Conduit Incentives: Eliciting Cooperation from Workers Outside of Managers' Control

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Abstract:
Can managers use monetary incentives to elicit cooperation from workers they cannot reward for their efforts? I study "conduit incentives," an innovative incentive design, whereby managers influence bonus-*ineligible* workers' effort by offering bonus-*eligible* employees a monetary reward for performance that critically depends on the cooperation of the bonus-ineligible workers. Motivated by the reward, bonus-eligible employees use social motivators to elicit cooperation from their ineligible colleagues. I examine an intervention in a California hospital in which a one-time bonus program aimed to improve handwashing compliance. State regulation prevented physicians from receiving bonus payments. However, because physicians' handwashing counted toward the bonus-related goal, bonus-eligible workers used social pressure to incentivize physicians' performance, absent any tangible benefits for the physicians. The physicians improved performance during the intervention, and their improvements persisted beyond the removal of the incentives. The response to the temporary intervention significantly predicted the persistence of the performance improvements.

Keywords: Conduit incentives; Social pressure; Image motivation; Prosocial behavior; Persistence of performance improvements; Healthcare; Handwashing.

JEL Classifications: I12, M4, M12, M14, M52

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I. INTRODUCTION

Can managers leverage a monetary bonus to elicit effort from workers who cannot receive it? When complex organizational processes require cooperation from workers outside the manager's span of control, the lack of direct authority over these workers impedes traditional incentives. In this study, I examine a particular incentive design whereby monetary rewards motivate bonus-*eligible* employees to achieve a performance target and pressure bonus-*ineligible* workers to help achieve the same target. I label this design "conduit incentives" because it aims to elicit bonus-ineligible workers' effort *through* a monetary incentive for their bonus-eligible colleagues. This design entails two contracts. The first one (hereafter financial contract) is an explicit contract between the manager and the workers she can control and reward directly (i.e., bonus-eligible employees). This contract exchanges a monetary reward for performance. However, because the rewarded performance depends on the collaboration of workers outside the manager's control (i.e., bonus-ineligible workers), the bonus-eligible employees establish a second contract (hereafter nonfinancial contract) with their bonus-ineligible colleagues. In this second contract, which is implicit in nature, the bonus-ineligible workers earn social rewards (punishments), such as recognition and appreciation (ostracism and denigration), for contributing (failing to contribute) effort to the performance for which bonus-eligible workers will be rewarded financially. I examine two research questions. Do conduit incentives elicit effort from bonus-ineligible workers *while* the incentive program is in place? Do any performance changes among bonus-ineligible workers persist *beyond* the removal of the incentives?

Understanding whether and under what conditions conduit incentives work can help managers deliver performance even when it critically depends on contributions they cannot control or incentivize directly. In a conduit incentives system, the nonmonetary exchange between bonus-eligible and bonus-ineligible workers constitutes a deliberate spillover from the bonus program. By motivating bonus-eligible employees to elicit bonus-ineligible colleagues' help to reach the performance goal, conduit incentives essentially introduce a public good to which bonus-ineligible workers are asked to contribute. Research shows that image-related motivators, such as status or self-esteem, encourage individual contributions to public goods

(Bénabou and Tirole 2006; Ariely, Bracha, and Meier 2009) and are particularly effective when appreciation is public (Andreoni 1990; Ariely et al. 2009; Lacetera and Macis 2010; Ashraf, Bandiera, and Jack 2014) or when effort is visible to others (Ozbay and Ozbay 2014). However, for conduit incentives to work, the monetary incentive must first motivate bonus-eligible workers not only to improve their own performance but also to socially induce bonus-ineligible workers to contribute to the public good. Additionally, any social rewards (punishments) must be valuable for the bonus-ineligible workers, and their cost of contribution to overall performance must be smaller than the utility (disutility) arising from the social reward (punishment). Therefore, whether conduit incentives elicit bonus-ineligible workers' cooperation is an empirical question.

I obtained field data from a natural experiment in a California hospital (hereafter indicated by the pseudonym JRH). In 2015, JRH's CEO implemented a temporary intervention to improve performance on a selected set of metrics, including one for compliance with hand hygiene protocols (hereafter: handwashing).[1] Consistent with industry practice, JRH measured handwashing at the hospital level by aggregating the performance of all clinical (physicians, nurses, etc.) and nonclinical staff.[2] In this setting, physicians were bonus-ineligible due to regulatory and contractual reasons.[3] The CEO established a one-time bonus, payable if hospital-level performance (which included the performance of both bonus-eligible and bonus-ineligible employees) met a predetermined target within three months.[4] If the target was met, all bonus-eligible employees would receive the same lump sum (about $1,200), regardless of individual performance. If the targets were not met, no payment would be made. During the intervention, all workers (bonus-eligible and bonus-ineligible alike) received bi-weekly aggregate feedback on the progress toward

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[1] As I describe in Section III, the intervention also included targets for other metrics.

[2] This practice is influenced by mandatory reporting requirements, whereby hospitals participating to government-led programs (e.g., the Value-Based Purchasing program managed by the Centers of Medicare and Medicaid) must report organization-wide handwashing compliance on a quarterly basis. For information about Value-Based Purchasing, see https://www.cms.gov/Medicare/Quality-Initiatives-Patient-Assessment-Instruments/Value-Based-Programs/HVBP/Hospital-Value-Based-Purchasing.html. Accessed January 5, 2022.

[3] See Section III for details.

[4] Hand washing is not an activity for which hospital workers are generally intrinsically motivated. In fact, there is a large literature documenting a wide selection of attempts to improve hand hygiene in healthcare provider organizations worldwide.

the collective goal. All preexisting protocols, monitoring procedures, performance reporting and communication, and other compensation practices remained unchanged throughout the sample period, which extended from one quarter before the intervention to two quarters afterward (i.e., one entire fiscal year).

Several characteristics make this setting attractive for studying conduit incentives. First, achieving the collective target depended critically on bonus-ineligible workers' cooperation. Bonus-eligible workers already performed at high levels before the intervention and knew that physicians' performance had been consistently worse than theirs,[5] thus offering a promising opportunity to raise hospital performance. Bonus-ineligible workers, for their part, knew that their collaboration would help bonus-eligible employees earn monetary bonuses but that they would not benefit financially. Second, the pressure to perform came from workers who had lower status, as physicians generally enjoy greater authority in a hospital than the rest of the clinical staff (McKay and Narasimhan 2012; Kerr 1986; Tellis-Nayak and Tellis-Nayak 1984). This biases against the effectiveness of the conduit incentives because bonus-eligible workers' threats to withdraw cooperation in future exchanges would likely have low credibility. Third, because of the nature of contractual relations between the hospital and the PO (see Section III), physicians had no reason to believe that their improved handwashing performance would impact their professional prospects. This reduces the concern about confounding effects arising from implicit incentives. Fourth, the intervention was implemented and communicated as a one-time bonus program, which prevented any expectation of future rewards and, at the same time, allowed me to study the persistence of the performance improvement.

I find that, on average, bonus-ineligible workers at JRH improved their handwashing performance during the intervention period, consistent with conduit incentives eliciting their cooperation. The performance improvements also persisted beyond the program period, suggesting that conduit incentives can lead to enduring behavioral changes, consistent with social norms arising from nonmonetary motivators

[5] At no point, before, during, or after the intervention, did management disclose individual-level performance information. Bonus-eligible employees' beliefs about physicians' lower hand hygiene performance were entirely based on their day-to-day observation of physicians' behaviors (see Section III).

and forming/reinforcing the bonus-ineligible workers' prosocial identity (Gneezy et al. 2012). These results are robust to accounting for the variation in the baseline level of performance, which correlates with the available room for improvement during the intervention. Exploratory analyses show a systematic relation between individual responses to the bonus program during the intervention period and the persistence of the behavioral modifications afterward. This finding has important practical implications. For example, managers using temporary incentives to establish permanent behaviors might use the short-term reaction to an intervention as an early signal of its endurance, allowing for timely remedies if needed. Finally, conduit incentives were less effective in the hospital's sterile environments, where handwashing compliance required greater individual effort (i.e., greater cost). This evidence suggests that the cost of bonus-ineligible workers' cooperation can limit the effectiveness of conduit incentives. My results withstand a series of robustness tests, including alternative estimation methods and alternative definitions of variables of interest (see Appendix 2).

This study contributes to the literature on the interplay between monetary and nonmonetary incentives to drive collective or interdependent performance. Specifically, I provide novel insights into how managers can use monetary incentives to motivate subordinates not only to improve their performance but also to pressure workers who will not benefit from an incentive program to do so as well. While prior research has examined how monetary incentives for collective or interdependent performance give rise to non-monetary motivators among workers (e.g., Che and Yoo 2001; Rowe 2004; Loughry and Tosi 2008; Sedatole, Swaney, and Woods 2016), these mechanisms aimed to reduce the free-riding problem – i.e., workers benefiting from the rewards associated with collective performance without exerting corresponding effort. Conduit incentives, in contrast, examine the *opposite* problem – i.e., eliciting effort from bonus-ineligible workers toward collective performance that will reward others financially. Additionally, I contribute to the academic discourse about informal collaboration and prosocial behaviors by showing how managers can improve collective performance by harnessing workers' altruism. Finally, this study has significant practical implications. The need to influence workers who may not obtain tangible rewards is not limited to hospitals. Conduit incentives are likely to be effective in many collegial settings, where bonus-eligible and bonus-ineligible workers interact while performing their tasks; where performance of bonus-eligible employees depends on contributions from others who are outside the span of control of the manager; and where, on average, bonus-eligible employees earn less than bonus-ineligible workers.

II. LITERATURE AND HYPOTHESES DEVELOPMENT

In most organizations, including healthcare providers, cooperation between workers across functions is critical for performance. However, cooperation is often difficult to motivate, measure, reward, and enforce (Abernethy, Hung, and van Lent 2020; Gibbons and Henderson 2012). Conduit incentives aim to elicit cooperation from workers who cannot be controlled or rewarded directly but whose contribution is critical to achieving a performance goal. Their structure involves two contracts. The financial contract – a formal and explicit contract – is between the manager and the bonus-eligible workers. The manager rewards these workers with a bonus for reaching a performance target. The nonfinancial contract – an implicit contract that is not enforceable in court – is between bonus-eligible and bonus-ineligible workers. Bonus-eligible employees assign social rewards (punishments) to bonus-ineligible workers for their contribution (lack of contribution) to the goal that will earn eligible employees their bonus. To the best of my knowledge, this is the first study to examine such an incentive structure.

I posit that the nonfinancial contract emerges as a spillover effect of the financial contract. Accounting researchers have yet to examine this type of spillover. Examples in the literature include performance spillovers between rewarded and unrewarded tasks performed in temporal proximity (Hecht, Tafkov, and Towry 2012), and the weighting of individual and collective performance metrics in bonus schemes to harness performance spillover effects between interdependent tasks and reduce the associated noise (Bouwens, Hofmann, and Van Lent 2018). In a conduit incentive structure, the spillover from an explicit monetary incentive contract between the manager and bonus-eligible employees is a second (nonfinancial) implicit contract between eligible and ineligible workers.

The two contracts are intertwined. Specifically, establishing the nonfinancial contract is contingent on several characteristics of the financial contract. First, the object of the financial contract must be a performance target that cannot be achieved without contributions of bonus-ineligible workers (i.e., bonus-eligible workers *need* the bonus-ineligible workers' cooperation). If bonus-eligible employees could reach the goal by themselves, they would not need to pressure their bonus-ineligible colleagues to help. Therefore, they would not need to establish a nonfinancial contract with their bonus-ineligible colleagues. Additionally, should the bonus-ineligible not perceive that bonus-eligible colleagues need their help to reach the goal, they would be less likely to contribute (Bekkers and Wiepking 2011).[6] Second, bonus-eligible and bonus-ineligible workers must believe the bonus-ineligible workers *can* help the bonus-eligible ones reach their goal (i.e., efficacy condition – see Bekkers and Wiepking 2011). That is, the collective performance goal in the financial contract must be attainable, and the contribution required of the bonus-ineligible workers must be within their power. Third, the bonus offered in the financial contract needs to have enough power to motivate bonus-eligible workers to exert effort not only to improve their performance but also to apply pressure on the bonus-ineligible workers, which is a costly activity (Barron and Paulson-Gjerde 1997). Institutional context further influences the establishment of the nonfinancial contract. First, there must not be a direct way for the manager to reward or control the bonus-ineligible workers, or she could do so directly. Second, bonus-eligible employees must represent a relevant reference group for the ineligible workers (Bandura 1986). That is, bonus-ineligible workers must care about their reputation and status in the eyes of the bonus-eligible employees. Third, bonus-eligible employees must believe that the ineligible workers will respond to the social pressure (Ariely et al. 2009). Fourth, the cost of cooperation incurred by bonus-ineligible workers must be smaller than the utility (disutility) they obtain from social rewards (punishments).

The idea of monetary incentives engendering nonmonetary ones is not entirely new to the literature. For example, research has found that linking monetary rewards to collective performance measures can give rise to informal social controls, such as horizontal monitoring and peer pressure (Che and Yoo 2001;

[6] Bonus-eligible employees may choose to pressure bonus-ineligible workers also when applying social pressures is less costly than increasing effort to reach the target. However, in this case, the nonfinancial contract may fail. Research shows that the donor's perception of recipients' "deservingness" is a critical factor in their decision to contribute to a public good (see, for example, Sorensen, Cappelen, Hole, and Tungodden (2007); Fong (2007); Aaroe and Petersen (2014)). Thus, should the bonus-ineligible workers perceive that the bonus-eligible employees are not exerting effort to reach the target, their willingness to help may be severely reduced.

Rowe 2004, Loughry and Tosi 2008). Workers rely on these mechanisms also when their rewards are based on individual tasks but these tasks are interdependent (Wageman and Baker 1997; Sedatole et al. 2016). Thus, even when costly, cooperation leads to better outcomes and greater payoffs for the team members (Abernethy et al. 2020). In all these studies, however, workers invoke social controls to address a free-riding problem—that is, the possibility that other workers can enjoy the tangible benefits of collective performance without exerting proportional effort (Alchian and Demsetz 1972; Holmstrom 1982; Lazear and Shaw 2007; Bushman, Indjejikian, and Smith 1995; Wageman and Baker 1997; Scott and Tiessen 1999; Che and Yoo 2001; Rowe 2004). In contrast, conduit incentives target the opposite problem—obtaining effort from unrewarded workers toward collective performance that will reward others.

This behavior is analogous to contributing to a public good. In many cases, individuals contribute to public goods from which they will not directly benefit (e.g., charitable donations, volunteering, etc.). Drivers of contributions to public goods include intrinsic motivation—that is, finding the very nature of the activity intrinsically rewarding (Deci 1971; Frey and Jegen 2001). Alternatively, workers may respond to reputational incentives and expend costly effort to benefit others because social norms attribute value (i.e., honor, recognition) to that effort (Bénabou and Tirole 2006; Ariely et al. 2009). In this case, what motivates the contribution is the desire to be liked and respected by the members of a reference group (i.e., social esteem) and themselves (i.e., self-esteem). Concerns about one's social image motivate contributions to public goods, especially when praise or criticism is visible to others (Andreoni 1990; Ariely et al. 2009; Lacetera and Macis 2010; Ashraf et al. 2014) and when others can observe the contribution of effort (Ozbay and Ozbay 2014). Thus, by asking bonus-ineligible workers to help them earn their bonus, bonus-eligible employees position the bonus-ineligible workers as contributing to a public good, and they incentivize the contribution using social and self-esteem.

Therefore, managers can reach the bonus-ineligible workers outside their span of control *through* a monetary incentive motivating bonus-eligible controllable workers. The following hypothesis summarizes a directional prediction about the effectiveness of conduit incentives.

H1: *Conduit incentives motivate bonus-ineligible workers to improve performance to contribute to goals rewarding bonus-eligible workers financially*.

A question arises as to whether the effects of conduit incentives on bonus-ineligible workers' behaviors persist beyond the removal of the incentive. Many organizations use temporary incentives to boost performance, hoping that the improvements continue beyond the intervention. While extensive research has studied the effectiveness of different types of incentives *while* these are operating, few studies have explored the persistence of the desired behaviors after incentive removal. Whether the performance improvements introduced or reinforced via conduit incentives persist their removal is an empirical question.

Research shows that, in transactional exchanges of actions for money, removing the prospect of the reward also eliminates the expectation of observing the desired behaviors (Greene and Podsakoff 1978; Kohn 1993; Hamman, Rick, and Weber 2007; Gneezy, Meier, and Rey-Biel 2011). Therefore, upon removing the monetary incentive, bonus-eligible workers' motivation to pressure the bonus-ineligible workers will likely disappear. Since the nonfinancial contract is contingent on the financial one, removing the monetary incentive also removes the public good to which bonus-ineligible workers are asked to contribute. Therefore, removing the monetary incentive and the ensuing social motivators could lead bonus-ineligible workers to return to pre-intervention performance levels.

In contrast, bonus-ineligible workers' performance improvements introduced via temporary conduit incentives may persist beyond the removal of the social pressures. When bonus-eligible employees use social motivators to reinforce ineligible workers' cooperation, they endorse certain behaviors and sanction others. Thus, they communicate information about *injunctive* norms (i.e., what one *ought* to do to be a member of the reference group) (Cialdini, Reno, and Kallagren 1990). If the incentivized behavior, the associated recognition, or both are widely visible, ineligible workers learn that other members of their reference group adopt the incentivized behavior, thus making the corresponding norm also a *descriptive* one (i.e., what the members of the reference group *typically* do) (Cialdini et al. 1990). When injunctive and descriptive norms reinforce each other, they affect behaviors (Bicchieri 2006; Thogersen 2008). Individuals strive to obtain social approval by repeating endorsed behaviors and avoiding those that are informally sanctioned (e.g., via gossip or shunning) (Bandura 1986; Kandel and Lazear 1992; Masclet, Noussair, Tucker, and Villeval 2003; Bénabou and Tirole 2006; Loughry and Tosi 2008). People infer their identity from their actions and "consult with their identity" before acting (Bénabou and Tirole 2011). Thus, performing behaviors consistent with the norms of the reference group—such as contributing to a public good—may develop or reinforce a prosocial identity for the ineligible workers, which, in turn, may lead to continuing those behaviors without the need for external reinforcement (Gneezy et al. 2012).[7] In sum, to the extent that social motivators lead bonus-ineligible workers to form or reinforce an identity that includes the behavior (i.e., performance improvement) originally targeted by conduit incentives, this behavior (improved performance) will continue beyond the removal of the monetary incentives and the eligible workers' social pressures.[8] This leads to my second hypothesis.

H2: *Conduit incentives are associated with performance improvements among the bonus-ineligible workers that persist beyond the incentive removal.*

III. RESEARCH SETTING

All JRH workers must disinfect their hands using soap and water or hand sanitizer every time they enter (*gel-in*) or exit (*gel-out*) a location where they might have physical contact with a patient.[9] A specialized

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[7] Ex-ante, the behaviors incentivized through social pressures need to be acceptable for the bonus-ineligible workers (i.e., they must not clash with the values and ethics by which bonus-ineligible workers operate) but need not be a high priority. Had the bonus-ineligible workers considered the incentivized behavior one with high importance before the intervention, the need for conduit incentives to entice their cooperation would likely be lower. However, descriptive and injunctive social norms manifested through social pressures attribute value to the incentivized behavior within the reference group. Consequently, conduit incentives lead bonus-ineligible workers to attribute greater importance to the incentivized behaviors. To the extent that the behavior becomes part of the bonus-ineligible worker's perception of their identity, it is likely to continue beyond the removal of the incentive.

[8] The persistence of behaviors elicited via temporary conduit incentives could also be explained by habit formation, especially when the incentivized behaviors require lower cognitive effort, like handwashing. I argue that the antecedents of habit formation are the same as those driving persistent behavioral changes in a conduit incentives context. The psychology literature explains habit formation using the theory of rational addiction (Becker and Murphy 1988), by which intentionally increased "consumption stock" of desired behaviors facilitates the formation of sustainable habits. However, there needs to be an *incentive* to build consumption stock (Becker and Murphy 1988; Verplanken and Wood 2006) and the behavior needs to *benefit* the individual performing it (e.g., Hussam, Rabbani, Reggiani, and Rigol 2022; Neal, Vujcic, Hernandez, and Wood 2015). In a conduit incentive setting, the social rewards offered by bonus-eligible workers *during* the intervention constitute the *incentive* to build the consumption stock, whereas the *benefit* for the bonus-ineligible worker lies in the pro-social image formation/reinforcement described earlier.

[9] Recommended hand hygiene best practices vary across healthcare governance organizations, including the World Health Organization (WHO) and, in the United States, the Centers for Disease Control (CDC) and the Joint

independent agency provides the hospital with secret shoppers who assess handwashing performance by covertly observing JRH personnel at random.[10] The secret shoppers disguise themselves as visitors, patients' family members, or professionals visiting the hospital on business. They are trained to assess correct handwashing and identify JRH personnel on sight, and they frequently rotate to prevent JRH staff from recognizing them.

Consistent with prior studies (see Haas and Larson (2007) for a review) and in line with the recommendations of the Joint Commission and the Centers for Medicare and Medicaid (CMS), JRH measures aggregated organization-level handwashing performance (*HHPerf_Org*) as follows.

$$HHPerf\_Org_t = \frac{\sum_i gel\_in_{i,t} + \sum_i gel\_out_{i,t}}{\sum_i gel\_in\_assess_{i,t} + \sum_i gel\_out\_assess_{i,t}}, \qquad (1)$$

where i represents the individual healthcare worker and t represents the quarter of assessment. *gel_in_assess* (*gel_out_assess*) is the number of assessments a secret shopper performs in the quarter. *gel_in* (*gel_out*) is the number of times secret shoppers observed workers performing *correct* handwashing upon entry (exit) into (out of) a location in which they may have physical contact with a patient. Aggregated handwashing performance is disclosed internally to all personnel through a quarterly scorecard.

In the United States, the importance of hand hygiene in hospitals and the consequences of poor compliance are stressed during training, frequently discussed within healthcare organizations, and reinforced by many academic and practitioner publications. Handwashing is a critical factor in preventing hospital-acquired infections (Haas and Larson 2007; Sax et al. 2009; Gould, Drey, Moralejo, Grimshaw, and Chudleigh 2008; Pessoa-Silva et al. 2007). These infections increase costs and degrade outcomes, as they correlate with complications, readmission rates, mortality, and risk of malpractice litigation (Hyman and Silver 2004; Guinan, McGuckin, Shubin, and Tighe 2005). However, despite the simplicity of the

Commission. The process adopted by JRH comports with the recommendations of the WHO and the Joint Commission.

[10] Direct observation by trained covert observers is recommended by the WHO as the most reliable method to assess hand hygiene compliance (Haas and Larson 2007). Advantages of this method include lower cost compared with solutions that involve greater intensity of technology and infrastructure (RFID, proximity sensors, etc.).

procedure and the consensus on its importance, compliance is often lacking (Pittet, Simon, Hugonnet, Pessoa-Silva, Sauvan, and Perneger 2004; World Health Organization 2009; The Joint Commission 2009). Known reasons include caregivers' busyness, incorrect risk self-assessments, and perceptions that hand hygiene protocols impede higher priorities (World Health Organization 2009). Consistent with industry averages, JRH's handwashing performance exhibited significant opportunities for improvement.

In 2015, the management team led by the CEO introduced a temporary incentive program to improve performance on a selected set of measures aligned with the hospital's mission to provide high-quality care. In addition to handwashing compliance, the program aimed to improve the quietness of the environment and the effectiveness in the communication about medications.[11] None of these metrics was new.[12] The assessment methodology also did not change. Quietness of the environment and communication about medication were evaluated based on randomized anonymous surveys of discharged patients. Secret shoppers' randomized assessments continued to be the data source for handwashing compliance measurement. Interviews with hospital management[13] confirmed that the secret shopper agency was not made aware of the intervention and it was not asked to make any changes to their assessment methodology or frequency.

Management set two target levels for each of the three metrics. For handwashing performance, the threshold goal was 92% compliance hospital-wide, and the stretch target was 95%.[14] Upon achieving the stretch targets within three months, each bonus-eligible employee would receive a one-time bonus of

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[11] JRH designed and implemented the intervention prior to the beginning of our collaboration. My statistical analyses are therefore based on archival data provided by the hospital.

[12] Regulatory provisions required JRH to collect all three metrics and report them to the Centers for Medicare and Medicaid (CMS) on a quarterly basis. These provisions date back as far as 2004.

[13] I conducted several informal field interviews at JRH to obtain insights and additional information about the program's implementation and the rationale that had pushed the CEO to introduce it. Interviewees included the CEO, the Director for Performance Improvement and Clinical Outcomes, the Manager of Clinical Outcomes, and members of the infection prevention and nursing staffs.

[14] The statements "The area around my room was always quiet at night" and "hospital staff always explained my medication before administering it" are two of the items included in the standardized survey instrument CMS used to assess patient satisfaction nationwide. Threshold (stretch) targets for the quietness of the environment were to have at least 70% (72%) of the survey respondents state that the area around their room or bed was always quiet at night. For the communication about medications, the threshold (stretch) target was to have at least 76% (80%) of the respondents indicate that staff always explained their medication before administering it.

$1,200, independent of that person's individual performance.[15] If the threshold goals were not met, no worker would receive the bonus, independent of individual performances. The bonus amount for collective performance between the two target levels would be determined ex-post. Before launching the incentive program, management communicated that it would be limited to three months and that it would not be repeated in the future to prevent expectations of future performance bonuses. In this study, I focus on the intervention's effect on handwashing performance.[16]

While handwashing protocols and expectations were the same for anyone working at JRH, physicians were ineligible for the monetary award. California laws forbid hospitals from directly employing physicians, so they were excluded from any performance-based incentives.[17] Therefore, while all caregivers who worked at JRH could contribute to achieving the hospital-level handwashing target, only direct employees—that is, nurses and other medical staff—were eligible for a bonus. Additionally, consistent with industry practice, JRH had no influence on physicians' career path and could not contract with the physicians on handwashing performance. Thus, physicians did not expect financial or career benefits arising from their improvements in handwashing performance.

Field interviews confirmed that, while individual-level handwashing data were never disclosed internally before, during, or after the intervention,[18] bonus-eligible employees believed that physicians'

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[15] JRH management did not communicate a specific bonus amount for reaching the threshold target before the intervention to stress the importance of reaching the stretch goal. However, during site interviews, the Director for Performance Improvement and Clinical Outcomes mentioned that employees expected to receive at least $200 as a bonus for meeting threshold targets. In her opinion, that expectation was reasonable.

[16] It was not possible to relate measures of communication about medication and quietness of the environment to individual workers' performance, as these metrics were collected via anonymous surveys assessing discharged patients' impressions of their experience with the hospital as a whole. Assessing individual workers' performance is critical to distinguish between bonus-eligible and bonus-ineligible workers. Additionally, while management had positioned the three performance dimensions to be equally important and there was no difference in the weights assigned to each of the three objectives for determining the bonus, field interviews indicated that workers felt they had the most control over handwashing improvements. None of the activities performed to improve quietness of the environment and communication about medications would likely interact with handwashing.

[17] State regulations prohibit hospital employment of physicians, thus prohibiting JRH from paying them any bonuses. Source: https://oig.hhs.gov/oei/reports/oei-01-91-00770.pdf. Accessed on March 10, 2020. Consistent with industry practice, physicians at JRH were employed by a physicians' organization (PO), which contracted with the hospital. Physicians' compensation and career progression were exclusively managed by the PO.

[18] The lack of disclosure of individual performance data, in particular about hand hygiene performance, is common practice among hospitals. Interviews with managers at JRH indicated that the hospital wanted to keep the focus on the aggregate effort and avoid singling anyone out. Additionally, the nature of the contractual relation between the hospital and the physicians' organization gave JRH's management little control over the physicians' day-to-day activities.

performance offered significant improvement opportunities. The lower hand hygiene compliance rate among physicians is consistent with trends observed worldwide and documented in the literature (e.g., Pittet et al. 2004; World Health Organization 2009). Additionally, bonus-eligible employees could observe physicians' handwashing practices during daily interactions, even before the intervention was announced. Therefore, bonus-eligible workers believed that improving physicians' performance was critical for earning the bonus.

During the intervention, a biweekly report communicated aggregate progress toward the handwashing target for the hospital as a whole and two main groups of departments (see Figure 1): Perioperative departments (Periop), which included pre-operative areas, operating rooms, and post-acute care units (PACU), and medical departments (Med/Surg), which included inpatient wards, physical therapy, and other services (e.g., technicians, nutritionists, other therapists). While helpful to monitor and communicate the collective progress toward the targets, these reports were insufficient to provide individual performance information. Furthermore, reported results merged physicians' and bonus-eligible employees' performance in a single metric for each department subgroup, so it was difficult to attribute any change to either category of workers.

----- Insert Figure 1 here -----

Motivated by the prospect of receiving a monetary award, bonus-eligible employees devised informal practices to pressure physicians to contribute to the collective goal. These practices varied across departments, but generally constituted positive reinforcements.[19] Examples included hand-shaped paper cards, on which bonus-eligible employees wrote the name of a physician they observed performing good hand hygiene. They would then affix the card to a wall as public recognition. During field interviews, bonus-eligible employees recalled several instances in which physicians inquired about the reason for the

[19] Bonus-eligible employees could have adopted negative reinforcement practices (i.e., social punishments). Their choice was likely driven by the collegial organizational culture at a relatively small hospital and concerns for potential future retaliation from workers with greater power (i.e., physicians). Whether positive reinforcement is more strongly correlated with the effectiveness of conduit incentives than negative reinforcement practices is an empirical question. I encourage further research efforts to better understand this relation.

cards on the wall, declared their intention to get their name on the wall, and celebrated earning it. In some cases, nurses would notify the chief nursing officer (CNO) about physicians whose compliance was particularly good or lacking. The CNO would then personally and privately email those physicians, either praising them (Figure 2, panel A) or reminding them of the importance of handwashing (Figure 2, panel B).[20]

----- Insert Figure 2 here -----

Manifestations of recognition, whether public or private, conveyed the reference group's social approval (or disapproval in the case of CNO's reprimands). Specifically, these social practices, which Brun and Dugas (2008) define as horizontal recognition of job dedication,[21] signaled to the physicians that (1) bonus-eligible workers were monitoring their performance and (2) that they appreciated their help to reach the goal that would earn them the bonus. Thus, bonus-ineligible workers' effort contributions were visible to the bonus-eligible ones. Additionally, when the recognition was public (e.g., in the case of the hand-shaped cards), it disclosed the names of bonus-ineligible workers contributing to the goal, adding pressure to perform for those whose names were not yet on the wall.

After three months, management paid the bonus to all eligible employees.[22] Performance for all the metrics included in the incentive program returned to being disclosed via the quarterly scorecard. All social

[20] Notably, no public recognition activities or reinforcement emails were directed to bonus-eligible colleagues. Additionally, these practices had not been performed prior to the beginning of the intervention period and stopped being performed immediately afterward. JRH did not collect any systematic data on these informal practices during the intervention period, so I cannot examine the variation between physicians that received more or less attention and recognition by the employees.

[21] See Brun and Dugas (2008, figure 2, page 726): Horizontal recognition of job dedication include "praise or effort (person, team); personalized letters acknowledging a co-worker's courage and perseverance; encouragement from peers to keep up effort and collective engagement; support among units."

[22] Figure 1 reports the progress and ultimate achievements pertaining to all three metrics involved in the intervention. Management decided to pay out the bonus in its entirety, despite missing the target they had set for communication on medication and barely attaining the threshold goal they had set for quietness of the environment. This possibility had not been communicated and workers had expected a pro-rated bonus amount. Had management decided not to pay the bonus, this might have demotivated the bonus-ineligible workers, who might have perceived missing the target as a failure on their part. Future research should examine the persistence of conduit incentives' behavioral effects when the goal is not attained and the bonus-eligible employees do not receive a bonus.

pressure practices related to hand hygiene performance ceased.[23] Management hoped that performance improvements would persist.

IV. EMPIRICAL TESTS

I obtained from JRH individual-level quarterly hand hygiene data collected over four consecutive quarters for 350 unique healthcare workers across seven departments. Table 1, Panel A, summarizes the sample construction. I removed from the sample eight part-time workers and ad-hoc collaborators[24] due to their weaker cultural affiliation with the hospital. I also removed 47 workers for which the site did not provide demographic information. Of the remaining 295 full-time workers, 225 (about 76 percent) were bonus-eligible. Physicians and physician assistants (70 people in total) were bonus-ineligible. The final sample includes 824 worker/quarter observations.

Table 1, Panel B, reports the number of unique individual workers observed in each quarter by bonus-eligibility (columns 1–3). Columns (4) to (6) report the number of individual secret shoppers' assessments by quarter and worker bonus-eligibility.[25] The *Baseline* period corresponds to the pre-intervention quarter (Q1). The *Intervention* period corresponds to the second quarter (Q2). The *PostIntervention* period includes the two quarters after the end of the intervention (Q3 and Q4). As mentioned, secret shoppers assessed JRH workers' handwashing compliance at random. This randomization introduced variation in the number of

[23] Specifically, the Director for Performance Improvement and Clinical Outcomes stated: "Once the three months were up, the paper hands came off the wall, the CNO stopped sending emails, and people weren't talking about hand hygiene anymore."

[24] Ad-hoc collaborators are physicians with privileges to practice at JRH, but are not part of the JRH physician staff and operate at JRH infrequently.

[25] In the remainder of the paper, I refer to the quarterly measure of an individual handwashing compliance as "worker observation" and the individual assessment by the secret shopper as "assessment." For example, if a worker was observed four times in the first quarter and performed proper hand washing in three of the four observations, there would be one worker observation equal to 0.75 for that worker, and the number of individual assessments would be equal to four. The data I obtained from the site included the following information about quarterly handwashing performance at the individual worker level: number of times a secret shopper observed the individual worker was observed to enter (*gel_in_assess*) or exit (*gel_out_assess*) a location in which they could have contact with a patient; number of times the worker was observed washing their hands upon entering (*gel_in*) or exiting (*gel_out*) the location; number of times the worker was observed *not* washing their hands upon entering (*gel_in_no*) or exiting (*gel_out_no*) the location; and handwashing compliance measure (*HHPerf*) calculated per Equation (2). I did not receive data on individual secret shopper assessments.

workers observed in each quarter and in the frequency with which an individual worker was assessed within and across quarters.[26]

Table 1, Panel C, reports the number of worker observations and individual workers observed in *both* periods in each row-column pair. For example, 166 workers were observed in the *Baseline* period, and 234 were observed in the *Intervention* period, but only 141 were observed in *both* periods. Therefore, the number of quarterly observations corresponding to workers observed in both the *Baseline* and the *Intervention* periods is 282.[27] Finally, Panel D reports the number of quarterly observations and unique workers who were observed in all *three* periods (i.e., *Baseline*, *Intervention*, and *PostIntervention*) for the pooled sample (column 1), bonus-eligible subsample (column 2), and bonus-ineligible subsample (column 3).

--- Insert Table 1 here ---

For each worker/quarter, JRH provided the number of secret shopper assessments upon entry (*gel_in_assess*), upon exit (*gel_out_assess*), the number of positive assessments upon entry (*gel_in*) and upon exit (*gel_out*), and the resulting worker/quarter measure of handwashing compliance, which corresponds to the following calculation.

$$HHPerf_{i,t} = \frac{gel\_in_{i,t} + gel\_out_{i,t}}{gel\_in\_assess_{i,t} + gel\_out\_assess_{i,t}}. \qquad (2)$$

Additional worker-level information included bonus eligibility, which I coded with the indicator variable *BonusEligible*, equal to one if worker *i* is bonus-eligible and zero if the worker is bonus-ineligible; gender, which I coded with the indicator variable *Female*, equal to one if the worker is female and zero otherwise; *Tenure*, expressed as the number of years the worker has worked with the organization; and

[26] As mentioned, management confirmed that the secret shoppers' agency did not have any knowledge of the intervention, nor were they asked to change in any way the frequency of their observations. According to management, the higher number of observations in Q2 is simply a random occurrence, unrelated to the intervention.

[27] Because the *PostIntervention* period includes both Q3 and Q4, the number of observations in the *PostIntervention* period in Table 1, Panel C, can exceed the number of unique workers observed in both periods (e.g., a worker could be observed once in Q2 and then once in both Q3 and Q4). As I will describe later, I subject all my results to a robustness test in which for each observed worker I calculate a comprehensive measure of performance corresponding to handwashing compliance in Q3 and Q4 combined. See Appendix 2 for details.

NAssess, which counts the total number of hand hygiene assessments performed on the worker in a quarter (i.e., the sum of *gel_in_assess* and *gel_out_assess*). All variables are defined in Appendix 1.

Table 2 summarizes the descriptive statistics. As Panel A shows, about 79% of the observations in the final sample correspond to bonus-eligible workers. About 62% of the observations refer to female workers, and the average tenure was about 7.2 years. On average, secret shoppers observed individual workers about eight times per quarter. On average, individual perfect compliance in the quarter (i.e., *HHPerf* = 1) was observed at least half the time, and at least 75% of the workers (independent from bonus-eligibility) exhibited compliance rates of 95.3% or better. Table 2, Panel B, shows that bonus-eligible workers exhibited higher average compliance (93.5%) than bonus-ineligible workers (83.9%). Panel C shows that average individual performance increased during the *Intervention* period (from 88.2% in the *Baseline* quarter to 93.0% in the *Intervention* quarter) and then slightly declined in the *PostIntervention* period (91.8%). Bonus-eligible employees' average performance rose from 93.0% to 95.1% and then fell to 92.7%, whereas bonus-ineligible workers' average performance improved monotonically from 72.8% to 85.7% to 88.1%.

--- Insert Table 2 here ---

Table 3 reports the pairwise Pearson correlations between the variables defined above. Consistent with the descriptive statistics, the correlation between bonus eligibility and handwashing compliance is positive and significant, albeit relatively small in magnitude ($\rho = 0.194$, $p<0.01$). Being female and having a longer tenure correlated with better performance ($\rho = 0.252$, $p<0.01$ and $\rho = 0.099$, $p<0.01$ respectively). The positive and significant correlation between the number of assessments (*NAssess*) and compliance is harder to interpret. One possibility is that the number of assessments correlates with the ease of observation by secret shoppers, thereby capturing workers' job characteristics beyond bonus-eligibility, gender, tenure, or department that correlate with performance.[28]

[28] For example, a secret shopper may more easily observe a physical therapist entering and exiting inpatient rooms, than a radiology technician, who spends more time in an enclosed laboratory. I examine the moderation effect of *NAssess* on the intervention effect of conduit incentives (*H1*) and the persistence of the ensuing behavioral modifications (*H2*). I don't find any significant evidence of such a moderating effect (untabulated results).

--- Insert Table 3 here ---

Predictor Model in the Baseline Period

To further explore the relation between worker characteristics and hand hygiene performance, I specify the following cross-sectional model predicting hand hygiene performance in the *Baseline* period.

$$HHPerf\_Pre_i = \alpha + \beta_1 BonusEligible_i + \beta_2 Female_i + \beta_3 Tenure_i + \sum_{j=1}^{7} \beta_j \, Dept_j + \varepsilon, \quad (3)$$

where *HHPerf_Pre* captures the *Baseline* performance for the individual JRH worker, and all the other variables are defined as previously described and reported in Appendix 1. I include department fixed effects to account for departments' unobserved idiosyncratic characteristics that may influence handwashing performance.[29]

Because the dependent variable *HHPerf_Pre* is naturally bounded below by zero and above by one, extremes included, and its distribution exhibits a significant mass of observations at the upper bound (see descriptive statistics in Table 2), I estimate Equation (3) using a fractional response model (FRM) (Papke and Wooldridge 1996, 2008). I perform separate estimations on the pooled sample and the subsamples of bonus-ineligible and bonus-eligible workers. Table 4 reports the results. Consistent with bonus-eligible workers' perceptions described above, their compliance rates are significantly higher than those for bonus-ineligible ones in the *Baseline* period (see column 1). Gender (captured by the indicator variable *Female*) appears to predict performance only for the bonus-ineligible workers. Average partial effects (APE) (Papke and Wooldridge 2008; Wooldridge 2002) reported in columns (4) through (6) correspond to "estimates of the partial effects averaged across the population of the predictor variable" (Papke and Wooldridge 2008, 122) and provide information about the economic significance of these relations.[30] Specifically, in the

[29] I do not include the variable *NAssess* in any of the estimations. While it may be informative about unobservable characteristics of workers' jobs in the hospital correlated with handwashing behaviors, it also introduces a mechanical relation with the dependent variable, as it corresponds to the denominator of the calculation in Equation (2). However, I use *NAssess* as input to the binomial regression estimations, which is one of the robustness tests to which I subject my results (Appendix 2). In the binomial regressions, the dependent variable is the count of the positive assessment outcomes of the secret shopper observations for worker *i* in period *t*, so there is no mechanical relation between predictor and outcome variable.

[30] APEs differ from the partial effects at the average, which are marginal effects estimated at the average value of the predictor variable. APEs can be compared with coefficients from a linear regression (Woodridge 2002, 750).

Baseline quarter, the average partial effect of bonus-eligibility is about 13.1 percentage points. The average partial effect estimated for *Female* suggests that, among bonus-ineligible workers, females' *Baseline* compliance is, on average, almost three times as high as that of males. These results withstand the robustness tests described in Appendix 2.

--- Insert Table 4 here ---

Test of H1: Effectiveness of Conduit Incentives

H1 formalizes my prediction about the effectiveness of conduit incentives. In my setting, *H1* is supported if bonus-ineligible workers improve handwashing during the intervention period. To test this hypothesis, I specify the following model.

$$HHPerf_{i,t} = \alpha + \beta_1 Intervention_t + \beta_2 Female_i + \beta_3 Tenure_i + \sum_{j=1}^{7} \beta_j \, Dept_j + \varepsilon. \quad (4)$$

The variable of interest in Equation (4) is *Intervention*, defined as an indicator variable equal to one for the *Intervention* quarter, zero for the *Baseline* quarter, and not defined for the *PostIntervention* quarters. All other variables are defined as previously described (see Appendix 1). As before, I estimate Equation (4) using a FRM. I include department fixed effects to control for unobservable department characteristics that could influence handwashing. I cluster standard errors by individual worker to account for worker-level idiosyncratic characteristics that could influence their performance over time.

I restrict the sample to observations corresponding to workers observed in *both* the *Baseline* and the *Intervention* periods, thus analyzing *within-subject* performance changes. As before, I estimate Equation (4) separately for the pooled sample, the bonus-ineligible workers, and the bonus-eligible ones. These three estimations serve different purposes. Estimating Equation (4) on the ineligible workers' subsample is the direct test of *H1*. A positive and significant coefficient β_1 would show that conduit incentives elicited ineligible workers' contribution to the collective goal during the *Intervention* period. The other two estimations offer information about the overall effectiveness of the intervention.

Table 5, Panel A, reports the estimation results. Column (1) corresponds to the pooled sample results. β_1 is positive and significant, indicating that the intervention improved performance overall, with an average partial effect of about 4.1 percentage points (column (4)). The overall effect, however, is driven by the bonus-ineligible workers, as reported in column (2), consistent with *H1*.[31] The average partial effect for bonus-ineligible workers is 13.9 percentage points (column (5)).[32] In contrast, the performance change during the *Intervention* period does not appear to be significant for the bonus-eligible workers.

The insignificant intervention effect for the bonus-eligible workers could be due to their high compliance rate in the *Baseline* period, making further improvements harder to attain. To further examine the effect of the *Baseline* performance levels on the intervention effects, I augment Equation (4) with a variable capturing the worker's pre-intervention level of handwashing performance ($HHPerf\_Pre_i$) and its interaction with the indicator variable *Intervention*. As before, I estimate the model for the pooled sample and then separately for each workers' type subsample.

Estimation results are reported in Table 5, Panel B. Higher *Baseline* performance levels are associated with a negative *incremental* effect, with average partial effect of 36.4 (68.6) [22.4] percentage points for the pooled sample (bonus-ineligible subsample) [bonus-eligible subsample]. Additionally, the estimated coefficients continue to suggest that the intervention had a significant positive main effect on the bonus-ineligible workers. However, accounting for the initial level of performance uncovers a significant intervention effect also for the bonus-eligible workers (see column (3)). It is not a goal of this study to determine which group of workers responded more strongly. However, finding evidence that, controlling for initial performance, eligible workers also improved handwashing during the intervention period corroborates the idea that the financial contract motivated them to achieve the collective goal.[33] That is,

[31] Bonus-ineligible workers might have improved hand washing simply because they felt compelled to help their bonus-eligible colleagues earn their bonus, independent of any social pressure. However, bonus-eligible workers would likely not have implemented costly social pressure if they believed they were not necessary to motivate their ineligible colleagues. Additionally, field interviews indicated that physicians reacted to these practices and linked them to their intention to improve hand hygiene (see Section III). Therefore, while I cannot rule out that some physicians would have improved performance even in the absence of social pressure, it is also unlikely that this could explain the entirety of my results.

[32] When the sample is restricted to bonus-ineligible workers, the variable *Female* is dropped from the estimation due to collinearity.

[33] The financial contract between managers and bonus-eligible workers corresponds to a setting that the literature has explored at great length. The availability of a monetary incentive to reward collective performance likely induces horizontal monitoring and social pressure among the bonus-eligible workers (e.g., Towry 2003; Pizzini 2010; Sedatole et al. 2016). In my setting, there was no formal manifestation of these pressures, as the observable social motivation

taken together, these results show that conduit incentives motivate bonus-eligible employees to try to elicit bonus-ineligible employees' effort via social pressure (i.e., to give rise to the nonfinancial contract). Bonus-ineligible workers, in turn, respond by improving their performance. Thus, these results continue to support *H1*.

As noted in Table 5, Panel A, although the intervention effect is statistically insignificant for the bonus-eligible workers, the magnitude of the performance change during the intervention period does not appear to be statistically different between bonus-eligible and bonus-ineligible workers (see Column (7): Chow test, p>0.10) This result is inconsistent with the interpretation that larger opportunities for improvement correlate with larger responses to the intervention. In untabulated tests, I find evidence suggesting that workers with lower *Baseline* performance might exhibit greater inertia to improving during the intervention.[34] The presence of such inertia and its variation across workers could contribute to explain the similar magnitudes in the intervention effect across the two worker groups.

Taken together, the results in Table 5 show that conduit incentives motivated bonus-eligible workers to improve performance and to elicit bonus-ineligible workers' contributions to the collective goal during the intervention, thus supporting *H1*.[35] These results withstand a battery of robustness tests, summarized in Appendix 2.

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practices were only directed from bonus-eligible to bonus-ineligible employees. Nonetheless, the absence of these manifestations does not preclude the possibility that bonus-eligible employees pressured each other in other ways.

[34] Specifically, I estimate the APEs for the interaction term *HHPerf_Pre*Intervention* at the fifth, 10th, 25th, 50th, 75th, 90th, and 95th percentiles of the distribution of *HHPerf_Pre*. This analysis shows that, while the APEs remain negative across the distribution, in the pooled sample and among bonus-eligible workers the incremental negative effect of the *Baseline* performance level *diminishes* as the *Baseline* performance increases. This suggests there could be greater inertia among the workers with poorer *Baseline* performance that prevents them from responding to the intervention. Furthermore, the incremental effect of pre-intervention performance exhibits a U-shaped trajectory for the bonus-ineligible workers while still maintaining a negative sign across the distribution. Future research is warranted to explore these unexpected trajectories.

[35] It is possible that some employees, regardless of their bonus-eligibility, improved performance because they expected greater monitoring during the intervention period. While my data does not allow me to rule out this possibility, this explanation is not likely to explain the entirety of my results. First, many studies in the health care literature have examined handwashing-related interventions based on increased monitoring (e.g., RFID monitoring systems; video-surveillance, etc.) which have not led to significant or sustainable improvements. Second, if increased monitoring were sufficient to improve performance, bonus-eligible employees would have been less prone to devise social pressure tactics. Third, recall that the contractual relationship between bonus-ineligible employees and the hospital did not allow JRH to influence their compensation or career progression and prevented JRH to contract directly with physicians on handwashing performance. Thus, increased monitoring would likely have little effect on bonus-ineligible workers' handwashing performance in the absence of the prospect of associated payoffs. Fourth, it

--- Insert Table 5 here ---

Test of H2: Persistence of the Behavioral Effects of Conduit Incentives

H2 predicts that the performance improvements associated with conduit incentives among bonus-ineligible workers' behaviors during the temporary intervention persist beyond the removal of the conduit incentives. In my setting, *H2* is supported if bonus-ineligible workers' *PostIntervention* handwashing performance level remains significantly higher than their *Baseline* level.[36] To test this hypothesis, I specify the following model.

$$HHPerf_{i,t} = \alpha + \beta_1 PostIntervention_t + \beta_2 Female_i + \beta_3 Tenure_i + \sum_{j=1}^{7} \beta_j \, Dept_j + \varepsilon. \quad (5)$$

The main variable of interest is *PostIntervention*, defined as an indicator variable equal to one in quarters Q3 and Q4, zero in the *Baseline* quarter (Q1), and undefined in the *Intervention* quarter (Q2). The estimation performed on the bonus-ineligible workers' subsample constitutes the direct test of *H2*. The estimations on the bonus-eligible subsample and on the pooled sample provide general information about the persistence of the intervention's effects. In all cases, the sample is restricted to workers observed in the *Baseline* period *and* in either quarter included in the *PostIntervention* period.

Table 6, Panel A, reports the estimation results for Equation (5), which I performed following the same econometric approach described for the tests of *H1*. In column (1), corresponding to the pooled sample, β_1 is positive and significant, which indicates persistent performance improvement, with an average partial effect of 3.8 percentage points. This result, however, is again driven by the bonus-ineligible workers (column (2)), and the average partial effect suggests that, after the intervention, bonus-ineligible workers' handwashing performance remained higher than their *Baseline* level by 13.8 percentage points, thus supporting *H2*. In contrast, bonus-eligible employees appear to return to performance levels that are indistinguishable from their baseline. The difference in the magnitude of the persistence effects between

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would be difficult to justify the persistence of the performance improvements beyond the removal of the incentive (and associated expectation of increased monitoring) in the absence of a mechanism that increases the value of the behavior in the mind of the bonus-ineligible workers by providing a benefit associated with the improved behavior (see the argument proposed for habit formation in footnote 9).

[36] Alternatively, persistence can be tested by comparing performance in the *PostIntervention* period with that in the *Intervention* period. I describe this robustness test in Appendix 2.

bonus-eligible and bonus-ineligible workers is marginally significant (Chow test, p = 0.107). As before, when the sample is restricted to bonus-ineligible workers, the variable *Female* is dropped from the estimation due to collinearity.

Similar to the test of H1, I augment Equation (5) with the variable $HHPerf\_Pre_i$ and its interaction with the indicator variable *PostIntervention* to explore the influence of baseline performance levels on the persistence of handwashing performance changes beyond the intervention period. Higher levels of *Baseline* performance may have caused maintaining an even higher level of performance over an extended period to be particularly difficult. Since I define persistence as the performance difference between the *PostIntervention* and the *Baseline* periods, better performance starting levels would require significantly greater effort to maintain higher compliance rates in the *PostIntervention* period. Table 6, Panel B, reports the estimation results. Accounting for initial performance uncovers a positive persistence effect for both bonus-eligible and bonus-ineligible workers ($\beta_1 > 0$, $p < 0.01$, in both columns (2) and (3)). In this analysis, the magnitude of the average main effect is statistically larger for bonus-eligible employees (Chow test, $p < 0.01$). The average partial effects of the main effect (*PostIntervention*) suggest that bonus-ineligible (bonus-eligible) workers' *PostIntervention* performance is 35.7 (38.9) percentage points higher than their *Baseline*. However, the incremental effect of *Baseline* performance is also significantly more negative (Chow test, $p < 0.01$) for bonus-eligible employees (APE = -46.4 percentage points) than for bonus-ineligible ones (APE = -40.7 percentage points). Similar to the test of *H1*, there is evidence of inertia influencing the persistence of the behavioral changes induced by conduit incentives (untabulated results).[37]

Taken together, the results reported in Table 6 support *H2* and show that conduit incentives produced performance improvements that persisted beyond the intervention period (i.e., beyond the removal of the

[37] The analysis of the APE estimated for the interaction term *HHPerf_Pre*PostIntervention* at the various percentiles of the *Baseline* performance distribution, again shows a monotonic trend, whereby the negative incremental effect diminishes in magnitude as the *Baseline* level increases for the pooled sample and the bonus-eligible workers, and a U-shaped trajectory, whereby the magnitude of the negative incremental effect first increases and then decreases for the bonus-ineligible workers. Future research should explore the possible causes of these trends.

conduit incentive), especially among bonus-ineligible workers. Robustness tests' results are summarized in Appendix 2.

--- Insert Table 6 here ---

V. ADDITIONAL ANALYSES

Relation Between Intervention Effects and Persistence

I explore whether individuals' performance *response* during the intervention period predicts the improvements' *persistence*. Answering this question has important practical implications. Managers can observe workers' reactions to the incentive and interpret it as an early indicator of the initiative's long-term success. Early evidence of ineffectiveness can then be promptly remedied.

I perform a cross-sectional test using the individual worker as my unit of analysis to estimate the relation between workers' response to the intervention and the persistence of performance improvements. I construct the dependent variable $PersistenceEffect_i$ as the difference between individual performance during the *PostIntervention* period and the *Baseline* level. I define the variable $InterventionEffect_i$ as the difference between individual performance during the *Intervention* period and the *Baseline* level. I then estimate the following model.

$$PersistenceEffect_i = \alpha + \beta_1 InterventionEffect_i + \beta_2 Female_i + \beta_3 Tenure_i + \sum_{j=1}^{7} \beta_j \, Dept_j + \varepsilon_i. \quad (6)$$

For this estimation, I restrict the sample to workers observed in all three periods (*Baseline*, *Intervention*, and *PostIntervention*). That is, the sample size for this analysis corresponds to the number of unique workers who were observed in *each* of the three periods (see Table 1, Panel D). As before, I estimate Equation (6) on the pooled sample and then on subsamples determined by workers' bonus-eligibility.

OLS estimation results reported in Table 7, Panel A, indicate a significant positive relation between the response to the intervention and the persistence of the effects beyond removing the incentive. That is, workers who respond to the intervention with larger performance improvements exhibit greater persistence once the stimuli are removed. There do not appear to be significant differences in this relation between bonus-eligible and bonus-ineligible employees (Wald test, $p>0.10$)

An untabulated analysis of the distribution of the intervention effect shows for that 14.18% (19.35%) [12.73%] of workers in the pooled sample (bonus-ineligible workers) [bonus-eligible workers] handwashing performance *deteriorated* during the intervention period. To account for potential asymmetries between positive and negative responses to the intervention, I construct an indicator variable $NegIntEff_i$ equal to one if the worker's performance deteriorated during the intervention period (i.e., performance in the *Intervention* period was worse than in the *Baseline* period) and zero otherwise. I then augment Equation (6) by including this variable and its interaction with $InterventionEffect_i$ among the predictors:

$$PersistenceEffect_i = \alpha + \beta_1 InterventionEffect_i + \beta_2 NegIntEff_i + \beta_3 NegIntEff_i * InterventionEffect_i + \beta_4 Female_i + \beta_5 Tenure_i + \sum_{j=1}^{7} \beta_j \, Dept_j + \varepsilon_i. \quad (7)$$

Estimating Equation (7) using the same approach used for Equation (6) indicates no evidence of significant asymmetries in the relation between *InterventionEffect* and *PersistenceEffect* attributable to the sign of the intervention response (Table 7, Panel B).

--- Insert Table 7 here ---

Moderating Effect of the Cost of Hand Hygiene Compliance

During field interviews, JRH representatives explained that hand washing requires significantly more effort in sterile environments. Unlike workers in nonsterile locations (i.e., inpatients wards), those in sterile environments wear personal protective equipment (masks, gloves, and gowns) at almost all times. Hand hygiene in these locations requires that they remove and discard their gloves, sanitize their hands, and then put on a new pair of gloves every time they cross the threshold into or out of a space where a patient might be located. Because the consequences of potential contamination are severe in a sterile environment, as patients are more vulnerable than in other locations, the hospital hand hygiene policy makes no accommodation to account for the increased effort required in this environment. In addition to the increased cost of compliance, healthcare workers tend to self-assess the risk of contamination (Pittet et al. 2000). Because they regularly wear protective equipment, many workers in sterile environments underestimate this risk. Univariate tests reported in Table 8, Panel A, show that hand washing rates are consistently lower in sterile departments than in the rest of the hospital. Whether the incremental cost of compliance influences the intervention's effectiveness and the persistence of associated performance improvements is an empirical question.

I define an indicator variable *SterileDept* equal to one if the observed individual works in a perioperative department (pre-op, operating rooms, post-op, or post-anesthesia care units) and zero otherwise. Departments for which *SterileDept* equals one are those in which the cost of handwashing compliance is higher. I then explore the moderating effect of *SterileDept* on the effectiveness of conduit incentives during the intervention period by estimating the following model.

$$HHPerf_{i,t} = \alpha + \beta_1 Intervention_t + \beta_2 SterileDept_i + \beta_3 Intervention_i * SterileDept_i + \beta_4 Female_i + \beta_5 Tenure_i + \varepsilon. \quad (8)$$

Because the variable *SterileDept* is defined at the department level, I do not include department fixed effects. As before, I restrict the sample to workers observed in both the *Baseline* and *Intervention* periods, and I estimate Equation (8) using a FRM and cluster the standard errors by worker. I perform separate estimations for the pooled sample, the subsample of bonus-ineligible workers, and the subsample of bonus-eligible ones. Estimation results are reported in Table 8, Panel B. The negative coefficient estimated for the interaction term *SterileDept***Intervention* when Equation (8) is estimated on the ineligible workers' subsample is evidence that higher cost of handwashing reduces the effectiveness of the intervention, with an average partial effect of 59.2 percentage points (column (5)). In contrast, the increased effort required in sterile environments does not appear to impact the intervention for bonus-eligible workers. This finding has important practical implications. Conduit incentives may prove ineffective if the cost of the pro-social behavior is too high, compared to the utility that ineligible workers gain from social rewards.

To assess the influence of the cost of compliance on the persistence of the intervention effects, I estimate the following model on the sample of workers observed both in the *Baseline* and *PostEstimation* periods.

$$HHPerf_{i,t} = \alpha + \beta_1 PostIntervention_t + \beta_2 SterileDept_i + \beta_3 PostIntervention_i * SterileDept_i + \beta_4 Female_i + \beta_5 Tenure_i + \varepsilon. \quad (9)$$

Table 8, Panel C, reports the fractional response model estimations. On average, the cost of compliance has a negative incremental effect on the persistence of behavioral modifications beyond removing the conduit incentives. Interestingly, however, this result is driven by the bonus-eligible workers and not by the bonus-ineligible ones. That is, the cost of compliance does not appear to influence the persistence of the intervention effects for the bonus-ineligible workers. The statistical power of this test may be insufficient to capture the marginal effect of the cost of compliance on the persistence of performance improvements among bonus-ineligible workers. However, the cost of compliance appears to damp the bonus-eligible workers' performance after the end of the intervention period. Specifically, the average partial effect estimated for the negative incremental effect of cost of compliance (i.e., operating in a sterile department) is about 6.9 percentage points.

--- Insert Table 8 here ---

VI. CONCLUSIONS

This study explores the effectiveness of a particular incentive design, which I label conduit incentives, whereby managers offer a monetary reward to workers under their direct control for performance that depends on the collaboration of workers who are ineligible for the reward. I posit that conduit incentives give rise to two contracts. The first is an explicit contract between the manager and the bonus-eligible workers and comprises an exchange of money for performance (i.e., financial contract). The second contract, implicit in nature, arises from the bonus-eligible employees' need to elicit the cooperation of the bonus-ineligible workers to earn the bonus promised in the financial contract. Thus, the second contract (i.e., nonfinancial contract) is between the bonus-eligible workers and their ineligible colleagues and comprises the provision of social rewards (punishments), such as appreciation and recognition (gossip and shunning) for contributing (failing to contribute) to the collective target.

I obtained proprietary field data from a California hospital where conduit incentives were adopted as part of a temporary intervention to improve performance on a set of quality metrics, including handwashing. The field setting characteristics allow me to examine the effectiveness of conduit incentives and the persistence of their effects. I predict and find that (1) conduit incentives elicit bonus-ineligible performance improvements and (2) such performance improvements persist among bonus-ineligible workers beyond the removal of the conduit incentives. My findings are robust to alternative specifications of the sample, alternative definitions of the main variables of interest, and to alternative estimation methods.

In additional analyses, I find that conduit incentives' effectiveness is attenuated when the cost of compliance is higher for the workers. Therefore, conduit incentives may not be an appropriate solution when the cost of the bonus-ineligible workers' behavior exceeds the utility (disutility) they derive from social rewards (punishments). Additionally, I document a systematic relation between the change in performance during the intervention period and its persistence after removing the incentive. This result has important practical implications. It suggests that managers can interpret the response of employees to temporary intervention programs as an early predictor of the persistence of the improvements and implement timely remedies as necessary. Additional advantages for practitioners include the fact that, in appropriate circumstances, managers may not need to reward every worker involved in collective tasks. Thus, conduit incentives may be less expensive than alternatives. This may be especially true when the ineligible workers are the higher earners, for whom providing incentives with adequate power would be costly.

My results generalize to settings in which performance of bonus-eligible workers depends significantly on the contribution of workers who are outside the span of control of the manager who, thus, cannot reward them directly for their performance. Conduit incentives are more likely to be effective in collegial settings, where bonus-ineligible workers care about their bonus-eligible colleagues and their prosocial reputation. Conduit incentives, therefore, may be less successful in settings with high interpersonal competition, an elevated focus on individual targets and achievements, where individual performance is less dependent on others, and where the cost of contributing to others' success exceeds the value associated with the social rewards (punishments).

This study is subject to several limitations. First, the behavior targeted by the temporary intervention was simple and required low cognitive effort. Future research should test the generalizability of my findings to more complex and costly activities. Second, the intervention characteristics do not allow me to isolate the influence that the change in the feedback's frequency during the intervention may have had on the documented results. As explained earlier, disclosed performance information was aggregated in a way that would not allow anyone to identify individual-level performance improvements or even by bonus-eligibility, thus limiting the ability of feedback to explain the entirety of my results. However, future research should examine potential complementarities and substitution effects between conduit incentives and feedback. Third, some ineligible employees may have contributed to helping their bonus-eligible colleagues earn their bonus independent of the social pressure. While anecdotal evidence points not only to the fact that the incentive program gave rise to social pressure but also that physicians responded, future research should further disentangle these mechanisms. Fourth, it is possible that some employees expected greater monitoring during the intervention period, thus improving performance due to a perception of being watched more. While I cannot rule out this explanation empirically, it is unlikely that this mechanism drove the entirety of my results, especially those documented with respect to the persistence of improved performance beyond the end of the intervention period. Lastly, while the characteristics of the setting in this study—especially the contractual relations between physicians and institutions—are common to many healthcare providers in the United States, the generalizability of settings conducive to the effectiveness of conduit incentives may be limited.

Despite its limitations, this study contributes to the incentive design literature by exploring a particular design (i.e., conduit incentives) that had not been studied before. While the literature is rich in studies on the interplay between monetary and nonmonetary incentives in team settings, the consequences of needing effort from workers outside the manager's span of control who could not benefit from any payoffs had not been explored. While research had explored the effectiveness of social pressures to reduce the free-riding problem (i.e., avoiding situations in which workers benefit from collective performance rewards without contributing sufficient effort), my study extends the academic inquiry by exploring the use of social pressures to obtain effort from unrewarded employees toward targets that reward others financially (i.e., the opposite of the free-riding problem). The two components of conduit incentives are not new mechanisms. Several studies have explored the effectiveness of monetary incentives on collective performance. Others have studied the effectiveness of social motivators on contributions to public goods. However, to the best of my knowledge, the *deliberate combination* of the two mechanisms to influence unrewarded workers' behaviors had yet to be analyzed. Future research should examine the effectiveness of conduit incentives in other organizational contexts and explore their interactions with other formal and informal controls, including different compensation structures, values and cultures, and degrees of performance interdependence.

APPENDIX 1: Variables Definition

Variable	Description
BonusEligible	Worker-level indicator variable assuming value one if the worker is eligible to receive the monetary bonus, and zero otherwise.
Female	Worker-level indicator variable assuming value one if the worker is female, and zero otherwise.
gel_in	Indicator variable equal to one if the secret shopper observes the worker complying with hand hygiene requirements upon entry into a location where they could have physical contact with a patient, and zero if the observed worker failed to comply with hand hygiene requirements.
gel_out	Indicator variable equal to one if the secret shopper observes the worker complying with hand hygiene requirements upon exit out of a location where they could have had physical contact with a patient, and zero if the observed worker failed to comply with hand hygiene requirements.
gel_in_assess	Variable counting the number of times secret shoppers assessed workers' hand hygiene behaviors when entering a location where they could have physical contact with a patient during a quarter.
gel_out_assess	Variable counting the number of times secret shoppers assessed workers' hand hygiene behaviors when entering a location where they could have had physical contact with a patient during a quarter.
HHPerf	Individual hand hygiene compliance is measured as the ratio of gel-in and gel-out observations with positive outcomes and the number of observations in the quarter for the worker. Continuous variable defined on the range [0,1].
HHPerf_Pre	Worker-level baseline measure of *HHPerf* captured in the pre-intervention period. Continuous variable defined on the range [0,1]. This variable is not defined for the other quarters.
Intervention	Quarter-level indicator variable equal to one if the quarter is Q2 (*Intervention* period) and zero if the quarter is Q1 (*Baseline* period). This variable is not defined for Q3 and Q4 (*PostIntervention* periods).
InterventionEffect	Difference between hand hygiene performance observed in Q2 (*Intervention* period) and Q1 (*Baseline* period).
NAssess	Worker/quarter variable capturing the number of hand hygiene observations the secret shoppers performed in the organization in a given quarter for an individual worker.
NegIntEff	Indicator variable equal to one of the worker's *InterventionEffect* is negative, and zero otherwise.
PersistenceEffect	Difference between hand hygiene performance observed in Q3 and Q4 (*PostIntervention* period) and Q1 (*Baseline* period).
PostIntervention	Quarter-level indicator variable equal to one if the quarter is Q3 or Q4 (*PostIntervention* periods) and zero if the quarter is Q1 (*Baseline* period). This variable is not defined for Q2 (*Intervention* period).
SterileDept	Department-level indicator variable equal to one if the worker's hand hygiene behavior is observed in a sterile environment, and zero otherwise. Sterile environments include perioperative areas, pre-operative areas, operating rooms, and post-acute care units.
Tenure	Worker-level measure of tenure with the organization, measured in years.

APPENDIX 2: Robustness Tests

I validate the robustness of the hypotheses tests results by introducing alternative specifications of the models described above, alternative estimation methods, and alternative definitions of some of the variables of interest. The hypotheses tests results described in the main body of the paper withstand the following robustness tests.

1. *Estimations without department fixed effects*. The small size of the sample, especially when estimating models on the ineligible workers' subsample, could raise concerns about the tests' statistical power. Therefore, I re-estimate the *Baseline* predictor model (Table 4) and the models used to test *H1* (Table 5) and *H2* (Table 6) excluding department fixed effects.

2. *Binomial regression*: I re-estimate all relevant models using a binomial regression method (Wooldridge 2002). This approach allows to "analyze count data conditional on a known upper bound" (Wooldridge 2002, 739). In my setting, the upper bound is the number of secret shoppers' assessments for the individual worker in each period (*NAssess*). In this approach, the individual secret shopper assessments are treated as independent Bernoulli random variables. Unobserved factors (such as variation in the ease of observability by the secret shopper) are allowed to influence the number of assessments. I define a new dependent variable capturing hand hygiene performance as $GelYesCount_{i,t}$, which is calculated as the sum of positive assessment outcomes for the individual worker *i* in period *t* ($gel\_in_{i,t} + gel\_out_{i,t}$). The binomial regression conditions the probability of success on the number of assessments (*NAssess*), which is therefore used as an input to the estimation algorithm.

3. *Alternative definition of persistence*: As described earlier, I define the persistence of the interventions effects beyond removing the conduit incentives as the difference in hand hygiene compliance between the *PostIntervention* and *Baseline* periods. A positive difference in performance is evidence that the intervention produced persistent behavioral modification effects. An alternative way to assess persistence is to compare performance between the *PostIntervention* and the *Intervention* periods. I define a new indicator variable *PostInterventionV2*, which is equal to 1 if the quarter is Q3 or Q4 and zero if the quarter is Q2 (the variable is not defined on the *Baseline* quarter Q1). In this specification, a nonnegative difference in performance (*PostIntervention* performance level – *Intervention* level) would indicate that workers did not drift back to pre-intervention behaviors. This approach makes sense only after establishing that the intervention was indeed associated with an average performance improvement.

4. *Alternative calculation of HHPerf in the PostIntervention period*: In the paper, the *PostIntervention* period includes two quarters (Q3 and Q4). Due to the random nature of the secret shoppers' observations, some of the workers are assessed in both quarters and some only in one, thus introducing some variation in the number of quarterly observations (one or two) in the sample for those workers who are assessed in the *PostIntervention* period. I construct an aggregate measure of performance in the *PostIntervention* period (*HHPerfV2*) for each worker observed in either Q3 or Q4, thus including at most one observation for each worker in the *PostIntervention* period. That is, the alternative specification of the variable capturing workers' hand washing (*HHPerfV2*) is computed as follows.

$$HHPerfV2_{i,t} = \frac{gel\_in_{i,Q3} + gel\_out_{i,Q3} + gel\_in_{i,Q4} + gel\_out_{i,Q4}}{gel\_in\_assess_{i,Q3} + gel\_out\_assess_{i,Q3} + gel\_in\_assess_{i,Q4} + gel\_out\_assess_{i,Q4}}$$

I then substitute the variable *HHPerfV2* in Equation (5) and re-estimate the model to test *H2*.

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Figure 1: Biweekly Performance Feedback Form during the Intervention Period

Metrics	Stretch	Goal	Baseline Data		
5 Star Rating/MSPB Incentive			Periop Scores	Med/Surg Scores	Overall
Quietness of Environment	72%	70%	64%	64%	64%
Communication about Medication	80%	76%	67%	67%	67%
Hand Hygiene	95%	92%	86%	96%	91%
			Pay Day Update 11/12/2015		
Quietness of Environment	72%	70%	68.2%	68.2%	68.2%
Communication of Medication	80%	76%	83.3%	83.3%	83.3%
Hand Hygiene	95%	92%	86.0%	97.0%	93.0%
			Pay Day Update 11/25/2015		
Quietness of Environment	72%	70%	66.7%	66.7%	66.7%
Communication of Medication	80%	76%	80.9%	80.9%	80.9%
Hand Hygiene	95%	92%	88%	97%	93% *
			Pay Day Update12/10/2015		
Quietness of Environment	72%	70%	61.6%	61.6%	61.6%
Communication of Medication	80%	76%	73.8%	73.8%	73.8%
Hand Hygiene	95%	92%	90.0%	97.0%	94.0%
			Pay Day Update 12/23/2015		
Quietness of Environment	72%	70%	63.8%	63.8%	63.8%
Communication of Medication	80%	76%	72.8%	72.8%	72.8%
Hand Hygiene	95%	92%	91.3%	97.5%	94.3%
			Pay Day Update 1/07/2016		
Quietness of Environment	72%	70%	63.0%	63.0%	63.0%
Communication of Medication	80%	76%	72.5%	72.5%	72.5%
Hand Hygiene	95%	92%	91.7%	98.0%	94.4%
			Pay Day Update 1/21/16		
Quietness of Environment	72%	70%	65.2%	65.2%	65.2%
Communication of Medication	80%	76%	72.9%	72.9%	72.9%
Hand Hygiene	95%	92%	91.7%	98.2%	94.6%
			Final Pay Day Update 2/4/2016		
Quietness of Environment	72%	70%	70.0%	70.0%	70.0%
Communication of Medication	80%	76%	74.0%	74.0%	74.0%
Hand Hygiene	95%	92%	91.7%	98.2%	94.6%

Keep up the good work. Remember quality is about sustaining and improving on past performance. The surveys are still coming in.

Hand Hygiene: Go team! We are at stretch for hand hygiene. Keep it up. Kudos to staff who have engaged our physicians in performing hand hygiene as well. YOU did it!

Opportunities for Quietness of Environment: You hit the mark and the numbers are slowly climbing up! Keep them going this way!

Opportunities for Communication of Medication: The numbers are moving up! Keep talking to patients about new medications and side effects. Please continue to talk and use teach back education with your patients.

Notes: Figure 1 shows the periodic updates all workers received during the intervention period, independently from their bonus-eligibility. In each communication, management added comments at the bottom of the table to highlight noteworthy performance aspects in the past two weeks. The comments reported at the bottom of Figure 1 relate to the last bi-weekly communication from management.

Figure 2: **Examples of Informal Social Recognition Practices**

Panel A: Example of "Love Note" sent by the CNO to a physician performing well during the incentive period

> *You have been observed to be a phenomenal role model for keeping our infection rates LOW.*
>
> *You have been observed to be 100% compliant this past month with hand hygiene.*
>
> *Your patients appreciate your strong work as do I.*
>
> *Keep up the great work.*

Panel B: Example of "Love Note" sent by the CNO to a physician performing poorly during the incentive period

> *Subject: Hand Hygiene Superstars, or not*
>
> *Hi,*
>
> *You have been identified as an opportunity waiting to improve your hand hygiene. Compliance with hand hygiene keeps our infection rates VERY LOW, shows as a great role model for all staff, and our patients are watching you!*
>
> *The rules of engagement are:*
>
> *1. Observed gelling before going in a room*
>
> *2. Observed gelling as you leave the room*
>
> *3. If you are gelling out and going directly into the room in the same pod, keep moving. Do not touch anything and you are free to enter the next room.*
>
> *4. The only exception is patients with c. diff. You must wash with soap and water. Don't be a vector for c. diff.*
>
> *Keep up the strong work and we will keep an eye on you!*

Table 1: Sample Characteristics

Panel A: Sample Construction

Total workers in the sample	350
Less: Part-Time workers	(8)
Remaining Full-time workers	342
Less: Workers with missing demographic information	(47)
Remaining workers	295
Of which:	
Bonus-Eligible	225 (76.2%)
Bonus-Ineligible	70 (23.7%)

Panel B: Individual Workers Observed and Random Individual Assessments by Period

		Unique Workers Observed in Each Quarter			Number of Individual Assessments		
Period	Quarter	Bonus Eligible (1)	Bonus Ineligible (2)	Total (3)	Bonus Eligible (4)	Bonus Ineligible (5)	Total (6)
Baseline	Q1	127	39	166	919	176	1,095
Intervention	Q2	181	53	234	1,770	412	2,182
PostIntervention	Q3	173	48	221	1,403	250	1,653
	Q4	169	34	203	1,520	137	1,657
Total		650	174	824	5,612	975	6,587

Panel C: Number of Observations Corresponding to Workers Observed in Both Periods in Each Row-Column Pair

		Worker Observations (Unique Workers)		
		Baseline	*Intervention*	*PostIntervention*
Pooled Sample	*Baseline*	166 (166)	282 (141)	414(148)
	Intervention		234 (234)	554 (202)
	PostIntervention			424 (257)
Bonus-Eligible Workers	*Baseline*	127 (127)	220 (110)	335 (117)
	Intervention		181 (181)	451 (162)
	PostIntervention			342 (202)
Bonus-Ineligible Workers	*Baseline*	39 (39)	62 (31)	79 (31)
	Intervention		53 (53)	103 (40)
	PostIntervention			82 (55)

Panel D: Number of Observations Corresponding to Workers Observed in All Periods

	Pooled Sample	*Bonus-Eligible*	*Bonus-Ineligible*
Observations (Unique Workers)	495 (129)	405 (104)	90 (25)

Notes: **Panel A**: The original sample included 350 workers affiliated with the organization. Of these, eight were contractors that operated on a part-time or ad-hoc basis at the hospital (e.g., external physicians with privileges). These external contractors were excluded from the sample because of their muted cultural affiliation with the organization. I excluded from the sample 47 workers for which demographic information (i.e., gender and tenure) was not available. The remaining full-time workers were then grouped based on their eligibility to receive bonus payments. **Panel B** reports the number of individual workers observed in each quarter (columns 1-3) and the number of secret shopper random assessments of hand hygiene compliance in each quarter (columns 4-6). I will refer to the rows in my dataset as "worker observations" and the individual secret shopper assessments as "individual assessments." Worker observations relate to the handwashing compliance ratio for one worker in one period, calculated per Equation (2). For example, if a worker was observed four times in Q1 and performed proper hand washing in three of the four observations, there would be one worker observation equal to 0.75 for that worker, and the number of individual assessments would be equal to four. **Panel C** reports the number of worker observations (unique workers observed) in *both* periods of each row-column pair (that is, the *intersection* of the workers observed in the row period and in the column period). For example, according to Panel B, there were 166 unique workers observed in the *Baseline* period and 234 in the *Intervention* period. However, of the 166 workers observed in the *Baseline* period, 141 were also observed in the *Intervention* period. Therefore, the number of unique workers observed in both periods is 141, and the total number of worker observations for these workers is 282. Because the *PostIntervention* period includes both Q3 and Q4, the number of worker observations in the *PostIntervention* period can exceed the number of unique workers observed (e.g., a worker could be observed in Q1 and in both Q3 and Q4). Therefore, for example, the number of worker observations corresponding to workers observed in the *Baseline* and the *PostIntervention* period is 414, which is more than double the number of unique workers observed in both periods (148*2 = 296) and less than triple that number (148*3 = 444), which would be the case if all workers that were observed in the *Baseline* period were also observed in both Q3 and Q4. **Panel D** indicates the number of worker observations performed (unique workers observed), in all three periods: *Baseline*, *Intervention*, and either quarter included in the *PostIntervention* period. In this case, the number of worker observations is between the number of unique workers and four times that number (as one worker could be observed in all four quarters) For example, column (1) indicates that there are 129 workers that were observed in all three periods, for a total of 495 quarterly observations (129*4 = 516).

Table 2: Descriptive Statistics

Panel A: Pooled Sample

	Variable	N	Mean	Std. Dev.	p25	p50	p75
Pooled Sample	*HHPerf*	824	0.914	0.200	0.953	1.000	1.000
	BonusEligible	824	0.789	0.408	1.000	1.000	1.000
	NAssess	824	7.994	7.503	3.000	5.500	11.000
	Female	824	0.621	0.485	0.000	1.000	1.000
	Tenure	824	7.193	5.876	4.500	6.000	8.000

Panel B: By Bonus-Eligibility

	Variable	N	Mean	Std. Dev.	p25	p50	p75
Bonus-Eligible	*HHPerf*	650	0.935	0.171	1.000	1.000	1.000
	NAssess	650	8.634	7.593	3.000	6.000	12.000
	Female	650	0.782	0.414	1.000	1.000	1.000
	Tenure	650	7.671	6.505	4.000	5.000	10.000
Bonus-Ineligible	*HHPerf*	174	0.839	0.272	0.750	1.000	1.000
	NAssess	174	5.603	6.650	2.000	4.000	6.000
	Female	174	0.023	0.150	0.000	0.000	0.000
	Tenure	174	5.408	1.217	6.000	6.000	6.000

Panel C: Descriptive Statistics for *HHPerf* by Bonus-Eligibility and Period

	Variable	N	Mean	Std. Dev.	p25	p50	p75
Baseline	*Pooled Sample*	166	0.882	0.229	0.833	1.000	1.000
	Bonus-Eligible	127	0.930	0.161	0.909	1.000	1.000
	Bonus-Ineligible	39	0.728	0.332	0.636	0.778	1.000
Intervention	*Pooled Sample*	234	0.930	0.179	1.000	1.000	1.000
	Bonus-Eligible	181	0.951	0.146	1.000	1.000	1.000
	Bonus-Ineligible	53	0.857	0.252	0.800	1.000	1.000
PostIntervention	*Pooled Sample*	424	0.918	0.199	1.000	1.000	1.000
	Bonus-Eligible	342	0.927	0.187	1.000	1.000	1.000
	Bonus-Ineligible	82	0.881	0.239	0.833	1.000	1.000

Notes: This table reports the descriptive statistics. The final sample, constructed as indicated in Table 1, includes 824 worker/period observations. All variables are defined in Appendix 1. **Panel A** reports the descriptive statistics for the pooled sample. **Panel B** reports the descriptive statistics separately for each group of workers, based on their bonus-eligibility. **Panel C** reports the descriptive statistics of the variable *HHPerf* by period and workers' bonus-eligibility.

Table 3: Correlation Matrices

Panel A: Pooled Sample

	HHPerf	*BonusEligible*	*Female*	*Tenure*	*NAssess*
HHPerf	1.000				
BonusEligible	0.194***	1.000			
Female	0.252***	0.638***	1.000		
Tenure	0.099***	0.157***	0.195***	1.000	
NAssess	0.070**	0.165***	0.102***	0.067*	1.000

Panel B: Bonus-Eligible Workers

	HHPerf	*Female*	*Tenure*	*NAssess*
HHPerf	1.000			
Female	0.209***	1.000		
Tenure	0.090**	0.128***	1.000	
NAssess	-0.006	0.000	0.047	1.000

Panel C: Bonus-Ineligible Workers

	HHPerf	*Female*	*Tenure*	*NAssess*
HHPerf	1.000			
Female	0.091	1.000		
Tenure	0.026	-0.052	1.000	
NAssess	0.165**	-0.066	-0.009	1.000

Notes: This table reports the pairwise Pearson correlations between the main variables of interest in this study. **Panel A** reports the correlation coefficients for the pooled sample. **Panel B** reports the coefficients for the bonus-eligible subsample of workers and **Panel C** for the bonus-ineligible ones. All variables are defined in Appendix 1. Statistical significance is expressed as follows: * = p<0.10; ** = p<0.05; ***= p<0.01.

Table 4: Predictors of Hand Hygiene Performance in the Pre-Intervention Period

DV = *HHPerf_Pre*	Fractional Response Model			Average Partial Effects		
	Pooled Sample (1)	Bonus-Ineligible (2)	Bonus-Eligible (3)	Pooled Sample (4)	Bonus-Ineligible (5)	Bonus-Eligible (6)
BonusEligible	1.385***			0.131***		
	(2.80)			(2.64)		
Female	0.227	14.919***	-0.148	0.021	2.908***	-0.009
	(0.46)	(14.09)	(-0.26)	(0.46)	(6.63)	(-0.26)
Tenure	0.061	-0.083	0.057	0.006	-0.016	0.004
	(1.35)	(-0.34)	(1.29)	(1.31)	(-0.34)	(1.20)
Intercept	0.643*	1.474	1.993***			
	(1.88)	(1.11)	(4.43)			
Dept FE	YES	YES	YES	YES	YES	YES
N	166	39	127	166	39	127
R^2	0.074	0.033	0.058			

Notes: This table reports the results of the estimation of a determinant model (Eq. 3) predicting the level of hand hygiene performance in the *Baseline Period*. The unit of observation is the individual worker, and the observations are restricted to handwashing assessments performed during Q1 (*Baseline Period*). All variables are defined in Appendix 1. Estimations were performed using the fractional response model (FRM). Columns 1-3 report the coefficients estimated for (1) the pooled sample, (2) the subsample of bonus-ineligible workers, which is the group of focus for this study (as the thick-line box indicates), and (3) for bonus-eligible workers only. Columns 4-6 report the average partial effects (APE) estimated for (1) the pooled sample, (2) bonus-ineligible workers, and (3) bonus-eligible workers. All estimations include department fixed effects to account for individual departments' idiosyncratic characteristics that may influence their members' hand hygiene performance. t-statistics are reported in parentheses underneath each coefficient. R^2 is calculated for each estimation as the squared value of the correlation between predicted and observed values. Statistical significance is reported as follows: * = p<0.10; ** = p<0.05; ***= p<0.01.

Table 5: Test of H1: Intervention Effects of Conduit Incentives

Panel A: Comparing hand hygiene performance in the *Intervention* period with the *Baseline*

DV = *HHPerf*	Fractional Response Model			Average Partial Effects			Chow Test
	Pooled Sample (1)	Bonus-Ineligible (2)	Bonus-Eligible (3)	Pooled Sample (4)	Bonus-Ineligible (5)	Bonus-Eligible (6)	(Ineligible – Eligible) (7)
Intervention	0.568**	0.880**	0.283	0.041**	0.139**	0.013	p>0.10
	(2.50)	(2.22)	(1.13)	(2.43)	(2.28)	(1.16)	
Female	1.176**	0.000	0.484	0.084**	0.000	0.021	
	(2.49)	(0.00)	(0.79)	(2.28)	(0.00)	(0.75)	
Tenure	0.014	-0.120	0.002	0.001	-0.019	0.000	
	(0.51)	(-0.47)	(0.09)	(0.51)	(-0.47)	(0.09)	
Intercept	1.241***	1.708	2.039***				
	(4.27)	(1.22)	(4.89)				
Department FE	YES	YES	YES	YES	YES	YES	
N	282	62	220	282	62	220	
R^2	0.124	0.016	0.110				

Panel B: Accounting for the *Baseline* level of performance

DV = *HHPerf*	Fractional Response Model			Average Partial Effects			Chow Test
	Pooled Sample (1)	Bonus-Ineligible (2)	Bonus-Eligible (3)	Pooled Sample (4)	Bonus-Ineligible (5)	Bonus-Eligible (6)	(Ineligible – Eligible) (7)
Intervention	5.855***	5.141***	5.099**	0.312***	0.552***	0.193**	p>0.10
	(6.40)	(5.92)	(2.53)	(6.65)	(6.19)	(2.46)	
HHPerf_Pre	8.973***	7.679***	11.676***	0.478***	0.825***	0.442***	p<0.01
	(9.24)	(7.62)	(10.02)	(8.21)	(6.32)	(7.49)	
HHPerf_Pre Intervention*	-6.841***	-6.387***	-5.914**	-0.364***	-0.686***	-0.224**	p>0.10
	(-6.32)	(-5.82)	(-2.48)	(-6.03)	(-5.01)	(-2.37)	
Female	0.133	0.000	-0.006	0.007	0.000	-0.000	
	(0.40)	(0.00)	(-0.02)	(0.40)	(0.00)	(-0.02)	
Tenure	-0.007	0.096	-0.016	-0.000	0.010	-0.001	
	(-0.34)	(0.62)	(-0.65)	(-0.34)	(0.62)	(-0.64)	
Intercept	-5.313***	-4.797***	-7.390***				
	(-6.98)	(-4.22)	(-7.98)				
Department FE	YES	YES	YES	YES	YES	YES	
N	282	62	220	282	62	220	
R^2	0.761	0.723	0.662				

Notes: This table reports the results of the estimation of Eq. (4), which predicts the effect of the temporary intervention by comparing hand hygiene performance between the *Baseline* period (Q1) and the *Intervention* period (Q2). I restricted the sample to workers whose hand hygiene compliance was assessed in both Q1 (*Baseline* period) and Q2 (*Intervention* period). I estimated Eq. (4) using a fractional response model (FRM). Columns 1-3 report the coefficients estimated for (1) the pooled sample; (2) the subsample of bonus-ineligible workers, which is the group of focus for this study (as the thick-line box indicates), and (3) the bonus-eligible workers. Columns 4-6 report the average partial effects (APE) for the same groups.

Panel A reports the results of the estimation of Eq. (4). **Panel B** reports the estimation results for Eq. (4) augmented with the inclusion among the predictors of the variable *HHPerf_Pre* and its interaction with *Intervention*. All estimations include department fixed effects to account for individual departments' idiosyncratic characteristics that may influence their hand hygiene performance. When the group is restricted to the ineligible workers, the variable *Female* is dropped from the estimation due to collinearity. In all estimations, standard errors are clustered by worker, and t-statistics are reported in parentheses underneath each coefficient. R^2 is calculated for each estimation as the square value of the correlation between the dependent variable's values predicted by Eq. (4) and the observed values. Statistical significance is reported as follows: * = p<0.10; ** = p<0.05; ***= p<0.01. All variables are defined in Appendix 1.

Table 6: Test of H2: Persistence Effects of Conduit Incentives

Panel A: Comparing hand hygiene performance in the *PostIntervention* period with the *Baseline*

DV = *HHPerf*	Fractional Response Model			Average Partial Effects			Chow Test
	Pooled Sample (1)	Bonus-Ineligible (2)	Bonus-Eligible (3)	Pooled Sample (4)	Bonus-Ineligible (5)	Bonus-Eligible (6)	(Ineligible – Eligible) (7)
PostIntervention	0.556**	0.970***	0.265	0.038**	0.138***	0.013	p=0.11
	(2.53)	(3.01)	(0.88)	(2.50)	(3.12)	(0.87)	
Female	0.779**	0.000	0.519	0.053*	0.000	0.026	
	(1.99)	(0.00)	(1.42)	(1.92)	(0.00)	(1.44)	
Tenure	0.066**	-0.072	0.056**	0.004**	-0.010	0.003**	
	(2.22)	(-0.30)	(2.26)	(2.15)	(-0.30)	(2.16)	
Intercept	0.778***	1.453	0.937***				
	(2.80)	(1.07)	(3.06)				
Department FE	YES	YES	YES	YES	YES	YES	
N	414	79	335	414	79	335	
R^2	0.130	0.011	0.129				

Panel B: Accounting for the *Baseline* level of performance

DV = *HHPerf*	Fractional Response Model			Average Partial Effects			Chow Test
	Pooled Sample (1)	Bonus-Ineligible (2)	Bonus-Eligible (3)	Pooled Sample (4)	Bonus-Ineligible (5)	Bonus-Eligible (6)	(Ineligible – Eligible) (7)
Post Intervention	5.239***	3.560***	9.067***	0.299***	0.357***	0.389***	p<0.01
	(5.75)	(4.52)	(6.35)	(5.59)	(4.36)	(4.97)	
HHPerf_Pre	8.277***	6.758***	11.219***	0.471***	0.678***	0.481***	p<0.01
	(8.81)	(10.26)	(9.70)	(7.80)	(6.70)	(6.81)	
HHPerf_Pre PostIntervention*	-6.275***	-4.056***	-10.823***	-0.362***	-0.407***	-0.464***	p<0.01
	(-5.61)	(-3.81)	(-6.23)	(-5.16)	(-3.25)	(-4.73)	
Female	0.082	0.000	0.363	0.001	0.000	0.016	
	(0.24)	(0.00)	(0.90)	(0.06)	(0.00)	(0.89)	
Tenure	0.037	-0.025	0.038*	0.002	-0.003	0.002*	
	(1.63)	(-0.12)	(1.78)	(1.54)	(-0.12)	(1.76)	
Intercept	-5.200***	-3.398***	-8.140***				
	(-6.98)	(-2.58)	(-7.80)				
Department FE	YES	YES	YES	YES	YES	YES	
N	414	79	335	414	79	335	
R^2	0.626	0.588	0.591				

Notes: This table reports the results of the estimation of Eq. (5), which predicts the persistence of the behavioral effects of the temporary incentive program beyond the duration of the intervention by comparing hand hygiene performance between the *Baseline* period and the *PostIntervention* period. Recall that the *PostIntervention* period includes two quarters (Q3 and Q4). The sample for this estimation was restricted to observations of hand hygiene compliance performed in Q1 (*Baseline* period), and Q3 or Q4 (*PostIntervention* period). That is, each worker in the sample was observed both in the *Baseline* period and in at least one quarter in the *PostIntervention* period. I estimated Eq. (5) using a fractional response model (FRM). Columns 1-3 report the coefficients estimated for (1) the pooled sample; (2) the subsample of bonus-ineligible workers, which is the group of focus for this study (as the thick-line box indicates), and (3) the bonus-eligible workers. Columns 4-6 report the average partial effects (APE) for the same groups. **Panel A** reports the estimation results for Eq. (5). **Panel B** reports the estimation results for Eq. (5) augmented with the inclusion of the variable *HHPerf_Pre* and its interactions with *PostIntervention*. All estimations include department fixed effects to account for individual departments' idiosyncratic characteristics that may influence their hand hygiene performance. When the group is restricted to the ineligible workers, the variable *Female* is dropped from the estimation due to collinearity. In all estimations, standard errors are clustered by worker, and t-statistics are reported in parentheses underneath each coefficient. R^2 is calculated for each estimation as the square value of the correlation between the dependent variable's values predicted by Eq. (5) and the observed values. Statistical significance is reported as follows: * = $p<0.10$; ** = $p<0.05$; ***= $p<0.01$. All variables are defined in Appendix 1.

Table 7: Relation between Intervention Effect and Persistence Effect

Panel A: Intervention and Persistence Effects

DV = PersistenceEffect	Pooled Sample (1)	Bonus-Ineligible (2)	Bonus Eligible (3)	Chow Test (Ineligible – Eligible) (4)
InterventionEffect	0.864***	0.722***	1.051***	p>0.10
	(5.95)	(3.06)	(9.25)	
Female	-0.073*	0.000	0.022	
	(-1.68)	(0.00)	(0.48)	
Tenure	0.003	0.077**	0.004	
	(1.36)	(2.42)	(1.51)	
Intercept	-0.255***	-0.550***	-0.449***	
	(-4.47)	(-3.83)	(-6.73)	
Department FE	YES	YES	YES	
N	129	25	104	
R^2	0.581	0.488	0.598	

Panel B: Exploring Asymmetries Between Positive and Negative Intervention Effects

DV = PersistenceEffect	Pooled Sample (1)	Bonus-Ineligible (2)	Bonus Eligible (3)	Chow Test (Ineligible – Eligible) (4)
InterventionEffect	1.111***	1.221***	1.180***	p>0.10
	(8.64)	(6.85)	(8.25)	
NegIntEff	0.046	0.388	0.027	p<0.10
	(0.38)	(1.63)	(0.50)	
InterventionEffect NegIntEff*	-0.761	-0.501	-0.190	p>0.10
	(-1.17)	(-0.82)	(-0.57)	
Female	-0.049	0.000	0.020	
	(-1.28)	(0.00)	(0.43)	
Tenure	0.002	0.035	0.003	
	(0.88)	(1.30)	(1.31)	
Intercept	-0.312***	-0.473***	-0.458***	
	(-6.12)	(-3.28)	(-6.73)	
Department FE	YES	YES	YES	
N	129	25	104	
R^2	0.630	0.697	0.603	

Notes: **Panel A** reports the results of a cross-sectional test estimating the relation between the *InterventionEffect* (i.e., the difference between *Intervention* level and *Baseline* level of performance) and the persistence of the effects of the intervention (defined as the difference between *PostIntervention* level and *Baseline* level of performance) beyond the removal of the incentive. In **Panel B**, I control for the sign of *InterventionEffect*. I repeat the estimation by augmenting the model with the inclusion of the indicator variable (*NegIntEff*) equal to one if the worker level performance difference between *Intervention* and *Baseline* periods is negative, and its interaction with the *IntervetionEffect* with the indicator variable *NegIntEff*. In both panels the unit of analysis is the individual worker. The sample for this analysis is restricted to workers who were observed in all three periods: *Baseline*, *Intervention*, and at least one quarter in the *PostIntervention* period. Column (1) reports the estimation results related to the pooled sample, column (2) reports the results related to the bonus-ineligible workers, and column (3) reports the coefficients estimated on the bonus-eligible workers' subsample. Column (4) reports the significance of Chow tests comparing the magnitude of the coefficients estimated for the main variables of interests between bonus-ineligible and bonus-eligible workers. When the group is restricted to the bonus-ineligible workers, the variable *Female* is dropped from the estimation due to collinearity. All estimations are performed using OLS regressions with heteroskedasticity-robust standard errors, include department fixed effects, and report t-statistics in parentheses underneath each coefficient. Statistical significance is indicated as follows: * = p<0.10; ** = p<0.05; ***= p<0.01. All variables are defined in Appendix 1.

Table 8: Moderating Effect of the Cost of Effort on the Intervention and Persistence Effects

Panel A: Univariate Analyses: Comparison of Mean Hand Hygiene Compliance Levels in Each Period between Sterile and Non-Sterile Departments

Period	N	(*Sterile* – *NonSterile*)
Baseline	166	-0.101 ***
Intervention	234	-0.082 ***
PostIntervention	424	-0.115 ***

Panel B: Comparison of the Effectiveness of the Intervention in Sterile and Non-Sterile Departments

DV = *HHPerf*	Fractional Response Model			Average Partial Effects			Chow Test
	Pooled Sample (1)	Bonus-Ineligible (2)	Bonus-Eligible (3)	Pooled Sample (4)	Bonus-Ineligible (5)	Bonus-Eligible (6)	(Ineligible – Eligible) (7)
Intervention	0.623 (1.12)	4.533*** (2.67)	-0.025 (-0.05)	0.045 (1.10)	0.689*** (2.65)	-0.001 (-0.05)	$p<0.01$
SterileDept	-0.716 (-1.40)	2.655** (2.02)	-1.167** (-2.31)	-0.052 (-1.41)	0.403** (2.06)	-0.052** (-2.35)	$p<0.01$
SterileDept Intervention*	-0.078 (-0.13)	-3.895** (-2.24)	0.453 (0.81)	-0.006 (-0.13)	-0.592** (-2.23)	0.020 (0.81)	$p<0.05$
Female	1.182*** (3.61)	0.000 (0.00)	0.487 (1.04)	0.085*** (3.12)	0.000 (0.00)	0.022 (0.98)	
Tenure	0.006 (0.25)	-0.119 (-0.47)	-0.006 (-0.26)	0.000 (0.25)	-0.018 (-0.47)	-0.000 (-0.26)	
Intercept	2.022*** (3.29)	-0.861 (-0.80)	3.229*** (4.37)				
Department FE	NO	NO	NO	NO	NO	NO	NO
N	282	62	220	282	62	220	
R^2	0.107	0.001	0.092				

Panel C: Persistence of the Intervention Effects in Sterile and Non-Sterile Departments

DV = *HHPerf*	Fractional Response Model			Average Partial Effects			Chow Test
	Pooled Sample (1)	Bonus-Ineligible (2)	Bonus-Eligible (3)	Pooled Sample (4)	Bonus-Ineligible (5)	Bonus-Eligible (6)	(Ineligible – Eligible) (7)
PostIntervention	1.201***	1.328	1.191**	0.084***	0.189	0.061**	p>0.10
	(2.94)	(1.63)	(2.34)	(2.66)	(1.54)	(2.09)	
SterileDept	-0.634	0.802	-0.660	-0.044	0.114	-0.034	p>0.10
	(-1.50)	(0.69)	(-1.42)	(-1.51)	(0.69)	(-1.44)	
SterileDept PostIntervention*	-0.860*	-0.421	-1.338**	-0.060*	-0.060	-0.069**	p>0.10
	(-1.83)	(-0.48)	(-2.25)	(-1.70)	(-0.47)	(-1.98)	
Female	0.887***	0.000	0.701**	0.062***	0.000	0.036**	
	(3.04)	(0.00)	(2.04)	(2.87)	(0.00)	(2.05)	
Tenure	0.045	-0.075	0.036	0.003	-0.011	0.002	
	(1.62)	(-0.31)	(1.45)	(1.57)	(-0.31)	(1.38)	
Intercept	1.699***	0.700	2.172***				
	(3.31)	(0.61)	(4.14)				
Department FE	NO	NO	NO	NO	NO	NO	
N	414	79	335	414	79	335	
R^2	0.106	0.010	0.102				

Notes: **Panel A** reports the results of univariate tests comparing the mean performance in sterile and non-sterile departments across the entire sample. **Panel B** reports the estimation of the moderating effect of the cost of performing correct handwashing (proxied by the variable *SterileDept*) on the effectiveness of the intervention. The sample for this estimation includes workers that were observed in both the *Baseline* period and the *Intervention* period. **Panel C** reports the estimation the moderating effect of the cost of handwashing compliance on the persistence of the effects of the intervention beyond its removal. The sample for this estimation includes workers that were observed in both the *Baseline* period and at least one of the quarters in the *PostIntervention* period. In panels B and C, Column (1) relates to the pooled sample, column (2) bonus-ineligible workers, and column (3) to the bonus-eligible ones. Because the variable *SterileDept* is defined at the department level and the unit of analysis is the worker, I do not include department fixed effects in these estimations. In all cases, standard errors are clustered by individual worker, and t-statistics are reported in parentheses underneath each coefficient. When the group is restricted to the ineligible workers, the variable *Female* is dropped from the estimation due to collinearity. R^2 is calculated for each estimation as the square value of the correlation between the dependent variable's values predicted by the equation being estimated and the observed values. Statistical significance is reported as follows: * = p<0.10; ** = p<0.05; ***= p<0.01. All variables are defined in Appendix 1.