Article

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Do Activity-Based Incentive Plans Work? Evidence from a Large-Scale Field Intervention

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Abstract

Many firms incorporate activity-based incentive (ABI) compensation into their pay plans. These ABIs are based on salespeople's activity measures derived from their call reports. Despite their prevalence and theory-based expectations, there is a distinct lack of empirical work studying the sales productivity effects of ABI pay. With the cooperation of a large pharmaceutical firm, the authors conducted a three-year-long intervention based on a "treatment-removal" design. Their first intervention added modest ABI pay for frontline salespeople and their supervisors across 305 sales territories; the second intervention removed ABI pay from the salespeople, and the third intervention removed ABI pay from the supervisors as well, returning to the status quo. Using detailed territory-level data from the intervention in conjunction with syndicated market-level data and employing synthetic control procedures, the authors find sales gains of around 6%–9% from each of the two ABI interventions relative to the no-ABI baseline. These effects are moderated by the number of salespeople in a territory, with territories with more salespeople showing larger effects. Analyses of activity effects show that when supervisors are paid ABIs, they exert behavior control downward on salespeople. Managerially, both ABI schemes improve performance over an output-only pay plan. Between the two, a rudimentary gross profit impact calculation shows that ABIs targeted at supervisors alone are more efficient than ABIs targeted at both salespeople and their supervisors. The results support tying compensation to call reports despite the potential for self-serving biases in these measures because supervisors are able to exercise more behavior control with ABIs.

Keywords

activity-based incentives, field experiments, sales compensation, principal-agent theory, synthetic control

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Understanding the relationship between incentives and sales force productivity has generated considerable interest among academics and practitioners alike. Recent estimates suggest that about half of all industrial and commercial sales forces employ incentive pay, with annual costs exceeding $800 billion (Steenburg and Ahearne 2012). The marketing literature also shows incentive pay studies appearing steadily over time, including analytical models (e.g., Basu et al. 1982; Weinberg 1975), observational studies (e.g., John and Weitz 1989), and field experiments (e.g., Chung and Narayandas 2017). The specific issues examined include commission rates for multiple products (e.g., Lal and Srinivasan 1993), output attribution effects (e.g., Anderson and Schmittlein 1984), firm size effects (e.g., Misra, Coughlan, and Narasimhan 2005), bonus versus commission incentives (e.g., Kishore et al. 2013), and national culture effects (e.g., Segalla et al. 2006).

Strikingly, despite the rich and varied sources of these data and settings, there are two visible gaps. First, without exception, the empirical work focuses on sales-based incentive pay plans to the exclusion of activity-based incentives (ABI).[1] This is a significant omission given theory and industry practice. The dominant theoretical lens, principal-agent theory (PAT; e.g., Holmstrom 1979), posits that incentive pay should

[1] Activities refer to salesperson interactions with customers, and metrics include number of visits, proposals submitted, quotations, prospecting, meetings and presentations, and so on.

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incorporate all available unbiased signals, including activity signals. Turning to practice, 15% of the firms surveyed incorporated activity signals in their incentive plans (e.g., Zoltners, Sinha, and Lorimer 2006). Thus, while both theory and practice suggest ABIs might be effective, we lack systematic evidence about their effects.

A second gap is an exclusive focus within the literature on frontline salespeople to the neglect of supervisors. This gap is surprising given the long-standing emphasis on supervisors' vital role in shaping salesperson behavior (e.g., Cravens et al. 1993; Oliver and Anderson 1994) in sales force control theory. The importance of supervisors is apparent if we consider that ABIs are fashioned from "call reports" that are submitted regularly by salespeople to their supervisors. These documents detail a salesperson's activities during the reporting period such as numbers of clients visited, product presentations made, requests for proposals generated, and so on. The veracity of call reports can be problematic because the activities at hand are generally undertaken in the field and often not in the presence of supervisors.

In practice, firms mitigate these potential biases by authorizing supervisors to adjust the initially reported numbers in consultation with salespeople. Supervisors routinely utilize call reports to assist in their role of evaluating and managing salespeople. This suggests that paying supervisors on salesperson activity measures could motivate them further to direct their subordinates' behavior more closely, likely yielding productivity gains. Unfortunately, the lack of work on supervisor incentive pay (for a notable exception, see Bandiera, Barankay, and Rasul [2007]) leaves these downward behavior control effects unresolved. In summary, notwithstanding the predicted effectiveness of ABIs in theory and the prevalence of ABIs in practice, the absence of empirical work on ABIs constitutes a significant gap in our understanding of sales force incentives.

Goals and Contributions

Do ABIs improve sales productivity? What moderators shed light on the mechanism(s) involved? Who is the locus of ABI effects—salespeople or supervisors? Answering these questions poses several challenges. First, longitudinal data are needed to control for unobserved territory effects. Second, we need to compare call reports that *are* incorporated into ABIs with the same call reports that are *not* incorporated into ABIs. This will enable a researcher to distinguish the mere measurement and monitoring effects of call reports from incentive effects. At the same time, we need to hold other compensation elements constant, particularly the prevalent sales-based incentives, to isolate the effects of ABI.

To meet these challenges, with a large South Asian pharmaceutical firm's cooperation, we undertake a longitudinal, large-scale field intervention for an entire sales unit in one of its strategic business units (SBUs). The salespeople within this SBU completed activity call reports, but these were not included in any formal incentive system. In a three-year-long "treatment-removal" field intervention, we introduced, and later withdrew, ABIs for both frontline salespeople and their first-line supervisors across 305 sales territories of this firm.

Our Year 1 data track outcomes under the status quo ante sales-based incentive plan. Salespeople and supervisors were paid a salary plus a monthly piecewise bonus on total territory sales. In territories with multiple salespeople, total territory sales were attributed equally to each salesperson. Call reports were recorded, and activity targets were assigned to salespeople and supervisors under this regime, but no ABIs were paid. At the start of the second quarter in Year 2, ABIs were added for both salespeople and their supervisors. We withdrew this treatment in two phases, first removing it from the salespeople after six months, then removing it from their supervisors at the end of Year 2. Year 3 again tracks the status quo ante plan regime.

Preview of Results

First, our "supervisor-only ABI" (SABI) treatment yields a significant increase in salesperson activity scores (about 7%) beyond the no-ABI baseline recorded from call reports. Second, our "supervisor + salesperson ABI" (SSABI) treatment also yielded similar increased salesperson activity scores beyond the no-ABI baseline. These results suggest that supervisors should be the important foci of our ABI treatment effects, corroborating arguments from sales force control theory about supervisory control shaping salesperson behaviors (e.g., Cravens et al. 1993; Oliver and Anderson 1994).

For sales effects, we find that the SABI treatment yields a significant increase in sales (about 6%–9%, depending on the model specification) beyond the no-ABI baseline. The SSABI treatment also increased sales (about 8%) beyond the same no-ABI baseline. In both instances, the effects are magnified in territories with larger numbers of salespeople. This moderating effect hints at the following moral hazard mechanism. The firm observed only total territory sales and consequently attributed these sales *equally* to all salespeople. This attribution yields a noisier signal when more salespeople are present, thus weakening the incentive effect as per the principal-agent model (e.g., Holmstrom 1979). In contrast, activity reports are produced at the individual level and are not similarly compromised by larger numbers of salespeople in a territory.

While our "treatment-removal" design enabled us to capture the effects of ABIs relative to no-ABIs, it lacks a control group. It is, therefore, possible that unobserved time-varying firm and market factors could affect our results. We collected and analyzed syndicated market-level data across other SBUs of the firm that were not exposed to our treatment. In particular, we used syndicated data to create a synthetic control using the synthetic control methodology (SCM) outlined in Abadie, Diamond, and Hainmueller (2010) on aggregated market sales data and calculated the resulting overall treatment effect. The results were similar to our original analysis without control. The treatment effects for SSABI and SABI were sales improvements of 7.81% and 7.07%, respectively. Next, because our treatment is on multiple territories and we have richer territory-level data, we extended the SCM approach to incorporate territory analysis using a generalized synthetic control (GSC) method proposed by Xu (2017). The results from this analysis are also consistent with our other results. The average treatment effect (ATE) for ABIs was sales improvement of around 8.73%.

Managerially, the principal takeaway is that ABIs improve the sales productivity of outbound salespeople, notwithstanding the presence of potential self-serving biases in activity reports. Furthermore, supervisors should be the principal foci of ABIs, as their supervision actively shapes salespeople's behavior. In most of our empirical specifications, the sales gains attributable to ABIs for both salespeople and supervisors are modestly higher than the gains attributable to using ABIs for supervisors only. However, it is possible that the gains from SSABI are likely to be offset by the additional compensation costs incurred by paying more people under this scheme. Indeed, a rudimentary gross profit impact (GPI) calculation suggests that firms aiming to utilize ABIs should prioritize incentivizing frontline supervisors.

Organization of the Article

The remainder of the article is structured as follows. In the next section, we selectively review adjacent literature streams to obtain insight into the issues. We provide a detailed description of our empirical context and data, followed by the intervention details and preliminary analysis. We then present the main analysis using the control SBUs and a description of both the synthetic control and GSC application within our context and associated GPI calculations. Finally, we present implications and suggestions for future scholarly work and practice.

Literature and Theory

Salesperson performance signals generated from the accounting system can be categorized into output (sales) and costs. A defining characteristic of accounting-based measures is the consistency with which information is recorded and the absence of idiosyncratic biases. However, these characteristics often constrain the breadth and scope of available accounting-based signals of costs. Availability depends significantly on the sophistication of the accounting system and the selling environment itself. For instance, relevant costs of sales are not available at the desired level in the absence of an activity-based costing system. In addition, formulaic allocations of costs across products, territories, and accounts are also quite common.

Even sales measures may be questionable signals because of noise. For instance, individual-level sales output is difficult to ascertain when team selling is prevalent (e.g., Anderson 1985) or when different channels touch customers at multiple points during their buying journey. Long selling cycles (e.g., John and Weitz 1989) and salespeople tasked with evangelizing products yield sales signals that are very noisy.

Call reports are a ubiquitous, formal self-reporting system wherein each salesperson reports periodically on a specific set of activity measures devised by the firm. For instance, at our research site, the firm's call reports included the number of physicians and pharmacies visited during a given period. When self-reports of calls are contrasted with corporate accounting-based sales measures, certain differences become obvious. First, call reports start with individual self-reports, so idiosyncratic, self-serving biases are likely to be present. To ameliorate this problem, supervisors are empowered to discuss and modify the salesperson's initial call report. Second, call reports are more widely available than are corporate accounting-derived measures. For instance, as noted previously, team selling makes it challenging to track individual-level sales signals. In contrast, call reports produce individual-level measures, albeit requiring supervisory systems.

In summary, both self-reporting and accounting systems provide useful information about sales performance, albeit with compromises. Accounting signals are objective measures of performance but could be noisy and unavailable at the individual level. Call reports are at the individual level but are subjective and require supervision to ameliorate biases.

Sales Force Control Systems

In sales force control theory, activity signals enhance behavioral control while sales signals further output control. Behavior control is a bureaucratic process that combines formalized procedures, supervision, and managerial authority to direct frontline salespeople. Call reports and supervisors are particularly important aspects of behavior control systems. Supervisors shape salesperson behavior by combining direct monitoring and activity signals from call reports. In contrast, output control (typically implemented via sales incentive pay) harnesses motivational forces (e.g., Ouchi 1979). Supervisory assessments and behavior mandates take a back seat to financial incentives, and straightforward, objective measures from accounting systems come to the forefront.

Output controls can be counterproductive when they crowd out behavior control. Financial incentives that tie compensation to performance signals in a formulaic way evoke more significant discretionary behavior and subgoal pursuits on the part of salespeople as they seek to maximize their income, thereby diminishing the supervisory shaping of behavior. For example, Oliver and Anderson (1994) found that salespeople on incentive compensation plans are less committed to their firm's procedures and policies.

PAT

Popularized by Holmstrom (1979), PAT explains how a principal can motivate an agent to undertake unobservable effort (which makes behavior control infeasible) by fashioning incentive compensation on the basis of observable signals.

According to PAT, incentives motivate salespeople in two ways. First, each salesperson exerts more effort toward desired organizational goals because their income is at risk if they were to slack off or focus on tasks unrelated to the metrics determining pay. Second, incentive pay accommodates more variance in income across salespeople on the same plan. Higher-ability salespeople can earn more money than their lower-ability counterparts, so motivational effects of incentive pay are greater among higher-ability salespeople (e.g., Kishore et al. 2013).

Lafontaine and Slade (1996) develop a model within the PAT framework wherein a firm has access to noisy sales and activity signals for an agent. Their model shows that the optimal incentive plan should incorporate both the signals, with the noisier signal getting a smaller weight. A principal source of noise in sales signals within our setting is that the firm only observes territory-level sales and employs the simple expedient of attributing observed sales equally to salespeople. Plainly, the noise included with this procedure increases with the number of salespeople, which dilutes the motivational effect.

Combining Behavior and Output Control

Sales force organizations consist of multiple vertical levels, and typical behavior control flows downward from the supervisory level to the salesperson level. Consider a situation in which ex ante salespeople respond to incentive control from their sales-based pay and behavior control emanating from their supervisor. According to sales force control literature, these elements are in tension with each other; for example, Oliver and Anderson (1994) found that incentive control diminished salespeople's commitment to following procedures and policies because of the enhanced desire to maximize their personal returns. Prendergast (2002) offers a similar line of reasoning. Agents often engage in activities over which they might have personal preferences ("personal benefits" in Prendergast's terminology, pp. 1081–83), and these preferences and the firm's payoffs may conflict. As such, these personal benefits often constrain the level of delegation (i.e., incentive control) to agents. In contrast, behavior control relies on supervisory behavior to circumscribe salesperson discretion to pursue personal gains. Supporting this notion, Phillips (1982) found increased supervisory control decreased control losses from salespeople's pursuit of personal returns in company sales branches.

Consider the introduction of ABI pay to supervisors in light of these theoretical arguments and empirical findings. Supervisory ABIs (SABIs) increase behavior control exerted over salespeople, particularly along the call report measures. Next, suppose the firm were to further extend ABI pay to salespeople (SSABI). This increases the salesperson's desire for discretion and autonomy, which *decreases* the effect of supervisory behavior control. It is therefore unclear whether the incentive effects dominate the effects of supervisory behavior control; either a modest increase or else even a decrease in productivity might happen. We let the data speak to this issue and revert to this discussion after detailing our empirical findings.

Summary

Our review yields the following expectations of adding ABIs in a sales force setting where sales signals are, ex ante, the principal basis of incentive pay: (1) adding ABIs to supervisors increases their subordinate salespeople's activity scores; (2) adding ABIs to supervisors increases salesperson sales productivity; and (3) sales productivity gains in (2) are moderated positively by the number of salespeople in a territory. Finally, we expect a positive sales productivity change when ABIs are extended to both levels (salespeople and supervisors), but it remains an empirical question to determine the effect of this intervention relative to (2) where ABIs are applied only to the salespeople.

Context and Data

We solicited the cooperation of an SBU of a large pharmaceutical company in a South Asian country that sells a range of prescription drugs nationally. The corporate team at the parent firm of this SBU decides on the pricing of individual brands and the incentive structures across all the SBUs. Specifically, the incentive structures across SBUs have been standardized and are fairly homogeneous. The pharmaceutical industry in the country is highly competitive because of a relatively relaxed regulatory regime and the continued prevalence of direct payments by patients. A patient is generally expected to pay for services—for both physician services and prescription fulfillment—when these services are rendered. Insurance coverage is increasing, but coverage is typically a reimbursement system, where the covered individual files a claim with the insurer after paying for the service. The doctor's prescription (which is typically written in chemical/generic form) is fulfilled at a pharmacy. Pharmacies are legally permitted to offer a chemically equivalent alternative, and as noted previously, the purchaser pays directly, notwithstanding insurance. Pharmacies are small, independent businesses that do not stock large volumes of drugs.

Sales Organization

Our cooperating SBU employs a sales force organized into 305 territories. Each territory has anywhere from 1 to 11 salespeople. A field salesperson is tasked with calling on a set of doctors and pharmacies and is assigned a list of physicians and pharmacies developed by their supervisor. These lists do not overlap. However, in some instances, multiple salespeople call on the same pharmacies and physicians that are not on anyone's list. The focal SBU classifies its sales regions geographically. As per its classification, the territory is the basic unit and is drawn geographically. These territories do not map precisely into the administrative demarcation of districts. Multiple territories are combined into an "area" under a single supervisor.

Supervisors are the frontline managers directly responsible for managing salespeople.[2] Across our intervention period of 36 months, an average of 412 salespeople were spread across 305 territories and were managed by 71 supervisors. Each supervisor managed between 4–11 salespeople in an area consisting of 3–6 territories.

Supervisor duties include both managing the salespeople and engaging in selling activities. Selling activities such as doctor visits almost invariably happen in the presence of a salesperson. While salespeople have a nonoverlapping set of pharmacies and doctors, supervisors can visit any of these in their territories and make periodic calls to check the veracity of claims made in call reports. Management emphasizes this aspect of the supervisor's role. A supervisor's promotion to the next level is strongly influenced by their success at these monitoring and management tasks. As such, it is in a supervisor's interest to make these reports as accurate as possible. Our discussions revealed that in about 30% of the cases, a correction was made to the report submitted made by a salesperson prior to our intervention.

The salesperson's job is to present information and provide samples to the physicians. All salespeople in this SBU carry the same line of products and do not make joint calls, and all activity targets are individually assigned. At pharmacies, in addition to presenting drug information, salespeople assist with the pharmacist's business processes, including assisting them with promotional campaigns, inventory, and billing issues.

Activity Signals

The firm's call report system requires each salesperson to report four key metrics each month; listed physicians visited, listed pharmacies visited, nonlisted physicians visited, and percentage of "medical area" covered. The last metric is a proxy for how wide a net a salesperson is casting (to disincentivize them from concentrating on a few pharmacies and doctors in a smaller area within a territory). The self-reported numbers are discussed with the supervisor, who makes any adjustments deemed necessary on the basis of their monitoring and cumulative knowledge of the territory.

Sales Signals

The firm's accounting system records revenues of each drug sold monthly to every pharmacy. Crucially, even though the firm assigns doctors and pharmacies to individual salespeople, an individual prescription cannot be tracked from the prescribing physician to a specific pharmacy, so observable sales are the aggregate sales across all pharmacies within a territory. Because individual prescriptions cannot be linked to the individual sales transaction, the firm simply divides total recorded territory sales equally among the number of territory salespeople to arrive at a salesperson-level sales signal. Notice that the same information leads to different inferences on the noise at two levels. At the salesperson's level, the noisiness of the derived individual salesperson's sales signal increases with the number of salespeople, but at the supervisor's level, the noisiness of the supervisor's sales signal is invariant to the number of salespeople.

Sales-Based Compensation

Each salesperson is paid a monthly salary plus a tiered bonus; the latter figure depends on attaining the territory sales target. At the start of the financial year, the firm declares all territory-monthly sales targets. In multiperson territories, all salespeople receive equal credit for sales. The bonus is calculated as follows.

For a salesperson i in territory k of type E ($E \in [H, L]$) in month t with target Q_{kt} and sales S_{kt}, the bonus (in units of local currency) is $B_{ikt(E)} = \begin{cases} 0, & S_{kt} < Q_{kt} \\ B_{1(E)}, & Q_{kt} \le S_{kt} < 1.05Q_{kt} \\ B_{2(E)}, & S_{kt} \ge 1.05Q_{kt} \end{cases}$.

There are several items of note in this scheme. First, there are two attainment bonuses (at 100% to 105%, and at 105% of Q_{kt} and above, respectively) carrying increasing payout amounts (i.e., $B_{2(E)} > B_{1(E)}$). Second, these bonuses do not carry a territory subscript k, as each one is set at just one of two levels depending on the territory's classification as either high yield or low yield; (i.e., $E \in [H, L]$). The classification of a territory into high yield or low yield is based on a territory threshold: a territory with a revenue potential equal to or higher than this threshold is classified as high yield, and as low-yield otherwise. The territories classified as high yield have higher targets. The bonus amounts are larger in H-type territories (i.e., $B_{1H} > B_{1L} > 0$, $B_{2H} > B_{2L} > 0$).

Supervisors

Each "area" supervisor is responsible for multiple territories that are geographically adjacent. The monthly target for an area "a" consisting of G territories is given as an average of all the territory targets: $Q_{at} = \sum_G Q_{kt}/G$. The bonus calculations for supervisors then follow a procedure similar to that of salespeople. The precise description of incentive plans along with numerical examples are communicated to salespeople and supervisors through monthly circulars. Note that this bonus scheme remained essentially unchanged throughout the three years of our observational period, but there were periodic adjustments in the targets.[3]

[2] Similarly, multiple "areas" are clubbed into a "region" managed by a regional manager, and a collection of regions form a "zone" controlled by a zonal manager (see Figure A1 in Part A of Web Appendix for the sales organization within the firm). The regional and zonal managers were not provided with ABIs and have different sets of output incentives.

[3] This raises the obvious concerns of ratcheting (which refers to the consequences of a firm's practice of raising quotas in response to good current sales and its potential demotivating effect on sales reps). However, because these concerns are present across both non-ABI and ABI regimes, they do not affect our estimates of ABI effects.

Activity-Based Compensation

The firm's behavior control system relies on individual call reports. Supervisors use these reports as a management and evaluation tool to create call lists and to change the assignment of salespeople to doctors and pharmacies. The firm creates a composite activity score (CAS) to be used in overall subjective performance evaluations. However, historically, the CAS has not been tied to incentive compensation.

ABI incentives for salespeople are based on a set of activities (j) for which individual targets are provided. Recall that salespeople are measured on the four dimensions described previously. Each salesperson gets an individual target (T_{ij}) on each of the four activities, and their performance is graded relative to this target.[4] Each activity receives a different weight generating the CAS. For a salesperson i in period t, denote O_{ijt} as his performance on activity j $\in\{1,2,3,4\}$. The salesperson's activity score (P_{ijt}) on activity j is calculated as follows:

$$P_{ijt} = \begin{cases} \frac{O_{ijt}}{T_{ij}} \times 100, \text{ if } O_{ijt} < T_{ij} \\ 100, \text{ if } O_{ijt} \geq T_{ij}. \end{cases}$$

CAS is calculated using all the activity scores with predefined weighting as $CAS_{it} = \sum_{j==1}^{4} w_j P_{ijt}$. The weights for the activities during the intervention were $w_1 = .35$, $w_2 = .10$, $w_3 = .20$ and $w_4 = .35$. A supervisor's score is the aggregate of all the individual activity targets and outcomes and carries the same weights for CAS.

Intervention and Preliminary Analysis

Intervention Design

Our design is a within-unit design where each treated unit serves as its own control, in that we subject it first to the treatment and then remove the treatment. In the language of lab experiments, this is a treatment-removal within-subject design (Blanco, Engelmann, and Normann 2011; Charness, Gneezy, and Kuhn 2012). Our observations span a pretreatment spell, a within-treatment spell, and a posttreatment spell, allowing us to control for time and history effects that would otherwise pose validity threats to causal inferences, given the absence of a control group. Note that the posttreatment spell returns the SBU to the status quo ante. To strengthen our analysis of treatment effects, we supplement our in-intervention data with syndicated data from other SBUs of the firm to apply synthetic control analyses.

Our design is compactly summarized as 36 monthly observations from each territory organized as $O_1, \ldots, O_{15}, X_1, O_{16}, \ldots, O_{21}, X_2, O_{22}, \ldots, O_{24}, X_3, O_{25}, \ldots, O_{36}$, where O_i consists of territory-month sales over 36 months and salesperson-level activity scores over the last 24 months. Following 15 months of the no-ABI status quo regime, X_1 represents the "supervisors + salesperson" ABI treatment (SSABI) introduced in Month 16. X_2 is the "supervisor-only" ABI treatment (SABI) introduced in Month 22 (which removed ABI pay for the salespeople), and X_3 is the no-ABI treatment beginning in Month 25, which removed the supervisors' ABI pay, returning the organization to the status quo ante.

Based on the CAS of a salesperson in a given period, the salesperson ABI incentives were designed as (in local currency): $ABI_{it} = \begin{cases} 0, & CAS_{it} < 90 \\ A_1, & 90 \leq CAS_{it} < 95 \\ A_2, & 95 \leq CAS_{it} < 98 \\ A_3, & CAS_{it} \geq 98. \end{cases}$

Note the subscript i in the incentives. As such, ABIs are salesperson-specific and based on individual-level CAS. Further, the ABIs are smaller and are about 10%–15% of the status quo sales bonus payments. That is, $A_1 < A_2 < A_3 < B_{1(E)} < B_{2(E)}$. Supervisor ABIs are based on the aggregate of salesperson ABIs and are larger than individual ABIs but smaller than the baseline output-based bonuses.

Table 1. Descriptive Statistics (Territory Data).

Variable	Territory Sales	CAS	Number of Reps	Target Sales
Mean	541,984	.929	1.35	534,649
Median	390,373	.978	1	388,000
SD	556,074	.140	.98	507,909
N Obs	10,927	9,355	10,927	10,927

Notes: The territory sales and target sales are in local currency.

Preliminary Analyses

Table 1 shows the descriptive statistics of the focal variables. The average territory sales are 541,984 in local currency units, and the average activity score is around 93%. To assess the impact of the interventions on activities and sales, we begin by exploring two dependent variables, individual-level CAS data and territory-level sales data.

CAS effects. We analyze the activity scores with the following statistical model:

$$\begin{aligned} CAS_{ikt} = \; & \beta_0 + \beta_1 INTV1_t + \beta_2 INTV2_t \\ & + \alpha_k[\text{Territory}]_k + \gamma_t[\text{Month, Year}]_t + \epsilon_{ikt}, \end{aligned} \quad (1)$$

where CAS_{ikt} is the activity score of salesperson i in territory k in month t, $INTV1_t$ is a dummy variable set to 1 for SSABI (months 16- 21) and 0 otherwise, $INTV2_t$ is a dummy variable set to 1 for SABI (months 22–24) and 0 otherwise, $[\text{Territory}]_k$ is a set of (K − 1) dummy variables capturing territory fixed effects, and $[\text{Month, Year}]_t$ is a set of (T − 1) dummy variables capturing month and year fixed effects.[5]

[4] Activity targets (T_{ij}) are mostly the same for all the salespeople, with a few minor variations across territories. However, the performance is tracked individually, and to emphasize this we put a subscript i in T.

[5] We have access to the activity scores only for Years 2 and 3 of our study. Furthermore, we have access only to the CAS, not the individual activity metrics.

Table 2. Estimated Effects of ABI Interventions on CAS.

	Coefficient (SE)	
Dependent Variable	Salesperson-Level CAS	Territory-Level CAS
Independent Variables	Model 1	Model 2
Constant	.94*** (.02)	1.78*** (.00)
Intervention 1 (dummy)	.057*** (.00)	.037*** (.015)
Intervention 2 (dummy)	.076*** (.012)	.088*** (.018)
Territory dummies	Included	Included
Year dummies	Included	Included
Month dummies	Included	Included
Adj. R^2	.38	.79
N Obs.	9,355	6,386

***$p < .01$.
Notes: CAS is available only for Years 2 and 3 of the intervention.

Table 2 shows the estimates of this model. The estimate for Intervention 2 (SABI) is positive and significant (.076, $p < .01$), showing that adding ABI pay to supervisors alone elevated CAS over the baseline. This translates to an improvement of about 7.6%. Turning to the SSABI effect, we see that paying ABIs to both supervisors and salespeople also increases CAS significantly above the no-ABI baseline. The Intervention 1 coefficient is positive and significant (.057, $p < .01$). To the best of our knowledge, we believe this is the first empirical evidence of the behavioral effect of ABI pay.

We reestimated this model with activity scores aggregated at the territory level to correspond to the treatment level. Model 2 in Table 2 shows that the number of observations shrinks, as expected. The explained variance increases considerably (adjusted $R^2 = .79$), which is unsurprising, as we are aggregating observations across individuals. Substantively, however, there is no change in our results. As before, the coefficients of Intervention 1 (.037, $p < .05$) and Intervention 2 (.088, $p < .05$) are positive and significant, mirroring our initial results. The only change is that incentives paid to supervisors only (SABI) now show activity scores improving significantly beyond the level attained from incentives paid to supervisors and salespeople (SSABI), with the difference evaluating to 5.1% ($p < .05$). This supports our expectation that more incentive pay at the frontline salesperson level makes them seek more discretion and less willing to be directed by the supervisors.

Anecdotally, based on discussion with the management, it appears that after the ABIs were implemented, supervisory adjustments to initial call reports happened at a higher rate (45% vs. 30% before ABI implementation). The implicated mechanism is that while call reports were available to the supervisor in the Year 1 ex ante regime, ABIs provided additional motivation to focus on this aspect of the job. This had multiple effects: (1) salespeople made more calls, (2) supervisors spent more time traveling with salespeople during visits, (3) greater scrutiny was applied to the call reports, and (4) more (downward) adjustments were made for the call reports.

Table 3. Estimated Effects of ABI Interventions on Territory-Level Sales.

Dependent Variable: Territory Sales	Coefficient (SE)	
Independent Variables	Model 1	Model 2
Constant	−65,846 (44,711)	−208,568* (109,642)
Intervention 1 (dummy)	43,862*** (10,255)	−11,710^{ns} (14,872)
Intervention 2 (dummy)	34,392*** (11,263)	−10,225^{ns} (13,508)
Number of salespeople in territory (NReps)	N.A.	32,004^{ns} (26,377)
Intervention 1 × Number of salespeople	N.A.	42,892*** (11,097)
Intervention 2 × Number of salespeople	N.A.	33,141*** (8,924)
Target sales (Target)	1.04*** (.05)	1.02*** (.05)
Territory dummies	Included	Included
Year dummies	Included	Included
Month dummies	Included	Included
Adj. R^2	.93	.93
N Obs	10,927	10,927

*$p < .1$.
**$p < .05$.
***$p < .01$.
Notes: Standard errors for are clustered at the territory level. The territory sales (dependent variable) are in local currency.

Sales effects. Next, we discuss the sales effects hypotheses with the set of statistical models in Table 3 with the following model:

$$\begin{aligned} y_{kt} = {} & \beta_0 + \beta_1 \mathrm{INTV1}_t + \beta_2 \mathrm{INTV2}_t + \beta_3 \mathrm{Target}_{kt} \\ & + \beta_4 \mathrm{NReps}_{kt} + \beta_5 \mathrm{NReps}_{kt} \times \mathrm{INTV1}_t + \beta_6 \mathrm{NReps}_{kt} \\ & \times \mathrm{INTV2}_t + \alpha_k[\mathrm{Territory}]_k + \gamma_k[\mathrm{Month,\ Year}]_t + \epsilon_{kt}, \end{aligned} \tag{2}$$

where y_{kt} is the sales in territory k in month t, Target_{kt} is the sales target for territory k in period t; NReps_{kt} is the number of salespeople in territory k in month t; and $\mathrm{NReps}_{kt} \times \mathrm{INTV1}_t$ and $\mathrm{NReps}_{kt} \times \mathrm{INTV2}_t$ are the interaction variables. The other variables have been defined previously.

In Table 3, Model 1 shows the estimates from this specification without the inclusion of NReps and the interaction variables. Model 1 fits the data well (adjusted $R^2 = .93$). Higher announced targets are associated with higher sales ($\beta_3 = 1.02$, $p < .01$) suggesting that the Target variable captures unobserved territory sales potential, as anticipated. Turning to the SABI and SSABI treatment variables, the positive, significant coefficient of Intervention 1 (Model 1; 43,862, $p < .05$) shows that SSABI pay improved sales productivity over the no-ABI baseline. Evaluated at the mean, this is an improvement in sales by about 8%. Turning to SABI, we once again see a sales increase over the no-ABI baseline; the Intervention 2 coefficient is positive and significant (34,392, $p < .05$). Evaluated at the mean, this is an improvement in sales by about 6.27%.

Results for Model 2 in Table 3 show that each of the two interaction terms is positive and statistically significant. The contingent effect of SABI pay is given by $\partial y/\partial \text{INTV2} = \beta_2 + \beta_6 \times$ NReps, which is $(-10,225 + 33,141 \times \text{NReps})$ at each level of the number of salespeople. This effect evaluates to a value of 22,886 in one-person territories, increasing to 56,027 in two-person territories, and higher still beyond that. The contingent effect of SSABI pay is given by $\partial y/\partial \text{INTV1} = \beta_1 + \beta_5 \times \text{NReps} = -11,710 + 42,892 \times \text{NReps}$. This effect evaluates to a value of 31,182 and 74,074 in one-person and two-person territories, respectively. Thus, for both SABIs and SSABIs, greater incremental productivity is observed for larger sales teams. This provides directional evidence that as the noisiness of the sales signals goes up, the efficacy of ABIs increases.

Overall, our preliminary analysis thus far indicates that both SSABI and SABI have positive and significant effects on incentivized activities and sales. However, the lack of a control group could raise concerns about the validity of these results, and next, we outline an approach to address this concern directly.[6]

ABI Effects Using Synthetic Controls

While the "before-after" designs have been employed to study similar issues in marketing (e.g., Kishore et al. 2013; Viswanathan et al. 2018) and economics (e.g., Bandiera, Barankay, and Rasul 2005, 2009), one concern with these designs is that time-varying unobservables could undermine the identification (Bandiera, Barankay, and Rasul 2011, p. 69). In particular, one could argue that the overall performance outcomes estimated by our previous analyses could be attributed to the unobserved firm or market-specific shocks over time. We turn to our main analysis, which explicitly addresses and alleviates this issue. Note that the time confound issue would have been directly addressed had we run ABI treatments within a subset of randomly selected territories and relegated the rest of the territories to serve as a control. However, because of institutional constraints, we could not do that. In lieu of that, we obtained sales data from the other SBUs of the firm that did not undergo ABI intervention to serve as quasicontrols. We describe this new data, followed by the details of the analyses.

Data

We obtained a detailed data set from a leading market research (MR) firm that specializes in collecting global health care–related data and has significant operations in the focal country. The MR firm has extensive resources to obtain reasonably precise monthly sales data at the brand-level for all major pharma players in the country. Specifically, manufacturer sales data is captured from pharma stockists (wholesalers/distributors) across the country. These data come from three sources: sales to pharmacies, direct sales to hospitals, and direct sales to doctors. Approximately 80% of the data from these three sources are captured by the MR firm monthly via remote web-based tools. In smaller cities/regions, where buyers might not be electronically linked to share the data, the firm's personnel conduct field visits to collect the data. Overall, this combined approach leads to a direct data collection of about 85% of pharmaceutical sales. Sophisticated region-wise projection factors are then applied to recreate missing data, and these projections are accepted by industry clients. We assessed the accuracy of the supplied data by checking their figures against our own within-study data. They match closely: the aggregate sales recorded using the data from the MR firm is about 96% of sales recorded by the focal SBU.[7] The high level of correspondence between the internal data provided by the firm and the newly procured syndicated data gives us reasonable confidence in the quality of the new data.

The data contain monthly sales for all brands sold by the focal SBU's parent firm that ran our ABI interventions. As mentioned previously, our focal SBU is one of the SBUs of this diversified pharma firm. The purchased data set includes sales of the products of all the SBUs. Note that pricing and much of the planning for all the SBUs in this conglomerate are handled at a centralized office and are fairly standardized. Further, we were able to verify that the incentive structure at all the SBUs was similar and that none of the SBUs experimented with their incentive pay structure during or around our intervention period. Thus, sales information from other SBUs could serve as a credible "quasicontrol" in our setting.

In addition to brand information, the data contains information about the brand's chemical composition (e.g., Atorvastatin), whether it is used to treat a chronic or acute ailment, and the broad therapeutic category the brand belongs to (e.g., gastrointestinal). Unlike our within-study data, these data are not disaggregated to territory or salesperson sales.

We aggregated the brand-level sales by different SBUs of the parent firm (including our focal SBU). As such, these data comprise monthly sales of 23 SBUs of the parent firm, of which 22 SBUs did not implement any ABI interventions (see Figure B2 in Part B of the Web Appendix for the average sizes of the 23 SBUs). The monthly sales are in tens of millions in local currency with a multiplier.[8]

One obvious and straightforward way to proceed with the analysis would be to compare the focal SBU (the "treated

[6] We also conducted a series of robustness checks for these results including estimating a random-coefficients model and the models accounting for attrition, carryover, and geography. These results are available from the authors upon request.

[7] Note that the MR firm provided us the sales numbers for individual brands across time for each SBU. We then checked the numbers reported at the brand and time period levels by the MR firm against the focal SBU's brand- and time-period-level sales information. For each brand, we collated these twin sets of numbers: the numbers from the MR firm were fairly accurate at the brand level and ranged between 93% to 100% of the SBU-provided numbers, with an overall average of 96%.

[8] The firm-level numbers and the SBU-level numbers have different multipliers (vs. territory numbers) to preserve confidentiality, so the raw estimates are not comparable across the two sets of analyses, but percentage effects are comparable.

SBU" hereinafter) against the other SBUs ("control SBUs" hereinafter) and employ difference-in-differences analyses. Our challenge is that such a comparison must address the possibility that the control SBUs are plausibly different from our treated SBU in size, drug product mix, and focal therapeutic categories. For instance, our focal SBU is a diversified unit with a portfolio of multiple therapeutic categories, whereas other SBUs are specialists. In addition, the geographical presence of many of the control SBUs is different from the treated SBU due to historical reasons related to different formation dates. However, all SBUs have a national presence and are governed by a common marketing strategy for sales procedures, promotion planning, and execution, as well as the information systems in place for collecting the field sales data. Technically, because the treatment is not randomized across treatment and control SBUs, the parallel trend identification assumption required for validity of difference-in-differences estimators is likely to be violated. Thus, we use the synthetic control approach (Abadie and Gardeazabal 2003) to alleviate these concerns.

Synthetic Control Approach

This approach involves assigning appropriate weights to the control units and generating the so-called "synthetic" unit that resembles the overall movement of the treated unit's outcomes in the pretreatment period. Within our setting, such a synthetic SBU can subsequently be used as a counterfactual during the treatment period to yield the causal impact of treatment. A data-driven approach is used to construct such a synthetic control unit (Abadie, Diamond, and Hainmueller 2010; Abadie and Gardeazabal 2003).

Recall that we observe 22 comparison SBUs throughout our intervention period (22×36 monthly observations), none of which were provided ABI interventions. SCM uses appropriate weights for the comparison SBUs (which could be zero) to create a synthetic SBU that best resembles the focal treatment SBU during the pretreatment period to generate an appropriate counterfactual. We briefly describe the procedure next.[9]

Let $\mathbf{Y_1}$ denote a (15×1) vector of pretreatment values for the monthly sales of the treated SBU (indicated as unit 1), and $\mathbf{Y_0}$ represents a (15×22) matrix of the monthly sales for the 22 control SBUs (units 2 to 23). Note that there are 15 months in our pretreatment period. We aim to develop a weighting scheme for the elements of $\mathbf{Y_0}$ such that the resulting outcome closely matches $\mathbf{Y_1}$. Let $\mathbf{W}$ be a (22×1) vector of weights $\hat{w}_s$ for s = 2, 3,.23, and each different set of values of $\mathbf{W}$ leads to a different synthetic control. The weights are chosen to minimize the weighted mean square error $(\mathbf{Y_1} - \mathbf{Y_0}\ \mathbf{W})'(\mathbf{Y_1} - \mathbf{Y_0}\ \mathbf{W})$, subject to $\hat{w}_s \geq 0$, and $\sum_{s=2}^{23} \hat{w}_s = 1$. Once we come up with the optimal weights W*, we can construct the synthetic SBU as $\hat{Y}^*_{1t} = \sum_{s=2}^{23} \hat{w}_s\ Y_{st}$.

Our estimated causal effects of ABI intervention are then calculated as follows:

$\hat{\alpha}_t = Y_{1t} - \hat{Y}^*_{1t}$, where t = 16, 17, 18, 19, 20, 21 for "SSABI intervention (INTV1)."

$\hat{\alpha}_t = Y_{1t} - \hat{Y}^*_{1t}$, where t = 22, 23, 24 for "SABI intervention (INTV2)."

The inference is conducted using a series of falsification exercises via placebo tests that can generate statistics akin to traditional *p*-values. Specifically, we assume that each of the 22 control SBUs underwent the treatment during the treatment period. For each nontreated SBU, we implemented an SCM using the 21 other nontreated SBUs generating the estimated causal effects.[10] Comparing the estimated effect on the treated SBU with the distribution of estimated effects for the nontreated SBUs enables us to infer the probability that our estimated causal impact might have occurred by chance—similar to a traditional *p*-value.

As a first step, we use the estimation approach developed by Abadie, Diamond, and Hainmueller (2010) to come up with the weights for the 22 untreated SBUs using the pretreatment sales observations from periods 1–15. The estimated weights are nonzero only on three SBUs, and these weights are .776, .162, and .062. As seen in Figure 1, the treated SBU's pretreatment sales are fairly closely tracked by the synthetic SBU constructed using the sales data from the three SBUs (see periods 1–15).

Figure 1 also clearly shows that the treated SBU's sales seem to improve significantly once the treatment starts in month 16 (relative to the synthetic SBU, which is the counterfactual for the treated SBU for nontreatment during the treatment period). The treatment effect is simply the gap between the treated SBU's sales and the synthetic SBU's sales. SCM allows us to obtain the time-varying treatment effects. Accordingly, for each of the treatment months from 16–24, we separately calculate the ATEs. We used the standard falsification tests described previously to compute the (pseudo) *p*-values, and Table 4 reports the results.

Overall, the results show the significant positive effects of ABIs. The estimated effects from all the nine treatment periods are positive, and six of these are statistically significant at a 1%

[9] Usually, the synthetic control is created such that it matches the outcome and predictor variables of the treated unit. However, we have only the outcome variable (sales) in our data, but the approach still remains valid (examples of applications in which the SCM is constructed using only the outcomes are Li [2020] and Kim, Lee, and Gupta [2020]). Similar to the arguments made by Kim, Lee, and Gupta (2020), we posit that noninclusion of covariates in synthetic control is valid. Sometimes, the specifications that use only the outcomes of control units have higher predictive power (Doudchenko and Imbens 2016) and lower concerns regarding overfitting (Powell 2018) relative to the specifications that incorporate covariates. Further, recent theoretical and empirical evidence shows that outcomes (Botosaru and Ferman 2019; Kaul et al. 2015) seem to account for most of the predictive power within SCM.

[10] Kim, Lee, and Gupta (2020) have recently developed a Bayesian approach to synthetic control that obviates the need for a placebo-based approach to obtain standard errors. Within their setting, they also show that the Bayesian approach provides superior predictive performance relative to the frequentist approaches.

Figure 1. Actual sales of the treated SBU and the predicted sales of the synthetic SBU.
Notes: The sales numbers are in local currency in tens of millions and have been multiplied with a constant to preserve the confidentiality of the focal firm. INTV1 = "salesperson + supervisor ABIs" (SSABI); INTV2 = "supervisor-only ABIs" (SABI). Each period refers to an intervention month.

Table 4. Estimated Treatment Effects on Sales Using SCM.

Treatment Period	(1)	*p*-value	(2)	(3)
1 (Month 16): INTV1	.809	.0476	4.05%	4.04%
2 (Month 17): INTV1	1.936	<.01	10.16%	10.19%
3 (Month 18): INTV1	1.745	<.01	8.94%	8.94%
4 (Month 19): INTV1	1.449	.0952	6.97%	7.16%
5 (Month 20): INTV1	1.738	<.01	8.38%	8.29%
6 (Month 21): INTV1	1.686	<.01	8.35%	8.00%
7 (Month 22): INTV2	1.659	<.01	7.37%	7.00%
8 (Month 23): INTV2	2.337	<.01	10.96%	10.43%
9 (Month 24): INTV2	.725	.2857	2.90%	2.44%

Notes: We constructed the *p*-values using the placebo (falsification) tests for 22 (control) categories. INTV1 = "salesperson + supervisor ABIs" (SSABI); INTV2 = "supervisor-only ABIs" (SABI). Column 1 shows the effects in local currency in tens of millions and have been multiplied with a constant to preserve the confidentiality of the focal firm. Column 2 reports translated percentage effects from the numbers in column 1. Column 3 reports percentage effects using the natural log of sales as the outcome variable, wherein the reported percentage effect is obtained using exp(effect) − 1.

level, one at a 5% level, one at a 10% level, and only one effect is not statistically significant ($p = .286$). We also translate the effects sizes into percentages for ease of comparison in Table 4. We reestimated SCM by replacing the SBU sales with their natural log as the outcome variable. The last column in Table 4 reports the results, and the percentage effects from this specification are very similar to the model that has SBU sales as the outcome (column 4 vs. column 5).

Finally, we estimate the "treatment effects" for the posttreatment period (Months 25–36), when ABIs were removed. If the "synthetic control" were adequately proxying the treated SBU's "no treatment" counterfactual, then we should find minimal effects (perhaps due to carryover) or no effects (if ABI removal brings the sales back to status quo). Overall, we find no consistent effect after the treatment removal. For the 12 months of the posttreatment period, we find seven periods for which the effect is negative and five months for which the treatment is positive. Furthermore, of the 12 months, only four months show statistically significant results: three of these are negative, and one is positive. See Figure B3 in the Part B of the Web Appendix for visual depiction of posttreatment effects and Table B1 for the posttreatment effects and associated *p*-values.

Table 5 provides a summary of the overall ATEs of ABI incentives (labeled "ABI"), the overall effect of INTV1 (labeled "SSABI"), the overall effect of INTV 2 (labeled "SABI"), and the overall effect during posttreatment (labeled "POST"). The table also constructs the 95% confidence

Table 5. ATEs on Sales and Confidence Intervals.

	Mean Effect with 95% CIs	%Effect
ABI	1.565 (1.056, 1.971)	7.56%
SSABI	1.560711 (1.044, 1.926)	7.81%
SABI	1.573829 (.820, 2.264)	7.07%
POST	−.57232 (−1.278, .126)	−2.39%

Notes: The confidence intervals (CIs) in parentheses are estimated using the subsampling approach (Li 2020).

interval around these effects (using a subsampling procedure described subsequently) and displays the effects in percentages. Overall, ABIs had an estimated average effect of 7.56%, while the SSABI (INTV1) had an estimated average impact of about 7.81%, and the average effect under the SABI (INTV2) was about 7.07%.

Confidence Intervals for Aggregate Treatment Effects

We adopted the placebo-driven procedure in generating the p-values reported in Table 4 for the treatment estimates for each period. However, Hahn and Shi (2017) and others have shown that the validity of these placebo tests rely on strong normality assumption on the distribution of idiosyncratic error terms and can often be distorted. To test the robustness of the statistical significance of our estimated treatment effects obtained using SCM, we rely on recent work by Li (2020) that develops a rigorous inference theory for synthetic control estimators using projection techniques. Li's theoretically validated approach uses a subsampling procedure to obtain confidence intervals. We used different subsample sizes (denoted by m) of the pretreatment period (m = 6, 8, 10, and 12) and, for each value of m, conduct 1,000 subsampling simulations to generate the subsampling-bootstrap statistic proposed by Li.[11] For each m, these statistics are then sorted to obtain $\alpha/2$ and $(1 - \alpha/2)$ percentiles for different significance levels (α) to estimate 75%, 80%, 90%, 95%, and 99% confidence intervals, respectively.

Employing this procedure and using m = 8 for illustration, the 95% confidence interval for overall ATE for the ABI intervention is (1.0555, 1.9714). The confidence intervals for INTV1 and INTV2 are (1.0435, 1.9261) and (.8200, 2.2637), respectively. Notice that none of the three confidence intervals contain 0, suggesting that the effect is positive and significant at the conventional 5% level. We also obtain ATEs for each treatment period, and Figure 2 shows the 95% confidence intervals for the estimated SCM ATEs. Figure 2 illustrates that none of the confidence intervals contain zero.[12] Taken together, both the placebo-based and subsampling-based inference procedures provide convergent validity on the statistical inference of our estimates.

Territory-Level Effects

Note that our preliminary analysis benefited from the richness of territory-level data but suffered validity issues due to the lack of adequate control. Our SBU-based SCM analysis sought to overcome this by using untreated SBUs as weighted-controls. SCM yielded ATEs for ABIs using aggregate data for the treated SBU and mitigated issues relating to the lack of a control group(s) in our treatment-removal design. It, however, resulted in the loss of heterogeneity present in our detailed territory-level data because we summed the territory numbers to generate the total sales for the treated SBU. Next, we perform an analysis that uses the information at both levels: the disaggregated information available within the treated SBU territories and the aggregate information available within 22 untreated SBUs.

Specifically, we use a GSC procedure that allows multiple treatments (305 in total in our case) to obtain causal estimates of interest. The GSC approach allows for efficient implementation of SCM over multiple treated units (territories, in our case) while controlling for outcomes in the pretreatment period and time-varying unobservable factors (latent factors) that could potentially bias the estimates (Xu 2017). GSC marries the semiparametric approach of modeling unobserved time-varying effects within an interactive fixed effects model (Bai 2009) with the SCM procedure. The advantage of this method is that it allows for flexible time fixed effects across different territories—in the process generating a synthetic control specific to each territory within the treated SBU. It uses a latent factor approach and provides readily interpretable estimates of uncertainty in treatment effects. The number of latent factors is selected via an automated cross-validation scheme that makes the method easy to implement. The method has recently been used in marketing to answer questions such as the impact of newspaper paywalls (Pattabhiramaiah, Sriram, and Manchanda 2019) and medical industry payment disclosures (Guo, Sriram, and Manchanda 2020). Next, we describe the procedure.

Our data are a panel of sales for multiple treated territories and nontreated SBUs, each referred to as a unit. We index a cross-sectional unit by $k = 1, \ldots, K$. The total number of units is $K = K_{INTV} + K_{CO}$, where K_{INTV} and K_{CO} are the numbers of treated (equaling 305) and control (equaling 22) units, respectively. All units are observed for periods $t = 1, \ldots, T$. For territories in the treated SBU, we let T_{0K} denote the number of pretreatment periods. Focal SBU territories are all first exposed to the ABI treatment at time $T_{0K} + 1$ and subsequently observed for $T - T_{0K}$ periods. We represent

[11] See Li (2020) for details, and for the specifics of the statistic, Equations 22 and 23 in that article. In addition, Li's theory relies on large T1 (pretreatment period) and large T2 (treatment period) against fixed N (control units). However, her simulations suggest that the method also works reasonably well when N is larger than T2 and comparable to T1. Note that in our case, T1 = 15, T2 = 9, and N = 22 for constructing the overall intervention effect. We are grateful to Kathy Li for helpful discussions on the implementation of this subsampling procedure.

[12] Our results are robust to other values of m and larger sizes of subsampling simulations.

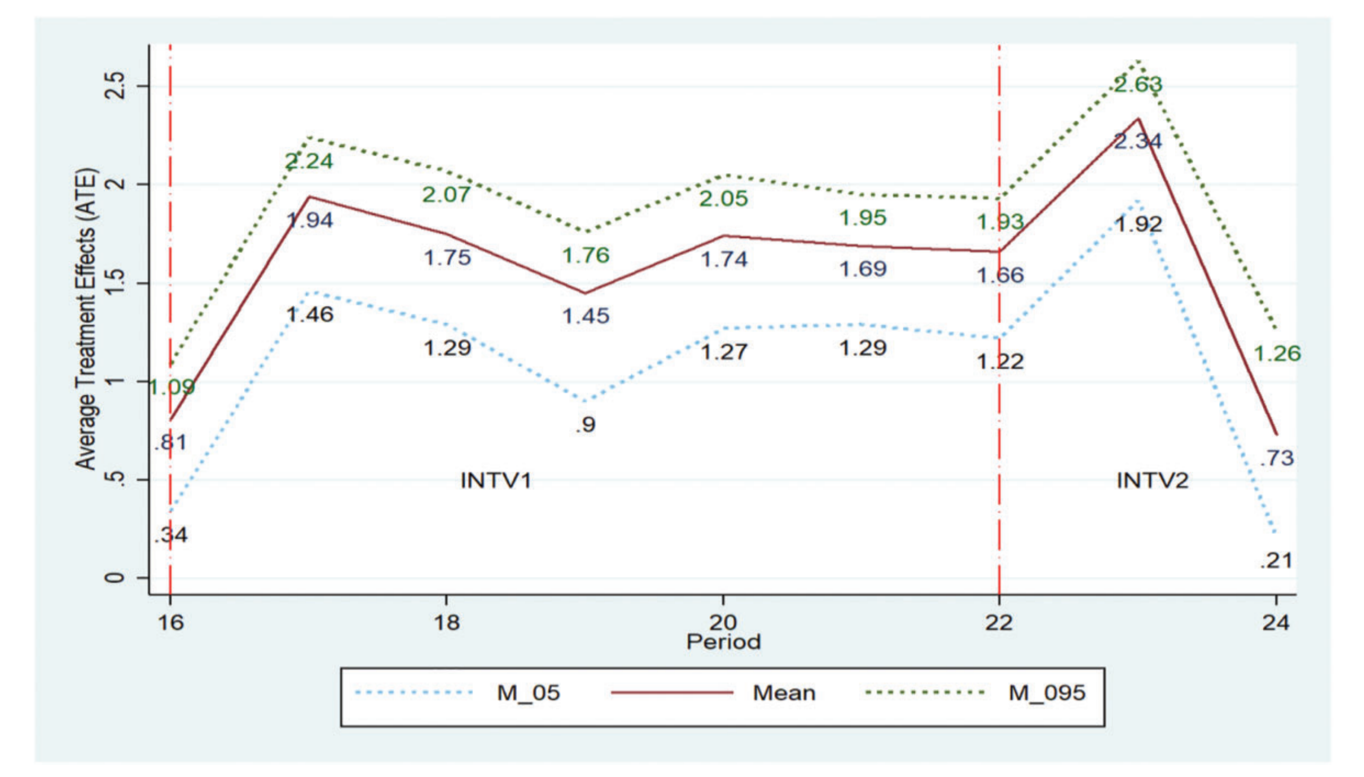

Figure 2. SBU-level ATEs.

Notes: INTV1 = "salesperson + supervisor ABIs" (SSABI); INTV2 = "supervisor-only ABIs" (SABI). The ATEs are in local currency in tens of millions. The point estimates of aggregate ATEs are shown along with 95% confidence interval.

Y_{kt} as the sales of a unit k at time t. Following the GSC method (Xu 2017), we assume that Y_{kt} is given by a latent factor model as follows:

$$Y_{kt} = \delta_{kt} D_{kt} + \lambda'_k f_t + \varepsilon_{kt} \tag{3}$$

The variable D_{kt} is the treatment indicator, which equals 1 for all treated units following ABI intervention. The parameter δ_k is the treatment effect on sales in the treated unit k. The vector $f_t = [f_{1t}, \ldots, f_{rt}]'$ consists of r unobserved common factors, while the vector $\lambda_k = [\lambda_{k1}, \ldots, \lambda_{kr}]'$ contains r unknown factor loadings. The factor model approach covers a wide range of unobserved heterogeneities and can accommodate two-way fixed effects for units and time periods.

We want to estimate the ATE on the treated units at t, $t > T_0$. The ATE at t, $t > T_0$ is given by the following:

$$\begin{aligned} ATE_{t,\, t>T_0} &= \frac{1}{K_{INTV}} \sum_{k \in K} \delta_{kt} \\ &= \frac{1}{K_{INTV}} \sum_{k \in K} [Y_{kt}(D_{kt} = 1) - Y_{kt}(D_{kt} = 0)], \end{aligned} \tag{4}$$

where K is the set of territories within the treated SBU. Please see Xu (2017) for the details of this estimator; we present a short summary of the estimation procedure in Part C of the Web Appendix.

GSC Results

We estimate the treatment effects using three different specifications of the GSC model: (1) imposing time fixed effects with the factor structure described previously, (2) imposing unit fixed effects with the factor structure, and (3) imposing two-way fixed effects (unit and time) with the factor structure. We consider Model 3 the most general specification to estimate the treatment effects. Unit fixed effects absorb all cross-sectional differences that are constant across time. Time fixed effects absorb common intertemporal changes across all units. The factors and factor loadings allow for flexible time fixed effects across cross-sectional units.

We start with the ATEs for the ABI intervention using the GSC procedure. The ATEs are available in Table 6. Across all the three specifications, three latent factors are produced, and the overall ATE is positive and statistically significant. The ATE using the specification M3, which includes two-way fixed effects, is .617 ($p < .05$). Using M3, we can also eyeball the average effects in each period in Figure 3. As the figure shows, the pretreatment effects are close to zero (the confidence interval of these effects always contains zero). In contrast, we observe a significant uptick in effects during the treatment period. Table 7 shows the ATEs by each period within the treatment. For all except one period, the ATEs are significant at a p-value of .1 or lower. The ATE in period 9 (last month of the treatment) is quite large (21.76%) but is also very imprecisely

Table 6. ATEs on Sales Using GSC Procedure.

Model	M1	M2	M3
ABI treatment estimate	.560	.647	.617
Unit fixed effects	No	Yes	Yes
Time fixed effects	Yes	No	Yes
Unobserved factors	3	3	3
Treated units	305	305	305
Control units	22	22	22
p-value of ATE	.044	.054	.038

Notes: Estimation of ATEs follows GSC procedure in Xu (2017). The *p*-values are constructed via placebo effects using 1,000 bootstraps. The treatment effects are in 100,000 of the local currency units.

Table 7. Treatment Effects by Period Using Territory Data.

Treatment Period	Effect	*p*-Value	%Effect
1 (Month 16): INTV1	.471	.052	9.72%
2 (Month 17): INTV1	.392	.028	7.50%
3 (Month 18): INTV1	.535	<.01	9.96%
4 (Month 19): INTV1	.264	.088	4.25%
5 (Month 20): INTV1	.549	.038	8.73%
6 (Month 21): INTV1	.539	.054	9.03%
7 (Month 22): INTV2	.872	.076	13.47%
8 (Month 23): INTV2	.867	.080	15.92%
9 (Month 24): INTV2	1.060	.798	21.46%

Notes: The reported numbers are for the specification M3 in Table 6, which includes three latent factors and two-way fixed effects. The *p*-values are constructed via placebo effects using 1,000 bootstraps. The treatment effects are in 100,000 of the local currency units. INTV1 = "salesperson + supervisor ABIs" (SSABI); INTV2 = "supervisor-only ABIs" (SABI).

estimated (see also Figure 3, and from Table 7, $p = .798$)—we cannot reject it as being different from zero (note that this effect was imprecisely estimated in the aggregated SCM as well). Overall, employing GSC, ABIs had an estimated average effect of 8.73%, while the SSABI (INTV1) had an estimated average impact of about 8.20%, and the average effect under the SABI (INTV2) was about 9.80%. These effects are slightly higher than the ATEs estimated under the aggregate SCM approach but are economically similar.

Individual Territory Effects via GSC

The key advantage of using GSC is its ability to include multiple treated units, in addition to allowing richer forms of heterogeneity. Because the factor loadings in GSC are specific to each territory, we can estimate the ATEs for an individual territory. As an illustration, Figure 4 plots the sales in four treatment territories against their synthetic counterfactual sales: two of these territories are single-person territories and the other two are multiperson territories. The treatment effects vary considerably across these territories: in T1, a single-person territory, the ATE (evaluated at the pretreatment mean) is large (54.31%) while the ATE in T3, another single-person territory, is relatively muted (10.82%). In contrast, the ATEs are significantly high within T2 (17.45%) and T4 (31.52%), both multiperson territories.

The estimated territory-level effects obtained via GSC allow us to study in a disciplined manner how the noisiness within

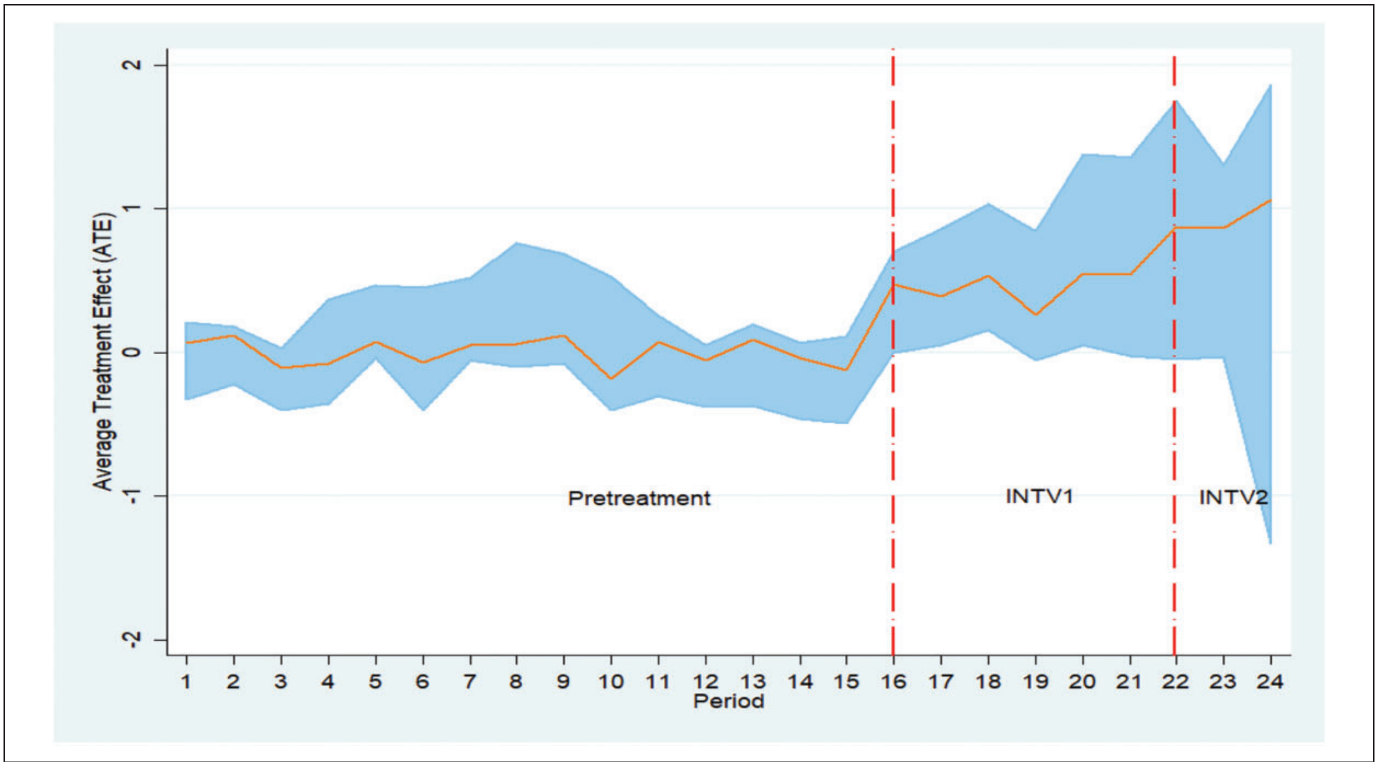

Figure 3. The ATEs using GSC.

Notes: The shaded area represents 95% confidence interval obtained using a parametric bootstrap procedure (Xu 2017). INTV1 = "salesperson + supervisor ABIs" (SSABI); INTV2 = "supervisor-only ABIs" (SABI).

Figure 4. Illustrative territories with actual sales versus synthetic counterfactuals.
Notes: The sales numbers are in 100,000 of the local currency units.

territories might impact the relative impacts of ABIs. Recall that our theory predicts differential effects of ABIs in noisier (larger) territories versus less noisy (smaller) territories. Similar to the preliminary analysis, we now investigate how the impact of ABIs might be different in single-person territories (low noise) versus multiperson territories (high noise). We took a simple approach of creating k-density plots of the ATEs across single-person and multiperson territories. Specifically, we plot the % ATEs across these two types of territories. As Figure 5 shows, the average percentage improvement in sales attributable to ABIs is 7.38% and 13.97% for single-person and multiperson territories, respectively. The significantly larger intervention effect for larger territories provides evidence in favor of our hypothesized relationship between noisiness and the impact of ABIs. Note that this evidence is significantly more credible than the evidence reported in Table 3 (using interaction effects) because we now combine the territory analysis with the data from 22 control SBUs, allowing for a flexible synthetic control for each territory using the GSC approach. Consistent with the theory, we find evidence that in larger territories, ABIs are significantly more impactful than in single-person territories. However, we note that, on average, ABIs have an economically meaningful impact across territories of all sizes.

Our empirical results conclusively suggest that both SSABI and SABI plans improve sales over the status quo. We now dig into more specific and practical questions related to these specific interventions. Do ABIs improve profits? Is one approach preferable over the other? Answers to these managerial questions require detailed cost data from the focal firm, as well as incentive payments made to the salespeople and supervisors during the baseline and the treatment periods. Although we lack salesperson-level cost and payout information, we were able to obtain access to the SBU-level cost data enabling us to perform rudimentary GPI calculations.

GPI Calculations

Production and administrative costs at the treated SBU are approximately 25% of revenue. In comparison, marketing costs of managing, administering, and sales-related activities (including a small fraction spent on physician conferences and gifts) constitute about 40% of the revenue generated. Of these marketing costs, about 40% went into the variable payment to salespeople and managers. Thus, we can make a working assumption that about 16% of the total revenue is the variable pay for incentives.

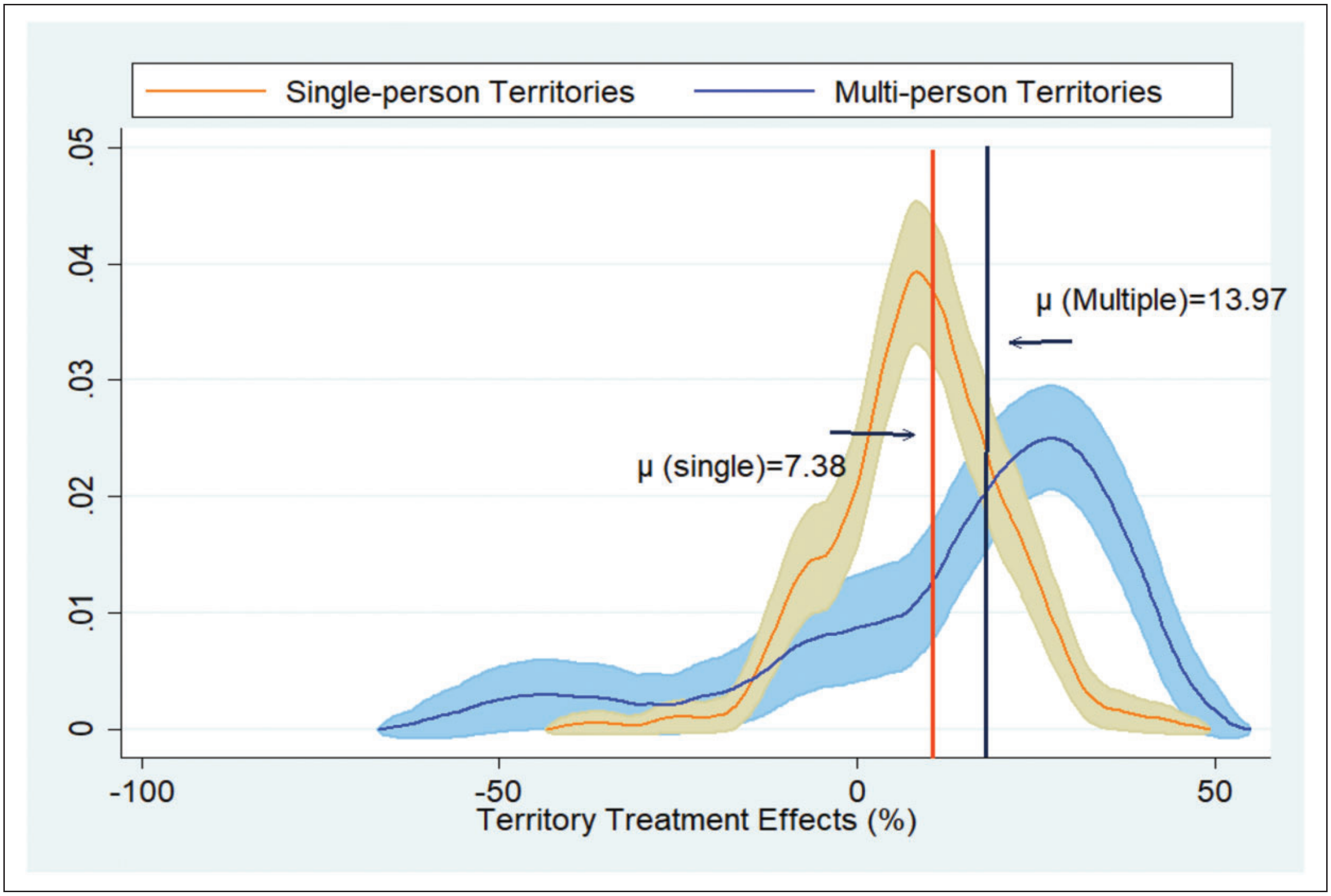

Figure 5. ATEs (%) across territories.
Notes: The shaded areas represent 95% confidence intervals of the estimates

ABIs increased payments to both salespeople and supervisors. The monthly ABI costs ranged between 3%–4% of marketing costs during the treatment period, and we use the conservative figure of 4% for ensuing calculations. In other words, about 1.6% of revenue was the additional ABI payment. Finally, to conduct the GPI analysis, we also need to decide the split of ABIs between salespeople and supervisors. For SSABI, we calculated this on the basis of the following information made available to us. There are 412 salespeople and 71 supervisors, and the ABIs are about 40% higher for a supervisor compared with a salesperson. Furthermore, 84% of the salespeople reached the targets, while 75% of supervisors reached their target under SSABI. Using these numbers, we are able to create the ABI split: 82.3% of total ABIs were awarded to salespeople, and 17.7% went to supervisors.[13]

For SABI, we were provided the information that 78% of the supervisors reached the targets, and thus we calculated that 18.4% of the ABI budget goes toward payment of ABIs for supervisors, while salespeople did not get any ABIs (see Part D in the Web Appendix for detailed worksheets of these calculations). An additional complication is that when ABIs increase sales, they increase the extant bonus payouts under output-based incentives as well. We make a conservative assumption that any incremental sales gain under ABIs results in a 16% non-ABI payment for the firm. This estimate is likely on the higher side because the bonuses are capped—thus, our analysis provides conservative estimates of ABI effects.

Armed with these assumptions, and SSABI and SABI estimates of 7.81% and 7.07%, respectively (from SCM estimates in Table 5), we now create a baseline scenario based on sales of 10,000 in local currency. Our baseline gross profits are 3,500, which increases to 3,788 under SSABIs, and 3,885 under SABIs.[14] These represent gross profit improvements of 8.22% and 11.02%, respectively, over the non-ABI regime.

[13] While not everyone met their targets, we make a conservative estimate that 1.6% of revenue went to ABIs, and the split is for this total amount.

[14] To see how we arrived at this number, consider SSABI. The baseline sales are 10,000, so under SSABI, using the 7.81% ABI effect, the sales are 10,781. The production costs are $.25 \times 10{,}781$, while the nonvariable marketing costs remain at 2400. The variable pay (non-ABI) accounts for $.16 \times 10{,}781$, and ABI costs are $.016 \times 10{,}781$. Thus, the gross margin is $10{,}781 - .25 \times 10{,}781 - .16 \times 10{,}781 - .016 \times 10{,}781 - 2{,}400 \approx 3{,}788$. The complete details are in part D of the Web Appendix.

Comparing SABIs to SSABIs, sales under SABI are slightly lower but are 34% more effective in terms of retained margins than ABIs across the hierarchy (see part D in Web Appendix). We feel that these are the lower bounds of the gains due to our conservative cost estimates and thus provide a clear managerial lesson to employ the ABIs and to do so mostly at the level of frontline managers.[15]

However, a note of caution is in order for our GPI analysis. To perform a complete return-on-investment analysis on the merits of SABI against SSABI, one needs to fully account for "behavioral control costs" incurred by supervisors from the increase in monitoring of salespeople during the period we implemented ABIs. These costs could include explicit monetary costs incurred from additional travel and time spent in the field with salespeople. To the best of our knowledge, in our context, the treated SBU did not consider these additional costs to be a significant factor once ABIs were incorporated. However, we caution readers to the possibility that our results could change in contexts where behavioral control costs are significant.

Implications and Conclusions

Call reports are ubiquitous in the sales force settings. Despite the reported use of activity measures from call reports to fashion incentive plans and theory-based conclusions about the value of including all available signals of salesperson efforts in incentive plans, there is a distinct lack of evidence of ABI effects. Our principal goal in this study was to provide such evidence.

Our large-scale three-year intervention at a single SBU of a large pharmaceutical firm yields some robust conclusions. It should be noted our field intervention was not a conventional field experiment with randomly chosen treatment and control groups. However, a long time-series data at the territory-level using treatment-removal design enabled us to obtain preliminary evidence on the efficacy of ABIs. We supplemented this analysis with data from other SBUs to construct synthetic counterfactuals of the treatment SBUs, effectively creating a quasi-experimental setup. Adding a modest level of ABI pay on top of existing sales-based incentives increases sales productivity by about 7%–9% (depending on the analysis procedure). In line with our theoretical conjecture, the effect increases as sales signals get noisier.

This additional pay appears to work in two ways. First, it intensifies output control at the level of the recipient, thus raising output when salespeople and their supervisors are both paid on these activity metrics. Second, it intensifies behavior control exerted by the supervisor. Therefore, in our study, when only supervisors were paid ABIs, their subordinate salespeople's activity scores and output increased. In fact, the increase in sales from incentivizing supervisors only is slightly lower than the increase when both salespeople and supervisors were paid ABIs.[16]

What explains this finding? In many contexts, selling involves multiple activities. This is certainly true within our empirical context. The salespeople market their products to doctors to increase prescriptions and promote their products to retailers and stockists to incentivize them to stock and push these products through their channels. The optimal amount of time salespeople allocate toward different activities is key to sales efficacy. Absent ABIs, supervisors do not have much authority in the activity allocation for salespeople; however, with the implementation of ABIs, they direct salespeople more intensively in their activity planning. Supervisors' input in salespeople's activity planning is valuable because they have extensive field experience—they typically start their careers as frontline salespeople and rise through the ranks to manage multiple territories (median time for a salesperson to become a supervisor is seven years). We surmise that this experience could be the source of their better understanding of the activity–output link, and thus they serve as effective mentors in salespeople's activity planning. Supervisors' better understanding of sales outcomes seems consistent with persuasive empirical evidence that has documented that market experience mitigates and sometimes eliminates biases (List 2003). Thus, when ABIs are in place, supervisors are the right channel to advise and coordinate these activities for salespeople. When SSABI is in place, this could result in an outcome where supervisors and salespeople might have different ideas on activity focus. So, this intervention is unlikely to result in significantly higher gain than what one gets under SABI. We hasten to add that we do not have detailed data to test our proposed conjecture, and future research should test this in a more disciplined manner.

Because providing these incentives only to the supervisors resulted in significantly lower incentive costs than the provision of ABIs at both levels, our analysis carries a distinct managerial recommendation: the frontline supervisors should be the primary locus of these plans, given that they exert behavior control over salespeople. This finding brings together two strands of the literature that have been disconnected. The long-standing sales force control literature (e.g., Cravens

[15] This raises an obvious question: If the ABIs were profitable, why did the treated SBU not continue with these? We want to clarify that this was a pilot program within the focal SBU that was *designed* to last for nine months. Based on our findings, the management of the parent company was to decide on implementing it at the focal SBU and other (control) SBUs. Applying it across the entire organization was especially crucial because the incentives are centralized and relatively homogeneous across the SBUs, and as such the company does not allow significantly different incentives across SBUs. ABIs were one of the many incentive changes that the company was planning to explore in the subsequent periods, including experimenting with sales contests and nonmonetary incentives. Our latest conversation with the cooperating SBU indicated that the firm was planning to introduce ABIs as a regular part of their incentives (on a small scale) throughout all their major SBUs, partly based on the success of our pilot program.

[16] Note that the effect is actually slightly higher under GSC. So, our GPI analysis is the most conservative analysis highlighting the comparative advantage of SABI against SSABI.

et al. 1993; Oliver and Anderson 1994; Phillips 1982) connects supervisors directly to salespeople pay plans through behavior control. This connection has been abstracted away in the principal-agent work that is the contemporary workhorse model of incentive pay, which emphasizes the own-pay incentive effects, ignoring the cross-level behavior effects seen in our data. Deeper theoretical integration of hierarchy effects and own-incentive effects is warranted.

The utility of our study's conclusions for managerial practice is based on its generalizability. Typically, field studies offer more external validity than laboratory work, at least specific to the real-world settings in which they are conducted because they rely on the naturally occurring behavior of participants (Gneezy 2017). However, Lynch's (1999) classic assertion that field studies offer no greater external validity beyond their specific context has a particular bearing on our intervention as it was performed within a single SBU. Thus, we are careful not to suggest the generalizability of our study to contexts that are very different from our research setting. Moreover, the intervention conducted in a naturally occurring setting comes with a cost: because we are limited to collecting observed outcomes such as sales and activity scores, we are unable to explore the theoretical mechanisms more deeply. This is more easily achieved through lab studies where the experimenters have much greater control over design and data collection (Gneezy 2017).

Our setting consists of an outbound sales force whose principal task is to drive sales volume. These salespeople do not have pricing authority, and their portfolio of tasks is well-defined. Furthermore, their existing sales-based incentive plan already puts a considerable amount of their income at risk (about 35%–40%). These features describe a large number of commercial and industrial sales forces, so our takeaway that adding a modest amount of activity-based incentives improves the sales applies in a straightforward way to many firms. ABIs are a valuable adjunct to noisy sales performance signals. In contrast, the same logic suggests that it is unlikely to be useful to extend such incentives to call-center salespeople working from scripts with real-time monitoring by supervisors. Given the ability to observe activity directly, it is very likely that a firm is better off paying flat wages to these inside salespeople.

Finally, a reasonable question to ask is why ABIs are not as prevalent if the effects are as strong as they were found in this study. We wish to point out here that while most firms do not incentivize activities, activity-based inputs are ubiquitous and routinely collected by firms through call reports. Often, these call reports are used to enforce a minimum requirement on effort or to monitor salespeople activities. As mentioned previously, an industry survey indicated that 15% of the firms surveyed incorporated activity signals in their incentive plans (e.g., Zoltners, Sinha, and Lorimer 2006).[17] The economic impact of using ABIs is less well known, and our article, therefore, provides much-needed clarity on this. Our contribution also lies in outlining the conditions under which such incentives might be more effective. We show that the impact of ABIs varies with the level of difficulty in measuring an individual agent's contribution to the output. Empirically, this translates into higher effectiveness of such incentive plans in larger territories wherein there are coordination and free-riding concerns. Further, we also show that the incentive alignment of frontline supervisors is an important determinant of the success of ABI plans. We now proceed to highlight some of the limitations of our study and some suggestions for future research.

Limitations

Like any large field intervention, our research is high in both internal validity and external validity. However, because it is situated within a single organization, there are some concerns with its generalizability to other industries and firms. While call reports are common in many firms, it is not hard to imagine industries where an alternate set of activities could be a more effective instrument for ABIs. For example, in areas like enterprise software sales, metrics such as the number of sales proposals, quotations, appointments set up with prospects, and so on might be more effective. In industry sectors with long lead times (e.g., large industrial systems), call report–based measures might not be appropriate for incentive design, and other long-term leading indicators of sales might be more appropriate. As such, we believe that more research is needed to determine whether our insights on supervisory control apply to these settings.

Another potential issue is that our study featured a "treatment-removal" design, and previous literature (Chung and Narayandas 2017; Lepper, Greene, and Nisbett 1973) has documented that the removal of extrinsic motivation (monetary incentives) could subsequently reduce the intrinsic motivations from a task, reducing the output below the baseline. This could be a concern in many contexts. However, we did not see strong evidence that suggests a drop in the motivation (see the confidence interval of POST estimate in Table 5). We conjecture that because ABIs are a relatively small fraction of take-home pay (relative to sales commissions), their removal as such did not significantly reduced monetary incentives, and thus we did not observe a reduction in intrinsic motivation. Further research is required to understand the circumstances in which these occur.

We close on a cautionary note. Despite our robustness checks and assessment of various threats to validity, the absence of a randomized control group in a classic sense could weaken our causal claims. Going forward, we feel that more detailed cost and compensation data for profitability analysis would enhance the managerial implications of our work. In addition, given that we kept the output-based performance pay constant in our intervention, we are not able to speak to the relative efficacy of ABIs compared with sales bonuses. Finally, a structural model specifying the mechanisms at hand would

[17] In the industry segment of the market where our study is situated, activities provide input for incentives for about 25% of the firms.

enable us to extrapolate to counterfactual scenarios. Hopefully, future work can close these gaps.

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