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Incentive Redesign and Collaboration in Organizations: Evidence from a Natural Experiment

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Research summary: *Separating the individual from the social effects of incentives has been challenging because of the possibility of synergies in team production. We observe a unique natural experiment in a South Korean e-commerce company in which a switch from pay-for-performance to fixed (but different) salaries took place in a staggered and effectively random manner across employees. In this case, social and individual effects perspectives make opposing predictions, enabling a critical test. We find evidence consistent with social effects of incentives, particularly as predicted by goal framing theory. The results have implications for the design of incentives to foster collaboration, organizational learning, and organizational performance.*

Managerial summary: *Managers often neglect the deeper hypothesis behind pay-for-performance schemes—that people primarily care about how much they are individually paid. An opposing school of thought contends that incentives have social effects too—that individuals care about not only what they receive but also what their peers receive. It is difficult to say whether individual or social effects would be more salient in a context, without a proper experiment with randomization. We exploit a rare opportunity provided by a company that changed its incentive system in a random order, thus unintentionally creating a natural experiment. The results strongly validate the existence of social effects of incentives, but also make the general case for the* opportunity to learn *from experimenting with organization design in a systematic manner.*

Incentives continue to attract significant interest in the field of strategy (e.g., Baumann & Stieglitz, 2014; Frank & Obloj, 2014; Lee & Meyer-Doyle, 2017). However, while there is a broad consensus that incentives matter, precisely *how* they matter remains a contested theoretical ground. In particular, a key fault line in the thinking about incentives lies between the views of the individual *versus* social effects of incentives.

By *individual effects*, we mean the ways in which incentives alter an individual A's compensation and therefore change A's behavior. The change in A's compensation can arise either because A's own incentive contract changes or that of others change. In addition, the incentive contracts in question could be either team or individual level (Kretschmer & Puranam, 2008; Oxley & Pandher, 2016). As long as a change in incentives causes a change in A's behavior *via* a change in A's compensation, we refer to it as an individual effect of incentives.

By the *social effects* of incentives, we mean the ways in which changing B's incentives (which may be of an individual or a team level) may lead to a change in A's behavior or performance, even though A's compensation remains unchanged. The perspective that focuses on the individual effects of incentives is strongly rooted in the tradition of

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agency theory in economics (Holmstrom & Milgrom, 1994; Jensen & Meckling, 1976), whereas that on the social effects of incentives is behavioral in nature, and draws its roots from the social psychological research on fairness (e.g., Adams, 1963) and on the salience of group *versus* individual interests (e.g., Lindenberg, 2001).

Testing the individual and social effects of incentives as competing explanations for observed behavior has proven to be challenging, for at least two reasons. The first is the usual problem of causal inference from naturally occurring data. A number of creative efforts in recent years have tackled this problem by employing experimental designs in the field, in which the treatment is a change in incentive regime (e.g., Burks, Carpenter, & Goette, 2009; Shearer, 2004). The second is more subtle; individual and social effects can be easily confounded under conditions of team production with synergies (Shaw, Gupta, & Delery, 2002; Wageman & Baker, 1997). To appreciate this second point, it is useful to think of incentives along two well-established dimensions: depth—what fraction of an individual's pay depends on performance, and breadth—how many others contribute to the performance measure used to determine pay (e.g., Baker, 2002; Kretschmer & Puranam, 2008; Oxley & Pandher, 2016; Rivkin & Siggelkow, 2003).

When one switches from fixed salary to pay-for-performance (e.g., Burks et al., 2009; Lazear, 2000; Shearer, 2004) the depth of incentives is increasing. If one switches from individual pay-for-performance to group pay-for-performance, the breadth is increasing (e.g., Beersma et al., 2003; Petty, Singleton, & Connell, 1992). A move from fixed pay to group pay-for-performance indicates an increase in both depth and breadth. Assume we observe an increase in collaborative behaviors by individuals under any of these changes. However, from this we could not infer that social effects exist, as these changes could arise purely from individual effects as incentives for individuals will change in each of these cases in the presence of synergies from collaboration (Milgrom & Roberts, 1995). For instance, a move from individual pay-for-performance to group pay-for-performance could trigger collaboration as long as the gains from synergy are sufficient to deter free-riding (Wageman & Baker, 1997), and a move from fixed salary to individual performance-based pay could do the same if individuals are able to coordinate to capture synergies (Camerer & Knez, 1996; Kretschmer & Puranam, 2008). Thus, what look like social effects of incentives can in fact be due purely to changes in individuals' incentives.

One approach to cleanly separate the individual from the social effects of incentives for the purposes of establishing their independent existence is to look for the kind of incentive regime change that leads to sharply distinct predictions from the two perspectives. A change that meets these requirements is that from individual pay-for-performance to fixed salary. In this case, the social effects perspective, as we explain in detail in our theorizing, predicts an increase in collaborative behaviors under at least some conditions, but the individual effects perspective rules this out. A critical test of these perspectives is thus possible (Lave & March, 1975; Platt, 1964). We were fortunate to observe exactly this transition in incentive regimes in a South Korean e-commerce company (that we name "Chimera"), in which a switch from pay-for-performance to fixed (but different) salaries took place in a staggered and effectively random manner across employees. Exploiting this setting, we run a competing test of three theories—*agency theory* (Eisenhardt, 1989), which represents an individual effects perspective, and *equity theory* (Adams, 1963) and *goal framing theory* (Lindenberg & Foss, 2011), which both represent the social effects perspectives—that each make different predictions about the individual *versus* social effect of incentives, and thus allows us to separate the two effects.

As with most field experiments, what we gain in terms of closer-to-causal inference comes at the expense of generalizability (e.g., Burks et al., 2009; Lazear, 2000; Shearer, 2004). We believe that offsetting these limits to generalizability, our analysis offers unique contributions in terms of being able to cleanly separate social from individual effects of incentives change in a field setting. Our critical test (with close-to-causal inference) of agency theory *versus* equity theory *versus* goal framing theory is among the first efforts to provide field evidence for the latter.

We also believe that understanding the social and individual effects of incentives and what that means for collaboration in organizations is of critical interest to students of strategy. The competitive advantage of organizations rests not only on their resource endowments (Barney, 1991), but also on the capabilities embodied in patterns of collaboration, knowledge sharing, and support

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among its constituent agents (Gottschalg & Zollo, 2007; Teece, Pisano, & Shuen, 1997). A sophisticated approach to the design of incentives can thus play an important role in building and maintaining these advantages (see also Agarwal, Croson, & Mahoney, 2010; Fey & Furu, 2008).

Switching from Pay-for-performance to Fixed Salary: Individual and Social Effect Perspectives

We develop the arguments from both the individual and social effects perspectives on the implications for (a) effort toward own tasks (hereafter referred to as "production efforts"), (b) efforts toward helping others ("outgoing collaboration"), as well as (c) being helped by others ("incoming collaboration") of a change in incentives from pay-for-performance to fixed salary.

The most prevalent "individual effects" perspective on incentives in the strategy literature is anchored in the economics of agency relationships (Holmstrom & Milgrom, 1994; Jensen & Meckling, 1976). Agency theory assumes that agents are risk averse and rational actors motivated by self-interest, and hence principals can motivate agents by controlling their incentives (Eisenhardt, 1989). While the evidence on the role of risk-bearing in incentive contracts is not compelling, the basic premise of agency theory—that given costly efforts, incentives have to be set such that the benefit of effort exceeds the costs—is well accepted (e.g., Prendergast, 1999, 2000). This focus on the marginal effects of incentives, rather than the absolute level of compensation, is unique to agency theory.

Many agency theory-based studies of incentive compensation support the notion that performance-related pay can drive an agent to significantly increase her effort level on her own tasks—that is, increase production efforts. For example, Lazear (2000) shows that the productivity of windshield installers in a large auto glass company increased 44% when the installers' pay scheme was changed from fixed wage to pay-for-performance. Similar increases in task productivity have been demonstrated in a randomized field experiment with Canadian tree planters (Shearer, 2004). To our knowledge, no study so far has examined a change in the opposite direction, such as the one we consider. Yet, within the agency theory paradigm, the predictions are straightforward, as they require a simple reversal of the arguments for the move from fixed salary to individual pay-for-performance.

A change in an individual's incentives from pay-for-performance to fixed salary should, according to agency theory, lead to an unambiguous decline in production efforts by an individual. This is because the returns from effort no longer bear a relationship to effort levels, when salary is fixed. Given costly efforts, it is rational to therefore reduce effort levels. The effect on production efforts of the change we contemplate should therefore be negative.

Social effects are assumed to be nonexistent in agency theory. This implies that the result of a change in a focal agent's incentives from pay-for-performance to fixed salary on collaboration efforts is either zero (if collaboration did not occur under the previous regime), or negative (if under pay-for-performance, agents had coordinated on a collaborative equilibrium to manage synergies), but it cannot be positive. By symmetry, this applies to both incoming and outgoing collaboration.

Since social effects do not exist in this perspective, it also follows that changes in the incentives of the team (which includes coworkers), from individual pay-for-performance to fixed salary should also have either no effect or negative effects on a focal worker's production and collaboration efforts. The negative effects are feasible for the same reasons as noted above—if collaboration did occur in the pay-for-performance regime, it would decline. These agency theory predictions can be summarized as follows:

Hypotheses 1a (H1a): The effect of a focal individual's incentives changing from pay-for-performance to fixed salary is negative on production efforts.

Hypotheses 1b (H1b): The effect of a focal individual's incentives changing from pay-for-performance to fixed salary is non-positive (either negative or zero) on the focal individual's own collaboration efforts, as well as the collaboration received from coworkers.

Hypotheses 2 (H2): The effect of the team's incentives changing from

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pay-for-performance to fixed salary is non-positive (either negative or zero) on the focal individual's production and collaboration efforts.

In contrast, equity theory (Adams, 1963) states that individuals care about fair treatment *in respect to* their closest comparable peers (also see Fehr & Schmidt, 1999; Festinger, 1954). The key premise of the theory is that people will care not only about the absolute amount of rewards they receive, but also the relative amount of rewards they receive compared to someone who is performing similar tasks. The most salient difference between this theory and agency theory is that it takes into explicit consideration the outcomes of peers—that is, the social effects of incentives, besides the individual effects. While in principle, the comparison group for social effects could extend beyond the immediate team an individual finds himself or herself in (to extend to the firm or even the industry), the strength of social comparison processes tends to decline with social proximity and comparability (Brewer & Gardner, 1996; also see Nickerson & Zenger, 2008, for an application of this idea to organization design). The team is therefore assumed to be the primary focus of comparison for the social effects of incentives, though the qualitative nature of our arguments below would be unaffected when extended to social comparisons with broader social circles.

Those who perceive themselves as being in an inequitable situation will seek to reduce the inequity either by distorting inputs and/or outcomes in their own minds ("cognitive distortion"), by directly altering inputs and/or outputs (compared to their peers), or by leaving the organization (Adams, 1963; Carrell & Dittrich, 1978). The perception of inequity is thus argued to have a demotivating effect on individuals. Equity theorists note that the mere fact that my peers receive more than I do (or that the pay variation is large in my peer group) need not by itself lead to the perception of unfairness. An individual will perceive that she is being treated fairly if she believes that the ratio of her inputs to her outcomes is equivalent to those around her. Ceteris paribus, a low-skilled individual would accept the fact that a more skilled colleague receives higher compensation since the value of such peers' input is higher (see also Trevor, Reilly, & Gerhart, 2012).

However, once we take into account another behavioral attribute, namely, overconfidence (Moore & Healy, 2008), the perception of inequity and its demotivating effects may exist whenever there is pay dispersion (Shaw et al., 2002). As Larkin, Pierce, and Gino (2012) note, overconfidence (in their own abilities) makes it likely that employees see receiving less than others as inequity (Martin, 1982; Zenger, 1994), but may not perceive inequity in their receiving more than others. This could be one of the reasons why the relationship between pay variation and the performance of teams and individuals has been hard to pin down, as the effects may be different for high and low earners (see review by Shaw, 2014).

Indeed, although the original formulation of equity theory (Adams, 1963) argues that overrewarded individuals (relative to peers) feel the same "unfairness" in their pay level compared to those who are being underrewarded, later studies have found that overrewarded individuals perceive significantly less unfairness or perhaps even no unfairness (Fehr & Schmidt, 1999; Ho & Su, 2009). This in turn implies that the effects of a change in incentives from pay-for-performance to fixed salary could possibly differ according to an individual's performance relative to others.

While a move from pay-for-performance to fixed salary could in theory increase pay variation (as the salary could be fixed but not the same across individuals), we assume in developing our arguments below that this is not the case. It is more often the case that fixed salary systems are scaled—that is, there exist a few bands that are ordered, for instance, by seniority (Larkin et al., 2012). *Our arguments below therefore build on the premise that a move from pay-for-performance to fixed salary implies a decrease in pay variation*. This is a premise that can be verified in our data.

We consider the effects of the change in the focal individual's and the team's incentives. Consider first the effect of changing the incentives of the focal individual. For high performers, the switch from pay-for-performance to fixed salary should be discouraging. This is because they would perceive that the value of their inputs is no longer fairly rewarded, as the switch (assuming it reduces pay variation) would necessarily imply a compression in their wages, reducing both the absolute as well as relative level of compensation. Hence, we would expect a decrease in production and collaboration efforts for them. However, in terms of incoming collaboration, we would expect an increase. This is because, from the perspective of low performers,

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pay variation has decreased, and therefore it could lead to improved perceptions of *equity* (Bloom, 1999; Shaw et al., 2002). Prior studies have found that when there is perceived fairness in compensation across organizational members, individuals will cooperate/collaborate more with each other to maximize output (Harder, 1992; Wolfe & Loraas, 2008). Hence, an increase in incoming collaboration is expected for high performers.

On the other hand, for low performers, we should expect to find the opposite results of the incentives change for the focal individual. In particular, production and outgoing collaboration efforts are expected to increase due to the low performers' enhanced perception of equity.[1] In contrast, incoming collaboration would probably decrease because others (i.e., the high performers) would be less willing to cooperate/collaborate with low performers due to impaired perception of equity.

Next, we consider the consequences of the change in coworkers' (i.e., team's) incentives. By symmetry to the arguments above, for high performers, the change to coworkers' incentives to fixed salary should diminish their own sense of equity, as the difference in their pay relative to the low performers should shrink (under the premise that the change reduces pay variation). For low performers, exactly the opposite should happen. Thus, the predicted effects of the change in incentives of the focal individual as well as of the change for coworkers on a focal individual's efforts are the same in equity theory (i.e., it is at the team-level that incentives changes matter). These predictions can be summarized as follows:

Hypotheses 3a (H3a): The effect of the team's incentives changing from pay-for-performance to fixed salary is negative on production efforts for the high performers in a group.

Hypotheses 3b (H3b): The effect of the team's incentives changing from pay-for-performance to fixed salary is negative on the individual's own collaboration efforts (outgoing collaboration), but positive on the collaboration received from coworkers (incoming collaboration), for the high performers in a group.

Hypotheses 4a (H4a): The effect of the team's incentives changing from pay-for-performance to fixed salary is positive on production efforts for the low performers in a group.

Hypotheses 4b (H4b): The effect of the team's incentives changing from pay-for-performance to fixed salary is positive on the individual's own collaboration efforts (outgoing collaboration), but negative on the collaboration received from coworkers (incoming collaboration), for the low performers in a group.

Another useful approach to thinking about the social effects of incentives comes from goal framing theory. This theory proposes taxonomy of overarching goals that influence the cognitions and motivations of individuals, and in turn affect individuals' behaviors (Foss & Lindenberg, 2013; Lindenberg, 2013; Lindenberg & Foss, 2011). Lindenberg and Foss (2011) introduced three types of overarching goals: hedonic, gain, and normative. A hedonic goal expresses the desire to improve one's feelings at the moment; a gain goal expresses the desire to improve one's own personal resources; and a normative goal expresses the desire to act appropriately in the service of a collective entity, such as the group one belongs to.

These goal frames govern what stimuli are considered relevant, what alternatives are considered, what knowledge we draw on, what beliefs we hold about the possible behaviors of others (e.g., trustworthiness), and of course most importantly, which subgoals are suppressed and which ones are activated. They vary in their a priori strength, with the hedonic frame being the strongest, the normative frame being the weakest, and the gain frame being intermediate. Without being reinforced in some way, the normative frame may be pushed into the background by the hedonic and gain frame. This ordering is justified on evolutionary grounds that give precedence to individual reproductive success, with group living merely being a means to this end (Caporael, 2007).

In organizational contexts, goal frame theorists argue that it is most important to support the normative goal (which is most difficult to maintain) in

[1] These outcome variables may remain unchanged if our argument that reduction in inequality leads to improved perception of equity does not hold.

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the foreground and the other two in the background in order to trigger "joint production motivation"—a mechanism through which organizational members are motivated to engage in joint productive endeavors in which they choose their actions regarding joint goals and exert intelligent effort to reach joint goals (Foss & Lindenberg, 2013). The normative goal frame, these authors argue, can unleash a cascade of prosocial behaviors within the group, such as cohesion and collaboration (Tomasello et al., 2005), adherence to and enforcement of group norms (Ostrom, Walker, & Gardner, 1992), and a strong sense of solidarity (Wageman, 1995).

The structure of incentives can play a critical role in triggering and maintaining the normative goal frame. Foss and Lindenberg (2013) argue that while individual rewards are important, excessive emphasis on these can undermine the normative goal frame. In particular, they argue, "(individual) contingent gain rewards, such as status advancement and money, must remain modest, because they can foster a gain goal frame, just as contingent hedonic rewards, such as especially enjoyable tasks and better offices, can foster a hedonic goal frame. When rewards get too strong, they can undermine the normative goal frame, and intelligent effort will be selectively driven by what leads to personal rewards (hedonic or gain) rather than by contribution to the realization of common goals (p. 93)."

For this reason, switching from a pay-for-performance incentive system (that puts strong emphasis on the personal gain goal frame) to a fixed salary system (which makes peers and the continued existence of the firm more salient for individuals) should allow the normative goal frame into the foreground, and therefore trigger a joint production motivation. In turn, this should encourage helping behaviors between organizational members in order to maximize the *joint* outcome. Foss and Lindenberg (2013) argue that under a normative goal frame, moral hazard and opportunism is likely to diminish. In addition, coordination cost will be reduced because joint production motivation implies that organizational members generate shared representations of actions and tasks in terms of joint goals, reducing the need for planning and formalization. Consequently, the change in incentive structure either for self or for coworkers (since both help to change the relative weights on gain and normative goal frames), should strengthen the normative goal frame, leading to enhanced production and collaboration efforts. This leads to the following prediction:

> *Hypotheses 5 (H5): The effect of the team's incentives changing from pay-for-performance to fixed salary is positive on the focal individual's own production efforts, collaboration efforts (outgoing collaboration), and the collaboration received from coworkers (incoming collaboration).*

Table 1 presents a summary of the predictions made by agency theory (a purely individual effects theory), equity theory, and goal framing theory (the latter two incorporate social effects). As can be seen at a glance, there are competing predictions made by the theories, which allows us to use evidence to choose not only between individual and social effects theories, but also between the social effects theories.

Empirical Setting

Our empirical context is an e-commerce company codenamed "Chimera" (to protect its identity as requested by the management) selling "flash-deals" through its online platform (e.g., Groupon, Living Social, etc.). Flash deals relate to goods that are sold at highly discounted prices compared to their retail prices, but for a relatively short period of time. As a general rule, Chimera does not warehouse the stock of the goods that they sell; instead, it contracts with suppliers who directly ship their products to customers after an order is completed on the company's online platform. When a deal expires, the total revenue generated by the deal is divided between Chimera and its supplier according to a predefined agreement. Afterward, a portion of the remaining revenue is passed on to the sales employee who sourced and executed the deal in the form of monetary compensation. This arrangement was in place at Chimera since its founding in June 2011 until December 31, 2012.

The sales employees who source and execute deals are called "MDs (merchandisers)." MDs are organized into 11 teams based on product categories (e.g., baby products, food/health products, appliances/electronics, etc.). Their job mainly consists of identifying and negotiating with suppliers that

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Table 1
Summary of Predictions

Natural experiment: Incentives change from pay-for-performance to fixed salary				
		Equity theory		
	Agency theory	High performer	Low performer	Goal framing theory
As focal individual's incentives change:	**Predictions**	**Predictions**	**Predictions**	**Predictions**
Production efforts (x)	− (H1a)	Same as team's incentives change predictions (below)	Same as team's incentives change predictions (below)	Same as team's incentives change predictions (below)
Outgoing collaboration (y_o)	− or no change (H1b)			
Incoming collaboration (y_i)	− or no change (H1b)			
As team's incentives change:	**Predictions**	**Predictions**	**Predictions**	**Predictions**
Production efforts (x)	− or no change (H2)	− (H3a)	+ (H4a)	+ (H5)
Outgoing collaboration (y_o)	− or no change (H2)	− (H3b)	+ (H4b)	+ (H5)
Incoming collaboration (y_i)	− or no change (H2)	+ (H3b)	− (H4b)	+ (H5)

would potentially sell their goods through the company's platform, and marketing such goods through the company's website using attractive visual aids (i.e., photos, videos, etc.). MDs are free to source any type of product as long as the product fits the product category of the team that they belong to. When sourcing deals, an outbound sales approach is the norm (i.e., MDs approach suppliers first, rather than suppliers contacting MDs first) because many suppliers are either small- or middle-size enterprises that have never considered e-commerce as a sales channel, or are very well-known companies that do not see an obvious need for additional sales channels, which could perhaps have conflicting interests with their existing ones. In particular, suppliers must forgo their usual profit margins in order to sell goods through Chimera's website, so MDs must invest significant amounts of effort to persuade potential suppliers to enter the business relationship.

In general, MDs at Chimera work like individual entrepreneurs and focus on sourcing their own deals. However, MDs can also help (collaborate with) their team peers by "sharing" suppliers, that is, allowing peers to source deals from their own pool of suppliers. Sharing suppliers is an important form of helping behavior in Chimera, as it opens up new business opportunities for those who are permitted access (at no significant cost) to the additional suppliers. However, sharing suppliers also involves the risk of cannibalization of sales for the helping MD, and possibly the risk of damaging relationships with the shared supplier in the case that the fellow employee who is being helped in this way does not have adequate knowledge or experience to work effectively with the supplier. The situation is fairly typical of cross-selling in services industries, where the organization may benefit but the individual may lose by sharing their client relationships with fellow employees (Rogan, 2013). Given the investment by an MD to build a client relationship, it was perceived as "unethical" among MDs to source deals from peers' existing suppliers without permission. Only when the "original" MD gave explicit permission or a referral could other MDs tap into the existing MD-supplier relationships. For instance, employees in the organization told us:

> Supplier relationships are created and maintained on a "first-come, first-served" basis. An MD who is first in striking a deal with a particular partner claims "exclusive" transaction rights with that partner, and therefore late-movers are put into disadvantage. Late-movers must look for new suppliers without stepping on the toes of early-movers. (Interview with manager of Human Resource

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> department, December 2013; translated from Korean to English)

> Successful deals receive lots of attention within teams and the organization. When I see peers achieving big success on a particular deal, I am tempted to source the same product from the same supplier. But that is not tolerated here. We put in so much effort in acquiring each supplier. (Interview with MD in Beauty Products team, December 2013; translated from Korean to English)

As the second quote also highlights, given the grouping of MDs into product teams, and the existence of sales price and volume data on the company's website, mutual observability of earning is high—which plays an important role for social effects of incentives (i.e., equity as well as goal framing).

Chimera provides a work context in which the key measures needed to test our theoretical arguments are easy to observe. We can observe MDs' production and collaboration efforts. In our empirical setting, production efforts refer to the sourcing of MDs' own suppliers and executing their own deals. Outgoing collaboration, on the other hand, refers to sharing suppliers with peers so that peers could utilize those suppliers to perform better. The mirror image is peers' effort toward helping the focal MD, or incoming collaboration.

The Experiment

MDs at Chimera experienced a drastic change in their compensation scheme in 2013. From the founding of the company in 2011 to the end of 2012, MDs were compensated under an individual pay-for-performance incentive scheme in which they received a low amount of baseline fixed salary plus an ex-ante determined portion of the total profit they generated for the company. Accordingly, the higher the profit an MD generated, the more he or she would receive in total compensation. However, after 22 months of maintaining such a scheme, Chimera switched MDs' compensation scheme to a fixed salary system in which MDs were paid (individually different) higher fixed salaries but no performance-based incentives. This was done to attract experienced MDs from other firms who were working under a relatively stable fixed salary system, and to decrease pay discrepancy between high and low performers in the firm, which was believed to have created a sense of inequity. Except for the change in compensation scheme, the other clauses of MDs' employment contracts remained unchanged.

One of the most salient and immediate results of this change was that the average annual compensation MDs received significantly decreased (especially for the previously high-performing ones). In particular, the average monthly compensation of MDs decreased about 16% from U.S.$3,427 to U.S.$2,885, and the maximum monthly compensation paid to an MD dropped from U.S.$8,025 to U.S.$4,583. This naturally led to a large decrease in the pay range (i.e., the difference between the maximum and minimum salaries) within the organization (and within teams); in specific, there was a 46% drop in pay range within the organization after the incentive system was switched to fixed salary.

The change in incentive regime at Chimera was implemented over 4 months in three stages. Originally, management expected to apply the change to all employees on a specific date (April 1). However, in practice, the changes were made on January 1 (3 months before), April 1 (on-time), and May 1 (1 month later) involving groups of 6 (Group 1), 30 (Group 2), and 11 (Group 3) MDs, respectively. Figure 1 graphically depicts how the change was implemented across three waves.

Randomization

Over multiple interviews with the management of Chimera, we discovered that the difference in the change dates for Groups 2 and 3 was simply due to random administrative delays rather than any systematic difference between the characteristics of the MDs in each group. In particular, the process of changing the compensation scheme of MDs involved the human resource (HR) department evaluating past performance of each MD and adjusting MDs' salaries in respect to salaries of comparable peers. This process was initiated within each team in alphabetical order of team members' last names. However, as the individual steps of the process were executed in a sequential manner by several different HR personnel, completing the process for all MD cases according to a single timeline was impossible due to unexpected contingencies. For example, simply the speed of working of different personnel contributed variation to the timing. Our interviews

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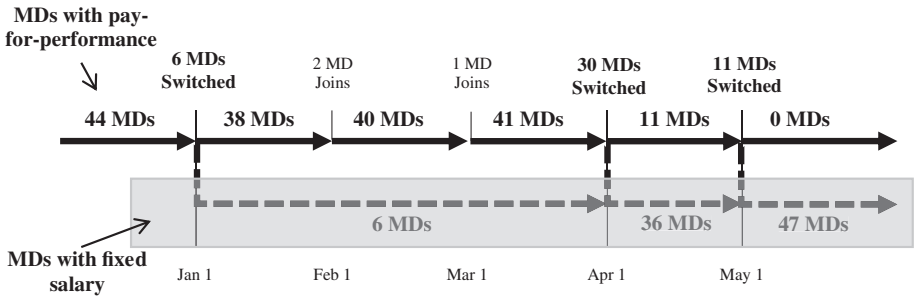

Figure 1. Graphical representation of compensation scheme change.

indicate that such nonsystematic factors led to some MDs having their incentives converted in Group 2 and others in Group 3.[2]

Further, the individuals that were included in each change group were also distributed effectively randomly across the product teams. This means that both the time at which an individual underwent a change in incentive structure, as well as the fraction of one's team members who had been converted to the new incentive structure at any point in time, were both effectively randomly assigned. These features are very advantageous in estimating the causal effects—both individual and social—of the incentives change that we hypothesized.

Finally, the MDs themselves were not notified in advance about the timing schedule of these changes, or which group they would be included in, though they were aware from January 1 that eventually they should expect their incentives to be changed.

Our empirical context thus approximates a natural experiment, in which (a) the treatment is the proportion of one's team members who have experienced an incentive regime change, (b) individuals in Groups 2 and 3 are randomly assigned into this treatment, and (c) control and treatment groups exist.

At the same time, this change is not a perfect natural experiment for two reasons. First, we did not conduct the randomization ourselves, and relied instead on our interviewing and access to company records to infer that randomization occurred. However, we can apply certain empirical tests to assess whether the assignment into treatment and control groups was indeed uncorrelated with other attributes of individuals. On observable attributes, we ran both *t* tests and Wilcoxon rank-sum tests to verify whether the aggregate means of a number of MD characteristics *statistically* differ between the change groups. Results showed no difference between the means of observable characteristics except for a small difference in education level between MDs in Group 2 and 3 (see Table 2).

We further verified through our interviews that education played no role in the allocation of individuals to Group 2 and Group 3, so it appears that this small difference should be seen as matter of chance. In our analysis using the differences-in-differences methodology (see details under "Empirical strategy" section), we are also able to test if there are any systematic differences in results with and without control variables (see details under "Results" section), and the assumption of randomization appears highly plausible.

Second, in an ideal experiment, the control group should not be anticipating a treatment for themselves at some future date. This is a concern for field experiments in general, where staggering treatments is common, but keeping control groups from knowing about the existence of a treatment group (and their own eventual receipt of the treatment) is difficult. In our case, this problem imposes a conservative bias as it makes less likely the possibility of detecting any treatment effect, if the control group behaves as if it too were treated (because they eventually will be) (Duflo, Glennerster, & Kremer, 2008). Our informants told us that while employees may have been aware of an impending change even before their own incentives were changed, the actual switch was a formal meeting involving a meeting with the HR managers. We therefore believe that despite a possible anticipation effect, something qualitatively different or quantitatively

[2] Note that Group 1 does not qualify as random assignment because MDs in this group were handling products that innately had low profit margins—causing them to receive relatively lower amounts of performance-based incentives—and thus were intentionally switched first. Nonetheless, Groups 2 and 3 are randomly assigned, which we use as the final sample (41 individuals).

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Table 2
Mean Difference Tests of Observable Characteristics of MDs in Different Incentives Change Waves

Variables (observable characteristics)	MDs switched on April 1 (mean values)	MDs switched on May 1 (mean values)	*t* test		Wilcoxon rank-sum test	
			t-value	*p*-value	*z*-value	*p*-value
Gender (women = 1)	0.57	0.36	1.14	.27	1.14	.26
Education (masters = 4)	2.96	2.44	1.66	.13	2.20	.02
Age	29.47	27.55	1.80	.09	1.82	.07
Avg. monthly salary under PFP (U.S.$)	3,366	3,740	−0.69	.50	−0.29	.77
Avg. monthly salary under fixed salary (U.S.$)	2,849	2,856	−0.03	.98	0.37	.71
Avg. change percent in compensation	−10.37	−18.22	1.23	.23	0.56	.58
Tenure on April 1	367.33	418.91	−0.7	.44	−0.77	.44
Accumulated deal experience on April 1	666.83	838.55	−0.98	.34	−0.94	.35
Avg. revenue per day until April 1 (U.S.$)	19,600	22,100	−0.62	.54	−0.68	.50
Number of observations	30	11	—	—	—	—

Note. PFP = pay-for-performance.

stronger (in psychological terms) occurs once the incentives are actually switched, creating sufficient difference between the treatment and control group for us to observe the effects we hypothesize.

Methodology

Data and Sample

The data used in our study was directly provided by Chimera. It included detailed information on 98,474 deals executed by MDs, and specific human resource information such as joining date, salary, date of pay scheme change, average monthly pay before the change, team affiliation, gender, age, and education of MDs. The data is available from 2011 onward, when the company was founded, to the end of 2013, when the data was collected. Observations were aggregated at the daily level for each MD, as MDs make deal execution decisions on a daily basis.

To allow a sufficient observation window for behavioral change, we only included in our sample the observations related to MDs who had worked for the company for at least 1 month under the old pay system *and* for at least 1 month under the new pay system, leaving us with observations for 47 MDs. From this sample, we drop the observations associated with the six MDs who were the first to be converted to a fixed salary scheme, as they were not randomly assigned into such group. We also confine our sample to observations related to days on which the current team size is two or larger, as by definition, helping behaviors cannot occur when a focal MD does not have a peer to help. Next, we dropped the days on which MDs did not post any deals due to public holidays or personal vacations. Finally, we limit our sample to daily observations of the January–December 2013 period to have broadly comparable durations before and after the change (which was completed by May 1, 2013). This resulted in a daily-level panel dataset with 8,916 observations, covering 46,392 deals done by 41 MDs in 11 product teams over a maximum of 342 days.

Empirical Strategy

In order to examine the effect of a focal individual's incentives change on such individual's production and collaboration efforts (i.e., the individual effect of the incentives change) (H1a and H1b), we use a difference-in-difference (DID) approach (Angrist & Pischke, 2008). In particular, April 1, 2013, is

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the point where the DID approach is employed (see Figure 1).

Excluding from the sample the six MDs who have been converted to a fixed salary scheme on January 1, 2013 (due to non-random assignment), there are 41 MDs who have not been switched to a fixed salary scheme until April 1, 2013. However, on April 1, 30 of these 41 MDs had their own incentive schemes switched (treatment group). The remaining 11 MDs remain under the pay-for-performance system until May 1, 2013, creating a control group of MDs. The 30 day time period between April 1 and May 1, 2013, captures the difference in productivity, outgoing collaboration, and incoming collaboration between the treatment and control group with different incentives, which allows our DID approach. From May 1 onward, although both groups have been treated, (a) the fact that one group was treated earlier may still matter, and (b) we account for this different regime in which both groups have now been treated through a dummy, *post-May 2013,* which controls for any effects related to this completion of the incentives change (see "Measures" section).

To test the effects of change in the team's incentives (while controlling for the change in focal individual's incentive structure) hypothesized in H2 through H5 (i.e., the social effects of the incentives change), we use the aforementioned sample related to 8,916 observations of 41 MDs (see "Data and sample" section) to examine the effect of the proportion of a team that has had its incentives converted to fixed salary, on individual behavior. Since the proportion of a team that has been converted at a point in time is randomly assigned, the estimated coefficient should be unbiased.

Since the team's incentive structure change could influence different individuals differently depending on whether an individual was a high performer or a low performer under the pay-for-performance system (H3a, H3b, H4a and H4b), we run additional analyses on two subsamples related to (a) individuals whose productivity (measured by *average revenue per day*) before the change was *higher* than the average productivity of all team members, and (b) those whose productivity before the change was *lower* the average productivity of all team members, respectively. The subsample for high performers included 3,255 observations for 15 MDs, and the subsample for low performers included 5,661 observations for 26 MDs. We test our three dependent variables on all three samples in the order of the high performer, low performer, and full sample.

Measures

Dependent variables. Our dependent variables are *production efforts* (denoted by x), *outgoing collaboration* (y_o), and *incoming collaboration* (y_i). Executing each deal requires significant time and effort for the MD to select the product, to contact and negotiate with suppliers, to create marketing images and messages, and to go through internal approval to finally upload the product on the online platform. Hence, *production efforts* (x) is measured by the total number of deals an MD executes on a focal day. *Outgoing collaboration* (y_o) is measured by the total number of suppliers shared by a *focal MD* to a team peer on a focal day. As aforementioned, peers can engage in commercial relationships with the focal MD's suppliers *only* when explicit permission is given by the focal MD. Sharing behavior is detected in our data when a particular peer starts to execute a deal sourced from a supplier who has prior history of sourcing products to a focal MD; in some cases, a single MD shares the same supplier to up to two peers on a focal day. Finally, *incoming collaboration* (y_i) is measured by the total number of suppliers shared by *team peers* to a focal MD on a focal day.

Independent variable. To employ the DID approach to test H1a and H1b, we first define an indicator variable, *post*, equal to 1 for observations related to dates from April 1 onward, and 0 otherwise. Next we define an indicator variable, *treatment*, equal to 1 for observations related to MDs who had their own incentives changed on April 1, 2013, and 0 for those who had their own incentives changed on May 1, 2013. Finally, we create an interaction variable, *post* x *treatment*, which multiplies the *treatment* variable with the *post* variable; the coefficient of this variable represents the individual effect of the incentives change.

For the tests of H2–H5, the main independent variable is *team's incentives change*, which is measured as the proportion of team members (in percentage terms) that have been changed from pay-for-performance to fixed salary. Thus, when all team members are still under the pay-for-performance system, the variable takes a value 0; when all team members are switched to fixed salary, the variable takes a value of 100.

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Table 3
*Descriptive Statistics and Correlations for Variables Used in Regression Analyses (*n = *8,916)*

	Variables	Mean	*SD*	Min	Max	1	2	3	4	5
1	Production efforts (x)	5.2	3.14	0	34					
2	Outgoing collaboration (y_o)	0.08	0.32	0	4	0.01				
3	Incoming collaboration (y_i)	0.08	0.32	0	4	0.09	0.16			
4	Team's incentives change	72.63	41.95	0	100	0.23	0.04	0.03		
5	Focal MD's incentives change	0.7	0.46	0	1	0.23	0.03	0.02	0.95	
6	MD's accumulated experience ('00 deals)	10.31	6.12	0	26.98	0.33	0.06	0.02	0.4	0.4
7	Potential synergy with peers	0.68	0.2	0.27	1	0.01	−0.01	−0.01	−0.14	−0.12
8	Pay range of team members ('000 U.S.$)	1.58	1.1	0.16	5.78	−0.08	0.01	0.01	−0.58	−0.49
9	Team size	4.97	1.55	2	7	−0.05	−0.09	−0.09	−0.11	−0.06
10	Team gender diversity	0.35	0.18	0	0.5	0.03	−0.06	−0.06	0.01	0.03
11	Team education diversity	0.36	0.28	0	1	−0.12	−0.06	−0.08	−0.05	−0.05
12	Team age diversity	0.1	0.04	0	0.2	0.04	−0.02	−0.02	0.05	0.05
13	Team tenure diversity	0.45	0.24	0	1.49	−0.03	−0.06	−0.06	−0.32	−0.31
14	Post-May 2013 dummy	0.63	0.48	0	1	0.21	−0.01	−0.02	0.85	0.85
	(continued)	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	—
7	Potential synergy with peers	0.09								
8	Pay range of team members ('000 U.S.$)	−0.11	0.24							
9	Team size	−0.08	−0.03	0.2						
10	Team gender diversity	−0.11	0.01	−0.2	0.41					
11	Team education diversity	−0.04	0.37	−0.1	0.28	0.63				
12	Team age diversity	−0.08	0.2	0.38	0.25	−0.28	−0.12			
13	Team tenure diversity	−0.43	0.05	−0.1	−0.06	0.32	0.19	0.06		
14	Post-May 2013 dummy	0.45	−0.14	−0.5	−0.07	−0.01	−0.04	0.02	−0.37	—

As highlighted above, the incentives change in our context was implemented in three waves (January 1, April 1, and May 1, 2013) on different individuals, affecting different teams at different times and in different degrees. For example, in a particular team with four members, two members had their incentives changed on January 1, while the others had theirs changed on April 1 and May 1, respectively. In another example of a team with seven members, three members had their incentives changed on April 1, while the others had theirs changed on May 1. In all, *team's incentives change* takes nine values: 0, 33.33, 40, 42.86, 50, 60, 66.67, 85.71, and 100.

Control variables. To begin with, we control for *MD's accumulated experience* using the total number of deals that the MD has historically executed up to the focal day, as experience may influence the MD's ability to source deals. We also control for *potential synergy with peers*, which captures the proportion of the focal MD's product categories that overlap with those of team peers, as MDs would presumably share more suppliers to peers who sell similar products. For example, in the Fashion/Accessory team, examples of product categories are jewelry, watches, and sneakers. We also control for pay variance, in particular for *pay range of team members*, which is a variable that is measured by subtracting the lowest monthly salary paid to a team member in a team from the highest amount of that team (Kepes, Delery, & Gupta, 2009). While no clear prediction exists for the effect of pay range (see Shaw, 2014, for a review), we nonetheless include it as a control. Further, we control for team characteristics such as *team size*, *team gender diversity*, *team education diversity*, *team age diversity*, and *team tenure diversity,* as these factors influence team dynamics and performance (Barreto, 2012; Chen & Miller, 2007). Importantly, we also include a *post-May 2013 dummy* (equal to 1 if observation date is between May 1 and December 31, 2013; 0 otherwise), to control for the period in which *all* MDs have been switched to a fixed salary. Finally, we include *day of week fixed effects*.

For the analyses that examine the effect of the team's incentives change, we also control for whether the focal individual's incentives has been changed, to observe the marginal effect of team's incentive change; the variable, *focal MD's incentives change*, takes a value of 0 if the focal MD is still paid according to the pay-for-performance

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Table 4
Negative Binomial Regression Results for Difference-in-Difference Analyses

	Production efforts (x)			Outgoing collaboration (y_o)			Incoming collaboration (y_i)		
Dependent variables	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Post	0.238	0.189	0.202	0.340	0.290	0.528	0.303	0.299	0.433
	(.00)	(.00)	(.00)	(.41)	(.48)	(.13)	(.43)	(.39)	(.14)
Treatment	0.081	−0.055	Omitted	−0.863	−0.570	Omitted	−1.145	−0.967	Omitted
	(.40)	(.49)	Omitted	(.12)	(.18)	Omitted	(.03)	(.03)	Omitted
Post × treatment	0.043	0.029	−0.004	0.911	0.716	0.492	1.032	0.807	0.593
	(.61)	(.73)	(.96)	(.08)	(.15)	(.28)	(.05)	(.09)	(.18)
Post-May 2013 dummy	Included	Included	Included	Included	Included	Included	Included	Included	Included
Control variables	Excluded	Included	Included	Excluded	Included	Included	Excluded	Included	Included
Individual fixed-effects	Excluded	Excluded	Included	Excluded	Excluded	Included	Excluded	Excluded	Included
Observations	8,916	8,916	8,916	8,916	8,916	8,916	8,916	8,916	8,916

Note. Exact p-values, based on robust standard errors clustered at the individual level, are reported in parentheses.

system, and a value of 1 if the focal MD's compensation scheme has been switched to fixed salary. Table 3 reports descriptive statistics and correlations for the variables used in this study.

Econometric Models

Since our dependent variable is a count variable that takes only discrete, nonnegative integer values, Poisson or negative binomial regression models may be appropriate. However, since overdispersion exists in the distribution of our dependent variables, we select the latter model (Angrist & Pischke, 2008; Greene, 2003). For all analyses, standard errors are clustered at the individual level (Wooldridge, 2003).

We first run a DID analyses on our three dependent variables *production efforts* (x), *outgoing collaboration* (y_o), and *incoming collaboration* (y_i). To begin with, we run the analyses excluding control variables other than the *post-May 2013 dummy*. Next, we show the results for the same analyses after including all relevant control variables to examine whether results are sensitive to including covariates. Finally, we also include individual fixed effects in the analyses to investigate whether unobservable and time-constant individual characteristics affect the results. The final step is important because it allows us to verify if MDs were actually randomly assigned into change waves by comparing the coefficients with those of the second step.

For the next set of analyses that test the effects of the team's incentives change, we similarly run individual fixed-effect negative binomial regression models in a step-wise manner. Models only including control variables, followed by models also including the main independent variable, *team's incentives change*, are run on the high-performer sample, low-performer sample, and the full sample, respectively.

For both sets of analyses, individual fixed effects are added by including dummies for each of the 41 MDs (Allison & Waterman, 2002). This allows us to abstract away from stable differences across individuals without having to explicitly control for them (because our effects are estimated within individuals).

Results

We first present the results of the DID approach in Table 4.

In this approach, the variable of interest to us is *post* × *treatment*, as the coefficient of this interaction term indicates the individual effect of the incentives change. Agency theory predicted in H1a and H1b, that the effect of a focal individual's incentives changing from pay-for-performance to fixed salary would be negative on production efforts and nonpositive (zero or negative) on collaboration efforts. However, the change showed to have no significant effect on production effort (Models 1–3; $p = .61$, $p = .73$, and $p = .96$, respectively) and positive effects on outgoing collaboration (Model 4; $p = .08$) and incoming collaboration (Models 7 and 8; $p = .05$ and $p = .09$, respectively), failing to provide support for H1a and H1b. Nonetheless, we observe that some behavior is changing post the period the incentives change has started (see coefficients of *post* in Models 1–3; $p = .00$). These

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Table 5
*Fixed-effect Negative Binomial Regression Results for Production Efforts (*x*)*

Dependent variable	High performers		Low performers		Average (all MDs)	
Production efforts (*x*)	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Independent variable						
Team's incentives change		0.003		0.004		0.003
		(.01)		(.02)		(.01)
Control variables						
Focal MD's incentives change	0.076	−0.179	0.208	−0.037	0.159	−0.050
	(.27)	(.09)	(.00)	(.74)	(.00)	(.56)
MD's accumulated experience ('00 deals)	0.021	0.022	−0.006	−0.005	0.006	0.006
	(.04)	(.03)	(.63)	(.66)	(.51)	(.50)
Potential synergy with peers	0.332	0.334	0.082	0.106	0.193	0.209
	(.04)	(.03)	(.58)	(.49)	(.09)	(.07)
Pay range of team members ('000 U.S.$)	−0.033	−0.012	−0.042	0.010	−0.037	−0.002
	(.53)	(.81)	(.28)	(.83)	(.22)	(.95)
Team size	−0.047	−0.034	−0.013	−0.019	−0.027	−0.029
	(.51)	(.61)	(.86)	(.79)	(.60)	(.57)
Team gender diversity	0.648	0.649	0.269	0.212	0.537	0.495
	(.10)	(.10)	(.44)	(.54)	(.04)	(.06)
Team education diversity	−1.278	−1.276	−1.876	−1.877	−1.584	−1.586
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Team age diversity	−0.787	−1.334	7.347	7.009	−0.061	−0.521
	(.71)	(.53)	(.05)	(.08)	(.97)	(.76)
Team tenure diversity	−0.044	0.046	−0.868	−0.784	−0.443	−0.381
	(.85)	(.85)	(.00)	(.00)	(.03)	(.07)
Post-May 2013 dummy	−0.025	−0.043	−0.021	−0.045	−0.009	−0.027
	(.58)	(.39)	(.69)	(.38)	(.80)	(.45)
Fixed effects						
Individual (MD) & day of week fixed effects	Included	Included	Included	Included	Included	Included
Constant	1.862	1.709	1.846	1.719	2.051	1.900
Observations	3,255	3,255	5,661	5,661	8,916	8,916
Number of individuals in sample	15	15	26	26	41	41
Log Likelihood	−7,830.97	−7,825.52	−12,457.22	−12,443.45	−20,384.40	−20,370.41

Note. Exact *p*-values, based on robust standard errors clustered at the individual level, are reported in parentheses.

results seem to suggest that individuals' behaviors are changing after the new incentives scheme has been initiated, but the change may not be due to the change in one's own incentives scheme. Rather, the change in behavior may be driven by the change in other's incentives.

In the DID approach, we also compare whether the coefficients of the *post* x *treatment* variable of the random-effect models (Models 2, 5, and 8) and of the fixed-effect models (Models 3, 6, and 9) for each outcome variable are statistically different from each other in order to verify the random assignment of individuals into different incentives change groups. Although the exact values of the coefficients for the *post* × *treatment* variable are different (i.e., comparing the coefficients for the *post* × *treatment* in Model 2 *versus* 3; Model 5 *versus* 6; Model 8 *versus* 9), they are not statistically distinguishable, as we verify by estimating *seemingly unrelated regressions* (Zellner, 1962).

In summary, the DID approach presents results that indicate that (a) changing the focal individual's incentives led to results that could not be predicted by agency theory (i.e., the individual effect perspective of the incentives), (b) the incentives change affected individual behavior even in the case that a focal individual's incentives were not changed, and (c) randomization of order of change seems to be a reasonable assumption for this data.

The results of the regression analyses that test the effects of the *team's incentives change* on individuals' behavior for each dependent variable are presented in Tables 5–7, respectively. Table 5 presents the results for *production efforts* (*x*).

Models 1, 3, and 5 are baseline models, which only include the control variables. Models 2 and

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Table 6
Fixed-effect Negative Binomial Regression Results for Outgoing Collaboration (y_o)

Dependent variable	High performers		Low performers		Average (all MDs)	
Outgoing collaboration (y_o)	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Independent variable						
Team's incentives change		0.013		0.029		0.017
		(.26)		(.00)		(.04)
Control variables						
Focal MD's incentives change	0.269	−0.761	1.215	−0.562	0.878	−0.258
	(.69)	(.30)	(.00)	(.43)	(.03)	(.69)
MD's accumulated experience ('00 deals)	−0.066	−0.062	−0.264	−0.265	−0.164	−0.164
	(.26)	(.28)	(.00)	(.00)	(.00)	(.00)
Potential synergy with peers	−0.659	−0.575	−0.155	−0.075	−0.494	−0.434
	(.22)	(.27)	(.83)	(.92)	(.26)	(.34)
Pay range of team members ('000 U.S.$)	−0.153	−0.096	−0.224	0.113	−0.233	−0.058
	(.57)	(.70)	(.29)	(.59)	(.20)	(.75)
Team size	−0.588	−0.544	−2.087	−2.115	−1.424	−1.437
	(.12)	(.16)	(.00)	(.00)	(.00)	(.00)
Team gender diversity	3.718	3.908	0.777	0.613	0.999	0.980
	(.11)	(.11)	(.65)	(.71)	(.41)	(.41)
Team education diversity	0.205	0.250	0.326	0.382	0.630	0.680
	(.83)	(.79)	(.62)	(.55)	(.19)	(.15)
Team age diversity	6.298	3.965	76.892	74.024	28.629	26.189
	(.53)	(.73)	(.00)	(.00)	(.00)	(.01)
Team tenure diversity	−1.849	−1.376	−7.390	−6.753	−4.197	−3.786
	(.08)	(.19)	(.00)	(.00)	(.00)	(.00)
Post-May 2013 dummy	−0.587	−0.543	−1.039	−1.092	−0.949	−0.973
	(.31)	(.31)	(.01)	(.00)	(.00)	(.00)
Fixed effects						
Individual (MD) & day of week fixed effects	Included	Included	Included	Included	Included	Included
Constant	2.193	1.622	7.991	6.926	4.639	3.810
Observations	3,255	3,255	5,661	5,661	8,916	8,916
Number of individuals in sample	15	15	26	26	41	41
Log likelihood	−930.90	−929.55	−1,343.10	−1,333.66	−2,299.66	−2,293.37

Note. Exact *p*-values, based on robust standard errors clustered at the individual level, are reported in parentheses.

4 test how the change in team's incentives affects production efforts of high and low performers. A positive and significant effect was found for both subsets of individuals ($p = .01$ and $p = .02$, respectively). These results support H4a but not H3a, providing partial but inconsistent evidence for the predictions derived from equity theory, but complete and consistent evidence for those of goal framing theory (H5). In addition, results in Model 6 show that, even *on average*, individuals put more effort into their own tasks as the proportion of team members with fixed salary increased ($p = .01$). In particular, further analyses suggest that an individual will increase his or her production efforts by approximately 58% when all of his or her team members' incentives are switched to the new regime. This result again presents support for H5 but not for H2, lending weight to the predictions of goal framing theory over agency theory in this particular behavioral dimension.

Next, Table 6 shows the results for *outgoing collaboration* (y_o).

Models 2, 4, and 6 test how the change in team's incentives affects outgoing collaboration. Analyses reveal that low performers, and also all individuals on average, increased their outgoing collaboration levels as more team members' incentive schemes were changed to fixed salary ($p = .00$ and $p = .04$, respectively). For example, on average, analyses show that an individual's outgoing collaboration increased about 3.8 times when all of his team members' incentives were switched to fixed salary. The coefficient for high performers was also positive, but was not statistically significant ($p = .26$). In all, the results show additional support for H5, the prediction made by goal framing theory, over

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Table 7
Fixed-effect Negative Binomial Regression Results for Incoming Collaboration (y_i)

Dependent variable	High performers		Low performers		Average (all MDs)	
Incoming collaboration (y_i)	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Independent variable						
Team's incentives change		0.046		0.018		0.024
		(.00)		(.05)		(.00)
Control variables						
Focal MD's incentives change	0.942	−2.763	0.710	−0.487	0.658	−1.033
	(.17)	(.00)	(.06)	(.45)	(.05)	(.10)
MD's accumulated experience ('00 deals)	−0.280	−0.270	−0.149	−0.150	−0.176	−0.179
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Potential synergy with peers	−0.091	0.339	−0.823	−0.696	−0.685	−0.510
	(.94)	(.79)	(.07)	(.11)	(.09)	(.20)
Pay range of team members ('000 U.S.$)	−0.198	−0.009	−0.316	−0.134	−0.344	−0.123
	(.55)	(.96)	(.09)	(.47)	(.04)	(.43)
Team size	−1.171	−1.080	−1.432	−1.474	−1.229	−1.282
	(.03)	(.10)	(.00)	(.00)	(.00)	(.00)
Team gender diversity	0.400	0.558	0.025	−0.048	0.595	0.417
	(.83)	(.80)	(.98)	(.97)	(.57)	(.69)
Team education diversity	−0.124	0.115	−2.936	−2.992	−1.452	−1.446
	(.84)	(.83)	(.00)	(.00)	(.07)	(.12)
Team age diversity	17.239	10.888	76.580	76.729	41.593	41.012
	(.18)	(.49)	(.00)	(.00)	(.07)	(.11)
Team tenure diversity	−8.695	−6.779	−2.814	−2.394	−4.187	−3.609
	(.00)	(.00)	(.01)	(.02)	(.00)	(.00)
Post-May 2013 dummy	−0.889	−0.565	−1.001	−0.982	−0.985	−0.936
	(.25)	(.39)	(.00)	(.00)	(.00)	(.00)
Fixed effects						
Individual (MD) & day of week fixed effects	Included	Included	Included	Included	Included	Included
Constant	7.205	4.909	4.134	3.560	2.481	1.102
Observations	3,255	3,255	5,661	5,661	8,916	8,916
Number of individuals in sample	15	15	26	26	41	41
Log Likelihood	−666.37	−654.79	−1,602.59	−1,597.68	−2,288.02	−2,275.03

Note. Exact *p*-values, based on robust standard errors clustered at the individual level, are reported in parentheses.

other predictions. The evidence supports H4b as well (consistent with equity theory), but recall that the results showing increase in production efforts of high performers in Table 5 do not support equity theory.

Table 7 presents the results for *incoming collaboration* (y_i).

Models 2, 4, and 6 test the effect of team's incentives change on *incoming collaboration*. Analyses reveal that both high and low performers, and also all individuals on average, received more help from their peers as more team members' incentive schemes were changed to fixed salary ($p = .00$, $p = .05$, and $p = .00$, respectively). In specific, on average, an individual's incoming collaboration increased about 5.4 times when all of her team members' incentives were switched to fixed salary. These results again rule in favor of H5 over the alternatives.

In conclusion, our analyses show that *production efforts* (x), *outgoing collaboration* (y_o), and *incoming collaboration* (y_i) all increase as more team members are switched from a pay-for-performance incentives scheme to a fixed salary scheme. These results hold after controlling for whether the focal individual has been switched or not. In relation to the predictions of agency theory (Eisenhardt, 1989), equity theory (Adams, 1963), and goal framing theory (Foss & Lindenberg, 2013; Lindenberg, 2013; Lindenberg & Foss, 2011), results from our empirical data best matches the predictions of *goal framing theory*.

To check the robustness of our results and to examine our findings in more detail, we performed several robustness checks and additional analyses. In our robustness checks, we show that our results are consistent across different model specifications. In our additional analyses, we present several results

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that deepen the understanding of our results. First, we found that the *performance* (in terms of sales revenue of deals) of individuals and teams also increased as a result of the team's incentives change, implying that the increase in own effort and collaboration due to the incentives change created beneficial synergies. Second, results show that *production efforts* (x), *outgoing collaboration* (y_o), and *incoming collaboration* (y_i) were larger for individuals who sold similar types of deals with team peers, suggesting these individuals exploited greater degrees of synergies. Finally, analyses suggest that the effects of the team's incentive change were not permanent and diminished over time. Details of the robustness checks and additional analyses are available in Appendix S1.

Discussion and Conclusions

Understanding how incentives work is a priority for both the research and practice of strategic management (Larkin et al., 2012). Incentives are a key instrument through which strategy is implemented, by motivating and directing efforts. For this instrument to work effectively, it is critical to understand the consequences of different incentives regimes and the changes between them. This research helps us advance toward this goal in three ways.

First, by examining a rare switch from pay-for-performance to fixed salary, we are able to set up a critical test of two competing perspectives on how incentives work: the individual effects perspective and the social effects perspective. This particular switch is one that generates contrasting predictions from the two perspectives, which is not always the case for other kinds of incentive changes when they occur in settings in which synergies in team production exist. In contrast, the change we observe leads to distinctive predictions from perspectives that restrict the effect of a change of incentives to a focal individual's behavior (e.g., Holmstrom & Milgrom, 1994; Jensen & Meckling, 1976), *versus* others that allow a focal individual's behavior to change even when other's incentives alone are changed (e.g., Adams, 1963; Foss & Lindenberg, 2013; Lindenberg & Foss, 2011).

In our empirical setting, the evidence is strongly supportive of the existence of social effects, and in particular of the form predicted by goal framing theory; the change of a focal agent A's incentives matters to B not because of effects on perceived equity, but because it makes both A and B more likely to adopt a particular goal frame.

Second, our article provides the first evidence for goal framing theory with naturally occurring data. While the ideas underlying the theory have been developed over more than a decade (Gottschalg & Zollo, 2007; Lindenberg, 2001; Lindenberg & Foss, 2011), we believe our article is the first to undertake an empirical test outside the laboratory. Doubtless much still needs to be done to establish mediating processes, but we see our research as a first step along a fruitful research trajectory.

Third, because our data were obtained from a natural experiment arising from the random order in which the incentives change was implemented, our inferences about the effects of this change are closer-to-causal than what can usually be obtained from naturally occurring data, either primary or secondary. Our results thus contribute to a growing number of field experiments that have begun to explore the causal effects of particular incentive regime changes (e.g., Burks et al., 2009; Shearer, 2004).

Since our context is one in which collaboration is primarily of the form of knowledge sharing, our results also generate some unique insights into the effects of incentives on organizational learning. The hoarding of knowledge is possible by self-interested agents because of certain features of knowledge such as its tacitness, complexity, and casual ambiguity (Gottschalg & Zollo, 2007; Szulanski, 1996). However, even when knowledge is relatively easy to document, codify, and transfer—such as the identity of key suppliers—norms that protect the private value of such knowledge for individuals may nonetheless arise that may be beneficial for the individual but not for the organization. This was the case in Chimera; but the incentive regime change effectively demolished the norm and allowed for freer sharing of such knowledge. Thus incentives may be relevant for promoting organizational learning through intra-organizational knowledge exchange, even when the knowledge is closer to the ideal type envisaged by Arrow (1962) (see also Winter & Szulanski, 2001).

We also note several limitations to our study. First, we should be cautious about assuming that our effects are truly causal, because while all the available evidence strongly suggests that randomization occurred for the order of incentive regime change across individuals, we were not present when this

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occurred. We are therefore content to claim that our results are closer to casual than prior work that relies on naturally occurring data without experimental design, but we do not stake a claim to a perfect experiment. Second, our sample may be idiosyncratic. It comes from one company embedded in the South Korean culture, which is known to be high on power distance and low on individualism (Hofstede, 1980). We have followed the lead of prior research involving field experiments that has accepted the decline in generalizability for the opportunity to get closer-to-causal inference (Duflo et al., 2008; Flyvbjerg, 2006; List, 2011; Scandura & Williams, 2000), but it would be interesting to see if our results hold in different contexts. Relatedly, the evidence is from a fairly small sample of 41 employees, albeit with multiple observations that vary on both the independent and dependent variables (and not just repeated measures).

Third, given this is a natural experiment, we observe only a single treatment (incentive regime switch), which makes it hard to parse out the precise mechanisms through which this switch worked: for instance, could the managerial framing and explanation of this change to employees have been sufficient to produce the observed effects? Would the goal framing effects of the incentive regime have been weaker if the managerial discourse around it was inconsistent with normative goal framing? Our field-based data does not allow us to answer these questions.

Finally, our analysis does not compare whether group-based performance incentives or fixed salaries would be better incentive schemes to foster collaboration. Although we cannot directly address this question with this study, prior work suggests that group incentives would be a more effective compensation scheme to use when realizing synergies between group members is critical (e.g., Che & Yoo, 2001; Kretschmer & Puranam, 2008). While we did find in our study that the effects of incentives switch within the team were stronger for individuals who had more potential synergies with team peers, it is quite possible that collaboration would be even stronger in the case where the realization of such synergies are made explicit through a group-based performance incentives scheme. We leave the exploration of this question in the hands of future studies.

Despite these limitations, we believe this study makes an important advance in the study of incentives in organizations because it demonstrates the existence of social effects of incentives in an unambiguous manner, both in terms of behavior and performance of individuals. The evidence should thus serve as a key building block in a fully articulated theory of incentives and their role in linking strategy, organization, and performance.

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Supporting Information

Additional supporting information may be found in the online version of this article:

Appendix S1. Robustness checks and additional analyses.