	[8.12]	[8.02]	[8.03]	[7.94]	[9.00]	[5.35]
Market Capitalization Percentile	−0.019	−0.019	−0.019	−0.018	−0.022	−0.014
	[−5.47]	[−5.45]	[−5.44]	[−5.36]	[−7.06]	[−3.04]
Past 12 Month Return	0.803	0.763	0.763	0.803	0.986	0.630
	[9.64]	[7.17]	[7.18]	[9.66]	[13.15]	[4.67]
Recent Splitter Dummy	−0.766	−0.735	−0.738	−0.771	−0.982	−0.537
	[−6.69]	[−5.92]	[−5.92]	[−6.70]	[−6.99]	[−2.99]
Low-Price Premium			0.807			
			[2.09]			
Number of Recent NYSE Splits			0.786			
			[0.77]			
Log(Average Share Price of Peers)				−0.280		
				[−2.67]		
Month Fixed Effects	Yes	No	No	Yes	Yes	Yes
Seasonal Controls	No	Yes	Yes	No	No	No
Number of Observations	254,462	277,778	277,778	254,462	137,133	117,329
Number of Splitters	1,969	1,969	1,969	1,969	1,252	717
Pseudo R^2	0.171	0.163	0.163	0.171	0.166	0.176

as month fixed effects. As expected, the estimated dummy coefficients increase monotonically with the price range. We then look at the differences in these coefficients. For example, the coefficient for price range \$20–\$25 is 0.19 larger than the coefficient for range \$15–\$20. Thus, the peer split dummy has about twice the effect as going from price range \$15–\$20 to \$20–\$25 does. On average, the difference in the coefficients between the adjacent \$5 intervals is 0.34, a level similar to the peer split dummy. Of eight such coefficient differences, the peer split dummy is larger in magnitude in five cases. Thus, in general, the magnitude of the peer split dummy is roughly comparable to a \$5 increase in the stock price.

The baseline model in Column 1 of the table uses fixed month effects, and 29 observation months without any split announcements must be excluded. The specification in Column 2 is otherwise identical, but it replaces the fixed effects with seasonal controls (year and calendar month dummies). This produces results that are almost identical to the baseline model. We present this comparison to facilitate the interpretation of some further results derived from specifications that use this seasonal controls strategy together with pure time series variables. Such variables cannot be used together with the fixed month effects.

Column 3 includes the low-price premium variable and the variable measuring the total number of splits in the stock market. Low-price premium is positive and significant, consistent with the results of Baker, Greenwood, and Wurgler (2009). The inclusion of these variables has virtually no effect on the estimate of the peer split dummy. Column 4 adds the (log) average share price of peer firms at the end of the previous month. A negative effect of peer stock prices on the propensity to split would be consistent with the results that link industries to nominal share price levels (Lakonishok and Lev, 1987; Weld, Michaely, Thaler, and Benartzi, 2009; Baker, Greenwood, and Wurgler, 2009). Consistent with these ideas, the coefficient for average peer price is negative and significant. However, this regressor could be difficult to interpret as nominal stock prices and firm size are correlated. A lower peer price is also correlated with peer firms having recently split their stock. The increased coefficient for the peer split dummy in Column 4 may be an indication of such effects. Unreported results show that removing the peer split

Table 4
Additional results and robustness checks

For these monthly panel logit regressions, the dependent variable is equal to one if a firm has split its stock in month t. Stock splits are defined as in Table 1. Percentage of peer splitters is calculated based on the number of peer firms that split their stock during the previous 12 months. Regression-Based Peer Split Dummy takes the value of one if the number of splits in a firm's peer group during the previous 12 months exceeds the number of splits predicted by peer firms' fitted monthly splitting probabilities from the following monthly regression for all NYSE firms between 1982 and 2009:

$$Firm\ \ i\ \ splits_t = \log(P_{i,t}) + NYP_{i,t} + r_{12\ Months,i,t} + RecentSplitter_{i,t} + e_{i,t}$$

The dependent variable is equal to one if a firm has split its stock in month t, and the explanatory variables are logarithm of price $\log(P_{i,t})$, Market Capitalization Percentile $NYP_{i,t}$, Past 12 Month Return $r_{12\ Months,\ i,t}$, and Recent Splitter Dummy, which is equal to one if the firm has announced a split during the previous 12 months. The regression includes year dummies and dummies for the 12 calendar months. The explanatory variables are defined as in Table 3, and they are also included as unreported control variables in all the regressions reported in this table. Other reported variables are Peer Split Dummy (defined as in Table 3), Past Peer 12 Month Return, Future 12 Month Return, Future Peer 12 Month Return, Past Amihud, Past Peer Amihud, Future Amihud, and Future Peer Amihud. Amihud refers to the Amihud (2002) illiquidity measure calculated based on the values of the preceding 12 months. Future 12 month return and future Amihud values are calculated based on the 12 months following the observation month. Peer returns and Amihud measures are calculated as equal-weighted averages among the firm's analyst-based peers. All the regressions include month fixed effects. z-Statistics based on standard errors clustered at the firm level are reported in brackets below the coefficients.

Variable	(1)	(2)	(3)
Percentage of Peer Splitters	1.707		
	[7.61]		
Regression-Based Peer Split Dummy		0.367	
		[6.76]	
Peer Split Dummy			0.296
			[5.55]
Past Peer 12 Month Return			0.264
			[2.05]
Future 12 Month Return			0.401
			[5.94]
Future Peer 12 Month Return			−0.345
			[−2.33]
Past Amihud			−0.435
			[−0.60]
Past Peer Amihud			0.534
			[0.71]
Future Amihud			0.281
			[1.35]
Future Peer Amihud			−0.836
			[−1.20]
Price, Return, Market Capitalization, and Recent Splitter Controls	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes
Number of Observations	187,874	251,835	219,050
Pseudo R^2	0.172	0.171	0.173

dummy from the regression causes the average peer price to become statistically insignificant.

We verify that the results are not driven by observations in which the peer group consists of only a few firms. A regression that excludes observations with the lowest quartile of peer group sizes in the data (peer groups of less than five firms) produces a coefficient of 0.41 for the peer split dummy and the corresponding z-value is 6.92. Finally, the two rightmost columns in Table 3 report subsample regressions for the 1983–1996 and 1997–2009 subperiods. The peer split dummy obtains a virtually identical coefficient under both time periods, although the total number of splits in the latter period is significantly smaller.

4.2. *Alternative measures of peer firms' splitting activity and other robustness checks*

As the first alternative to the baseline model, in place of the peer split dummy we use the percentage of peer firms that have announced a split in the past 12 months. We exclude observations with fewer than five peer firms to limit the effect of very small peer groups and to manage the distributional properties of the percentage variable. Column 1 of Table 4 presents the results of a regression using this percentage-based variable. It attracts an economically and statistically significant coefficient of 1.71 (z-value of 7.61).

As the second alternative, we form a dummy variable that accounts for variation in peer firms' propensity to split. But instead of assigning the value of one when split activity in the peer group exceeds the market activity level, as in the baseline model, we condition the cutoff value of the dummy on peer firms' predicted month- and firm-specific splitting probabilities. This provides an alternative strategy for addressing time varying industry-specific shocks. To do this, we first run the following logit panel regression for all sample firms:

$$Firm\ \ i\ \ splits_t = \log(P_{i,t}) + NYP_{i,t} + r_{12\ Months,i,t} + RecentSplitter_{i,t} + e_{i,t}. \tag{1}$$

The dependent variable $Firm\ i\ splits_t$ is equal to one if firm i has split its stock in month t. The control variables log of price $\log(P_{i,t})$, NYSE market capitalization percentile $NYP_{i,t}$, 12-month return $r_{12\ Months,\ i,t}$, and the recent splitter dummy $RecentSplitter_{i,t}$ are defined as earlier. The regression also includes seasonal controls (year dummies and dummies for the 12 calendar months). Based on the estimated regression coefficients, we calculate fitted month-specific splitting probabilities for each firm. The alternative peer split dummy then takes the value of one if the number of splits in a firm's peer group during the previous 12 months is higher than predicted by this method.

If a positive industry shock causes the stock prices of peer firms to increase this is reflected in higher predicted split probabilities for these firms. Only abnormal amounts of splits cause the dummy to take the value of one. This model addresses any mechanism that works through linear effects in the included explanatory variables. The model in Eq. (1) can be correctly specified only under the null of no peer effects. If there are significant peer effects, as the results so far suggest, the model for predicting splits suffers from an omitted variable bias because it does not include the actions of peer firms as an explanatory variable. But, importantly, under the null hypothesis it has the power to refute the peer effects story by producing an insignificant coefficient for the peer split dummy.

Column 2 of Table 4 reports the results when using this alternative specification of the peer split dummy. The peer split dummy remains highly statistically significant, and the coefficient value is similar to the baseline models reported in Table 3. This provides confirming evidence that industry correlation in stock returns, nominal share prices, or firm size does not explain the peer effect on the propensity to split. We also experiment with various nonlinear share price controls when calculating the predicted number of peer splits and the results are similar.

In additional unreported robustness checks we run the baseline regressions with two-way (firm and time) clustered standard errors, using a simple version of the peer split dummy that takes the value of one whenever there are any splits among the peers, with nonlinear stock price controls composed of dummies for the level of stock price in \$5 intervals, and using a linear probability model (ordinary least squares) rather than logit. The peer split dummy remains positive and highly significant in all these alternative specifications.

4.3. Shocks to future returns and liquidity

To further consider the hypothesis that an unobserved factor operating at the level of a peer group would affect the desirability of the split at a given time we also run a regression specification with a number of additional control variables related to potential benefits of splits. To measure this effect, we add the firm's future 12-month return in the baseline regression. This should drive out the effect of the peer split dummy if group level shocks to split benefits reflected in future market value are behind our main results. We also control for peer firms' past as well as future returns. Splits could also improve the stock's liquidity. To control for possible liquidity effects, we include the firm's Amihud illiquidity measure (past and future) and the corresponding measures for its peer firms.[6] These liquidity variables should pick up any liquidity externalities not directly captured by the stock price.

Column 3 of Table 4 presents the results of this regression. The future 12 month return variable is positive and significant, consistent with firm's private information partially affecting split decisions. However, the future peer return is negative and statistically significant. This is against the idea that unobserved group level shocks reflected on market values affect splits. Most important, the peer split dummy remains strong and significant, with coefficient values similar to the baseline regressions. None of the illiquidity variables is significant.

In sum, the results discussed in this subsection suggest that unobserved factors affecting the benefits to splits are unlikely to be driving our main results. A remaining caveat is the possibility of group-level shocks to unobserved split benefits, uncorrelated with market value or liquidity.

4.4. Peer group-specific effects

In this subsection, we report on two further approaches addressing group-specific shocks. First, we account for potential group-specific persistence in splitting activity by including a control variable that measures the long-term split activity among peer firms. This variable, *Peers' 10-y Split History*, is constructed by counting the splits of peer firms during a rolling time period of past ten years and dividing by the number of firms. We skip the most recent 12 months so as not to overlap with the measurement of recent peer split activity reflected in the main explanatory variable *Peer Split Dummy* (i.e., for observation month t, *Peers' 10-y Split History* is calculated from observations during month t_{-132} to month t_{-13}). We include only firms with sufficient split histories available. For robustness, we also use alternative versions of this variable that are otherwise identical, except that, instead of ten years, we use five- and 20-year past split histories, respectively.

Columns 1–3 of Table 5 show the baseline regression augmented with the three different specifications of the split history variable. It is statistically significant in all the specifications, lending support to the idea of some group-specific persistence in splitting activity. The coefficients of the peer split dummy nevertheless remain very close to their values obtained in the baseline regressions of Table 3, with similar statistical significance.

The coefficient values of the peers' split history variable decrease with the length of the time period. This is because the average number of splits increases mechanically with the number of years used for measuring split

[6] The Amihud (2002) illiquidity measure is defined as the average of daily ratio of absolute stock return to dollar trading volume. We calculate it based on the trading days within the 12 months preceding the observation month (or in future Amihud, within the 12 months following the observation month). Following Amihud, the measure is calculated only for stocks that have at least two hundred trading days during the 12-month period, end-of-period stock price higher than \$5, and market capitalization data available at the end of the period. For each month, outliers are eliminated by excluding stocks that have a value at the highest or lowest 1% tails of the distribution.

Table 5
Controlling for industry and peer group-specific effects.

For these monthly panel logit regressions, the dependent variable is equal to one if a firm has split its stock in month *t*. Stock splits are defined as in Table 1. Peer Split Dummy is equal to one if the number of splits by analyst-based peers during the previous 12 months is higher than the average number of splits by NYSE firms during the same period and zero otherwise. Other variables in the regressions are the logarithm of price Log(Price), Market Capitalization Percentile (based on NYSE cutpoints), Past 12 Month Return, and Recent Splitter Dummy, which is equal to one if the firm has announced a split during the previous 12 months. Peers' 5-y Split History, Peers' 10-y Split History, and Peers' 20-y Split History measure the average number of splits announced by peer firms during five, ten, and 20 years, respectively, preceding the 12-month observation period that is used to calculate the peer split dummy. The table includes regressions with three different industry fixed effects specifications: Fama and French (49) industries, three-digit Standard Industrial Classification (SIC) codes and six-digit Global Industry Classification Standard (GICS) codes. The regression with GICS codes is run for the period 1999–2009, because GICS codes are not available in Compustat before the year 1999. All regressions include month fixed effects. *z*-Statistics based on standard errors clustered at the firm level are reported in brackets below the coefficients.

Variable	(1)	(2)	(3)	(4)	(5)	(6)
Peer Split Dummy	0.320	0.320	0.331	0.288	0.210	0.355
	[6.38]	[6.31]	[6.17]	[5.53]	[4.06]	[3.39]
Log(Price)	1.879	1.873	1.836	2.297	2.995	2.594
	[8.24]	[8.21]	[7.90]	[17.79]	[21.93]	[11.91]
Market Capitalization Percentile	−0.018	−0.018	−0.016	−0.023	−0.030	−0.023
	[−5.29]	[−5.30]	[−4.54]	[−10.54]	[−13.62]	[−6.01]
Past 12 Month Return	0.827	0.857	0.884	0.737	0.679	0.728
	[9.13]	[8.97]	[8.17]	[9.13]	[7.01]	[10.31]
Recent Splitter Dummy	−0.772	−0.783	−0.810	−0.867	−1.083	−0.910
	[−6.93]	[−6.87]	[−6.62]	[−7.72]	[−9.43]	[−4.37]
Peers' 5-y Split History	0.341					
	[6.66]					
Peers' 10-y Split History		0.219				
		[6.13]				
Peers' 20-y Split History			0.088			
			[2.96]			
Industry Fixed Effects	None	None	None	Fama-French	Three-Digit SIC	Six-Digit GICS
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	249,352	242,947	228,762	248,645	242,398	88,116
Number of Splitters	1,928	1,904	1,761	1,956	1,962	495
Pseudo R^2	0.172	0.171	0.170	0.186	0.213	0.221

history. The mean effects of the variable are similar in different specifications, however. The mean value for the average number of peer splits is 0.6 with five-year history, 1.2 with ten-year history, and 2.2 with 20-year history. Coefficient value multiplied by the variable's mean shows no trend. It is 0.20 with five-year history, 0.27 with ten-year history, and 0.20 with 20-year history. The economic significance of the split history variable is relatively low compared with the peer split dummy. A 2 to 3 standard deviation increase from the mean level is needed to produce an effect comparable to the peer split dummy.

The second approach utilizes traditional industry fixed effects. Because our analyst-based peer groups are year-specific and peer relations are non-transitive, we cannot directly include peer group fixed effects in our regression.[7] We thus use industry fixed effects based on traditional industry classifications, namely Fama and French 49, three-digit SIC, and six-digit GICS (see Section 2.3). There is some data loss due to dropping groups with no splits and observations with missing classification codes. This, as well as data availability (GICS is available since 1999), generates variation in *N* across methodologies. Results of industry fixed effects regressions are reported in Table 5. The peer split dummy remains statistically significant, and coefficient estimates are not too far from baseline values. Based on Table 1, the six-digit GICS classification has the greatest overlap with analyst-based peer groups (the median number of different GICS codes in an analyst-based peer group is one).

In sum, the results discussed in this subsection show that the findings are robust to controlling for persistent group-level tendencies to split, by including either a measure of peers' long-term split history or industry fixed effects.

4.5. Placebo regressions

In this subsection we run placebo regressions to study how likely it is that intra-group correlations in variables affecting the split decision would produce results similar to our main analysis. Specifically, we run a simulation in which firms execute splits randomly in proportion to their firm- and month-specific splitting probabilities, obtained

[7] In a non-transitive peer relation, A can be in B's peer group, but this does not necessarily mean that B is in A's peer group. Because of this, as well as the year-by-year changes, it is less likely that static characteristics of some groups drive the results. While that can be considered an advantage, the drawback is that traditional fixed effects models cannot be used with these peer groups.

from a regression with our standard set of control variables [see Section 4.2, Eq. (1)]. That is, we have the firms split each month with probability p, which represents the fitted value from the logit regression. Based on these simulated splits, we form a dummy variable, *Shadow Peer Split Dummy*. It is calculated just like the real peer split dummy used earlier, except that instead of real splits it is based on the simulated shadow splits.

We then run the baseline regression explaining the decision to split as before, but now using the *Shadow Peer Split Dummy* as the main explanatory variable. We repeat the simulation one hundred times, so that the shadow dummy regression is run with a different set of simulated shadow splits each round. The results show that the mean coefficient for *Shadow Peer Split Dummy* is -0.084 and the mean z-value is -1.40. In contrast, the coefficient for the real peer split dummy in the same regression in Table 3 is 0.35 and the z-value is 7.08.

This analysis shows that having such firm-month observations in the peer group that are likely to produce peer splits—but that did not necessarily de facto produce peer splits—does not positively affect a firm's propensity to split.

4.6. Instrumental variable analysis

In this subsection, we discuss an instrumental variables strategy to further assess the possibility of unobservable shocks driving splits within groups of peers. As an instrument for peer splits, we use the percentage of peer firms whose 12-month lagged stock price is above the price at which they previously conducted a split. This lag is essential because we want the instrument to predict the split activity in the peer group during months $[-12, 0]$, but not the other way around, i.e., we do not want actual splits during $[-12, 0]$ to affect the instrument. The detailed specifications of this variable are illustrated in Fig. 1. The instrument has the following desirable properties.

First, past split price levels likely reflect firm- or management-specific views and motives or reference points. Empirically, trading above the previous split price is strongly positively associated with the likelihood of splitting. For example, a logit model regressing the dummy for splits on an indicator of trading above previous split price on a firm-month panel (with standard controls) produces the following result (z-value in brackets below the coefficient):

$$Firm\ \ i\ \ splits_t = a_t + \underset{[15.39]}{1.50}\begin{pmatrix} Price \\ Above\ \ Previous \\ Split\ \ Price\ \ \%_t \end{pmatrix} + Controls + e_{i,t}. \tag{2}$$

Second, there is no reason that the instrument, calculated based on firm i's peers, should directly affect firm i's own split decision. Merely having peers with high stock prices is not empirically associated with the likelihood that firm i splits (as shown in Table 3). Furthermore, instances in which the prices are above the previous split price are not strongly correlated across peer firms. The instrument is a poor predictor of incidents (months) in which firm i's own share price is above its previous split price. Running a regression produces an R-squared of only 0.006. The instrument thus appears to do a good job at aggregating idiosyncratic information among a group of firms that is relevant to their probability of splitting, without being driven by market or industry returns.

To more formally test for the exclusion restriction, we run a monthly logit regression explaining the decision to split with both the instrument and the actual percentage of peer splitters as separate explanatory variables (with standard controls). Except for the addition of the instrument, the regression is identical to regression (1) of Table 4. This produces the following estimates (z-values below coefficients in brackets):

$$Firm\ \ i\ \ splits_t = a_t + \underset{[7.16]}{1.68}\begin{pmatrix} Peer \\ Splitters\ \ \%_t \end{pmatrix} + \underset{[0.29]}{0.08}\begin{pmatrix} Peers\ \ Trading \\ Above\ \ Previous \\ Split\ \ Price\ \ \%_t \end{pmatrix} + Controls + e_{i,t}. \tag{3}$$

As shown above, the actual percentage of peer splitters is highly statistically significant but the instrument is not. This indicates that the instrument influences the dependent variable only through its effect on peer splits and has no direct effect on a firm's split decision.

We conduct the analysis with IV-probit regressions in which the dependent variable in the second stage takes the value one if a firm has split its stock in month t. The instrumented variable is the percentage of peer firms that have split their stock during the previous 12 months.[8] For a meaningful calculation of the percentage of peers, and also to limit the effect of firms with very small peer groups, we exclude observations in which the peer group has fewer than five firms.

Table 6 reports the results. The first stage results show that the instrument is a strong predictor of peers' splitting activity. The second stage results show that the instrumented *Peer Splitters %* is also significant in all the specifications. The first specification includes the standard control variables and is similar to regression (1) in Table 4 except that the *Peer Splitters %* is now instrumented. The coefficient is 0.86 (z-value of 2.3). The second specification adds Fama and French industry fixed effects, and the coefficient increases to 1.06 (z-value of 2.9). The third specification shows that a dummy for firms whose own lagged price is above their previous split price is not statistically significant in the first stage regression. This implies that instances in which firm i's stock price is above its previous split price are not statistically related to splits by firm i's peers. This is consistent with the earlier results in which the stock price levels of i's peers, in turn, were not positively associated with i's tendency to split and is another manifestation of the idiosyncratic nature of past split prices. We include *Log* (*Average Peer Price*) as a control here as well.

[8] Here we use the percentage of peer splitters instead of the peer split dummy, as also previously done in column 1 of Table 4. The reason is that the IV-probit model should not be used with discrete instrumented variables. The percentage of peer splitters and the peer split dummy produce qualitatively similar results in the regular panel regressions.

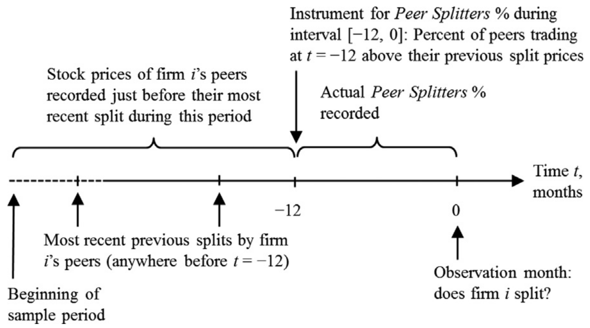

Fig. 1. Illustrating the calculation and use of the instrument for *Peer Splitters %*. As illustrated in the figure, we consider only prior splits that occurred at least 12 months before the current observation month, so as not to have the events of the past 12 months to affect the instrument. The timing of splits is based on their announcement months, and the stock price at the previous split is the prior month's closing price. Firms must have at least one split to be included in the sample. Beginning of sample period refers to the beginning of Center for Research in Security Prices data in this context. That is, even if the sample period in our empirical tests starts in 1983, the instrument can utilize older data as well.

Overall, the instrumental variable results discussed in the section provide further confirmation to the causal peer effects story and evidence against the alternative explanation of unobservable group-specific shocks.

5. Results for the effect of peer splits' announcement returns on the propensity to split

In this section, we pursue the idea that observational learning from peer outcomes plays a role in the decision to split. We test whether the split announcement returns of peers are related to the likelihood of a firm conducting a split. To do this, we measure the average announcement return among the splits of peer firms during the previous 12-month period. The announcement return is calculated as the stock return, in excess of the Standard & Poor's 500 index, from the day before the announcement to ten trading days after.

We begin with a univariate sort of split observations into quintiles based on the average split announcement return of peers during the prior 12 months. We exclude firm-months with no peer splits when peer returns are undefined. Table 7 shows the monthly percentage of splitters in different peer announcement return quintiles. The monthly percentage of splitters increases monotonically in peer announcement returns. Firms in the highest quintile are 42% more likely to split than firms in the lowest quintile. These results indicate that higher split announcement returns lead to increased occurrence of splits among peer firms in the future.

We study this effect in more detail with monthly logit regressions explaining the decision to split. The specifications are similar to the earlier regressions with the peer split dummy. We focus first on the effect of the sign of the average peer announcement return. We include a dummy for firms that have peer splitters with positive average announcement return during the previous 12 months and another dummy for firms that have peer splitters with negative average announcement return. The omitted category represents firm-month observations with no peer splits. The results reported in Table 8 indicate that firms with positive average peer announcement return are significantly more likely to split than firms with negative average peer announcement return. The coefficient values of the positive peer announcement return dummy are approximately two times as large as the corresponding values of the negative return dummy, and the difference in z-values is even higher. Firms with negative announcement return peer splitters are still more likely to split than firms with no peer splitters. The coefficient of the negative peer split announcement return dummy is positive and statistically significant, albeit at the 10% level.

Table 8 also reports results for similar regressions excluding observations with no splitters in peer group during the past 12 months (i.e., observations with no average peer announcement return are dropped). These subsample regressions study whether the difference between the effects of positive and negative average peer announcement returns is statistically significant conditional on having splitters in the peer group. A single dummy variable indicates a positive average peer announcement return, and the omitted category contains observations with negative announcement returns in these regressions. The estimated coefficient for the dummy suggests that the sign of the announcement return has a statistically significant, incrementally positive effect on the propensity to split. When month fixed effects are used with the sample of firms that have splitters in their peer group, 50 sample months need to be excluded because they include no observations of firms that split during the month. The number of excluded months is larger than in the regressions with all sample firms, but the differences in the results of regressions with month fixed effects and regressions with seasonal controls are nevertheless small.

We also run regressions that include the average peer announcement return directly as a continuous explanatory variable. However, outlier observations are a potential problem with a continuous announcement return variable. Stock splits do not have very high announcement returns on average, and if individual firms make other corporate announcements concurrently with the split, the reactions to these other announcements can produce extremely high or low returns. It seems unlikely that managers would attribute such extreme returns to the stock split. To eliminate the effect of possible outliers in the regressions that directly include the average peer announcement return, we exclude peer splits with the highest and lowest 5% of announcement returns in the data when calculating the average announcement return for peer firms' splits. The sample for these regressions consists of firms that have observed at least one peer split during the previous 12 months so the number of observations is smaller than in the regressions with the peer split dummy.

Results with the continuous peer announcement return variable are reported in Table 9. The average announcement return is positively related to the propensity to split and statistically significant at the 5% level. The results with month fixed effects and seasonal controls are close to each other. The statistical significance is lower in the 1983–1996 and 1997–2009 subsample regressions due to the smaller

Table 6
Instrumental variable regressions.

For these instrumental variable probit regressions, the instrumented variable is the percentage of peer firms that have split their stock during the previous 12 months, and the instrument is the percentage of peer firms whose share price lagged by 12 months is above the share price at which they made their previous split announcement (see Fig. 1 for an illustration). The dependent variable in the first stage regressions is the percentage of peer splitters. The dependent variable in the second stage probit regressions is equal to one if a firm has split its stock in month t, and the main explanatory variable is the instrumented Peer Splitters %. Stock splits are defined as in Table 1. Other variables in the regressions are the logarithm of price Log(Price), Market Capitalization Percentile (based on NYSE cutpoints), Past 12 Month Return, Recent Splitter Dummy, which is equal to one if the firm has announced a split during the previous 12 months, a dummy for firms whose 12-month lagged price is above their previous split price, and Log(Average Share Price of Peers), which is calculated as the logarithm of average share price of analyst-based peer firms at the end of the previous month. Firms with fewer than five peer firms are excluded from the analysis. All regressions include month fixed effects, and there is also one specification with Fama and French (49) industry fixed effects. z-Statistics based on standard errors clustered at the firm level are reported in brackets below the coefficients.

	First stage	Second stage	First stage	Second stage	First stage	Second stage
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Peer Splitters %		0.861		1.057		1.199
		[2.29]		[2.92]		[2.60]
Peers Trading Above Previous Split Price %	0.252		0.271		0.227	
	[34.39]		[36.62]		[27.77]	
Log(Price)	0.013	0.824	0.014	0.950	0.006	0.801
	[6.03]	[7.91]	[7.53]	[12.91]	[2.59]	[6.53]
Market Capitalization Percentile	−1.23E-04	−0.007	−2.28E-04	−0.009	−1.58E-04	−0.007
	[−1.95]	[−4.51]	[−4.51]	[−7.68]	[−2.19]	[−3.84]
12 Month Return	0.027	0.449	0.026	0.438	0.029	0.495
	[11.08]	[17.14]	[11.08]	[17.01]	[10.58]	[15.49]
Recent Splitter Dummy	0.039	−0.340	0.029	−0.390	0.036	−0.391
	[12.03]	[−7.28]	[9.56]	[−8.21]	[10.08]	[−7.66]
Own Price Above Previous Split Price Dummy					−0.002	0.107
					[−0.65]	[2.79]
Log (Average Share Price of Peers)					0.041	−0.163
					[8.52]	[−2.55]
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Fama-French Industry Fixed Effects	No	No	Yes	Yes	No	No
Number of Observations	187,652	187,652	183,635	183,635	150,440	150,440

Table 7
Splitting activity in different peer split announcement return quintiles.

The table reports the monthly percentage of splitters in different peer split announcement return quintiles. Stock splits are defined as in Table 1. The quintiles are based on monthly observations of firms that have at least one peer firm that has split its stock during the previous 12 months. The sample period is 1983–2009. The observations are sorted into five groups based on the average split announcement return of the peer firms during the previous 12 months. The split announcement returns are calculated as the return over the Standard & Poor's 500 index from the day before the split announcement to ten trading days after the announcement.

	Peer split announcement return quintile					Full Sample
Splits	1	2	3	4	5	
Number of splits	229	258	302	320	326	1435
Percentage of splitters	0.70	0.79	0.92	0.97	0.99	0.87

number of observations (z-values are 1.78 and 1.66, respectively), but the coefficient values are close to the full sample coefficients. Altogether, these results reinforce the conclusion that peer announcement return is a positive predictor of the propensity to split.

As our results indicate that recent splits by peer firms and positive announcement effects of peer splits both increase the propensity to split, a natural question is to what extent the two effects are separate. The results in Table 8 show that peer splits with negative announcement returns do increase the propensity to split, suggesting that the mere fact that a peer company executes a stock split is a positive factor. Column 4 of Table 9 includes both the peer split dummy (as used in Table 3) and the average peer announcement return in a single regression. The results show that both variables are statistically significant in the same regression. The coefficient value of the peer split dummy differs by less than 2% from the coefficient value obtained in the full sample baseline regression with similar control variables in Table 3. These findings suggest that peer firms' splits and their announcement effects have separate effects on the propensity to split. Column 5 uses raw stock returns instead of adjusting for market returns. We use this specification to test the idea that managers overweight the extent to which the announcing firm's price reaction stems from the split, perhaps failing to fully adjust for other concurrent events. If this is the case, we would expect to see stronger results with raw stock returns, and this is what we find. The coefficient for the peer announcement return is increased by over 50%.

Positive announcement effects do not seem to carry over to peers' split announcements. The correlation between past and current announcement returns is only

Table 8
Peer firms' positive and negative average split announcement returns and the propensity to split.

The table reports monthly panel logit regressions studying the relation between stock splits and peer firms' positive and negative average split announcement returns. There are separate regressions for all sample firms and for firms that have peer firms that split their stock during the previous 12 months. The dependent variable is equal to one if a firm has split its stock in month t. Stock splits are defined as in Table 1. The timing of splits is based on the declaration date (DCLRDT) item in the Center for Research in Security Prices (CRSP). Firms with peers that have split their stock during the previous 12 months have value one for Positive Peer Split Announcement Return Dummy if the average announcement return of peer splits has been positive and value one for Negative Peer Split Announcement Return Dummy if it has been negative. Firms without splitters have value zero for both dummies, and only the Positive Peer Split Announcement Return Dummy is included in the subsample regressions for firms that have splitters in their peer group. Split announcement returns are calculated as the return over the Standard & Poor's 500 index from the day before the split announcement to ten trading days after the announcement. Other variables in the regressions are the logarithm of price Log(Price), Market Capitalization Percentile (based on NYSE cutpoints), Past 12 Month Return, Recent Splitter Dummy, which is equal to one if the firm has announced a split during the previous 12 months, Low-Price Premium and Number of Recent NYSE Splits (the last two variables are defined as in Table 3). The regressions include either month fixed effects or seasonal controls (year dummies and dummies for the 12 calendar months). z-Statistics based on standard errors clustered at the firm level are reported in brackets below the coefficients.

	All firms			Firms with peer splitters		
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Positive Peer Announcement Return Dummy	0.273	0.278	0.277	0.129	0.145	0.143
	[4.61]	[4.75]	[4.71]	[1.97]	[2.21]	[2.19]
Negative Peer Announcement Return Dummy	0.143	0.130	0.131			
	[2.05]	[1.91]	[1.92]			
Log(Price)	1.887	1.881	1.881	1.883	1.867	1.867
	[8.19]	[8.09]	[8.10]	[8.40]	[8.44]	[8.46]
Market Capitalization Percentile	−0.019	−0.020	−0.020	−0.018	−0.018	−0.018
	[−5.67]	[−5.66]	[−5.66]	[−4.93]	[−5.01]	[−5.01]
Past 12 Month Return	0.808	0.767	0.767	0.927	0.909	0.910
	[9.72]	[7.21]	[7.21]	[13.75]	[14.20]	[14.20]
Recent Splitter Dummy	−0.740	−0.711	−0.713	−0.881	−0.884	−0.887
	[−6.43]	[−5.68]	[−5.67]	[−6.81]	[−6.84]	[−6.83]
Low-Price Premium			0.771			0.391
			[2.00]			[0.82]
Number of Recent NYSE Splits			0.611			0.696
			[0.59]			[0.56]
Month Fixed Effects	Yes	No	No	Yes	No	No
Seasonal Controls	No	Yes	Yes	No	Yes	Yes
Number of Observations	254,466	277,782	277,782	126,168	137,970	137,970
Number of Splitters	1,969	1,969	1,969	1,283	1,283	1,283
Pseudo R^2	0.169	0.162	0.162	0.169	0.156	0.156

0.023 and statistically insignificant. When the announcement return is regressed on the average peer announcement return variable and a constant, the t-value of the average peer return is 0.86. Another way to look at this is to measure the announcement returns for firms that split after their peers have split with positive announcement returns. The announcement returns for such firms is only 8 basis points higher than average (t-value 0.26), and their difference to splits after negative peer returns is nowhere near statistically significant either (t-value 0.62). Combined with the earlier observation of peer firms' split announcement returns increasing the propensity to split, the lack of any carryover effects in these returns suggests that firms could be imitating peers with false hopes of achieving similar increases in market value.

6. Conclusion

Stock splits offer an excellent setting for studying corporate peer effects. This is because the key fundamental factor—the stock price—can be directly observed and controlled for. This paper shows that recent stock splits by peer firms increase a firm's propensity to follow suit and split its stock. Based on regression coefficients, a peer split dummy has the same effect size as a 40–50% increase in share price over the previous year does. In addition to utilizing the measurability of stock price, we amass analyses such as instrumental variables and placebo regressions whose results are very difficult to reconcile with alternative explanations involving unobserved heterogeneity or peer group shocks.

We also find that the propensity to split is increasing in the announcement returns of peer splits. This is consistent with social learning from peer firms' outcomes. Managers can interpret peer splits as evidence of the benefits of share price management and conclude that peers are splitting because they see that a lower nominal share price has a positive impact on firm value. The announcement return results support the idea that perceived valuation differences are one of the motives for stock splits, consistent with the catering hypothesis of nominal share prices proposed by Baker, Greenwood, and Wurgler (2009).

Table 9
Peer firms' split announcement returns and the propensity to split.

The table reports monthly panel logit regressions in which the dependent variable is equal to one if a firm has split its stock in month *t*. Stock splits are defined as in Table 1. The regression sample consists of firms that have at least one peer firm that has split its stock during the previous 12 months. The effect of the announcement returns is measured with the average announcement return of peer firms' splits during the previous 12 months (variable Peer Announcement Return). It is calculated as the return from the day before the split announcement to ten trading days after the announcement. In regressions with market-adjusted announcement returns, the return is calculated as the return over the Standard & Poor's 500 index during the period and otherwise the return is unadjusted. Other variables in the regressions are the logarithm of price Log(Price), Market Capitalization Percentile (based on NYSE cutpoints), Past 12 Month Return, Recent Splitter Dummy, which is equal to one if the firm has announced a split during the previous 12 months, Low-Price Premium, Number of Recent NYSE Splits (the last two variables are defined as in Table 3), and Peer Split Dummy, which is equal to one if the number of splits by analyst-based peers during the previous 12 months is higher than the average number of splits by NYSE firms during the same period. The regressions include either month fixed effects or seasonal controls (year dummies and dummies for the 12 calendar months). There are also subsample regressions for the periods 1983–1996 and 1997–2009. *z*-Statistics based on standard errors clustered at the firm level are reported in brackets below the coefficients.

	Full sample					1983–1996	1997–2009
Variable	(1)	(2)	(3)	(4)	(5)		
Peer Announcement Return	1.941	2.185	2.168	1.887	3.173	1.979	2.444
	[2.08]	[2.36]	[2.34]	[1.99]	[3.90]	[1.78]	[1.66]
Log(Price)	1.893	1.878	1.878	1.868	1.904	1.942	1.799
	[8.37]	[8.38]	[8.40]	[8.27]	[8.38]	[8.86]	[5.29]
Market Capitalization Percentile	−0.018	−0.018	−0.018	−0.017	−0.017	−0.019	−0.016
	[−4.80]	[−4.84]	[−4.83]	[−4.47]	[−4.67]	[−5.82]	[−2.80]
Past 12 Month Return	0.924	0.908	0.909	0.914	0.918	0.965	0.845
	[13.57]	[13.79]	[13.79]	[13.39]	[13.42]	[11.04]	[9.29]
Recent Splitter Dummy	−0.859	−0.862	−0.867	−0.885	−0.852	−1.011	−0.631
	[−6.59]	[−6.60]	[−6.61]	[−6.83]	[−6.52]	[−5.91]	[−3.18]
Low-Price Premium			0.421				
			[0.86]				
Number of Recent NYSE Splits			1.016				
			[0.81]				
Peer Split Dummy (the number of peer splits exceeds 12-month NYSE average)				0.347			
				[4.66]			
Month Fixed Effects	Yes	No	No	Yes	Yes	No	No
Seasonal Controls	No	Yes	Yes	No	No	Yes	Yes
Market-adjusted announcement returns	Yes	Yes	Yes	Yes	No	Yes	Yes
Number of Observations	120,572	131,785	131,785	120,572	120,572	79,151	52,634
Number of Splitters	1,244	1,244	1,244	1,244	1,244	824	420
Pseudo R^2	0.169	0.154	0.155	0.170	0.169	0.153	0.159

Some of our results suggest that firms might not weigh information fully rationally and, thus, could end up executing unnecessary splits. First, firms that split after positive peer announcement returns do not themselves enjoy any greater than average returns. Second, we find that the tendency to follow successful splitters is even stronger when peer firms' success is measured by raw stock returns unadjusted for market returns. These results are consistent with firms mistaking noise for a signal. Because there seems to be little value added from imitating peers in this respect, firms could be overreacting to their peers' actions and outcomes.

How might the results of this paper generalize to other corporate actions? There are two key factors to consider. First, if splits are just peanuts in the buffet of corporate decisions, managers might not give much thought to the splitting decision and could be more likely to just follow their peers. However, while clearly not in the same ballpark as, say, large merger and acquisition decisions, the costs of stock splits are not trivial. Based on Weld, Michaely, Thaler, and Benartzi (2009) and our own calculations, the typical split incurs administrative costs of $250,000–$800,000, investors' trading costs increase by $4.5 million to $7.5 million per year, and the bid-ask spread typically widens. The magnitudes are thus comparable to many types of corporate investment and financing decisions. Second, peer influence can be particularly strong when the private signal regarding the optimal course of action is weak (Banerjee, 1992; Bikhchandani, Hirshleifer and Welch, 1992). Splits arguably fall into that category of decisions. However, even in settings, such as capital structure, in which traditional theory is better able to characterize an optimum, model uncertainty and estimation errors could introduce substantial noise. Even in those settings, the actions and outcomes of peer firms could constitute a more accessible source of information for corporate managers.

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