

Improving Perceptions of Fairness and Performance through Explanation and Perspective Taking

by

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I hereby declare that I am the sole author of this thesis. This is a true copy of my thesis, including any required final revisions, as accepted by my examiners.

I understand that my thesis may be made electronically available to the public.

ABSTRACT

Employee performance can be negatively impacted due to the occurrence of unforeseen negative events. To filter out the influence of these negative exogenous shocks on employee compensation, management can commit *ex-ante* to consider performing *ex-post* adjustments to objectively determined compensation. However, due to compensation interdependence between subordinates common to these types of incentive schemes, management may be reluctant to perform such *ex-post* adjustments. Employees not receiving *ex-post* adjustments may feel unfairly treated, suggesting the need to examine how management's selective exercise of *ex-post* adjustments impact employee fairness perceptions and subsequent performance. Employees' reaction to management's apparent non-helping behaviour may stem in part from a lack of sensitivity to the difficulty management faces in making such adjustments. I therefore examine two interventions, perspective taking and explanation, aimed at improving employee fairness perceptions and performance. To test my predictions, I conduct an experiment with undergraduate business student participants. I find that the announcement of an *ex post* adjustment policy does not significantly impact participant perceptions of fairness but significantly improves performance when they encounter their first negative shock. In addition, I find that although both explanation and perspective taking significantly improve perceptions of fairness, only perspective taking improves performance after not receiving an *ex-post* adjustment. The current study contributes to the growing management accounting literature examining how management subjectivity in compensation contracts influence subordinate performance. This study further contributes to organizational justice literature by examining the link between fairness perceptions and task performance. Finally, my results show that perspective taking can be an effective intervention to improve employee perceptions of fairness and performance in response to receiving unfavourable outcomes, which is relevant to practitioners in designing compensation contracts for employees.

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DEDICATION

This thesis is dedicated to my family and friends. Jeff, Jager, Tom, Virginia, Jen, and Daniella, I could have never done this without your unconditional love and support. Thank you.

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CHAPTER 1: INTRODUCTION

I examine how employees' perceptions of fairness and performance change in response to management's selective application of ex-post goal adjustments. Ex-post goal adjustments refer to subjective adjustments to employee goals made at the discretion of management, typically after performance for a contract period has already been realized (Bol 2008). One of the purposes of these adjustments are to neutralize the effects of negative uncontrollable events (e.g. natural disasters) on employees' measured performance (Bol, 2008; Bol, Keune, Matsumura, and Shin, 2010) out of concern for the controllability principle, which holds that individuals should only be evaluated based on elements of their performance that they can control (Vancil and Buddrus, 1979; Giraud, Langevin, and Mendoza, 2008). In support of the application of the controllability principle, prior accounting research has shown that the neutralization of uncontrollable events through ex-post adjustments can have a positive effect on employee performance through improved perceptions of procedural justice (Kelly, Webb and Vance, 2015).

There is experimental evidence suggesting that even when individuals in the role of managers are allowed discretion in allocating bonus pools (Bailey, Hecht, and Towry, 2011; Maas, van Rinsum, and Towry, 2012) or in performing ex-post bonus adjustments (Bol, Hecht, and Smith, 2015), discretion is not always exercised. Furthermore, Höppe and Moers (2011) find that of the firms who disclose the use of ex-post bonus adjustments in their SEC Proxy Statements, only about 24% actually exercise the option to do so. Bol et al. (2015) reason that when the likelihood of uncontrollable negative shocks to employee performance is high, managers may withhold making adjustments in order to encourage employees to adapt to these

negative shocks. Similarly, when there is a high degree of compensation interdependence between employees, managers may be reluctant to exercise discretion because helping one employee by allocating more rewards to them necessarily decreases the pool of rewards available to other employees (Bol et al. 2015). If management has discretion to neutralize the effects of negative uncontrollable events through ex-post goal adjustments but declines to do so for some employees, these employees may feel unfairly treated. It is important to understand how employees may react to this selective application of ex-post goal adjustments, as their reaction could attenuate any positives of implementing such a policy in the first place.

Organizational justice research shows that employees sometimes respond to unfair organizational outcomes and procedures by engaging in counterproductive work behaviours such as withholding effort. Conversely, employees respond to fair organizational outcomes and procedures by increasing effort and engaging in organizational citizenship behaviours (Colquitt et al., 2001; Cohen-Charash and Spector, 2001; Colquitt et al., 2013). The accounting literature has also well documented the positive effects of justice perceptions on performance. For example, in a field study, Wentzel (2002) finds that participation in the budgeting process improves perceptions of fairness, leading to improved performance through increased goal commitment. Similarly, Lau and Moser (2008) find in a survey of managers that justice perceptions are positively associated with performance through organizational commitment. Thus, I first predict that in my setting, when management announces an ex-post goal adjustment policy, the mere announcement of the policy will result in improved perceptions of fairness and performance. However, if management selectively performs ex-post goal adjustments in response to negative uncontrollable events, those employees who do not receive an adjustment may perceive unfairness.

Even if management has legitimate reasons and fair intentions in not performing discretionary ex-post goal adjustments, employees may nevertheless feel unfairly treated due to egocentric bias (Babcock and Loewenstein, 1997; Epley, Caruso, and Bazerman, 2006; Pronin, 2008). Egocentric bias results from individuals' propensity to disregard others' thoughts and intentions when making judgments because these internal thoughts and intentions are not immediately available to the individual and thus effortful to infer (Pronin 2008). Instead, individuals have a tendency to over-rely on observable behaviour, which is relatively less effortful, resulting in egocentrically biased judgments. Several pervasive human tendencies have been attributed to egocentric bias, such as the actor-observer bias (Jones and Nisbett 1972) and the tendency for individuals to overestimate the degree to which others share their beliefs and preferences (Pronin 2008). In my setting, egocentric bias causes employees to underweight management's legitimate reasons for not performing ex-post adjustments, and overweight management's perceived non-helping behaviour in assessing the fairness of management's actions. Therefore, I next predict that absent an intervention aimed at mitigating egocentric bias, in response to management's non-helping behaviour, employees will continue to disregard management's legitimate reasons for not performing ex-post adjustments in their fairness judgments, resulting in a deterioration of justice perceptions and performance.

In an organizational context, explanations are causal accounts or justifications provided by management to employees for management decision making (Colquitt and Chertkoff 2002). Research on explanations have shown that when employees receive unfavourable outcomes, such as having their input ignored, a sincere, logical, and thorough explanation from management for these unfavourable outcomes can improve employee perceptions of fairness and performance (Libby 1999, Colquitt and Chertkoff 2002). Therefore, I predict that when management exhibits

non-helping behaviour by not performing ex-post goal adjustments, providing an explanation will improve employee justice perceptions and performance relative to employees that receive no intervention at all.

Perspective taking refers to the process whereby an individual, referred to as the perspective taker, imagines or simulates the internal state (e.g. thoughts, feelings, and intentions) of another individual, referred to as the perspective taking target (Davis, Conklin, Smith, and Luce 1996). Research on perspective taking has shown that taking the perspective of others can mitigate the effect of egocentric bias in fairness judgments by increasing other regarding thoughts (Epley et al., 2006). If employees are asked to simulate management's perspective with respect to the difficult decision of making ex-post adjustments, then management's legitimate reasons for not performing ex-post adjustments may become more available to employees when making fairness judgements, reducing the impact of egocentric bias. Therefore, I predict that when management exhibits non-helping behaviour by not performing ex-post goal adjustments, if employees take part in a perspective taking training exercise, their organizational justice perceptions will improve, resulting in increased effort and performance relative to employees that receive no intervention.

Since both explanation and perspective taking are predicted to improve employee performance through justice perceptions, it is unclear what the interactive effects will be if both interventions are administered to the same employee. For example, if management's explanation completely attenuates the negative fairness effects of observed non-helping behaviour, then there may be little scope for perspective taking to further improve justice perceptions and performance. However, it is also possible that perspective taking, by combatting egocentric bias directly, improves employees' receptiveness to management's explanation, resulting in

explanation and perspective taking acting as complements. Therefore, in addition to the predictions described above, I pose a research question about the interactive effects of explanation and perspective taking on employee justice perceptions and performance.

To test my predictions, I conduct a laboratory experiment using undergraduate students as participants. Participants work in a two-task setting over four four-minute production rounds, where both tasks are simple, effort-sensitive, computer-based tasks. The two tasks used are the number counting task based off Abeler, Falk, Goette, and Huffman (2011), and the letter decoding task based off Chow (1983). Further, one of the tasks is characterized as participants' primary task (counting task), performance on which is incentivized through a bonus for goal attainment incentive scheme. Participants have an opportunity to earn a bonus in each production round for meeting an output goal on the counting task, where output goals are individualized and based off practice round performance, and earn a small wage based on time spent on the decoding task. Participants are responsible for allocating their time in each round between the two tasks and can switch between tasks at will. Participants face a volatile production environment on the counting task, such that their performance can be significantly impacted by negative exogenous shocks. Ex-ante, they are aware of the possibility of negative shocks to performance, but not the frequency or timing of these shocks.

I randomly assign participants to one of five experimental conditions in a 2x2 full factorial design with a control condition. I manipulate the presence or absence of two fairness interventions, explanation and perspective taking. Further, in the non-control conditions, participants learn of and are eligible for ex-post goal adjustments to compensate for the occurrence of negative shocks to performance on the counting task, whereas in the control condition, participants do not learn of and are not eligible for such adjustments. My primary

dependent variables are perceptions of overall justice and performance. Justice perceptions are measured using a self-reported survey measure adapted from Kelly et al. (2015), administered three times throughout the experiment: 1) after learning of the basic bonus for goal attainment incentive scheme, 2) just prior to the production rounds, and 3) immediately after the end of the production rounds. Performance is measured as the units of production completed on the counting task in each of the production rounds.

In total, I recruit 169 undergraduate business students from two large Canadian universities to test my predictions. Contrary to expectations, I do not find support for my hypothesis that the announcement of an ex-post goal adjustment policy improves justice perceptions but do find some support for my prediction that it improves performance in the face of negative uncontrollable events. Furthermore, I find evidence that after not receiving a goal adjustment (non-helping behaviour), participant perceptions of overall justice and performance deteriorate, but that this effect is not contingent on the availability of ex-post adjustments. With respect to my justice interventions, I find that both providing an explanation and perspective taking training improve justice perceptions after observing non-helping behaviour, and that perspective taking training further improves performance, whereas explanation does not. With respect to my research question, I observe that explanation and perspective taking have a negative interactive effect on overall justice perceptions, but no such interactive effect on performance. Further, the nature of the negative interactive effect of explanation and perspective taking on justice perceptions is consistent with explanation and perspective taking acting as substitutes with respect to justice perceptions.

In addition to my formal tests of hypotheses, I examine how the theoretical model used to derive my predictions fits my data and find superior fit using an alternative model where

expectancy of goal attainment, the perceived likelihood of achieving one's goals, mediates the effect of justice perceptions on performance. I also perform supplemental analyses focused on effort duration, the amount of time directed towards the primary task, and effort intensity, the amount of attentional resources directed towards current performance (Bonner and Sprinkle 2002). I measure effort duration the amount of time spent by participants on the primary task each round, and measure effort intensity as performance scaled by effort duration. Analyses of duration and intensity further show that the performance effects observed in my formal tests of hypotheses are driven by changes in effort duration in difficult rounds, and effort intensity in normal rounds.

I believe my study makes several important contributions to management accounting and psychology research. First, I contribute to a small but growing management accounting literature on how ex-post adjustments affect employee performance, building on work by Kelly et al. (2015), Arnold and Artz (2015), Burt, Libby, and Presslee (2019), and Cai, Gallani, and Shin (2019). Importantly, while each of the above studies examine the consequences of performing ex-post adjustments, none examine specifically the effect of the announcement of ex-post adjustments prior to management's first opportunity to exercise the policy. This setting is important to study because in practice, if targets are set on an annual basis, there may be considerable time between the announcement of an ex-post adjustment policy and its first application. My finding of a positive announcement effect on employee performance in difficult rounds suggests that employees change their effort choices in response to the content of management announcements.

Further, none of the above studies on ex-post adjustments examine the consequences of selectively exercising the policy. This is important to study because as described above, the

selective exercise of a helpful policy can potentially undermine the positives of announcing the policy in the first place. Further, this setting has ecological validity, since prior studies (e.g. Höpfe and Moers 2011; Bol et al. 2015) show that in practice, such policies are exercised selectively. I find that observed non-helping behaviour does not significantly impact justice perceptions, and only has a directionally negative impact on performance, suggesting that existing management practice of selectively exercising ex-post adjustments may not be detrimental to employee justice perceptions and performance.

My study also contributes to a large body of organizational justice research examining the organizational consequences of justice perceptions (see Cohen-Charash and Spector 2001, Colquitt et al. 2001, or Colquitt et al. 2013 for reviews). First, building on Libby (1999) and Colquitt and Chertkoff (2002), I examine the effectiveness of explanation at improving justice perceptions and performance, finding explanation to be an effective fairness intervention, but unsuccessful at improving performance. This finding highlights the equivocal nature of the justice perception to performance relationship described by Colquitt et al. (2001). Second, to the best of my knowledge, I am one of the few studies examining the use of perspective taking as a fairness intervention in an organizational setting, building on work by Epley et al. (2006). My findings suggest that perspective taking can be an effective fairness intervention that also improves task performance. Further, results of my mediation analyses show that the justice perception to performance relationship is mediated by expectancies, furthering our understanding of the justice perception to performance relationship.

The remainder of this dissertation is organized as follows. Chapter 2 reviews the literature on subjectivity in compensation contracting, organizational justice, explanations, and perspective taking, which are relevant to the current investigation. Chapter 3 develops my four

sets of formal hypotheses and my set of research questions. Chapter 4 describes the experimental method through which I test my hypotheses and research question, and Chapter 5 summarizes the results of my experiment. Finally, I discuss my results, including limitations and implications in Chapter 6, as well as provide some concluding remarks.

CHAPTER 2: LITERATURE REVIEW

2.1 Introduction

In this chapter, I review accounting and psychology literature to describe the ex-post adjustment setting and examine how organizational justice and egocentric bias shape how employees react to this setting. The remainder of the chapter is organized as follows. In section 2.2, I review the literature examining subjectivity in compensation contracting. In section 2.3 I provide an overview of the organizational justice literature, particularly how it relates to my economic setting, and to organizational outcomes such as performance. In section 2.4 I describe egocentric bias, how it affects management control system design, and how it can be overcome through perspective taking. In section 2.5 I provide some concluding remarks.

2.2 Subjectivity in Compensation Contracting

In this section, I describe my economic setting. From an efficient contracting perspective, I explain the need for subjectivity in compensation contracts, and I provide a broad overview of the various forms that subjectivity can take in compensation contracts. I also review existing literature relating to ex-post adjustments, which is the particular form of subjectivity under investigation in my research.

2.2.1 An Overview of Subjectivity in Compensation Contracting

Employees perform a variety of tasks within organizations. The performance outcomes of some tasks can be easy to objectively measure and verify. For example, for sales staff whose main role is to secure sales, total dollar sales in a given period is a performance outcome that is

both measurable and verifiable by a third party. However, to the extent that the performance outcomes for some tasks are difficult to measure and or verify, management must use some form of subjectivity in evaluating performance outcomes for employees. Consistent with this reasoning, the management and accounting literatures have well documented the importance and ubiquity of subjectivity in compensation contracting in practice (e.g. Prendergast 1999; Ittner, Larcker, and Meyer 2003; Gibbs, Merchant, Van der Stede, and Vargus 2004; Höpfe and Moers 2011).

Bol (2008, p. 2) defines subjectivity from a compensation contracting perspective in the following way:

“subjectivity entails judgment based on personal impressions, feelings, and opinions, rather than on external facts. The correctness of a subjective assessment cannot be determined by a third party. This means that, by its very nature, a subjective assessment is unverifiable for contracting purposes.”

There are several ways in which management can introduce subjectivity into compensation contracts, such as in the choice of contract to offer employees (Kuang and Moser 2009), choice of performance measures and their relative weights to be used in evaluations (Ittner et al. 2003), or in the setting of and design of goals (Webb 2004; Anderson, Dekker, and Sedatole 2010).

In addition to these, Bol (2008) asserts that one of the most important ways in which subjectivity plays a role in compensation contracting is in evaluating employee performance after performance has occurred (i.e., ex-post). Bol (2008) further provides a useful typology for describing subjectivity in ex-post performance evaluations as relating to one of three categories: 1) subjective performance measures, 2) subjective weighting of performance measures, or 3)

subjective ex-post adjustments to measured performance.¹ The third category is the specific form of ex-post performance evaluations under investigation in my research, and will be examined more thoroughly in subsection 2.2.3.

There are many benefits to including subjectivity in compensation contracts. As described by Holstrom and Milgrom (1991), some aspects of employee performance, such as physical outputs, are easy to objectively measure and contract on. However, employee activities that affect asset prices (e.g. asset maintenance, generating goodwill), are often prohibitively costly to measure and contract on. Further, to the extent that management relies solely on objective performance measures to evaluate employees, employees may excessively focus on the objective measure to the detriment of overall firm value (Holmström and Milgrom 1991; Choi, Hecht, and Tayler 2012). For example, employment agencies who set client placement rate goals for employees may find that their employees excessively focus on ‘easy to place’ employees to the detriment of needier clients (Grizzle 2002).

Further, contracting on specific objective performance measures prior to the measurement period (i.e., ex-ante) allows employees some opportunity to manipulate their measured performance to maximize their compensation per their contract. For example, for many firms, accounting income is an important performance metric for executive employees (Xu, Taylor, and Dugan 2007). There is a vast accounting literature (see for example Healy and Wahlen 1999, or Xu et al. 2007) documenting the actions that executive employees acting as agents for the firm perform to manipulate accounting income in part to maximize their

¹ Subjective measures used to evaluate employee performance could include a superior’s rating of the employee’s perceived attitude or helpfulness, which cannot be verified. Ex-post subjective weighting involves explicitly specifying performance measures in a contract, but allow the weighting of measures in compensation decisions to be subjective (Höppe and Moers 2011). Ex-post adjustments allow for managers to consider information other than that explicitly contracted on to make adjustments to employee compensation.

incentives, such as changing the level of discretionary accruals (Bergstresser and Philippon 2006) or reducing discretionary spending such as on research and development (Roychowdhury 2006; Zang 2012). Introducing ex post subjectivity into such compensation contracts is beneficial because it allows management to adjust compensation based on suspected manipulation, both directly mitigating the impact of performance measure manipulation, and indirectly discouraging it through management's ability to make such adjustments (Bol 2008).²

Another benefit that accrues from introducing subjectivity into compensation contracting relates to the flexibility that it provides management to adjust compensation for information unavailable ex-ante. In efficient contracting, management offers an employment contract to employees based on the information available ex-ante (Baiman and Rajan 1995). However, events outside an employee's control can alter their measured performance, introducing volatility to their compensation, for which a risk premium must be paid (Holmström 1979). Management can reduce this risk for employees, and the related compensation premium, by committing ex-ante to subjectively consider additional factors influencing employee performance ex-post (Gibbs et al. 2004; Höppe and Moers 2011). Another benefit of this flexibility is the ability for management to adapt their compensation contracts to match changes in their business environment (Bol 2008). For example, in response to critical online reviews of the sales experience, management may believe that customer satisfaction is now more important than before. Incorporating subjective weighting of performance measures in the compensation contract ex-ante allows management to increase the weight on customer satisfaction after the end

² After the Sarbanes-Oxley Act was passed, many firms adopted 'clawback' provisions to executive compensation contracts, allowing firms to recoup compensation paid to executives if financial misreporting came to light (Dehaan, Hodge, and Shevlin 2013; Chan, Chen, Chen, and Yu 2014).

of contract period, allowing for re-alignment of compensation contract to firm strategy without the need to renegotiate the contract (Bol 2008).

However, the introduction of subjectivity to compensation contracts is not without cost. Just as subjectivity can be introduced to correct distortions to compensation contracts it can introduce its own distortions to compensation contracts through biased performance evaluations on the part of managers, and through encouraging rent-seeking behaviour on the part of employees (Prendergast and Topel 1993). Prendergast and Topel (1993) emphasize that in most organizations, management decision making is undertaken by employees, who to some degree, make decisions affecting compensation contracts subject to their own preferences, beliefs, and biases.

The accounting literature has examined a variety of decision-making biases exhibited when subjectivity is introduced in compensation contracts. For example, the common measures bias describes the tendency for managers to over-weight performance measures common to business units over measures that are unique to the business units (Lipe and Salterio 2000; Libby, Salterio, and Webb 2004). Similarly, Ittner et al. (2003) find that managers have a tendency to overweight financial versus non-financial performance measures. Bol (2011) finds that managers' subjective performance evaluations of employees exhibited leniency bias (upwards bias) and centrality bias (ratings compression), in part to avoid costly confrontation and information gathering costs. Ding and Beaulieu (2011) find that even the mood of the evaluator can significantly influence subjective performance evaluations. These managerial decision-making biases create distortions to compensation contracts as they introduce measurement error to performance evaluations, weakening the incentives provided by the compensation contract (Bol 2008). Further, because managerial decision making is subject to the personal preferences

and beliefs of the manager, opportunistic employees may shift their efforts away from productive tasks and towards rent-seeking behaviour meant to improve their subjective performance evaluations (and thus compensation) through image management (Milgrom 1988; Prendergast and Topel 1993).

Another class of costs associated with subjectivity in compensation contracting relates to the uncertainty it imposes on employees (Bol 2008). Because subjective evaluations are unverifiable, employees must make assumptions about the managers' beliefs and intentions who are making such evaluations. In the case of subjective adjustments (e.g., ex-post adjustments), employees must trust that managers will exercise their discretion fairly to reduce distortions to payoffs from the compensation contract (Baiman and Rajan 1995). Uncertainty over how managers will exercise the discretion bestowed upon them weakens the incentives provided by the contract (Holmström 1979). In particular, employees' perceptions of fairness relating to the use of management discretion could potentially have important consequences for employee motivation and productivity. This idea is central to my research and will be examined further in section 2.3.

In summary, the need for subjectivity in compensation contracting arises due to the imperfect nature of objective performance measures in capturing all aspects of employee performance pertinent to firm value. The incorporation of subjectivity in compensation contracts can reduce distortions caused by uncontrollable factors, but can also introduce its own distortions in the form of biased managerial decision making and employee uncertainty over how their managers will use the discretion.

2.2.3 Ex-post Adjustments

As described in the preceding section, management establishes compensation contracts with the information available *ex-ante*. However, unforeseen events outside of the control of employees may occur during the contract period that are pertinent to evaluating employee performance. In order to control for the impact of uncontrollable events on employee performance and compensation, management may commit *ex-ante* to *consider* performing *ex-post* adjustments to performance measures to neutralize the impact of uncontrollable events.

Much of the management and accounting literature on *ex-post* adjustments has focused on how and when managers choose to use their discretion to perform them. In a survey of profit centre managers, Merchant (1989) provides some evidence that organizations make *ex-post* adjustments to control for the effect of uncontrollable events. Merchant (1989) finds that organizations differ greatly in the extent to which they adopt *ex-post* adjustments, with some firms making adjustments to completely neutralize the effect of uncontrollable events, and others not performing any *ex-post* adjustments. Similarly, in a survey of German firms, Arnold and Artz (2015) find that organizations vary in their degree of target flexibility, *i.e.*, their willingness to perform *intra-period* adjustments to performance targets.³ Consistent with these findings, Höpfe and Moers (2011) find that 24.3 percent of firms who disclose the use of *ex-post* bonus adjustments in their SEC Proxy Statements actually exercise the option to do so. Clearly, there is considerable variation in how firms choose to operationalize *ex-post* adjustments.

³ Arnold and Artz (2015) study *intra-period* adjustments, which occur *during* the contracting period, as opposed to *after* the end of the contracting period, as described in other studies examining *ex-post* adjustments (e.g. Bol et al. 2015; Kelly et al. 2015).

Merchant (1989) reasons that for some organizations, particularly ones that have a more decentralized structure, employees (in this case profit centre managers) are expected to react and adapt to uncontrollable events to mitigate their effects on the organization. In line with this reasoning, Bol et al. (2015) find in an experiment that managers are less likely to exercise discretion to make ex-post adjustments to offset the negative impact of uncontrollable events when the likelihood of future uncontrollable events is high compared to when the future likelihood is low. Interestingly, Höppe and Moers (2011) find that the use of ex-post adjustments as a contracting feature is positively associated with the noisiness of objective performance measures, as it would be when the likelihood of future uncontrollable events is high.⁴ This means that although ex-post adjustments are most likely to appear in contracts when the likelihood of uncontrollable events is high, managers in these situations are not always willing to exercise their discretion in order to try and induce adaptive behavior in their subordinates.

Bol et al. (2015) also find in an experiment that where employee compensation is highly interdependent, in this case a fixed bonus pool setting, managers choose to make fewer ex-post adjustments in order to avoid a negative reaction from employees unaffected by the negative event.⁵ This is because in a fixed bonus pool setting, any adjustments to employee compensation are a zero-sum game, and so any positive adjustments to employees affected by negative events necessarily involves a reciprocal negative adjustment to other employees. Interestingly, Woods (2012) finds in a field study that the majority of ex-post adjustments performed are positive.⁶

⁴ Höppe and Moers (2011) describe noise as any information that becomes available ex-post that is decision irrelevant (i.e., does not change optimal course of action), such as uncontrollable events.

⁵ Bol et al. (2015) explain that due to the scarcity of resources within firms, and the importance of performance evaluations to promotion and other reward allocation decisions, employees' compensation are interdependent to some degree in most firms.

⁶ Woods (2012) examines ex-post adjustments more broadly, not exclusively fixed bonus pool settings as examined in Bol et al. (2015).

When taken together, these findings suggest that in practice, managers may exhibit leniency bias in ex-post adjustment decisions in order to avoid confrontation and information gathering costs.⁷

There is also a small but growing accounting literature examining the implications of ex-post adjustments on employee motivation and performance. Kelly et al. (2015) find in an experiment that exercising ex-post adjustments to employee goals in response to uncontrollable events had a positive impact on employee perceptions of fairness and performance when initial goals were moderately difficult, but not when initial goals were highly difficult.⁸ Similarly, in a field study, Cai et al. (2019) find that employees directly benefiting (losing) from management exercise of ex-post adjustment exhibited higher (lower) performance in subsequent periods. They find evidence for these effects operating under two different channels: 1) an informativeness channel, and 2) a motivational channel. The informativeness channel describes an effect whereby management exercises discretion to correct for the impact of negative uncontrollable events on employee performance that distort outcomes on objective measures of performance. Assuming no negative event occurs for this employee in the subsequent period, objectively measured performance should revert to ‘normal’ levels. Consistent with Kelly et al. (2015), the motivation channel described by Cai et al. (2019) refers to employees responding reciprocally to management’s actions benefitting or hurting them with increased or decreased effort respectively.

In contrast to the above two studies, based on survey results, Arnold and Artz (2015) find a negative association between target flexibility, the likelihood of intra-period adjustments, and

⁷ This is also consistent with Bol (2011), who finds in a field study that subjective performance evaluations exhibit centrality and leniency bias due to managers trying to avoid information gathering and confrontation costs.

⁸ Kelly et al. (2015) explain that when initial goals are difficult, ex-post adjustments may not always help employees attain their goals, attenuating any positive effects of ex-post adjustments on perceptions of fairness, effort, and performance.

firm performance. They reason that opportunistic employees withhold costly effort in anticipation of a target adjustment from managers. In an experiment, Burt et al. (2019) similarly find that the availability of ex-post adjustments has a negative impact on employee performance when the degree of identity (feelings of connectedness, similarity) between managers and subordinates is low. However, Burt et al. (2019) attribute their findings to decreased expectancy of goal attainment when manager-subordinate identity is low due to lack of trust over management's use of discretionary ex-post adjustments to help them attain their goals.

Thus, existing literature is equivocal on whether ex-post adjustments have a positive or negative impact on employee performance. Further, none of the four above studies distinguish between the availability of and exercise of ex-post adjustments.⁹ This distinction is important because as described above, there are a variety of factors that reduce the likelihood of managers actually exercising discretion to perform ex-post adjustments. This suggests a need for further research examining the consequences of managers exercising discretion selectively on employee performance, which is the focus of the current investigation.¹⁰

To summarize, there is considerable variation in the extent to which firms allow for ex-post adjustments and how managers choose to exercise discretion to perform these adjustments. In addition to the efficient contracting reasons to reduce distortions in compensation contracting, managers' decision of whether or not to exercise ex-post adjustments can be motivated by

⁹ Both Merchant (1989) and Arnold and Artz (2015) examine firms that differ in their attitudes and practices with respect to ex-post adjustments, and Arnold and Artz (2015) additionally examine how attitudes towards target adjustments (target flexibility) impact performance. The current investigation differs from these two studies in that I focus on employees as the unit of observation, whereas the other two studies focus on firms.

¹⁰ Based on the above discussion, another fruitful avenue for research might be to examine factors influencing managers' decision to exercise discretion to perform ex-post adjustments. However, there is already a considerable literature examining this or similar topics (e.g. Bailey et al. 2011; Bol 2011; Maas et al. 2012; Bol et al. 2015), and so I instead focus on the implications of managerial discretion on employee performance.

desires to induce adaptive behaviour or to reduce personal information gathering and confrontation costs. There is also mixed evidence on the consequences of ex-post adjustments for employee motivation and performance. In the next section, I examine the motivational consequences of ex-post adjustments through the lens of organizational justice.

2.3 Organizational Justice

In this section, I provide an overview of the organizational justice literature and explore the relationship between justice and organizational outcomes.¹¹

2.3.1 An Overview of Organizational Justice

Organizational justice as a field of study is concerned with how outcomes, procedures, and interactions between employees, managers, and their organizations affect perceptions of fairness in the workplace (Colquitt et al. 2001). Fairness is a multi-dimensional construct that is made up of four dimensions: distributive justice, procedural justice, interpersonal justice, and informational justice; the latter two sometimes combined as a single dimension, interactional justice (Colquitt et al. 2001). Perceptions of distributive justice are based on employee perceptions of outcome fairness, which are arrived at by comparing outputs (incentives, rewards) to inputs (effort) (Adams 1965). An output to input ratio that is significantly smaller than a referent other's is judged to be unfair (Adams 1965). Perceptions of procedural justice are affected by the process by which organizational decisions are made. Procedures are judged as fair if they follow six 'Leventhal criteria', that is, if they are: applied consistently, based on

¹¹ Goldman and Cropanzano (2015) argue that justice and fairness are separate constructs, with justice describing the adherence of actions to norms and values, and fairness describing individuals' reaction to justice. However, following Cohen-Charash and Spector (2001) and Colquitt et al. (2001), I use these terms interchangeably throughout this dissertation.

accurate information, free from decision maker bias, appealable (process in place for employees to appeal unfair outcomes or procedures), ethical, and representative of all parties affected by the decision (Thibaut and Walker 1975; Leventhal 1980). Interpersonal justice and informational justice are both concerned with the operationalization of organizational rules and procedures (Bies and Moag 1986). Interpersonal justice relates to the manner (e.g. courteousness, sincerity) in which procedures and outcomes are communicated to employees, whereas informational justice relates to the content (e.g. validity, adequacy) of these communications (Colquitt et al. 2001).

2.3.2 Justice and Organizational Outcomes

Accounting and psychology academics have long been concerned with the impact of organizational justice perceptions on important organizational outcomes. Cohen-Charash and Spector (2001) group organizational outcomes examined in the literature into four broad categories: 1) individual performance, 2) organizational citizenship behaviours, 3) counter-productive work behaviours, and 4) emotional or attitudinal reactions. Since my dissertation is primarily concerned with individual performance as an organizational outcome, the focus of this subsection is on the relationship between organizational justice and performance.

According to equity theory (Adams 1963; 1965), employees strive for distributive justice in the ratio of their inputs to the organization (e.g. task effort) and the outputs received (e.g. compensation). If an employee perceives an output to input ratio that is unfair, they can adjust their inputs up or down respectively to restore their sense of distributive justice (Adams 1963; Griffeth, Vecchio, and Logan 1989). For example, if an employee believes that they are being underpaid for their efforts, they can respond by withholding effort in the future, leading to

reductions in performance. Conversely, if an employee believes that they are being overpaid for their efforts, they may decide to exert even more effort in the future to return the perceived input to output ratio to a fair level.¹² Based on this reasoning, equity theory predicts a positive relationship between perceptions of distributive justice and future performance.

Employing a different perspective, social exchange theory considers organizations “arenas for long-term mutual social transactions between the employees and the organization” (Cohen-Charash and Spector 2001, p 285). This stands in contrast to the more economic and concrete nature of the resource exchange described by equity theory (Adams 1963). Using social exchange theory as a lens, procedural, interpersonal, and informational justice are desired outcomes for employees in their interactions with managers (Colquitt et al. 2001). In exchange for fair procedures and interactions, employees reciprocate with high task performance (Wayne, Shore, Bommer, and Tetrick 2002). Based on this perspective, social exchange theory predicts a positive relationship between perceptions of procedural, interactional, and informational justice and performance.

Based on the above theoretical discussion, it seems plausible to expect the psychology literature to support a strong positive relationship between each of the four justice dimensions and performance. However, when considering the literature as a whole, the evidence is equivocal on the direction and significance of the relationship between the four justice dimensions and performance (Cohen-Charash and Spector 2001; Colquitt et al. 2001). In fact, Colquitt et al. (2001, p 430) describes the relationship between justice perceptions and performance as “perhaps the most unclear of all relationships in the justice literature”. For example, in a

¹² Adams (1963) explains that in assessing the fairness of input to output ratios, employees compare their ratio to a reference point, which could include a co-worker’s ratio if known or industry benchmarks.

laboratory experiment, Griffeth et al. (1989) find that participants who were underpaid relative to other participants subsequently performed worse on a proof-reading task, supporting a positive relationship between distributive justice and performance. Similarly, in a survey-based field study, Wayne et al. (2002) find a positive relationship between employees' ratings of distributive and procedural justice and their performance ratings from a supervisor.¹³ In contrast, in a field study, Aryee, Budhwar, and Chen (2002) find no correlation between distributive justice and task performance, but find a positive and significant correlation between procedural and interactional justice and task performance. To further complicate matters, in a survey-based study on employee reactions to punishments, Ball, Trevino, and Sims (1994) find that distributive justice positively impacted subsequent performance, but that procedural justice did not.¹⁴

To help shed light on the equivocal nature of the findings with respect to justice perceptions and organizational outcomes, meta-analyses of psychology research in this area have been conducted (e.g. Cohen-Charash and Spector 2001; Colquitt et al. 2001). Colquitt et al. (2001) find that perceptions of procedural justice are moderately positively correlated with task performance ($r = 0.30$, $p < 0.01$, two-tailed), but that distributive justice perceptions are only weakly correlated with task performance ($r = 0.13$, $p < 0.01$, two-tailed). Similarly, Cohen-Charash and Spector (2001) find that in their sample of field studies, procedural justice ($r = 0.47$, $p < 0.01$, two-tailed) is the strongest correlate of future performance, with distributive justice perceptions weakly correlated to future performance ($r = 0.15$, $p < 0.01$, two-tailed). However, in

¹³ Wayne et al. (2002) find that the positive effect of justice perceptions on performance ratings is mediated by perceived organizational support (quality of employee-organization relationship) and organizational citizenship behaviours.

¹⁴ Ball et al. (1994) measure distributive justice as employees' subjective assessment of the 'harshness' of punishments, ranging from reprimands to lay-offs, received by them relative to other employees who have performed similarly. Ball et al. (1994) argue that this represents a distributive justice concern and not an interactional justice concern as it asks the employee to compare the severity of their punishment (a quality of the outcome) to other employees in similar situations creating a reference point.

their sample of laboratory studies, they find that distributive and procedural justice are only weakly correlated to future work performance ($r = 0.06$ and 0.13 , $p = 0.11$ and $p < 0.01$, two-tailed, respectively). Taken together, the results of these meta-analyses supports a positive relationship between justice perceptions and performance, particularly the relationship between procedural justice and performance.

In contrast to psychology research, accounting research generally finds a positive and significant relationship between procedural and distributive justice perceptions and performance. In a laboratory experiment, Libby (1999) finds that pseudo-participation, where employee input to a budgeting process was obtained but ignored, leads to negative impacts on performance through procedural justice, but that fairness is restored and performance enhanced through an explanation. In another laboratory experiment, Libby (2001) finds that unfair procedures (in budget setting) leads to poor performance, but only when the outcome of the procedures (assigned budget) is perceived to be unfair as well. In a field study, Wentzel (2002) finds that voice, operationalized as participation in a budgeting process, positively affects perceptions of procedural justice, and that this positively impacts performance through goal commitment. In a survey of managers, Lau and Moser (2008) find that their sample of managers typically find the use of non-financial measures in performance evaluations to be procedurally fair, and that perceptions of procedural justice are positively associated with performance through organizational commitment. In a field study, Burney, Henle, and Widener (2009) investigate a complex model of justice and performance. They find that perceptions of distributive justice are positively associated with perceptions of procedural justice, and that procedural justice

perceptions positively impact performance ratings.¹⁵ Finally, Kelly et al. (2015) find in a laboratory experiment that the exercise of ex-post adjustments to predetermined performance goals led to improved perceptions of procedural justice and performance, but only when the original goals are moderately difficult.

In summary, psychology and accounting literatures have long been concerned with the impact of justice perceptions on organizational outcomes, such as performance. Psychology research provides mixed evidence on the impact of justice perceptions on performance. However, meta-analyses of psychology literature, as well as the accounting literature support a positive relationship between justice perceptions and performance.

2.4 Egocentric Bias

In this section, I examine egocentric bias, a decision-making heuristic. This discussion is relevant to the current investigation because egocentric bias can potentially impact employee reactions to the selective exercise of ex-post adjustments, and may potentially impact the effectiveness of fairness interventions. I first discuss the source of egocentric bias before moving on to discuss two interventions that have potential to combat egocentric bias: providing an explanation, and perspective taking.

2.4.1 Source of Egocentric Bias

Egocentric bias is a form of the availability heuristic (Tversky and Kahneman 1973) whereby individual judgments of self and others are biased by the lack of availability of

¹⁵ The effect of procedural justice perceptions on performance ratings are mediated through observed organizational citizenship behaviours, i.e., employees who observe fair procedures reciprocate through beneficial extra-role behaviours, which in turn positively influence management ratings of performance.

information relating to others' internal states, beliefs, and intentions (Jones and Nisbett 1972; Pronin 2008). When individuals make judgments about others, the thoughts and intentions of the others are not immediately available to them and thus are effortful to infer and incorporate into judgments. Instead, individuals opt to incorporate more readily available information into their judgments, such as the observed behaviour of others (Pronin 2008). Egocentric bias in evaluations of the self and other is responsible for a variety of pervasive human behaviours (Pronin 2008).

For example, the actor-observer bias describes the tendency for individuals to consider contextual factors when judging their own actions, but not the actions of others (Jones and Nisbett 1972). This occurs because the contextual factors affecting one's own behaviour are readily available to incorporate into judgments, whereas the factors affecting others' behaviour are relatively less available (even if known), and so are less readily incorporated into judgments (Pronin 2008). Similarly, individuals tend to believe that others' observable behaviour is sufficient to judge their internal motives and intentions, but that observable behaviour is insufficient to judge internal motives and intentions when they are the subject of judgment (Pronin, Kruger, Savtisky, and Ross 2001). Finally, egocentric bias results in the tendency for individuals to overestimate the degree to which others share their preferences and beliefs (Pronin, Gilovich, and Ross 2004). When these preferences and beliefs conflict with those of others, individuals have a tendency to believe that the other's reasoning is biased, and their own reasoning is objective, due to their own intentions and motivations being relatively more available than the other's (Pronin et al. 2004).¹⁶

¹⁶ Although the accounting literature has examined similar biases, such as availability (Moser 1989), actor-observer bias (Wong-on-Wing et al. 2007), and self-serving attributions (King 2002; Libby and Rennekamp 2012), to the best of my knowledge, egocentric bias has not been examined in accounting research.

Based on the above discussion, it follows that with respect to justice perceptions within organizations, egocentric bias will likely result in a tendency for employees to ignore or discount the fair intentions that others, such as management, may have, instead fixating on observed outcomes (Pronin, 2008). This occurs because the fair intentions of management are relatively less available to them when making fairness judgments than the readily observable actions of management. Thus, employees receiving unfavourable outcomes will tend to fixate on these outcomes, discounting any fair intentions management may have had. Therefore, management must consider the existence of egocentric bias when designing any interventions designed to improve perceptions of fairness.

Although many fairness interventions exist, I choose to focus on providing an explanation and perspective taking. Psychology research has focused on restoring equity of distributions (e.g. Adams 1963; Griffeth et al.1989), satisfying Leventhal criteria (Leventhal 1980), improving process control (e.g. Thibaut and Walker 1978, Libby 1999), or improving interactions between employees and their managers/organization (e.g. Bies and Moag 1986; Bies and Shapiro 1988). Although explanations fall under the last category, and have been studied extensively, to the best of my knowledge, there have been no studies examining the use of perspective taking as a fairness intervention. Further, these two interventions show promise in directly combatting egocentric bias, by providing information about management's internal state (explanation) or by asking employees to infer or simulate management's internal state (perspective taking). In the next two subsections, I discuss explanation and perspective taking as possible fairness interventions.

2.4.2 Providing an Explanation

In the context of the organizational justice and management literature, explanations are justifications or causal accounts provided to employees for the actions of management (Bies 1987; Libby 1999). Explanations are especially important when providing justification for unfavourable outcomes, since as discussed above, employees facing these outcomes may already have a tendency towards feeling unfairly treated. In order for an explanation to be effective at improving perceptions of fairness, it must be perceived as credible (Colquitt and Chertkoff 2002), sincere, and informationally valid (Greenberg 1993; Libby 1999).

Prior studies have shown that providing employees with an explanation for negative outcomes can mitigate their negative reactions to those outcomes (Bies and Shapiro 1988; Shapiro 1991; Libby 1999; Colquitt and Chertkoff 2002). For example, Shapiro (1991) created a setting where members of dyads were led to believe that the actions of their partner resulted in financial losses for both members of the group. Shapiro (1991) found that individuals' desire to punish their partner was less severe when they were provided with an explanation from their partner explaining that their actions were intentional and altruistic compared to when their partner's explanation indicated intentional but selfish motives. Libby (1999) allowed employees the opportunity to provide input into the budgeting process, but ultimately ignored that input. She found that providing an explanation for management's decision to ignore input from employees mitigated the negative performance effects of ignoring their input.

Providing an explanation may be effective at combatting egocentric bias because if it follows Greenberg's (1993) criteria for effective explanations, it should in part communicate

management's internal thoughts and intentions to employees, making them more available to employees when making fairness judgments.

2.4.3 Perspective Taking

Perspective taking refers to the phenomenon whereby an individual, either consciously or subconsciously, imagines the thoughts, feelings, or intentions of another individual, i.e., the perspective taking target (Davis et al. 1996). Perspective taking increases the degree to which the mental representations of the perspective taker and the target overlap with each other, a process described as self-other overlap (Davis et al. 1996; Galinsky, Ku, and Wang 2005). Psychology research has shown that this self-other overlap results in two distinct effects: 1) the internal state (i.e., the thoughts, feelings, and intentions) of the other becomes more accessible to the self when making judgments and decisions, and 2) the other is judged as more similar to the self than before perspective taking, allowing self-evaluations (either positive or negative) to transfer from the self to the other.¹⁷

Using a laboratory experiment to examine perspective taking and negotiations, Galinsky, Maddux, Gilin, and White (2008) find that perspective taking allows buyers to better understand sellers' internal motivations, resulting in better outcomes for both parties. Similarly, Epley et al. (2006) find that taking the perspective of other players in a public goods game increases the availability of others' internal states in fairness judgments and decision making.¹⁸ Important to note is that this effect of perspective taking depends on features of the perspective taker's

¹⁷ To the best of my knowledge, there are no accounting studies examining the use of perspective taking.

¹⁸ In both co-operative and competitive settings, perspective taking increases the degree to which the other's internal state is accessible to the perspective taker, and reduced egocentrism in their fairness judgments (the percentage of a pool of shared resources they were entitled to). However, in the case of competitive games, perspective taking led to heightened selfish behaviour (actual amount drawn from the pool of shared resources). Epley et al. (2006) explain that a competitive setting activates cynical thoughts about others' motives, which result in egoistic behaviour.

information environment. For example, Skorinko and Sinclair (2013) find that individuals taking the perspective of a target with stereotypic features assigned more stereotype consistent attributes to the target. Skorinko and Sinclair (2013) explain that when highly salient, the stereotypic behaviour of the target is used as a basis for forming the other's perspective in perspective taking.¹⁹ Therefore, it appears that when individuals have an accurate basis for understanding the target's perspective, perspective taking can result in greater availability of the target's internal state, and incorporation of this information into judgment and decision making.

Another consequence of perspective taking is that the perspective taking individual feels more similar to the target (Todd, Bodenhausen, Richeson, and Galinsky 2011). Due to self-serving biases, individuals have a tendency to maintain a positive self-concept (Taylor and Brown 1988; Pronin 2008). Since individuals on average maintain a positive self-concept, self-other overlap triggered by perspective taking allows for the subconscious transfer of positive self-evaluations about themselves to the perspective taking target (Todd and Burgmer 2013). For example, in an experiment on stereotyping, Galinsky and Moskowitz (2000) find that when individuals take the perspective of others, they evaluate others more favourably and use less stereotypic language when describing the other. Galinsky and Moskowitz (2000) explain that the process of self-other overlap makes the other seem more like the self, allowing for generally positive self-evaluations to transfer to the other. An important point here is that the effect of automatic transfer of self-evaluations to the other depends critically on the valence of the perspective taker's self-evaluation. For example, Todd and Burgmer (2013) find that perspective taking reduces in-group bias, but not when participants were measured or manipulated to have

¹⁹ Skorinko and Sinclair (2013) use pictures of a hospitalized old man, and a forlorn looking old man with a cane as their stereotype consistent perspective taking targets, and pictures of an old man sitting next to a newspaper stand and a heashot of an old man as their ambiguously stereotypic perspective taking targets.

negative self-evaluations. Todd and Burgmer (2013) reason that when the perspective taker has a negative self-evaluation, there are no positive self-evaluations to transfer to the target. Similarly, Galinsky and Ku (2004) find that when self-esteem was manipulated downwards through negative feedback, perspective taking did not reduce prejudice as it did when self-esteem was manipulated upwards through positive feedback.²⁰

The above two effects are both informative to predicting how perspective taking impacts fairness judgments. First, to the extent that employees can accurately simulate management's perspective, they should better understand the considerations management must make when deciding whether or not to make ex-post adjustments. Secondly, self-other overlap triggered by perspective taking should increase the degree to which employees feel like they are similar to management, allowing for the automatic transfer of generally positive self-evaluations from the employee to management.

2.5 Conclusion

In this chapter, I described the economic setting of ex-post adjustments, and provided an overview of organizational justice literature, especially as it relates to my outcome of interest, individual performance. I described egocentric bias, a decision making heuristic pertinent to the understanding of how employees form fairness judgments and react to fairness interventions. Finally, I introduced two possible interventions that could potentially mitigate egocentric bias.

Overall, existing literature suggests that while ex-post adjustments may be efficient from a contracting perspective, uncertainty in how managers choose to exercise their discretion

²⁰ Interestingly, neither Todd and Burgmer (2013) nor Galinsky and Ku (2004) find that negative self-evaluations were transferred to the other. The conclusion in both studies is that negative self-evaluations/self-esteem simply suppress the transfer of positive evaluations from the self to the other.

imposes a cost to adopting ex-post adjustments as a contracting feature. Prior research shows that there is considerable variation in the frequency and extent to which firms use ex-post adjustments (Merchant 1989; Höppe and Moers 2011). One cost of ex-post adjustments is that employees, in forming fairness judgments about unfavourable ex-post adjustment outcomes, will have a tendency to fixate on the unfavourable outcome, discounting any fair intentions management may have (Pronin 2008). Employees' biased fairness judgments may then have a negative impact on performance, which may be mitigated through provision of an explanation or through perspective taking. I explore these issues in the next section, where I develop my hypotheses.

CHAPTER 3: HYPOTHESES DEVELOPMENT

3.1 Introduction

In this chapter, I use organizational justice as a theoretical lens to examine the consequences of the ex-post adjustment setting on employee motivation and performance. As described in section 2.2, the contracting literature has identified ex-post adjustments as an efficient contracting feature for addressing the impact of uncontrollable events on employee compensation (Baiman and Rajan 1995; Bol 2008). However, there are costs to ex-post adjustments in the form of employees experiencing uncertainty over how managers will exercise discretion to perform ex-post adjustments. To date, only a few studies (Arnold and Artz 2015; Kelly et al. 2015; Burt et al. 2019) have examined the implications of ex-post adjustments on employee performance. Further, as described in section 2.2, prior research has not distinguished between the availability of ex-post adjustments and the exercise of ex-post adjustments. To address this gap in the literature, the current investigation examines the consequences of both the availability of, and selective use, of ex-post adjustments on employee perceptions of fairness and performance.

In addition to studying the implications of ex-post adjustments on justice perceptions and performance, I also explore the effectiveness of two fairness interventions: explanation, and perspective taking. Explanation and perspective taking both have potential to reduce egocentric bias, which may impact employees' fairness judgments with respect to management's decision of whether or not to exercise ex-post adjustments. Through its impact on fairness judgments, I expect explanation and perspective taking to improve employee performance. Although the

effects of explanation on performance have been studied in prior literature (Libby 1999), to my knowledge, the relationship between perspective taking and performance has received less attention from accounting and management scholars.²¹

The remainder of the chapter is organized as follows. In section 3.2, I describe important features of my setting relevant to the hypotheses development. In section 3.3 I examine the motivational implications of the mere availability of ex-post adjustments. In section 3.4 I develop hypotheses relating to management discretion in the exercise of ex-post adjustments. In section 3.5, I explore the possibility of using explanation and perspective taking as interventions to improve performance. In section 3.6 I provide a conclusion.

3.2 Important Features of my Setting

As described in section 2.2, I employ an ex-post adjustment setting, which entails an explicit compensation contract to employees with the provision that management can subjectively adjust employee compensation in response to new information that arises during the contract period (Baiman and Rajan 1995; Höpfe and Moers 2011). In addition to these features typical in an ex-post adjustment setting, I employ the following important features in my setting: 1) the explicit contract offered to employees includes performance based pay in the form of a bonus for goal attainment, 2) employees work in a multi-task environment, where they choose to devote effort towards the focal task to which the compensation contract relates, or a secondary task, 3) employees work for multiple periods, and 4) I make no assumptions about the prior

²¹ A notable exception here is Parker and Axtell (2001), who examine in a field study the antecedents and consequences of perspective taking, with superior contextual performance (manager ratings of co-operativeness) as one of the consequences of perspective taking. The focus of my investigation is on conventional or objective performance.

relationship between employees and their supervisors. Below I explain the importance and rationale for these features.

A bonus for goal attainment compensation contract involves management offering employees a monetary reward for achieving some benchmark of performance (i.e., a goal) on a given task. The use of performance goals in compensation contracting is ubiquitous and has been shown to be effective in motivating performance gains by employees (Locke and Latham 1990; 2002).²² However, as discussed in section 2.2, events outside of the control of employees may impact their measured performance, making performance a noisy measure of employee effort, distorting the incentives provided by the compensation contract. Because of this, firms that make use of bonus for goal attainment compensation schemes often incorporate ex-post adjustments into their compensation contracts (Libby and Lindsay 2010; Höppe and Moers 2011). Due to the frequency that ex-post adjustments appear in bonus for goal attainment settings, it is an important setting to study.²³

In practice, employees must balance a variety of work responsibilities and choose how to allocate their time and effort. Even where an explicit compensation contract rewards effort on a focal task, employees typically have some secondary task they can perform while at work which can provide utility (Holmström and Milgrom 1991). Some examples of these secondary tasks include administrative duties, or cultivating personal relationships to gain favour with co-workers and supervisors (Prendergast and Topel 1993). Aside from the fact that this feature of

²² Research shows that for goals to be effective, they must be specific, and difficult but attainable (Locke and Latham 2007).

²³ Baiman and Rajan (1995, p. 559) assert that “incomplete contracts are a widely observed phenomenon and the use of discretion to partially complete these contracts is also widespread”. Höppe and Moers (2011) find that 19% of their sample disclosed the use of ex-post adjustments. Arnold and Artz (2015) find that each of the 97 firms surveyed indicated some degree of target flexibility, with no firm indicating that targets were never adjusted intra-year, and average target flexibility (3.57) being above the midpoint on a 5-point Likert scale.

my setting is ubiquitous in practice, making it an important setting to study, it also offers some experimental advantages, which will be elaborated upon in the next chapter.

One of the research objectives of this study is to examine the differing consequences of the announcement and exercise of ex-post adjustments on justice perceptions and performance. This necessitates multiple contract periods to create temporal separation between the announcement and exercise of ex-post adjustments. Finally, in my study, I make no assumptions about the prior relationship between employees and the managers that will be evaluating them. Although prior psychology research has shown that prior relationships affect concerns for fairness (Loewenstein, Thompson, and Bazerman 1989) and perspective taking (Drolet, Larrick, and Morris 1998), due to the already complex nature of my ex-post adjustment setting and fairness interventions, I decided to leave an examination of prior relationships to future research.

3.3 Availability of Ex-Post Adjustments

As discussed above, both the mere availability and the eventual exercise of ex-post adjustments are related but distinct constructs that have potential implications on employee performance. I expect that the mere availability of ex-post adjustments to be salient for employee judgments in the period between policy announcement and the first-time employees observe how management enacts the policy.²⁴ However, I believe that it is still valuable to study this phenomenon. In a real-world setting, if targets are set on an annual basis, the time between the enactment of such a policy and the first-time employees observe the enactment of the policy can

²⁴ Höpfe and Moers (2011) provide evidence from SEC filings that ex-post discretion is a formal feature of employee compensation contracts. Given the formal nature of ex-post discretion, it is likely that it is committed to ex-ante by management.

be considerable. In this subsection, I develop hypotheses with respect to how the mere availability of ex-post adjustments impact employee justice perceptions and performance.

When management announces the availability of ex-post adjustments to neutralize the negative impact of uncontrollable events, I expect this to improve employees' justice perceptions. This is because prior to observing the exercise of ex-post adjustments, employees' justice perceptions will be based solely on management's announced policy as this will be one of the only informational cues with which to form fairness judgments.²⁵ I expect management's announced policy to signal fair intentions to employees and will likely result in improved perceptions of procedural justice through improvements to the Leventhal criteria, accuracy (Leventhal 1980). Accuracy is improved through ex-post adjustments because employees anticipate that the negative performance effects of uncontrollable events will be neutralized through ex-post adjustments. At the beginning of the contract period, since the employee has not yet observed any outcomes, I do not expect employees' perceptions of distributive justice to be affected by the announcement of ex-post adjustments.²⁶

However, this prediction is not without tension. As described in section 2.2, although management's introduction of an ex-post policy is a signal of their fair intentions, employees face uncertainty as to how and when management intends to exercise the policy. In contracting terms, employees may view management's policy as cheap talk, that is, that management has not

²⁵ To the best of my knowledge, there have only been a handful of studies examining the effect of announcements on justice perceptions. Mansour-Cole and Scott (1998) provide evidence that the content and manner in which layoff announcements were announced affect survivors' perceptions of procedural and distributive justice. Roberts (1994) find that public service announcements answering common concerns about the income tax system improved individuals' perceptions of fairness of the taxation system, and improved tax compliance intentions. These two studies provide evidence that individuals attend to the information content of announcements in their formation of fairness judgments.

²⁶ It is possible that when employees perceive fair procedures, they will anticipate fair outcomes as well, which could cause an announcement effect on distributive justice (Burney et al. 2009). Therefore, any impact of the announcement on distributive justice would be mediated through procedural justice perceptions.

committed ex-ante to perform goal adjustments ex-post, only to consider performing them. This uncertainty may undermine the effectiveness of the announcement at improving procedural justice perceptions.

In summary, I expect that the mere announcement of an ex-post adjustment policy will result in improved perceptions of procedural justice in the periods between the announcement of the policy and the first instance of a negative uncontrollable event impacting employee performance. This leads to my first hypothesis, stated in the alternative form:

H1a: Perceptions of procedural justice following the announcement of an ex-post adjustment policy but prior to management's discretionary use of that policy will be more positive relative to a setting where no such policy has been announced.

As discussed in section 2.3, justice perceptions particularly procedural justice perceptions, are positively associated with performance (Cohen-Charash and Spector 2001; Colquitt et al. 2001). Employees value fairness in the procedures enacted by management, and when procedures are judged to be fair, employees reciprocate with increased effort (Cohen-Charash and Spector 2001; Wayne et al. 2002). Further, prior accounting research has shown that even in the presence of a bonus for goal-attainment incentive scheme, justice perceptions are important to individuals and positively impact performance (Libby 1999; Kelly et al. 2015).²⁷ Therefore, to the extent that fairness concerns are important to employees and positively impact performance, I expect that an announcement that impacts justice perceptions to also be positively associated with future performance. Based on the above discussion, I expect performance effects to mirror procedural justice effects in the contract periods immediately following the

²⁷ Both Libby (1999) and Kelly et al. (2015) employ a budget linear incentive scheme where bonuses consist of a fixed amount for attaining the goal, and a piece-rate for production in excess of the goal. This contract elicits effort from employees even after they reach their goals, in theory reducing the scope for fairness concerns to have an impact on performance. Nevertheless, both studies find that fairness concerns are an important determinant of employee effort.

announcement of an ex post adjustment policy. This, leads to my next hypothesis, stated in the alternative form:

H1b: Performance following the announcement of an ex-post adjustment policy but prior to management’s discretionary use of that policy will be greater relative to a setting where no such policy has been announced.

As with my first hypothesis, this hypothesis is not without tension. Findings of Arnold and Artz (2015) call into question the positive impact of ex-post adjustments on performance. They find that target flexibility in the form of intra-year performance target adjustments is negatively associated with firm performance. Their explanation for this finding is that when targets are flexible, employees will opportunistically withhold effort in anticipation of having their targets adjusted downwards.²⁸ My predictions in H1a and H1b are shown graphically in Figure 1, Panel A.

3.4 Manager Discretion to Exercise Ex-Post Adjustments

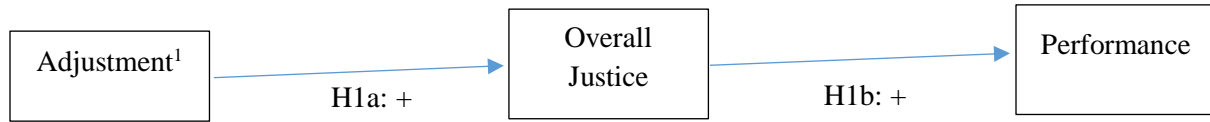
As discussed above, the second important event that has potential fairness and motivational consequences in the ex-post adjustment setting is the extent to which managers actually exercise their discretion to make ex post adjustments. When management has announced an ex-post adjustment policy, and an employee fails to meet their performance goal due to an uncontrollable event, the employee will likely anticipate an ex-post adjustment.²⁹ If management

²⁸ There is a key difference between Arnold and Artz (2015) and the other studies examining the motivational consequences of ex-post adjustments (Kelly et al. 2015; Burt et al. 2019; Cai et al. 2019). First, Arnold and Artz (2015) examine intra-year adjustments, which occur *during* the contract period. To the extent that managers are able to change their effort choices *after* learning of the realization of intra-year adjustments but *before* the end of the contract period, employees in their setting have greater incentives to opportunistically withhold effort. Because adjustments occur ex-post in my study, it would be risky for employees to withhold effort in the expectation of receiving an adjustment, as they risk missing out on their bonus if no adjustment is made.

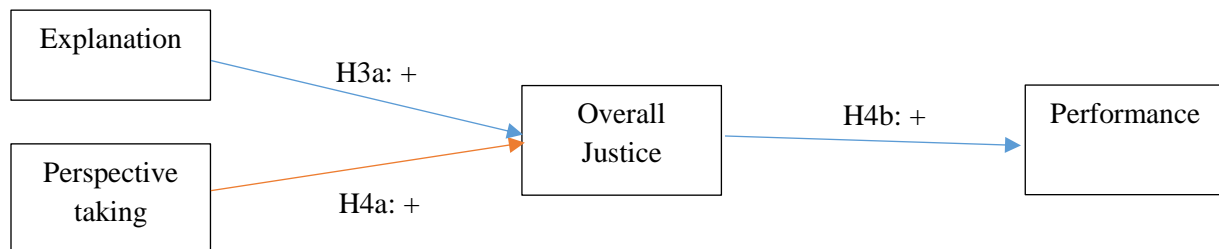
²⁹ I assume that employees recognize that the uncontrollable event caused them to miss their goal. I believe this is a reasonable assumption for the following reasons. First, as described by Merchant (1989) and Bol et al. (2015), the nature of these uncontrollable events is such that employees have superior information about the impact of these events than management. Therefore, if management is in the position to perceive the impact of these events and is

Figure 1: Theoretical framework

Panel A: Before observing non-helping behaviour



Panel B: After observing non-helping behaviour



Notes:

¹ Adjustment refers to the availability of ex-post adjustments as part of the compensation contract.

² Explanation refers to justifications provided by management for observed non-helping behaviour.

³ Perspective taking refers to employees simulating the internal state (thoughts, intentions, feelings) of management.

Summary of hypotheses:

H1a: Before observing non-helping behaviour, the announcement of ex-post adjustments will improve perceptions of overall justice.

H1b: Before observing non-helping behaviour, the announcement of ex-post adjustments will improve performance.

H2a: Perceptions of overall justice will worsen after observing non-helping behaviour.

H2b: Performance will worsen after observing non-helping behaviour.

H3a: After observing non-helping behaviour, an explanation from management will attenuate the worsening of overall justice perceptions.

H3b: After observing non-helping behaviour, an explanation from management will attenuate the decline in performance.

H4a: After observing non-helping behaviour, perspective taking will attenuate the worsening of overall justice perceptions.

H4b: After observing non-helping behaviour, perspective taking will attenuate the decline in performance.

considering an ex-post adjustment, I assume employees understand that the event was responsible for them not attaining their goals. Secondly, if the nature of these events were not so significant as to impact goal attainment, there would be little reason for management to consider making an ex-post adjustment in the first place.

then decides not to perform an adjustment (non-helping behaviour), this will likely have negative implications for perceptions of fairness and motivation. In contrast to H1a, where I do not make predictions with respect to distributive justice perceptions, I expect that post-observation of non-helping behaviour, perceptions of both procedural and distributive justice perceptions will be affected. Employees have now observed how management has distributed rewards, which informs perceptions of distributive justice.

I choose to focus on situations where management decides not to perform adjustments for several reasons. First, there is empirical evidence that management may choose not to make such adjustments even when they are bestowed discretion to do so (e.g. Merchant 1989, Höppe and Moers 2011), making this an important and relevant setting to study. Second, all prior studies on the impact of ex-post adjustments on performance examine settings where ex-post adjustments are made in at least some of the periods (Arnold and Artz 2015; Kelly et al. 2015; Burt et al. 2019; Cai et al. 2019), and so examining a setting where adjustments are not made fills a gap in the literature. Finally, as expanded upon below, I believe that management's non-helping behaviour will have negative consequences for employee justice perceptions and performance, making the examination of this setting and its consequences important.

If management decides not to exercise ex-post adjustments to help employees in response to a negative uncontrollable event, management's perceived inaction will likely counteract positive procedural fairness effects engendered by announcing the policy. This will likely have a negative impact on procedural justice perceptions as employees re-evaluate management's motives for announcing such a policy. Similarly, to the extent that employees were actively engaged in goal pursuit, they had a reasonable expectancy of goal attainment, and may attribute

their failure to attain their expected rewards to management inaction, further negatively affecting perceptions of distributive justice (Folger 1977; Carmichael 1989; Bol 2008).

As described by Bol et al. (2015), management may have legitimate reasons to not make ex-post adjustments to neutralize the effects of uncontrollable events. For example, if employees work in a volatile industry where such uncontrollable events occur often, management may choose to reserve ex-post adjustments for extraordinary circumstances in order to encourage employees to adapt to the common occurrence of uncontrollable events (Merchant 1989; Bol et al. 2015). As another example, where employees' compensation is highly interdependent, as in the case of a fixed bonus pool, management may not be able to make favourable ex-post adjustments for all employees due to the zero-sum nature of adjustments in the fixed bonus pool setting (Baiman and Rajan 1995; Bol et al. 2015). However, due to employees' egocentric bias, it is management's observed behaviour that will be most salient and available to employees when making fairness judgments.³⁰ Thus, I expect the effects described above to dominate any positive impact that the existence of the policy would have on procedural and distributive justice perceptions. Important to note is that these predictions are expected to hold so long as management decides not to perform ex-post adjustments.³¹ As in the case of my first set of hypotheses, based on the established link between both procedural and distributive justice

³⁰ Prior studies (e.g. Burney et al. 2009) find evidence that the receipt of unfavourable outcomes triggers reconsideration about the fairness of procedures used to arrive at those outcomes, resulting in a positive indirect effect of outcome valence on procedural justice perceptions. This further supports my predictions.

³¹ If employees were to subsequently receive an ex-post adjustment in the future, this may attenuate some of the negative effects described. However, in practice, ex-post adjustments are observed to occur with relatively low frequency, even for firms disclosing ex-post adjustments as a part of the compensation contract (24.3%, Höppe and Moers 2011). Further, as described above, I believe that employees *not* receiving an ex-post adjustment has greater implications for justice perceptions and performance, and so my predictions centre around employees *not* receiving ex-post adjustments.

perceptions and performance, I make a prediction about performance in parallel with my fairness prediction.³² This leads to my next set of hypotheses, stated in the alternative form:

H2a: Procedural and distributive justice perceptions following management's decision to not exercise ex-post adjustments will worsen relative to before management's decision to not exercise ex-post adjustments.

H2b: Performance following management's decision to not exercise ex-post adjustments will be lower relative to before management's decision to not exercise ex-post adjustments.

3.5 Explanation and Perspective Taking Interventions

As described in the preceding sections, the announcement of ex-post adjustments is predicted to have positive fairness and motivational consequences, whereas management's subsequent inaction threatens to attenuate these positive effects. In this section, I explore two possible interventions aimed at improving perceptions of fairness and performance. Examining more than one fairness intervention in my setting allows me to compare and contrast the effectiveness of these interventions at improving fairness perceptions and performance.

One of the most intuitive and often-studied fairness interventions in the workplace is providing an explanation for management decision making (Bies and Moag 1983; Greenberg 1993; Libby 1999). In an organizational context, explanations are causal accounts or justifications provided to employees for management decision making, usually accompanying unfavourable management decisions (Bies and Shapiro 1988; Libby 1999). Bies and Shapiro (1988) suggest that in response to unfavourable management decisions, employees search for a

³² My predictions in H2a and H2b are specific to a setting where ex-post adjustments are possible. However, it is also possible that in a setting where ex-post adjustments are *not* possible, employee justice perceptions and performance will nonetheless decrease in response to simply not attaining their goals, even when management has no ability to help them through ex-post adjustments.

causal account or justification for management's decision. In the absence of an explanation, employees may attribute sinister motives to management (Kelley 1973). Therefore, providing an explanation is a way for management to head off employee speculation or misunderstandings and manage employee justice perceptions (Greenberg 1990).

Explanations studied in the accounting and psychology literatures have generally focused on: 1) why the unfavourable outcome *could not* be any different (e.g., Bies and Shapiro 1988; Shapiro 1991), or 2) why the unfavourable outcome *should not* be any different (e.g. Libby 1999; Colquitt and Chertkoff 2002). An example of a 'could not' explanation is used in Bies and Shapiro (1988), who explained to participants that the unfavourable outcome was due to policy 'imposed by top management', and thus out of their control. An example of a 'should not' explanation is used in Libby (1999), who explained to participants that their unfavourable outcome was carefully reviewed by management with no errors found. The accounting and psychology literatures have also explored the determinants of effective explanations, finding explanations that are sincere, logical, and thorough to improve perceptions of fairness and performance (Libby 1999; Colquitt and Chertkoff 2002).

In my setting, due to the discretion bestowed upon management to make ex-post adjustments, management cannot argue that they *could not* have made an ex-post adjustment to help employees. Instead, management must appeal to why they *should not* have made an ex-post adjustment. Consistent with Libby (1999), I expect that an explanation from management providing logical, sincere reasons for why they *should not* exercise ex-post adjustments to attenuate the negative effects of observed non-helping behaviour. As described in my development of H2a and H2b, observed non-helping behaviour is expected to worsen both procedural and distributive justice perceptions, and therefore performance. Thus, an effective

explanation based on the above criteria should improve both procedural and distributive justice perceptions, as well as subsequent performance.

Although I expect providing an explanation to employees in my setting to improve both procedural and distributive fairness perceptions, the effectiveness of an explanation may be limited. The employee in my setting has failed to attain their goal and earn their desired reward, negatively impacting perceptions of distributive justice. In addition, as described in the preceding section, management's decision to not exercise ex-post adjustments contrasts with their expectations, deteriorating employee trust in management, which could undermine the effectiveness of an explanation provided ex-post. Despite this, based on the discussion above, I expect that providing an explanation to employees for management inaction will mitigate some of the negative fairness and therefore performance consequences caused by management's decision to not exercise ex-post adjustments. This leads to my next set of hypotheses, stated in the alternative form:

H3a: The worsening of procedural and distributive justice perceptions following management's decision to not exercise ex-post adjustments will be attenuated when management provides an explanation for their decision relative to when management provides no such explanation.

H3b: The decline in performance following management's decision to not exercise ex-post adjustments will be smaller when management provides an explanation for their decision relative to when management provides no such explanation.

In subsection 2.4.3, I described two possible consequences of perspective taking due to self-other overlap: 1) an increase in the availability of the internal thoughts, feelings, and intentions (i.e., the internal state) of the perspective taking target to the perspective taker, and 2) an increase in the degree to which the perspective taker believes they are similar to the target,

allowing for positive self-evaluations to automatically transfer from perspective taker to target.³³ I expect that both of these effects of self-other overlap will lead to improved justice perceptions and performance in my setting, for different reasons, described below.

As described above, management may have legitimate reasons for not performing ex-post adjustments.³⁴ If employees are asked to take the perspective of management who made this decision, these legitimate reasons will become more available to employees when making fairness judgments, as long as they are able to accurately simulate management's perspective. As discussed in subsection 2.4.3, if employees are not provided with the basis with which to form accurate mental representations of management, they will use other salient cues in their information environment, such as stereotypic behaviour, in simulating management's perspective (Skorinko and Sinclair 2013), perhaps ascribing a self-serving motive for management's non-helping behaviour (Kelley 1973). However, if employees are provided with a basis to help them accurately simulate management's internal state, then perspective taking should result in the increased availability of management's internal state in fairness judgments (Pronin 2008). I expect that this increased availability of management's legitimate reasons for not performing ex-post adjustments will improve employees' procedural and distributive justice perceptions. Another consequence of the greater availability of management's internal state is a deeper understanding of the motivations and considerations management must make in performing ex-post adjustments. This should lead to an increased understanding of when and how management

³³ As discussed in subsection 2.4.3, perspective taking results in the automatic transfer of self-evaluations from the perspective taker to the target. Since individuals are motivated to maintain a positive self-concept (Taylor and Brown 1988; Pronin 2008), on average, perspective taking should result in the transfer of positive self-evaluations from the self to the other.

³⁴ These could include trying to induce adaptive behaviour if the employees operate in a volatile environment (Merchant 1989; Bol et al. 2015), or that their options are constrained by the zero-sum nature of ex-post adjustments in a fixed bonus pool setting (Baiman and Rajan 1995; Bol et al. 2015).

is likely to exercise these adjustments, resulting in reduced uncertainty over how such adjustments will impact the effort-performance-reward relationship. All else equal, this reduction in uncertainty should strengthen the effort-performance-reward relationship inherent to the compensation contract (Holmström 1979; Bol, 2008), improving performance.

Individuals are generally motivated to maintain a positive self-concept (Taylor and Brown 1988; Todd and Burgmer 2003). Therefore, self-overlap induced through perspective taking should also cause employees' positive self-evaluations to automatically transfer to management as the perspective taking target, leading to more favourable evaluations and opinions about the target (Todd and Burgmer 2003). As a result of perspective taking, perspective takers have been shown to reduce stereotyping of the target (Galinsky and Moskowitz 2000; Todd and Burgmer 2003), have greater empathic concern for the target (Cialdini et al. 1997), and have improved attitudes and more positive evaluations of the attributes of the target (Galinsky and Ku 2004). To the extent that self-other overlap improves attitudes and evaluations of the target, employees taking the perspective of management should have more positive procedural and distributive justice perceptions, and therefore performance, relative to employees who do not take management's perspective.

The above discussion of perspective taking leads me to predict that in my setting, a perspective taking intervention will attenuate egocentric bias and improve perceptions of procedural and distributive justice and performance. This leads to my next set of hypotheses, stated in the alternative form:

H4a: The worsening of procedural and distributive justice perceptions following management's decision to not exercise ex-post adjustments will be attenuated when employees take management's perspective relative to when no such perspective taking occurs.

H4b: The decline in performance following management's decision to not exercise ex-post adjustments will be smaller when employees take management's perspective relative to when no such perspective taking occurs.

My predictions in H3a, H3b, H4a, and H4b are shown graphically in Figure 1, Panel B.

In addition to the above two sets of hypotheses on the effects of explanation and perspective taking, I also examine whether explanation and perspective taking are substitutes or complements as fairness interventions. As described above, explanation does not directly combat egocentric bias, whereas perspective taking does. Based on this, it is possible that perspective taking can enhance the effectiveness of explanations, acting like a complement and resulting in a multiplicative effect on justice perceptions and performance. However, it is also possible that possible that because both explanation and perspective taking increase the availability of management's internal state to employees, that these two fairness interventions will act as substitutes, resulting in either an additive or ceiling effect on justice perceptions and performance. Finally, it is possible that the presence of one intervention will interfere with or suppress the effect of the other. For example, taking management's perspective may result in employees arriving at an adequate justification for management's observed non-helping behaviour. To the extent that the explanation provided ex-post by management conflicts with the justifications arrived at through perspective taking, this may cause employees to question management's true motivations, decreasing the effectiveness of both interventions. Due to the considerable uncertainty over how my two fairness interventions will interact, I pose the following research questions:

RQ1a: Will explanation and perspective taking have interactive effects in attenuating the worsening of procedural and distributive justice perceptions that occur in response to observing non-helping behaviour?

RQ1b: Will explanation and perspective taking have interactive effects in reducing performance decreases that occur in response to observing non-helping behaviour?

3.6 Summary

This chapter develops four sets of hypotheses and one set of research questions regarding employee reactions to events triggering fairness considerations in an ex-post adjustment setting. In summary, I hypothesize that although employees will react favourably to the initial announcement of an ex-post adjustment policy (H1), these initial favourable reactions will be attenuated when management decides not to exercise adjustments for employees (H2). I further hypothesize that both explanation (H3) and perspective taking (H4) will at least partly mitigate the negative impact of management's decision to not exercise ex-post adjustments. Finally, I pose a research question about how explanation and perspective taking will interact when used together. In the next section, I discuss the research design of this investigation that I employ to test my four hypotheses and research question.

CHAPTER 4: EXPERIMENTAL DESIGN AND METHOD

4.1 Design Overview

To test my hypotheses, I conduct laboratory experiments employing a 2x2 between subjects, full factorial design with a control condition. My manipulated factors are: 1) the presence or absence of a perspective taking exercise, and 2) the presence or absence of an explanation for decision making, for a total of four non-control conditions and one control condition. In each of these four non-control conditions, the availability of discretionary ex-post goal adjustments are disclosed to participants in the experiment instructions. In the control condition, instructions do not disclose the availability of ex-post goal adjustments and will not be performed.³⁵ My experimental conditions are summarized in Figure 2.

The remainder of this chapter is organized as follows. Section 4.2 describes the experimental tasks as well as the operationalization of important features in my experimental setting. Section 4.3 provides an overview of experimental procedures, as well as my experimental manipulations. Section 4.4 discusses the operationalization and measurement of my dependent variables, and section 4.5 discusses the operationalization and measurement of my control and process variables. This chapter concludes with section 4.6.

³⁵ The purpose of this control condition is to test H1 and H2, which examines the effect of the availability of ex-post goal adjustments.

Figure 2. Summary of experimental conditions

No ex-post adjustments available	Ex-post adjustments available	
Control	No explanation / No perspective taking	Explanation / No perspective taking
	No explanation / Perspective taking	Explanation / Perspective taking

Summary of conditions:

Control: In this condition, participants receive individualized goals, learn about the bonus for goal attainment incentive scheme, and learn about negative uncontrollable events. They do not learn about ex-post adjustments and are not eligible to receive them.

No explanation/no perspective taking: In this condition, participants learn about everything that the *control* participants learn. In addition, they learn about the possibility of ex-post adjustments being employed and are eligible to receive them.

Explanation/no perspective taking: In this condition, participants learn about everything that the *no explanation/no perspective taking* participants learn. In addition, they receive an explanation for management’s decision to perform or not perform ex-post adjustments at the end of each difficult production round.

No explanation/perspective taking: In this condition, participants learn about everything that the *no explanation/no perspective taking* participants learn. In addition, they receive perspective taking training just prior to the beginning of the production rounds.

Explanation/perspective taking: In this condition, participants learn about everything that the *no explanation/no perspective taking* participants learn. In addition, they receive perspective taking training just prior to the beginning of the production rounds and receive an explanation for management’s decision to perform or not perform ex-post adjustments at the end of each difficult production round.

4.2 Experiment Details

4.2.1 Task Description

In my experiment, participants work on two computer based effort sensitive tasks: 1) a task where participants are required to count the number of '1's that appear in a 7x7 matrix of '0's and '1's (hereafter the counting task), and 2) a task where participants decode strings of symbols with the aid of a decoding key that translates symbols to letters (hereafter the decoding task). Both tasks were adapted from Anand, Webb, and Wong (2019).³⁶ The counting task in my experiment maps to an employee's focal task in an organization and is reinforced by a bonus for goal attainment incentive scheme. The decoding task is not reinforced through performance-based pay and proxies for an ancillary task that is not directly compensated by the principal, but for which the agent derives some token utility.

At any time during any of the production rounds, participants are able to switch between the counting and decoding tasks at will. I chose to allow participants to switch between tasks at will for ecological validity. In real organizations, employees with multiple task options must choose how to allocate their time between these tasks. If participants were not allowed to switch at will (i.e., switching from counting to decoding was permanent), then participants who simply wanted a break or distraction from the counting task may feel compelled to remain in the counting task to avoid forfeiture of their bonus, introducing noise to my measure of effort duration (subsection 4.4.3).

³⁶ The counting task was also used in Abeler et al. (2011), and the decoding task was originally adapted from (Chow 1983).

4.2.2 Incentive Structure

There are four ways in which participants in my experiment can earn remuneration, each described in further detail in this subsection: 1) all participants are paid \$5 for showing up to the experiment, 2) participants earn \$0.10 for each unit of the counting task completed in the practice round, 3) participants earn \$3 for each of the four production rounds in which they meet or beat an assigned output goal, and 4) participants earn \$0.25 for each minute they spend on the decoding task instead of the counting task in the production round.

I decided to pay participants a \$5 show-up fee to show appreciation for their participation in my experiment. Since the other sources of remuneration in the experiment depend on the actions taken by the participant, the \$5 show up fee ensures that all participants leave the study with some remuneration. As described in more detail in in section 4.3, I am interested in measuring performance on my focal task (counting task) in order to test my hypotheses. To support this aim, I measure performance in a practice round as a measure of baseline performance. To elicit effort from participants in this practice round, I pay a piece-rate of \$0.10 per unit of the counting task completed.

The most substantial opportunity for remuneration provided to participants is in the form of a bonus for output goal attainment incentive scheme in the production round (discussed in more detail in the next section). In each of four production rounds, participants have an opportunity to earn a \$3 bonus if they meet or beat an assigned output goal on the counting task. If they do not meet their output goal, they receive no bonus. Unlike in the practice round, participants are not paid piece-rate compensation for units of the counting task completed in the production round.

The other opportunity for participants to earn remuneration during the production round is through time spent on the decoding task. As described above, the decoding task in my experiment represents any outside options (i.e. anything unrelated to the focal task) available to employees in an organization that provides them with some utility (e.g. leisure time). To represent this outside option in my experiment, participants can choose not to work on the counting task, instead spending time on the decoding task in order to earn \$0.25 per minute.

4.2.3 Negative Uncontrollable Events

To simulate negative uncontrollable events and their impact on employee performance, I introduce negative shocks to participant performance on the counting task by varying its difficulty across the four production rounds within the production round. I vary the difficulty of the counting task by varying the proportion of '1's in the 7x7 matrices from 40% in normal rounds to 60% in difficult rounds.³⁷ For experimental consistency, as well as to strengthen the salience of management decision making, all participants experience the same sequence of normal and difficult rounds: normal, difficult, difficult, normal. Ex-ante, participants only know that the difficulty will vary across rounds, not the order or frequency of difficult rounds. Recall that some of my predictions require employees to have observed non-helping behaviour from management, i.e., not performing an ex-post adjustment in response to negative events. Employees can only observe management's non-helping behaviour when they have encountered a difficult round (negative event) and management chooses not to perform an ex-post adjustment. Therefore, by isolating my difficult rounds in the middle of the production rounds, I create a

³⁷ The time required to count the '1's in a 7x7 matrix increases with the proportion of '1's in the table, increasing the difficulty of the counting tasks in periods with a relatively larger proportion of '1's per table. I corroborated this through pilot testing, where I observed that increasing the proportion of '1's in the table from 40% to 60% resulted in a 33% decrease in performance.

clear pre-observation of non-helping behaviour period (rounds 1 and 2), and a clear post-observation of non-helping behaviour period (rounds 3 and 4), allowing me to test my hypotheses requiring such a pre-/post- period (H2, H3, H4, and RQ1).

4.2.4 Ex-Post Adjustments

Participants in the four non-control conditions are told that in response to the varying difficulty across production rounds (described above), at the discretion of the experimental administrator, their goals may be adjusted downwards in light of a particularly difficult round. However, participants are also informed that any downward goal adjustments will have to be balanced with an upward goal adjustment to one or more other participants in a zero-sum game. This design choice was made for two reasons. First, the high degree of compensation interdependency inherent to the zero-sum game of ex-post adjustments in my setting is the reason management is selective about making adjustments. This is consistent with Bol et al. (2015), who find that managers are less likely to make ex-post adjustments when compensation interdependency is high. Second, Baiman and Rajan (1995) find analytically that such a fixed bonus setting can be optimal if the principal commits ex-ante to the size of the bonus pool but retains discretion to make ex-post adjustments based on non-contractible information, such as the occurrence of negative uncontrollable events during the contract period.³⁸ Baiman and Rajan (1995) provide some anecdotal evidence that such fixed bonus pool arrangements exist in practice, which is corroborated by Cai et al. (2019), who find evidence in a field study of ex-post

³⁸ Baiman and Rajan's (1995) study was in part motivated by the ubiquity of such fixed bonus pool arrangements in practice, but also notes that such arrangements can have motivational consequences because it sets up a zero-sum game between managers with respect to compensation.

adjustments being made in a zero-sum game. Therefore, this design choice has ecological validity.

Notably, my experiment differs from the fixed bonus pool arrangements described by Baiman and Rajan (1995) in that my setting employs a fixed *goal* setting, where the zero-sum game is not in the allocation of bonuses as in a fixed bonus pool setting, but in the setting of employee goals, i.e., the sum of employee goals must equal some overall divisional or organizational goal. The reason for this difference is based on feedback received during my first pilot study. As part of my first pilot study ($n = 45$, subsection 4.2.5), I recruited undergraduate students to get feedback on my perspective taking manipulation as well as the various elements of the compensation contract. In this study, the compensation contract included the possibility of ex-post adjustments in a fixed bonus pool setting, consistent with Baiman and Rajan (1995). In interviews conducted with participants after the pilot study, the majority of participants indicated that the process behind making ex-post adjustments to objectively determined bonuses was confusing and seemed arbitrary. When asked whether adjustments to goals in a fixed goal setting would be easier to understand, participants indicated that it would, since it maintained the link between goal attainment and receiving a bonus. I believe it is appropriate to use a fixed goal setting, as the contract feature relevant to examining the motivational consequences of ex-post adjustments (zero-sum game played by managers) is retained in a fixed goal setting. Further, there is evidence that such fixed goal settings also occur in practice. For example, in a field study, Bol et al. (2010) observe a setting where managers make discretionary adjustments in a zero-sum game to objectively determined goals based on non-contractible information.

Finally, as discussed in my hypotheses development, I am interested in studying the consequences of management *not* performing ex-post adjustments on justice perceptions and

performance. As such, I am primarily interested in data collected from participants *not* receiving an ex-post adjustment. However, to avoid deception, since I disclose the availability of ex-post adjustments, I must actually perform ex-post adjustments for some participants. To maximize the number of participants not receiving an ex-post adjustment, the incidence of ex-post adjustments, which are randomly determined in my experiment, is set quite low at 15%. The low incidence of ex-post adjustments in my experiment is consistent with Höppe and Moers (2011), who find evidence from SEC filings that of firms disclosing the use of ex-post bonus adjustments, only 24.3% report exercising upwards bonus adjustments.

4.2.5 Participants

I am interested in studying employees' fairness reactions to the implementation of management controls and the resulting impact on employee performance. As discussed in section 2.4, employee fairness perceptions are heavily influenced by egocentric bias, which is inherent to all individuals (Pronin 2008). Although age has been shown to moderate egocentric tendencies, individuals' initial reactions are still coloured by egocentric bias, regardless of age (Epley, Morewedge, and Keysar 2004). Further, my experimental tasks are simple computer-based tasks which require no special skills or knowledge to perform. For these reasons, testing my hypotheses using university student participants is appropriate. For my main study, I recruited a total of 169 participants, of which 22 received random ex-post goal adjustments. As described above, my hypotheses centre around individuals *not* receiving ex-post adjustments, so the 22 participants receiving adjustments are excluded from my analyses, leaving 147 participants in my final sample.³⁹

³⁹ Of the 169 participants recruited for my main study, 140 of them were assigned to non-control conditions and were eligible for ex-post adjustments. Therefore, the 22 participants receiving ex-post adjustments represents an

4.2.6 Pilot Studies

To pilot my instrument and test my hypotheses, I conducted three pilot studies, all using undergraduate business students from large Canadian universities. In my first pilot study ($n = 45$), I used two conditions equivalent to the No Intervention and Perspective Only conditions in the final study, in order to test my perspective taking manipulation, as well as elements of the compensation contract (e.g. incentive structure, ex-post adjustments) and information environment (e.g. negative uncontrollable events). In this pilot study, my perspective taking manipulation required participants to write a narrative from the perspective of management, and ex-post adjustments were endogenously rather than exogenously determined. I did not observe any significant effects of perspective taking on justice perceptions or performance. Based on the results of this pilot test, I decided to greatly simplify my experimental instrument, exogenously determine ex-post adjustments, and use a more practical perspective taking manipulation (i.e., a training exercise).

In my second pilot study ($n = 110$), I tested three conditions equivalent to the Control, Explanation Only, and Both Interventions conditions in my final study. In this pilot study, I tested refinements to my perspective taking manipulation, as well as changes to the compensation contract made as a result of pilot test 1. In the second pilot study, I observed a positive but insignificant effect of both explanation and perspective taking on cumulative performance, but no effects of either on justice perceptions. In this pilot study, there was only one production round (with six periods), and participants received feedback at the end of each period on their progress towards goal attainment. As a result of this pilot study I decided to

adjustment rate of 15.7%, evidence of the successful operationalization of my random ex-post adjustments, which was parameterized at 15% (subsection 4.2.4).

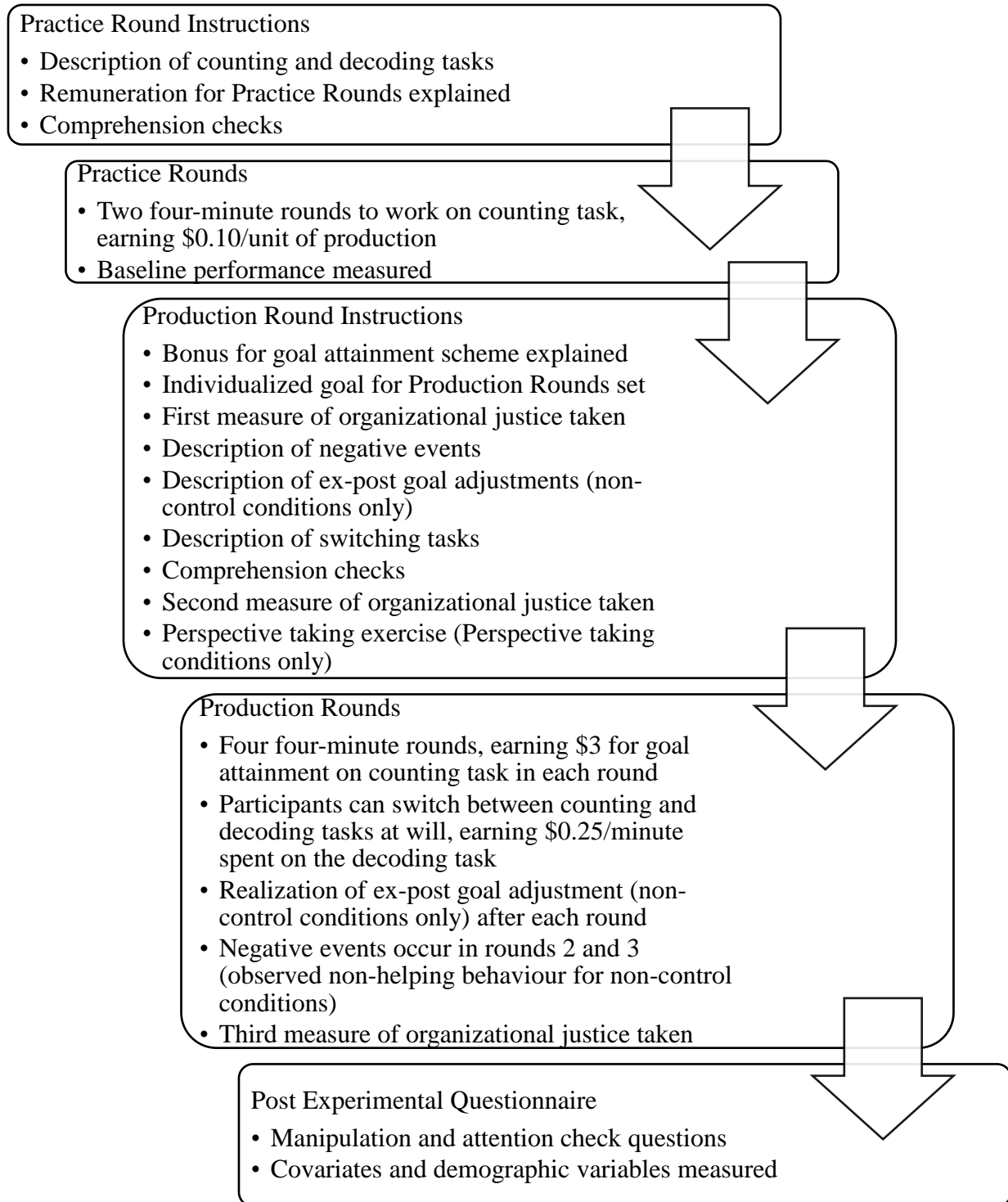
change the structure of the production period such that instead of one long production round with one bonus, participants experience multiple production rounds. I did so based on my belief that the failure to observe results consistent with my predictions was due in part to participants not observing the realization of bonus attainment (or failure to attain the bonus) until the end of the experiment. When participants experienced one long production round with one bonus, it may have been difficult for participants to fully understand how receipt or non-receipt of a goal adjustment affected their remuneration. By splitting my production round into multiple periods, the impact of goal adjustments on remuneration is clearer to participants, making it more salient to them as they make effort choices in subsequent rounds.

In my third pilot study ($n = 54$), I further refined my perspective taking and explanation manipulations by testing two conditions equivalent to my Explanation Only and Both Interventions conditions in the final study. I observed that after observing non-helping behaviour, receiving both explanation and perspective taking improved performance and justice perceptions, but that receiving only explanation did not improve either. In this pilot study, my explanation was quite terse. I believed that this terse explanation hindered its ability to improve justice perceptions and performance. Accordingly I revised my explanation to include expressions of sincerity (e.g. use of words like “carefully”, “sincerely”) to signal sensitivity towards participants’ perceived unfairness (see subsection 4.3.2), consistent with explanations used in literature, such as Libby (1999).

4.3 Experimental Design

The basic flow of the experimental task is depicted in Figure 3 and is described as follows. First, participants are provided with brief instructions on how to perform the counting

Figure 3. Experimental flowchart



Notes:

¹Task 1 is based on Abeler et al. (2011) and involves participants counting the number of ‘1’s in a matrix of ‘1’s and ‘0’s. Task 2 is based on Chow (1983) and involves participants decoding a string of symbols into a string of letters using a decoding key.

and decoding tasks (described in subsection 4.2.1). Next, participants are given two four-minute rounds to practice the counting task. This allows participants to acquaint themselves with the focal task (counting task), to get used to the four-minute length of the production rounds, and more accurately estimate their expectancy of goal attainment in four-minute production rounds. Further, participant performance on the counting task in the practice rounds serves as a measure of their baseline performance and is used to set individualized goals. To elicit high effort, I pay participants \$0.10 for each unit of the counting task completed in the practice rounds.

After the practice round, participants receive further instructions that they have an opportunity to earn a bonus by meeting an individualized output goal on the counting task.⁴⁰ In each of four upcoming four-minute production rounds, participants can earn a bonus of \$3 if they achieve their output goal on the counting task. Participants also learn that at any point during the four-minute production rounds, they may switch freely between the counting and decoding tasks and earn \$0.25 per minute spent on the decoding task.⁴¹ However, participants are also informed that only units of the counting task completed contribute progress towards their output goal and achievement of their bonus. At this point, the first measure of justice perceptions is recorded, which is described in subsection 4.4.2. Participants are then informed about the existence of negative shocks to their performance, and participants in the non-control conditions also learn about the availability of discretionary ex-post goal adjustments. Throughout the instructions, I

⁴⁰ Consistent with Anand et al. (2019), goals are based on rounds of ‘normal’ difficulty and incorporate expected learning. Participant goals are set as the lesser of 20 tables and $2.9 + 0.65 * (\textit{Practice})$, where *Practice* represents cumulative participant performance in the two four-minute practice rounds. Goals assigned in a similar manner resulted in an overall goal attainment rate of 62% in Anand et al. (2019), which is broadly consistent with a moderately difficult goal attainment rate of 50% (Merchant and Manzoni, 1989).

⁴¹ The parameterization of pay in the decoding task was kept consistent throughout my pilot studies, and resulted in participants spending a reasonable amount of time on the decoding task. I intended to keep the guaranteed pay of the decoding task to the lowest level possible that would still be considered non-trivial by participants, leading me to use \$0.25 per minute.

administer comprehension checks to ensure that participants attend to the salient features of the compensation contract and setting. Participants are not permitted to proceed with the instructions until they correctly answer the comprehension check questions. All participant responses to comprehension checks are recorded.

Just prior to the start of the production rounds, the second measure of justice perceptions are taken, then participants in the perspective taking conditions perform a perspective taking exercise (subsection 4.3.1). During the production rounds, participants are provided with the following information in real-time: 1) units of production completed in the round, 2) amount of time spent on the decoding task, and 3) amount of time left in the round.

At the end of each round, all participants receive feedback showing their performance for the round, a reminder of their goal, and the average number of ‘1’s per matrix in the round just completed.⁴² In addition to this, participants in the non-control conditions learn whether they will receive a goal adjustment or not. After difficult rounds only, participants in the explanation conditions receive an explanation for management’s decision to perform or not perform ex-post adjustments (subsection 4.3.2). After the end of the fourth four-minute production round, the third and final measure of justice perceptions are taken, and then participants complete a post-experimental questionnaire (section 4.5), after which they are paid and debriefed.

⁴² Disclosing the average number of ‘1’s per table in the round just completed removes any ambiguity about the difficulty of the past round. This reduces uncertainty to participants about whether or not to anticipate ex-post adjustments, since in the instructions they are told that ex-post adjustments *may* be performed in response to a difficult round.

4.3.1 Perspective Taking Manipulation

As described above, just prior to the beginning of the production rounds, participants in the perspective taking conditions perform a perspective taking exercise. Specifically, participants are provided with a table summarizing the performance of two hypothetical employees who have both just faced a difficult round and have similar goals (Figure 4). Based on the performance and individual goals of the two hypothetical employees, one employee has just beat their goal by one unit of production, and the other has missed their goal by two units of production. Participants are required to propose a goal adjustment (if any) for these two employees such that the net goal adjustment equals zero, i.e., an upwards adjustment for one employee must be balanced with a downwards adjustment for the other.

Importantly, participants are unable to propose a goal adjustment to help the lower performing employee without causing the higher performing employee to lose their bonus. This feature causes participants to experience the tension faced by management inherent to the zero-sum nature of ex-post adjustments in my setting, that helping one employee necessarily hurts another. This choice was made in response to the results of my second pilot test, where I tried an implicit induction similar to the method described above, except that there were three employees: two who had met their goal, and one who had not. Importantly, it was possible for participants to suggest goal adjustments that would help the lower employee attain their goal without causing either of the higher performing employees to lose theirs. The majority of pilot participants suggested adjustments to help the lowest performing employee, and so did not experience the tension of being unable to help one employee without materially hurting another. Notably, this perspective taking manipulation does not provide perspective taking participants with incremental information over participants in the no perspective taking conditions. Relevant

Figure 4. Perspective taking manipulation

Based on the information below, please propose goal adjustments to participants, if any, in the ‘Proposed goal adjustment’ column. Remember that any proposed decreases to goals must be accompanied by an increase to another participant’s goal. The total of the ‘Proposed goal adjustment’ column (net adjustment) must sum to zero.

Employee	Difficulty of last round	Performance ¹	Original Goal ²	Proposed goal adjustment ³	Adjusted Goal ⁴	Bonus Earned ^{4,5}
1	Difficult	32	31		31	\$3
2	Difficult	27	29		29	\$0
			Net Adjustment ⁴			

When you are satisfied with your proposed goal adjustments, please fill in the following text fields and press the ‘Submit Adjustments’ button.

Please explain your rationale for your proposed goal adjustments:⁶

Imagine that you now have to provide explanations to these participants for your proposed goal adjustments. Please write out an explanation that you would provide to the participants who are expected not to earn their bonus (noted above with the red ‘\$0’ in the ‘Bonus earned’ column):⁶

Notes:

¹ Performance shown for employee 1 is always one greater than their goal, and always two less than their goal for employee 2. Therefore, there are no adjustments that participants can propose that would result in both employees attaining their goal and earning their bonus.

² Employees’ original goals are parameterized to be equal to the perspective taking participant’s own goal +/- 1.

³ The proposed goal adjustment column is initially blank and contains a field in each row for participants to record their proposed goal adjustment. The instrument does not allow participants to continue if either field is left blank or if the net adjustment does not sum to zero.

⁴ These fields update dynamically based on participant proposed adjustments.

⁵ The amount of bonus earned is colour coded: green for employees earning a bonus, and red otherwise.

⁶ The instrument does not allow participants to proceed if they leave either of these fields blank.

incremental information in this context could include the actual likelihood of goal adjustment or the number and sequence of normal and difficult rounds, which might influence participant effort choices for reasons other than those described in my hypotheses development.

Most perspective taking manipulations in laboratory experiments involve asking participants to think about the internal state (thoughts, feelings, intentions) of some perspective taking target, then either writing a narrative from the target's perspective (e.g. Galinsky and Moskowitz 2000), or having a group discussion about their thoughts (e.g. Epley et al. 2006). In my experiment, I never ask participants to *explicitly* take the perspective of management. Instead, it is through sharing the experience of management's decision making that participants come to *implicitly* take the perspective of management by performing an exercise from management's perspective.

I chose to operationalize my perspective taking manipulation implicitly for several reasons. First, in my first pilot test, I used an explicit perspective taking induction similar to Galinsky and Moskowitz (2000), asking participants to write a short essay about the thoughts, feelings, and intentions that management might have when faced with making ex-post adjustments. I found that participant narratives were still overwhelmingly egocentric in that the majority of participants fixated on helping employees who had missed their goals, ignoring the negative impact that goal adjustments would have on employees receiving upwards adjustments to goals. I view these narratives as egocentric, since they fail to recognize the tension faced by management, instead fixating on the types of helpful actions they would benefit from in the upcoming production rounds.

Second, studies have shown that explicit perspective taking relies on the perspective taker's initial beliefs about the target. For example, Drolet et al. (1998) find that the positive effect of perspective taking is attenuated when the perspective taker has a negative working relationship with the target. Further, Skorinko and Sinclair (2013) find that if the perspective taking target exhibits clearly stereotypical traits, perspective taking increases stereotyping rather than suppressing it. Thus, explicit perspective taking can be coloured by contextual factors, such as salient stereotypes or prior interactions between employees and management. Theoretically, an implicit induction should avoid some of the cognitive biases described above, since management's perspective is gained by employees through sharing management's experience of the difficulty in deciding whether to make ex-post adjustments, rather than imagining what it must feel like. Finally, in my perspective taking manipulation, participants share the experience of management's dilemma in deciding whether to make ex-post adjustments. Cortland et al. (2017) find in a series of experiments that increasing the salience of shared experiences between groups of racial and sexual minorities improve evaluations of and empathic concern for the other group. This provides evidence that a perspective taking intervention aimed at providing a shared experience between employees and management can be effective at improving employee evaluations of and concern for management.

4.3.2 Explanation Provided

As described above, after each of the two difficult rounds (rounds 2 and 3), participants in the explanation conditions are provided an explanation for management decision making.⁴³ For the 85% of participants who will not receive goal adjustments, the explanation provides a

⁴³ Participants in the no explanation conditions simply receive a message showing the realization of whether ex-post adjustments were performed or not (e.g. "I have decided not to reduce your goal." or "I have decided to reduce your goal".)

justification for why management did not make an ex-post adjustment in their case. As described in my hypotheses development, both Libby (1999) and Colquitt and Chertkoff (2002) find that explanations can be effective at improving performance when management accepts ownership of the decision resulting in unfavourable outcomes to employees, and when the explanation is perceived to be sincere and informationally valid. The explanation provided to participants after a difficult round (not receiving a goal adjustment) is as follows:

“I have carefully considered making a goal adjustment. However, even though the last round was fairly difficult, I have decided not to reduce your goal. As experiment administrator, I must make difficult decisions that may not result in favourable outcomes for everyone. I sincerely hope that this outcome does not discourage you from pursuing your goal in future rounds.”

Following Libby (1999) and Colquitt and Chertkoff (2002), I designed the explanation to focus on why the unfavourable outcomes experienced by participants *should not* be any different. This represents the decision-maker owning the decision that resulted in unfavourable outcomes, as opposed to claiming that the outcomes *could not* be any different by shifting blame to external factors. Further, the above explanation satisfies Greenberg’s (1993) sincerity criterion through its choice in language. Following Libby (1999), I use language such as ‘carefully considered’, and ‘sincerely hope’ to signal my sensitivity towards my subordinates’ perceived unfairness. Greenberg (1993) found that such signals of interpersonal sensitivity improved perceptions of fairness and attenuated counter-productive work behaviours. The above explanation also satisfies Greenberg’s (1993) informational validity criterion by including information that is verifiable (previously included in instructions). In my third pilot test, I tested an explanation similar to the one shown above, except that it did not include the expressions of sincerity (first and last sentences); it was ineffective at improving justice perceptions or performance. I therefore added

the first and last sentences shown above to illustrate to participants my sensitivity towards the fact that they have just received an unfavourable outcome.^{44, 45}

4.4 Dependent Variables

To test my hypotheses, I measure three primary dependent variables: 1) justice perceptions (both procedural and distributive), 2) performance, and 3) effort. I describe these variables and their measurement in this section.

4.4.1 Justice Perceptions

Each of my hypotheses make predictions with respect to justice perceptions. To measure justice perceptions in my experiment, I collect survey based self-reported measures of distributive and procedural justice perceptions based on Kelly et al. (2015), who use modified justice scales developed originally by Leventhal (1980), Thibaut and Walker (1975), and validated by Colquitt (2001) (Appendix I). In my first pilot test, I also included items in my justice questionnaire designed to measure perceptions of interactional justice, based on Colquitt and Rodell (2015). However, based on feedback from participants that the instrument was too lengthy, and the high degree of correlation between perceptions of interactional and procedural justice (Cohen-Charash and Spector 2001; Colquitt et al. 2011), I decided to remove these interactional justice items from my scale. As described above, I measure justice perceptions at three points in the experiment: 1) immediately after participants learn about the basic

⁴⁴ It is possible that the final sentence in my explanation intervention could be perceived as explicitly encouraging participants to work hard. However, given the results of my third pilot test, I decided to employ a stronger manipulation to improve the likelihood of observing an explanation effect.

⁴⁵ I did not explicitly mention the zero-sum nature of goal adjustments in my explanation, since Colquitt and Chertkoff (2002) note that explanations are more effective when the source of the explanation takes responsibility for negative outcomes (as in my explanation) as opposed to shifting blame for the negative outcome to some other target.

remuneration scheme in the production round (bonus for goal attainment), 2) immediately before the beginning of the production round, and 3) immediately after the end of the production round.

The first measurement of justice perceptions is intended to act as a baseline for justice perceptions in response to the basic bonus for goal attainment incentive contract. The second measurement captures participants' reaction to important features of their environment, such as the existence of negative shocks (varying difficulty between rounds) and the availability of ex-post adjustments, and serves as my measure of justice perceptions pre-observation of non-helping behaviour used in all of my tests of hypotheses and the examination of my research question. The third and final measurement of justice perceptions captures participants' reaction to experiencing negative exogenous shocks to their performance, not receiving an ex-post adjustment, as well as to my experimental manipulations (perspective taking and explanation), and serves as my measure of justice perceptions post-observation of non-helping behaviour required to evaluate H2, H3, H4, and RQ1.

4.4.2 Performance

Each of my hypotheses also makes a prediction about performance. As described above, the focal task in my experiment is the counting task so I am interested in measuring participant performance on that task. I measure performance as the number of units of the counting task completed by participants in a given production round. I define performance pre-observation of non-helping behaviour (used in all tests) as performance in rounds 1 and 2, with performance post-observation of non-helping behaviour (H2, H3, H4, RQ1) as performance in rounds 3 and 4.

4.4.3 Effort

Effort is an important organizational outcome for management and accounting scholars, consisting of four dimensions, direction, duration, intensity, and strategy development (Bonner and Sprinkle 2002). Bonner and Sprinkle (2002, p 306) characterize the first three dimensions as effort “directed towards current performance”, whereas strategy development is effort “directed towards learning” and future performance. Since I use a simple, effort sensitive task, there are minimal opportunities for learning and strategy development. Therefore, in the current investigation, I focus on the dimensions of effort directed towards current performance: direction, duration, and intensity.

Bonner and Sprinkle (2002) note that the performance management literature has typically focused on performance as the construct of interest, resulting in a lack of understanding of how incentives impact the various dimensions of effort (e.g., duration, intensity, and direction). In recent years, accounting academics have examined the impact of management controls on effort direction (e.g. Hannan, McPhee, Newman, and Tafkov 2013; 2019), but to the best of my knowledge, no studies have directly measured effort duration and intensity in the same experiment as constructs of interest.⁴⁶

As described in my hypotheses development, I employ a multi-task setting in my experiment. As a result, I am able to decompose performance on the counting task into effort duration and effort intensity. In my experiment, effort direction is constrained to the two tasks available to participants, counting and decoding. As such, effort direction cannot be

⁴⁶ Psychology and accounting studies have measured effort duration as a proxy for effort (e.g. Bettman, Johnson, and Payne 1990; Libby and Lipe 1992), but these studies do not measure effort intensity. Choi, Clark, and Presslee (2019) measure performance on effort sensitive tasks as a proxy for effort intensity, but do not measure effort duration.

distinguished from effort duration in my experiment, and so I focus on effort duration in this investigation. Consistent with Libby and Lipe (1992), I define effort duration as the amount of time a participant spends on the counting task in each production round, measured in seconds. Consistent with Anand et al. (2019), effort intensity is measured by taking performance for a given production round and scaling by effort duration. I believe this is a suitable measure of effort intensity as it captures changes in effort as measured through performance, controlling for duration.

4.5 Controls and Other Measured Variables

In addition to the dependent variables of interest discussed in section 4.4, I measure one co-variate: baseline performance, and three process variables: 1) self-other overlap, 2) expectancy of goal attainment, and 3) expectancy of goal adjustment (for those in the non-control conditions only). I describe these variables and their measurement in this section.

4.5.1 Baseline Performance

Prior studies using tasks similar to my counting task (e.g. Abeler et al. 2011, Anand et al. 2019) have reported between subjects variation in performance, attributed to individual differences in ability. To control for these between subjects differences, I collect a measure of baseline performance equal to the cumulative units of the counting task completed in the two practice rounds. Based on my pilot study and other studies who have adopted similar methods to control for individual ability (e.g. Libby 1999; Anand et al. 2019), practice round performance is a strong predictor of production round performance that is uncorrelated with my experimental manipulations. I believe practice round performance is an appropriate co-variate to control for individual ability in my analyses.

4.5.2 Self-other Overlap

As discussed in subsection 2.4.3, self-other overlap describes a process whereby an individual's mental representation becomes similar to an other's, increasing the degree to which the individual feels that they are similar to the other. This phenomenon is expected to mediate many of my predicted effects of perspective taking (section 3.5). For example, employees taking the perspective of management is expected to result in the transfer of positive self-evaluations (e.g. positive attributes, perceptions of fairness) from the employee to management, resulting in improved justice perceptions and performance. Therefore, I collect a survey based self-report measure of self-other overlap using the Inclusion of Other in the Self (IOS) Scale (Appendix II) (Aron et al. 2012). The IOS scale has been well validated, consists of a one-item pictorial scale, and has been used in accounting studies to measure degree of connectedness (Bauer 2015). I collect my measure of self-other overlap in the post-experimental questionnaire, and use it as a manipulation check for perspective taking.

4.5.3 Expectancy of Goal Attainment

Expectancy theories of motivation suggest that individuals are motivated to exert high levels of effort when they believe that doing so will result in high performance, leading to a desired reward (Vroom 1964; Klein 1991; Klein et al. 1999; Presslee, Vance, and Webb 2013). Although I am interested in examining the motivational implications of managers not making ex-post adjustments, my hypothesized effects on effort and performance are mediated through justice perceptions rather than expectancy. However, as described in my literature review, in a volatile production environment, due to noise in performance measurement the effort-performance relationship (expectancy) can become impaired (Bol 2008). Interventions aimed at

restoring the effort-performance relationship in such volatile environments, therefore, may be mediated through expectancy.

Following Anand et al. (2019), I measure expectancy of goal attainment by asking participants to indicate the percentage likelihood that they will attain their performance goal in a production round. I measure expectancy of goal attainment at the same time as I administer the justice questionnaire, i.e., at three points in my experiment: 1) after learning about the basic incentive contract (bonus for goal attainment), 2) just prior to the beginning of the production round, and 3) just after the end of the production round. As with the first measurement of justice perceptions, the first measurement of expectancy of goal attainment is intended to act as a baseline for use as a possible control variable. The second measurement will allow me to assess whether or not contextual factors in the production environment (negative shocks, absence of ex-post adjustments) change expectancy of goal attainment, and the third will allow me to assess whether or not my interventions affect expectancy of goal attainment.

4.5.4 Expectancy of Goal Adjustment

Arnold and Artz (2015) find that the availability intra-period adjustments leads to decreased firm performance, hypothesized to occur due to employees opportunistically withholding effort in anticipation of an intra-year adjustment. As outlined in section 3.3, my predictions are somewhat at odds with those of Arnold and Artz (2015).⁴⁷ However, Arnold and Artz (2015) are one of the few accounting studies to examine the motivational consequences of

⁴⁷ In contrast to Arnold and Artz (2015), I predict a positive effect of the availability of ex-post adjustments on performance, through improved justice perceptions.

ex-post adjustments. I believe this warrants collecting the expectancy of goal adjustment as a process measure to test their model as an alternative to mine.

To measure expectancy of goal adjustment, I ask participants to indicate the percentage likelihood that they will receive a goal adjustment in response to a negative event. I measure expectancy of goal adjustment at the same time as I administer the justice questionnaire, and intend to examine it as a possible alternative mediator for performance effects observed in my experiment. The expectancy of goal adjustment item is only administered to those in the non-control conditions, since participants in the control condition do not learn of and are not eligible for ex-post adjustments.

4.6 Summary

In this chapter, I explained that to test my hypotheses, I run a laboratory experiment employing a $2 \times 2 + 1$ control condition between subjects design. I described the economic setting of my experiment, including the basic incentive contract (bonus for goal attainment), and how I operationalized important contextual factors inherent to the ex-post adjustment setting (existence of negative events, availability of ex-post adjustments). I described my choice of participant pool, as well as an overview of my experimental procedures and manipulations. Finally, I described the measurement and collection of my key dependent variables (justice perceptions, performance, effort), as well as for my intended covariates (baseline performance) and process variables (self-other overlap, expectancy of goal attainment, expectancy of goal adjustment). In the next chapter, I discuss the results of this experiment.

CHAPTER 5: RESULTS

5.1 Introduction

In this chapter, I describe the results of my four sets of formal hypotheses tests, and examine my one set of research questions. The remainder of the chapter is organized as follows. In section 5.2, I discuss the results of my attention and manipulation checks. In section 5.3, I report results of confirmatory factor analysis for my measurement model of organizational justice. In sections 5.4 – 5.8, I formally test my four sets of hypotheses and my one set of research questions described in Chapter 3. In section 5.9, I test the fit of my theoretical model and compare it to alternative models. In section 5.10, I examine the effect of my experimental manipulations on effort duration and effort intensity. In section 5.11, I analyze participant goal attainment in normal rounds. I provide summary remarks in section 5.12.

5.2 Attention and Manipulation Checks

In this section, I discuss the results of my attention and manipulation checks. In total, I recruited 169 undergraduate business students from a Canadian university to take part in my experiment. Of these participants, 22 received random ex-post adjustments and are excluded from my main analyses, leaving me with a final sample of 147 participants (Table 1). On average, participants were 20.2 years old, and had 22.2 months of work experience, with 49.7% of participants identifying as female. One-way ANOVA of my demographic variables on *Condition* (untabulated), a categorical variable with five levels, one for each experimental

condition, show that these demographic variables do not differ significantly by condition (all $p > 0.20$, two-tailed).⁴⁸

Table 1. Mean (standard deviation) of gender, age, and work experience by condition

Condition ¹	Condition Label ²	<i>n</i>	Gender ³	Age ⁴	Work Experience ⁵
Control	Control	29	0.41 (0.50)	20.0 (1.1)	19.6 (16.8)
No explanation/No perspective taking	No Intervention	29	0.52 (0.51)	20.5 (1.7)	21.6 (16.7)
Explanation/No perspective taking	Explanation Only	30	0.50 (0.51)	20.1 (1.6)	22.6 (18.3)
No explanation/Perspective taking	Perspective only	30	0.63 (0.49)	20.0 (1.1)	23.5 (24.2)
Explanation/Perspective taking	Both Interventions	29	0.41 (0.50)	20.4 (2.6)	23.6 (23.6)
Mean			0.50 (0.50)	20.2 (1.7)	22.2 (20.0)

Notes:

¹ *Condition* describes which of the five experimental conditions participants belong to. Participants in the explanation (no explanation) conditions receive (do not receive) an explanation for management decision making accompanying their end of round feedback. Participants in the perspective taking (no perspective taking) conditions are provided (are not provided) with perspective taking training just prior to the start of the first production round.

² *Condition label* provides a descriptive label for each condition, and is used throughout Chapter 5 (not italicized, title cased), and in the remainder of the tables to describe conditions.

³ *Gender* describes the proportion of participants who indicated female in the demographic questionnaire.

⁴ *Age* describes participants' mean stated age in years.

⁵ *Work Experience* describes participants' mean stated work experience in months.

5.2.1 Attention Checks

In addition to the comprehension check questions administered during the experimental instructions described in section 4.3, I ask participants two attention check questions in the post-experimental questionnaire (Q2-Q3 in Appendix III). In my first attention check (Q2), I ask participants to rate their agreement with the statement “*My performance in some periods was*

⁴⁸ Further, none of the demographic variables are significantly correlated with any of my dependent variables. Therefore, I do not include any demographic variables as covariates in my tests of hypotheses.

negatively impacted by the increased difficulty of these periods".⁴⁹ This question assesses participant attention to negative uncontrollable events as a feature of their production environment, as well as their belief that the existence of these events negatively impacts performance. In my second attention check question (Q3), I ask participants to rate their agreement with the statement "*The experimental administrator had the ability to reduce the impact of this increased difficulty on my performance*". The purpose of this question is to assess whether participants in the Non-Control conditions attended to the ex-post adjustment policy and whether they believed that the experimental administrator could help them.⁵⁰ I focus on Non-Control participants for Q3 because Control participants do not learn of and do not have ex-post adjustments in their compensation contract.⁵¹

The average participant response to Q2 across all conditions is 3.2, which based on results of a one-sample *t*-test, is significantly greater than the midpoint of 2 ($p < 0.01$, one-tailed), indicating that on average, participants agreed that negative uncontrollable events negatively impacted their performance. A one-sample *t*-test on Q3 responses reveals that average participant response in Non-Control conditions is 2.9, which is significantly greater than the midpoint of 2 ($p < 0.01$, one-tailed). This indicates that on average, Non-Control participants

⁴⁹ All attention check questions were asked on a 5-point Likert scale ranging from "Strongly Disagree" (0) to "Strongly Agree" (4).

⁵⁰ Attention to all key elements of my compensation contracts are assessed through comprehension checks administered during the instructions (see section 4). However, I decided to ask participants these two attention check questions in the post-experimental questionnaire because they are key features of the participants' information environment. Further, as worded, these questions do not merely capture attention, they in part measure participants' perceptions about how these features impact their performance on the experimental task.

⁵¹ For consistency and clarity, throughout Chapter 5, I refer to conditions by their condition label defined in Table 1, non-italicized, title cased. Participants that are not in the Control condition are referred to as Non-Control participants.

attended to the ex-post adjustment policy and believed that the experimental administrator was able to reduce the impact of negative events using ex-post adjustments.⁵²

5.2.2 Manipulation Checks

As discussed in my literature review (subsection 2.4.2), in an organizational context, explanations are causal accounts or justifications provided to employees for management decision making. To assess the effectiveness of my explanation manipulation, I ask participants to rate their agreement with the statement “*I was provided adequate justification for decisions made during this experiment that impacted my performance and pay.*” (Appendix III, Q1).⁵³ The purpose of this question is to assess whether participants receiving an explanation perceive the explanation provided to be adequate. Results of a one-sample *t*-test indicate that participant responses in the Explanation Only condition differ significantly from the scale midpoint of 2 ($M = 2.7, p < 0.01$, one-tailed).⁵⁴ This suggests that on average, participants receiving only an explanation perceived these explanations to be adequate.⁵⁵ To further assess the effectiveness of my explanation manipulation, I ask Non-Control participants to rate their agreement with the statement “*Deciding whether or not to make a goal adjustment to help out an employee is a difficult decision.*” (Appendix III, Q4). This question assesses the degree to which participants receiving either intervention are aware of the dilemma faced by management making goal adjustments in my setting. Because participants stand to benefit from ex-post adjustments during

⁵² A one-way ANOVA on *Condition* (untabulated, excluding Control participants for Q3) show that responses to attention check questions do not differ by treatment condition (all $p > 0.20$).

⁵³ All manipulation check questions with the exception of the IOS Scale were asked on a 5-point Likert scale ranging from “Strongly Disagree” (0) to “Strongly Agree” (4).

⁵⁴ Since I do not make predictions about the interactive effects of perspective taking and explanation, analysis of manipulation checks and tests of hypotheses regarding the effects of perspective taking and explanation exclude Both Intervention participants.

⁵⁵ Including participants in the Both Interventions condition yields the same result that participants receiving an explanation perceive it to be adequate ($M = 2.4, p < 0.01$, two-tailed).

the experiment, but do not receive any during the production periods, I expect egocentrically biased participants to rate the decision to make ex-post adjustments as easy. I expect participants receiving either intervention to rate the decision as more difficult as they should have a more nuanced understanding of the tension management faces in making these adjustments. A two-sample t -test on participant responses indicates that participants in the Explanation Only condition ($M = 2.9$) rated the decision to make ex-post adjustments as more difficult than participants in the No Intervention condition ($M = 2.8$). However, this difference is not significant ($p = 0.28$, one-tailed).⁵⁶ Overall, results from the manipulation check questions provide some evidence that my explanation manipulation was successful.

To assess the effectiveness of my perspective taking manipulation, I ask two manipulation check questions in my post-experimental questionnaire, and also code participant responses to the perspective taking exercise described in subsection 4.3.1. The hypothesized effects of perspective taking in my experiment (section 3.5) are predicted to occur due to increased self-other overlap between the participant and the experimental administrator. To measure self-other overlap, I administer the *Inclusion of the Other in the Self* (hereafter IOS, Appendix 2) scale to participants in the post-experimental questionnaire. The IOS scale was developed by Aron et al. (2012) and is a one-item 7-point pictorial scale depicting two circles representing the self and the other in varying degrees of overlap, ranging from no overlap (0) to almost fully overlapped (6). A two-sample t -test of participant responses to the IOS scale indicates that participants in the Perspective Only condition ($M = 0.8$) did not perceive a greater degree of self-other overlap than participants in the No Intervention condition ($M = 1.1$) ($p =$

⁵⁶ Including participants in the Both Interventions condition, I observe that participants receiving an explanation ($M = 3.0$) rate the ex-post adjustment decision as more difficult than participants in the No Intervention condition ($M = 2.8$) but the difference is not significant at conventional levels ($p = 0.12$, one-tailed).

0.77, one-tailed). These results suggest that my perspective taking training exercise did not induce self-other overlap as measured using the IOS scale.⁵⁷ As in the case of explanation, I ask Non-Control participants to indicate whether performing an ex-post adjustment is a difficult decision (Appendix III, Q4). Results of a two-sample *t*-test indicate that Perspective Only participants ($M = 2.7$) did not rate the decision to make ex-post adjustments as more difficult than participants in the No Intervention condition ($M = 2.8, p = 0.54$, one-tailed). These results suggest that my perspective taking manipulation did not increase participants' perceptions of the difficulty of the ex-post goal adjustment decision.

To further assess the effectiveness of my perspective taking manipulation, I code and analyze participant responses to the perspective taking exercise. In the perspective taking exercise, participants perform goal adjustments for a pair of hypothetical employees, provide a justification for their decision as well as an explanation to the hypothetical employees for their decision. On average, participants receiving perspective taking training suggested a positive adjustment to the top performing hypothetical employee (Table 2, Panel A, $M = 0.8$).⁵⁸ Results of a one-sample *t*-test indicate that the average adjustment is significantly greater than zero ($p < 0.01$, two-tailed), but not significantly different from one ($p = 0.33$, two-tailed). An increase to the top performer's goal of one maintains the status quo in that there is no change in goal attainment for each employee as a result of the adjustment. This exercise reinforces the idea to perspective taking participants that management's decisions can be constrained such that they are

⁵⁷ Batson et al. (1997) notes that although the IOS scale is often used to measure self-other overlap, it can be low powered when examining relationships between strangers. Appropriately, I have collected additional manipulation checks to assess the effectiveness of my perspective taking manipulation.

⁵⁸ Recall that the top performing employee has beaten their goal by one unit of production, whereas the bottom performing employee has missed their goal by two units of production. Due to the zero-sum nature of goal adjustments, participants are unable to suggest a goal adjustment that would allow both participants to meet their goal.

Table 2. Descriptive statistics for metrics of perspective taking success**Panel A. Mean (standard deviation) of adjustment, cognitive perspective taking success, and group orientation by condition**

Condition Label ¹	<i>n</i>	Adjust ²	CogPT ³	Group ⁴
Perspective Only	30	0.8 (1.0)	0.83 (0.65)	0.60 (0.62)
Both Interventions	29	0.9 (1.3)	0.97 (0.78)	0.55 (0.63)
Mean		0.8 (1.2)	0.90 (0.71)	0.58 (0.62)

Panel B. Coding distribution by condition

Condition Label	Code ⁵	Adjust	CogPT	Group
Perspective Only	0	9	9	14
	1	14	17	14
	2	7	4	2
Both Interventions	0	11	9	15
	1	11	12	12
	2	7	8	2

Notes:

¹ For variable definitions, please refer to Table 1.

² *Adjust* describes the mean proposed adjustment to the top performing employee in the perspective taking exercise performed by participants in the perspective taking conditions.

³ *CogPT* describes participants' degree of success in taking the cognitive perspective of management in the perspective taking exercise on a scale from 0 to 2. Participants were coded as high in cognitive perspective taking if their response indicated a sensitivity towards the constraints faced by management in making ex-post adjustments, and were coded as low in cognitive perspective taking otherwise.

⁴ *Group* describes participants' degree of group or other-focused orientation on a scale from 0 to 2. Participants were coded as high in group orientation if their response focused on group outcomes over individual outcomes, and were coded as low in group orientation otherwise.

⁵ *Code* describes the value coded for each participant. For *Adjust*, values less than 0 are included in the '0' code category, and values greater than 2 are included in the '2' code category.

not always able to help underperforming employees. The fact that the average suggested goal adjustment to the top performer is less than, but not significantly different from one, indicates that on average, participants in the Perspective Only and Both Interventions conditions did not

suggest egocentric goal adjustments.⁵⁹ This provides some evidence that my perspective taking manipulation accomplished its intended purpose. That is, to reinforce and make available the cognition that management must sometimes make decisions that do not result in favourable outcomes for all.

In addition to the magnitude of participant adjustments discussed above, I also examine participants' justification for their proposed adjustments. With the help of an independent coder, I coded participant perspective taking responses on two variables: the degree to which participants took management's cognitive perspective (*CogPT*), and the degree to which participants adopted a group orientation in their response (*Group*).⁶⁰ Participant responses were coded on a scale from 0 to 2 increasing in their degree of successful cognitive perspective taking and group orientation.⁶¹ Participants whose responses indicate an awareness or sensitivity of management's constraints in performing ex-post adjustments, such as by referencing the zero-sum nature of adjustments, are coded as being high (2) in *CogPT*. Responses that fixate only on helping the underperforming employee are coded as being low (0) in *CogPT*, with responses falling in between coded as being moderate (1) in *CogPT*. Similarly, participants whose responses indicate a group or other-focused orientation, such as by referencing how goal adjustments impact employees at a group level, are rated as being high in *Group* (2). Similarly, responses that mainly reference individual outcomes are coded as low in *Group* (0), with

⁵⁹ Participants are not provided with any contextual cues that would justify helping the lower performing employee at the expense of materially hurting the top performing employee. Therefore, I consider an adjustment of two or greater to the top performer's goal to be egocentrically biased, since it is consistent with their preference to be helped in the upcoming production rounds.

⁶⁰ The independent coder is a doctoral student who is blind to condition and hypotheses.

⁶¹ I expect perspective taking to improve participants' ability to take management's cognitive perspective, which is why I code perspective taking responses on *CogPT*. In addition to this, it is possible that my perspective taking exercise improves other-regarding behaviour more broadly, which is why I code responses on *Group*.

responses falling in between coded as being moderate (1) in *Group*.⁶² Cronbach's alpha for both *CogPT* and *Group* show a high degree of initial inter-rater reliability ($\alpha = 0.96$ and 0.95 respectively). Initial inter-rater agreement for both *CogPT* and *Group* was 91.5%, with all coding discrepancies discussed and reconciled to generate the final codes used in analysis.

Analysis of participant responses show that on average, participants in the Perspective Only and Both Interventions conditions were moderately successful in taking management's cognitive perspective, with a mean *CogPT* of 0.90 (Table 2, Panel A).⁶³ Further, only 18 participants (9 per condition, 30%) were coded as having failed at taking management's cognitive perspective (0), indicating that the remaining 70% of participants were at least moderately successful in taking management's cognitive perspective (Table 2, Panel B).

Analysis of *Group* shows that participants were split on whether they adopted an other-focused group orientation, with mean *Group* being 0.58 across the two perspective taking conditions (Table 2, Panel A). Further, 29 participants in total (49%), 14 in the Perspective Only condition and 15 in the Both Interventions condition, were coded as 0 on *Group*, suggesting that the perspective taking exercise is only weakly successful in inducing a group or other-focused orientation. I further explore the consequences of *CogPT* and *Group* on manipulation and attention check responses by regressing responses to my manipulation check questions on *CogPT* and *Group*, finding that neither are significantly associated with self-other overlap as measured by the IOS scale (untabulated, all $p > 0.20$, two-tailed), or the perception of whether negative events influenced performance (untabulated, all $p > 0.20$, two-tailed). I find that *CogPT* has a negative but insignificant effect on perceptions of the adequacy of management explanations

⁶² A more detailed coding protocol provided to the independent coder is provided in Appendix IV.

⁶³ Mean *CogPT* of 1 suggests that on average, participants achieved moderate success in taking management's cognitive perspective.

(untabulated, $p = 0.28$, two-tailed). However, I find that *CogPT* has a significantly negative impact on participant perception of whether management is able to use goal adjustments to offset the impact of negative events (untabulated, $p = 0.05$, two-tailed), whereas *Group* has a directionally positive impact on these perceptions (untabulated, $p = 0.15$, two-tailed). Taken together, these results suggest that my perspective taking manipulation was somewhat successful in helping participants to adopt management's cognitive perspective, but was not successful in inducing a group or other-focused orientation.

5.3 Confirmatory Factor Analysis of Justice Questionnaire

As discussed in subsection 4.4.1, I measure procedural and distributive justice perceptions at three points in my experiment: 1) immediately after the bonus for goal attainment compensation contract is described to participants, 2) immediately before the start of the production rounds, and 3) immediately after the end of the production rounds. These are labelled timepoints A, B, and C respectively. Further, organizational justice scholars consider procedural and distributive justice to be two distinct theoretical constructs (subsection 2.3.1). To assess how my data fits the theoretical two-factor model, I perform confirmatory factor analysis on my seven justice questionnaire items (Appendix I). Results of my confirmatory factor analysis indicates that at all timepoints, justice questionnaire items load significantly on their respective theoretical latent constructs (Table 3, Panel A; β ranges from 0.70 to 0.90, all $p < 0.01$). Analysis of fit statistics also show reasonable fit to the theoretical two-factor model at each of the three timepoints (Table 3, Panel B). Both the χ^2 statistic (all $p < 0.01$) and RMSEA (all RMSEA >

Table 3. Confirmatory factor analysis of organizational justice measurement model

Panel A. Standardized coefficients for confirmatory factor analysis on two-factor and one-factor models of organizational justice at timepoints A, B, and C

Timepoint ¹	Two-factor model				One-factor model			
	Item	Latent Factor	β	SE	Item	Latent Factor	β	SE
A	PJ1	PJ	0.75		PJ1	J	0.72	
	PJ2	PJ	0.86	0.12	PJ2	J	0.84	0.12
	PJ3	PJ	0.75	0.12	PJ3	J	0.74	0.13
	DJ1	DJ	0.72		DJ1	J	0.71	0.13
	DJ2	DJ	0.71	0.11	DJ2	J	0.69	0.12
	DJ3	DJ	0.70	0.12	DJ3	J	0.69	0.13
	DJ4	DJ	0.90	0.12	DJ4	J	0.90	0.12
	B	PJ1	PJ	0.84		PJ1	J	0.80
PJ2		PJ	0.82	0.09	PJ2	J	0.78	0.09
PJ3		PJ	0.78	0.09	PJ3	J	0.74	0.10
DJ1		DJ	0.77		DJ1	J	0.75	0.10
DJ2		DJ	0.82	0.09	DJ2	J	0.79	0.09
DJ3		DJ	0.74	0.09	DJ3	J	0.72	0.09
DJ4		DJ	0.89	0.10	DJ4	J	0.90	0.09
C		PJ1	PJ	0.71		PJ1	J	0.64
	PJ2	PJ	0.86	0.14	PJ2	J	0.82	0.16
	PJ3	PJ	0.76	0.12	PJ3	J	0.71	0.14
	DJ1	DJ	0.87		DJ1	J	0.86	0.18
	DJ2	DJ	0.88	0.06	DJ2	J	0.86	0.17
	DJ3	DJ	0.81	0.07	DJ3	J	0.81	0.16
	DJ4	DJ	0.83	0.07	DJ4	J	0.83	0.16

Panel B. Goodness of fit statistics for two-factor and one-factor models of organizational justice at timepoints A, B, and C

Timepoint	Two-factor model				One-factor model			
	χ^2	SRMR ²	RMSEA ³	CFI ⁴	χ^2	SRMR	RMSEA	CFI
A	70.13	0.055	0.173	0.908	76.19	0.062	0.174	0.900
B	46.07	0.044	0.132	0.951	63.27	0.055	0.155	0.927
C	56.32	0.043	0.151	0.940	75.02	0.054	0.172	0.916

Table 3. Confirmatory factor analysis of organizational justice measurement model (continued)

Panel C. Goodness of fit statistics for two-factor and one-factor models of organizational justice at timepoints A, B, and C

	Two-Factor model		One-Factor model	
	AIC ⁵	BIC ⁵	AIC	BIC
A	2492.24	2558.03	2496.29	2559.10
B	2427.19	2492.99	2442.39	2505.19
C	2588.74	2654.53	2605.43	2668.23

Notes:

¹ Timepoint refers to one of three points in the experiment when the justice questionnaire was administered to participants. Timepoint A occurs right after the basic bonus for goal attainment compensation contract is explained to participants; timepoint B occurs right before the commencement of the production rounds; timepoint C occurs right after the end of the production rounds.

² SRMR represents the standardized root mean square residual, is an absolute measure of fit, and is calculated as the standardized difference between observed and predicted correlations. Values less than 0.08 are indicative of good model fit (Hu and Bentler, 1999).

³ RMSEA represents the root mean square error of approximation, is an absolute measure of fit based on the non-centrality parameter. Values less than 0.05 are indicative of good model fit (Hu and Bentler, 1999).

⁴ CFI represents the comparative fit index, is an incremental measure of fit based on the non-centrality parameter. Values greater than 0.90 are indicative of acceptable model fit (Hu and Bentler, 1999).

⁵ AIC and BIC represent the Akaike information criterion, and the Bayesian information criterion respectively, and describe the amount of information lost when fitting a candidate model to a data generating process. When selecting between candidate models, models with lower AIC and BIC lose less information when fitting to the data, and are preferred over models with higher AIC and BIC (Vrieze, 2012).

0.06) indicate poor model fit, whereas the SRMR (all SMRM < 0.08) indicates good model fit, and CFI indicates mediocre to good fit (ranging from 0.908 to 0.951).⁶⁴

Because examination of the fit indices does not unanimously support the conclusion of a good model fit, I examine the fit of an alternative one-factor model where my justice questionnaire simply captures general justice perceptions, not distinguishing between procedural and distributive justice. Analysis of this alternative model shows that the justice questionnaire items load significantly onto a single justice construct (Table 3, Panel A; β ranges from 0.64 to

⁶⁴ Hu and Bentler (1999) suggest that RMSEA < 0.06, SRMR < 0.08, and CFI > 0.95 are reasonable cut-offs for good model fit.

0.90, all $p < 0.01$). Further, fit indices suggest this model fits the data similarly to the two-factor model, but has directionally poorer fit according to SRMR, RMSEA, and CFI fit indices (Table 3, Panel B).⁶⁵ In addition, examination of comparative fit indices AIC and BIC (Table 3, Panel C) indicate that at each timepoint, there is somewhat greater information lost (greater AIC and BIC) by the one-factor model relative to the two-factor model, suggesting that the two-factor model better fits the data (Vrieze 2012).⁶⁶ Finally, Cronbach's alpha indicates a high degree of inter-item reliability for my justice scale at all timepoints under assumptions of a two-factor model (α ranges from 0.83 to 0.91) or a one-factor model (α ranges from 0.90 to 0.92). The above analyses indicate that the two-factor model fits the data no better than the one-factor model. Since the one-factor model is more parsimonious, and there is no other justification to favour the two-factor model, I have decided to opt for the one-factor model in my measurement of organizational justice. As such, in subsequent tests of hypotheses and supplemental analyses involving justice perceptions, I use a single composite measure of overall justice calculated as the sum of participant responses to all seven justice items in the questionnaires, one for each timepoint.⁶⁷

⁶⁵ Since my models are not nested, I cannot use a difference in χ^2 test to assess whether my theoretical two-factor model fits significantly better than my alternative one-factor model. However, I can examine comparative fit indices such as AIC or BIC, which measure the amount of information lost by model to compare non-nested models (Vrieze, 2012).

⁶⁶ AIC and BIC represent the Akaike information criterion, and the Bayesian information criterion respectively, and describe the amount of information lost when fitting a candidate model to a data generating process. A perfectly fitting model would have zero information loss when fitting the model to the data generating process (Burnham and Anderson 2004).

⁶⁷ Since my justice items are measured on the same scale, and the range of standardized loadings is small (β ranges from 0.70 – 0.90), I calculate composite variables through summation of item scores instead of calculating factor scores to make the change in justice perception across timepoints more comparable.

5.4 Test of Hypothesis 1

My first hypotheses predict a positive effect of the announcement of an ex-post adjustment policy on perceptions of overall justice (H1a) and performance (H1b). Recall that this announcement effect is expected to manifest in the period between the announcement and the first realization of management's decision to not perform an ex-post adjustment. Specifically, overall justice perceptions immediately after announcement (*PJB*) and performance in the first two rounds (*Prod12*) are expected to be greater for Non-Control participants relative to Control participants.⁶⁸ Descriptive statistics for overall justice perceptions can be found in Table 4. Across all conditions mean baseline overall justice perceptions (*JA*) is 19.8 out of a maximum of 28 (Panel A). A one-way ANOVA of *JA* on *Condition* (untabulated) show that baseline justice perceptions do not differ significantly by condition ($p > 0.20$, two-tailed). Examination of *JB* show that post-announcement, mean perceptions of overall justice decreases from 19.8 to 18.0.

Descriptive statistics for practice round and production rounds one and two performance can be found in Table 5, Panel A. Mean performance for the eight-minute practice round (*Practice*) is 33.3 units of production across all conditions. A one-way ANOVA of *Practice* on *Condition* (untabulated) shows that *Practice* does not differ significantly by condition ($p > 0.20$, two-tailed). Performance in the first normal four-minute production round (*Prod1*) on average is 23.8 units of production across all conditions, and performance in the first difficult four-minute production round (*Prod2*) on average is 11.8 units.

⁶⁸ Only production in the first two rounds is considered for H1b, as it is after the end of the 2nd production round that participants have faced their first difficult period, for which they may expect an ex-post adjustment. After the 2nd production round, I expect the observation that management has not performed an ex-post adjustment to result in different justice and performance effects, discussed in the next subsections.

Table 4. Mean (standard deviation) of overall justice perceptions at timepoints A, B, and C, by condition

Condition Label ¹	<i>n</i>	JA ²	JB ²	JC ²
Control	29	19.3 (5.5)	18.5 (6.4)	15.5 (7.0)
No Intervention	29	18.2 (6.3)	17.6 (5.9)	15.0 (7.1)
Explanation Only	30	20.7 (6.4)	17.4 (6.9)	17.9 (7.0)
Perspective Only	30	20.1 (5.8)	17.5 (5.9)	18.7 (5.9)
Both Interventions	29	20.4 (5.4)	18.9 (5.5)	18.3 (6.5)
Mean		19.8 (5.9)	18.0 (6.1)	17.1 (6.8)

Notes:

¹ For variable definitions, please refer to Table 1.

² *JA*, *JB*, and *JC* refer to participants' mean perceptions of overall justice, as measured using an organizational justice scale based on Thibaut and Walker (1975) and Leventhal (1980), and validated by Colquitt (2001), containing seven items measuring procedural and distributive justice, responses to which are summed to form a composite overall justice measure at timepoints A, B, and C.

In order to test H1a, I regress *JB* on the dummy variable *Adjustment*, which describes whether participants were in the Control condition (0) or not (1), and baseline overall justice perceptions (*JA*) as a co-variate.^{69,70} Results of this regression (Table 6, Panel A) show no effect of *Adjustment* on *JB*, ($p = 0.32$, two-tailed). Therefore, I do not find support for H1a.

⁶⁹ Though I present my multi-variate analyses as regressions, as modelled, they are mathematically equivalent to ANCOVA (van Breukelen, 2013). Further, I employ a levels approach to analyzing my data in H1, using the level of post-treatment as my dependent variable and controlling for the level of pre-treatment. van Breukelen (2013) argue that so long as groups are randomly assigned, as in my experiment, this approach is preferred over using change scores (post-treatment less pre-treatment) as the dependent variable.

⁷⁰ Although I considered the possibility that baseline ability on the task (*Practice*) may affect justice perceptions, I observe that *Practice* does not correlate significantly with any of my composite justice variables at any timepoint. So, I do not include *Practice* as a covariate in any analyses regarding justice perceptions.

Table 5. Descriptive statistics for performance variables**Panel A. Mean (standard deviation) of performance in practice round and production rounds 1-2 by condition**

Condition Label¹	<i>n</i>	Practice²	Prod1³	Prod2³	Prod12³
Control	29	31.7 (8.0)	23.2 (6.9)	9.7 (6.6)	32.9 (11.8)
No Intervention	29	35.2 (7.9)	25.6 (6.2)	12.3 (7.5)	37.9 (9.7)
Explanation Only	30	31.4 (9.3)	22.5 (7.7)	11.7 (7.1)	34.3 (12.7)
Perspective Only	30	35.5 (10.2)	25.8 (6.6)	13.4 (7.1)	39.2 (11.1)
Both Interventions	29	32.6 (7.3)	22.0 (7.7)	11.9 (5.4)	33.9 (10.0)
Mean		33.3 (8.7)	23.8 (7.1)	11.8 (6.8)	35.6 (11.3)

Panel B. Mean (standard deviation) of performance in production rounds 3-4 by condition

Condition Label	<i>n</i>	Prod3³	Prod4³	Prod34³
Control	29	9.6 (6.3)	18.3 (10.9)	27.9 (12.6)
No Intervention	29	9.2 (7.4)	17.2 (14.0)	26.4 (16.3)
Explanation Only	30	10.1 (6.5)	15.8 (12.1)	25.9 (15.9)
Perspective Only	30	8.5 (7.6)	23.0 (12.0)	31.6 (14.5)
Both Interventions	29	9.7 (7.3)	20.4 (10.2)	30.2 (13.9)
Mean		9.4 (7.0)	19.0 (12.0)	28.4 (14.7)

Notes:

¹ For variable definitions, please refer to Table 1.

² *Practice* refers to the mean number of tables counted by participants in the eight-minute practice round.

³ *Prod** refers to the mean number of tables counted by participants in round *. For example, *Prod1* represents the number of tables counted by participants in round 1, and *Prod12* represents the cumulative number of tables counted by participants in rounds 1 and 2.

Table 6. Tests of Hypothesis 1**Panel A. Effect of announcement on perceptions of procedural justice (n=147)**

$$\text{Model: } JB_i^1 = \beta_0 + \beta_1 \text{Adjustment}_i^2 + \beta_2 JA_i^1 + u_i^3$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	6.2	4.17	<0.001
<i>Adjustment</i>	-1.0	-1.00	0.321
<i>JA</i>	0.6	8.31	<0.001
Adjusted R ²	31.59%		

Panel B. Effect of announcement on cumulative performance in rounds 1 and 2 (n=147)

$$\text{Model: } Prod12_i^4 = \beta_0 + \beta_1 \text{Adjustment}_i + \beta_2 \text{Practice}_i^4 + u_i$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	13.9	4.29	<0.001
<i>Adjustment</i>	2.1	1.06	0.291
<i>Practice</i>	0.7	7.22	<0.001
Adjusted R ²	26.64%		

Panel C. Effect of announcement on performance in round 1 (n=147)

$$\text{Model: } Prod1_i^4 = \beta_0 + \beta_1 \text{Adjustment}_i + \beta_2 \text{Practice}_i + u_i$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	4.9	2.80	0.006
<i>Adjustment</i>	0.4	0.34	0.736
<i>Practice</i>	0.6	11.48	<0.001
Adjusted R ²	47.17%		

Table 6. Tests of Hypothesis 1 (continued)**Panel D. Effect of announcement on performance in round 2 (n=147)**

$$\text{Model: } Prod2_i^4 = \beta_0 + \beta_1 Adjustment_i + \beta_2 Practice_i + u_i$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	9.0	4.01	<0.001
<i>Adjustment_i</i>	2.5	1.79	0.076
<i>Practice_i</i>	0.1	1.54	0.126
Adjusted R ²	26.64%		

Panel E. Effect of announcement on performance in round 2 controlling for procedural justice perceptions (n=147)

$$\text{Model: } Prod2_i = \beta_0 + \beta_1 Adjustment_i + \beta_2 Practice_i + \beta_3 JB_i + u_i$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	6.2	2.15	0.033
<i>Adjustment_i</i>	2.6	1.85	0.067
<i>Practice_i</i>	0.1	1.68	0.095
<i>JB_i</i>	0.1	1.52	0.130
Adjusted R ²	3.61%		

Panel F. Effect of expectancy of goal adjustment on performance in round 2, including Control participants (n=147)

$$\text{Model: } Prod2_i = \beta_0 + \beta_1 Adjustment_i + \beta_2 Practice_i + \beta_3 JB_i + \beta_4 ExpectancyGAdjB_i^5 + u_i$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	6.1	1.93	0.055
<i>Adjustment_i</i>	2.5	1.31	0.192
<i>Practice_i</i>	0.1	1.68	0.096
<i>JB_i</i>	0.1	1.50	0.135
<i>ExpectancyGAdjB_i</i>	<0.1	0.08	0.937
Adjusted R ²	2.93%		

Table 6. Tests of Hypothesis 1 (continued)**Panel G. Effect of expectancy of goal adjustment on performance in round 2, excluding Control participants (n=118)**

$$\text{Model: } \text{Prod2}_i = \beta_0 + \beta_1 \text{Practice}_i + \beta_2 \text{JB}_i + \beta_3 \text{ExpectancyGAdjB}_i + u_i$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	7.1	1.99	0.049
<i>Practice_i</i>	0.1	1.06	0.291
<i>JB_i</i>	0.1	1.40	0.166
<i>ExpectancyGAdjB_i</i>	<0.1	0.03	0.979
Adjusted R ²	-0.25%		

Notes:

¹ For variable definitions, please refer to Table 4.

² *Adjustment* is a dummy variable that takes on a value of one if the participant is in a Non-Control condition, and a value of zero otherwise. Participants in the Control condition do not learn of the existence of ex-post adjustments and are not eligible for such adjustments.

³ u_i represents the error term.

⁴ For variable definitions, please refer to Table 5.

⁵ *ExpectancyGAdj** represents participants views on the likelihood that they will receive a goal adjustment from management, measured as a percentage at one of three timepoints, A, B, or C. For example, *ExpectancyGAdjB* represents the mean perceived likelihood of goal adjustment for participants as measured at timepoint B.

In order to test H1b, I regress *Prod12* on *Adjustment*, including baseline ability (*Practice*) as a co-variate.⁷¹ Results of this regression (Table 6, Panel B) show a positive but insignificant effect of *Adjustment* on performance ($p = 0.15$, one-tailed). To gain a better understanding of this relationship, I perform my test of H1b with alternate dependent variables: 1) performance in round 1, the first normal round (*Prod1*), and 2) performance in round 2, the first difficult round (*Prod2*). Results of these regressions (Table 6, Panel C and Panel D) show that there is no effect of *Adjustment* on *Prod1* ($p = 0.37$, one-tailed), but that there is a significant positive effect of

⁷¹ Other than *Practice*, representing baseline ability on the production task, no demographic variable correlates significantly with performance in any round, and so only *Practice* is included as a covariate in my tests of hypotheses concerning performance.

Adjustment on *Prod2* ($p = 0.04$, one-tailed). Overall, these results provide some support for H1b in showing a positive announcement effect on performance in the first difficult round.

Further analysis (Table 6, Panel E) shows that this positive announcement effect persists when controlling for *JB* ($p = 0.03$, one-tailed). These results suggest that although overall justice perceptions positively impact performance as predicted ($p = 0.07$, one-tailed), there is an announcement effect that operates independently of justice perceptions. Theory suggests that participants anticipating a goal adjustment may persist longer in the face of a difficult period because they may anticipate a goal adjustment to compensate for the difficulty of the round. To explore this possibility, I include the expectancy of goal adjustment at timepoint B (*ExpectancyGAdjB*) as a regressor in my model of round 2 performance. I perform this analyses two ways, including or excluding Control participants.⁷² Results of the analysis including Control participants (Table 6, Panel F) show no effect of *ExpectancyGAdjB* on *Prod2* ($p = 0.47$, one-tailed), with the effect of *Adjustment* on *Prod2* still being marginally significant ($p = 0.10$, one-tailed). Results of the analysis excluding Control participants (Table 6, Panel G) show no effect of *ExpectancyGAdjB* on *Prod2* ($p = 0.49$, one-tailed). Taken together, these analyses do not support the assertion that the positive announcement effect on round 2 performance is due to Non-Control participants expecting a goal adjustment.

In summary, my formal tests do not provide support for H1a, but provide some support for H1b. In contrast to my predictions, I do not observe a positive announcement effect on overall justice perceptions. However, I observe a significantly positive announcement effect on

⁷² I collect a measure of participants' expectancy of goal adjustment, but only present it to Non-Control participants, since Control participants do not have goal adjustments in their contract. Therefore, I cannot directly test the relationship between *Adjustment* and *ExpectancyGAdjB*. As an alternative test, I code *ExpectancyGAdjB* as zero in my analysis including Control participants.

performance in the first difficult round, round 2. Further analyses show that this positive announcement effect on round 2 performance persists when controlling for overall justice perceptions, suggesting that part of the announcement effect on performance occurs independently of overall justice perceptions. Finally, I find no support for the conjecture that the positive announcement effect observed in the first difficult round is due to Non-Control participants anticipating goal adjustments.

5.5 Test of Hypothesis 2

My second set of hypotheses predict that after participants first face a difficult round and do not receive an ex-post adjustment, i.e. they observe non-helping behaviour, their perceptions of overall justice will worsen (H2a) and their performance will suffer (H2b). Recall that this effect is expected to occur in the absence of a fairness intervention. Specifically, overall justice perceptions post-observation of non-helping behaviour (*JC*) are expected to worsen relative to overall justice perceptions prior to observing non-helping behaviour (*JB*) for participants in the No Intervention condition. Similarly, performance post-observation of non-helping behaviour (*Prod34*) is expected to suffer relative to performance prior to observing non-helping behaviour (*Prod12*) for No Intervention participants.

Descriptive statistics for overall justice perceptions pre and post-observation of non-helping behaviour can be found in Table 4. Mean overall justice perceptions for participants in the No Intervention condition in the pre-period is 17.6 (*JB*), dropping to 15.0 (*PJC*) in the post-period. Descriptive statistics for performance post-observation of non-helping behaviour can be found in Table 5, Panel A and Panel B. Mean cumulative performance in the pre-period for No Intervention participants is 37.9 units of production (Panel A), dropping to 26.4 units of

production (Panel B) in the post-period. Examination of performance in normal rounds (*Prod1* and *Prod4*) and difficult rounds (*Prod2* and *Prod3*) shows that the performance decrease occurs for both types of rounds. Mean performance in normal rounds is 25.6 units for No Intervention participants in the pre-period (Panel A), dropping to 17.2 in the post-period (Panel B). Mean performance in difficult rounds is 12.3 units for the same participants in the pre-period (Panel A), dropping to 9.2 units in the post-period (Panel B).

In order to test H2a, for participants in the Control and No Intervention condition, I regress overall justice perceptions (*J*) on the dummy variable *Post*, which takes on a value of one if the observation occurred post-observation of non-helping behaviour (i.e., at timepoint C), and zero otherwise, as well as the *Post*Adjustment* interaction, using a panel regression with participant fixed effects.⁷³ *Adjustment* is as defined above for the tests of H1a and H1b. The coefficient on *Post* describes the effect of the time trend on my dependent variable when all other factors in the model (i.e., *Adjustment*) take on a value of zero, whereas the coefficient on the *Post*Adjustment* interaction describes the marginal effect of *Adjustment* in the post-period. Results of this regression (Table 7, Panel A) show a negative and significant coefficient on *Post* ($p = 0.02$, one-tailed), and an insignificant coefficient on the *Post*Adjustment* interaction ($p = 0.79$, two-tailed). Therefore, I do not find support for my prediction that non-helping behaviour has a negative effect on justice perceptions. The significantly negative coefficient on *Post*, combined with the insignificant coefficient on *Post*Adjustment* suggests that as predicted, overall justice perceptions are significantly worse in the post-period for No Intervention

⁷³ Since I include participant fixed effects in the panel regression analyses used in my tests of H2, I do not include any time-invariant factors in my model, such as condition, baseline justice perceptions, or practice round performance. All time-invariant participant level factors that might affect justice perceptions or performance are controlled for with participant fixed effects.

Table 7. Tests of Hypothesis 2**Panel A. Effect of observed non-helping behaviour on perceptions of procedural justice for No Intervention participants, with participant fixed effects (n=116)**

$$\text{Model: } J_{it}^1 = \beta_0 + \beta_1 \text{Post}^2 + \beta_2 \text{Post} * \text{Adjustment}_i + a_i^3 + u_{it}^3$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	18.05	30.37	<0.001
<i>Post</i>	-2.55	-2.15	0.036
<i>Post*Adjustment_i</i>	0.45	0.27	0.791
Adjusted R ²	4.18%		

Panel B. Effect of observed non-helping behaviour and round difficulty on performance for No Intervention participants, with participant fixed effects (n=232)

$$\text{Model: } \text{Prod}_{it} = \beta_0 + \beta_1 \text{Post} + \beta_2 \text{Difficult}^5 + \beta_3 \text{Post} * \text{Adjustment}_i + \beta_4 \text{Post} * \text{Difficult} + \beta_5 \text{Adjustment}_i * \text{Difficult} + \beta_6 \text{Post} * \text{Adjustment}_i * \text{Difficult} + a_i + u_{it}$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	24.40	15.93	<0.001
<i>Post</i>	-8.34	-4.18	<0.001
<i>Difficult</i>	-13.21	-6.61	<0.001
<i>Post*Adjustment_i</i>	-3.45	-1.22	0.224
<i>Post*Difficult</i>	5.21	1.84	0.067
<i>Adjustment_i*Difficult</i>	0.38	0.13	0.893
<i>Post*Adjustment_i*Difficult</i>	0.38	0.09	0.925
Adjusted R ²	32.42%		

Notes:

¹ J_{it} represents overall justice perceptions for participant i at timepoint t . In the above panel regressions, justice perceptions measured at different timepoints represent separate observations. For measurement and scale information, refer to Table 4.

² *Post* is a dummy variable that takes on a value of one if the observation occurs after participants observe non-helping behaviour from management in the form of not receiving an ex-post adjustment, and zero otherwise.

³ a_i and u_{it} represent participant fixed effects and the error term respectively.

⁴ Prod_{it} represents the mean number of tables counted by participant i in round t . In the above panel regressions, performance in different rounds represent separate observations.

⁵ *Difficult* is a dummy variable that takes on a value of one if performance in the round in question is subject to a negative exogenous shock, and zero otherwise. The negative shock takes the form of an increase in the ratio of 1's to 0's in the tables to be counted by participants, increasing the time required to count the 1's in each table.

participants. However, this negative effect is no more pronounced for No Intervention participants relative to Control participants, and so I cannot attribute this effect for No Intervention participants to *Adjustment*.

Overall, I do not find support for my prediction (H2a), that non-helping behaviour will lead to less favourable overall justice perceptions. I do find that overall justice perceptions are lower in the post-period for No Intervention participants, but this effect is attributable to the time trend common to Control and No Intervention participants. This suggests that the decrease in overall justice perceptions pre-/post-observation of non-helping behaviour appears to be driven by factors common to the Control and No Intervention conditions, such as the existence of negative uncontrollable events causing my participants to not meet their goals in difficult rounds.

To test H2b, for Control and No Intervention participants, I regress performance (*Prod*) on the dummy variable *Post*, the dummy variable *Difficult*, which takes on a value of one if the observation occurs in a difficult round (i.e., rounds 2 or 3) and zero otherwise, as well as every two and three-way interaction between *Post*, *Adjustment*, and *Difficult*, using a panel regression with participant fixed effects. The coefficients of interest in this regression are the coefficients on *Post*, *Post*Adjustment*, and *Post*Adjustment*Difficult*. The interpretation of coefficients on *Post* and *Post*Adjustment* are as interpreted for the test of H2a, except that the *Post*Adjustment* effect is conditional on *Difficult* taking a value of zero (i.e., the marginal effect of *Adjustment* in the post-period in normal rounds). The coefficient on the *Post*Adjustment*Difficult* interaction is interpreted as the interactive effect of *Adjustment* and *Difficult* in the post-period. Results of this test (Table 7, Panel B) show a significantly negative coefficient on *Post* ($p < 0.01$, one-tailed), a negative but insignificant coefficient on the *Post*Adjustment* interaction ($p = 0.11$, one-tailed), and an insignificant coefficient on the *Post*Adjustment*Difficult* interaction ($p = 0.93$,

two-tailed). Taken together, these results suggest that performance is significantly worse in the post-period for Control participants, that this effect is directionally worse for No Intervention participants relative to Control participants and does not depend on round difficulty. Therefore, I do not find support for H2b, that participants perform worse after observing non-helping behaviour.

In summary, I find mixed support for my second set of hypotheses. On the one hand, I find strong evidence that overall justice perceptions and performance are significantly worse in the post-period relative to the pre-period. On the other hand, I find no evidence that the deterioration of overall justice perceptions is due to observation of non-helping behaviour, and only find directional evidence that the decrease in performance is due to non-helping behaviour. Taken together, these results suggest that as a result of experiencing negative shocks to performance in round 2, causing participants to miss their goals, participant overall justice perceptions and performance suffer, and the existence of ex-post adjustments does not seem to have an effect on this, other than the directionally negative effect it has on performance.

5.6 Test of Hypothesis 3

My third set of hypotheses predict that after participants first face a difficult round and do not receive an ex-post adjustment (observed non-helping behaviour), receiving an explanation for this decision will attenuate the negative effects observed on justice perceptions (H3a) and performance (H3b). Specifically, overall justice perceptions (*JC*) post-observation of non-helping behaviour for participants in the Explanation Only condition are expected to improve

relative to those of participants in the No Intervention condition.⁷⁴ Similarly, performance post-observation of non-helping behaviour (*Prod34*) for Explanation Only participants is expected to improve relative to No Intervention participants.

Descriptive statistics for *JC* can be found in Table 4. Mean *JC* in the Explanation Only condition ($M = 17.9$) is greater than for participants in the No Intervention condition ($M = 15.0$). Descriptive statistics for *Prod34* can be found in Table 5, Panel B. Mean *Prod34* in the Explanation Only condition ($M = 25.9$) is less than for participants in the No Intervention condition ($M = 26.4$).

In order to test H3a, for participants in the No Intervention and Explanation Only conditions, I regress *J* on the dummy variable *Post* and the *Post*ExplanationOnly* interaction, using a panel regression with participant fixed effects. *ExplanationOnly* is a dummy variable that takes on a value of 1 if participants are in the Explanation Only condition, and 0 otherwise.⁷⁵ The interpretation of the coefficient on *Post* is as above in the test of H2a, and the coefficient on the *Post*Explanation* interaction describes the marginal effect of receiving an explanation in the *Post* period. Results of this regression (Table 8, Panel A) show a significant negative coefficient on *Post* ($p = 0.02$, one-tailed) and a positive and significant coefficient on the *Post*ExplanationOnly* interaction ($p = 0.04$, one-tailed). These results suggest that as with my tests of H2a, there is a significant decrease in overall justice perceptions in the No Intervention condition after observing non-helping behaviour, and that this decrease is attenuated by

⁷⁴ I do not make any predictions about the interactive effects of explanation and perspective taking (RQ1). Accordingly, my tests of H3 exclude Perspective Only and Both Interventions participants, and my tests of H4 exclude Explanation Only and Both Interventions participants.

⁷⁵ For clarity, I use *ExplanationOnly* and *PerspectiveOnly* in my tests of H3 and H4 respectively, and *Explanation* and *Perspective* in my tests of RQ1, to identify that I am excluding Both Intervention participants from my tests of H3 and H4.

Table 8. Tests of Hypothesis 3**Panel A. Effect of explanation on perceptions of procedural justice post-observation of non-helping behaviour for participants in the No Intervention and Explanation Only conditions, with participant fixed effects (n=118)**

$$\text{Model: } J_{it}^1 = \beta_0 + \beta_1 \text{Post}^1 + \beta_2 \text{Post} * \text{ExplanationOnly}_i^2 + a_i^1 + u_{it}^1$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	17.47	28.62	<0.001
<i>Post</i>	-2.55	-2.07	0.043
<i>Post*ExplanationOnly_i</i>	3.09	1.79	0.079
Adjusted R ²	2.82%		

Panel B. Effect of explanation on performance post-observation of non-helping behaviour for participants in the No Intervention and Explanation Only conditions, with participant fixed effects (n=236)

$$\text{Model: } \text{Prod}_{it}^1 = \beta_0 + \beta_1 \text{Post} + \beta_2 \text{Difficult}^1 + \beta_3 \text{Post} * \text{ExplanationOnly}_i + \beta_4 \text{Post} * \text{Difficult} + \beta_5 \text{ExplanationOnly}_i * \text{Difficult} + \beta_6 \text{Post} * \text{ExplanationOnly}_i * \text{Difficult} + a_i + u_{it}$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	24.02	23.97	<0.001
<i>Post</i>	-8.34	-4.13	<0.001
<i>Difficult</i>	-13.21	-6.53	<0.001
<i>Post*ExplanationOnly_i</i>	1.58	0.56	0.578
<i>Post*Difficult</i>	5.21	1.82	0.070
<i>ExplanationOnly_i*Difficult</i>	2.41	0.85	0.397
<i>Post*ExplanationOnly_i*Difficult</i>	-0.07	-0.02	0.985
Adjusted R ²	26.25%		

Notes:

¹ For variable definition, please see notes to Table 7.

² *ExplanationOnly* is a dummy variable that takes on a value of one if the participant is in the Explanation Only condition, and a value of zero otherwise. Participants in Explanation Only condition receive an explanation for management's decision to exercise ex-post adjustments or not after each of the four production rounds, and do not receive perspective taking training.

ExplanationOnly. Therefore, I find support for my third hypothesis that an explanation for management decision making improves justice perceptions after employees observe non-helping behaviour relative to employees receiving no intervention.

In order to test H3b, for participants in the No Intervention and Explanation Only conditions, I regress *Prod* on the dummy variables *Post* and *Difficult*, as well as every two and three-way interaction between *Post*, *ExplanationOnly*, and *Difficult*, using a panel regression with participant fixed effects. Coefficients of interest in this regression are coefficients on *Post*, *Post*ExplanationOnly*, and *Post*ExplanationOnly*Difficult*, with the interpretation of these coefficients as above in the tests of H2b, except that I examine the marginal effect of *ExplanationOnly* as opposed to *Adjustment*. Results of this regression (Table 8, Panel B) show a significant negative coefficient on *Post* ($p < 0.01$, one-tailed), and insignificant coefficients on the *Post*ExplanationOnly* interaction ($p = 0.29$, one-tailed), as well as on the *Post*ExplanationOnly*Difficult* interaction ($p = 0.99$, two-tailed). These results suggest that performance decreases significantly in the post-period for No Intervention participants, that this effect is not attenuated by providing an explanation on its own.

In summary, I find some support for my third set of hypotheses. Providing an explanation on its own for observed non-helping behaviour significantly improves overall justice perceptions relative to individuals not receiving an intervention. However, I fail to find support for my prediction that providing an explanation on its own improves performance relative to receiving no intervention. Taken together, these results suggest that although providing an explanation can improve employees' justice perceptions, these improved justice perceptions may not lead to improved performance when they observe non-helping behaviour from management.

5.7 Test of Hypothesis 4

My fourth set of hypotheses predict that after participants first observe non-helping behaviour, having undergone perspective taking training will attenuate the negative effects of non-helping behaviour on justice perceptions (H4a) and performance (H4b). Specifically, overall justice perceptions post-observation of non-helping behaviour (*JC*) are expected to improve for participants receiving only perspective taking training relative to participants receiving no fairness intervention at all. Further, performance post-observation of non-helping behaviour (*Prod34*) for participants receiving Perspective Only is expected to improve relative to participants receiving no fairness intervention.

Descriptive statistics for *JC* can be found in Table 4. Mean *JC* in the Perspective Only condition ($M = 18.7$) is greater than for participants in the No Intervention condition ($M = 15.0$). Descriptive statistics for *Prod34* can be found in Table 5, Panel B. Mean *Prod34* in the Perspective Only condition ($M = 31.6$) is greater than for participants in the No Intervention condition ($M = 26.4$).

To test H4a, for participants in the No Intervention and Perspective Only conditions, I regress *J* on the dummy variable *Post* and the *Post*PerspectiveOnly* interaction, using a panel regression with participant fixed effects. *PerspectiveOnly* is a dummy variable that takes on a value of 1 if participants are in the Perspective Only condition, and 0 otherwise. Interpretation of coefficients are as above in the test of H3a, except I examine the marginal effects of *PerspectiveOnly* as opposed to *ExplanationOnly*. Results of this regression (Table 9, Panel A) show a significantly positive *Post*PerspectiveOnly* interaction ($p = 0.01$, one-tailed), indicating that after observing non-helping behaviour, providing perspective taking only significantly

Table 9. Tests of Hypothesis 4

Panel A. Effect of perspective taking on perceptions of procedural justice post-observation of non-helping behaviour for participants in the No Intervention and Perspective Only conditions, with participant fixed effects (n=118)

$$\text{Model: } J_{it}^1 = \beta_0 + \beta_1 \text{Post}^1 + \beta_2 \text{Post} * \text{PerspectiveOnly}_i^2 + a_i^1 + u_{it}^1$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	17.53	31.72	<0.001
<i>Post</i>	-2.55	-2.29	0.026
<i>Post*PerspectiveOnly_i</i>	3.79	2.42	0.019
Adjusted R ²	4.57%		

Panel B. Effect of perspective taking on performance post-observation of non-helping behaviour for participants in the No Intervention and Perspective Only conditions, with participant fixed effects (n=236)

$$\text{Model: } \text{Prod}_{it}^1 = \beta_0 + \beta_1 \text{Post} + \beta_2 \text{Difficult}^1 + \beta_3 \text{Post} * \text{PerspectiveOnly}_i + \beta_4 \text{Post} * \text{Difficult} + \beta_5 \text{PerspectiveOnly}_i * \text{Difficult} + \beta_6 \text{Post} * \text{PerspectiveOnly}_i * \text{Difficult} + a_i + u_{it}$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	25.69	24.18	<0.001
<i>Post</i>	-8.34	-3.89	<0.001
<i>Difficult</i>	-13.21	-6.16	<0.001
<i>Post*PerspectiveOnly_i</i>	5.54	1.84	0.067
<i>Post*Difficult</i>	5.21	1.72	0.088
<i>PerspectiveOnly_i*Difficult</i>	0.77	0.26	0.797
<i>Post*PerspectiveOnly_i*Difficult</i>	-7.27	-1.71	0.089
Adjusted R ²	36.48%		

Table 9. Tests of Hypothesis 4 (continued)**Panel C. Effect of perspective taking on performance in normal rounds post-observation of non-helping behaviour for participants in the No Intervention and Perspective Only conditions, with participant fixed effects (n=236)**

$$\text{Model: } Prod_{it} = \beta_0 + \beta_1 Post + \beta_2 Post * PerspectiveOnly_i + a_i + u_{it}$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	24.02	25.04	<0.001
<i>Post</i>	-8.34	-4.03	<0.001
<i>Post*PerspectiveOnly_i</i>	5.54	1.91	0.061
Adjusted R ²	10.45%		

Panel D. Effect of perspective taking on performance in difficult rounds post-observation of non-helping behaviour for participants in the No Intervention and Perspective Only conditions, with participant fixed effects (n=236)

$$\text{Model: } Prod_{it} = \beta_0 + \beta_1 Post + \beta_2 Post * PerspectiveOnly_i + a_i + u_{it}$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	12.88	13.91	<0.001
<i>Post</i>	-3.14	-1.68	0.098
<i>Post*PerspectiveOnly_i</i>	-1.73	-0.66	0.512
Adjusted R ²	6.94%		

Notes:

¹ For variable definition, please see notes to Table 7.

² *PerspectiveOnly* is a dummy variable that takes on a value of one if the participant is in the Perspective Only condition, and a value of zero otherwise. Participants in the Perspective Only condition perform a perspective taking exercise just prior to the commencement of the production rounds, where they simulate the experimental administrator's decision to make ex-post adjustments on a pair of hypothetical employees and provide justification for their decision. These participants do not receive an explanation for management decision making.

improves overall justice perceptions relative to participants receiving no intervention. Therefore, I find support for my fourth hypothesis that perspective taking training can improve overall justice perceptions after employees observe non-helping behaviour relative to employees receiving no intervention.⁷⁶

To test H4b, for participants in the No Intervention and Perspective Only conditions, I regress *Prod* on the dummy variables *Post*, *PerspectiveOnly*, and *Difficult*, as well as every two and three-way interaction between *Post*, *PerspectiveOnly*, and *Difficult*, using a panel regression with participant fixed effects. Coefficients of interest and their interpretation are as above in the test of H3b, except that I examine the marginal effect of *PerspectiveOnly* as opposed to *ExplanationOnly*. Results of this regression (Table 9, Panel B) show a significantly positive coefficient on the *Post*PerspectiveOnly* interaction ($p = 0.03$, one-tailed), and a marginally significant negative coefficient on the *Post*PerspectiveOnly*Difficult* interaction ($p = 0.09$, two-tailed). These results suggest that providing perspective taking training on its own significantly improves performance after observing non-helping behaviour relative to providing no intervention, and that this positive effect is marginally weaker in difficult rounds. Additional analyses (Table 9, Panel C and Panel D) show that the *Post*PerspectiveOnly* interaction is significant in normal rounds ($p = 0.03$, one-tailed), but not in difficult rounds ($p = 0.51$, two-tailed).⁷⁷ Overall, I find support for my hypothesis that providing perspective taking training on

⁷⁶ It is possible that the effects of my manipulations on justice perceptions could be attributable in part to demand effects. Specifically, the experimental administrator is responsible for paying participants and so participants feel compelled to rate the administrator as fair. However, I do not believe demand characteristics are driving any of my observed effects, since: 1) I observe that my manipulations only have a significant effect on justice perceptions *post* observing non-helping behaviour but not prior, and 2) with respect to perspective taking, I also observe real performance effects, requiring the provision of real effort.

⁷⁷ This is consistent with Kelly et al. (2015), who only find positive effects of ex-post adjustments when goals were moderately but not highly difficult. In my difficult rounds, due to the occurrence of negative events, goal attainment becomes highly difficult, impairing motivation to provide effort towards goal attainment.

its own improves performance subsequent to observed non-helping behaviour relative to providing no intervention, and that this effect is only significant in normal rounds.

In summary, I find support for my hypothesis that providing perspective taking training on its own improves perceptions of justice relative to providing no intervention, and that these improved justice perceptions result in improved performance. Specifically, I find that perspective taking on its own has a significant positive effect on overall justice perceptions. Further, I find that perspective taking on its own improves performance subsequent to observed non-helping behaviour, but only in normal rounds. Taken together, this suggests that providing perspective taking training can improve employees' justice perceptions and performance when they observe non-helping behaviour from management.

5.8 Examination of Research Questions

In section 3.5, I pose a set of research questions about the interactive effects of explanation and perspective taking on justice perceptions and performance. In order to examine RQ1a, I regress J on the dummy variable $Post$, as well as on the $Post*Explanation$, $Post*Perspective$, and $Post*Explanation*Perspective$ interactions, using a panel regression with participant fixed effects. $Explanation$ is a dummy variable that takes on a value of one if participants receive an explanation, and zero otherwise. Similarly, $Perspective$ is a dummy variable that takes on a value of one if participants receive perspective taking training, and zero otherwise. The coefficient of interest in this analysis is the coefficient on the $Post*Explanation*Perspective$ interaction, which describes the interactive effects of explanation and perspective taking in the post-period. Results of this regression (Table 10 Panel A) show a significantly negative coefficient on the $Post*Explanation*Perspective$ interaction ($p = 0.02$,

Table 10. Analysis of Research Question

Panel A. Effect of explanation and perspective taking on perceptions of procedural justice post-observation of non-helping behaviour for Non-Control participants, with participant fixed effects (n=236)

$$\text{Model: } J_{it}^1 = \beta_0 + \beta_1 \text{Post}^1 + \beta_2 \text{Post} * \text{Explanation}_i^2 + \beta_3 \text{Post} * \text{Perspective}_i^3 + \beta_5 \text{Post} * \text{Explanation}_i * \text{Perspective}_i + a_i^1 + u_{it}^1$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	17.83	48.02	<0.001
<i>Post</i>	-2.55	-2.41	0.018
<i>Post*Explanation_i</i>	3.09	2.08	0.040
<i>Post*Perspective_i</i>	3.79	2.55	0.012
<i>Post*Explanation_i*Perspective_i</i>	-4.90	-2.33	0.021
Adjusted R ²	2.18%		

Panel B. Effect of explanation on perceptions of procedural justice post-observation of non-helping behaviour for participants in the Perspective Only and Both Interventions conditions, with participant fixed effects (n=118)

$$\text{Model: } J_{it} = \beta_0 + \beta_1 \text{Post} + \beta_2 \text{Post} * \text{Explanation}_i + a_i + u_{it}$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	18.19	43.01	<0.001
<i>Post</i>	1.23	1.47	0.147
<i>Post*Explanation_i</i>	-1.82	-1.52	0.134
Adjusted R ²	0.08%		

Table 10. Analysis of Research Question (continued)

Panel C. Effect of perspective taking on perceptions of procedural justice post-observation of non-helping behaviour for participants in the Explanation Only and Both Interventions conditions, with participant fixed effects (n=118)

Model: $J_{it} = \beta_0 + \beta_1 Post + \beta_2 Post * Perspective_i + a_i + u_{it}$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	18.14	36.54	<0.001
<i>Post</i>	0.53	0.54	0.590
<i>Post*Perspective_i</i>	-1.12	-0.80	0.428
Adjusted R ²	0.06%		

Panel D. Effect of explanation and perspective taking on performance post-observation of non-helping behaviour for Non-Control participants, with participant fixed effects (n=472)

Model: $Prod_{it}^1 = \beta_0 + \beta_1 Post + \beta_2 Difficult^1 + \beta_3 Post * Explanation_i + \beta_4 Post * Perspective_i + \beta_5 Post * Difficult + \beta_6 Difficult * Explanation_i + \beta_7 Difficult * Perspective_i + \beta_8 Post * Explanation_i * Difficult + \beta_9 Post * Perspective_i * Difficult + \beta_{10} Difficult * Explanation_i * Perspective_i + \beta_{11} Post * Explanation_i * Perspective_i + a_i + u_{it}$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	23.98	34.80	<0.001
<i>Post</i>	-8.15	-4.44	<0.001
<i>Difficult</i>	-13.01	-7.09	<0.001
<i>Post*Explanation_i</i>	1.19	0.50	0.618
<i>Post*Perspective_i</i>	5.16	2.16	0.031
<i>Post*Difficult</i>	4.81	2.01	0.046
<i>Difficult*Explanation_i</i>	2.02	0.85	0.398
<i>Difficult*Perspective_i</i>	0.39	0.16	0.872
<i>Post*Explanation_i*Difficult</i>	0.70	0.26	0.799
<i>Post*Perspective_i*Difficult</i>	-6.50	-2.36	0.019
<i>Difficult*Explanation_i*Perspective_i</i>	0.67	0.24	0.809
<i>Post*Explanation_i*Perspective_i</i>	0.45	0.16	0.871
Adjusted R ²	30.42%		

Table 10. Analysis of Research Question (continued)

Notes:

¹ For variable definition, please see notes to Table 7.

² *Perspective* is a dummy variable that takes on a value of one if the participant is in the Perspective Only or Both Interventions condition, and a value of zero otherwise. Participants in these conditions perform a perspective taking exercise just prior to the commencement of the production rounds, where they simulate the experimental administrator's decision to make ex-post adjustments on a pair of hypothetical employees and provide justification for their decision.

³ *Explanation* is a dummy variable that takes on a value of one if the participant is in the Explanation Only or Both Interventions condition, and a value of zero otherwise. Participants in these conditions receive an explanation for management's decision to exercise ex-post adjustments or not after each of the four production rounds.

two-tailed). These results suggest are suggestive of suppression, or that these interventions act as substitutes, i.e., each intervention appears to improve justice perceptions on their own (H3a, H4a), but their effectiveness is hindered when used together.

In order to further explore the nature of this negative interaction, I examine the simple effects of *Explanation* and *Perspective* at each level of *Perspective* and *Explanation* respectively. Recall that my tests of H3a and H4a show that the simple effect of each intervention on justice perceptions in the absence of the other intervention are positive and significant. Therefore, here I only report results of the marginal effects of each intervention on justice perceptions in the *presence* of the other intervention, i.e., the marginal effects of *Perspective* and *Explanation* for participants already receiving the other intervention.⁷⁸ Results of this further analysis show that the effect of *Explanation* on *J* conditional on receiving perspective taking training is negative but not significant (Table 10 Panel B, $p = 0.13$, two-tailed). Similarly, the effect of *Perspective* on *J* conditional on receiving an explanation is not significant (Table 10, Panel C, $p = 0.43$, two-tailed). All considered, these results suggest that with respect to justice perceptions, *Explanation*

⁷⁸ For these analyses, I compare the: (a) the Explanation Only to the Both Interventions condition to examine the marginal effect of perspective taking on participants already receiving an explanation; and (2) Perspective Only to the Both Interventions condition to examine the marginal effect of explanation on participants already receiving perspective taking training.

and *Perspective* act as substitutes, because conditional on receiving one intervention, there is no marginal effect of receiving the other.

In order to examine RQ1b, I regress *Prod* on the dummy variables *Post* and *Difficult*, as well as every two and three-way interaction between *Post*, *Explanation*, *Perspective*, and *Difficult*, using a panel regression with participant fixed effects.⁷⁹ As in the case of RQ1a, the coefficient of interest is the one on the *Post*Explanation*Perspective* interaction. Results of this analysis (Table 10, Panel D) show that the coefficient on the *Post*Explanation*Perspective* interaction is insignificant ($p = 0.87$, two-tailed), suggesting no significant interactive effect of *Explanation* and *Perspective* on *Prod*.

In summary, I find evidence of a significant negative *Explanation* by *Perspective* interaction on overall justice perceptions, but not on performance. Further analyses show that the nature of the interaction for overall justice perceptions is consistent with *Explanation* and *Perspective* being substitutes. Further, I find no evidence that *Explanation* and *Perspective* have an interactive effect on *Prod*.

5.9 Mediation Model

In my hypotheses development (Chapter 4), my hypothesized effects of explanation and perspective taking on performance are mediated by justice perceptions. To test this mediation model, I perform path analyses on the theoretical model (Figure 1, Panel B).⁸⁰ For the theoretical

⁷⁹ The *Explanation*Perspective* interaction is excluded from the panel regression. Since this interaction term is participant specific and time invariant, it cannot be estimated directly and instead is captured by the participant fixed effects term. I further exclude the *Post*Explanation*Perspective*Difficult* interaction due to complexity in interpreting a coefficient on such an interaction. Inferences remain unchanged if I including this interaction term, and the coefficient on this interaction is not significant.

⁸⁰ Because I do not find any effect of announcement on justice perceptions, it does not appear that the effect of announcement on performance is mediated through justice perceptions. Therefore, the focus of my tests of mediation are on the effect of my interventions on *Prod* in the post-period, where I observe effects of my

model, χ^2 is significant ($p < 0.01$, one-tailed), RMSEA = 0.185, CFI = 0.623, and SRMR = 0.086, all of which indicate poor model fit (Table 11, Panel B).⁸¹ Due to this poor fit, I explore an alternative model specification. Motivated by the efficient contracting literature suggesting that employees face uncertainty over whether management will exercise ex-post adjustments (Baiman and Rajan 1995), and that this uncertainty weakens the effort-performance relationship (expectancy) in the incentive contract (Holmström 1979; Bol 2008), I examine an expectancy based model of performance (Figure 5). My expectancy-based model of performance suggests that the effect of fairness on performance is mediated through expectancy. That is, when individuals observe unfair outcomes and procedures, the perceived strength of the effort-performance relationship (i.e. expectancy) with respect to the incentive scheme weakens, resulting in low effort provision and thus poor performance. This model is identical to the theoretical model, except that it estimates an additional two paths, one from *JC* to expectancy of goal attainment at timepoint C (*ExpectancyC*), and one from *ExpectancyC* to *Prod4*.⁸²

Examination of goodness of fit statistics for these two models show that the expectancy based model appears to fit the data better than the theoretical model (Table 11, Panel B).

Examination of the comparative fit indices AIC and BIC show that information loss appears to be lower for the expectancy based models relative to the theoretical model.

interventions on justice perceptions (H3a and H4a) and performance in normal rounds (H4b). Since I am only concerned with *Prod* in the post period in normal rounds (i.e., *Prod4*), I test for mediation using a levels model, controlling for baseline levels of my measured variables, as opposed to the fixed effects model used in my tests of H2-H4. I believe that this is appropriate, and a conservative test of mediation, since I observe significant negative time trends on *DJ* and *Prod* in my tests of H2-H4, which work against finding the predicted positive indirect effects of my interventions on *Prod4*.

⁸¹ Hu and Bentler (1999) suggest cut-offs for good model fit of: SRMR < 0.08, RMSEA < 0.06, and CFI > 0.900.

⁸² I considered another alternative model that reversed the relationship between *ExpectancyC* and *JC* to test reverse causality. Although this model fits somewhat better than the expectancy model I have presented (results untabulated) I believe the expectancy model presented is better grounded in theory, and so is preferred over the reverse-causality model.

Table 11. Mediation analyses

Panel A. Standardized path coefficients for theoretical, theoretical with distributive dominance, expectancy, and expectancy with distributive dominance models (n = 118)

Paths	Model	
	Theoretical ¹	Expectancy ¹
Path coefficients:		
<i>Perspective</i> ⁵ → <i>JC</i> ²	0.282***	0.282***
<i>Explanation</i> ⁶ → <i>JC</i>	0.226**	0.226**
<i>Perspective</i> * <i>Explanation</i> → <i>JC</i>	-0.280**	-0.280**
<i>JB</i> ² → <i>JC</i>	0.586***	0.586***
<i>Perspective</i> → <i>Prod4</i> ³	0.184*	0.105
<i>Explanation</i> → <i>Prod4</i>	-0.027	-0.116
<i>Perspective</i> * <i>Explanation</i> → <i>Prod4</i>	-0.018	0.048
<i>JC</i> → <i>Prod4</i>	0.169**	0.072
<i>Practice</i> → <i>Prod4</i>	0.320***	0.320***
<i>ExpectancyB</i> ⁴ → <i>ExpectancyC</i> ⁴	0.184**	0.089
<i>JC</i> → <i>ExpectancyC</i>		0.281***
<i>ExpectancyC</i> → <i>Prod4</i>		0.363***
Indirect effects:		
<i>Explanation</i> → <i>Prod4</i>		0.969*
<i>Perspective</i> → <i>Prod4</i>		1.205*
<i>Perspective</i> * <i>Explanation</i> → <i>Prod4</i>		-1.390*

* represents coefficients significant at the 0.10 level, one-tailed, bolded for emphasis.

** represents coefficients significant at the 0.05 level, one-tailed, bolded for emphasis.

*** represents coefficients significant at the 0.01 level, one-tailed, bolded for emphasis.

Panel B. Goodness of fit statistics for theoretical, theoretical with distributive dominance, expectancy, and expectancy with distributive dominance models (n = 118)

	df	χ^2	SRMR ⁷	RMSEA ⁷	CFI ⁷	AIC ⁷	BIC ⁷
Theoretical	11	55.46	0.086	0.185	0.623	5756.23	5800.57
Expectancy	9	29.19	0.055	0.138	0.829	5733.96	5783.84

Panel C. Difference in chi square test comparing fit of nested models

	Δdf	$\Delta \chi^2$	<i>p-value</i>
Theoretical – Expectancy	2	26.27	<0.001

Table 11. Mediation analyses (continued)

Notes:

¹ In this analysis, I compare the fit of two models: 1) the theoretical model, and 2) an alternative expectancy based model. These models are depicted in Figure 1 Panel B and Figure 5 respectively. The theoretical model describes the model implied in my hypotheses development. The expectancy based model includes an additional two paths, one from *DJC* to *ExpectancyC*, and one from *ExpectancyC* to *Prod4*.

² For variable definition, please see notes to Table 4.

³ For variable definition, please see notes to Table 5.

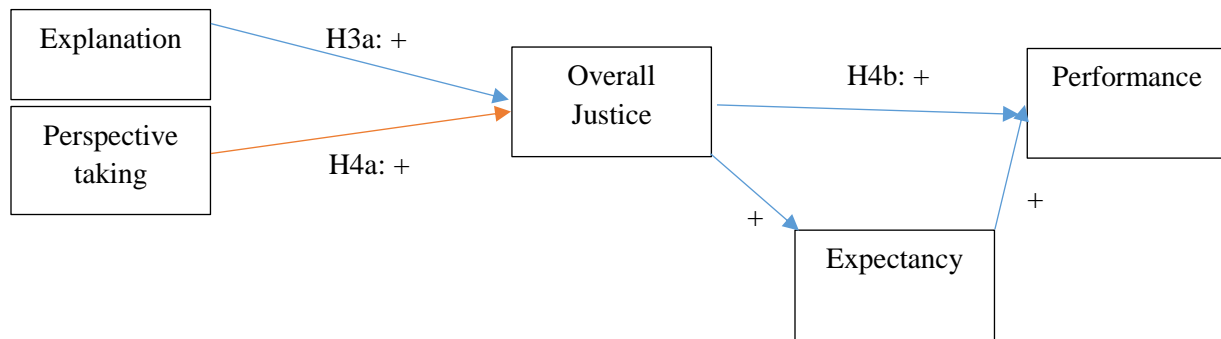
⁴ *Expectancy** represents participants views on the likelihood that they will achieve their goals, measured as a percentage at one of three timepoints, A, B, or C. For example, *ExpectancyC* represents the mean perceived likelihood of goal attainment for participants as measured at timepoint C.

⁵ For variable definition, please see notes to Table 9.

⁶ For variable definition, please see notes to Table 8.

⁷ For variable definition, please see notes to Table 3.

Figure 5: Alternative expectancy based model



Notes:

¹ Explanation refers to justifications provided by management for observed non-helping behaviour.

² Perspective taking refers to employees simulating the internal state (thoughts, intentions, feelings) of management.

³ Overall justice refers to perceptions of overall justice measured post- observed non-helping behaviour.

⁴ Expectancy refers to employees' perceptions of the strength of the effort-performance relationship.

⁵ Performance refers to the number of tables counted on the counting task in round 4 of the production rounds.

⁶ The expectancy based model is identical to the theoretical model presented in Figure 1, Panel B, with the exception of two additional paths, one from overall justice to expectancy, and one from expectancy to performance, reflecting the assertion that the effect of distributive justice perceptions on performance is mediated by employees' perceptions of the strength of the effort-performance relationship.

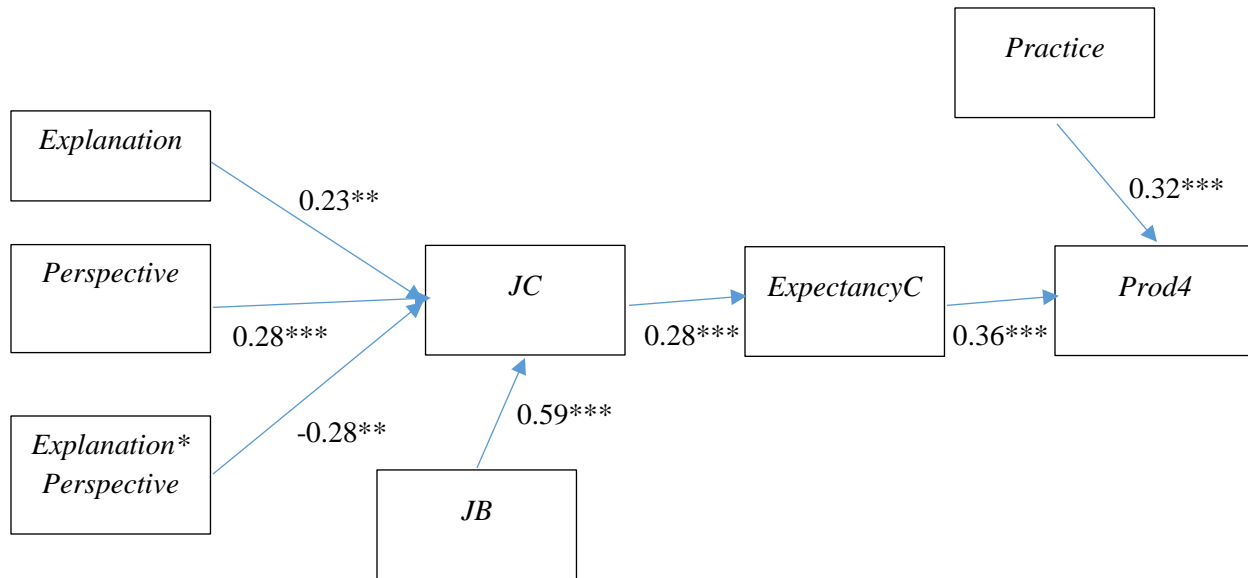
Further, because my models are nested within each other, I am able to quantitatively compare fit between my competing models using the difference in chi-square test (Kline, 1998; Burney et al., 2009) Results of this test (Table 11, Panel C) show that the expectancy based model fits the data significantly better than my theoretical model ($p < 0.01$, two-tailed).⁸³ Examination of goodness of fit statistics for the expectancy based model show adequate model fit, with SMRM indicating good model fit (SRMR = 0.055), RMSEA indicating mediocre model fit (RMSEA = 0.138), and CFI indicating adequate model fit (CFI = 0.829) (Hu and Bentler, 1999). I conclude that the expectancy based model fits my data best, and thus use this model for the remainder of my mediation analyses.

Standardized path coefficients for the expectancy based model are shown in Table 11, Panel A, with significant paths represented in Figure 6. Examination of the path coefficients show that in this model, the only significant direct effects of my manipulations are on *JC*, and the only significant direct effects on *Prod4* are from *ExpectancyC* and *Practice*. Further analysis shows that there are marginally significant positive indirect effects of *Perspective* ($p = 0.05$, one-tailed) and *Explanation* ($p = 0.07$, one-tailed) on *Prod4* through *JC* and *ExpectancyC*, and a negative but insignificant indirect of the *Perspective* by *Explanation* interaction on *Prod4* through *JC* and *ExpectancyC* ($p = 0.14$, two-tailed) (Table 11, Panel A). These results suggest that the effects of *Perspective* and *Explanation* on *Prod4* are mediated through *JC* and *ExpectancyC*.⁸⁴ In other words, both perspective taking training and providing an explanation

⁸³ The chi-square statistic represents the degree to which observations in a sample distribution deviate from some other referent distribution, and due to its derivation, is always a positive number (Pearson 1900). As a result, chi-square goodness of fit tests are inherently one-sided tests, since we are concerned with the right tail of the distribution (i.e., deviation from referent distribution is too large), and not with the left tail (i.e. deviation from referent distribution is too small). Accordingly, I report one-tailed p values for all chi-square goodness of fit tests.

⁸⁴ Following Preacher and Hayes (2004), I interpret significant indirect effects as illustrative of mediation.

Figure 6: Expectancy based model with significant paths shown



Notes:

Only significant paths are shown above, standardized path coefficients shown. Paths significant at the 0.05 level, two-tailed, are noted with **, paths significant at the 0.01 level, two-tailed, are noted with ***.

¹ *Explanation* is a manipulated variable taking on a value of one if participants receive an explanation for observed non-helping behaviour and zero otherwise.

² *Perspective taking* is a manipulated variable taking on a value of one if participants have undergone perspective taking training, and zero otherwise.

³ *JB* and *JC* refer to perceptions of overall justice measured pre-/post- observed non-helping behaviour respectively.

⁴ *ExpectancyC* refers to employees' perceived likelihood of attaining their goal, as measured post-observed non-helping behaviour.

⁵ *Practice* refers to the number of tables counted on the counting task in the eight-minute practice round.

⁶ *Prod4* refers to the number of tables counted on the counting task in round 4 of the production rounds.

appear to improve overall justice perceptions, which in turn sustain the effort-performance relationship, improving performance.

In summary, path analyses indicate mediocre fit between my data and my theoretical mediation model (Figure 1, Panel B). My alternative expectancy based model fits the data significantly better, and I observe that both perspective taking and explanation have significantly positive indirect effects on performance through overall justice perceptions and expectancy.

Taken together with the results of my formal hypotheses tests, these results suggest that the

positive impact of perspective taking on performance observed in my test of H4b is mediated by overall justice perceptions and expectancy.

5.10 Analysis of Effort Duration and Intensity

As discussed in subsection 4.4.3, my multi-task environment allows me to collect data on two dimensions of participant effort, effort duration (*Duration*) and effort intensity (*Intensity*). This allows me to assess which dimensions of effort are affected by my experimental manipulations and their impact on the performance effects observed in my formal tests of hypotheses. Descriptive statistics for *Duration* and *Intensity* by round and condition can be found in Table 12, Panel B and Panel C respectively. Examination of *Duration* shows that the mean *Duration* in round 1 is 221.8 seconds out of a maximum of 240 seconds per round. In the first difficult round, round 2, *Duration* drops to 176.3 seconds, dropping further to 145.6 seconds in round 3, before increasing to 171.1 seconds in round 4. *Intensity* appears to follow a similar pattern, with round 1 intensity being the highest at 0.105 tables per second, dropping to 0.060 and 0.056 in round 2 and round 3 respectively, before increasing to 0.089 in round 4.⁸⁵

In my tests of H1, I observe a significantly positive announcement effect on round 2 performance (Table 6, Panel D). With respect to the announcement effect on effort, I conjecture that it would likely operate through effort duration, i.e., participants aware of the possibility of an adjustment may continue to provide effort in the face of a difficult round in anticipation of a goal adjustment.

⁸⁵ Recall that in difficult rounds, the ratio of '1's to '0's in the counting task is 60% compared to 40% in normal rounds, making the counting task more difficult. Therefore, effort intensity is expected to be lower in these rounds. In my regression analyses, I control for the difference in difficulty between rounds with the dummy variable *Difficult*.

Table 12. Descriptive statistics for alternate dependent variables**Panel A. Mean (standard deviation) of goal attainment in production rounds 1-4 by condition**

Condition Label¹	<i>n</i>	Attainment1²	Attainment2²	Attainment3²	Attainment4²
Control	29	0.69 (0.47)	0 (0)	0 (0)	0.45 (0.51)
No Intervention	29	0.55 (0.51)	0 (0)	0 (0)	0.48 (0.51)
Explanation Only	30	0.63 (0.49)	0.03 (0.18)	0 (0)	0.53 (0.51)
Perspective Only	30	0.60 (0.50)	0.03 (0.18)	0.03 (0.18)	0.73 (0.45)
Both Interventions	29	0.48 (0.51)	0 (0)	0 (0)	0.55 (0.51)
Mean		0.59 (0.49)	0.01 (0.12)	0.01 (0.08)	0.55 (0.50)

Panel B. Mean (standard deviation) of effort duration in production rounds 1-4 by condition

Condition Label	<i>n</i>	Duration1³	Duration2³	Duration3³	Duration4³
Control	29	223.3 (44.7)	155.8 (85.3)	154.0 (82.9)	181.6 (85.8)
No Intervention	29	233.3 (11.7)	176.8 (88.0)	137.4 (96.7)	153.3 (105.8)
Explanation Only	30	211.9 (49.5)	176.3 (77.7)	157.6 (81.9)	146.9 (100.1)
Perspective Only	30	222.7 (29.5)	182.1 (79.1)	126.0 (93.3)	185.7 (86.2)
Both Interventions	29	218.1 (54.3)	190.3 (73.7)	153.1 (97.6)	191.7 (84.9)
Mean		221.8 (41.0)	176.3 (80.6)	145.6 (90.3)	171.1 (93.7)

Table 12. Descriptive statistics for alternate dependent variables (continued)**Panel C. Mean (standard deviation) of effort intensity in production rounds 1-4 by condition**

Condition Label	<i>n</i>	Intensity1 ⁴	Intensity2 ⁴	Intensity3 ⁴	Intensity4 ⁴
Control	29	0.10 (0.03)	0.06 (0.03)	0.06 (0.02)	0.08 (0.05)
No Intervention	29	0.11 (0.03)	0.06 (0.03)	0.06 (0.03)	0.08 (0.06)
Explanation Only	30	0.10 (0.04)	0.06 (0.03)	0.06 (0.03)	0.08 (0.06)
Perspective Only	30	0.12 (0.02)	0.07 (0.03)	0.06 (0.03)	0.10 (0.05)
Both Interventions	29	0.10 (0.03)	0.06 (0.02)	0.05 (0.03)	0.09 (0.04)
Mean		0.11 (0.03)	0.06 (0.03)	0.06 (0.03)	0.09 (0.05)

Notes:

¹ For variable definitions, please refer to Table 1.

² *Attainment** represents the mean proportion of participants whose performance on the counting task in round * exceeded their individually set goals. For example, *Attainment1* represents the goal attainment rate in round 1.

³ *Duration** represents the mean amount of time in seconds spent by participants on the counting task in round *. For example, *Duration1* represents the amount of time in seconds spent by participants on the counting task in round 1.

⁴ *Intensity** represents the mean tables counted per second for participants on the counting task in round *, calculated as *Prod** scaled by *Duration**. For example, *Intensity1* represents the mean tables counted per second for participants in round 1, calculated as *Prod1* divided by *Duration1*.

Analysis of *Duration* in round 2 (Table 13, Panel A) shows a positive and marginally significant announcement effect on round 2 *Duration* ($p = 0.08$, two-tailed). Analysis of *Intensity* in round 2 (Table 13, Panel B) show no announcement effect on round 2 *Intensity* ($p = 0.74$, two-tailed). Taken together, these results suggest that the announcement of an ex-post adjustment policy improves participant persistence on task in the face of difficult periods.

In my tests of H2, I observe that for Control and No Intervention participants, performance significantly decreases after observing non-helping behaviour, but that this effect is only directionally stronger for *No Intervention* participants (Table 7, Panel C). Analyses of

Table 13. Tests of Hypothesis 1 using Effort Duration and Effort Intensity as dependent variable

Panel A. Effect of announcement on effort duration (n=147)

Model: $Duration2_i^1 = \beta_0 + \beta_1 Adjustment^2 + \beta_2 Practice^3$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	242.56	9.17	<0.001
<i>Adjustment</i>	29.02	1.77	0.079
<i>Practice</i>	-1.82	-2.41	0.017
Adjusted R ²	8.51%		

Panel B. Effect of announcement on effort intensity (n=147)

Model: $Intensity2_i^1 = \beta_0 + \beta_1 Adjustment + \beta_2 Practice$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	0.03	2.80	0.006
<i>Adjustment</i>	0	0.34	0.736
<i>Practice</i>	0	11.48	<0.001
Adjusted R ²	8.51%		

Notes:

¹ For variable definition, please see notes to Table 12.

² For variable definition, please see notes to Table 6.

³ For variable definition, please see notes to Table 4.

Duration (Table 14, Panel A) show that after observing non-helping behaviour, *Adjustment* has a directionally negative effect on *Duration* ($p = 0.16$, two-tailed), and that this effect does not depend on round difficulty ($p = 0.98$, two-tailed).⁸⁶ Similarly, analyses of *Intensity* (Table 14, Panel B) show a directionally negative effect of *Adjustment* on *Intensity* in the post-period ($p = 0.16$, two-tailed), and that this effect does not differ by round difficulty ($p = 0.74$, two-tailed).

⁸⁶ I report two-tailed tests of significance since I do not make any formal predictions with respect to the effects of my experimental manipulations on effort duration, effort intensity, or goal attainment.

Table 14. Tests of Hypothesis 2 using Effort Duration and Effort Intensity as dependent variable

Panel A. Effect of observed non-helping behaviour and round difficulty on effort duration for No Intervention participants, with participant fixed effects (n=232)

$$\text{Model: } Duration_{it}^1 = \beta_0 + \beta_1 Post^2 + \beta_2 Difficult^2 + \beta_3 Post * Adjustment_i^3 + \beta_4 Post * Difficult + \beta_5 Adjustment_i * Difficult + \beta_6 Post * Adjustment_i * Difficult + a_i^2 + u_{it}^2$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	228.32	23.74	<0.001
<i>Post</i>	-80.06	-4.16	<0.001
<i>Difficult</i>	-56.53	-2.94	0.004
<i>Post*Adjustment_i</i>	-38.34	-1.41	0.161
<i>Post*Difficult</i>	40.69	1.50	0.137
<i>Adjustment_i*Difficult</i>	10.91	0.40	0.689
<i>Post*Adjustment_i*Difficult</i>	0.83	0.09	0.983
Adjusted R ²	14.08%		

Panel B. Effect of observed non-helping behaviour and round difficulty on effort intensity for No Intervention participants, with participant fixed effects (n=232)

$$\text{Model: } Intensity_{it}^1 = \beta_0 + \beta_1 Post + \beta_2 Difficult + \beta_3 Post * Adjustment_i + \beta_4 Post * Difficult + \beta_5 Adjustment_i * Difficult + \beta_6 Post * Adjustment_i * Difficult + a_i + u_{it}$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	0.11	28.10	<0.001
<i>Post</i>	-0.03	-4.32	<0.001
<i>Difficult</i>	-0.05	-6.28	<0.001
<i>Post*Adjustment_i</i>	-0.02	-1.42	0.157
<i>Post*Difficult</i>	0.03	2.39	0.018
<i>Adjustment_i*Difficult</i>	>-0.01	-0.10	0.923
<i>Post*Adjustment_i*Difficult</i>	<0.01	0.33	0.742
Adjusted R ²	22.61%		

Notes:

¹ For variable definition, please see notes to Table 12.

² For variable definition, please see notes to Table 7.

³ For variable definition, please see notes to Table 6.

These results suggest that the directionally negative performance effects of observed non-helping behaviour are attributable to changes in both *Duration* and *Intensity*.

In my tests of H3, I fail to find an effect of *ExplanationOnly* on *Prod* in the post-period (Table 8, Panel C). Since I do not find performance effects of *ExplanationOnly* on *Prod*, I do not expect *ExplanationOnly* to significantly impact *Intensity* or *Duration*. Analyses of *Duration* (Table 15, Panel A) show no effect of *ExplanationOnly* on *Duration* ($p = 0.66$, two-tailed). Similarly, analyses of *Intensity* (Table 15, Panel B) show no effect of *ExplanationOnly* on *Intensity* ($p = 0.43$, two-tailed).

In my tests of H4, I find that *PerspectiveOnly* significantly improves *Prod* in the post-period, but only in normal rounds (Table 9, Panel C). Analysis of *Duration* and *Intensity* for No Intervention and Perspective Only participants (Table 16, Panel A and Panel B) indicate that *PerspectiveOnly* directionally improves *Duration* in the post-period ($p = 0.13$, two-tailed) and has a marginally significant positive impact on *Intensity* in the post-period ($p = 0.06$, two-tailed). Taken together, these results suggest that the positive performance effects of *PerspectiveOnly* on *Prod* are driven by increases in *Intensity*.

In the analysis of RQ1, I examine the interactive effects of *Perspective* and *Explanation* on performance, finding, no interactive effect of *Perspective* and *Explanation* on *Prod* (Table 10 Panel G). Therefore, I do not expect to observe an interactive effect of *Perspective* and *Explanation* on *Prod*. Analysis of *Duration* and *Intensity* (Table 17, Panel A and Panel B) confirm this expectation, showing no interactive effect of *Perspective* and *Explanation* on either *Duration* ($p = 0.95$, two-tailed), or *Intensity* ($p = 0.91$, two-tailed).

Table 15. Tests of Hypothesis 3 using Effort Duration and Effort Intensity as dependent variable

Panel A. Effect of explanation on duration for participants in the No Intervention and Explanation Only conditions (n=236)

$$\text{Model: } Duration_{it}^1 = \beta_0 + \beta_1 Post^2 + \beta_2 Difficult^2 + \beta_3 Post * ExplanationOnly_i^3 + \beta_4 Post * Difficult + \beta_5 ExplanationOnly_i * Difficult + \beta_6 Post * ExplanationOnly_i * Difficult + a_i^2 + u_{it}^2$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	222.45	23.19	<0.001
<i>Post</i>	-80.06	-4.14	<0.001
<i>Difficult</i>	-56.53	-2.92	0.004
<i>Post*ExplanationOnly_i</i>	12.02	0.44	0.658
<i>Post*Difficult</i>	40.69	1.49	0.139
<i>ExplanationOnly_i*Difficult</i>	20.89	0.77	0.443
<i>Post*ExplanationOnly_i*Difficult</i>	8.64	0.23	0.822
Adjusted R ²	11.78%		

Panel B. Effect of explanation on intensity for participants in the No Intervention and Explanation Only conditions (n=236)

$$\text{Model: } Duration_{it}^1 = \beta_0 + \beta_1 Post + \beta_2 Difficult^1 + \beta_3 Post * ExplanationOnly_i + \beta_4 Post * Difficult + \beta_5 ExplanationOnly_i * Difficult + \beta_6 Post * ExplanationOnly_i * Difficult + a_i + u_{it}$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	0.11	27.57	<0.001
<i>Post</i>	-0.03	-4.16	<0.001
<i>Difficult</i>	-0.05	-6.04	<0.001
<i>Post*ExplanationOnly_i</i>	0.01	0.79	0.428
<i>Post*Difficult</i>	0.03	2.30	0.022
<i>ExplanationOnly_i*Difficult</i>	<0.01	0.35	0.724
<i>Post*ExplanationOnly_i*Difficult</i>	-0.01	-0.41	0.680
Adjusted R ²	21.09%		

Notes:

¹ For variable definition, please see notes to Table 12.

² For variable definition, please see notes to Table 7.

³ For variable definition, please see notes to Table 8.

Table 16. Tests of Hypothesis 4 using Effort Duration and Effort Intensity as dependent variable

Panel A. Effect of perspective taking on duration for participants in the No Intervention and Perspective Only conditions (n=236)

$$\text{Model: } Duration_{it}^1 = \beta_0 + \beta_1 Post^2 + \beta_2 Difficult^2 + \beta_3 Post * PerspectiveOnly_i^3 + \beta_4 Post * Difficult + \beta_5 PerspectiveOnly_i * Difficult + \beta_6 Post * PerspectiveOnly_i * Difficult + a_i^2 + u_{it}^2$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	227.92	22.78	<0.001
<i>Post</i>	-80.06	-3.97	<0.001
<i>Difficult</i>	-56.53	-2.80	0.006
<i>Post*PerspectiveOnly_i</i>	43.09	1.52	0.130
<i>Post*Difficult</i>	40.69	1.43	0.156
<i>PerspectiveOnly_i*Difficult</i>	15.97	0.56	0.573
<i>Post*PerspectiveOnly_i*Difficult</i>	-59.81	-1.49	0.137
Adjusted R ²	16.45%		

Panel B. Effect of perspective taking on intensity for participants in the No Intervention and Perspective Only conditions (n=236)

$$\text{Model: } Intensity_{it}^1 = \beta_0 + \beta_1 Post + \beta_2 Difficult^1 + \beta_3 Post * PerspectiveOnly_i + \beta_4 Post * Difficult + \beta_5 PerspectiveOnly_i * Difficult + \beta_6 Post * PerspectiveOnly_i * Difficult + a_i + u_{it}$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	0.11	29.56	<0.001
<i>Post</i>	-0.03	-4.20	<0.001
<i>Difficult</i>	-0.05	-6.11	<0.001
<i>Post*PerspectiveOnly_i</i>	0.02	1.89	0.060
<i>Post*Difficult</i>	0.03	2.33	0.021
<i>PerspectiveOnly_i*Difficult</i>	>-0.01	-0.35	0.730
<i>Post*PerspectiveOnly_i*Difficult</i>	-0.02	-1.47	0.143
Adjusted R ²	28.43%		

Notes:

¹ For variable definition, please see notes to Table 12.

² For variable definition, please see notes to Table 7.

³ For variable definition, please see notes to Table 9.

Table 17. Analysis of Research Question using Effort Duration and Effort Intensity as dependent variable

Panel A. Effect of explanation and perspective taking on duration post-observation of non-helping behaviour for Non-Control participants, with participant fixed effects (n=472)

$$\text{Model: } Duration_{it}^1 = \beta_0 + \beta_1 Post^2 + \beta_2 Difficult^2 + \beta_3 Post * Explanation_i^3 + \beta_4 Post * Perspective_i^3 + \beta_5 Post * Difficult + \beta_6 Difficult * Explanation_i + \beta_7 Difficult * Perspective_i + \beta_8 Post * Explanation_i * Difficult + \beta_9 Post * Perspective_i * Difficult + \beta_{10} Difficult * Explanation_i * Perspective_i + \beta_{11} Post * Explanation_i * Perspective_i + a_i^2 + u_{it}^2$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	221.44	33.43	<0.001
<i>Post</i>	-80.09	-4.54	<0.001
<i>Difficult</i>	-56.57	-3.20	0.001
<i>Post*Explanation_i</i>	12.09	0.53	0.599
<i>Post*Perspective_i</i>	43.16	1.88	0.061
<i>Post*Difficult</i>	40.76	1.77	0.078
<i>Difficult*Explanation_i</i>	20.96	0.91	0.362
<i>Difficult*Perspective_i</i>	16.04	0.70	0.485
<i>Post*Explanation_i*Difficult</i>	8.50	0.32	0.749
<i>Post*Perspective_i*Difficult</i>	-59.95	-2.26	0.024
<i>Difficult*Explanation_i*Perspective_i</i>	-8.30	-0.31	0.754
<i>Post*Explanation_i*Perspective_i</i>	-1.60	-0.06	0.952
Adjusted R ²	12.70%		

Table 17. Analysis of Research Question using Effort Duration and Effort Intensity as dependent variable (continued)

Panel B. Effect of explanation and perspective taking on intensity post-observation of non-helping behaviour for Non-Control participants, with participant fixed effects (n=472)

$$\text{Model: } Intensity_{it}^1 = \beta_0 + \beta_1 Post + \beta_2 Difficult + \beta_3 Post * Explanation_i + \beta_4 Post * Perspective_i + \beta_5 Post * Difficult + \beta_6 Difficult * Explanation_i + \beta_7 Difficult * Perspective_i + \beta_8 Post * Explanation_i * Difficult + \beta_9 Post * Perspective_i * Difficult + \beta_{10} Difficult * Explanation_i * Perspective_i + \beta_{11} Post * Explanation_i * Perspective_i + a_i + u_{it}$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	0.11	41.10	<0.001
<i>Post</i>	-0.03	-4.65	<0.001
<i>Difficult</i>	-0.05	-6.78	<0.001
<i>Post*Explanation_i</i>	0.01	0.89	0.374
<i>Post*Perspective_i</i>	0.02	2.21	0.028
<i>Post*Difficult</i>	0.02	2.74	0.007
<i>Difficult*Explanation_i</i>	<0.01	0.35	0.724
<i>Difficult*Perspective_i</i>	>-0.01	-0.49	0.621
<i>Post*Explanation_i*Difficult</i>	>-0.01	-0.48	0.631
<i>Post*Perspective_i*Difficult</i>	-0.02	-2.04	0.042
<i>Difficult*Explanation_i*Perspective_i</i>	0.01	1.02	0.311
<i>Post*Explanation_i*Perspective_i</i>	<0.01	0.11	0.913
Adjusted R ²	23.74%		

Notes:

¹ For variable definition, please see notes to Table 12.

² For variable definition, please see notes to Table 7.

³ For variable definition, please see notes to Table 10.

In summary, analyses of *Duration* and *Intensity* largely mirror the performance effects observed in my formal tests of hypotheses. However, a pattern emerges where performance effects in difficult rounds, as in the case of H1, appear to be attributable to *Duration*, whereby participants abandon their goals in favour of guaranteed pay in the decoding task. In contrast, performance effects in normal rounds, where goals are reasonably attainable, as in the case of

H4, are driven primarily by increases in *Intensity*, representing participants engaging in active goal pursuit.

5.11 Analysis of Goal Attainment

In my formal tests of hypotheses, I examine the performance effects of *Adjustment*, non-helping behaviour, *PerspectiveOnly*, and *ExplanationOnly*. In this subsection, I examine how these manipulations affect participant goal attainment (*Attainment*). Descriptive statistics for goal attainment can be found in Table 12, Panel A. Examination of goal attainment reveals that the mean goal attainment rate in round 1 (*Attainment1*) is 59.2%. Goal attainment drops sharply in the two difficult rounds, rounds 2 and 3 (*Attainment2* and *Attainment3*), to 1.4% and 0.7% respectively. In round 4, *Attainment4* increases to 55.1%. Examination of *Attainment1* shows that goal attainment is highest in the Control condition (69.0%), and lowest in the Both Interventions condition (48.3%). In round 4, *Attainment4* is highest in the Perspective Only condition (73.3%), and lowest in the Control condition (44.8%).

Since goal attainment in difficult rounds is low and does not vary significantly by condition, I restrict my analyses of goal attainment to normal rounds. Recall that in my tests of H1, controlling for *Practice*, I find no effect of *Adjustment* on *Prod1*. Therefore, I do not expect any effects of *Adjustment* on *Attainment1*. Examination of *Attainment1* (Table 18, Panel A) show no effect of *Adjustment* on *Attainment1* ($p = 0.23$, two-tailed). Therefore, as expected based on the results of my formal tests, I find no effect of *Adjustment* on *Attainment1*.

In my tests of H2, I find that performance for No Intervention participants decreases post-observation of non-helping behaviour, but that this effect is only directionally stronger relative to Control participants. Based on this, I do not expect *Attainment* to be significantly impacted by

Table 18. Analysis of Goal Attainment**Panel A. Effect of announcement on goal attainment in round 1 (n=147)**

$$\text{Model: } \textit{Attainment}_{i1}^1 = \beta_0 + \beta_1 \textit{Adjustment}^2 + \beta_2 \textit{Practice}^3$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	0.53	3.17	0.002
<i>Adjustment</i>	-0.12	-1.21	0.229
<i>Practice</i>	<0.01	0.26	0.792
Adjusted R ²	-0.35%		

Panel B. Effect of observed non-helping behaviour on goal attainment in normal rounds for No Intervention participants, with participant fixed effects (n=116)

$$\text{Model: } \textit{Attainment}_{it} = \beta_0 + \beta_1 \textit{Post}^4 + \beta_3 \textit{Post} * \textit{Adjustment}_i + a_i^4 + u_{it}^4$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	0.62	11.43	<0.001
<i>Post</i>	-0.07	-0.63	0.528
<i>Post*Adjustment_i</i>	0.17	1.12	0.267
Adjusted R ²	1.89%		

Panel C. Effect of explanation on goal attainment in normal rounds for participants in the No Intervention and Explanation Only conditions (n=118)

$$\text{Model: } \textit{Attainment}_{it} = \beta_0 + \beta_1 \textit{Post} + \beta_2 \textit{Post} * \textit{ExplanationOnly}_i^5 + a_i + u_{it}$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	0.59	11.29	<0.001
<i>Post</i>	-0.07	-0.65	0.518
<i>Post*ExplanationOnly_i</i>	-0.03	-0.21	0.835
Adjusted R ²	0.54%		

Table 18. Analysis of Goal Attainment (continued)**Panel D. Effect of perspective taking on goal attainment in normal rounds for participants in the No Intervention and Perspective Only conditions (n=118)**

$$\text{Model: } \textit{Attainment}_{it} = \beta_0 + \beta_1 \textit{Post} + \beta_2 \textit{Post} * \textit{PerspectiveOnly}_i^6 + a_i + u_{it}$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	0.58	11.35	<0.001
<i>Post</i>	-0.07	-0.67	0.503
<i>Post*PerspectiveOnly_i</i>	0.20	1.41	0.164
Adjusted R ²	3.36%		

Panel E. Effect of explanation and perspective taking on goal attainment in normal rounds for Non-Control participants (n=236)

$$\text{Model: } \textit{Attainment}_{it} = \beta_0 + \beta_1 \textit{Post} + \beta_2 \textit{Post} * \textit{Explanation}_i^7 + \beta_3 \textit{Post} * \textit{Perspective}_i^7 + \beta_4 \textit{Post} * \textit{Explanation}_i * \textit{Perspective}_i + a_i + u_{it}$$

	Coefficient	t-statistic	p-value (two-tailed)
Constant	0.57	15.13	<0.001
<i>Post</i>	-0.07	-0.64	0.521
<i>Post*Explanation_i</i>	-0.03	-0.21	0.837
<i>Post*Perspective_i</i>	0.20	1.35	0.181
<i>Post*Explanation_i*Perspective_i</i>	-0.03	-0.16	0.876
Adjusted R ²	1.27%		

Notes:

¹ For variable definition, please see notes to Table 12.

² For variable definition, please see notes to Table 6.

³ For variable definition, please see notes to Table 4.

⁴ For variable definition, please see notes to Table 7.

⁵ For variable definition, please see notes to Table 8.

⁶ For variable definition, please see notes to Table 9.

⁷ For variable definition, please see notes to Table 10.

Adjustment. Analysis of the effect of *Adjustment* on *Attainment* in the post-period (Table 18,

Panel B) shows no marginal effect of *Adjustment* on *Attainment* ($p = 0.27$, two-tailed),

confirming my expectations.

In my tests of H3, I find no effect of *ExplanationOnly* on *Prod*, and so do not expect an effect of *ExplanationOnly* on *Attainment*. Analysis of *Attainment* (Table 18, Panel C) corroborate this expectation ($p = 0.84$, two-tailed). In my tests of H4, I find a positive effect of *PerspectiveOnly* on *Prod*, and so expect a similar pattern of results for *Attainment*. Results of this analysis (Table 18, Panel D) show that *PerspectiveOnly* directionally improves *Attainment* in the post-period ($p = 0.16$, two-tailed). In my examination of RQ1, I find no interactive effect of *Perspective* and *Explanation* on *Prod*. Repeating these analyses on *Attainment* (Table 18, Panel E) similarly show no evidence of an interactive effect of *Perspective* and *Explanation* on *Attainment* in the post-period ($p = 0.88$, two-tailed). In summary, my analyses of goal attainment reveal results largely consistent with but weaker than the findings from my formal tests of hypotheses.

5.12 Summary of Results

A summary of my main findings is presented in Table 19. In summary, my results do not provide support for my hypothesis that the announcement of ex-post adjustments would result in more positive justice perceptions (H1a). However, I find evidence of a positive announcement effect on performance in difficult rounds (H1b), suggesting that the announcement of an ex-post adjustment policy can sustain effort in the face of negative uncontrollable events. This is somewhat corroborated by analyses on effort duration and intensity suggesting that the positive announcement effect on round 2 performance is driven by effort duration rather than intensity. Further, I find that overall justice perceptions and performance significantly decrease in the post-period. However, I fail to find support for my assertion that these effects are driven by observing non-helping behaviour (H2a and H2b). With respect to my fairness interventions, I find evidence that although providing an explanation improves overall justice perceptions (H3a), explanation

Table 19. Summary of results

Test	Effect Tested	Predicted Sign	Actual Sign ¹	Prediction Supported? ²	
Formal Tests	H1a	<i>Adjustment</i> ³ → <i>JB</i> ⁴	+	n/a	No
	H1b	<i>Adjustment</i> → <i>Prod1</i> ⁵	+	n/a	No
		<i>Adjustment</i> → <i>Prod2</i>⁵	+	+	Yes
	H2a	Non-helping behaviour ⁶ → <i>J</i> ⁷	-	n/a	No ¹³
	H2b	Non-helping behaviour → <i>Prod</i> ⁸	-	n/a	No
	H3a	<i>ExplanationOnly</i>⁹ → <i>J</i>	+	+	Yes
	H3b	<i>ExplanationOnly</i> → <i>Prod</i>	+	n/a	No
	H4a	<i>PerspectiveOnly</i>¹⁰ → <i>J</i>	+	+	Yes
	H4b	<i>PerspectiveOnly</i> → <i>Prod</i>	+	+	Yes
	RQ1a	<i>Explanation</i> ¹¹ * <i>Perspective</i> ¹¹ → <i>J</i>	n/a	-	n/a
	RQ1b	<i>Explanation</i> * <i>Perspective</i> → <i>Prod</i>	n/a	n/a	n/a
	Mediation Analyses	Indirect: <i>Explanation</i> → <i>Prod4</i>	+	+	Yes
Indirect: <i>Perspective</i> → <i>Prod4</i>		+	+	Yes	
	Indirect: <i>Explanation</i> * <i>Perspective</i> → <i>Prod4</i>	n/a	n/a	n/a	
Supplemental Analyses of Duration, Intensity, and Attainment		<i>Adjustment</i> → <i>Duration2</i> ¹²		+	
		<i>Adjustment</i> → <i>Intensity2</i> ¹²	n/a	n/a	n/a
		<i>Adjustment</i> → <i>Attainment1</i> ¹²		n/a	
		Non-helping behaviour → <i>Duration</i> ¹²		n/a	
		Non-helping behaviour → <i>Intensity</i> ¹²	n/a	n/a	n/a
		Non-helping behaviour → <i>Attainment</i> ¹²		n/a	
		<i>ExplanationOnly</i> → <i>Duration</i>		n/a	
		<i>ExplanationOnly</i> → <i>Intensity</i>	n/a	n/a	n/a
		<i>ExplanationOnly</i> → <i>Attainment</i>		n/a	
		<i>PerspectiveOnly</i> → <i>Duration</i>		n/a	
		<i>PerspectiveOnly</i> → <i>Intensity</i>	n/a	+	n/a
		<i>PerspectiveOnly</i> → <i>Attainment</i>		n/a	
	<i>Explanation</i> * <i>Perspective</i> → <i>Duration</i>		n/a		
	<i>Explanation</i> * <i>Perspective</i> → <i>Intensity</i>	n/a	n/a	n/a	
	<i>Explanation</i> * <i>Perspective</i> → <i>Attainment</i>		n/a		

Supported predictions bolded for emphasis.

Table 19. Summary of Results (continued)

Notes:

¹ Actual Sign refers to the sign of the effect observed. Only signs that are marginally significant or better are labelled with a sign.

² Prediction Supported refers to whether the Actual Sign matches the Predicted Sign.

³ For variable definition, please see notes to Table 6.

⁴ For variable definition, please see notes to Table 4.

⁵ For variable definition, please see notes to Table 5.

⁶ Non-helping behaviour refers to participants not receiving ex-post goal adjustments to compensate for increased difficulty in some rounds, despite being made aware of the availability of such adjustments.

⁷ For variable definition, please see notes to Table 7.

⁸ For variable definition, please see notes to Table 7.

⁹ For variable definition, please see notes to Table 8.

¹⁰ For variable definition, please see notes to Table 9.

¹¹ For variable definition, please see notes to Table 10.

¹² For variable definition, please see notes to Table 12.

¹³ With respect to the effect of non-helping behaviour on J , I observe that J is lower after observing non-helping behaviour, but because this effect is not significantly different from that in the Control condition, I cannot attribute it to non-helping behaviour.

has no effect on subsequent performance (H3b). I also find evidence that perspective taking

training improves overall justice perceptions and subsequent performance (H4a and H4b).

Analyses on the interactive effects of my interventions show a significant negative interaction

between explanation and perspective taking on justice perceptions (RQ1a), but not on

performance (RQ1b). Analyses of the effects of explanation and perspective taking on justice

perceptions conditional on receiving the other intervention suggest that these two interventions

are substitutes with respect to overall justice perceptions.

To gain a better understanding of the mechanism underlying the above results, I tested a

mediation model based on my theoretical framework, and compared it against an alternative

theory consistent expectancy based model, finding support for this alternative model of

performance. I further find that the indirect effect of my manipulations on performance are

mediated by both overall justice perceptions and expectancy of goal attainment. This suggests

that fairness perceptions influence the perceived strength of the effort-performance relationship,

which in turn affects performance.

Taken together, my results suggest that both the announcement and enactment of ex-post adjustment policies influence subsequent performance, though only the latter significantly impacts justice perceptions. Further, although both explanation and perspective taking show promise in improving justice perceptions after observing non-helping behaviour, only perspective taking is effective as an intervention in improving subsequent performance. This suggests that perspective taking in the form of a training exercise can be an effective fairness intervention to improve justice perceptions and performance after employees have been negatively impacted by management decision making.

CHAPTER 6: CONCLUSION

6.1 Introduction

Employee performance can be negatively impacted by the occurrence of uncontrollable negative events, potentially reducing the motivating power of performance contingent rewards. In order to restore the motivational strength of performance contingent incentive contracts, management may implement an ex-post goal adjustment policy to filter out the effects of uncontrollable negative events on employees' objectively measured performance. However, if management decides to seldomly exercise ex-post goal adjustments, employees who fail to meet their goals may perceive unfairness and respond by withholding effort. This perceived unfairness about observed non-helping behavior may result in part from employees' inability to take management's perspective, due to egocentric bias. Therefore, if management helps employees to understand their motivations, through either provision of an explanation or through perspective taking training, perhaps negative fairness and performance effects can be attenuated. To test my predictions, I conduct a laboratory study to examine the effects of: 1) the announcement of an ex-post adjustment policy, 2) not exercising this policy to help employees, 3) providing an explanation, and 4) providing perspective taking training, on employee justice perceptions and performance.

The remainder of the chapter is organized as follows. I discuss the results of my four formal hypotheses tests and one research question in section 6.2. I discuss limitations and opportunities for future research in section 6.3. Finally, I discuss the contributions of my study and provide concluding remarks in section 6.4.

6.2 Discussion of Results

In contrast to my predictions in H1a, I do not find that the announcement of an ex-post adjustment policy improves overall justice perceptions. I see three possible explanations for the failure to observe this effect. First, it could be that the fair intentions signaled by the announcement of such a policy were too weak to significantly alter participant justice perceptions. Second, it is possible that participants did not react positively to the zero-sum nature of goal adjustments. Third, it is also possible that due to the considerable uncertainty over how these ex-post adjustments would be exercised, participants were unsure of how to react to the announcement of such a policy.

I find it most likely that participants either reacted negatively to the zero-sum nature of goal adjustments, or that uncertainty over the execution of the policy caused participants to reserve updating their fairness judgments until gaining some observations on how the policy would be enacted. Both of the above explanations are consistent with Bol (2008) and Baiman and Rajan (1995), who both note that from an agency theory perspective, the announcement of a discretionary ex-post adjustment policy can be perceived as cheap-talk, since it is non-binding. Baiman and Rajan (1995) further note that management's concerns for reputation may assuage employee concerns that management will renege on their commitment to consider making ex-post adjustments. However, in my experiment, by design, participants have no prior beliefs about the nature of their relationship with management, and given the limited number of periods in my experiment, participants have no reason to believe that concern for reputation would affect management decision making. Therefore, I find it likely that participants did not react to the announcement due to uncertainty over how it would be implemented.

Despite failing to observe an announcement effect on justice perceptions, I observe a positive announcement effect on performance in the first difficult round in my test of H1b. This provides evidence that the announcement of an ex-post adjustment policy accomplishes its intended purpose, to sustain employee effort and performance in the face of negative exogenous shocks to performance. This builds on prior research illustrating that exercising ex-post adjustments leads to performance improvements (e.g. Kelly et al. 2015; Cai et al. 2019) by showing that even the mere announcement of an ex-post adjustment policy can also improve performance.

However, the pattern of results in H1a and H1b suggests that the positive announcement effect on performance is not mediated through justice perceptions as predicted, conflicting with Kelly et al. (2015), who find that the positive effect of exercising ex-post adjustments is partially mediated through procedural justice perceptions. Although Arnold and Artz (2015) find and predict a negative effect of target flexibility on performance, they suggest that the expectancy of goal adjustment plays a role in employee effort choices.⁸⁷ It is possible that when ex-post adjustments are available, expectancy of goal adjustment sustains effort and performance in anticipation of a goal adjustment to filter out the effects of negative exogenous shocks on performance. To further explore this possibility, I examine the effect of participants' expectancy of goal adjustment on performance in the first difficult round, finding no effect. However, in my analyses of effort duration and intensity, I find a marginally positive announcement effect on effort duration, and no effect of announcement on effort intensity. This suggests that the positive announcement effect on performance is driven by an increase in task persistence for participants

⁸⁷ Arnold and Artz (2015) suggest that when targets are flexible, employees may withhold effort opportunistically in anticipation of a target adjustment.

aware of the possibility of ex-post adjustments. Taken together, the results of these supplemental analyses provide conflicting evidence on the nature of the observed positive announcement effect.

In my tests of H2, I observe a negative time trend in Control participants' perceptions of overall justice and performance. Further, in contrast to my predictions, I find that the non-helping behaviour observed by No Intervention participants does not have an incremental negative effect on overall justice perceptions, and a negative but insignificant incremental effect on performance. The significantly negative time trend on overall justice perceptions suggests that in between the pre- and post-periods, an event triggered participants to feel worse about the procedures and outcomes in the experiment. Further, the lack of an incremental negative effect of non-helping behaviour on overall justice perceptions suggests that this event was common to both Control and No Intervention participants. This pattern of results is strongly suggestive of participants in both conditions reacting negatively to the occurrence of negative exogenous shocks to performance in round 2 and round 3, resulting in participants failing to attain their goals and earn their bonus in these rounds. The finding of a directionally negative but insignificant incremental effect of non-helping behaviour on performance in the post-period, combined with the observed positive effect of announcement on round 2 performance suggests that observing non-helping behaviour attenuates the positive effects of announcement. That is, although the announcement of an ex-post adjustment policy can sustain effort in the face of negative exogenous shocks to performance, once non-helping behaviour is observed, this effect is lost.

The fact that explanation improved overall justice perceptions but not performance suggests that simply improving justice perceptions does not automatically translate into

improved performance. This is consistent with Colquitt et al. (2001), who point to the equivocal nature of findings in the justice literature with respect to this relationship. The equivocal nature of the findings with respect to the justice to performance relationship could owe in part to the manner in which distributive justice perceptions are formed. According to equity theory, individuals arrive at distributive justice perceptions by comparing the ratio of their outputs to inputs (Adams, 1965). This rational approach to examining outputs to inputs may lead participants to take a measured approach to updating effort choices in response to improved distributive justice perceptions, only increasing effort when the marginal benefits exceed the marginal cost. In contrast, using social exchange theory as a lens (Cropanzano and Prehar 1999), procedural justice indicates to employees that they are valued members of the firm, which is reciprocated through high task effort. Importantly, unlike equity theory's conception of distributive justice, it is not based on consideration of output to input ratios, and improvements in procedural justice may result in effort improvements with little regard to the marginal costs and benefits of effort provision.

The results of my model testing show that my theoretical model has inferior fit to an alternative expectancy-based model of performance. In this alternative model, the effects of my fairness interventions on performance are mediated not only by justice perceptions, but also by expectancies. The rationale behind this alternative model is that when individuals feel fairly treated, the effort-performance relationship is reinforced, resulting in greater effort provision. In testing this model, I observe significant and positive indirect effects of both explanation and perspective taking on performance. This, combined with the findings in H3 of no overall effect of explanation on performance, suggests that there is some lingering negative direct effect of explanation on performance masking the significant indirect effect.

In the examination of my research question, I find interactive effects of explanation and perspective taking on procedural and distributive justice perceptions. I find that the nature of these interactive effects is suggestive of the two interventions being substitutes. Based on the above discussion, equity theory may predict a more rational response to my fairness interventions, such that if individuals perceive equitable output to input ratios in response to one intervention, there is little scope for the other intervention to improve or deteriorate overall justice perceptions.

6.3 Limitations and Opportunities for Future Research

A limitation of my study is that based on the sample sizes per cell, I may have not had adequate power to observe some of my hypothesized effects, resulting in my failure to find support for some of my hypotheses. This applies primarily to H1 and H2, as well as H3b. However, I do not believe lack of power played a significant role in my failure to observe my predicted effects, since in each of these cases, I observe an insignificant effect directionally *opposite* to my predictions (and thus cannot perform conventional power analyses). Nonetheless, I cannot rule out that lack of power contributed to my failure to observe my predicted effects for these hypotheses.

In the preceding section, I discussed two possible reasons for my inability to observe a positive announcement effect on justice perceptions, the first being that participants may have reacted negatively to the zero-sum nature of goal adjustments, undermining the possible positive impact of the announcement. This is an important setting to study, since Baiman and Rajan (1995) acknowledge that employees may react negatively to the zero-sum nature of ex-post goal adjustments in a fixed bonus pool setting. Unfortunately, due to the fact that the zero-sum nature

of adjustments is perfectly confounded with the availability of adjustments in my experiment, I cannot assess whether the failure to find a positive announcement effect on justice perceptions is due to participants reacting negatively to the zero-sum nature of adjustments, representing a limitation of my study. Future research can examine whether the zero-sum nature of ex-post adjustment policies impacts employee reactions to the announcement of these policies.

The second explanation discussed in the preceding section for my failure to find support for H1a was that uncertainty over how the policy would be enacted blunted participant responses to the announcement. I conjectured that this uncertainty would likely be related to prior beliefs about and interactions with management, but by design, I provide participants no information about their prior relationship with management as to not overcomplicate my experiment, and so could not test this possibility in my experiment. This represents a limitation of my study. Future research could examine the moderating role of prior relationships on the effect of both the announcement of and enactment of ex-post adjustment policies. On the one hand, it is possible that a strong, positive prior relationship between employee and management could strengthen the initial announcement effect. On the other hand, this strong, positive prior relationship may make subsequent management inaction feel more like a betrayal, exacerbating the negative reaction to management inaction.

In my tests of H3 and H4, I observe a complex pattern of results, whereby explanation improved overall justice perceptions, but not performance, whereas perspective taking improves both. I conjectured that the difference in how distributive and procedural justice perceptions are formed may affect the overall justice to performance relationship such that improvements to procedural justice may lead to improvements in performance despite questionable returns on costly effort. Because I do not manipulate the cost of effort in my study, I cannot test this

conjecture. This is an important phenomenon to examine, since fairness interventions cost valuable time and money to implement, and firms should target the dimension of justice perceptions that are most likely result in returns in the form of improved employee performance. Future research could examine how the nature of fairness interventions target different dimensions of justice perceptions, and how this in turn affects employee performance.

One of the key contributions of my study is in developing a novel fairness intervention based on perspective taking through shared experience. I also believe this to be one of the most fruitful avenues for future research. Future research could examine whether such a perspective taking intervention can be applied to reduce other cognitive biases, such as surrogation, which describes the tendencies for employees to conflate performance measures with the strategic construct of interest (Choi et al. 2012). Future research could also examine possible moderators of the perspective taking performance relationship, such as prior relationships between the perspective taker and the target. Conceivably, if the prior relationship between the perspective taker and the target is negative, then increasing the availability of the target's internal state may have the unintended consequence of actually worsening performance as it may trigger employees to recall past negative interactions.

6.4 Contributions and Concluding remarks

I believe my study makes several important contributions to management accounting and psychology research. First, I contribute to a small but growing literature on how ex-post adjustments influence employee performance. To the best of my knowledge, there are only a handful of studies examining this phenomenon (Kelly et al. 2015, Arnold and Artz 2015, Burt et al. 2019, and Cai et al. 2019). Further, I am unaware of any such studies that examine the period

between the announcement of an ex-post adjustment policy and its enactment as I do in my study. As discussed in my literature review, this is an important setting to study, since there can be considerable time between when such policies are announced, and when they are first enacted, especially if the organization sets employee goals with long time horizons, such as in the case of annual performance targets. My finding of a positive announcement effect on performance suggests that employees respond to the content of announcements by changing effort choices, contributing to our understanding of how employees react to unverifiable signals from management. Further, this finding suggests that the mere existence of these policies may be sufficient in motivating performance in the face of negative exogenous shocks to performance, which is the intended purpose of such a policy.

In addition, the above studies focus on settings where ex-post adjustments are available and exercised, whereas I focus on situations where adjustments are available and *not* exercised. As discussed in my literature review, this is an important setting to study, since in practice, such policies are exercised selectively (Höppe and Moers 2011; Bol et al. 2015), and this selective exercise may undermine the positive announcement effects described above. However, I find that even when management decides not to exercise ex-post adjustments, this observed non-helping behaviour does not significantly worsen justice perceptions, and only directionally worsens performance. This suggests that the existing management practice of selectively exercising ex-post adjustments may not be detrimental to employee justice perceptions and performance, possibly contributing to the low rate at which these adjustments are exercised in practice.

My study also contributes to organizational justice research by examining the effects of explanation and perspective taking on justice perceptions. Although explanations have been studied extensively by psychologists (e.g. Bies and Moag 1986; Bies and Shapiro 1988; Colquitt

and Chertkoff 2002), I contribute to this literature by demonstrating that explanation can suppress the positive effects of perspective taking. Further, by demonstrating that explanation can be effective at improving justice perceptions but is not effective at improving performance in my setting, I contribute to the literature highlighting the equivocal nature of the justice perception to performance relationship described by Colquitt et al. (2001). Further, I shed some light onto the equivocal nature of this relationship by showing through my mediation analyses that the justice to performance relationship is mediated through expectancy.

I also contribute to organizational justice and perspective taking literature by demonstrating that perspective taking can be used as an effective fairness intervention, building on work by Epley et al. (2006). To the best of my knowledge, I am one of the few studies examining the use of perspective taking as a fairness intervention in an organizational setting, and the first to demonstrate its use as a performance management tool. This represents a significant contribution, since as described in my hypotheses development, perspective taking has the capacity to directly combat egocentric bias, which can undermine the effectiveness of other fairness interventions, such as explanation. This makes perspective taking an excellent candidate as a fairness intervention in situations where employees' limited perspective causes them to doubt the legitimate motives of management, since perspective taking should theoretically be able to provide them with a more expansive view of the situation possessed by management.

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APPENDIX

I. Justice Questionnaire

Please answer the following questions about the experiment administrator, who designed this experiment and is responsible for making all decisions in this experiment.

Are the procedures used to determine your pay in this experiment...

Applied consistently?

Strongly Disagree Disagree Neither Agree nor Disagree Agree Strongly Agree

Fair?

Strongly Disagree Disagree Neither Agree nor Disagree Agree Strongly Agree

Free of bias?

Strongly Disagree Disagree Neither Agree nor Disagree Agree Strongly Agree

Does your pay earned in this experiment...

Reflect the effort you have put into your work?

Strongly Disagree Disagree Neither Agree nor Disagree Agree Strongly Agree

Seem appropriate for the work you have completed?

Strongly Disagree Disagree Neither Agree nor Disagree Agree Strongly Agree

Reflect how you will perform on the task?

Strongly Disagree Disagree Neither Agree nor Disagree Agree Strongly Agree

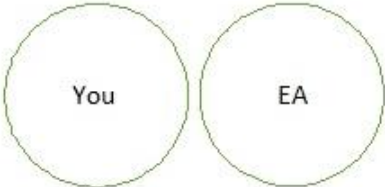
Seem fair?

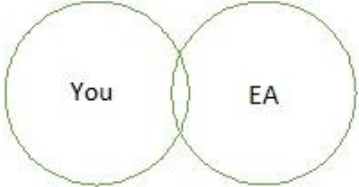
Strongly Disagree Disagree Neither Agree nor Disagree Agree Strongly Agree

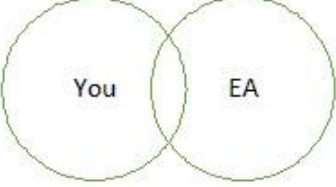
Appendix continued


II. Inclusion of Other in the Self Scale

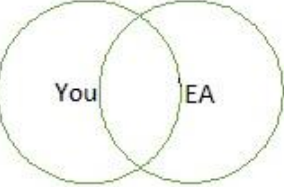
Please select the picture that best describes the level of closeness or connection between you and the experimental administrator.

















Appendix continued

III. Attention and Manipulation Checks

1. I was provided adequate justification for decisions made during this experiment that impacted my performance and pay.

Strongly Disagree Disagree Neither Agree nor Disagree Agree Strongly Agree

2. My performance in some periods were negatively impacted by the increased difficulty of these periods.

Strongly Disagree Disagree Neither Agree nor Disagree Agree Strongly Agree

3. The experimental administrator had the ability to reduce the impact of this increased difficulty on my performance.

Strongly Disagree Disagree Neither Agree nor Disagree Agree Strongly Agree

4. Deciding whether or not to make a goal adjustment to help out an employee is a difficult decision.

Strongly Disagree Disagree Neither Agree nor Disagree Agree Strongly Agree

Appendix continued

IV. Coding Protocol

Code Assigned	<i>CogPT</i>
0	Participant rationale and explanation for adjustment does not demonstrate an understanding that using goal adjustments to help the underperforming employee materially hurts the top performing employee. Response only references impact on bottom performing employee. Participant suggests an adjustment to the top performing employee of 2 or greater.
1	Participant rationale and explanation for adjustment does not demonstrate an understanding that using goal adjustments to help the underperforming employee materially hurts the top performing employee, but does not suggest an adjustment to the top performing employee of 2 or greater.
2	Participant rationale clearly demonstrates an understanding that using goal adjustments to help the underperforming employee materially hurts the top performing employee.

Code Assigned	<i>Group</i>
0	Participant response only references impact of goal adjustments on individual level outcomes, does not mention impact of adjustment on group as a whole.
1	Participant response only directly references impact of goal adjustments on individual level outcomes, but response indicates that participant considered group outcomes.
2	Participant response directly references impact of goal adjustments on employees as a group.