# The Impact of the Media in Financial Markets:

# Evidence from Newspaper Strikes*

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**Abstract**

This paper investigates the *causal* impact of the media in financial markets by exploiting exogenous media blackouts resulting from national newspaper strikes in several countries. Trading volume falls by approximately 18% on strike days, while volatility is mostly unaffected. These findings indicate that the media influences investors' trading behavior but not stock returns. They support theories in which information propagates gradually among investors thanks to the media but is incorporated immediately into stock prices as a result of arbitrageurs' trades.

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## Introduction

What role do the media play in financial markets? Classical theory argues that there is none: information is incorporated into stock prices as soon is it is made public. Even if most investors do not pay attention to this information, the few who do will ensure, by trading on it, that it finds its way immediately into stock prices. Yet mounting evidence suggests that information diffuses gradually across the investor population and that this gradual diffusion affects prices. In particular, a large body of research documents many instances of underreaction to corporate events such as dividend initiations and omissions, stock splits, earnings announcements, changes in analyst recommendations, tender offers and seasoned equity offerings.[1] In addition, the pervasive return momentum phenomenon of Jegadeesh and Titman (1993) can be viewed as evidence of investors' inattention, because it weakens when trading volume is larger (Hou, Peng and Xiong (2006)). In this context, one may suspect the media to matter in financial markets.

But a causal link from the media to financial markets is difficult to establish. A simple correlation may reflect an omitted variable (both the media and the market respond to fundamental news without being directly related) or reverse causality (the media may report newsworthy market developments). In this paper, I exploit newspaper strikes to identify the *causal* impact of the media. I rely on strikes that affect the press on a national scale and involve

[1] For dividend initiations and omissions, see Michaely (1995); for stock splits, see Ikenberry and Ramnath (2002); for earnings announcements, see Bernard and Thomas (1990); for changes in analyst recommendations, see Womack (1996) and Michaely and Womack (1999); for tender offers, see Ikenberry (1995); for seasoned equity offerings, see Loughran and Ritter (1995). Chan (2003) studies underreaction to public news about a firm, identified by the presence of a newswire or a press article. For further evidence on investors'' inattention, see Cohen and Frazzini (2006)), DellaVigna and Pollet (2006) and Hirshleifer et al. (2006).

the media sector only (I exclude general strikes affecting all sectors). Over the period 1989-2010, I found 52 eligible national newspaper strikes, lasting on average 1.7 business days and amounting to 88 strike days in total. They are concentrated in four countries: France, Greece, Italy and Norway. They are called by journalists, print or distribution workers in reaction to planned government policies. Most of the time, they have to do with their economic conditions, such as employment, pay, pensions, tax breaks, state subsidies and other benefits. Sometimes, they are called to fight censorship and defend the freedom of the press. Therefore, these nationwide newspaper strikes are not driven by (i.e. are exogenous to) stock market movements on the day of the strike nor the preceding days.

I find that on the day a newspaper strike occurs, the share turnover on the country's stock market is on average 18% lower, but remains unchanged on the days before and after. The statistical significance is also remarkably strong in spite of the relatively small number of strike events which serve to identify the impact of a strike – the coefficient estimate is significant at the 0.1% level. The volatility of the market (measured as the absolute value of the return on the market) is also reduced but not significantly so. Moreover, when stocks are sorted into quintiles according to their market capitalization, the magnitude of the strike effect on turnover decreases monotonically from the bottom to the top quintile, remaining significant in all but the top quintile. In contrast, volatility is significantly lower in the bottom two quintiles and unaffected in the remaining three. These findings are robust to many checks such as changing the way turnover and volatility are measured, excluding any of the four sample countries, and using only the first day of a strike. These results demonstrate that the media have a causal impact in financial markets: the media stimulate trading of all stocks except the very large, and contribute to the volatility of the smallest stocks.

This paper contributes to the growing literature on the role of the media on financial markets. It relates in particular to Engelberg and Parsons (2011) who are the first to demonstrate unambiguously a casual effect of the media. They analyze how trades by individual investors located in various U.S. cities relate to business news coverage by local newspaper distributed in these cities. Their identification strategy relies on extreme weather conditions which disrupt the delivery of newspapers, and on differences in time zones across the country which influence print deadlines. Because of these features, on a given day, investors located in a city may read or not about an event for reasons that are exogenous to the stock market. They provide strong evidence that investors trade individual stocks in response to newspaper reports. My finding that the share turnover is reduced on newspaper strike days is consistent with theirs. I add to their work on several dimensions.[2] First, I provide evidence of the causal effect of the media using an entirely different identification strategy that uses international data and establish its pervasiveness at the level of the market rather than individual stocks. Second, I examine the impact of the media on stock returns, an aspect that Engelberg and Parsons (2011) who rely on local trades cannot investigate. The divergence in the behaviors of turnover and volatility which I find – the absolute value of stock return is by and large *not* reduced on strike days even though turnover weakens – suggests that the media are not essential to the informational efficiency of stock prices, even though they play an important role in propagating information among investors. The speedy incorporation of news into prices reflects the forces of arbitrage: news is

[2] In the last section of the paper, I also conduct an analysis similar to that in Engelberg & Parson (2011). I identify local newspaper strikes that hit U.S. cities. I use the same retail trades data from a large discount broker as in Engelberg & Parson (2011), which covers the period 1991-1996. Over that period, three cities experienced local newspapers strikes: Pittsburgh (*Post-Gazette* and *The Pittsburgh Press*), San Francisco (*San Francisco Chronicle* and *San Francisco Examiner*), and Detroit (*Detroit Free Press* and *The Detroit News*). In all three instances, I find that investors located in the striking city trade less relative to other investors in the country on the first day of the strike. While only suggestive, this evidence provide out-of-sample support for the newspaper strike effect documented in the cross-country study.

incorporated into stock prices even though many investors do not participate in the market for these stocks, thanks to the trading of investors who remain informed in spite of the news blackout. Thus, the paper delivers a nuanced message on the role of the media, they matter to investors but are not essential to stock market efficiency.

The paper also contributes to the debate on the determinants of trading volume in the stock market. Trading volume is extremely large across most developed stock markets. Several theories have been put forward to explain this high trading intensity.[3] The findings reported here are consistent with the gradual diffusion of information being a cause of the large observed turnover, and with the media contributing to this diffusion.

The balance of the paper is organized as follows. Section 1 reviews the related literature. Section 2 describes the methodology and the data. Section 3 presents the main results of the paper, namely how newspaper strikes affect aggregate stock market activity. Section 4 examines how this effect varies across stocks. Section 5 conducts a series of robustness checks. Section 6 analyses the impact of local newspaper strikes in three U.S. cities. Section 7 concludes.

## 1. Methodology and Data

### 1.1. Methodology

Assessing the causal effect of the media on the stock market raises difficult identification issues. A simple association between media activity and stock market activity (e.g. trading volume, stock returns, volatility) may result from unobserved news shocks which create an omitted variable bias. Indeed, if such shocks generate an unusual market reaction and are

[3] In the current sample, the ratio of the value of all shares traded in a stock market to its capitalization (the average value-weighted turnover) equals on average 0.32% per day or 75% per year. This means that the entire market value of a typical firm changes hands every 16 months.

simultaneously reported in the press, then the market reaction and the media reports are correlated but the media does not cause the unusual reaction. Even in the absence of news shocks, the press may report on the market activity itself, thereby inducing a correlation between the media and the market's response.

I exploit variations in media coverage that are exogenous to stock market activity to measure the causal impact of the media on the stock market. Specifically, I examine whether stock market activity is different on days with media blackouts resulting from newspaper strikes.

### 1.2. National Newspaper Strikes

I collect data on newspaper strikes that prevent readers from receiving news, either because newspapers are not written (a journalists' strike), not printed (a printers' strike) or not distributed (a distributors' strike). I focus on nationwide strikes affecting a large number of newspapers. I search for such events across OECD countries over the period 1989-2010. I start in 1989 because trading volume becomes available in many countries in the early 1990's. I exclude from the sample strikes that occur on non-business days because market activity cannot be measured (e.g. a journalists' strikes on Friday that stops newspapers from coming out on the Saturday). I also eliminate strikes that are not specific to the media sector, i.e. strikes that are part of general action affecting all sectors, to ensure I do not mistaken the impact of a general strike for that of a media blackout.

Detailed data on industrial actions in media outlets are difficult to obtain. I search Factiva, an aggregator of information from a large number of sources around the world, for national newspaper strikes.[4] Over the sample period, the strikes I have found that fulfill my requirements

[4] I search for the term "strike" and its translation in several languages in the full text of news stories, classified by Factiva as referring to the "media" industry and to the subject of "labor/personnel issues".

are concentrated in four countries: France, Greece, Italy and Norway. Unions in these countries are powerful and capable of mobilizing the workforce beyond a firm, at the level of an entire sector.

These nationwide newspaper strikes are not driven by (i.e. are exogenous to) stock market movements on the day of the strike nor the preceding days. They are a reaction to government and planned policy changes. Most of the time, they have to do with economic conditions, such as employment, pay, pensions, tax breaks, state subsidies and other benefits. For example in January 2002, Italian printworkers halted production of Italy's newspapers to protest planned labor and pensions reforms by the government of Silvio Berlusconi; later in June, Norwegian journalists silenced the press for 9 days (7 business days) over disputed vacation benefits; in July 2004, Greek journalists went on strike for 48 hours following the breakdown of talks for a collective wage agreement. Disputes over technology are also an important source of unrest. In France for example, workers at the NMPP (later relabeled Presstalis), a company which has a monopoly on newspaper deliveries in Paris, called strikes on numerous occasions over plans to adopt new technologies that would change work practices, thereby disrupting the distribution of newspapers in the country. Journalists also go on strike to fight censorship and defend the freedom of the press. On June 10, 2003, Italian journalists went on strike on Tuesday to protest the concentration of media in the hands of Prime Minister Silvio Berlusconi, and on July 10, 2010, to challenge a proposed law that they say violates media freedom.

Organizing a strike on a national scale requires some coordination between newspapers so strikes are usually scheduled one to several days in advance. This need not be the case for actions called by print and distribution workers who use the element of surprise to prevent management from arranging substitute schemes.

I found 52 eligible national newspaper strikes, lasting on average 1.7 business days and amounting to 88 strike days in total. They are listed in Table 1. I create an indicator variable, $Strike_{t,k}$, which equals one if an eligible national newspaper strike occurs on day *t* in country *k* and zero otherwise. In the subsequent analysis, I eliminate strikes that affected the printing and distribution of papers after 1996 because newspapers were available online from that date on. The year 1996 was chosen as a cutoff because these strikes occurred mostly in France and the French leading newspaper, *Le Monde*, started offering a free online version on December 19, 1995. Of course, other papers may have come online later. Moreover, it is not clear whether the online edition can fully substitute for the print edition. My strategy is conservative, to only retain strikes that undoubtedly lead to a drop in media access. Nonetheless, I check in unreported regressions that increasing the cutoff to a later year or retaining these strikes does not alter the results.

### 1.3. Stock Market Variables

Measuring the extent to which trading activity is altered on media strike days requires a model of normal trading activity. I measure, for each firm and day, the share turnover, which equals the ratio of the number of shares traded in the firm on that day to the number of shares outstanding. Next, I aggregate turnover at the country level by averaging turnover across all firms in the country, using both equal weights and market-capitalization weights.[5] I then divide

[5] The average value-weighted turnover equals the ratio of the value of all shares traded in a market to its capitalization.

by a 100-day backward moving average and take logs.[6] Thus, abnormal turnover, *ATurnover*, is defined as:

$$ATurnover_t = \ln\left(Turnover_t \Big/ \frac{1}{100}\sum_{s=1}^{100} Turnover_{t-s}\right).$$

I also investigate the impact of media strikes on stock return volatility, measured as the natural logarithm of one plus the absolute value of daily stock market returns.

I download individual stock data (number of shares outstanding, stock price, number of shares traded) on a daily frequency from Compustat Global.

Throughout the paper, I adjust standard errors for heteroskedasticity and, in pooled regressions, cluster them by date to account for worldwide shocks to equity markets. I include in regressions day-of-the-week and month dummies to control for calendar effects, and year dummies to control for time trends. The level of significance used in the analysis deserves comment. Though the time series of daily turnover and returns is long, the number of strike events, which serve to identify the impact of a strike, is fairly small. This implies a relatively large standard error on the strike dummy. In this context, a 10% level would be appropriate to evaluate its statistical significance.  Nonetheless, most results are significant at the 1% level.

## 2. Results

I study the behavior of the aggregate stock market in each country, starting with abnormal turnover and then proceeding to the absolute value of stock returns.

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[6] An alternative measure of abnormal turnover is obtained by first taking the log of turnover and then subtracting a 100-day backward moving average of log turnover. This measure is highly correlated to the one used in the paper (the correlation coefficient is 0.85) but its distribution looks more non-normal (higher skewness and kurtosis).

## 2.1. Trading Activity

The analysis starts with a country-by-country investigation of abnormal turnover on media strike days. Table 2 shows the results of regressions of daily abnormal turnover on the media strike dummy and various controls, including day-of-the-week, month and year dummies. When turnover is equally weighted across firms (Panel A), the coefficient on the media strike dummy is negative in all specifications and across all countries. It is statistically significant at the 10% level in two thirds of the regressions. The magnitude of the coefficient is reduced (the coefficient is less negative) when lagged abnormal turnover, $ATurnover_{t-1}$, is included as a regressor. This reflects the persistence of turnover and the fact that newspaper strikes are associated with low abnormal turnover on the day of the strike but also on the day before, as will be discussed in Section 5. The pattern is less clear when market capitalization weights are used to measure average turnover. In Panel B, the coefficient estimates on the media strike dummy are positive in 5 regressions out of 8, though never significantly, and negative in the remaining three with only one significant instance (Greece at the 5% level). The contrast between Panels A and B suggests that the media strike effect is concentrated among smaller firms.

With few newspaper strikes per country, it is not so surprising that the results of country-by-country regressions are not overwhelming. In the next regressions, I pool all countries together. In specifications 1 and 2 of Table 3, I average abnormal turnover across countries using equal weights, and regress it on a media strike dummy which equals one on days on which a country has a newspaper strike and zero otherwise.[7] In specifications 3 and 4, I estimate a panel regression model with country fixed effects and adjust standard errors for clustering by date in

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[7] This media strike dummy equals the average media strike dummy across countries multiplied by the number of countries.

addition to heteroskedasticity. In all four specifications (respectively specifications 3 and 4), the slope coefficient is negative and statistically significant at the 5% (respectively 1%) level when turnover is equally weighted across stocks (Panel A). As in the country-by-country analysis, the media effect weakens while remaining strongly significant when lagged abnormal turnover is included in regressions 1 and 3, but it looses its statistical significance with value-weighted turnover (Panel B). These findings support the notion that trading volume is reduced when newspapers go on strike and that this effect is strongest among smaller stocks.

The economic magnitude of the media strike effect in Panel A is sizable. The slope coefficient in specification 4 measures the average percentage difference in abnormal turnover between strike and non-strike days: on average, equally-weighted abnormal turnover falls by 18.43% on media strike days, while the value-weighted abnormal turnover falls by a more modest 2.68% (not statistically different from zero).

The panel regressions in Table 3 force all slope coefficients to be identical across countries. In Table 4, I implement a more flexible two-step procedure that allows countries to load differently on lagged abnormal turnover and calendar dummies. In the first step, I regress, for each country, abnormal turnover on a set of control variables:

$$ATurnover_{t,k} = a_k + b_k ATurnover_{t-1,k} + \sum_l c_{k,l} calendar\_dummy_{t,l} + \varepsilon_{t,k} \ ,$$

where $k$ denotes a country, $calendar\_dummy_{t,l}$ is a set of dummy variables indexed by $l$ and indicating the day of the week, the month and the year, and $\varepsilon_{t,k}$ is a residual.[8] The first specification in Table 4 does not include any lag of abnormal turnover, while the second includes

[8] Strike days are excluded from these regressions.

one lag. The residuals from these regressions are then estimated according to $\hat{\varepsilon}_{t,k} = ATurnover_{t,k} - \hat{a}_k - \hat{b}_k ATurnover_{t-1,k} - \sum_l \hat{c}_{k,l} calendar\_dummy_{t,l}$ where a ^ denotes an estimate. The second step consists of a panel regression of these estimated residuals $\hat{\varepsilon}_{t,k}$ on the newspaper strike dummy, $Strike_{t,k}$ :

$$\hat{\varepsilon}_{t,k} = \gamma Strike_{t,k} + \nu_{t,k} .$$

The results, displayed in Table 4 for equally-weighted turnover, confirm the media strike effect reported so far. The coefficient estimates on the media strike dummy are negative, of similar magnitude as those in Table 3 (Panel A, regressions 3 and 4), and statistically significant at the 1% levels in specifications 1 and 2.

Finally, I consider a third specification, similar to the previous two except that the variance of residuals varies over time in the spirit of Gallant, Rossi, and Tauchen (1992). In the first step, I run the same regression as before adding a second lag of abnormal turnover, a time trend and its square:

$$ATurnover_{t,k} = a_k + b_k ATurnover_{t-1,k} + b_{2,k} ATurnover_{t-2,k} + \sum_l c_{k,l} calendar\_dummy_{t,l} + d_k t + d_{2,k} t^2 + \varepsilon_{t,k}$$

Next, I estimate the residual as:

$$\hat{\varepsilon}_{t,k} = ATurnover_{t,k} - \hat{a}_k - \hat{b}_k ATurnover_{t-1,k} - \hat{b}_{2,k} ATurnover_{t-2,k} - \sum_l \hat{c}_{k,l} calendar\_dummy_{t,l} - \hat{d}_k t - \hat{d}_{2,k} t^2 ,$$

and its variance according to the regression model:

$$\ln(\hat{\varepsilon}_{t,k}{}^2) = a'_k + \sum_l c'_{k,l}\ calendar\_dummy_{t,l} + d'_k\ t + d'_{2,k}\ t^2 + \xi_{t,k} ,$$

where $\xi_{t,k}$ denotes the residual from this variance regression. Finally, I define the residual turnover as:

$$w_{t,k} = \exp(\hat{\xi}_{t,k} / 2) = \frac{\hat{\varepsilon}_{t,k}}{\exp\left[ (\hat{a}'_k + \sum_l \hat{c}'_{k,l}\ calendar\_dummy_{t,l} + \hat{d}'_k\ t + \hat{d}'_{2,k}\ t^2) / 2 \right]}.$$

Step two consists of regressing $w_{t,k}$ on the newspaper strike dummy. Table 4 shows again that the slope coefficient is negative and statistically significant at the 1% level, consistent with a reduction in trading volume on media strike days.[9]

## 2.2. Stock Return Volatility

Several studies have documented a positive association between trading volume and return volatility. This section analyses whether the market volatility (measured as the absolute value of the return on the market) falls on media strike days in line with trading activity. I conduct a set of tests similar to that presented for abnormal turnover, but find only weak evidence of a decline in the absolute value of market return. For brevity, I only report the findings from the analysis on the pooled data. The results for the other tests are comparable. While most of the coefficient estimates on the media strike dummy reported in Table 5 are negative, only one is statistically different from zero at the 10% significance level. These results suggest that market volatility is unchanged or slightly reduced on newspaper strike days.

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[9] Gallant, Rossi, and Tauchen (1992) use the natural logarithm of the dollar trading volume as dependent variable rather than turnover. They focus on the U.S. stock market while my sample contains several different countries. Turnover is better suited for a cross-country analysis given the important differences in stock market sizes and currencies across countries. Using dollar trading volume instead of turnover yields a coefficient estimate on the newspaper strike dummy of -0.1052, statistically significant at the 12% level.

## 3. Impact of Newspaper Strikes across Stocks

In this section, I examine how the media strike effect varies across stocks. I expect this effect to be stronger among stocks held by investors who depend more on the domestic press for their access to news. These include retail investors since most institutions subscribe to professional news services (e.g. Bloomberg, Reuters etc…), as well as domestic investors as the foreign press may cover the striking country (e.g. business news in France may be covered in Belgian or Swiss newspapers). Empirical studies indicate that institutional and foreign investors favor large stocks while retail and local investors tend to hold small local stocks. For example, Lee, Shleifer, and Thaler (1991) document that small stocks are disproportionately held by individual investors in the U.S., while Kang and Stulz (1997) and Dahlquist and Robertsson (2001) that they are underweighted by foreign investors in Japan and Sweden. Therefore, the media strike effect is expected to be strong among small stocks and to diminish as their size increases.

On each day and in each country, I sort stocks into 5 quintiles based on their market capitalization. I then run a panel regression model as in Table 3 separately for each size quintile. The results are displayed in Table 6. In Panel A which focuses on (equally-weighted) abnormal turnover, the coefficient estimate on the media strike dummy is negative across all size quintiles, and significantly so in all quintiles but the largest. The magnitude of the coefficient decreases monotonically from the bottom to the top quintile (i.e. it is less negative), with stocks in the bottom quintile experiencing a 27% drop in abnormal turnover (statistically significant at the 0.1% level) as opposed to 1.6% for stocks on the top quintile (not statistically different from zero). These results are consistent with the findings reported in Table 2 and 3, in which the media strike effect is strong when abnormal turnover is equally weighted and weaker to insignificant when it is value-weighted. They confirm that the impact of newspaper strikes declines with firm size.

Panel B shows the impact of newspaper strikes on the absolute value of (equally-weighted) returns across size quintiles. The coefficient estimate on the media strike dummy is negative across all size quintiles and specifications but for one instance, but it is only statistically significant within the bottom quintile (at a 5% significance level). Among the smallest stocks, the absolute value of returns falls by approximately 11% on media strike days.

The similarity in the behaviors of turnover and volatility is noteworthy. Both variables drop on strike days and the drops are the strongest within the bottom size quintile. Their differences are also interesting. Within quintiles 2 to 4, abnormal turnover falls strongly but volatility does not. This feature is particularly striking for the middle quintile (quintile 3), in which turnover contracts by 13% (*p*-value 4.6%) to 18% (*p*-value 0.8%) while the absolute value of returns falls by an insignificant 2% (*p*-value 57%) to 3% (*p*-value 36%).[10] This disparity between turnover and volatility reflects the forces of arbitrage: news is incorporated into stock prices (absolute value of stock return is *not* reduced) even though many investors do not participate in the market for these stocks (turnover weakens), thanks to the trading of investors who remain informed in spite of the news blackout. This suggests that the media are not essential to the informational efficiency of stock prices, even though they play an important role in propagating information among investors.

[10] This asymmetry is even more prominent when I restrict the turnover and return regressions to the same sample of observations (the turnover series starts later than the return series). In this case, the turnover effect is unchanged but the return effect is weakened and is only marginally significant in the bottom quintile (the *p*-value rises to 19% when lagged abnormal turnover is included and to 7% when it is not).

These findings also speak to the debate on the determinants of trading volume in the stock market. Trading volume is extremely large across most developed stock markets. In the current sample, the ratio of the value of all shares traded in a stock market to its capitalization (the average value-weighted turnover) equals on average 0.32% per day or 75% per year. This means that the entire market value of a typical firm changes hands every 16 months.[11] Several theories have been put forward to explain this high trading intensity. The findings reported here are consistent with the gradual diffusion of information being a cause of the large observed turnover, and with the media being a means of this diffusion.[12]

## 4. Robustness Checks

In this section, I conduct a number of robustness checks to the baseline results presented in Table 3 for equally-weighted turnover.  First, I examine how turnover behaves on the days before and after a newspaper strike. Then I try to alleviate the concern that the strike effect could be driven by a few long-lasting strikes, a misspecified model for normal trading activity, or one particular country.

I start by examining how turnover behaves on the days surrounding a newspaper strike. In principle, if trading indeed weakens on day $t$ because of the news blackout, then it should not weaken on day $t$-1 nor day $t$+1. This prediction is complicated by two features. First, about half of newspaper strikes last more than one day. Second, several national newspaper strikes are

[11] Turnover has increased over time. It equals 0.21% per day (52% per year) in the 1990’s vs. 0.39% (98% per year) in the 2000’s.  Hong and Stein (2007) report a similar figure for the U.S. (102% in 2005).

[12] A key element of information diffusion models is that, contrary to the standard rational expectations framework, investors do not learn from stock prices or from the trades of informed investors. That is, investors must be “overconfident” and “agree to disagree”.

surrounded by other media strikes such as national strikes in other media (news agencies, television or radio stations), or by strikes in one or several leading newspapers. To identify these confounding events, I search Factiva for any occurrence of a strike on the day before or after a national newspaper strike used in my sample. I find that half (a third) of the strikes are preceded (followed) by a strike in any kind of media, i.e. by a strike affecting the working of a media outlet without paralyzing the entire newspaper sector. I define a set of indicator variables to track the occurrence of these confounding events. The variable $Any\_Strike_{k,t}$ equals one when a media strike of any kind is identified on day *t*, and zero otherwise. Conversely, the variable defined as one minus $Any\_Strike_{k,t}$ marks days with no media strike of any kind. Then I interact the media strike dummy $Strike_{t,k}$ with these indicators to distinguish the impact of national newspaper strikes which are preceded (followed) by some kind of strike, $Strike_{k,t} * Any\_Strike_{k,t-1}$ ( $Strike_{k,t} * Any\_Strike_{k,t+1}$ ), from those that are not, $Strike_{k,t} * (1 - Any\_Strike_{k,t-1})$ ($Strike_{k,t} * (1 - Any\_Strike_{k,t+1})$), and run a panel regression as in Table 3. The results, presented in Table 7, reveal that abnormal turnover tends to be lower on the days before and after a national newspaper strike, but these effects are mostly insignificant (the *p*-value is 21% for abnormal turnover on day *t*-1, and 55% for abnormal turnover on day *t*+1). Moreover, it appears that the (small) day-*t*-1 effect is entirely imputable to the confounding media strikes occurring on that day since the coefficient estimate on $Strike_{k,t} * Any\_Strike_{k,t-1}$ is significant (6% significance level) while that on $Strike_{k,t} * (1 - Any\_Strike_{k,t-1})$ is not. I conclude that the impact of a national newspaper strike is concentrated on the strike day, as expected.

Several strikes lasted more than one day. In particular, the 2002 and 2004 Norwegian strikes lasted 7 business days. To make sure that the results are not driven by these long-lasting events, I rerun the analysis with a strike dummy that equals one on the first day of a strike, and zero on other days (so it equals zero on a non-strike day and on day 2, 3 …of a strike). The results, displayed in Panel A of Table 8, indicate that the strikes have a statistically and economically significant impact on their first day. The economic magnitude is similar to that of the baseline regression (Table 3, Panel A, regressions 3 and 4), slightly stronger when lagged turnover is included as a regressor (regression 2) and slightly weaker when it is not (regression 1).

In regressions 3 to 5 of Panel A of Table 8, I model normal turnover in a more flexible way. In regression 5, I add an additional lag of turnover, $ATurnover_{t-2}$, and in regressions 3 and 4, I allow the coefficient on lagged turnover to vary with calendar effects, i.e. include as regressors $ATurnover_{t-1}$ interacted with year, month and day-of-the-week dummies. In both cases, the estimated coefficient on the strike dummy is reduced a little relative to the baseline regression (Table 3, Panel A, regressions 3 and 4) but remain statistically significant at the 1.7% and 1.3% levels respectively.

I check whether the effect of a strike is driven by a single country. I remove from the sample each of the four countries in turn and estimate the baseline regression. Panel B of Table 8 shows that, though they weaken at times, the coefficient estimates on the media strike dummy remain strongly statistically significant (the *p*-values range from 0.1% to 1.6%).

I winsorize abnormal turnover and absolute returns at the 1% level and obtain a similar media strike effect (not reported).

## 5. Evidence from the U.S.

While national newspaper strikes have not occurred in the U.S., several cities have experienced local newspapers strikes. Given the size of the country and the breadth of stock ownership (integrated market), these local news blackouts are unlikely to significantly affect stocks' turnover or return. Nonetheless, they may influence the trading behavior of local investors, i.e. of investors who rely on the striking local newspapers for news. I investigate this hypothesis using household trading data from a large discount brokerage. The data contain the trades of 78,000 households from January 1991 through December 1996.[13] Over this 5-year period, three cities experienced strikes that prevented readers from receiving their newspapers. 1) A strike by drivers forced Pittsburgh's two daily newspapers, the *Post-Gazette* and *The Pittsburgh Press*, to stop publishing on May 18, 1992 for several weeks; 2) San Francisco's two main daily newspapers, the *San Francisco Chronicle* and *San Francisco Examiner*, had to shutdown printing plants on November 3rd, 1994 for 11 days because of a strike by 2,600 journalists, editors, lorry drivers, press operators and paper handlers;[14] 3) Detroit's two largest newspapers, the *Detroit Free Press* and *The Detroit News*, were hit by a strike on July 14, 1995 which lasted several months.

I study the trading behavior of investors located in a 100 km radius of the striking city, around the first day of the strike.[15] A drawback of an examination of local trades is that it tells us nothing about the impact of newspaper strikes on stock returns. An advantage is that these data allow to control for shocks to the stock market occurring on strike days. Suppose, for example,

[13] See Barber and Odean (2000) for a compete description of these data.

[14] The San Francisco strikes contributed to the development of online media. **I**ndeed, the two newspapers launched a free electronic version in response to the strike, which was one of the earliest examples of online newspaper edition.

[15] The brokerage dataset provides zipcode information for 54,297 households.

that May, 18 1992 (the first day of the Pittsburgh strikes) is a day on which investors pay little attention to the economy, either because there is little going on, or because they are distracted (e.g. on a Friday, or a day with major non-economic news or international events). Then trading volume by Pittsburgh investors will be low on that day, regardless of the newspaper strike, but excess trading volume relative to the rest of the country will not.

On each day *t*, I aggregate the dollar trading volume over all investors located in the striking city *k* and over all stocks in the country, denoted $V\_Strike_{k,t}$. Similarly, I aggregate the dollar trading volume over all investors located outside the striking city and over all stocks in the country, $V\_NoStrike_{k,t}$. I estimate the abnormal trading volume in a striking city relative to the rest of the country as: $AV_{k,t} = \ln\left[\left(1+V\_Strike_{k,t}\right)/\left(1+V\_NoStrike_{k,t}\right)\right]$. I regress, for each newspaper strike, abnormal trading volume on lagged abnormal trading volume and calendar dummies, using data from a 100-day window centered on the strike day (excluding 5 days surrounding the strikes). Finally, I estimate the residual from this regression.

Figure 1 displays the histogram of the residual abnormal trading volume for each of the three newspaper strikes. The thick line shows abnormal trading volume on the strike day. It is negative in all three instances. To gauge the statistical significance of this effect, I regress residual abnormal trading volume on an indicator variable, $Strike_{t,k}$, which equals one if a newspaper strike occurs on day *t* in city *k* and zero otherwise, allowing for strike fixed effects. The coefficient estimate on $Strike_{t,k}$ equals to -0.49 with a standard error adjusted for heteroskedasticity and clustered by strike of 0.13, and a *p*-value of 7%.[16] With only three

[16] There is no point in clustering the standard errors by date because the time intervals for each strike do not overlap.

observations, this evidence is only suggestive. But it does provide out-of-sample support for the newspaper strike effect documented in the cross-country study.

## 6. Conclusion

In this paper, I provide evidence that the media have a causal impact in financial markets. I employ a novel identification strategy based on media blackouts that are exogenous to stock market movements, and which result from nationwide newspaper strikes. I document that trading activity is considerably weaker on strike days in several countries (18% lower on average across all stocks). However, I find only weak evidence of a matching reduction in stock return volatility. Together, these findings suggest that the media are not essential to the informational efficiency of stock prices, even though they play an important role in propagating information among investors. Indeed, the trades of few informed arbitrageurs seem to be sufficient to ensure the incorporation of relevant information into stock prices. These findings are consistent with the gradual diffusion of information being a cause of the high levels of turnover observed across stock markets, and with the media contributing to this diffusion.

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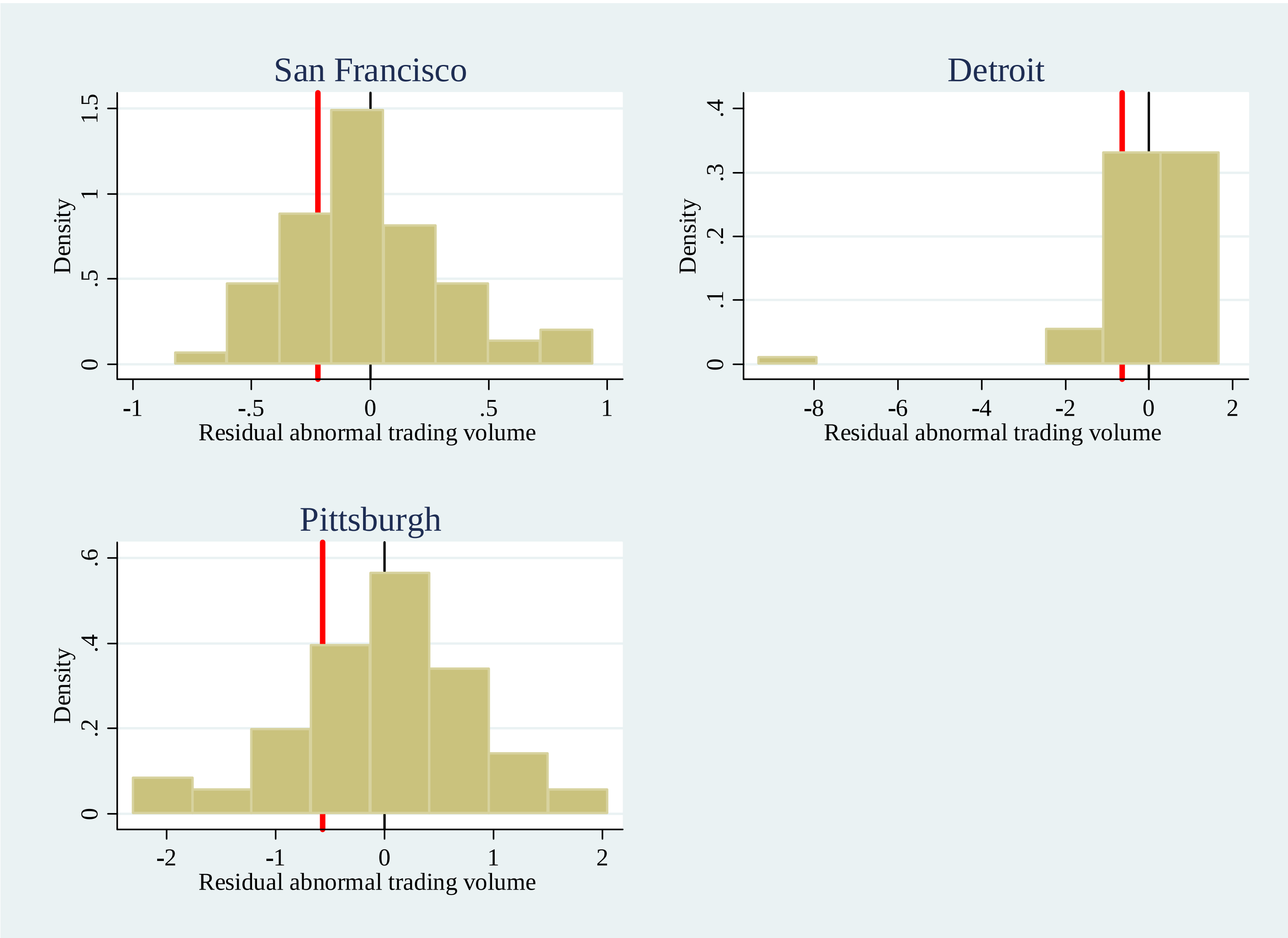
**Figure 1: The Impact of Regional Newspaper Strikes in the U.S.**

This figure displays the histogram of the abnormal trading volume around three regional newspaper strikes occurring in the U.S.. The top left panel refers to the November 3rd, 1994-San Francisco strike, the top right panel to the July 14, 1995-Detroit strike, and the bottom left panel to the May 18, 1992-Pittsburgh strike. Abnormal trading volume in a striking city is measured relative to the rest of the country as $AV_{k,t} = \ln\left[\left(1+V\_Strike_{k,t}\right)/\left(1+V\_NoStrike_{k,t}\right)\right]$, where $V\_Strike_{k,t}$ denotes the dollar trading volume aggregated over all investors located within a 100 km radius from the striking city, and $V\_NoStrike_{k,t}$ denotes the dollar trading volume aggregated over all investors located outside the striking city. Then $AV_{k,t}$ is regressed, for each newspaper strike, on lagged abnormal trading volume and calendar dummies, using data from a 100-day window centered on the strike day. The histograms represent the probability distribution of the residual from this regression. The thick line shows abnormal trading volume on the strike day.
A regression with strike fixed effects of residual abnormal trading volume on an indicator variable which equals one if a newspaper strike occurs and zero otherwise, yields a coefficient estimate on the strike indicator of -0.49 with a standard error adjusted for heteroskedasticity and clustered by strike of 0.13, and a *p*-value of 7%.



## Table 1: Sample of National Newspaper Strikes

This table lists national newspaper strikes that occur on a business day and are specific to the publishing and media sector, i.e. they do not coincide with a general strike.

| Country | Date | Duration (business days) | Who strikes? |
|---|---|---|---|
| France | Wednesday, March 08, 1989 | 2 | Print & distribution workers |
| | Wednesday, June 28, 1989 | 1 | Print & distribution workers |
| | Friday, December 15, 1989 | 6 | Print & distribution workers |
| | Thursday, February 20, 1992 | 1 | Journalists |
| | Thursday, April 29, 1993 | 1 | Print & distribution workers |
| | Thursday, October 14, 1993 | 1 | Print & distribution workers |
| | Wednesday, November 08, 1995 | 1 | Print & distribution workers |
| | Wednesday, October 16, 1996 | 1 | Journalists |
| | Friday, November 15, 1996 | 1 | Journalists |
| | Thursday, April 10, 1997 | 1 | Print & distribution workers |
| | Tuesday, July 08, 1997 | 1 | Print & distribution workers |
| | Wednesday, April 07, 1999 | 1 | Print & distribution workers |
| | Wednesday, June 13, 2001 | 1 | Print & distribution workers |
| | Thursday, September 08, 2005 | 1 | Print & distribution workers |
| | Thursday, June 12, 2008 | 1 | Print & distribution workers |
| | Tuesday, September 16, 2008 | 1 | Print & distribution workers |
| | Thursday, October 30, 2008 | 1 | Print & distribution workers |
| | Wednesday, October 28, 2009 | 1 | Print & distribution workers |
| | Wednesday, April 21, 2010 | 2 | Print & distribution workers |
| Greece | Tuesday, April 10, 2001 | 1 | Journalists |
| | Thursday, February 07, 2002 | 2 | Journalists |
| | Thursday, March 07, 2002 | 1 | Journalists |
| | Thursday, March 28, 2002 | 2 | Journalists |
| | Wednesday, July 14, 2004 | 4 | Journalists |
| | Friday, November 25, 2005 | 1 | Journalists |
| | Wednesday, May 09, 2007 | 1 | Journalists |
| | Wednesday, November 28, 2007 | 1 | Journalists |
| | Thursday, October 02, 2008 | 1 | Journalists |
| | Wednesday, June 24, 2009 | 1 | Journalists |
| | Friday, June 04, 2010 | 1 | Journalists |
| Italy | Wednesday, January 30, 1991 | 1 | Journalists |
| | Tuesday, May 28, 1991 | 3 | Journalists |
| | Monday, July 29, 1991 | 2 | Journalists |
| | Thursday, September 30, 1993 | 1 | Journalists |
| | Wednesday, March 16, 1994 | 2 | Journalists |
| | Tuesday, April 11, 1995 | 1 | Journalists |
| | Friday, April 28, 1995 | 1 | Journalists |
| | Friday, October 20, 1995 | 3 | Journalists |
| | Friday, December 10, 1999 | 1 | Journalists |
| | Thursday, November 30, 2000 | 2 | Journalists |
| | Tuesday, December 12, 2000 | 1 | Journalists |
| | Tuesday, January 22, 2002 | 1 | Print & distribution workers |
| | Wednesday, June 11, 2003 | 1 | Journalists |
| | Tuesday, October 28, 2003 | 1 | Journalists |
| | Wednesday, November 09, 2005 | 2 | Journalists |
| | Friday, October 06, 2006 | 2 | Journalists |
| | Thursday, November 16, 2006 | 1 | Journalists |
| | Friday, December 22, 2006 | 3 | Journalists |
| | Friday, July 09, 2010 | 1 | Journalists |
| Norway | Monday, June 11, 1990 | 2 | Journalists |
| | Thursday, May 30, 2002 | 7 | Journalists |
| | Thursday, May 13, 2004 | 7 | Journalists |

## Table 2: Turnover Regressions by Country

This table reports the results of time-series regressions estimated country by country. The dependent variable is the abnormal turnover in the country, defined as $ATurnover_t = \ln\left( Turnover_t \Big/ \frac{1}{100}\sum_{s=1}^{100} Turnover_{t-s} \right)$ where $Turnover_t$ is obtained by estimating for each firm and day the ratio of the number of shares traded in the firm on that day to the number of shares outstanding, and then taking the average across all firms in the country, using equal weights (Panel A) and market-capitalization weights (Panel B). The main independent variable is an indicator variable, $Strike_{t,k}$, which equals one if a newspaper strike occurs on day *t* in country *k* and zero otherwise. Year, month and day-of-the-week dummy variables are included in the regressions. Standard-errors and *p*-values adjusted for heteroskedasticity are displayed in parentheses in this order below the coefficient estimates. *, **, *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

### Panel A: Equally-weighted turnover

| | ATurnover(t) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | France | | Greece | | Italy | | Norway | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Strike(t) | -0.0820 | -0.1335* | -0.2115*** | -0.0733 | -0.1407** | -0.0704* | -0.2474*** | -0.0595 |
| | (0.137) | (0.073) | (0.068) | (0.047) | (0.062) | (0.039) | (0.086) | (0.043) |
| | (0.550) | (0.066) | (0.002) | (0.121) | (0.022) | (0.069) | (0.004) | (0.162) |
| ATurnover(t-1) | | 0.5453*** | | 0.5597*** | | 0.7481*** | | 0.4476*** |
| | | (0.019) | | (0.045) | | (0.014) | | (0.020) |
| | | (0.000) | | (0.000) | | (0.000) | | (0.000) |
| Observations | 4498 | 4389 | 4006 | 3846 | 4334 | 4232 | 4396 | 4281 |
| R-squared | 0.1603 | 0.4162 | 0.1225 | 0.4045 | 0.1811 | 0.6439 | 0.1401 | 0.3176 |

### Panel B: Value-weighted turnover

| | ATurnover(t) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | France | | Greece | | Italy | | Norway | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Strike(t) | 0.1869 | 0.0463 | -0.1444** | -0.0448 | 0.0193 | -0.0356 | 0.0053 | 0.0355 |
| | (0.139) | (0.066) | (0.063) | (0.056) | (0.058) | (0.042) | (0.086) | (0.064) |
| | (0.180) | (0.483) | (0.022) | (0.422) | (0.741) | (0.400) | (0.951) | (0.580) |
| ATurnover(t-1) | | 0.4544*** | | 0.4904*** | | 0.5417*** | | 0.3982*** |
| | | (0.019) | | (0.020) | | (0.026) | | (0.023) |
| | | (0.000) | | (0.000) | | (0.000) | | (0.000) |
| Observations | 4498 | 4389 | 4006 | 3846 | 4334 | 4232 | 4396 | 4281 |
| R-squared | 0.2024 | 0.3624 | 0.0782 | 0.3005 | 0.1908 | 0.4300 | 0.1541 | 0.2910 |

## Table 3: Pooled Turnover Regressions

This table reports the results of pooled time-series regressions. The dependent variable is the abnormal turnover in the country, defined as $ATurnover_t = \ln\left( Turnover_t \middle/ \frac{1}{100}\sum_{s=1}^{100} Turnover_{t-s} \right)$ where $Turnover_t$ is obtained by estimating for each firm and day the ratio of the number of shares traded in the firm on that day to the number of shares outstanding, and then taking the average across all firms in the country, using equal weights (Panel A) and market-capitalization weights (Panel B). In regressions 1 and 2, abnormal turnover is averaged across countries using equal weights, and regressed on a media strike dummy $Strike_t$ which equals one on days on which one of the sample countries has a newspaper strike and zero otherwise. In regressions 3 and 4, a panel regression model with country fixed effects is estimated, in which the main independent variable is an indicator variable, $Strike_{t,k}$ , which equals one if a newspaper strike occurs on day *t* in country *k* and zero otherwise. Year, month and day-of-the-week dummy variables are included in the regressions. Standard-errors and *p*-values adjusted for heteroskedasticity and, in the panel regressions, clustered by date are displayed in parentheses in this order below the coefficient estimates. *, **, *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

### Panel A: Equally-weighted turnover

| | ATurnover(t) | | | |
|---|---|---|---|---|
| | Average across countries | | Panel | |
| | (1) | (2) | (3) | (4) |
| Strike(t) | -0.0411* | -0.0715** | -0.0720*** | -0.1843*** |
| | (0.021) | (0.030) | (0.024) | (0.041) |
| | (0.051) | (0.016) | (0.003) | (0.000) |
| ATurnover(t-1) | 0.5868*** | | 0.5886*** | |
| | (0.018) | | (0.017) | |
| | (0.000) | | (0.000) | |
| Country dummies | | | Yes | Yes |
| Observations | 4500 | 4562 | 16748 | 17234 |
| R-squared | 0.4790 | 0.1923 | 0.4120 | 0.0928 |

### Panel B: Value-weighted turnover

| | ATurnover(t) | | | |
|---|---|---|---|---|
| | Average across countries | | Panel | |
| | (1) | (2) | (3) | (4) |
| Strike(t) | -0.0093 | -0.0130 | -0.0218 | -0.0268 |
| | (0.026) | (0.028) | (0.028) | (0.040) |
| | (0.715) | (0.645) | (0.429) | (0.499) |
| ATurnover(t-1) | 0.4974*** | | 0.4860*** | |
| | (0.022) | | (0.012) | |
| | (0.000) | | (0.000) | |
| Country dummies | | | Yes | Yes |
| Observations | 4500 | 4562 | 16748 | 17234 |
| R-squared | 0.4090 | 0.2118 | 0.3213 | 0.1128 |

## Table 4: Pooled Turnover Regression with Country-Specific Slope Coefficients

This table reports the results of pooled time-series regressions The independent variable is an indicator variable, $Strike_{t,k}$, which equals one if a newspaper strike occurs on day *t* in country *k* and zero otherwise. The dependent variable is the *residual* abnormal turnover in the country, estimated through country-specific regressions of abnormal turnover on year, month and day-of-the-week dummy variables (first-step regression). Abnormal turnover is defined as $ATurnover_t = \ln\left(Turnover_t \Big/ \frac{1}{100}\sum_{s=1}^{100} Turnover_{t-s}\right)$ where $Turnover_t$ is obtained by estimating for each firm and day the ratio of the number of shares traded in the firm on that day to the number of shares outstanding, and then taking the average across all firms in the country, using equal weights. In specification 2, lagged abnormal turnover is included as a regressor in the first-step regression. In specification 3, a second lag of abnormal turnover, a time trend and its square are also included, and the variance of residuals in the first-step regression varies over time in the spirit of Gallant, Rossi, and Tauchen (1992). Standard-errors and *p*-values adjusted for heteroskedasticity and clustered by date are displayed in parentheses in this order below the coefficient estimates. *, **, *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| | Residual Turnover | | |
|---|---|---|---|
| | No lag of ATurnover in step-1 regression | One lag of ATurnover in step-1 regression | Gallant, Rossi, and Tauchen (1992) on ATurnover |
| | (1) | (2) | (3) |
| Strike(t) | -0.1794*** | -0.0732*** | -0.6917*** |
| | (0.039) | (0.023) | (0.254) |
| | (0.000) | (0.001) | (0.007) |
| Observations | 17234 | 16748 | 16688 |
| R-squared | 0.0008 | 0.0002 | 0.0003 |

## Table 5: Pooled Volatility Regressions

This table reports the results of pooled time-series regressions. The dependent variable is the log of one plus the absolute value of daily stock market returns in the country where stock market returns are equally weighted (Panel A) or market-capitalization weighted (Panel B). In regressions 1 and 2, absolute return is averaged across countries using equal weights, and regressed on a media strike dummy $Strike_t$ which equals one on days on which one of the sample countries has a newspaper strike and zero otherwise. In regressions 3 and 4, a panel regression model with country fixed effects is estimated, in which the main independent variable is an indicator variable, $Strike_{t,k}$, which equals one if a newspaper strike occurs on day *t* in country *k* and zero otherwise. Year, month and day-of-the-week dummy variables are included in the regressions. Standard-errors and *p*-values adjusted for heteroskedasticity and, in the panel regressions, clustered by date are displayed in parentheses in this order below the coefficient estimates. *, **, *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

### Panel A: Equally-weighted return

| | Abs. return(t) | | | |
|---|---|---|---|---|
| | Average across countries | | Panel | |
| | (1) | (2) | (3) | (4) |
| Strike(t) | 0.0021 | -0.0092 | -0.0102 | -0.0420 |
| | (0.024) | (0.023) | (0.036) | (0.035) |
| | (0.929) | (0.695) | (0.774) | (0.224) |
| Abs. return(t-1) | 0.2181*** | | 0.2091*** | |
| | (0.017) | | (0.010) | |
| | (0.000) | | (0.000) | |
| Country dummies | | | Yes | Yes |
| Observations | 5511 | 5562 | 21119 | 21668 |
| R-squared | 0.1925 | 0.1514 | 0.2211 | 0.1867 |

### Panel B: Value-weighted return

| | Abs. return(t) | | | |
|---|---|---|---|---|
| | Average across countries | | Panel | |
| | (1) | (2) | (3) | (4) |
| Strike(t) | -0.0007 | -0.0090 | -0.0585 | -0.0834* |
| | (0.029) | (0.029) | (0.044) | (0.044) |
| | (0.982) | (0.754) | (0.184) | (0.056) |
| Abs. return(t-1) | 0.1461*** | | 0.1491*** | |
| | (0.016) | | (0.009) | |
| | (0.000) | | (0.000) | |
| Country dummies | | | Yes | Yes |
| Observations | 5511 | 5562 | 21119 | 21668 |
| R-squared | 0.1717 | 0.1530 | 0.1179 | 0.0975 |

## Table 6: Impact of Newspaper Strikes across Stock Size Groups

This table reports the results of panel regression models estimated separately for each size quintiles. Stocks are sorted into 5 quintiles based on their market capitalization on each day and in each country. The main independent variable is an indicator variable, $Strike_{t,k}$, which equals one on the first day *t* on which a newspaper strike occurs in country *k* and zero otherwise. In Panel A, the dependent variable is the abnormal turnover, averaged over all stocks in the quintile using equal weights, where abnormal turnover is defined as $ATurnover_t = \ln\left( Turnover_t \Big/ \frac{1}{100}\sum_{s=1}^{100} Turnover_{t-s} \right)$ and $Turnover_t$ is obtained by estimating for each firm and day the ratio of the number of shares traded in the firm on that day to the number of shares outstanding. In Panel B, the dependent variable is the log of one plus the absolute value of the average daily stock returns in a quintile, where the average uses equal weights. Country, year, month and day-of-the-week dummy variables are included in the regressions. Standard-errors and *p*-values adjusted for heteroskedasticity and clustered by date are displayed in parentheses in this order below the coefficient estimates. *, **, *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

### Panel A: Turnover

| | ATurnover(t) | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Quintile 1 | | Quintile 2 | | Quintile 3 | | Quintile 4 | | Quintile 5 | |
| Strike(t) | -0.2737*** | -0.2119*** | -0.2346*** | -0.1538*** | -0.1836*** | -0.1328** | -0.1261** | -0.0650 | -0.0157 | -0.0254 |
| | (0.068) | (0.056) | (0.064) | (0.058) | (0.069) | (0.067) | (0.063) | (0.054) | (0.045) | (0.040) |
| | (0.000) | (0.000) | (0.000) | (0.008) | (0.008) | (0.046) | (0.045) | (0.230) | (0.727) | (0.520) |
| ATurnover(t-1) | | 0.3669*** | | 0.3961*** | | 0.3596*** | | 0.3428*** | | 0.3759*** |
| | | (0.012) | | (0.013) | | (0.011) | | (0.013) | | (0.015) |
| | | (0.000) | | (0.000) | | (0.000) | | (0.000) | | (0.000) |
| Observations | 17222 | 17008 | 17239 | 17074 | 17238 | 17105 | 17244 | 17108 | 17257 | 17054 |
| R-squared | 0.0468 | 0.1773 | 0.0523 | 0.2008 | 0.0554 | 0.1787 | 0.0699 | 0.1804 | 0.0772 | 0.2103 |

### Panel B: Absolute return

| | Absolute return(t) | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Quintile 1 | | Quintile 2 | | Quintile 3 | | Quintile 4 | | Quintile 5 | |
| Strike(t) | -0.1074*** | -0.0792** | -0.0681* | -0.0452 | -0.0347 | -0.0208 | -0.0172 | 0.0032 | -0.0623 | -0.0404 |
| | (0.038) | (0.040) | (0.038) | (0.038) | (0.038) | (0.036) | (0.038) | (0.036) | (0.042) | (0.042) |
| | (0.005) | (0.045) | (0.076) | (0.232) | (0.357) | (0.567) | (0.648) | (0.930) | (0.137) | (0.335) |
| Abs. return(t-1) | | 0.2225*** | | 0.2052*** | | 0.1962*** | | 0.1896*** | | 0.1570*** |
| | | (0.009) | | (0.009) | | (0.009) | | (0.009) | | (0.009) |
| | | (0.000) | | (0.000) | | (0.000) | | (0.000) | | (0.000) |
| Observations | 21851 | 21726 | 21828 | 21709 | 21764 | 21626 | 21817 | 21688 | 21886 | 21769 |
| R-squared | 0.2390 | 0.2756 | 0.2270 | 0.2591 | 0.1982 | 0.2285 | 0.1696 | 0.1989 | 0.1117 | 0.1336 |

## Table 7: Turnover on the days surrounding newspaper strikes

This table reports the results of panel regression models with country fixed effects. The dependent variable is the abnormal turnover in the country, defined as $ATurnover_t = \ln\left(Turnover_t \Big/ \frac{1}{100}\sum_{s=1}^{100} Turnover_{t-s}\right)$ where $Turnover_t$ is obtained by estimating for each firm and day the ratio of the number of shares traded in the firm on that day to the number of shares outstanding, and then taking the average across all firms in the country, using equal weights. The main independent variable is an indicator variable, $Strike_{t,k}$, which equals one on the first day *t* on which a newspaper strike occurs in country *k* and zero otherwise. In regressions 2, the media strike dummy $Strike_{t,k}$ is interacted with a lag of an indicator variable $Any\_Strike_{k,t}$ which equals one when a media strike of any kind (national strikes in other media such as news agencies, television or radio stations, or strikes in one or several leading newspapers) is identified on day *t*, and zero otherwise. In regressions 4, the media strike dummy $Strike_{t,k}$ is interacted with a lead of the indicator variable $Any\_Strike_{k,t}$. Year, month and day-of-the-week dummy variables are included in the regressions. Standard-errors and *p*-values adjusted for heteroskedasticity and clustered by date are displayed in parentheses in this order below the coefficient estimates. *, **, *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

| | ATurnover(t-1) | | ATurnover(t+1) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Strike(t) | -0.0545 | | -0.0194 | |
| | (0.043) | | (0.032) | |
| | (0.210) | | (0.548) | |
| Strike(t) x Any_Strike(t-1) | | -0.0981* | | |
| | | (0.052) | | |
| | | (0.057) | | |
| Strike(t) x (1-Any_Strike(t-1) ) | | 0.0414 | | |
| | | (0.074) | | |
| | | (0.575) | | |
| Strike(t) x Any_Strike(t+1) | | | | -0.0223 |
| | | | | (0.029) |
| | | | | (0.449) |
| Strike(t) x (1-Any_Strike(t+1)) | | | | -0.0178 |
| | | | | (0.047) |
| | | | | (0.704) |
| ATurnover(t-2) | 0.5867*** | 0.5868*** | | |
| | (0.018) | (0.018) | | |
| | (0.000) | (0.000) | | |
| ATurnover(t) | | | 0.5887*** | 0.5887*** |
| | | | (0.017) | (0.017) |
| | | | (0.000) | (0.000) |
| Observations | 16534 | 16534 | 16748 | 16748 |
| R-squared | 0.4116 | 0.4117 | 0.4119 | 0.4119 |

## Table 8: Robustness Checks on Panel Turnover Regressions

This table reports the results of panel regression models with country fixed effects. The dependent variable is the abnormal turnover in the country, defined as $ATurnover_t = \ln\left(Turnover_t \Big/ \frac{1}{100}\sum_{s=1}^{100} Turnover_{t-s}\right)$ where $Turnover_t$ is obtained by estimating for each firm and day the ratio of the number of shares traded in the firm on that day to the number of shares outstanding, and then taking the average across all firms in the country, using equal weights. In Panel A, all countries are included, while in Panel B, each country is removed in turn to estimate the panel regression. In regressions 1 and 2 of Panel A, the main independent variable is an indicator variable, $Strike_{t,k}$, which equals one on the first day *t* on which a newspaper strike occurs in country *k* and zero otherwise. In regressions 3 to 5 of Panel A and in Panel B, the main independent variable is an indicator variable $Strike_t$ which equals one on days on which one of the sample countries has a newspaper strike and zero otherwise. Year, month and day-of-the-week dummy variables are included in all the regressions. In regressions 3 and 4 of Panel A, lagged abnormal turnover interacted with year, month and day-of-the-week dummies are included as regressors. In regression 5, an additional lag of abnormal turnover is included as a regressor. Standard-errors and *p*-values adjusted for heteroskedasticity and clustered by date are displayed in parentheses in this order below the coefficient estimates. *, **, *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

### Panel A: First day of strike and other specifications

| | ATurnover(t) | | | | |
|---|---|---|---|---|---|
| | First day of strike | | Lag turnover interacted with calendar dummies | | 2 lags of turnover |
| | (1) | (2) | (3) | (4) | (5) |
| Strike(t) | -0.1576*** | -0.0906*** | -0.0589** | -0.0589** | -0.0608** |
| | (0.049) | (0.031) | (0.024) | (0.024) | (0.025) |
| | (0.001) | (0.003) | (0.013) | (0.013) | (0.017) |
| ATurnover(t-1) | | 0.5886*** | | 0.4562*** | 0.4443*** |
| | | (0.017) | | (0.062) | (0.025) |
| | | (0.000) | | (0.000) | (0.000) |
| ATurnover(t-2) | | | | | 0.2546*** |
| | | | | | (0.021) |
| | | | | | (0.000) |
| Observations | 17234 | 16748 | 16748 | 16748 | 16266 |
| R-squared | 0.0924 | 0.4120 | 0.4272 | 0.4272 | 0.4545 |

## Panel B: Excluding one country at a time

| | ATurnover(t) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Excluding France | | Excluding Greece | | Excluding Italy | | Excluding Norway | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Strike(t) | -0.1896*** | -0.0641** | -0.1588*** | -0.0693** | -0.2205*** | -0.0760** | -0.1722*** | -0.0819*** |
| | (0.042) | (0.025) | (0.048) | (0.027) | (0.055) | (0.032) | (0.044) | (0.027) |
| | (0.000) | (0.012) | (0.001) | (0.012) | (0.000) | (0.016) | (0.000) | (0.003) |
| ATurnover(t-1) | | 0.5903*** | | 0.5851*** | | 0.5402*** | | 0.6329*** |
| | | (0.020) | | (0.012) | | (0.021) | | (0.024) |
| | | (0.000) | | (0.000) | | (0.000) | | (0.000) |
| Observations | 12736 | 12359 | 13228 | 12902 | 12900 | 12516 | 12838 | 12467 |
| R-squared | 0.0891 | 0.4115 | 0.1152 | 0.4215 | 0.0893 | 0.3606 | 0.1051 | 0.4700 |