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FIRM-SPECIFIC HUMAN CAPITAL, ORGANIZATIONAL INCENTIVES, AND AGENCY COSTS: EVIDENCE FROM RETAIL BANKING

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This paper explores conflicting implications of firm-specific human capital (FSHC) for firm performance. Existing theory predicts a productivity effect that can be enhanced with strong incentives. We propose an offsetting agency effect: FSHC may facilitate more-sophisticated 'gaming' of incentives, to the detriment of firm performance. Using a unique dataset from a multiunit retail bank, we document both effects and estimate their net impact. Managers with superior FSHC are more productive in selling loans but are also more likely to manipulate loan terms to increase incentive payouts. We find that resulting profits are two percentage points lower for high-FSHC managers. Finally, profit losses increase more rapidly for high-FSHC managers, indicating adverse learning. Our results suggest that FSHC can create agency costs that outweigh its productive benefits. Copyright © 2013 John Wiley & Sons, Ltd.

INTRODUCTION

The design of organizational incentives is of crucial importance for firms. Accordingly, a broad range of research demonstrates the effects of organizational incentives on employee behavior and business performance (Foss, 2003; Vroom and Gimeno, 2007; Zenger, 1994). Yet just as the same employee will react differently to alternative incentive instruments (Lazear, 2000; Zenger and Marshall, 2000), heterogeneous employees will react differently to the same incentive instrument. Indeed, because the central problem in incentive design is how to influence the behavior of autonomous human agents, the theory of incentives is intimately linked to human capital theory. Coff (1997: 387) notes that, unlike physical assets, human beings inherently give rise to agency problems and that 'firms are more likely to generate rent from human assets when they adopt rent-sharing strategies (e.g., profit sharing, group incentives, or performance-based compensation)'. This perspective raises the possibility that the effectiveness of strong organizational incentives increases with the quality of human assets. Correspondingly, the conclusion that incentives and human capital are complements is standard in the research literature on organizations (Huckman and Pisano, 2006; Ichniowski, Shaw, and Prennushi, 1997; Milgrom and Roberts, 1995).

However, there is ample anecdotal evidence that—for a given contract—the combination of strong financial incentives and highly skilled employees may actually be counterproductive for the firm. For example, a best-selling chronicle of the Enron saga is entitled 'The Smartest Guys in the Room' (McLean and Elkind, 2003)—a reference to the highly compensated 'whiz kids' whose dubious financial engineering fueled the company's rise and fall. Similarly, recent financial

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crises suggest that high-powered incentives can contribute to long-term destruction of value (Sorkin, 2010). These examples highlight the hidden costs of organizational incentives, which have been recognized ever since the seminal work by Kerr (1975).

While the dark side of incentives is well documented, the examples of Enron and the financial crisis suggest something more: that human capital may exacerbate the problem. In this paper, we maintain that the agency costs from incentive gaming may be increasing in employee human capital—introducing a trade-off that has heretofore not been recognized in the literature. On the one hand, high-human-capital employees have a greater latent potential to generate profits for the firm, thus increasing the firm's returns to offering stronger incentives. On the other hand, these employees may also be more skilled at exploiting incentive systems for private gain at the firm's expense, thus reducing the firm's returns to stronger incentives. In this paper we develop theoretically and test empirically these conflicting implications of human capital for employee responses to incentives and organizational performance.

We test our hypotheses using detailed daily records from a multiunit retail bank over a 13-month period following the introduction of a new incentive plan. Branch managers' pay is tightly tied to performance, and they have some autonomy in operating decisions such as the loan terms offered to clients. We group managers according to their accuracy in predicting performance targets set at bank headquarters. Because managers are not informed about how targets are set, prediction ability reflects superior organizational knowledge—a form of firm-specific human capital (Becker, 1962; Nonaka, 1994). Using a novel empirical strategy to estimate local demand for loans, we show that—compared with either of two theoretical benchmarks—greater firm-specific human capital is associated with a two percentage point *reduction* in the organization's profits. We build up to this result with an in-depth analysis of the conflicting implications of human capital for organizational outcomes. We find that managers with superior organizational knowledge are more productive by a variety of measures. However, we also find that they are more likely to manipulate loan terms to increase incentive payouts. Finally, the proportion of profits lost due to incentive gaming increases more rapidly for managers with superior organizational knowledge—suggesting that this metric is associated with an underlying learning ability.

Our theory and empirical results imply that firm-specific human capital *may* be counterproductive on net, not that it is necessarily so. Our contribution is to highlight a previously unrecognized trade-off between the productivity and agency implications of human capital—fully recognizing that this trade-off is subject to contingencies that will vary across contexts and hierarchical levels.[1] Nonetheless, our empirical results challenge the traditional assumption—dating to Becker (1962)—that human capital is an unalloyed good for the firm (see also Amit and Schoemaker, 1993; Hall, 1992). This challenge has been raised by others, but for different reasons. In the existing literature, the overriding concern is bargaining: human capital may be costly because employees may have bargaining power (Blyler and Coff, 2003; Coff, 1999; Dencker, 2009) or may be unwilling to apply their human capital without financial inducements (Coff, 1997). In contrast, we submit that for a fixed incentive contract—that is, controlling for these bargaining mechanisms—higher levels of human capital can decrease firm performance through gaming of precisely those incentive contracts that the extant research literature proposes as a solution to the problem of human capital.

Our research provides a new perspective on the long-standing debate about the benefits and costs of specialization.[2] Ever since Adam Smith, it has been generally recognized that specialization is beneficial because it leads to increased productivity and hence wealth creation. However, specialization may lead to agency costs from increased information asymmetry (North, 1990) and fears of value expropriation (Amit and Schoemaker, 1993). Because firm-specific human capital results from specialization, we propose the existence of an important trade-off between its productive and adverse consequences, which we empirically document.

In addition, our study sheds light on how bounded rationality influences the efficacy of

[1] We note that we develop a novel and replicable empirical methodology to investigate these contingency factors. Our method also provides one possible path for estimating rent appropriation in firms, which Coff (1999) has highlighted as a major challenge in organizational research.

[2] We are grateful to the associate editor, Joe Mahoney, for pointing us in this direction.

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contracts. Firm-specific human capital reflects employees' understanding of the environment in which they are operating. Azoulay and Shane (2001) show that entrepreneurs differ in their ability to estimate the payoffs of different contract provisions, affecting franchise survival rates. Similarly, Argyres and Mayer (2007) maintain that different kinds of employees have different capabilities with respect to contract design and that superior performance depends upon the alignment of contract terms, transaction attributes, and employees' contracting capabilities. In this paper, we contribute to this literature by showing not only that bounded rationality affects contract design but also how employees respond to a given incentive contract.

Finally, our results contribute to our understanding of the sources of heterogeneity in opportunistic behaviors. Harris and Bromiley (2007), for example, find that the structure of organizational incentives affects the likelihood of financial misrepresentation. Pierce (2012) shows how varying incentives can hamper knowledge transfer within hierarchies. Yet Nagin *et al.* (2002) find that many employees under an output-contingent pay plan refrain from opportunities to game their incentives, and this reluctance is partly explained by employees' feelings about how their employer treats them. An alternative hypothesis, put forward in the current paper, is that employees differ in their ability to game their incentives because they vary in their understanding of how the organization works. Differences in this type of human capital seem to be associated with opportunistic behavior in the firm we study, and we find that the more knowledgeable employees are costly to the bank on net.

THEORY AND HYPOTHESES

Firm-specific human capital and productivity

The importance of human capital for productivity, firm performance, and competitive advantage has long been recognized (Barney, 1991; Becker, 1962; Penrose, 1959). Indeed, even to make the analogy between, on the one hand, the knowledge, skills, and abilities residing in a human being and, on the other hand, physical capital is to assume that human capital is, by its very nature, a productive asset. Empirical research has consistently found a positive relationship between a firm's ability to select, manage, and enhance its human resources and its labor productivity (Huselid, 1995; Koch and McGrath, 1996; Youndt *et al.*, 1996). Becker (1962) distinguishes two types of human capital: general (equally valuable to all firms) and specific (most valuable to a specific firm). Controlling for the former, our analysis focuses on the latter, which we refer to as firm-specific human capital (FSHC). Some research has argued that, of the two types of human capital, FSHC is the more valuable one for the focal firm (Hitt *et al.*, 2001; Huselid, 1995). As a form of human capital, FSHC should lead to higher productivity, as previous research suggests (Hatch and Dyer, 2004). Therefore, we hypothesize

> *H1* (*productivity effect): High firm-specific human capital will be associated with greater productivity*.

Firm-specific human capital and agency costs

Pay contingent on individual performance is commonly regarded as a means of increasing productivity and mitigating contracting problems (Holmstrom, 1979; Levinthal, 1988). Such incentives have been shown to motivate higher effort, decrease the free-riding problem, and increase productivity (Ichniowski *et al.*, 1997; Lazear, 2000; Paarsch and Shearer, 1999). Strong individual incentives also screen, via self-selection, for employees with higher human capital (Lazear, 2000; Zenger, 1994).

However, apart from their desirable properties, high-powered incentives can introduce an agency cost if employees game their incentives—i.e., divert their effort from 'true' to measured objectives—to maximize their private benefits (Harris and Bromiley, 2007; Larkin, 2013; Obloj and Sengul, 2012; Oyer, 1998). Accordingly, organizations might choose to reduce the intensity of incentives in order to avoid incentive gaming (Holmstrom and Milgrom, 1991; Zenger and Marshall, 2000).

Agency costs in incentive contracts exist because of 'human agency' (individuals' capacity to make choices). These problems are a natural consequence of specialization and the division of labor. Because of the costs of monitoring and policing performance, even slaves retain some measure of autonomy in decision making (North, 1990: 32–33). And while superior human capital may make employees more productive in their work, it does not ensure superior organizational

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performance. Rent generation is a necessary but not sufficient condition for appropriation (MacDonald and Ryall, 2004). In relating organizational resources to performance, one must jointly consider issues of value creation and value capture (Coff, 1999; Kim and Mahoney, 2010). While high levels of human capital are likely to result in increased value creation, they can also allow employees to appropriate a disproportionate amount of value through bargaining (Blyler and Coff, 2003; Carpenter, Sanders, and Gregersen, 2001; Dencker, 2009).

Holding bargaining concerns fixed, the extant literature suggests that one way in which organizations can generate more rents from their human assets is to offer stronger individual incentives (Coff, 1997). This assertion would indicate that the strength of incentives and human capital are complements (Huckman and Pisano, 2006; Ichniowski *et al.*, 1997; Milgrom and Roberts, 1995). The explanation for this relationship is simple and intuitive: the marginal enhancements to organizational performance—induced by the strength of incentives—are increasing in the levels of human capital.

However, we maintain that neither of the two assumptions on which this prediction rests—(1) that employees apply their human capital productively (from the firm's perspective) and (2) that the structure of incentives can successfully channel effort into productive output—necessarily holds. The same knowledge, skills, and abilities that make employees productive at the firm's chosen task may also make these employees more productive at finding and exploiting weaknesses in their incentive systems. This possibility is especially of concern for FSHC. Because it gives access to organizational routines and peculiarities of the organizational structure, it may also aid managers in exploiting these routines and structures for private gain. For example, CEOs Dennis Kozlowski (Tyco), Bernard Ebbers (WorldCom), and John Rigas (Adelphia) all went to prison for misleading investors about their firms' actual performance and expropriating value via performance-based compensation. All three CEOs relied on their CFOs' FSHC (detailed knowledge of the firms' accounts and transactions) to manipulate their performance measures (share price, income statements, etc.).[3] Therefore, the dark side of organizational incentives may be exacerbated by higher levels of FSHC. Accordingly, we hypothesize

H 2 (agency effect): High firm-specific human capital will be associated with an increased incidence of incentive gaming.

Firm-specific human capital and firm performance

We have argued that FSHC gives rise to two competing effects: a productivity effect (Hypothesis 1) and an agency effect (Hypothesis 2). The former effect is well understood and has been extensively examined. In contrast, the agency effects of FSHC have heretofore not been recognized and so there is no developed theory to guide predictions about which of the two effects should dominate—i.e., whether the net effect of FSHC for firm performance is positive or negative. We believe that the answer to this question will be context-dependent. For example, multitasking theory (Holmstrom and Milgrom, 1991) tells us that the severity of gaming will depend on factors such as the quality of performance measures, job design, and the employee's level of authority. Therefore, for our empirical setting, we remain agnostic about FSHC's net effect and hypothesize:

H3a: The net effect of high firm-specific human capital on organizational performance will be positive.

H3b: The net effect of high firm-specific human capital on organizational performance will be negative.

Dynamics: firm-specific human capital and adverse learning

Because employment relationships evolve over time, there is a dynamic aspect to incentive

[3] We define 'gaming' to be the manipulation by the agent of performance measures that are imperfectly correlated with value creation. This is the essential feature of agency models such as those in Baker (1992) and Gibbons (2005). This behavior may or may not be illegal. An apparently legal analogue to the examples given here is the Greek debt crisis. There, the Greek government (the agent) allegedly misrepresented to the European Union (the 'firm'/principal) its public debt (the performance measure) with the help of Goldman Sachs, which used its knowledge of the intricacies of EU law (its FSHC) to allow the Greek government to borrow through transactions that did not technically meet the definition of debt (*New York Times*, 2010).

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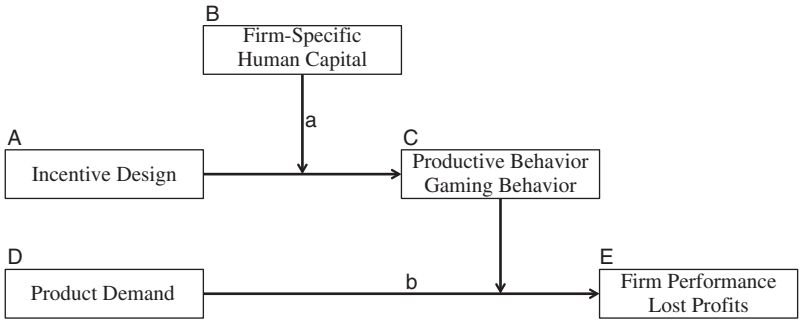

Figure 1. Structure of the empirical analysis

contracts. Just as employees learn, over time and with experience, how do their jobs more productively (Benkard, 2000), these employees may also learn how to game incentives for their private benefit (Obloj and Sengul, 2012). Such adverse learning would increase the incidence and cost of incentive gaming and exacerbate over time the agency effect that we posit in Hypothesis 2.

Yet the pace of adverse learning is unlikely to be uniform across employees and may be moderated by their human capital. Indeed, Hatch and Dyer (2004) show that human capital is an important determinant of productive learning-by-doing in that higher levels of human capital result in an increased pace of learning. Extending this logic, we posit that if FSHC allows employees to game incentives more, it is also likely to accelerate the pace of adverse learning. Consequently, we submit that agency losses due to gaming will increase over time and, particularly, that the pace of adverse learning will differ by FSHC. Thus we hypothesize:

Hypothesis 4 (adverse learning): Agency losses will increase at a faster rate for high levels of firm-specific human capital.

Figure 1 presents an outline of our subsequent analysis and empirical strategy. We first describe our data and setting, with emphasis on our measure of FSHC (block B). Subsequently, we empirically show the productivity and agency effects of FSHC (arrow a). Next, we estimate the net impact on the organization of these two competing effects. The first step in doing so is to estimate local demand functions for the organization's product (block D). This approach allows us to calculate 'lost profits' as the difference between potential and observed profits (block E). Next, we investigate how lost profits vary with our FSHC measure (arrow b). Finally, we introduce a dynamic analysis wherein we look for learning effects: differential rates of the evolution of lost profits by FSHC.

INSTITUTIONAL BACKGROUND AND INCENTIVE STRUCTURE

We examine a large private retail bank, employing several thousand people and serving loans to hundreds of thousands of customers yearly. Its focus is on the sale of simple banking products (e.g., deposit accounts and small consumption loans) to mass-market customers. The bank operates through a network of over 200 outlets located in large to midsize towns. A typical outlet employs three to four salespeople, including the outlet manager, who is responsible for meeting the outlet's sales target.

The division of labor at the bank creates an information asymmetry between branch managers and executives that might give rise to agency problems. Outlet managers' effort is not observable or contractible directly, and managers have local information about customers' habits, preferences, etc. All of these are difficult for executives to monitor at a distance. As a result, the bank gives managers some discretion over marketing expenditures, loan size, pricing, and approvals. To induce effort, outlet managers are rewarded based on an imperfect measure of performance: loan sales. This measurement is consistent with North's (1990) observation, discussed earlier, that non-zero monitoring costs will inevitably lead to some measure of employee autonomy. Furthermore, the bank's choice indicates that its cost of direct monitoring is high and that it is more efficient to align

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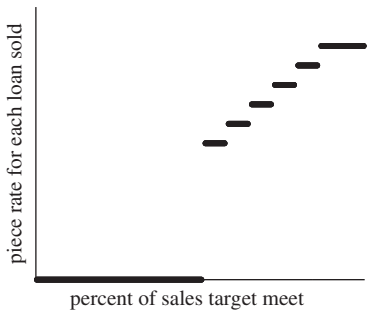
(even if imperfectly) mangers' interests with those of the bank via an incentive contract (Baker, 1992; Conlon and Parks, 1990).

During our sample period, the incentive scheme is constant and uniform across outlets. The pay of outlet managers and salespeople is tightly linked to outlet performance, with the variable share of managers' monthly pay averaging more than 40%. Each month, headquarters assigns each outlet a monetary sales target for 'primary loans' (loans sold to new customers). These loans are the bank's focus during the period we examine and account for 70% of pretax profits. Banks also sell 'secondary' loans to repeat customers, which do not feature in our study or in the incentive plan we describe here.[4]

Outlet managers receive a 'piece rate' bonus for each primary loan sold but only after the outlet's sales exceed 80% of the target. The bonus rate increases in a stepwise fashion until 130% of the sales target is reached, after which it remains constant (see Figure 2). This incentive design—featuring sales quotas with discrete bonus echelons—is the most widespread structure of sales compensation (Larkin, 2013; Oyer, 2000). It also closely resembles the structure of typical CEO incentive plans, described in Murphy and Jensen (2011). Although neither we nor the outlet managers know the exact algorithm by which sales targets are set, our data indicate that they are largely based on three factors: (1) the outlet's past performance, (2) the past performance of similar outlets, and (3) headquarters' analysis of market trends.

DATA DESCRIPTION AND VARIABLES

Our analysis draws on three sources: (1) archival sales (panel) data; (2) interviews with bank executives and managers (which yielded detailed knowledge about the bank's production process and incentive systems); and (3) a large-scale survey of outlet managers (which yielded information on a variety of managers' personal characteristics). Appendix A describes the interview and survey methodologies. In this section, we detail the archival data and the most important data from the surveys: the FSHC measure. Summary statistics are reported in Table 1.



Note: Y-axis values suppressed for confidentiality reasons.

Figure 2. Graphical representation of the primary-loan incentive plan

Our dataset contains confidential archival data on sales, loan performance, and incentives that span the 13 months following the bank's introduction of the incentive plan described previously. The dataset comprises all primary loans granted by all outlets during this time, a total of more than 500,000 loans.[5]

Loan interest rate

Because of confidentiality concerns, the bank did not release the exact interest rate of each loan; instead, loans were aggregated into groups of similar size and price. For each group, the data contain the loan interest rate category on a scale of 1 to 5. We worked closely with the bank's data coders to ensure that the category definitions were stable over time and equidistant from one another. Thus, our data are essentially linear transformations of the confidential values,

[4] Although primary loans are the bank's main source of profits, there may be concerns with omitting the secondary loans. The most serious of these concerns is that secondary loan sales might have patterns that offset those documented here for primary loans—for example, if apparent losses in primary loans among certain managers are compensated by gains in secondary loans among the same managers. Analyses of the secondary loans reported later in the paper do not support this hypothesis. Other possible concerns have to do with unobserved heterogeneity in the demand for secondary loans, which could affect managers' relative effort cost of selling primary loans, their propensity to sell 'bundled' primary and secondary loans at discounted prices, and so on. Bundling is almost entirely ruled out in our data. Clients are not eligible for a secondary loan until they have repaid their primary loan, typically after 10 months or more.

[5] For analyses that combine the archival and survey data, we drop outlets for which there is either turnover in the outlet manager position or missing data due to survey nonresponse. In the most restricted case this involves omitting 31% of outlets. We find no evidence to suggest that the retained outlets are nonrepresentative of the broader sample. See Appendix A for further discussion.

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Table 1. Summary statistics

	Mean	Min.	Max.	S.D.
Dependent variables				
Value-weighted daily interest rate[a]	3.67	1	5	1.07
Daily loan sales (volume)	0.18	0	3.24	0.19
Daily loan sales (number)	1.66	0	19	1.72
Lost profits	0.033	−0.74	0.71	0.11
Independent variables				
Plan position	0.48	0	3.31	0.35
Plan position dummy (0.8, 1.3)	0.16	0	1	0.36
Plan position dummy > 1.3	0.02	0	1	0.15
Monthly sales target[a]	4.23	0.1	11.42	1.63
Time remaining in the month	14.98	0	30	8.68
High firm-specific human capital	0.53	0	1	0.44
National central bank interest rate	4.2	4	4.75	0.25

[a] Transformed due to confidentiality of data.

and so standard linear techniques of analysis are appropriate.[6]

Number of loans

We observe the number of loans in each aggregated bundle corresponding to an observation in our data.

Sales target and plan position

We observe the exact value of the 'sales target' for each outlet each month. Because we also know the exact total value of loans issued per day, we can compute the outlet's daily 'plan position': its cumulative sales with respect to its monthly target.

Depending on the information in the loan application, a computer algorithm assigns the client to one of three risk categories. For clients in the lowest risk category, the outlet manager is fully empowered to grant the loan, which can be issued immediately ('fast loans'). For higher risk categories, a loan must be approved by the bank's risk department; this entails a delay of up to 30 days ('slow loans'). A loan is not included in the manager's performance measure until it is approved. Our interviews suggest that the risk management procedures are independent of outlet and manager characteristics and are also independent of outlet performance. One implication is that the entry of slow loans into the manager's performance measure is a random variable that is exogenous to any determinants of local consumer demand. We will exploit this fact later when we estimate these demand parameters. Note that the data give only the loan issue date, not the loan approval date. However, for fast loans the approval and issue dates almost always coincide. In much of the analysis that follows, we need to know the date on which the outlet manager approved the loan; hence, in these cases, the dataset includes only the fast loans.

Firm-specific human capital (FSHC)

Our measure of FSHC is motivated by Becker (1962: 17), which cites familiarity with the organization as a type of knowledge that raises productivity more in the focal firm than in other firms. We measure managers' familiarity with the organization through their prediction ability. In our surveys, outlet managers were asked to predict their sales targets for the following month for four products. We compare the predictions with the actual targets and separate managers into high- and low-prediction ability groups according to their average prediction error.

Accurate predictions reveal superior knowledge of how the organization works. The sales target is the bank's assessment of what the manager should sell in the upcoming month; it reflects bank executives' priorities, evaluation of future demand trends, and judgment of the manager's ability. It therefore draws on a variety of bank processes: strategic planning, demand forecasting,

[6] Some measurement error is introduced by aggregating continuous variables into categories, but this error's impact is mitigated because 60% of all observations consist of individual loans.

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and employee evaluation. To make accurate predictions, managers must infer not only the information held by executives but also the algorithm used to produce the sales targets—which is a closely guarded secret.[7] Prediction ability thus indicates superior insight into the bank's internal processes. Although the process of setting sales targets is only one aspect of the bank's operations, we will show in a moment that prediction ability is highly correlated with knowledge of how the bank works.

One potential concern with this measure is that it might simply measure lucky guesses. If every manager made a random guess, prediction accuracy would reveal nothing about managers' knowledge of the bank. Our measure is robust to this concern in three ways. First, if prediction ability were just random noise, it would have no explanatory power in our regressions, yet it does. Second, if our measure merely captured random luck, prediction errors across products would not be highly correlated, as they are. Finally, if the measure reflected only luck, then it would be uncorrelated with independent evaluations of managers' FSHC.

To investigate this last point, we provided four researchers, who were not familiar with our work, with transcripts of interviews conducted with 17 branch managers 1 month before the start of the period analyzed in this paper. Two of the researchers received full interview transcripts, and two received transcripts excluding responses to questions that directly asked about the incentive system. Based on the transcripts, the researchers independently rated each manager's organizational knowledge according to a definition based on Becker (1962) (see Appendix B) using a four-point Likert scale, which we converted to a binary scale. As is standard in the analysis of interviews, the researchers resolved differences in ratings by discussion (Earley and Mosakowski, 2000). Our prediction-based measure of FSHC matched with the interview-based ratings in 15 out of 17 cases for the full transcript group and in 14 out of 17 cases for the partial transcript group. Despite the small sample size, these correlations are statistically significant at $p < 0.01$ for both groups.

The independent transcript analysis validates the prediction-based measure in three important ways. First, it indicates that prediction ability does not merely measure random luck. Second, it indicates that prediction ability can justifiably be interpreted as an indicator of FSHC. Even though prediction ability is measured within a relatively narrow domain, it is highly correlated with more general assessments of managers' FSHC. Finally, the transcript analysis indicates that we measure a stable manager characteristic. This is because the interviews were conducted one month *before* the study's time frame, whereas managers' predictions were elicited three months *after* the study's time frame. Since we observe the predictions only once, if prediction ability changed over time (for example as a result of different rates of employee learning), then our measure would be more precise late in the observation period but noisier early on. The fact that independent measures spanning the study's time frame show very high levels of congruence indicates significant stability in managers' relative levels of FSHC.

We believe that our measure of FSHC allows us to proxy the actual content of managers' knowledge about the firm. Prior studies have predominantly used tenure as a measure of FSHC, both at the individual (Carpenter *et al.*, 2001; Pennings, Lee, and Van Witteloostuijn, 1998) and organizational level (Hitt *et al.*, 2001). However, while tenure is easily comparable across organizations, it is an imperfect measure, as it may not reflect the actual accumulation of knowledge and skills (Gimeno *et al.*, 1997). As mentioned below, in our empirical specifications we use tenure as a control variable.

Our prediction-based measure is conceptually similar to the one used in Ceci and Liker (1986), in which habitual horse-track patrons predicted the post-time odds for a set of races on the basis of factual information about the horses. The authors find that this prediction ability is distinct from general human capital and experience and is associated with a more cognitively complex use of information from the daily racing forms. The managers in our sample are similar to the horse handicappers in that, in order to formulate an accurate prediction of their sales targets, they need to make complex, context-specific inferences.

[7] In our survey, 85% of outlet managers either disagree or strongly disagree with the following statement: 'The bank informs me about how the sales plan for my unit is constructed'. Some readers may wonder why the bank deliberately withholds information about the contract from employees. A full justification is beyond the scope of this paper, but Ederer, Holden, and Meyer (2013) characterize conditions under which the principal is better-off providing agents with ambiguous incentive schemes.

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We should however emphasize that our interest in Ceci and Liker (1986) is because of the mechanics of their measure, not its content. In their setting, the human capital is clearly not firm-specific. In contrast, in our setting prediction ability reveals human capital that is specific, because it depends on knowledge of a firm-specific algorithm.[8]

In summary, FSHC (as measured by prediction ability) indicates organizational knowledge that we would expect to be useful in successfully operating a bank branch: optimally allocating the marketing budget; qualifying and prioritizing sales leads; managing staff; and so on. However, this same organizational knowledge could be useful in successfully running a bank branch for private gain: eluding credit controls; substituting price reductions for sales effort; and generally recognizing where the rules can be bent—and how far—without triggering sanctions. Our analysis will explore these conflicting implications of FSHC and estimate the net impact on the bank's profits.

Before proceeding, several points are worth mentioning. First, managers' predictions concern their future sales *targets*, not their future sales. As already noted, outlet managers do not participate in the target-setting process. Therefore, prediction ability does not indicate their ability to regulate their own output or negotiate targets. Second, prediction errors are not correlated with any observable outlet characteristics, including within-outlet monthly sales variance or sales target variance; this means that low prediction error does not merely measure outlets with more stable sales. In some specifications we include controls for outlet managers' personal characteristics obtained in our survey: gender, marital status, education, tenure, age, home ownership, and identification with the organization.[9] Third, we report results based on the 'raw' prediction error. None of our results are materially affected if we instead use the residual from an ordinary least squares (OLS) regression of the prediction error on the following fixed characteristics: the outlet's size, type, and location; and all of the fixed manager characteristics just described.

RESULTS

Evidence of productivity and agency effects of firm-specific human capital

Our central premise is that FSHC has both productivity and agency effects whose net impact on the employer's profits is theoretically indeterminate. In this section, we explore these conflicting effects separately. The link between FSHC and agency costs receives relatively more attention, since our main contribution is to highlight this side of the trade-off.

Firm-specific human capital and productivity

Table 2 shows how FSHC affects managers' productivity, measured by the average daily value and number of loans sold. Recall that the units for these variables have no economic interpretation due to the bank's rescaling of the data. All regressions include controls for the central bank interest rate, outlet type, region, quarter, week of the month, and quarter–week fixed effects. Column 1 shows that high-FSHC managers sell 0.01 more loan value units per day, equal to 5.6% of the average value sold per day by all managers. Column 4 shows that high-FSHC managers sell 0.115 more primary loans per day than low-FSHC managers, which represents 6.9% of the 1.66 average for all managers. These estimates remain stable when progressively adding controls for plan position (columns 2 and 5) and fixed manager characteristics (columns 3 and 6): education (dummy variable for MSc and above), tenure (in years), age (in years), gender, marital status, home ownership, and identification. Thus, we find strong support for Hypothesis 1, that FSHC is associated with increased productivity.

[8] As noted by an anonymous reviewer, prediction accuracy could reflect human capital that is not firm-specific—for example, knowledge of other banks' algorithms that resemble the one in use at our bank. To investigate this possibility, we examined the correlation between prediction ability and prior experience in the financial services industry. We find that the correlation is negative and insignificant, contrary to what we would expect if our prediction measure were picking up industry-specific human capital.

[9] Home ownership is an indicator equal to 1 if the manager pays a mortgage. Identification was measured in our survey using a scale developed by Elsbach and Bhattacharya (2001) and was based on the following statements: (1) The Bank's successes are my successes. (2) When someone praises the Bank, it feels like a personal compliment. (3) When someone criticizes the Bank, it feels like a personal insult. Cronbach's alpha = 0.79.

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Table 2. Productivity as a function of firm-specific human capital (FSHC)

	Dependent variable					
	Daily loan sales (volume)			Daily loan sales (number)		
	(1)	(2)	(3)	(4)	(5)	(6)
High FSHC	0.011***	0.009***	0.01***	0.115***	0.100**	0.110**
	(0.003)	(0.003)	(0.003)	(0.043)	(0.041)	(0.044)
Plan position		0.169***	0.166***		0.746***	0.753***
		(0.008)	(0.009)		(0.083)	(0.084)
Central Bank rate	0.037***	0.037***	0.038***	0.031	0.034	0.018
	(0.005)	(0.004)	(0.004)	(0.059)	(0.062)	(0.061)
Outlet type f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Quarter f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Region f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Week f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Region f.e. × quarter f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Personal traits	No	No	Yes	No	No	Yes
Observations	30,807	30,807	30,807	30,807	30,807	30,807
R-squared	0.17	0.31	0.33	0.36	0.43	0.45

*Significant at 10%; **significant at 5%, ***significant at 1%.
Coefficients from OLS regressions; constant included, not reported. Robust standard errors (clustering on outlet) in parentheses. Personal traits include: age, tenure, marital status, education, home ownership, and identification with the firm.

Firm-specific human capital and incentive gaming

A primary vehicle for gaming is managers' discretion over the price (interest rate) of the loan. One manager told us:

> *When a client walks into an outlet asking for a loan and I need to sell, there's no way she's going out without one. I'll match any competitor's price and add something on top.*

However, our interviews also suggest that managers use their discounting power sparingly:

> *Of course we give discounts. Everybody does. The trick is to give the discounts when you need to sell [loans] and the customer wants it, not just when the customer wants it.*

What would dissuade managers from giving the maximum discount all the time? Two possibilities are fear of sanctions and dynamic considerations. Managers pay a dynamic penalty for finishing the month too far behind or ahead of their sales targets. If a manager finishes far behind target, she risks being fired and incurring job-search costs. If she finishes far ahead of target, she risks having her sales target raised in the following month (recall that past performance is one predictor of the target), which means that her expected pay (net of effort costs) will decrease. Interviews with managers suggest that some are sensitive to dynamic considerations and seek to minimize deviations from their targets. One manager noted:

> *I know at all times where I stand with regard to the sales target. If I'm behind, I do all I can to catch up. If I'm ahead I take it easy.*

Next we investigate the following questions: Do managers use their discounting power to fine-tune their performance against target throughout the month? How responsive is the interest rate to distance from the sales target? How do these responses vary with FSHC?

Empirical specification

We estimate the following three models:

$$r_{u,t} = \beta_0 + \beta_1 PP_{u,t} + \beta_2 Z_{u,t} + \mu_u + \epsilon_{u,t} \quad (1)$$

$$r_{u,t} = \beta_0 + \beta_1 PP_{u,t} + \beta_2 Z_{u,t} + \beta_3 H_u + \epsilon_{u,t} \quad (2)$$

$$r_{u,t} = \beta_0 + \beta_1 PP_{u,t} + \beta_2 Z_{u,t} + \beta_4 \left(PP_{u,t} \times H_u\right) + \mu_u + \epsilon_{u,t} \quad (3)$$

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The dependent variable, $r_{u,t}$, is the value-weighted interest rate on loans sold by outlet u on day t. The independent variable $PP_{u,t}$ is the outlet's plan position at the start of day t. Z contains the same controls used earlier in the productivity analysis. H denotes the FSHC measure. The term μ_u is an outlet fixed effect. Because FSHC is time-invariant, when we introduce the variable H to Equation (2), we must drop μ_u since the two are not separately identifiable. However, we are mainly interested in the *interaction* of FSHC and plan position. This interaction can be separately identified from outlet fixed effects, which is the specification in Equation (3). One concern in estimating Equations (1)–(3) is that plan position measures where managers stand with respect to their sales target, so it could be affected by prior-period discounting. Any serial correlation in the error terms could lead to correlation between the plan position variable and the error term, which would lead to biased coefficient estimates (Greene, 2003). Using the test proposed by Wooldridge (2002: 282), we reject the hypothesis of serial correlation in our panel ($p = 0.37$).

Results

Table 3 reports the results. Columns 1–3 correspond to Equations (1)–(3), respectively. The positive coefficient on plan position in column 1 indicates that outlets charge lower prices (lower interest rates) when behind schedule and higher prices when they are ahead. This suggests that all managers use discounting as a tool to meet their own performance targets. Column 2 additionally shows that managers with high FSHC price lower on average. Finally, column 3 shows that the sensitivity of the loan price to plan position is greater for high-FSHC managers. They are more prone to use discounting to meet their own performance targets.

A manager's plan position will naturally improve as each month progresses, which means that a manager at 50% of plan in week 1 will not be as anxious to cut prices as a manager at 50% of plan in week 4 will be. The specifications in columns 1–3 contain week of the month dummies to control for this effect. With these controls, the plan position variable therefore measures "time-adjusted" performance—i.e., that portion of performance that is not explained by calendar effects. In column 4, we introduce a continuous calendar effects control (days remaining in the month); the results are qualitatively unchanged. Similarly, the results of columns 1–4 are qualitatively unchanged in columns 5–8, respectively, where we replace the continuous plan position measure with indicator variables for each of three performance thresholds: 50, 80, and 130% of plan. The 50% level (the omitted category) is an important psychological threshold according to our interviews. As discussed earlier, the 80% threshold is significant because it is here that managers begin earning the per-customer sales bonus; this bonus rate stops increasing at the 130% level. Finally, in columns 9 and 10, we replicate the specifications in columns 4 and 8 respectively, substituting outlet type effects for outlet fixed effects. The sales department categorizes outlets according to location type (hypermarket, city center, or suburban), outlet format (stand-alone or kiosk), and employment. Including the outlet type in columns 9 and 10 allows us to control for much of the outlet-level heterogeneity while still including the un-interacted FSHC measure. The results are qualitatively unchanged.

Columns 9 and 10 also include controls for two possible confounding factors in the analysis. The first is identification with the firm, defined earlier, which may cause managers to behave less opportunistically (Nagin *et al.*, 2002). The second possible confound is general human capital. Columns 9 and 10 show that our results are robust to the inclusion of the identification variable and a battery of variables that should be correlated with managers' general knowledge and sophistication: age, marital status, education, and home ownership.

In summary, Table 3 shows that high-FSHC managers both offer lower prices on average and use their discounting power more aggressively to meet their sales targets. The lower average prices among high-FSHC managers could indicate one of several things. First, they could be evidence that *low*-FSHC managers are setting sub-optimally *high* prices and thus selling too few loans. However, the profit analysis below shows that it is the high-FSHC managers' prices that are sub-optimal. Second, the lower prices could reflect a more sophisticated 'loss-leader' sales strategy, if high-FSHC managers attract new customers with low interest rates and later sell them secondary loans. However, high-FSHC managers neither sell significantly more secondary loans, nor are the

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Table 3. Loan interest rate as a function of position in sales plan

	Main results								Robustness checks	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Plan position	0.141***	0.119***	0.123***	0.154***					0.137***	
	(0.031)	(0.04)	(0.036)	(0.042)					(0.02)	
Plan position dummy (0.80, 1.30)					0.08***	0.074***	0.077***	0.078***		0.081***
					(0.02)	(0.02)	(0.02)	(0.02)		(0.02)
Plan position dummy > 1.30					0.15***	0.13***	0.18***	0.17***		0.18***
					(0.04)	(0.04)	(0.05)	(0.05)		(0.05)
High firm-specific human capital (FSHC)		−0.036**				−0.036**			−0.036***	−0.036**
		(0.015)				(0.015)			(0.01)	(0.017)
Plan position × high FSHC			0.043**	0.026**					0.02**	
			(0.019)	(0.01)					(0.01)	
Plan position dummy (0.80, 1.30) × high FSHC							0.04*	0.03*		0.04*
							(0.023)	(0.02)		(0.02)
Plan position dummy > 1.30 × high FSHC							0.07**	0.05**		0.08**
							(0.03)	(0.02)		(0.04)
Time remaining in the month				0.009***				0.006***	0.008***	0.007***
				(0.002)				(0.002)	(0.003)	(0.002)
Central Bank rate	1.01***	1.02***	1.05***	1.04***	1.00***		1.04***	1.04***	1.05***	1.04***
	(0.25)	(0.25)	(0.26)	(0.25)	(0.24)		(0.25)	(0.25)	(0.25)	(0.24)
Outlet f.e.	Yes	No	Yes	Yes	Yes	No	Yes	Yes	No	No
Outlet type f.e.	No	No	No	No	No	No	No	No	Yes	Yes
Quarter f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region f.e. × quarter f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Personal traits (age, tenure, marital status, education, home ownership, identification)	No	No	No	No	No	No	No	No	Yes	Yes
Observations	39,609	30,807	30,807	30,807	39,609	30,807	30,807	30,807	30,807	30,807
R-squared	0.47	0.08	0.49	0.51	0.47	0.09	0.49	0.50	0.11	0.11

OLS estimates. Robust standard errors, clustered by outlet, in parentheses; constant included but not reported.
*Significant at 10%; **significant at 5%; ***significant at 1%.

interest rates on those loans significantly higher. Third, lower prices might indicate negative risk premia if high-FSHC managers attract higher-quality borrowers. However, high-FSHC managers actually have worse-performing loan portfolios. Finally, lower interest rates could reflect agency costs: using the interest rate as a substitute for effort in selling loans. This explanation is further suggested by the significant interaction of high FSHC with plan position. These results therefore provide strong support for Hypothesis 2: that higher FSHC is associated with higher levels of incentive gaming. This conclusion is reinforced by the lost profits analysis reported in the next section.

Firm-specific human capital and firm performance: lost profits analysis

The foregoing analysis suggests that high-FSHC managers are both more productive at selling loans and more likely to engage in opportunistic behavior that is personally beneficial but costly to the bank. In this section, we estimate the net impact of FSHC on the bank's profits according to the following thought experiment: For a given loan demand, what profits does the branch earn, compared with what it 'should' earn (a benchmark to be described below)? Productive effects of FSHC will move the branch's observed profits closer to the theoretical benchmark, while agency

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cost effects will move them farther away. If FSHC is a net asset to the bank, then high-FSHC managers will come closer to the benchmark; if not, then low-FSHC managers will come closer. Our general approach therefore resembles the theoretical 'lost profits' analysis in Anton and Yao (2007), and we will refer to the distance from the benchmark as 'lost profits.' Such an analysis poses two challenges: determining what the bank's objective is (in order to construct the benchmark) and estimating the bank's demand for loans. We address these challenges in turn immediately below.

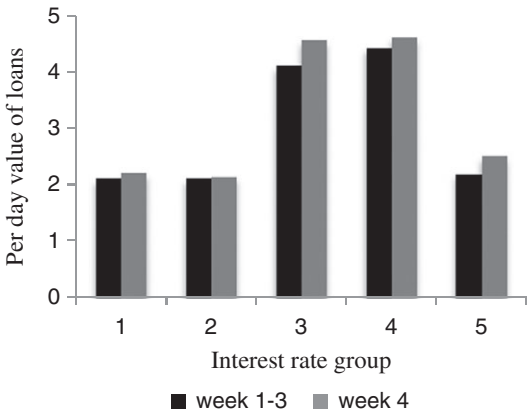
The bank's objective: the fourth-week effect

Our interviews give us direct insight into the bank's objectives, against which managers' behavior can be evaluated. One bank executive told us

> *There usually is a fourth-week week effect. In some months we do not observe it as clearly as in others but the demand tends to increase late in the month. Of course this gives [outlet] directors an opportunity to boost their sales. This is why we discourage them from lowering their prices late in the month.*

The 'fourth-week' effect (which is driven partly by the behavior of consumers 'bridging' to the next payday) is visible in our data. Figure 3 compares the average daily value of loans sold (by interest rate group) in the first three weeks of the month versus the fourth week.[10] In each group, average daily sales are higher in the fourth week than in the first three weeks of the month (all differences are statistically significant). That is, conditional on the price, loan sales are higher in the fourth week; this observation is consistent with the reported spike in demand.

The bank's policy of discouraging fourth-week discounts is a clear statement of the bank's objectives, consistent with profit-maximizing behavior: simple supply-and-demand models predict that prices should rise in periods of peak demand. Yet



Note: *Y*-axis values transformed for confidentiality reasons.

Figure 3. Daily loan sales (value) by interest rate group

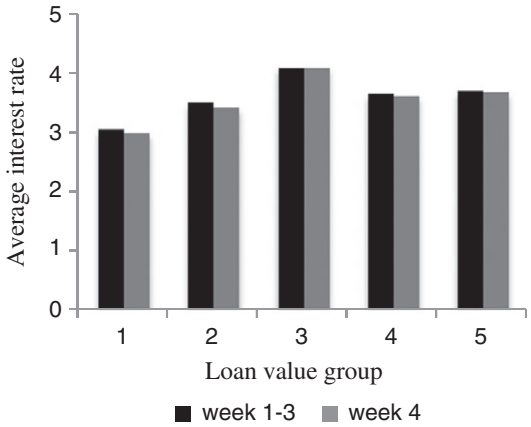


Figure 4. Average interest rate by loan value group

this is not what actually occurs, as illustrated in Figure 4. In all but one loan size group (group 3), the average interest rate granted in the fourth week is significantly lower ($t > 3$, $p < 0.01$) than in the first 3 weeks.

Is it possible that bank executives err in their pricing guidelines? That is, could fourth-week discounting actually be profit-maximizing? Chevalier, Kashyap, and Rossi (2003) discuss and test three classes of models that justify price reductions in peak demand periods: (I) cyclical demand elasticities; (II) countercyclical collusion models (collusive agreements are more likely to break down when demand rises); and (III) loss-leader advertising models. None of these models appears to be relevant in our setting.

With regard to type I models, we estimate the bank's demand function and find no evidence that

[10] We divide each month into four 'weeks.' Because these 'weeks' are of unequal duration across different months, the figure reports statistics at the daily level. The results are robust to alternative division patterns.

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demand elasticity changes in week four. Type II models do not apply here for two reasons. First, most evidence in favor of such models indicates defection from collusive agreements during peak demand *seasons* (Borenstein and Shepard, 1996). Yet the bank's demand cycle is measured in weeks, not months, and a collusive agreement that breaks down every fourth week for exactly one week is implausible. Anecdotal evidence from the bank is also inconsistent with collusion, since branch managers report that they compete aggressively for new clients. Finally, bank executives engaged in countercyclical collusion would *not* discourage price discounting in week four. Type III—the loss-leader advertising model for which Chevalier *et al.* (2003) find support in grocery retailing—has at least three features that are inconsistent with our setting: (1) advertising campaigns and promotional prices timed to coincide with the demand increase (such promotions last for weeks or even months, whereas the bank's demand increase lasts only about one week); (2) the potential for the retailer to 'hold up' the consumer because of the latter's sunk travel costs (a customer is more likely to walk away from an overpriced loan than from an overpriced can of green beans); and (3) high-margin products that are bought *concurrently* with the loss-leader product (in contrast, the bank's complementary products are typically sold at a later date).

To summarize, if demand elasticity does not change in week four (and we will show that it does not), then we can take executives' instructions to maintain price levels as a credible statement of a profit-maximizing objective. We can therefore estimate lost profits by comparing observed profits with the profits the bank would have earned had the executives' instructions been followed.

Potential sales: the bank's loan demand function

Estimating lost profits requires knowing the bank's demand for loans as a function of the interest rate. We estimate this demand function at the local level using a novel identification strategy. We have shown that managers manipulate the price of loans in response to daily changes in their performance against the sales plan. These changes are partially driven by two exogenous factors: (1) daily shocks to local customer demand (e.g., events within the branch's catchment area that affect the arrival rate of potential customers at that branch); and (2) the random arrival of 'slow loans' (those sold in the past but requiring centralized approval, as discussed earlier). These factors constitute exogenous sources of variation in the daily performance of managers at otherwise comparable outlets. If the shocks on date $t-1$ (which shift the manager's performance against plan and thereby alter his loan supply decisions on date t) are uncorrelated with the shocks on date t (which shift the manager's observed sales on date t), then performance against plan at the start of date t is a statistically valid instrument for the date-t price when estimating the average daily demand for loans. We shall present evidence that the combined daily shocks from demand heterogeneity and arrival of slow loans are not serially correlated, which supports our instrumentation strategy.

Empirical specification

We model an outlet's daily demand for primary loans as follows:

$$Y_{u,t} = \beta_0 + \beta_1 r_{u,t} + \beta_2 \left(r_{u,t} \times I_{week-4}\right) + \beta_3 \left(X_{u,t}\right) + \epsilon_{u,t} \quad (4)$$

where Y denotes the value of loans sold by outlet u on day t, and $r_{u,t}$ denotes the value-weighted interest rate.[11] We allow the slope of the demand function to change in the last week of the month (indicator $I_{\text{week-4}}$). The vector X includes controls for region, month, outlet type, and week of the month. Thus, our specification posits that the level of demand can vary by region, time, and outlet characteristics. Our instrument for the endogenous interest rate is the measure of managers' performance against their incentive plan: plan position.

We emphasize that the purpose of our demand estimation is to measure lost profits *within the bank*, not to estimate the impact of conduct by the bank's rivals. Our demand function is therefore not a market-level demand function but rather the bank's *own* demand function. Whatever the nature of competition in this industry, the impact

[11] Although loans differ in size, the price does not differ statistically across the different size categories. Hence we estimate the demand function as daily demand for monetary value of loans. We obtain virtually identical results when using the number of loans as the dependent variable.

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of rivals' behavior is reflected in the parameters we estimate.[12]

Results

Table 4 reports OLS estimates and first- and second-stage estimates for the instrumental variables specification. The estimated slope of the demand function is not statistically different in week four than in the month's first three weeks,[13] so we report results for the pooled estimation.

Our main focus is not on the parameters of the demand function itself; rather, we seek to use the demand function to estimate lost profits. Hence we discuss Table 4 briefly. First, we cannot reject the hypothesis of zero autocorrelation of errors in the first-stage regression ($p = 0.39$), which supports the main identifying assumption behind our instrument. The Hausman test rejects the null hypothesis of equality of coefficients obtained under OLS and two-stage least squares (2SLS) ($\chi^2 = 159.68$, $p < 0.01$), and the instrument is highly significant in the first-stage regression ($F = 105.47$). The instrumental variables estimation also moves the point estimate of the price coefficient in the expected direction (we would expect OLS estimates to be biased upward). Finally, our point estimates imply a price elasticity of loan demand that is in line with values documented by Dehejia, Montgomery, and Morduch (2012) providing additional evidence that our instrument is correcting appropriately for the endogeneity of the price variable.[14]

With the demand function in hand, we need only marginal cost to compute lost profits. We assume that the bank's marginal cost of loans is the interest rate offered on its savings deposit accounts. Because our data on loan interest rates are disguised and rescaled by the bank, we obtain the savings interest rate on the same scale using the procedure described in Appendix C.

Firm-specific human capital and lost profits

Predicting hypothetical profits is inherently difficult. Therefore, we rely on two benchmarks, which can be viewed as establishing bounds on lost profits. The first benchmark ('fourth-week effect') is based on bank executives' pricing guidelines discussed above. Using this benchmark, we define lost profits as the difference between observed profits and the profits that would result if, in week four, managers maintained prices at the levels observed during the first three weeks. This is a conservative estimate, because our results indicate that managers manipulate loan terms throughout the month, and so week 1–3 prices are likely to be sub-optimal themselves. Our second benchmark ('monopoly pricing') is the profits the bank would have earned had managers priced as monopolists under the estimated demand function. This benchmark considers behavior throughout the month, but it is likely to overstate lost profits. Because primary-loan customers may buy complementary products in the future, it is possible that the price that maximizes long-run profits is below the price that maximizes short-run profits from primary loan sales.

Table 5 compares the average observed daily price with the profit-maximizing price (Benchmark 2, monopoly pricing), first pooled across all outlets and then separately for managers with high and low FSHC. The table reveals that managers price loans well below the theoretical profit-maximizing level—at about 83% of the benchmark value. Furthermore, the table shows that managers with high FSHC offer significantly lower prices than managers with low FSHC, both in absolute terms and relative to the profit-maximizing price.[15]

[12] As pointed out by a referee, this behavior is reflected *on average* in our estimates, but this average may mask variation across markets due to differences in the intensity of local competition. This could be a problem if the distribution of FSHC across markets is correlated with the intensity of price competition. However, our data indicate that this is not the case. The probability that a given outlet is led by a high-FSHC manager is not affected by any of the following indicators of the intensity of competition: outlet size, location, type, and regional GDP per capita.

[13] Since 'week four' is an ambiguous concept in months of more than 28 days, we perform robustness checks in which that week is defined as the last 5, 6, 7, 8, or 9 days of the month. We find no significant differences in the slope estimates for any of these alternate specifications.

[14] The demand elasticity that we find is −0.87; Dehejia *et al.* (2012) report elasticities ranging from −0.73 to −1.04.

[15] Because the dependent variables in our counterfactual scenarios are nonlinear functions of the estimated demand parameters, statistical inference is based on bootstrapping. The theoretical basis for bootstrapping is described in Efron and Tibshirani (1993). We conservatively report significance levels based on a 'two-tailed' test. Note that inference is based on the *actual distribution* of estimated coefficients. Because this distribution need not be normal, standard errors can be misleading guides to significance levels and so we do not report them.

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Table 4. Demand estimation

Dependent variable	OLS Daily demand	Stage 1 Interest rate	2SLS with instrument Daily demand
Interest rate	0.158***		−0.873***
	(0.004)		(0.105)
Constant	2.338***		7.189***
	(0.034)		(0.589)
Plan position		0.169***	
		(0.012)	
Region fixed effects	Yes	Yes	Yes
Outlet fixed effects	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes
R-squared	0.08	0.27	
Observations	39,609	39,609	39,609

Robust standard errors, clustered by outlet, in parentheses.
***Significant at 1%.

Table 5. Profit-maximizing prices versus observed prices

All outlets		High FSHC		Low FSHC		Difference in p-max.	Difference in p-obs.
p-max.	p-obs.	p-max.	p-obs.	p-max.	p-obs.	high−low[a]	high−low[a]
4.86	4.03	4.84	3.98	4.89	4.08	−0.05***	−0.10***

[a] Differences are significant at the 0.01 level both for the absolute measure of prediction ability and for the residual-imputed measure.
***Significant at 1%. Significance levels based on bootstrapping. Because inference is based on the empirical distribution of estimated coefficients, standard errors are not reported.
Profit-maximizing price is a theoretical value computed from estimated demand parameters.

In Table 6 we compare the bank's actual profits with the profits it would have earned under each of the two benchmark scenarios. Because the bank provided 'transformed' data, the profit levels have no economic meaning and so we report only the *ratio* of actual to theoretical profits. The first row of Table 6 reports the comparison with Benchmark 1 (fourth-week effect) and shows that managers' pricing decisions cost the bank on average 3% of its profits. The profit losses are significantly greater for managers with high FSHC. Do these figures really represent lost profits? One alternative possibility is that the bank executives' instructions not to lower prices in week 4 are mistaken. We have already discussed why this is unlikely. Furthermore, even if it were true, to interpret branch managers' behavior as profit-maximizing for the bank would require two rather unorthodox assumptions: (1) that the managers (agents) know better than the executives (principals) what the firm's objective function is; and (2) that the agents are behaving altruistically. A more conservative position is to take the bank executives' statement of their objectives at face value. The other possible explanation for the results in the first row of Table 6 is that they represent exceptions to the injunction on price reductions that the bank executives would endorse. For example, it might be optimal for high-FSHC managers to offer lower loan interest rates because either they are better at spotting low-risk borrowers or they are better at selling follow-on products. However, we have already discussed that high-FSHC managers do not sell more or higher-priced secondary loans. Finally, high-FSHC managers may be better at engaging in price discrimination. However, the results obtained using Benchmark 1 are inconsistent with this hypothesis. Benchmark 1 is based on comparing managers' week-4 prices with *their own* prices in weeks 1–3. We find that the elasticity of demand is constant throughout the month. If managers are optimally price discriminating, then we should observe the same pattern of discounting throughout the month within an outlet. This is not what we find. Also, the price discrimination hypothesis does not explain

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Table 6. Actual profits versus theoretical profits (ratio)

Theoretical benchmark	All outlets (aggregate)	High FSHC	Low FSHC	Difference high − low[a]
1	0.97	0.96	0.98	−0.02**
2	0.88	0.87	0.89	−0.02***

[a] Differences are significant at the 0.05 level both for the absolute measure of FSHC and for the residual-imputed measure.
**Significant at 5%; ***Significant at 1%. Significance levels based on bootstrapping. Because inference is based on the empirical distribution of estimated coefficients, standard errors are not reported.
Benchmark 1 assumes that managers maintain loan prices in week 4 at the level of weeks 1–3; Benchmark 2 assumes profit maximization based on estimated demand parameters.

why high-FSHC managers' discounting is more sensitive to plan position. Therefore, we conclude that Benchmark 1 provides a reliable lower bound on lost profits.

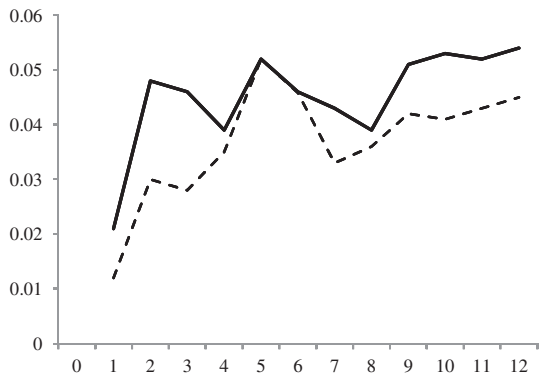
The second row of Table 6 shows the lost profit estimates based on Benchmark 2 (monopoly pricing). On average, the bank loses 12% of its profits as a result of managers' pricing decisions. This loss is greater for managers with high FSHC: 13% versus 11%. Thus, by either benchmark, estimates of lost profits are higher for managers with higher FSHC. These differences are statistically and economically significant.

To summarize, the evidence supports the conclusion that, although high-FSHC managers are more productive in their primary task of acquiring new customers (selling primary loans), this apparent productivity comes at a high cost. These same managers are more likely to engage in opportunistic behavior that increases incentive payouts but reduces the bank's profits. The net effect is at least a two percentage point reduction in profits. Thus, for this organization, the evidence favors Hypothesis 3b over Hypothesis 3a.

Dynamic effects of firm-specific human capital: adverse learning

As mentioned previously, managers' learning may work in two directions. On the one hand, it may increase their productivity, as much research would suggest (Benkard, 2000). On the other hand, it may lead to increasing agency costs if managers are mainly learning about opportunities to game their incentives. If the latter effect is present, then we would expect to see the ratio of actual to theoretical profits decrease over time.

Figure 5 plots average lost profits, which are defined as 1 − (actual profits ÷ theoretical profits),



Key: solid line = high firm-specific human capital (FSHC); dashed line = low FSHC.
Note: See text for definition of lost profits (vertical axis). Units for the horizontal axis are months elapsed since the incentive plan's introduction.

Figure 5. Lost profits—time trend

over time.[16] Lost profits are defined according to Benchmark 1 (fourth-week effect). The plot makes it clear that lost profits increase with time, and it also confirms that managers with high FSHC are associated with higher average lost profits. However, the figure does not indicate whether the *rate* of increase in lost profits differs across the two FSHC types. To examine this question, we estimate a log of the following learning model:

$$Lost\ profits_{u,t} = (Elapsed\ time)^{\beta_1+\beta_2 FSHC_u+Z} \quad (5)$$

Here time is measured in months elapsed since the introduction of the new incentive regime, and Z is a vector of control variables.

[16] Observe that the demand level (and hence theoretical profits) may vary monthly and so the analysis controls for general demand trends.

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Table 7. Lost profits—learning effects

	(1)	(2)	(3)	(4)
Elapsed time	0.01***	0.009***	0.008***	0.007***
High firm-specific human capital (FSHC)		0.002***		
Elapsed time × high FSHC				0.005**
Outlet fixed effects	No	No	Yes	Yes
R-squared	0.07	0.08	0.53	0.55
No. observations	2,759	2,165	2,759	2,165

**Significant at 5%; ***significant at 1%. Significance levels based on bootstrapping. Because inference is based on the empirical distribution of estimated coefficients, standard errors are not reported.
Dependent variable is log[1 − (actual profits ÷ theoretical profits)]; because the argument of the log function can take negative values, we add 1 before evaluating it. Constant included but not reported.

Results are reported in Table 7. Columns 1 and 2 report results of the model without outlet fixed effects, while columns 3 and 4 report coefficient estimates for fixed effects models; we focus the discussion on column 3. The point estimate for β_1 is positive and significant, in line with the general increase in lost profits observed in Figure 5. The point estimate of β_2 is positive and significant, indicating that lost profits increase more rapidly for managers with high FSHC. Overall, these results strongly suggest that adverse learning is present and, in addition, that this phenomenon is more pronounced for managers with high FSHC—supporting Hypothesis 4.

We should point out that it is not possible to identify conclusively learning effects off of a *single* time series. However, we observe *different* time series for high- and low-FSHC managers. These differences are unlikely to be caused by external factors, such as changes in monitoring intensity or demand characteristics, since these would affect high- and low-FSHC managers equally.[17] A learning hypothesis is more plausible, especially because bank executives tell us they believe it is happening:

> *They [managers] all try to game the system. Fortunately it takes them time to figure out how to do it. In fact, as soon as I realize that they have figured out how to game, I start to think about the new structure of incentives*. (*Sales Director*)

CONCLUSION

Our goal was to explore the productive and adverse consequences of employee firm-specific human capital (FSHC) and learning for organizational performance in the presence of strong financial incentives. Empirically, we find that high-FSHC managers are more productive in their primary task of customer acquisition, but they also are more likely to engage in costly loan term manipulation that boosts their incentive payouts. The net effect of FSHC is a two percentage point reduction in the bank's profits. Finally, lost profits increase more rapidly over time for managers with superior FSHC, suggesting that adverse learning is present and that the rate of adverse learning is greater for the high-FSHC managers.

Our results highlight an important trade-off that links human capital theory and incentive theory in a way that neither research literature has recognized to date. We also offer a new answer to a long-standing puzzle: Why do not all employees behave opportunistically, as the agency model would suggest? Past work (Nagin *et al.*, 2002) has proposed a form of positive reciprocity for good treatment by the employer. In contrast, our results indicate that 'gaming' the system is more widespread among managers who have greater

[17] If the bank increased monitoring in a nonuniform way, the most likely scenario is that it would focus on managers who show signs of gaming. This would make it *harder* for the differences we observe to emerge, because it would disproportionately affect the high-FSHC types who are gaming more. Changes in demand that are correlated with managers' FSHC are—we believe—less plausible than a learning hypothesis, especially in light of the extensive controls in the demand estimation.

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insight into the bank's internal operations. We are however not claiming to offer a complete theory of all of the reasons why employees game their incentives. Therefore, while we focus in particular on FSHC, we do not believe that it is the only determinant. Factors such as inherent honesty, risk tolerance, or personal wealth could also drive gaming behavior. We believe that more research is needed to understand more fully the drivers and consequences of adverse responses to organizational incentives.

There is an extensive literature suggesting that human capital and strong financial incentives are complements.[18] We neither dispute nor overturn those results here; rather, we propose that the complementarity, though pervasive, is not universal. A growing body of literature indicates that explicit performance incentives induce not only productive responses (Lazear, 2000; Paarsch and Shearer, 1999) but also distortionary, gaming responses (Harris and Bromiley, 2007; Larkin, 2013; Oyer, 1998; Pierce, 2012). We add to this literature by proposing and showing empirically that these perverse effects—corresponding to the multitasking problem described in Holmstrom and Milgrom (1991)—may be amplified by FSHC.

While we believe that this mechanism is a general one, we also acknowledge that its importance may vary with the context. Our setting resembles any contractual arrangement with separation of ownership and control in the presence of asymmetric information (Williamson, 2005). Indeed, it seems that the 'agency issue is ubiquitous in hierarchical organizations' (North, 1990: 32). However, in cases of very high incentive alignment (e.g., a self-financed entrepreneur), incentive gaming will not arise and therefore will not be exacerbated by FSHC. Similarly, at levels of the hierarchy other than the one that we study, the problem may be altered due to differences in the costs and efficiency of monitoring and the nature of the FSHC. Finally, the size of the organization is potentially another important contingency factor. In general, we should expect the magnitude of agency costs to increase with the size of the organization because monitoring becomes more costly and information asymmetry and specialization of labor increase (North, 1990; Rosen, 1982; Williamson, 1991).[19]

One additional question raised by our results is: Has the bank written a sub-optimal contract with its employees? We do not believe that we can answer this question unequivocally.[20] On the one hand, if the contract is sub-optimal, then it would appear that the bank is not alone. Murphy and Jensen (2011) observe that incentive plans that closely resemble those at the bank are common for CEOs at large US public firms. The authors argue that these incentive plans are sub-optimal and relay anecdotal evidence of the same types of gaming strategies that we document. On the other hand, the bank managers' incentive plan may well be optimally designed. In any contract, including the optimal one, we would expect heterogeneous managers to display heterogeneous behavior. Also, in any contract subject to multitasking we would expect to observe effort 'distortion' (e.g., gaming) in equilibrium. We might even expect to find that the 'best' managers are more costly to the bank. For example, a typical result in contract theory models is that different agent types earn different levels of 'information rents' (Laffont and Tirole, 1988). This could mean that high information rents appropriated by managers with high levels of FSHC are an inevitable cost of using high-powered incentives. Also, some theories in behavioral economics predict that 'naïve' types subsidize 'sophisticated' types in equilibrium (e.g., Gabaix and Laibson, 2006). This could well be happening at the bank. For example, our evidence suggests that high-FSHC managers learn quickly how to game the incentives, whereas low types learn more slowly.

The evidence that managers learn to game the system also means that there is an important dynamic element to this problem that previous work has overlooked. Because of the phenomenon of 'adverse learning,' an initially optimal contract may not remain so indefinitely. Therefore, one possible response to the problems we identify is for firms to change their incentive plans when the costs of gaming become excessive (Obloj and

[18] Relevant recent studies include Milgrom and Roberts (1995), Ichniowski *et al.* (1997), Huckman and Pisano (2006); and Lemieux, McLeod, and Parent (2009).

[19] We thank an anonymous reviewer for suggesting these important contingencies.

[20] Indeed, some research has suggested that such a question is unanswerable: '. . . it seems unlikely that economic research will ever tell us exactly where the typical CEO pay arrangement lies on the spectrum from completely inefficient to completely optimal' (Oyer and Schaefer, 2011:18).

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Sengul, 2012). Indeed, the bank managers tell us that exactly such a game of 'cat and mouse' is behind their periodic changes in their incentive plans. This highlights an important point. The firm may temporarily 'tolerate' the agency costs we identify (since securing loans through the existing production process may be less costly than alternative means of securing loans). However, because adverse learning leads to mounting agency costs over time, the firm is unlikely to remain indifferent forever.

There are several possible remedies for the agency costs we identify. Prior research shows that firms may strategically reduce the strength of incentives in order to mitigate multitasking distortions such as those seen here (Baker, 1992; Zenger and Marshall, 2000). Many of the distorted behaviors we observe at the bank are a response to nonlinearities in the bonus formula (e.g. performance thresholds and targets). Murphy and Jensen (2011) argue that more linear pay schedules can lead to less distorted behavior. Furthermore, if the bank could commit to a constant bonus schedule, rather than 'ratcheting' up performance standards over time, this might also discourage the gaming behaviors we observe. The bank could also increase monitoring or limit managers' autonomy. Finally, it could strengthen its recruiting procedures and/or replace high-FSHC managers. However, these last two remedies are problematic. First, it may be very difficult *ex ante* to screen employees who are likely to acquire harmful forms of human capital. Second, *ex post* identification is also difficult because of the information asymmetry problem and because high-FSHC managers appear to be more productive on several performance measures.[21] Still, firms might be wise to distinguish productive and dangerous forms of FSHC in deciding which information and learning opportunities employees are exposed to. For example, it is no accident that the bank keeps its target-setting formula a secret. Similarly, a bank might want to discourage interactions between commercial employees and controllers, as many do.

Beyond this, current theory offers little guidance, because it has not considered the conflicting implications of human capital that we highlight here. More generally, the contracting problem we examine could be framed as one of an imperfectly informed principal facing boundedly rational agents. We know of no existing theory that treats this problem. It is our hope that this paper will inspire future work aimed at closing these theoretical gaps.

[21] Please note that we can identify the high-FSHC managers because they revealed information to us (under strict confidentiality) that they would be unlikely to reveal to their supervisors.

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APPENDIX A.

Data collection methodology

The data collection procedure consisted of three phases: interviews, survey, and archival data collection. The data collected from archival sources are detailed in the main body of the article. Here we briefly describe the other two phases.

In the first phase, we conducted interviews with the bank's top executive team (CEO, Sales Director, Human Resources Director, and Risk and Accounting Director) followed by semi-structured interviews with 17 outlet managers in different regions of the country. Most of the interviews were recorded. In cases where managers objected to being recorded, two researchers took notes and compared them immediately after the interview. Each interview lasted between 40 and 90 minutes.

In the second phase, we administered an online survey to all outlet managers in the bank. Our choice of questions and measurement scales was guided by the interviews from the first phase and by a review of existing literature. Before administering the final survey, we pretested it with academics and bank executives to ensure clarity and unidimensionality of the measures; this led to several revisions of the initial questionnaire. We then sent the final survey to all outlet managers of the bank. Following the guidelines of Dillman (1978), we mailed two follow-up letters to all nonrespondents. More than 200 usable surveys were returned, a response rate exceeding 86%. In 43% of the outlets that did not respond there was a vacant manager position rather than a nonresponding manager. We found no significant nonresponse bias with regard to outlet type, outlet size, outlet performance, or outlet managers' personal traits. Neither did we find any significant differences in the responses across the different waves of the survey.

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APPENDIX B.

Validation of firm-specific human capital measure

Two researchers who validated the prediction ability measure received 17 full interview transcripts (totaling over 140,000 words). Two other researchers received interview transcripts from which we removed all questions directly asking about the incentive plans (about 10–20% of the transcript length). All four were then asked to respond to the following statement:

Please rate the interviewee's familiarity with their organization (i.e., how well they understand how the organization works).

APPENDIX C.

Cost of loan capital

The bank provided data on the timing of television advertisements for its primary loans, from which we were able to trace the advertised interest rate. We took the rate offered during the first promotion in our sample period (which was in the first of the 13 months studied) as our baseline loan interest rate, and we matched this to the interest rate offered on a deposit account during the same time period. We then computed the savings interest rate scaled to our data as the product of (a) the ratio of the deposit interest rate to the loan interest rate and (b) the lowest loan interest rate value in our disguised data. Because the central bank's base interest rate rose during our sample period, we assume that the bank's marginal cost does not remain constant. Therefore, we allow for the marginal cost to change in proportion to changes in the central bank's base interest rate.

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