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RESEARCH ARTICLE

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# Face-to-face interactions and the returns to acquisitions: Evidence from smartphone geolocational data

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## Abstract

**Research Summary:** We examine the effect of face-to-face interactions between acquirers and targets before the acquisition announcements on acquisition returns. We argue that frequent interactions increase the target management's trust in the acquirer and benefit the acquirer by mitigating competition in the bidding process. For a sample of U.S. domestic acquisitions, we use smartphone geolocational data to measure the movement of people between merging companies in the months before the announcement. We find that with more frequent interactions, acquirers earn higher stock market returns at the announcement and targets receive fewer later bids from other bidders. Moreover, more frequent interactions are associated with lower returns to public targets vis-à-vis their acquirers. The effect of interactions is weaker when shareholder-manager agency problems in the target are less severe.

**Managerial Summary:** Previous research shows that while acquisitions can create synergistic gains, the presence of potentially competing bidders forces acquirers to pay a high price for their targets, which makes acquisitions generally unprofitable for acquirers. We provide evidence suggesting that frequent social

interactions between the acquirer's and the target's management in the pre-acquisition phase increase the target management's trust in the acquirer, making it more willing to cede control to the acquirer and less eager to seek alternative bidders. By mitigating competition in the bidding process, social interactions make acquisitions more profitable for acquirers vis-à-vis targets. Social interactions are less effective when the target's management owns a larger share of the target or is better monitored by shareholders (e.g., in companies with concentrated ownership or private companies).

**KEYWORDS**

abnormal returns, agency theory, competitive bidding, mergers and acquisitions, smartphone geolocational data

## 1 | INTRODUCTION

Mergers and acquisitions (M&As) are important means to expand the scale and scope of a firm (Villalonga & McGahan, 2005; Wang & Zajac, 2007). These transactions can create value by allowing firms to obtain operational, financial, or collusive synergies (Chatterjee, 1986; Larsson & Finkelstein, 1999). However, the presence of potentially competing bidders can force acquirers to pay a high price to acquire their targets, which allows targets to appropriate most of the value created by acquisitions (Aktas, de Bodt, & Roll, 2010; Barney, 1988; Capron & Pistre, 2002). Indeed, researchers have typically found that while acquisitions produce positive combined returns for the two merging firms, the majority of the gains accrue to the target's shareholders, and the acquirer's shareholders obtain close to zero or negative returns (Haleblian, Devers, McNamara, Carpenter, & Davison, 2009). While much is known about what factors maximize the value that can be created by M&As (e.g., Birkinshaw, Bresman, & Håkanson, 2000; Datta, 1991; Larsson & Finkelstein, 1999), less is known about what acquirers can do during the pre-acquisition phase to capture more value.

In this paper, we examine the effect of social interactions between acquirers and targets before the acquisition announcement on the returns to acquirers. We argue that frequent social interactions allow the acquirer to capture more value from M&As by altering the competitiveness of the bidding process. It is well established that social interactions promote the development of mutual understanding and trust among agents (e.g., Bell & Zaheer, 2007; Sorenson & Stuart, 2001; Storper & Venables, 2004; Tsai & Ghoshal, 1998). Such interactions can also occur between prospective merging partners (e.g., Davis & Schreiber, 2017; Share, 1998). As a consequence of the increased trust, the target's management may be more willing to cede control to the focal acquirer and less likely to seek alternative bidders, allowing the acquirer to engage in a one-to-one negotiation and therefore to appropriate more acquisition value (Bulow & Klemperer, 1996). However, while the target's management may prefer to merge with a trusted

partner, the target's shareholders would be better off in an auction-like contest that allows maximization of the acquisition price. Because our hypothesized mechanism implies that the target's management trades shareholder value with the ongoing benefit of merging with a trusted acquirer, we expect the effect of social interactions to be weaker in targets where owner-manager agency problems are less severe.

Previous literature has analyzed how preexisting relationships between merging companies such as alliances (Porrini, 2004; Zaheer, Hernandez, & Banerjee, 2010) or board interlocks (Cai & Sevilir, 2012) can affect the returns from M&As. Yet, anecdotal evidence suggests that social interactions in the pre-acquisition phase can occur between companies with no established relationship (Cullinan, Roux, & Weddigen, 2004; Share, 1998; Wheelwright, Holloway, Tempest, & Kasper, 2000), and some acquirers strategically seek these interactions to increase their value capture from M&As (Davis & Schreiber, 2017). These social interactions in the pre-acquisition phase were largely unmeasured and escaped empirical examinations in previous research.

While contacts between the management of prospective merging companies are likely to arise to some extent in any deal, frequent face-to-face interactions enable socialization and the development of trust between managers (Bell & Zaheer, 2007; Sorenson & Stuart, 2001; Storper & Venables, 2004; Tsai & Ghoshal, 1998). To capture the frequency of these interactions between merging companies in the pre-acquisition phase, we introduce a new measure based on anonymized smartphone geolocational data. Using these data, we define the "employees" of the acquirer and of the target as those who appeared in the companies' headquarters (HQs) during business hours in the 8 months preceding the acquisition announcement. We proxy interactions by the number of times employees visited the other company's HQs. Using a sample of U.S. domestic acquisitions by public acquirers announced between July 2016 and January 2018, we find that deals with more frequent pre-announcement interactions induce higher abnormal returns on the acquirer's stock at the acquisition announcement. Targets that interact frequently with their acquirer receive fewer bids from other bidders after the announcement. In the subset of acquisitions with public targets, more frequent interactions are associated with lower gains to the target relative to the acquirer. This effect is weaker when the target's management owns a considerable share of the target, suggesting that the observed effects are related to an agency problem between the target's shareholders and management (e.g., Bargeron, Schlingemann, Stulz, & Zutter, 2008). Overall, these results are consistent with our theory that social interactions mitigate the competitiveness of the bidding process and allow acquirers to appropriate more value from the acquisition. In further analyses, we present complementary results that are consistent with our proposed mechanism, including how social interactions affect the duration of post-announcement negotiations with the target, the probability that the deal is canceled due to the target's refusal, the probability that managers of the two firms share the leadership of the merged entity, and how concentration of the target's ownership or the target's private status reduces the effect of interactions (likely by further reducing agency problems).

However, while our correlational results are in line with our hypotheses, they do not allow us to infer a causal connection between social interactions and the acquirer's value capture. Unobservable deal-specific characteristics, such as idiosyncratic cultural compatibility or synergies, may trigger social interactions and, at the same time, increase the acquirer's value capture. In such case, observed interactions should be considered a proxy for an unobservable driver of the acquirer's ability to appropriate value vis-à-vis the target rather than its cause. To account for the potential endogeneity of intercompany visits, we

run instrumental variable regressions. We use two alternative instruments capturing exogenous changes in the ability of companies to interact: the presence of airline flights between merging companies' core-based statistical areas (CBSAs)[1] or deviations of the number of snowfall days from that in average years in the target's CBSA. While new airline routes reduce the travel time between companies (Bernstein, Giroud, & Townsend, 2016; Giroud, 2013; Testoni, 2019), snowfalls disrupt travel due to traffic and flight delays. After confirming the relevance of these instruments in predicting visits and that instrumented visits affect the acquirer's returns in our 2016–2018 sample, we run reduced-form instrumental variable regressions on a larger sample of deals announced between 1980 and 2019 by replacing our visits measure with the instruments to test the generalizability of our findings to a longer period. The results confirm that targets appropriate less value relative to their acquirers when intercompany visits are easier. Further tests suggest that the observed patterns are due to a treatment effect (i.e., an effect of the instruments holding constant all the characteristics of a deal) rather than a selection effect (i.e., an effect of the instruments due to an improved screening of deals by acquirers). Finally, we describe how social interactions in the pre-acquisition phase can affect acquisition returns through alternative mechanisms, and discuss why our results are not consistent with these alternative mechanisms.

This study contributes to the strategy literature in several ways. Whether and how acquisitions create economic value are key questions in strategy research (Birkinshaw et al., 2000; Datta, 1991; Larsson & Finkelstein, 1999; Rawley, Godart, & Shipilov, 2018). Studies have analyzed what acquirer or target characteristics facilitate acquirers' value capture in M&As, such as idiosyncratic synergies (Capron & Pistre, 2002), the target's product market positioning (Ahern, 2012), or M&A experience (Cuypers, Cuypers, & Martin, 2017). Yet, less is known about what acquirers can *do* in the pre-acquisition phase to increase their value capture. Corroborating anecdotal evidence (e.g., Davis & Schreiber, 2017; Share, 1998), we provide evidence in line with our claim that social interactions with the target in the pre-acquisition phase can allow acquirers to appropriate more value from M&As.

Another contribution of our study is to provide a novel measure of social interactions between firms, which escaped previous investigations. Previous studies relied on measures such as alliances (Gulati, 1995; Porrini, 2004; Zaheer et al., 2010), board interlocks (Cai & Sevilir, 2012), or geographical proximity (Chakrabarti & Mitchell, 2013, 2016; Ragozzino & Reuer, 2011; Uysal, Kedia, & Panchapagesan, 2008) to capture social interactions between the management of two firms. Yet, managers may interact with firms with whom they do not have a preexisting relationship, or that are not in their near surroundings. Indeed, our measure is empirically distinct from these alternative measures. While the social interactions that we observe were largely unmeasured in previous research, they appear to provide economic value to firms. Smartphone geolocational data have been used in political science (Chen & Rohla, 2018) and in organizational studies (de Vaan, Mumtaz, Nagaraj, & Srivastava, 2021), but as far as we know this paper is the first to use such data in strategy research. This paper highlights that this novel measure can be used by strategy scholars to investigate fundamental questions about the role of social interactions on firm performance.

[1]CBSAs consist of one or more counties with a high degree of socioeconomic integration that are centered on an urban center with population of at least 10,000.

## 2 | THEORY

### 2.1 | Value capture in M&As

The returns that an acquirer obtains from an acquisition are determined by the difference between the synergistic value created by the acquisition and the price premium paid to gain control of the target. The value that can be created by M&As stems from operational, financial, or collusive synergies (Chatterjee, 1986; Larsson & Finkelstein, 1999). Synergistic gains from M&As are not rare. Indeed, the literature shows that, on average, M&As create value for the shareholders of the acquirer and the target company combined (Haleblian et al., 2009).

Holding constant the synergistic value of a transaction, the fraction of value that is appropriated by the acquirer depends on the price the target demands to cede control of its assets. The presence of potential competing bidders provides bargaining power to the target, increasing the fraction of value appropriated by the target vis-à-vis the acquirer (Ahern, 2012; Aktas et al., 2010; Barney, 1988; Capron & Pistre, 2002). The literature on M&As has typically found that targets capture most of the value created by M&As (Haleblian et al., 2009), suggesting that they generally have high bargaining power in price negotiations. An acquirer may face intense competition even when it is the only observed bidder. Indeed, the competitive threat posed by latent potential bidders often forces the acquirer to offer a high price to preempt competitors (Aktas et al., 2010; Capron & Pistre, 2002). To appropriate some value from the acquisition, the acquirer needs to have an advantage relative to other potential bidders (Barney, 1988; Chatterjee, 1986). For instance, studies have shown that uniquely valuable synergies with the target (Capron & Pistre, 2002) or superior information about the target's value (Cai & Sevilir, 2012; Capron & Shen, 2007; Ragozzino & Reuer, 2011; Uysal et al., 2008) can provide an advantage to the acquirer, allowing it to capture more value.

The target's management also plays a role in determining the price of an acquisition. When the target's management has a strong preference for one acquirer, it may accept a lower price to cede control and less actively pursue alternative bidders (Betton, Eckbo, & Thorburn, 2008; Hartzell, Ofek, & Yermack, 2004; Qiu, Trapkov, & Yakoub, 2014). The reluctance of the target's management to cede control to a specific acquirer is evident in hostile takeovers. In these cases, the management publicly discloses its intention to obstruct the takeover through defense tactics, sometimes forcing the hostile bidder to compete with a friendlier bidder (a "white knight"). Since the 1990s, however, hostile takeovers have become a rare phenomenon due to the diffusion of powerful anti-takeover mechanisms (Betton et al., 2008). For example, Andrade, Mitchell, and Stafford (2001) show that the percentage of hostile bids dropped from 14.3% in the 1980s to just 4% in the 1990s.[2] M&As in the past three decades are typically friendly transactions with a single bidder, for which no competition is observed ex post. Nevertheless, Aktas et al. (2010) show that the presence of potential but unobserved latent buyers increases the targets' bargaining power in price negotiations. Hence, even in friendly transactions, the target's management generally has a choice over which potential acquirer they intend to partner with and how intensively they want to involve other bidders. The active collaboration of the target's management is essential in the deal planning and due diligence process[3] as well as in the post-deal integration process (Chatterjee, Lubatkin, Schweiger, & Weber, 1992; Larsson &

[2]In the sample of this study that covers the 2016–2018 period, only one transaction is classified as a hostile takeover.
[3]For example, a study by Bain & Company illustrates the importance of the due diligence process in successful M&As and highlights that "*[g]etting ground-level numbers [about a target] usually requires the close cooperation of the acquisition target's top brass. An adversarial posture almost always backfires*" (Cullinan et al., 2004).

Finkelstein, 1999). Hence, the lack of such involvement can be an important deterrent for less desired potential bidders.

While the target's managers may act to maximize the acquisition price in order to serve the interest of their shareholders, they may also act in their own interest. For instance, there is evidence that the targets' CEOs sometimes negotiate a lower acquisition price in exchange for personal monetary benefits, including cash bonuses, increased golden parachutes, and job retention (Hartzell et al., 2004; Qiu et al., 2014). The extent to which the target's management trusts a focal acquirer is likely to be another important factor driving the target's management preference over other potential acquirers. Indeed, extensive literature shows that M&As can be stressful transitions and are often perceived negatively by the target's management (Larsson & Finkelstein, 1999). M&As can challenge the targets' managers by threatening their cultural traditions, reducing their individual influence, and affecting their career plans (Chatterjee et al., 1992; Krug & Hegarty, 1997, 2001; Lubatkin, Schweiger, & Weber, 1999; Walsh & Ellwood, 1991). Because of these uncertainties, the targets' managers are likely to prefer partnering with an acquirer that they trust and with whom they have developed a common view about the future of the merging companies.

We argue that frequent social interactions with the target's management in the pre-acquisition phase is a tool acquirers can use to increase the level of trust of the target's management, which allows acquirers to appropriate more value from M&As. Below we present our hypotheses.

## 2.2 | The role of pre-announcement social interactions

As mentioned, the possibilities of a stressful post-acquisition integration process, cultural conflicts, and changes in status and career plans create significant uncertainties for the target's management (Chatterjee et al., 1992; Krug & Hegarty, 1997, 2001; Larsson & Finkelstein, 1999; Lubatkin et al., 1999; Walsh & Ellwood, 1991). Given these uncertainties, the extent to which managers trust a prospective acquirer is likely to affect their willingness to cede control to the focal acquirer. It is well established that the development of mutual understanding and trust among agents is promoted by social interactions (e.g., Bell & Zaheer, 2007; Sorenson & Stuart, 2001; Storper & Venables, 2004; Tsai & Ghoshal, 1998). Thus, we argue that frequent social interactions with the target's management before the acquisition help the acquirer to gain the trust of the target's management. Such trust serves the dual purpose of convincing the target's management that partnering with the acquirer is a more promising option than continuing operations as a standalone entity, and of reducing the chances that the management will seek alternative bidders. As a consequence, the acquirer gains an advantage in the bidding process, which allows it to appropriate more value from the acquisition.

Previous literature analyzed the role of preexisting intercompany relationships, such as alliances (Porrini, 2004; Zaheer et al., 2010) or board interlocks (Cai & Sevilir, 2012), as antecedents of M&A performance. Yet, anecdotal evidence suggests that social relationships between potential merging partners can arise during the pre-acquisition period (Cullinan et al., 2004; Share, 1998; Wheelwright et al., 2000), and some acquirers strategically seek these relationships to improve returns from M&As.[4] This acquisition strategy is vividly illustrated by the following

[4]Managers of public companies need to act discreetly when interacting with a prospective merging partner before the acquisition is announced to avoid spreading rumors about the potential acquisition and possible market abuses. For instance, information leaks before a deal is signed may cause a runup in the target's stock price, potentially making the acquisition more expensive for the acquirer (Davis & Schreiber, 2017; Intralinks, 2017). Nevertheless, press releases and

quote from industry experts: "*Many buyers [...] seek 'proprietary deals' by forming a relationship over a period of time with a potential target company, its owners and management before ultimately making an offer to buy that company. One of the objectives of this approach is to afford no opportunity for other potential buyers to make a competing bid*" (Davis & Schreiber, 2017, p. 1).

A case in point of the type of relationship that may arise between the management of two companies before a merger is Duke Power's acquisition of PanEnergy, announced in 1996. As later revealed, the executives of the two companies interacted frequently in the months preceding the transaction announcement: "*[The two executives] met in Houston and in Charlotte, played golf and visited the others' operations. They soon found that they not only could work together, but actually liked each other. The pair agreed to share the leadership of the new company*" (Share, 1998, p. 6). When this type of friendly relationship arises between the management of two companies, the target's management is likely to be more willing to engage in the transaction and less likely to aggressively seek other buyers. Hence, the acquisition process is likely to take the form of a bilateral negotiation with one preferred bidder rather than a competitive contest with multiple possible buyers. By lowering the competitive pressure faced by the acquirer, the friendly relationship may therefore allow it to appropriate more value from the acquisition.[5]

As Bulow and Klemperer (1996) show under fairly reasonable assumptions, an acquirer is always better off in a bilateral negotiation than in an auction with multiple bidders. Hence, our central claim is that frequent social interactions with the target in the months preceding an acquisition announcement and the resulting social connection with the target's management can significantly impact the acquirer's returns from M&As. In particular, we posit that the increased propensity of the target's management to cede control to the focal acquirer provides an advantage to the acquirer relative to other latent bidders and allows the acquirer to appropriate more value from the transaction. As is common in the literature (e.g., Cai & Sevilir, 2012; Zaheer et al., 2010), we measure acquisition gains with the abnormal returns on the acquirer's stocks at the acquisition announcement, which capture investors' expectations about the profitability of the acquisition. Moreover, we measure the frequency of social interactions with the frequency of intercompany visits between the two merging companies in the months preceding the acquisition announcement. We therefore predict:

> **Hypothesis (H1).** *The greater the frequency of intercompany visits between two merging companies in the months preceding the deal announcement, the greater the acquirer's abnormal returns from the acquisition.*

We argue that the increase in acquirer's gains stems from the reduced willingness of the target's management to engage in negotiations with other bidders, which makes the acquisition process a one-to-one negotiation. Accordingly, we should expect that when acquirers interact

company filings reveal that face-to-face interactions before deal announcements are not rare. For example, it is stated that prior to Allergan's acquisition of Vitae, "*On July 18, 2016, Mr. Hatfield [CEO of Vitae] met with Mr. Saunders [CEO of Allergan] [...] at Allergan's administrative headquarters in the U.S. in Parsippany, New Jersey*" (Vitae Pharmaceuticals SEC 14D9, September 26, 2016).

[5]Regarding Duke Power's acquisition of PanEnergy, the acquisition premium, computed as the percentage difference of per share acquisition price relative to PanEnergy's stock price 4 weeks before the acquisition announcement, was 30.3% (source: SDC Platinum). This premium is slightly lower than the average premium in that year, about 39%, but considerably lower than the average final premium among deals with multiple observed bidders in that year, about 59%.

frequently with their targets before the acquisition announcement, the average number of later acquisition bids for the targets by alternative bidders will be lower. Thus:

> **Hypothesis (H2).** *The greater the frequency of intercompany visits between two merging companies in the months preceding the deal announcement, the fewer the acquisition bids the target receives from other bidders after the focal deal announcement.*

The acquirer's gains can increase because the transaction creates more synergistic value or because the acquirer is able to appropriate more value vis-à-vis the target.[6] Our arguments suggest that the increase in acquirer's returns is due to an increase in the acquirer's value capture vis-à-vis the target's shareholders rather than to an increase in the value created by the transaction. For public targets, we can measure the target's stock market gains at the acquisition announcement. Hence, we can compare the target's gains vis-à-vis the acquirer's gains to measure the two parties' appropriation of deal value (Ahern, 2012; Cuypers et al., 2017). As discussed, the presence of latent potential bidders generally allows targets to appropriate most of the value created by M&As (Aktas et al., 2010; Barney, 1988; Capron & Pistre, 2002; Haleblian et al., 2009). If social interactions mitigate the competitiveness in the bidding process by increasing the appeal of the focal acquirer relative to other latent potential bidders, we should expect the target's value appropriation to be lower in deals with frequent social interactions during the pre-acquisition period. Hence, we predict:

> **Hypothesis (H3).** *The greater the frequency of intercompany visits between two merging companies in the months preceding the deal announcement, the lower the target's abnormal returns from the acquisition relative to those of the acquirer.*

By building the target management's trust in the acquirer and therefore increasing the acquirer's appeal, frequent social interactions are expected to make the transaction a de facto one-to-one negotiation. While it can be beneficial for the target's management to partner with an acquirer that they trust, the target's shareholders would gain more in a fully competitive auction than in a one-to-one negotiation (Bulow & Klemperer, 1996). Hence, the effects described in the previous hypotheses are expected to arise from an agency problem between the target's shareholders and management. Previous research has shown that the target's management can sacrifice shareholder value for personal benefits (Feldman, Amit, & Villalonga, 2019; Hartzell et al., 2004; Qiu et al., 2014). In our context, this implies that the target's managers may be willing to trade shareholder gains with the ongoing benefit of merging with a trusted acquirer with whom they have developed a common view of the future of the merged company. This agency problem may be mitigated when the target's management owns a large share of the target, which should realign the target's management incentives with those of their shareholders (e.g., Bargeron et al., 2008; Hartzell et al., 2004). The greater the share of the management ownership of the target, the more the target's management internalizes the trade-off between the

[6]Similarly, the acquisition premium (i.e., the target's gains) can increase because the deal creates more synergistic value or because the target is able to appropriate more value vis-à-vis the acquirer. Because our theory concerns changes in value appropriation, our hypothesis focuses on the difference in gains between the target and the acquirer, as suggested by Ahern (2012), rather than on the acquisition premium.

gains from selling the company and the benefit of establishing a one-to-one negotiation with a trusted acquirer. Hence, we expect the negative effect of social interactions on the target's value appropriation vis-à-vis the acquirer to be weakened by the target's management ownership. Thus:

> **Hypothesis (H4).** *The greater the share of the target owned by the target's management, the weaker the negative effect of the frequency of intercompany visits on the target's abnormal returns relative to those of the acquirer.*

# 3 | METHODS

## 3.1 | Data

### 3.1.1 | M&A data

The sample includes U.S. domestic acquisitions by public companies that were announced between July 2016 and January 2018, excluding deals involving financial firms (companies with primary Standard Industrial Classification [SIC] codes from 60 to 69). This period is selected due to the availability of smartphone data. We consider acquisitions of majority stakes (or of the totality) of companies, thus excluding acquisitions of assets and acquisitions of partial interests. Data on M&A transactions are collected from Thomson SDC Platinum. As is common in the literature (e.g., Cai & Sevilir, 2012; Savor & Lu, 2009; Uysal et al., 2008), we avoid considering small and economically insignificant deals in SDC. Specifically, we include only transactions in which the deal value is at least $10 million and at least 1% of the year-end market capitalization of the acquirer before the announcement. Accounting data are from Compustat, and stock market data are from CRSP. During our sample period, there are 333 firm-acquisition announcements involving public acquirers in non-financial industries with known transaction value and known locations of the acquirer's and target's HQs. Applying our selection criteria and excluding missing values on the acquirer's financials in Compustat-CRSP yields 234 deals for our analysis, including 97 deals involving a public target with data in Compustat-CRSP.

For every firm in the sample, we verified and sometimes corrected the addresses of the HQs reported in SDC using companies' websites, business news, and companies' publications reported in LexisNexis.[7] We then visually identified the perimeter of the HQs' buildings on Google Maps and geocoded the locations using the *geohash* system. Geohash is a publicly available geocoding system that assigns a string of letters and numbers to geographic locations. This system subdivides space using a hierarchical grid structure with different levels of precision. As more characters are included in the geohash string, the rectangular cell corresponding to the geohash becomes smaller. In most cases, we find that companies' perimeters are best described by a set of geohashes at the six- or seven-character level. If a firm's HQs comprise multiple buildings, we recorded a set of geohashes for each of them.

[7]Even though firms can have many secondary locations (e.g., plants, subsidiaries, and branches), companies' HQs are likely to be central to our analysis, since they represent the primary location of firms' top management teams.

### 3.1.2 | Smartphone data

We obtained location tracking data from SafeGraph, a company that aggregates anonymized smartphone location data from numerous applications on both Apple and Android smartphones. The SafeGraph data cover approximately 10% of the smartphone users in the United States and consist of "pings," each of which identifies the latitude and longitude of a smartphone at a moment in time. Smartphones are assigned unique and anonymous identifiers. We obtained the SafeGraph data for the period November 2015–November 2017. We then pulled all the pings that appeared in the companies' HQs during the 8 months preceding the acquisition announcement. Most of the observed visits fall into these 8-month windows. Indeed, as is described in Section 4, most of the interactions occur within the 3–4 months preceding the acquisition announcement.[8]

From this sample, we removed all the pings associated with smartphones that were moving within the proximity of the companies (e.g., passersby). We then assumed that a smartphone belonged to an employee of the company if it appeared in the company's location during a business day (i.e., excluding weekends and national holidays) between 7:00 a.m. and 7:00 p.m. in the pre-announcement period. If the "employee" appeared in both the acquirer's and the target's location, we assigned the person to the company where he or she appeared on most business days. Because smartphone data are anonymous, we cannot be certain that a person in a company's HQs is an actual employee. We discuss this limitation in the concluding section.

## 3.2 | Baseline regression models

In our baseline regressions, we test the effect of intercompany visits on different M&A outcomes. The key independent variable is the number of days that the acquirer's or the target's employees visited the HQs of the other company in the 8 months preceding the acquisition announcement. To test Hypothesis (H1), we run ordinary least squares (OLS) regressions on the cumulative abnormal returns (CAR) of the acquirer's stock at the announcement. To test Hypothesis (H2), we run a Poisson pseudo-likelihood regression (Correia, Guimarães, & Zylkin, 2019a, 2019b) on the number of bids the target receives from other bidders after the announcement of the focal acquisition. To test Hypothesis (H3), we run OLS regressions using the subset of deals with a public target, where the dependent variable is the difference of the weighted CAR of the two merging companies, and the weights are a function of each firm's pre-announcement market value of equity (Ahern, 2012). To verify whether intercompany visits also affect the total value created by the acquisition (in addition to the two parties' value appropriation), we also run OLS regressions on the total CAR of the two merging companies. Finally, we test Hypothesis (H4) by checking whether the effect of intercompany visits on the difference in gains between merging companies is moderated by the share of the target that is owned by the target's management. Because visits might be affected by location-specific shocks (such as average traffic, airports' connectivity, or weather conditions), we allow the error of the models

[8]Because our smartphone data cover the period from November 2015 to November 2017, the most recent acquisition announcements (in December 2017 and January 2018) have a shorter pre-announcement window with which to observe intercompany visits. In the regressions, we control for differences in data coverage by including period fixed effects. In Online Appendix C, we report a robustness analysis excluding deals announced in December 2017 and January 2018.

to be correlated among deals with firms in the same CBSAs. Specifically, we use robust *SE* that are two-way-clustered by acquirer CBSA and target CBSA (Cameron, Gelbach, & Miller, 2011). Results hold equally using *SE* derived from asymptotic theory, robust (non-clustered) *SE*, or *SE* clustered by state pair.

## 3.3 | Measures

### 3.3.1 | Acquirer's CAR

The acquirer's returns from the acquisition are measured as the percentage CAR on the acquirer's stock over a 3-day window centered on the deal announcement (date $t = 0$) (Brown & Warner, 1985). First, we estimate on the period from 260 to 20 trading days before the announcement the market model $r_{it} = \alpha_i + \beta_i r_{mt} + \varepsilon_{it}$, where $r_{it}$ is the stock return of firm $i$ on day $t$, $r_{mt}$ is the daily market return on the CRSP value-weighted index, $\alpha_i$ and $\beta_i$ are parameters specific to the company, and $\varepsilon_{it}$ is the error term. Abnormal returns are then calculated as the residuals $\widehat{\varepsilon}_{it} = r_{it} - \widehat{r}_{it}$, where $\widehat{r}_{it} = \widehat{\alpha}_i + \widehat{\beta}_i r_{mt}$ are the predicted returns, and $\widehat{\alpha}_i$ and $\widehat{\beta}_i$ are the estimated coefficients. Finally, the CAR are calculated by summing the daily abnormal returns $\widehat{\varepsilon}_{it}$ over a 3-trading day window surrounding the announcement date [−1, 1]. Results are substantially unchanged using alternative event windows, such as [−5, 1], [−1, 5], [−5, 5], or [−15, 1]. To mitigate the effect of any outlier, we winsorize CAR at the 2nd and 98th percentiles.

### 3.3.2 | Later bids for the target

Hypothesis (H2) predicts that frequent social interactions with the acquirer reduce the target management's willingness to solicit other bidders. To test this hypothesis, ideally we would need to observe whether the target's management solicits alternative acquirers after the announcement of the offer by the focal acquirer. While the timing of the solicitation is not observable in our data, we can observe whether the target receives bids from other bidders after the announcement of the focal acquisition. Allowing some lag between the target's solicitation and the announcement of later bids, we define the variable *later bids* as the number of bids the target receives from other bidders up to 2 years after the announcement of the focal acquisition.[9]

### 3.3.3 | Difference in gains between merging companies and total gains

For the subset of deals with a public target, we can measure the value appropriated by the target relative to that of the acquirer as the difference in weighted abnormal gains between the two merging companies. We apply the method proposed by Ahern (2012) and compute the variable $\Delta CAR$, defined as $\Delta CAR = (MVAL_t \times CAR_t - MVAL_a \times CAR_a)/(MVAL_t + MVAL_a)$, where $CAR_t$ ($CAR_a$) is the percentage CAR of the target (acquirer) and $MVAL_t$ ($MVAL_a$) is the market

[9]Table S4 in Online Appendix C reports regressions considering a less conservative lag of up to 1 year to construct the *later bids* variable. Results are similar.

value of equity of the target (acquirer) 50 trading days before the announcement. The CAR for the target is computed as described above for the acquirer. By scaling CARs by each firm's market value, this measure accounts for differences in size between merging firms to determine the difference in gains. Moreover, this measure can correctly estimate the target's gains relative to the acquirer's in the case of negative total gains. The target's CAR or the acquisition premium could also measure the target's surplus from the acquisition (see Online Appendix C for results using these measures), but the key advantage of $\Delta CAR$ is that it clarifies whether higher or lower returns to the target are due to differences in value allocation between the merging parties as opposed to differences in total value created.

While our hypotheses focus on changes in merging firms' value appropriation, we further verify whether intercompany visits affect the total value created by the deal. We measure the total abnormal gains of the two merging companies with the variable *total CAR* (e.g., see Cai & Sevilir, 2012), defined as $total\ CAR = (MVAL_t \times CAR_t + MVAL_a \times CAR_a)/(MVAL_t + MVAL_a)$. As with the acquirer's CAR, we winsorize $\Delta CAR$ and *total CAR* at the 2nd and 98th percentiles.

### 3.3.4 | Intercompany visits

Our key explanatory variable is *number of visits*, which is the number of days in which the acquirer's or the target's employees visited the other company's HQs between 7:00 a.m. and 7:00 p.m. of a business day in the 8 months preceding the acquisition announcement.[10] Because a few observations have highly frequent visits, we winsorize the number of visits at the 2nd and 98th percentiles. Results remain similar using the unwinsorized variable or the top/bottom 1 or 5% of the distribution as alternative thresholds.

### 3.3.5 | Target's management ownership

For public targets, we collect data on the percentage of the target that is collectively owned by the management (broadly defined as executives and directors), labeled *target management ownership*, from the last proxy statement before the deal.[11]

### 3.3.6 | Control variables

We control for different deal- or firm-specific characteristics that could correlate with intercompany visits and the acquisition outcomes.

*Distant target*

Geographic distance can reduce the synergistic benefits of an acquisition and increase the level of information asymmetry between merging companies (e.g., Chakrabarti & Mitchell, 2013, 2016; Uysal et al., 2008). At the same time, greater distance may reduce the ability of managers

[10]In the following analyses, similar results are found if we consider only the acquirer employees' visits to the target or the target employees' visits to the acquirer.

[11]For results using the share of the target CEO's ownership as alternative measure, see Table S6 in Online Appendix C.

to visit the other company, due to the greater transportation time. Hence, we include a dummy variable *distant target* indicating whether the two companies are more than 100 miles away from each other. The shortest distance between the acquirer's and the target's HQs is computed using the great-circle distance formula (see Online Appendix B for details).

### *Industry relatedness*

The industry similarity of the two merging companies might affect their level of information asymmetry and synergistic complementarities (Capron & Shen, 2007; Chatterjee, 1986; Coff, 1999). We control for this factor with the dummy *unrelated*, which indicates whether the acquirer and the target have different primary two-digit SIC codes.

### *Board interlock*

The presence of a board interlock may provide additional informational benefits that could affect the acquirer's returns (Cai & Sevilir, 2012). We include the dummy *board interlock*, which indicates whether the two companies share a common director. Data on boards of directors are obtained from Capital IQ Professional.

### *Previous alliance*

The presence of a preexisting partnership between the merging companies could also provide informational benefits (Porrini, 2004; Zaheer et al., 2010) and possibly correlate with inter-company visits. Hence, we include the dummy *previous alliance*, which equals one if the two merging companies announced an alliance in the 5 years before the acquisition announcement. Data on alliances are obtained from SDC.

### *Ownership position*

A partial ownership position in the target could provide an informational advantage to the acquirer or affect the intensity of competition in the bidding process (Betton, Eckbo, & Thorburn, 2009; Schijven & Hitt, 2012). Hence, we include the dummy variable *toehold*, indicating whether the acquirer held a stake in the target before the announcement.

### *Previous bids for the target*

The bargaining power of the target is expected to be higher if the target has ongoing offers from other bidders. Hence, we control for the number of bids the target has received from other bidders in the year before the announcement (labeled *previous bids*).

### *Target relative size*

Larger transactions could impose greater risks on the acquirer (Hansen, 1987) and increase managers' incentive to perform due diligence on the target. We include the variable *target relative size*, which is the ratio of the deal value to the sum of the deal value and the acquirer's market capitalization, computed at the end of the fiscal year before the deal.

### *Stock payment*

Stock payments usually induce negative stock market reactions, as they may signal that the acquirer's stock is overvalued or that the transaction is riskier for the acquirer (e.g., Hansen, 1987). Hence, we include the dummy variable *stock payment*, which equals one if the payment included stock considerations and zero otherwise.[12]

*High-tech target*

Targets in knowledge-intensive sectors can be more informationally opaque, since their value depends more on intangible assets (Capron & Shen, 2007; Coff, 1999). Hence, we include the dummy *high-tech target*, which indicates if the target's primary four-digit SIC code is a high-tech sector, as defined by the American Electronics Association (Walcott, 2000).

*Private target*

Acquisitions of private targets are generally found to be more profitable for acquirers, since private firms can be acquired at a discount due to their greater informational opacity (Capron & Shen, 2007). Hence, we include a dummy *private target* that equals one if the target is a private firm and zero if it is a public firm or a subsidiary of a public firm in SDC.

*Other controls*

We control for the size of the acquirer with the logarithm of total assets before the announcement, denoted by *log*(*assets*). We also control for other financial characteristics of the acquirer, measured at the end of the fiscal year before the announcement. For the subset of public targets, we include financial characteristics for the target as well. *Tobin's Q* is the market value of assets divided by the book value of assets. The market value of assets is measured as the market value of equity plus the book value of assets net of the book value of equity and deferred taxes (Kaplan & Zingales, 1997). *ROA* is return on assets. *Leverage* is the ratio of total debt to total assets. We winsorize these financial ratios at the 2nd and 98th percentiles. Moreover, to control for the size of the two companies' smartphone-user base, we include the average number of smartphones that appear during business hours in the acquirer's and the target's locations (*average number smartphones*). To control for the unobservable heterogeneity of industries, we include the acquirers' industry fixed effects, where industries are defined at the two-digit SIC code level. To account for potential idiosyncrasies of firms' locations, in some specifications we include acquirers' and targets' CBSA fixed effects. Finally, because the coverage of the data collected by SafeGraph increases over time, we include month-year or quarter-year fixed effects.

# 4 | RESULTS

## 4.1 | Baseline results

Table 1 presents the descriptive statistics of the variables introduced in the previous section. Table 2 provides the descriptive statistics for the number of intercompany visits. We observe at least one intercompany visit for 101 transactions (43% of the sample), including 79 acquirers visiting their target, and 71 targets visiting their acquirer. Among the transactions with at least one intercompany visit, on average, we observe one smartphone per visit and approximately 11 intercompany visits. The bottom of the table shows how the number of visits observed in a calendar month preceding the announcement changes as we move closer to the announcement month. Because the coverage of the smartphone data increases over time, we adjust the monthly number of visits by the average in that calendar month among all deals in the sample

[12]Since the method of payment can be an outcome of the quality of the negotiation, it might be endogenous to visits. Hence, it might capture some of the effect of our key predictor. However, results do not change substantially by removing this variable.

**TABLE 1** Descriptive statistics and correlation matrix

| | | *N* | Mean | *SD* | Min | Max | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| (1) | *Acquirer's CAR* (%) | 234 | 1.39 | 8.44 | −19.67 | 25.59 | | | | | | | | | |
| (2) | *Total CAR* (%) | 97 | 2.61 | 5.47 | −11.26 | 14.87 | .75 | | | | | | | | |
| (3) | Δ*CAR* (%) | 97 | 2.89 | 5.22 | −4.18 | 18.22 | −.72 | −.18 | | | | | | | |
| (4) | *Later bids* | 234 | 0.05 | 0.29 | 0 | 3 | −.08 | −.13 | .16 | | | | | | |
| (5) | *Number of visits* | 234 | 4.76 | 13.71 | 0 | 70 | .10 | .04 | −.08 | .02 | | | | | |
| (6) | *Distant target* | 234 | 0.82 | 0.38 | 0 | 1 | −.01 | .09 | −.14 | −.04 | −.47 | | | | |
| (7) | *Unrelated* | 234 | 0.33 | 0.47 | 0 | 1 | .08 | .08 | −.20 | −.09 | .02 | .06 | | | |
| (8) | *Board interlock* | 234 | 0.06 | 0.24 | 0 | 1 | −.07 | .00 | .13 | .02 | −.08 | .02 | −.03 | | |
| (9) | *Previous alliance* | 234 | 0.02 | 0.14 | 0 | 1 | −.06 | −.14 | −.11 | −.03 | −.02 | −.01 | .02 | .09 | |
| (10) | *Toehold* | 234 | 0.04 | 0.19 | 0 | 1 | −.07 | .02 | .01 | −.04 | .04 | .03 | .05 | .14 | −.03 |
| (11) | *Previous bids* | 234 | 0.03 | 0.18 | 0 | 1 | .05 | −.09 | −.08 | −.03 | −.02 | .09 | −.08 | .05 | .13 |
| (12) | *Target relative size* | 234 | 0.23 | 0.21 | 0.01 | 0.97 | .13 | .04 | .39 | .01 | .22 | −.03 | .06 | .08 | .06 |
| (13) | *Stock payment* | 234 | 0.39 | 0.49 | 0 | 1 | −.11 | −.13 | .40 | −.05 | .17 | −.07 | −.02 | .17 | .00 |
| (14) | *High-tech target* | 234 | 0.28 | 0.45 | 0 | 1 | −.07 | .00 | −.04 | .06 | −.02 | −.02 | −.07 | −.04 | −.03 |
| (15) | *Private target* | 234 | 0.46 | 0.50 | 0 | 1 | .10 | | | −.02 | −.02 | .04 | .11 | −.23 | −.14 |
| (16) | *Log(acquirer assets)* | 234 | 7.81 | 2.03 | 2.54 | 12.91 | −.22 | −.25 | −.14 | .09 | −.02 | .00 | −.16 | .00 | .22 |
| (17) | *Acquirer Tobin's Q* | 234 | 1.97 | 0.98 | 0.73 | 5.05 | .05 | .08 | −.12 | −.05 | .00 | −.05 | −.07 | .07 | −.08 |
| (18) | *Acquirer ROA* | 234 | 0.01 | 0.14 | −0.66 | 0.21 | −.10 | .11 | .04 | .00 | −.20 | .06 | −.09 | −.09 | .03 |
| (19) | *Acquirer leverage* | 234 | 0.54 | 0.22 | 0.07 | 1.00 | −.11 | −.14 | .01 | .04 | −.04 | .09 | −.05 | .00 | .04 |
| (20) | *Avg. num. smartphones* | 234 | 69.14 | 144.88 | 1.15 | 1,093.7 | −.05 | .05 | −.09 | .09 | .34 | .07 | .01 | −.10 | .08 |
| (21) | *Target management ownership* (%) | 97 | 11.18 | 13.12 | 0.16 | 57.4 | .02 | .08 | .09 | .05 | −.04 | .00 | −.23 | −.15 | −.16 |
| (22) | *Target Tobin's Q* | 97 | 2.23 | 1.50 | 0.76 | 8.51 | .09 | −.04 | −.11 | −.12 | −.05 | .02 | −.04 | −.18 | .15 |
| (23) | *Target ROA* | 97 | −0.04 | 0.21 | −0.76 | 0.28 | .11 | .18 | −.02 | −.04 | .10 | −.02 | .15 | −.27 | .08 |
| (24) | *Target leverage* | 97 | 0.56 | 0.27 | 0.09 | 1.00 | −.12 | −.02 | .14 | −.04 | .02 | .11 | .09 | .05 | .03 |

**TABLE 1** (Continued)

| | | (10) | (11) | (12) | (13) | (14) | (15) | (16) | (17) | (18) | (19) | (20) | (21) | (22) | (23) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | (10) | (11) | (12) | (13) | (14) | (15) | (16) | (17) | (18) | (19) | (20) | (21) | (22) | (23) |
| (11) | *Previous bids* | −.04 | | | | | | | | | | | | | |
| (12) | *Target relative size* | −.08 | −.02 | | | | | | | | | | | | |
| (13) | *Stock payment* | −.02 | .04 | .45 | | | | | | | | | | | |
| (14) | *High-tech target* | −.07 | −.01 | −.07 | −.06 | | | | | | | | | | |
| (15) | *Private target* | −.05 | −.08 | −.12 | −.18 | −.11 | | | | | | | | | |
| (16) | *Log(acquirer assets)* | .09 | .09 | −.34 | −.07 | .06 | −.40 | | | | | | | | |
| (17) | *Acquirer Tobin's Q* | −.08 | .11 | −.29 | −.12 | .13 | .10 | −.14 | | | | | | | |
| (18) | *Acquirer ROA* | .04 | −.06 | −.23 | −.21 | .08 | −.14 | .43 | −.05 | | | | | | |
| (19) | *Acquirer leverage* | .09 | .10 | .01 | −.01 | −.08 | −.23 | .40 | −.21 | .20 | | | | | |
| (20) | *Avg. num. smartphones* | .02 | .21 | .11 | .09 | .04 | −.06 | .12 | −.03 | −.06 | .05 | | | | |
| (21) | *Target management ownership* (%) | −.08 | −.07 | −.05 | .09 | −.03 | | −.16 | .00 | .06 | −.01 | −.01 | | | |
| (22) | *Target Tobin's Q* | −.02 | .09 | −.27 | −.18 | .21 | | .37 | .10 | .16 | −.04 | .08 | −.07 | | |
| (23) | *Target ROA* | −.01 | −.14 | .23 | .10 | .10 | | .11 | −.13 | .43 | .20 | −.04 | −.07 | −.19 | |
| (24) | *Target leverage* | .06 | −.01 | .19 | .16 | −.21 | | .13 | −.27 | .20 | .48 | −.03 | .06 | −.23 | .14 |

*Note*: The full sample includes 234 deals with public and private targets. The target's financial and management ownership variables are defined for 97 deals involving a public target with data in Compustat-CRSP (correlations of these variables with *private target* are missing since *private target* is a constant = 0 for these deals).
Abbreviations: CAR, cumulative abnormal returns; ROA, return on assets.

**TABLE 2** Intercompany visits descriptive statistics

| | Intercompany visits (acquirer's or target's visits) | | Acquirer's visits to the target | | Target's visits to the acquirer | |
|---|---|---|---|---|---|---|
| Observations with at least 1 visit | 101 | | 79 | | 71 | |
| **For the subsample with at least 1 visit** | | | | | | |
| | **Mean** | ***SE*** | **Mean** | ***SE*** | **Mean** | ***SE*** |
| Number of people per visit | 1.26 | 0.14 | 1.34 | 0.18 | 1.36 | 0.20 |
| Number of visits | 11.04 | 1.91 | 8.29 | 1.48 | 8.27 | 1.40 |
| Adjusted number of visits by month (month of M&A announcement = 0) | | | | | | |
| −1 | 0.68 | 0.40 | 0.38 | 0.39 | 0.79 | 0.46 |
| −2 | 0.49 | 0.42 | 0.42 | 0.41 | 0.46 | 0.46 |
| −3 | 0.38 | 0.41 | 0.40 | 0.40 | 0.39 | 0.41 |
| −4 | 0.26 | 0.39 | 0.38 | 0.40 | 0.04 | 0.40 |
| −5 | 0.05 | 0.35 | 0.01 | 0.36 | 0.01 | 0.37 |
| −6 | −0.14 | 0.35 | −0.14 | 0.33 | −0.17 | 0.36 |
| −7 | −0.30 | 0.35 | −0.24 | 0.39 | −0.35 | 0.36 |
| −8 | −0.69 | 0.28 | −0.59 | 0.28 | −0.63 | 0.33 |

*Note*: The columns with intercompany visits include observations with either visits by the acquirer's employees to the target's HQs or visits by the target's employees to the acquirer's HQs. Adjusted number of visits by month is the difference between the number of visits in a calendar month and the average number of visits observed in that calendar month among all deals in the sample. For example, for an announcement in February 2017, the adjusted number of visits in January 2017 (month −1) is the difference between the number of visits in January 2017 and the average number of visits observed in January 2017 among all deals.

Abbreviations: HQs, headquarters; M&A, mergers and acquisitions.

(see table notes for details). As the table indicates, visits are more likely to occur in the months closer to the announcement, and they typically occur in the 3 or 4 months preceding the announcement. Similar patterns are observed in the subsamples of acquirers' visits to the target and targets' visits to the acquirer. Lee and Yerramilli (2019) collect information on deal initiation months from SEC forms for a sample of M&As over the period 1985–2015. They report that the median lag between deal initiation and public announcement is 3 months (the 25th and 75th percentiles are 2 and 6 months, respectively), which is consistent with the timing of visits we observe in Table 2.

The SafeGraph data that we use do not cover the full population of smartphones in the United States. Moreover, when smartphones or their geolocation signals are switched off, people's movements are unobservable. The partial coverage of smartphone data implies that intercompany visits are observed with an error: some visits may not be observed with our data. In Online Appendix A, we provide evidence that the smartphone data coverage is similar across locations and that firms for which we do and do not observe intercompany visits are similar in terms of observable characteristics. Hence, we believe it is safe to assume the error in the measurement of intercompany visits to be random in our baseline regressions and we expect the measurement error to increase the *SE* in our regressions.

Table 3 shows how the average number of visits and the percentage of deals with at least one observed visit vary depending on whether merging companies are located in the same

**TABLE 3** Distance between merging companies and visits

| | Number of visits | | | |
|---|---|---|---|---|
| | **Observations** | **Mean** | ***SE*** | **% of deals with visits >0** |
| Same CBSA | 32 | 21.50 | 4.71 | 90.63 |
| Different CBSAs | 202 | 2.11 | 0.53 | 35.64 |
| Same state | 54 | 13.87 | 3.17 | 74.07 |
| Different states | 180 | 2.03 | 0.53 | 33.89 |
| Travel time ≤1 hr | 35 | 21.40 | 4.46 | 94.29 |
| Travel time in (1, 3] hr | 32 | 3.16 | 1.34 | 40.63 |
| Travel time >3 hr | 167 | 1.59 | 0.50 | 32.93 |

Abbreviation: CBSAs, core-based statistical areas.

CBSA or state, or depending on the travel time between companies. To calculate travel time, we follow Giroud (2013) (see Online Appendix B for details). The table indicates that visits are less frequent when merging companies are in different CBSAs or states and when the travel time between companies increases. This pattern corroborates the prediction of previous studies that geographical distance reduces face-to-face interactions (e.g., Chakrabarti & Mitchell, 2013, 2016; Ragozzino & Reuer, 2011; Uysal et al., 2008). However, the table also highlights that visits are observed not only in local deals, but also when companies are in different CBSAs, in different states, or located more than 2 or 3 hr from each other.

Table 4 reports the baseline OLS regressions showing how the average acquirer's CAR changes with the number of visits. The sample includes acquisitions of both public and private targets. Model (1) includes the control variables with acquirer industry and month-year fixed effects, and model (2) adds the variable *number of visits*. To test whether the effect of visits is driven by firm-location idiosyncrasies, model (3) includes acquirer's and target's CBSA fixed effects. In line with Hypothesis (H1), the coefficient of *number of visits* in both models (2) and (3) shows that more visits are associated with higher returns to the acquirer ($p$-value = .026 in model (2) and .006 in model (3)). In terms of magnitude, the coefficients in models (2) and (3) indicate that for each *SD* increase in the number of visits, the average acquirer's CAR increases by approximately 1.7 and 2.5 percentage points, respectively.

In Online Appendix C, we provide robustness tests that confirm a positive relation between *number of visits* and the acquirer's CAR, including regressions with alternative specifications for the geographic distance control or omitting it (Table S2), and regressions controlling for potential differences in data coverage over locations and months (Table S3). Table S2 also reports regressions including the logarithm of the number of visits plus one or a dummy indicating whether at least one visit is observed as alternative specifications for *number of visits*. The table reports both OLS regressions on the acquirer's CAR and logit regressions on the probability that the acquirer's CAR is positive. In both OLS and logit regressions, the measure of visits ($log(number\ of\ visits + 1)$ or $number\ of\ visits > 0$) has a positive effect, confirming the results of Table 4. The OLS regressions indicate that a 1% increase in $number\ of\ visits + 1$ increases the average acquirer's CAR by 0.03 percentage points ($p$-value of $log(number\ of\ visits + 1) = .004$) and that in deals with at least one observed visit the average acquirer's CAR is 1.35 percentage points higher ($p$-value of $number\ of\ visits > 0 = .495$). The $p$-value of the dummy $number\ of\ visits > 0$ is considerably larger since the variability in the number of visits

**TABLE 4** Regressions on acquirer's CAR

| | *Acquirer's CAR (%)* | | |
|---|---|---|---|
| **Dependent variable** | **(1)** | **(2)** | **(3)** |
| *Number of visits* | | 0.13 | 0.18 |
| | | (0.06) | (0.06) |
| *Distant target* | −0.53 | 1.35 | 1.71 |
| | (1.50) | (1.45) | (1.87) |
| *Unrelated* | 0.91 | 0.96 | −1.29 |
| | (1.35) | (1.25) | (2.08) |
| *Board interlock* | −2.01 | −1.67 | −0.32 |
| | (3.03) | (2.94) | (3.54) |
| *Previous alliance* | −1.75 | −0.80 | −5.85 |
| | (3.93) | (4.21) | (5.81) |
| *Toehold* | −1.39 | −1.95 | −4.73 |
| | (2.30) | (2.34) | (3.07) |
| *Previous bids* | 5.65 | 5.84 | −0.64 |
| | (3.88) | (3.85) | (6.48) |
| *Target relative size* | 10.52 | 9.30 | 11.60 |
| | (6.01) | (5.93) | (4.60) |
| *Stock payment* | −3.79 | −3.96 | −1.84 |
| | (1.41) | (1.50) | (2.56) |
| *High-tech target* | 0.13 | 0.15 | −2.97 |
| | (2.04) | (2.06) | (1.91) |
| *Private target* | 0.52 | 0.34 | 2.12 |
| | (1.07) | (1.12) | (2.62) |
| *Log(acquirer assets)* | −0.52 | −0.58 | −0.25 |
| | (0.34) | (0.35) | (0.48) |
| *Acquirer Tobin's Q* | 0.58 | 0.54 | −0.51 |
| | (0.79) | (0.83) | (1.03) |
| *Acquirer ROA* | 0.84 | 2.31 | 3.12 |
| | (7.06) | (6.86) | (6.36) |
| *Acquirer leverage* | −0.41 | −0.13 | −0.14 |
| | (3.41) | (3.45) | (5.59) |
| *Avg. num. smartphones* | 0.00 | −0.00 | −0.01 |
| | (0.00) | (0.00) | (0.01) |
| Fixed effects | | | |
| Month-year | Yes | Yes | Yes |
| Acquirer industry | Yes | Yes | — |
| Acquirer CBSA | — | — | Yes |

**TABLE 4** (Continued)

| | *Acquirer's CAR* (%) | | |
|---|---|---|---|
| **Dependent variable** | **(1)** | **(2)** | **(3)** |
| Target CBSA | — | — | Yes |
| $R^2$ | 0.307 | 0.325 | 0.587 |
| *N* | 234 | 234 | 234 |

*Note*: *SE* (in parentheses) are two-way-clustered by acquirer CBSA and target CBSA.
Abbreviations: CAR, cumulative abnormal returns; CBSA, core-based statistical area; ROA, return on assets.

is largely ignored and therefore the effect of visits can be estimated less precisely. The logit regressions confirm that the measures of visits have a positive effect on the probability that the acquirer's CAR is positive (average marginal effect of $log(number\ of\ visits + 1) = .12$ and *p*-value of the coefficient = .011; average marginal effect of $number\ of\ visits > 0 = .17$ and *p*-value of the coefficient = .041).

Table 5 reports the tests of Hypotheses (H2)–(H4). Model (1) is the Poisson pseudo-likelihood regression of the number of bids the target receives from other bidders after the announcement of the focal deal. The model is estimated with the full sample of M&A announcements, including public and private targets. In this regression, we control for changes in smartphone-data coverage over time with quarter-year fixed effects instead of month-year fixed effects, since several months perfectly predict zero later bids for targets. The result indicates that more frequent visits are associated with a lower number of later bids (*p*-value of *number of visits* = .048), in line with Hypothesis (H2). The average marginal effect reveals that for each *SD* increase in visits, the expected number of later bids for the target drops by 0.04. Models (2)–(4) in Table 5 are the OLS regressions on $\Delta CAR$ and *total CAR*. These models are estimated using the subset of deals with public targets, and include the control variables for the deal, the acquirer's and the target's financial characteristics, and acquirer industry and month-year fixed effects. In line with Hypothesis (H3), model (2) indicates that the fraction of value that is appropriated by the target drops with the number of intercompany visits: for each *SD* increase in visits, the target appropriates about 4.9 cents less for every dollar of pre-merger market value of equity of the merging firms (*p*-value = .044). Consistent with Hypothesis (H4), model (3) shows that this effect is weaker the larger the share of the target owned by its managers (*p*-value of the interaction = .048). Finally, model (4) indicates that the total abnormal gains of the two merging companies combined are not strongly related to the *number of visits* (*p*-value = .252). Tables S4 and S5 in Online Appendix C show that all results of Table 5 continue to hold by replacing *number of visits* with $log(number\ of\ visits + 1)$. Considering the $number\ of\ visits > 0$ dummy, Table S4 indicates that the average number of later bids for the target is lower in the subset of deals with at least one observed visit. Table S5 shows that in this subset, targets capture less value when the target management's ownership is small. Table S7 in Online Appendix C shows that the negative relationship between the number of visits and the target's gains also emerges when using the target's CAR or the acquisition premium as alternative measures of the target's gains. However, since the target's CAR or the premium can change as a function of either the target's value capture vis-à-vis the acquirer or the total value created by the deal, these regressions should be considered a more limited test of Hypothesis (H3).

**TABLE 5** Regressions on the number of later bids for the target, ΔCAR, and total CAR

| Model | (1) Poisson pseudo-likelihood | (2) OLS | (3) | (4) OLS |
|---|---|---|---|---|
| **Dependent variable** | ***Later bids*** | **Δ*CAR* (%)** | | ***Total CAR* (%)** |
| *Number of visits* | −0.05 | −0.36 | −0.72 | −0.27 |
| | (0.03) | (0.17) | (0.26) | (0.23) |
| *Number of visits × target management ownership* (%) | | | 0.02 | |
| | | | (0.01) | |
| *Distant target* | −1.40 | −4.60 | −4.75 | −0.98 |
| | (0.67) | (2.62) | (2.44) | (2.93) |
| *Unrelated* | −1.57 | 1.74 | 2.39 | −0.82 |
| | (1.53) | (1.95) | (2.21) | (2.41) |
| *Board interlock* | 1.75 | −2.83 | −2.97 | 5.91 |
| | (1.62) | (2.92) | (2.96) | (2.92) |
| *Previous alliance* | −19.07 | −5.43 | −5.35 | −2.84 |
| | (1.47) | (4.04) | (4.03) | (6.07) |
| *Toehold* | −16.96 | 1.20 | 0.70 | 0.47 |
| | (0.52) | (3.75) | (3.93) | (3.99) |
| *Previous bids* | −16.86 | 1.49 | 1.71 | −7.55 |
| | (1.34) | (4.41) | (4.35) | (3.39) |
| *Target relative size* | 3.40 | 2.55 | 2.14 | 5.71 |
| | (2.97) | (6.86) | (7.06) | (7.16) |
| *Stock payment* | −2.16 | 9.68 | 9.84 | −5.64 |
| | (1.56) | (2.74) | (2.71) | (3.34) |
| *High-tech target* | 0.85 | 0.31 | 0.50 | −2.84 |
| | (0.96) | (1.81) | (1.91) | (3.01) |
| *Private target* | 0.41 | | | |
| | (0.66) | | | |
| *Log(acquirer assets)* | 0.40 | 0.17 | 0.22 | −0.46 |
| | (0.22) | (0.52) | (0.51) | (0.72) |
| *Acquirer Tobin's Q* | −0.22 | −0.53 | −0.33 | −0.72 |
| | (0.18) | (0.95) | (0.83) | (1.02) |
| *Acquirer ROA* | −3.14 | 18.78 | 19.04 | −11.36 |
| | (4.36) | (10.48) | (10.66) | (13.87) |
| *Acquirer leverage* | 0.87 | −9.90 | −8.16 | −0.82 |
| | (2.56) | (5.49) | (5.83) | (7.63) |
| *Avg. num. smartphones* | 0.01 | 0.01 | 0.01 | 0.02 |

**TABLE 5** (Continued)

| Model | (1) Poisson pseudo-likelihood | (2) OLS | (3) | (4) OLS |
|---|---|---|---|---|
| **Dependent variable** | ***Later bids*** | **ΔCAR (%)** | | ***Total CAR* (%)** |
| | (0.00) | (0.01) | (0.01) | (0.01) |
| *Target management ownership* (%) | | 0.01 | −0.02 | 0.06 |
| | | (0.05) | (0.05) | (0.08) |
| *Target Tobin's Q* | | −2.00 | −2.14 | 1.12 |
| | | (0.75) | (0.77) | (1.00) |
| *Target ROA* | | −9.61 | −11.02 | 17.32 |
| | | (5.58) | (5.43) | (5.42) |
| *Target leverage* | | 0.68 | 0.40 | 4.24 |
| | | (2.71) | (2.95) | (2.51) |
| Fixed effects | | | | |
| Quarter-year | Yes | — | — | — |
| Month-year | — | Yes | Yes | Yes |
| Acquirer industry | — | Yes | Yes | Yes |
| $R^2$ | | 0.686 | 0.697 | 0.563 |
| Pseudo $R^2$ | 0.246 | | | |
| *N* | 234 | 97 | 97 | 97 |

*Note*: *SE* (in parentheses) are two-way-clustered by acquirer CBSA and target CBSA.
Abbreviations: CAR, cumulative abnormal returns; CBSA, core-based statistical area; OLS, ordinary least squares; ROA, return on assets.

## 4.2 | Complementary analyses on the mechanism of social interactions

We conduct several additional analyses to test if our theorized mechanism of social interactions is the main driver of our findings. The results are generally consistent with our theory.

### 4.2.1 | Time to completion and probability of target refusal

If frequent social interactions increase the willingness of the target's management to agree to the acquisition, we may observe a faster negotiation process after the announcement and a lower chance of deal cancellation due to disagreement with the target's management. Disagreement over the price or the integration strategy is likely to result in longer negotiations and delays in the closure of the deal. Moreover, acquisitions are sometimes not completed after being announced due to the target's refusal of the offer, management conflicts over the assignment of top positions, or the inability to conclude the negotiations (e.g., Savor & Lu, 2009). First, we measure the number of days between the announcement and the completion of the

deal. Table S10 in Online Appendix C reports regressions showing that the number of days between announcement and completion drops with the number of intercompany visits in the pre-announcement period. This result suggests that when there are frequent social interactions between the managers of the two merging firms in the months before the announcement, the negotiation process after the announcement is smoother.

Second, for the subset of deals in our sample that are withdrawn, we verified the cause of deal cancellation in press releases (e.g., see Savor & Lu, 2009) and recorded whether the target's poor collaboration in the negotiation or its refusal of the offer was identified as the cause (see Online Appendix C for details). In our sample, 11 deals were canceled, 6 of which appeared to be canceled due to the target. Table S10 reports logit regressions providing some evidence that the probability of withdrawal due to the target drops with the frequency of visits. However, given the small number of cases with target's refusal, this result should be interpreted with caution.

### 4.2.2 | Leadership of the merged company

Our theory suggests that social interactions increase the acquirer's appeal by gaining the trust of the target's management. When a friendly relationship between the managers of two merging companies arises, the probability that the managers will agree to share the leadership of the merged entity may be higher. To test this conjecture, for each deal in the subset of acquisitions of public targets that are eventually completed (86 deals), we checked the companies' SEC filings to determine whether the target's CEO joined the board of directors of the merged company after the acquisition (e.g., see Hartzell et al., 2004; Qiu et al., 2014). Table S11 in Online Appendix C shows that in the subset of deals with observed visits, 24.3% of the target CEOs join the new board, while this percentage is 10.2 in the subset of deals with no observed visits ($p$-value of the $z$-test of difference in proportions = .079). Table S12 reports logit regressions showing that, after controlling for the covariates, the probability that the target's CEO joins the new board is 0.28 higher in the subset of deals with observed visits ($p$-value of *number of visits* > 0 in the logit regression = .049).

### 4.2.3 | The moderating effects of the target's ownership concentration and private status

Hypothesis (H4) considers the target's management ownership share as a mitigator of the agency problem between the target's shareholders and management. Agency problems may also be mitigated when the ownership of a company is concentrated, given the better owner-manager monitoring (e.g., Bargeron et al., 2008; Feldman et al., 2019; Shleifer & Vishny, 1986). Hence, we test whether the concentration of ownership of the target moderates the effect of social interactions. For public targets, we collected data on the percentage of the target owned by non-insider beneficial owners (excluding executives and directors) from the latest proxy statement before the acquisition announcement. We recorded the percentage owned by the largest non-insider owner as a measure of concentration. Table S13 reports the regressions on $\Delta CAR$ (%). The table shows that, while the percentage owned by the largest owner does not moderate the effect of *number of visits*, it weakens the negative effect of *log*(*number of visits* + 1) ($p$-value of the interaction = .067) and of the *number of visits* > 0 dummy ($p$-value of the

interaction = .008). Similar results are obtained when using the Herfindahl–Hirschman index of ownership concentration (Table S14).

Owner-manager agency problems are generally considered to be more serious in public companies than in private ones, given the diffused ownership of public companies (e.g., Bargeron et al., 2008; Jensen, 1986). Hence, in the full sample with both private and public targets, we test whether the positive effect of intercompany visits on the acquirer's returns is weaker with private targets. Table S15 reports the regression results. The table shows that the positive effects of *number of visits* and of *log*(*number of visits* + 1) on the acquirer's CAR (found in Table 4 and Table S2, respectively) are close to null when the target is private. Overall, these results suggest that the target's ownership concentration and private status can be other important factors reducing the effect of social interactions on the acquirer's value appropriation.

## 4.3 | Causal interpretation and generalizability to a larger sample

The results presented above suggest that frequent social interactions with the target in the months before the acquisition allow the acquirer to appropriate more value from the deal, most likely by mitigating the competitive pressure it faces in the bidding process. However, before we can draw final conclusions, we need to address several limitations of the analysis. First, managers' propensity to visit the merging partner before the acquisition may be a function of the gains they expect to obtain from the deal. For instance, cultural compatibility or synergies between the merging companies could affect the frequency of visits as well as the returns from the acquisition. The potential endogeneity of visits could result in biased coefficient estimates. Second, because the smartphone data do not cover the full population of smartphones, there is an unavoidable error in the measurement of intercompany visits. While we show that the samples of deals for which we do and do not observe at least one visit are similar in terms of observable characteristics (see Online Appendix A), unobservable idiosyncrasies in smartphone data coverage might bias our coefficient estimates. Third, the availability of smartphone data limits our analysis to a period of less than 2 years, raising questions about the generalizability of our results to a larger sample covering a longer period. Fourth, the observed patterns may be due to a sample selection effect rather than to a treatment effect (Certo, Busenbark, Woo, & Semadeni, 2016). Below we describe our methodology to address these four limitations.

To address the potential endogeneity of intercompany visits arising from managers' selective decisions to visit or from errors in measurement, we adopt an instrumental variable approach. Specifically, we rely on two alternative instruments for intercompany visits that are exogenous to companies and are observed for each deal. The first instrument is the presence of flight routes (either direct or one connection, denoted by a dummy variable *flight*) between the CBSAs where the two merging firms are located, as computed in Testoni (2019).[13] The presence of flights facilitates managers' visits to the other firm by reducing travel time and does not depend on managers' will (Bernstein et al., 2016; Giroud, 2013). The second instrument is the abnormal number of days without snowfall (*days without snowfall*) in the target's CBSA in the 8 months before the acquisition is announced, calculated as the difference between the number of days without snowfall and the mean of that in the previous 10 years. Snowfalls disrupt travel due to

[13]Flights can reduce the travel time between distant locations. When firms are near to each other, travel time is the lowest and yet no flight route exists. To account for the fact that flights are only relevant for distant firms, the first-stage regressions include the flight variable as a moderator of the effect of the geographic distance between firms.

traffic and flight delays and thus make mobility between two firms more difficult. Hence, face-to-face interactions are likely to be more frequent between firms that are merging after periods with less frequent snowfalls. Since abnormal snowfalls are relevant only to the areas that usually receive snowfalls, we use the flight measure as our primary instrument and the snowfall measure as a complementary instrument. Online Appendix B reports the details on the instruments, and Online Appendix C (Tables S8 and S16) shows their descriptive statistics.

Table 6 shows the first and second stages of the two-stage least-squares (2SLS) regression on the acquirer's CAR in the 2016–2018 sample, where we use *flight* as an exogenous source of variation for the number of intercompany visits. The regressions include fixed effects for month-year, the acquirer's CBSA, and the target's CBSA. Given the small sample, we do not have enough statistical power to include CBSA-pair fixed effects. Since the presence of flights is expected to increase the number of visits as the geographic distance between merging companies increases, in the first stage we include the interaction between the *flight* dummy and the logarithm of the straight-line distance between the merging firms' coordinates, denoted by *log* (*distance*). The first-stage regression indicates that for each 1% increase in distance between merging firms, the average number of visits drops by 0.11 ($p$-value of *log*(*distance*) < .001). The *flight* dummy has a positive effect on visits for distant deals: for each 1% increase in the distance between merging companies, the presence of flights increases the average number of visits by 0.11 ($p$-value of the interaction <.001). The $F$-test of the excluded instruments shows that the first-stage effect is relevant ($p$-value of the $F$-test <.001). The second-stage regression confirms that the number of visits positively affects the acquirer's CAR ($p$-value = .016). The coefficient indicates that a *SD* increase in *number of visits* increases the acquirer's CAR by about 6.1 percentage points. This coefficient is larger than the comparable OLS coefficient reported in Table 4, model (3), which suggests that endogeneity may induce a downward bias (i.e., acquirers may visit less profitable targets more). Table S9 in Online Appendix C shows that results are similar using *log*(*number of visits* + 1) as an alternative endogenous variable or adding the snowfall instrument.

While we can measure visits with smartphone data only for recent years, the two instruments can be measured for any deal present in the SDC database. Hence, we can run reduced-form instrumental variable regressions on a larger sample by replacing the number of visits in the main regression with the instruments (e.g., Bernstein et al., 2016; Giroud, 2013; Levitt, 1996). The larger sample provides us with more statistical power to control for high-dimensional fixed effects. Specifically, unlike in our 2SLS regression, we can include CBSA-pair fixed effects, which allows us to compare merging firms from the same pair of locations over time, as airline routes can be added or removed and weather conditions can change. In addition, the longer sample period also allows us to test the generalizability of our findings to a broader sample. We report a summary of this analysis here and provide a complete description in Online Appendix C.

Applying the same deal selection criteria described in Section 3, we obtain a sample of 3,464 deals between public companies that are announced between 1980 and 2019. On this sample, we estimate how the ease of interaction between merging companies affects value appropriation. Specifically, we estimate the following linear model:

$$\Delta CAR_{ij} = \alpha_{year(ij)} + \alpha_{CBSA(i)\times CBSA(j)} + \alpha_{industry(i)} + \alpha_{industry(j)} + \beta z_{ij} + \lambda X_{ij} + \varepsilon_{ij}, \quad (1)$$

where the dependent variable is the $\Delta CAR$ between acquirer $i$ and target $j$; $\alpha_{year(ij)}$ is a year-specific fixed effect; $\alpha_{CBSA(i)\times CBSA(j)}$ is a fixed effect specific to deals with acquirers in the same

**TABLE 6** 2SLS regression on acquirer's CAR

| | (1) | (2) |
|---|---|---|
| **2SLS model** | **1st stage** | **2nd stage** |
| **Dependent variable** | ***Number of visits*** | ***Acquirer's CAR (%)*** |
| *Flight* | −46.14 | |
| | (10.73) | |
| *Flight* × *log(distance)* | 11.32 | |
| | (2.57) | |
| *Number of visits* | | 0.44 |
| | | (0.17) |
| *Log(distance)* | −10.89 | 1.51 |
| | (2.97) | (0.86) |
| *Unrelated* | −0.41 | −1.27 |
| | (2.09) | (1.87) |
| *Board interlock* | 2.80 | 0.18 |
| | (3.24) | (3.26) |
| *Previous alliance* | −10.97 | −3.60 |
| | (13.72) | (8.48) |
| *Toehold* | −0.81 | −6.57 |
| | (9.24) | (2.80) |
| *Previous bids* | 5.57 | −3.46 |
| | (9.24) | (7.75) |
| *Target relative size* | 6.86 | 9.56 |
| | (8.85) | (3.57) |
| *Stock payment* | 2.53 | −2.24 |
| | (2.74) | (2.59) |
| *High-tech target* | 0.45 | −2.56 |
| | (1.91) | (1.99) |
| *Private target* | −1.47 | 3.37 |
| | (3.43) | (2.19) |
| *Log(acquirer assets)* | 0.25 | −0.38 |
| | (0.58) | (0.61) |
| *Acquirer Tobin's Q* | −0.18 | −0.34 |
| | (1.34) | (1.08) |
| *Acquirer ROA* | −7.47 | 7.25 |
| | (8.48) | (6.56) |
| *Acquirer leverage* | 6.16 | −0.80 |
| | (4.62) | (6.46) |
| *Avg. num. smartphones* | 0.03 | −0.02 |

**TABLE 6** (Continued)

| 2SLS model | (1) 1st stage | (2) 2nd stage |
|---|---|---|
| **Dependent variable** | ***Number of visits*** | ***Acquirer's CAR (%)*** |
| | (0.01) | (0.01) |
| Fixed effects | | |
| Month-year | Yes | Yes |
| Acquirer CBSA | Yes | Yes |
| Target CBSA | Yes | Yes |
| *N* | 234 | 234 |
| *F*-test of excluded instruments | 11.45 | |
| *p*-value of *F*-test | .000 | |

*Note*: *SE* (in parentheses) are two-way-clustered by acquirer CBSA and target CBSA. In the second-stage regression, *number of visits* is instrumented with the *flight* dummy and the interaction between the *flight* dummy and *log*(*distance*).
Abbreviations: CAR, cumulative abnormal returns; CBSA, core-based statistical area; ROA, return on assets; 2SLS, two-stage least-squares.

CBSA of $i$ and targets in the same CBSA of $j$; $\alpha_{industry(i)}$ and $\alpha_{industry(j)}$ are fixed effects for the acquirer's and the target's industry, respectively; $z_{ij}$ is the instrument for intercompany visits (either *flight* or *days without snowfall*), which measures how easy it is for companies $i$ and $j$ to interact before the deal; $X_{ij}$ is a vector of control variables; and $\varepsilon_{ij}$ is the error term.

A final problem is whether the effect of the visits instrument $z$ (the "ease of interaction") in Equation (1) is indeed due to a treatment rather than a sample selection effect (e.g., see Certo et al., 2016). A treatment effect occurs if $z$ affects $\Delta CAR$ holding constant all observable and unobservable characteristics of the deal. However, there might be some unobservable characteristics of the deal (omitted variables) that affect $\Delta CAR$. The instrument $z$ may increase the chances that deals with a lower expected $\Delta CAR$ (due to these unobservable characteristics) are announced. In this case, the effect of $z$ on $\Delta CAR$ would be due to a sample selection effect. To investigate whether the effect of $z$ on $\Delta CAR$ is due to a treatment effect or to a sample selection, we employ an entropy-balancing technique (Hainmueller, 2012) and estimate Equation (1) on a weighted sample that is as similar as possible to a sample of potential (unselected) deals in terms of all observable characteristics (including $z$).[14] In this weighted sample, deal selection is not systematically affected by $z$ or the other covariates. If deal selection is random conditional on $z$ and the other covariates, any sample selection bias should be substantially reduced, which would allow us to consistently estimate the treatment effect of $z$ in Equation (1). In particular, the weights are set so that the mean of $z$ and all the control variables are as close as possible to the mean of the same variables in a sample of unselected deals (see Online Appendix C for details).

[14]A common method to correct for sample selection bias is the Heckman selection model. However, the Heckman model requires an exclusion restriction. To apply the Heckman model in our case, we would need to find a variable that affects the decision to acquire, but not the expected gains from the acquisition. Since most observable variables we can think of would affect both, we opt for a different procedure. We thank Zilin He for suggesting this approach.

Table S16 in Online Appendix C reports the descriptive statistics for the 1980–2019 deals. Table S17 reports the regressions for Equation (1) in the unweighted and weighted samples of deals. The regressions in both samples indicate that the presence of a flight reducing the travel time between merging companies decreases the target's value appropriation. Since selection should not depend on the observable covariates (including the flight variable) in the weighted sample, these results suggest that the presence of a flight affects $\Delta CAR$ through a treatment rather than a sample selection effect. Using *days without snowfall* as an alternative instrument, we obtain similar conclusions. Overall, these instrumental variable approaches reveal that: (a) the negative effect of interactions on the target's value capture also emerges from regressions that include exogenous measures of the ability of firms to interact face-to-face; (b) our key findings generalize to a longer time period; and (c) the observed effects are likely to be driven by a treatment rather than a sample selection effect.

# 5 | DISCUSSION AND CONCLUSION

## 5.1 | Summary

Previous literature highlights how competition in the M&A market limits acquirers' ability to capture value from M&As. We hypothesize that social interactions with the target's management in the pre-acquisition phase make the target's management more willing to cede control to the focal acquirer and less eager to seek alternative bidders. Thus, social interactions should allow the acquirer to appropriate more value from M&As. Measuring intercompany visits with smartphone geolocational data in a sample of deals announced between July 2016 and January 2018, we find that in deals with more frequent pre-announcement interactions, acquirers earn higher abnormal returns at the acquisition announcement and targets receive fewer bids from other bidders after the announcement. In the subset of acquisitions with public targets, more frequent visits are associated with lower abnormal gains to the target relative to the acquirer. This effect is weaker when the target's management owns a larger share of the target, suggesting that the effect of social interactions may depend on agency problems between the target's shareholders and management. We also find that frequent intercompany visits in the pre-acquisition phase are associated with a shorter time between deal announcement and completion and lower chances (albeit in a small sample) that the deal is canceled due to disagreement with the target. These findings suggest that frequent social interactions reduce the chances of a problematic negotiation process with the target. We also find that when intercompany visits are observed, targets' CEOs are more likely to join the new board and therefore to share the leadership of the merged company. Moreover, we find some evidence that the negative effect of visits on the gains of a public target vis-à-vis its acquirer is weaker when the target has a more concentrated ownership structure. Similarly, the positive effect of visits on the acquirer's returns is negatively moderated by the target's private status. These findings indicate that the target's ownership concentration and private status can be other factors that mitigate the effect of social interactions, further suggesting that the observed effects of visits are likely related to an agency problem. To account for the potential endogeneity of intercompany visits, we run instrumental variable regressions by using exogenous measures for the ease of face-to-face interactions between merging companies and confirm that frequent interactions increase the acquirer's returns. Finally, in reduced-form instrumental variable regressions on a larger sample of deals between 1980 and 2019, we find that targets appropriate less value relative to their acquirers when intercompany visits are easier, even after accounting for possible sample selection.

## 5.2 | Alternative mechanisms

While this paper focuses on a single hypothesized mechanism, we acknowledge that alternative mechanisms may explain the effect of intercompany visits on acquisition outcomes. Below we describe alternative mechanisms that do not find empirical support in our results.

### 5.2.1 | Informational advantages to the acquirer

Previous literature has highlighted that acquirers often face severe information asymmetry problems when buying a company (e.g., Chakrabarti & Mitchell, 2013; Coff, 1999; Ragozzino & Reuer, 2011; Zaheer et al., 2010). From the acquirer's perspective, social interactions may provide more information to evaluate the target. More information about the target and the achievable synergies may allow the acquirer to better screen deals and to select deals that create greater overall value (e.g., see Cai & Sevilir, 2012; Uysal et al., 2008). Hence, social interactions could increase the acquirer's returns by improving deal selection, which could be an alternative mechanism consistent with Hypothesis (H1). However, as seen in Table 5 and Table S5, we do not find that visits are associated with greater value creation, as measured by the total returns from the deal. Moreover, if social interactions benefit acquirers by providing more information to select deals, we should expect the effect of visits to be stronger when the acquirer faces greater adverse selection risks, such as with private targets (Capron & Shen, 2007) or with knowledge-intensive firms (Coff, 1999; Uysal et al., 2008). However, Table S15 shows that the effect of visits on the acquirer's returns is actually stronger with public targets. In unreported results, we also do not find that the effect of visits is moderated by the target's knowledge intensity, as proxied by the dummy *high-tech target*.

More private information about the target may also give the acquirer an advantage in the bidding process. If other potential bidders are aware of having an informational disadvantage relative to the focal acquirer, they could avoid bidding for the target or bid less aggressively if they do, allowing the informed acquirer to appropriate more value from the transaction (e.g., Hendricks & Porter, 1988). For instance, previous studies have shown that acquirers that have a board interlock with their target (Cai & Sevilir, 2012) or that are geographically closer (Uysal et al., 2008) are able to appropriate more value from M&As. In our context, it could be that social interactions provide an informational advantage to the focal acquirer relative to other bidders. This could be an alternative mechanism consistent with Hypothesis (H3). Nevertheless, this argument assumes that other bidders are aware of the focal acquirer's preferential access to information. While board interlocks and geographical proximity to the target are publicly observable, intercompany visits are unlikely to be observed by other companies. On the contrary, managers of public companies are likely to keep interactions confidential to avoid information leaks to the stock market.

### 5.2.2 | Information leakage

A possible alternative explanation for the negative relationship between visits and the target's returns at the acquisition announcement (Table 5; Tables S5 and S7) is that visits may cause information leakage regarding the target's acquisition. If managers are not able to keep their interactions confidential, rumors about the prospective acquisition could spread, leading to a

runup in a public target's stock price in the months preceding the announcement. If the market anticipates that the target will be acquired at a premium, the target's gains may already be factored in the target's market price when the acquisition is announced. To test for this possibility, we computed the increase in the target's stock price from 250, 200, 150, 100, and 50 trading days before the announcement to 2 days before the announcement. We do not find evidence that the number of visits is associated with price runups.

#### 5.2.3 | Cash benefits to the target's CEO

Hartzell et al. (2004) indicate that acquirers sometimes compensate the target's CEO with cash payments, in the form of special bonuses or increased golden parachutes, in exchange for a lower acquisition price. In our context, it may be that during intercompany visits the target's CEO negotiates these cash benefits. If so, the greater value capture by acquirers observed with frequent visits might be driven by such benefits. To test how intercompany visits correlate with these benefits, for the subset of deals with public targets, we collected data on targets' CEO monetary compensations from SEC filings, as in Hartzell et al. (2004). Table S11 in Online Appendix C shows that these cash benefits are offered in 35.1% of the deals with observed visits and 40.8% of the deals with no observed visits ($p$-value of the difference in proportions = .592). Logit regressions controlling for the covariates confirm that cash benefits are not more likely to be offered when visits are observed.

### 5.3 | Limitations

Several limitations of our study are worth mentioning. One concern is our measure of social interactions. Because the smartphone data are anonymous, we cannot be certain that the smartphones we identify belong to employees. For instance, people who visited the HQs of both merging companies in the months before the acquisition might be external agents, such as consultants or investment bankers hired to perform due diligence and plan the integration process. If such external agents help establish a better information flow between merging partners, they may increase the target management's confidence in the deal and the integration process, which could be consistent with the observed patterns. Alternatively, these people could be common business connections. Potentially, referral by common business partners could be another channel through which trust between merging firms is built (e.g., Gulati, 1995).

Moreover, our data do not allow us to conclusively define the context in which intercompany visits occur. The data show that interactions disproportionally occur in the few months preceding the acquisition announcement (in line with the timing about deal initiation reported in Lee & Yerramilli, 2019), which could suggest that they are related to the initiation of the acquisition deal. Our results are robust to the inclusion of controls for the presence of a preexisting relationship between companies, including a common director, a prior alliance, and a partial ownership position in the target. While we cannot draw final conclusions on the nature of the observed visits, these patterns suggest that our results can be driven by interactions that occur as part of the deal initiation process.

Finally, while our instrumental variable analysis provides suggestive evidence of causality, we would like to further acknowledge that such evidence relies on the validity of the exclusion

restrictions of our instruments, since visits may be a function of variables omitted in our non-instrumented regressions.

## 5.4 | Conclusion and implications for future research

Despite these limitations, this study contributes to the literature on corporate strategy by describing an underexplored factor promoting acquirers' value capture in M&As: social interactions with the target in the pre-acquisition phase. These social interactions were unmeasured in previous research. The results show that these interactions can provide great economic value to acquirers, suggesting a potential strategy to increase gains from acquisitions. Overall, while M&A research has often conceptualized acquisitions as competitive auctions, this paper highlights that social relationships pose a boundary condition to such auctions. Hence, future research should consider that although the relative scarcity of the acquirer's and target's resources can affect the intensity of rivalry in M&As (Ahern, 2012; Barney, 1988; Capron & Pistre, 2002), managers' social relationships can make an acquisition a de facto one-to-one negotiation even in the presence of alternative potential acquirers.

We also believe that the novel measure of temporary human capital movements proposed in this study can benefit different areas of research in strategy. Smartphone geolocational data can provide a fine-grained measure of temporary interactions and resulting information flows that may not be observed otherwise. While such interactions may not leave traces in traditional data sources, they can be highly valuable for companies and should therefore be investigated by researchers. In corporate strategy research, smartphone geolocational data can be used to study, for instance, the allocation of human resources among business units, HQs-subsidiary monitoring, the level of integration between firms after a merger, the frequency of social interactions in alliances, or knowledge spillovers induced by social interactions within and across firms.

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## DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from SafeGraph Inc, Securities Data Company (SDC) Platinum, Compustat, and the Center for Research in Security Prices (CRSP). Restrictions apply to the availability of these data, which were used under license for this study. Data are available from the authors with the permission of the data providers.

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