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Boom and Gloom

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ABSTRACT

We study the performance of investments made at different points of an investment cycle. We use a large data set covering hotels in the United States, with rich details on their location, characteristics, and performance. We find that hotels built during hotel construction booms underperform their peers. For hotels built during *local* hotel construction booms, this underperformance persists for several decades. We examine possible explanations for this long-lasting underperformance. The evidence is consistent with information-based herding explanations.

HOW WELL DO investments perform, if they were made during a boom? And if their performance is different, can strategic interactions between decision makers help explain the difference? Given the exuberance that has characterized many booms, they have received substantial attention (see, e.g., Kindleberger (1978), Greenspan (1996), Akerlof and Shiller (2009), or Glaeser (2013)). However, the type of data needed to study these questions has been lacking.

So far, evidence has been available only in aggregate form. For example, IPOs and private equity (PE) and venture capital (VC) funds seem to perform less well if they raise funds during "hot" periods.[1] Data aggregated at the firm or fund level, however, make it infeasible to study how strategic interactions between projects in their markets relate to investment booms and their performance. In a particular market, competition at the time of entry may affect performance after investment booms. Also at the market level,

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[1] See Ritter (1991), Gompers and Lerner (2000), Gompers et al. (2008), Kaplan and Schoar (2005), Kaplan and Strömberg (2009), or Robinson and Sensoy (2013).

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informational spillovers may affect whether investments are made in the first place. To explore whether such strategic interactions can help explain the booms we observe, and the performance of investments made during such booms, we need detailed data at the project/investment level in clearly delimited markets.

In this paper, we address these issues. We use a unique proprietary data set on the characteristics and performance of hotels in the United States. The data are available at the hotel level and contain detailed information about the properties as well as the economic and competitive environment in which each hotel operates. The data also include the year of construction for virtually all hotels built in the United States, and their location, allowing us to identify whether having been built during aggregate (nationwide) and market-level (county-level) investment booms has an impact on their operating performance. Importantly, investments in the hotel industry are long-lived and irreversible, allowing us to study both the short-term and long-term impact of local and aggregate investment booms on performance. In addition, the hotel industry is particularly suitable to study the role of strategic interactions on investment booms, as agency problems among decision makers are not a major concern: most hotels are owned and operated by individuals or partnerships.[2]

We find that investments made during a boom perform significantly less well. Consistent with earlier papers on "cohort effects" for IPOs and PE or VC fund investments (cited above), we find that investments made during aggregate (nationwide) booms underperform for a few years.[3] More importantly, we find that, after controlling for aggregate booms, investments made during *local* booms underperform for a long time: the effects are significant even 30 years after a hotel was built. This underperformance is economically significant. A one-standard-deviation increase in the number of hotels built in the same county-year reduces a hotel's performance by 3% to 5%.[4] Interestingly, we find that the underperformance of a hotel built during a local boom is driven by the number of hotels from *different* quality segments entering the same geographic market at the same time.[5]

What can explain the underperformance of hotels built during *local* booms? Credit conditions at the time of a hotel's construction cannot do so, as aggregate credit conditions have an effect on aggregate investment cycles, which we control for. In addition, our results are robust to the inclusion of variables that control for aggregate and local credit conditions.

[2] Institutional details about the hotel industry are described in Section II.

[3] This is also consistent with a "real options" view of investments (e.g., capital may be available at a low cost, inducing investors to exercise investment options early). See Grenadier (1996).

[4] We use revenue per available room (RevPAR), the standard measure of performance in the hotel industry. Details on this measure are described in Section II. The underperformance we find reduces the NPV of a hotel project significantly. An example of NPV reduction is given in Section V.C. Details are in the Internet Appendix in the online version of the article.

[5] There are six segments: luxury/upper-upscale, upscale, midscale with food and beverage, midscale without food and beverage, economy, and independent. See Sections II and III below for more details.

Other simple explanations based on local conditions are not sufficient either. Consider changes in local demand. If more hotels are built because market participants expect a surge in demand, hotels built during local investment booms should not perform worse than otherwise equivalent hotels. Similarly, the underperformance is not due to a worsening pool of available sites in a specific market. We find that the underperformance is not less pronounced for hotels in segments where site selection is a priori less relevant (e.g., economy hotels).[6]

More promising explanations for the underperformance of hotels built during *local* booms focus on the strategic interactions between market participants. These interactions are strongest at the local market level, where hotels compete directly and where information about future demand and the attractiveness of possible investments is transmitted directly or indirectly.

Competition might explain the underperformance if developers neglect possible entry by competitors during boom periods. Such "competition neglect" may occur if agents base their decisions on noisy but easily available information (Veldkamp (2006), Hoberg and Phillips (2010)), if there are coordination failures (Carlsson and van Damme (1993)), or if agents are overconfident (Camerer and Lovallo (1999), Simonsohn (2010), Greenwood and Hanson (2015)). If hotels compete most strongly with hotels of a similar vintage, this could lead to excessive entry and long-run underperformance for hotels built during a local boom.

However, competition neglect cannot explain the underperformance we find, since in our data underperformance is related to the number of hotels opened in the same year and market but in a *different* quality segment. That does not mean that competition is irrelevant. To the contrary, a hotel's performance is reduced if it competes with a larger number of hotels in the *same* segment, irrespective of their vintage. That is, more competitors in the same quality segment and market *do* reduce a hotel's performance, but that is irrespective of the year in which those hotels entered the market. Interestingly, but consistent with earlier agglomeration studies, more hotels operating in *different* quality segments, irrespective of their vintage, do not reduce a hotel's performance. Thus, it is unlikely that underperformance, which is driven by the number of hotels opened in the same market and year but in *different* quality segments, is caused by some sort of competition neglect across quality segments.

Alternative explanations based on strategic interactions focus on problems of information acquisition and transmission. Hotels are long-lived investments, so expectations about future demand are crucial for the decision of whether to enter a particular market. If such information is hard to come by and noisy, it may be tempting to observe other developers' entry decisions and imitate them. Following the literature (for an overview, see Hirshleifer and Teoh (2003)), we use the term "herding" to describe any such imitation.

[6] In an earlier version of the paper we show that hotels built during the peak of a local boom perform less well than hotels built slightly later. This is also inconsistent with a worsening pool of available sites in a specific market.

Existing models of rational herding and informational cascades (starting with Bikhchandani, Hirshleifer and Welch (1992), Banerjee (1992), and Welch (1992)) study how decisions change when agents can observe the decisions that other agents made earlier. These models do not make predictions about performance, however: while cascades can be harmful, they can also be beneficial, and payoffs are identical for all agents who make the same decision. We therefore develop a simple model in which agents are not equally well informed and herding can motivate entry, and we derive testable implications about performance.

In our model, a developer may decide to enter a market for several reasons: demand in that market in general may look promising, demand in a particular market *segment* may look promising, or the developer may have identified an unusual opportunity that cannot be replicated by others. The first potential entrant has information that is more precise than that received by other entrants. Those other entrants observe the first entrant's decision and try to infer its motivation, using that inference to update their (multi-dimensional) beliefs.[7] They benefit from imitating the first entrant if the main motivation was that the market or a particular segment are promising. However, if the developer entered because of an unusual opportunity, then subsequent imitation will lead to subpar performance.

Intuitively, the performance realized by these imitators is not as good as that of the better-informed entrant, since they may enter when a market or segment are not attractive, and they may stay out even though a market or segment would be attractive. The realized performance is particularly low for a potential entrant whose signals favor a segment different from that chosen by the better-informed entrant. Such entrants must make a decision based on conflicting signals about segments and are therefore more likely to make a decision that they will later regret.

The evidence supports this herding intuition. Hotels built during years of more intense entry in a given market perform less well, consistent with the idea that imitation leads (on average) to subpar performance. Importantly, hotels perform less well if a larger number of hotels opened in the same market and year but in a different market segment. This result is consistent with the intuition that a signal extraction problem can lead to entry in a segment that is not promising.

We perform several tests to assess the validity of the herding explanation. First, we distinguish entry in similar quality segments from entry in very different segments. Arguably, competition should be stronger among hotels in similar quality segments. We find that the negative effect of other-segment entry on performance is driven by entry in very different segments, so the effect cannot be explained by competition neglect arguments, while the evidence is consistent with the herding explanation. Second, we exploit differences in time-to-build for hotels of different quality segments. We find that the negative effect

[7] This type of signal extraction problem is analyzed in Avery and Zemsky (1998) and Rhodes-Kropf and Viswanathan (2004).

of different-segment entry on performance is only present if the entry decision was likely part of the information set of underperforming hotels at the time when they decided to enter. This result is again consistent with the herding explanation.

Finally, we separate the decision to open a hotel, possibly based on herding (imitation), from the effect that simultaneous entry has on a given hotel's performance. We use a two-stage least squares approach. In the first stage, we study which hotels are more likely to have entered a county, partly motivated by herding, that is, based on noisy signals and on inference drawn from observing other entrants' decisions. We find that independent hotels are more likely to enter during an other-segment boom (e.g., enter in an upscale segment when there was a boom of economy hotels in the market). This result is consistent with our herding interpretation as independent hotels are likely less well informed (brand owners may give advice, and some require past experience of having managed a hotel). In the second stage, we find that, after controlling for organizational form and other hotel and market characteristics, hotels opened during other-segment booms underperform.

Overall, the evidence is consistent with the herding explanation. Competition is important, but arguments based on competition or competition neglect cannot explain all of our findings. We thus identify the mechanism through which entrants make decisions that lead to underperformance: updating of beliefs and imitation. The exact cause of the lower performance (the "mistake" an underperforming entrant made) is not clear, however, as it is likely hotel-specific. A "herder" hotel may underperform because the chosen location does not "work" for that type of hotel, because it offers an inadequate quality segment, or because it is missing certain facilities that would make it attractive. Some hotels may suffer if demand is largely repeat business, and tour companies or conference organizations that were not interested in a given hotel when it opened cannot be convinced to give it another chance. Similarly, a hotel may perform less well if suitably skilled staff members cannot be found initially, creating a bad "culture" that is hard to improve.[8]

We contribute to the literature by being the first to study the performance of investments made at different points of an investment cycle using disaggregated, project-level data. We are able to distinguish aggregate investment cycles from investment cycles at the local market level, since the relevant markets are defined geographically in the hotel industry, instead of other product-space dimensions that are hard to delimit. We show that hotels built during investment booms underperform others, but the impact of aggregate investment booms on performance is only short-lived, while the effect of local investment booms is much more long-lasting. Moreover, we are able to explore the sources of the underperformance of assets created during an investment boom. We show that direct competition at the time of entry is not the key driver of the long-run underperformance of hotels built during investment booms. Rather,

[8] The last two explanations are related to research on the role of "initial conditions" of job market entrants, and how they affect career and compensation paths. See, for example, Oyer (2006).

hotel underperformance is related to the entry decision of hotels of different quality segments, suggesting that herding on the entry decision is the most likely driver of the underperformance. Overall, we show that the use of disaggregated data is critical to understanding the performance of investments made at different points of an investment cycle.

Our results also contribute to the empirical literature on herding. This literature is quite small, given the challenges of finding proper micro-level data to test for herding behavior. Our results are consistent with those of Welch (2000) and Kennedy (2002). However, unlike those earlier papers, we obtain our findings in a setting in which career concerns are not likely to play a role (see Section II for details).

Our results (both theoretical and empirical) have implications for many settings, not merely the hotel industry. Any investment decision made under uncertainty can lead to herding if others' decisions can be observed and inference can be drawn about their motivation. For example, Kennedy (2002) analyzes the programming choices of TV networks, and Welch (2000) studies the buy or sell recommendations of security analysts. Other examples include telecommunications cable providers, movie theaters, semiconductors, shipping, and drilling for oil or gas. In all of these settings, investments are significant but the value of investing is uncertain, so it is possible that later investors update their beliefs after observing the details of an earlier investment. There are several advantages of focusing on the hotel industry: the data have great coverage and detail, hotel investments are very long-lived (so performance can be measured over fairly long periods), and it is clear which hotels are competitors. Our analysis suggests that strategic interactions between competitors are important determinants of how investments perform. In particular, decision-makers can learn from the decisions of others, and this updating of beliefs affects both investment decisions and the performance of investments.

The rest of the paper is organized as follows. In Section I, we develop hypotheses for the underperformance of investments made during boom periods based on strategic interactions. In Section II, we describe important features of the hotel industry relevant for our analysis. Section III describes the data, and Section IV presents our empirical strategy. In Section V, we document that hotels opened during local investment booms exhibit lower performance, and that simple explanations cannot account for this finding. In Section VI, we examine explanations based on local strategic interactions. Section VII concludes.

I. Hypotheses

In this section we discuss hypotheses derived from two types of strategic interaction—herding and competition neglect—that could explain lower performance for investments made during local booms. Simple explanations not based on local strategic interactions between entrants are discussed (and tested) in Section V.

We first describe a simple model in which potential entrants to a market or industry are uncertain about the value of entering, and they update their beliefs about that value by observing the entry decision of a better-informed potential entrant. This signal inference may lead them to imitate the earlier decision, that is, there may be herding.

Entry involves choosing in which segment of a market to enter. The model shows that "informed" entry increases potential entrants' posterior beliefs about the value of entering a particular segment, but also that, for them, entry is on average (conditional on seeming advantageous) less attractive than it is for a better-informed entrant. The difference in value is particularly large when a less well-informed ("uninformed") potential entrant chooses a segment different from the segment chosen by the informed entrant.

Below, we first derive hypotheses from the herding model. We then derive hypotheses implied by arguments based on competition neglect.

A. Herding

There are N potential entrants, $i = 1, \ldots, N$. One of them is an informed entrant, with more precise information than the other uninformed entrants. The informed entrant decides first whether to enter. Each entrant can enter at most one segment j,k.[9] After observing that decision, the uninformed entrants simultaneously make entry decisions.[10] The value of entering segment j,

$$V_{ij} = t + v_j + a_{ij},$$

depends on three random variables: the state of the market, t, which is common to all entrants; the state of demand in segment j, v_j, which is also common to all entrants; and each entrant's segment-specific individual ability, a_{ij}. The variables t, v_j, and a_{ij} are distributed uniformly on [–1,1]. Entrants must pay a fixed cost of entry K, so the net payoff from entry is $V_{ij} - K$.

Each entrant privately observes signals about t, v_j, and a_{ij}. The informed entrant's signals are accurate, that is, she observes the true realizations t, v_j, and a_{ij}. Each uninformed entrant observes noisy signals s_i, m_{ij}, and u_{ij} about t, v_j, and a_{ij},

[9] That better-informed agents move first is intuitive when entry decisions can be delayed and there are differences in the precision of the agents' information. See Chamley and Gale (1994), Zhang (1997), or Grenadier (1999).

[10] We have analyzed a model with two informed entrants. The analysis is much more complicated, but the results are qualitatively the same (double informed entry in one segment is more positive information and makes uninformed entry more attractive). Similarly, we could make the model more general by allowing for sequential entry, such that the uninformed entrants update their beliefs after each decision, including uninformed-entrant decisions. That would greatly complicate the analysis, without promising any interesting new results (informational "cascades" may arise, i.e., entrants completely disregard their own information).

$$s_i = \begin{cases} t & \text{w/prob.} \quad \varphi \\ \tau_i & \text{w/prob.} \quad 1-\varphi \end{cases}$$

$$m_{ij} = \begin{cases} v_j & \text{w/prob.} \quad \varphi \\ \upsilon_{ij} & \text{w/prob.} \quad 1-\varphi \end{cases}$$

$$u_{ij} = \begin{cases} a_{ij} & \text{w/prob.} \quad \varphi \\ \alpha_{ij} & \text{w/prob.} \quad 1-\varphi \end{cases}$$

The random variables τ_i, υ_{ij}, and α_{ij} follow the same distribution as t, v_j, and a_{ij}, that is, they are uniformly distributed on $[-1,1]$. Note that $E[t] = E[v_j] = E[a_{ij}] = 0$, and that changes in the informativeness φ of the uninformed signals s_i, m_{ij}, and u_{ij} do not affect the distribution of the possible values of uninformed entry.[11]

An uninformed entrant's expected payoff from entry, given signals s_i, m_{ij}, and u_{ij}, and given the informed entry decision δ_1 ($\delta_1 = j,k,0$, where $\delta_1 = 0$ means the entrant did not enter), is

$$\begin{aligned} &E\left[t + v_j + a_{ij} \mid s_i,\, m_{ij},\, m_{ik},\, u_{ij},\, u_{ik},\, \delta_1\right] \\ &\quad = \varphi s_i + (1-\varphi)\, E\left[t \mid \delta_1\right] + \varphi m_{ij} + (1-\varphi)\, E\left[v_j \mid \delta_1\right] + \varphi u_{ij} + (1-\varphi)\, E\left[a_{ij}\right] \\ &\quad = \varphi s_i + (1-\varphi)\, E\left[t \mid \delta_1\right] + \varphi m_{ij} + (1-\varphi)\, E\left[v_j \mid \delta_1\right] + \varphi u_{ij}. \end{aligned}$$

The problem is nontrivial if $K < 3$. Values of K less than one lead to complications and counterintuitive results, since uninformed entrants can infer that some low signals are false. We therefore assume that $K \geq 1$. We also assume that $\varphi < K/3$, which simplifies the analysis because uninformed entrants will then not enter based on their signals alone.[12] For tractability, below we assume that $K = 1$. That restricts the parameter φ to $\varphi < 1/3$.

The informed entrant enters if $V_{ij} = t + v_j + a_{ij} \geq K$. Intuitively, the probability of informed entry is decreasing in K, and the average realized value of informed entry is increasing in K. Given K, the average realized value of informed entry is

$$E\left[t + v_j + a_{ij} \mid t + v_j + a_{ij} \geq K\right] = K + 10\frac{3-K}{31 + 6K - K^2}. \tag{1}$$

(For details on the derivation, see the Internet Appendix.[13]) As shown in Figure 1, the expected value of entry is increasing in K, and the probability of informed entry is decreasing in K. The assumption $K = 1$ (introduced below) seems reasonable: with higher values of K, the average realized value of

[11] This model of noisy signals has been used in Povel and Singh (2010) to analyze stapled finance and in Povel and Sertsios (2014) to analyze toehold acquisitions prior to mergers.

[12] The uninformed entrants' average realized value of entry increases if their signals are more informative, but it is below the average value realized by informed entrants as long as $\varphi < 1$. So this assumption does not affect the results.

[13] The Internet Appendix can be found in the online version of this article on the *Journal of Finance* website.

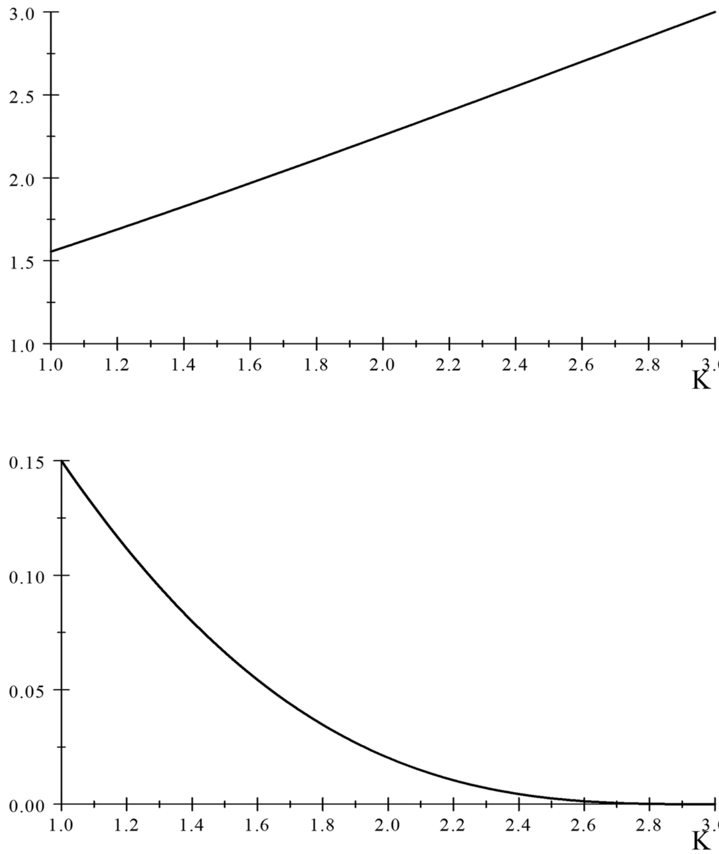

Figure 1. The figures show the average realized value of informed entry (top panel) and the probability of informed entry (bottom panel), for different entry costs K.

informed entry increases but the probability of informed entry becomes small and eventually approaches zero.

We now turn to the decisions of the uninformed entrants. We first compute the expected values of $t + v_j$ and $t + v_k$, conditional on the informed entry decision. Note that, for the informed entrant to enter at all, we must have $t + v_j + a_{ij} \geq K$ in one segment, so informed entry in segment j implies $t \geq K - 2$. Furthermore, if the informed entrant entered segment j instead of segment k, then $v_j \geq K - t - 1$ and $v_k + a_{ik} < v_j + a_{ij}$.

Ignoring the signals s_i, m_{ij}, and u_{ij}, an uninformed entrant's posterior beliefs about the expected value of $t + v_j$ contingent on informed entry in segment j ($\delta_1 = j$), informed entry in segment k ($\delta_1 = k$), and informed non-entry ($\delta_1 = 0$) are as follows:

$$E\left[t + v_j | \delta_1 = j\right] = \frac{-7K^3 + 39K^2 + 195K + 213}{12\left(-K^2 + 6K + 31\right)}, \tag{2}$$

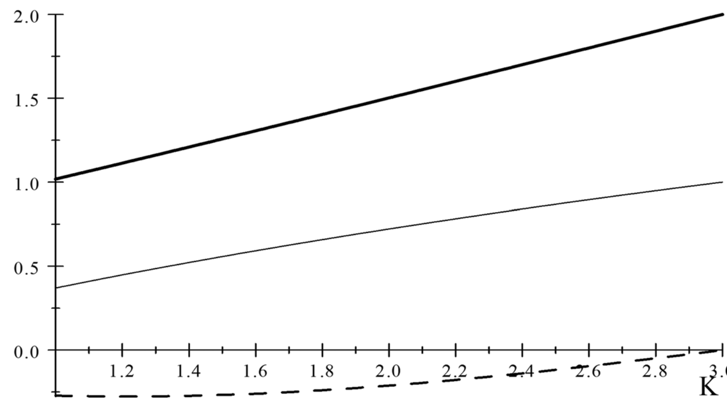

Figure 2. This figure plots the posteriors about the value of $t + v_j$ after the informed entrant entered segment j (thick line), segment k (thin line), or did not enter at all (dashed line) for different values of K.

$$E\left[t + v_j | \delta_1 = k\right] = \frac{-K^3 - 3K^2 + 105K - 21}{6\left(-K^2 + 6K + 31\right)}, \tag{3}$$

$$E\left[t + v_j | \delta_1 = 0\right] = \frac{2\left(-2K^4 + 15K^3 + 18K^2 - 117K - 54\right)}{3\left(-3K^3 + 27K^2 - K + 321\right)}. \tag{4}$$

(For details on the derivation, see the Internet Appendix.) The posteriors after informed entry are positive because informed entry suggests that the value of t is likely high. The posterior is higher for the segment in which the informed entrant entered, since that segment's value v_j is likely high. The posterior is negative after non-entry, since it suggests that t, v_j, and v_k are low. The posteriors are plotted in Figure 2.

Consider the situation of uninformed entrant i, after having observed the informed decision δ_1 (where $\delta_1 \in \{j, k, 0\}$) and signals s_i, m_{ij}, m_{ik}, u_{ij}, and u_{ik}. We distinguish three possible outcomes: entry in the same segment as the informed entrant, entry in a different segment, and entry after the informed entrant did not enter. An uninformed entrant enters segment j if both of these conditions are satisfied:

$$\begin{aligned} &\varphi\left(s_i + m_{ij} + u_{ij}\right) + (1-\varphi)\, E\left[\, t + v_j \mid \delta_1\right] \geq K \\ &\varphi\left(s_i + m_{ij} + u_{ij}\right) + (1-\varphi)\, E\left[\, t + v_j \mid \delta_1\right] \\ &\qquad \geq \varphi\left(s_i + m_{ik} + u_{ik}\right) + (1-\varphi)\, E\left[\, t + v_k \mid \delta_1\right]. \end{aligned}$$

For tractability, we assume that $K = 1$ in what follows. Under that assumption, we have $E[t + v_j \mid \delta_1 = j] = 55/54$, $E[t + v_j \mid \delta_1 = k] = 10/27$, and $E[t + v_j \mid \delta_1 = 0] = -35/129$.

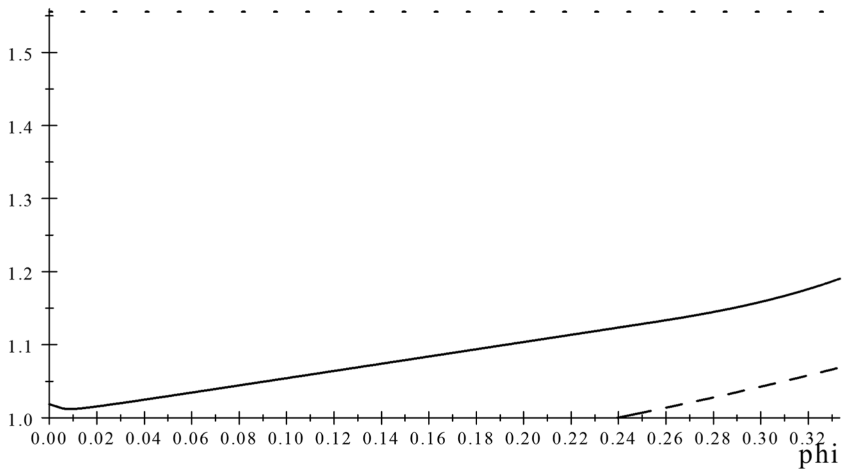

Figure 3. This figure plots the average value of uninformed entry in the same segment as the informed entrant (solid line); the value of uninformed entry in a different segment (dashed line); and the average value of informed entry (dotted line), for different values of φ.

PROPOSITION 1: *The average realized value of uninformed entry is smaller than that of informed entry. The average realized value of uninformed entry is particularly small for uninformed entrants who entered a segment different from the segment chosen by the informed entrant.*

The proof is in the Internet Appendix. The result is intuitive. The uninformed entrants make decisions under uncertainty, so they may enter even though the realized value of entering is below its cost, and conversely, uninformed entrants may not enter even though they would benefit from entering (if their signals are incorrectly low). Even making inferences about the true value of entering from the informed entrant's decision cannot make up for the noise in the signals. While our model is simple, there is no reason to believe that relaxed assumptions would lead to qualitatively different results. For example, the uniform distribution of the signals greatly simplifies the calculation of posteriors, but changes to its support or functional form should merely change the cutoffs in the case distinctions and the conditional expected payoffs (the terms would be much more complex). Further, the restrictions on K can be lifted, but as discussed above, uninteresting additional cases would be introduced, and the analysis would be much more complex.

Figure 3 plots the average realized value of entry (for different values of φ) for the informed entrant (dotted line), an uninformed entrant who enters the same segment as the informed entrant (solid line), and an uninformed entrant whose signals induced her to enter but not in the same segment as that chosen by the informed entrant (dashed line). The average realized value of entering is generally larger than the cost of entering ($K = 1$).

The uninformed entrants enter if the informed entrant entered and if the sum of their signal realizations is sufficiently high. The signals may be pure noise, however, so their average realized value is much lower. This implies the following:

HYPOTHESIS 1: *More entry reduces the average realized value of entry.*

If an uninformed entrant's signals about segment k are sufficiently strong, she prefers entering that segment even if the informed entrant entered segment j. That happens only if the informativeness φ of the signals is sufficiently high, so the uninformed entrant's signals can persuade her to ignore the informed entrant's segment choice (but not her decision about entry or non-entry). Given the higher posterior about the sum $t + v_j$ than about the sum $t + v_k$ if the informed entrant entered segment j, uninformed entry in segment k will happen only if an uninformed entrant's signals in favor of entering segment j are weak and those in favor of segment k are strong, so if segment k is preferred the expected value of entering is likely small. An uninformed entrant is more likely to favor segment j, and on average uninformed entrants who imitate the entrant's decision to enter segment j expect to realize a higher value of entry than an uninformed entrant who chooses segment k (because the informed entry in segment j suggests that v_j is higher than v_k). Entrants in segment j thus include the informed entrant, whose average realized value is much higher anyway, and other uninformed entrants, who are likely to have received stronger signals about segment j. This implies:

HYPOTHESIS 2: *An entrant's realized value of entering is on average lower if more entrants entered a different segment.*

In our model, each of the N potential entrants could enter, if all signals are high. In practice, demand in any given market is limited, so increases in demand offer limited scope for entry, particularly in small markets that do not experience much entry on average. In small markets, only one (informed) entry may be feasible, while in larger markets, both informed and uninformed entrants may enter. Competition should reduce the number of entrants, and at the margin, when an uninformed entrant has signals that favor a segment different from that chosen by the informed entrant, competition makes that other segment more attractive. None of this would change the result that uninformed entrants expect to realize lower values of entry on average, since their information is noisy. Similarly, given the informational disadvantage of a potential entrant with conflicting signals (signals favoring segment k while the informed entrant chose segment j), that entrant should expect to realize a particularly low value of entry.

B. Competition Neglect

Competition may reduce the performance of investments made during a boom if entrants neglect possible entry by competitors during boom periods. Such competition neglect may occur if agents base their decisions on noisy but easily available information (Veldkamp (2006), Hoberg and Phillips (2010)), if there are coordination failures (Carlsson and van Damme (1993)), or due to overconfidence (Camerer and Lovallo (1999), Simonsohn (2010), Greenwood and Hanson (2015)). If competition is strongest among hotels of approximately equal age, then competition neglect can cause long-lasting underperformance. This implies:

HYPOTHESIS 3: *More entry reduces the average realized value of entry*.

Note that Hypothesis 3 makes the same prediction as Hypothesis 1, but for very different reasons. Note also that Hypothesis 3 focuses on (unanticipated) competition from hotels of a similar vintage, so its tests require that, when measuring a hotel's performance, we control for the number of hotels competing in the same market (including older or newer hotels).

Arguably, hotels compete most strongly with hotels operating in the same quality segment, so competition neglect should have the strongest effect on performance if a large number of hotels entered a given market and segment in a given year. This implies:

HYPOTHESIS 4: *An entrant's realized value of entering is on average lower if more entrants entered the same segment*.

Thus, while Hypotheses 1 and 3 make identical predictions, Hypotheses 2 and 4 make different predictions. This allows us to distinguish empirically the validity of the herding and competition neglect explanations.

II. Investments, Operations, and Performance in the Hotel Industry

A. Investments in the Hotel Industry

Branded hotels dominate the hotel market in the United States, but surprisingly few hotels are actually owned by the company that owns the brand (e.g., Marriott International, Starwood Hotels & Resorts, Hilton Worldwide, Hyatt, etc.). Instead, hotels are typically owned by individuals, partnerships, or limited liability companies (LLCs), who either operate the hotels themselves or hire management companies. Specifically, around 85% of hotels are owned by individuals, partnerships, or LLCs, while only around 10% are owned by large corporations (see Corgel, Mandelbaum, and Woodworth (2011)).[14] The typical investor who builds a hotel is a real estate developer, who selects a location and negotiates the financing. While planning the project, the real estate developer also decides whether to build an independent hotel or a hotel prototyped under some brand and chooses an appropriate organizational form (see below for details).

[14] Investments/ownerships by hotel REITs (real estate investment funds) account for less than 2% of hotels, while other institutional investors (e.g., pension funds or financial institutions) represent less than 1% of investors.

The hotel industry is thus characterized by a decentralized ownership structure, with very small units making investments and start-up decisions.[15] As only a small fraction of the assets are owned by large corporations, there are no major concerns about bureaucracy or agency problems due to career concerns that often complicate the analysis in other contexts. In particular, at the planning and investment stage of a hotel, the developer holds equity in the project and thus has a strong incentive to make value-maximizing decisions.

The decision to build a hotel is based on the assessment of future demand in a particular market. This requires forecasts about the volume of demand for hotel services, but also forecasts about the type of traveler that is expected (business, leisure, etc.). A developer must choose a promising market, a promising site in that market, and the most promising quality segment (for branded hotels, the quality segment depends on the chosen brand). Not surprisingly, these decisions are made under uncertainty, and hotels are planned (and construction starts) well before the expected increase in demand materializes.[16]

Investments in the hotel industry are long-term and irreversible. Developers invest large amounts, financed partly with bank loans (mortgages). Once completed, hotels are long-lived. With occasional renovations, a hotel can be operated for several decades. It is rare for hotels to be closed permanently—according to practitioners' comments, conversions (say, into offices, apartments, or retirement homes) are extremely rare, and only 0.5% to 1% of the existing stock is demolished per year. And sales and bankruptcies do not change the supply of hotel rooms in a given market, but rather merely change the ownership of a hotel, and maybe the brand under which it operates.[17] Not surprisingly, given the low exit rate, the entry rate in the industry is low too: on average, the entry rate was 2.9% per year between 1993 and 2006, while the entry rate for other industries was about 10% over the same period (see Freedman and Kosová (2012)).

The time needed to plan and construct a hotel varies, depending on the chosen quality segment and (related to that choice) the amenities the hotel will offer (e.g., restaurant, conference facilities). Economy and midscale hotels

[15] This has been the prevalent business model in the hotel industry, except during the early 1980s, when some parent companies also built hotels and then sold them to partnerships created to produce tax shelters. That became an unattractive business model with the passage of the 1986 Tax Reform Act (see Follain, Hendershott, and Ling (1992)). This inflow of capital is unlikely to drive our results: it affected all markets in the United States, so in our analysis below, it should affect the cohort effect but not the local entry effect.

[16] For example, the construction of an upscale hotel in the "NoMa" neighborhood of Washington, D.C., is the first investment in a large mixed-use commercial development; see "JBG launches Capitol Square with new hotel site," *Washington Business Journal*, Thursday, October 18, 2012. The investment was initiated with the expectation that demand would materialize once the development is complete.

[17] It is also very difficult for a hotel to change its quality segment after construction is completed. Only 2% of the observations in our sample register changes in quality segments, and most of those changes are to an adjacent segment. Raising the quality segment is costly (including forgone revenues during a renovation) and often not feasible due to space or location limitations. Lowering the quality segment is also costly, as it means having to give up revenue.

can be built in one year, but more upscale hotels (with more facilities) require around two years. News about a planned new hotel becomes public during the planning stage (e.g., when permits are requested) or once the site clearance and construction work begins. Thus, the decision to build a hotel can incorporate prior decisions to build by others even if their construction has just started. As a consequence, it is common to observe many hotels entering in a given year, whose entry decisions were actually sequential.

B. Operations in the Hotel Industry

The hotel developer can choose to operate the hotel independently or under a nationally or globally recognized brand name (e.g., Courtyard by Marriott, Hilton Garden Inns) that belongs to a large corporation (e.g., Marriott International, Hilton Worldwide). The choice of brand is also related to the organizational form under which the hotel will operate. Some brands are offered to developers only through franchise agreements (e.g., Microtel, Travelodge) or only through management contracts (e.g., Fairmont, Four Seasons), while other brands make no such restrictions (e.g., Courtyard by Marriott).

Under a franchise agreement, the corporation owning the brand (the franchisor; e.g., Hilton Worldwide), grants to the owner/developer of a hotel (the franchisee) the right to use its brand name (Waldorf Astoria, Hilton, DoubleTree, Hampton Inn, etc., in the case of Hilton Worldwide) and to operate the hotel under its entire business format (e.g., use the hotel company's supplier network and booking system), while providing ongoing support and monitoring. The franchisor does not manage the hotel property, but rather leaves most day-to-day management decisions to the franchisee. Many franchisors require their franchisees to have experience operating hotels.

Under a management contract, in contrast, the hotel corporation owning the particular brand is hired by the hotel owner/developer to manage the hotel. Thus, the corporation owning the chosen brand handles day-to-day operations and all the management decisions at the given hotel. (Usually, the hotel's owner cannot interfere with the operator's management of the property).

Both franchise agreements and management contracts tend to have long time horizons—usually 20 years, with renewal options—but can be terminated before the contract expires under certain circumstances (see Kosová and Sertsios (2016)). A consumer normally cannot tell whether a branded hotel is operated under a franchise agreement or a management contract. Each brand targets a particular quality segment defined by the brand requirements, in terms of service and amenities offered.

C. Performance Measurement in the Hotel Industry

The hotel industry is characterized by large up-front investments. The cost of a hotel development can range from $5 million for an economy hotel to well above $100 million for a luxury hotel. The most important component of a hotel investment is the construction cost, which amounts to approximately 86% of the total investment. The cost of purchasing land represents only about 14% of the total development cost.[18]

When it comes to operations, the hotel industry is a typical revenue-management industry, with operating costs that are mostly fixed. Thus, hotels of the same brand (with similar construction costs, space needs, and operating costs) operating in the same location type (e.g., urban area, near an airport), in markets with similar economic characteristics (in terms of tourist attractiveness, income, population, etc.), with the same number of rooms, amenities, age, and other characteristics are expected to have comparable performance in terms of revenue. This is why the industry's key performance measure is *Revenue Per Available Room* (RevPAR), defined as the revenue earned from all rooms sold during a certain time period divided by the number of room-nights available during that time period.[19]

Once controlling for the characteristics of hotels and their markets, hotels that have lower RevPAR are identified as underperforming hotels. Given that most of a hotel's costs are fixed, and given the strong competition in this industry, small reductions in RevPAR can significantly reduce the NPV of a hotel project. (An example of this is given in Section V.C; details are provided in the Internet Appendix.)

III. Data and Aggregate Data Patterns

A. Data Sources

We use a unique (proprietary) data set on the hotel industry. This data set combines hotel Census data compiled by Smith Travel Research (STR) with hotel revenue data also from STR.[20] The STR Census data cover around 98% of the hotel properties in the United States and represent one of the most comprehensive sources on the hotel industry available. The data provide information about the state and county in which the hotel is located, each hotel's organizational form (company-managed, franchised, or independent), a description of the hotel's location (urban, small town, suburban, etc.), and other property characteristics including the number of rooms, the quality segment, and the year in which the property was built (i.e., the year in which construction ended and the hotel first opened). The data also include information about which hotels operate under the same brand, but the brand identities are coded to preserve anonymity.

The revenue database contains a decade of performance information from 2000 to 2009, for the *universe* of branded hotels in the United States and some

[18] These numbers were obtained from HVS Global Hospitality Service, Hotel Development Cost Survey 2011. The cost of land, as a fraction of development cost, does not display substantial differences across quality segments: for economy, midscale, and upscale and luxury hotels, land costs represent 14%, 13%, and 14% of the total development cost, respectively.

[19] See Corgel, deRoos, and Fitzpatrick (2011).

[20] STR is an independent research company that collects information about hotel properties in the United States and other countries. We obtained access to all STR data under a strict confidentiality agreement.

independent hotels. Performance data are provided voluntarily by hotels to STR. The incentive to provide such information is strong since doing so is a requirement for getting access to aggregated benchmark data for a hotel's local market, and there are no drawbacks to doing so, since competing hotels can only obtain aggregated data, not data on individual hotels.

We use the key hotel performance metric mentioned above, namely, monthly RevPAR. Since in our analysis we use the average monthly RevPAR *per year*, we restrict our final sample to those hotel-years for which we have monthly RevPAR for all 12 months in a given year. Using annual averages of RevPAR helps us to smooth out outliers and avoid biases in performance measurement due to monthly seasonality.

Our analysis focuses on hotel properties that were built in 1940 or later, as during earlier years hotel construction patterns were sparse. Our final sample consists of 219,849 hotel-year observations across 30,283 unique hotel properties, distributed across 2,216 counties. Of these, about 89.4% of the annual RevPAR observations correspond to branded hotels that belong to 221 unique brands. The remaining 10.6% of observations correspond to independent/unbranded hotels. Notice that, although we restrict our sample to those hotels for which we have performance data, we use *all* hotels in the Census to construct some of our variables: the number of hotels *built* in the same year as a given hotel and the number of hotels *operating* in a given county-year.

To control for various market factors in our analysis, we complement the hotel data with data from the Census Bureau, the Bureau of Labor Statistics (BLS), and Zillow. These sources provide annual information on demographics and employment at the county level, namely, population (from the Census Bureau's annual population estimates), unemployment rate (from the BLS), median household income (from the Census Bureau), median housing values (from Zillow),[21] and the number of establishments in the accommodation industry and two related industries—arts, recreation and entertainment, and food and beverage (all from the Census Bureau's County Business Patterns data). In our analysis, we rely on market characteristics at the county level for two reasons. First, counties represent the best available approximation of the relevant geographic area in which hotels interact with each other and over which consumers typically consider alternative lodging options around their target destination (see Freedman and Kosová (2012) for a discussion). Second, county-level data represent the lowest level of aggregation at which time-varying market characteristics are regularly reported for each year.

B. Aggregate Investment Cycles: The Cohort Effect

Using the population of U.S. hotels from STR Census, Figure 4 shows the number of hotels built each year between 1940 and 2009. One can clearly see

[21] Zillow has information on county-level median housing values for around two thirds of our observations. For the county-years for which Zillow does not have county-level housing value data, we use state-level housing data from the same database. Our main results are not sensitive to controlling for this information.

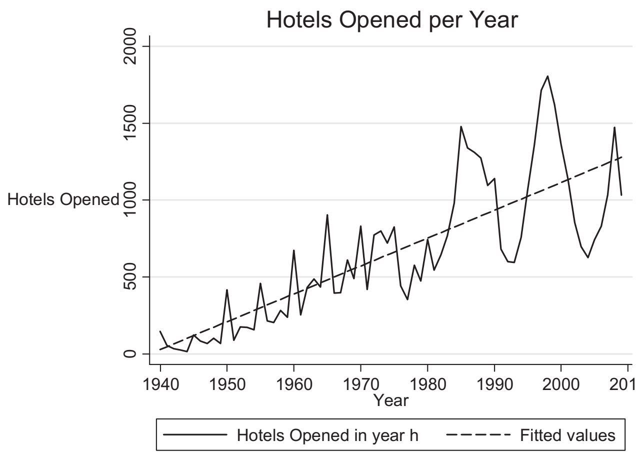

Figure 4. The figure plots the number of hotels built each year between 1940 and 2009 using STR Census data. The fitted value line shows the predicted values from the regression $Total\ \ Hotels_h = \alpha + \delta * Trend_h + e_h$.

patterns of cyclical aggregate or nationwide activity (i.e., an aggregate cohort effect), with hotel construction sometimes above and sometimes below the long-term trend. In what follows, we use h to denote the year of a hotel's construction, in order to later differentiate that year from the years of operation (indexed by t) over which we measure RevPAR.

To construct our measure of the aggregate cohort effect, we regress the total number of hotels built in the United States in year h, $Total\ Hotels_h$, on a time trend as follows:

$$Total\ Hotels_h = \alpha + \delta * Trend_h + e_h. \tag{5}$$

Using the estimated residuals from this regression, we measure the cohort effect as

$$Cohort\ Effect_h \equiv \hat{e}_h / \sigma,$$

where σ is the sample standard deviation of $\hat{e}_h$. The two advantages of this measure are that it captures the annual deviations in hotel entry from the common trend and it standardizes those annual deviations by the overall variation in our sample.

Though our main measure of the aggregate cohort effect is detrended, we also use an alternative measure without detrending to assess the robustness of our results:

$$Cohort\ Effect\ (levels)_h \equiv Total\ Hotels_h.$$

Since our analysis uses disaggregated hotel-level data, we measure the aggregate cohort effect (both detrended and levels) for each hotel i based on the year h in which it was built.

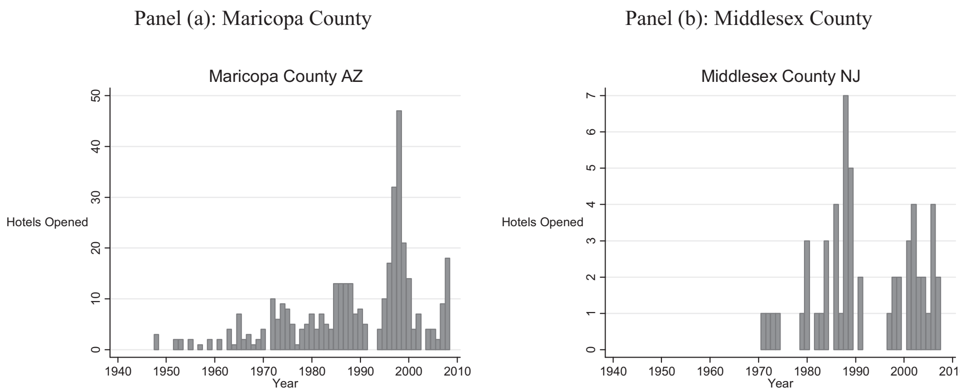

Figure 5. Panels A and B plot the entry patterns in two counties in terms of the number of entrants per year: Maricopa County in Arizona and Middlesex County in New Jersey.

As Figure 4 shows, the mid-1980s and late 1990s experienced the largest spikes in hotel construction relative to the time trend (i.e., a positive estimated residual and a large standard error), while the early 1990s and mid-2000s experienced slow investment (i.e., a negative estimated residual and a large standard error). The *Cohort Effect* reached its maximum in 1998, when the total number of hotels built was 2.7 standard deviations above the long-term trend, and its minimum in 2004, when the number of hotels built was 2.1 standard deviations below the long-term trend.

C. Local Investment Cycles: The County Entry Effect

Since we have detailed data on hotel entry at the county-year level, we can distinguish the impact of aggregate hotel entry (i.e., the aggregate cohort effect) from the local market/county entry effect. To identify the *local* investment cycles that each hotel i faces in its county c in the year of construction h, we define $Entrants_{ich}$ as the number of hotels that were built in county c during the same year h as hotel i, including hotel i itself. If hotel i is the only hotel built in county c in year h, the value of $Entrants_{ich}$ equals one. The highest value of this variable (47 hotels) appears in our sample in Maricopa County, Arizona in 1998. Thus, all hotels that were built in Maricopa County in 1998 will have their value of $Entrants_{ich}$ set to 47.[22]

Figure 5 plots the county entry patterns for two counties. Panel A shows the entry patterns for the county that experienced the largest spike in terms of the number of entrants in a single year—Maricopa County, Arizona—while Panel B shows the entry patterns in a county whose extent of entry was much smaller—Middlesex County, New Jersey. The figures indicate that, although entry patterns within counties tend to follow aggregate investment cycles, there

[22] We have also used alternative definitions of local entry, such as $Entrants_{ich}$ divided by the county population at the time of entry. Our results continue to hold.

is substantial variation across counties in terms of the timing and magnitude of entry. For example, the upsurge in construction in the mid-1980s was relatively more pronounced for Middlesex while the upsurge in construction in the late 1990s was relatively more pronounced in Maricopa. In addition, the distribution of hotel openings in Middlesex is much more "lumpy" compared with Maricopa (Middlesex had no hotel construction for over a 30-year period after 1940). In the overall sample the correlation coefficient between *Cohort Effect* and *Entrants* is 0.25.

D. Summary Statistics

Table I, Panel A presents the summary statistics for our data. The panel itself is divided into three parts, presenting descriptive statistics on hotel characteristics, county (market) characteristics, and year of construction (h) characteristics.

On average, a hotel in our sample has 123 rooms, generates \$53 per room-night available (RevPAR), and realizes total revenues of nearly \$3 million per year (in 2009 U.S. dollars). Hotel performance is measured from 2000 to 2009. Thus, hotels built before 2000 have 10 years of performance data, while newer hotels have fewer performance year observations. Hotel age is defined as the difference between the year of operation during our sample period (2000 to 2009) and the year of the hotel's construction, plus one. The average age of hotels in our sample is 18 years. When it comes to organizational form, 71% of the hotel-year observations represent operations of franchisees, 18% of the hotel-year observations represent operations of company-managed properties, and the remainder represent operations of independent hotels.

Regarding market characteristics, the average hotel in our sample operates in a county with a median annual household income of \$52,200, an unemployment rate of 5.5%, a median housing value of \$228,000, and a population of 797,000. The average number of hotels in a county in a year of hotel operation t is 108 during our sample period, while the average number of more broadly defined accommodation establishments (including hotels, hostels, motels, etc.) in a county is 129. The average hotel in our sample operates in a county with on average 394 art, recreation, and entertainment establishments and 1,487 food and beverage establishments. Counties with more establishments in these hotel-related industries are likely to be more attractive tourist/business destinations and thus have higher demand for hotels as well.

Finally, at the bottom of Panel A, we present summary statistics on hotel characteristics by year of construction, as defined in Sections III.B and III.C: *Cohort Effect*, *Cohort Effect (level),* and *Entrants*. In this panel, and in all subsequent tables, we show these variables with subscripts to remind the reader that they have different levels of aggregation and that both *Entrants* and *Cohort Effect* are measured in the year of a hotel's construction, h, rather than the year in which we measure a hotel's performance, t. For simplicity, we do not include the subscripts of the other variables in the tables, as they are all measured in year t, although different variables have different levels of

Table I

Summary Statistics

Panel A presents descriptive statistics for the variables in our sample—split into hotel characteristics, county (market) characteristics, and year of construction (h) characteristics—across 219,849 hotel-year observations for 30,283 hotels over the period 2000 to 2009. Panel B summarizes the distribution of observations (as well as hotels) in our sample with different numbers of *Entrants*, that is, hotels entering in the same county-year as a given hotel. The remaining panels summarize the distribution of hotels and RevPAR observations by year (Panel C), by location types (Panel D), and across quality segments (Panel E).

Panel A: Descriptive Statistics

Variable	Mean	Pctile 10	Pctile 50	Pctile 90	*SD*	*N*
Hotel Characteristics						
RevPAR	53	22.9	45.7	89.7	35	219,849
Rooms	123	50	97	216	117	219,849
Yearly Revenues (000)	2,937	538	1,469	5,787	5,827	219,849
Year	2005	2001	2005	2009	3	219,849
Age	18	4	15	36	12	219,849
Franchise	0.71	0	1	1	0.45	219,849
Company Managed	0.18	0	0	1	0.39	219,849
County (Market) Characteristics						
Income (000)	52.2	38.5	49.7	69.8	12.8	219,849
Unemployment Rate (%)	5	3	5	8	2	219,849
Population (000)	797	37	295	1,804	1,458	219,849
Median House Value (000)	228	137	189	385	119	219,849
Hotels in County	108	9	54	273	152	219,849
Art, Recreation, and Entertainment Establishments	394	13	116	682	1,288	219,849
Food and Beverage Establishments	1,487	63	578	3,598	2,595	219,849
Accommodation Establishments	129	11	65	341	186	219,849
Year of Construction (h) Characteristics						
Cohort Effect$_h$	0.66	−1.10	0.66	2.43	1.27	219,849
Cohort Effect$_h$ (levels)	1,079	489	1,095	1,715	431	219,849
Entrants$_{ch}$	4	1	2	10	5.67	219,849

Panel B: Distribution of Observations and Hotels by Number of Entrants in the Same County-Year

Entrants$_{ch}$	Obs	Hotels	% of Obs	% of Hotels
1	77,780	11,083	35.4%	36.6%
2	43,463	6,075	19.8%	20.1%
3	23,746	3,331	10.8%	11.0%
4	16,889	2,230	7.7%	7.4%
5	11,379	1,570	5.2%	5.2%
>5	46,592	5,994	21.2%	19.8%
Total	219,849	30,283	100%	100%

(*Continued*)

Table I—*Continued*

Panel C: Distribution of Observations by Year of Operations

Year	Obs	% of Total
2000	18,778	8.5%
2001	19,654	8.9%
2002	20,670	9.4%
2003	21,382	9.7%
2004	21,668	9.9%
2005	21,720	9.9%
2006	22,235	10.1%
2007	23,216	10.6%
2008	24,514	11.2%
2009	26,012	11.8%
Total	219,849	100%

Panel D: Distribution of Observations and Hotels by Location Type

Location	Obs	Hotels	% of Obs	% of Hotels
Urban	20,564	2,786	9.4%	9.2%
Suburban	93,756	12,350	44.3%	40.8%
Airport	14,071	1,817	6.4%	6.0%
Interstate	34,657	4,896	16.5%	16.2%
Resort	13,511	1,931	2.3%	6.4%
Small Town	43,290	6,503	23.6%	21.5%
Total	219,849	30,283	100%	100%

Panel E: Distribution of Observations by Segment

Location	Obs	% of Obs
Luxury/Upper Upscale	14,274	6.5%
Upscale	22,702	10.3%
Midscale with F&B	21,831	9.9%
Midscale without F&B	66,587	30.3%
Economy	70,839	32.2%
Independent	23,616	10.7%
Total	219,849	100%

aggregation (county versus hotel level). As Panel A shows, the detrended measure *Cohort Effect* is positive on average, as more hotels were built during years of high investment activity than during years of low investment activity. The mean for *Entrants* is four, indicating that on average a hotel in our sample was built along with three other hotels in the same county-year.

Panel B presents more detailed statistics for the variable *Entrants,* which captures county investment cycles. Specifically, 37% of the hotels (35% of our sample observations) represent hotels that were the only entrants in their county in their year of construction, while 20% of the hotels (20% of the observations) were built together with one other hotel in the same county-year.

Hotel-year observations with three, four, and five hotels built at the same time comprise 11%, 7%, and 5% of our data, respectively. Interestingly, more than 20% of the observations represent properties that were built in the same county-year as five or more other hotels.

Panel C reports the annual frequency of hotel performance observations (RevPAR) in our sample. Overall, the distribution of hotel-year observations is relatively similar across the years, with gradual increases over time due to new hotel construction. Panel D reports the distribution of hotels in our sample across location types (i.e., urban, suburban, small town, resort, near a highway, or near an airport). Finally, Panel E reports the distribution of observations, distinguishing branded hotels, by quality segment, from independent hotels. As expected, a small fraction of the branded hotel-year observations operates in the luxury/upper-upscale (6.6%) and upscale (10.3%) segments, while more than 40% operate in the midscale segments (with and without food and beverage) and 32% in the economy segment. Independent hotels in our sample represent less than 11% of the observations. For 54% of the independent hotel observations we also have information on the quality segment in which they operate. Product differences across segments, and examples of brand names associated with each segment, are summarized in the Internet Appendix.

IV. Empirical Methodology

To analyze the impact of the aggregate investment cycles (aggregate cohort effect) and local/county-level investment cycles (county entry effect) on hotel performance, we estimate several variations of the following baseline empirical model:

$$y_{igct} = \alpha + \beta * \mathit{Cohort\ Effect}_{ih} + \gamma * \mathit{Entrants}_{ich} + \mathbf{Q}'\Omega_{\boldsymbol{ct}} + \boldsymbol{Z}'\Gamma_{\boldsymbol{i}} + \boldsymbol{M}'\Psi_{\boldsymbol{igct}} + \mu_{st} + \delta_g + \varepsilon_{igct}. \tag{6}$$

The subscript i indexes hotels, t indexes the year of a hotel's operation during our sample period (2000 to 2009), c indexes the county, h indexes the year of a hotel's construction, s indexes a hotel's quality segment, and g indexes a hotel's brand. The dependent variable y_{igct} represents our asset performance measure—the average monthly RevPAR in a given year t.

Differences in market size and economic conditions across counties and over time could affect hotel performance and at the same time be correlated with our variables of interest, thus biasing our estimates. To control for this, we include in $\mathbf{\Omega_{ct}}$ a set of market characteristics at the county level, namely, median household income, population, median housing value, and unemployment rate. In addition, we control for the number of hotels that operate in a given county-year using the STR Hotel Census database. To control for the attractiveness of a market as a business or tourist destination, we control for the number of establishments in two related industries—arts, entertainment, and recreation and food and beverage—as well as the number of establishments in the broadly defined accommodation/lodging industry (not just hotels). Counties with more establishments in these industries are likely to be more attractive travel destinations and thus to have higher demand for hotels.

Another important set of controls, $\mathbf{\Gamma_i}$, captures hotel-specific characteristics, which include the number of rooms and dummy variables for six hotel location types: urban, suburban, small town, resort, near a highway, or near an airport. It is important to control for differences in hotel characteristics and location types since they likely affect a hotel's performance. For example, building a hotel in an urban area is likely more expensive than in a suburb or small town, but the expected revenue is likely higher as well. By using location-type dummies, we can compare the performance of hotels *within* given location types.[23]

We also include a set of time-varying hotel-specific controls, $\mathbf{\Psi_{igct}}$, which include a hotel's age (we include both a linear and a quadratic term) and dummy variables for a hotel's operation/organizational form: franchised, company-managed, or independent. The organizational form of hotels in our sample displays little variation over time; on average, the yearly rate of change in organizational form within a brand is 0.7%. We include hotel brand fixed effects, δ_{g}, to control for unobserved differences across brands, such as different levels of popularity and quality segments[24] (quality segments are subsumed within brands so we cannot include segment dummies together with brand dummies).[25] The omitted category is independent hotels, as these do not have any brand affiliation. We also include segment-year fixed effects, μ_{st}, to capture unobserved macroeconomic shocks that could affect hotel performance differently for each segment.

In a nutshell, our empirical strategy compares hotels that share the same brand, location type, organizational form, and other characteristics, that operate in similar markets. We ask whether, after controlling for all these factors that also capture differences in hotels' construction and operating costs, the entry of a larger number of hotels has a persistent effect on a hotel's performance. Key to our identification of the aggregate cohort effect and county-level entry effect is that our performance and control variables are measured at time t (post-entry years of hotel operation), while *Cohort Effect* and *Entrants* are measured at time h (year of hotel entry). In our data we have only 360 hotels/observations for which we measure their 12-month performance during

[23] In unreported analysis we have also included location-segment indicators to control for potential differences in the cost of operating certain segments in particular location types (e.g., operating a luxury hotel in an urban area, versus operating a luxury hotel in a small town). All our results hold.

[24] We also have data on whether hotels offer different sorts of amenities (e.g., a conference center, golf courses, spa). Only a few brands have variation in the amenities offered. Controlling for these characteristics does not change our findings.

[25] For the same reason, we do not include parent fixed effects, as parent dummies are subsumed within brands. For example, the parent company Marriott includes the brands JW Marriott, Courtyard by Marriott, etc.

their first year of operations (i.e., $t = h$): all our results hold if we drop these observations from our sample.

Using our baseline empirical specification (equation (6)), we explore the impact of the aggregate cohort effect and county-level entry effect on hotel performance for the overall sample, as well as for different subsamples based on the hotels' age. We then repeat the analysis for subgroups of branded (upscale, economy, etc.) and independent hotels, as well as for subgroups of hotels by location type (urban, resort, etc.), to explore whether the county-level entry effect is driven by a particular subgroup of hotels. This also allows us to examine whether simple explanations (e.g., site availability or agency problems between real estate developers and brand owners) can affect our findings. For robustness, we also replicate the results including an additional set of controls that capture financing conditions at the time of entry. We show that our results are not driven by credit market conditions.

Next, we extend our baseline empirical specification by splitting the impact of local entry into the impact from the "same" and "other" segments, to explore what type of hotel entry correlates with a hotel's performance. This analysis can shed light on whether product market competition or informational concerns are more likely to explain the local entry effect and its role in a hotel's performance. We later present further robustness analyses to disentangle the competition neglect and herding explanations.

V. Main Results

A. Determinants of Hotel Performance

We present the results of our baseline regressions (equation (6)) in Table II. In all regressions, we adjust standard errors for heteroskedasticity and county-level clusters. Since many of our explanatory variables are aggregated at the county level, unclustered standard errors may be underestimated (see Moulton (1990)).

In column (1) we only include *Cohort Effect* as a variable of interest, in column (2) we only include *Entrants*, and in column (3) we include both variables together. We find that, when studying the impact of aggregate and local investment cycles separately, both have a negative and statistically significant impact on hotels' long-term performance. However, when we include both variables together, only *Entrants*, which captures local investment cycles, has a negative and statistically significant impact on hotel performance. Once we control for investment activity at the local level, the negative aggregate cohort effect tends to disappear (as we discuss later, the cohort effect remains significant during the first few years of a hotel's operation when we split the sample by hotel age).

The results from column (3) do not depend on the definition of the cohort effect. To show this, in column (4) we replicate column (3) using our second definition of the cohort effect, *Cohort Effect (levels)* and find similar results. As our main results do not vary according to the definition of the cohort effect, in

Table II

Cohort Effect and County-Level Entry

The table presents the results from our baseline empirical equation (6). The dependent variable in all columns is hotel performance, log(*RevPAR*), in a given year t over the period 2000 to 2009. The variables of interest are: *Cohort Effect*, which captures the impact of the aggregate investment cycles, and *Entrants*, which captures the impact of local/county-level investment cycles. *Entrants* is the number of all hotels that entered the same county c in the same year h as hotel i. *Cohort Effect* in columns (1) and (3) is our detrended measure (i.e., the standardized residual from the time trend of the total number of hotels built in the United States in year h; see Section III.B). *Cohort Effect (levels)* in column (4) is the total number of hotels built in the United States in the same year h as hotel i. In all regressions, robust standard errors (reported in parentheses) are adjusted for heteroskedasticity and county-level clustering. *, **, and *** indicate significant at the 10%, 5%, and 1% level, respectively.

Variable	log(RevPAR) (1)	log(RevPAR) (2)	log(RevPAR) (3)	log(RevPAR) (4)
Cohort Effect$_{ih}$	−0.0040***		0.0021	
	(0.0015)		(0.0017)	
Entrants$_{ich}$		−0.0068***	−0.0070***	−0.0070***
		(0.0015)	(0.0016)	(0.0016)
Cohort Effect$_{ih}$ (levels)				0.0080
				(0.0062)
log(Income)	−0.0090	−0.0041	−0.0043	−0.0043
	(0.0455)	(0.0436)	(0.0435)	(0.0435)
Unemployment	−0.0184***	−0.0191***	−0.0192***	−0.0192***
	(0.0040)	(0.0039)	(0.0039)	(0.0039)
log(Popul.)	−0.1051***	−0.1011***	−0.1012***	−0.1012***
	(0.0361)	(0.0357)	(0.0357)	(0.0357)
log (Housing Prices)	0.2294***	0.2238***	0.2238***	0.2238***
	(0.0264)	(0.0253)	(0.0253)	(0.0253)
Hotels in County	−0.0002**	−0.0001	−0.0001	−0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
log(AE&R estab.)	0.0751**	0.0661**	0.0659**	0.0659**
	(0.0329)	(0.0320)	(0.0320)	(0.0320)
log(F&B estab.)	0.0839**	0.0873***	0.0875***	0.0875***
	(0.0345)	(0.0338)	(0.0338)	(0.0338)
log(Acc. estab.)	0.0053	0.0111	0.0113	0.0113
	(0.0158)	(0.0151)	(0.0151)	(0.0151)
log(Rooms)	−0.0549***	−0.0554***	−0.0554***	−0.0554***
	(0.0127)	(0.0124)	(0.0124)	(0.0124)
Age	−0.0132***	−0.0135***	−0.0136***	−0.0135***
	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Age^2	0.0002***	0.0002***	0.0002***	0.0002***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Location type fixed effects	Yes	Yes	Yes	Yes
Org. form fixed effects	Yes	Yes	Yes	Yes
Brand fixed effects	Yes	Yes	Yes	Yes
Segment-year fixed effects	Yes	Yes	Yes	Yes
County clustering	Yes	Yes	Yes	Yes
R^2	0.6615	0.6636	0.6636	0.6636
N	219,849	219,849	219,849	219,849

Table III
Cohort Effect and County-Level Entry by Hotel Age

The table presents the results from our baseline empirical equation (6) for different subsamples based on hotels' age. The dependent variable in all columns is hotel performance, log(*RevPAR*), in year t over the period 2000 to 2009. The variables of interest are: *Cohort Effect*, which captures the impact of the aggregate investment cycles, and *Entrants*, which captures the impact of local/county-level investment cycles. *Entrants* is the number of all hotels that entered the same county c in the same year h as hotel i. *Cohort Effect* in all columns is our detrended measure (i.e., the standardized residual from the time trend of the total number of hotels built in the United States in year h; see Section III.B). The year t control variables are log(*Income*), *Unemployment*, log(*Population*), log(*Housing Prices*), *Hotels in County*, log(*AE&R estab.*), log(*F&B estab.*), log(*Acc. estab.*), log(*Rooms*), *Age*, and *Age* squared. In all regressions, robust standard errors (reported in parentheses) are adjusted for heteroskedasticity and county-level clustering. *, **, and *** indicate significant at the 10%, 5%, and 1% level, respectively.

	Hotel Age				
	"1-5"	"6-10"	"11-20"	"21-30"	">30"
	log(RevPAR)	log(RevPAR)	log(RevPAR)	log(RevPAR)	log(RevPAR)
Variable	(1)	(2)	(3)	(4)	(5)
Cohort Effect$_{ih}$	−0.0123***	−0.0019	−0.0003	−0.0006	−0.0017
	(0.0027)	(0.0021)	(0.0027)	(0.0044)	(0.0076)
Entrants$_{ich}$	−0.0083***	−0.0079***	−0.0050**	−0.0064***	0.0009
	(0.0016)	(0.0023)	(0.0020)	(0.0022)	(0.0052)
Performance year t controls	Yes	Yes	Yes	Yes	Yes
Location type fixed effects	Yes	Yes	Yes	Yes	Yes
Org. form fixed effects	Yes	Yes	Yes	Yes	Yes
Brand fixed effects	Yes	Yes	Yes	Yes	Yes
Segment-year fixed effects	Yes	Yes	Yes	Yes	Yes
County clustering	Yes	Yes	Yes	Yes	Yes
R^2	0.6483	0.6828	0.6864	0.6831	0.6289
N	34,253	44,118	66,100	38,411	36,967

the remainder of the paper we only provide the results using our detrended measure (i.e., *Cohort Effect*). Overall, we find that local investment booms, measured as the number of entrants in a county-year, are negatively associated with long-term performance, even after controlling for a comprehensive set of hotel and market characteristics.

B. Determinants of Hotel Performance by Hotel Age

To better understand the drivers of the negative impact of local investment cycles on performance, we run equation (6) for different subsamples according to the hotels' age. We present these results in Table III. Column (1) reports the performance of hotels in the first five years of operation, column (2) considers hotels in their 6th to 10th years of operation, column (3) considers hotels in their 11th to 20th years of operation, column (4) considers hotels in their 21st to 30th years of operation, and column (5) considers hotels in operation for more than 30 years.

We find that both *Cohort Effect* and *Entrants* have a negative and statistically significant impact on performance during a hotel's first five years of operation. However, for hotels older than five years, the cohort effect vanishes completely, while the effect of *Entrants* decreases only moderately over time, remaining statistically significant for all hotel ages except those in the last category (31 and above).

The fact that the cohort effect is only short-lived (up to five years) suggests that, while capital inflows experienced by an industry may affect its willingness to fund projects (Gompers and Lerner (2000), Kaplan and Schoar (2005), Kaplan and Strömberg (2009)), that does not seem to affect performance in the hotel industry, except in the short run. This result is also consistent with a real options view of investments (see Grenadier (1996)), as hotels may have taken advantage of better financing terms to enter the market "earlier" than they would have under normal financing conditions. If this is the case, hotels might have been built when market demand was not yet high enough, and this is why they appear to perform worse than their peers in the short run.

The fact that *Entrants* has a negative and pervasive effect on hotel performance is intriguing. To shed more light on what may be driving this effect, in the next two subsections we disaggregate the results from Table III by separately studying the performance of hotels operating in different quality segments and hotels in different types of location.

C. Determinants of Hotel Performance by Quality Segment

In Table IV, we repeat the analysis from Table III for subgroups of branded hotels and independent hotels. The first subgroup of branded hotels contains hotels belonging to economy brands, the most frequent segment in our sample (see Table I, Panel E). The second subgroup contains hotels belonging to midscale brands (with and without food and beverage). The third subgroup contains branded hotels from the two top-quality segments: upscale and luxury/upper upscale.

When estimating equation (6) separately for branded hotels of different quality segments and independent hotels, we would like to know the organizational form and brand under which hotels started their operations. We only have information about their organizational form, brand, and quality segment at the time of performance measurement, not at the time they were built. This distinction is unlikely to be of particular relevance when making our group classifications, however, because, as mentioned in Section II, management contracts and franchise agreements are usually long term (about 20 years), with a high renewal rate.

By replicating our baseline specification for different hotel subsamples, we can test for simple potential explanations for our findings. For example, brand owners may have an incentive to push hotel developers into locations that are of strategic value to the brand owners but are poor investments for the developer. If such conflicts of interest are more likely during boom times, or if a brand owner's bad advice is more likely to be followed by a developer during a boom,

Table IV

Cohort Effect and County-Level Entry by Hotel Age and Quality Segment

The table reports the results from our baseline empirical equation (6) for different subsamples based on hotels' age. The dependent variable in all columns is hotel performance, log(*RevPAR*), in year t over the period 2000 to 2009. Panel A uses performance data of hotels affiliated with a nationwide recognized brand in the economy segment. Panel B uses performance data of hotels affiliated with a nationwide recognized brand in the midscale segments. Panel C uses performance data of hotels affiliated with a nationwide recognized brand in upscale or luxury/upscale segments. Panel D uses performance data of independent hotels (i.e., not affiliated with a nationwide recognized brand). The variables of interest are: *Cohort Effect*, which captures the impact of the aggregate investment cycles, and *Entrants*, which captures the impact of local/county-level investment cycles. *Entrants* is the number of all hotels that entered the same county c in the same year h as hotel i. *Cohort Effect* in all columns is our detrended measure (i.e., the standardized residual from the time trend of the total number of hotels built in the United States in year h; see Section III.B). The year t control variables are log(*Income*), *Unemployment*, log(*Population*), log(*Housing Prices*), *Hotels in County*, log(*AE&R estab.*), log(*F&B estab.*), log(*Acc. estab.*), log(*Rooms*), *Age*, and *Age* squared. In all regressions, robust standard errors (reported in parentheses) are adjusted for heteroskedasticity and county-level clustering. *, **, and *** indicate significant at the 10%, 5%, and 1% level, respectively.

	Hotel Age				
Variable	"1-5" log(RevPAR) (1)	"6-10" log(RevPAR) (2)	"11-20" log(RevPAR) (3)	"21-30" log(RevPAR) (4)	">30" log(RevPAR) (5)
Panel A: Economy Hotels					
Cohort effect$_{ih}$	−0.0058	−0.0021	0.0090**	0.0062	−0.0070
	(0.0071)	(0.0044)	(0.0043)	(0.0067)	(0.0123)
Entrants$_{ich}$	−0.0088***	−0.0084***	−0.0042	−0.0100***	−0.0005
	(0.0021)	(0.0026)	(0.0026)	(0.0026)	(0.0048)
R^2	0.3609	0.2758	0.2211	0.2954	0.3653
N	6,990	11,913	23,700	15,360	12,876
Panel B: Midscale Hotels					
Cohort effect$_{ih}$	−0.0153***	−0.0033	−0.0043	−0.0070	−0.0075
	(0.0033)	(0.0025)	(0.0033)	(0.0072)	(0.0111)
Entrants$_{ich}$	−0.0077***	−0.0066***	−0.0037*	−0.0035	0.0125
	(0.0019)	(0.0025)	(0.0019)	(0.0025)	(0.0076)
R^2	0.3879	0.4413	0.4406	0.4741	0.5229
N	17,707	22,775	25,454	10,727	11,755
Panel C: Upscale Hotels					
Cohort effect$_{ih}$	−0.0107**	−0.0024	−0.0012	0.0013	−0.0128
	(0.0048)	(0.0046)	(0.0059)	(0.0089)	(0.0194)
Entrants$_{ich}$	−0.0068***	−0.0057***	−0.0041**	−0.0012	0.0048
	(0.0016)	(0.0014)	(0.0016)	(0.0025)	(0.0060)
R^2	0.6191	0.6749	0.6371	0.6149	0.6212
N	7,386	7,020	11,593	7,190	3,787

(*Continued*)

Table IV—*Continued*

	Hotel Age				
	"1-5"	"6-10"	"11-20"	"21-30"	">30"
	log(RevPAR)	log(RevPAR)	log(RevPAR)	log(RevPAR)	log(RevPAR)
Variable	(1)	(2)	(3)	(4)	(5)
Panel D: Independent Hotels					
Cohort effect$_{ih}$	−0.0207	−0.0145	−0.0041	−0.0015	0.0011
	(0.0134)	(0.0104)	(0.0114)	(0.0149)	(0.0180)
Entrants$_{ich}$	−0.0054	−0.0149**	−0.0118*	−0.0100**	−0.0050
	(0.0047)	(0.0059)	(0.0067)	(0.0051)	(0.0079)
R^2	0.5390	0.5554	0.4543	0.4554	0.4417
N	2,170	2,410	5,353	5,134	8,549
Performance year t controls	Yes	Yes	Yes	Yes	Yes
Location type fixed effects	Yes	Yes	Yes	Yes	Yes
Org. form fixed effects	Yes	Yes	Yes	Yes	Yes
Brand fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
County clustering	Yes	Yes	Yes	Yes	Yes

this might explain the underperformance. However, we find that *Entrants* has a negative effect on performance for both branded and independent hotels—the long-run effect is actually larger for independent hotels. Since independent hotels do not deal with brand owners, the above conflict of interest cannot explain our results.

Alternatively, the underperformance of hotels built during local booms could be due to those hotels being built on cheaper and less attractive sites, in which case lower operating performance would have been expected. If that was the case, then the underperformance should be more pronounced for the highest quality segment hotels (luxury/upper upscale and upscale) than for economy hotels, as most economy hotels are built in very homogeneous sites (e.g., near a highway). We find, however, that the results for economy hotels are actually slightly stronger than those of higher quality segment hotels, making the "cheaper location" hypothesis unconvincing.

In addition, our results are economically too large to be driven by hotels choosing cheaper locations during boom times. In the overall sample, a one-standard-deviation increase in *Entrants* (5.67 additional hotels built in a county-year) decreases RevPAR by 3.97% on average. To get a better sense of what this performance reduction means in terms of NPV, we asked STR how hotel revenue translates into yearly cash flows. They provided us with aggregated information for economy hotels. Using this information and hotel development cost information available from HVS Global Hospitality Service (Hotel Development Cost Survey 2011), we estimate the NPV for the average economy hotel in our sample. We then estimate how this NPV would change after a one-standard-deviation increase in the number of *Entrants* (details are in the Internet Appendix). We find that the NPV for the average economy hotel in our sample is about $301,000 (the total development cost is $5.255 million).

A one-standard-deviation increase in *Entrants* reduces the present value of room revenues of an economy hotel by 3.5% (on average), reducing the NPV by \$299,000, to nearly zero (\$2,000). Given that the cost of land for the average economy hotel in our sample is about \$736,000 (land costs represent approximately 14% of development costs per the 2011 Hotel Development Cost Survey), the cheaper locations story can explain underperformance only if hotels built during booms use locations that are 41% cheaper (within a given location type), assuming there is no increase in construction costs. This scenario is unlikely as both land and construction costs are highly procyclical.

D. Determinants of Hotel Performance by Location Type

We now study whether a hotel's location matters for the underperformance result. In some cases, the classification may be too coarse, and two hotels with the same location type may not be regarded as equivalent by consumers. If early movers choose the best sites available, this might explain the underperformance result. For example, a resort hotel may have been built on a beachfront site early in an investment boom, and later resort hotels may be limited to sites that are not beachfront sites. Similarly, an airport hotel built early in a boom may have direct access to the terminal, while hotels built later can only be built "near" the terminal, such that guests need transportation to it. And an urban hotel may have been built facing a park or other landmark, but no such sites are available later. In contrast, for suburban or small town hotels, or for hotels near interstates, the supply of suitable sites should be much less constrained, so hotels built early in a boom are unlikely to enjoy a location advantage.

To explore this possibility, in Table V we repeat the analysis from Table III for subgroups of hotels according to their location type. If the underperformance of hotels built during local booms is due to those hotels being built in less attractive sites—and lower operating performance was expected—the underperformance should be more pronounced for hotels in areas where site selection is more relevant (i.e., in urban areas, near airports, and in resorts).

Our results are similar for both groups. The coefficient on *Entrants* is smaller (in absolute value) for the subgroup of hotels located in areas where site selection is a priori *more* relevant (Panel A) than for the subgroup of hotels located in areas where site selection is a priori *less* relevant (Panel B) for the first 10 years of operations. This pattern is reversed for older hotels. Overall, the data do not support the hypothesis that a worsening pool of available sites for a hotel causes the underperformance we find. Further tests (reported in the Internet Appendix) show that hotels built during the peak of a local boom perform less well than hotels built slightly later; this again suggests that a worsening pool of available sites is not driving underperformance.

E. Credit Conditions at the Time of Entry

In this section we address whether credit conditions at the time of entry can explain our findings. Arguably, when interest rates decrease, additional projects—with marginally lower operating performance—may become

Table V
Cohort Effect and County-Level Entry by Hotel Age and Location Type

The table reports the results from our baseline empirical equation (6) for different subsamples based on hotels' age. The dependent variable in all columns is hotel performance, log(*RevPAR*), in year t over the period 2000 to 2009. Panel A uses performance data of hotels located in urban areas, near airports, and in resort areas. Panel B uses performance data of hotels located in suburban areas, near an interstate, and in small towns. The variables of interest are: *Cohort Effect*, which captures the impact of the aggregate investment cycles, and *Entrants*, which captures the impact of local/county-level investment cycles. *Entrants* is the number of all hotels that entered the same county c in the same year h as hotel i. *Cohort Effect* in all columns is our detrended measure (i.e., the standardized residual from the time trend of the total number of hotels built in the United States in year h; see Section III.B). The year t control variables are log(*Income*), *Unemployment*, log(*Population*), log(*Housing Prices*), *Hotels in County*, log(*AE&R estab.*), log(*F&B estab.*), log(*Acc. estab.*), log(*Rooms*), *Age*, and *Age* squared. In all regressions, robust standard errors (reported in parentheses) are adjusted for heteroskedasticity and county-level clustering. *, **, and *** indicate significant at the 10%, 5%, and 1% level, respectively.

	Hotel Age				
	"1-5"	"6-10"	"11-20"	"21-30"	">30"
	log(RevPAR)	log(RevPAR)	log(RevPAR)	log(RevPAR)	log(RevPAR)
Variable	(1)	(2)	(3)	(4)	(5)
Panel A: Urban, Airport, and Resort Hotels					
Cohort Effect$_{ih}$	−0.0147**	−0.0083	0.0069	0.0072	0.0098
	(0.0070)	(0.0061)	(0.0078)	(0.0100)	(0.0160)
Entrants$_{ich}$	−0.0058***	−0.0069***	−0.0071**	−0.0082**	−0.0078
	(0.0020)	(0.0021)	(0.0029)	(0.0038)	(0.0058)
R^2	0.6867	0.7336	0.7204	0.6930	0.6213
N	6,319	7,599	12,678	9,933	11,617
Panel B: Suburban, Interstate, Small Town Hotels					
Cohort Effect$_{ih}$	−0.0129***	−0.0011	−0.0025	−0.0013	−0.0026
	(0.0029)	(0.0022)	(0.0026)	(0.0046)	(0.0091)
Entrants$_{ich}$	−0.0094***	−0.0083***	−0.0033	−0.0052***	0.0068
	(0.0019)	(0.0027)	(0.0021)	(0.0015)	(0.0099)
R^2	0.6116	0.6486	0.6412	0.6046	0.5627
N	27,934	36,519	53,422	28,478	25,350
Performance year t controls	Yes	Yes	Yes	Yes	Yes
Location type fixed effects	Yes	Yes	Yes	Yes	Yes
Org. form fixed effects	Yes	Yes	Yes	Yes	Yes
Brand fixed effects	Yes	Yes	Yes	Yes	Yes
Segment-year fixed effects	Yes	Yes	Yes	Yes	Yes
County clustering	Yes	Yes	Yes	Yes	Yes

profitable. One could thus argue that a financing channel may link more entry with weaker performance, as observed in the data.

Such a financing channel cannot explain our local boom findings, however. Interest rates are very homogeneous across counties and have an extremely high covariance, as the cost of funds is determined by U.S. monetary policy. In our empirical setting, nationwide market conditions at the time of entry are controlled by *Cohort Effect*, which reflects how aggregate market conditions at the time of entry correlate with a hotel's performance. Thus, a financing channel argument may explain the coefficient on *Cohort Effect*, but not the coefficient on *Entrants*.

To further allay any concerns, we repeat our analysis from Table III, using additional variables to control for the nationwide cost of funds and a proxy for local market credit standards in year $h-1$ (the year prior to entry). We use three proxies to capture the cost of funds: Mortgage rates, Fed rates, and the spread between Aaa bonds and Fed rates. Given that there is no historical county-level information on credit standards, we include county GDP growth from years $h-4$ to $h-1$. If local income growth relaxed local credit standards, then county GDP growth in the years before a hotel was built could have a negative effect on its performance. We also control for other aspects of local economic conditions at the time of entry: the normalized standard deviation of county GDP growth, the logarithm of county GDP in year $h-1$, and the logarithm of state-level housing prices in year $h-1$ (obtained from the Lincoln Institute of Land Policy).[26]

Our sample is limited to hotels built after 1972, since the BEA data on county demographics are only available as of 1969 and we use four lags to construct county GDP growth. Thus, our sample of hotels older than 30 years is reduced to only 1,943 observations. For this subsample, the spread between Aaa bonds and Fed rates is omitted due to multicollinearity with the other variables that describe economic conditions at the time of entry. The results are presented in Table VI.

Table VI shows that the cost of funds variables does not play a significant role in explaining long-run performance, which is not surprising given that aggregate conditions are captured by *Cohort Effect*. Importantly, the negative effect of *Entrants* on performance is not altered by the inclusion of these additional variables. All in all, credit conditions (and other aspects of local economic conditions) at the time of entry do not seem to explain our findings. Nonetheless, for robustness we keep these controls in all of our following regressions.

F. Other Simple Explanations

Anticipated increases in local demand may explain some of the local booms, but they cannot explain the underperformance that we find. If more hotels are built because the market is projected to have a surge in demand, hotels built during local investment booms should not perform worse than otherwise equivalent hotels. Survivorship bias cannot explain our results either, for two reasons. First, as pointed out in Section II.A, hotels are rarely demolished or converted into alternative uses. Second, if poorly performing hotels are among the few that cease operations, then our estimates should be biased against

[26] County-level data on residential and commercial property would be preferred (e.g., Zillow), but such data are available only starting in 1996.

Table VI

Cohort Effect, County-Level Entry, and Entry Conditions

The table replicates our analysis from Table III, using additional variables to control for relevant economic conditions at the time of entry, h. We use three variables that capture the cost of funds: mortgage rates, Fed rates, and the spread between Aaa bonds and Fed rates. To proxy for county-level credit standards, we include the county GDP growth rate from $h-4$ to $h-1$. We also control for the normalized standard deviation of county GDP growth, the logarithm of county GDP, and the logarithm of housing prices. Given that the Bureau of Economics Analysis (BEA) started compiling data on county demographics in 1969, and we use four lags for county GDP growth, we can only include in this analysis hotels that were built starting 1973. Columns (1) to (5) report the results for different hotel age cohorts. The year t control variables are log(*Income*), *Unemployment*, log(*Population*), log(*Housing Prices*), *Hotels in County*, log(*AE&R estab.*), log(*F&B estab.*), log(*Acc. estab.*), log(*Rooms*), *Age*, and *Age* squared. The spread between Aaa bonds and Fed rates is omitted due to collinearity for hotels over 30 years old. In all regressions, robust standard errors (reported in parentheses) are adjusted for heteroskedasticity and county-level clustering. *, **, and *** indicate significant at the 10%, 5%, and 1% level, respectively.

	Hotel Age				
Variable	"1-5" log(RevPAR) (1)	"6-10" log(RevPAR) (2)	"11-20" log(RevPAR) (3)	"21-30" log(RevPAR) (4)	">30" log(RevPAR) (5)
Cohort Effect$_{ih}$	−0.0070*	0.0046	−0.0001	−0.0042	0.3605
	(0.0037)	(0.0031)	(0.0029)	(0.0064)	(0.8176)
Entrants$_{ich}$	−0.0072***	−0.0053***	−0.0040*	−0.0077***	−0.0328**
	(0.0014)	(0.0019)	(0.0021)	(0.0021)	(0.0141)
Mortgage rate$_{h\text{-}1}$	−0.0031	0.0380	0.0451***	−0.0089	1.7262
	(0.0268)	(0.0263)	(0.0169)	(0.0172)	(3.8893)
Fed rate$_{h\text{-}1}$	−0.0194	−0.0430	−0.0411**	0.0179	−0.5097
	(0.0263)	(0.0287)	(0.0185)	(0.0204)	(1.1812)
Spread, Aaa Bonds and Fed rates$_{h\text{-}1}$	0.0088	0.0309	0.0270	−0.0218	
	(0.0234)	(0.0274)	(0.0165)	(0.0183)	
log(County GDP)$_{ch\text{-}1}$	0.1377**	0.0309	0.0608*	0.0485*	0.0983**
	(0.0659)	(0.0591)	(0.0345)	(0.0255)	(0.0387)
County GDP Growth$_{h\text{-}4 \text{ to } h\text{-}1}$	−0.0336	−1.0472***	−0.4451	−0.3044	1.7127***
	(0.3803)	(0.3571)	(0.3848)	(0.2504)	(0.6316)
County GDP Growth Volatility$_{h\text{-}4 \text{ to } h\text{-}1}$	−0.0194**	−0.0075	−0.0060	0.0009	0.0386
	(0.0092)	(0.0085)	(0.0076)	(0.0120)	(0.0372)
log(Housing Prices)$_{ch\text{-}1}$	0.1172***	0.1633***	0.1921***	0.2415***	0.2383***
	(0.0208)	(0.0248)	(0.0196)	(0.0306)	(0.0798)
Performance year t controls	Yes	Yes	Yes	Yes	Yes
Location type fixed effects	Yes	Yes	Yes	Yes	Yes
Org. form fixed effects	Yes	Yes	Yes	Yes	Yes
Brand fixed effects	Yes	Yes	Yes	Yes	Yes
Segment-year fixed effects	Yes	Yes	Yes	Yes	Yes
County clustering	Yes	Yes	Yes	Yes	Yes
R^2	0.6545	0.6904	0.6965	0.7025	0.7005
N	34,237	44,097	66,089	33,042	1,943

finding any long-run underperformance. Sample selection issues, due to voluntary reporting of performance data, are not a concern either, as in practice the *universe* of branded hotels operating in the United States report their data to STR.

Given that simple explanations cannot account for the lower performance of hotels opened during local booms, we now turn to explanations based on strategic interactions at the local level.

VI. Herding versus Competition Neglect

As shown in Section I, both herding and competition neglect predict that hotels built during local booms underperform (Hypotheses 1 and 3). To distinguish the validity of the two explanations, we now focus on Hypotheses 2 and 4, where the two explanations make very different predictions.

A. Same-Segment and Other-Segment Entrants

Our herding model predicts that performance will be particularly weak if a larger number of other entrants chose a different segment (Hypothesis 2). This is because choosing a different segment from an informed entrant (and consequently from the majority of entrants) means that the information inferred from the informed entrant's decision is partly overridden; entrants who choose the same segment, on the other hand, use both that information and their own signals, which likely favored that same segment. In contrast, competition neglect should lead to weaker performance if more entrants chose the same segment as a given hotel (Hypothesis 4). If more hotels enter the same segment, and such entry was not anticipated (i.e., it was neglected), then excessive within-vintage competition hurts a given hotel's performance (even after controlling for competition from hotels of a different vintage).

We divide our proxy for local booms (number of entrants in a county-year) into two mutually exclusive categories: number of hotels opened in the same quality segment as hotel i, and number of hotels opened in other segments. Hypothesis 2 (herding) predicts that *Entrants (other segments)* has a negative impact on a hotel's performance. Conversely, Hypothesis 4 (competition neglect) predicts that *Entrants (same segment)* has a negative impact on a hotel's performance.

When measuring a hotel's performance, we control for the number of hotels operating in that county and year. For consistency, we also distinguish same-segment and other-segment competition, creating the variables *Hotels in County (same segment)* and *Hotels in County (other segments)*. Since there is no quality benchmark for some independent/unbranded hotels (i.e., we cannot classify them as "same segment" or "other segment"), for better identification we do not consider those observations in this analysis. However, competition from independent hotels without segment information is still captured through the variable *Accommodation Establishments*.[27]

[27] Whether independent hotels are included or excluded does not change the results qualitatively.

Table VII presents the results for the specifications that use both within-segment and between-segment *Entrants* and competition (*Hotels in County*) for subsamples of hotels of different ages. When looking at the *Hotels in County* variables, which measure the intensity of within-segment and between-segment competition, we find that a hotel's performance is worse if there are more competitors operating in the same market segment (within-segment competition). We also find some evidence consistent with an "agglomeration effect" (Freedman and Kosová (2012), Canina, Enz, and Harrison (2005)): a hotel's performance is slightly better if it has more competitors operating in different quality segments, due to an agglomeration externality. These results show that contemporaneous competition and agglomeration are indeed important in explaining the performance of hotels in a given year.

Importantly, there is no support for Hypothesis 4 and thus for competition neglect driving underperformance. The number of entrants in the same county-year (year h) and the same segment as a given hotel i does not seem to have an important impact on that hotel's long-term performance. The number of entrants in the same county-year but in *other* segments than hotel i, in contrast, has a negative and significant impact on its long-term performance. This is consistent with Hypothesis 2 and thus with the herding explanation.

Conceivably, our definition of what constitutes a "market" may be regarded as too broad when relying on a county. Earlier studies on the hotel industry have regarded counties as markets (see Freedman and Kosová (2012)). However, hotels themselves may not necessarily regard a county as their relevant market. For example, in larger cities, a "downtown" area may be regarded as separate territory from an "uptown" area. Hence, as an additional robustness test, we replicate our analysis using a much narrower definition of a market based on a hotel's ZIP code. Even though this definition is extremely narrow (many homogeneous urban areas include several ZIP codes), and the variation in our data is greatly reduced, our qualitative findings remain (see the Internet Appendix). In particular, *Entrants* (at the ZIP code level) in other segments continues to have a negative impact on a hotel's long-term performance.

B. Robustness Tests

One potential concern with the results for same-segment and other-segment *Entrants* is that the negative coefficient on *Entrants (other segments)* might be driven by the entry of hotels in similar segments. Specifically, if hotels of a similar vintage compete strongly across similar segments, then competition neglect at the time of entry might drive the results, at least in part.

To shed more light on this possibility, we split *Entrants* into entrants in the same quality segment, entrants in the segment just below, entrants in the segment just above, and entrants in all other segments. Current competition variables (*Hotels in County*) are also split analogously. We report the results in Table VIII (column (1)).

The results in Table VIII (column (1)) show that the number of entrants in the segments just above or just below is not driving the underperformance.

Table VII
Same-Segment and Other-Segment Entrants

The table reports the results from our empirical equation (6) when we split the variable of interest, *Entrants,* and the control variable for competition during RevPAR years, *Hotels in County* at time *t,* between same-segment and other-segment hotels relative to hotel i. Given that there is no specific quality benchmark for *some* independent hotels (i.e., we do not know whether they are in the same or in some other segment as a given branded hotel), we exclude them from the sample, both as RevPAR observations, and when counting *Entrants* and *Hotels in County*. Their competitive presence is still captured by the variable *Accommodation Establishments*. The dependent variable in all columns is hotel performance, log(RevPAR), in a given year t over the period 2000 to 2009. *Entrants (same segment)* is the number of all hotels in the same segment as hotel i that entered the same county c in the same year h. *Entrants (other segments)* is the number of hotels in segments other than that of hotel i that entered the same county c in the same year h. Columns (1) to (5) show the results for different hotel age cohorts. The year t control variables are log(*Income*), *Unemployment*, log(*Population*), log(*Housing Prices*), log(*AE&R estab.*), log(*F&B estab.*), log(*Acc. estab.*), log(*Rooms*), *Age*, and *Age* squared. The year h control variables include year $h-1$ mortgage rates, Fed rates, and the spread between Aaa bonds and Fed rates, county GDP growth from $h-4$ to $h-1$, the normalized standard deviation of county GDP growth from $h-4$ to $h-1$, the logarithm of county GDP in $h-1$, and the logarithm of housing prices in $h-1$. In all regressions, robust standard errors (reported in parentheses) are adjusted for heteroskedasticity and county-level clustering. *, **, and *** indicate significant at the 10%, 5%, and 1% level, respectively.

	Hotel Age				
	"1-5"	"6-10"	"11-20"	"21-30"	">30"
	log(RevPAR)	log(RevPAR)	log(RevPAR)	log(RevPAR)	log(RevPAR)
Variable	(1)	(2)	(3)	(4)	(5)
Cohort Effect$_{ih}$	−0.0091***	0.0027	−0.0009	−0.0033	1.0436**
	(0.0035)	(0.0031)	(0.0026)	(0.0061)	(0.4591)
Entrants$_{ich}$ (same segment)	−0.0002	0.0023	0.0080**	0.0011	0.0474
	(0.0025)	(0.0025)	(0.0033)	(0.0068)	(0.0366)
Entrants$_{ich}$ (other segment)	−0.0066***	−0.0049***	−0.0056***	−0.0109***	−0.0439**
	(0.0014)	(0.0017)	(0.0019)	(0.0022)	(0.0210)
Hotels in County (same segment)	−0.0039***	−0.0034***	−0.0035***	−0.0034***	−0.0077***
	(0.0015)	(0.0012)	(0.0009)	(0.0008)	(0.0017)
Hotels in County (other segments)	0.0001	0.0003***	0.0001*	0.0001	0.0004*
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Performance year t controls	Yes	Yes	Yes	Yes	Yes
Entry year h controls	Yes	Yes	Yes	Yes	Yes
Location type fixed effects	Yes	Yes	Yes	Yes	Yes
Org. form fixed effects	Yes	Yes	Yes	Yes	Yes
Brand fixed effects	Yes	Yes	Yes	Yes	Yes
Segment-year fixed effects	Yes	Yes	Yes	Yes	Yes
County clustering	Yes	Yes	Yes	Yes	Yes
R^2	0.6690	0.7059	0.7190	0.7266	0.7367
N	33,022	42,818	63,284	30,925	1,782

Table VIII

Entry from Segments Above and Below

The table reports the results from our empirical equation (6) when we split the variable of interest, *Entrants*, into different categories. In column (1), *Entrants* is split into *Entrants* in the same segment, *Entrants* in the segment just below, *Entrants* in the segment just above and *Entrants* in all other segments. In column (2), the main explanatory variables are *Entrants* in years h and $h-1$, split into *Entrants* in the same segment, *Entrants* in all segments below (i.e., lower quality) and *Entrants* in all segments above (i.e., higher quality). RevPAR observations of independent hotels with no quality benchmark are excluded. The year t control variables are log(*Income*), *Unemployment*, log(*Population*), log(*Housing Prices*), *Hotels in County* (split in an analogous way as *Entrants* in the same column), log(*AE&R estab.*), log(*F&B estab.*), log(*Acc. estab.*), log(*Rooms*), *Age*, and *Age* squared. The year h control variables include year $h-1$ mortgage rates, Fed rates, and the spread between Aaa bonds and Fed rates, county GDP growth from $h-4$ to $h-1$, the normalized standard deviation of county GDP growth from $h-4$ to $h-1$, the logarithm of county GDP in $h-1$, and the logarithm of housing prices in $h-1$. In all regressions, robust standard errors (reported in parentheses) are adjusted for heteroskedasticity and county-level clustering. *, **, and *** indicate significant at the 10%, 5%, and 1% level, respectively.

Variable	log(RevPAR) (1)	log(RevPAR) (2)
Cohort Effect$_{ih}$	−0.0021	−0.0021
	(0.0018)	(0.0017)
Entrants$_{ich}$ (same segment)	0.0010	0.0005
	(0.0017)	(0.0018)
Entrants$_{ich}$ (segment just below)	0.0008	
	(0.0024)	
Entrants$_{ich}$ (segment just above)	−0.0021	
	(0.0034)	
Entrants$_{ich}$ (all other segments)	−0.0034**	
	(0.0013)	
Entrants$_{ich}$ (all segments below)		0.0010
		(0.0009)
Entrants$_{ich}$ (all segments above)		−0.0053**
		(0.0021)
Entrants$_{ich\text{-}1}$ (same segment)		0.0035*
		(0.0021)
Entrants$_{ich\text{-}1}$ (all segments below)		−0.0023**
		(0.0010)
Entrants$_{ich\text{-}1}$ (all segments above)		−0.0023*
		(0.0012)
Performance year t controls	Yes	Yes
Entry year h controls	Yes	Yes
Location type fixed effects	Yes	Yes
Org. form fixed effects	Yes	Yes
Brand fixed effects	Yes	Yes
Segment-year fixed effects	Yes	Yes
County clustering	Yes	Yes
R^2	0.7104	0.7141
N	171,831	171,831

Instead, the number of entrants in the more distant "all other segments" has the strongest and most significant negative effect on performance. Thus, the evidence continues to run counter to the competition neglect explanation.

That "competition neglect" cannot explain our findings is in line with institutional details about the hotel industry. It takes time to build a new hotel, and an entry decision is revealed long before the hotel is completed (as described in Section II, a developer can observe other developers' construction decisions while they are still at the planning stage, before construction begins). So it is unlikely that market participants were "surprised" by the number of rival entrants.

As a further test of the validity of the herding explanation, we now refine the analysis in Table VIII (column (1)). The herding model in Section I is based on entrants being able to observe the entry decision of the informed entrant. In practice, it would be sufficient to observe the *start* of a hotel's construction. Importantly, it takes longer to build a hotel operating in a higher quality segment. This allows us to design a new test. Specifically, if two hotels entered different segments in a given county c and year h, then the hotel that entered the lower quality segment is much more likely to have imitated the other hotel's entry decision. The higher quality hotel's construction would have taken much longer than the lower quality hotel's, so the higher quality hotel's construction would have started much earlier than that of the lower quality hotel. Thus, the lower quality hotel's entry decision would have been made *after* the higher quality hotel's construction started. So if the two opened (started operating) in the same year h, the lower quality hotel is more likely to have imitated the higher quality hotel's entry decision—not the other way around.

The herding model thus predicts that the performance of a hotel opened in year h is negatively associated with the entry (also in year h) of hotels in higher quality segments, while there should be no effect from entry in lower quality segments. However, performance should be negatively associated with the entry of hotels in lower quality segments in the preceding year, $h{-}1$ (the difference in time-to-build is one to two years, depending on the segments).

Based on the above logic, we adapt equation (6) by splitting the variable *Entrants* as follows: we use as main explanatory variables *Entrants (same segment), Entrants (all segments below)* (including entrants in all segments of lower quality), and *Entrants (all segments above)* (including entrants in all higher quality segments). Importantly, we include the number of entrants in each of these categories for both year h and year $h - 1$. The current competition variables (*Hotels in County*) are similarly split.

The results are reported in Table VIII, column (2). For a given hotel, the number of entrants in higher quality segments (which take longer to build) during both the same year (h) and the previous year ($h - 1$) have a negative effect on performance. Similarly, the number of entrants in lower quality segments during the preceding year ($h - 1$) has a negative effect. However, the number of entrants in lower quality segments during the same year (h) does not have a significant effect on performance. These results support the herding explanation: earlier decisions to enter different segments, which were likely observed by later entrants (and thus belonged to their information set), correlate negatively with the performance of the later entrants.

C. Two-Stage Approach

To shed further light on the forces at work, we now separate the decision to open a hotel, possibly based on herding (imitation), from the effect that simultaneous entry has on a given hotel's performance. We do so using a two-stage least squares approach.

In the first stage, we examine which hotels are more likely to have entered a county partly motivated by herding, that is, based on noisy signals and on inference drawn from observing other entrants' decisions. The most likely scenario consistent with herding is an entrant's decision to choose a particular segment even though most other entry was concentrated in a different segment (this entrant's performance is then expected to be particularly low according to our model). We distinguish hotels that opened during such an other-segment boom from hotels that opened during same-segment booms (most entry was in the same segment) and from booms in which no segment attracted the majority of entrants. If entry happened during the latter types of booms, or if it happened during a nonboom year, the pool of entrants likely includes both informed nonherders and less well-informed herders.

The instruments for the entry decision (nonbooms; other-segment booms; all other booms) are characteristics of the economic environment that market participants can observe when they make the entry decision. Given the differences in time-to-build for hotels in different segments (see Section II), we assume that Economy and Midscale hotels made their entry decision in the year before the hotel started to operate, while more upscale hotels made their decision two years earlier. That is, we use the variable b to denote the year of a hotel's entry decision (the start of its construction), where $b = h - 1$ for economy and midscale hotels and $b = h - 2$ for upscale, upper upscale, and luxury hotels. Given this taxonomy, we now also simplify our definition of segments to economy, midscale (with and without food and beverage), and upscale (which includes upscale, upper upscale, and luxury).

The first-stage instruments we use are the county GDP growth rate from years $b - 3$ to b, the normalized standard deviation of county GDP growth from years $b - 3$ to b, the logarithm of county GDP in year b, the logarithm of housing prices in year b, and the stock of hotels in the same and other segments in year b. Intuitively, economic variables at the time of the entry decision affect the entry decision. However, these variables should not affect a hotel's later performance, other than through the types of hotels that self-select to enter during each entry regime, once we control for the hotels' observable characteristics and market economic conditions at time t. In other words, the exclusion restriction is satisfied in our setting: economic conditions in year b should not have a direct effect on a hotel's performance measured in year t if we control for economic conditions in year t itself.[28]

[28] The exclusion restriction is actually satisfied by definition under any Markov chain representation of the economic environment.

We consider a county as having experienced a boom only if there was enough entry cyclicality, by restricting our attention to counties that had five or more entrants in at least one year.[29] We use two definitions of county booms. The first definition classifies a county-year as a boom year if the entry intensity was among the top 10% of years for that county. The second definition classifies a county-year as a boom year if that year represented a peak of entry activity in that county, that is, at least five hotels entered in that year, and the year was preceded by nonnegative entry growth and followed by a decline in hotel entry. Under the second definition, there were 552 peak years in 230 counties that experienced sufficient entry cyclicality. That is, on average each county experienced between two and three boom cycles (two to three peak years). This is consistent with the graphical evidence presented in Figures 4 and 5.

We classify a boom as an "*other-segment boom*" if a hotel enters a given segment and the fraction of hotels entering one other segment in years $b-1$ and $b-2$ was particularly high, that is, 50% higher (or more) than the average proportion of hotels entering that segment historically. For example, if the historical proportion of economy hotels in counties that experienced a boom is 32%, we classify a boom-year entry as *Entry in other-segment boom* if a hotel enters in boom year b (top 10% of entry activity in a county) in, say, the midscale segment, and in years $b-1$ and $b-2$ the fraction of economy hotels that started construction in that county was higher than 48% ($= 1.5\times 32\%$). Similarly, we classify a boom-year entry as *Entry in peak year & other-segment boom* if a hotel enters in peak year b in, say, the midscale segment, and in the years $b-1$ and $b-2$ the fraction of economy hotels that started construction in that county was higher than 48%. (Using different cutoffs, e.g., 25% or 100% instead of 50%, yields similar results.)

Table IX, Panels A and B present the first-stage regressions (separately for the two definitions of booms), and Panel C reports the second-stage results. We provide the coefficients on the organizational form dummies (company-managed and independent—franchise is the default) in the first stage. These dummies are not part of the set of *excluded* instruments in the second stage (they are also used as controls in the second stage). Showing these coefficients in the first stage is useful as they can shed some light on whether herding is more likely for entrants with a particular organizational form. Specifically, independent entrants may be on average less well informed, in which case herding is more likely for independent hotels: maybe brand owners give useful advice to company-managed and franchised hotels, or the prior experience in hotel management, required by some franchisors, makes some franchisees better informed.

Panels A and B show that independent hotels are more likely to enter in a boom in which most other entrants target a different segment. This is consistent with the intuition just described that independent hotels are opened by investors who are more prone to herding, maybe because they are less well

[29] This criterion is satisfied by 230 counties. Using stronger requirements (e.g., 7 or 10 entrants) yields similar results.

Table IX

2SLS, Entry, and Performance

The table reports the results from two sets of 2SLS estimations. Panels A and B report first-stage regressions, and the second stages are shown in columns (1) and (2) of Panel C. In the first stages, we estimate the probability of entry during boom years in a county, using as instruments several variables (listed in the text) capturing the economic environment at the time the decisions to start hotel construction were made (year b). We define year b as the opening year $h-1$ for economy and midscale hotels, and $h-2$ for upscale hotels. In the second stages, we estimate the impact of various entry boom years on hotel performance. Panel A defines boom years as years that are in the top 10% years of hotel entry in a county (with at least five entrants in a year). Panel B defines peak years as years in a county (with at least five entrants in a year) that were preceded by nonnegative entry growth and followed by a decline in hotel entry. In both first-stage regressions we differentiate entry during booms (peaks) for hotels that entered in a segment different from the category (or categories) that experienced unusual growth, and hotels that entered in all other types of booms (booms in the same segment, or booms with no single segment growing preponderantly). Control variables included in both the first-stage and second-stage regressions include the year t control variables included in previous specifications, organizational form fixed effects, location type fixed effects, brand fixed effects, *Cohort Effect*, and segment-year fixed effects. In all regressions, robust standard errors (reported in parentheses) are adjusted for heteroskedasticity and county-level clustering. *, **, and *** indicate significant at the 10%, 5%, and 1% level, respectively.

Panel A: 2SLS, First Stage: Entry in a Boom (Top Decile Years of Entry in a County)

Variable	Entry during other-segment boom	Entry during all other booms
Company Managed	−0.0056	−0.0002
	(0.0049)	(0.0085)
Independent	0.0506**	0.0123
	(0.0211)	(0.0290)
All control variables and fixed effects included in the second stage	Yes	Yes
Year b instruments	Yes	Yes
County clustering	Yes	Yes
Weak identification test (joint F-statistic)	39.6	
N	171,831	171,831

Panel B: 2SLS, First Stage: Entry in a Boom (Peak Years in a County)

Variable	Entry in peak year & other-segment boom	Entry in other peak years
Company Managed	−0.0012	−0.0028
	(0.0059)	(0.0070)
Independent	0.0527**	0.0215
	(0.0248)	(0.0373)
All control variables and fixed effects included in the second stage	Yes	Yes
Year b instruments	Yes	Yes
County clustering	Yes	Yes
Weak identification test (joint F-statistic)	56.1	
N	171,831	171,831

(*Continued*)

Table IX—*Continued*

Panel C: 2SLS, Second Stage: Effect on Performance of Entering During Other-Segment Booms, or All Other Booms

Variable	Boom: Top decile years of entry in a county log(RevPAR) (1)	Boom: Peak years in a county log(RevPAR) (2)
Entry during other-segment boom	−1.0546***	
	(0.2622)	
Entry during all other booms	0.1322	
	(0.0936)	
Entry in peak year & other-segment boom		−0.8642***
		(0.1488)
Entry in other peak years		−0.0745
		(0.0805)
Control variables	Yes	Yes
Fixed effects	Yes	Yes
County clustering	Yes	Yes
N	171,831	171,831

informed than the average entrant. Interestingly, independent hotels are not more likely to enter during all other booms (i.e., booms in the same segment, or booms with no single segment growing preponderantly). Both first-stage regressions show that the instruments are strong, with F-statistics of 39.6 and 56.1, respectively. These numbers exceed the threshold of $F = 10$.[30]

Panel C shows that, when a hotel enters in a boom in a segment different from the booming segment, the performance is worse. This is consistent with Hypothesis 2: an entrant's realized value of entering is on average lower if more entrants entered in a different segment.[31]

[30] The threshold of $F = 10$ follows a rule of thumb in which the maximal bias in the IV estimation is no more than 10% of the bias of an OLS estimation. Using Stock and Yogo (2005) exact critical values for our estimations, the precise value of F is 11.02, which is substantially lower than the F-tests we report.

[31] Using county fixed effects is not appropriate in our setting: doing so would bias the coefficients of interest, since we would be comparing differences in investment cyclicality across all counties, including counties that did not experience a boom. Additionally, in the tests reported in Table IX, it is *infeasible* to use county fixed effects for the entire sample. That would eliminate observations from counties that did not experience any booms, since any county fixed effects included in the second stage of the 2SLS regression should necessarily also be included in the first-stage regressions, for reasons of identification (see Angrist and Pischke (2009)). But in the first-stage regression, those county fixed effects would be perfectly collinear with the predicted probability of entry for hotels in counties that never experienced a boom. The first-stage regression would thus suffer from perfect failure/success determination issues for those hotels, making the 2SLS analysis infeasible unless those counties (without booms) are dropped. We would then lose an important control group—hotels in counties without investment cyclicality. For robustness, we have replicated our results for the subset of counties that have substantial investment variation using county fixed effects. Our results are qualitatively unchanged.

In sum, it is possible to empirically separate the motivation of entry (the likelihood of entry being motivated by herding) from the effects that intense contemporaneous entry has on a hotel's performance. The results provide further support for the herding explanation.

VII. Conclusions

In this paper, we use a unique proprietary micro-level data set from the U.S. hotel industry to study investment cycles and how the performance of an investment is affected by its timing over a cycle. The evidence we have presented in this paper is intriguing. Why are hotels built in booms at the local level? And why do hotels built during booms underperform others for decades? Our interpretation of the evidence is that there is herding: the decision to build a hotel is made under great uncertainty about future demand, and relying on information inferred from other market participants' actions is therefore tempting.

There is a large body of theoretical work on herding, but this literature does not make predictions about performance. Moreover, empirical evidence on herding and its consequences is scarce. The main reason are difficulties in obtaining appropriate micro-level data that allow for rigorous tests. Specifically, measuring the performance of an investment is hard if performance data are reported at the corporate level, not at the level of a particular investment. Furthermore, there can be many different reasons for imitative behavior, and identifying such reasons is challenging. Herding can arise if decisions are based on noisy information, or in the presence of career concerns (so the destruction of information is the goal). Imitation can also be spurious, as information that is available to market participants might be unavailable to researchers.

Our detailed project/investment-level data allow us to overcome many of these difficulties. Unobserved positive information cannot be driving our findings, since investments made during the peak of a cycle *under*perform others. Career concerns in connection with investment decisions are not an issue either, since the vast majority of investments into hotel developments is made by individuals, partnerships, or LLCs. Moreover, our performance measure is not aggregated over several investments, since we measure performance at the hotel level rather than at a more aggregate company level. Additionally, our data include important hotel and market characteristics that also tend to affect performance, allowing us to control for factors that might confound with local and aggregate investment cycles.

The evidence supports the implications of our model, in which potential entrants with noisy information about the attractiveness of an investment update their beliefs after observing a better-informed entrant's decision. Intuitively, an agent with strong signals about a particular segment may wrongly infer from a better-informed agent's entry into a *different* segment that the market is *generally* attractive, across all segments, with adverse consequences for the realized value of entering. Performance also suffers if the less well-informed agent imitates the better-informed agent's segment choice, if the latter's entry decision was motivated mostly by an unusual opportunity that cannot be replicated.

We consider several alternative explanations for our empirical findings. They fail to fully explain the findings, and the more promising explanation makes predictions that are contradicted by the data. Only the herding explanation is consistent with all the tests we perform.

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Internet Appendix