THREE ESSAYS ON FINANCE, CULTURE AND INVESTOR BEHAVIOR

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A DISSERTATION SUBMITTED TO
THE FACULTY OF GRADUATE STUDIES
IN PARTIAL FULFILLMENT OF THE REQUIREMENTS
FOR THE DEGREE OF
DOCTOR OF PHILOSOPHY

GRADUATE PROGRAM IN BUSINESS ADMINISTRATION
YORK UNIVERSITY
TORONTO, ONTARIO

MARCH 2017

ABSTRACT

This dissertation consists of three essays that examine the effects of corporate culture and investor psychology on corporate decisions and financial markets. The first essay focuses on the role of corporate culture in acquisitions, whereas the last two essays investigate deviations from market efficiency.

The first essay uses textual analysis of firms' annual reports to develop an estimate of the differences in corporate cultures of the combining firms, and finds that greater cultural differences between the firms lead to higher synergistic gains, but only when the acquirer has a stronger culture than its target. The synergy gains concentrate among deals where the acquirer's values are not antagonistic to the target's. Further analysis of profitability and productivity (measured as earnings per employee) around the acquisition transaction corroborates these findings. Overall, the evidence suggests that differences in corporate culture are an important driver of announcement returns in mergers and acquisitions.

The second essay investigates whether stock misvaluation drives industry-level merger waves by examining intra-wave patterns in acquirers' valuation levels in a sample of acquisitions during 1981-2010. The essay contrasts two types of merger waves: "*stock*" waves defined on pure stock acquisitions, and "*cash*" waves formed on pure cash offers. Consistent with the misvaluation hypothesis, the essay finds that the occurrence of *stock* merger waves is tightly associated with industry stock valuation, and bidder stock valuation is negatively associated with long-run abnormal returns, especially so during waves of stock mergers. In contrast, there is little evidence of such patterns using the *cash* wave definition.

The third essay investigates the effects of sunshine, wind, rain, snow, and temperature on daily index returns of 49 countries from 1973 to 2012. The paper finds pervasive weather effects that

vary across temperature regions (cold, hot, and mild) and months. A hedge strategy that exploits the return predictability of daily weather generates up to 25% (11.8%) annualized out-of-sample gross (net) profits during 1993-2012. The systematic patterns of weather effects together with the relationship between their strength and timing and individuals' seasonal propensity to spend time outdoors, suggest a plausible mechanism through which weather-induced mood influences index returns.

ACKNOWLEDGMENTS

This dissertation could not have been completed without the guidance and encouragement from my supervisor, Ming Dong. I am grateful for his patience, long-term commitment and continuous enthusiasm. I am also indebted to the other members of my supervisory committee, Mark Kamstra and Ambrus Kecskés, who provided constructive suggestions at various stages of this study.

In addition, the second chapter of this dissertation, *Cultural Differences, Synergies and Mergers and Acquisitions*, has benefitted from comments from Ling Cen, Jarrad Harford, Ron Masulis, Lukas Roth, Sergei Sarkissian, Hongpin Tan and seminar participants at Instituto Tecnológico Autónomo de México (ITAM), Université du Québec à Montréal (UQÀM), Université Laval, University of Manitoba, York University, 2016 Northern Finance Association, 2016 FMA Doctoral Consortium, and 2016 FMA Annual Meeting.

I am grateful to the following persons for their suggestions on the third chapter of this dissertation (*Does Stock Misvaluation Drive Merger Waves?*): Lawrence Booth, Ling Cen, Rui Duan, Ran Duchin, David Hirshleifer, Lawrence Kryzanowski, Frederik P. Schlingemann and seminar participants at the 2015 Midwest Finance Association conference, the 2015 Multinational Finance Society conference, the 2016 Telfer Annual Conference on Accounting and Finance, the 2016 Northern Finance Association, the 2016 FMA Annual Meeting, Concordia University, and York University.

I also thank Andrew An, Kee-Hong Bae, Jason Berkowitz, Melanie Cao, Doug Cumming, Bing Han, David Hirshleifer, Harrison Hong, Tim Loughran, Tyler Shumway, Lin Sun, Qiguang Wang, and seminar participants at the 2014 Society for the Advancement of Behavioral Economic conference, the 2014 Financial Management Association conference, the 2014 Research in

Behavioral Finance conference in Rotterdam, the 2014 Australasia Banking and Finance conference, the 2015 Midwest Finance Association, and York University for their helpful comments on previous versions of the fourth chapter of this dissertation. Roy Shelef provided excellent research support and discussions.

Last but not least, I wish to thank Schulich finance area support staff for their continuous support and my fellow doctoral students for their support and helpful discussions.

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CHAPTER 1

INTRODUCTION

Over the past few decades and with increasing frequency, researchers have highlighted empirical phenomena that are in direct violations of the fundamental premises of traditional finance models (see Barberis and Thaler, 2003, and Baker, Ruback and Wurgler, 2004, for a survey of the literature). For example, violations to efficient market hypothesis have been documented and there are now numerous articles that present evidence consistent with the irrational investors assumption (see Baker, Ruback and Wurgler, 2004 for a brief review). Although the behavioral finance literature is abundant, there are still many instances where either the empirical evidence is divided, or no empirical link has been established. In this dissertation, I consider three of such situations.

This dissertation consists of three essays. The first essay investigates the role of corporate culture in mergers and acquisitions. Anthropologists have long defined culture as the full range of learned human behavior patterns (Tylor, 1871). To the extent that corporations are manned by human beings and that corporate culture reflects, in great part, the culture of the corporation's members, this essay examines the financial implications of the intersection of learned corporate (human) behavior and corporate policy, in this case, mergers and acquisitions.

Indeed, the first essay of the dissertation, *Cultural Differences, Synergies and Mergers and Acquisitions*, examines how differences in the acquirer's and its target's corporate cultures relate to the combined post-announcement abnormal stock performance. Surveys of executives report that cultural differences are relevant to acquisition success. For example, approximately half of the executives in Graham et al.'s (2016) sample would not acquire a "culturally misaligned" target, although no precise definition of cultural alignment is offered. However, theory predicts both negative (for instance, Tajfel's, 1982; Williams and O'Reilly, 1998) and positive (e.g. Watson et al., 1993) relations between cultural differences and mergers outcomes. Whether differences in corporate cultures are positively or negatively related to synergistic gains in mergers is ultimately an empirical issue.

I use textual analysis of the acquirers' and targets' annual reports (10-K forms) to develop an estimate of the differences in corporate cultures between the merging entities. I draw on Cameron et al.'s (2006) Competing Values Framework to describe corporate culture in terms of four values, namely, creativity, competition, collaboration and control. *Cultural distance* (*CD*) is the Euclidean distance, across the four cultural values, between an acquirer and its target. The measure is well calibrated: Chapter 2 provides the results of various tests that validate the measure as a proxy for the intensity of the analyzed values in the sample firms. Textual analysis-based measures of cultural values allow for a larger sample than is typical in earlier studies (see for example, Hartnell, Ou and Kinicki, 2011), while also permitting a more precise estimation of cultural values, relative to proxies such as being listed on Fortune's Best Companies to Work For ranking.

Using a sample of hypothetical acquirer-target pairs, where targets are matched to real targets by industry, market capitalization and market-to-book ratios of equity, the essay documents a negative relation between the probability that a firm will be targeted by an acquirer and the cultural distance between said acquirer and the potential target.

Given these selection issues, and consistent with theoretical predictions, announced acquisitions between culturally different firms must either create value, if cultural distance is related to expected synergies, or destroy value, if cultural distance relates to frictions not accounted for by the management team. Using the acquirer's and target's combined cumulative abnormal announcement returns measured over three days centered on the announcement day as a proxy for synergistic gains, I find a positive relation between *CD* and combined announcement returns, even after controlling for deal, bidder and target characteristics, state-level differences in the economic, geographic and social environments, and the selection effects documented above. The *CD* effect is economically significant: a one-standard deviation increase in the cultural distance translates into an increase of approximately 1% in combined three-day abnormal returns, which amounts to about one third of the combined abnormal returns.

However, I find a discontinuity in the CD effect. Conceptually, if corporate culture is a valuable intangible asset, strong-culture firms should be more profitable and productive than weak-culture firms, and differences in corporate cultures should benefit an acquirer only if the acquirer has the cultural strength to assimilate the target. Chapter 2 thus defines a measure of cultural strength. It then provides empirical evidence that culturally stronger firms are more profitable and efficient than culturally-weak firms and, in line with the prediction, finds that the positive relation between cultural distance and combined abnormal announcement returns arises only in the subsample of acquirers that are culturally stronger than their targets.

The announcement returns tests suggest that differences in corporate cultures correlate, at least partially, to expected operating synergies. Regressions of post-merger industry-adjusted operating performance on pre-merger industry-adjusted operating performance provide evidence consistent with the prediction. Post-merger increases in industry-adjusted operating performance are larger for acquirers culturally stronger than their target.

In short, Chapter 2 shows that human values and behavior, and corporate cultural values by extension, have a financial impact on the firm. The other essays of this dissertation examine other instances where investors behavior interacts with corporations and financial markets.

Chapter 3, *Does Stock Misvaluation Drive Merger Waves?*[1], considers possible explanations for the occurrence of observed merger waves, defined as periods of industry-specific increased merger activity. More specifically, we contrast two hypotheses: the neoclassical theory (also known as the *Q* hypothesis) which posits that merger activity is driven by synergy and efficiency factors and by extension, that merger waves are caused by economic and regulatory shocks, and the misvaluation hypothesis, which predicts that stock misvaluation affects merger intensity. Under the misvaluation hypothesis, merger waves are triggered by sufficiently large deviations of stock prices from fundamental values.

In contrast to previous studies of merger waves, we define two sets of industry-specific merger waves: *stock* waves are defined on the subsample of acquisitions paid by pure stock, and *cash* waves are defined using the subsample of pure cash acquisitions. These merger waves indicate periods of increased industry-specific merger activity, where merger activity is measured using either pure stock deals or pure cash deals. Of course, deals of all considerations are announced during these periods of intense merger activity; contrasting their patterns in valuations and post-announcement returns provides an adequate setting for our tests.

Namely, we distinguish the misvaluation hypothesis from the *Q* hypothesis using two approaches. First, we test whether there is a positive association between bidder valuation levels and the occurrence of merger waves, and whether the strength of this association is the same irrespective of the wave definition (*stock* versus *cash* wave). According to the *Q* hypothesis, because the level of stock valuation reflects firms' fundamentals and merger waves are triggered by economic factors such as deregulations, the strength of the relation between valuation levels and merger activity should be the same, independently of the deal consideration. In contrast,

[1] The second and third essays are co-authored with Ming Dong.

according to the misvaluation hypothesis, that relation should be especially strong for deals paid entirely by stock. Second, we examine bidder long-run stock performance around merger waves. If merger waves result from firms acting in response to economic shocks, acquisitions announced during waves should create bidder shareholder value. However, overvaluation-driven merger waves should be associated with poor post-announcement abnormal stock performance of the acquirers.

We use a broad sample of U.S. domestic mergers and acquisitions announced between 1981 and 2010. To define our misvaluation proxy, we apply the residual income model of Ohlson (1995), sometimes called "intrinsic value" (V), and use the ratio of this value to market price (VP). We find that bidder valuation peaks exactly during in-wave periods, relative to pre-, post- and non-wave periods; this valuation spread is much larger around *stock* waves than around *cash* waves. Logit tests of the likelihood of merger wave occurrence confirm our results. These findings support the misvaluation hypothesis because the effects of industry equity valuation on industry-specific merger intensity are contingent on the definition of merger waves (*stock* versus *cash* waves).

We also examine the post-bid long-run returns of the acquirers to further distinguish the misvaluation hypothesis from the *Q* hypothesis. Long-run performance is measured by the buy-and-hold abnormal return (BHAR), where benchmark portfolios are matched on size and market-to-book ratios. Under the *Q* hypothesis, in-wave bidders should benefit the most in the post-announcement period, because their high valuation indicates high growth prospects. However, we find that in-wave acquirers have the lowest 5-year BHARs, when using the *stock* waves definition. Furthermore, in multivariate regressions of BHARs on bidder valuation and merger phase indicator variables, we find that in- and post-wave bidders perform significantly worse than pre-wave and non-wave bidders, and this effect is stronger for bidders with high valuation. However, we observe no clear patterns of bidder long-run abnormal stock performance around *cash* merger waves. These results lend further support to the misvaluation hypothesis. Therefore, even though Chapter 3 does not explore why misvaluation arises, it presents evidence consistent with the persisting existence of firm and industry-level misvaluation and its consequences on acquisitions decisions.

The third essay of this dissertation, *Chapter 4: Does the Weather Influence Global Stock Returns?*, examines another violation to the efficient market hypothesis, as well as a departure from the assumption that investors are purely rational. In this case, the essay explores the relation between investors psychology and mood and stock returns. Because there is no ambiguity in causality between the weather and stock returns, and because daily weather conditions are exogenous and transitory events, considering the influence of weather on stock returns provides a clean setting to test whether investors' mood relates to stock returns.

There are strong reasons to believe that the psychological effects of weather on mood, optimism or risk-taking vary with regional and seasonal conditions. Indeed, the psychology literature reports that the valence of mood is sensitive to temperature. In addition, some weather phenomena (e.g. snow) are climate-contingent. Therefore, we analyze a wide range of weather variables, allowing for the possibility that the weather effects vary by climate and season.

More specifically, we investigate the effects of five weather variables—sunshine, wind speed, rain, snow depth on the ground, and temperature—on nominal daily index returns of 49 countries from 1973 to 2012. We use daily weather variables observed in the cities where our sample countries' national exchanges are located as proxies for the most relevant conditions for each country, and conduct both ordinary least squares regressions (of daily returns) and logit regressions (of the probability of a positive return) on the weather variables. Contrary to the previous literature, we sort the countries into three groups based on the average annual temperature, shift the timing of countries in the Southern Hemisphere by six months to align the seasons, and conduct month-by-month tests for each temperature region.

The regression results indicate statistical significance of all five weather variables. To confirm that our results are not driven by a spurious relation between weather and returns, we test the profitability of a trading strategy based purely on the predictability of the daily weather. Our out-of-sample tests indicate that the hedge portfolios generate annualized gross returns as high as 25% during 1993-2012. Our second approach to ensure that the weather-returns relation is not spurious is to evaluate whether the patterns of monthly weather effects found in both the OLS and logit tests can be interpreted in a systematic way that is consistent with finance and psychology theories. We find that in general, “comfortable weather”, which is climate and season specific, is positively associated with returns. In addition, using estimates of time spent outdoors, we find that these weather effects are stronger when people spend more time outdoors or when outdoor time is more valuable. Taken together, our results show that the weather effects are real and support the hypothesis that comfortable weather conditions promote investor optimism and lead to high stock returns, especially during seasons of increased outdoor activity.

In short, this dissertation examines three instances where finance intersects with culture, human behavior and psychology. The rest of this dissertation is structured as follows: Chapter 2 introduces the essay *Cultural Differences, Synergies and Mergers and Acquisitions*. The essay *Does Stock Misvaluation Drive Merger Waves?* is presented in Chapter 3 and Chapter 4 consists of the essay *Does the Weather Influence Global Stock Returns?* Finally, Chapter 5 concludes and offers future research avenues.

CHAPTER 2

Cultural Differences, Synergies and Mergers and Acquisitions

"When you merge cultures well, value is created."

George Bradt, "The Root Cause of Every Merger's Success or Failure: Culture", Forbes, August 29, 2015.

2.1 Introduction

In a recent article, Graham et al. (2016) report that 78% of the executives they survey believe that corporate culture is one of the principal factors affecting firm value. Consistent with this view, Guiso, Sapienza and Zingales (2015) find that a culture of trust is associated with stronger firm performance. The value-relevance of corporate culture has motivated the creation of a new literature that examines the effects of culture on corporate policies such as CEO turnover (Fiordelisi and Ricci, 2014), financial reporting risk (Davidson, Dey and Smith, 2015), financial stability (McNulty and Akhigbe, 2015), or firm-level investment policy (Pan, Siegel and Wang, 2015).

I extend this literature by exploring the corporate culture effects in mergers and acquisitions (M&As). A priori, it is not clear whether cultural differences impact mergers and acquisitions. On the one hand, there is the notion that managers are generally unaware of corporate culture issues in M&As (AON Hewitt, 2011), and it is hard to argue that acquirers select targets on the basis of their cultural similarity. On the other hand, Graham et al. (2016) report that approximately half of the executives they survey would not acquire a target that is culturally misaligned, although no further definition of cultural misalignment is offered. Furthermore, any test of corporate culture effects is subject to noise in the measurement of corporate culture itself.

Additionally, theory does not have a clear prediction on how cultural differences between the acquirer and target affect both the probability of target selection and synergistic gains from the merger transaction. Indeed, on the one hand, the cultural adversity proposition suggests that a merger of culturally diverse firms produce frictions which could lead to stress, tension and other "soft" negative externalities. Under Tajfel's (1982) and Williams and O'Reilly's (1998) theories, team members demonstrate favoritism towards culturally similar, inner-team members, in opposition to the treatment for outer-group people. This inter-group bias may hinder the sharing of information during post-acquisition integration (for example, Hoffman, McCabe and Smith, 1996). In that scenario, the unobservable integration costs increase with the acquirer-target cultural distance, which could lead, if anticipated at the announcement, to lower announcement returns.

On the other hand, the information processing theory (e.g., Watson et al., 1993) proposes that in culturally diverse teams, team members benefit from a broader range of perspectives, which leads to enhanced problem-solving, creativity, innovation and adaptability. Under that theory, cultural differences are associated with expected operating synergies. The information processing theory thus predicts that a greater cultural distance is associated to higher announcement returns.

Therefore, whether the combining firms' cultural differences are positively or negatively related to wealth effects is ultimately an empirical issue that I examine in this paper. In contrast with previous studies (for instance, Bargeron, Lehn and Smith (2015), Bereskin et al. (2016) and Pan, Siegel and Wang (2015)), I use textual analysis of the acquirers' and targets' annual reports (10-K forms) to develop an estimate of the differences in corporate cultures between the merging entities. I draw on Cameron et al.'s (2006) Competing Values Framework to describe corporate culture in terms of four values, namely, creativity, competition, collaboration and control. *Cultural distance* (*CD*) is thus the Euclidean distance across the four cultural values between an acquirer and its target.

I first examine whether the cultural attributes measures are well calibrated. I use OLS regressions to verify whether the four cultural attributes predict, in turn, R&D expenses, number of employees, profitability and asset turnover. I find that each cultural attribute significantly predicts the observable financial output that is theoretically associated with the attribute, and the economic significance of the correct attribute is the highest. To further support the evidence of non-random distribution of cultural attributes, I do not find such associations in placebo tests where I use a sample of randomly generated cultural attributes. Consistent with the definition of corporate culture, the paper also provides evidence of within-industry variation in cultural attributes.

Using a sample of hypothetical acquirer-target pairs, where targets are matched to real targets by industry, market capitalization and market-to-book ratios of equity, I then test whether differences in corporate cultures between an acquirer and a potential target affect the probability that the potential target will be acquired. Consistent with the survey evidence reported by Graham et al. (2016), I find that potential acquisitions between firms with very different corporate cultures tend to fail the “cultural feasibility” test, and are either not considered or abort before the announcement. However, for the acquisitions between culturally different firms that are announced, the cultural distance is unrelated to the probability of acquisition completion.

Given the selection issues documented, announced acquisitions between culturally different firms must either create value, if cultural distance is related to expected synergies, or destroy value, if cultural distance relates to frictions and managers do not diagnose cultural issues accurately before proceeding to the acquisition. I thus examine the post-announcement wealth effects associated with cultural differences. If cultural effects are anticipated or strongly associated with observable characteristics of the firms, their effects should be realized in full upon the acquisition announcement, or earlier. Otherwise, if cultural effects occur (or are revealed) during the integration phase of the merger, they could impact long-term performance.

I find that differences in corporate cultures positively impact the combined acquirer's and target's short-term announcement returns, a common proxy for synergies in the acquisitions literature. The effect is economically significant: a one-standard deviation increase in cultural distance translates into an increase of about 0.8% in combined three-day abnormal returns, which amounts to approximately 15-20% of the combined abnormal returns. The cultural distance effects remain after controlling for deal and firm characteristics, state-level differences in economic and cultural environments, product market synergies, quality of the acquirer's corporate governance or market-wide policy uncertainty. The positive cultural distance effects remain in longer event windows. For example, high-distance acquirers earn higher six-month CAR than low-distance acquirers. I also find that the cultural effects remain in the longer term (24 and 36 months post-announcement), but endogeneity concerns cast doubts over the causal link between cultural effects and long-run post-acquisition abnormal returns.

To ensure that the reported results are not affected by the selection bias documented above, I use the sample of hypothetical acquirer-target pairs and estimate a two-stage model. The first stage is a probit regression of the probability that a randomly selected pair of firms is a real pair in merger announcements; the second stage is the OLS regression of combined abnormal announcement returns on the cultural distance variable and controls. I find that the cultural distance effects remain even after accounting for the selection bias.

Drawing from the disciplinary takeovers literature, I condition the tests on the acquirer having a stronger culture than its target. Intuitively, if corporate culture is a valuable intangible asset, strong-culture firms should be more profitable and productive than weak-culture firms, and differences in corporate cultures should benefit an acquirer only if the acquirer has the cultural strength to assimilate the target. This thesis is in line with Sørensen (2002), who finds a positive relation between the strength of corporate culture, defined as the intensity with which beliefs are held within the firm, and corporate performance. In other words, if strong-culture firms are more productive, the acquisition of a target by a strong-culture acquirer leads to a transfer of control of the target's assets from the target management to the (more productive and more profitable) strong-culture acquirer management. All else being equal, the transfer of the target's assets to a more productive, culturally strong firm should lead to value creation.

I rank sample firms along their highest (normalized) cultural attributes and define strong-culture (weak-culture) firms as the sample firms where the highest of the firm's normalized cultural attributes is higher than the full sample median. I find evidence that strong-culture firms have higher market share, sales per employee, operating profit per employee and cash flows per employee than weak-culture firms. I also show that the cultural distance effects concentrate in the subsample of strong-culture acquirers, that is, acquirers with stronger corporate culture than their targets'.

However, the relation between cultural differences and post-acquisition performance is not linear. Drawing on one prediction of Cameron et al.'s (2006) Competing Values Framework, I examine whether certain values are incompatible. Cameron et al.'s (2006) framework indeed states that certain pairs of values (e.g. creativity and control) compete against each other, resulting in higher frictions and, ultimately, cultural incompatibility. Consistent with the framework's prediction, the results show that the positive relation between cultural distance and announcement returns is strongest for some levels of compatibility between the acquirer's and target's core values.

The main results of the paper are robust to alternative measures of firm's culture (for example, Hofstede's, 1980, O'Reilly et al.'s, 1991 and Fiordelisi and Ricci's, 2014) and different methodologies of textual analysis. I compare the main findings with those that results from a limited KLD Socrates employee treatment dataset and a dataset of culture variables estimated from text analysis of job reviews web-scrapped from a career intelligence website. However, the alternative two datasets are too limited in size to reach meaningful conclusions.

By documenting a positive relation between combined announcement returns and cultural differences, the announcement returns tests suggest that differences in corporate cultures correlate, at least partially, to expected operating synergies. To confirm this interpretation, I explore the changes in acquirers' post-merger, industry-adjusted operating performance. Changes in post-merger operating performance should be more positive for strong-culture acquirers, consistent with the results that strong-culture acquirers are more productive and profitable firms on average. Regressions of post-merger industry-adjusted operating performance on pre-merger industry-adjusted operating performance provide evidence in line with the interpretation that cultural differences account for a portion of expected synergies. Namely, improvements in operating performance around the merger, measured by operating profit margin or net profit margin, are sharpest for strong-culture acquirers. Similarly, strong-culture acquirers' asset turnover and return on assets decrease less around the merger, relative to weak-culture acquirers.

This paper's contributions are twofold. First, the paper confirms that differences in corporate cultures relate to both the probability of acquisition and wealth effects in mergers and acquisitions. By showing that greater differences in corporate cultures are associated with higher combined announcement returns and post-merger increases in industry-adjusted operating performance, the paper also suggests that differences in corporate cultures correlate with expected operating synergies. Second, the paper confirms the relevance of corporate culture on corporate policies -- in this case, mergers, a finding attributable to the granularity of the *CD* measure (at the firm-level, and available dynamically).

This paper is closely related to the literature that examines the impact of cultural distance on mergers and acquisitions. While Ahern, Daminelli and Fracassi (2015) and Chakrabarti, Gupta-Mukherjee and Jayaraman (2009) consider national cultures, other papers focus on corporate cultures instead. For instance, Bargeron, Lehn and Smith (2015) find that firms included in the Best Companies to Work For (BCW) list make smaller acquisitions and earn slightly larger announcement returns relative to comparable non-BCW firms, while Bereskin et al. (2016), using KLD data that encompass seven categories (community, corporate governance, diversity, employee relations, environment, human rights and products), report a positive relation between cultural similarity and announcement returns.[2] Alexandridis et al. (2015) use a similar measure of corporate culture for a small sample of FTSE firms and find that cultural distance is negatively related to abnormal announcement returns and deal completion probability .

Relatedly, Pan, Wang and Siegel (2015a and 2015b) trace back the cultural heritage of CEOs and executives to compute a firm-level Uncertainty Avoidance Index (UAI) and find that high-UAI firms undertake less acquisitions than low-UAI firms. While Pan, Wang and Siegel's (2015a) focus is on understanding the origin of within-firm commonality in attitudes toward risk, the focus of this paper is on examining the effects of combining the merging firms' workforces, taking the pre-announcement corporate cultures and skillsets as given.

[2] While cultural values may affect a firm's choices in any of these seven categories, Bereskin et al.'s (2016) definition of corporate culture is significantly broader in scope than mine. Additionally, for any individual variable, KLD does not report how strongly a firm emphasizes this dimension, relative to other variables/dimensions. For these two reasons, their results differ from mine.

This paper also contributes to the emerging literature on the relationship between corporate culture and corporate policies. This literature has documented a link between a firm's corporate culture and its propensity to commit accounting fraud (Davidson, Dey and Smith, 2015), its governance (Popadak, 2014), its CEO tenure (Fiordelisi and Ricci, 2014), its corporate performance (Guiso, Sapienza and Zingales, 2015), and its risk-taking behavior (Stulz, 2016). Edmans (2011) and Edmans, Li, and Zhang (2014) also link work satisfaction to stock returns.

Finally, this paper also adds to the literature that examines the source of acquisition-related synergies. For example, Hoberg and Philips (2010) show that product market synergies are a driver of acquisition activity, while Bena and Li (2014) report that the combination of innovation capabilities is another source of acquisition-related synergies. These conclusions are consistent with those of Devos, Kadapakkam and Krishnamurthy (2009), who find that operating synergies are relatively more important than financial synergies. Devos, Kadapakkam and Krishnamurthy's (2009) proxy of expected synergies has great predictive power; unfortunately, its calculation requires firms to be followed by Value Line and is thus limited to large firms. In contrast, cultural distance can be estimated for any pair of public firms; the paper shows that differences in corporate cultures correlate with operating synergies.

The remainder of this paper is structured as follows. Section 2.2 describes the sample and methodological issues. Section 2.3 tests whether cultural distance relates to the probability of two firms merging. Section 2.4 examines the cultural distance effects on announcement returns while Section 2.5 explores the post-merger operating performance. Section 2.6 discusses further tests and Section 2.7 concludes.

2.1 Method and sample

2.2.1 Cultural distance

I follow the existing literature and define culture as a set of assumptions, beliefs and values, from which norms and roles are derived (among others, O'Reilly, 1991; Schein, 2009; Guiso, Sapienza, and Zingales, 2015). Crémer (1993) and Hermalin (2001) differentiate corporate cultures from national cultures by pointing that cross-sectional variations in corporate cultures prevail even at the domestic level and underlining the firm-specific nature of these beliefs and values; commonality in beliefs and values exists however among subsets of firms. Both Crémer (1993) and Hermalin (2001) conclude that corporate culture can be a valuable, intangible asset.

To estimate the cultural distance between an acquirer and its target, I use textual analysis of the bidders' and targets' 10-K forms to extract scores that mirror the relative importance of specific values to the firm. The cultural distance is the Euclidean distance between the acquirers' and targets' scores across all values.

More specifically, I follow Loughran and McDonald (2011) and retrieve 10-K and 10-K405 forms from EDGAR for bidder and target firms involved in a domestic acquisition between 1994 and 2014.[3] I parse the 10-K forms and remove html tags, and stop words.[4] The main results of this paper derive from the textual analysis of the full, parsed 10-K forms, although similar results obtain if I restrict the analysis to the Management Discussion and Analysis (MD&A) section.[5] For

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[3] Firms whose executives did not disclose their insider trading information within the prescribed period are required to file form 10-K405 instead of form 10-K. I assume that any market reaction to the non-disclosure is priced immediately and therefore unlikely to affect significantly my results.

[4] I exclude negation words (no, not, neither, none, nobody, nowhere, nor, never) from stop words in order to make the identification of negative loadings on the culture variables possible; frequency counts of cultural variables that subtract 1 for each cultural word in the vicinity of a negation word are qualitatively similar to simple counts. In fact, Tottie (1991) finds that negation accounts for 1.28% of words in written English. Therefore, even if the negation-adjusted algorithm does not capture all negated attributes, the low frequency of negation makes this non-detection nonconsequential. Results using negation-adjusted variables are therefore not reported.

[5] Results are also robust to imposing the restriction that 10-K forms have at least 2,000 or 3,000 words (Loughran and McDonald, 2014; Li, 2010).

each acquisition, I consider only the bidders' and targets' 10-K forms that are filed before the announcement.[6] Appendix A1 provides more details about the parsing of 10-K forms.

Deciding which values to measure is admittedly an ad hoc decision. Following Bouwman (2013), Fiordelisi and Ricci (2014) and an established literature from the management field[7], I use the values of the Competing Values Framework (CVF; Cameron et al., 2006). The CVF describes a firm's corporate culture along two axes: stability versus flexibility and external focus versus internal focus, and the quadrants defined by the intersection of the axes correspond to four value-driver-specific corporate cultures. The four-dimensional operationalization of the CVF comprehends four values (create, compete, control and collaborate); Figure 2.1 presents a graphical representation of the CVF.

The arbitrariness of the selection of a given cultural framework is mitigated in great part by the extent to which cultural frameworks overlap. For example, Cameron et al.'s (2006) CVF parallels the super-groupings from Schwartz's (1992) Value Inventory. Schwartz indeed classifies values along two axes, namely conservatism versus openness to change (similar to the individual flexibility versus stability control in the CVF) and self-enhancement versus self-transcendence (comparable to the internal versus external focus in the CVF). The CVF values therefore partially reflect those of Schwartz's (1992) inventory, even though the CVF describes corporate culture whereas Schwartz's framework was developed as a characterization of individuals' values (as opposed to an organization).

[6] To minimize the effects that extraordinary or unusual events (such as prior large investments, market crashes, etc.) discussed in the 10-K forms could have on the culture measures, I consider all 10-K forms filed prior to the announcement date, but after 1994. Such assumption is in line with the management literature, which defines corporate culture as the cumulative organizational knowledge that develops through time (for example, Cameron et al., 2006; Schein, 2010).

[7] For example, Hartnell, Ou and Kinicki (2011) perform a meta-analysis on 84 empirical studies that use the CVF; their initial search for CVF-based papers returned 4,637 articles, book chapters, dissertations and conference proceedings for consideration.

As a further robustness test, I also use the values proposed by O'Reilly et al. (1991), namely, adaptability, attention to detail, collaboration, customer orientation, integrity, results-orientation and transparency. Although Hofstede's (1980) framework is now associated with national values, it was originally designed as a study of cross-country variations in within-firm (IBM) corporate cultures. As an additional robustness test, I thus estimate the importance of Hofstede's values – power distance, individualism, masculinity and uncertainty avoidance, for the firms in the sample.

For each value of each framework, I develop a lexical field of words related to this value, following Fiordelisi and Ricci (2014, for the CVF-based values), O'Reilly et al. (1991) and Hofstede (1980). I expand the fields using WordNet thesaurus. Appendix A2 presents the complete lexical fields developed for each value.[8] I follow Tetlock (2007) and compute the frequency of the lexical fields' words in the parsed 10-Ks. I aggregate the frequencies over each value and divide by the total number of words in the parsed 10-K forms.[9] For each cultural dimension, Table 2.1 reports the top twenty words, by average frequency. Words that overlap with the financial lexicon and are expected in a 10-K form are indicated in bold figures.[10]

Table 2.2 reports descriptive statistics of the cultural scores; Panel A provides statistics for the acquirers, whereas Panel B shows statistics for the targets. I find for example that 3.3% of the words in bidders' 10-K forms relates to the value *create*, whereas 6.8%, 9.5% and 5.7% relate to, respectively, the values *compete*, *control* and *collaborate*.[11]

[8] Appendix A2 presents the complete lexical fields; words are stemmed to facilitate the matching with the 10-K forms. Most results are robust to an operationalization of the CVF that uses Fiordelisi and Ricci's (2014) bags of words or a two-dimensional operationalization of the CVF that includes only two values, flexibility and external focus.
[9] I use the frequency counts for simplicity. However, the main results of the paper remain if I use weighted frequencies, following Loughran and McDonald's (2001) term-weighting methodology. Section 2.6.1 has more details.
[10] The main results of this paper remain if I filter out the frequencies of financial words from the cultural scores. (results untabulated).
[11] The raw scores are higher than those reported by Fiordelisi and Ricci (2014), which is due in part to the extended lexical fields that I use. However, the ordering of the individual dimensions with respect to their frequency is respected: compete and control are the dimensions on which firms score the highest, on average, while create is the dimension on which firms score the lowest.

The distributions of the cultural scores are comparable with those reported by Fiordelisi and Ricci (2014), even if the mean cultural scores I find are slightly higher than Fiordelisi and Ricci's. Notably, the maximum scores are very similar. For example, approximately 30% of 10-K words relate to *control*; Fiordelisi and Ricci (2014) report that 33.3% of the words in their sample 10-K forms relate to *control*. These frequencies are significantly higher than the positive and negative sentiment statistics reported by Loughran and McDonald (2011). This discrepancy arises principally because Loughran and McDonald (2011) do not remove stop words, whereas I follow Fiordelisi and Ricci (2014) and eliminate the stop words listed in Appendix A1.

I assess that the value loadings are well calibrated by confirming that each estimated value correlates strongly with an observable output that, according to Cameron et al.'s (2006) framework, should in theory be a manifestation of the measured value. I thus estimate the following OLS regressions: $Output_{it+1} = \alpha + Create_{it} + Collaboration_{it} + Compete_{it} + Control_{it}$, where cultural scores and outputs are measured for each firm-year *it*. Table 2.3 presents the results. The dependent variable is an observable output measured in year *t+1* and is, in turn, the R&D expenses scaled by total assets (Column 1), the number of employees (in thousands) scaled by total assets (Column 2), the earnings before interest and taxes (EBIT) scaled by sales (Column 3) or asset turnover, measured as EBIT scaled by total assets (Column 4). The economic significance of each cultural value is presented [in brackets] and is estimated as the change in the dependent variable associated with a one-standard deviation change in cultural distance, while maintaining the other independent variables at their sample means.

I do not include any control variables in the regressions beyond the four cultural scores because cultural factors are unlikely to be the primary determinants of R&D expenses, number of employees, EBIT or asset turnover. Furthermore, the objective of these calibration tests is not to accurately predict future economic outputs using cultural scores, but rather to confirm that the text-based cultural scores load on the appropriate factors.

Table 2.3 reports that, in accordance with Cameron et al.'s (2006) theory, *Create* is the cultural value with the strongest association with R&D expenses (Column1; economic impact: 0.0589, in contrast with 0.0165 for *Compete* and 0.0102 for *Control*). Similarly, Column 2 shows that *Collaboration* has the strongest association with the number of employees, scaled by total assets (0.1947, versus 0.166 for *Control*). In Column 3, *Compete* is the only cultural value positively and significantly associated with the operating margin, consistent with its description in Cameron et al.'s (2006) framework. Finally, *Control*, which is associated with efficiency, has the strongest association with asset turnover (Column 4, economic impact: 0.1761, versus 0.1468 for *Collaboration*). These associations are all statistically significant at standard levels. Table 2.3's findings thus support the interpretation that the text-based estimates of the firm-level cultural values provide relevant information about the strength of firm-level cultural values.

To ensure that my multivariate results are not driven by the fundamental factors (R&D expenses, number of employees, ROA and profitability) that correlate with cultural values, I add the fundamental factors as control variables in the multivariate regressions of announcement returns (results untabulated) and find that results remain similar. Therefore, cultural attributes are not proxies for economic fundamentals, even though they correlate with their associated observable outputs.

As an additional, ad hoc test, I rank the firms by their respective cultural scores; Appendix A3 shows the top and bottom three firms for each cultural values. I delete repeated firms (multiple 10-Ks filled in different years by a single firm) because cultural scores are not transient from year to year. Consistent with the create definition and the intuition, the top three firms in terms of creativity are in the (life) sciences industry, whereas the bottom three are in the financial industry. Similarly, the highest-ranked firms in terms of control are in industries where an emphasis on efficiency and cost-cutting are important; the lowest-ranked firms are instead in life sciences and technology industries, where the focus is on creating margins. Same-industry firms can rank differently, depending on their business model: for instance, there is a bank in both the highest- and lowest-ranked firms in terms of competitiveness (competitiveness being here a focus on increasing market shares). Taken together, these rankings provide anecdotal evidence that further supports the text-based measures.[12,13]

As the raw frequencies underscore (Tables 2.1 and 2.2), the 10-K forms call for a specific language that overlaps with the *control* lexical field more than with the other values. To attenuate this distortion, I demean the raw scores by subtracting the industry mean score using Fama-French 12-industry classification; the demeaned measures therefore indicate whether a firm emphasizes a given dimension more or less than its peers. Finally, I combine the demeaned scores along various values into a single variable, *cultural distance* (CD), that captures the differences in emphasis that an acquirer and its target put on specific values. Specifically, following, among others, Kogut and Singh (1988), I compute the Euclidean distance between an acquirer's and its target's scores on all values of a given culture framework.[14] For example, using the four-dimensional CVF, I compute the cultural distance (CD) between acquirer *a* and target *t* as:[15]

[12] For each cultural attribute, I also compute the average value by state, and map the results. Although some states rank consistently among the top (bottom) states in terms of cultural strength, there is large variability in the distribution of cultural scores (results untabulated).

[13] I also perform placebo tests to show that firms' cultural scores are not assigned randomly. Appendices A3, A4, and A5 have more details.

[14] The main results of the paper hold if CD is instead calculated as the simple difference between the acquirer's and the target's scores: CD = ($Create_a + Comp_a + Control_a + Coll_a$) - ($Create_t + Comp_t + Control_t + Coll_t$). Section 2.6.1 has more details.

[15] The main results of the paper remain qualitatively similar if CD is computed using the Mahalanobis distance between the vectors of the acquirer's and target's cultural attributes, instead of the Euclidean distance (results untabulated).

$$CD_{CVF}=[(Create_a\text{-}Create_t)^2+(Comp_a\text{-}Comp_t)^2+(Control_a\text{-}Control_t)^2+(Coll_a\text{-}Coll_t)^2]^{0.5}$$

Similarly, I compute a cultural distance variable based on Hofstede's framework (CD_H) and another based on Cameron et al.'s (2006) CVF framework, but estimated using the bags of words published in Fiordelisi and Ricci (2014, CD_{FR}). Panel C of Table 2.2 reports descriptive statistics for CD_{CVF}, notably, that cultural differences are not uniformly distributed. Mean CD (1.699) is considerably larger than the median CD (1.34), and the standard deviation is also relatively large (1.386).[16]

2.2.2 Merger sample and other variables

I extract the acquisitions sample from Thompson's Securities Data Company's (SDC) Mergers and Acquisitions database. Owing to the need to perform textual analysis, I collect data on domestic acquisitions by public U.S. acquirers, from 1994 to 2014 inclusively. I exclude stock repurchases and restrict the sample to acquisitions of public targets. I limit the sample to domestic acquisitions, in order to disentangle the effects of corporate and national cultures. Consistent with the previous literature, I also limit the sample to acquisitions worth at least $50 millions and at least one percent of the acquirer's market capitalization, measured four weeks prior to the announcement. The mode of payment, deal value, announcement and completion dates, completion status, and deal attitude come from SDC. Stock daily returns come from the Center for Research in Security Prices (CRSP) and accounting variables are retrieved from Compustat.

I control for possible product market synergies by using Hoberg and Phillips' (2010) product similarity measure and an indicator variable (*DIV_DEAL*) that equals 1 if the acquirer's two-digit SIC code is the same as the target's, and zero otherwise. I also control for the acquirer's equity

[16] With the exception of the robustness tests, all the tests of this paper are performed using CD_{CVF}. For ease of reading, I thus omit the subscript, unless necessary.

Market-to-Book ratio, calculated using month-end values for the month that immediately precedes the announcement. Observations with negative Market-to-Book ratios are omitted.

State-pair variables control for the potential of a state to attract cross-state investment, relative to the home state, and possible associated synergies. I thus control for the geographical proximity (logarithm of the straight-line distance, in miles, between the capital cities of the states where the acquirer and the target are headquartered), differences in the acquirer's and the target's states' corporate tax rates, differences in Gallup-Healthways' well-being indices, differences in state political allegiance and because Kumar et al. (2011) show that religious beliefs relate to gambling propensity, state-level religious affiliations[17]. Appendix A7 describes the sample construction.

Panel C of Table 2.2 shows that the mean acquisition is large, with a value close to $1.7 billion representing on average 42.5% of the acquirers' pre-announcement market capitalization. However, *REL_SIZE* is heavily skewed; the median deal represents 23.3% of the acquirer's pre-merger market capitalization. This bias toward large acquisitions follows from restricting the sample to public firms and is necessary for studying the impact of potential synergies arising from combining different skillsets: the effects of integrating the target must indeed be large enough to possibly impact returns. To some extent, the sample is similar to that of Devos, Kadapakkam and Krishnamurthy (2009).

2.3 Cultural distance and target selection

Graham et al. (2016) report that approximately half of the executives they survey are reluctant to acquire a firm whose corporate culture is very different. In this section, I thus examine

[17] *Churches and Church Membership in the United States, 1980-2010.* Collected by the Association of Statisticians of American Religious Bodies (ASARB) and distributed by the Association of Religion Data Archives (www.theARDA.com).

empirically whether the differences in corporate cultures affect the probability of two firms merging (or one being acquired by the other).

To perform this test, I create a sample of hypothetical acquirer-target pairs by matching, for each acquirer in the sample, a hypothetical target that is in the same industry (using Fama-French 48-industry classification) as the real target and has a market capitalization, measured four weeks before the announcement, that is between 75% and 125% of the real target's. If there are more than five matches, I select the five closest to the real target in terms of their Market-to-Book ratio of equity.[18] Using the sample of real and hypothetical acquirer-target pairs, I estimate the following logistic model:

Pr(Acq-Target is real = 1) = $\frac{1}{1+e^{-(\alpha+\beta_1 CD_{ijt}+\beta_2 X_{ijt})}}$, where CD_{ijt} is the cultural distance measure for the real or hypothetical acquisition of target *j* by acquirer *i* in year *t*. X_{ijt} is a vector of control variables, all defined in the Appendix A8. Industry -defined using Fama-French's 12-industry classification, year and acquirer's state dummy variables are also included in the model. Standard errors are clustered by industry and year.

Table 2.4 presents the results. Consistent with the Graham et al.'s (2016) survey evidence, I find that a greater distance in corporate culture between two firms is associated with a reduced probability of these firms merging (CD coefficient in Column 1 = -17.03, $p < 0.001$). The cultural distance effect is economically significant: a one-standard deviation increase in the cultural distance, while holding other variables at their sample mean, decreases the probability of the two firms merging by 31.8% (Column 1). The effect remains if control variables for acquirer, target and deal characteristics (Column 2), state-level differences (Column 3) and product market

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[18] I obtain qualitatively similar results if I restrict the hypothetical sample to the best two or ten matches, or if I include all potential target firms that meet the industry and market capitalization criteria (results untabulated).

synergies (Column 4) are included. Although the economic magnitude decreases, the cultural differences effect remains economically important (approximately 10% when all control variables are included). To assess that the cultural distance effect is robust to the choice of the hypothetical sample, I reestimate the model using a sample of hypothetical acquirer-target pairs where the hypothetical target's market capitalization is between 50% and 150% of the real target's, and where the top ten matches (in terms of closeness to the real target's Market-to-Book ratio of equity) are retained. Column 5 presents the results and shows that results are qualitatively similar to those of Columns 1-4. For instance, the economic impact of cultural distance is approximately 8.2%, comparable in magnitude to the economic impact reported in Column 4; the lesser magnitude is consistent with the increased heterogeneity in the potential targets that arises by relaxing selection criteria.

In short, the results are consistent with the interpretation that ex ante, executives are able to accurately estimate their own culture as well as the target's and that they consciously ignore culturally different firms as potential targets. Acquisitions between drastically different firms are therefore either not considered, or the negotiations abort before the merger announcement. In spite of this bias, acquisitions between culturally different firms occur on a periodical basis. In the next section, I examine whether these acquisitions are associated with value creation or destruction.

2.4.1 Cultural differences, cultural distance and post-announcement returns

The previous section showed that even though acquisitions between culturally different firms are less likely to happen, such acquisitions occur periodically. I now consider whether cultural differences are related to inferior or superior post-announcement returns for the acquirers. Using an event study setting mitigates the concerns that the cultural distance measure is co-determined with the dependent variable. In addition, considering combined announcement returns as a measure of expected synergies as perceived by the market allows to expand the sample, since bidders disclose an estimate of expected synergies in less than 20% of the deals (Dutordoir, Rosenboom and Vasconcelos, 2014).

For these tests, I follow Moeller, Schlingemann and Stulz (2005) and limit the sample to acquisitions transactions worth at least $50 million and that represent at least 1% of the acquirer's pre-announcement market capitalization, measured four weeks before the announcement. Announcement returns are calculated using the market model, for the three-day and seven-day windows centered around the event day.[19] Combined acquirer and target announcement returns are the average of the acquirer's and target's abnormal announcement returns, weighted by their respective market capitalization four weeks before the announcement.

I first compare the announcement returns and deal characteristics for high- and low-CD acquisitions. High-CD (Low-CD) acquisitions are the acquisitions with CD_{CVF} above (below) the full-sample CD_{CVF} median. Table 2.5 presents the results of t-tests for differences in means between the two subsamples. Panel A reports that High-CD acquisitions generate higher three- and seven-days acquirer and combined announcement returns. The average acquirer CAR is negative, but less so for high-CD acquirers (difference for three-day acquirer CAR: 0.0149; $p < 0.001$). In contrast, the average target CAR is positive, making the combined CAR positive, significantly more so for high-CD acquirers (difference in three-day combined CAR: 0.0094, $p = 0.0151$).

Panel B shows that the deals announced by high-CD acquirers are undistinguishable from those announced by low-CD acquirers in terms of relative size, acquirers' market-to-book ratio of equity, percentage of diversifying deals, percentage of completed deals, days until the effective

[19] I obtain qualitatively similar results when abnormal returns are calculated as the return in excess of the market return, using CRSP value-weighted index as the market proxy.

date, percentage of shares held at the announcement and form of the merger (that is, merger versus acquisition of assets). The larger announcement returns of high-CD acquirers cannot be attributed to any of these variables.[20] However, high-CD acquirers have larger market capitalization, and they announce fewer pure-stock deals than low-CD acquirers. In addition, there is a higher proportion of unsolicited deals among high-CD deals, relative to low-CD deals (difference: 5.5%; $p < 0.001$). These variables are known determinants of announcement returns; multivariate tests thus include them as control variables.[21]

Table 2.6 shows the results of multivariate OLS regressions of the following model:

$CAR_{it} = \beta_1(CD_{ijt}) + \beta_2(X_{ijt}) + \varepsilon_{it}$, where CAR_{it} are the combined acquirer and target announcement returns. and CD_{ijt} is the cultural distance measure for the acquisition of target *j* by acquirer *i* in year *t*. X_{ijt} is a vector of control variables, all defined in the Appendix A8. Industry - defined using Fama-French's 12-industry classification, and acquirer's state indicator variables are also included in the model. Standard errors are clustered by industry and year. Column 1 shows that the cultural distance, CD_{CVF}, is positively related to three-day CAR (coefficient = 0.072; $p = 0.013$). The cultural distance effect remains if control variables for state-level differences are included (Column 2), if the model is estimated with acquirer state fixed effects or industry fixed effects only (Columns 3 and 4, respectively), or if more deal-level control variables are included

[20] In untabulated tests, I estimate logit regressions of the form $Completed_{it} = \beta_1(CD_{ijt}) + \beta_2(X_{ijt}) + \varepsilon_{it}$, where the dependent variable is equal to one if the acquisition announced by acquirer *i* at time *t* was completed (as recorded by SDC), and zero otherwise. CD_{ijt} is the cultural distance measure for the acquisition of target *j* by acquirer *i* in year *t* and X_{ijt} is a vector of control variables. I find that CD is not significantly related to the deal completion probability. Thus, results of Table 2.6 cannot be attributed to investors pricing in the completion probability of the announced deal.

[21] I also find that high-CD acquirers pay higher fees (in proportion of the deal value) to their financial advisors than low-CD acquirers (difference: 0.114%; $p = 0.065$; results untabulated). However, fees paid are reported unfrequently, which limits the power of tests and reduces sample size. I therefore do not include fees paid in the multivariate tests.

(Column 5). In untabulated tests, I find that cultural distance is also significantly related to seven-day combined CAR.[22]

The economic significance is presented in brackets and calculated as the change in the dependent variable that results from a one-standard deviation change in the cultural distance distribution while holding all the other variables at their sample mean. To better capture the proportions of the cultural effects, the economic significance is scaled by the sample mean three-day combined abnormal returns. The cultural distance effect is economically significant: a one-standard deviation change in CD translates into a 0.7 and 1.0 percent increase in three-day combined announcement returns, which in turn accounts for 13% (Column 5) to 23% (Column 4) of the total three-day abnormal return.

In untabulated regressions, I confirm that the above results are robust to excluding large-loss acquisitions, where large-loss deals are those with losses of at least $1 billion in market capitalization (Moeller, Schlingemann and Stulz, 2005). Results also hold if acquisitions representing less than 5% of the acquirer's pre-announcement market capitalization are excluded.[23] By conducting multiple acquisitions in a short time-frame, repeat acquirers could be more experienced than first-time acquirers, which could translate into a higher CD effect. I confirm that the results hold for the subsample of first-time, or sporadic, acquirers, defined as acquirers not involved in acquisitions during the five years surrounding the merger announcement. Eliminating acquisitions announced during the merger wave of the late 1990s (1998-2000) or

[22] In untabulated tests, I confirm that the CD effect remains qualitatively similar if combined CAR(-2,+2) are used as the dependent variable. In addition, a plot of CAR(-124,124) for high-CD and low-CD acquirers reveals that high-CD and low-CD acquirers' CAR are undistinguishable before the announcement, but following the announcement, high-CD acquirers' CAR slowly trend upwards, while low-CD acquirers' CAR trend downwards (graph available upon request).
[23] Results are marginally insignificant if no minimal size constraint is imposed. However, relaxing the size constraint results in the inclusion of many acquisitions that are too small to significantly affect the acquirer, thus adding noise to the results.

during the financial crisis (2007-2008) improves the results; the results presented are therefore not driven by acquisitions taking place during special market-wide circumstances. In addition, results remain qualitatively similar, though slightly less significant, if I eliminate acquisitions by financial firms.

I also find that cultural scores do not predict firm-level future returns (results untabulated). Consistent with the findings of Cohen, Malloy and Nguyen (2016) and the stability of corporate culture through time, I find that the cultural information that can be inferred from the 10-k forms is already incorporated into prices. In turn, my results highlight that the information about structural changes in corporate cultures such as those that arise from an acquisition create is rapidly reflected into prices.

At first glance, the positive CD effects seem contradictory with both the strong opinion executives voiced in Graham et al.'s (2016) survey and the results of the previous section. In fact, the previous section highlighted that potential acquisitions where the cultural distance is beyond an unobserved critical level are less likely to be considered or announced. To account for this selection bias, Columns 6 and 7 of Table 2.6 estimate a two-stage Heckman model. The first stage estimates a probit regression where the dependent variable is equal to one if the acquirer-target pair is a real acquirer-target pair, and zero otherwise. The second stage estimates an OLS regression similar to the one estimated in Column 2, with the Inverse Mills Ratio of the first stage included as an additional independent variable.

I use the sample of hypothetical acquirer-target pairs for the first stage estimation, where potential targets are firms for which the cultural attributes can be estimated at the announcement date, are in the same industry as the real target (using Fama-French 48-industry classification) and have pre-announcement market capitalization within 50% to 150% of the real target's pre-announcement market capitalization. In addition to state characteristics and potential targets' attributes, I use targets' cumulative returns over the six-month period immediately before the announcement month as an additional independent variable because a firm's recent stock performance might alter its attractiveness to acquirers (Palepu, 1986). Column 6 of Table 2.6 shows that, consistent with the findings of Graham et al. (2016) and the results of the previous section, the cultural distance between two firms is negatively related to the probability that these two firms will merge together (or acquire each other).[24] After accounting for this selection bias, CD is positively related to combined three-day announcement returns (Column 7), confirming previous results; the magnitude of the CD effect in Column 7 is also directly comparable to that of Column 2.

In short, the results support the cultural benefits hypothesis, which is consistent with the interpretation that cultural differences relate to expected synergies. To confirm or infirm this possibility, I examine the potential sources of this cultural distance effect in the following sections.

2.4 Announcement returns

2.4.2 Possible sources of the cultural distance effect

Differences in corporate cultures may be correlated with differences in the firms' product mixes. Table 2.6's regressions control for whether the acquisition is horizontal (that is, same two-digit SIC code) or not. However, Hoberg and Phillips (2010) show that such control variable does not properly account for product market synergies. I thus estimate the baseline OLS regression, but add Hoberg and Phillips' (2010) product market synergies (*Score_HP*) as an additional

[24] The average (median) CD for the sample of potential matches is 2.1 (1.65), versus 1.69 (1.34) for the sample of announced acquisitions.

independent variable.[32] Column 1 of Table 2.7 shows that the cultural distance effect becomes marginally insignificant (coefficient = 0.0632; $p = 0.050$), although the loss in significance could be due partially to the decrease in sample size.

Alternatively, differences in corporate cultures could be related to differences in governance. Indeed, Popadak's (2014) results provide evidence of a link between corporate culture and corporate governance. In Columns 2 to 4 of Table 2.7, I add, in turn, different independent variables that proxy for the quality of the acquirer's corporate governance: number of directors (Column 2), acquirer's entrenchment index (Bebchuk, Cohen and Ferrell, 2009; Column 3) and acquirer's governance index (Gompers, Ishii and Metrick, 2001; Column 4). In all cases, the cultural distance effect remains; corporate governance therefore does not appear to drive the cultural distance effect.[33]

Finally, differences in corporate cultures could appear larger during periods of market-wide uncertainty. To control for this possibility, I develop a high uncertainty indicator variable based on the Economic Policy Uncertainty Index (Baker, Bloom and Davis, 2016), which is a monthly composite index that considers the newspaper coverage of policy-related economic uncertainty, a list of temporary federal tax code provisions and elements from the Federal Reserve Bank of Philadelphia's Survey of Professional Forecasters.[34] More specifically, the *High_uncertainty* indicator variable equals one if month *t*'s index value is greater than the previous 24-months average value of the index, and zero otherwise. Column 5 of Table 2.7 presents the

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[32] Including Hoberg and Phillips' (2010) product market similarity score reduces the sample size, as their publicly available data require firms with pairwise similarities above a given threshold. Data is retrieved from the Hoberg and Phillips library (http://hobergphillips.usc.edu/idata/readme_tnic3.txt).

[33] In untabulated regressions, I control for the difference between the acquirer's and the target's respective *G* scores (Gompers, Ishii and Metrick, 2001). The CD coefficient becomes marginally insignificant, but the drastic reduction in sample size decreases the power of tests. Similarly, including the differences in the number of directors or entrenchment index translates into a sharp reduction in sample size, and makes statistical inference meaningless.

[34] I retrieve data from http://www.policyuncertainty.com/index.html.

results, and shows that the cultural distance effect remains. In untabulated regressions, I confirm that the cultural distance effect also remains if the raw, continuous uncertainty index is used as an independent variable.

In brief, the cultural distance effect documented above cannot be explained by either of the product market synergy, corporate governance or policy uncertainty channels, as differences in corporate cultures remain significantly and positively associated with combined announcement returns. In the next section, I examine which firms benefit from these cultural differences.

2.4.3 Which firms gain from the cultural distance effect?

The results so far assume that the effects of cultural differences are homogeneous across firms. However, it is reasonable to believe that some acquirers are better prepared to integrate a culturally different target. Additionally, there could be some nonlinearities in the cultural distance effects. In this section, I consider these possibilities.

I first examine which acquirers benefit from the acquisition of a culturally different target. The field-based evidence highlights both the reluctance of executives to acquire firms whose corporate culture is perceived as very different and the importance of the cultural integration strategy (Graham et al., 2016; AON Hewitt, 2011). Drawing from the disciplinary takeover literature and from Sørensen (2002) who finds a positive relation between corporate culture strength and operating performance, acquirers whose corporate culture is "stronger" than their target's are presumably in a comparatively better position to integrate their target and capitalize on the cultural differences. For example, extending Wang and Xie's (2009) argument to corporate cultures, if strong-culture firms are more productive and profitable than weak-culture firms, the transfer of control of a weak-culture target to a strong-culture acquirer should create value, as the comparatively more productive firm will now control a larger asset base. In contrast, for weak-culture acquirers, the acquirer-target cultural differences could be irreconcilable, as for example no dominant culture exists, or the dominant culture is not aligned with the acquirer's corporate structures in place.

To investigate this possibility, I first need to define strong- and weak-culture firms. I use the maximum of a firm's four normalized cultural attributes to define cultural strength. More precisely, I rank sample firms along their highest (normalized) cultural attributes and define strong-culture (weak-culture) firms as the sample firms where the highest of the firm's normalized cultural attributes is higher than the full sample median.[35,36] A strong-culture firm should be more productive than a weak-culture firm. I ensure that this definition is reasonable by comparing the productivity of strong- and weak-culture firms. Table 2.8 presents the results: Columns 2 and 3 report average productivity metrics for strong- and weak-culture firms, respectively, while Column 4 presents the results of t-tests for differences in means between strong- and weak-culture firms.

On average, firms with above-median maximum cultural score have higher market share than firms with below-median maximum cultural scores ($p < 0.001$ for all differences). However, strong-culture firms are not more profitable than weak-culture firms (difference in ROA and EBIT margins: 0.0091 and 0.0478; $p = 0.166$ and 0.515, respectively) and even though they have higher cash flows (scaled by assets), the difference is not statistically significant ($p = 0.149$). Also, consistent with the definition of corporate culture being an intangible asset that, if used correctly, improves productivity, Table 2.8 shows that strong-culture firms generate higher sales, EBIT and

[35] This definition implies that strong-culture firms place more emphasis on cultural values (one, or many) than their peers. It does not imply that a given culture is better than another, however. Whether a culture is better depends on the cultural strength, but also the congruence between the culture, the firm and its environment.
[36] The results remain if I instead define culture strength as the principal component of the four individual cultural values, or if I define culturally strong firms as those where the sum of their normalized cultural attributes is higher than the sample median (or higher than the sum of the normalized cultural attributes of their target).

cash flows per employee than weak-culture firms ($p < 0.001$ for all differences, with the exception of the difference for cash flows per employees, which has $p = 0.005$). Building and maintaining such intangible is not free, however: strong-culture firms also record higher Sales, General and Administrative expenses per employee than weak-culture firms. Overall, it seems that the strong-culture definition identifies correctly the firms with highest productivity per employee.

The question that remains, then, is whether strong-culture firms are better prepared to integrate a culturally distant target. To answer this question, I slightly modify the strong-culture definition: in the remainder of the paper, strong-culture (weak-culture) acquirers are those whose highest normalized cultural attribute (that is, the maximum of create, compete, control and collaborate) is higher (lower) than their target's highest, normalized cultural attribute. [37,38] I then repeat the tests of Table 2.6, but splitting the sample into the acquisitions where the acquirer is a strong-culture acquirer and those where the acquirer is a weak-culture one.

Table 2.9 presents the results of the OLS regressions of three-day combined announcement returns on the cultural distance and control variables, conditioning on the acquirer being a strong- or weak-culture one.[39] As in Table 2.6, acquirer state and industry dummies are included, and standard errors are clustered by industry and year. Columns 1 to 4 show the results for the subsample of strong-culture acquirers; in that subsample, cultural distance is positive and significant, even after controlling for deal, acquirer, target, state and product market characteristics. The cultural distance effect remains economically significant: a one-standard

[37] Collaboration is the most important cultural attribute for approximately 20% of the acquirers, while Compete, Control and Create are the most important cultural attributes for 35%, 37% and 7% of the acquirers, respectively. Targets have similar distribution of highest attributes.
[38] It is likely that some so-defined strong-culture acquirers have a maximum cultural score that falls below the full sample median maximum cultural score, but these misclassifications are not frequent and only add noise to the tests.
[39] Results remain qualitatively similar if I estimate a pooled model with a strong-culture indicator variable and an interaction term for the interaction of CD and strong-culture. However, conditioning the results allows to better contrast the different patterns.

deviation increase in cultural distance, while maintaining the other independent variables at their sample mean, translates into an increase in combined abnormal announcement returns of approximately 0.8 percent, which amounts to about 35% of the average three-day combined abnormal returns (for bids by strong-culture acquirers only). In contrast, in the subsample of weak-culture acquirers (that is, acquirers whose total cultural score is less than their target's; Columns 5 to 8), the cultural distance effect is insignificant.

In untabulated tests, I confirm that strong-culture acquirers do not pursue diversifying acquisitions more often than weak-culture acquirers, nor do they formulate more pure-stock offers than the weak-culture acquirers. Therefore, differences in offer characteristics cannot explain why the cultural distance effect concentrates in the subsample of strong-culture acquirers. I also replicate the results, increasing the minimum relative size of the target to exclude, in turn, deals that represent less than 5%, 10%, 20%, 30% and 40% of the acquirer's pre-announcement market capitalization. In all cases, the general pattern remains and the CD effect is strongest in the subsample of strong-culture acquirers. However, and possibly due to the reduction in tests power associated with progressively smaller sample sizes, the CD effect at times becomes marginally significant.

I also confirm that the CD effect is not driven by differences in operating profitability, efficiency (proxied by asset turnover), R&D expenses, number of employees (results untabulated). Furthermore, in line with Baruch and Lev (1996), I use the ratio of advertising expenses to sales as a proxy for brand value. I set missing advertising expenses to zero to maintain sample size and I show that the corporate culture effects that I document do not completely overlap with differences in brand values. The CD coefficient for example is 0.4397 (p = 0.0289) for the subsample of culturally strong acquirers, while it is insignificant for the subsample of culturally weak acquirers. Similarly, I repeat the tests controlling for toeholds, merger form (merger versus acquisition of assets), unsolicited deals and whether the merger is a merger of equals. Results are qualitatively similar to those tabulated.

The results support the claim that pre-existing cultural strength is required for the acquirers to successfully integrate a culturally distant target. Counting with a stronger intangible asset gives strong-culture acquirers the latitude to merge with riskier targets, where risk comes from cultural differences. An examination of the changes in the acquirer's corporate culture around the merger provides additional evidence. In line with Bargeron, Lehn and Smith (2015), I find that the acquirers in the strong culture subsample experience a weakening of their culture in the two years following the acquisition, consistent with a loss of identity coming from the integration of a different (and typically, weaker culture) target. The acquirers in the weak culture subsample also experience a degradation in their culture following the acquisition, although of lesser magnitude than the strong-culture acquirers. For example, estimating pre-acquisition cultural scores as the weighted average of the acquirers' and its target's highest normalized cultural score, weighted by the acquirer's and target's pre-announcement market capitalizations, I find that the average decrease for strong-culture acquirers is 2.61 units in the two years following the acquisition, a larger change than in the subsample of weak-culture acquirers (decrease: 2.27 units; *p-value* of the difference in means: 0.036).[40,41]

Thus, strong-culture acquirers benefit from merging with culturally different firms, as evidenced by the positive cultural distance effect. However, it is not clear whether the cultural

[40] However, I find that for large acquisitions (where the target's market capitalization is at least 5% of the acquirer's pre-announcement market capitalization), the post-merger decrease in cultural score is smaller for acquirers whose culture is stronger than their target's.
[41] On average, cultural scores recover three years after the merger announcement.

benefits are homogeneous along the cultural distance, or whether there are some nonlinearities in the relation. I turn to this next.

A possible nonlinearity comes from incompatible corporate values. In fact, I exploit one prediction of Cameron et al. (2006), namely, that acquirer-target pairs where the entities' core values are opposite are likely to be competing against each other. Bouwman (2013) hypothesizes that such mergers are likely to underperform mergers between non-opposite entities. To explore this avenue, I split the sample into strong- and weak-culture acquirers, as before, and re-estimate the OLS regressions, with two additional independent variables, *Opposite* and the interaction of *Opposite* and CD. *Opposite* is an indicator variable that takes the value 1 if, for each acquirer-target pair, one of the following is true: 1) the acquirer's dominant trait is create and the target's dominant trait is control; 2) the acquirer's dominant trait is control and the target's dominant trait is create; 3) the acquirer's dominant trait is compete and the target's dominant trait is collaboration; or 4) the acquirer's dominant trait is collaboration and the target's dominant trait is compete. Otherwise, that is, if the acquirer's dominant trait is not diagonally opposing the target's dominant trait, *Opposite* is equal to zero.

Columns 4 and 8 of Table 2.9 present the results. First, Column 4 shows that the CD effect becomes marginally insignificant for strong-culture acquirers (coefficient = 0.1173; p = 0.064); Columns 8 confirms that the CD effect is insignificant for weak-culture acquirers. Turning to the Opposite variable and the interaction term, the insignificant Opposite coefficients in both Columns 4 and 8 report that acquisitions between firms with opposite core values do not outperform the other acquisitions. Therefore, the results support the interpretation that cultural differences benefit merging firms, but only to a certain point; merging firms with competing core values do not benefit from cultural differences. Power of these tests is however limited by the small number of Opposite deals, in particular in the subsample of strong-culture acquirers (72 deals, but only 40 for which full accounting information is available). Tests for difference in means reveal that Opposite deals are slightly more prevalent in the subsample of weak-culture acquirers (15.7% of weak deals versus 12.4% of strong deals; $p = 0.095$). In other words, the results of Table 2.9 point to two sources of the aggregate cultural distance effect: benefits are more important the greater the cultural differences, but only if the acquirer is culturally stronger than its target, and there is some commonality or compatibility in core values.

A possible explanation for the cultural differences effect is that acquirers merging with a culturally distant target complete a more thorough due diligence, including possibly cultural due diligence, and are therefore better prepared to transform differences into value. I briefly consider this explanation. Practitioners insist on the importance of due diligence, including cultural due diligence, for the success of a merger (AON Hewitt, 2011). To proxy for the cultural due diligence, I estimate the intensity of acquirer-target in-person meetings during the merger negotiation phase. I retrieve the section "Background of the Merger" from forms DEFM14A, DEFM14C, PREM14A and PREM14C that were filed within three months of the acquisition announcement.[42] The meeting intensity measure is the frequency of words that relate to person-to-person interactions.

Untabulated results show that approximately 34% of the acquirers-target pairs with above-median cultural distance meet more frequently during the pre-merger negotiations than the acquirer-target pairs with below-median cultural distance (*p-value* of the t-tests for difference in means = 0.006). Acquirers whose core values are not at odds with their target's (non-opposite acquisitions) also meet more frequently during the negotiations phase (difference in means:

[42] For many acquirer-target pairs, forms DEFM14A, DEFM14C, PREM14A and PREM14C were not filed within three months of the acquisitions or were not available (for example, if the proposed merger did not require a vote). For this part of the analysis, there is thus a considerable decrease in sample size.

0.1239; p = 0.019). Thus, although I perform no formal tests of causality, preliminary evidence suggests that pre-merger interaction between the acquirer and its future target could help acquirers transform cultural differences into synergies.

2.4.3.1 Long-run returns

Because information regarding the cultural (in)compatibility of merging firms could be revealed during the integration process, I examine the effect of cultural distance on the acquirers' long-run buy-and-hold abnormal returns. The buy-and-hold abnormal returns (BHAR) are the differences between acquirers' buy-and-hold 24- or 36-month returns and the compound return of an equally-weighted portfolio matched on size and book-to-market. The 24- and 36-month horizon reflects a trade-off: the horizon needs to be sufficiently long to capture the post-integration "new normal", yet selecting a too long horizon decreases the likelihood that the observed performance is related to the merger.

I estimate OLS regressions of the form $BHAR_{it} = \beta_1(CD_{ijt}) + \beta_2(X_{ijt}) + \varepsilon_{it}$, where $BHAR_{it}$ are the acquirer i's 24- or 36-month BHAR starting on month $t+1$, CD_{ijt} is the cultural distance between acquirer i and target j for the deal announced at time t, and X_{ijt} is a vector of deal, acquirer, Appendix A9 presents the results.

Similar to short-term returns, long-run returns are positively related to cultural distance. Without conditioning on the cultural strength of the acquirer, CD is positively related to both 24-months and 36-months BHAR (coefficients: 1.664 and 3.556, respectively; p = 0.004 and 0.001). Using the same measure of cultural strength as above, I find that the effect is driven by acquirers that are stronger culturally than their target. Indeed, Columns 2 and 5 repeat the tests for the subsample of culturally strong acquirers and find that CD is positively related to acquirers' 24-month and 36-month BHARs (coefficients: 2.076 and 4.076, respectively; $p < 0.001$ in both cases).

However, in the subsample of culturally weak acquirers, cultural distance does not relate to 24-month BHAR. An exception to the finding that only strong-culture acquirers benefit from cultural distance arises with the 36-month BHAR (Column 6, CD coefficient: 3.265; $p = 0.020$). However, the interpretation of these results is subject to the possibility of unobserved factors increasing over longer-term horizons. Still, because the effect is smaller in magnitude than for the strong-culture acquirers, the evidence thus generally supports the cultural benefits interpretation that relates cultural distance positively to merger performance, but only if the acquirer is culturally stronger than its target.

Ultimately, if cultural differences benefit acquirers, as the results suggest, strong-culture acquirers merging with culturally distant targets should see their operating performance improve in the years following the acquisition. I turn to this in the next section.

2.5 Operating performance

To examine whether strong-culture acquirers benefit from the acquisition of culturally distant targets, I compare the acquirers' pre- and post-merger operating performance. More specifically, in the spirit of Healy, Palepu and Ruback (1992) and Fu, Lin and Officer (2013), I regress the industry-adjusted median operating performance from years +1 to +2 (relative to the year of the acquisition) on the industry-adjusted median operating performance from years -2 to -1, again relative to the acquisition year. The intercept thus represents the change in operating performance that can be attributed to the merger.[43]

[43] The results remain if the mean (instead of median) pre- and post-merger operating performances are contrasted, or if the pre- or post-merger windows are increased to 36 months, or decreased to 12 months. The results are also robust to the addition of the logarithm of the acquirer's pre-announcement market capitalization, acquirer's pre-announcement market-to-book ratio of equity and acquirer's pre-announcement leverage ratio as independent variables (results untabulated).

The pre-merger operating performance is the weighted average of the acquirer's and target's operating performance, where the weights are their respective market capitalization four weeks before the merger announcement. If the acquirer and target are from different industries (using Fama-French 48 industries classification), the industry benchmark is also adjusted, using the pre-acquisition acquirer and target market capitalizations as weights. The industry benchmark is the average operating performance of the ten, same-industry, closest firms to the acquirer in terms of market capitalization and market-to-book ratio of equity.[44] Different operating performance metrics are used: return on assets, operating profits (scaled by sales), net income (scaled by sales), asset turnover (ratio of EBIT to total assets) and the ratio of net income per employee. All accounting ratios are winsorized at the 5th and 95th percentiles (but results remain qualitatively the same, and are in fact more contrasted, if ratios are instead winsorized at the 1st and 99th percentiles).

In line with the evidence reported above, I first split the sample into strong- and weak-culture acquirers, where strong-culture (weak-culture) acquirers are those whose total cultural score is above (below) their target's. Consistent with the interpretation that (strong) corporate culture is a valuable intangible asset, Table 2.8 reported that strong-culture firms are more profitable and productive than weak-culture firms. In the context of mergers, the post-merger improvements in operating performance should thus be sharper for strong-culture acquirers.

Panels A and B of Table 2.10 report that this is indeed the case: While the ROA of strong-culture acquirers does not significantly change in the two years following the merger, that of weak-culture acquirers decreases significantly (coefficient = -0.012; p = 0.067). Both the operating and net profit margins increase in the post-merger period for both the strong- and weak-culture

[44] Results are qualitatively similar if I restrict the number of comparable firms to five, or if I do not impose size limits.

acquirers, although the increase is larger for strong-culture acquirers (considering for example the net profit margin, 0.253 for strong-culture acquirers versus 0.035 for weak-culture acquirers, both significant at 1%). Consistent with the decrease in ROA, the asset turnover decreases in the post-merger period for both the strong- and weak-culture acquirers, most likely driven by the increased asset base. However, the decrease is sharpest for weak-culture acquirers (-0.024 versus -0.018 for strong-culture acquirers). In short, Panels A and B of Table 2.10 provide evidence that acquirers with a stronger corporate culture perform better in the two years following the merger announcement than the acquirers with weaker culture.

Table 2.9, Columns 4 and 8, reports a nonlinearity in the cultural distance effect. Indeed, it shows that the cultural distance effect decreases when the acquirer's and target's core values are opposite (or competing, in the CVF nomenclature). Consistent with those tests, I examine whether strong-culture acquirers that merge with a target whose core values are not opposite to the acquirers' register better post-announcement operating performance than those strong-culture acquirers merging with a competing target. Statistical inferences are however limited by the reduced number of opposite acquisitions performed by strong-culture acquirers (only 31 observations for which industry-adjusted operating performance between years *t-2* and *t+2* is not missing). Nonetheless, I find that strong-culture acquirers that merge with a non-competing target report higher post-merger improvements in operating and net profit margins (respectively, 0.357 and 0.375, both significant at 1%, versus 0.025 (p = 0.133) and 0.061 ($p < 0.001$) for the opposite subsample), relative to strong-culture acquirers merging with a competing target (results untabulated).

In short, the operating performance regressions provide evidence that further support the findings of the previous section. Notably, I find that culturally stronger acquirers improve their industry-adjusted operating performance in the two years following the acquisition, relative to their pre-acquisition industry-adjusted performance. I also find that differences in the acquirer's and target's corporate cultures matter. Consistent with the previous section's results that report a positive cultural distance effect, I find here that strong-culture acquirers, that is, acquirers with the cultural strength to integrate a culturally distant target, indeed benefit more from the acquisition of a culturally distant target. In fact, strong-culture acquirers report larger improvements in post-announcement operating performance, provided that there is some level of compatibility in core values.

2.6 Further tests

2.6.1 Alternative culture measures

As noted above, the decision to use the cultural values from Cameron et al.'s (2006) Competing Values Framework is justified by the framework's prominence in the managerial sciences and by its natural prediction regarding acquisitions between firms whose core values are opposed. To assess the robustness of the previous results to the use of alternative cultural frameworks, Columns 1 to 3 of Table 2.11 estimate OLS regressions of three-day combined announcement returns on CD and control variables, with alternative cultural distance measures: In Column 1, CD is estimated using Cameron et al.'s (2006) framework, but lexical fields are from Fiordelisi and Ricci (2014). Column 2 uses CD_H as the cultural distance proxy, where the cultural attributes are from Hofstede's (1980) framework, whereas Column 3 uses $CD_{O'Reilly}$, where the cultural attributes are from O'Reilly et al.'s (1991) framework.[45] Columns 1 to 3's results are qualitatively similar to those reported in Table 2.6; the CD effect is therefore not contingent on the specific cultural values of a framework.

[45] Appendix A2 reports the lexical fields associated with each of these frameworks.

In Column 4, I express the cultural attributes as proportions of the total cultural stock before calculating CD: $(Create_a/Tot_a + Control_a/Tot_a + Collaboration_a/Tot_a + Compete_a/Tot_a) - (Create_t/Tot_t + Control_t/Tot_t + Collaboration_t/Tot_t + Compete_t/Tot_t)$, where Tot_a (Tot_t) is the sum of the acquirer's (target's) cultural attributes. Column 4 reports the results; consistent with the previous results, the CD effect is positive. In Column 5, I modify the textual analysis methodology used to compute the cultural scores. Loughran and McDonald (2011) show that weighting terms by their relative frequency in the full document affects their measure of text-based sentiment. I thus use Loughran and McDonald's (2011) term-weighting methodology (instead of the simple frequency counts) when computing the individual cultural scores. Cultural distance is otherwise computed as described in Section 2.2.1. Column 5 shows that the CD effect remains even when using the term-weighting methodology.

A question of interest is whether the direction of the cultural distance is important. To examine this issue, I depart from the literature and measure CD as $(Create_a + Control_a + Collaboration_a + Compete_a) - (Create_t + Control_t + Collaboration_t + Compete_t)$. Column 6 estimates the OLS regression of three-day combined announcement returns on CD and control variables for the subsample of positive CD, whereas Column 7 does the same for the subsample of observations with negative CD. Consistent with the results of Section 2.4.3, the CD effect is positive, but only for the subsample where the acquirer has a stronger culture than the target. These results are in line with the synergy interpretation: greater cultural distance are associated with greater announcement returns, but only when the acquirer has the better culture and, therefore, the necessary intangible assets to successfully integrate the target.

Another possibility is that the cultural scores capture the degree of transparency of the firm, instead of the cultural dimensions of the firm. To control for this possibility, I develop a lexical field associated with transparency.[46] I then calculate cultural scores as before, but I remove words that also associate with transparency; the cultural distance, net of transparency words, is calculated as detailed in Section 2.2.1. I confirm that results remain qualitatively similar using the cultural distance, net of transparency words (results untabulated). For example, the CD coefficients in regressions similar to those reported in Columns 1 and 2 of Table 2.6 are 0.0938 ($p = 0.0011$) and 0.0748 ($p < 0.001$), respectively. Therefore, the cultural scores do not appear to capture the disclosure environment of the firm.

Finally, I repeat the tests using each one of the four cultural values individually (results untabulated). I find that differences in acquirer-target's competitiveness drive the cultural distance effects, as differences in competitiveness are positively related to combined abnormal three-day announcement returns, both for the full sample (coefficient = 0.0065; $p < 0.001$) and the subsample of strong-culture acquirers (coefficient = 0.0119; $p = 0.028$). Differences in other cultural attributes are positively signed, but their magnitude and statistical significance are secondary to differences in competitiveness. These results are consistent with the argument that the cultural distance effects originate in more productive, strong-cultured acquirers taking control of comparatively weak-cultured targets; synergies come from the strong-culture acquirers controlling an increased asset base.

[46] The transparency lexical field includes the following words: accessible, advertised, announced, apparent, available, broadcast, clear, comprehendible, comprehensible, decipherable, declared, discernable, discernible, disclosed, distinct, divulged, evident, explicit, exposed, graspable, intelligible, knowable, legible, limpid, lucid, luminous, manifest, nonambiguous, noticeable, observable, obvious, open, palpable, plain, posted, prevalent, proclaimed, promulgated, public, publicized, readable, reported, reputed, revealed, see-through, self-evident, self-explanatory, sharing, straightforward, translucent, transparent, unambiguous, uncomplicated, understandable, undisguised, unequivalent, unfolded, unlocked, unmistakable, unsealed, visible, well-defined.

2.6.2 Alternative sources of written text

To confirm that the cultural scores are not driven by noise from the scripted parts of the 10-K forms, I compute cultural scores for each one of Cameron et al.'s (2006) four dimensions using only the Management's Discussion and Analysis sections of the 10-K forms. Computation of cultural scores and cultural distance are otherwise exactly as described in Section 2.2. Untabulated results confirm that using the whole 10-K forms to extract cultural scores does not distort the cultural distance effects. Magnitude and significance of the cultural distance coefficients are directly comparable to their equivalent in Table 2.6.

As a further robustness test, I repeat the tests using corporate culture scores estimated from earnings conference calls. Because the Q&A section of earnings conference calls is not scripted, unlike firms' annual reports, the culture scores inferred from the conference calls are possibly more representative of a firm's corporate culture. I retrieve the transcripts of earnings conference calls from Fair Disclosure Wire and estimate culture scores as described in Section 2.1, using the full transcripts (scripted and Q&A portions together). Because the earliest calls in the Fair Disclosure Wire database are from 2004, the final sample has fewer observations than the full sample used in the main tests. However, in spite of the small sample, I find that the correlation among individual cultural scores is positive and significant for all four attributes. The correlation between 10k-based *Create* and conference call-based *Create* is 0.368, while the correlation coefficients are 0.182, 0.259 and 0.329 for the *Compete*, *Control* and *Collaboration* attributes, respectively ($p < 0.001$ for all four correlations). Also, I find that multivariate results are in line with those of Table 8. For example, replicating the restricted models of Columns 1 and 5, I find that the CD coefficient is 0.0836 ($p = 0.0336$) for the strong-culture acquirers and 0.0943 ($p = 0.4224$; results untabulated) for the acquirers whose culture is weaker than their targets'.

Therefore, the text of the conference calls, including their non-scripted portion, seems to accurately reflect firms' corporate culture.

The use of forms 10-K to infer a firm's corporate culture rests on the assumption of within-firm homogeneity in corporate culture. However, if multiple cultures coexist within a firm, the corporate culture inferred from forms 10-K may reflect the management's culture (or view thereof) better than the rank-and-file employees' culture. Similar to Popadak (2014), I retrieve job reviews written by current and former employees from a career intelligence website. I compute the cultural scores exactly as described in Section 2.2, but using the free-form portion of the job review only. Because the career intelligence website has limited coverage (the earliest reviews were published in 2011), the sample size reduces dramatically to 63 observations for which combined CAR and CD are non-missing.

In untabulated tests, I find that there is no statistical difference between the three- or seven-day combined announcement returns of the high-CD and low-CD acquisitions. This can be due to within-firm cultural heterogeneity, but most likely, the lack of significance is due to the small sample size. Considering reviews published *after* the announcement date greatly increases the sample size, but assumes that corporate events as significant as large acquisitions are unlikely to change a firm's culture (an assumption invalidated by Popadak, 2014), and raises endogeneity concerns. As the number of published reviews will grow through time, the review-based estimates of culture should be more precise and numerous; examination of the relation between employees' opinions and merger effects is a topic for future research.

2.6.3 Employee treatment versus corporate culture

Possibly, there is a natural overlap between corporate culture and employee treatment. Employees in firms with better employee relations may feel differently towards their employer and their position, which could in turn affect the firm's culture. To investigate this issue further, I follow Bae, Kang and Wang (2011) and retrieve data from KLD Socrates (now part of MSCI).[47] I compute, for each firm-year, an employee treatment score by summing the scores over five categories of employee relations: union relations, cash profit-sharing, employee involvement in stock options plans, strength of the retirement benefits and health and safety strength. The total Employee Treatment index ranges between zero and five. The acquirer-target treatment distance is the absolute value of the difference in Employee Treatment indices.[48]

Due to the small sample size, I again limit the analysis to a comparison of combined announcement returns between high- and low-CD acquisitions, where high- and low-CD acquisitions are, respectively, the acquisitions with above-median (below-median) employee treatment distance. In untabulated results, I find that high-treatment distance acquisitions generate lower combined announcement returns than acquisitions where both the acquirer's and target' employees are treated comparably (difference: -0.018; p = 0.055). The contrasting effects of the Employee Treatment distance could be due to limitations of my KLD dataset or differences in methodologies (for instance, KLD's score on any individual dimension records only the existence, and not the magnitude, of the employee relation dimension). Alternatively, treatment distance is possibly not a substitute for cultural differences, and both variables capture different intangibles.

2.7 Conclusion

Using a sample of domestic acquisitions of public firms announced between 1994 and 2014, I examine whether cultural distance between an acquire and its target is related to the

[47] A partially-complete employee relations dataset is retrieved owing to subscriptions rights.
[48] I also compute the treatment distance as the Euclidean distance over the five employee treatment categories. The untabulated results are similar to those described, but slightly weaker.

combined announcement returns. To estimate the cultural distance of an acquisition, I use textual analysis of the firms' 10-K forms to extract scores that reflect the relative importance of specific values (e.g. collaboration, competitiveness, etc.) for each firm. The cultural distance, CD, is the Euclidean distance between the acquirer's and the target's pre-announcement cultural value scores.

Consistent with the survey evidence of Graham et al. (2016) and using a sample of hypothetical acquirer-target pairs where the targets are matched on industry, market capitalization and market-to-book ratios of equity, I first document that differences in corporate cultures between two firms negatively relate to the probability of the target firm being selected as a target. On average, and assuming that executives accurately diagnose corporate cultures, mergers between culturally distant firms will either never be seriously considered, or the negotiations abort before the announcement. The announced mergers and acquisitions thus need to clear an unobserved "cultural feasibility" threshold.

In the sample of announced acquisitions, I find that greater differences between the acquirer's and the target's cultures are positively associated with the combined acquirers' and targets' three-day abnormal stock returns. The cultural distance effect remains after controlling for product market synergies, corporate governance quality of the acquirers, market-wide policy uncertainty and the selection bias documented above. After providing evidence that a strong corporate culture is a valuable intangible, I show that the cultural distance effect concentrates in the subsample of strong-culture acquirers. Intuitively, only acquirers with a clear cultural identity are prepared to integrate a target with vastly different norms and values.

Consistent with the interpretation that the positive association between cultural distance and announcement returns should translate into operating synergies, I find that strong-culture acquirers merging with a culturally distant targets register larger post-merger improvements in operating performance (operating and net profit margins) than strong-culture acquirers merging with culturally similar targets.

In short, the paper contributes to the literature by showing that corporate culture matters. More specifically, the paper documents that corporate culture is a valuable intangible asset that affects the probability of being acquired. Conditioning on the acquirer having a stronger corporate culture than its target, the paper finds that differences in corporate cultures are positively related to combined announcement returns and post-merger improvements in operating performance. Therefore, thanks to the granularity of the CD measure (at the firm-level, and available dynamically), the paper confirms the relevance of corporate culture for mergers and more broadly, for corporate policies, thus contributing to the burgeoning field of culture and finance.

Figure 2.1. Competing Values Framework

Individual flexibility

Internal maintenance, focus and integration	Culture type: CLAN Orientation: **COLLABORATE** Value drivers: Commitment Communication Development	Culture type: ADHOCRACY Orientation: **CREATE** Value drivers: Agility Innovation Transformation	*External positioning, focus and differentiation*
	Culture type: HIERARCHY Orientation: **CONTROL** Value drivers: Efficiency Timeliness Consistency Uniformity	Culture type: MARKET Orientation: **COMPETE** Value drivers: Market share Goal Achievement Profitability	

Stability control

Source: Cameron et al. (2006), Figure 3.1.

Table 2.1. Frequency of Individual Cultural Words

Create		Compete		Collaboration		Control	
Word	%	Word	%	Word	%	Word	%
Style	0.228	Agreement	0.396	**Share**	0.413	Operations	0.396
New	0.210	End	0.348	Service	0.374	Require	0.281
Change	0.165	**Market**	0.241	Relation	0.232	Manage	0.248
Develop	0.122	Result	0.221	Inform	0.168	**Value**	0.242
Future	0.107	**Invest**	0.165	Receive	0.151	Productive	0.231
Risk	0.083	**Revenues**	0.144	Certitude	0.150	Usage	0.184
Design	0.072	**Benefit**	0.114	Partnership	0.134	Control	0.167
Make	0.063	Performance	0.110	Standard	0.117	Additional	0.161
Adjust	0.060	Customer	0.107	**Chief**	0.089	Employee	0.142
Original	0.059	Accord	0.096	Association	0.073	Grant	0.099
Establish	0.052	Earn	0.091	Document	0.072	Conditional	0.097
Begin	0.052	Present	0.083	Participation	0.072	Accord	0.096
Initiation	0.039	Charge	0.068	Affiliation	0.058	Reason	0.087
Institute	0.032	Acquire	0.065	Federation	0.066	Facilitator	0.086
Generate	0.031	Position	0.058	Connection	0.056	Expectation	0.070
Venture	0.027	Direct	0.052	Method	0.041	Procedure	0.062
Produce	0.022	**Gain**	0.051	Work	0.038	Address	0.058
Commence	0.021	**Profit**	0.043	Supportive	0.033	Approval	0.056
Install	0.019	**Return**	0.035	Combination	0.031	Process	0.052
Experiment	0.019	Signal	0.032	Involvement	0.031	Direct	0.052

This table reports, for each cultural dimension, the top twenty words in terms of average frequency, where frequency is calculated as the occurrences of a word in an individual 10-K form, divided by the total number of words in the parsed 10-K form.
Bolded words indicate words that overlap with the financial lexicon.

Table 2.2. Descriptive Statistics

Panel A. Acquirers characteristics						
	Mean (%)	Median (%)	Min (%)	Max (%)	Std. Dev.	N
CREATE	3.347	2.718	0.237	12.435	2.361	1209
COMPETE	6.784	5.820	0.337	20.091	4.436	1209
CONTROL	9.509	8.085	0.869	30.264	6.213	1209
COLLABORATION	5.665	4.773	0.436	18.630	3.689	1209

Panel B. Target characteristics						
	Mean (%)	Median (%)	Min (%)	Max (%)	Std. Dev.	N
CREATE	2.997	2.381	0.054	12.739	2.285	1209
COMPETE	6.039	4.866	0.164	20.842	4.409	1209
CONTROL	8.545	7.008	0.206	30.751	6.207	1209
COLLABORATION	5.003	3.988	0.095	19.946	3.593	1209

Panel C. Deal characteristics				
	Mean	Median	Std. Dev.	N
CD	0.176	0.119	0.167	1209
CAR (-1,1), acquirer	-0.018	-0.012	0.071	1201
CAR (-3,3), acquirer	-0.025	-0.018	0.098	1201
Combined CAR (-1,1)	0.021	0.011	0.066	1172
Combined CAR (-3,3)	0.030	0.023	0.089	1167
DEAL_VALUE	1.695	0.391	4.810	1209
REL_SIZE	0.425	0.233	0.557	1012
MB, acquirer	4.087	2.677	3.620	1012
PURE_STOCK	0.356	0.000	0.479	1209
SCORE_HP	0.085	0.070	0.069	944
GEO_DISTANCE	4.575	6.081	3.142	1209
DIFF_WB	1.560	1.792	1.254	1209
DIFF_TAX	0.788	0.688	0.751	1183
SAME_POLITIC	0.728	1.000	0.445	1209
SAME_REL	0.712	1.000	0.453	1209

This table provides descriptive statistics. Panel A reports cultural scores for acquirers and acquirers' characteristics, and Panel B reports cultural scores for targets. Panel C provides statistics at the deal level, including cultural variables, post-acquisition returns, deal characteristics and distance variables. Variables descriptions are in Appendix 2.

Table 2.3. Calibration of Cultural Attributes

	Dependent variable			
	R&D/AT (1)	Employees/AT (2)	EBIT/Sale (3)	Turnover (4)
CREATE	**0.1911**	-0.3532	-0.0055	-0.0132
	(<0.001)	(0.0163)	(0.9967)	(0.9137)
	[0.0589]	[-0.1088]	[-0.0017]	[-0.0040]
COLLABORATION	0.002	**0.4686**	0.0266	0.3448
	(0.8305)	**(0.0005)**	(0.9825)	(0.0011)
	[0.0008]	**[0.1947]**	[0.0110]	[0.1468]
COMPETE	0.0451	0.0236	**2.4980**	0.2079
	(<0.001)	(0.8704)	**(0.0551)**	(0.0703)
	[0.0165]	[0.0086]	**[0.9138]**	[0.0786]
CONTROL	0.0174	0.2843	-2.8019	**0.2973**
	(0.0181)	(0.0069)	(0.0031)	**(0.0003)**
	[0.0102]	[0.1660]	[-1.6364]	**[0.1761]**
Intercept	-0.1318	-0.2378	1.4574	-0.2066
	(<0.001)	(0.0678)	(0.2127)	(0.0427)
R-Square	0.1919	0.0248	0.0029	0.0422
N	2171	2047	2165	2171

This Table reports the results of the OLS estimation of the following model:
$Output_{it+1} = \alpha + Create_{it} + Collaboration_{it} + Compete_{it} + Control_{it}$, where cultural scores and outputs are measured for each firm-year *it*.
Each column uses a different output variable. In Column 1, the dependent variable is the R&D expenses scaled by total assets. Column 2's dependent variable is the number of employees (in thousands), scaled by total assets. Column 3 uses the Earnings Before Interest and Taxes (EBIT), scaled by total sales, and in Column 4, the dependent variable is the asset turnover, measured as EBIT scaled by total assets.

The number of observations and R-squared of each regression are also reported. *P*-values (in parentheses) and economic significance [in brackets] are also reported. Economic significance is calculated as the change in the dependent variable that is associated with a one-standard deviation change in the independent variables, while maintaining the other independent variables at their sample mean. Boldfaced coefficients and associated *p*-values and economic significance indicate the cultural value with the highest economic significance in each model.

Table 2.4. Cultural Distance and the Probability of Merging

	(1)	(2)	(3)	(4)	(5)
CD	**-17.033**	**-16.6598**	**-16.6204**	**-16.0973**	**-11.2738**
	(0.0000)	**(0.0000)**	**(0.0000)**	**(0.0000)**	**(0.0000)**
	[0.3183]	**[0.4101]**	**[0.1316]**	**[0.0988]**	**[0.0818]**
REL_SIZE	.	**-0.2304**	**-0.3589**	**-0.4117**	**-0.4051**
		(0.0000)	**(0.0000)**	**(0.0006)**	**(0.0003)**
MKT_CAP	.	**0.0426**	**0.0657**	0.0342	-0.0078
		(0.0223)	**(0.0426)**	(0.5046)	(0.8046)
DIV	.	0.3399	0.1756	0.2037	0.1665
		(0.1731)	(0.5182)	(0.4418)	(0.5101)
MB	.	-0.0231	-0.0369	-0.0338	-0.0205
		(0.0005)	**(0.0000)**	**(0.0000)**	**(0.0001)**
MB_TARGET	.	-0.0009	0.0014	-0.002	0.0085
		(0.9530)	(0.9503)	(0.9405)	(0.6757)
LOG_WB	.	.	**-0.1505**	**-0.1524**	**-0.1006**
			(0.0090)	**(0.0001)**	**(0.0006)**
DIFF_TAX	.	.	**-0.1166**	-0.1261	0.0096
			(0.0500)	(0.1078)	(0.8364)
SAME_POLITIC	.	.	0.0831	**0.1517**	**0.1852**
			(0.3552)	**(0.0051)**	**(0.0000)**
SAME_RELIGIOUS	.	.	0.0629	0.0737	0.101
			(0.5897)	(0.5672)	(0.4118)
GEO_DISTANCE	.	.	**-0.2037**	**-0.2141**	**-0.2144**
			(0.0032)	**(0.0185)**	**(0.0137)**
SCORE_HP	.	.	.	**-0.8449**	**-0.469**
				(0.0054)	**(0.0703)**
INTERCEPT	**1.8429**	**1.5142**	**2.7004**	**2.9117**	**1.8535**
	(0.0001)	**(0.0000)**	**(0.0000)**	**(0.0000)**	**(0.0000)**
Pseudo R-Sq.	(0.3693)	(0.3618)	(0.4001)	(0.3969)	(0.2145)
N	5257	5106	4985	3816	6877
Industry Dummies	Y	Y	Y	Y	Y
Year Dummies	Y	Y	Y	Y	Y
State Dummies	Y	Y	Y	Y	Y

This table reports the results of the logistic regressions where the dependent variable is equal to 1 if the acquirer-target pair announced a merger, and zero otherwise (that is, if the acquirer-target pair is drawn from the sample of hypothetical mergers). Hypothetical acquirer-target pairs are formed by matching acquirers with potential targets, where the potential targets are in the same Fama-French 48-industry as the real targets and have a pre-announcement market capitalization between 75% and 125% of the real target's pre-announcement market capitalization (between 50% and 150% of real target's pre-announcement market capitalization for Column 5 matches). If there are more than 5 matches, potential targets are sorted by how close their pre-announcement market-book ratio of equity is to the real target's pre-announcement market-book ratio, and the 5 closest matches are retained. Column 5 retains the top 10 matches.
Independent variables are defined in Appendix A8. The number of observations and pseudo R-squared of each regression are also reported. *P*-values (in parentheses) are calculated using standard errors clustered by both industry and year. Economic significance [in brackets] is calculated as the change in the dependent variable that results from a one-standard deviation change in cultural distance, while maintaining the other independent variables at their sample means. Year and acquirers' industry and state indicator variables are included in the regressions. Boldfaced coefficients and associated *p*-values indicate statistical significance at the 5% level or higher.

Table 2.5. Announcement Returns and Characteristics of High-CD and Low-CD Acquisitions

Panel A. Announcement Returns

	Full Sample	High CD	Low CD	High CD - Low CD
ACQUIRER CAR (-1,1)	-0.0184	-0.0109	-0.0259	**0.0149**
				(0.0002)
ACQUIRER CAR (-3,3)	-0.0248	-0.019	-0.0306	**0.0115**
				(0.0409)
COMBINED CAR (-1,1)	0.0212	0.0259	0.0165	**0.0094**
				(0.0151)
COMBINED CAR (-3,3)	0.0296	0.0332	0.026	0.0072
				(0.1632)

Panel B. Deal and Acquirer Characteristics

	Full Sample	High CD	Low CD	High CD - Low CD
REL_SIZE	0.4251	0.392	0.4587	**-0.0667**
				(0.0566)
MKTCAP	10.8376	14.2244	7.4452	**6.7792**
				(0.0001)
MB	4.0923	3.9012	4.2865	**-0.3853**
				(0.0921)
MB_TARGET	2.7392	2.76	2.7181	0.0419
				(0.7745)
PURE_STOCK	0.3557	0.2562	0.4553	**-0.1991**
				(0.0000)
PURE_CASH	0.2746	0.3669	0.1821	**0.1848**
				(0.0000)
DIV_DEAL	0.2878	0.2843	0.2914	-0.0071
				(0.7856)
FRIENDLY	0.9363	0.9174	0.9553	**-0.0379**
				(0.0069)
COMPLETED	0.8743	0.8562	0.8924	**-0.0362**
				(0.0578)
DAYS_TO_COMPLETE	133.4759	128.6815	138.0835	**-9.402**
				(0.0668)
TOEHOLD	0.8649	0.8871	0.8429	0.0443
				(0.9103)
UNSOLICITED	0.0775	0.1082	0.0468	**0.0614**
				(0.0001)
MERGER FORM	0.9596	0.9612	0.958	0.0032
				(0.7730)

This table reports descriptive statistics. Panel A presents statistics about the acquirer and combined acquirer and target abnormal announcement returns, whereas Panel B reports statistics about deal and acquirer characteristics. All variables are defined in the Appendix A8.
Statistics are presented for the full sample, and for the High-CD and Low-CD acquisitions. High-CD (Low-CD) acquisitions are those with CD above (below) the full-sample median CD value. The last column presents the results of t-tests for the differences in means between the High-CD and Low-CD subsamples. *P*-values are presented in parentheses. Boldfaced coefficients indicate statistical significance at the 10% level or higher.

Table 2.6. Combined Announcement Abnormal Returns

Dependent variable	CAR(-1,1)	CAR(-1,1)	CAR(-1,1)	CAR(-1,1)	CAR(-1,1)	$1_{\text{real merger pair}}$ First Stage	CAR(-1,1) Second Stage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CD	**0.0716**	**0.0718**	**0.0569**	**0.0868**	**0.0487**	**-0.5940**	**0.0380**
	(0.0130)	**(0.0000)**	**(0.0118)**	**(0.0001)**	**(0.0190)**	**(0.000)**	**(0.008)**
	[0.1905]	**[0.1912]**	**[0.1514]**	**[0.2310]**	**[0.1297]**		
REL_SIZE	0.0089	0.0079	0.0094	0.0091	0.0101	0.0116	**0.0090**
	(0.2424)	(0.3188)	(0.2209)	(0.2272)	(0.1733)	(0.742)	**(0.007)**
MKT_CAP	**-0.0057**	**-0.006**	**-0.0055**	**-0.0067**	**-0.0054**	**0.0767**	**-0.0101**
	(0.0000)	**(0.0000)**	**(0.0000)**	**(0.0000)**	**(0.0000)**	**(0.000)**	**(0.000)**
PURE_STOCK	-0.0074	-0.0012	-0.0063	-0.0015	-0.0046		**-0.0113**
	(0.3484)	(0.8668)	(0.4085)	(0.8475)	(0.5412)		**(0.044)**
PURE_CASH	0.0104	**0.0115**	**0.0103**	0.0099	**0.011**		**0.0101**
	(0.0737)	**(0.0270)**	**(0.0471)**	(0.0842)	**(0.0214)**		**(0.013)**
DIV_DEAL	0.0027	-0.0015	0.0014	0.0004	0.0015	0.2501	0.0009
	(0.6972)	(0.8336)	(0.8416)	(0.9485)	(0.8188)	(0.054)	(0.950)
FRIENDLY	0.007	0.0099	0.0088	0.0086	0.0186		0.0078
	(0.4523)	(0.3175)	(0.3796)	(0.3884)	(0.1069)		(0.234)
MB	0.0001	0	0	0	0	**-0.0118**	-0.0002
	(0.7763)	(0.9907)	(0.9678)	(0.9864)	(0.9941)	**(0.000)**	(0.762)
MB_TARGET	0.0003	0.0002	0.0003	0.0003	0.0003	**-0.0122**	**0.0012**
	(0.3115)	(0.4356)	(0.2992)	(0.5047)	(0.3508)	**(0.025)**	**(0.006)**
DIFF_WB		0.0026	0.0035	0.0019	0.0033	**-0.0533**	0.0045
		(0.3515)	(0.2035)	(0.4750)	(0.2420)	**(0.000)**	(0.164)
DIFF_TAX		-0.0001	-0.0001	-0.0011	-0.0004	-0.0286	-0.0024
		(0.9865)	(0.9848)	(0.7767)	(0.9369)	(0.111)	(0.579)
SAME_POLITIC		0.0048	0.0041	0.0075	0.0045	0.0713	-0.0029
		(0.3455)	(0.4217)	(0.0517)	(0.3630)	(0.087)	(0.656)
SAME_REL		**0.0105**	**0.0098**	**0.0068**	**0.0101**	0.0131	0.0048
		(0.0000)	**(0.0000)**	**(0.0422)**	**(0.0000)**	(0.812)	(0.528)

Table 2.6 (Continued). Combined Announcement Abnormal Returns

GEO_DISTANCE		0.0008	0.0007	0.0014	0.0004		0.0008
		(0.6453)	(0.6832)	(0.3677)	(0.8214)		(0.6453)
TOEHOLD					0.0002		
					(0.4117)		
MERGER FORM					**0.0352**		
					(0.0011)		
UNSOLICITED					**0.0189**		
					(0.0033)		
MERGER OF EQUALS					-0.0326		
					(0.0556)		
PAST_RETURNS							
IMR							
INTERCEPT	**0.0372**	0.0152	0.0129	0.032	**-0.0316**	**0.0372**	0.0152
	(0.0140)	(0.2895)	(0.1635)	(0.0511)	**(0.0008)**	**(0.0140)**	(0.2895)
R-square	(0.0838)	(0.1154)	(0.0891)	(0.0798)	(0.1038)	(0.0838)	(0.1154)
N	1224	1166	1166	1166	1155	1224	1166
State Dummies	Y	Y	Y	N	Y	Y	Y
Industry Dummies	Y	Y	N	Y	Y	Y	Y

This table presents the OLS estimation of the model: *Post-Bid Abnormal Return*$_{it} = \beta_1(CD_{ijt}) + \beta_2(X_{ijt}) + \varepsilon_{it}$
In Columns 1 through 5, the dependent variable is the combined cumulative three-day abnormal returns (CAR[-1,1]). Columns 6 and 7 present the results of the two-stage Heckman estimation: Column 6 presents the results of the first-stage probit regression, where the dependent variable is an indicator variable that equals one if an acquirer-target pair is a pair of firms that announced an acquisition, and zero otherwise. Column 7 presents the results of the second-stage OLS regression, where the dependent variable is the three-day combined abnormal announcement returns.
X_{ijt} is a vector of acquirer and target deal control variables and includes: relative size of the acquisition, acquirers' pre-announcement market capitalization, pure stock offer indicator, pure cash offer indicator, diversifying deal indicator, friendly deal indicator, acquirer's pre-announcement market-to-book ratio of equity, target's pre-announcement equity market-to-book ratio, state-level differences in well-being index, state-level differences in marginal corporate tax rate, state-level difference in state GDP, differences in the state-wide proportion of Catholics, state-level , indicator variable, logarithm of the distance in miles between state capitals. All variables are defined in Appendix A8.

The number of observations and R-squared of each regression are also reported. *P*-values (in parentheses) are calculated using standard errors clustered by both industry and year, except in the two-stage model where errors are clustered by industry only. Economic significance [in brackets] is calculated as the change in the dependent variable that results from a one-standard deviation change in cultural distance, while maintaining the other independent variables at their sample means. Acquirers' industry and state indicator variables are included in the regressions. Boldfaced coefficients and associated *p*-values indicate statistical significance at the 10% level or higher.

Table 2.7. Possible Sources of the CD Effect.

	(1)	(2)	(3)	(4)	(5)
CD	0.0632	**0.0551**	**0.0734**	**0.0694**	**0.0696**
	(0.0501)	**(0.0061)**	**(0.0000)**	**(0.0000)**	**(0.0000)**
	[0.1681]	**[0.1466]**	**[0.1954]**	**[0.1848]**	**[0.1853]**
SCORE_HP	-0.0084				
	(0.8009)				
N_DIRECTORS_ACQ		**0.0024**			
		(0.0021)			
E_INDEX_ACQ			0.0022		
			(0.3969)		
G_INDEX				-0.0005	
				(0.1432)	
HIGH_UNCERTAINTY					**-0.0045**
					(0.0394)
REL_SIZE	0.0018	0.0073	0.0081	0.0078	0.008
	(0.8539)	(0.3703)	(0.3152)	(0.3242)	(0.3136)
MKT_CAP	**-0.0057**	**-0.0083**	**-0.0062**	**-0.0059**	**-0.0061**
	(0.0017)	**(0.0000)**	**(0.0000)**	**(0.0000)**	**(0.0000)**
PURE_STOCK	-0.002	-0.0016	-0.0013	-0.0009	-0.0009
	(0.7790)	(0.8098)	(0.8522)	(0.8959)	(0.8966)
PURE_CASH	0.0097	**0.0113**	**0.0116**	**0.0115**	**0.0119**
	(0.1140)	**(0.0342)**	**(0.0295)**	**(0.0257)**	**(0.0222)**
DIV_DEAL	-0.0016	-0.0005	-0.0015	-0.0014	-0.0016
	(0.7536)	(0.9454)	(0.8332)	(0.8359)	(0.8215)
FRIENDLY	-0.0147	0.0099	0.0095	0.0099	0.0098
	(0.2699)	(0.3138)	(0.3335)	(0.3110)	(0.3201)
MB	-0.0003	0.0001	0	0	0
	(0.6455)	(0.9002)	(0.9929)	(0.9824)	(0.9950)
MB_TARGET	0.0002	0.0003	0.0002	0.0002	0.0002
	(0.3321)	(0.3709)	(0.4400)	(0.4679)	(0.4339)
DIFF_WB	**0.0056**	0.0031	0.0026	0.0026	0.0026
	(0.0366)	(0.2671)	(0.3534)	(0.3400)	(0.3509)
DIFF_TAX	-0.0025	-0.0003	0.0002	0	-0.0001
	(0.7622)	(0.9705)	(0.9715)	(0.9955)	(0.9856)
SAME_POLITIC	**0.0129**	0.0056	0.0052	0.0048	0.0049
	(0.0323)	(0.2468)	(0.3591)	(0.3447)	(0.3363)
SAME_REL	**0.0075**	**0.0107**	**0.0106**	**0.0106**	**0.0107**
	(0.0036)	**(0.0000)**	**(0.0000)**	**(0.0000)**	**(0.0000)**
GEO_DISTANCE	0.0008	0.0007	0.0007	0.0008	0.0008
	(0.6928)	(0.6726)	(0.6766)	(0.6511)	(0.6366)
INTERCEPT	**0.0362**	0.0106	0.0166	0.0164	0.0171
	(0.0299)	(0.5262)	(0.2462)	(0.2185)	(0.2584)

Table 2.7 (Continued). Possible Sources of the CD Effect.

R-square	(0.1436)	(0.1242)	(0.1162)	(0.1158)	(0.1163)
N	855	1166	1166	1166	1166
Industry Dummies?	Y	Y	Y	Y	Y
State Dummies?	Y	Y	Y	Y	Y

This table presents the OLS estimation of the model: *Post-Bid Abnormal Return*$_{it} = \beta_1(CD_{ijt}) + \beta_2(X_{ijt}) + \varepsilon_{it}$
The dependent variable is the combined cumulative three-day abnormal returns (CAR[-1,1]). X_{ijt} is a vector of acquirer and target deal control variables and includes: relative size of the acquisition, acquirers' pre-announcement market capitalization, pure stock offer indicator, pure cash offer indicator, diversifying deal indicator, friendly deal indicator, acquirer's pre-announcement market-to-book ratio of equity, target's pre-announcement equity market-to-book ratio, state-level differences in well-being index, state-level differences in marginal corporate tax rate, state-level difference in state GDP, differences in the state-wide proportion of Catholics, state-level , indicator variable, logarithm of the distance in miles between state capitals. All variables are defined in Appendix A8.
Each column adds, in turn, another explanatory variable: Column 1 uses Hoberg-Philips (2010) product market similarity measure; Column 2 uses the acquirers' number of directors; Column 3 includes Bebchuk, Cohen and Ferrell (2009) entrenchment index for the acquirers; and Column 4 adds Gompers, Ishii and Metrick index (2003) for the acquirers.

The number of observations and R-squared of each regression are also reported. *P*-values (in parentheses) are calculated using standard errors clustered by both industry and year. Economic significance [in brackets] is calculated as the change in the dependent variable that results from a one-standard deviation change in cultural distance, while maintaining the other independent variables at their sample means. Acquirers' industry and state indicator variables are included in the regressions. Boldfaced coefficients and associated *p*-values indicate statistical significance at the 10% level or higher.

Table 2.8. Operating Performance of Firms with Stronger Culture.

	Full Sample	Strong Culture	Weak Culture	Strong Culture - Weak Culture	
ROA	0.0292	0.0337	0.0246	0.0091	(0.1660)
Cash Flow / Total Assets	0.0373	0.0438	0.0309	0.0129	(0.1487)
Sales per employee	366.97	407.87	325.99	81.87	**(0.0000)**
EBIT per employee	36.69	49.29	24.08	25.21	**(0.0006)**
Cash Flow per employee	41.20	50.25	32.12	18.12	**(0.0054)**
SGA per employee	69.71	77.20	62.21	14.99	**(0.0000)**
EBIT margin	-0.0408	-0.019	-0.0627	0.0438	(0.5508)
Employees / Total Assets	0.0034	0.0031	0.0037	-0.0006	**(0.0012)**
Market share	0.0015	0.0016	0.0014	0.0002	**(0.0072)**
Culture	3.3771	5.9422	0.8072	5.1351	**(0.0000)**
N	3227	1615	1612	.	

This table reports descriptive statistics about the operating performance for the full sample firms (Column 1).
ROA is the return on assets, calculated as EBIT/Total assets. Cash Flows are the sum of EBIT and Depreciation. SGA are the Sales, General and Administrative expenses. EBIT margin is the ratio of EBIT/Sales. Market share is calculated as the ratio of the firm's yearly sales, divided by the industry's total sales for that same period. Fama-French 48-industry classification is used.

Columns 2 and 3 present operating performance measures for the high-culture and low-culture subsamples, where high (low) culture firms are those where max(create, compete, control, collaborate) is above (below) the full-sample's median. All cultural attributes are normalized before computing the maximum value. The last column presents the results of t-tests for the differences in means between the High-Culture and Low-Culture subsamples. *P*-values are presented in parentheses. Boldfaced coefficients and associated *p*-values indicate statistical significance at the 10% level or higher.

Table 2.9. Combined Announcement Returns, for Strong- and Weak-Culture Acquirers

	Strong Culture					Weak Culture		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CD	**0.1221**	**0.1278**	**0.1542**	0.1173	0.0211	-0.0075	-0.0762	-0.0187
	(0.0010)	**(0.0065)**	**(0.0000)**	(0.0637)	(0.6062)	(0.7686)	(0.0690)	(0.2964)
	[0.3437]	**[0.3599]**	**[0.4341]**	[0.3302]	[0.0532]	[0.0188]	[0.1924]	[0.0472]
REL_SIZE	0.0068	0.0066	-0.0021	0.0064	0.0127	0.0146	0.0108	0.0146
	(0.4939)	(0.5051)	(0.8403)	(0.5111)	(0.1329)	(0.1165)	(0.4062)	(0.1187)
MKT_CAP	**-0.0084**	**-0.0086**	**-0.009**	**-0.0089**	**-0.0027**	**-0.0023**	-0.002	**-0.0023**
	(0.0004)	**(0.0005)**	**(0.0000)**	**(0.0003)**	**(0.0304)**	**(0.0232)**	(0.3663)	**(0.0373)**
PURE_STOCK	0.0068	0.0072	0.007	0.0073	**-0.0211**	**-0.0195**	-0.0147	**-0.0195**
	(0.5584)	(0.5705)	(0.5277)	(0.5544)	**(0.0147)**	**(0.0386)**	(0.0531)	**(0.0400)**
PURE_CASH	**0.0181**	0.016	0.0215	0.0157	**0.0077**	**0.0089**	0.0083	**0.0087**
	(0.0387)	(0.1216)	(0.0621)	(0.1169)	**(0.0016)**	**(0.0075)**	(0.2924)	**(0.0103)**
DIV_DEAL	-0.0008	-0.001	0.002	-0.0014	0.0068	0.0044	0.003	0.0044
	(0.9193)	(0.9064)	(0.7146)	(0.8674)	(0.3770)	(0.5650)	(0.7467)	(0.5678)
FRIENDLY	-0.0193	-0.0125	-0.015	-0.0117	**0.0347**	**0.0338**	-0.0049	**0.0339**
	(0.0857)	(0.2122)	(0.3102)	(0.2415)	**(0.0207)**	**(0.0436)**	(0.7178)	**(0.0412)**
MB	0.0005	0.0004	0.0004	0.0004	0	-0.0002	-0.0003	-0.0001
	(0.1274)	(0.1920)	(0.3273)	(0.1762)	(0.9591)	(0.8212)	(0.6739)	(0.8327)
MB_TARGET	0.0005	0.0005	**0.0002**	0.0005	0.0001	0.0002	0.0005	0.0002
	(0.2589)	(0.2600)	**(0.0047)**	(0.2334)	(0.8550)	(0.7404)	(0.3959)	(0.7425)
DIFF_WB		0.0039	**0.0116**	0.0041		0.0039	0.0062	0.004
		(0.2197)	**(0.0070)**	(0.1994)		(0.3370)	(0.2964)	(0.3142)
DIFF_TAX		0.0012	-0.0027	0.0007		0.0008	-0.0019	0.0011
		(0.8548)	(0.7263)	(0.9109)		(0.9144)	(0.8211)	(0.8804)
SAME_POLITIC		0.0016	0.0108	0.0012		0.0022	0.0038	0.0022
		(0.8311)	(0.2004)	(0.8694)		(0.7870)	(0.6252)	(0.7948)
SAME_REL		0.0014	-0.0061	0.0009		**0.017**	**0.0205**	**0.0173**
		(0.8904)	(0.4996)	(0.9235)		**(0.0033)**	**(0.0014)**	**(0.0025)**
GEO_DISTANCE		-0.0005	-0.002	-0.0004		0.0008	0.0014	0.0007
		(0.1996)	(0.1772)	(0.3985)		(0.7288)	(0.5859)	(0.7643)

Table 2.9 (Cont.). Combined Announcement Returns, for Strong- and Weak-Culture Acquirers

SCORE_HP			-0.0047				-0.0491	
			(0.8712)				(0.5787)	
OPPOSITE				0.0049				-0.0024
				(0.5637)				(0.8418)
*OPPOSITE * CD*				0.0553				0.0525
				(0.7007)				(0.5470)
INTERCEPT	**0.0751**	0.0525	**0.0511**	0.0529	-0.0075	-0.0279	0.011	-0.0277
	(0.0054)	(0.0570)	**(0.0368)**	(0.0672)	(0.6762)	(0.2313)	(0.6699)	(0.2558)
R-square	(0.1499)	(0.1497)	(0.1906)	(0.1521)	(0.1061)	(0.1154)	(0.1569)	(0.1157)
N	620	598	452	598	599	564	403	564
State Dummies	Y	Y	Y	Y	Y	Y	Y	Y
Industry Dummies	Y	Y	Y	Y	Y	Y	Y	Y

This table presents the OLS estimation of the model: *Post-Bid Abnormal Return*$_{it} = \beta_1(CD_{ijt}) + \beta_2(X_{ijt}) + \varepsilon_{it}$

The dependent variable is the combined cumulative three-day abnormal returns (CAR[-1,1]). X_{ijt} is a vector of acquirer and target deal control variables and includes: relative size of the acquisition, acquirers' pre-announcement market capitalization, pure stock offer indicator, pure cash offer indicator, diversifying deal indicator, friendly deal indicator, acquirer's pre-announcement market-to-book ratio of equity, target's pre-announcement equity market-to-book ratio, state-level differences in well-being index, state-level differences in marginal corporate tax rate, state-level difference in state GDP, differences in the state-wide proportion of Catholics, state-level , indicator variable, logarithm of the distance in miles between state capitals. All variables are defined in Appendix A8.

Columns 1 to 4 present the results for the strong-culture subsample, whereas Columns 5 to 8 present the results for the weak-culture subsamples. Strong (weak) culture acquirers are those where the *max*(create, compete, control, collaborate) is larger (smaller) than the their target's *max*(create, compete, control, collaborate). All cultural attributes are normalized before computing the maximum value.

The number of observations and R-squared of each regression are also reported. *P*-values (in parentheses) are calculated using standard errors clustered by both industry and year. Economic significance [in brackets] is calculated as the change in the dependent variable that results from a one-standard deviation change in cultural distance, while maintaining the other independent variables at their sample means. Acquirers' industry and state indicator variables are included in the regressions. Boldfaced coefficients and associated *p*-values indicate statistical significance at the 10% level or higher.

Table 2.10. Post-Announcement Operating Performance

	Panel A: Strong-Culture Acquirers				*Panel B: Weak-Culture Acquirers*			
	ROA	Op. Profits	Net Income	Turnover	ROA	Op. Profits	Net Income	Turnover
Intercept	-0.0007	**0.2528**	**0.2822**	**-0.0181**	**-0.0123**	**0.0351**	**0.0178**	**-0.024**
	(0.8734)	**(0.0021)**	**(0.0010)**	**(0.0056)**	**(0.0669)**	**(0.0000)**	**(0.0392)**	**(0.0000)**
Past performance	**0.6311**	**0.8624**	**0.7968**	**0.7976**	**0.6288**	**0.9064**	**0.8916**	**0.884**
	(0.0000)	**(0.0000)**	**(0.0000)**	**(0.0000)**	**(0.0000)**	**(0.0000)**	**(0.0000)**	**(0.0000)**
R-Square	(0.4508)	(0.7828)	(0.7508)	(0.7197)	(0.4374)	(0.7347)	(0.7263)	(0.7655)
N	526	526	526	526	414	414	414	414

This table presents the OLS estimation of the post-announcement operating performance on past performance. As in Healy, Palepu and Ruback (1992), the post-announcement operating performance is the firm-level industry-adjusted median operating performance from years +0 to +2 (relative to the year of the acquisition). The pre-merger operating performance is the industry-adjusted weighted average of the acquirer's and target's operating performance, and if the acquirer and target are from different industries (using Fama-French 48 industries), the industry benchmark is adjusted, using the pre-acquisition acquirer and target market capitalization as weights. Industry-adjusted operating performance is firm *i*'s operating performance, minus the average operating performance of the ten closest same-industry firms by Market-Book ratios of equity, where matched firms have capitalization between 50% and 150% of firm *i*'s.

Panel A presents the results for the subsample of strong-culture acquirers whereas Panel B reports the results for the subsample of weak-culture acquirers. Strong (weak) culture acquirers are those where the *max*(create, compete, control, collaborate) is larger (smaller) than the their target's *max*(create, compete, control, collaborate). All cultural attributes are normalized before computing the maximum value. ROA is the ratio of EBIT to total assets, Operating Profits is the ratio of EBIT to sales and Net Income is the ratio of Net Income to sales. Turnover is the ratio of sales to total assets. NI/Emp is the ratio of net income to the number of employees.

The number of observations and R-squared of each regression are also reported. *P*-values (in parentheses) are calculated using standard errors clustered by both industry and year. Year dummies are included in the regressions. Boldfaced coefficients and associated *p*-values indicate statistical significance at the 5% level or higher.

Table 2.11. Robustness Tests: Alternative Measures of CD

Culture	CD_{FR}	CD_{H}	$CD_{O'Reilly}$	$CD_{CVF, vect}$	$CD_{CVF, term-weighted}$	$CD_{CVF} > 0$	$CD_{CVF} < 0$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CD	**0.008**	**0.007**	**0.6002**	**0.0748**	**0.0008**	**0.1072**	0.0034
	(0.0017)	**(0.0156)**	**(0.0001)**	**(0.0000)**	**(0.0006)**	**(0.0334)**	(0.7622)
	[0.2738]	**[0.1713]**	**[0.2069]**	**[0.1942]**	**[0.3345]**	**[0.4722]**	[0.0157]
REL_SIZE	0.0075	0.0079	0.0077	0.008	0.008	-0.0027	0.0134
	(0.3492)	(0.3179)	(0.3301)	(0.3178)	(0.3121)	(0.7304)	(0.0926)
MKT_CAP	**-0.0062**	**-0.0061**	**-0.0061**	**-0.006**	**-0.0064**	**-0.0082**	**-0.0034**
	(0.0000)	**(0.0000)**	**(0.0000)**	**(0.0000)**	**(0.0000)**	**(0.0000)**	**(0.0024)**
PURE_STOCK	0	-0.0015	-0.0016	-0.0011	-0.0003	0.0096	-0.0027
	(0.9998)	(0.8369)	(0.8230)	(0.8689)	(0.9640)	(0.3980)	(0.7436)
PURE_CASH	**0.0125**	**0.0118**	**0.0114**	**0.0115**	**0.0107**	0.0083	**0.0145**
	(0.0168)	**(0.0189)**	**(0.0286)**	**(0.0261)**	**(0.0403)**	(0.4029)	**(0.0074)**
DIV_DEAL	-0.0019	-0.0015	-0.0016	-0.0015	-0.0016	0.0051	-0.0016
	(0.7774)	(0.8278)	(0.8237)	(0.8269)	(0.8253)	(0.5633)	(0.8580)
FRIENDLY	0.0105	0.0102	0.0103	0.0099	0.0097	0.0075	0.011
	(0.2768)	(0.2913)	(0.2854)	(0.3158)	(0.3172)	(0.6988)	(0.5327)
MB	0	0	0	0	0	-0.0001	-0.0001
	(0.9879)	(0.9966)	(0.9792)	(0.9987)	(0.9709)	(0.9137)	(0.8321)
MB_TARGET	0.0003	0.0002	0.0002	0.0002	0.0003	0.0003	0.0002
	(0.3510)	(0.4365)	(0.4345)	(0.4494)	(0.3851)	(0.3289)	(0.6142)
DIFF_WB	0.0025	0.0025	0.0024	0.0026	0.0027	0.0011	0.0033
	(0.3364)	(0.3478)	(0.3623)	(0.3402)	(0.3203)	(0.7973)	(0.3553)
DIFF_TAX	-0.0002	-0.0003	-0.0002	-0.0002	0.0001	0.0105	-0.0058
	(0.9728)	(0.9598)	(0.9692)	(0.9805)	(0.9911)	(0.2733)	(0.4801)
SAME_POLITIC	0.0048	0.005	0.005	0.0047	0.0048	0.0119	-0.0023
	(0.3521)	(0.3395)	(0.3279)	(0.3582)	(0.3293)	(0.2487)	(0.6174)
SAME_REL	**0.0103**	**0.0104**	**0.0106**	**0.0105**	**0.0104**	0.0031	**0.0175**
	(0.0000)	**(0.0000)**	**(0.0000)**	**(0.0000)**	**(0.0000)**	(0.7591)	**(0.0035)**
GEO_DISTANCE	0.0008	0.0008	0.0009	0.0008	0.0007	0.0001	0.0006
	(0.6285)	(0.6234)	(0.5872)	(0.6466)	(0.6631)	(0.9350)	(0.7888)
INTERCEPT	0.0128	0.0143	0.0135	0.0152	0.0122	0.0508	-0.0046
	(0.4092)	(0.3228)	(0.3622)	(0.2869)	(0.4084)	(0.2330)	(0.8327)
R-square	(0.1183)	(0.1150)	(0.1160)	(0.1155)	(0.1199)	(0.1730)	(0.1646)
N	1166	1166	1166	1166	1166	516	647
State Dummies	Y	Y	Y			Y	Y
Industry Dummies	Y	Y	Y			Y	Y

Table 2.11 (Continued). Robustness Tests: Alternative Measures of CD

This table presents the OLS estimation of the model: *Post-Bid Abnormal Return*$_{it}$ = $\beta_1(CD_{ijt}) + \beta_2(X_{ijt}) + \varepsilon_{it}$
The dependent variable is the combined cumulative three-day abnormal returns (CAR[-1,1]), where the weights are the acquirer's and target's market capitalization four weeks before the announcement.
X_{ijt} is a vector of acquirer and target deal control variables and includes: relative size of the acquisition, acquirers' pre-announcement market capitalization, pure stock offer indicator, pure cash offer indicator, diversifying deal indicator, friendly deal indicator, acquirer's pre-announcement market-to-book ratio of equity, target's pre-announcement equity market-to-book ratio, state-level differences in well-being index, state-level differences in marginal corporate tax rate, state-level difference in state GDP, differences in the state-wide proportion of Catholics, state-level , indicator variable, logarithm of the distance in miles between state capitals and Hoberg-Philips' (2010) product market synergies. All variables are defined in Appendix A8.

In Columns 1, 2 and 3, the cultural distance (CD) variable is estimated on the basis of, respectively, Fiordelisi and Ricci's (2014), Hofstede's (1984) or O'Reilly et al.'s (1991) frameworks. In Column 4, CD is also calculated as $(Create_a + Control_a + Collaboration_a + Compete_a)$ - $(Create_t + Control_t + Collaboration_t + Compete_t)$, but each attribute is normalized by the sum of all four attributes (e.g. $Create_a /(Create_a + Control_a + Collaboration_a + Compete_a))$. In Column 5, CD is calculated using the term-weighting methodology outlined in Loughran and McDonald (2011), where the individual cultural scores are derived from the weighted frequencies of each individual word.

In Columns 6 and 7, CD is calculated as $(Create_a + Control_a + Collaboration_a + Compete_a)$ - $(Create_t + Control_t + Collaboration_t + Compete_t)$, where the subscripts refer to the acquirer and target, respectively. All attributes are measured before the announcement. Column 6 reports the results for the subsample of positive CD, whereas Column 7 reports the results for the subsample of negative CD.

The number of observations and R-squared of each regression are also reported. *P*-values (in parentheses) are calculated using standard errors clustered by both industry and year. Economic significance [in brackets] is calculated as the change in the dependent variable that results from a one-standard deviation change in cultural distance, while maintaining the other independent variables at their sample means. Acquirers' industry and state indicator variables are included in the regressions. Boldfaced coefficients and associated *p*-values indicate statistical significance at the 5% level or higher.

CHAPTER 3

Does Stock Misvaluation Drive Merger Waves?[49]

3.1. Introduction

The notion that merger and acquisition activity clusters by time and industry is well-known in the literature (e.g., Nelson (1959), Gort (1969), and Mitchell and Mulherin (1996)), but the debate on the causes of merger waves is far from settled. According to the neoclassical theory (also known as the *Q* hypothesis), merger activity is driven by synergy and efficiency factors, and merger waves are caused by economic and regulatory shocks (e.g., Brainard and Tobin (1968), Mitchell and Mulherin (1996) and Jovanovic and Rousseau (2002)). In contrast, the misvaluation hypothesis posits that stock misvaluation affects merger intensity; merger waves are triggered by sharp deviations of stock prices from fundamental values (Shleifer and Vishny (2003) and Rhodes-Kropf and Viswanathan (2004)). Despite strong evidence that stock misvaluation affects takeovers at the individual deal level (e.g., Dong, Hirshleifer, Richardson, and Teoh (2006), Cai and Vijh (2007), Savor and Lu (2009), Gu and Lev (2011), and Fu, Lin, and Officer (2013)), empirical evidence remains intensely divided about whether stock market misvaluation drives the aggregate or industry-level merger activity.[50]

This polarization of the evidence is puzzling, especially considering that the majority of the above-mentioned papers study similar samples of acquisitions. We argue there are two reasons for

[49] The current version is co-authored with Ming Dong.

[50] Rhodes-Kropf, Robinson and Viswanathan (2005) and Baker, Pan, and Wurgler (2012) document that consistent with theories of market-driven merger waves, mergers are more likely when firms have higher valuation multiples. On the other hand, some studies find evidence supporting other drivers of merger waves. For instance, Bouwman, Fuller, and Nain (2009) argue that merger intensity is driven by herding, whereas Duchin and Schmidt (2013) argue that agency factors cause merger waves. Mitchell and Mulherin (1996), Jovanovic and Rousseau (2002), Andrade and Stafford (2004), Harford (2005), Gorton, Kahl and Rosen (2005), and Ahern and Harford (2014) also find evidence supporting the neoclassical theories.

this phenomenon. First, when identifying merger waves, prior literature does not distinguish valuation-sensitive takeovers from other deals. Since pure stock offers are more likely to be influenced by stock (mis)valuation compared to cash deals, mixing all types of transactions in the analysis decreases the power of detecting valuation effects.[51] Second, prior literature typically does not examine *both* the valuation multiples (such as value-to-price and market-to-book ratio) and long-run stock performance around merger waves. Because valuation multiples may contain information about both growth prospects and misvaluation, inferences are ambiguous if either equity valuation or long-run stock performance is not examined.[52]

In this paper, we aim to remedy these issues. Specifically, we distinguish the misvaluation hypothesis from the *Q* hypothesis using two approaches. First, we test whether the bidder valuation levels correlate with the occurrence of merger waves and whether the effects of stock valuation on industry-level merger waves depend on the wave definition. According to the *Q* hypothesis, the level of stock valuation reflects firms' economic fundamentals. Economic triggers of merger waves, such as technological shocks and industry-level deregulations, should affect merger activity irrespective of the transactions' method of payment. By contrast, under the misvaluation hypothesis, we expect stronger misvaluation effects for deals paid entirely by stock. When both firms' shares are traded, bidder and target stock valuations have an impact on takeovers—overvalued bidders acquire relatively undervalued targets with stock (Shleifer and Vishny's

[51] One exception is Rau and Stouraitis (2011) who differentiate cash from stock merger waves. However, they draw inferences from comparing patterns across different kinds of corporate event waves and do not examine bidder equity valuation and stock performance across wave phases.

[52] Rhodes-Kropf, Robinson and Viswanathan (2005) show that merger activity is strongly correlated with stock valuation levels and conclude that market overvaluation leads to merger waves. However, since they do not examine long-run acquirer stock performance, this is challenged by later studies. For instance, Duchin and Schmidt (2013) find that acquirers during merger waves have poor long-run stock performance and conclude agency issues, rather than misvaluation, drive merger waves; however, they do not examine the valuation patterns around waves. Maksimovic, Philips and Yang (2013) document high productivity gains of on-the-wave, high valuation acquirers, but they do not examine the long-run stock performance of these acquirers.

(2003)).[53] Also, when the target is private, the bidder can use overvalued stock to pay for the transaction; the limited bargaining power of the low-liquidity private target may offer the acquirer increased incentive to take advantage of its overvalued stock.[54]

Our second approach is to examine bidder long-run stock performance around merger waves. If merger waves result from firms acting in response to economic shocks, acquisitions announced during waves should create, or at least not destroy, bidder shareholder value. In contrast, overvaluation-driven merger waves should be associated with poor post-bid abnormal stock performance of the acquirers.[55]

Using a broad sample of U.S. mergers and acquisitions announced between 1981 and 2010, we define two sets of merger waves: *stock* waves are defined on the subsample of acquisitions paid by pure stock (we find 52 industry-specific *stock* waves), and *cash* waves are defined using the subsample of pure cash acquisitions (we identify 82 industry-specific *cash* waves).[56] Isolating valuation-sensitive deals from other acquisitions increases the power of our tests to detect the valuation effect on merger waves, even if there is little valuation effect on the aggregate merger sample.

[53] Regardless of bidder valuation, bidders can profit by acquiring undervalued targets with cash. However, the economic impact of cash deals is likely to be limited because firms are reluctant to make large transactions in cash, and managerial compensation related incentives for mergers are weaker in cash deals when target shares are undervalued (e.g., Cai and Vijh (2007)). Consistent with the prediction that stock bidders are overvalued, prior literature finds that stock bidders have lower announcement returns than cash bidders in transactions between public firms (e.g., Travlos, 1987; Brown and Ryngaert, 1991; Fuller, Netter, and Stegemoller, 2002; Moeller, Schlingemann, and Stulz, 2004; Dong et al., 2006).

[54] Several papers find positive announcement abnormal returns in a sample of bidders when they acquire private targets, especially when the form of payment is stock (Chang (1998), Fuller, Netter, and Stegemoller (2002), Moeller, Schlingemann, and Stulz (2004), and Faccio, McConnell, and Stolin (2006)). Officer (2007) provides evidence that unlisted targets are often sold at discounts. These results suggest that bidders of private firms tend to get better deals especially when the method of payment is stock.

[55] We also consider the possibility that value-relevant information is fully reflected in the short-run announcement-period bidder returns, but we find such short-run stock reactions are dwarfed by long-run abnormal returns.

[56] The *stock* and *cash* merger waves identify periods of increased merger activity of stock and cash deals, respectively. Once these periods of intense merger activity are identified, we contrast valuation patterns of all types of deals, sorted by target public status and means of payment, around these periods.

We apply the residual income model of Ohlson (1995), sometimes called "intrinsic value" (V), and use the ratio of this value to market price (VP) as our baseline misvaluation proxy. Since intrinsic value reflects growth opportunities, normalizing market price by intrinsic value filters out the firm growth effects to provide a purified measure of misvaluation. To provide further assurance, we also measure equity valuation by the book-to-price ratio (BP), and the industry component of the decomposed market-to-book ratio advocated by Rhodes-Kropf, Robinson, and Viswanathan (2005). Our conclusions are robust to using these measures.[57]

We examine the pattern of bidder stock valuation across the phases of *stock* and *cash* merger waves. We define pre-, in-, post- and non-wave periods and we find that bidder valuation peaks exactly during in-wave periods. In addition, in-wave acquirers have higher valuation ratios than non-wave acquirers, and this valuation spread is much larger around *stock* waves than around *cash* waves. The contrast between cash and stock waves is even starker when we conduct logit tests of the likelihood of a merger wave occurring in an industry. We find that the occurrence of merger waves is strongly and positively correlated with industry equity valuation only for *stock* waves. These findings support the misvaluation hypothesis, because according to the *Q* hypothesis, the effect of industry equity valuation on industry-specific merger intensity should be the same regardless of the type of merger waves.

In our second approach to differentiating the misvaluation hypothesis from the *Q* hypothesis, we examine the post-announcement long-run returns of the acquirers to complement evidence on valuation level. We measure long-run performance by the buy-and-hold abnormal return (BHAR)

[57] Our focus on the contrast of equity misvaluation effects between *stock* and *cash* waves makes our inference less sensitive to the misvaluation measure used, compared to other studies on merger waves. Even though a particular valuation proxy may contain noise, as long as such noise does not vary systematically across the types of merger waves or across the wave phases, our conclusion is less dependent on the misvaluation proxy. The examination of long-run bidder stock performance further alleviates the reliance on valuation measures.

of the acquiring firm, using size-and-MB matched portfolios as the benchmark. Under the *Q* hypothesis, in-wave bidders—whose growth prospects are among the highest—should benefit the most from synergistic gains. However, we find that for *stock* waves, in-wave acquirers have the lowest 5-year BHARs. Furthermore, the post-bid long-run performance is negatively correlated with the pre-bid valuation ratio. For instance, bidders of private targets and pure-stock bidders, who have the highest valuation during *stock* waves, have the lowest mean 5-year BHARs (-98% with $p < 0.001$ for both groups of bidders). The multivariate regressions of BHARs on bidder valuation and merger phase indicator variables confirm these results: In- and post-wave bidders perform significantly worse than pre-wave bidders and bidders in non-wave years, and this effect is magnified for bidders with high valuation. Remarkably, the negative effect of bidder overvaluation on post-bid long-run stock performance is significant only in stock deals, or during the in-wave and post-wave periods—periods of heightened merger activity. We observe no clear patterns of bidder long-run abnormal stock performance and cash merger intensity. These results lend further support to the misvaluation hypothesis.

The Shleifer and Vishny (2003) model predicts that stock market driven mergers should benefit bidder shareholders in the long run, despite the fact that these bidders tend to be overvalued at the time of the bid. However, we find no positive BHAR for the pooled sample of in-wave acquirers, which suggests that even though merger intensity is triggered by stock market overvaluation, bidder shareholders do not actually benefit from the takeover, even during *stock* merger waves. This finding suggests that overvaluation-induced agency costs adversely influence the long-term value of bidder shareholders (e.g., Jensen (2004, 2005), Polk and Sapienza (2009), Fu, Lin, and Officer (2013), and Duchin and Schmidt (2013)).

This paper is related to Dong et al. (2006) because it also examines the drivers of merger activity by differentiating the *Q* hypothesis from the misvaluation hypothesis. However, unlike Dong et al. (2006), which focuses on the deal-level drivers of takeover activity, this paper considers aggregate, industry-level patterns in merger activity. A priori, it is not clear whether firm-specific stock misvaluation is strong enough or prevalent enough to drive industry-level merger activity. Our paper provides empirical evidence that answers this question.

Our paper is also closely related to recent work on the causes of merger waves. Rhodes-Kropf, Robinson and Viswanathan (2005) and Baker, Pan, and Wurgler (2012) document that, consistent with the misvaluation hypothesis, merger intensity is positively correlated with stock valuation multiples. However, several other recent studies dispute the role of misvaluation in affecting merger waves. We discuss below how our testing strategies shed new light to the debate over the drivers of merger waves.

Bouwman, Fuller and Nain (2009) document that takeovers occurring during booming markets are fundamentally different from those announced during depressed markets. Like us, they find that bidders during booming markets have lower post-bid long-run stock performance, but they also find that earlier bidders outperform later bidders during booming markets, therefore arguing that their evidence is consistent with managerial herding. In contrast, by isolating valuation-sensitive stock acquisitions from valuation-insensitive cash deals, our tests reveal that bidder overvaluation peaks exactly during *stock* waves, and that in-wave merger announcements are followed by lower long-run performance, supporting the misvaluation hypothesis.

Duchin and Schmidt (2013) define merger waves on all acquisitions and find that in-wave acquirers have poor long-run stock performance, but they argue that agency-related factors, rather than stock overvaluation, trigger merger waves. We provide contrasting evidence by showing that during *stock* waves, extreme acquirer valuation precedes the dismal long-run returns, a pattern consistent with the misvaluation hypothesis. Our results suggest that stock misvaluation triggers merger waves, but our findings are also compatible with Duchin and Schmidt's in that agency costs associated with overvaluation possibly prevent bidder shareholders from benefiting from in-wave transactions.

Finally, Maksimovic, Philips and Yang (2013) study the properties of merger waves using acquisition samples in the manufacturing sector differentiated by the public status of the acquirers. They find that public merger waves are more affected by market valuation. However, they also find that productivity gains are greater when the acquirer's stock is highly valued, and they posit that in-wave acquisitions lead to positive efficiency outcomes. In contrast, by showing that acquirers have inferior post-announcement stock performance during *stock* waves, we provide evidence that valuation-driven merger waves do not benefit acquirer shareholders. Still, Maksimovic, Philips and Yang's findings can be reconciled with ours, because investors can overvalue acquirers' business prospects during merger waves, leading to a sharp post-bid correction of their stock prices.

The remainder of this paper is structured as follows. Section 3.2 describes the sample. Section 3.3 discusses our hypotheses and methodology. Section 3.4 contrasts how industry-level stock valuation affects *stock* and *cash* merger waves. Section 3.5 analyzes the long-run stock performance for bids announced during different phases of merger waves. Section 3.6 discusses robustness tests. Section 3.7 concludes.

3.2. Sample and Merger Wave Identification

We extract the acquisitions sample from Thompson's Securities Data Corporation's (SDC) Mergers and Acquisitions database. We keep bids made by U.S. acquirers for U.S. targets, independent of both the acquirer's and target's public status, between 1981 and 2010,[58] and with a value of at least $10 million. Stock daily returns come from the Center for Research in Security Prices (CRSP) and accounting variables are retrieved from Compustat. We exclude firms with total assets or book value of equity worth less than $1 million at the time of the acquisition. Table 3.1 reports the number of bids per year, the yearly break-down per bid type, as well as the industry-level mean VP and Book-to-Price (BP) ratios, where the industries are defined following Fama-French's 48-industries classification.

Because Table 3.1 reports the average valuation levels across industries, VP (BP) appears to be relatively stable in the time-series. For instance, the pre-dotcom bubble mean industry-level mean VP in 1999 is 0.972, relative to a mean industry-level mean VP of 0.988 in 2009, during the financial crisis. An (untabulated) examination of mean industry-level VP ratios by industry reveals that mean industry-level VP ratios increased by at least 25% between 1999 and 2009 in the following industries: food, soda, beer, tobacco, drugs, automobiles, mining, oil, utilities, personal services, computers, electronic equipment, and others. In contrast, the mean industry-level VP ratios of the textile, construction, construction materials, steel works, aircraft and financial industries decreased by at least 25%.[59] Furthermore, in unreported tests, we verify that VP significantly and positively predicts cross-sectional stock returns in our sample period. These results confirm the validity of VP as a misvaluation proxy for our tests.

We adapt the methodology used in Harford (2005) and Duchin and Schmidt (2013) in order to identify merger waves. Specifically, we identify merger waves by comparing the per-decade and per-industry highest concentration of bids in any period of 24 consecutive months with the

[58] We end our takeover sample in 2010 because we require five year of stock return data to calculate long-run returns. The return data end in 2014 and the sample period for 5-year bidder stock returns ends in 2009.

[59] The 2009 mean VP ratios in these industries were on average 34% higher than in 2007. In other words, the valuation peak in these industries occurred before the financial crisis (results untabulated).

99th percentile of a simulated distribution. The simulated distribution is generated by calculating the total number of bids per industry and decade from the real sample and then assigning randomly each bid occurrence to any given month in a decade. Each month has the same probability (1/120) of being assigned. The highest 24-month concentration is calculated and retained. The process is then repeated 1,000 times, thus generating, for each industry and decade, a distribution of highest 24-month bids concentrations.

To contrast the *Q* hypothesis and the misvaluation hypothesis more sharply, we partition our full sample of acquisitions into 27 subsamples, one each for the interaction of the means of payment (all, pure stock, pure cash), public status of the acquirer (all, public, private), and the public status of the target (all, public, private).[60] We apply our wave-identification methodology to each subsample, which results in 27 sets of waves. Each set has a maximum of 144 waves, one per industry (48) and per decade (3). Among the 27 sets of waves, we focus on two: *stock* waves defined using pure stock acquisitions, and *cash* waves defined using pure cash acquisitions.

Table 3.2 shows the number of waves in each of the subsample-specific sets, as well as the number of in-wave acquisitions and the total number of acquisitions in each subsample. For example, we identify 2,462 stock acquisitions, of which 1,668 are part of the 52 *stock* waves. Similarly, our subsample of pure cash acquisitions contains 6,016 acquisitions, of which 2,580 occur during one of 82 *stock* waves that we identify.[61]

Table 3.2 shows that waves are concentrated among public acquirers. Waves defined using acquisitions by public firms also concentrate more bids than waves defined using acquisitions by

[60] Our analysis is unaffected if we also include deals involving the acquisition of or by a subsidiary firm.

[61] The waves identification methodology is applied to each one of the 36 subsamples independently. Therefore, there is no relation between the number of waves among the different subsamples. This explains for example why in Panel A of Table 3.2, and in the "All means of payment" subpanel, the number of waves by public bidders for all types of targets (107) is greater than the number of waves by all types acquirers for all types of targets (105).

private firms. For instance, approximately 47% of the acquisitions by public acquirers (all means of payment, all types of targets) are included in a wave, whereas only 37% of the acquisitions by private bidders (all means of payment, all types of target) occur during a wave. These differences in concentration of merger activity may reflect the sensitivity of these types of acquisitions to market- or industry-wide triggers, as opposed to firm-specific motivations. In untabulated tests, we also find that the number of waves is not evenly distributed across decades; there is a peak in acquisition activity in the 1990s, for almost all types of acquisitions, with the exception of cash acquisition of public targets by subsidiaries.

An examination of Table 3.2 reveals that we have identified certain unlikely merger waves that encompass very few acquisitions, such as the waves of stock acquisitions by private acquirers. Imposing a minimum number of bids for a wave to be identified as such is a straightforward modification of the methodology that would avoid classify these clusters as waves; however, the two types of merger waves we focus on, *stock* and *cash* waves, are hardly affected by alternative ways of wave identification. Section 3.6.2 discusses alternative definitions of merger waves.

3.3. Hypotheses and Methodology

3.3.1 Hypotheses

Research on the drivers of merger waves has produced conflicting evidence. A body of the literature maintains that economic variables, such as industry-level economic or deregulatory shocks, technological innovation and liquidity constraints, trigger merger waves (e.g., Jovanovic and Rousseau, 2002; Andrade and Stafford, 2004; Harford, 2005; Maksimovic, Philips and Yang, 2013). Another line of the literature suggests instead that high levels of misvaluation motivate firms to engage in acquisitive activity (Shleifer and Vishny, 2003; Rhodes-Kropf and Viswanathan, 2004; Rhodes-Kropf, Robinson and Viswanathan, 2005).

Acquisitions paid by stock should be more strongly affected by stock valuation than deals paid by cash. Therefore, it should be more likely to detect the effect of stock misvaluation on the occurrence of merger waves if we define waves using stock deals only.

We depart from the bulk of the current literature and instead of considering aggregate merger waves, we define characteristics-specific industry-level clusters of acquisitions. Specifically, we adapt the methodology of Harford (2005) and Duchin and Schmidt (2013) and use the subsamples of pure stock acquisitions and pure cash offers to define two sets of merger waves: *stock* and *cash* merger waves.

In Shleifer and Vishny's (2003) model, even though overvaluation is the key driver behind an acquisition, some perceived synergies are needed to convince shareholders to accept the acquisition. Similarly, we believe that both misvaluation and pure economics motivations coexist in certain mergers, but we expect that misvaluation is relatively more important in valuation-sensitive deals. Under the misvaluation hypothesis, stock overvaluation should lead to higher merger activity, and the effects of stock misvaluation should be strongest among deals paid by pure stock. This leads to our first pair of hypotheses:

H1a (Q hypothesis): The pattern of acquirers' stock valuation level with respect to the phase of merger waves is not contingent on the use of the cash or stock wave definition.

H1b (Misvaluation hypothesis): In-wave acquirers have higher levels of overvaluation than pre-, post- or non-wave acquirers. This pattern should be especially strong for stock waves.

We use the residual-income value-to-price ratio (VP) as our baseline misvaluation measure (described in Section 3.3.2 below). We compare the VP ratios of firms that announced an acquisition in the year prior to the beginning of a wave ("pre-wave" period), to the VP ratios of acquirers that announced an acquisition during the wave and in the three years following the wave ("in-wave" and "post-wave" periods, respectively).[62]

We use VP because it is less subject to the criticism that using ratios like MB to measure misvaluation reflects both stock misvaluation and growth opportunities. Still, to rule out the possibility that the misvaluation metric proxies for growth opportunities, we examine the acquirers' post-announcement stock performance in order to separate the growth opportunities from the mispricing interpretations, as in Savor and Lu (2007) and Gu and Lev (2011).

We posit that misvaluation, as measured by the industry mean market-book ratio of equity, is a significant driver of merger waves. However, the relevance of stock misvaluation should be particularly important when acquirers use pure stock to pay for the transaction. This leads to our second pair of hypotheses:

H2a (Q hypothesis): The effect of industry-level stock valuation on industry-specific merger waves is not contingent on the use of the cash or stock wave definition.

H2b (Misvaluation hypothesis): Industry-level stock valuation has a stronger effect on the occurrence of stock, rather than cash, industry merger waves.

To test these hypotheses, we run logit regressions of the form:

$$P(Wave_{it} = 1) = \frac{1}{1 + e^{-(\alpha + \beta_1 VP_{it-1} + \beta_2 Spread_{t-1})}}$$

Where $P(\text{Wave}_{\text{it}} = 1)$ is an indicator variable that is equal to 1 if year t is a wave year in industry i, and zero otherwise. $\text{VP}_{it\text{-}1}$ is the industry i mean VP ratio, and $Spread_{t\text{-}1}$ is the spread between Moody's corporate BAA bond yield and the federal fund rate, both measured the end of year t-1.

[62] Because our wave identification methodology identifies the beginning of a wave and uses a pre-determined wave length (2 years), it is possible that the end tail of certain waves is not identified as such. To capture these long tails, we use a longer post-wave period (3 years, versus 1 year for the pre-wave period), following Harford (2005).

Following Harford (2005), whenever they occur, waves last two years, and no more than one wave per industry per decade can occur.[63]

In order to further distinguish the misvaluation hypothesis from the *Q* hypothesis, we analyze the acquirers' post-bid 5-year abnormal buy-and-hold returns (buy-and-hold abnormal returns, or BHARs, are calculated using the returns of size and MB matched portfolios as the benchmark). We contrast post-announcement performance for pre-, in-, post-, and non-wave acquirers, and compare the within-wave variations for deals with different characteristics.

H3a (Q hypothesis): Acquisitions should create the greatest bidder shareholder value for deals announced during merger waves. In-wave deals should lead to higher, or at least not negative, post-bid acquirer abnormal returns.

H3b (Misvaluation hypothesis): Acquisitions are made by the most overvalued bidders during stock *merger waves. In-wave stock deals should lead to lower post-bid acquirer abnormal returns.*

We also use multivariate regressions to test the third pair of hypotheses. Section 3.5 has more details.

Finally, to allow for the possibility that the value-relevant information of the bid is fully reflected in the short window around the takeover announcement, we analyze the three-day acquirer CARs around the announcement date for pre-, in-, post- and non-wave acquirers around *cash* and *stock* waves. We check whether the short-run announcement period acquirer returns are consistent with the long-run post-bid stock performance. In further robustness tests, we also check the variation in premium paid by pre-wave, in-wave, post-wave, and non-wave acquirers to see whether post-bid stock returns are related to bidder overpayment.

[63] We relax these assumptions in some of our robustness tests; see Section 3.6.1 for more details.

3.3.2 Measuring Misvaluation

We use the residual-income-model value-to-price (VP) ratio as our baseline proxy for stock misvaluation. There is strong support for VP as an indicator of mispricing. It is a superior return predictor than BP (Lee, Myers, and Swaminathan (1999), Frankel and Lee (1998), Ali, Hwang, and Trombley (2003)). The residual income value has at least two important advantages over book value as a fundamental measure. First, it is designed to be invariant to accounting treatments (to the extent that the 'clean surplus' accounting identity obtains; see Ohlson (1995)), making VP less sensitive to such choices. Second, in addition to the backward-looking information contained in book value, it also reflects analyst forecasts of future earnings. When compared to MB or Tobin's Q, VP is a ratio of equity rather than total asset misvaluation, and equity misvaluation rather than total misvaluation is more likely to matter for acquisition decisions. VP takes into account the future earnings power of the firm and filters out growth opportunities from valuation, and therefore is in principle a purer measure of misvaluation than MB. A limitation of the VP measure is that it requires analyst forecasts data which are scarce for smaller firms.[64] However, this is of little consequence for our purposes given firms involved in acquisitions are relatively large firms.

Lee, Myers, and Swaminathan (1999) and Dong et al. (2006, 2012) provide further details of the model estimation procedure; we also offer a summary of the procedure in Appendix B1. Since negative residual-income-model value reflects overvaluation, using the value-to-price ratio (rather than price-to-value) allows inclusion of negative residual-income value observations.

We eliminate stocks whose pre-announcement price per share is less than \$5. To measure acquirer valuation (Table 3.3), we estimate the firm-level VP at the end of the month prior to the

[64] This requirement causes the sample to decrease to 915,144 firm-month observations. In comparison, the sample has 1,777,434 firm-month observations with valid BP ratios.

acquisition announcement. We winsorize VP at the 95th percentile to minimize the impact of outliers. For our logit regressions which test the determinants of industry merger waves (Table 3.4), we use the industry-level mean VP, measured at the end of the year preceding the identification of merger wave occurrence.

Of course, the residual income V does not perfectly capture growth, so our misvaluation proxy VP does not perfectly filter out growth effects. We provide further means of valuation measurement. First, we examine the post-announcement acquirer stock performance (5-year buy-and-hold abnormal returns, in Section 3.5). Long-run stock returns can be viewed as an ex post misvaluation measure (e.g., Baker, Stein, and Wurgler (2003)). Second, for further robustness, we use two alternative valuation ratios: the book-to-price ratio (BP), and the Rhodes-Kropf, Robinson, and Viswanathan (2005) valuation measure (Section 3.6).

3.4. Stock Valuation around Merger Waves

3.4.1 Variations in Acquirer Valuation around Waves

Under the *Q* hypothesis, the pattern in valuation levels around waves should not be contingent on the definition of merger waves (H1a). In contrast, under the misvaluation hypothesis, valuation levels should peak during waves, especially during *stock* waves (H1b).

To test our first pair of hypotheses, we adopt the following terminology. We name firms that announced an acquisition during a two-year wave period *in-wave acquirers,* whereas firms that announced an acquisition in the year prior to the beginning of a wave or in the three years following the end of a wave are *pre-wave* and *post-wave* acquirers, respectively. We calculate the mean VP of pre-, in-, and post-wave acquirers. For each acquirer, VP is measured at the end of month *t*-1, where month *t* is the announcement month. Hereafter in the empirical tests, we increase the minimum deal value requirement to \$50 million and 1% of the acquirer's pre-announcement market capitalization, to ensure we are capturing economically significant merger effects.

In Table 3.3, we use the *stock* and *cash* waves to contrast the acquirer VP patterns around merger waves. We also classify the acquisitions announced around these waves by deal type (acquirer and target public status and means of payment). Here and in all subsequent tables, Panel A reports results using the *stock* wave definition, whereas Panel B shows results using the *cash* waves. We find that, using the timing of *stock* waves, bidder valuation levels is highest precisely during the in-wave period, followed by the valuation in the post-wave years, for all subsamples of acquisitions. For example, considering the column of "stock acquisitions", the in-wave and non-wave acquirers have a mean VP of 0.472 and 0.552 (a low VP indicates higher valuation), respectively, substantially lower than the VP of pre-wave bidders (0.720) or non-wave bidders (0.788).[65] In-wave stock bidders have a much lower VP than non-wave bidders (difference = 0.316; $p < 0.001$). Using the cash wave definition, this pattern is weaker and less clear-cut.

In addition, as one would expect if an overvalued bidders tends to make a stock rather than cash offer, in-wave bidder valuation around *stock* waves is systematically higher than that around *cash* waves, as evidenced by the lower VP ratio of in-wave bidders around *stock* waves relative to the in-wave bidders around *cash* waves, for all subsamples of acquisitions.

These patterns indicate that the behavior of bidder stock valuation is contingent on how waves are defined: bidder valuation peaks during wave periods, and the valuation gap between in-wave and non-wave bidders is much larger using *stock* waves. These findings lend support to the misvaluation hypothesis (H2b) rather than the *Q* hypothesis (H2a).

[65] The very high valuation levels of in-wave acquirers are partially driven by small firms and technology firms, but even when we remove such outlier acquirers our results are qualitatively unchanged. Similarly, our results remain if we broaden the set of firms to exclude to all technology firms.

It is interesting to note that in Panel A, in-wave bidders have the lowest VP ratio in private target acquisitions (0.446), compared to the in-wave bidders in pure stock transactions (0.472), or in-wave bidders in pure stock acquisitions of a public target (*pubpubstock* deals; 0.540). A possible interpretation is that private target firms, facing illiquidity discount, have limited bargaining power and are open to offers from even the highest valued bidders during merger waves.

The patterns identified in Table 3.3 are more pronounced in the 1990s (results untabulated). For instance, the difference in VP of non-wave and in-wave acquirers is 0.301 ($p < 0.001$) around *stock* waves. In contrast, the magnitude of the differences in acquirers' VP around *stock* waves in the 1980s is smaller, although significant still (difference in VP between non-wave and in-wave acquirers is 0.254 with $p < 0.001$).

3.4.2 Does Stock Valuation Trigger Merger Waves?

Our second pair of hypotheses states that under the *Q* hypothesis, the association between industry valuation levels and the occurrence of merger waves should be independent of which types of acquisitions are used to define merger waves (H2a), whereas under the misvaluation hypothesis, the association between industry valuation levels and the occurrence of merger waves should be strongest when valuation-sensitive acquisitions are used to define merger waves (H2b). To test these hypotheses, we estimate the logit regression described in Section 3.3 and we contrast the strength of the VP effect between *stock* and *cash* waves.

Table 3.4 presents the results of the logit tests when using the *stock* wave (Panel A) and the *cash* wave (Panel B) definitions. We see from Table 3.3 that acquirer valuation is high in both the in-wave and post-wave periods. We therefore set the dependant variable to 1 for wave years (either in-wave or post-wave) and 0 otherwise.[66] When we use the full sample of firms in an industry to compute the industry mean VP (the left "all firms" panel), Panel A shows that the VP effect is highly significant for the 1981-2000 subsample (coefficients = -1.931; p = 0.002), and the VP effect is weaker but still significant for the full 1981-2010 sample (coefficients = -0.997; p = 0.038), possibly due to the low frequency of stock merger waves in the 2000s.[67] In contrast, Panel B shows that the industry mean VP has no significant association with the incidence of *cash* merger waves. For both the *cash* and *stock* waves, credit spread has a significant negative impact on wave occurrence for the 1981-2000 period, indicating a role of liquidity in triggering merger waves.

Baker, Pan, and Wurgler (2012) document that the valuation of potential bidders has a particularly strong impact on merger waves. Therefore, we repeat the analysis by considering only potential bidders when calculating the industry mean valuation ratios. Potential bidders are firms whose VP ratio is lower than the industry median. Using potential bidders to measure industry stock valuation, we find qualitatively similar results to those using all firms to measure industry valuation, with the noticeable exception that the effect of VP on the initiation of *stock* merger waves becomes much more significant for the full 1981-2010 sample (coefficients = -1.385; p = 0.002). This result lends support to the misvaluation hypothesis (H2b): the existing relation between industry valuation levels and the occurrence of merger waves is contingent on the type of

[66] In similar industry-level logit tests, Harford (2005) sets the dependant variable to 1 for years in which a merger wave starts. Since merger waves are defined to last two years, and equity valuations can remain high throughout the post-wave period, setting the dependent variable to 0 for the second wave year and the post-wave years, as in Harford (2005), would reduce the power of detecting the link between industry-level equity valuation and merger intensity.

[67] Conducting regression tests by decade limits the power of detecting the effect of VP, because there are only 10 yearly observations per industry, there is less within-decade variation in VP, and there are fewer industry merger waves per decade (e.g., only one *stock* wave in the 2000s).

merger waves, and is especially strong when merger waves are defined using valuation-sensitive acquisitions.[68]

To confirm our findings, we extend the analysis to all 27 sets of waves defined in Table 3.2 (results untabulated). We use in turn the VP and BP ratios as our valuation proxy, and for both valuation measures, we find that mean industry valuation levels are associated with the occurrence of pure-stock waves, with the pure-stock results likely driving the all means of payment results, but the association between valuation levels and the occurrence of cash merger waves is generally insignificant. The occurrence of *stock* or *pubpubstock* waves is particularly sensitive to valuation levels. These patterns hold when the valuation proxy is the industry median valuation ratio, although the effects are of a smaller magnitude (results untabulated). The systematic differences in the magnitude and strength of the relation between industry valuation levels and the occurrence of different sets of merger waves challenge the *Q* hypothesis but are consistent with the misvaluation hypothesis.

3.5. Long-Run Stock Performance across Merger Wave Phases

The previous section shows that industry-level stock valuation triggers *stock* merger waves. We now turn to the acquirers' long-run stock performance, as analysis of the long-run stock performance gives further insight about whether stock misvaluation, rather than other factors such as growth prospects, drives merger waves.

As shown in Table 3.3, acquirers' valuation levels, as measured by the VP ratio, display a large variation around the waves, especially if we consider the *stock* waves; this variation is more

[68] In unreported tests, we include more controls, such as economic shock and capital tightness variables (as in Harford (2005)). These controls are almost always insignificant in the regressions, and they do not alter the coefficient of VP in any meaningful way.

accentuated for acquirers that made a pure stock offer or that acquired a private target. Even though VP may reflect both stock misvaluation and growth opportunities, the patterns in VP around waves are already difficult to reconcile with the *Q* hypothesis. In this section, we further validate that the acquirer's VP ratio is an effective proxy for mispricing by examining the acquirers' post-bid stock performance.

The long-term market responses to acquisitions allow us to test hypotheses H3a and H3b. Specifically, looking at the long-term stock performance of acquirers allows us to separate the growth opportunities from the mispricing interpretations. We calculate the acquirers' raw and style-adjusted 5-year buy-and-hold returns, and contrast the post-announcement stock performance of pre-, in-, post- and non-wave acquirers across acquisitions classified by the public status of the target and the means of payment.

For the tests of long-run returns, we impose an additional constraint. Namely, to avoid distorting the long-term returns of repeat acquirers with the market reaction to the announcement of a subsequent bid, we keep repeat acquirers' largest bid (in constant dollar value) in any 5-year windows, so that there are no overlapping returns for any bidder.

3.5.1 Portfolio Sorts

We measure acquirer long-run stock performance by the buy-and-hold abnormal return (BHAR), calculated as the difference between acquirers' buy-and-hold 5-year returns and the compounded return of an equally weighted portfolio matched on size and book-to-market.[69] We use the Fama-French breakpoints and portfolio returns to assign our sample firms to matching

[69] Monthly delisting returns are calculated following the methodology of Beaver, McNichols and Price (2007).

portfolios and calculate the compound returns of the matching portfolios.[70] The matching portfolios are the interaction of five size (market equity) portfolios and five Book-to-Market portfolios.[71]

Table 3.5 presents the BHARs for different types of acquirers that announced an acquisition around the *stock* and *cash* waves. We first note that for all acquisitions, the overall mean BHAR is negative (-41.3%; $p < 0.001$), indicating that mergers on average do not create value for shareholders. Panel A shows that using the timing of *stock* waves, with the sole exception of bidders in pure-cash acquisitions, all types of in-wave acquirers generate post-bid 5-year BHARs that are significantly lower than those of non-wave acquirers. For all acquisitions, the in-wave acquirers underperform non-wave acquirers by 58.3% ($p < 0.001$). The spread of BHAR between in-wave and non-wave bidders are greater for stock acquisitions (difference = 58.4%; $p < 0.001$) than for cash acquisitions (difference = 35.8%; $p = 0.129$).[72] In clear contrast, Panel B shows that using the *cash* wave definition, there is no significant in-wave bidder underperformance relative to non-wave bidders.

Strikingly, the mean 5-year BHAR of bidders that make stock offers during the in-wave and post-wave periods is a dismal -98.0% and -130.4%, respectively. A similar remark can be made for in-wave and post-wave bidders of private targets. When viewed in combination with the peak

[70] Data are available on Kenneth French's website: http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/index.html.

[71] As a robustness test, we also use Savor and Lu's (2009) methodology for building reference portfolios. Specifically, for each acquisition in our sample, we find potentially matching firms with the same two-digit SIC code and market value of equity between 50% and 150% of the market value of equity of the acquirer. Because we want to compare the performance of acquirers versus non-acquirers, we exclude firms having completed an acquisition in the previous five years from entering the matching portfolio. We then rank the potential matches by their book-to-market ratios and select the ten firms whose book-to-market ratios are closest to the acquirer's. The reference portfolio is an equally-weighted portfolio of these ten matching firms. If less than ten matching firms are available, we select them all. We do not replace matching firms that delist from CRSP. Our main results are robust to the use of either methodology.

[72] Our finding that even acquirers in cash deals have negative post-merger performance is consistent with other recent studies (e.g., Song (2007) and Bouwman, Fuller, and Nain (2009)).

valuation of the same types of bidders (Table 3.3), this result confirms the intuition that bids of most overvalued acquirers during *stock* merger waves are followed by a major market correction in the subsequent years. The negative post-bid performance of highly valued acquirers, together with the underperformance of in-wave acquirers relative to non-wave acquirers around *stock* waves, is at odds with the *Q* hypothesis prediction that in-wave merges will generate greatest value for bidder shareholders (H3a), and supports the misvaluation hypothesis (H3b).

For robustness, we also examine the raw 5-year buy-and-hold bidder returns starting at the end of the acquisition month for a 5-year period or until delisting, whichever is earlier. Appendix B4 highlights that even though most types of acquirers generate a positive 5-year raw post-announcement stock performance, in-wave acquirers' performance significantly lags that of the non-wave acquirers for all types of deals, especially using the *stock* wave definition (difference = 76.5%; $p < 0.001$). Notably, using *stock* waves, in-wave acquirers of private targets and pure stock acquirers generate negative 5-year raw returns (-7.2% and -7.6%), even though such raw returns are insignificantly different from zero.[73]

Because the pattern—that in-wave bidders possess the highest valuation and have the lowest post-announcement long-run stock performance—applies to all types of acquisitions, including pure cash deals, the effects of stock misvaluation on merger waves are not limited to stock acquisitions; even in-wave bidders who pay cash have higher valuation than their non-wave counterparts. Such a pattern on bidder valuation is difficult to reconcile with other theories of merger waves, such as managerial herding (e.g., Bouwman, Fuller, and Nain (2009) or envy (Goel

[73] A negative raw 5-year buy-and-hold bidder return is not to be expected from a risk-based rational theory, because a negative raw 5-year return would imply a negative benchmark-adjusted return even if we use the risk-free T-bill rate as the benchmark.

and Thakor (2010)), but is consistent with the misvaluation view that firms engage in mergers and acquisitions during a time of stock overvaluation.

The underperformance of in-wave relative to non-wave bidders applies also for *pubpubstock* acquisitions (Table 3.5, difference = 49.7%; $p = 0.005$), a subsample of acquisitions that should be especially sensitive to valuation levels. Therefore, even the *pubpubstock* mergers announced during *stock* waves—deals that have the highest potential for the acquirer to exploit the target misvaluation according to the Shleifer and Vishny (2003) theory—do not benefit acquirer shareholders. This finding suggests that agency costs of overvalued equity (Jensen (2004, 2005) and Polk and Sapienza (2009)) adversely affect the long-run value of the acquirer. Fu, Lin, and Officer (2013) and Duchin and Schmidt (2013) provide further evidence of agency-related costs associated with in-wave acquirers.

Figure 2.1 graphically presents the main findings of the paper. Panel A shows the stock valuation level (measured by VP), and Panel B shows the long-run abnormal stock performance (measured by BHAR), of pre-wave, in-wave, post-wave and non-wave acquirers. Both panels use the *stock* wave definition. Bidder valuation peaks precisely during the in-wave phase of the merger waves for all categories of deals, including *stock* deals. Panel B shows that the long-term stock performance of the in-wave and post-wave acquirers is marked by severe underperformance relative to non-wave acquirers. In sum, our approaches—focusing on valuation-sensitive stock deals in defining merger waves, and analyzing acquirer valuation level and long-run stock performance—allow us to uncover patterns that support the misvaluation hypothesis as opposed to the *Q* hypothesis.

3.5.2 Regression Test

We confirm the univariate results of Table 3.5 by estimating the following OLS regressions:

$$BHAR_{it} = \alpha + VP_{it-1} + In\_and\_post\_wave_{it} + VP_{it-1} * In\_and\_post\_wave_{it} + Prewave_{it}$$
$$+VP_{it-1} * Prewave_{it} + +\varepsilon_{it}$$

where *In_and_post_wave* and *Prewave* are indicator variables that are equal to 1 if year *t* is an in-wave or post-wave year, or a pre-wave year, respectively, in firm *i's* industry. *P*-values (in parentheses) are calculated using standard errors clustered by both industry and year.

Table 3.6 presents the regression results. Using the *stock* wave definition (Panel A), we find acquirers who announced bids during the in-wave or post-wave years underperform relative to other bidders, as indicated in the significant negative *In_and_post_wave* indicator in "all acquisitions" column (coefficient = -0.905; p = 0.022) and in the subsamples of public targets, private targets, and stock acquisitions. Strikingly, VP is insignificant in the "all acquisitions" model, but its interaction with the *In_and_post_wave* indicator is insignificant and positive (coefficient = 0.672; p = 0.050), indicating that bidder overvaluation as measured by VP has a negative effect on BHAR only during in-wave and post-wave years—periods of peak merger activity. This VP interaction effect is particularly strong in acquisitions involving private targets (coefficient = 0.826; p = 0.011) and in stock acquisitions (coefficient = 0.718; p = 0.001), consistent with the Table 3.3 finding that stock bidders and bidders of private targets possess the highest valuation during *stock* wave periods.

Moving to the *cash* waves (Panel B), we find no significant BHAR effects of the *in_and_post_wave* indicator variable; such contrast between the *stock* and *cash* waves lends further support to the misvaluation hypothesis. Interestingly, the only significant interaction between VP and the *In_and_post_wave* indictor is found in the subsamples involving pure stock payment (stock acquisitions and *pubpubstock* acquisitions), which is consistent the interpretation that these transactions are most valuation sensitive, and overvaluation has an effect on acquirer post-bid performance even when we use the *cash* wave definition.

3.6. Robustness

Our results rely on two key variables: the identification of merger waves and the proxy for overvaluation. Although both Harford's (2005) methodology to identify merger waves and the use of MB as a proxy for mispricing have been widely adopted in the literature, a valid question is whether our results are sensitive to these measures. In Section 3.6.1, we keep our original waves, but vary the measure of mispricing, whereas in Section 3.6.2, we replicate our analysis with waves identified using a different methodology.

3.6.1 Alternative Measures of Mispricing

We verify that our results are robust to using other mispricing proxies. We first consider book-to-price ratio (BP).[74] A drawback of BP is that it can be a relatively noisy proxy for mispricing; it closely relates to the empirical proxies for Tobin's Q and is sometimes used to measure growth opportunities. However, BP (or the inverse, market-to-book) ratio is also commonly used to measure valuation, both in the industry and in academia.

We replicate Table 3.3 with BP instead of VP (Appendix B2). Our main findings remain: Valuation peaks during the in-wave phase of the wave, and the difference between in-wave and non-wave acquirers' BP is highly significant and has greater magnitude around *stock* waves, especially so for valuation-sensitive acquisitions.

[74] We use the market-to-book equity, as opposed to market-to-book assets, ratio because what matters most for the misvaluation hypothesis is the valuation level of the equity rather than assets. This contrasts with some studies in the literature including Harford (2005) and Rhodes-Kropf, Robinson and Viswanathan (2005)).

To assess further the robustness of our results, we follow Fu, Lin and Officer (2013) and Rhodes-Kropf, Robinson and Viswanathan (2005; hereafter, RKRV) and we decompose the equity market-book ratio into firm-specific and industry-specific components. In Appendix B3, we contrast RKRV's misvaluation measure across wave phases and we find results qualitatively similar to when using VP or BP as the misvaluation measure. For example, using the *stock* wave definition, for all acquisitions, valuation peaks in the in- or post-wave phase, and the difference in valuation between in- and non-wave acquirers is significant (difference = 0.884, p = 0.041). In contrast, the in-wave and non-wave valuation gap is insignificant using *cash* waves. Our main results that support the misvaluation hypothesis thus are robust to alternative valuation measures.

3.6.2 Alternative Definitions of Waves

Although many researchers have adopted Harford's (2005) methodology to identify waves, Netter, Stegemoller and Wintoki (2011) find that patterns of merger waves are sensitive to the methodology of wave identification and the sample used. We thus assess the robustness of our results by considering an alternative methodology to define merger waves.

We first modify our original waves by allowing waves to bridge over two decades. The late-1990s and late-2000s were periods of increased merger activity and as such, it is possible for the highest concentration of acquisitions to extend over a single decade. For example, a two-year wave starting in 2000 would bridge over two decades. We thus apply our waves-identification methodology to the full sample (1981-2010), but we limit the number of two-year waves to three per industry,[75] and we require at least one non-wave year between two waves. We then proceed to perform our tests (results untabulated) and confirm that our main results remain.

3.6.3 Short-Run Announcement-Period Returns

The style-adjusted long-term returns (Table 3.5) show a clear underperformance of in-wave acquirers with respect to non-wave acquirers for *stock* waves. A natural question is thus whether this underperformance starts immediately upon the acquisition announcement. We consider the acquirers' three-day centered cumulative abnormal returns (CARs) using the market-adjusted model. As in Section 3.5, we then contrast the mean CARs by deal type and timing relative to the wave.

Table 3.7 shows that short-run CARs are quite inconsistent with the long-run stock performance of the acquirers. When considering either the *stock* or *cash* waves, we find insignificant differences between CARs of in-wave and non-wave acquisitions. Such differences fall short of the difference in post-announcement long-run returns (Table 3.5) by a large margin. Furthermore, the short-run CARs are often inconsistent with the long-run returns in direction. For instance, in Panel A, in-wave bidders of private targets earn an average of positive CAR (2.3%; $p < 0.001$), consistent with the literature (e.g., Fuller, Netter, and Stegemoller (2002)), but in sharp contrast with the large negative BHAR documented in Table 3.5. These results suggest that short-run market reactions may reflect certain characteristics of the deal (e.g., private targets offer an opportunity of illiquidity discount), while ignoring the degree of stock overvaluation of these acquirers.

[75] Roughly, one per decade, although we do not impose a one-per-decade constraint.

3.6.4 Bid Premiums

Variations in bid premium paid around waves allow us to gain more understanding about the source of the in-wave acquirers' post-announcement underperformance relative to non-wave acquirers. Bid premium is the SDC-calculated ratio of the offer price to the target price, where the target price is calculated four weeks before the announcement date. The non-availability of market value for private targets significantly reduces our sample size.[76]

In unreported tests, we examine the mean bid premiums around the *cash* and *stock* waves. The average premium paid by all acquirers is 41.6%. The premium paid is stable throughout the waves if we consider *cash* waves and the full sample (all acquirers). There is some slight variation in premiums paid around *stock* waves: the mean in-wave premium paid is 45.3%, whereas the mean non-wave premium is 41.1% (p of the difference = 0.239). The insignificant differences in bid premium across wave phases rule out the possibility that differences in the post-bid stock performance between in-wave and non-wave years are primarily caused by differences in bid premium.

3.7. Conclusion

We use a broad sample of acquisition announcements from 1981 to 2010 to test the hypothesis that stock misvaluation triggers merger waves. In contrast to previous research, we differentiate valuation-sensitive takeovers paid by stock from valuation-insensitive cash deals and analyze *stock* and *cash* merger waves separately.

We distinguish the misvaluation hypothesis from the efficiency-based Q hypothesis in two ways. First, we consider the association between industry equity valuation levels and the

[76] Mulherin (2014) discusses the limitations of SDC's bid premium data.

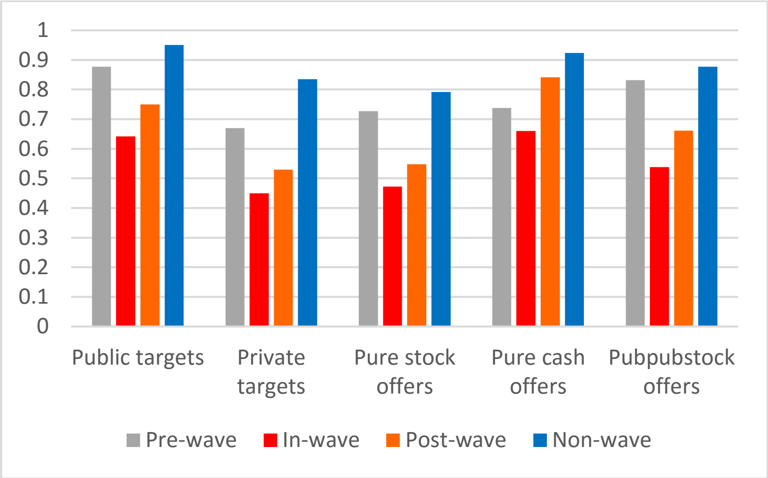
occurrence of merger waves. The misvaluation hypothesis predicts this association should be stronger for *stock* waves than for *cash* waves, because stock waves are defined using the valuation sensitive pure stock acquisitions rather than the comprehensive sample. Meanwhile, the *Q* hypothesis is silent on this prediction. Second, we examine the long-run post-bid stock performance of the acquirers. The misvaluation hypothesis predicts poor post-acquisition bidder returns when the bids are made during *stock* waves, whereas the *Q* hypothesis forecasts the greatest value gains for acquirers during either type of merger waves.

Our industry-level logit tests show that the incidence of a merger wave is strongly correlated with industry-level stock valuation only for *stock* waves, and acquirer valuation peaks during *stock* waves. On bidder stock performance, we find that acquirers have low post-bid stock performance in the 5-year period after the merger bid, and acquirer underperformance is especially acute for mergers announced during *stock* waves. These results lend credence to the misvaluation hypothesis, and suggest that bidders close to the peaks of *stock* merger waves make acquisition offers when their stocks are most overvalued.

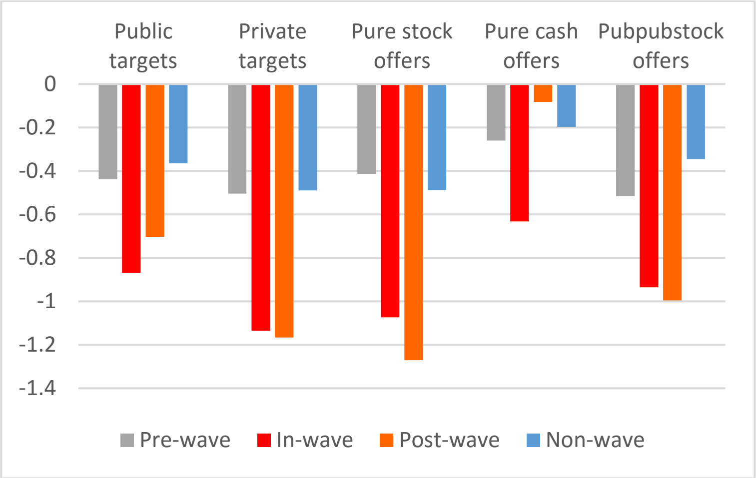
However, acquirers earn lower rather than higher post-bid long-run returns, especially for bids made during *stock* waves. Therefore, while stock acquisitions provide the acquirers with the *opportunity* to exploit target shareholders and benefit their own shareholders as in the Shleifer and Vishy (2003) model, other considerations such as the agency costs associated with overvalued equity (Jensen (2004, 2005)) can distort managerial incentives, so that market-driven acquisitions actually create large value losses to bidder shareholders.

Figure 3.1. Mean Acquirer Value-to-Price Ratio and 5-Year BHAR, by Merger Wave Phase and Type of Acquisition

Panel A: Acquirers' VP Ratio



Panel B: Acquirers' 5-Year BHAR



This figure presents the acquirers' mean value-to-price (VP) ratios (Panel A) and their post-acquisition 5-year BHAR (Panel B) during the pre-wave, in-wave, post-wave, and non-wave periods. We sort the bidders according to the timing of the announcement date relative to the industry-specific waves. The pre-wave period includes the year before the beginning of the wave, the in-wave period includes the two years of the wave, and the post-wave period spans the three years following the end of the wave. Non-wave periods cover the rest of the time periods. Both panels report results using the *stock* wave definition.

Panel A reports the mean VP for each category of bidders. Panel B reports the acquirers' 5-year post-announcement buy-and-hold abnormal returns (BHARs). Post-announcement 5-year buy-and-hold abnormal returns are the difference between acquirers' buy-and-hold 60-month returns and the compound return of an equally weighted portfolio matched on size and book-to-market. For repeat bidders, only the largest bid (in constant dollar value) within a five-year period is considered.

Table 3.1. Summary Statistics of the Sample

(1) Year	(2) No. Bids	(3) Public Targets		(4) Private Targets		(5) Stock Acq.		(6) Cash Acq.		(7) VP	(8) BP
		N (%)	Value (%)	N (%)	Value (%)	N (%)	Value (%)	N (%)	Value (%)		
1981	184	35.3	58.9	31	23.3	2.2	0.7	8.2	10.1	1.336	0.826
1982	188	31.4	41.7	38.3	21.3	0.5	0.6	0	0	1.697	1.046
1983	282	29.1	46.1	26.2	19.3	0.4	0.1	0.4	0.2	1.048	0.707
1984	372	35.8	46.8	15.1	7.6	1.9	1.5	8.9	13	1.368	0.738
1985	333	46.8	69.4	9.6	5.9	11.1	8.9	43.2	38.8	1.292	0.708
1986	508	37.2	53.2	17.1	9.7	9.3	8.7	35.8	41.2	0.982	0.594
1987	472	37.7	51.5	13.8	7.8	8.3	7.1	37.3	37.0	0.989	0.583
1988	613	34.7	59.0	15.3	7.6	4.7	2.8	40.1	44.3	1.222	0.674
1989	524	30.2	57.6	17	6.5	8.8	16.9	38.2	29.2	1.195	0.603
1990	332	19.3	42.4	16.6	7.7	10.5	17.5	29.5	26.6	1.302	0.759
1991	286	26.6	45.8	17.5	10.3	16.8	29.6	25.2	22.7	1.141	0.746
1992	358	25.7	36.5	20.7	12.1	21.2	22.1	27.4	24.2	0.957	0.620
1993	471	28.0	57.9	21.9	8.2	24.0	26.4	27.6	15.4	0.917	0.516
1994	635	32.0	47.5	22.2	12.3	22.7	20.4	30.1	35.5	1.022	0.516
1995	738	34.0	65.2	22.4	7.8	23.8	36.8	27.8	23.1	1.087	0.530
1996	989	31.5	62.4	29.3	11.1	22.5	27.4	24.9	18.2	0.947	0.505
1997	1349	32.7	59.1	30	11.7	23.1	32.7	23.1	15.4	0.857	0.469
1998	1474	32.7	80.7	29.8	5.7	23.7	58.0	23.3	9.9	0.899	0.519
1999	1414	36.8	73.7	28.9	8.6	24.3	34.8	26.1	12.1	0.972	0.632
2000	1377	30.7	73.6	36.4	11.5	28.1	43.8	26.5	11.0	1.096	0.806
2001	826	31.6	55.8	27.4	7.9	16.2	18.8	28.3	17.4	1.103	0.756
2002	747	25.0	46.4	27.6	12.5	8.2	25.3	33.5	22.8	1.033	0.728
2003	865	25.4	54.9	25.8	9.4	8.4	27.2	35.4	22.8	1.050	0.721
2004	985	22.2	61.5	31.8	11.6	7.6	23.8	37.5	31.7	0.825	0.490
2005	1146	21.5	61.4	34.6	13.2	5.5	13.0	40.1	28.6	0.747	0.453
2006	1325	24.5	66.5	31.4	9.0	3.9	8.3	43.8	31.8	0.695	0.460
2007	1296	25.2	57.6	30.9	9.2	4.3	4.0	43.8	55.4	0.626	0.468
2008	722	23.1	62.0	34.8	15.3	4.6	17.2	43.1	41.4	0.806	0.675
2009	525	24.8	61.6	28.8	8.6	7.4	4.1	38.7	19.3	0.988	0.922
2010	865	24.3	50.1	30.4	12.3	3.6	6.3	47.9	44	0.865	0.637
All	22201	29.9	56.9	25.4	10.8	11.9	18.2	30.9	25.6	1.035	0.647

This table reports the sample characteristics. Columns 2 reports the distribution of the number of deals in our sample, by year. Columns 3 to 7 report the percentage and value of, respectively, bids for public targets, bids for private targets, pure stock bids, and pure cash bids. Percentages are calculated with respect to the total number of bids in our sample in each year. Values are the total dollar value of each subsample in each year, relative to the total dollar value of acquisitions in our sample in that year. Column 7 shows the mean of industry-level mean VP ratios and Column 8 shows the mean of industry-level mean BP. VP and BP ratios are calculated using months-end equity market values and year-end accounting values of equity. We winsorize the right tail of the VP distribution at the 95% (the left tail is winsorized at zero for all calculations). Negative VP ratios are set to missing. We delete observations where the stock price is less than $5. VP ratios include all observations, including the firms that did not complete a merger.

Table 3.2. Descriptive Statistics: Industry Merger Waves

Panel A: Full sample

	All means of payment			Pure stock acquisitions			Pure cash acquisitions		
	All bidders	Public bidders	Private bidders	All bidders	Public bidders	Private bidders	All bidders	Public bidders	Private bidders
All targets									
No. of industry-specific waves	105	107	31	**52**	41	5	**82**	71	16
No. of in-wave Acquisitions	6018	5317	947	**1668**	1522	11	**2580**	1476	181
No. of Acquisitions considered	13146	11211	2572	**2462**	2375	27	**6016**	3686	1170
Public targets									
No. of industry-specific waves	86	84	16	40	41	0	62	53	13
No. of in-wave Acquisitions	2685	2077	285	904	881	0	983	511	183
No. of Acquisitions considered	5526	4154	669	1534	1486	9	2120	1260	398
Private targets									
No. of industry-specific waves	67	59	19	35	35	4	27	21	2
No. of in-wave Acquisitions	2559	2044	168	722	707	6	452	303	18
No. of Acquisitions considered	5321	4169	489	891	862	11	1316	970	145

This table reports the counts of industry-level merger waves. Waves are defined following Harford's (2005) methodology. We identify merger waves by comparing the per-decade highest concentration of bids in each industry and in any period of 24 consecutive months with the 99th percentile of a simulated distribution. The simulated distribution is generated by calculating the total number of bids per industry and decade from the real sample and then assigning randomly each bid occurrence to any given month in a decade. Each month has the same probability (1/120) of being assigned. The highest 24-month concentration is calculated and retained. The process is then repeated 1000 times.

We fraction our full sample of acquisitions into 27 subsamples, one each for the interaction of the means of payment (all, pure stock, pure cash), public status of the acquirer (all, public, private), and the public status of the target (all, public, private). We apply our wave identification methodology to each subsample, which results in 48 sets of waves. Each set has a maximum of 144 waves, that is, one per industry (48) per decade (3).

Each panel of this table has 48 cells, one each for our 48 subsamples. In each cell, we report a) the number of industry-specific waves we identified; b) the number of acquisitions encompassed in these waves; and c) the total number of acquisitions considered to define these waves. Bold-faced entries indicate the two types of waves we focus on: *stock* waves and *cash* waves.

Table 3.3. Residual-Income Value-Price (VP) Ratio of Acquirers, by Merger Wave Phase and Type of Acquisition

Panel A: 1981-2010, stock waves

	All acquisitions (1)		Public targets, all acquirers (2)		Private targets, all acquirers (3)		Stock acquisitions (4)		Cash acquisitions (5)		"Pubpubstock" deals (6)	
	N	VP	N	VP	N	VP	N	VP	N	VP	N	VP
1) All (including non-wave industries)	6770	0.7761	2612	0.8100	2156	0.6652	1741	0.6298	1806	0.7869	1135	0.7082
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
2) Pre-wave	704	0.7897	307	0.8732	220	0.6632	254	0.7199	135	0.7371	163	0.8251
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
3) In-wave	1165	0.5774	542	0.6448	366	0.4460	596	0.4720	161	0.6627	378	0.5395
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
4) Post-wave	689	0.6870	317	0.7538	208	0.5276	248	0.5521	126	0.8505	156	0.6672
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
5) Non-wave (only wave industries)	786	0.8929	324	0.9444	209	0.8337	297	0.7879	165	0.9215	188	0.8714
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
6) Non-wave (all industries)	4323	0.8391	1481	0.8726	1391	0.7400	676	0.7590	1403	0.7964	458	0.8198
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
Non-wave – In-wave (5 - 3)		**0.3156**		**0.2996**		**0.3876**		**0.3160**		**0.2588**		**0.3318**
p-value of difference		**(0.0000)**		**(0.0000)**		**(0.0000)**		**(0.0000)**		**(0.0000)**		**(0.0000)**

Table 3.3 (Continued). Residual-Income Value-Price (VP) Ratio of Acquirers, by Merger Wave Phase and Type of Acquisition

Panel B: 1981-2010, cash waves

	All acquisitions (1)		Public targets, all acquirers (2)		Private targets, all acquirers (3)		Stock acquisitions (4)		Cash acquisitions (5)		"Pubpubstock" deals (6)	
	N	VP	N	VP	N	VP	N	VP	N	VP	N	VP
1) All (including non-wave industries)	6770	0.7761	2612	0.8100	2156	0.6652	1741	0.6298	1806	0.7869	1135	0.7082
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
2) Pre-wave	1268	0.7773	501	0.8397	434	0.6899	339	0.7015	277	0.7588	222	0.7832
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
3) In-wave	1750	0.6989	688	0.7658	547	0.5722	412	0.5791	596	0.7601	264	0.6616
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
4) Post-wave	1151	0.7366	472	0.8532	389	0.5594	345	0.7079	292	0.7680	233	0.8526
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
5) Non-wave (only wave industries)	1494	0.8728	614	0.8486	420	0.8141	469	0.6199	309	0.8065	317	0.6588
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
6) Non-wave (all industries)	2712	0.8380	986	0.8099	815	0.7586	678	0.5804	660	0.8231	436	0.6211
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
Non-wave – In-wave (5 - 3)		**0.1739**		**0.0827**		**0.2420**		0.0408		0.0464		-0.0028
p-value of difference		**(0.0000)**		**(0.0119)**		**(0.0000)**		(0.2382)		(0.1446)		(0.9474)

This table reports the acquirers' mean residual-income model value-to-price (VP) ratios. We sort the bidders according to the timing of the announcement date relative to the industry-specific waves. The pre-wave period includes the year before the beginning of the wave, the in-wave period includes the two years of the wave, and the post-wave period spans the three years following the end of the wave. Non-wave periods cover the rest of the time period. Panel A reports the results using the *stock* waves (industry merger waves defined using only pure stock offers), whereas Panel B reports the results using the *cash* waves (industry merger waves defined using only pure cash offers). We report the mean market-book ratios for each category, along with the p-value associated with the t-test for statistical difference from zero. The bottom part of each panel shows the results of a t-test for difference in mean VP ratios of non-wave acquirers (line 5) and in-wave acquirers (line 3).

We report the mean acquirer VP ratios for: all types of acquisitions (column 1), the acquisitions of public and private targets (columns 2 and 3), the pure stock and pure cash acquisitions (columns 4 and 5), and public acquirers that made a pure stock offer for a public target (column 6). P-values are presented in parentheses. Boldfaced differences in mean market-book ratios and associated p-values are significant at the 5% level or higher. For ease of reading, we do not use boldface characters for simple mean VP ratios, even when they are significantly different from zero.

Table 3.4 - Determinants of Industry Merger Waves

Panel A. Stock waves.

	All firms		*Potential bidders only*	
	1981-2010	1981-2000	1981-2010	1981-2000
Intercept	-0.5051	**2.3408**	-0.6805	1.4473
	(0.5431)	**(0.0077)**	(0.3390)	(0.0590)
VP	**-0.9971**	**-1.9314**	**-1.3846**	**-1.8616**
	(0.0380)	**(0.0015)**	**(0.0021)**	**(0.0022)**
Spread	-0.0427	**-0.7469**	-0.0620	**-0.7388**
	(0.7968)	**(0.0001)**	(0.7034)	**(0.0000)**
R-squared	(0.0193)	(0.1237)	(0.0329)	(0.1210)
N	1440	960	1440	960

Panel B. Cash waves.

	All firms		*Potential bidders only*	
	1981-2010	1981-2000	1981-2010	1981-2000
Intercept	0.0009	0.7741	-0.0618	0.7595
	(0.9984)	(0.2922)	(0.8633)	(0.2124)
VP	-0.4367	-0.1990	-0.6194	-0.3292
	(0.2071)	(0.6302)	(0.0796)	(0.4184)
Spread	-0.0913	**-0.4412**	-0.0991	**-0.4399**
	(0.1864)	**(0.0062)**	(0.1507)	**(0.0063)**
R-squared	(0.0115)	(0.0638)	(0.0159)	(0.0651)
N	1440	960	1440	960

This table presents the results of the logit estimation of the following model:

$$P(Wave_{it} = 1) = \frac{1}{1+e^{-(\alpha+\beta_1 VP_{i,t-1}+\beta_2 Spread_{t-1})}},$$

where $P(Wave_{it} = 1)$ is an indicator variable that is equal to 1 if year t corresponds to a wave year or a post-wave year in industry i. Industry-level stock valuation (VP_{t-1}) is the mean VP value of all firms in an industry measured at the end of year t-1. $Spread_{t-1}$ is the spread between Moody's corporate BAA bond yield and the federal fund rate.

We use Firth's penalized likelihood logistic regressions to mitigate the issues stemming from quasi- or complete separation. In Panel A, waves are defined using only pure stock offers (*stock* waves), whereas in Panel B, the waves considered are defined using only the cash bids (*cash* waves). For both Panels A and B, the left "all firms" panel uses all firms in an industry to calculate industry mean VP; the right "potential bidders only" panel uses potential bidders (firms with below median VP) to calculate industry mean VP. We report results for the full sample (1981-2010), and subsample results for the 1981-2000 period. The number of observations and pseudo R-squared of each regression are also reported. *P*-values (in parentheses) are calculated using standard errors clustered by both industry and year. Boldfaced coefficients and associated *p*-values indicate statistical significance at the 5% level or higher.

Table 3.5. Post-Announcement Acquirer 5-Year BHAR, by Merger Wave Phase and Type of Acquisition

Panel A: 1981-2009, stock waves

	All acquisitions (1)		Public targets, all acquirers (2)		Private targets, all acquirers (3)		Stock acquisitions (4)		Cash acquisitions (5)		"Pubpubstock" deals (6)	
	N	BHAR	N	BHAR	N	BHAR	N	BHAR	N	BHAR	N	BHAR
1) All (including non-wave industries)	3182	-0.4137	1368	-0.4976	915	-0.4119	774	-0.7656	828	-0.2128	544	-0.6849
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
2) Pre-wave	276	-0.3890	134	-0.3675	80	-0.5016	105	-0.3378	42	-0.2528	67	-0.3945
		(0.0000)		(0.0002)		(0.0008)		(0.0148)		(0.1583)		(0.0145)
3) In-wave	522	-0.7939	268	-0.7990	125	-0.9778	252	-0.9798	66	-0.6857	177	-0.8582
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0002)		(0.0000)
4) Post-wave	306	-0.7358	155	-0.7727	92	-1.0959	105	-1.3040	50	0.0343	70	-1.0915
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.8854)		(0.0000)
5) Non-wave (only wave industries)	283	-0.3478	129	-0.3847	69	-0.3404	101	-0.3963	59	-0.3276	68	-0.3613
		(0.0000)		(0.0005)		(0.0774)		(0.0085)		(0.0386)		(0.0200)
6) Non-wave (all industries)	2079	-0.2736	811	-0.3669	618	-0.1841	312	-0.5554	670	-0.1821	230	-0.5124
		(0.0000)		(0.0000)		(0.0001)		(0.0000)		(0.0000)		(0.0000)
Non-wave – In-wave (5 - 3)		**0.4460**		**0.4143**		**0.6374**		**0.5835**		0.3580		**0.4969**
p-value of difference		**(0.0000)**		**(0.0011)**		**(0.0044)**		**(0.0005)**		(0.1292)		**(0.0045)**

Table 3.5 (Continued). Post-Announcement Acquirer 5-Year BHAR, by Merger Wave Phase and Type of Acquisition

Panel B: 1981-2009, cash waves

	All acquisitions (1)		Public targets, all acquirers (2)		Private targets, all acquirers (3)		Stock acquisitions (4)		Cash acquisitions (5)		"Pubpubstock" deals (6)	
	N	BHAR	N	BHAR	N	BHAR	N	BHAR	N	BHAR	N	BHAR
1) All (including non-wave industries)	3182	-0.4137	1368	-0.4976	915	-0.4119	774	-0.7656	828	-0.2128	544	-0.6849
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
2) Pre-wave	554	-0.2796	245	-0.3537	175	-0.2227	150	-0.4247	97	-0.2224	100	-0.4405
		(0.0000)		(0.0000)		(0.0080)		(0.0000)		(0.0212)		(0.0001)
3) In-wave	797	-0.3900	368	-0.5043	206	-0.3994	175	-0.8568	262	-0.1471	132	-0.7314
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0315)		(0.0000)
4) Post-wave	578	-0.3660	251	-0.4186	185	-0.4262	144	-0.8711	154	-0.0889	98	-0.7250
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.2385)		(0.0000)
5) Non-wave (only wave industries)	628	-0.4998	284	-0.6122	161	-0.4888	194	-0.7464	137	-0.2823	148	-0.7236
		(0.0000)		(0.0000)		(0.0001)		(0.0000)		(0.0039)		(0.0000)
6) Non-wave (all industries)	1254	-0.5091	504	-0.6019	349	-0.5067	305	-0.8312	315	-0.3251	214	-0.7521
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
Non-wave – In-wave (5 - 3)		-0.1098		-0.1079		-0.0894		0.1104		-0.1353		0.0078
p-value of difference		(0.1091)		(0.2283)		(0.5417)		(0.3949)		(0.2485)		(0.9553)

This table reports the mean acquirers' 5-year post-acquisition, buy-and-hold abnormal returns (BHARs). Post-acquisition 5-year buy-and-hold abnormal returns are the difference between acquirers' buy-and-hold 60-month returns and the compound return of an equally weighted portfolio matched on size and book-to-market. We sort the bidders according to the timing of the announcement date relative to the industry-specific waves. The pre-wave period includes the year before the beginning of the wave, the in-wave period includes the two years of the wave, and the post-wave period spans the three years following the end of the wave. Non-wave periods cover the rest of the time period. Panel A reports the results using the *stock* waves (industry merger waves defined using only pure stock offers), whereas Panel B reports the results using the c*ash* waves (industry merger waves defined using only pure cash offers). We report the mean return for each category, along with the p-value associated with the t-test for statistical difference from zero. The bottom part of each panel shows the results of a t-test for difference in mean return of non-wave acquirers (line 5) and in-wave acquirers (line 3).

We report the mean acquirers' 5-year post-acquisition BHARs for: all types of acquisitions (column 1), the acquisitions of public and private targets (columns 2 and 3), the pure stock and pure cash acquisitions (columns 4 and 5), and public acquirers that made a pure stock offer for a public target (column 6). P-values are presented in parentheses. Boldfaced differences in mean post-acquisition returns and associated p-values are significant at the 5% level or higher. For ease of reading, we do not use boldface characters for simple mean returns, even when they are significantly different from zero.

Table 3.6. Regressions of Acquirer 5-Year BHAR on Value-to-Price Ratios and Merger Wave Phases

Panel A: 1981-2009, stock waves

	All acquisitions	Public targets, all acquirers	Private targets, all acquirers	Stock acquisitions	Cash acquisitions	"Pubpubstock" deals
Intercept	**-0.3742**	**-0.4341**	**-0.3603**	**-0.7730**	**-0.2135**	**-0.6964**
	(0.0050)	**(0.0009)**	**(0.0288)**	**(0.0000)**	**(0.0701)**	**(0.0000)**
VP	0.0994	0.0617	0.2165	**0.3060**	0.0061	**0.2355**
	(0.3384)	(0.5276)	(0.1145)	**(0.0003)**	(0.9471)	**(0.0000)**
In-and-post-wave	**-0.9053**	**-0.8210**	**-1.2302**	**-0.8208**	**-0.3790**	**-0.7320**
	(0.0222)	**(0.0277)**	**(0.0012)**	**(0.0253)**	**(0.0988)**	**(0.0844)**
In-and-post-wave*VP	**0.6721**	**0.6078**	**0.8264**	**0.7176**	0.2556	**0.6427**
	(0.0497)	**(0.0272)**	**(0.0112)**	**(0.0012)**	(0.3573)	**(0.0364)**
Pre-wave	-0.1594	-0.1331	-0.0779	0.2119	0.0130	0.0687
	(0.2016)	(0.3770)	(0.7983)	(0.3776)	(0.9712)	(0.8249)
Pre-wave*VP	0.0775	0.1880	-0.2969	0.0728	-0.0667	0.1243
	(0.5430)	(0.2314)	(0.2181)	(0.7532)	(0.8338)	(0.6553)
R-squared	(0.0689)	(0.0735)	(0.1336)	(0.1486)	(0.0071)	(0.1111)
N	3042	1333	870	768	753	545

Table 3.6 (Continued). Regressions of Acquirer 5-Year BHAR on Value-to-Price Ratios and Merger Wave Phases

Panel B: 1981-2009, cash waves

	All acquisitions	Public targets, all acquirers	Private targets, all acquirers	Stock acquisitions	Cash acquisitions	"Pubpubstock" deals
Intercept	**-0.7992**	**-0.7904**	**-1.0382**	**-1.1296**	**-0.5216**	**-0.9209**
	(0.0000)	**(0.0000)**	**(0.0000)**	**(0.0000)**	**(0.0307)**	**(0.0000)**
VP	**0.3028**	**0.2050**	**0.6374**	**0.5265**	0.1723	0.2689
	(0.0148)	**(0.0965)**	**(0.0009)**	**(0.0046)**	(0.3873)	(0.1008)
In_and_post_wave	0.0707	-0.0091	0.2356	-0.2687	0.3733	-0.3403
	(0.7704)	(0.9727)	(0.4330)	(0.2461)	(0.1599)	(0.2685)
In_and_post_wave*VP	0.1664	0.2112	-0.0213	**0.3913**	-0.1706	**0.5530**
	(0.3914)	(0.2293)	(0.9421)	**(0.0213)**	(0.4634)	**(0.0078)**
Prewave	**0.4305**	0.2656	**0.8099**	**0.6555**	0.0662	0.3406
	(0.0073)	(0.1438)	**(0.0052)**	**(0.0049)**	(0.7622)	(0.3358)
Prewave*VP	-0.1919	0.0007	**-0.6300**	**-0.4575**	0.1051	-0.1151
	(0.1133)	(0.9965)	**(0.0079)**	**(0.0166)**	(0.7226)	(0.6967)
R-squared	(0.0367)	(0.0377)	(0.0656)	(0.1170)	(0.0135)	(0.0890)
N	3042	1333	870	768	753	545

This table presents the results of the logit estimation of the following model:

$$BHAR_{it} = \alpha + \beta_1 VP_{it-1} + \beta_2 In\_and\_post\_wave_{it} + \beta_3 VP_{it-1} * In\_and\_post\_wave_{it} + \beta_4 Prewave_{it} + \beta_5 VP_{it-1} * Prewave_{it} + \varepsilon_{it}$$

where *BHAR* is the acquirers' 5-year post-acquisition, buy-and-hold abnormal returns (BHARs). Post-acquisition 5-year buy-and-hold abnormal returns are the difference between acquirers' buy-and-hold 60-month returns and the compound return of an equally weighted portfolio matched on size and book-to-market. VP_{t-1} is the firm *i*'s Value-to-Price ratio measured at the end of year *t*-1. *Prewave* and *In_and_post_wave* are indicator variables that take the value 1 if year *t* is a pre-wave or an in-wave or post-wave year in firm *i's* industry.

In Panel A, waves are defined using only pure stock offers (*stock* waves), whereas in Panel B, the waves considered are defined using only the cash bids (*cash* waves). We report regression coefficients for all types of acquisitions (column 1), the acquisitions of public and private targets (columns 2 and 3), the pure stock and pure cash acquisitions (columns 4 and 5), and public acquirers that made a pure stock offer for a public target (column 6). The number of observations and R-squared of each regression are also reported. *P*-values (in parentheses) are calculated using standard errors clustered by both industry and year. Boldfaced coefficients and associated *p*-values indicate statistical significance at the 5% level or higher.

Table 3.7. Acquirers' Announcement-Period Cumulative Abnormal Return (CAR), by Merger Wave Phase and Type of Acquisition

Panel A: 1981-2009, stock waves

	All acquisitions (1)		Public targets, all acquirers (2)		Private targets, all acquirers (3)		Stock acquisitions (4)		Cash acquisitions (5)		"Pubpubstock" deals (6)	
	N	CAR	N	CAR	N	CAR	N	CAR	N	CAR	N	CAR
1) All (including non-wave industries)	3181	0.0025	1368	-0.0170	921	0.0156	777	-0.0101	829	0.0118	546	-0.0250
		(0.0540)		(0.0000)		(0.0000)		(0.0032)		(0.0000)		(0.0000)
2) Pre-wave	277	0.0062	134	-0.0138	81	0.0258	106	-0.0012	42	0.0059	67	-0.0215
		(0.1419)		(0.0115)		(0.0022)		(0.8847)		(0.5289)		(0.0336)
3) In-wave	530	-0.0006	271	-0.0217	129	0.0232	257	-0.0131	69	0.0227	179	-0.0288
		(0.8694)		(0.0000)		(0.0227)		(0.0457)		(0.0056)		(0.0000)
4) Post-wave	308	-0.0076	155	-0.0314	93	0.0183	105	-0.0067	51	-0.0003	70	-0.0196
		(0.2040)		(0.0000)		(0.1918)		(0.5155)		(0.9797)		(0.0351)
5) Non-wave (only wave industries)	285	0.0067	130	-0.0199	69	0.0242	102	-0.0108	60	0.0065	69	-0.0262
		(0.1060)		(0.0009)		(0.0017)		(0.1414)		(0.4120)		(0.0024)
6) Non-wave (all industries)	2092	0.0042	817	-0.0133	621	0.0125	313	-0.0111	674	0.0115	231	-0.0247
		(0.0028)		(0.0000)		(0.0000)		(0.0201)		(0.0000)		(0.0000)
Non-wave – In-wave (5 - 3)		0.0074		0.0018		0.0011		0.0023		-0.0162		0.0025
p-value of difference		(0.1981)		(0.8105)		(0.9325)		(0.8154)		(0.1515)		(0.8128)

Table 3.7 (Continued). Acquirers' Announcement-Period CAR, by Merger Wave Phase and Type of Acquisition

Panel B: 1981-2005, cash waves

	All acquisitions (1)		Public targets, all acquirers (2)		Private targets, all acquirers (3)		Stock acquisitions (4)		Cash acquisitions (5)		"Pubpubstock" deals (6)	
	N	CAR	N	CAR	N	CAR	N	CAR	N	CAR	N	CAR
1) All (including non-wave industries)	3181	0.0025	1368	-0.0170	921	0.0156	777	-0.0101	829	0.0118	546	-0.0250
		(0.0540)		(0.0000)		(0.0000)		(0.0032)		(0.0000)		(0.0000)
2) Pre-wave	556	0.0016	245	-0.0168	177	0.0148	151	-0.0106	98	0.0071	100	-0.0312
		(0.5761)		(0.0001)		(0.0006)		(0.1699)		(0.1929)		(0.0003)
3) In-wave	805	0.0038	371	-0.0129	209	0.0146	177	-0.0028	267	0.0139	133	-0.0113
		(0.1613)		(0.0001)		(0.0169)		(0.7242)		(0.0014)		(0.0804)
4) Post-wave	582	-0.0030	251	-0.0195	188	0.0039	145	-0.0125	154	0.0051	98	-0.0221
		(0.3743)		(0.0000)		(0.5318)		(0.1123)		(0.3471)		(0.0107)
5) Non-wave (only wave industries)	634	0.0002	288	-0.0195	161	0.0145	196	-0.0162	139	0.0101	150	-0.0283
		(0.9341)		(0.0000)		(0.0148)		(0.0068)		(0.0299)		(0.0000)
6) Non-wave (all industries)	1264	0.0044	510	-0.0190	350	0.0232	308	-0.0123	317	0.0137	216	-0.0318
		(0.0303)		(0.0000)		(0.0000)		(0.0167)		(0.0000)		(0.0000)
Non-wave – In-wave (5 - 3)		-0.0036		-0.0067		-0.0002		-0.0134		-0.0038		**-0.0170**
p-value of difference		(0.3581)		(0.1946)		(0.9845)		(0.1714)		(0.5519)		**(0.0462)**

This table reports the mean 3-day acquirers' cumulative abnormal returns (CARs) centered around the announcement date. CARs are calculated using a 3-day window centered on the announcement day. We use the market model, using CRSP value-weighted index as the benchmark, to calculate abnormal returns. We sort the bidders according to the timing of the announcement date relative to the industry-specific waves. The pre-wave period includes the year before the beginning of the wave, the in-wave period includes the two years of the wave, and the post-wave period spans the three years following the end of the wave. Non-wave periods cover the rest of the time period. Panel A reports the results using the *stock* waves (industry merger waves defined using pure stock offers), whereas Panel B reports the results using the *cash* waves (industry merger waves defined using only pure cash offers). We report the mean return for each category, along with the p-value associated with the t-test for statistical difference from zero. The bottom part of each panel shows the results of a t-test for difference in mean return of non-wave acquirers (line 5) and in-wave acquirers (line 3).

We report the mean 3-day acquirers' CARs for: all types of acquisitions (column 1), the acquisitions of public and private targets (columns 2 and 3), the pure stock and pure cash acquisitions (columns 4 and 5), and public acquirers that made a pure stock offer for a public target (column 6). P-values are presented in parentheses. Boldfaced differences in mean post-acquisition returns and associated p-values are significant at the 5% level or higher. For ease of reading, we do not use boldface characters for simple mean returns, even when they are significantly different from zero.

CHAPTER 4

Does the Weather Influence Global Stock Returns?[77]

4.1. Introduction

Does daily weather influence investor behavior? This question is of great interest to financial economists as well as psychologists. Indeed, because there is no ambiguity in causality between the weather and stock returns, studying the relationship between weather and returns avoids the endogeneity problem that plagues finance research. Moreover, because daily weather conditions such as sunlight and wind are extraneous and transitory events that are hardly related to economic prospects, a significant association between weather and returns, if established, will strongly suggest that weather influences stock returns through the channel of investor psychology. However, prior research has produced mixed evidence of the impact of weather on stock returns. On the one hand, some authors document a positive association between sunlight and stock returns (Saunders (1993), Hirshleifer and Shumway (2003) and Goetzmann et al. (2015)). On the other hand, it has been argued that such a relationship is either spurious or sample-specific (e.g., Trombley (1997), Loughran and Schultz (2004), and Dowling and Lucey (2008)). Evidence on the effects of other weather variables is either weak or more controversial.[78]

Existing studies have assumed that the effects of weather on returns are uniform across geographical regions and seasons. However, as we argue below, there are strong reasons to expect that the psychological effects of weather on optimism or risk-taking will be highly dependent on regional and seasonal conditions. This implies that the same weather variable could be expected

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[77] The current version is co-authored with Ming Dong.

[78] For example, Hirshleifer and Shumway (2003) find that rain and snow are unrelated to returns after controlling for sunshine. Cao and Wei (2005) document a negative effect of temperature on returns, but others have raised doubt on this effect (e.g., Jacobsen and Marquering (2008)). There is additional sporadic research that tries to relate stock returns to the weather. Among others, Shu and Hung (2009) show a negative effect of wind on stock returns of a sample of European countries, but it is unclear why a wind effect is not reported for other countries.

to have opposite mood effects in different climates. If so, prior research may not adequately capture such weather effects. In this paper, we probe more deeply into the effects of weather on investor mood by analyzing a wide range of weather variables, allowing for the possibility that the weather effects vary by climatic conditions and season.[79] In addition, weather-related research increasingly focuses on the mechanisms through which weather-influenced mood affects asset prices (e.g. Bassi, Colacito and Fulghieri (2013) and Goetzmann et al. (2015)). By relating the strength and timing of the weather effects to individuals' seasonal propensity to spend time outdoors, we make further headway in this direction.

We believe that the effects of the weather on mood depend critically on geographical regions, and more precisely, regions defined by their annual average temperature. The psychology literature shows that the valence of mood (e.g., good versus bad mood) is sensitive to temperature: mood is positively associated with temperature, except in very high or low temperature environments (e.g., Wyndham (1969), Allen and Fisher (1978), and Howarth and Hoffman (1984)). Countries located near the Equator experience lower seasonal variations in temperature than colder countries; as such, we expect differentiated behavioral responses to weather across climates. In addition, other weather variables may also have a climate-specific impact on affect, thus reinforcing the case for climate-specific tests. For example, rain and wind may adversely influence mood in cold countries because they tend to exacerbate the perceived temperature, but in hot countries, rain and wind may be much less disruptive, or may even be welcomed, if they reduce the effective temperature.

[79] The literature on the effects of weather on stock returns has moved from the aggregate index level effects to the effects on individual stocks of a particular country, and most often focuses on the sunshine effect (e.g., Loughran and Schultz (2004) and Goetzmann et al. (2015)). In contrast, we examine the effects of five weather variables on country index returns by conditioning the weather effects on climate and season. As discussed below, we uncover previously undocumented patterns of effects of all five weather variables on index returns.

Similarly, there are strong reasons to expect the psychological effects of the weather to vary with seasons. To the extent that the effects of temperature fluctuations and other weather conditions depend on temperature, the effects should vary across the seasons. Also, there is psychological evidence of seasonal shifts in human mood (e.g., Keller et al. (2005), Kamstra, Kramer, and Levi (2003)). Furthermore, if the weather effects are felt more strongly in the outdoors, the strength of these effects may show seasonal patterns because individuals allocate outdoor time differently across the seasons.

Two additional considerations further support our idea to conduct separate tests sorted by climate and season. First, this approach isolates the well-known seasonality in returns from the genuine weather effects. For example, since winter tends to have higher returns than summer (e.g., Jacobsen and Marquering (2008)), in testing the effect of snow on returns, summer days should not be pooled with winter days to isolate the effect of snow from non-weather related seasonality. Second, conducting separate tests by climate and season allows us to capture weather effects unique to each climate/season. For instance, we find that summertime wind and rain have a negative effect on returns in cold countries, but they have a positive effect in hot countries. Such effects are unable to be picked up in a pooled all-month or all-region test, even if we allow nonlinearity in the specification.

We investigate the effects of five weather variables—sunshine, wind speed, rain, snow depth on the ground, and temperature—on nominal index returns of 49 countries from 1973 to 2012. Following Hirshleifer and Shumway (2003), we use daily weather variables observed in the cities where our sample countries' national exchanges are located as proxies for the most relevant conditions for each country, and conduct both ordinary least squares regressions (of daily returns) and logit regressions (of the probability of a positive return) on the weather variables, with standard errors clustered by both country and day to take into account the possible error correlations across countries and time. We sort the countries into three groups based on the average year-around temperature, shift the timing of countries in the Southern Hemisphere by six months to align the seasons, and conduct month-by-month tests for each temperature region. Our large sample is essential for conducting tests for each region and each month; since stock returns are primarily driven by non-weather economic events, a large sample is necessary to neutralize various economic effects and detect the effects of the weather.[80]

The regression results indicate wide-spread statistical significance of all five weather variables. We use two approaches to verify that the relation between weather and returns is real rather than spurious. First and most directly, we test the profitability of a trading strategy based purely on daily weather. Since the null hypothesis predicts no relationship between weather and daily return, finding significant profits from this trading strategy would indicate that the associations between weather and returns are real. Such a conclusion does not rely on specific interpretations of the weather effects. Second, we examine whether the patterns of relationships between weather and returns in both the OLS and logit tests can be interpreted in a systematic way that is consistent with finance and psychology theories.

To test the profitability of a weather-based trading strategy, we assume that the weather variables can predict daily index returns, and we use the predicted returns (estimated with the OLS regression coefficients and the pre-market weather conditions) to construct a hedge strategy. Specifically, for each day we form a hedge portfolio by taking a long position in the index of the country that has the highest predicted return and a short position in the country that has the lowest

[80] Trombley (1997) also conducts month-by-month tests on the relation between U.S. index returns and cloud cover, but fails to establish a clearly positive relationship between sunshine and returns, presumably because the sample used is only one index series spread across the 12 calendar months.

predicted return, and we rebalance the hedge portfolio on a daily basis. We test the profitability of two types of strategies. The first strategy defines one hedge portfolio for each of the three temperature regions, whereas the second defines one hedge portfolio for each of the three time zones (the Americas, Asia-Pacific, and Europe-Africa).

We find that using an out-of-sample estimation in which we construct trading portfolios using only ex ante information, a temperature-region based hedge strategy generates significant gross profits for the cold and mild regions. We also find that the time-zone based hedge strategy produces significant profits for the Europe-Africa region. The gross profits range from an annualized return of 17.6% (t = 2.38) for the Europe-Africa time zone to 25.1% (t = 2.80) for the mild temperature region (with trading of the latter strategy limited to the Northern Hemisphere) during 1993-2012.[81] Assuming transaction costs of five basis points per transaction, the net profits of the weather-based hedge strategy range from an annualized return of 4.3% for the Europe-Africa time zone to 11.8% for the mild temperature region (trading again limited to the Northern Hemisphere). Therefore, a strategy that exploits the stock return predictability of the weather can indeed generate substantial profits.

Our second approach to judging whether the results from the regression analysis represent real effects is to evaluate whether the patterns of the weather effects can be interpreted in a systematic way compatible with financial and psychological theories. Drawing from the finance and psychology literatures, we make two hypotheses about the weather effects (Section 4.4.1 has more details). First, comfortable weather should lead to an upbeat investor mood and therefore

[81] Such high magnitude of hedge profits is only possible when we use the full observable weather variables to predict daily returns. If we use one weather condition at a time (such as sunshine) to construct hedge portfolios, trading profits are mostly insignificant in the out-of-sample estimation, with the sunshine-only hedge strategy producing significant but weaker results in the full-sample estimation and insignificant profit in the out-of-sample estimation. Section 4.3 has more details.

high stock returns. This "comfortable weather" hypothesis offers a basic guidance as to the *sign* of each weather variable in different seasons and regions. Second, the weather effects on returns should be stronger when people spend more time outdoors or when outdoor time is more valuable. This "outdoors" hypothesis offers guidance about the *strength* of the weather effects and the times when we are more likely to observe them. Owing to the contingent nature of the weather effects, the purpose of these hypotheses is to provide a useful general guidance of expectations from the tests. We estimate the average time spent outdoors following the methodology of Graff Zivin and Neidell (2014), and confirm the intuition that for all temperature regions, the outdoor time is longest in the summer and shortest in the winter, and that the hot region has considerable outdoor time in the winter. Most of the weather effects appear to be consistent with our "comfortable weather" and "outdoors" hypotheses. The following are our salient findings:

- Sunshine has a positive effect on daily returns for all temperature regions, and the strength of this effect tends to correlate with the time spent outdoors.
- In the cold region, wind and rain have a negative effect on returns both in the summer and in the spring, suggesting that windy or rainy conditions are disruptive to outdoor activity during the long-awaited warmer seasons. In the hot region, wind and rain have a positive effect on returns in the summer, consistent with the cooling effects they provide, and in sharp contrast to their effects in the cold region.
- Snow depth (applicable only to cold countries from December to March) has a negative effect on returns.
- Temperature exhibits nonlinear effects on mood, some of which are consistent with the comfortable weather hypothesis. Returns in the cold region are negatively influenced by summertime temperature, suggesting a preference for cooler weather under high temperatures.

In hot countries, returns are higher on cool days in the summer and on warm days in the winter and spring, possibly because people in hot countries welcome warmer, but not sweltering, weather.

- However, in cold and mild countries, stock returns and temperature are strongly negatively correlated in the winter (from December to February). This is incompatible with the comfortable weather hypothesis, but it is consistent with evidence from experimental psychology that at very low temperatures, subjects tend to exhibit increased aggression or risk-seeking behaviors (e.g, Howarth and Hoffman (1984) and Schneider et al. (1980)).[82]

In summary, even though the exact psychological effect of each weather condition for each climate and season warrants further investigation, the preponderance of our results tends to support the hypothesis that comfortable weather conditions promote investor optimism and lead to high stock returns, especially during seasons of increased outdoor activity. The sole exception, but one that is also consistent with psychological evidence, is our finding that in cold environments, low temperature is associated with high returns, consistent with bitter low temperature stimulating risk seeking and stock buying. The existence of systematic patterns of the weather effects on stock returns suggests that emotions are the channel through which weather and returns are related.

We make several contributions to the literature on how weather affects investor psychology. First, we confirm that weather has real effects on global stock returns. We show that these effects are substantial by constructing profitable hedge strategies based purely on ex ante

[82] The regression tests suggest very high statistical significance of the weather effects. For example, based on the *p*-values of the OLS estimates, in the cold region, the probability of randomness causing the observed negative wind effects during June-August is approximately 0.0002 (assuming independent effects for different months); the probability of random negative temperature effects during December-February is 0.00006. Similar conclusions can be made simply from the sign of the weather effects. For example, the probability of randomly observing 15 negative signs (out of 16) for the wind and rain effects (both OLS and logit) during March-October is 0.0002. Considering the entire pattern of weather effects strengthens our case against the spurious effects argument.

weather variables. Second, while prior literature treats the weather effects as uniform across climates and seasons, we show that the weather effects are contingent on the geographical and seasonal environments. This explains why we find a number of previously undocumented effects and why we find all five weather conditions significantly influence returns. In contrast, previous research primarily focuses on the effect of sunshine on financial markets (e.g., Saunders (1993), Hirshleifer and Shumway (2003), and Goetzmann et al. (2015)), and treats other effects such as temperature as either insignificant or uniformly negative on returns (e.g., Dowling and Lucey (2008), Cao and Wei (2005), and Kamstra, Kramer, and Levi (2003)); rain, snow, and wind have never been documented as significantly affecting returns in a well-presented global sample in prior literature.[83] Third, while most of our findings of the weather effects support the intuitive interpretation that comfortable conditions lead to higher stock returns (especially during seasons when people expect to spend time in the outdoors), we also uncover a strong perverse effect in cold climates that low temperature leads to higher returns, suggesting a rich structure of the weather effects on mood.

4.2. Sample and Research Design

4.2.1. Sample

We retrieve daily index returns from Datastream. All countries for which Datastream's Global Equity Index is available are included in our sample. Table 4.1 lists the countries included in our sample, as well as the coverage period where both the returns and weather data are available.

[83] The relatively consistent effect of sunshine on returns across regions and seasons helps explain why prior research finds a positive sunshine effect even without conditioning the analysis on region and season. Still, this unconditional approach misses the more subtle aspects of the sunshine effect, such as its higher strength during seasons of increased outdoor activity. In addition, given that rain has the opposite emotional effect under cold versus warm climates, the perception in prior literature of a negative rain effect on mood seems to be attributable to negligence of the hot region. Lastly, the overall negative relation between temperature and returns conceals the fact that both positive and negative associations exist between temperature and returns for both the cold and hot regions, with differing strengths of this relation across seasons.

For some countries, there are gaps in coverage; hence the varying numbers of valid of observations. Table 4.1 also shows each country's mean and standard deviation of percentage daily returns over that coverage period. All returns are nominal returns in local currency and include dividends.

We collect weather data from the Integrated Surface Database (ISD) managed by the National Climatic Data Center (NCDC, http://www.ncdc.noaa.gov/data-access/quick-links#dsi-3505). For each country included in our sample, we select the weather station closest to the country's main stock exchange. If the selected series have a gap in coverage, we complement the weather data series with data from the second-nearest weather station, if available, as long as the complementing weather station is within a distance of 50 kilometers from the country's main stock exchange.[84]

We sort our full sample into three geographical regions. Specifically, we classify cold, mild, and hot countries using the 33rd and 67th percentiles of the full sample's distribution of annual temperature. Panel A of Table 4.2 lists the countries included in each region.

We retrieve sky cover, temperature, wind speed, precipitation, and snow depth data from the ISD. We construct all five weather variables based on the average value of hourly observations of each weather condition between 6:00 AM and 4:00 PM local time, following Hirshleifer and Shumway (2003). We operationalize the descriptive sky cover variable (SKC) by following Hirshleifer and Shumway (2003) and assigning a value of 0 to clear skies, a value of 2.5 to scattered cloud cover, a value of 6 to broken cloud cover, and a value of 8 to completely overcast skies. Wind speed (SPD) is measured in miles per hour and temperature (TEMP) is in Fahrenheit. RAIN

[84] Our results remain if we do not complement the principal weather series, or if we take the average of the observations from the weather station closest to the financial exchange and the second-nearest station.

is an indicator variable that is equal to 1 if some liquid precipitations were recorded between 6:00 AM and 4:00 PM local time on the day of the measurement.[85,86] Otherwise, RAIN is equal to zero.

We measure snowiness condition by snow depth (SD), which is the average daily snow cover in inches on the ground measured between 6:00 AM and 4:00 PM. Non-zero or non-missing snow related variables are sparse in the mild and hot regions. We thus exclude SD from the regression models for the mild and hot countries. Likewise, for the cold countries, we omit SD in the regression tests from April through November owing to the sparse and sometimes implausible non-zero records for those months.[87]

We run month-by-month tests to take into account any seasonality effects. Like Loughran and Schultz's (2004), our methodology departs from Hirshleifer and Shumway's (2003) by not deseasonalizing the weather variables. We shift the timing of Southern Hemisphere countries by six months, so that seasons in both hemispheres are synchronous.

Panel B of Table 4.2 reports the mean, median and standard deviation of the annual average temperatures of the countries included in each region. Panel B also shows the number of observations with non-missing weather and returns data by region, and reveals that countries with shorter coverage periods are almost exclusively mild or hot countries. Panel C reports the mean, median and standard deviation of our main weather variables and of percentage daily returns, by

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[85] The ISD dataset contains four different precipitation variables: PCP01, PCP06 and PCP24 record the liquid precipitations (in inches) in, respectively, the 1, 6 and 24 hours immediately preceding the weather record. PCPXX records the liquid precipitations in an indefinite period of time immediately preceding the record. We construct the variable RAIN using the PCP06 raw variable, as this variable is the one with the least missing observations. As such, RAIN is defined based on the average PCP06 value observed between 12:00 AM and 4:00 PM local time.

[86] In unreported tests, we find similar results when RAIN is defined with the further requirement that the average temperature on the measurement day is equal or above 32° Fahrenheit. Also, if we define RAIN to be a continuous rather than an indicator variable, our results remain materially unchanged.

[87] In untabulated tests, we exclude observations with extreme weather conditions. Specifically, we exclude observations for which SPD and SNOW are higher than the historical country-month 95th SPD and SNOW percentiles, and observations for which SKC and TEMP are either above the historical country-month 95th SKC or TEMP percentiles, or below the 5th SKC or TEMP percentiles, respectively. Our main results are robust to the exclusion of such observations, suggesting that the weather effects we document are not caused by extreme weather conditions.

region. Cold countries are significantly cloudier, windier and rainier than hot countries. In cold countries, RAIN seems to be correlated with SKC. We address this concern in our multivariate tests by testing for multicollinearity (results unreported) and conclude that multicollinearity is not a concern in our sample, with the highest correlation being 0.289 (between SKC and RAIN). Panel C also shows that cold countries have mean returns that are lower and less volatile than hot countries, although the difference in mean returns is not significant.

4.2.2. Regression Test Design

We sort our sample by region and month (rather than by country and month, to allow for a sufficient sample size in each region-month group), and estimate the following pooled regression of daily index returns of countries in each region-month group: [88]

$$r_{it} = \alpha_t + \beta_1 SKC_{it} + \beta_2 SPD_{it} + \beta_3 RAIN_{it} + \beta_4 SD_{it} + \beta_5 TEMP_{it} + \varepsilon_{it},$$

where *i* indexes countries in a particular region-month group and *t* denotes trading day. For the mild and hot countries and for the months from April through November in the cold countries, we estimate a reduced form of this model and drop SD from the regression, to reflect the absence of snow cover in these periods and regions. In addition, because it is possible that weather effects are related to the sign of the returns and not their magnitude, we estimate the following logit model:

$$P(r_{it} > 0) = \frac{1}{1 + e^{-(\alpha + \beta_1 SKC_{it} + \beta_2 SPD_{it} + \beta_3 RAIN_{it} + \beta_4 SD_{it} + \beta_5 TEMP_{it})}},$$

where $P(r_{it} > 0)$ is an indicator variable that is equal to 1 if the returns of country *i*'s market index on day *t* is positive, and zero otherwise.

[88] In unreported tests, we also regress returns on the changes in the weather variables from the previous day. Results show that the change variables are much less significant and consistent compared to the level variables, suggesting that it is the current weather conditions themselves that influence returns. Also, to the extent that countries in the same temperature region still have differing climates, forming region-month groups tends to bias our tests against finding significant weather effects on returns.

For both the OLS and the logit regressions, in addition to estimating the model on a month-by-month basis, we also run an "all-month" regression by pooling all months together, to see the net effect of the weather on daily returns by region only. In both the OLS and the logit regressions, standard errors are clustered by country and day to account for the regression residuals' contemporaneous correlation for each region-month group and within-group autocorrelation across time, in line with the recommendations of Petersen (2009) and Cameron, Gelbach, and Miller (2011).

A technical point in detecting the effects of the weather on stock returns is the treatment of return outliers. Our purpose is to examine the effects of *non-economic,* weather variables. If extreme daily returns are primarily caused by economic events, it seems necessary to remove extreme return outliers from our tests, because such outliers are least likely caused by the weather while exerting the largest impact on regression results. We therefore follow Saunders (1993) and remove returns with absolute value greater than a certain threshold, and assess the robustness of our results by varying this threshold. The selection of the threshold value reflects a tradeoff: a stricter (i.e., lower) threshold eliminates non-weather driven observations, but if we remove too many large return observations, we risk omitting valuable signals and making our tests influenced by noises (i.e., small returns) caused by liquidity trading.

When we do not apply any filter and keep all observations, we find significant effects of the weather variables, at least for certain regions and months. (The OLS and logit regression results for the full sample with no filter rule applied are available from the authors upon request.) We obtain our base case results, presented in this paper, when we apply a 2.5% filter rule (corresponding to filtering out 4.9% of all observations). Our results are fairly robust to the specific filters we use: results strengthen (relative to using no filters) if we apply the 3% filter rule, and vary only slightly if we impose a 2% filter rule.[89]

We report the OLS and logit regression results in Tables 4.3 and 4.4, respectively. For each of these regression tables, Panels A, B, and C report results for the cold, hot, and mild region, respectively. For both the OLS and logit regressions, we report the estimates of the coefficients, their associated *p*-values (in parentheses) and the economic impact [in square brackets]. The economic impact estimation procedures are described in Section 4.4.3, following a discussion of the results in Section 4.4.2.

In untabulated tests, for the "all-month" pooled regression, we also follow Jacobsen and Marquering (2008) and include a Sell-in-May (SIM) variable as an additional independent variable. SIM is an indicator variable equal to 1 during the months of January, February, March, April, November and December, and it is equal to 0 otherwise. This indicator is included to allow for the possibility that the effect of any weather variable may be caused by a seasonal weather pattern such as the Seasonal Affective Disorder (SAD) as presented in Kamstra, Kramer, and Levi (2003) or certain unidentified non-weather related seasonality factor.[90] We find that the all-month weather effects are robust to the inclusion of SIM, indicating that the weather effects we document are not caused by unknown seasonality factors.[91]

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[89] Under the null hypothesis of market efficiency, there should be no relation between weather and daily return no matter what filter rule we use. In Section 4.3, we discuss whether we can form profitable trading strategies by making use of the relation between weather and daily return. We find that trading strategies in which filters are used to estimate the weather-return relation lead to higher profits than when filters are not used, confirming that the use of filters helps capture genuine relationship between weather and returns.

[90] In further untabulated tests, we use the daylight-related Seasonal Affective Disorder (SAD) variable, as defined in Kamstra, Kramer and Levi (2003), instead of the Sell-In-May indicator variable. The weather effects remain unchanged.

[91] In addition, since we examine the weather effects on country index returns, the effects we study here are different from the seasonality in cross-sectional returns documented in Heston and Sadka (2010).

4.2.3. Assessing the Weather Effects

The results in Tables 4.3 and 4.4 indicate wide-spread statistical significance of all five weather variables in both the OLS and logit regressions. We use a two-stage approach to verify whether the relation between weather and returns is real rather than spurious. First, before getting to the interpretation of the weather effects, we test (in Section 4.3) whether a trading strategy based purely on daily weather can be profitable; significant profitability from the weather-based trading strategies debilitates the spuriousness argument. We use the OLS regression coefficients and ex ante weather variables to predict daily returns and form hedge strategies. Second, we examine whether the salient effects of the weather on returns exhibit systematic patterns across climates and season (Section 4.4). The consistency of these patterns with hypotheses based on finance and psychology literatures is further evidence that the weather effects are real.

4.3. Trading Profits of Weather-Based Hedge Strategies

The premise of a weather-based strategy is that, if the weather variables predict daily returns, we can use the predicted returns to form a profitable hedge strategy that is long the country index with the highest predicted return and short the country index with the lowest predicted return. We adjust the hedge position on a daily basis and hold it for at least a 10-year horizon in order to neutralize non-weather effects.

Specifically, a hedge strategy consists of two steps. In the first step, we measure all five weather variables using pre-market hourly observations between 5:00 and 9:00 AM local time. We run OLS regressions (as in Table 4.3) excluding observations with absolute returns greater than 2.5%, because using this filter better captures the effects of the weather on returns and leads to more reliable return predictability. For each country, we estimate the predicted daily return using the OLS coefficients and the observed 5:00 – 9:00 AM weather variables.[92]

In the second step, for each region and each day, we form a hedge portfolio that takes a long position in the country with the highest predicted return and a short position in the country with the lowest predicted return, and we compute the daily portfolio's return (we keep all return observations including return outliers when computing portfolio profits). The daily actual return of the long-short strategy is *R_hedge= R_high – R_low*, where *R_high* and *R_low* are the realized returns of the countries with the highest and lowest predicted returns, respectively. We rebalance the hedge portfolio on a daily basis.

For each region *i*, we compute the mean and *t*-statistic of the profit from the hedge portfolio for a certain trading period, and we estimate the following time-series regression:

$$R\_hedge_{it} = alpha_i + b_i\, R\_w_{it} + \varepsilon_{it},$$

where *R_w* denotes Datastream's world index return. The intercept alpha is the "CAPM-adjusted" average daily hedge profit for a particular region. We use standard errors corrected for heteroskedasticity and autocorrelation up to four lags to calculate *t*-statistics of the mean hedge returns and *p*-values of the regression coefficients.

We construct hedge strategies both by temperature regions (cold, mild, and hot countries, as defined in Section 4.2) and by time zones (the Americas, Europe-Africa, and Asia-Pacific).[93] For implementality, a strategy within a certain temperature region assumes that we can accurately forecast and make use of same-day pre-market weather information for all countries in the same

[92] There is a loss of weather variable observations using pre-market weather data compared to using the 6:00 AM – 4:00 PM (local time) data, possibly because weather stations are less than fully staffed during pre-market hours. This loss is even greater if we use weather data observed during 5:00 AM – 8:00 AM as in Hirshleifer and Shumway (2003) to estimate the predicted daily returns, although hedge results remain qualitatively unchanged.
[93] These "time zones" are geographical regions that each encompasses several contiguous official time zones.

temperature region, whereas a time-zone strategy imposes a relatively minor assumption that weather information of countries in the same time zone is available, or easily forecast due to the short forecast horizon, at the time of forming the daily hedge positions.

4.3.1. Hedge Profits Using Out-of-Sample Estimation

In the out-of-sample estimation, we use only ex ante data to predict daily returns and construct hedge portfolios. Specifically, we use the first half of the sample period (1973-1992) as the start-up period to estimate the OLS regressions of daily returns on the weather variables, and we form hedge portfolios in the second half of the sample period, starting January 1993. Then, we increase the length of the estimation period incrementally by one full year at a time. For instance, we use the observations from 1973-1993 to predict the returns starting January 1994, and so forth. The equal split between the start-up estimation period and the trading period reflects a trade-off: a longer estimation period increases the accuracy of predicting returns but decreases the sample size of the trading period needed to reduce non-weather related effects.[94, 95]

Tables 4.5 and 4.6 report the (gross) profits of hedging strategies using the out-of-sample estimation method, sorted by temperature region and by time zone, respectively. Since the mean daily hedge return and the corresponding regression alpha are highly consistent with each other, we focus on the mean hedge returns in the discussions below.

Panel A of Table 4.5 shows that the weather-based strategies are profitable for the cold and mild regions. The hedge profits are substantial: a daily return of 0.055% (14.8% annually; $t = 2.55$)

[94] Our results are not highly sensitive to the choice of the estimation period. In unreported tests, when we use 1973-1997 as the start-up estimation period and 1998-2012 as the trading period, we find qualitatively similar but slightly stronger hedge profits.

[95] The out-of-sample method likely understates the hedge profits, because many of the observations in the estimation period come from the early part of the sample period and are of relatively poor data quality, introducing noise in calculating predicted returns and forming hedge portfolios.

for the cold region, and a daily return of 0.066% (18.0% annually; t = 2.26) for the mild region. When trading is limited to the Northern Hemisphere (Panel B), we observe a similar pattern of profits, but the hedge profits for the mild region are larger: daily return is 0.090% (25.1% annually; t =2.80). This may be a result of offsetting forces at play: allowing Southern Hemisphere countries in the strategy expands tradable assets, but at the same time introduces more region-specific risks that are more difficult to hedge away, if the Southern Hemisphere economies are less integrated with the rest of the world markets.

Panel A of Table 4.6 confirms that when forming portfolios by time zone, the out-of-sample hedge strategy is still profitable for the Europe-Africa time zone, when both hemispheres are tradable (daily return of 0.065%, or 17.6% annually; t = 2.38). When trading is limited to the Northern Hemisphere (Panel B), this strategy becomes less profitable (daily 0.048%, or 12.7% annually; t = 1.70).

Therefore, the gross profits of strategies formed using pre-market weather conditions appear substantial: the out-of-sample profits range from an annualized return of 17.6% for the Europe-Africa time zone to 25.1% for the mild region (limited to the Northern Hemisphere). The weather-based trading strategy remains profitable if we factor in transaction costs of five basis points per day (for the long and short positions): the net out-of-sample profits then range from an annualized return of 4.3% for the Europe-Africa time zone to 11.8% for the mild region (again, with trading limited to Northern Hemisphere countries). Our estimated transaction costs appear reasonable, because index funds of the countries our trading strategy invests in are, for the most part, highly liquid Northern Hemisphere countries (at least, during the out-of-sample period). However, our hypotheses do not hinge on the accuracy of the transaction costs associated with our trading strategy: under the efficient market hypothesis, our weather-based trading strategy should not generate profits even before transaction costs. Our findings that a weather-based hedge strategy yields significant (gross or net) profits thus further validate the realness of weather effects.

4.3.2. Hedge Profits Using Full-Sample Estimation

In the full-sample estimation, we use the weather and returns data of the entire 1973-2012 sample period to estimate the OLS regressions (with the weather observed between 5:00 AM and 9:00 AM local time each day), and to calculate the daily predicted returns. Given the importance of sample size for estimating the weather effects, this method has the advantage of obtaining the most reliable estimation of the OLS coefficients as well as the daily predicted returns. This method also allows us to examine the time trend of the hedge profits over the entire sample period. However, this method is subject to the look-ahead criticism that ex post weather-returns relationship is used to predict each daily return. Therefore, we only present the hedge profits graphically to compare the magnitude of weather-based trading profits across time.

Figure 4.1 displays the hedge profits for each of the four decades in our sample (1973-1982, 1983-1992, 1993-2002, and 2003-2012), for the temperature-region and time-zone sorted strategies. It is evident that the weather-based strategies are more profitable in more recent periods, especially the last decade. This holds true both when trading is open to both hemispheres and when trading is limited to the North Hemisphere (results untabulated). Despite the increased popularity of global equity investments in more recent periods, there are no signs of fading profitability of weather-based strategies; the emotions of investors seem increasingly influenced by the weather over time. However, data quality may be partly responsible for the increasing trend of the hedge profits—the recent weather data are of higher quality, thus allowing a more precise estimation of the OLS regression coefficients, and there are more countries to trade in the more recent periods.[97] Additionally, in unreported tests, we find the hedge strategies (both full-sample and out-of-sample) mostly generate insignificant profits if we use only one weather variable (such as SKC) at a time to predict daily returns, suggesting the importance of including all the observable weather conditions for generating a profitable strategy.[98]

In summary, the strong and significant profits of weather-based hedge strategies suggest systematic, rather than spurious, weather effects on returns. We next discuss possible interpretations of the weather effects.

4.4. Discussion of the Weather Effects

4.4.1. Hypotheses

To provide a general guidance of expectations of the weather effects, we develop two hypotheses about the effects of the weather on stock returns. The first one concerns the sign of the effect of each weather variable on returns for each season and each temperature region; the second hypothesis predicts the strength of the weather effects and the probable times to observe these effects.

4.4.1.1. What should be the sign of each weather effect?

A body of psychology and finance literatures suggests that "comfortable" or "pleasant" weather should promote investor happiness and optimism, and an upbeat mood tends to lead to enhanced "spending" or "buying" tendency. For example, good moods lead to positive assessment

[97] The limited number of tradable countries in the first half of our sample is one reason for the close to zero profits of several strategies for the first two subperiods.

[98] For example, using only SKC to predict daily returns, the full-sample hedge strategy for all regions produces a mean daily return of 0.0165% (t = 1.16), or 0.0297% (t = 1.98) if we exclude Southern Hemisphere countries. The out-of-sample hedge profits based on SKC only are insignificant.

of various outcomes (Wright and Bower (1992)); inducement of positive affect stimulates risk taking (Isen and Patrick (1983)); good weather is found to be related to tipping and giving (Cunningham (1979), Lockard et al. (1976), and Rind (1996)); sunshine positively predicts returns around the world (Hirshleifer and Shumway (2003)); good weather is associated with buying propensities of institutional or retail investors (Goetzmann et al. (2015) and Schmittmann et al. (2015)); stock returns are higher before holidays (Ariel (1990) and Kim and Park (1994)); sports-induced bad moods negatively affect stock returns (Edmans, Garcia, and Norli (2007)); happy investors are more optimistic (Kaplanski et al. (2014)); and experimental research finds a positive association between mood and financial risk taking (Kuhnen and Knutson (2011), Kramer and Weber (2012), and Bassi, Colacito, and Fulghieri (2013)). Therefore, we predict a positive relation between "comfortable" weather conditions and stock returns:

H1 (The "comfortable weather" hypothesis): Comfortable and pleasant weather conditions lead to higher stock returns.

Owing to the contingent nature of the weather effects on mood, it is not possible to pin down effects to each climate and each month. However, for our five weather variables, we can make broad testable predictions as follows. First, sunshine is well-known in the literature to lead to an upbeat mood (e.g., see Hirshleifer and Shumway (2003) and their extensive literature review). We therefore expect a positive sunshine effect on returns. Second, wind and rain are generally disruptive to the outdoor experience, but when the temperature is extremely high, rain and wind may be likable cooling conditions. Third, snow cover on the ground exacerbates the winter toughness and hinders outdoor activity. We therefore expect a negative effect of snow depth on stock returns. Finally, temperature should have a contingent effect on mood. Research suggests that people would prefer higher temperatures in the coldest months and lower temperatures in the hottest months (Rehdanz and Maddison (2005)). We therefore expect low temperature during summer times and high temperature during the winter to promote happiness.[99] There may also be temperature effects during seasonal transitions.[100]

4.4.1.2. When should we observe stronger weather effects?

Because happiness is positively related to leisure time spent outdoors (e.g., MacKerron and Mourato (2014)), and also because the effects of the weather on mood are felt more strongly in the outdoors than indoors (Keller et al., (2005)), we expect that conditions conducive to a pleasant outdoor experience should be especially effective in promoting investors' emotions when investors are likely to spend more time outdoors. Our second hypothesis is thus:

H2 (The "outdoors" hypothesis): The effects of the weather on returns are stronger when individuals expect to spend more time outdoors and when they place a higher value on outdoor time.

The relevance of this hypothesis rests on an estimation of the time people spend outdoors each month. Casual intuition suggests that people would spend more time outdoors during the summer than during the winter. However, to obtain a more precise measure, we adopt the methodology of Graff Zivin and Neidell (2014) who estimate the time spent outdoors based on American Time Use Survey (ATUS) data. The ATUS data contain information about how people living in various regions of the U.S. spent their time during 2003-2006. Graff Zivin and Neidell (2014) apply econometric models to estimate the relationship between daily leisure time spent

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[99] A priori, we do not have a firm definition of "low" and "high" temperature. In untabulated tests, we find that most weather effects disappear when we define our weather variables in terms of deviations from their monthly country mean. It thus appears that individuals respond to current absolute weather conditions.

[100] Research in psychology also finds that individuals suffer from apathy under extreme high temperatures (Wyndham (1969) and that individuals exhibit risk-taking and aggression behaviors during extreme low temperatures (Howarth and Hoffman (1984) and Schneider et al. (1980)). Such effects are not in our basic comfortable weather hypothesis, but may still exert an influence on investor mood.

outdoors and daily maximum temperature.[101] Since the temperatures in the ATUS data cover a wide range, from 25°F to 105°F, we can use the estimated relationship between outdoor time and daily maximum temperature to provide an estimation of the time spent outdoors for all months and for all three temperature regions.

Specifically, the estimation of the outdoor leisure time is a three-step process. First, leisure time spent outdoors as a function of the maximum daily temperature is retrieved from Graff Zivin and Neidell (2014). Second, for each country and each month, we calculate the average maximum daily temperature. For each country and each month, we then estimate the time spent outdoors relative to when the temperature is between 76°F and 80°F. Third, we use the unconditional daily average outdoor leisure time (0.73 hours) estimated in Graff Zivin and Neidell (2014) to convert the relative outdoor leisure time into the total leisure time spent outdoors, in minutes, and we compute the average outdoor leisure time for each month by temperature region. Appendix C2 contains the daily maximum temperature and estimated time spent outdoors for each month for each temperature region.

We observe several patterns of outdoor time, from which we can make more specific predictions of the outdoors hypothesis. First, in all temperature regions, individuals spend the most and least time outdoors in the summer and winter, respectively. Therefore, we should expect most weather effects to be particularly strong around summer time for all temperature regions.

Second, individuals spend very limited time outdoors during the winter in both the cold and mild countries. Consequently, the marginal utility of outdoor time should be especially high in the spring when the transition from winter to increasingly mild weather translates into more

[101] Graff Zivin and Neidell (2014) find that the time spent outdoors increases with daily maximum temperature at low temperatures until 76°F -80°F, remains fairly stable until 100°F, and declines after that.

opportunities for outdoor activity. We thus expect stronger weather effects in the spring than in the autumn in the cold and mild regions.

Third, hot countries have the least variation in outdoor time across the seasons, and individuals spend considerable time outdoors even during the winter. This implies that we could observe some strong weather effects even in the winter in the hot region.

Fourth, the hot region has much higher temperatures and longer outdoor time in all seasons than both the cold and mild regions, but the mild region is closer to the cold than to the hot region in terms of temperature and outdoor time. Therefore, we expect the mild region to have more similar weather effects to the cold region than to the hot region.

4.4.2. Interpretation of the Weather Effects

As mentioned earlier, Tables 4.3 and 4.4 present the OLS and logit regression results, respectively, with the 2.5% filter rule applied to filter out return outliers. To help gain an overall picture of the effects of all five weather variables, we summarize both the OLS and logit test results in Appendix C1 by keeping only results that are significant at the 20% level or higher.[102] Given the large number of regressions, the climate/season contingent nature of the weather effects, and the fact that the "pure" emotional weather effects are inevitably mixed with fundamental economic effects as reflected in index returns, we do not claim we can identify the definitive source of weather's emotional effects. Instead, gaining insights into these effects in the light of our

[102] Although the overall patterns of the weather effects do not rely on the cut-off significance levels, keeping results significant at the 20% level (rather than a higher significance level) is helpful in obtaining a more comprehensive picture of the weather effects, especially when viewed in combination with the neighboring effects either in terms of calendar months or the same-month OLS and logit tests. We judge the patterns based on how consistently the effects vary across regions and seasons. For instance, in the cold region, the SKC (OLS) effects for March and April are both negative and significant at the 20% level, with a *p*-value of 0.161 and 0.136, respectively. Taken together, the probability of these two effects being caused by randomness is only 0.022, assuming independent sunshine effects for different months. These results in combination with the February SKC effect strengthen the case of a significant sunshine effect from February to April in the cold countries.

hypotheses helps support the case that the effects of weather on stock returns are systematic rather than spurious.

We first examine the effect of sunshine. Considering the pooled results (column 13) of Table 4.3 and Table 4.4, we confirm that the magnitude of the daily market index returns is negatively related to the current cloudiness (a negative proxy for sunshine), consistent with the comfortable weather hypothesis. Results are particularly strong for cold countries, both in the OLS (p = 0.0004) and in the logit specification (p = 0.0041). Judging by the OLS results of Table 4.3, there is clear evidence of a sunshine effect in hot countries (p = –0.0014) and weaker evidence in mild countries (p = 0.093), but the logit results of Table 4.4 indicate much weaker significance levels of SKC for hot and mild countries. These results suggest that while sunshine has a global positive effect on returns, this effect is strongest and most consistent in the cold region.

The strength of the sunshine effect in all temperature regions exhibits patterns consistent with the outdoors hypothesis. In the cold countries, the SKC effect concentrates in the summer when individuals spend the longest time outdoors (Table 4.3: p = 0.11 for August; Table 4.4: p = 0.019 and 0.053 for June and July, respectively) and in the spring when the marginal utility of outdoor time is presumably the highest (Table 4.3: p = 0.053, 0.16, and 0.14 for February, March, and April, respectively; Table 4.4: p = 0.083 for February). Likewise, in the mild region, the SKC effect is negative in June (Table 4.3: p =0.098; Table 4.4: p =0.0095) and in March (Table 4.3: p =0.020; Table 4.4: p =0.015). In the hot countries, the sunshine effect is present during the warmer months of May, August and September ($p < 0.20$ in at least one test) when individuals spend the most time outdoors, and during the winter when people still spend considerable time outdoors (Table 4.3: $p < 0.1$ for December, January and February; Table 4.4: p = 0.018 for December).

The insignificance of the sunshine effect in July and August may reflect the fact that sunshine aggravates the oppressive summer heat.

An interesting but surprising regularity in the logit tests of Table 4.4 is that in the mild region, cloudiness is positively associated with the probability of a positive return in February and December, although this pattern is not robust to the OLS tests. We also find a marginally significant positive October SKC effect in the hot region. While these results do not counter the overwhelming positive relation between sunlight and returns globally, it does indicate that even the sunshine effect is not completely uniform across temperature regions and seasons.

Moving to the effects of wind and rain, we find further revealing evidence in support of both the comfortable weather and the outdoors hypotheses for all three temperature regions. Considering the pooled results (Column 13), we find that wind is generally negatively associated with returns in cold (Table 4.3: $p = 0.070$) and mild countries (Table 4.4: $p = 0.064$). This is consistent with the comfortable weather hypothesis in that the wind's cooling and disruptive effects make the weather uncomfortable.

In line with the outdoors hypothesis, we find in the OLS tests of Table 4.3 that there is a significant negative wind (SPD) effect in cold countries in March ($p = 0.0006$) and in the summer months (June through August, $p < 0.10$). It appears that the wind's cooling effect is especially unwelcome in the spring, when the marginal utility of outdoor time is possibly at its highest, and in the few months of warmer weather that cold countries enjoy. In the mild region, there is a similar pattern of negative wind effect in the spring and summer, but there is also a negative wind effect in December ($p < 0.05$ in both the OLS and logit tests), possibly because of the wind-chill effect in the early winter when the mild region still offers outdoor opportunities, albeit limited.

In contrast, and in line with both the comfortable weather and the outdoors hypotheses, the same cooling effect of wind appears to be appreciated in the hot countries' warmer months; we find a positive wind effect in June in hot countries (Table 4.4: $p = 0.019$) and in April (Table 4.3: $p = 0.027$). Wind's disruptive effect appears to dominate in November (Table 4.3: $p = 0.048$) and in later summer (Table 4.3: $p < 0.20$ in both August and September). The later summer negative wind effect may also be related to the tropical storms.[103] The varying effects of wind across the seasons result in an insignificant all-month wind effect in the hot region.

In cold countries, rain is negatively associated with returns in the summer (Table 4.3: $p = 0.017$ in June) and in the spring (Table 4.3: $p = 0.13$ for April; Table 4.4: $p = 0.20$ for May). RAIN has an overall negative effect on returns (Table 4.3, all-month regression: $p = 0.061$). However, in line with our comfortable weather hypothesis, and in sharp contrast with the rain effect observed in the summer of cold countries, in hot countries, and especially when average maximum daily temperatures are higher than 85°F (in June, July and August, Table A2), rain is positively perceived (Table 4.3: $p = 0.003$ and 0.081 for July and August, respectively; Table 4.4: $p = 0.021$ for June). Accordingly, RAIN in the hot region has an overall positive effect on returns as shown in the all-month regression (Table 4.3: $p = 0.016$; Table 4.4: $p = 0.038$).

There is a noteworthy pattern about the sign of the wind and rain effects in the cold region. In the OLS regressions of Table 4.3, the negative signs of both SPD and RAIN concentrate in the warmer portion of the year, in line with the outdoors hypothesis. Out of the eight months from March through October, SPD has a negative sign in all months except April, and RAIN is negative

[103] Tropical hurricanes and tropical storms, both in the Atlantic and Eastern Pacific basins, tend to peak on September 10; see http://www.nhc.noaa.gov/climo/.

in all eight months.[104] This pattern reinforces the idea that in the cold region, the negative effect of wind and rain on mood is stronger when people expect to spend more time outdoors.

The temperature-contingent rain effect also exists in the mild countries. In the logit tests, RAIN has a negative sign in 10 out of the 12 months, which suggests that rain is disliked in the mild region, and in the spring in particular (Table 4.4: $p = 0.058$ and 0.022 for March and April, respectively). On the other hand, rain positively affects returns in the warm month of June (Table 4.3: $p = 0.044$; Table 4.4: $p = 0.12$), possibly because it provides a cooling effect similar to the one observed in the hot countries. The all-month logit regression indicates a strong negative effect of RAIN (Table 4.4: $p < 0.0001$), confirming the overall negative emotion associated with RAIN in the mild countries and lending support to the comfortable weather hypothesis.[105]

Results regarding snow depth are in line with both the comfortable weather and the outdoors hypotheses. In the all-month regression, snow cover on the ground has a negative impact on the probability of getting positive returns in the cold region (Table 4.4: $p = 0.026$). The month-level regressions further reveal that this effect is significant after December ($p = 0.0008$, 0.12, and 0.0079 for January, February and March, respectively). This suggests that snow accumulations may hinder daily activities and make outdoor experiences less pleasant.

The comfortable weather and outdoors hypotheses also explain at least part of our findings regarding the daily average temperature. The hypotheses are consistent with the negative TEMP

[104] If each of the 16 signs was independently binomial ($p = 0.5$) as implied by the null hypothesis of zero weather effects, the probability of observing 15 negative SPD and RAIN coefficients would be 0.00024.

[105] The cooling role of wind should not account for the marginally positive January RAIN effect in the logit test in the cold and hot regions. Rather, this effect is possibly caused by the fact that individuals in dry regions would prefer more precipitation (Rehdanz and Maddison (2005)); January is among the driest months in both regions. This interpretation is confirmed in our unreported tests, where we divide the sample into dry and non-dry countries based on median January precipitation and find the positive January RAIN effect is entirely driven by the dry countries, for which RAIN is significant at the 5% level in the logit test for January for both the cold and hot regions. By contrast, dryness does not appear to be a major factor in January in the mild region as January has average precipitation in that region.

coefficients observed in the summer of cold and mild countries (Table 4.3 and Table 4.4: $p < 0.05$ for at least one test in June), as cooler temperatures are more comfortable in the summer. The hypotheses' predictions are also consistent with the positive TEMP coefficient observed in September in cold countries (Table 4.3: $p = 0.0053$; Table 4.4: $p = 0.0016$). In untabulated tests, we find that the positive TEMP effect is driven by the second half of September, and we also find a weaker, but still significant positive TEMP effect in the second half of May ($p < 0.05$ in both the OLS and logit tests). These findings are particularly noteworthy because May and September stand out as the only two months in the cold countries with a significant positive temperature effect. These positive temperature effects suggest that people in cold countries favor warmer weather that permits enjoyable outdoor activity just before or after the summer period.[106]

In the hot region, returns are negatively affected by average daily temperature in the summer, particularly in June (Table 4.3: $p = 0.11$; Table 4.4: $p = 0.018$) and August (Table 4.3: $p = 0.0045$; Table 4.4: $p = 0.017$). Investors may find the hot countries' maximum temperatures of above 85°F (Appendix C2) too hot for the outdoors to be enjoyable, thus explaining the negative temperature effect on returns.[107] However, returns are positively related to temperature in the winter (Table 4.3: $p = 0.12$ for December; Table 4.4: $p = 0.0002$ and 0.20 for December and January, respectively) and spring ($p < 0.10$ in April and May in at least one test), when investors possibly want warmer temperatures, so that they can enjoy more of the outdoors.

[106] Temperature in the cold region has a negative effect on returns in October ($p < 0.05$ in Tables 4.3 and 4.4). A possible interpretation is that a cool temperature in October is comfortable. Alternatively, a lower temperature in October may alert people of the cold weather to come, leading to increased risk-seeking behaviors. We leave it to future research to distinguish the interpretations.

[107] The insignificant (rather than significantly negative) temperature effect in July for the hot region seems to be consistent with findings in psychology that in extreme hot temperature, individuals experience apathy and inactivity (e.g., Wyndham (1969)). Cao and Wei (2005) cite this literature in explaining the less negative effect of temperature on returns during the summer than during the winter across all countries in their sample, but we find here that the summertime negative effect of temperature on returns is strongest in the hot region, which suggests that the overall summertime negative effect of temperature is more consistent with the comfortableness rather than the apathy interpretation.

However, the highly consistent and negative TEMP coefficients that we observe in both the cold and mild regions between December and February (for the cold region, in Tables 4.3 and 4.4, $p < 0.05$ in five out of the six TEMP coefficients; in the mild region, $p < 0.10$ in five out of the six TEMP coefficients, with three less than 0.01) are not consistent with the basic comfortable weather hypothesis, but rather with the explanation of Cao and Wei (2005), Howarth and Hoffman (1984) and Schneider et al. (1980) that in extremely cold temperatures, individuals exhibit risk-seeking behaviors.[108] The strong association between extremely low temperature and high stock returns is a major departure from the nearly ubiquitous "positive-emotion-promotes-optimism" theme (see footnote 19), and confirms the complex relation between mood and risk assessment (Isen (2000)).[109]

Overall, as seen in the summary of Appendix C1, the signs of the weather effects on returns are broadly consistent with the comfortable weather hypothesis, with the main exception of a strong negative effect of winter-time temperature in the cold and mild regions. Furthermore, Appendix C1 also makes it apparent that in the cold and mild regions, the weather effects show similar patterns and are stronger in the summer and spring when individuals like to spend time outdoors. In the hot region, there are stronger weather effects over the warmer portion of the year, and winter-time sunshine and warmer temperature—conditions conducive to a pleasant outdoor time—also impact stock returns. These patterns are consistent with the outdoors hypothesis.

[108] The link between extreme low temperature and stock returns can potentially be attributed to two reasons: a more optimistic attitude of investors under very low temperatures, and/or a more risk-seeking attitude under such low temperatures. Since the former stretches credulity (e.g., the financial press often blames the cold winter weather for a slow pace in economic activity), the latter seems to be the more logical reason.

[109] Novy-Marx (2014) documents that New York City temperatures are correlated with the monthly returns of a number of asset pricing anomaly strategies. He also notes the apparently contradictory interpretations of the negative temperature effect of Cao and Wei (2005) and the positive sunshine effect of Hirshleifer and Shumway (2003). Our comprehensive approach of studying five weather effects on daily returns makes it clear to what extent the comfortable weather hypothesis holds among all these effects.

4.4.3. Economic Impact

In the OLS tests, we estimate the economic impact of a continuous weather variable (SKC, SPD, SD or TEMP) as the change in stock returns (in terms of annualized returns) that results from a change in that weather variable from the 10th percentile to the 90th percentile, holding all other variables at their sample mean values;[110] similarly, the impact of an indicator variable (RAIN and SIM) is the change in annualized returns caused by a change from 0 to 1 of the indicator variable, keeping all other variables at their sample means.[111] In the logit tests, the economic impact of a weather variable is the change in the dependent variable (the probability of a positive daily return) as a result of a change in that weather variable from the 10th to the 90th percentile (or for RAIN, from 0 to 1), holding all other variables at their sample mean values.

The figures [in brackets] in Tables 4.3 and 4.4 indicate substantial economic impacts of the weather on stock returns. For example, based on the OLS results in Table 4.3, in the cold region, SKC, SPD, RAIN, and TEMP all have a significant impact on stock returns, with an annualized return impact as high as 13.3% for RAIN in July, 13.4% for SKC in February, 13.7% for SPD in March, and 28.7% for TEMP in February. In comparison, SD has a relatively low economic impact, reaching as high as 5.0% in March, although this may partly be due to the smaller number of valid snow depth observations. Similar conclusions hold based on the logit test in Table 4.4. For instance, a decrease in February TEMP from the 90th to the 10th percentile increases the probability of a positive daily return by 6.4%.

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[110] SKC is considered as a continuous variable even though it takes discrete values, because when estimating the economic impact, we use the same calculation method for SKC as for other continuous variables. Furthermore, our operationalized SKC variable is the daily mean cloud cover observed between 6:00 AM and 4:00 PM local time and as such, it can take any value between 0 and 8.

[111] For example, to estimate the impact of SKC in the cold region in January, we follow a 3-step procedure. First, we compute the change in daily return (denoted as d) caused by a change in SKC from the 10th to the 90th percentile (for the cold region in January) and holding other variables at their sample means. Second, we compute the mean daily index return (denoted as r) of all cold countries in January, and estimate the range of the daily return caused by a change in SKC to be $(r - d/2, r + d/2)$. Third, we calculate the economic impact as the corresponding change in annualized returns (using 250 trading days per year) in absolute value.

We rank the economic impacts of the weather variables for each geographical region. In the cold region, the top weather effect based on the OLS results is TEMP in February [-28.7%]; based on the logit regression, the top effect is February TEMP [-6.4%]. In the hot region, the top effect based on the OLS is February SKC [-28.9%]; based on the logit test, the top effect is December TEMP [+5.7%]. Even though sunshine has the most consistent positive effect across regions and months, temperature often exerts the highest economic impact on returns, with rain's effect comparable to sunshine's, on an individual region-month basis. These results also indicate that despite the differences in the way weather affects stock returns across regions and seasons, the economic magnitude of the weather effects on investor behavior is comparable across the geographical regions.

4.4.4. Robustness Tests

Our OLS and logit results are not highly sensitive to the filter rules used to control for the effect of return outliers: our results remain if we use filters ranging from 1.5% to 3%, and some results remain when we do not filter out return outliers. We discuss several further robustness tests below (results are untabulated). Finally, the Internet Appendix contains tables briefly referred to in the paper.

4.4.4.1. Classification of Temperature Regions

The definition of temperature regions reflects a trade-off: Sorting the full sample into more temperature-subsamples makes each region more uniform, but reduces the sample size of each region. In addition to our base-line classification into hot, mild, and cold regions, we also conduct tests using a 2-region or 4-region classification. In both schemes (especially the 2-region one), the results for the cold region are very much in line with our baseline; all results with respect to the five weather variables remain unaffected with only minor changes in magnitude and significance levels. The results for the hot region are also quite similar to our baseline, with the only exception that the temperature effect tends to be noticeably weaker, especially under the 4-region scheme. In addition, we conduct a full-sample, 1-region test of the weather effects. Results suggest a net set of weather effects more resembling the cold region: SKC, SPD, and TEMP are all negatively related to returns in the all-month OLS regression, but the effects of RAIN and SD vanish, confirming the importance of dividing the sample into temperature regions.

4.4.4.2. Northern and Southern Hemispheres

In our baseline specification, we shift the timing of variables of the Southern Hemisphere countries by six months to align the season with the Northern Hemisphere. However, one issue arising from this shift is that the clustering of standard errors by day implies clustering together errors of day *t* (for the Northern Hemisphere countries) and day *t+6 months* (for the Southern Hemisphere countries). To deal with this issue, we repeat our tests using observations of the Northern Hemisphere countries only. Section 4.3 discusses how removing Southern Hemisphere countries affects the profitability of the hedge strategies. OLS and logit results indicate that all the baseline weather effects are preserved for the cold region. The calendar patterns of the weather effects for the mild region is less pronounced regarding SPD and RAIN. For the hot region, which suffers the largest sample size reduction, the effects of SKC and SPD are little affected relative to the baseline results, the effect of RAIN is weakened but remains positive in June and July, but TEMP shows a substantially weakened pattern. Therefore, while including both hemispheres tends to strengthen our OLS and logit test results, the identification of weather effects is unaffected by whether we include the Southern Hemisphere countries in our analysis.

4.4.4.3. Subperiods

To verify whether the weather effects hold in the more recent period, we split our sample into two subperiods, 1973-1992 and 1993-2012. The beginning of the 1990s is a natural cut-off point because Datastream started covering many of our sample countries around that date. Our OLS and logit results hold in both subperiods. Most exceptions to our prior results occur in the earlier period, where data quality is sometimes an issue, especially in hot countries, and where the reduced number of observations makes the regression results particularly sensitive to outliers.

4.5. Conclusion

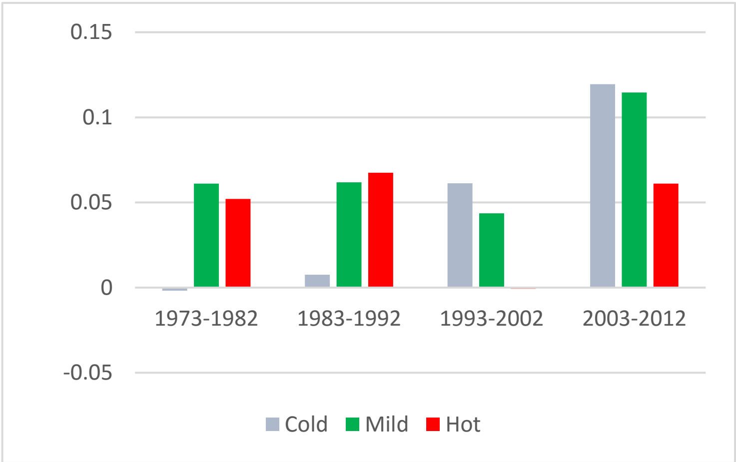
We test the effects of five weather variables (sunshine, wind, rain, snow, and temperature) on stock index returns of 49 countries from 1973 to 2012 by sorting our sample by temperature region and calendar month. We conjecture that the weather effects on mood are contingent on climate and season, and uncover a number of new weather effects on stock returns. Weather appears to exert a substantial real impact on returns judging by the highly profitable weather-based hedge strategies. Furthermore, the weather effects across climates and seasons suggest systematic patterns. These patterns are in turn consistent with two themes. First and primarily, comfortable weather conditions promote optimism and lead to higher returns, especially during seasons when individuals like to spend time outdoors. Secondly, in a cold environment (i.e., winter times in cold or mild regions), low temperature elevates the risk-taking tendency and leads to higher returns.

We recognize that a study of the effects of weather on stock returns has, by construction, limitations. Even though our research design tries to pick up the weather effects (by allowing effects to vary by climate and season, removing return outliers, and using a large sample to neutralize other effects), returns are obviously affected by economic events. Also, given the latitude in interpreting our hypotheses, further independent research is needed to confirm our findings. Nonetheless, our results do indicate that there exist substantial weather effects on stock returns, and the patterns of the effects across climates and seasons suggest systematic effects of the weather on investor psychology. Our evidence that the strength of the weather effects tends to vary with the time spent outdoors sheds light on the mechanism through which weather-induced mood affects asset prices, lending support to the notion that temporary emotional states influence individuals' judgment about long-term prospects (e.g., Shiller (1981), Schwarz and Clore (1983), and Lowenstein et al. (2001)).

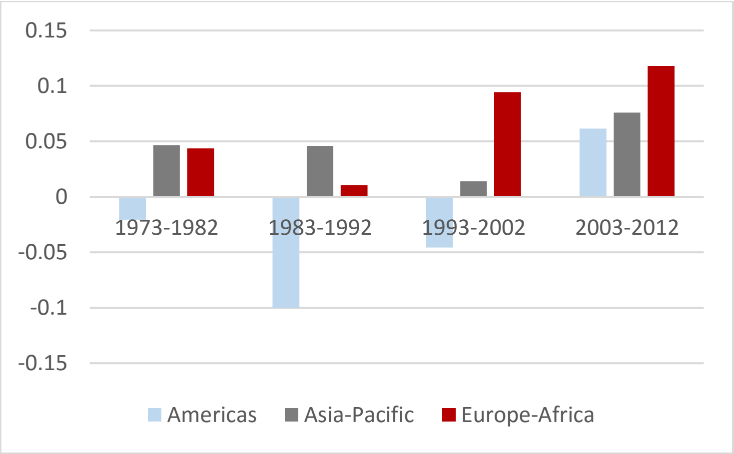
Our results should also be relevant to psychology research. Our evidence suggests that global stock markets are not only a barometer of the world's financial fortunes, but also a convenient platform to study how weather influences human emotions. We have suggested possible interpretations for our salient results, but we believe that many intriguing weather effects on mood need to be further explored.

Figure 4.1. Average Daily Hedge Portfolio Profits by Temperature Region and Time Zone, Using Full-Sample Estimation

Panel A: Temperature Regions



Panel B: Time Zones



This figure presents the per-decade average daily trading profits (in percentage) associated with the hedge portfolios for each temperature region (Panel A) and time zone (Panel B). In the full-sample estimation, we use the full sample (1973-2012) to estimate the coefficients of the following OLS regression for each temperature region and month: $r_{it} = \alpha + \beta_1 SKC_{it} + \beta_2 SPD_{it} + \beta_3 RAIN_{it} + \beta_4 SD_{it} + \beta_5 TEMP_{it} + \varepsilon_{it}$.
Returns are calculated using the Datastream Global Equity country indices. All weather variables are based on the average of hourly readings between 5:00 AM and 9:00 AM local time on the day of the measurement. Absolute returns greater than 2.5% are excluded from the sample for the estimation of the regression coefficients. We then use the estimated coefficients and the weather variables, based on hourly readings between 5:00AM and 9:00AM, to calculate daily predicted returns for each country. For each temperature region (time zone) and each day, we form a hedge portfolio by taking a long position in the country with the highest predicted return and a short position in the country with the lowest predicted return. For each temperature region (time zone), the daily return of the hedge portfolio is the difference between the realized returns of the long and short positions. In Panel A, we form one portfolio for each temperature region (cold, mild and hot countries). In Panel B, we form one portfolio for each time zone (Americas, Asia-Pacific and Europe-Africa).

Table 4.1. Summary Statistics, by Country

This table lists the countries and cities included in our sample. Stock return and standard deviation are in percentage points. For each country, the begin date (column 3) is the first year for which neither the returns nor the weather information is missing. Series for all countries end on December 31, 2012. Columns 4 and 5 list the mean and standard deviations of percentage returns in local currency for each country. Column 6 shows the number of observations with valid daily return and hourly weather data for each country.

Country (1)	City (2)	Begin date (3)	Mean return (4)	Standard deviation, return (5)	N (6)
Argentina	Buenos Aires	1988	0.198	2.898	5993
Australia	Sydney	1973	0.026	1.084	10184
Austria	Vienna	1973	0.021	0.959	10434
Belgium	Brussels	1973	0.027	0.968	10427
Brazil	Sao Paolo	1994	0.059	1.617	4783
Bulgaria	Sofia	2000	0.055	1.883	3186
Canada	Toronto	1982	0.029	0.952	8073
Chile	Santiago	1989	0.061	0.956	6098
China	Shanghai	1991	0.033	1.716	3702
Colombia	Bogotá	1992	0.058	1.023	5260
Denmark	Copenhagen	1973	0.038	1.083	10397
Finland	Helsinki	1988	0.024	1.770	6434
France	Paris	1973	0.029	1.218	10002
Germany	Frankfurt	1973	0.028	0.998	8841
Greece	Athens	1988	0.025	1.763	6500
Hong Kong	Hong Kong	1987	0.014	1.651	4238
Hungary	Budapest	1996	0.036	1.721	3233
India	Mumbai	1990	0.052	1.689	5982
Indonesia	Jakarta	1990	0.028	1.855	4714
Ireland	Dublin	1973	0.024	1.189	10327
Israel	Tel Aviv	1993	0.031	1.273	5182
Italy	Milan	1973	0.026	1.360	10381
Japan	Tokyo	1996	-0.012	1.331	4127
Korea	Seoul	1996	0.043	1.977	3548
Luxemburg	Luxemburg	1992	0.016	1.259	5471
Malaysia	Kuala Lumpur	1986	0.037	1.341	6936
Mexico	Mexico City	1988	0.083	1.514	5410
Netherlands	Amsterdam	1973	0.021	1.102	10419
New Zealand	Wellington	1989	0.015	0.988	5664
Norway	Oslo	1980	0.032	1.468	8505
Pakistan	Karachi	1992	0.020	1.722	5222
Peru	Lima	1994	0.033	1.394	4945
Philippines	Manila	1987	0.046	1.355	6593

Table 4.1 (Continued). Summary Statistics, by Country

Country (1)	City (2)	Begin date (3)	Mean return (4)	Standard deviation, return (5)	N (6)
Poland	Warsaw	1994	0.008	1.746	4912
Portugal	Lisbon	1996	0.007	1.117	3509
Romania	Bucharest	1996	0.061	2.274	4167
Russia	Moscow	1998	0.089	2.822	3863
Singapore	Singapore	1973	0.017	1.328	10138
South Africa	Johannesburg	1973	0.053	1.286	10383
Spain	Madrid	1987	0.023	1.340	6709
Sri Lanka	Colombo	1987	0.055	1.317	6091
Sweden	Stockholm	1982	0.041	1.397	7354
Switzerland	Zurich	1979	0.030	0.949	8833
Taiwan	Taipei	1987	0.024	1.834	6552
Thailand	Bangkok	1987	0.038	1.762	6773
Turkey	Istanbul	1988	0.144	2.540	6504
UK	London	1973	0.029	1.091	10344
USA	New York	1973	0.029	1.092	10428
Venezuela	Caracas	1990	0.126	2.224	4835

Table 4.2. Classification of Countries According to Yearly Average Temperature

This table describes the composition of the temperature regions. Panel A lists the countries included in each region. We define cold, mild, and hot regions based on the 33rd and 67th percentiles of the full sample's distribution of annual temperatures. Panel B shows the mean, median and standard deviation of the annual temperature (in Fahrenheit), by region. N is the number of observations with valid return and weather data for each region.

Panel C reports summary statistics for each of the temperature regions. All weather variables are based on the average of hourly readings between 6:00 AM and 4:00 PM local time on the day of the measurement. SKC is the average sky cover. SPD is the average wind speed (in miles per hour). RAIN is an indicator variable that is equal to 1 if the average of the hourly records of liquid precipitations (in inches) registered in the 6 hours prior to any hourly readings is positive; and zero otherwise. SD is equal to the depth (in inches) of the snow cover on the ground. SD is set to zero in summer months and in hot and mild countries. RET is each country's daily percentage returns of Datastream's Global Equity Index, in local currency.

The last two columns of Panel C show the difference in means of the weather variables and the returns, between cold and hot countries, and between mild and hot countries. ***; **, * indicate that the hypothesis of the equality of means was rejected using a standard t-test at the 1%, 5%, and 10% level, respectively.

Panel A: Countries in Each Temperature Region

Cold	Mild	Hot
Austria	Argentina	Australia
Belgium	Bulgaria	Brazil
Canada	China	Greece
Chile	Colombia	Hong Kong
Denmark	France	India
Finland	Hungary	Indonesia
Germany	Italy	Israel
Ireland	Japan	Malaysia
Luxemburg	Korea	New Zealand
Netherlands	Mexico	Pakistan
Norway	Philippines	Peru
Poland	Portugal	Singapore
Russia	Romania	South Africa
Sweden	Spain	Sri Lanka
Switzerland	Turkey	Taiwan
United States	United Kingdom	Thailand
		Venezuela

Table 4.2 (Continued). Classification of Countries According to Yearly Average Temperature

Panel B: Summary Statistics of Temperature, in Fahrenheit, by Region			
	Cold	**Mild**	**Hot**
Mean	49.930	57.530	76.317
Median	50.545	56.636	80.000
Standard deviation	15.130	13.509	10.885
N	130813	109875	91728

Panel C: Summary Statistics of Weather and Returns in the Cold, Mild, and Hot Regions

Variable		Cold (1)	Mild (2)	Hot (3)	Difference (1 - 3)	Difference (2 - 3)
SKC	Mean	5.032	4.319	4.589	0.4431***	-0.296***
	Median	5.500	4.500	5.125		
	Standard deviation	2.139	2.286	2.235		
SPD	Mean	9.149	7.832	6.982	2.167***	0.851***
	Median	8.316	6.750	6.250		
	Standard deviation	5.352	5.334	4.349		
RAIN	Mean	0.167	0.091	0.111	0.056***	-0.019***
	Median	0.000	0.000	0.000		
	Standard deviation	0.373	0.288	0.314		
SD	Mean	0.546	0.000	0.000	0.546***	N/A
	Median	0.000	0.000	0.000		
	Standard deviation	2.788	0.000	0.000		
RET	Mean	0.045	0.040	0.040	0.005*	0.001
	Median	0.007	0.000	0.000		
	Standard deviation	0.752	0.840	0.824		

Table 4.3. Ordinary Least Square (OLS) Regressions of Daily Return on Weather Variables: 2.5% Absolute Return Filter Applied

This table presents the results of the OLS estimation of the following model: $r_{it} = \alpha + \beta_1 SKC_{it} + \beta_2 SPD_{it} + \beta_3 RAIN_{it} + \beta_4 SD_{it} + \beta_5 TEMP_{it} + \varepsilon_{it}$.
Returns are calculated using the Datastream Global Equity country indices. Returns include dividends. All weather variables are based on the average of hourly readings between 6:00 AM and 4:00 PM local time on the day of the measurement. SKC is the average sky cover. SPD is the average wind speed (in miles per hour). RAIN is an indicator variable that is equal to 1 if the average of the hourly records of liquid precipitations (in inches) registered in the 6 hours prior to any hourly readings is positive; and zero otherwise. SD is equal to the depth (in inches) of the snow cover on the ground. SD is set to zero in summer months and in hot and mild countries. TEMP is the daily average temperature, in Fahrenheit.

Panels A, B, and C present the results for the cold, mild, and hot countries, respectively. We define cold, mild, and hot regions based on the 33^{rd} and 67^{th} percentiles of the full sample's distribution of annual temperatures. Absolute returns greater than 2.5% were deleted from the sample. The number of observations and adjusted R-squared of each regression are also reported. ***P*-values** are presented in parentheses and boldfaced coefficients and associated *p*-values are significant at the 10% level or higher. Figures in brackets indicate the economic significance of the independent variables. The economic impact of a variable is the change in annualized return as a result of a change in that variable from the 10^{th} to the 90^{th} percentile (or for RAIN, from 0 to 1), holding all other variables at their sample mean values. Standard errors are clustered by day and country.

Table 4.3 (Continued). Ordinary Least Square (OLS) Regressions of Daily Return on Weather Variables: 2.5% Absolute Return Filter Applied

Panel A. Cold Countries.

	Jan (1)	**Feb (2)**	**March (3)**	**April (4)**	**May (5)**	**June (6)**
SKC	0.0004	**-0.0069**	-0.0068	-0.0062	-0.0008	-0.0047
	(0.9332)	**(0.0535)**	(0.1611)	(0.1360)	(0.8419)	(0.3483)
	[0.0072]	**[0.1339]**	[0.1197]	[0.1076]	[0.0124]	[0.0691]
SPD	0.0005	-0.0011	**-0.0032**	0.0011	-0.0002	**-0.0039**
	(0.7085)	(0.4984)	**(0.0006)**	(0.4498)	(0.8789)	**(0.0343)**
	[0.0282]	[0.0553]	**[0.1365]**	[0.0413]	[0.0069]	**[0.1203]**
RAIN	0.0024	0.0301	-0.0152	-0.0382	-0.0134	-0.0071
	(0.8935)	(0.3170)	(0.5959)	(0.1346)	(0.5907)	(0.7797)
	[0.0076]	[0.0953]	[0.0437]	[0.1139]	[0.0358]	[0.0194]
SD	-0.0019	-0.0007	-0.0035			
	(0.2430)	(0.6411)	(0.1129)			
	[0.0361]	[0.0147]	[0.0497]			
TEMP	**-0.0027**	**-0.0031**	-0.0019	-0.0007	0.0012	**-0.0029**
	(0.0085)	**(0.0183)**	(0.3015)	(0.6535)	(0.4899)	**(0.0323)**
	[0.2485]	**[0.2869]**	[0.1281]	[0.0441]	[0.0666]	**[0.1545]**
Intercept	**0.1803**	**0.2464**	**0.2131**	0.1306	-0.0319	**0.2803**
	(0.0001)	**(0.0000)**	**(0.0168)**	(0.1462)	(0.7783)	**(0.0037)**
R^2	(0.0009)	(0.0021)	(0.0016)	(0.0006)	(0.0002)	(0.0009)
N	10440	9716	10536	10211	10519	10306

Panel A (Continued). Cold Countries.

	July (7)	**Aug (8)**	**Sept (9)**	**Oct (10)**	**Nov (11)**	**Dec (12)**	**All (13)**
SKC	-0.0063	-0.0063	0.0003	-0.0029	-0.0044	-0.0020	**-0.0052**
	(0.2171)	(0.1088)	(0.9624)	(0.5451)	(0.2899)	(0.6419)	**(0.0004)**
	[0.0938]	[0.0919]	[0.0040]	[0.0423]	[0.0640]	[0.0329]	**[0.0868]**
SPD	**-0.0037**	**-0.0037**	-0.0023	-0.0016	0.0022	0.0003	**-0.0009**
	(0.0966)	**(0.0714)**	(0.3204)	(0.4339)	(0.3127)	(0.8004)	**(0.0703)**
	[0.1104]	**[0.1111]**	[0.0713]	[0.0548]	[0.0841]	[0.0146]	**[0.0328]**
RAIN	**-0.0486**	-0.0314	-0.0116	-0.0197	0.0149	-0.0006	**-0.0115**
	(0.0171)	(0.4399)	(0.6081)	(0.3161)	(0.4797)	(0.9727)	**(0.0606)**
	[0.1328]	[0.0844]	[0.0279]	[0.0494]	[0.0396]	[0.0019]	**[0.0321]**
SD						0.0028	-0.0001
						(0.3331)	(0.9322)
						[0.0248]	[0.0000]
TEMP	-0.0016	-0.0017	**0.0048**	**-0.0049**	-0.0021	-0.0009	**-0.0017**
	(0.2597)	(0.2457)	**(0.0053)**	**(0.0055)**	(0.2304)	(0.3887)	**(0.0000)**
	[0.0791]	[0.0809]	**[0.1979]**	**[0.2406]**	[0.1229]	[0.0690]	**[0.1864]**
Intercept	**0.2112**	**0.2126**	**-0.2768**	**0.2787**	0.1106	**0.1021**	**0.1656**
	(0.0419)	**(0.0310)**	**(0.0158)**	**(0.0022)**	(0.1734)	**(0.0329)**	**(0.0000)**
R^2	(0.0012)	(0.0010)	(0.0016)	(0.0021)	(0.0006)	(0.0004)	(0.0010)
N	10626	10561	10215	10293	10252	10722	124397

Table 4.3 (Continued). Ordinary Least Square (OLS) Regressions of Daily Return on Weather Variables: 2.5% Absolute Return Filter Applied

Panel B. Mild Countries.

	Jan (1)	**Feb (2)**	**March (3)**	**April (4)**	**May (5)**	**June (6)**
SKC	-0.0058	0.0034	**-0.0109**	0.0045	-0.0060	**-0.0095**
	(0.2519)	(0.5534)	**(0.0203)**	(0.4719)	(0.4055)	**(0.0975)**
	[0.1296]	[0.0734]	**[0.2210]**	[0.0837]	[0.0918]	**[0.1488]**
SPD	0.0013	-0.0021	0.0004	**-0.0033**	-0.0008	0.0009
	(0.4233)	(0.2137)	(0.7815)	**(0.0910)**	(0.6952)	(0.6581)
	[0.0541]	[0.0846]	[0.0152]	**[0.1270]**	[0.0231]	[0.0285]
RAIN	-0.0061	0.0055	0.0208	-0.0284	0.0241	**0.0543**
	(0.8850)	(0.9199)	(0.5638)	(0.4275)	(0.4090)	**(0.0436)**
	[0.0187]	[0.0163]	[0.0591]	[0.0846]	[0.0607]	**[0.1450]**
TEMP	**-0.0025**	**-0.0042**	-0.0020	-0.0029	-0.0003	**-0.0023**
	(0.0728)	**(0.0002)**	(0.3411)	(0.2593)	(0.8627)	**(0.0300)**
	[0.1905]	**[0.2940]**	[0.1156]	[0.1561]	[0.0146]	**[0.1423]**
Intercept	**0.2006**	**0.2684**	**0.1992**	0.2521	0.0573	**0.2226**
	(0.0032)	**(0.0000)**	**(0.0766)**	(0.1391)	(0.6468)	**(0.0026)**
R^2	(0.0007)	(0.0017)	(0.0007)	(0.0008)	(0.0002)	(0.0007)
N	6968	6549	7044	6779	6962	6987

Panel B (Continued). Mild Countries.

	July (7)	**Aug (8)**	**Sept (9)**	**Oct (10)**	**Nov (11)**	**Dec (12)**	**All (13)**
SKC	-0.0006	-0.0047	-0.0014	-0.0041	-0.0026	0.0037	**-0.0031**
	(0.9240)	(0.3918)	(0.8373)	(0.5150)	(0.5933)	(0.4642)	**(0.0927)**
	[0.0100]	[0.0736]	[0.0210]	[0.0697]	[0.0496]	[0.0763]	**[0.0575]**
SPD	-0.0027	-0.0027	0.0002	-0.0007	-0.0010	**-0.0040**	-0.0011
	(0.1559)	(0.2403)	(0.9150)	(0.7318)	(0.5659)	**(0.0450)**	(0.1342)
	[0.0881]	[0.0886]	[0.0066]	[0.0225]	[0.0338]	**[0.1521]**	[0.0367]
RAIN	0.0093	0.0197	-0.0419	-0.0361	0.0295	-0.0270	-0.0001
	(0.7001)	(0.7728)	(0.2725)	(0.3150)	(0.5265)	(0.4382)	(0.9883)
	[0.0256]	[0.0526]	[0.1038]	[0.0929]	[0.0771]	[0.0765]	[0.0003]
TEMP	0.0001	-0.0010	0.0002	-0.0027	-0.0005	-0.0017	**-0.0014**
	(0.9250)	(0.5029)	(0.8955)	(0.2197)	(0.7281)	(0.1487)	**(0.0019)**
	[0.0082]	[0.0731]	[0.0089]	[0.1350]	[0.0277]	[0.1184]	**[0.1484]**
Intercept	0.0655	0.1453	-0.0040	0.2011	0.0605	**0.1385**	**0.1455**
	(0.5358)	(0.2174)	(0.9654)	(0.1306)	(0.4063)	**(0.0200)**	**(0.0000)**
R^2	(0.0002)	(0.0004)	(0.0002)	(0.0006)	(0.0001)	(0.0013)	(0.0005)
N	7200	7128	7021	7067	6985	7233	83923

Table 4.3 (Continued). Ordinary Least Square (OLS) Regressions of Daily Return on Weather Variables: 2.5% Absolute Return Filter Applied

Panel C. Hot Countries.						
	Jan	**Feb**	**March**	**April**	**May**	**June**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
SKC	**-0.0067**	**-0.0120**	-0.0006	-0.0041	**-0.0121**	-0.0031
	(0.0818)	**(0.0487)**	(0.9227)	(0.4655)	**(0.0189)**	(0.6904)
	[0.1578]	**[0.2893]**	[0.0123]	[0.0770]	**[0.1826]**	[0.0488]
SPD	0.0015	0.0014	-0.0033	**0.0045**	0.0004	-0.0002
	(0.4640)	(0.5355)	(0.2050)	**(0.0274)**	(0.8814)	(0.9361)
	[0.0447]	[0.0440]	[0.0930]	**[0.1316]**	[0.0119]	[0.0058]
RAIN	0.0083	0.0498	-0.0087	0.0229	0.0283	0.0458
	(0.8394)	(0.2038)	(0.7991)	(0.3655)	(0.3628)	(0.2176)
	[0.0247]	[0.1491]	[0.0236]	[0.0640]	[0.0758]	[0.1308]
TEMP	-0.0002	-0.0002	-0.0008	**0.0016**	-0.0004	-0.0024
	(0.8239)	(0.8583)	(0.5132)	**(0.0654)**	(0.7155)	(0.1116)
	[0.0203]	[0.0168]	[0.0615]	**[0.1267]**	[0.0236]	[0.1428]
Intercept	0.0942	0.1199	0.1202	-0.0994	0.1078	**0.2633**
	(0.1802)	(0.1437)	(0.2600)	(0.2203)	(0.2486)	**(0.0672)**
R^2	(0.0004)	(0.0011)	(0.0003)	(0.0009)	(0.0006)	(0.0007)
N	8482	8038	8533	8344	8417	8493

Panel C (Continued). Hot Countries.							
	July	**Aug**	**Sept**	**Oct**	**Nov**	**Dec**	**All**
	(7)	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**
SKC	-0.0086	-0.0072	-0.0119	0.0050	-0.0020	**-0.0091**	**-0.0059**
	(0.2151)	(0.1992)	(0.1402)	(0.2527)	(0.6406)	**(0.0020)**	**(0.0014)**
	[0.1339]	[0.1081]	[0.1802]	[0.0772]	[0.0356]	**[0.2033]**	**[0.1023]**
SPD	0.0023	-0.0036	-0.0039	0.0009	**-0.0038**	-0.0013	-0.0002
	(0.3097)	(0.1425)	(0.1240)	(0.7578)	**(0.0483)**	(0.5303)	(0.7673)
	[0.0657]	[0.0957]	[0.1006]	[0.0220]	**[0.0936]**	[0.0393]	[0.0066]
RAIN	**0.0713**	**0.0454**	0.0229	-0.0177	0.0059	0.0057	**0.0250**
	(0.0029)	**(0.0814)**	(0.4048)	(0.5578)	(0.8698)	(0.7654)	**(0.0155)**
	[0.2068]	**[0.1240]**	[0.0634]	[0.0464]	[0.0152]	[0.0172]	**[0.0699]**
TEMP	0.0005	**-0.0042**	-0.0018	-0.0005	-0.0003	0.0013	-0.0004
	(0.8055)	**(0.0045)**	(0.2051)	(0.7340)	(0.7969)	(0.1178)	(0.3980)
	[0.0260]	**[0.1918]**	[0.0852]	[0.0314]	[0.0180]	[0.1179]	[0.0275]
Intercept	0.0307	**0.4409**	**0.2632**	0.0327	0.0625	0.0174	**0.0974**
	(0.8625)	**(0.0003)**	**(0.0226)**	(0.8136)	(0.4699)	(0.7654)	**(0.0150)**
R^2	(0.0009)	(0.0014)	(0.0009)	(0.0002)	(0.0003)	(0.0009)	(0.0002)
N	8557	8317	8417	8432	8384	8617	101031

Table 4.4. Logit Regressions of the Probability of A Positive Daily Return on Weather Variables: 2.5% Absolute Return Filter Applied

This table presents the results of the logit estimation of the following model:

$$P(r_{it} > 0) = \frac{1}{1+e^{-(\alpha+\beta_1 SKC_{it}+\beta_2 SPD_{it}+\beta_3 RAIN_{it}+\beta_4 SD_{it}+\beta_5 TEMP_{it})}},$$

where $P(r_{it} > 0)$ is an indicator variable that is equal to 1 if the market return in country *i* on day *t* is positive, and zero otherwise. Returns are calculated using the Datastream Global Equity country indices. Returns include dividends. All weather variables are based on the average of hourly readings between 6:00 AM and 4:00 PM local time on the day of the measurement. SKC is the average sky cover. SPD is the average wind speed (in miles per hour). RAIN is an indicator variable that is equal to 1 if the average of the hourly records of liquid precipitations (in inches) registered in the 6 hours prior to any hourly readings is positive; and zero otherwise. SD is equal to the depth (in inches) of the snow cover on the ground. SD is set to zero in summer months and in hot and mild countries. TEMP is the daily average temperature, in Fahrenheit.

Panels A, B, and C present the results for the cold, mild, and hot countries, respectively. We define cold, mild, and hot regions based on the 33rd and 67th percentiles of the full sample's distribution of annual temperatures. Absolute returns greater than 2.5% were deleted from the sample. The number of observations and pseudo R-squared of each regression are also reported. *P*-values are presented in parentheses and boldfaced coefficients and associated *p*-values are significant at the 10% level or higher. Figures in brackets indicate the economic significance of the independent variables. The economic impact of a variable is the change in the dependent variable (the probability of a positive daily return) as a result of a change in that variable from the 10th to the 90th percentile (or for RAIN, from 0 to 1), holding all other variables at their sample mean values. Standard errors are clustered by day and country.

Table 4.4 (Continued). Logit Regressions of the Probability of A Positive Daily Return on Weather Variables: 2.5% Absolute Return Filter Applied

Panel A. Cold Countries.

	Jan (1)	**Feb (2)**	**March (3)**	**April (4)**	**May (5)**	**June (6)**
SKC	-0.0021	**-0.0148**	-0.0029	-0.0096	0.0087	**-0.0256**
	(0.8832)	**(0.0834)**	(0.8147)	(0.2981)	(0.4789)	**(0.0192)**
	[0.0029]	**[0.0217]**	[0.0044]	[0.0138]	[0.0117]	**[0.0313]**
SPD	0.0062	0.0007	-0.0056	0.0013	0.0028	-0.0018
	(0.2788)	(0.8508)	(0.1161)	(0.7663)	(0.5772)	(0.7464)
	[0.0249]	[0.0024]	[0.0201]	[0.0042]	[0.0080]	[0.0046]
RAIN	0.0634	0.0566	-0.0400	-0.0073	-0.0835	0.0101
	(0.1667)	(0.2765)	(0.5120)	(0.9236)	(0.1964)	(0.8778)
	[0.0158]	[0.0137]	[0.0098]	[0.0018]	[0.0203]	[0.0024]
SD	**-0.0087**	-0.0057	**-0.0106**			
	(0.0008)	(0.1176)	**(0.0079)**			
	[0.0035]	[0.0028]	**[0.0045]**			
TEMP	**-0.0061**	**-0.0095**	-0.0036	-0.0043	0.0055	**-0.0060**
	(0.0457)	**(0.0001)**	(0.3692)	(0.3485)	(0.1277)	**(0.0680)**
	[0.0430]	**[0.0636]**	[0.0208]	[0.0227]	[0.0275]	**[0.0267]**
Intercept	**0.5175**	**0.6653**	**0.4714**	**0.5410**	-0.1461	**0.6961**
	(0.0000)	**(0.0000)**	**(0.0155)**	**(0.0387)**	(0.5178)	**(0.0041)**
R^2	0.0011	0.0025	0.0010	0.0003	0.0006	0.0007
N	10440	9716	10536	10211	10519	10306

Panel A (Continued). Cold Countries.

	July (7)	**Aug (8)**	**Sept (9)**	**Oct (10)**	**Nov (11)**	**Dec (12)**	**All (13)**
SKC	**-0.0188**	-0.0101	-0.0112	-0.0066	-0.0092	0.0127	**-0.0109**
	(0.0533)	(0.3592)	(0.3457)	(0.4978)	(0.3552)	(0.2345)	**(0.0041)**
	[0.0243]	[0.0136]	[0.0154]	[0.0090]	[0.0125]	[0.0174]	**[0.0158]**
SPD	-0.0032	-0.0057	-0.0000	-0.0017	0.0044	0.0054	0.0008
	(0.5733)	(0.2055)	(0.9956)	(0.7359)	(0.4049)	(0.2244)	(0.7031)
	[0.0085]	[0.0157]	[0.0001]	[0.0053]	[0.0161]	[0.0208]	[0.0028]
RAIN	-0.0785	0.0651	-0.0446	-0.0347	0.0346	-0.0404	-0.0132
	(0.1460)	(0.3608)	(0.4392)	(0.3225)	(0.5062)	(0.4101)	(0.4649)
	[0.0188]	[0.0163]	[0.0104]	[0.0081]	[0.0086]	[0.0100]	[0.0032]
SD						-0.0079	**-0.0044**
						(0.4649)	**(0.0257)**
						[0.0017]	**[0.0006]**
TEMP	-0.0040	-0.0001	**0.0106**	**-0.0095**	-0.0048	**-0.0085**	**-0.0048**
	(0.1359)	(0.9653)	**(0.0016)**	**(0.0174)**	(0.2124)	**(0.0127)**	**(0.0000)**
	[0.0173]	[0.0006]	**[0.0412]**	**[0.0427]**	[0.0258]	**[0.0537]**	**[0.0453]**
Intercept	**0.5621**	0.2535	**-0.5170**	**0.6094**	**0.3467**	**0.5459**	**0.5006**
	(0.0039)	(0.2121)	**(0.0178)**	**(0.0010)**	**(0.0515)**	**(0.0001)**	**(0.0000)**
R^2	0.0007	0.0003	0.0017	0.0014	0.0006	0.0015	0.0012
N	10626	10561	10215	10293	10252	10722	124397

Table 4.4 (Continued). Logit Regressions of the Probability of A Positive Daily Return on Weather Variables: 2.5% Absolute Return Filter Applied

Panel B. Mild Countries.

	Jan (1)	**Feb (2)**	**March (3)**	**April (4)**	**May (5)**	**June (6)**
SKC	0.0028	**0.0209**	-0.0145	0.0065	-0.0071	**-0.0392**
	(0.7185)	**(0.0151)**	(0.1691)	(0.5542)	(0.6207)	**(0.0095)**
	[0.0050]	**[0.0378]**	[0.0250]	[0.0096]	[0.0106]	**[0.0478]**
SPD	-0.0024	0.0007	0.0004	-0.0060	-0.0059	0.0082
	(0.6120)	(0.8131)	(0.9192)	(0.1118)	(0.1184)	(0.2120)
	[0.0079]	[0.0025]	[0.0014]	[0.0178]	[0.0175]	[0.0207]
RAIN	-0.1150	-0.0802	**-0.0988**	**-0.1781**	0.0531	0.0805
	(0.1334)	(0.4389)	**(0.0584)**	**(0.0216)**	(0.5214)	(0.1153)
	[0.0279]	[0.0193]	**[0.0238]**	**[0.0407]**	[0.0132]	[0.0178]
TEMP	**-0.0070**	**-0.0103**	-0.0056	**-0.0087**	-0.0021	**-0.0094**
	(0.0645)	**(0.0029)**	(0.2844)	**(0.0936)**	(0.6042)	**(0.0035)**
	[0.0429]	**[0.0571]**	[0.0270]	**[0.0357]**	[0.0102]	**[0.0456]**
Intercept	**0.5845**	**0.5878**	**0.5314**	**0.7347**	0.2823	**0.8906**
	(0.0001)	**(0.0007)**	**(0.0672)**	**(0.0302)**	(0.3442)	**(0.0003)**
R^2	0.0014	0.0028	0.0008	0.0014	0.0003	0.0023
N	6968	6549	7044	6779	6962	6987

Panel B (Continued). Mild Countries.

	July (7)	**Aug (8)**	**Sept (9)**	**Oct (10)**	**Nov (11)**	**Dec (12)**	**All (13)**
SKC	0.0070	-0.0066	0.0044	-0.0146	-0.0050	**0.0256**	-0.0016
	(0.5789)	(0.5390)	(0.7287)	(0.3157)	(0.6348)	**(0.0076)**	(0.7078)
	[0.0106]	[0.0095]	[0.0064]	[0.0235]	[0.0091]	**[0.0445]**	[0.0027]
SPD	**-0.0060**	-0.0050	0.0018	0.0023	-0.0042	**-0.0123**	**-0.0024**
	(0.0364)	(0.2554)	(0.6670)	(0.8129)	(0.2489)	**(0.0215)**	**(0.0643)**
	[0.0179]	[0.0147]	[0.0053]	[0.0067]	[0.0130]	**[0.0373]**	**[0.0075]**
RAIN	-0.0490	-0.0690	-0.1181	-0.0298	-0.0745	**-0.1659**	**-0.0785**
	(0.5296)	(0.4552)	(0.1221)	(0.7264)	(0.4623)	**(0.0534)**	**(0.0000)**
	[0.0122]	[0.0168]	[0.0293]	[0.0072]	[0.0186]	**[0.0379]**	**[0.0191]**
TEMP	-0.0006	-0.0032	0.0031	-0.0049	-0.0004	**-0.0127**	**-0.0048**
	(0.8069)	(0.2776)	(0.1773)	(0.2972)	(0.8803)	**(0.0006)**	**(0.0000)**
	[0.0041]	[0.0205]	[0.0171]	[0.0228]	[0.0023]	**[0.0688]**	**[0.0432]**
Intercept	0.2075	**0.4275**	-0.1831	0.4066	0.1729	**0.8184**	**0.4597**
	(0.3325)	**(0.0668)**	(0.2775)	(0.1637)	(0.2813)	**(0.0000)**	**(0.0000)**
R^2	0.0003	0.0004	0.0005	0.0005	0.0004	0.0066	0.0011
N	7200	7128	7021	7067	6985	7233	83923

Table 4.4 (Continued). Logit Regressions of the Probability of A Positive Daily Return on Weather Variables: 2.5% Absolute Return Filter Applied

Panel C. Hot Countries.

	Jan (1)	Feb (2)	March (3)	April (4)	May (5)	June (6)
SKC	-0.0061	-0.0064	0.0080	-0.0100	-0.0157	0.0023
	(0.6373)	(0.6931)	(0.5950)	(0.5281)	(0.1956)	(0.9200)
	[0.0121]	[0.0128]	[0.0147]	[0.0161]	[0.0215]	[0.0029]
SPD	0.0005	0.0012	-0.0050	0.0046	0.0014	**0.0130**
	(0.9329)	(0.8530)	(0.2554)	(0.4222)	(0.8256)	**(0.0194)**
	[0.0011]	[0.0031]	[0.0129]	[0.0115]	[0.0036]	**[0.0292]**
RAIN	0.1052	0.1113	-0.0118	0.0153	0.0658	**0.1696**
	(0.1437)	(0.2742)	(0.8365)	(0.7533)	(0.4863)	**(0.0205)**
	[0.0258]	[0.0278]	[0.0029]	[0.0037]	[0.0158]	**[0.0382]**
TEMP	0.0036	0.0006	0.0019	**0.0056**	**0.0054**	**-0.0105**
	(0.1972)	(0.7954)	(0.4782)	**(0.0513)**	**(0.0655)**	**(0.0188)**
	[0.0278]	[0.0046]	[0.0138]	**[0.0363]**	**[0.0313]**	**[0.0455]**
Intercept	0.0342	0.2036	0.0271	-0.2504	-0.2313	**0.9185**
	(0.8520)	(0.2388)	(0.9005)	(0.3617)	(0.3930)	**(0.0290)**
R^2	0.0007	0.0002	0.0004	0.0012	0.0010	0.0033
N	8482	8038	8533	8344	8417	8493

Panel C (Continued). Hot Countries.

	July (7)	Aug (8)	Sept (9)	Oct (10)	Nov (11)	Dec (12)	All (13)
SKC	-0.0086	-0.0072	-0.0119	0.0050	-0.0020	**-0.0091**	**-0.0059**
	(0.2151)	(0.1992)	(0.1402)	(0.2527)	(0.6406)	**(0.0020)**	**(0.0014)**
	[0.1339]	[0.1081]	[0.1802]	[0.0772]	[0.0356]	**[0.2033]**	**[0.1023]**
SPD	0.0023	-0.0036	-0.0039	0.0009	**-0.0038**	-0.0013	-0.0002
	(0.3097)	(0.1425)	(0.1240)	(0.7578)	**(0.0483)**	(0.5303)	(0.7673)
	[0.0657]	[0.0957]	[0.1006]	[0.0220]	**[0.0936]**	[0.0393]	[0.0066]
RAIN	**0.0713**	**0.0454**	0.0229	-0.0177	0.0059	0.0057	**0.0250**
	(0.0029)	**(0.0814)**	(0.4048)	(0.5578)	(0.8698)	(0.7654)	**(0.0155)**
	[0.2068]	**[0.1240]**	[0.0634]	[0.0464]	[0.0152]	[0.0172]	**[0.0699]**
TEMP	0.0005	**-0.0042**	-0.0018	-0.0005	-0.0003	0.0013	-0.0004
	(0.8055)	**(0.0045)**	(0.2051)	(0.7340)	(0.7969)	(0.1178)	(0.3980)
	[0.0260]	**[0.1918]**	[0.0852]	[0.0314]	[0.0180]	[0.1179]	[0.0275]
Intercept	0.0307	**0.4409**	**0.2632**	0.0327	0.0625	0.0174	**0.0974**
	(0.8625)	**(0.0003)**	**(0.0226)**	(0.8136)	(0.4699)	(0.7654)	**(0.0150)**
R^2	(0.0009)	(0.0014)	(0.0009)	(0.0002)	(0.0003)	(0.0009)	(0.0002)
N	8557	8317	8417	8432	8384	8617	101031

Table 4.5. Hedge Portfolio Profits during 1993-2012 by Temperature Region, Using Out-of-Sample Estimation

This table reports the daily trading profits (in percentage) associated with the hedge portfolios. We present results for each temperature region (cold, mild and hot countries) as well as for an equal-weighted portfolio of these three regions. In the out-of-sample estimation, we use the first half of the sample (1973-1992) as a start-up period to estimate the following OLS regression for each temperature region and month:

$r_{it} = \alpha + \beta_1 SKC_{it} + \beta_2 SPD_{it} + \beta_3 RAIN_{it} + \beta_4 SD_{it} + \beta_5 TEMP_{it} + \varepsilon_{it.}$

All weather variables are based on the average of hourly readings between 5:00 AM and 9:00 AM local time on the day of the measurement. Absolute returns greater than 2.5% are excluded from the sample for the estimation of the regression coefficients. We use the first set of estimated coefficients to predict the 1993 returns. We then expand the estimation period by one year, so that observations from 1973-1993 are used to predict 1994 returns, and so forth. We use the estimated coefficients and the weather variables to calculate daily predicted returns for each country. For each temperature region and each day, we form a hedge portfolio by taking a long position in the country with the highest predicted return and a short position in the country with the lowest predicted return. The daily return of the hedge portfolio is the difference between the realized returns of the long and short positions. In Panel A, trading is open for countries in both hemispheres. In Panel B, we exclude Southern Hemisphere countries before forming the hedge portfolio. Hedge profits are calculated for the period 1993-2012. Each panel presents the mean daily return from the hedge portfolio with its associated *t*-statistic, as well as the results from the regression of the daily returns of the hedge portfolio on the daily Datastream's world index return. *T*-statistics of the mean hedge returns and *p*-values of the regression coefficients use standard errors robust to heteroskedasticity and autocorrelation up to four lags. The number of observations and adjusted R-squared of each regression are also reported. Boldface indicates statistical significance at the 10% level or higher.

Panel A: Both Hemispheres Tradable					*Panel B: Only Northern Hemisphere Tradable*				
	Cold countries	Mild countries	Hot countries	All regions		Cold countries	Mild countries	Hot countries	All regions
Mean hedge return	**0.0553**	**0.0662**	-0.0013	**0.0392**	Mean hedge return	**0.0552**	**0.0896**	0.0308	**0.0575**
T-statistic	**2.5487**	**2.2641**	-0.0494	**2.6191**	T-statistic	**2.4098**	**2.8047**	1.0626	**3.5243**
Alpha	**0.0553**	**0.0664**	-0.0016	**0.0391**	Alpha	**0.0542**	**0.0891**	0.0300	**0.0567**
(P-value)	**(0.0113)**	**(0.0238)**	(0.9520)	**(0.0093)**	(P-value)	**(0.0188)**	**(0.0055)**	(0.3020)	**(0.0005)**
World return	-0.0196	-0.1129	0.2053	0.0382	World return	0.6962	0.3280	0.6126	0.5301
(P-value)	(0.9692)	(0.8432)	(0.7009)	(0.9063)	(P-value)	(0.2025)	(0.5711)	(0.2636)	(0.1259)
R^2	(0.0000)	(0.0000)	(0.0000)	(0.0000)	R^2	(0.0005)	(0.0001)	(0.0003)	(0.0006)
N	4909	4923	4915	4948	N	4901	4910	4917	4948

Table 4.6. Hedge Portfolio Profits during 1993-2012 by Time Zone, Using Out-of-Sample Estimation

This table reports the daily trading profits (in percentage) associated with the hedge portfolios. We present results for each time zone (Americas, Asia-Pacific, and Europe-Africa) as well as for an equal-weighted portfolio of the three time zones. In the out-of-sample estimation, we use the first half of the sample (1973-1992) as a start-up period to estimate the following OLS regression for each temperature region and month:

$r_{it} = \alpha + \beta_1 SKC_{it} + \beta_2 SPD_{it} + \beta_3 RAIN_{it} + \beta_4 SD_{it} + \beta_5 TEMP_{it} + \varepsilon_{it}$

All weather variables are based on the average of hourly readings between 5:00 AM and 9:00 AM local time on the day of the measurement. Absolute returns greater than 2.5% are excluded from the sample for the estimation of the regression coefficients. We use the first set of estimated coefficients to predict the 1993 returns. We then expand the estimation period by one year, so that observations from 1973-1993 are used to predict 1994 returns, and so forth. We use the estimated coefficients and the weather variables to calculate daily predicted returns for each country. For each time zone and each day, we form a hedge portfolio by taking a long position in the country with the highest predicted return and a short position in the country with the lowest predicted return. The daily returns of the hedge portfolio is the difference between the realized returns of the long and short positions. In Panel A, trading is open for countries in both hemispheres. In Panel B, we exclude Southern Hemisphere countries before forming the hedge portfolio. Hedge profits are calculated for the period 1993-2012. Each panel presents the mean daily return from the hedge portfolio with its associated *t*-statistic, as well as the results from the regression of the daily returns of the hedge portfolio on the daily Datastream's world index return. *T*-statistics of the mean hedge returns and *p*-values of the regression coefficients use standard errors robust to heteroskedasticity and autocorrelation up to four lags. The number of observations and adjusted R-squared of each regression are also reported. Boldface indicates statistical significance at the 10% level or higher.

Panel A: Both Hemispheres Tradable					*Panel B: Only Northern Hemisphere Tradable*				
	Americas	Asia-Pacific	Europe-Africa	All regions		Americas	Asia-Pacific	Europe-Africa	All regions
Mean hedge return	-0.0201	0.0174	**0.0648**	0.0205	Mean hedge return	0.0125	0.0215	**0.0478**	**0.0262**
T-statistic	-0.9374	0.6063	**2.3773**	1.4023	T-statistic	0.5874	0.7204	**1.6978**	**1.7463**
Alpha	-0.0206	0.0177	**0.0669**	0.0211	Alpha	0.0123	0.0213	**0.0491**	**0.0265**
(P-value)	(0.3402)	(0.5394)	**(0.0143)**	(0.1493)	(P-value)	(0.5664)	(0.4776)	**(0.0817)**	**(0.0788)**
World return	0.2925	-0.1869	**-1.3943**	-0.4256	World return	0.1488	0.1584	**-0.8426**	-0.1672
(P-value)	(0.4875)	(0.7564)	**(0.0047)**	(0.1358)	(P-value)	(0.7063)	(0.7889)	**(0.0994)**	(0.5500)
R^2	(0.0001)	(0.0000)	(0.0016)	(0.0005)	R^2	(0.0000)	(0.0000)	(0.0005)	(0.0001)
N	4929	4918	4910	4951	N	4908	4905	4905	4950

CHAPTER 5

CONCLUSION

The three essays of this dissertation consider various ways in which human behavior interacts with corporations and financial markets. Anthropologists define culture as the full range of learned human behavior patterns (Tylor, 1871). The first essay thus examines the impact of differences in learned corporate behavior around mergers. Specifically, it relies on textual analysis of the acquirers' and targets' annual reports to estimate each merging firm's corporate culture, and to then compute a measure of cultural distance between the acquirer and the target. The essay finds that while cultural distance relative to an acquirer reduces the probability that a firm will be selected as a target, upon the announcement, greater cultural distance is associated with positive combined acquirer and target abnormal announcement returns. However, the positive synergistic gains arise only if the acquirer is culturally stronger than its target, which is consistent with the interpretation that the acquirer is in a power position to culturally integrate the target, while benefiting from the diversity associated with the cultural differences. Post-merger improvements in industry-adjusted operating performance confirm the announcement returns results.

The second essay, *Does Stock Misvaluation Drive Merger Waves?*, considers the consequences of large variations in industry-wide valuations ratios on the intensity of industry-specific merger activity. Although the essay does not analyze the behavioral factors leading to such variations, it hypothesizes that if these variations represent deviations from stocks' intrinsic value, then at the industry level, managers respond by using their misvalued stock as means of payment for a relatively-undervalued target. More precisely, the essay contrasts two hypotheses: the Q hypothesis predicts that merger activity is driven by synergy factors at the deal level and economic and regulatory shocks at the aggregate or industry level, whereas the misvaluation hypothesis posits that stock misvaluation affects merger propensity and merger waves are triggered by sharp deviations of stock prices from fundamental values.

To differentiate the two hypotheses, the essay uses two sets of industry-specific merger waves, "*stock*" waves defined on pure stock acquisitions, and "*cash*" waves formed on pure cash offers. The paper exploits the opposite predictions of the hypotheses by contrasting different metrics: valuation levels around *stock* and *cash* merger waves, the relation between industry valuation levels and the likelihood of the occurrence of a wave and the bidders' long-run post-announcement abnormal returns. Using the evidence and the opposite predictions of the *Q* and misvaluation hypotheses regarding the patterns of valuation levels around waves and the post-announcement returns, the paper finds support for the misvaluation hypothesis.

Finally, the third essay, *Does the Weather Influence Global Stock Returns?*, examines a direct link between investors' psychology and financial markets. Indeed, the essay investigates the effects of five weather variables (sunshine, rain, wind, snow and temperature) on daily index returns of 49 countries from 1973 to 2012 and documents pervasive weather effects that are climate and season specific. A hedge strategy that exploits the predictability of daily weather generates significant out-of-sample gross profits. Furthermore, the patterns of weather effects are consistent with the interpretation that "comfortable weather", which is contingent on climate and season, positively affects investors mood and that these effects are particularly strong when people spend more time outdoors. Taken together, these findings show that the weather effects are real rather than spurious, and that investors' collective mood relates to financial markets.

In short, the three essays included in this dissertation find that investors' and workers' psychology, behavior and culture interact significantly with corporate policies and financial markets. These findings raise further research questions. For example, does corporate culture impact other corporate policies and if so, through which mechanisms? Is it possible to design a profitable trading strategy based on corporate culture? How do corporate culture and national culture interact and what are the marginal impact of each? And as a natural extension of this dissertation, how does culture moderate or modify the impact of investors psychology? Taken together, these questions constitute a rich agenda for future research.

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Appendix A1. Parsing the 10-K Forms

The following Appendix provides a detailed description of the parsing method applied to obtain cultural scores from forms 10-K.

I first retrieved annual reports (forms 10-K and 10-K405) from SEC's EDGAR FTP server, in text or HTML format. I used a text parsing software to convert all characters into lowercase characters and contractions into full-form expressions (for example, "don't" into "do not"). I then remove punctuation, html tags and stop words, using Python's Natural Language Tool Kit's list of stop words.[112] I deliberately keep negation words (no, not, neither, none, nobody, nowhere, nor, never) in the text, to make adjustments for semantical negation possible. I remove morphological affixes from words in the remaining text with Python's Natural Language Tool Kit's English Snowball Stemmer algorithm.[113] Finally, tables and appendixes to the financial statements are removed.

I follow Tetlock (2007) and compute the frequency of the lexical fields' words in the parsed 10-Ks, net of the frequency of the negated form of each word (for example: the net frequency for "known" is the total count of the string "known", minus the occurrences of the strings "unknown" and "unbeknownst"). Following Audi, Loughran and MacDonald (2016), I also adjust for common idioms. For instance, the expression "with respect to" does not count toward the frequency count of "respect". Finally, I divide each word's frequency count by the total number of words in the parsed 10-K forms.

[112] The list of stop words that I used contains the following words: through, itself, any, to, our, and, theirs, because, few, some, of, how, have, same, on, above, who, or, were, only, my, more, while, from, such, up, was, whom, having, each, at, me, they, yourself, these, about, again, against, should, down, them, then, myself, those, do, you, out, all, the, does, during, your, an, am, him, into, ourselves, where, own, by, his, as, what, very, we, there, but, their, them, did, doing, can, just, being, its, yours, herself, has, is, than, below, are, if, why, hers, had, in, s, himself, before, this, she, will, been, it, once, I, yourselves, for, a, her, both, further, when, under, now, over, between, with, that, until, after, be, so.

[113] I obtain similar results if I lemmatize (using Python's WordNet's English lemmatizer) the words instead of stemming.

Appendix A2. Lexical Fields

Framework	Dimension	Lexical Field
Cameron et al. (2006), O’Reilly et al (1991)	Collaborate	affiliation, alliance, assist, association, certitude, chief, cohesion, collaboration, collegiality, combination, communion, community, connection, cooperation, coordination, detail, document, efficacy, efficiency, federation, fellowship, help, human, inform, involve, joint, kinship, liaison, logic, method, mutualism, oneness, outcoming, participation, partnership, people, predict, quality, receive, reciprocity, relation, relationship, service, share, solidarity, solution, solve, standard, supportive, symbiosis, synergy, team, teamwork, teamwork, togetherness, train, uniform, unity, work
Cameron et al. (2006)	Compete	accord, achieve, acquire, aggressive, agreement, attack, benefit, budget, challenge, charge, client, compete, customer, deliver, direct, drive, earn, end, excellent, expand, fast, gain, goal, growth, hard, invest, market, move, outsource, performance, position, present, pressure, profit, rapid, reputation, result, return, revenue, satisfaction, scan, success, signal, speed, strong, superior, target, win
	Create	acclimatize, accommodate, adapt, adjust, begin, catalyze, change, commence, convert, create, cultivate, design, develop, discontinue, dream, elaborate, encourage, engender, entrepreneur, envision, establish, experiment, fantasy, father, foster, freedom, future, generate, idea, inaugurate, induce, initiation, innovation, install, institute, intellectual, introduce, invoke, launch, learn, make, new, nourish, nurture, original, pioneer, prediction, produce, promote, prompt, radical, risk, start, style, tailor, thought, trend, unafraid, venture, vision
	Control	accelerate, acceptance, accession, accomplished, accord, accustom, additional, address, adept, adhesion, adroitness, alliance, approval, aptitude, assent, assist, assurance, capable, charge, collective, commanding, commit, competence, concurrence, conditions, conflict, consensus, control, culture, decentralization, deftness, delegation, directing, embrace, employee, empowerment, engage, expectation, expedite, experienced, expert, facilitator, fortify, govern, grant, habituate, hasten, hire, hurry, improve, instinct, instruct, interpersonal, involve, long-term, loyal, manage, mandate, master, mentor, monitor, mutual, norm, operations, ordering, orientate, parent, participation, power, prepared, procedure, process, productive, proficient, qualified, quicken, require, resourcefulness, retain, retention, seasoned, simplify, skill, smooth, social, speed, straighten, streamline, supervise, talent, tension, trained, unanimity, unanimous, unison, usage, usefulness, value

Appendix A2 (Continued). Lexical Fields

Framework	Dimension	Lexical Field
O'Reilly et al (1991)	Adaptability	adapt, chance, experience, fast-moving, flexible, groundbreak, innovate, modern, opportune, quick to take advantage, risk-taking, taking in, try out
	Customer-orientation	client, custom, customer-oriented, customers first, hear, listen, market-driven
	Attention to Detail	analytical, calibre, detail, item, lineament, paying attention to detail, point, precis, quality, select, tone
	Integrity	appropriate, correct, decency, decor, decorum, ethic, etiquette, fair, good, high ethical standard, honest, honor, honour, incorrupt, integral, irreproachable, lesson, moral, nicety, probity, pure, rectitude, reward, right, right-minded, righteous, scrupulous, true, upright, virtuous, whole
	Results-orientation	achievement, achievement-oriented, carrying into act, expect, high expectations for perform, not calm, not easy go, perform, prospect, public present, results-oriented
	Transparency	individual goals are transparent, partake in, share-out, sharing information freely, transparent
Hofstede (1980)	Individualism	case, character, character refer, distinct, distinguishing characteristic, harden, humour, ident, identity, idiosyncrasy, independent, indistinguishable, individual, irritable, lineament, manner, mollify, normal, one, peculiar, person, persona, rarity, self-ident, selfhood, separate, severalty, single, singular, straightforward, surlier, temper, tough, unique, unity
	Masculinity	achievement, assert, bold, boy, boyish, brave, brave out, caveman, courage, entire, gay, hardier, hearty, heroism, hoyden, human, intensity level, isle of man, machismo, macho, male, male person, man, manhood, manly, masculine, military personnel, muscular, posture, self-assert, sheer, stallion, strength, sturdy, succeed, success, tomboy, unfear, valiant, vigor, vim, viril, zip
	Uncertainty avoidance	abid, accord, ambiguous, average, banner, bromide, commonplace, conform, decree, disquietude, dubious, equivocal, exemplar, find, formula, good example, intend, jurisprudence, law, law of nature, legal philosophy, linguistic rule, mean, meanspirit, medial, median, mental reject, monetary standard, natural law, norm, normal, ordinary, par, practice of law, precarious, prescript, prevail, queasy, receive, regulate, rein, rigid, rule, ruler, sceptic, skeptic, stand for, standard, status quo, stiff, stock, strict, suspicious, suspicion, think, tight, touchstone, unbend, uncertainty, uncomfortable, uneasy, unglamourous, unorthodox, well-worn
	Power distance	admit, arrest, assurance, author, bear, center, command, concord, contain, control, custody, dictate, direct, dominate, dominion, eminence, exclusive right, force, give, go for, grasp, grip, hierarchy, higher-rank, hold, hold back, hold up, imperium, index, instruct, keep, manage, master, moderate, note, nurse, obligate, obtain, overtop, pecking order, personnel, power, power distance, predominance, prepotency, prerogative, prevail, program line, ram, reign, require*, rule, sanction, say-so, shake, sovereignty, spellbind, storage area, strength, subordinate, suitcase*, superior, superordinate, superpower, supremacy, sure, sway, take for, take hold, thrust, tilt, tycoon, verify, wait, way

This table presents the non-stemmed lexical fields used to compute corporate culture scores by matching these fields to the lemmatized reviews or 10-K forms. The initial lexical fields come from descriptions included in the listed frameworks and from Fiordelisi and Ricci (2014) for the Cameron et al. (2006) lists. I extend all lexical fields using WordNet's synonyms, with an imposed path similarity threshold of 0.10.

Appendix A3. Cultural Scores for Selected Companies

Panel A. Creativity		*Panel B. Competitiveness*	
1	Bio Rad Laboratories	1	Getty Realty Corp.
2	Texas Instruments	2	Colonial Gas
3	Laboratory Corp. of America Holdings	3	Citizens South Banking
…	…	…	
83713	Midwestone Financial Corp.	83713	Patient Safety Technologies
83714	Rurban Financial Group	83714	Paradigm Holdings
83715	First Place Financial Corp.	83715	First Place Financial Corp.

Panel C. Control		*Panel D. Collaboration*	
1	Laclede Steel Co.	1	FFP Partners LP.
2	V.F. Corp.	2	Radioshack Corp.
3	Convergys Group	3	Lifepoint Hospitals Inc.
…		…	
83713	Orbital Sciences Corp.	83713	Albemarle Corp.
83714	Micros Systems	83714	First Place Financial Corp.
83715	Patient Safety Technologies	83715	Greenman Brothers Inc.

This table gives examples of the highest and lowest ranked firms in terms of each of the four cultural value scores (Cameron et al., 2006). Panel A presents the ranking for creativity, whereas Panels B, C and D do so for Competitiveness, Control and Collaboration, respectively. The ranking was done across the full, pooled sample and in the perspective of mentioning more firms, I deleted the repeat firms within the same category.

Appendix A4. Randomization and Placebo Tests.

As a further robustness test to ensure that cultural scores provide value-relevant information about the firm, I perform placebo tests. More specifically, I randomized the cultural scores assigned to each sample firm by assigning, for each firm-year and each cultural attribute, a cultural score c_i selected randomly from a normal distribution $N(x_i, \theta_i)$, where x_i is the non-randomized sample mean score for cultural attribute *i* and θ_i is the standard deviation of the non-randomized cultural score *i* in the full sample. I randomly assign scores for each of the four cultural attributes (*i* = create, compete, control or collaboration). This process is repeated 1,000 times and for each firm-year and each cultural score, the average value is retained. The (randomized) cultural scores are then treated as described in Section 2.2.

I re-estimate Table 2.2 using the randomized cultural scores; Panel A of Appendix A5 presents the results. Appendix A5, Panel A, shows that none of the randomized cultural scores is significantly related to the observable outputs that in theory relates to the cultural values measured. Therefore, the results of Table 2.2 do not seem to be caused by spurious correlations. In fact, a strong argument against the possibility of spurious correlations is the finding that for each regression, the economic impact[114] is largest precisely for the cultural score that according to Cameron et al.'s (2006) framework, theoretically associates with the observable output in the dependent variable. I thus conclude that in spite of the noise with which they are estimated, the cultural scores constructed by textual analysis provide value-relevant information about firm-specific intangibles.

To further confirm this interpretation, I conduct additional robustness tests to ensure that the cultural variables do not reduce to noisy proxy variables for the observable accounting

[114] The economic impact is calculated as the change in the dependent variable associated with a change from the 10th to the 90th percentile in the distribution of cultural score *i* while maintaining the other variables at their sample mean.

variables they associate with. Namely, I develop a lexical field for R&D (associated with *Create*), employees (*Collaboration*), profitability (*Compete*) and turnover (*Control*). The lexical fields are developed using Merriam-Webster and WordNet thesauruses. Appendix A6 presents the lexical fields; bolded words are words common in both the accounting variable lexical field, and in the lexical field of the cultural value associated with the observable accounting measure. Appendix A6 shows that for any cultural value, no more than three words overlap with the associated accounting measure's lexical fields.

I then re-estimate the raw cultural scores, but using non-overlapping words only. Panel B of Appendix A5 shows that the adjusted cultural scores retain their association with observable accounting variables. More specifically, using the economic significance to gauge the strength of the association of a cultural variable with an observable accounting variable, *Create* remains the variable with the strongest association with R&D expenses and similarly, *Collaboration*, *Compete* and *Control* are the cultural attributes with the highest correlations to, respectively, the number of employees, operating profit margin and total asset turnover. In addition, main results of this paper remain when using the cultural scores adjusted for common words (results untabulated). In short, evidence supports the interpretation that cultural scores provide value-relevant information beyond the informational contents of accounting variables associated with the cultural values.

Appendix A5. Placebo Tests.

Panel A. Randomization of Cultural Variables

	Dependent variable			
	R&D/AT (1)	Employees/AT (2)	EBIT/Sale (3)	Turnover (4)
CREATE	-0.3542	3.8208	1.1911	-1.4156
	(0.2471)	(0.3677)	(0.7316)	(0.6548)
	[-0.0043]	[0.0462]	[0.0144]	[-0.0171]
COLLABORATION	-0.0925	1.7235	2.1481	1.0927
	(0.6435)	(0.5340)	(0.3432)	(0.5973)
	[-0.0017]	[0.0323]	[0.0403]	[0.0205]
COMPETE	-0.1729	2.0699	1.2024	-2.6514
	(0.2676)	(0.3341)	(0.4965)	(0.1005)
	[-0.0042]	[0.0501]	[0.0291]	[-0.0642]
CONTROL	-0.1647	0.5372	-0.2924	0.0181
	(0.2003)	(0.7634)	(0.9840)	(0.9892)
	[-0.0047]	[0.0154]	[-0.0084]	[0.0005]
Intercept	**0.0358**	**0.4527**	-0.0252	**0.0742**
	(<0.001)	**(<0.001)**	(0.8787)	**(<0.001)**
R-Square	0.0002	0.0011	0.0007	0.0014
N	2252	2109	2244	2252

Panel B. Cultural Variables, Adjusted for Common Words

	Dependent variable			
	R&D/AT (1)	Employees/AT (2)	EBIT/Sale (3)	Turnover (4)
CREATE	**0.1642**	**-0.3826**	-1.1482	0.0393
	(0.0097)	**(0.0064)**	(0.3673)	(0.7209)
	[0.0485]	**[-0.1131]**	[-0.3394]	[0.0116]
COLLABORATION	0.0001	**0.3554**	-2.7029	**0.2559**
	(0.9876)	**(0.0098)**	(0.0292)	**(0.0165)**
	[0.0001]	**[0.1469]**	[-1.1175]	**[0.1058]**
COMPETE	**0.0404**	0.1855	2.0485	**0.2942**
	(0.0001)	(0.2133)	(0.1296)	**(0.0112)**
	[0.0151]	[0.0695]	[0.7672]	**[0.1102]**
CONTROL	**0.021**	**0.2503**	0.2104	**0.2599**
	(0.0047)	**(0.0204)**	(0.8298)	**(0.0021)**
	[0.0117]	**[0.1391]**	[0.1169]	**[0.1445]**
Intercept	**0.0326**	**0.3513**	-0.0900	**0.6596**
	(0.0001)	**(0.0001)**	(0.6671)	**(0.0001)**
R-Square	0.1723	0.0228	0.0012	0.0383
N	2115	1996	2109	2115

Appendix A5 (Continued). Placebo Tests.

This Table reports the results of the OLS estimation of the following model:
$Output_{it+1} = \alpha + Create_{it} + Collaboration_{it} + Compete_{it} + Control_{it}$, where cultural scores and outputs are measured for each firm-year *it*.
Each column uses a different output variable. In Column 1, the dependent variable is the R&D expenses scaled by total assets. Column 2's dependent variable is the number of employees (in thousands), scaled by total assets. Column 3 uses the Earnings Before Interest and Taxes (EBIT), scaled by total sales, and in Column 4, the dependent variable is the asset turnover, measured as EBIT scaled by total assets.

Panel A presents the results when the cultural scores are the randomized cultural scores. The Internet Appendix describes how they are generated. Panel B presents the results when the cultural scores are adjusted for the presence of common words between the cultural value lexical field and the lexical field of the accounting variable associated with the cultural value. The Internet Appendix has more details.

The number of observations and R-squared of each regression are also reported. *P*-values (in parentheses) and economic significance [in brackets] are also reported. Economic significance is calculated as the change in the dependent variable that is associated with a change from the 10th to the 90th percentile in the independent variables, while maintaining the other independent variables at their sample mean. Boldfaced coefficients and associated *p*-values indicate statistical significance at the 5% level or higher.

Appendix A6. Overlapping of Lexical Fields

Accounting Measure	Associated Cultural Value	Lexical Field
R&D	Create	addition, advance, audit, augment, better, blossom, challenge, check, checkup, cross-examination, delve, diagnosis, disquisition, **elaborate**, emerge, enhance, evolution, evolve, examen, examine, expansion, explore, feeler, flourish, flower, going-over, grill, **growth**, hear, improve, incubate, inquest, inquiry, inquisitive, inspect, interrogate, investigate, mature, maturity, metamorphosis, perfect, poll, probation, probe, probe, progress, progress, query, quest, question, questionaries, questionnaire, quiz, refine, rehear, reinvestigate, ripen, self-examination, self-explore, self-quest, self-reflect, self-scrutinize, soul-search, study, supplement, survey, trial
Employees	Collaboration	**assist**, **associate**, cog, colleague, coworker, drudge, flunky, gandy dancer, grub, hack, hand, hireling, jobber, jobholder, labor, navvy, nine-to-fiver, retain, subordinate, temp, temporary, toiler, underling, wage earn, wage slave, wageworker, **worker**, workingman, workingwoman, workman, workwoman, yes-man
Profitability	Compete	advantage, bankable, beneficial, economy, fat, favor, **gain**, juicy, lucrative, money-maker, money-spin, pay, remuneration, reward, use, worthwhile
Turnover	Control	abandon, abnegate, cede, **commit**, consign, cough up, deliver, desert, discard, **entrust**, forfeit, forsake, give up, hand over, intrust, lay down, part, release, relinquish, render, renounce, resign, shed, surrender, transfer, turn in, waive, **yield**

This table presents the lexical fields, developed using Merriam-Webster and WordNet thesauruses, that associate with observable accounting measures. The first column lists the accounting measures, while the second column lists the cultural values associated with each observable accounting measure. The last column presents the lexical fields associated with each accounting measure. Words in bold are also part of the lexical field for the associated cultural value.

Appendix A7. Sample Selection

N	Selection Criteria
3,249	Domestic U.S. mergers and acquisitions announced between 1994 and 2014, between public firms, worth at least fifty million dollars and 1% of the acquirer's pre-announcement market capitalization and that resulted in a change in control.
1,817	CD can be estimated: valid 10-K forms (filed before the announcement and parsed 10-K forms have at least 1,000 words) are available for both the acquirer and the target.
1,427	Accounting information is available for both the acquirer and the target.
1,133	Market-to-Book ratios of equity are available and positive for both the acquirer and the target; Cumulative announcement returns can be estimated for both the acquirer and the target.
1,133	Final sample for which the full model can be estimate

This table describes the sample construction and the various filters imposed successively in order to construct the final sample.

Appendix A8. Variable Definitions

Variable	Definition
CAR(-t, t)	Acquirer's cumulative three- or seven-day abnormal returns, centered around the announcement date.
COMBINED(-t, t)	Combined acquirer's and target's cumulative three- or seven-day abnormal returns centered around the announcement date. The combined returns are an average of the acquirer's and target's abnormal returns, weighted by their respective market capitalization, four weeks before the announcement.
CD	Cultural distance between the acquirer and the target, calculated as the Euclidean distance along Cameron et al's (2006) four dimensions of corporate culture (create, compete, control, collaborate).
COMPLETED	Indicator variable that equals one if the acquisition is completed, and zero otherwise.
DAYS_TO_COMPLETE	Number of days between the announcement date and the date the acquisition deal becomes effective.
DEAL_VALUE	Acquisition value, in million dollars.
DIFF_TAX	Natural logarithm of 1 plus the absolute difference in corporate tax rates between the acquirer's and the target's states.
DIFF_WB	Natural logarithm of the difference between Gallup-Healthways well-being scores. Yearly, Gallup-Healthways ranks states according to a well-being index; the difference is the difference in ranks between the states where the acquirer and the targets are headquartered.
DIV_DEAL	Indicator variable that equals 1 if the acquirer and the target do not share the same 2-digit SIC code.
FRIENDLY	Indicator variable that equals 1 if the deal's attitude is friendly, and zero otherwise. Deal attitude comes from Thompson SDC Platinum.
GEO_DISTANCE	Natural logarithm of the straight line distance, in miles, between the capital cities of the states where the acquirer and the target are headquartered.
MB	Acquirer's Market-to-Book equity ratio, calculated using month-end values of the month immediately prior to the acquisition announcement.
MB_TARGET	Target's Market-to-Book equity ratio, calculated using month-end values of the month immediately prior to the acquisition announcement.
MERGER_FORM	Indicator variable that equals 1 if the transaction form is a merger, and zero otherwise.
MERGER OF EQUALS	Indicator variable that equals 1 if the transaction is classified as a merger of equals in SDC database, and zero otherwise.
MKT_CAP	Natural logarithm of the acquirer's market capitalization, measured at the end of the month preceding the acquisition announcement.
MKTCAP	Acquirer's market capitalization, in billions, measured at the end of the month preceding the acquisition announcement.
PURE_CASH	Indicator variable that equals 1 the acquisition was entirely paid in cash.
PURE_STOCK	Indicator variable that equals 1 the acquisition was entirely paid in stock.
REL_SIZE	Deal value, relative to the acquirer's pre-announcement market capitalization.
SAME_POLITIC	Indicator variable that equals 1 if the acquirer's state governor is of the same political allegiance as the state governor of the target's state.
SAME_REL	Indicator variable that equals 1 if the proportion of Catholics is above the national median in both the acquirer's and the target's states.
SCORE_HP	Hoberg and Phillips's (2010 and 2015) measure of product similarity.
TOEHOLD	Percentage of target shares held by the acquirer at the announcement.
UNSOLICITED	Indicator variable that equals 1 if the transaction is described as a merger of equals in SDC, and zero otherwise.

Appendix A9. Buy-and-Hold Abnormal Returns

	24-month BHAR			36-month BHAR		
	All	Strong Acq.	Weak Acq.	All	Strong Acq.	Weak Acq.
	(1)	(2)	(3)	(4)	(5)	(6)
CD	**1.6643**	**2.0758**	1.7091	**3.5563**	**4.0754**	**3.2654**
	(0.0036)	**(0.0000)**	(0.0548)	**(0.0010)**	**(0.0005)**	**(0.0196)**
	[0.1795]	**[0.2135]**	[0.1914]	**[0.2215]**	**[0.2347]**	**[0.2175]**
REL_SIZE	0.0548	0.0153	0.1053	0.1427	0.1697	0.1313
	(0.1878)	(0.6376)	(0.2234)	(0.1277)	(0.0743)	(0.3753)
MKT_CAP	-0.0296	-0.0404	-0.0133	-0.0573	-0.0783	-0.0364
	(0.1536)	(0.1858)	(0.7412)	(0.0532)	(0.1763)	(0.3950)
PURE_STOCK	**-0.1398**	**-0.2662**	0.0155	**-0.2792**	**-0.481**	-0.0323
	(0.0000)	**(0.0000)**	(0.8246)	**(0.0000)**	**(0.0064)**	(0.8148)
PURE_CASH	**0.1827**	**0.2103**	0.0802	**0.1941**	0.1834	0.0575
	(0.0071)	**(0.0469)**	(0.2917)	**(0.0110)**	(0.2857)	(0.7351)
DIV_DEAL	-0.0477	-0.014	-0.1265	0.0247	0.0703	-0.062
	(0.5937)	(0.9001)	(0.2638)	(0.6244)	(0.1365)	(0.6842)
FRIENDLY	-0.0529	-0.0547	-0.0635	-0.0301	-0.1507	-0.0115
	(0.5854)	(0.5526)	(0.5749)	(0.8631)	(0.5854)	(0.9508)
MB	**-0.0362**	**-0.0396**	**-0.0305**	**-0.0554**	**-0.0507**	**-0.0569**
	(0.0000)	**(0.0013)**	**(0.0070)**	**(0.0000)**	**(0.0008)**	**(0.0001)**
MB_TARGET	-0.0072	-0.0002	**-0.0191**	-0.0165	-0.0107	-0.0255
	(0.2004)	(0.9784)	**(0.0015)**	(0.1327)	(0.2641)	(0.0939)
DIFF_WB	-0.0078	-0.0683	0.0329	0.0313	0.0345	0.0481
	(0.8366)	(0.4747)	(0.6016)	(0.5361)	(0.7419)	(0.5682)
DIFF_TAX	-0.1182	-0.1083	-0.1697	-0.0434	-0.0025	-0.121
	(0.1836)	(0.2954)	(0.1504)	(0.7517)	(0.9894)	(0.4827)
SAME_POLITIC	**0.0598**	0.0359	**0.1186**	0.0588	0.1163	0.0374
	(0.0168)	(0.5454)	**(0.0003)**	(0.4075)	(0.3129)	(0.6074)
SAME_REL	-0.048	-0.0388	-0.1377	-0.1235	-0.3235	-0.0233
	(0.2694)	(0.5531)	(0.2579)	(0.1403)	(0.0663)	(0.8510)
GEO_DISTANCE	0.0192	0.0356	0.0099	0.0106	-0.0148	0.0311
	(0.1609)	(0.1858)	(0.6374)	(0.7227)	(0.7126)	(0.5199)
INTERCEPT	-0.1194	0.319	**-0.5616**	-0.2518	0.4292	**-0.7236**
	(0.4108)	(0.4279)	**(0.0111)**	(0.3764)	(0.5852)	**(0.0138)**
R-square	(0.1994)	(0.2673)	(0.2106)	(0.2231)	(0.2602)	(0.2667)
N	903	477	421	903	477	421
State Dummies	Y	Y	Y	Y	Y	Y
Industry Dummies	Y	Y	Y	Y	Y	Y

Appendix A9. Buy-and-Hold Abnormal Returns

This table presents the OLS estimation of the model: $Post\text{-}Bid\ Abnormal\ Return_{it} = \beta_1(CD_{ijt}) + \beta_2(X_{ijt}) + \varepsilon_{it}$
The dependent variable is the acquirer's 24-month or 36-month buy-and-hold abnormal returns, calculate as the difference between acquirers' buy-and-hold (24) 36-month returns and the compound return of an equally weighted portfolio matched on size and book-to-market. X_{ijt} is a vector of acquirer and target deal control variables and includes: relative size of the acquisition, acquirers' pre-announcement market capitalization, pure stock offer indicator, pure cash offer indicator, diversifying deal indicator, friendly deal indicator, acquirer's pre-announcement market-to-book ratio of equity, target's pre-announcement equity market-to-book ratio, state-level differences in well-being index, state-level differences in marginal corporate tax rate, state-level difference in state GDP, differences in the state-wide proportion of Catholics, state-level , indicator variable, logarithm of the distance in miles between state capitals. All variables are defined in Appendix A8.

Columns 1 to 3 present the results where the dependent variable are the 24-month BHAR, whereas Columns 4 to 6 repeat the analysis using the 36-month BHAR as the dependent variable. Results are shown for the full sample (Columns 1 and 4), as well as for the strong-culture subsample (Columns 2 and 5) and weak-culture subsample (Columns 3 and 6).. Strong (weak) culture acquirers are those where the highest of their four, normalized, cultural values is larger (smaller) than the highest of their target's four, normalized, cultural values.

The number of observations and R-squared of each regression are also reported. *P*-values (in parentheses) are calculated using standard errors clustered by both industry and year. Economic significance [in brackets] is calculated as the change in the dependent variable that results from a one-standard deviation change in cultural distance, while maintaining the other independent variables at their sample means. Acquirers' industry and state indicator variables are included in the regressions. Boldfaced coefficients and associated *p*-values indicate statistical significance at the 10% level or higher.

Appendix B1. Estimation Procedure of VP

We follow Dong, Hirshleifer, and Teoh (2012) and estimate VP ratios, for each month *t*, as the ratio of *V(t)* and *P(t)*, where *V(t)* is the residual income model price in month *t* and *P(t)* is the market price at the end of month *t*. We use a three-period forecast horizon to estimate *V(t)*:

$$V(t) = B(t) + \frac{[f^{ROE}(t+1) - r_e(t)]B(t)}{1+r_e(t)} + \frac{[f^{ROE}(t+2) - r_e(t)]B(t+1)}{[1+r_e(t)]^2} + \frac{[f^{ROE}(t+3) - r_e(t)]B(t+2)}{[1+r_e(t)]^2 r_e(t)},$$

where $f^{ROE}(t+1)$ is the forecasted return on equity for period *t+1* and the length of a period is 1 year. The last term discounts the residual income of period *t+3* as a perpetuity.

We compute forecasted returns on equity as

$$f^{ROE}(t+i) = \frac{f^{EPS}(t+i)}{[B(t+i-1)+B(t+1-2)]/2},$$

where $f^{EPS}(t+i)$ is the forecasted earnings per share (EPS) for period *t+i*. We delete observations where the forecasted ROE is greater than one. We compute future book values of equity as

$$B(t+i) = B(t+i-1) + \left(1 - \frac{D(t)}{EPS(t)}\right) f^{EPS}(t+i),$$

where *D(t)* is the dividend for period t and *EPS(t)* is the earnings per share for the same period. When *EPS(t)* is negative, we follow Lee et al. (1999) and divide dividends by (0.06 × total assets), which implicitly assumes that earnings are on average 6% of total assets. We delete observations with a dividend payout ratio *D(t)/EPS(t)* greater than one.

We estimate the annualized cost of equity, $r_e(t)$, using the Capital Asset Pricing Model (CAPM), where the time-*t* beta is estimated using the trailing five years (or, if there is not enough data, at least two years) of monthly return data. The market risk premium is the average annual premium over the risk-free rate of the CRSP value-weighted index and is estimated using the preceding 30 years. We winsorize CAPM costs of capital that lie outside the range of 5-20%, so that outliers take the border values of that range.

Appendix B2. Book-Price Ratio of Acquirers across Different Phases of Merger Waves, by Type of Acquisition

Panel A: 1981-2010, stock waves

	All acquisitions (1)		Public targets, all acquirers (2)		Private targets, all acquirers (3)		Stock acquisitions (4)		Cash acquisitions (5)		"Pubpubstock" deals (6)	
	N	BP	N	BP	N	BP	N	BP	N	BP	N	BP
1) All (including non-wave industries)	6770	0.4302	2612	0.4373	2156	0.3769	1741	0.3319	1806	0.4384	1135	0.3720
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
2) Pre-wave	704	0.4178	307	0.4579	220	0.3164	254	0.3639	135	0.3979	163	0.4270
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
3) In-wave	1165	0.3298	542	0.3496	366	0.2658	596	0.2576	161	0.3815	378	0.2852
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
4) Post-wave	689	0.3995	317	0.4303	208	0.3028	248	0.2892	126	0.5086	156	0.3403
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
5) Non-wave (only wave industries)	786	0.4595	324	0.4823	209	0.4204	297	0.3798	165	0.4666	188	0.4258
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
6) Non-wave (all industries)	4323	0.4643	1481	0.4682	1391	0.4272	676	0.4044	1403	0.4411	458	0.4402
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
Non-wave – In-wave (5 - 3)		**0.1297**		**0.1328**		**0.1546**		**0.1222**		**0.0851**		**0.1406**
p-value of difference		**(0.0000)**		**(0.0000)**		**(0.0000)**		**(0.0000)**		**(0.0046)**		**(0.0000)**

Appendix B2 (Continued). Book-Price Ratio of Acquirers across Different Phases of Merger Waves, by Type of Acquisition

Panel B: 1981-2010, cash waves

	All acquisitions (1)		Public targets, all acquirers (2)		Private targets, all acquirers (3)		Stock acquisitions (4)		Cash acquisitions (5)		"Pubpubstock" deals (6)	
	N	BP	N	BP	N	BP	N	BP	N	BP	N	BP
1) All (including non-wave industries)	6770	0.4302	2612	0.4373	2156	0.3769	1741	0.3319	1806	0.4384	1135	0.3720
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
2) Pre-wave	1268	0.4278	501	0.4507	434	0.3885	339	0.3556	277	0.4337	222	0.3977
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
3) In-wave	1750	0.3945	688	0.4239	547	0.3376	412	0.3306	596	0.4181	264	0.3853
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
4) Post-wave	1151	0.4387	472	0.4658	389	0.3507	345	0.3626	292	0.4640	233	0.4235
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
5) Non-wave (only wave industries)	1494	0.4567	614	0.4310	420	0.4468	469	0.3258	309	0.4220	317	0.3433
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
6) Non-wave (all industries)	2712	0.4509	986	0.4286	815	0.4104	678	0.3086	660	0.4444	436	0.3290
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
Non-wave – In-wave (5 - 3)		**0.0622**		0.0071		**0.1091**		-0.0048		0.0040		**-0.0420**
p-value of difference		**(0.0000)**		(0.6783)		**(0.0000)**		(0.8038)		(0.8421)		**(0.0995)**

This table reports the acquirers' mean Book-Price (BP) ratios. We sort the bidders according to the timing of the announcement date relative to the industry-specific waves. The pre-wave period includes the year before the beginning of the wave, the in-wave period includes the two years of the wave, and the post-wave period spans the three years following the end of the wave. Non-wave periods cover the rest of the time period. Panel A reports the results using the *stock* waves (industry merger waves defined using only pure stock offers), whereas Panel B reports the results using the *cash* waves (industry merger waves defined using only pure cash offers). We report the mean market-book ratios for each category, along with the number of observations and p-value associated with the t-test for statistical difference from zero. We winsorize the BP at the 95th level. The bottom part of each panel shows the results of a t-test for difference in mean BP ratios of non-wave acquirers (line 5) and in-wave acquirers (line 3).

We report the mean acquirer BP ratios for: all types of acquisitions (column 1), the acquisitions of public and private targets (columns 2 and 3), the pure stock and pure cash acquisitions (columns 4 and 5), and public acquirers that made a pure stock offer for a public target (column 6). P-values measuring whether mean values are significantly different from zero are presented in parentheses. Boldfaced differences in mean BP ratios are significant at the 5% level or higher. For ease of reading, we do not use boldface for simple mean BP ratios, even when they are significantly different from zero.

Appendix B3. RKRV Valuation Measure of Acquirers, by Merger Wave Phase and Type of Acquisition

Panel A: 1981-2010, stock waves

	All acquisitions (1)		Public targets, all acquirers (2)		Private targets, all acquirers (3)		Stock acquisitions (4)		Cash acquisitions (5)		"Pubpubstock" deals (6)	
	N	RKRV	N	RKRV	N	RKRV	N	RKRV	N	RKRV	N	RKRV
1) All (including non-wave industries)	8920	2.9074	3132	2.9866	2822	3.0326	1915	3.4050	2321	2.9086	1235	3.3460
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
2) Pre-wave	2355	2.9598	949	3.1737	773	2.9699	879	3.5381	375	2.7570	551	3.4981
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
3) In-wave	2355	2.9598	949	3.1737	773	2.9699	879	3.5381	375	2.7570	551	3.4981
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
4) Post-wave	798	3.2938	344	3.4025	235	3.3518	271	3.7744	146	3.0324	167	3.6105
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
5) Non-wave (only wave industries)	904	2.2937	370	1.8013	247	2.6349	318	3.1141	169	3.0631	207	3.0649
		(0.0000)		(0.0781)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
6) Non-wave (all industries)	5767	2.8325	1839	2.8122	1814	3.0179	765	3.1212	1800	2.9301	517	3.0984
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
Non-wave – In-wave (5 - 3)		**-0.8837**		-1.5224		**-0.5549**		**-0.5702**		0.1188		**-0.5762**
p-value of difference		**(0.0409)**		(0.1384)		**(0.0084)**		**(0.0000)**		(0.6365)		**(0.0000)**

Appendix B3 (Continued). RKRV Valuation Measure of Acquirers, by Merger Wave Phase and Type of Acquisition

Panel B: 1981-2010, cash waves

	All acquisitions (1)		Public targets, all acquirers (2)		Private targets, all acquirers (3)		Stock acquisitions (4)		Cash acquisitions (5)		"Pubpubstock" deals (6)	
	N	RKRV	N	RKRV	N	RKRV	N	RKRV	N	RKRV	N	RKRV
1) All (including non-wave industries)	8920	2.9074	3132	2.9866	2822	3.0326	1915	3.4050	2321	2.9086	1235	3.3460
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
2) Pre-wave	3927	3.0675	1402	2.9673	1281	3.1496	805	3.3617	1063	3.2946	510	3.2599
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
3) In-wave	3927	3.0675	1402	2.9673	1281	3.1496	805	3.3617	1063	3.2946	510	3.2599
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
4) Post-wave	1481	2.9082	548	2.6637	492	3.2098	372	3.4584	397	2.4679	245	3.2232
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
5) Non-wave (only wave industries)	1898	2.7287	708	3.0695	557	2.6879	491	3.3337	407	3.2565	335	3.3628
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
6) Non-wave (all industries)	3512	2.7281	1182	3.1592	1049	2.8065	738	3.4253	861	2.6352	480	3.5001
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
Non-wave – In-wave (5 - 3)		-0.3107		0.2107		**-0.5491**		-0.1606		0.0116		-0.0492
p-value of difference		(0.1340)		(0.6639)		**(0.0062)**		(0.1299)		(0.9499)		(0.7383)

This table reports the acquirers' mean Rhodes-Kropf, Robinson and Viswanathan (RKRV) (2005) misvaluation measures. We sort the bidders according to the timing of the announcement date relative to the industry-specific waves. The pre-wave period includes the year before the beginning of the wave, the in-wave period includes the two years of the wave, and the post-wave period spans the three years following the end of the wave. Non-wave periods cover the rest of the time period. Panel A reports the results using the *stock* waves (industry merger waves defined using only pure stock offers), whereas Panel B reports the results using the *cash* waves (industry merger waves defined using only pure cash offers). We report the mean market-book ratios for each category, along with the p-value associated with the t-test for statistical difference from zero. The bottom part of each panel shows the results of a t-test for difference in mean RKRV measures of non-wave acquirers (line 5) and in-wave acquirers (line 3).
We report the acquirer's mean RKRV measures for: all types of acquisitions (column 1), the acquisitions of public and private targets (columns 2 and 3), the pure stock and pure cash acquisitions (columns 4 and 5), and public acquirers that made a pure stock offer for a public target (column 6). P-values are presented in parentheses. Boldfaced differences in mean market-book ratios and associated p-values are significant at the 5% level or higher. For ease of reading, we do not use boldface characters for simple mean RKRV measures, even when they are significantly different from zero.

Appendix B4. Post-Announcement Acquirer 5-Year Raw Return, by Merger Wave Phase and Type of Acquisition

Panel A: 1981-2009, stock waves

	All acquisitions (1)		Public targets, all acquirers (2)		Private targets, all acquirers (3)		Stock acquisitions (4)		Cash acquisitions (5)		"Pubpubstock" deals (6)	
	N	Return	N	Return	N	Return	N	Return	N	Return	N	Return
1) All (including non-wave industries)	3182	0.3011	1368	0.2686	915	0.2505	774	0.1467	828	0.3180	544	0.2051
		(0.0000)		(0.0000)		(0.0000)		(0.0001)		(0.0000)		(0.0000)
2) Pre-wave	276	0.3162	134	0.3474	80	0.1502	105	0.3410	42	0.4976	67	0.3265
		(0.0000)		(0.0004)		(0.2377)		(0.0117)		(0.0048)		(0.0475)
3) In-wave	522	0.0389	268	0.0316	125	-0.0724	252	-0.0762	66	0.1339	177	0.0261
		(0.3184)		(0.4977)		(0.3379)		(0.1085)		(0.3870)		(0.6482)
4) Post-wave	306	0.3716	155	0.3800	92	0.0308	105	0.0383	50	0.9411	70	0.2158
		(0.0000)		(0.0000)		(0.7953)		(0.6481)		(0.0002)		(0.0452)
5) Non-wave (only wave industries)	283	0.8039	129	0.7922	69	0.8013	101	0.7964	59	0.7590	68	0.8084
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
6) Non-wave (all industries)	2079	0.3546	811	0.3126	618	0.3615	312	0.2978	670	0.2784	230	0.3042
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
Non-wave – In-wave (5 - 3)		**0.7650**		**0.7606**		**0.8737**		**0.8726**		**0.6251**		**0.7824**
p-value of difference		**(0.0000)**		**(0.0000)**		**(0.0000)**		**(0.0000)**		**(0.0048)**		**(0.0000)**

Appendix B4 (Continued). Post-Announcement Acquirer 5-Year Raw Return, by Merger Wave Phase and Type of Acquisition

Panel B: 1981-2009, cash waves

	All acquisitions (1)		Public targets, all acquirers (2)		Private targets, all acquirers (3)		Stock acquisitions (4)		Cash acquisitions (5)		"Pubpubstock" deals (6)	
	N	Returns	N	Returns	N	Returns	N	Returns	N	Returns	N	Returns
1) All (including non-wave industries)	3182	0.3011	1368	0.2686	915	0.2505	774	0.1467	828	0.3180	544	0.2051
		(0.0000)		(0.0000)		(0.0000)		(0.0001)		(0.0000)		(0.0000)
2) Pre-wave	554	0.2639	245	0.2198	175	0.3193	150	0.2072	97	0.2747	100	0.1894
		(0.0000)		(0.0006)		(0.0000)		(0.0344)		(0.0027)		(0.0961)
3) In-wave	797	0.1583	368	0.1003	206	0.0802	175	-0.0303	262	0.2789	132	0.0595
		(0.0000)		(0.0350)		(0.2473)		(0.6417)		(0.0001)		(0.4433)
4) Post-wave	578	0.3311	251	0.3546	185	0.1894	144	0.1517	154	0.3857	98	0.2913
		(0.0000)		(0.0000)		(0.0089)		(0.0308)		(0.0000)		(0.0005)
5) Non-wave (only wave industries)	628	0.6049	284	0.4994	161	0.6365	194	0.3904	137	0.5736	148	0.3761
		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)		(0.0000)
6) Non-wave (all industries)	1254	0.3946	504	0.3723	349	0.3488	305	0.2161	315	0.3307	214	0.2628
		(0.0000)		(0.0000)		(0.0000)		(0.0004)		(0.0000)		(0.0001)
Non-wave – In-wave (5 - 3)		**0.4466**		**0.3992**		**0.5563**		**0.4206**		**0.2947**		**0.3166**
p-value of difference		**(0.0000)**		**(0.0000)**		**(0.0000)**		**(0.0001)**		**(0.0096)**		**(0.0051)**

This table reports the mean acquirers' 5-year post-acquisition raw returns. Post-acquisition 5-year raw returns are the compound returns, calculated for months t+1 through t+60, where month t is the month of the merger announcement, or until a firm is delisted, whichever is earlier. We sort the bidders according to the timing of the announcement date relative to the industry-specific waves. The pre-wave period includes the year before the beginning of the wave, the in-wave period includes the two years of the wave, and the post-wave period spans the three years following the end of the wave. Non-wave periods cover the rest of the time period. Panel A reports the results using the *stock* waves (industry merger waves defined using only pure stock offers), whereas Panel B reports the results using the *cash* waves (industry merger waves defined using only pure cash offers). We report the mean return for each category, along with the p-value associated with the t-test for statistical difference from zero. The bottom part of each panel shows the results of a t-test for difference in mean return of non-wave acquirers (line 5) and in-wave acquirers (line 3).

We report the mean acquirers' 5-year post-acquisition raw returns for: all types of acquisitions (column 1), the acquisitions of public and private targets (columns 2 and 3), the pure stock and pure cash acquisitions (columns 4 and 5), and public acquirers that made a pure stock offer for a public target (column 6). P-values are presented in parentheses. Boldfaced differences in mean post-acquisition returns and associated p-values are significant at the 5% level or higher. For ease of reading, we do not use boldface characters for simple mean returns, even when they are significantly different from zero.

Appendix C1. Summary of Ordinary Least Squares (OLS) and Logit Regression Results

This table summarizes the main findings of the OLS regressions of Table 4.3 and logit regressions of Table 4.4, where the 2.5% filter rule is applied. The first (second) row of each weather variable contains results for the OLS (logit) regressions. Only the signs of regression coefficients significant at the 20% level or higher are reported. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, two-tailed tests, respectively. The superscript [a] indicates significance at the 20% level. The dependent variables of the OLS regression is daily index return, and that of the logit regression is the probability of a positive daily return. All weather variables are based on the average of hourly readings between 6:00 AM and 4:00 PM local time on the day of the measurement. SKC is the average sky cover. SPD is the average wind speed. RAIN is an indicator variable that is equal to 1 if the average of the hourly records of liquid precipitations registered in the 6 hours prior to any hourly readings is positive; and zero otherwise. SD is the depth of the snow cover on the ground; it is set to zero in summer months and in hot and mild countries. TEMP is the daily average temperature. Panel A, B, and C summarize results for the cold, mild, and hot countries, respectively. We define cold, mild, and hot regions based on the 33rd and 67th percentiles of the full sample's distribution of annual temperatures. Blue, red, and black colors of the signs indicate results consistent with, inconsistent with, or neutral to the interpretation that comfortable weather leads to higher returns, respectively.

Panel A: Cold countries

	Jan (1)	Feb (2)	March (3)	April (4)	May (5)	June (6)	July (7)	Aug (8)	Sept (9)	Oct (10)	Nov (11)	Dec (12)	All (13)
SKC		−*	−[a]	−[a]				−[a]					−***
		−*				−**	−*						−***
SPD			−***			−**	−*	−*					−*
			−[a]										
RAIN				−[a]			−**						−*
	+[a]				−[a]		−[a]						
SD			−[a]										
	−***	−[a]	−***										−**
TEMP	−***	−**				−**			+***	−***			−***
	−**	−***			+[a]	−*	−[a]		+***	−**		−**	−***

Appendix C1 (Continued). Summary of Ordinary Least Squares (OLS) and Logit Regression Results

Panel B: Mild countries

	Jan (1)	**Feb (2)**	**March (3)**	**April (4)**	**May (5)**	**June (6)**	**July (7)**	**Aug (8)**	**Sept (9)**	**Oct (10)**	**Nov (11)**	**Dec (12)**	**All (13)**
SKC			−**			−*							−*
		+**	−[a]			−***						+***	
SPD				−*			−[a]					−**	−[a]
				−[a]	−[a]		−**					−**	−*
RAIN						+**							
	−[a]		−*	−**		+[a]			−[a]			−*	−***
TEMP	−*	−***				−**						−[a]	−***
	−*	−***		−*		−***			+[a]			−***	−***

Appendix C1 (Continued). Summary of Ordinary Least Squares (OLS) and Logit Regression Results

Panel C: Hot countries

	Jan (1)	Feb (2)	March (3)	April (4)	May (5)	June (6)	July (7)	Aug (8)	Sept (9)	Oct (10)	Nov (11)	Dec (12)	All (13)
SKC	−*	−**			−**			−[a]	−[a]			−***	−***
					−[a]			−[a]		+[a]		−**	
SPD				+**				−[a]	−[a]		−**		
						+**							
RAIN							+***	+*					+**
	+[a]					+**							+**
TEMP				+*		−[a]		−***				+[a]	
	+[a]			+*	+*	−**		−**				+***	

Appendix C2. Daily Time Spent Outdoors for Each Month, by Temperature Region

This table reports the averages of daily maximum temperature and daily time spent outdoors for the months, in descending order of time spent outdoors, for each temperature region. We estimate the outdoor leisure time using the relationship between time spent outdoors and maximum daily temperature retrieved from Graff Zivin and Neidell (2014).

Cold Countries			*Mild Countries*			*Hot Countries*		
Month	Maximum Daily Temperature (°F)	Time Spent Outdoors (Minutes)	Month	Maximum Daily Temperature (°F)	Time Spent Outdoors (Minutes)	Month	Maximum Daily Temperature (°F)	Time Spent Outdoors (Minutes)
July	75.3	40.8	July	79.3	41.4	August	85.5	45.9
August	72.5	39.4	August	76.4	40.5	June	85.6	45.2
June	70.5	37.7	June	79.5	40.2	July	85.3	45.1
September	67.1	34.8	September	73.9	38.9	September	84.4	44.8
May	62.9	29.2	May	70.0	37.0	May	84.4	44.3
October	55.6	22.8	April	65.3	32.6	October	82.9	44.0
April	54.5	21.7	October	63.1	30.3	April	83.1	43.3
November	45.9	16.0	November	56.5	23.3	November	79.8	42.6
March	43.7	14.7	March	55.3	21.9	March	79.8	40.9
December	39.2	12.1	December	49.2	18.5	December	76.3	39.3
February	35.6	10.6	February	48.4	16.9	February	77.1	39.1
January	34.4	10.1	January	46.6	16.2	January	75.2	38.4