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How Controlling Failure Perceptions Affects Performance: Evidence from a Field Experiment

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ABSTRACT: We conducted a clustered randomized field experiment with 20 Brazilian distributorships of a multi-national direct sales organization to examine whether controlling failure perceptions through formal communications increases performance. We used the organization's weekly sales meetings to deliver a video-based message from the regional head that either communicates workers should view failure as a natural part of learning rather than an indictment of their ability (treatment condition) or simply summarizes the organization's history (control condition). We find that those who were assigned to the treatment condition were more likely to sustain their effort in response to economic adversity that coincided with our experiment. Additional analyses suggest that our treatment accomplished this by increasing job-specific confidence, and by reinforcing social norms that encourage workers to persevere after failure. Overall, our findings highlight that formal communications from senior management are a viable control mechanism for sustaining effort in the face of failure.

I. INTRODUCTION

Failure in the workplace is ubiquitous and can harm future performance by making workers less confident in their ability to perform sufficiently well to attain rewards (Lee, Locke, and Phan 1997). This occurs because workers with lower job-specific confidence set easier goals, rely on lower-quality problem-solving strategies, experience more physiological stress, or reduce their effort (Bandura 1986, 1991, 1997).¹ Moreover, failure can set in motion a negative performance cycle where negative expectations encourage self-destructive actions that reduce the likelihood of success (Vroom 1964; Baumeister and Scher 1988; Eden 1990). Such behavior is especially problematic when failure is not necessarily indicative of a worker's ability to succeed in future periods. Thus, it is important for organizations to control how workers interpret and respond to failure (Merchant 1985; Rockness and Shields 1988; Abernethy and Stoelwinder 1995). In this study, we examine one potential method for accomplishing this – formal communications from senior management.

We define failure as a significant negative performance outcome for which workers can feel at least partially responsible. For most organizational tasks, there is significant ambiguity in the causes and implications of failure even when uncontrollable events have clearly contributed to this outcome (Gibbs, Merchant, Van der Stede, and Vargus 2004). For example, a sales consultant who was unable to close a sale might have sensed that the client was wavering about the purchase due to a downturn in the economy, but how much that mattered and what the sales consultant could have done instead is ultimately speculation. Moreover, the desirability of a given response to failure, such as quitting or doubling-down and increasing effort, depends on subjective evaluations

¹ Confidence is also studied using the term “self-efficacy,” but this tends to be used to represent a kind of global self-confidence. While self-efficacy and confidence use similar psychological and motivational mechanisms, to avoid confusion and bound the particular type of confidence that is focal to the study we use “job-specific confidence”.

of the social appropriateness of this response (Abrams, Ando, and Hinkle 1998; Fischer and Huddart 2008). As a result, not only the perceived causes and implications of failure but also the perceived desirability of responses to failure are subjective and can be shaped by formal communications.

Drawing on theory from social psychology and management about how the organizational environment shapes behavior (Weick 1995; Gioia and Thomas 1996; Bamberger 2008), we hypothesize that senior managers can reduce the adverse performance consequences of failure by communicating that workers should view failure as a natural part of learning rather than an indictment of their ability. We argue that such communication increases effort directly by making failure seemingly less diagnostic of the relation between effort and performance (Gist and Mitchell 1992; Moore and Small 2007) and indirectly by reinforcing social norms that increase the perceived desirability of persevering after failure (Cialdini, Kallgren, and Reno 1991; Messick 1999; Fischer and Huddart 2008; Tayler and Bloomfield 2011).

To test our hypothesis, we conducted a field experiment in which we randomly assigned a video-based message to 20 Brazilian distributorships of a multi-national direct sales organization. This video message was delivered by the regional head and was shown at four routine weekly sales meetings in August 2016, which distributorships use to educate workers about new products and marketing campaigns, celebrate success, and strengthen commitment to the organization. Distributorships that were assigned to the treatment condition screened a video that communicated workers should view failure as a natural part of learning rather than an indictment of their ability; those assigned to the control condition screened an equally up-beat informational video that summarized the organization's proud history without mentioning any notion of failure.

The findings that we report in this study are based on field interviews, surveys, and weekly sales commission data from the 17 weeks before and after the treatment period. We conducted two surveys to track how our predictor variables changed during the experiment. The first survey was completed before the video was screened at the first sales meeting, and the second survey was completed after the video was screened at the fourth sales meeting. Further, to be included in our sample, we required workers to have completed at least one of the surveys, to be working for the organization at the beginning of the sample period, and to have earned sales commissions during at least one week of the sample period (28,832 worker-week observations).

An important aspect of our research setting is that workers were likely to experience failure because we conducted our experiment at a time when several political scandals caused an economic downturn in Brazil's economy that made it difficult for workers to achieve their personal sales goals. Thus, while we designed our experiment with the goal of examining how workers respond to failure in general, our empirical findings mostly apply to failure that is driven by external shocks.

The empirical analysis proceeds in two steps. In the first step, we examine whether our treatment mitigated the negative impact of Brazil's deteriorating economy on sales commissions. In accordance with this economic downturn making it more difficult for workers to achieve their personal sales goals, we find that average sales commissions decreased from the pre- to post-treatment period for both the treatment and control group. However, using a difference-in-differences design that controls for worker and week fixed effects, we find that sales commissions of workers assigned to the treatment condition are approximately 14% or \$55 per month higher

than those of workers assigned to the control condition.² This difference is meaningfully important given that the monthly wage for similar work in Brazil ranges from \$292 to \$392 (Wage Indicator 2019).

Additional analyses find that this result is primarily driven by an increase in the incidence of weeks with nonzero sales commissions, rather than the magnitude of weekly sales commissions. Because field interviews indicate that workers generally earn sales commissions in weeks they perform work, these results suggest that our treatment encouraged workers to devote more effort to their job, but did not significantly increase the productivity of their effort. Importantly, inconsistent with our treatment having short-lasting effects, the economic and statistical significance of the treatment effect gradually increases over time. These additional results indirectly support our theorized mechanism because our treatment should not directly increase the productivity of effort and because social norms tend to develop slowly over time as members of a group become aware of the group's support for a given behavior (Feldman 1984; Bettenhausen and Murnighan 1985; Chatman and Flynn 2001).³

In the second step, we perform various tests that provide direct support for our theoretical mechanism. These tests show that our treatment increased job-specific confidence, but not general life attitudes, and that increased job-specific confidence in turn increased performance. In addition, they show that the treatment effect is less pronounced when it is more difficult for workers to attribute their failures to external forces rather than to their own shortcomings and when workers are less emotionally reactive to failure. Furthermore, they show that the treatment effect is more

² Various analyses that we report in this study confirm that workers assigned to the control condition are a valid counterfactual (i.e., comparable sample) for workers assigned to the treatment condition.

³ Our treatment indirectly increases the productivity of effort only to the extent that sustained motivation increases the rate of learning in our setting (Zollo and Winter 2002; Arthur and Huntley 2005).

pronounced when workers are more frequently exposed to the treatment message and when this message is more likely to result in the development of social norms.

This study contributes to the accounting literature in several ways. Our primary contribution is the discovery that formal communications from senior management are a viable control mechanism for sustaining effort in the face of failure. This discovery extends prior literature on achievable and flexible performance targets (Merchant and Manzoni 1989; Sprinkle, Williamson, and Upton 2008; Bol, Keune, Matsumura, and Shin 2010; Webb, Williamson, and Zhang 2013; Arnold and Artz 2015) by showing that organizations can increase worker productivity by managing how workers interpret and respond to failure rather than by preventing failure from occurring. It also compliments existing literature on the motivational effects of performance measurement noise. While this literature stresses the importance of filtering out the effects of adverse uncontrollable events from evaluations (Gibbs et al. 2004; Bol and Smith 2011; Maas, Van Rinsum, and Towry 2012; Bol and Lill 2015), we show that it is important to reduce ambiguity about the future performance implications of these events through formal communications from senior management.

Second, our study contributes to the literature on how social controls encourage workers to take goal congruent actions. Most research in this area assumes that social controls are difficult to influence by senior management, very broad, subtle, and slow to change (Hopwood 1974; Langfield-Smith and Smith 2003; Malmi and Brown 2008; Merchant and Van der Stede 2012). In addition, the studies that have examined how senior management can influence social controls have focused on monitoring and incentive pay (Coletti, Sedatole, and Towry 2005; Tayler and Bloomfield 2011; Chen and Sandino 2012; Maas and Van Rinsum 2013; Cardinaels and Yin 2015).

Our findings suggest that senior managers can use formal communications to relatively quickly modify the perceived appropriateness of a specific type of behavior – persevering after failure.

Finally, we believe that we identified an organizational practice that may be useful for explaining variation in performance dimensions that we do not explore in our study but are of interest to academic accountants. For example, communicating that workers should view failure as a natural part of learning rather than an indictment of their ability may discourage them from manipulating performance and budget reports (Schweitzer, Ordóñez, and Douma 2004; Indjejikian and Matějka 2006; Church, Hannan, and Kuang 2012; Maas and Van Rinsum 2013; Brown, Fisher, Sooy, and Sprinkle 2014; Indjejikian, Matějka, and Schloetzer 2014; Bol and Lill 2015) and encourage them to engage in beneficial forms of risk-taking, such as outside-the-box thinking (Webb et al. 2013).

The remainder of the paper proceeds as follows. In the next section, we develop our research hypotheses. Section 3 discusses the research setting while Section 4 describes how we select our sample and measure our variables. Section 5 presents our empirical results and the final section concludes and suggests directions for future studies.

II. HYPOTHESIS DEVELOPMENT

Background on Failure Perceptions and Performance

The belief that one can successfully perform a job, which we label job-specific confidence, affects performance through several motivational, cognitive, and affective mechanisms (Bandura 1986, 1991, 1997). Specifically, individuals who believe that they are able to successfully perform a job set more challenging goals, rely more on high-quality problem-solving strategies, experience

less physiological stress in the presence of challenges, and are more likely to heighten and sustain effort in the face of failure.

Bandura suggests that job-specific confidence can be thought of as an overall subjective evaluation of performance capability, which is in part, shaped by the integration of both positive and negative experiences. Negative experiences, however, are likely to have more pronounced and persistent effects than positive experiences because negative information generally receives more attention and thorough processing than positive information (Kahneman and Tversky 1982, 1984; Baumeister, Bratslavsky, Finkenauer, and Vohs 2001; Taylor 1991). Accordingly, we focus on those significant negative performance outcomes that workers can feel at least partially responsible for, and label these outcomes as failures.

Workers reassess their capabilities based on their interpretation of the causes and implications of failure. Specifically, while attributing failure to a lack of skill decreases job-specific confidence, attributing failure to external causes, such as an uncontrollable event, does not affect job-specific confidence. Because failure can reduce job-specific confidence, it can set in motion a negative performance cycle where negative expectations encourage self-destructive actions that reduce the likelihood of success (Vroom 1964; Baumeister and Scher 1988; Eden 1990). For example, workers who do not believe that they can successfully complete a task may reduce their effort in order to be able to blame failure on low effort rather than low ability.

Assessments of job-specific confidence are, however, inherently subjective because for most organizational processes the causes and implications of failure are ambiguous even when uncontrollable events have clearly contributed to this outcome (Gibbs et al. 2004). Moreover, not only the perceived causes and implications of failure but also the perceived desirability of responses to failure are subjective. This is the case because the perceived appropriateness of

actions depends on perceptions of peer behavior and values (Abrams et al. 1998; Fischer and Huddart 2008). As a result, we expect workers to make sense of failure using cues from the organizational environment.

Organizational Environment

Prior research indicates that the organizational environment plays an important role in shaping behavior by providing the context within which information is interpreted (Bamberger 2008). Although anyone could help workers make sense of failure (Maitlis and Lawrence 2007), we expect top management to be most influential because they are uniquely positioned to provide the most credible information about not only the causes and implications of failure but also the most appropriate response to failure (Mullen 1994, 1998; Moore and Small 2007). While prior research has focused on the pivotal role top management plays in shaping workers' interpretations of organizational level failures (Maitlis and Lawrence 2007; Sonenshein 2010), we focus on how top management can use formal communication to control how workers interpret and respond to their own failures.

Job-Specific Confidence and Social Norms

We propose that senior managers can reduce the adverse performance consequences of failures that are driven by uncontrollable events by communicating that workers should view failure as a natural part of learning rather than an indictment of their ability. We expect that such communication helps workers regain and preserve their job-specific confidence, and consequently motivation to devote effort to a task, by making failure appear less diagnostic of the relationship between effort and performance (Gist and Mitchell 1992). That is, if the organization expects that some failure is a normal part of learning, workers should reason that failure does not preclude future success (Moore and Small 2007).

In addition, we expect these communications to reinforce social norms that encourage workers to persevere after failure. Social norms must be passed on to have an effect on behavior (Cialdini and Trost 1998). This often occurs passively through observing, interpreting, and copying the verbal and nonverbal behaviors of others (Langfield-Smith and Smith 2003; Malmi and Brown 2008; Merchant and Van der Stede 2012). It can, however, also be transmitted deliberately through active instruction (Lumsden 1988; Allison 1992). Consequently, we expect communicating that workers should view failure as a natural part of learning rather than an indictment of their ability to alter norms regarding appropriate responses to failure. We expect this to increase worker effort in two ways. First, it increases social pressure to not quit after failure (Abrams et al. 1998).⁴ This increases worker effort because individuals have an innate preference to meet social expectations (Fischer and Huddart 2008). Second, it creates the shared belief that it is safe to not only speak out about failure but also suggest methods for overcoming it without suffering interpersonal costs such as embarrassment (Edmondson 2004; Tjosvold, Yu, and Hui 2004; Edmondson and Lei 2014). In turn, this encourages greater worker effort by not only making failure less unpleasant (Avey, Luthans, and Jensen 2009) but also by giving workers hope that they can succeed (Tugade and Fredrickson 2004).

We formulate our predictions regarding the performance consequences of formal communications from senior management as follows:

- H:** Senior managers can reduce the adverse performance consequences of failure by communicating that workers should view failure as a natural part of learning rather than an indictment of their ability.

⁴ In general, control mechanisms can alter people's tendency to comply with social norms by activating either self-interested or socially interested norms (Tayler and Bloomfield 2011). It is, however, unlikely that this occurs in our setting because workers are not rewarded for complying with formal communications.

Before concluding this section, we clarify why we believe that our hypothesis is an empirical question that warrants study. First, communicating that failure is a normal part of learning may actually harm performance by reducing the degree to which workers believe they have control over the expected outcome (Vroom 1964; Merchant and Manzoni 1989; Arnold and Artz 2015). Second, social persuasion, such as praising workers' job credentials, tends to have short-lived effects (Bandura 1977), and is more effective when workers already believe themselves capable of performing a task (Bandura 1997; McNatt and Judge 2004). Third, individuals at any level of an organization, and even those outside its boundaries, help workers make sense of failure (Maitlis and Lawrence 2007). All of these individuals compete to shape the meaning of failure, and thus, it may be difficult for top management to change how workers perceive and respond to failure (Mantere, Schildt, and Sillince 2012). This is particularly a challenge when, as in our setting, an organization has cultivated a culture that celebrates success (Strang and Macy 2001). Finally, more confident workers may curtail their effort because they believe that they do not need to put as much effort in to achieve their desired results (Vancouver and Kendall 2006; Vancouver, More, and Yoder 2008).

III. RESEARCH SETTING

Our study is the result of a multi-year collaboration with PSI (a pseudonym), which is a publicly traded multi-national organization that is a leading marketer of design-centric preparation, serving, and storage solutions for the kitchen and home. PSI uses a direct-sales model, in which a self-employed, mostly female, sales force promotes its products through group and individual product demonstrations. In 2016, PSI reported \$2.2 billion in revenue and an independent sales network across six continents.

Independent distributors, who have the right to market PSI's products and use its trademark within a geographic area, process all customer orders, provide distribution and warehousing services, and disseminate materials about sales promotions and new products.⁵ Sales force members can be promoted from sales consultant to manager, and from manager to director, by increasing their individual sales and by recruiting, training, and motivating new sales consultants. These promotions not only provide prestige, but also tangible benefits because sales force members earn commissions on both their individual sales and their recruits' sales.⁶ As is common for direct sales organizations, sales force members are free to choose how much time they devote to their job and, consequently, most work on a part-time basis. Furthermore, most sales force members do not have other extra-domestic work and have a working spouse. Of note, we focus in our study on the commissions sales consultants earn on their personal sales. This sales commission is a fixed percentage of their sales revenues that does not vary across participating distributors during our sample period. Furthermore, for ease of exposition we refer to a distributor's geographic area as *distributorship* and to sales consultants as *workers*.

Importance of Failure Perceptions at Research Site

In the first stage of our study, we interviewed more than 300 workers. These semi-structured interviews lasted typically less than one hour and were mostly conducted in group settings where workers were free to chime in whenever they felt the need to do so. We completed these interviews in two phases. Initially, we asked workers broad questions about their experiences with PSI to identify the factors that affect their job performance. Once it became evident to us that failure perceptions have a profound impact on productivity, we asked more specific questions about

⁵ Sales force members are not required to hold inventories.

⁶ Managers and directors can also earn non-monetary rewards such as cars and vacations.

failure perceptions and job-specific confidence. At this point, we also distributed a survey to collect interview data that is more easily quantifiable.

This survey was completed by 148 individuals who participated in group interviews. Their responses suggest that workers typically join PSI because of their love for its products (55 percent), financial needs (55 percent), and desire for personal growth (35 percent). Consistent with our theory, an overwhelming majority of respondents indicated that confidence is crucial for success (67 percent) and that they remind themselves at least once a month to remain confident after difficult situations (78 percent). The latter in turn increases effort as it is associated with a 12 percent decrease in the incidence of turnover considerations. Importantly, survey responses suggest that managers typically (67 percent) motivate workers after a bad sales period by framing this negative performance outcome as a learning opportunity.⁷ Responses from workers suggest, however, that senior management had until the time of our field experiment not made an effort to systematically communicate this to workers. That is, only 23 percent of respondents indicated that they preserve their productive effort after failure by viewing it as a learning opportunity. Accordingly, the goal of our field experiment is to design an intervention that helps senior management control how workers interpret and respond to failure through formal communications.

The Field Experiment

We conducted our field experiment in Brazil because our multi-year collaboration with PSI has given us the opportunity to become familiar with this market and develop a close working relationship with local headquarters staff. PSI's local headquarters in São Paulo has been in

⁷ Less common motivational approaches include reminding workers of their financial and personal growth objectives (23 percent) and encouraging workers to attend sales meetings (12 percent).

operations for 40 years and oversees a network of approximately 60 distributorships that covers the majority of Brazil.

Our treatment is a video-based message that communicates workers should view failure as a natural part of learning rather than an indictment of their ability. The video was developed in close collaboration with local headquarters staff to ensure that our message would resonate with the sales force and was delivered in Portuguese. The following is the script's key passage, which was translated by a PSI employee who was not involved in our field experiment⁸:

“Don't be afraid of failure. Setbacks and failures are part of the journey of all of us, but when we make mistakes trying it should only inspire us to think more creatively the next time. Talk to others, this can open our eyes to a new way of thinking. So next time you think "what will I do now?!" Remember, don't be afraid of failure; setbacks are normal, and you are not alone.”

We recruited the President of PSI Brazil, the highest-ranking executive of the local headquarters staff, to emphasize that our message reflects how the organization views failure. She delivered this message, via video, at weekly sales meetings organized by managers and directors to educate sales consultants about new products and marketing campaigns, celebrate successes, and strengthen commitment to the organization. We attended several dozen sales meetings as part of our field research and observed that they are upbeat events (often with singing and dancing), last 60 to 90 minutes, have at least 50 participants, and are held at convenient locations, such as product warehouses, private residences, and community centers.⁹ We considered other approaches for delivering our message, including an existing PSI web portal and text messages via WhatsApp. We ultimately decided to use sales meetings in order to have more control over how our message

⁸ See Appendix A for translated versions of the full treatment and control scripts. The untranslated versions of the scripts are available upon request.

⁹ Sales force members routinely invite potential new recruits to sales meetings, and thus, not all meeting participants work for PSI.

was delivered and to avoid technology that may not be widely and uniformly available to PSI's diverse sales force in Brazil.

We used a sampling approach with a clustered assignment mechanism. Specifically, 20 randomly selected distributorships were equally divided into two groups by balancing the following characteristics: annual sales, annual sales growth, sales force size, annual sales force growth, and geographic location. Subsequently, one group was randomly assigned to the treatment condition and the other group to the control condition. Distributorships that were assigned to the treatment condition screened the video-based message that communicates workers should view failure as a natural part of learning rather than an indictment of their ability. In contrast, distributorships that were assigned to the control condition screened an equally positive and informative video-based message (again from the President of PSI Brazil) that summarized the organization's proud history and did not mention the notion of failure.¹⁰ We used this control condition to ensure that the screening of a video-based message from top management does not confound our findings.

Importantly, we attempted to eliminate any intentional and unintentional biases by ensuring that we and the distributorships did not know which experimental condition they were assigned to. Specifically, the above mentioned sampling approach was performed by a lower-level employee who was employed at PSI's local headquarters in São Paulo, does not routinely travel to distributorships, and did not know the objectives and treatment intervention of our study.¹¹ In

¹⁰ Our reading of open-ended survey responses confirms that our treatment and control videos are equally positive and informative.

¹¹ After the conclusion of our field experiment, we independently verified that at the start of our experiment the treatment and control distributorships did not significantly differ with respect to annual sales, annual sales growth, sales force size, annual sales force growth, and geographic location. In addition, even though this was not one of the selection criteria, we found that they also did not differ with respect to sales per sales force member. We do find, however, that the 20 participating distributorships are on average larger, with respect to both annual sales and sales

addition, distributors were not made aware of the objectives of the field experiment and were only given enough information to help us conduct our experiment. To accomplish this, we developed a set of instructions that informed sales meeting leaders (i.e., managers and directors) of the 20 participating distributorships how to conduct the experiment.¹² These instructions were in Portuguese and stored on thumb drives that were handed out to distributors at individualized training sessions hosted by PSI's local headquarters in São Paulo.

Conducting the Field Experiment

The experiment was conducted over a four-week period during weekly sales meetings in August 2016. At the beginning of the first meeting, meeting leaders showed attendees a video that briefly explained that PSI was studying how empowerment affects them, and that they were requested to complete a survey to help PSI conduct this research.¹³ Subsequently, meeting leaders distributed a survey designed to collect baseline data on our predictor variables. After workers completed this survey, meeting leaders collected the surveys and sealed them in an envelope. Meeting leaders then completed their typical meeting activities.

Towards the end of each sales meeting during our four-week experiment, meeting leaders screened either the treatment or control video depending on whether their distributorship was assigned to the treatment or control condition. Of note, at no time were meeting attendees made aware that the treatment and control videos were part of the experiment. Moreover, the generally large geographic distance between distributors significantly reduces the risk of interference

force size, than the 40 distributorships that were not selected for our experiment. We have no reason to believe that this biases us towards finding results.

¹² The instructions are available upon request.

¹³ This video-based message was not delivered by the President of PSI Brazil. All distributorships, irrespective of treatment condition, viewed the same video.

between treatment and control conditions.¹⁴ At the end of these videos, workers were given a questionnaire with open-ended questions that was designed to encourage them to reflect on the video-based message they were shown. Similarly, meeting leaders also collected the questionnaires and sealed them in an envelope. We performed this procedure four times in order to ensure that meeting attendees assigned to the treatment condition internalized the video-based message about how PSI views failure (Zajonc 1968).¹⁵

Responses to the self-reflection questionnaire from workers who viewed the treatment video are consistent with these workers learning something new about how PSI views failure. For example, one worker noted: “this message touched me a lot because sometimes when you fail to do something you become ashamed and you don’t want to tell people, so this time I learn if you fail tell to the others.” Importantly, our treatment altered failure perceptions by externalizing failure. As one worker noted: “business is not always good for you. Sometimes it’s good, sometimes bad.” Furthermore, consistent with these altered failure perceptions increasing performance, responses suggest that workers felt less apprehensive about devoting effort to their job after failure. For example, one worker noted that “failure is normal but how you react to it is what will determine your destiny.” Of note, responses from workers who viewed the control video are unrelated to failure perceptions.¹⁶

At the end of the last sales meeting, meeting leaders showed a video thanking the attendees for their participation in the research project.¹⁷ The meeting leader then distributed the same survey

¹⁴ The distance between distributors that were assigned to the treatment condition and distributors that were assigned to the control condition is on average 1,900 kilometers and is at least 100 kilometers.

¹⁵ Workers who completed the post-experiment survey typically viewed our video-based messages only three times because meeting attendance is voluntary.

¹⁶ We only perform a qualitative analysis of the self-reflection questionnaires because the majority of responses are incomplete or missing.

¹⁷ This video-based message was not delivered by the President of PSI Brazil. All distributorships, irrespective of treatment condition, viewed the same video.

that we distributed at the beginning of our experiment in order for us to track how our predictor variables changed during the experiment. After attendees completed this post-experiment survey, meeting leaders collected their responses and sealed them in an envelope. We ensured that all surveys and questionnaires were mailed in a timely fashion to PSI's local headquarters.¹⁸

After completion of the field experiment, a third-party firm in São Paulo processed all surveys and questionnaires. We did not inform this firm of our study's research objective in order to reduce bias, and we randomly audited the data to reduce errors. The firm took roughly four weeks to process the data. Because meeting attendees recorded their unique sales force identification numbers on surveys and questionnaires, we were able to obtain their weekly sales commission data and personnel records from PSI Brazil's external database management firm. We discuss our sample and variable definitions in the next section.

IV. SAMPLE AND DESCRIPTIVE STATISTICS

To be included in our sample, workers must have completed either the baseline survey or the post-experiment survey.¹⁹ We impose this requirement in order to ensure that all included workers participated in our experiment.²⁰ A total of 1,265 respondents were entered into our survey database. We used personnel records to determine these respondents' age, gender, tenure, and weekly performance. For our main analysis, we eliminated 417 respondents who did not work for PSI at the beginning of our sample period or did not earn any sales commissions during the 34-week window around our treatment period (see below for further details on this restriction). We

¹⁸ We were informed whenever materials were received.

¹⁹ Workers who filled out the post-experiment survey do not significantly differ from those who chose not to do so with respect to their age, gender, tenure, and their propensity to be assigned to the treatment condition.

²⁰ We were unable to obtain attendee lists from the distributors.

impose the latter requirement to ensure that subjects were actively working during at least some part of the sample period. As a result, our main sample consists of 848 unique workers of which 320 were assigned to the treatment condition and 528 were assigned to the control condition.

Panel A of Table 1 provides descriptive statistics on these workers, where *Treatment* is an indicator variable that equals one for workers who were assigned to the treatment condition, and zero otherwise. The panel shows that the demographic and work-related characteristics of workers are fairly well balanced between treatment and control conditions with workers assigned to the treatment condition having only slightly longer tenures than those assigned to the control condition^{21, 22}. Thus, it is unlikely that our results are influenced by differences in these characteristics across the treatment conditions.

In order to examine how formal communications from senior management about failure affect worker performance, we obtain weekly sales commission data from the 17 weeks before and after the four-week treatment period.²³ This results in a sample of 28,832 worker-week observations of which 10,880 are from the treatment condition and 17,952 are from the control condition. The average weekly sales commission is 357 Brazilian Real (\$98) with a standard deviation of 514 Brazilian Real (\$140). Given that the prevailing wage for similar work in Brazil ranges from \$292 to \$392 per month or \$67 to \$90 per week (Wage Indicator 2019), these descriptive statistics suggest that sales commissions are of meaningful economic importance to workers. Our performance measure equals the natural logarithmic transformation of one plus

²¹ Further investigation of this difference shows that it is driven by 12 workers with tenures greater than 25 years and that dropping these workers does not change our inferences.

²² An untabulated analysis shows that workers do not significantly differ with respect to their self-reported overall work experience and job-specific confidence at the first sales meeting. We do not tabulate the results for these characteristics because they are missing for many included workers.

²³ We use a 34-week window around the treatment period because PSI's fiscal year ends 17 weeks after the treatment period and we were unable to obtain sales data for the following fiscal year.

weekly sales commissions (*Performance*). We use a logarithmic transformation to ensure that our results are not driven by extreme values and increase weekly sales commissions by one because many sales force members work for PSI on a part-time basis and, consequently, do not earn sales commissions every week (approximately 53% of worker-week observations).

[insert Table 1 about here]

Figure 1 presents the trend in performance for the 34-week window around our treatment period separately for workers who viewed either the treatment or control video. For the purpose of this figure we de-mean performance at the worker level to remove worker fixed effects and average the resulting values by month. Of note, we use PSI's 13-month calendar, which has 4 weeks in the first 12 months and 5 weeks in the last month of 2016.^{24,25}

In accordance with Brazil's deteriorating economy making it more difficult for workers to attain their personal sales goals, the figure shows that average performance declines from the pre- to post-treatment period for both the treatment and control group. In addition, the figure shows a temporary reversal in this trend towards the end of the pre-treatment period that is driven by a temporary reprieve in political instability and increased spending fueled by the 2016 Brazil Summer Olympics (Imbert 2016; Watts 2016; McBride 2018).²⁶ Importantly, consistent with our treatment helping workers cope with failure, the overall decline in performance from the pre- to

²⁴ Month 4 consists of one week in Figure 1 because we have only 17 weeks of pre-treatment period sales commission data.

²⁵ Our inferences are unchanged when we use monthly sales commissions instead of weekly sales commissions as our performance measure. We focus on weekly sales commissions because this helps us determine whether our treatment increases performance by increasing the level and/or productivity of worker effort.

²⁶ On May 12th of 2016 lawmakers decided to go ahead with impeachment proceedings against former Brazilian President Dilma Rousseff and appointed Vice President Michel Temer as interim president who was viewed very favorably by financial markets and businesses (Bevins 2016).

post-treatment period appears to be less pronounced for the treatment group than for the control group.

[insert Figure 1 about here]

Panel B of Table 1 formalizes this visual analysis and presents the results of a univariate test that compares the performance decline from the pre- to post-treatment period across treatment conditions. The panel documents that while workers from both treatment conditions performed significantly worse during the post-treatment period, those assigned to the treatment condition experienced a less pronounced performance decline. This difference is statistically significant at $p < 1\%$ (two-tailed). Overall, then, the descriptive evidence presented in this section is consistent with formal communications about failure helping workers cope with failure.

V. HYPOTHESIS TESTS

Our hypothesis predicts that controlling failure perceptions through formal communications from senior management increases worker performance. Our analysis of this prediction is as follows. We begin by estimating a difference-in-differences model of the following form:

$$Performance_{it} = \alpha + \beta Treatment_i \times Post_t + \delta_i + \lambda_t + \varepsilon_{it} \quad (1)$$

where *Performance* equals our performance measure for worker i in week t , α is a constant term, $Treatment_i \times Post_t$ is an interaction of our treatment indicator ($Treatment_i$) and our post-treatment period indicator ($Post_t$), and δ_i and λ_t are worker and week fixed effects, respectively. In this model, the performance effect of our treatment is identified by β , which measures the change in performance for workers who viewed the treatment video relative to workers who viewed the control video. Of note, we do not include the main effects of our treatment and post-treatment

indicator variables because they are absorbed by worker and week fixed effects. Finally, we use distributorship-clustered bootstrap standard errors to control for residual dependence within our sampling units (Kline and Santos 2012).^{27, 28}

Column (1) of Table 2 presents the results of estimating model (1) using ordinary least squares (OLS). Consistent with Hypothesis 1, the column shows that the coefficient on $Treatment_i \times Post_t$ is positive and statistically significant ($p < 5\%$, two-tailed).²⁹ This OLS parameter estimate may, however, be biased and inconsistent due to the high percentage of worker-week observations with zero sales commissions (Cameron and Trivedi 2010). We address this issue by using a two-part model (Dow and Norton 2003) where the first part models the probability of earning sales commissions and the second part models the magnitude of earned sales commissions. This approach is appropriate for our setting because field interviews suggest that workers earn at least some sales commissions in weeks they perform work for PSI. In other words, earning zero sales commissions does not arise from a standard corner solution for which a Tobit model is appropriate but instead represents a separate discrete choice – the decision to work or not work for PSI.³⁰ Furthermore, Heckman models are both inappropriate and impractical in our setting because we are modeling actual outcomes, as opposed to potential outcomes, and the random assignment of the treatment condition makes it impossible for us to identify instrumental variables (Dow and Norton 2003).³¹

²⁷ We use the bootstrap method proposed by Kline and Santos (2012) because it corrects standard errors for the small number of clusters (20 distributorships) and is appropriate for both linear and non-linear models.

²⁸ Our inferences are unchanged when we repeat our main analysis after collapsing the time series of sales commissions into a pre- and post-period (Bertrand, Duflo, and Mullainathan 2004). Thus, it is unlikely that our results are driven by residual dependence within our sampling units.

²⁹ We use the percentile bootstrap approach to compute p -values.

³⁰ Our empirical results confirm that zero and positive sales commissions are not driven by the same underlying mechanism, and thus, that Tobit models are inappropriate (Cameron and Trivedi 2005).

³¹ We find similar results when we perform the Heckman two-stage approach without instrumental variables.

Columns (2) and (3) of the table present the results of our two-part model. To facilitate the interpretation of interaction effects in non-linear models, we exponentiate coefficient values from our logit model to obtain more easily interpretable odds ratios (Buis 2010).^{32, 33} Consequently, β in column (2) is a multiplicative measure of how the treatment condition modifies the change in the odds of earning sales commissions from the pre- to post-treatment period. Consistent with our treatment encouraging workers to devote more effort to their job, column (2) shows that β is positive and statistically significant at $p < 5\%$ (two-tailed). Because β is statistically insignificant in column (3), however, it appears that our treatment does not affect the magnitude of earned sales commissions in weeks that workers devote effort to their job. Thus, managing failure perceptions through formal communications from senior management increases the level but not productivity of worker effort. Of note, this is consistent with our theorized mechanism given that our treatment should not directly increase the productivity of effort.

Importantly, the coefficient estimate in column (1) is not only statistically but also economically significant. Specifically, it implies that our treatment increased sales commissions by 14% or \$55 per month, which is of significant economic importance to these workers given that the monthly wage for similar work in Brazil ranges from \$292 to \$392 (Wage Indicator 2019). Furthermore, columns (2) and (3) show that this finding results from a 16% increase in the odds of earnings sales commissions. The latter confirms that our treatment affects the performance of workers by encouraging them to devote effort to their job.

[insert Table 2 about here]

³² Interpreting interaction effects in logit models can be difficult and misleading because the marginal effects differ for each observation (Ai and Norton 2003). Because logit models are linear in log-odds, however, exponentiated coefficients represent the constant effect of a variable on the odds of an event.

³³ Our inferences are unchanged when we use a linear probability model instead of a logit model (Wooldridge 2002).

Dynamics of Treatment Effect

Next, we estimate a dynamic model that examines performance differences across treatment conditions in the months prior to and following the treatment period. That is, we interact *Treatment* with indicators that capture the months before, of, and after the treatment period. Our leads and lags model is specified as follows:

$$\begin{aligned} Performance_{it} = & \alpha + \beta_1 Treatment_i \times Month_{t-3} + \beta_2 Treatment_i \times Month_{t-2} \\ & + \beta_3 Treatment_i \times Month_{t-1} + \beta_4 Treatment_i \times Month_{t0} + \beta_5 Treatment_i \times Month_{t+1} \\ & + \beta_6 Treatment_i \times Month_{t+2} + \beta_7 Treatment_i \times Month_{t+3} \\ & + \beta_8 Treatment_i \times Month_{t+4} + \delta_i + \lambda_t + \epsilon_{it} \end{aligned} \quad (2)$$

where *Performance* equals our performance measure for worker *i* in week *t*, α is a constant term, *Treatment_i* is our treatment indicator, *Month_{t+q}* are month indicators that capture how many months until (*t+q*) or since (*t-q*) the treatment month (*Month_{t0}*), *Treatment_i* \times *Month_{t+q}* are interactions of our treatment indicator and month indicators, and δ_i and λ_t are worker and week fixed effects, respectively. Of note, for the purpose of this analysis we add sales commission data from the four-week treatment period and use the first 5 weeks of the pre-treatment period as our reference period.

We perform this analysis for two reasons. First, our difference-in-differences analysis assumes that in the absence of treatment, the average outcome for the treated and the untreated would have followed parallel trends. This assumption is supported by several aspects of our field experiment. Foremost, our 20 randomly selected distributorships were equally divided into two groups by balancing distributorship characteristics that may influence worker performance. In addition, Panel A of Table 1 shows that workers from treatment and control distributorships who participated in our experiment are fairly well balanced with respect to demographic and work-related characteristics that may influence their performance. Finally, Figure 1 indicates that the parallel trends assumption is not at odds with the data. Nevertheless, to provide additional evidence

for our setting not violating the parallel trends assumption, we examine the coefficients on the treatment indicator in the months before the treatment month (Autor 2003; Angrist and Pischke 2008). If the parallel trends assumption is not violated, we expect these coefficients to be insignificantly different from zero.

Second, a concern regarding our findings is that our treatment has only short-lasting effects on job attitudes and performance. Prior research suggests that this may occur because the effects of social persuasion tend to be short-lived (Bandura 1977). Moreover, more confident workers may believe that they do not need to put as much effort in to achieve their desired results (Vancouver and Kendall 2006; Vancouver et al. 2008). Thus, while job-specific confidence encourages workers to perform a job, it may decrease how much effort they devote. If our treatment has short-lasting effects, we expect the magnitude of the coefficients on the treatment indicators to decrease in the months after the treatment month.

Table 3 presents the results of estimating model (2). The table indicates that the parallel trends assumption is consistent with the data as the coefficient on the treatment indicator is not significantly different from zero in the months before the treatment month. This confirms that workers assigned to the control condition are a valid counterfactual for workers assigned to the treatment condition.^{34, 35} In addition, the table shows that the magnitude and statistical significance of the treatment effect gradually increases over time. This pattern is inconsistent with our treatment

³⁴ We also performed a comprehensive set of placebo experiments where we limit our sample to the pre-treatment period and randomly assign a week in this period as being the start of the post-treatment period (Lechner 2011). We find that these placebo treatment effects are never statistically significant in our OLS and two-part model regressions, further supporting the parallel trends assumption.

³⁵ Our inferences are unchanged when we use a matched sample that is balanced in average worker performance for each week of the pre-treatment period (results not tabulated). We obtained this matched sample by matching treatment to control observations using a one-to-one nearest neighbor propensity score matching model with no replacement.

having short-lasting effects. Instead, it is consistent with our treatment facilitating the gradual strengthening of organizational norms that encourage workers to persevere after failure.³⁶

[insert Table 3 about here]

Evidence on Mechanism

Our theory suggests that communicating to workers that they *should view failure as a natural part of learning rather than an indictment of their ability* increases performance not only by helping them regain and preserve their confidence after failure but also by reinforcing organizational norms that encourage workers to persevere after failure. In this section, we examine key elements of this mechanism, including job-specific confidence, internalization of formal communication from senior management, emotional responses to failure, external attribution, and social norm development.

Job-Specific Confidence

We test whether our treatment increased job specific-confidence by comparing job-specific confidence scores from our baseline survey with those from the post-experiment survey. Job-specific confidence scores are based on eight survey questions that are on a five-point Likert scale and range from 1 (strongly disagree) to 5 (strongly agree). Following Bandura (2006) we tailored these questions to activities that are of importance to earning sales commissions: learning new products, explaining products, recruiting new consultants, collecting payments, closing deals, and making random sales calls (see Appendix B for survey questions). To obtain an overall measure of job-specific confidence, we normalized the scores for each question and added them together

³⁶ Our inferences are similar when we use a nonparametric kernel regression that does not require us to specify a functional form between performance and the treatment condition (results not tabulated). Thus, our findings regarding the persistence of the treatment effect are unlikely to result from misspecification error (Härdle 1990).

(for a similar approach, see Lee et al. 1997).³⁷ Scores were normalized using the mean and standard deviation from all surveys with non-missing responses for job-specific confidence questions.³⁸ A total of 160 unique workers had non-missing responses for the job-specific confidence questions from both surveys. Of these workers 64 were assigned to the treatment condition (*Treatment* = 1) and 96 were assigned to the control condition (*Treatment* = 0).

If our treatment helped workers regain and preserve their job-specific confidence, we expect workers who were assigned to the treatment condition to have a less pronounced decrease in their job-specific confidence during our four-week experiment relative to workers who were assigned to the control condition. Column (1) of Table 4 show the results of an analysis that tests this prediction by regressing the change in job-specific confidence scores on our treatment indicator. Consistent with our theory, the column shows that the coefficient on *Treatment* is positive and statistically significant at $p < 5\%$ (two-tailed).

Next, we examine whether our treatment affected other attitudes that could have encouraged workers to devote more effort to their job (Bono, Foldes, Vinson, and Muros 2007). Specifically, we focus on the following two attitudes: the perceived ability to overcome life challenges and optimism. We perform this analysis because it sheds light on how wide-ranging the effects of formal communication about failure may be, and whether altering general life attitudes that are not directly related to work-related failure can have similar performance effects. In order to stimulate engagement and reduce ambiguity and fatigue (Leutner, Yearsley, Codreanu, Borenstein, and Ahmetoglu 2017), we used one survey question per perception and image-based response scales (see Appendix B for survey questions). As in the previous analysis, we normalized each measure

³⁷ We reverse-coded the negatively phrased survey questions.

³⁸ The Chronbach's α of this measure is above 0.6, and thus, suggests that our survey questions reliably measure job-specific confidence (Hair, Black, Babin, Anderson, and Tatham 2006).

using the mean and standard deviation from all surveys with non-missing responses. Subsequently, we repeat the previous analysis using either the change in the perceived ability to overcome life challenges or the change in optimism as our dependent measure.

The results of this analysis are presented in columns (2) and (3). The columns show that our treatment did not affect the perceived ability to overcome life challenges and optimism. Thus, it does not appear that our results are driven by changes in other attitudes that could have encouraged workers to devote more effort to their job.

[Insert Table 4 about here]

Job-Specific Confidence and Performance

To corroborate that our findings are driven by higher job-specific confidence, we examine whether higher job-specific confidence scores are associated with higher sales commissions in the post-treatment period. We accomplish this by estimating the following model:

$$Performance_{it} = \alpha + \beta \Delta Job-Specific\ Confidence_{it} + Z + \delta_i + \lambda_t + \varepsilon_{it} \quad (3)$$

where *Performance* equals our performance measure for worker *i* in week *t*, α is a constant term, $\Delta Job-Specific\ Confidence$ is the change in the job-specific confidence score from the baseline survey to the post-experiment survey, *Z* is a vector of control variables, and δ_i and λ_t are distributorship and week fixed effects, respectively. We control in our analysis for a worker's job-specific confidence score from the baseline survey, age, gender, and tenure with PSI. In this model, β captures the performance effect of helping workers regain and preserve their job-specific confidence after failure.

The sample for this analysis consists of 113 unique workers from the main analysis who completed the job-specific confidence questions from both surveys. Of these workers 48 were

assigned to the treatment condition and 65 were assigned to the control condition. We measure the performance effect of higher job-specific confidence using weekly sales commission data from the post-treatment period. The resulting sample consists of 1,921 worker-week observations. Consistent with our treatment increasing performance by helping workers regain and preserve their job-specific confidence, column (1) of Table 5 shows that β is positive and significant at $p < 5\%$ (two-tailed). Moreover, as in our main analysis, columns (2) and (3) of the table show that this effect is primarily driven by an increase in the odds of earning sales commissions, rather than the magnitude of sales commissions.³⁹

[Insert Table 5 about here]

Alternative Confidence Measure

To further corroborate that our findings are driven by higher job-specific confidence, we repeat the analyses presented in Table 4 and Table 5 using a survey question from the post-experiment survey that asked respondents to indicate the extent to which they agree with the following statement: “Watching the video made me feel more confident.” This question is on a six-point Likert scale and ranges from 1 (strongly disagree) to 6 (strongly agree). As in the previous analyses, we use normalized responses to this question to construct our alternative confidence measure ($\Delta Confidence$). The sample for this analysis consists of 370 unique workers (6,290 worker-week observations) from the main analysis who completed the alternative job-specific confidence question. Of these workers 136 were assigned to the treatment condition and 234 were assigned to the control condition.

³⁹ We find that job-specific confidence is positively associated with sales commissions and the probability of earning sales commissions in the post-treatment period when we include all workers (582) who completed the job-specific confidence questions from at least one of our two surveys (results not tabulated).

The results of this analysis are presented in Table 6. Consistent with our previous results, Panel A of the table shows that watching the treatment video made workers more confident than watching the control video. Importantly, consistent with this resulting in increased performance, Panel B shows that feeling more confident after watching our videos is positively associated with performance in the post-treatment period. Thus, the results in this table corroborate that our treatment increased performance by helping workers regain and preserve their job-specific confidence.

[Insert Table 6 about here]

Internalization of Formal Communication from Senior Management

Our treatment should only work to the extent that workers internalize the video-based message they were assigned to. This internalization may occur gradually over time as workers discuss the treatment video with other workers from the same distributorship. However, it may also occur relatively quickly by watching the treatment video multiple times during the treatment period (Zajonc 1968). To test this and to corroborate that our findings are driven by our treatment, we next examine whether the treatment effect is more pronounced and emerges more quickly among workers who viewed our treatment video multiple times.

The sample for this analysis consists of 265 unique workers from the main analysis who completed a question from the post-experiment survey regarding how many times they watched their assigned video. Of these workers 111 were assigned to the treatment condition and 154 were assigned to the control condition. Consistent with workers internalizing our treatment message only after being repeatedly exposed to it, Panel A of Table 7 shows that the treatment effect is only present among workers who viewed their assigned video multiple times. Moreover, Panel B of the table shows that the treatment effect emerges after two months for these workers compared to four

months for all workers (Table 3). Thus, the results in this table suggest that workers internalize the treatment message only after being repeatedly exposed to it and, importantly, corroborate that our results are driven by our treatment message.

[Insert Table 7 about here]

Emotional Responses to Failure

Thus far, we have implicitly assumed that workers experienced failure during the pre-treatment period. This assumption is plausible given the downturn in Brazil's economy and necessary because we cannot directly identify instances of failure given that PSI does not set explicit performance targets and workers do not routinely communicate their personal sales targets to managers. To corroborate that failure perceptions drive our results, however, we use an indirect measure of the extent to which workers experienced failure during the pre-treatment period. Specifically, we make a distinction between workers who have other work outside of PSI and those who do not. We expect this distinction to capture variation in the extent to which workers experienced failure because prior research suggests that workers whose goal commitment is undermined by the presence of alternative goals are less emotionally reactive to failure (Shah and Kruglanski 2002, 2003).

The sample for this analysis consists of 722 unique workers from the main analysis who completed a question from either the baseline or post-experiment survey regarding whether they have paid work outside of PSI. Of these workers 279 were assigned to the treatment condition and 443 were assigned to the control condition. Only about a third of the workers assigned to each condition reported having paid work outside of PSI. Consistent with our results being driven by altered failure perceptions, Table 8 shows that the treatment effect is only present among workers who do not have paid work outside of PSI.

[Insert Table 8 about here]

External Attribution

Recall that we expect our treatment message to increase productivity by making failure appear less diagnostic of the relationship between effort and performance. If our treatment indeed operates through this mechanism, it should have a less pronounced effect when workers strongly believe that they cannot adapt to economic adversity (Bandura 1997; McNatt and Judge 2004). To test this, we examine whether the treatment effect is less pronounced for workers who frequently underperform their peers during the pre-treatment period. Specifically, we label workers as having underperformed when they have a non-zero weekly sales commission below the weekly median of their distributorship. Subsequently, we classify workers as having frequent underperformance when the number of weeks with low performance in the pre-treatment period exceeds the overall sample median (3 weeks), and as having infrequent underperformance otherwise. Finally, we repeat our main analysis separately for workers who have frequent underperformance and those who do not.

Table 9 presents the results of this analysis. Consistent with our treatment message increasing productivity by making failure appear less diagnostic of the relationship between effort and performance, the table shows that the treatment effect is only significant among workers who have not frequently underperformed their peers.

[Insert Table 9 about here]

Reinforcement of Organizational Norms

The exchange and inculcation of normative behavior regarding failure should be more pronounced within distributorships with greater age similarity among workers.⁴⁰ Such similarity facilitates the development of common beliefs about appropriate behavior in two ways. First, it increases communication within workgroups (Zenger and Lawrence 1989). Second, it makes workers more receptive to influence from their coworkers (Grant and Patil 2012). Consequently, age similarity among workers does not only increase the extent to which workers are exposed to formal communication from senior management but also facilitates the development of informal rules of behavior (Cialdini and and Trost 1998). Thus, if our results are at least in part driven by the strengthening of organizational norms that encourage workers to persevere after failure, our results should be more pronounced for distributorships that have greater age similarity among workers.^{41, 42}

To test this, we repeat our main analysis separately for distributorships that have high and low age similarity among workers. As is common in research on organizational demography, we use the coefficient of variation to compute the age similarity of each distributorship (Zajac and Westphal 1996) and classify those below the median as having high age similarity, and those above the median as having low age similarity. The results of this analysis are reported in Table 10. The left half and the right half of the table report, respectively, the results for the distributorships that have high and low age similarity among workers. As expected, the table indicates that the treatment

⁴⁰ We focus on age similarity because in our setting there is little variation in other demographic characteristics within distributorships such as gender and ethnicity.

⁴¹ We acknowledge that the more frequent sharing and communication of our treatment message among workers can increase the treatment effect even when social norms regarding failure do not change within distributorships. Unfortunately, we are unable to directly determine the extent to which our treatment message affects social norms due to data limitations.

⁴² Distributorships that were assigned to the treatment condition do not significantly differ from those that were assigned to the control condition with respect to the age similarity among workers.

effect is statistically and economically important for distributorships that have high age similarity but not for distributorships that have low age similarity. Thus, the results in this table are consistent with our findings resulting at least in part from social norms.⁴³

[Insert Table 10 about here]

VI. CONCLUSION

This study examines how formal communications from senior management about failure affect worker performance and is the result of a multi-year collaboration with a publicly traded multi-national direct sales organization. Our findings, derived from a field experiment, indicate that communicating workers should view failure as a natural part of learning rather than an indictment of their ability increases the performance of workers by making them more confident in their ability to successfully perform job-specific tasks and by reinforcing social norms that encourage workers to persevere after failure. This performance effect appears to be driven by an increase in how much effort workers devote to their job rather than by an increase in the productivity of their effort.

Our primary contribution is the discovery that formal communications from senior management are a viable control mechanism for sustaining effort in the face of failure. Existing research indicates that organizations use highly achievable or flexible performance targets to prevent failure perceptions from decreasing performance. Our findings imply that rather than altering performance targets, organizations can control how workers interpret and respond to their failures. It also compliments existing literature on the motivational effects of performance

⁴³ Because the average age and age similarity of a distributorship's workforce are negatively correlated we have examined in an untabulated analysis whether average age affects the treatment effect. We find that this is not the case. Thus, it is unlikely that the results presented in Table 10 are driven by the average age of a distributorship's workforce.

measurement noise. While this literature stresses the importance of filtering out the effects of adverse uncontrollable events from evaluations, we show that it is important to reduce ambiguity about the future performance implications of these events through formal communications from senior management. Finally, our study contributes to the literature on how social controls encourage workers to take goal congruent actions. Prior research in this area has either assumed that social controls are difficult to influence by senior management or has focused on how senior management can influence social controls through monitoring and incentive pay. Our findings suggest that senior managers can use formal communications to relatively quickly modify the perceived appropriateness of a specific type of behavior – persevering after failure. In doing so, we have identified an organizational practice that may be useful for explaining variation in performance dimensions that we do not explore in our study but are of interest to accountants such as the manipulation of performance and budget reports and risk-taking.

This study has several limitations that could be addressed by future research. First, because attending weekly sales meetings is voluntary, our findings are limited to workers who choose to attend these meetings. It is possible that formal communications about failure have different effects on workers who do not attend these events, or in organizations where workers may be forced to attend meetings. Second, our findings could result from an increase in the number of weeks workers devoted effort to their job and a decrease in turnover. We are unable to tease out the effects of these two factors on our findings because in our setting workers are free to choose how much effort they devote to their job. Third, workers do not have to meet specific performance targets to earn sales commissions in our research setting. As a result, we were not able to examine whether formal communications about failure interact with how organizations communicate performance targets to workers. Finally, while we believe that organizations should be able to achieve similar

results using different approaches, such as communicating via email as opposed to a video based message, we are unable to verify this with the available data. Nonetheless, our study shows that organizations can increase worker performance by controlling how workers interpret and respond to their failures.

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APPENDIX A
Scripts of Video-Based Messages

Treatment Condition

Hi guys, how are you? I wanted to send a quick message to congratulate you for the successful year we are having so far in increasing our sales and bringing new consultants into our PSI family.

Sometimes people ask me, "What is the secret to so many achievements?" I have no doubt it is because of the work of each of you. Regardless of any challenge, you always excel. So let me say how grateful I am to have you as partners.

I know it is not always easy to get where you are. Each of you has your dreams and hopes, but also you have each had your difficulties. Because of this, we know that as important as celebrating success is supporting each other in difficult times. Especially when things seem complicated, when we cannot achieve our goals or when we are facing obstacles that seem impossible.

There is no one who has not gone through difficult times and who has not doubted his or her potential. What happens is that after these moments, when we find solutions, we become stronger. Indeed, everyone always told me that Brazil does not give up, and I realize that in each of you.

I want to remind each of you that you are not alone. Trust your leader, manager, and distributor, for we are all here to support you in day to day business and in your personal development. Tomorrow is up to you!

Don't be afraid of failure. Setbacks and failures are part of the journey of all of us, but when we make mistakes trying it should only inspire us to think more creatively the next time.

Talk to others, this can open our eyes to a new way of thinking.

So next time you think "what will I do now?!" Remember, don't be afraid of failure; setbacks are normal, and you are not alone. Tomorrow is up to you. I wish you much success that your dreams come true and you always can count on us.

(Continued on next page)

APPENDIX A (Continued)

Control Condition

Hi guys, how are you? I wanted to give send a quick message to congratulate you for the successful year we are having so far in increasing our sales and bringing new consultants into our PSI family.

2016 is a very important year for us. For 40 years we have been present in Brazil bringing quality products to the homes of this country and the opportunity for personal and professional growth for so many people in our sales force. That's why we have much to celebrate.

Throughout the year 2016 there have been and will continue to be many special activities. We have iconic products like the cruet and the “sensation bowl”, commemorative decorations with the decades of the 70s, 80s and 90s, activity programs and fantastic recruitment, in addition to events, travel and more!

Do not miss your chance to learn about the history of our company, to monitor TV programming PSI, do not forget to read the VP magazine and access the PSI City. Who knows, maybe you will not find your story there.

We continue in this together, hoping to bring you more and more fantastic products, unmissable campaigns and news that delight you and your customers.

I wish you much success that your dreams come true and you always can count on us.

APPENDIX B Survey Questions

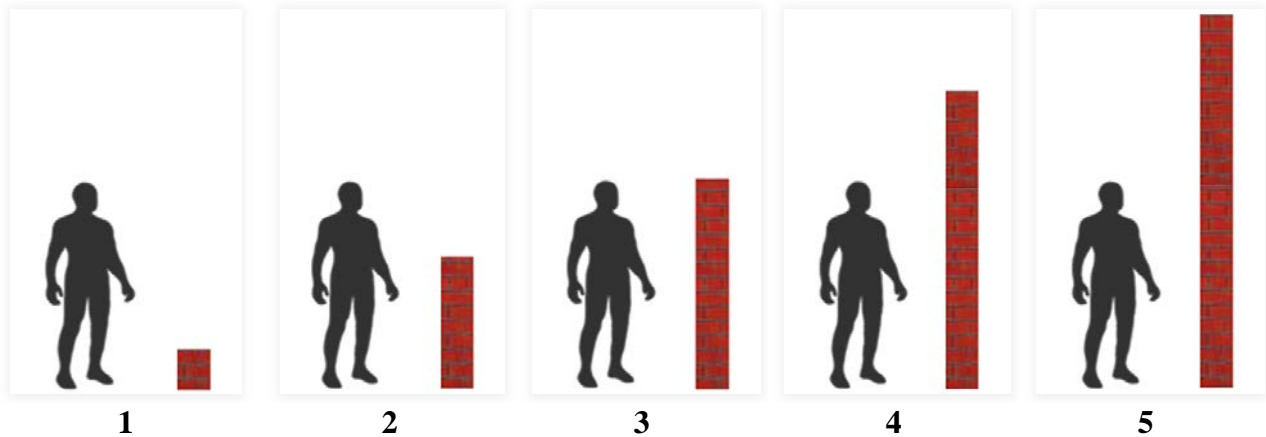
Job-Specific Confidence:

For the questions below, please focus on how you feel about your PSI selling skills. Please indicate how strongly you agree or disagree with each statement.

	Strongly Disagree	Disagree	Slightly Disagree	Slightly Agree	Agree	Strongly Agree
In general, I am not good at learning about new products	1	2	3	4	5	6
I am confident in my ability to recruit new consultants	1	2	3	4	5	6
I am not very good at collecting payment from my customers	1	2	3	4	5	6
I have trouble closing the deal when selling product	1	2	3	4	5	6
I am good at making myself call people I don't know	1	2	3	4	5	6
I am better than most at learning new products	1	2	3	4	5	6
I have a hard time finding new recruits	1	2	3	4	5	6
I feel confident about my ability to sell <i>all</i> PSI products and to explain how the products work	1	2	3	4	5	6

Ability to Overcome Life Challenges:

Imagine that the red wall is a life challenge you are facing. How big a life challenge can you get over? Please circle the picture that best represents how large a challenge you feel mostly sure you can get over if you work hard.



(Continued on next page)

APPENDIX B (Continued)

Optimism:

Think of this ladder as representing where people stand in their community. Think of a community as your neighborhood. At the TOP of the ladder are the people who have the highest standing in their community. At the BOTTOM are the people who have the lowest standing in their community.



Where would you place yourself on this ladder today? Put an X on the rung of the ladder where you would place yourself.



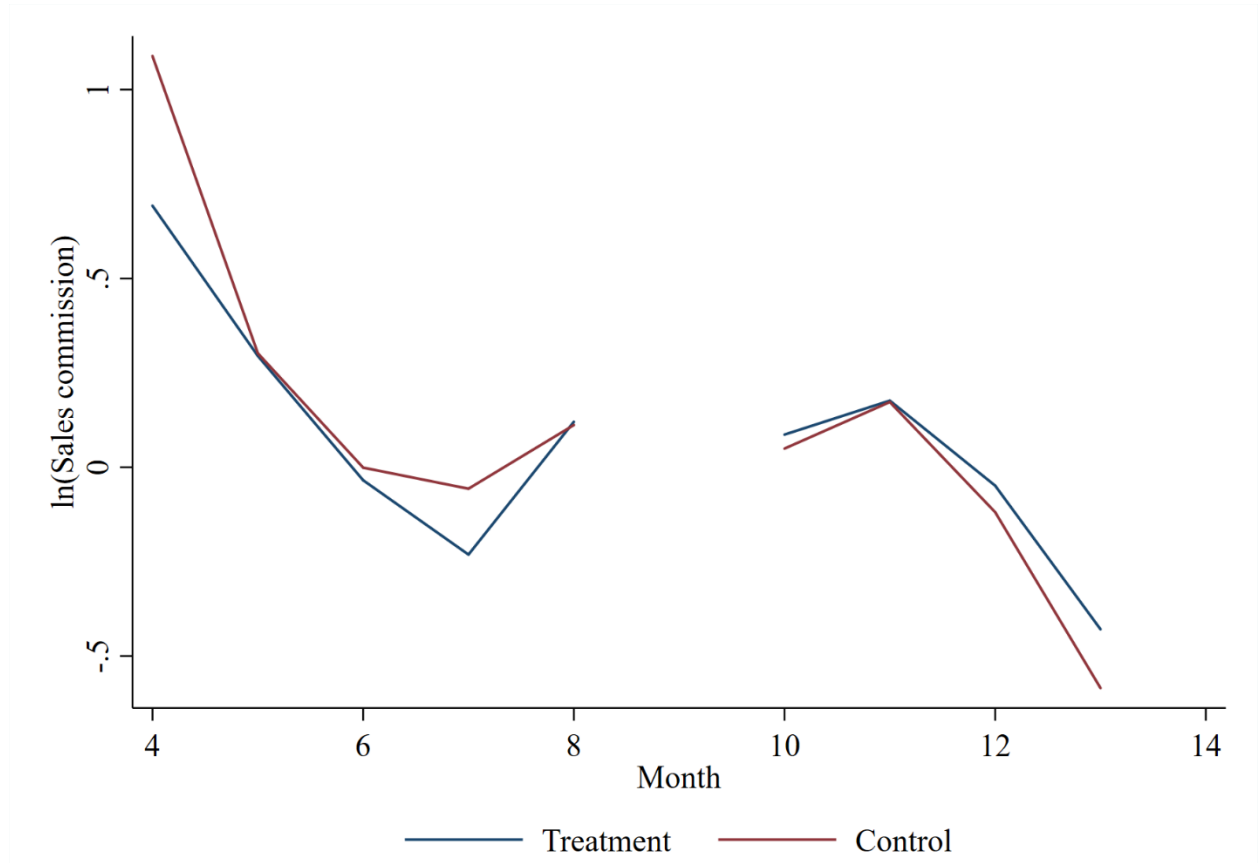
Remember back to 5 years ago. Where would you be on the ladder? You can mark yourself higher, lower, or in the same place.



Pretend you are now 5 years in the future. Where will be on the ladder? You can mark yourself higher, lower, or in the same place.

Our optimism measure equals the difference between a worker's future and current standing.

FIGURE 1
Trend in Sales Commissions



This figure presents the trend in performance for the 34-week window around the treatment period separately for workers that either viewed the treatment or control video. Performance equals the natural logarithmic transformation of one plus weekly sales commission. For the purpose of this figure we de-mean performance at the worker level to remove worker fixed effects and average the resulting values by month. Our sample consists of 848 unique workers of which 320 were assigned to the treatment condition ($Treatment = 1$) and 528 were assigned to the control condition ($Treatment = 0$).

TABLE 1
Summary Statistics

Panel A: Descriptive Statistics on Biographic and Work-Related Characteristics

	Treatment = 1 (N=320)			Treatment = 0 (N=528)			Difference <u>Mean</u>
	<u>Mean</u>	<u>Median</u>	<u>Std. Dev.</u>	<u>Mean</u>	<u>Median</u>	<u>Std. Dev.</u>	
<i>Log(Age)</i>	3.77	3.78	0.32	3.75	3.78	0.32	0.01
<i>Female</i>	0.98	1.00	0.16	0.98	1.00	0.14	-0.00
<i>Log(Tenure)</i>	1.02	1.12	1.21	0.76	0.70	1.17	0.26*

Panel B: Change in Performance across Treatment Conditions

	<u>Post = 0</u>	<u>Post = 1</u>	<u>Δ Performance</u>
<i>Treatment = 1</i>	3.37 N=5,440	3.22 N=5,440	-0.15*** N=10,880
<i>Treatment = 0</i>	3.05 N=8,976	2.75 N=8,976	-0.30*** N=17,952
<u><i>Difference-in-Differences</i></u>	0.32	0.47	0.14**

This table presents descriptive statistics on our sample. Panel A provides summary statistics of biographic and work-related characteristics of workers across treatment conditions, where *Treatment* is an indicator variable that equals one for workers from the treatment group and zero for workers from the control group; *Log(Age)* is a natural logarithmic transformation of workers' age in years; *Female* is an indicator variable that equals one for female workers and zero for male workers; and *Log(Tenure)* is a natural logarithmic transformation of worker's tenure with PSI in years. Our sample consists of 848 unique workers of which 320 were assigned to the treatment condition (*Treatment* = 1) and 528 were assigned to the control condition (*Treatment* 0).

Panel B provides a univariate analysis comparing how workers' performance changed from the pre- to post-treatment period, where performance equals a natural logarithmic transformation of one plus weekly sales commission. The sample includes weekly sales commission data from the 17 weeks before (*Post* = 0) and after (*Post* =1) the treatment period. We use ordinary least squares regressions to test differences in means and use the Kline and Santos (2012) bootstrap method at the distributorship level to compute standard errors.

*, **, and *** indicate two-tailed statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 2
Primary Analysis

<u>Explanatory Variable</u>	OLS	Two-Part Model	
	(1)	(2)	(3)
	Coefficient	Odds Ratio	Coefficient
	[<i>t</i> -statistic]	[<i>z</i> -statistic]	[<i>t</i> -statistic]
<i>Treatment</i> × <i>Post</i>	0.14** [2.18]	1.16** [2.40]	-0.00 [-0.15]
Worker fixed effects	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes
Number of obs.	28,832	28,832	13,529
<u>Adjusted-<i>R</i>²/<i>p</i>-value Wald-χ^2</u>	0.37	0.00	0.36

This table presents the results of ordinary least squares (OLS) and two-part model regressions that examine the influence of formal communications about failure on worker performance. Our sample consists of 848 unique workers of which 320 were assigned to the treatment condition (*Treatment* = 1) and 528 were assigned to the control condition (*Treatment* = 0). We use weekly sales commission data from the 17 weeks before (*Post* = 0) and after (*Post* = 1) the treatment period to construct our performance measure. The dependent measure in columns (1) and (3) equals the natural logarithmic transformation of one plus sales commission, and the dependent measure in column (2) is an indicator variable that equals one for worker-week observations with nonzero sales commissions and zero otherwise. Of note, the analysis presented in column (3) is based on the subset of worker-week observations with nonzero sales commissions. We use the Kline and Santos (2012) bootstrap method at the distributorship level to compute standard errors.

** indicates two-tailed statistical significance at the 5% level.

TABLE 3
Dynamics of Treatment Effect

<u>Explanatory Variable</u>	OLS	Two-Part Model	
	(1) Coefficient <u>[t-statistic]</u>	(2) Odds Ratio <u>[z-statistic]</u>	(3) Coefficient <u>[t-statistic]</u>
<i>Treatment</i> × <i>Month</i> _{<i>t-3</i>}	0.05 [0.44]	1.05 [0.48]	0.04 [1.56]
<i>Treatment</i> × <i>Month</i> _{<i>t-2</i>}	-0.09 [-0.58]	0.93 [-0.60]	-0.01 [-0.39]
<i>Treatment</i> × <i>Month</i> _{<i>t-1</i>}	0.09 [0.63]	1.10 [0.85]	-0.01 [-0.37]
<i>Treatment</i> × <i>Month</i> _{<i>t</i>}	0.12 [0.82]	1.13 [0.90]	-0.00 [-0.09]
<i>Treatment</i> × <i>Month</i> _{<i>t+1</i>}	0.12 [0.94]	1.13 [1.19]	-0.01 [-0.21]
<i>Treatment</i> × <i>Month</i> _{<i>t+2</i>}	0.09 [0.74]	1.09 [0.85]	-0.01 [-0.39]
<i>Treatment</i> × <i>Month</i> _{<i>t+3</i>}	0.16 [1.35]	1.17* [1.71]	0.01 [0.63]
<i>Treatment</i> × <i>Month</i> _{<i>t+4</i>}	0.24** [2.34]	1.32*** [3.30]	0.02 [0.70]
H0: lead effects _[<i>t-3</i>; <i>t-1</i>] = 0	0.34	0.37	0.27
Worker fixed effects	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes
Number of obs.	32,224	32,224	15,255
Adjusted- <i>R</i> ² / <i>p</i> -value Wald- χ^2	0.37	0.00	0.36

This table presents the results of ordinary least squares (OLS) and two-part model regressions that examine the dynamic effect of formal communications about failure on worker performance. Our sample consists of 848 unique workers of which 320 were assigned to the treatment condition (*Treatment* = 1) and 528 were assigned to the control condition (*Treatment* = 0). We use weekly sales commission data from the treatment period (4 weeks) and the 17 weeks before and after this period to construct our performance measure. The dependent measure in columns (1) and (3) equals the natural logarithmic transformation of one plus sales commission, and the dependent measure in column (2) is an indicator variable that equals one for worker-week observations with nonzero sales commissions and zero otherwise. Of note, the analysis presented in column (3) is based on the subset of worker-week observations with nonzero sales commissions. *Month*_{*t+q*} are indicators for how many months until (*t+q*) or since (*t-q*) the treatment month (*Month*_{*t*}) with the first 5 weeks of our sample period serving as our reference period. We use the Kline and Santos (2012) bootstrap method at the distributorship level to compute standard errors.

*, **, and *** indicate two-tailed statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 4
Treatment Effect on Job-Specific Confidence and General Job Attitudes

<u>Explanatory Variable</u>	ΔJob-Specific Confidence Coefficient <u>[t-statistic]</u>	ΔOvercome Life Challenges Coefficient <u>[t-statistic]</u>	ΔOptimism Coefficient <u>[t-statistic]</u>
<i>Treatment</i>	0.96** [2.00]	0.10 [1.34]	0.00 [0.04]
Number of obs.	160	235	244
<i>R</i> ²	0.02	0.00	0.00

This table presents the results of an analysis that examines how our treatment changed job-specific confidence, perceived ability to overcome life-challenges, and optimism during our four-week experiment. The analyses presented in this table are based on survey responses from workers who completed the relevant questions from both the baseline survey and the post-experiment survey. Further, *Treatment* is an indicator variable that equals one for workers from the treatment group and zero for workers from the control group. See Appendix A for survey questions that were used to construct our measures of workers' job-specific confidence, perceived ability to overcome life-challenges, and optimism. We use the Kline and Santos (2012) bootstrap method at the distributorship level to compute standard errors.

** indicates two-tailed statistical significance at the 5% level.

TABLE 5
Performance Effects of Increasing Job-Specific Confidence

<u>Explanatory Variable</u>	OLS	Two-Part Model	
	(1)	(2)	(3)
	Coefficient	Odds Ratio	Coefficient
	[<u>t-statistic</u>]	[<u>z-statistic</u>]	[<u>t-statistic</u>]
<i>Job-Specific Confidence Week 1</i>	0.18*** [3.78]	1.15*** [3.98]	0.01 [1.76]
Δ <i>Job-Specific Confidence</i>	0.16*** [4.00]	1.13*** [4.29]	0.01 [0.97]
<i>Log(Age)</i>	0.36 [0.69]	1.42 [0.81]	-0.17* [-2.31]
<i>Female</i>	0.79 [1.48]	1.73 [1.32]	0.18** [1.92]
<i>Log(Tenure)</i>	0.45* [2.14]	1.35 [1.91]	0.06*** [2.95]
Distributorship fixed effects	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes
Number of obs.	1,921	1,921	1,088
<u>Adjusted-R²/p-value Wald-χ^2</u>	0.23	0.00	0.19

This table presents the results of an analysis that examines whether increases in job-specific confidence are associated with improved performance in the post treatment period. This analysis is based on 113 unique workers from the analysis presented in Table 2 that completed the job-specific confidence questions from both the baseline survey and the post-experiment survey. See Appendix A for survey questions that were used to construct our measures of workers' job-specific confidence. Further, *Job-Specific Confidence Week 1* equals the job-specific confidence score from the baseline survey, Δ *Job-Specific Confidence* equals the change in job-specific confidence score from the baseline survey to the post-experiment survey, *Log(Age)* is a logarithmic transformation of workers' age in years; *Female* is an indicator variable that equals one for female workers and zero for male workers; and *Log(Tenure)* is a logarithmic transformation of a worker's tenure with PSI in years. The dependent measure in columns (1) and (3) equals the natural logarithmic transformation of one plus sales commission, and the dependent measure in column (2) is an indicator variable that equals one for worker-week observations with nonzero sales commissions and zero otherwise. Of note, the analysis presented in column (3) is based on the subset of worker-week observations with nonzero sales commissions. We use the Kline and Santos (2012) bootstrap method at the distributorship level to compute standard errors.

*, **, and *** indicate two-tailed statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 6
Alternative Confidence Measure

Panel A: Descriptive Statistics on Impact of Video on Confidence

	Treatment = 1 (N=136)			Treatment = 0 (N=234)			Difference Mean
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	
<i>ΔConfidence</i>	0.17	0.31	0.93	-0.10	0.31	1.03	0.27**

Panel B: Impact of Confidence from Video on Performance

<u>Explanatory Variable</u>	OLS		Two-Part Model	
	(1)	(2)	(3)	(3)
	Coefficient	Odds Ratio	Coefficient	Coefficient
	[t-statistic]	[z-statistic]	[t-statistic]	[t-statistic]
<i>ΔConfidence</i>	0.14**	1.10**	0.02	
	[2.42]	[2.39]	[1.71]	
<i>Log(Age)</i>	-0.46*	0.74	-0.19***	
	[-2.58]	[-2.09]	[-4.90]	
<i>Female</i>	0.73	1.90	0.12**	
	[1.82]	[1.87]	[2.18]	
<i>Log(Tenure)</i>	0.61***	1.52***	0.07***	
	[5.41]	[5.54]	[3.22]	
Distributorship fixed effects	Yes	Yes	Yes	
Week fixed effects	Yes	Yes	Yes	
Number of obs.	6,290	6,290	2,836	
Adjusted-R ²	0.16	0.00	0.13	

This table presents the results of an analysis that repeats the analyses presented in tables 4 and 5 using an alternative confidence measure. The results presented in this table are based on 370 unique workers from the analysis presented in Table 2 who indicated the extent to which they agreed with the following statement from the post-experiment survey: “Watching the video made me feel more confident.” This question is on a six-point Likert scale and ranges from 1 (strongly disagree) to 6 (strongly agree). We normalized our measure using the mean and standard deviation from all surveys with non-missing responses (*ΔConfidence*). Panel A presents summary statistics of this measure across treatment conditions, where *Treatment* is an indicator variable that equals one for workers from the treatment group and zero for workers from the control group. The analysis presented in Panel B examines whether feeling more confident after watching either the treatment or control video is positively associated with performance in the post-treatment period, where *Log(Age)* is a logarithmic transformation of workers’ age in years; *Female* is an indicator variable that equals one for female workers and zero for male workers; and *Log(Tenure)* is a logarithmic transformation of a worker’s tenure with PSI in years. The dependent measure in columns (1) and (3) equals the natural logarithmic transformation of one plus sales commission, and the dependent measure in column (2) is an indicator variable that equals one for worker-week observations with nonzero sales commissions and zero otherwise. Of note, the analysis presented in column (3) is based on the subset of worker-week observations with nonzero sales commissions. We use the Kline and Santos (2012) bootstrap method at the distributorship level to compute standard errors.

*, **, and *** indicate two-tailed statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 7
Internalization of Communication from Senior Management

Panel A: Magnitude of Treatment Effect

<u>Explanatory Variable</u>	Watched Video Multiple Times			Watched Video Once		
	OLS	Two-Part Model		OLS	Two-Part Model	
	(1)	(2)	(3)	(4)	(5)	(6)
	Coefficient	Odds Ratio	Coefficient	Coefficient	Odds Ratio	Coefficient
	<u>[t-statistic]</u>	<u>[z-statistic]</u>	<u>[t-statistic]</u>	<u>[t-statistic]</u>	<u>[z-statistic]</u>	<u>[t-statistic]</u>
<i>Treatment</i> × <i>Post</i>	0.33** [3.21]	1.33*** [3.30]	0.04 [1.41]	-0.10 [-0.39]	0.93 [-0.34]	-0.05 [-0.78]
Worker fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	6,290	6,290	3,264	2,720	2,720	961
Adjusted- R^2 / p -value Wald- χ^2	0.37	0.00	0.38	0.26	0.00	0.26

Panel B: Timing of Treatment Effect when Watching Video Multiple Times

<u>Explanatory Variable</u>	OLS	Two-Part Model	
	(1)	(2)	(3)
	Coefficient	Odds Ratio	Coefficient
	<u>[t-statistic]</u>	<u>[z-statistic]</u>	<u>[t-statistic]</u>
<i>Treatment</i> × <i>Month</i> _{<i>t-3</i>}	0.08 [0.37]	1.04 [0.22]	0.06 [1.26]
<i>Treatment</i> × <i>Month</i> _{<i>t-2</i>}	-0.05 [-0.16]	0.93 [-0.30]	-0.01 [-0.13]
<i>Treatment</i> × <i>Month</i> _{<i>t-1</i>}	0.31 [1.69]	1.34 [1.84]	-0.03 [-0.47]
<i>Treatment</i> × <i>Month</i> _{<i>t0</i>}	0.20 [0.74]	1.16 [0.57]	0.03 [0.57]
<i>Treatment</i> × <i>Month</i> _{<i>t+1</i>}	0.24 [1.25]	1.23 [1.22]	-0.01 [-0.15]

(Continued on next page)

TABLE 7 (Continued)

<i>Treatment</i> × <i>Month</i> _{<i>t</i>+2}	0.48** [2.35]	1.50* [2.16]	0.06 [1.27]
<i>Treatment</i> × <i>Month</i> _{<i>t</i>+3}	0.42* [1.75]	1.40* [1.86]	0.07 [1.60]
<i>Treatment</i> × <i>Month</i> _{<i>t</i>+4}	0.47* [2.21]	1.51** [2.33]	0.06 [1.19]
H0: lead effects _[<i>t</i>-3; <i>t</i>-1] = 0	0.32	0.25	0.31
Worker fixed effects	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes
Number of obs.	7,030	7,030	3,683
Adjusted- <i>R</i> ² / <i>p</i> -value	0.36	0.00	0.39
Wald- χ^2			

This table presents the results of an analysis that examines whether the treatment effect is more pronounced (Panel A) and emerges more quickly (Panel B) when workers viewed the video that they were assigned to multiple times. Our sample consists of 265 unique workers of which 111 were assigned to the treatment condition (*Treatment* = 1) and 154 were assigned to the control condition (*Treatment* = 0). To be included in our sample, workers must have indicated on the post-experiment survey how many times they watched the video that they were assigned to. Panel A of the table repeats the analysis presented in Table 2 separately for workers who watched their assigned video multiple times and those who watched their assigned video only once, and Panel B of the table repeats the analysis presented in Table 3 for workers who viewed their assigned video multiple times. See Table 2 and Table 3 for further details on these analyses. We use the Kline and Santos (2012) bootstrap method at the distributorship level to compute standard errors.

* and ** indicate two-tailed statistical significance at the 10% and 5% levels, respectively.

TABLE 8
Emotional Response to Failure

<u>Explanatory Variable</u>	Other Work			No Other Work		
	OLS (1) Coefficient [<i>t</i> -statistic]	Two-Part Model (2) Odds Ratio [<i>z</i> -statistic]	Two-Part Model (3) Coefficient [<i>t</i> -statistic]	OLS (4) Coefficient [<i>t</i> -statistic]	Two-Part Model (5) Odds Ratio [<i>z</i> -statistic]	Two-Part Model (6) Coefficient [<i>t</i> -statistic]
<i>Treatment</i> × <i>Post</i>	0.05 [0.63]	1.06 [0.73]	-0.01 [-0.15]	0.28** [2.69]	1.32** [3.06]	-0.02 [-1.03]
Worker fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	8,942	8,942	3,886	15,742	15,742	8,028
<u>Adjusted-<i>R</i>²/<i>p</i>-value Wald-χ^2</u>	0.37	0.00	0.35	0.37	0.00	0.36

This table presents the results of an analysis that examines whether the treatment effect is less pronounced when workers are less likely to set personal performance targets due to other paid work outside of PSI. Our sample consists of 722 unique workers of which 279 were assigned to the treatment condition (*Treatment* = 1) and 443 were assigned to the control condition (*Treatment* = 0). To be included in our sample, workers must have completed the following question from either the baseline survey or post-experiment survey: “Do you have other work for which you receive an income?” We classify workers as having other work if they indicate having paid work outside of PSI, and as having no other work otherwise. Columns (1) through (3) present the results for workers who have other work, and columns (4) through (6) present the results for the remaining workers. The dependent measure in columns (1), (3), (4), and (6) equals the natural logarithmic transformation of one plus sales commission, and the dependent measure in columns (2) and (5) is an indicator variable that equals one for worker-week observations with nonzero sales commissions and zero otherwise. Of note, the analyses presented in columns (3) and (6) are based on the subset of worker-week observations with nonzero sales commissions. We use the Kline and Santos (2012) bootstrap method at the distributorship level to compute standard errors.

* and ** indicate two-tailed statistical significance at the 10% and 5% levels, respectively.

TABLE 9
External Attributions

<u>Explanatory Variable</u>	Frequent Underperformance			Infrequent Underperformance		
	OLS	Two-Part Model		OLS	Two-Part Model	
	(1)	(2)	(3)	(4)	(5)	(6)
	Coefficient	Odds Ratio	Coefficient	Coefficient	Odds Ratio	Coefficient
	<u>[t-statistic]</u>	<u>[z-statistic]</u>	<u>[t-statistic]</u>	<u>[t-statistic]</u>	<u>[z-statistic]</u>	<u>[t-statistic]</u>
<i>Treatment × Post</i>	0.09	1.10	-0.00	0.22*	1.25*	-0.02
	[0.74]	[0.82]	[-0.13]	[1.82]	[2.07]	[-0.64]
Worker fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	13,362	13,362	7,594	15,470	15,470	5,935
<u>Adjusted-R²/p-value Wald-χ^2</u>	0.36	0.00	0.29	0.35	0.00	0.39

This table presents the results of an analysis that examines whether the treatment effect is less pronounced when frequently underperforming peers makes it more difficult for workers to attribute failure to external causes. Our sample consists of 848 unique workers of which 320 were assigned to the treatment condition (*Treatment* = 1) and 528 were assigned to the control condition (*Treatment* = 0). We label workers as having underperformed when they have a non-zero weekly sales commission below the weekly median of their distributorship. Subsequently, we classify workers as having frequent underperformance when the number of weeks with low performance in the pre-treatment period exceeds the overall sample median (3 weeks), and as having infrequent underperformance otherwise. Columns (1) through (3) present the results for workers with frequent underperformance, and columns (4) through (6) present the results for the remaining workers. The dependent measure in columns (1), (3), (4), and (6) equals the natural logarithmic transformation of one plus sales commission, and the dependent measure in columns (2) and (5) is an indicator variable that equals one for worker-week observations with nonzero sales commissions and zero otherwise. Of note, the analyses presented in columns (3) and (6) are based on the subset of worker-week observations with nonzero sales commissions. We use the Kline and Santos (2012) bootstrap method at the distributorship level to compute standard errors.

* and ** indicate two-tailed statistical significance at the 10% and 5% levels, respectively.

TABLE 10
Social Norms

<u>Explanatory Variable</u>	High Age Similarity			Low Age Similarity		
	OLS (1)	Two-Part Model (2)	Two-Part Model (3)	OLS (4)	Two-Part Model (5)	Two-Part Model (6)
	Coefficient [<i>t</i> -statistic]	Odds Ratio [<i>z</i> -statistic]	Coefficient [<i>t</i> -statistic]	Coefficient [<i>t</i> -statistic]	Odds Ratio [<i>z</i> -statistic]	Coefficient [<i>t</i> -statistic]
<i>Treatment</i> × <i>Post</i>	0.21** [2.44]	1.21* [2.26]	0.04 [0.21]	0.08 [0.98]	1.11 [1.46]	-0.01 [-0.21]
Worker fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	14,008	14,008	6,659	14,824	14,824	6,870
<u>Adjusted-<i>R</i>²/<i>p</i>-value Wald-χ^2</u>	0.38	0.00	0.33	0.36	0.00	0.39

This table presents the results of an analysis that examines whether the treatment effect is more pronounced when greater age similarity among workers of a distributorship facilitates the development of social norms that encourage workers to persevere after failure. Our sample consists of 848 unique workers of which 320 were assigned to the treatment condition (*Treatment* = 1) and 528 were assigned to the control condition (*Treatment* = 0). We use the coefficient of variation to compute age similarity and classify distributorships that have a coefficient of variation below the median as having high age similarity, and as having low age similarity otherwise. Columns (1) through (3) present the results for distributorships with high age similarity, and columns (4) through (6) present the results for the remaining distributorships. The dependent measure in columns (1), (3), (4), and (6) equals the natural logarithmic transformation of one plus sales commission, and the dependent measure in columns (2) and (5) is an indicator variable that equals one for worker-week observations with nonzero sales commissions and zero otherwise. Of note, the analyses presented in columns (3) and (6) are based on the subset of worker-week observations with nonzero sales commissions. We use the Kline and Santos (2012) bootstrap method at the distributorship level to compute standard errors.

* and ** indicate two-tailed statistical significance at the 10% and 5% levels, respectively.