The Economics of Behavior

Essays on the Organizational Effects of Identification, Wage Expectations, and Fairness Concerns

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Lea M. Petters, M.Sc.

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Referent:	Professor Dr. Dirk Sliwka
Korreferent:	Professor Dr. Matthias Heinz
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Chapter 1

Introduction

"To understand how economies work and how we can manage them and prosper, we must pay attention to the thought patterns that animate people's ideas and feelings, their animal spirits."

George A. Akerlof and Robert J. Shiller (2009)

This thesis deals with behavioral factors that critically influence the outcomes of organizational practices. Based on theory, we use experimental and field data to investigate to what extent employees' decisions are driven by psychological motives such as emotional attachment, expectations, and fairness perceptions. We show that behavioral mechanisms play a crucial role in determining the consequences of organizational practices, often affecting them in unexpected ways.

Traditional economics has evolved around the idea of an economic agent that is rational, forward-looking, has unlimited cognitive abilities and only cares about his own monetary payoff - the *homo oeconomicus*. Although researchers were well aware early on that this concept constitutes a very simplified model of human nature, it took until the second half of the twentieth century for behavioral economics to establish itself as a subfield of the economic discipline (Dohmen 2014). The ever-growing empirical evidence contradicting the neoclassical view contributed significantly to the advancement of the behavioral approach, which aims at explaining these phenomena (Dhami 2016). This new strand of literature explicitly models psychological factors as determinants of individual economic decision-making, giving credit to heterogenous, time-inconsistent, and other-regarding preferences, biased beliefs, as well as bounded rationality. Since human behavior is at the heart of most questions in labor and particularly in personnel economics, the models and methods of behavioral economics have received increased attention in this field within recent decades (Gächter and Fehr 2002; Charness and Kuhn 2011; List and Rasul 2011; Babcock, Congdon, et al. 2012). When looking at micro-level interactions between employers and employees or amongst employees respectively, the importance of psychological factors for individual behavior becomes especially evident and cannot be neglected (Dohmen 2014). Therefore, understanding the factors that influence human decision-making and accounting for this when implementing managerial practices, is crucial for organizations.

In the following chapters, we shed some light on human reactions to managerial measures. We do so by examining behavioral mechanisms that lead to ambiguous and potentially adverse organizational effects. One channel we are looking at is the employee's identification with the employer. In their seminal paper, Akerlof and Kranton (2000) are the first to introduce the concept of identity into the individual's utility function. Later work transfers this concept to the organizational context and establishes identification with the employer as a major source for employee motivation (Akerlof and Kranton 2005; Besley and Ghatak 2005). We extend the literature by considering the role of identification in employees' job search and wage bargaining behavior and their implications for wage growth. Additional behavioral mechanisms we are interested in are fairness concerns (Konow 1996; Bolton and Ockenfels 2000; Cappelen et al. 2007) and their implications for organizational interactions (see Fehr, Goette, and Zehnder 2009, for an overview). Based on previous findings that show that the acceptance of affirmative action policies depend on their perceived fairness (Harrison et al. 2006; Balafoutas, Davis, and Sutter 2016; Ip, Leibbrandt, and Vecci 2018), we study how differences in procedural fairness induced by affirmative action and/or ex-ante disadvantages affect peer-review behavior in competitive settings. Lastly, building on the work by Akerlof and Yellen (1990) that introduced the concept of the fair wage-effort hypothesis, we consider the role of training participation in fair wage expectations and its effect on subsequent effort provision and productivity. All mechanisms we investigate deal with non-standard preferences, i.e., social comparisons and social preferences (DellaVigna 2009). The presented research, therefore, falls into the subfield of behavioral economics that challenges the neoclassical assumption that individuals only take their own absolute payoff into account when deciding in economic situations.

Our research features a mix of different methods combining theoretical arguments with evidence from field and experimental data. The combination of methods allows us to analyze a problem from different perspectives and to gather evidence from complementary sources, thus giving a more complete picture of the question of interest (Dhami 2016; Kampkötter and Sliwka 2016). Theoretical considerations provide a structured and mathematically formalized approach to think about an economic problem. Furthermore, it enables the derivation of clearly defined hypotheses with respect to the research question in focus. Based on the theoretical foundations, empirical field data can give insights as to whether the predicted patterns can be observed in the real world. While field data are excellent to detect whether these patterns can be generalized to environments where different mechanisms are at play and many forces interact, it is often difficult to control for unobserved factors and retain clear causal evidence. This is where experimental methods have their strengths (Gächter and Fehr 2002; Dohmen 2014). Experiments can establish causal relationships by implementing truly exogenous variation in a very controlled environment. Furthermore, using these methods, researchers can collect data on a very granular and detailed level, that can hardly be found in field data. This allows specific behavioral mechanisms to be uncovered and is therefore particularly well suited to test theoretical predictions (Falk and Heckman 2009; Charness and Kuhn

2011; Ludwig, Kling, and Mullainathan 2011). In the following paragraphs, we shortly summarize the contents and the individual contributions of the different chapters.

Chapter 2 studies the role of employees' identification with their employer as a component of match quality for determining job satisfaction, effort provision, job search, bargaining behavior, and resulting wage growth.[1] Previous research has mainly focused on outcomes of match quality (e.g., wage, tenure, productivity), instead of trying to explicitly measure its components. In a first step, we analyze a stylized formal model, which integrates the emotional attachment to the employer into the employee's utility function. In line with previous results in the literature (Akerlof and Kranton 2000; Besley and Ghatak 2005), our theoretical framework predicts that a higher identification with the employer is related to (i) a lower marginal utility from wages and (ii) higher work effort. Furthermore, we consider wage bargaining behavior and identify two different channels through which the employee's emotional attachment can affect wage growth. In theory, there is a trade-off between a "compensating wage differential" effect, i.e., the employer can provide lower wage growth because the employee actually enjoys working for the employer, and a "motivation" effect, i.e., the employer would be willing to grant higher wage growth as the employee exerts higher effort for a given wage level. The relative bargaining position determines which of the two effects dominates. When the employer's bargaining power is sufficiently high, the model predicts that (iii) employees with a higher emotional attachment experience lower wage growth. This is also driven by (iv) lower search efforts on the side of the employee and thus a lower likelihood of obtaining an external offer. However, when the employee has obtained an external offer, the bargaining situation reverses and an employee with higher identification can (v) negotiate a higher wage growth since he is more valuable to the organization.

As a second step, we test the predicted patterns using a novel employer-employee panel dataset. We take advantage of a validated survey measure of "affective commitment" (Meyer and Allen 1991) as a proxy for employee identification. Consistent with our theoretical model, we find that, for committed employees, absolute wage is significantly less predictive for job satisfaction. Additionally, we observe that employees with higher commitment have significantly fewer absence days and more hours of unpaid overtime, which represent our effort measures. Moreover, higher commitment predicts a lower wage growth in the future and is associated with a lower propensity to search for alternatives, receive an external offer, and to quit the current employment voluntarily. However, we also find evidence that employees can successfully exploit their higher threat point when they have obtained an external offer, thus resulting in increased wage growth. This relationship seems to be even more pronounced for more committed employees.

Our research adds to the current literature by providing a theoretical framework that models identification as a component of non-monetary match quality which not only affects job satisfaction and effort provision, but also influences job search behavior and thus wage trajectories. Furthermore, we present evi-

[1] Chapter 2 is joint work with Patrick Kampkötter and Dirk Sliwka and based upon Kampkötter, Petters, and Sliwka (2019)

dence for the predictive power of a validated survey measure, thereby emphasizing the empirical relevance of employee identification for employer-employee relationships.

In chapter 3, we experimentally analyze the effect of quota interventions on peer-review behavior.[2] While affirmative action policies in the form of quotas are increasingly used by regulators to promote the representation of minority groups in leading positions in management and academia (Wallon, Bendiscioli, and Garfinkel 2015; European Commission 2016), the scientific evidence on the effectiveness of quota interventions remains mixed (Beaman et al. 2009; Niederle, Segal, and Vesterlund 2013; Leibbrandt, Wang, and Foo 2018). Our study gives insights into potential negative side effects and shows that quotas can lead to distortions in subjective peer-reviews, and therefore harm the group that is supposed to benefit from the quota.

We study the impact of a quota intervention in a situation where subjective peer-reviews are a crucial determinant of the career advancement of an individual. Such situations frequently arise in business environments where hiring and promotion decisions are based on subjective evaluations by (potential) supervisors or co-workers. They also play an essential role in the academic profession where peer-reviews are decisive for publication success, research funding or tenure. Since the introduction of a quota substantially changes the competitive structure within a tournament, such an intervention might also affect peer-review behavior. On the one hand, a quota increases competition among the group which is affirmed under the quota regime and therefore provides an incentive for this group to evaluate other affirmed peers less favorably. On the other hand, by design a quota favors the affirmed group over the non-affirmed group and thus creates inequality within the tournament, which also may lead to a reaction in peer-review behavior due to procedural fairness concerns. These fairness concerns, however, might be mitigated depending on the justification behind the introduction of the quota (Balafoutas, Davis, and Sutter 2016; Ip, Leibbrandt, and Vecci 2018).

To shed light on these questions, we conduct a real-effort tournament experiment, in which we randomly assign participants to affirmed or non-affirmed types. Participants are asked to work on a creative task (Laske and Schröder 2016) and subsequently to evaluate the performance of the three other peers within their group. The outcome of this peer-review process determines which participants win one of two prizes. In a two-by-two design, we vary (i) whether or not a quota is implemented and (ii) whether or not the affirmed group faces ex-ante procedural disadvantages. When a quota is implemented, one of the two prizes is reserved for the best-performing participant within the affirmed group. In treatments with ex-ante inequality, affirmed participants face procedural disadvantages in form of a shorter working time to fulfill the task. This ex-ante disadvantage might serve as a potential justification for the introduction of a quota.

Our results show that quotas have a significant impact on peer-review behavior. First, we find that quotas affect the overall level of peer-reviews provided. This effect, however, depends on the perceived procedural fairness which varies

[2]Chapter 3 is joint work with Marina Schröder and based upon Petters and Schröder (2019)

between the two quota treatments. In our research design, the absolute level of peer-reviews has no impact on tournament outcomes since both affirmed and non-affirmed groups are affected equally. In real-life applications, though, this might lead to negative effects on overall motivation and work climate, and might distort evaluations when levels are compared across different environments. Second, we show that quotas lead to systematic biases in peer-reviews against the affirmed group. They receive significantly less favorable peer-reviews relative to their non-affirmed peers. This second effect is not related to the perceived procedural fairness since it remains robust across both quota treatments. Instead, these distortions seem to be a result of the enhanced competition among affirmed invividuals under the quota regime as they are fully driven by peer-reviews provided by affirmed individuals to other affirmed peers. This result has strong implications for the effectiveness of quota interventions. Unfavorable peer-reviews might hinder the career advancement of the affirmed group and thus counteract the initial goal of the the quota intervention. Lastly, we study spillover effects of quotas on giving in an additional dictator experiment after the conclusion of the main experiment. We find that a quota in the previous experiment significantly reduces altruistic behavior among individuals that were affirmed before. Therefore, we provide evidence of negative spillover effects of quotas to non-competitive environments. The results indicate that a quota regime might actually impede the establishment of social networks and mutual support within the affirmed group, thereby undermining another goal behind the introduction of affirmative action.

Our research points towards negative side effects of quota interventions that mainly affect the group which was supposed to benefit from preferential treatment. Therefore governments and organizations, which seek to implement quotas in order to promote minority groups, need to pay special attention to potential adverse effects of such interventions as they might render them ineffective.

Chapter 4 presents a further behavioral mechanism that impedes the initial goal of an organizational measure.[3] We study the effect of training participation on employees' fair wage expectations, effort provision, and finally productivity. Firms invest in training to increase the skills and, through this, the productivity of their employees. We theoretically argue that the relationship between an increase in skills and higher productivity might not be as clear-cut since behavioral factors might also play a role. Given that labor productivity is determined by two factors, namely skills and effort, training should increase the employee's skill level and thus ceteris paribus have a positive impact on productivity. Following the fair wage-effort hypothesis as introduced by Akerlof and Yellen (1990), the second component, effort, can be described as a function of wage relative to some "fair wage". If the actual wage is equal to or exceeds this fair wage, maximum effort is provided by the employee. If, however, the actual wage falls below what is perceived as fair, the model predicts that the employee feels unfairly treated and, as a consequence, reduces effort. We argue that the wage the employee perceives as fair depends on the employee's skill level and thus is also affected by training. Therefore, we hypothesize that training not only has a direct effect on skills, which positively affects productivity, but also an indirect effect on effort. This indirect effect works through the adjustment of

[3] Chapter 4 is based upon Petters (2019)

the fair wage, which - for a constant wage - has a negative effect on overall productivity. Training participation can therefore cause two countervailing effects, so that the net effect on productivity is ambiguous.

We use an extensive linked employer-employee survey dataset to study the relationship between training participation and future wage expectations. In our analyses, we apply an identification approach introduced by Leuven and Oosterbeek (2008). This approach exploits an alternative control group of accidental training non-participants to account for the potential selection bias into training, which would lead to biased estimation of the training effects. To be able to exogenously assign training and wages, as well as to explicitly measure skills, effort, and productivity, we conduct an additional laboratory experiment. We apply a newly developed experimental design which uses an employer-employee gift exchange setting. Employees work for two working phases for a fixed wage on a real-effort decoding task that benefits the employer. In the first working phase, all employees face the same working conditions, thus serving as a form of control phase. Thereafter, we vary (i) whether or not an employee receives training between the two working phases, and (ii) whether or not an employee receives a wage increase for the second working phase. Both after the first working phase and before the second working phase, we elicit a measure of the fair wage.

The analyses of our field dataset indicate that employees hold higher future wage expectations as a result of training participation. Our experimental results confirm this relationship and give additional insights into the behavioral mechanisms behind training participation. We find that even though training is effective in increasing the skills and thus productivity potential of an employee, this does not neccessary translate into increased productivity for the employer. Instead, our results show that trained employees negatively adjust effort both on the extensive and intensive margin. Additional analyses reveal that, in line with our theoretical considerations, the difference between the actual and the perceived fair wage is a determinant of whether or not an employee releases his productivity potential. Thus, these results indicate that fairness concerns can impair the positive productivity effects of training.

Our results have broad implications for organizational training investments. We show that behavioral factors play an important role in determining whether training is effective in increasing productivity. Not accounting for these channels might lead to lower than expected returns on training and thus result in reduced human capital investments by firms.

The research presented in this thesis demonstrates the importance of psychological factors for employer-employee interactions and the outcomes of managerial measures. We show that it is critical to understand the behavioral mechanisms at play in order to achieve the intended results and potentially take precautionary actions to prevent adverse effects.

Chapter 2

Employee Identification and Wages[1]

2.1 Introduction

In labor economics, it has often been stressed that an employee's decision on whether to stay or move to a different employer not only depends on wages, but also on non-monetary aspects of the job match (e.g., Sullivan and To 2014). In most of the literature, however, this "match quality" is treated as an unobserved black box and is only proxied by directly observable outcomes such as wages, tenure, firm size, worker skills or productivity (W. Johnson 1978; Jovanovic 1979; Mortensen 1988; Bowlus 1995; Abowd, Kramarz, and Margolis 1999; Gaure, Røed, and Westlie 2012; Eeckhout 2018; Eeckhout and Kircher 2018). This chapter opens part of this black box by studying one important component of match quality: employees' emotional identification with their employer. First, we analyze a formal model in which an employee works for an employer and is characterized by the degree to which he identifies with the incumbent employer. We assume that a higher identification increases the extent to which the employee internalizes the employer's payoff. In line with Akerlof and Kranton (2005) or Besley and Ghatak (2005), in such a framework, a higher identification naturally leads to higher work efforts. Moreover, the model predicts that an employee's well-being depends on his wage to a lesser extent when he identifies more strongly with his employer. In a next step, we consider wage negotiations and show that when the employer has sufficiently high bargaining power or when there is no moral hazard problem, wages are downward sloping in affective commitment. This constitutes essentially a "compensating wage differential" effect (e.g., Rosen 1986) as well known from the literature on public sector and non-profit motivation (Delfgaauw and Dur 2007; Delfgaauw and Dur 2008): an employee who attaches some intrinsic value to staying with the employer has a weaker bargaining position and thus stays with the firm at a lower wage level. However, the picture changes when the employee has a higher

[1]This chapter is joint work with Patrick Kampkötter and Dirk Sliwka and based upon Kampkötter, Petters, and Sliwka (2019)

threat point by having obtained an external offer and chooses an unobservable work effort. In this case, a higher identification with the firm has a higher value for the employer[2] as such an agent will exert higher efforts ex-post. In turn, a more "committed" employee will be able to negotiate a higher wage. Hence, the model does not make a clear prediction on the effect of employee identification on wage growth as there are two countervailing effects. However, the model does predict that, *conditional on effort,* wage growth should be downward sloping in affective commitment. Additionally, conditional on having an external offer, i.e., all bargaining power lies with the employee, more committed employees should be able to negotiate higher wages, and thus wage growth should be upward sloping in affective commitment.

Second, to test the predictions generated by this model, we analyze a novel linked employer-employee dataset. In order to quantify employees' identification with their employer, we use a standard survey measure of emotional attachment from the literature in organizational psychology (*affective organizational commitment*, see e.g., Meyer and Allen 1991) to predict future wage growth and search behavior in the labor market.[3] We find that (i) the predictive power of the wage level for job satisfaction is significantly weaker for employees with a higher affective commitment; (ii) a higher affective commitment is associated with higher work efforts, i.e., a lower number of absence days and more unpaid overtime; (iii) a higher affective commitment in period t predicts a lower wage growth in $t{+}1$; (iv) the effect is more pronounced when we control for a measure of employee effort; (v) a higher affective commitment is associated with a lower likelihood that an employee searches for another job, receives an external outside offer or voluntarily quits his job with his incumbent employer; and (vi) employees that have obtained an outside offer can negotiate significantly higher wage growth with their incumbent employer. In addition, we find evidence that this relationship tends to be even stronger for employees with higher affective commitment. This indicates, that employees with higher affective commitment are able to overcome the "compensating wage differential" effect by presenting a higher threat point in the form of an outside offer. However, they do so less often.

We contribute to the existing research in several ways. Even though the labor economics literature has considered the quality of the job match as an important determinant of worker satisfaction and retention (Bowlus 1995; Ferreira and M. Taylor 2011; Barmby, Bryson, and Eberth 2012), only few studies have attempted to measure aspects of match quality explicitly (see Fredriksson, Hensvik, and Skans 2018, for an example of the latter). With our focus on employee identification as an important non-monetary aspect of job match quality[4], we add to the discussion in labor economics and relate to concepts

[2] In this respect, identification underlies similar mechanisms as firm-specific human capital, which is also only valuable for the incumbent employer but not for potential external employers, and thus has strong implications for counteroffers by the incumbent employer once an external offer is available (see e.g., Yamaguchi 2010; Lazear 2012).

[3] Bömer and Steffes (2019) study supervisory support as a component of match quality, which determines employees' job search behavior using the same dataset.

[4] In contrast to rather stable cognitive skills and personality traits (or non-cognitive skills), i.e., "personal attributes not thought to be captured by measures of abstract reasoning power" (Heckman and Kautz 2012, p. 452), which the previous literature has identified as important factors for labor market success (e.g., Heckman, Stixrud, and Urzua 2006), emotional

discussed in the fields of behavioral economics, organizational psychology, and management. With the emergence of the behavioral economics literature and the consideration of social preferences in economic decision-making (Fehr and Schmidt 1999; Bolton and Ockenfels 2000; Charness and Rabin 2002), also the concept of (group) identity has been introduced into the field of economics (Akerlof and Kranton 2000; Akerlof and Kranton 2002; Akerlof and Kranton 2005). Recent experimental evidence has shown that social preferences are affected by group identity (Van Dijk, Sonnemans, and Van Winden 2002; Y. Chen and Li 2009; Goette, Huffman, and Meier 2006), i.e., the concern for the well-being of another individual is stronger when this person shares a common group identity. In the context of organizations, Akerlof and Kranton (2005) stress the importance of employees' identification for work motivation. In line with this reasoning, Besley and Ghatak (2005) argue that organizations benefit when employees share their mission (see also Francois 2000; Glazer 2004; Delfgaauw and Dur 2007; Delfgaauw and Dur 2008). Several recent contributions provide empirical evidence supporting this view (Tonin and Vlassopoulos 2015; Burbano 2016; Carpenter and Gong 2016; Cassar 2019). While the mission match between employer and employees specifically refers to the channel of overlapping preferences towards a higher non-monetary goal, identification can be defined in a broader context. Mission alignment, thus, can be understood as one mechanism that may evoke identification with the employer. Based on the theory of psychological needs by Deci and Ryan (2000), Cassar and Meier (2018) apply the concept of self-determination theory to the organizational context and define "meaning of work" along the four dimensions mission, autonomy, competence, and relatedness. They describe relatedness as a feeling of connectedness to the organization and its members, thus this dimension of meaning of work closely relates to our understanding of identification.

To capture identification in the empirical part of this chapter, we make use of the widely applied and validated survey measure of "affective commitment". The notion of "affective commitment", which describes the strength of the emotional attachment of an employee to the employer, has first been considered in the field of organizational psychology.[5] A large body of evidence (see e.g., Meyer and Allen 1984; Tett and Meyer 1993; Rhoades, Eisenberger, and Armeli 2001) has shown that employees differ in the extent to which they feel attached to the organization and that such "affective commitment" is generally considered to be the most important dimension to predict individual turnover (intention), job performance, and absenteeism (see Meyer, Stanley, et al. 2002, for a meta-analysis).

We contribute to this literature by analyzing the relationship between identification and job satisfaction, effort provision, wage growth, job search behavior, and employee mobility[6], both in a theoretical model and with field data. We provide

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attachment can be viewed as a match-specific component. This means that an individual's affective commitment is typically rather stable within an organization, but is likely to vary in a different job match at a different employer.

[5] In a very influential contribution, Meyer and Allen (1991) argue that an employee's "organizational commitment", i.e., the individual's psychological attachment to the organization, consists of three components. Besides affective commitment, the other components are "continuance commitment" as the awareness of the costs associated with leaving the organization and "normative commitment" as the feeling of obligation to continue the employment.

[6] Kampkötter and Sliwka (2014) show that incumbent employees with high levels of firm

empirical evidence from a representative linked employer-employee dataset that not only provides ample information on individual characteristics, attitudes, and labor market outcomes, but also detailed knowledge of specific job search behavior and outcomes, which previous datasets typically lack. This allows us to study the nexus between commitment to an employer and the job matching process in more detail. Additionally, we present evidence for the predictive power of a self-reported survey measure of identification for actual wage trajectories and turnover outcomes, and thereby contribute to the recently emerging literature which emphasizes the relevance of validated survey measures for economic behavior and decision-making (Blinder and Krueger 2013; Bender, Bloom, et al. 2018; Falk and Hermle 2018; Falk, A. Becker, et al. 2018).

2.2 The Model

Consider the following simple model to illustrate the key ideas. An employee works for two periods $t = 0, 1$. In period 0, the employee is hired by a firm. The employee's utility function in period t is

$$U(\pi_{Wt}, \pi_{Ft}) = \pi_{Wt} + \gamma \pi_{Ft},$$

where π_{Wt} is the material well-being of the employee and π_{Ft} are the profits of the employer. Let γ be a measure of the employee's identification with the employer or his "affective commitment" towards the employer: the higher γ, the greater the extent to which the employee internalizes the employer's well-being. Employee and employer learn the realization of γ after the employee is hired in period 0. The employee is initially hired at a market wage $w_0 = w_M$. In period 1, the employee and the firm negotiate the wage w_1 and the bargaining outcome is determined by the generalized Nash bargaining solution, where the employee has bargaining power λ. In each period, the employee chooses a work effort a which generates a profit $\pi_{Ft} = K(a_t) - w_t$ for the employer and a material well-being $\pi_{Wt} = w_t - c(a_t)$ for the employee with $K_a, c_a, c_{aa} > 0$ and $K_{aa} \leq 0$.

2.2.1 Analysis

The employee's utility in a period t is thus

$$w_t - c(a_t) + \gamma(K(a_t) - w_t)$$

and the employee chooses an effort such that

$$\gamma K'(a_t) - c'(a_t) = 0 \tag{2.1}$$

which implicitly defines his effort $a(\gamma)$ such that

$$\frac{\partial a(\gamma)}{\partial \gamma} = -\frac{K'(a)}{\gamma K''(a) - c''(a)} > 0$$

and this implies the following simple first result:

tenure have lower wages compared to newly hired employees in the same position arguing that the fact that these employees did not leave the firm in the past indicated higher mobility costs (which also capture some non-monetary elements such as affective commitment to the incumbent employer), which weakens their bargaining position.

Proposition 1 *When the employee exhibits a stronger identification with the employer, (i) his marginal utility from wages is lower and (ii) his work effort is higher.*

Note that this corresponds to typical results in the literature on employee identification (Akerlof and Kranton 2005), mission motivation (Besley and Ghatak 2005; Cassar 2019), or public sector and non-profit motivation (Delfgaauw and Dur 2007; Delfgaauw and Dur 2008): The well-being of an employee with a higher identification with the employer depends on his wage level to a lesser extent. Moreover, as he internalizes the employer's output to a greater extent, such an employee will work harder.

2.2.2 Wage Bargaining

In a next step, we analyze the wage bargaining outcome in period 1 and the resulting change in wages between periods 0 and 1. The employee's utility when staying with the firm is

$$(1-\gamma)\, w_1 + \gamma K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right)$$

and his threat point utility is equal to u_M.[7] The employer's utility when the employee stays is

$$K\left(a\left(\gamma\right)\right) - w_1$$

and we normalize the employer's threat point utility to 0.[8] Note that the agent stays with the firm if there are gains from trade, i.e., a wage level exists in which both the firm and the agent are better off when the agent stays, which will be the case if

$$K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right) \geq u_M.$$

In this case, we apply the generalized Nash bargaining solution to obtain the rate of wage growth:[9]

Proposition 2 *When $K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right) > u_M$ the employee stays with the firm and his wage increases by*

$$\Delta\left(\gamma, a\right) = \frac{w_1}{w_0} = \frac{\lambda K\left(a\left(\gamma\right)\right) + (1-\lambda)\,\frac{u_M - \left(\gamma K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right)\right)}{(1-\gamma)}}{w_M}. \tag{2.2}$$

Conditional on effort a, wage growth is downward sloping in γ, i.e.,

$$\frac{\partial \Delta\left(\gamma, a\right)}{\partial \gamma} < 0.$$

[7] If the worker does not know the level of identification realized in a different job and has some beliefs about the realization of emotional attachment at the new employer, u_M is, for instance, equal to $E_\gamma\left[(1-\gamma)\, w_M + \gamma K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right)\right]$.

[8] This is, for instance, the case in a competitive labor market where $w_M = E_\gamma\left[K\left(a\left(\gamma\right)\right)\right]$.

[9] Note that here we characterize the relative wage growth as this is what we will explore empirically in the subsequent section.

When efforts are endogenous, then

$$\frac{\partial\Delta\left(\gamma,a\left(\gamma\right)\right)}{\partial\gamma}=\frac{\lambda}{w_M}\underbrace{K'\left(a\left(\gamma\right)\right)a'\left(\gamma\right)}_{>0}+\left(1-\lambda\right)\underbrace{\frac{u_M-\left(K\left(a\left(\gamma\right)\right)-c\left(a\left(\gamma\right)\right)\right)}{w_M\left(1-\gamma\right)^2}}_{<0}.$$

When the employer has some bargaining power ($0<\lambda<1$*), there is a trade-off between a "compensating wage differential" effect and a "motivation" effect. Wage increases are downward sloping in the employee's degree of identification with the employer if, and only if, the employee's bargaining power is sufficiently small.*

Proof: See the appendix to chapter 2.

Hence, there are two effects. On the one hand, there is a "compensating wage differential" effect: The employer can push committed employees to a lower wage as they enjoy working for the firm – and this joy will be lost when the employee leaves his incumbent employer. But, on the other hand, there is also a countervailing "motivation effect": When efforts are endogenous, committed employees work harder and are therefore more valuable for their incumbent employer, allowing them to reap part of this value in negotiations. Conditional on efforts, wage growth is thus downward sloping in γ. However, the net effect of affective commitment on wage growth is ambiguous when efforts are endogenous. When the employee has a strong bargaining power, the motivation effect dominates and wage growth is upward sloping in affective commitment. If, however, the employee's bargaining power is sufficiently small, the compensating wage differential effect is stronger and wage growth is downward sloping in affective commitment.

2.2.3 Job Search and External Offers

Now we consider an employee's effort to search for a new job. Assume now that before period 1, the worker can choose a search effort p at cost $k\left(p\right)$ with $k_p, k_{pp}>0$. This search effort determines the likelihood of receiving an outside offer generating utility u_O that may improve his outside option. The worker's search is successful ($d=1$) with probability p. In this case, the new outside option is drawn from a probability distribution with pdf $f\left(u_O\right)$ on the support $]u_M,\infty[$. If the search is not successful ($d=0$), the outside option remains u_M.

When the worker receives the external offer, he thus either negotiates a higher wage or leaves the firm obtaining a utility u_O. He will again stay with the firm if there are gains from trade, i.e., $K\left(a\left(\gamma\right)\right)-c\left(a\left(\gamma\right)\right)>u_O$. The negotiated wage increase when he stays is again determined by Nash bargaining analogously to Proposition 2 and thus will be equal to

$$\Delta\left(u_O\right)=\frac{\lambda K\left(a\left(\gamma\right)\right)+\left(1-\lambda\right)\frac{u_O-\left(\gamma K\left(a\left(\gamma\right)\right)-c\left(a\left(\gamma\right)\right)\right)}{\left(1-\gamma\right)}}{w_M}.$$

The outside offer will thus increase the agent's wage by $\frac{(1-\lambda)}{(1-\gamma)}(u_O - u_M)$ and utility by $(1-\lambda)(u_O - u_M)$ when staying. But if u_O is sufficiently large, the employee leaves the firm and his utility then increases by

$$u_O - [(1-\lambda) w_M + \lambda (K(a(\gamma)) - c(a(\gamma)))].$$

Hence, the expected utility gain from obtaining an external offer is

$$\begin{aligned} E[\Delta u] = & \int_{u_M}^{K(a(\gamma))-c(a(\gamma))} (1-\lambda)(u_O - u_M) f(u_O)\, du_O \\ & + \int_{K(a(\gamma))-c(a(\gamma))}^{\infty} (u_O - (1-\lambda) w_M - \lambda (K(a(\gamma)) - c(a(\gamma)))) f(u_O)\, du_O \end{aligned}$$

which determines the worker's optimal search effort. We can show:

Proposition 3 *If the employee obtains an external offer d providing utility $u_O > u_M$, he will stay with the firm if $K(a(\gamma)) - c(a(\gamma)) > u_O$. In this case the worker's expected wage increase conditional on the offer d is*

$$\begin{aligned} E[\triangle | d] &= \frac{\lambda K(a(\gamma)) + (1-\lambda)\frac{u_M - (\gamma K(a(\gamma)) - c(a(\gamma)))}{(1-\gamma)}}{w_M} && (2.3) \\ &\quad + d \cdot \frac{(1-\lambda)}{(1-\gamma)} \left(\frac{E[u_O | u_O \leq K(a(\gamma)) - c(a(\gamma))] - u_M}{w_M} \right) && (2.4) \end{aligned}$$

The stronger the employee's identification with the firm γ, the larger is the wage growth the agent achieves when having obtained an external offer. A stronger employee identification, however, reduces the employee's search effort and thus the likelihood that he leaves the firm.

Proof: See the appendix to chapter 2.

As we have seen before, without an external offer, wages may increase to a lesser extent for more emotionally attached workers (when either their bargaining power λ is small or when efforts are held constant). However, as the result shows, once the worker has obtained an external offer but stays with the employer, there is always a countervailing effect. To see this, note that

$$E[\triangle | d = 1] - E[\triangle | d = 0] = \frac{(1-\lambda)}{(1-\gamma)} \left(\frac{E[u_O | u_O \leq K(a(\gamma)) - c(a(\gamma))] - u_M}{w_M} \right)$$

is strictly *increasing* in γ. Hence, an external wage offer comes along with higher wage increases for more emotionally attached workers. The reason is twofold: First, the firm matches higher wage offers when a worker is more emotionally attached as such workers are more productive, i.e., $E[u_O | u_O \leq K(a(\gamma)) - c(a(\gamma))]$ is increasing in γ. But moreover, as such a worker's utility is less sensitive to money, the firm has to raise the worker's wage by a greater extent to match the higher threat point resulting from the external offer.[10]

[10] Note that the *utility increase* obtained through an external offer does not depend on γ when the worker stays.

The question naturally arises why an employee with a higher γ exerts lower search efforts. The reason is that with positive probability, the utility provided by the external offer u_O is so large that the worker leaves the firm. But for more attached workers this is less likely, as such workers have a higher productivity, i.e., $K(a(\gamma)) - c(a(\gamma))$ is larger. Moreover, if such workers leave, their utility gain from moving is smaller as they lose the psychological benefit of the larger emotional attachment. Thus, it may be that an employee with a higher emotional attachment to the firm will have a lower wage growth without an external offer, but achieves a higher wage growth once having obtained an external offer.

2.2.4 Predicted Patterns

Our model takes the strength of the employees' emotional attachment to the employer as given and derives predictions for the future employer-employee relationship and behavior. Note that we do not aim at identifying causal effects of employee identification with the employer, but rather use our formal model to describe qualitative characteristics of the conditional expectation function of future wage growth, work efforts, and search activities, conditional on the degree of employee identification. The following stylized expected patterns sum up our theoretical results: A stronger identification of an employee with the employer predicts

- *a lower marginal utility from wages:*
$$\frac{\partial E\left[u(w,\gamma)\,|w,\gamma\right]}{\partial w \partial \gamma} < 0,$$
- *higher work effort:*
$$\frac{\partial E\left[a\,|\gamma\right]}{\partial \gamma} > 0,$$
- *a lower wage growth (conditional on work effort):*
$$\frac{\partial E\left[\triangle\,|\gamma, a\right]}{\partial \gamma} < 0,$$
- *lower search efforts and a lower likelihood of obtaining an external wage offer:*
$$\frac{\partial E\left[p\,|\gamma\right]}{\partial \gamma} < 0.$$
- *a higher wage growth when having obtained an external offer*
$$\frac{\partial\left(E\left[\triangle\,|\gamma,\ d=1\right] - E\left[\triangle\,|\gamma,\ d=0\right]\right)}{\partial \gamma} > 0.$$

We test these patterns empirically using a representative matched employer-employee panel dataset.

2.3 Data and Descriptive Statistics

The empirical analysis is based on the first three waves of the Linked Personnel Panel (LPP), an employer-employee panel dataset that has been developed by the authors jointly with the Centre for European Economic Research (ZEW) Mannheim and the Institute for Employment Research (IAB) Nuremberg on behalf of the German Federal Ministry of Labor (BMAS). The LPP is a linked employer-employee dataset that is representative for German private sector establishments with more than 50 employees subject to social security contributions (see Kampkötter, Mohrenweiser, et al. 2016, for details on the construction and design of the dataset).[11] The employer survey is based on a subsample of the IAB Establishment Panel and is stratified according to four employment classes (50-99; 100-249; 250-499; 500 and more employees), five industries (metalworking and electronic industries; further manufacturing industries; retail and transport; services for firms; information and communication services) and four regions of Germany (North; East; South; West). The sample comprises 1,219 establishments in the first wave (2012/13), 771 in the second wave (2014/15) and 846 in the third wave (2016/17) and is representative for the above-mentioned establishment characteristics. A random sample of employees was drawn from participating establishments in each wave to take part in at home telephone interviews (CATI). The employee survey was carried out in 2012/13 (first wave) comprising 7,508 employees, in 2014/15 (second wave) comprising 7,109 employees, and in 2016/17 (third wave) comprising 6,428 employees.

Besides information on the workforce structure and composition, employee representation, ownership, legal structure and establishment-level performance measures originating from the IAB establishment panel, the LPP employer survey focuses on human resource management practices in firms in more detail. The employee survey includes a rich set of items on socio-demographic characteristics and detailed survey scales to assess job characteristics, personal characteristics, attitudes, and behavioral outcome variables.

Our main independent variable is *affective commitment* to the organization. This is a psychological construct that is widely used in organizational psychology and management research which captures an employee's emotional attachment to or identification with his employer. The dataset includes a six-item short scale by Meyer, Allen, and Smith (1993). This construct is a reduced but embedded scale of the original version introduced by Allen and Meyer (1990). Items were measured on a five-point Likert scale and show a high level of scale reliability with a value of Cronbach's alpha of 0.83. The six items read as follows: *"I would be very happy to spend the rest of my career with this organization", "This organization has a great deal of personal meaning for me", "I really feel as if this organization's problems are my own", "I do not feel a strong sense of 'belonging' to my organization", "I do not feel 'emotionally attached' to this organization", "I do not feel like 'part of the family' at my organization".*[12] The mean and median for this construct (unstandardized) range around 3.7 and 3.8 in both the first and the second wave.

[11] This study uses the Linked Personnel Panel (LPP), wave 1617, http://dx.doi.org/10.5164/IAB.LPP1617.de.en.v1

[12] The latter three items are reverse coded.

Further survey variables we use are *job* and *pay satisfaction*, which are measured on an 11-point Likert scale adapted from the German Socio-economic Panel Study from zero to ten with a mean of 7.5 and 7.6 (median 8) and 6.7 and 6.8 (median 7) in the first and second wave, respectively. Both commitment and job satisfaction are standardized with zero mean and unit variance before entering the regressions. Furthermore, we use the number of sick days within a year and the hours of unpaid overtime per week reported by the employees as proxies for effort within our analyses. Additional individual-level control variables include job status (blue collar vs. white collar), supervisory position, part time, gender, secondary and tertiary education, age, gross hourly wage, limited work contract, marital status, and household size. The set of establishment-level controls comprises industry, region, establishment size, ownership structure, and independent establishment. In table 2.6 in the appendix to this chapter, we provide an overview of the descriptive statistics of all the relevant variables on the employee and establishment level we use in our regressions.

Hourly wage growth is measured as annual change in hourly wages from the first to the second wave and the second to the third wave respectively (measured in percent).[13] In order to discard data outliers, we winsorize this variable at the 1% level in each tail. Average hourly wage growth equals 8.2 and 5.6% respectively within the time span of two years, the median hourly wage growth ranges comparably lower at 6.7 and 3.9%. *Active job search* is defined as dummy variable with value 1 if an employee has actively searched for a job in the 12 months prior to being surveyed. *Job offer* is a dummy variable coded 1 if an employee has been approached by another employer within the 12 months prior to the interview and has, as a consequence of the poaching behavior, received a specific job offer, and 0 otherwise (no job offer received and not being approached by an employer). *Realized voluntary turnover* is coded as 1 if the reason for the realized job change is voluntary, i.e., a termination by the employee itself and 0 if the employee is still with his incumbent employer.

2.4 Results

2.4.1 Job Satisfaction, Wages, and Affective Commitment

In order to test our first stylized prediction, we regress job satisfaction in period *t+1* on hourly wage in *t+1*, commitment in t and the interaction of both. The key idea of our first analysis is that we take job satisfaction as a measure of employee well-being and test the prediction that for employees with high affective commitment, the conditional expectation of their well-being is less dependent on their wages.

[13] Most of the predicted patterns from our theory section, which we will analyze empirically in the following, refer to changes between period t and $t + 1$ or outcomes in $t + 1$ based on variables in t. Therefore, given the structure of our data, t either refers to the first wave in 2012/13 or the second wave in 2014/15 and $t + 1$ to the second wave in 2014/15 or the third wave in 2016/17 respectively. Thus, the difference between t and $t + 1$ always relates to a two-year window. This also implies that the data from the third wave, in most of our analyses, will only be used to construct our dependent variables, but not as predictor variables.

In the first specification of table 2.1, we analyze pooled cross-sectional data from all three waves without any additional controls. In the second specification, we add employee and establishment characteristics. In specification (3), we include establishment fixed effects and in specification (4) employee fixed effects. The results show that total hourly wage is positively associated with job satisfaction but that the economic magnitude is small. This result mirrors findings from previous work (see e.g., Clark and Oswald 1996), where the absolute wage level also played a minor role for the prediction of job satisfaction. In line with our first stylized prediction, the coefficient for the interaction term between affective commitment and hourly wage has a negative sign. Thus, indicating that the conditional expectation function of job satisfaction has a weaker slope with respect to wages for employees who exhibit a stronger emotional attachment towards their employer. The size of the interaction term roughly corresponds to about 40 to 60% of the size of the wage coefficient in all three specifications, i.e., for a person with an affective commitment that is about 1.5 standard deviations above the mean, wages are not predictive for job satisfaction while the predictive power of wages for satisfaction is much higher for less emotionally attached workers. The interaction term remains statistically significant when we include establishment fixed effects. When we include worker fixed effects, the point estimate still shows a positive relationship but is no longer significant.

Table 2.1: Job Satisfaction and commitment

Dependent variable	Job satisfaction$_{t+1}$(std.)			
	(1)	(2)	(3)	(4)
Hourly wage$_{t+1}$(wins.)	0.008***	0.010***	0.011***	0.021**
	(0.002)	(0.003)	(0.004)	(0.009)
Commitment$_t$(std.)	0.433***	0.488***	0.438***	0.112
	(0.046)	(0.047)	(0.060)	(0.132)
Commitment$_t$(std.) *	-0.005***	-0.006***	-0.005**	-0.008
Hourly wage$_{t+1}$(wins.)	(0.002)	(0.002)	(0.002)	(0.005)
Constant	-0.202***	-0.176	-0.293	-1.039**
	(0.046)	(0.110)	(0.242)	(0.510)
Observations	3,450	3,237	3,237	3,237
Number of clusters	613	583	583	583
R-squared (within)	0.128	0.168	0.362	0.057
Employee & establ. controls	No	Yes	Yes	Yes
Establishment fixed effects	No	No	Yes	Yes
Employee fixed effects	No	No	No	Yes

Notes: Robust standard errors clustered on establishments in parentheses. Control variables on employee level include: blue collar, supervisory position, part time, female, secondary and tertiary education, age, limited work contract, marital status, household size, and year dummies. Control variables on establishment level include: industry, region, establishment size, ownership structure, and independent establishment. *** p<0.01, ** p<0.05, * p<0.1.

2.4.2 A Proxy for Work Effort

The second stylized prediction refers to the relationship between affective commitment and work effort in the same year. Since work effort is hard to measure across a broad number of firms, we use the number of sick days within a year and the amount of unpaid overtime hours per week, which essentially constitutes a gift to the employer, as two alternative proxies for work effort (see e.g., Engellandt and Ribhahn 2011). In table 2.2, we first analyze the pooled cross-section and then gradually include employee and establishment controls as well as establishment and employee fixed effects. Again, all specifications show the expected sign, i.e., more committed employees take fewer sick days (specifications (1) to (4)) and work, on average, more overtime (specifications (5) to (8)). We find that employees with a one standard deviation higher affective commitment are, on average, two days less absent. This result is robust to the inclusion of establishment fixed effects, however, it becomes smaller and statistically insignificant when we apply employee fixed effects.[14] With respect to unpaid overtime, the analyses show that employees with a higher commitment of one standard deviation work between 0.07 and 0.2 hours per week more overtime compared to their counterparts with lower affective commitment. For both effort proxies, the coefficients correspond to about a 10% higher effort provision for a one standard deviation higher affective commitment compared to the respective mean values (see table 2.6 in the appendix to this chapter).

[14] This may be due to the fact that affective commitment is rather stable over time such that there is little within person variation: The Pearson correlation coefficient is 0.65 ($p<0.0001$) for affective commitment in t and $t+1$ and 0.60 ($p<0.0001$) for affective commitment in t and $t+2$. Moreover, measurement error may lead to attenuation bias.

Table 2.2: Sick days and unpaid overtime

Dependent variable	Sick days$_t$				Unpaid overtime$_t$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Commitment$_t$ (std.)	-2.011***	-1.897***	-1.793***	-0.998	0.192***	0.065**	0.105***	0.014
	(0.234)	(0.252)	(0.267)	(0.697)	(0.031)	(0.029)	(0.031)	(0.041)
Constant	11.974***	18.257***	18.364***	7.088	0.618***	-0.381*	-0.961**	0.657
	(0.342)	(1.496)	(4.449)	(5.000)	(0.045)	(0.198)	(0.442)	(0.535)
Observations	14,930	14,340	14,340	14,340	14,898	14,302	14,302	14,302
Number of clusters	1,166	1,150	1,150	1,150	1,166	1,149	1,149	1,149
R-squared	0.007	0.046	0.143	0.016	0.005	0.082	0.207	0.010
Employee & establ. controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Establishment fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
Employee fixed effects	No	No	No	Yes	No	No	No	Yes

Notes: Robust standard errors clustered on establishment level in parentheses. Control variables on employee level include: blue collar, supervisory position, part time, female, secondary and tertiary education, age, limited work contract, marital status, household size, and year dummies. Control variables on establishment level include: industry, region, establishment size, ownership structure, and independent establishment.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

2.4.3 Predicting Wage Growth

In the following section, we study the extent to which affective commitment as measured in period t predicts actual wage growth between t and t+1. Again, note that t either refers to the first wave in 2012/13 or the second wave in 2014/15 and t+1 to the second wave in 2014/15 or the third wave in 2016/17 respectively. Hence, wage growth is always calculated over a period of two years. Recall that without information on the employee's bargaining power, our model makes no prediction on the sign of the slope of the conditional expectation function of wage growth between t and t+1 as a function of affective commitment γ as measured in t. However, it predicts that the slope should be negative when we condition on work effort a

$$\frac{\partial E\left[\Delta\,|\gamma, a\right]}{\partial \gamma} < 0.$$

As a first step, we descriptively explore the connection between affective commitment in period t and wage growth between t and t+1. Figure 2.1 shows mean wage growth when using a median split of all workers in the sample by their level of affective commitment, both pooled across all waves as well as separately for wage growth from 2012/13 to 2014/15 and 2014/15 to 2016/17. The figure already indicates a sizeable compensating wage differential effect: Employees with above median levels of affective commitment exhibit a substantially lower wage growth.

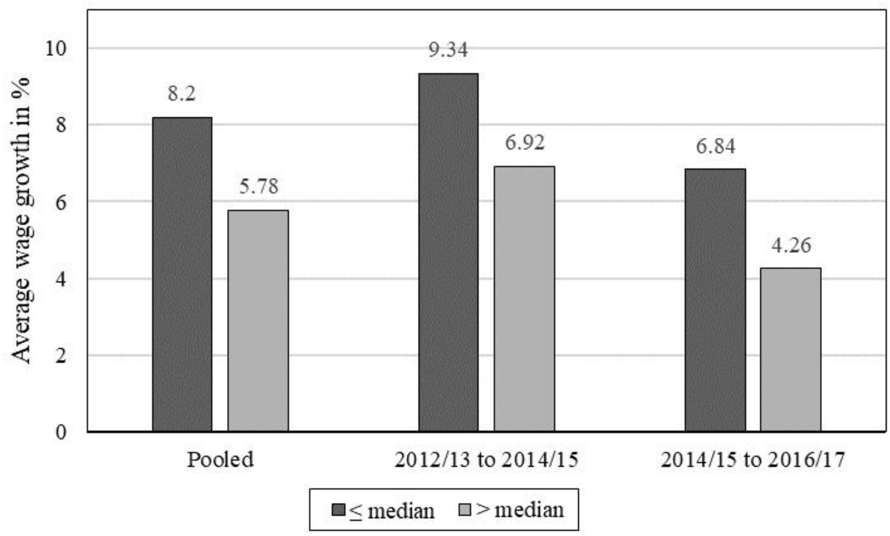

Figure 2.1: Wage growth for employees by degree of affective commitment

The corresponding regression results are reported in table 2.3. As before, we first include employee and establishment controls, before we show the results with establishment and employee fixed effects. In the specifications reported in columns (5) to (8), we additionally control for our two proxies for work effort (sick days and unpaid overtime).

First, note that the coefficient of affective commitment is negative in all specifications, indicating that employees with higher levels of affective commitment experience lower wage growth. Hence, the compensating wage differential effect seems to dominate the motivation effect. Second, the coefficient becomes more negative and remains (weakly) statistically significant throughout all specifications when we control for effort proxies. This result is in line with the idea that the conditional expectation function is downward sloping in affective commitment conditional on effort. The point estimates indicate that a person with a one standard deviation higher affective commitment faces a 1 to almost 3 percentage points lower wage growth. As average wage growth between two waves in the sample is about 7 percent, this constitutes a sizeable effect of about 12 to 40% lower wage growth for such employees.[15]

[15]Work engagement is often used as an alternative measure of effort in the literature. As a robustness check, table 2.7 in the appendix to this chapter uses work engagement as an additional control variable when regressing wage growth on commitment. In the LPP, we operationalize work engagement with the nine-item short scale of the Utrecht Work Engagement Scale (Schaufeli et al. 2002), measured on a five-point Likert scale. The results remain robust and become even slightly more significant, but we caution that some of this may be due to correlated measurement error in the two constructs. As an additional falsification check, table 2.8 in the appendix to this chapter regresses wage growth on work engagement instead of commitment. Even though these two measures are highly correlated (Pearson correlation coefficient: 0.49, $p<0.0001$), all regression coefficients for engagement are statistically insignificant showing that affective commitment rather than work engagement is driving our results.

Table 2.3: Wage growth and commitment

Dependent variable	Hourly wage growth $\triangle$ in % between t and $t+1$ (wins.)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Commitment_t (std.)	-1.231***	-0.897**	-0.757	-2.330	-1.284***	-0.940***	-0.871*	-2.786*
	(0.366)	(0.359)	(0.494)	(1.623)	(0.361)	(0.356)	(0.491)	(1.633)
Sick days_t					0.008	0.008	0.019	0.033
					(0.019)	(0.018)	(0.022)	(0.036)
Unpaid overtime_t					0.144	0.189*	0.202	-0.218
					(0.103)	(0.107)	(0.139)	(0.392)
Constant	8.112***	7.953***	5.597	8.875	7.850***	7.932***	5.690	9.180
	(0.489)	(1.857)	(9.703)	(14.166)	(0.506)	(1.907)	(9.814)	(14.733)
Observations	3,013	2,983	2,983	2,983	2,982	2,953	2,953	2,953
Number of clusters	574	571	571	571	574	571	571	571
R-squared (within)	0.009	0.040	0.208	0.057	0.009	0.041	0.208	0.059
Employee & establ. controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Establishment fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
Employee fixed effects	No	No	No	Yes	No	No	No	Yes

Notes: Robust standard errors clustered on establishment level in parentheses. Control variables on employee level include: blue collar, supervisory position, part time, female, secondary and tertiary education, age, limited work contract, marital status, household size, and year dummies. Control variables on establishment level include: industry, region, establishment size, ownership structure, and independent establishment.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

2.4.4 Job Search and Turnover

In this section, we present empirical evidence for our prediction with respect to job search behavior and outcomes of the search process. We estimate probit regressions to study the relationship between commitment, our effort proxies, satisfaction with pay in period *t,* and the propensity to engage in active job search, receipts of external offers, and realized voluntary turnover in *t+1.* Table 2.4 reports marginal effects for the three different dependent variables.

Specifications (1) and (2) show that more committed employees indeed exhibit a lower probability to actively engage in search for alternative employment opportunities in the future. The coefficient is robust to the inclusion of additional explanatory variables and indicates that employees with a one standard deviation higher commitment have, on average, a 5 to 7 percentage points lower propensity to actively search for alternative employment offers. Again, this is a sizeable difference: As the baseline likelihood that somebody actively looks for a new job is 25% in the sample, this likelihood is, thus, nearly 30% lower for employees with an affective commitment that is one standard deviation above the mean.

As a potential consequence, we also find that employees with higher commitment have a lower likelihood to receive external job offers. Both specifications (3) and (4) show that employees with affective commitment that is one standard deviation above the mean, have around 2 percentage points lower propensity to receive an external offer. Given that the average likelihood to receive an external offer within our dataset is around 9%, this corresponds to a reduction of around 20%.

Furthermore, with respect to realized voluntary turnover, we consistently find that employees with higher levels of commitment exhibit a significantly lower probability to quit their current job voluntarily. The average turnover rate in our sample is 2%, which is reduced by around 1 percentage point, i.e., by 40 to 50%, for employees with an affective commitment that is one standard deviation above the mean.[16]

[16] As previous research in psychology has shown that personality traits are predictive of turnover decisions (see e.g., Zimmerman 2008) and may be correlated with affective commitment, we also include the Big5 personality traits as additional control variables in table 2.10 in the appendix to this chapter. All of our results remain robust.

Table 2.4: Job search, turnover, and commitment

Dependent variable	Active job search$_{t+1}$		External offer$_{t+1}$		Voluntary turnover$_{t+1}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Commitment$_t$ (std.)	-0.067***	-0.053***	-0.018***	-0.015***	-0.010***	-0.008***
	(0.013)	(0.013)	(0.004)	(0.005)	(0.001)	(0.001)
Sick days$_t$		0.001		0.000		0.000
		(0.001)		(0.000)		(0.000)
Unpaid overtime$_t$		-0.006		0.002**		0.001**
		(0.004)		(0.001)		(0.000)
Satisfaction with pay$_t$ (std.)		-0.037***		-0.010*		-0.005***
		(0.014)		(0.005)		(0.002)
Observations	1,292	1,255	3,908	3,358	3,701	3,612
Number of clusters	481	478	623	606	601	598
Pseudo R-squared	0.0814	0.0887	0.0992	0.0998	0.174	0.194

Notes: Robust standard errors clustered on establishment level in parentheses. Marginal effects reported. All specifications include employee and establishment controls. Control variables on employee level include: blue collar, supervisory position, part time, female, secondary and tertiary education, age, limited work contract, marital status, household size, and year dummies. Control variables on establishment level include: industry, region, establishment size, ownership structure, and independent establishment. *** p<0.01, ** p<0.05, * p<0.1.

2.4.5 Wage Growth with External Offer

Finally, we study the relationship between affective commitment in *t* and hourly wage growth between *t* and *t+1,* conditional on having obtained an external offer in *t+1.* In other words, we investigate to what extent the wage increase that an employee has obtained after an external offer depends on the employee's affective commitment. Recall that our formal model predicts that external offers should be associated with higher wage increases for more emotionally attached workers.

We regress the hourly wage growth between *t* and *t+1* on commitment in *t*, a dummy variable indicating whether an employee received an outside offer in *t+1*, and the interaction of the two. In specifications (1) to (3), we stepwise include employee and establishment controls, as well as establishment fixed effects. In specifications (4) to (6), we additionally control for our effort proxies. First of all, we find that when an employee received an external offer, the associated wage growth with his incumbent employer is around four percentage points higher compared to employees without an external offer. As average wage growth within our dataset is around 7 percent, this corresponds to between 55 and 63% higher wage growth for employees that have received an external offer. The coefficient of the interaction term with affective commitment has the expected sign, indicating that highly committed employees are able to reap some of the value they generate for the employer in wage negotiations when they have an external offer. However, the interaction term is significant in only one specification.[17]

[17]Table 2.9 in the appendix to this chapter shows the relationship between affective commitment and the wages offered by an external employer. While we only have very few observations (around 100) to study this question, the point estimates indicate that conditional on having obtained an external offer, employees with higher commitment get offered significantly higher wages on the market compared to candidates with lower affective commitment.

Table 2.5: Wage growth and commitment with external offer

Dependent variable	Hourly wage growth △ in % between *t* and *t+1* (wins.)					
	(1)	(2)	(3)	(4)	(5)	(6)
Commitment_t (std.)	-1.310***	-1.000***	-1.006**	-1.285***	-0.974***	-1.062**
	(0.374)	(0.365)	(0.501)	(0.374)	(0.367)	(0.501)
$\text{External offer}_{t+1}$	4.316***	3.884***	3.873**	4.456***	3.960***	3.775**
	(1.240)	(1.239)	(1.641)	(1.245)	(1.245)	(1.681)
Commitment_t (std.) * $\text{External offer}_{t+1}$	1.579	1.638	2.727*	1.008	1.079	2.254
	(1.218)	(1.193)	(1.568)	(1.210)	(1.189)	(1.584)
Sick days_t				0.009	0.008	0.019
				(0.019)	(0.018)	(0.022)
Unpaid overtime_t				0.109	0.169	0.179
				(0.101)	(0.104)	(0.134)
Constant	7.841***	7.813***	6.073	7.560***	7.745***	6.106
	(0.502)	(1.865)	(9.590)	(0.514)	(1.914)	(9.711)
Observations	3,013	2,983	2,983	2,982	2,953	2,953
Number of clusters	574	571	571	574	571	571
R-squared (within)	0.014	0.044	0.212	0.014	0.044	0.211
Employee & establ. controls	No	Yes	Yes	No	Yes	Yes
Establishment fixed effects	No	No	Yes	No	No	Yes

Notes: Robust standard errors clustered on establishment level in parentheses. Control variables on employee level include: blue collar, supervisory position, part time, female, secondary and tertiary education, age, limited work contract, marital status, household size, and year dummies. Control variables on establishment level include: industry, region, establishment size, ownership structure, and independent establishment. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

2.5 Conclusion

In this chapter, we studied a stylized theoretical model to analyze the effect of emotional attachment of an employee to the employer on wage bargaining and search behavior. The model predicted several patterns that we investigated empirically using a novel, representative matched employer-employee panel dataset. In particular, the model predicts that higher affective commitment has two countervailing effects. On the one hand, the employer can exploit the emotional attachment by offering lower wage growth. On the other hand, an agent with a higher emotional attachment exerts higher efforts and is thus more valuable for the employer. The employee's bargaining position in the wage negotiations determines which of the two effects dominates.

Previous literature has identified on-the-job search and subsequent wage bargaining (including external offers) with the incumbent employer as the main source for rapid wage growth (Greenwald 1986; Golan 2005; Barron, Berger, and D. Black 2006; Yamaguchi 2010; Bagger et al. 2014). Our model integrates identification with the incumbent employer as a non-monetary determinant of employee's utility. We predict that the employee's emotional attachment to the employer, thus, affects effort choice and that highly committed employees will, on average, experience lower wage growth. Furthermore, a more committed employee will be less willing to invest in costly search for alternative employment opportunities, therefore the employee will be less likely to receive external offers, and finally have a lower tendency to switch employers. However, when highly committed employees have obtained an external offer from an outside employer, they tend to be able to negotiate higher wages with their incumbent employer as they are more valuable to them.

In our empirical analysis, we find that a widely applied, short survey scale measuring an employee's "affective commitment" towards the employer has substantial predictive power for on-the-job search and future wage growth. Our empirical results show that more committed workers experience sizeably lower wage growth in subsequent years compared to less committed workers. We additionally find evidence for lower investments into on-the-job search by high commitment workers, and a lower likelihood of receiving an external offer and leaving the incumbent employer. In line with our model, our data indicate that conditional on having obtained an external offer, employees who reported a higher commitment with their incumbent employer, can overcome this negative "compensating wage differential" effect.

Of course, we have to caution that affective commitment is not exogenously assigned in our dataset. It will be an important endeavor for future work to study the dynamic interplay between wages and affective commitment in more detail.

2.6 Appendix to Chapter 2

Theoretical Proofs

Proof of Proposition 2:

The generalized Nash bargaining solution is obtained by maximizing the Nash product

$$\max_{w_1}\left((1-\gamma)\,w_1+\gamma K(a)-c(a)-u_M\right)^{\lambda}\left(K(a)-w_1\right)^{1-\lambda}$$

with first order condition

$$\begin{aligned}0 &= \lambda(1-\gamma)\left((1-\gamma)\,w_1+\gamma K(a)-c(a)-u_M\right)^{\lambda-1}\left(K(a)-w_1\right)^{1-\lambda}\\ &\quad-\left((1-\gamma)\,w_1+\gamma K(a)-c(a)-u_M\right)^{\lambda}(1-\lambda)\left(K(a)-w_1\right)^{-\lambda}\end{aligned}$$

$$\Leftrightarrow w_1=\lambda K(a)+(1-\lambda)\frac{u_M-(\gamma K(a)-c(a))}{(1-\gamma)}$$

such that

$$\Delta(\gamma,a)=\frac{w_1}{w_0}=\frac{\lambda K(a)+(1-\lambda)\frac{u_M-(\gamma K(a)-c(a))}{(1-\gamma)}}{w_M}.$$

When keeping efforts fixed,

$$\begin{aligned}\frac{\partial\Delta(\gamma,a)}{\partial\gamma} &= (1-\lambda)\frac{-K(a)(1-\gamma)+(u_M-(\gamma K(a)-c(a)))}{w_M(1-\gamma)^2}\\ &= (1-\lambda)\frac{u_M-(K(a)-c(a))}{w_M(1-\gamma)^2}<0.\end{aligned}$$

When efforts are endogenous, then

$$\frac{\partial\Delta(\gamma,a(\gamma))}{\partial\gamma} = \frac{\lambda K'(a(\gamma))\,a'(\gamma)}{w_M}$$

$$+\frac{(1-\lambda)\frac{\left(-\left(K(a(\gamma))+\left(\gamma K'(a(\gamma))-c'(a(\gamma))\right)a'(\gamma)\right)\right)(1-\gamma)+(u_M-(\gamma K(a(\gamma))-c(a(\gamma))))}{(1-\gamma)^2}}{w_M}$$

and using that $\gamma K'(a)-c'(a)=0$ thus

$$\begin{aligned}\frac{\partial\Delta(\gamma,a(\gamma))}{\partial\gamma} &= \frac{\lambda K'(a(\gamma))\,a'(\gamma)+(1-\lambda)\frac{u_M-(K(a(\gamma))-c(a(\gamma)))}{(1-\gamma)^2}}{w_M}\\ &= \frac{\lambda}{w_M}\underbrace{K'(a(\gamma))\,a'(\gamma)}_{>0}+\underbrace{(1-\lambda)\frac{u_M-(K(a)-c(a))}{w_M(1-\gamma)^2}}_{<0}.\end{aligned}$$

■

Proof of Proposition 3:

To see that the wage increase due to an external offer is increasing in γ consider

$$\begin{aligned}&E\left[\Delta\right| d=1] - E\left[\Delta\right| d=0] \\ &= \frac{(1-\lambda)}{(1-\gamma)}\left(\frac{E\left[u_O\right| u_O \leq K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right)] - u_M}{w_M}\right)\end{aligned}$$

and note that the first derivative w.r.t. γ is

$$\begin{aligned}&\frac{(1-\lambda)}{(1-\gamma)^2}\left(\frac{E\left[u_O\right| u_O \leq K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right)] - u_M}{w_M}\right) \\ &+ \frac{(1-\lambda)}{(1-\gamma)\,w_M}\left(\frac{\partial E\left[u_O\right| u_O \leq K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right)]}{\partial\gamma}\right) \quad > \quad 0.\end{aligned}$$

To determine the employee's search efforts, consider his choice problem

$$\max_p p \cdot E\left[\Delta u\right] - k\left(p\right)$$

with first order condition $E\left[\Delta u\right] - k'\left(p\right) = 0$ such that p is strictly increasing in $E\left[\Delta u\right]$ by the implicit function theorem. Recall that is

$$\begin{aligned}E\left[\Delta u\right] = &\int_{u_M}^{K(a(\gamma))-c(a(\gamma))} (1-\lambda)\left(u_O - u_M\right) f\left(u_O\right) du_O \\ &+ \int_{K(a(\gamma))-c(a(\gamma))}^{\infty} \left(u_O - (1-\lambda)\,w_M - \lambda\left(K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right)\right)\right) f\left(u_O\right) du_O.\end{aligned}$$

Now note that by Leibniz' integral rule we have that

$$\begin{aligned}\frac{\partial E\left[\Delta u\right]}{\partial\gamma} &= (1-\lambda)\left(K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right) - u_M\right) f\left(K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right)\right) \tfrac{\partial(K(a(\gamma))-c(a(\gamma)))}{\partial\gamma} \\ &- \left(K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right) - (1-\lambda)\,u_M - \lambda\left(K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right)\right)\right) f\left(K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right)\right) \\ &\tfrac{\partial(K(a(\gamma))-c(a(\gamma)))}{\partial\gamma} + \int_{K(a(\gamma))-c(a(\gamma))}^{\infty}\left(-\lambda\tfrac{\partial(K(a(\gamma))-c(a(\gamma)))}{\partial\gamma} f\left(u_O\right)\right) du_O \\ &= \int_{K(a(\gamma))-c(a(\gamma))}^{\infty}\left(-\lambda\tfrac{\partial(K(a(\gamma))-c(a(\gamma)))}{\partial\gamma} f\left(u_O\right)\right) du_O\end{aligned}$$

which is strictly negative as

$$\begin{aligned}\frac{\partial\left(K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right)\right)}{\partial\gamma} &= \left(K'\left(a\left(\gamma\right)\right) - c'\left(a\left(\gamma\right)\right)\right) a'\left(\gamma\right) \\ &> \left(\gamma K'\left(a\left(\gamma\right)\right) - c'\left(a\left(\gamma\right)\right)\right) a'\left(\gamma\right) = 0\end{aligned}$$

by equation (2.1).

Finally, the likelihood that the employee leaves the firm is

$$\Pr\left(u_O > K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right)\right) = 1 - F\left(K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right)\right)$$

such that

$$\frac{\partial \Pr\left(u_O > K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right)\right)}{\partial \gamma} = -f\left(K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right)\right) \tfrac{\partial\left(K\left(a\left(\gamma\right)\right) - c\left(a\left(\gamma\right)\right)\right)}{\partial \gamma} < 0.$$

$\blacksquare$

Summary Statistics

Table 2.6: Summary statistics

	2012/13				2014/15			
	Obs.	Mean	Median	SD	Obs.	Mean	Median	SD
Commitment	5825	3.75	3.83	.892	5187	3.69	3.83	.886
Engagement	5715	3.79	3.89	.797	5118	3.71	3.83	.82
Job satisfaction	5917	7.59	8	1.74	5246	7.49	8	1.7
Pay satisfaction	5914	6.68	7	2.16	5245	6.83	7	2.05
Sick days	5856	11.9	4	24.7	5185	12.8	5	25.4
Unpaid overtime	5848	.616	0	2.8	5172	.85	0	3.32
Hourly wage growth $\triangle$ in % btw. t and $t+1$ (wins.)	1782	8.20	6.67	19.8	1404	5.61	3.93	18.2
Hourly wage	4944	20.2	18.4	9.45	4518	21.7	20.1	9.91
Active job search	754	.241	0	.428	587	.256	0	.437
Voluntary turnover	2215	.0185	0	.135	1716	.0332	0	.179
External offer	2246	.0712	0	.257	1765	.11	0	.313
External offer wage	0	.	.	.	333	5339	3800	7640
Control variables:								
Blue collar (1/0)	5920	.586	1	.493	5247	.614	1	.487
Supervisory position (1/0)	5917	.305	0	.461	5241	.299	0	.458
Part time (1/0)	5920	.116	0	.321	5247	.134	0	.34
Female (1/0)	5920	.273	0	.446	5247	.284	0	.451
Secondary education:								
None (1/0)	5907	.00593	0	.0768	5235	.00439	0	.0661
Certificate of Secondary Education (1/0)	5907	.263	0	.44	5235	.228	0	.42
General Cert. of Secondary Education (1/0)	5907	.433	0	.495	5235	.442	0	.497
Adv. Technical College Entrance Qual. (1/0)	5907	.101	0	.301	5235	.105	0	.307
University-entrance Diploma (1/0)	5907	.19	0	.392	5235	.212	0	.409
Other (1/0)	5907	.00796	0	.0889	5235	.00821	0	.0903
Tertiary education:								
None (1/0)	5915	.025	0	.156	5242	.0223	0	.148
Apprenticeship (1/0)	5915	.497	0	.5	5242	.477	0	.5
Vocational training (1/0)	5915	.104	0	.306	5242	.0981	0	.297
Master craftsman (1/0)	5915	.202	0	.402	5242	.209	0	.406
University of Applied Sciences (1/0)	5915	.083	0	.276	5242	.0881	0	.284
University (1/0)	5915	.0849	0	.279	5242	.102	0	.303
Other (1/0)	5915	.00406	0	.0636	5242	.00305	0	.0552

continued on next page

	2012/13				2014/15			
	Obs.	Mean	Median	SD	Obs.	Mean	Median	SD
Age class:								
<25 years (1/0)	5920	.051	0	.22	5247	.0305	0	.172
25-39 years (1/0)	5920	.231	0	.421	5247	.212	0	.409
40-54 years (1/0)	5920	.544	1	.498	5247	.522	1	.5
>55 years (1/0)	5920	.173	0	.378	5247	.234	0	.423
Limited work contract (1/0)	5911	.0641	0	.245	5240	.0429	0	.203
Marital status (1/0)	5910	.84	1	.366	5241	.847	1	.36
Household size	5911	2.81	3	1.23	5243	2.77	3	1.2
Industry:								
Metalworking and electronic industries (1/0)	5920	.315	0	.464	5144	.316	0	.465
Further manufacturing industries (1/0)	5920	.37	0	.483	5144	.372	0	.483
Retail and transport (1/0)	5920	.111	0	.314	5144	.111	0	.315
Services for firms (1/0)	5920	.138	0	.345	5144	.128	0	.334
Information and communications services (1/0)	5920	.0667	0	.25	5144	.0737	0	.261
Establishment size:								
50-99 employees (1/0)	5920	.155	0	.362	5144	.115	0	.32
100-249 employees (1/0)	5920	.237	0	.425	5144	.261	0	.439
250-499 employees (1/0)	5920	.263	0	.44	5144	.254	0	.435
500 and more employees (1/0)	5920	.344	0	.475	5144	.37	0	.483
Ownership structure:								
Family/Founder (1/0)	5905	.48	0	.5	5125	.431	0	.495
Management (1/0)	5905	.138	0	.345	5125	.179	0	.384
Investor (1/0)	5905	.0757	0	.265	5125	.073	0	.26
Shareholders (1/0)	5905	.0882	0	.284	5125	.122	0	.327
Public (1/0)	5905	.0171	0	.13	5125	.022	0	.147
Other (1/0)	5905	.20	0	.4	5125	.172	0	.378
Independent establishment (1/0)	5863	.698	1	.459	5139	.689	1	.463
Region:								
North (1/0)	5920	.172	0	.378	5144	.16	0	.367
East (1/0)	5920	.256	0	.436	5144	.27	0	.444
South (1/0)	5920	.253	0	.435	5144	.257	0	.437
West (1/0)	5920	.318	0	.466	5144	.313	0	.464

Additional Regressions

Table 2.7: Wage growth and commitment with control for engagement

Dependent variable	Hourly wage growth △ in % between t and $t+1$ (wins.)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Commitment_t (std.)	-1.736***	-1.319***	-1.167*	-2.461	-1.800***	-1.373***	-1.296**	-2.837
	(0.442)	(0.429)	(0.594)	(1.738)	(0.441)	(0.427)	(0.594)	(1.750)
Engagement_t (std.)	0.800**	0.736*	0.701	1.696	0.854**	0.784**	0.783	1.231
	(0.393)	(0.386)	(0.479)	(1.525)	(0.393)	(0.386)	(0.479)	(1.539)
Sick days_t					0.011	0.011	0.022	0.022
					(0.019)	(0.019)	(0.023)	(0.035)
Unpaid overtime_t					0.125	0.173	0.211	0.015
					(0.108)	(0.110)	(0.144)	(0.439)
Constant	8.074***	8.231***	5.484	8.519	7.809***	8.140***	5.521	8.458
	(0.492)	(1.857)	(9.751)	(14.402)	(0.509)	(1.911)	(9.856)	(15.064)
Observations	2,957	2,928	2,928	2,928	2,930	2,902	2,902	2,902
Number of clusters	572	569	569	569	572	569	569	569
R-squared (within)	0.011	0.043	0.210	0.057	0.011	0.043	0.211	0.057
Employee & establ. controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Establishment fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
Employee fixed Effects	No	No	No	Yes	No	No	No	Yes

Notes: Robust standard errors clustered on establishments in parentheses. Control variables on employee level include: blue collar, supervisory position, part time, female, secondary and tertiary education, age, limited work contract, marital status, household size, and year dummies. Control variables on establishment level include: industry, region, establishment size, ownership structure, and independent establishment. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.8: Wage growth and engagement

Dependent variable	Hourly wage growth △ in % between *t* and *t+1* (wins.)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Engagement$_t$ (std.)	-0.093	0.099	0.166	0.870	-0.057	0.127	0.201	0.331
	(0.326)	(0.325)	(0.400)	(1.448)	(0.324)	(0.323)	(0.397)	(1.471)
Sick days$_t$					0.014	0.013	0.024	0.017
					(0.019)	(0.019)	(0.023)	(0.034)
Unpaid overtime$_t$					0.091	0.158	0.199	0.018
					(0.108)	(0.111)	(0.142)	(0.444)
Constant	7.946***	8.690***	5.615	8.643	7.652***	8.570***	5.702	8.895
	(0.484)	(1.839)	(9.757)	(14.507)	(0.503)	(1.896)	(9.859)	(15.162)
Observations	2,977	2,947	2,947	2,947	2,949	2,920	2,920	2,920
Number of clusters	573	570	570	570	573	570	570	570
R-squared (within)	0.005	0.041	0.209	0.053	0.005	0.041	0.209	0.052
Employee & establ. controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Establishment fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
Employee fixed effects	No	No	No	Yes	No	No	No	Yes

Notes: Robust standard errors clustered on establishments in parentheses. Control variables on employee level include: blue collar, supervisory position, part time, female, secondary and tertiary education, age, limited work contract, marital status, household size, and year dummies. Control variables on establishment level include: industry, region, establishment size, ownership structure, and independent establishment. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.9: External offer wages and commitment

Dependent variable	External offer wage$_{t+1}$		
	(1)	(2)	(3)
Commitment$_t$ (std.)	1,005.516***	548.740*	523.558*
	(247.084)	(293.610)	(309.955)
Sick days$_t$			-10.119
			(7.512)
Unpaid overtime$_t$			28.508
			(56.422)
Constant	5,713.998***	1,409.745	1,621.968
	(444.321)	(941.325)	(998.849)
Observations	716	701	682
Number of clusters	431	425	416
R-squared (within)	0.019	0.198	0.195
Employee & establ. controls	No	Yes	Yes

Notes: Robust standard errors clustered on establishments in parentheses. Control variables on employee level include: blue collar, supervisory position, part time, female, secondary and tertiary education, age, limited work contract, marital status, household size, and year dummies. Control variables on establishment level include: industry, region, establishment size, ownership structure, and independent establishment. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.10: Job search, turnover, and commitment with control for personality traits

Dependent variable	Active job search$_{t+1}$		External offer$_{t+1}$		Voluntary turnover$_{t+1}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Commitment$_t$ (std.)	-0.072***	-0.057***	-0.020***	-0.017***	-0.010***	-0.008***
	(0.013)	(0.013)	(0.004)	(0.005)	(0.001)	(0.001)
Sick days$_t$		0.001		0.000		0.000
		(0.001)		(0.000)		(0.000)
Unpaid overtime$_t$		-0.007*		0.002*		0.001
		(0.004)		(0.001)		(0.000)
Satisfaction with pay$_t$ (std.)		-0.038***		-0.008		-0.005***
		(0.015)		(0.005)		(0.002)
Extraversion$_t$ (std.)	0.010	0.009	0.010**	0.012**	0.003**	0.003*
	(0.014)	(0.014)	(0.004)	(0.005)	(0.002)	(0.001)
Conscientiousness$_t$ (std.)	-0.000	-0.002	0.001	0.000	0.000	0.001
	(0.015)	(0.015)	(0.004)	(0.005)	(0.002)	(0.002)
Neuroticism$_t$ (std.)	-0.010	-0.014	-0.003	-0.003	-0.000	0.000
	(0.013)	(0.013)	(0.004)	(0.005)	(0.001)	(0.001)
Openness$_t$ (std.)	0.042***	0.039***	0.013***	0.012**	0.003	0.002
	(0.014)	(0.014)	(0.005)	(0.005)	(0.002)	(0.002)
Agreeableness$_t$ (std.)	-0.017	-0.019	-0.003	-0.002	0.003*	0.003**
	(0.013)	(0.013)	(0.004)	(0.005)	(0.002)	(0.002)
Observations	1,248	1,214	3,767	3,250	3,584	3,500
Number of clusters	475	471	617	600	597	594
Pseudo R-squared	0.0912	0.0984	0.108	0.109	0.190	0.208

Notes: Robust standard errors clustered on establishments in parentheses. All specifications include employee and establishment controls. Control variables on employee level include: blue collar, supervisory position, part time, female, secondary and tertiary education, age, limited work contract, marital status, household size, and year dummies. Control variables on establishment level include: industry, region, establishment size, ownership structure, and independent establishment. *** p<0.01, ** p<0.05, * p<0.1.

Chapter 3

Negative Side Effects of Affirmative Action[1]

3.1 Introduction

Despite numerous endeavours to promote the career advancement of females and ethnic minorities, these groups are still underrepresented in leading positions in management (European Commission 2016; Beech et al. 2017; 2020 Women on Boards 2018) and academia (American Economic Association 2018; Lundberg and Stearns 2019). Research reveals two important channels that explain the underrepresentation of females and ethnic minorities in leading positions: differences in career-relevant behavior (Niederle and Versterlund 2007; Croson and Gneezy 2009; Coffman 2014; Babcock, Recalde, et al. 2017) and discrimination (Beaurain and Masclet 2016; Bertrand and Duflo 2017; Sarsons 2017; Mengel, Sauermann, and Zölitz 2018). As a mean to counteract this underrepresentation, some countries and organizations implement affirmative action policies in the form of quota regulations.[2] Supporting the introduction of such intervention, several studies show that quotas are effective at reducing differences in career-relevant behavior and that quotas therefore increase the representation of the affirmed group without harming efficiency (Balafoutas and Sutter 2012; Niederle, Segal, and Vesterlund 2013; A. Banerjee et al. 2017).

While designed to promote an important goal, affirmative action may also entail negative effects. In this chapter, we provide evidence for negative side effects of quota interventions on subjective peer-reviews. Distortions in subjective performance evaluation are considered a crucial facilitator for discrimination (Nieva and Gutek 1980; Borgida and Fiske 2008). As peer-reviews are especially relevant for career success in management and academia (Edwards and Ewen 1996;

[1] This chapter is joint work with Marina Schröder and based upon Petters and Schröder (2019)

[2] Countries that have passed laws to implement gender quotas for management positions include Norway, Spain, Italy, Belgium, France, and Germany. Sowell (2004) and Bagde, Epple, and L. Taylor (2016) present examples for quotas based on ethnic background. Wallon, Bendiscioli, and Garfinkel (2015) provide an overview for the use of quota interventions in academia.

Bracken 2001; L. K. Johnson 2004), distorted evaluations may have long-term detrimental effects.[3] Our results show that quotas lead to distortions in peer-reviews of the affirmed group and therefore may actually increase discrimination against the group that is supposed to benefit from the quota. Furthermore, the anticipation of distortions in performance evaluation may mitigate the positive effect of quotas on career-relevant behavior of the affirmed group (Leibbrandt and List 2018; Leibbrandt, Wang, and Foo 2018). Finally, distortions in peer-review may affect the self-image and actual performance of affirmed individuals in an unfavorable way (Turner and Pratkanis 1995; Heilman and Barocas Alcott 2001).

Previous research on the effect of affirmative action on performance evaluation is mixed and has focused on evaluations provided by bystanders or superiors. On the one hand, increased exposure to competent affirmed individuals due to the introduction of quotas is shown to reduce biases in performance evaluation (Beaman et al. 2009; Baskaran and Hessami 2018). On the other hand, affirmative action is shown to have a negative effect on the evaluation of successful affirmed individuals because success is overly ascribed to affirmative action rather than ability (Heilman, C. Block, and J. Lucas 1992; Heilman, C. Block, and Stathatos 1997; Bijkerk et al. 2018; M. Gürtler and O. Gürtler 2019). In a recent closely related paper on the effect of quotas on peer-reviews in a gender context, Leibbrandt, Wang, and Foo (2018) show that women are significantly more likely to be a victim of sabotage (intentional misreporting of objective performance measures through peers) whenever a female quota is implemented. While Leibbrandt, Wang, and Foo (2018) provide first indications that quotas may have an impact on peer-review behavior, we know little about the mechanism driving this effect or about the generalizability of this result beyond the gender context.

Quotas can impact peer-review behavior for different reasons. First, affirmative action interventions substantially change the competitive structure of a tournament (Schotter and Weigelt 1992; Holzer and Neumark 2000; Franke 2012; Calsamiglia, Franke, and Rey-Biel 2013; Chowdhury and O. Gürtler 2015). Enhanced competition among affirmed individuals due to a quota is likely to lead to distortions in peer-reviews as affirmed individuals have an incentive to provide less favorable peer-reviews to affirmed peers.[4] Second, quotas increase the winning probabilities of affirmed individuals at the cost of non-affirmed peers. Inequity-averse individuals (affirmed and non-affirmed) may react to a quota by providing distorted peer-reviews favoring non-affirmed individuals to counteract procedural unfairness (Konow 1996; Cappelen et al. 2007).

In our experiment, we randomly assign individuals to affirmed or non-affirmed types.[5] Within groups of four, participants compete for two prizes each by per-

[3] In management, peer-reviewing is widespread. The majority of companies listed in the Fortune 500 use peer-reviews as a tool for subjective performance evaluation of management positions (Edwards and Ewen 1996; Bracken 2001). In academia, peer-reviews are relevant for placement and tenure decisions, publication success, and research funding.

[4] Subjective peer-reviews are prone to - conscious or unconscious - biases. In settings without a quota, peer-reviews are shown to be sensitive to changes in the incentive structure (Carpenter, Matthews, and Schirm 2010; Rosaz and Villeval 2012; Balietti, Goldstone, and Helbing 2016). Harbring et al. (2007) show that sabotage behavior in a contest is affected by the symmetry of the tournament.

[5] Random assignment of a quota is an important difference between our experimental study

forming a creative real-effort task (Laske and Schröder 2016).[6] Prizes in the tournament are awarded according to subjective peer-reviews and - depending on the treatment - a quota. In treatments without a quota, the two prizes are rewarded to the two (out of four) participants with the highest score in the peer-review. In treatments with a quota, at least one of the two prizes has to be awarded to one of the two randomly determined affirmed type participants. As previous research reveals that the effect of affirmative action depends on its perceived justification (Harrison et al. 2006; Balafoutas, Davis, and Sutter 2016; Ip, Leibbrandt, and Vecci 2018), we conduct our experiment in two different settings. In the ex-ante equal setting, affirmed and non-affirmed individuals face the same procedure for working on the task. In the ex-ante unequal setting, affirmed individuals face procedural disadvantages that provide a possible justification for the introduction of a quota.

We find evidence for substantial biases in peer-reviews due to the introduction of a quota. First, quotas have an impact on the average level of peer-reviews provided. The observed level effects vary depending on the setting (ex-ante equal or ex-ante unequal) and thus seem to be related to the perceived procedural fairness of the quota. Second, quotas lead to substantial distortions in peer-reviews, such that under a quota, affirmed individuals receive significantly less favorable peer-reviews compared to non-affirmed peers with similar performance according to an independent measure. Importantly, we show that these distortions in peer-reviews are unrelated to procedural inequalities and are driven by reviews provided by affirmed individuals. Thus, it seems that distortions in peer-reviews are the result of enhanced competition among affirmed individuals due to the introduction of a quota. In a subsequent dictator game (Forsythe et al. 1994), we find that facing a quota in the previous tournament experiment significantly reduces subsequent altruistic behavior among affirmed individuals. Thus, we provide evidence for behavioral spillovers of quotas beyond the context in which they are implemented.

3.2 Experimental Design

In our experiment, participants face a tournament setting in which groups of four participants compete for two prizes. At the beginning of the experiment, we randomly assign participants to one of two types (yellow or green). Each group consists of two yellow and two green type participants. Each round of the tournament consists of two stages: a working stage and a peer-review stage. In the working stage, participants perform a creative real-effort task. The task

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and most previous experimental studies on the effect of quotas (see e.g. Niederle, Segal, and Vesterlund 2013; Leibbrandt, Wang, and Foo 2018). Randomly assigning the quota provides an advantage by allowing us to cleanly induce unequal opportunities and thus study the effect of justification of a quota. Furthermore, due to the random assignment of a quota, we can rule out that our findings are specific to certain groups, i.e., females or non-whites. Thus, our findings can also be applied to other types of quotas, i.e., quotas based on race or caste.

[6] The use of a creative task has two important advantages. First, the use of subjective performance evaluations is very natural in this context because by definition creative performance cannot be quantified through objective measures. Second, creative work is especially relevant in leading positions in management and in academia. Thus, creative work is highly relevant for the type of work in which quotas are often implemented.

consists of illustrating predefined objects using a given set of materials. It is a modified version of the task introduced by Laske and Schröder (2016). Participants receive a set of materials: one string, two O-rings, four wooden sticks, and twelve colored glass pebbles (see figure 3.1) and are asked to use these materials to illustrate specific objects, i.e., eyeglasses, a flower, and a car. Participants are instructed to take pictures of their illustrations using a special software and a pre-installed webcam. See figure 3.2 for examples of pictures of the illustrations and the appendix to this chapter for the experimental instructions. The time available in the working stage is restricted and depends on the treatment and the type of the participant (see figure 3.3 for an overview of our treatments). Within the limited time frame, participants can take as many pictures as they want. Once the time is up, participants choose one of these pictures to be payoff-relevant. All other pictures are deleted and not payoff-relevant.

Figure 3.1: Set of materials

In the peer-review stage of each tournament round, participants see the pictures of the illustrations created by their group members (including their own illustration) and are asked to rate the illustrations of their peers (participants did not rate their own illustrations) on a scale from 0.0 to 10.0 (0.0 being the worst rating and 10.0 being the best rating). Thus, in the peer-review stage, the illustrations of all participants are evaluated by the three other participants of each group. Prizes are awarded according to the mean rating from this peer-review and, if applicable, a quota. We conduct three rounds of the tournament without feedback and with random rematching within matching groups of eight participants between rounds.

Between treatments, we vary whether or not a quota is implemented. In treatments without a quota, the two participants with the highest and the second highest peer-review receive a prize independent of their type. In treatments with a quota, at least one of the two prizes is awarded to a participant of the affirmed type. Thus, in treatments involving a quota, the participant among the affirmed types with the highest mean rating from the peer-review receives a prize for sure (even if this participant is not among the two participants with the highest mean ratings) and the participant among the remaining three participants of either type with the highest mean rating from the peer-review receives a prize.

We consider two different settings in which quotas are implemented. In the ex-ante equal setting, all participants face the same procedure absent of a quota, which means that all types have five minutes to work on the task in each round. In the ex-ante unequal setting, one type faces procedural disadvantages by having a reduced working time of only 2 minutes and 30 seconds.

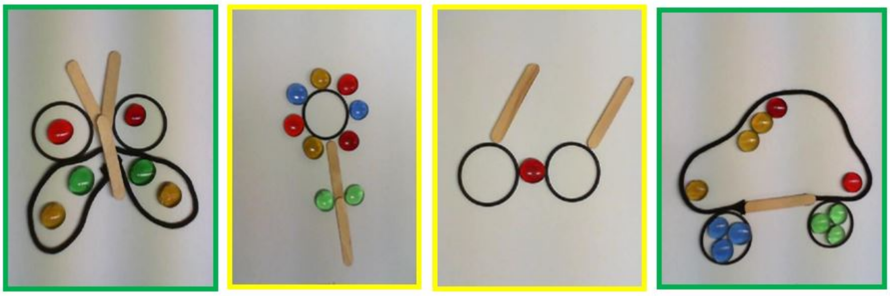


Figure 3.2: Examples of illustrations

When a quota is implemented in this setting, the type with the shorter working time is also affirmed. Figure 3.3 summarizes the treatments of our experiment. In the *Baseline_equal* treatment all participants face the same procedure and no quota is implemented. In the *Quota_equal* treatment, we implement a quota in a setting where all participants face the same procedure. In the *Baseline_unequal* treatment, one type faces reduced working time, but no quota is implemented. In the *Quota_unequal* treatment[7], the type that faces reduced working time is also affirmed by a quota.

The quota is implemented according to the randomly assigned type (yellow or green) and thus independent of any characteristics of the participants. In all treatments, we randomized whether green or yellow individuals are affirmed and/or face procedural disadvantages. In the peer-review stage, the pictures are shown in a randomized order. They have a colored frame, which indicates the type of the ratee (see frames in example pictures provided in figure 3.2). Thus, the types - but not the identity - of the other participants are common information.

Participants are only paid for one randomly determined round of the main experiment. The two winners of the tournament in this round receive a prize of 16 euros each while the other participants receive zero for their performance in the task. In addition to payment for performance in the task, all participants receive a show-up fee of 4 euros and can earn up to 2 euros in the subsequent dictator experiment. At the end of each experimental session, we distributed a pen and paper questionnaire (see the appendix to this chapter for the full questionnaire), in which we ask about demographics as well as perceived fairness of the experimental procedure, which participants should rate on a scale from 1 (very unfair) to 5 (very fair). We conducted the experiments in April and October 2016 and May 2017 at the Cologne Laboratory for Economic Research

[7]One group had to be eliminated from the analysis of this treatment because one subject from this group did not pass the control questions.

(CLER). Overall, 632 subjects participated in our experiment and we ran 40 experimental sessions. We used Java to program our experiments and recruited the participants through the online recruitment software ORSEE (Greiner 2015).

Figure 3.3: Treatments

	Ex-ante equal	Ex-ante unequal
No quota	***Baseline_equal*** No quota Equal working time for all n=160; N=20	***Baseline_unequal*** No quota Disadvantaged type has reduced working time n=160; N=20
Quota	***Quota_equal*** At least one prize awarded to affirmed type Equal working time for all n=160; N=20	***Quota_unequal*** At least one prize awarded to affirmed & disadvantaged type Affirmed & disadvantaged type has reduced working time n=152; N=19

n indicates the total number of participants in each treatment.
N indicates the number of independent observations, i.e., matching groups of 8 participants, in each treatment.

Our treatment interventions can have an effect on peer-review behavior, but it can also affect performance per se. To be able to distinguish between these two effects, we elicit a performance measure that is independent of our treatment interventions. We conduct an online experiment, in which we ask a total of 400 independent raters to evaluate the illustrations from the experiment on the same scale as in the laboratory experiment (0.0 to 10.0). To avoid overload of the raters, each rater evaluates a subset of 64 pictures. In the online experiment, each screen shown to the raters displays four pictures from one group in one round. The composition of pictures on one screen shown to the independent raters was identical to that shown to the participants of the experiment. The pictures also have the yellow and green frames as in the laboratory experiment. One important difference to the main experiment is that the evaluators are blind to the treatment and do not know what the frames mean. Each set of pictures is evaluated by ten different raters. The average of these ten evaluations constitutes our independent quality measure. Participants in the online experiment receive a fixed payment of 2 euros and can earn an additional bonus of up to 4 euros, which is awarded according to the quadratic deviation from the mean evaluation for one randomly chosen picture (we follow the procedure suggested by Selten 1998). We programmed this experiment using the online survey tool SoSci Survey (Leiner 2014). Online raters were also recruited through the online recruitment software ORSEE (Greiner 2015) using the same subject pool as in the main experiment but ensuring that evaluators did not participate in any previous related experiments.

3.3 Results

3.3.1 Biases in Peer-reviews

Across all treatments, the average peer-review is 2.59 points, while the average score on the independent evaluation is equal to 6.00 (see table 3.4 in the appendix to this chapter for summary statistics). For all treatments and types, we find that peer-reviews are significantly less favorable as compared to the independent ratings (pairwise Wilcoxon rank-sum test, $p<0.01$ for all types and treatments), but significantly larger than zero (Wilcoxon rank-sum test, $p<0.01$ for all types and treatments).[8] Thus, we find evidence for a substantial difference between the independent measure and the peer-review.[9]

To analyze biases in peer-reviews that are due to the introduction of a quota, we provide regression analysis results in table 3.1. In specifications (1) and (2), we focus on the ex-ante equal setting and in specifications (3) and (4), we focus on the ex-ante unequal setting.[10] In specification (1) we only include a quota dummy and a dummy for affirmed ratees (receiver of an evaluation). The coefficient of the quota dummy is informative of the effect of a quota on the overall level of peer-reviews. The coefficient of the affirmed ratee dummy is informative of distortions in peer-reviews, i.e., systematic differences of peer-reviews provided depending on the type of the ratee.

In the ex-ante equal setting, we find a slight but insignificant negative level effect. Additionally, we find a significant and negative coefficient of the affirmed ratee dummy. Affirmed ratees receive around 0.6 points less favorable evaluations, which amounts to 21% of the average evaluation in the baseline. This provides evidence for a significant distortion effect, where affirmed types receive less favorable peer-reviews compared to non-affirmed peers. To better understand the extent to which this distortion is due to changes in the performance of affirmed types, we include the independent performance measure as an additional control in specification (2).[11] Introducing this control reduces the distortion effect slightly, but the coefficient for affirmed ratees is still large and statistically significant.

In the ex-ante unequal setting (specifications (3) and (4)), we find that the introduction of a quota has a positive and significant effect on the overall level

[8] For the non-parametric analysis in this chapter, we use mean values for the matching groups of eight participants and over all rounds of the experiment. We always report p-values for two-sided tests.

[9] In order to address the concern that our independent online raters have no experience with the task that they evaluate, we asked a subset of our participants in the laboratory experiment to rate illustrations created in different sessions after they have completed the main experiment. We only elicited this measure for the ex-ante equal setting. In this setting, however, we can show that the main results presented in this chapter are robust to using this alternative performance measure as a control variable. Analysis using experienced lab raters can be found in table 3.5 in the appendix to this chapter.

[10] In table 3.6 in the appendix to this chapter, we provide an additional specification which analyzes both settings in one model.

[11] In table 3.7 in the appendix to this chapter, we provide an analysis of the effect of our treatment interventions on performance. We do not find evidence for a significant effect of quotas on the performance as measured through independent ratings. However, we do find that procedural disadvantages induced by reduced working time are in fact relevant and have a negative effect on performance.

of peer-reviews provided. That is, we find that overall peer-reviews provided are about 0.6 points more favorable and thus, closer to the independent rating, when a quota is implemented. As in the ex-ante equal setting, we find evidence for a significant distortion effect, where affirmed types receive less favorable peer-reviews compared to non-affirmed peers. Affirmed ratees receive evaluations that are around 0.6 points less favorable compared to non-affirmed ratees. This distortion amounts to around 25% less favorable evaluations compared to the corresponding reference group in the baseline. Interestingly, we do not find a significant effect of being disadvantaged on the peer-reviews received. In column (4), we additionally control for performance. If anything, including this control explains a very small part of the distortion in peer-reviews, since the coefficient of affirmed remains large and highly significant. Again, we find no significant difference in peer-reviews provided to disadvantaged types compared to non-disadvantaged peers.

Table 3.1: Regression analysis peer-reviews provided

Dependent variable:	Ex-ante equal		Ex-ante unequal	
Peer-review	(1)	(2)	(3)	(4)
Quota	-0.130	-0.036	0.634**	0.645**
	(0.217)	(0.240)	(0.283)	(0.300)
Affirmed ratee	-0.562***	-0.502***	-0.632***	-0.544***
	(0.153)	(0.143)	(0.229)	(0.199)
Disadvantaged ratee			-0.011	0.178
			(0.135)	(0.118)
Independent rating		0.341***		0.382***
		(0.026)		(0.026)
Constant	2.884***	0.734***	2.383***	0.012
	(0.157)	(0.214)	(0.221)	(0.247)
Observations	2,880	2,880	2,808	2,808
Number of participants	320	320	312	312
Number of groups	40	40	39	39

Notes: Two-way error component linear model, allowing for creator and evaluator random effects. The dependent variable is peer-review received. Independent variables: Quota (dummy equal to one in treatments with a quota), Affirmed ratee (dummy equal to one for affirmed ratees in the treatments with a quota), Disadvantaged ratee (dummy equal to one for disadvantaged ratees in all treatments involving less working time for disadvantaged type), Independent Rating (continuousvariable with the mean evaluation of independent raters who are blind to treatments). In all specifications robust standard errors are clustered by matching groups of eight participants. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

To understand the source of these biases in peer-reviews, we analyze the behavior of affirmed and non-affirmed raters (sender of an evaluation) separately. Table 3.2 provides results using the same regression procedure as in specifications (2) and (4) of table 3.1, but splits the sample depending on the type of the rater. In the ex-ante equal setting, we use all observations from the baseline as a reference group for both the affirmed and the non-affirmed types. Specification (1) in table 3.2 presents the results for affirmed raters in the ex-ante equal setting. While we find no effect of quotas on the overall level of peer-reviews provided by affirmed raters, we find evidence for a strong distortion effect where affirmed raters provide significantly less favorable peer-reviews to affirmed peers, i.e., around 0.6 points less favorable evaluations. Specification (2) presents the results for non-affirmed raters in the ex-ante equal setting. We find evidence for a significant level effect, i.e., non-affirmed raters provide around 0.8 points less favorable peer-reviews overall whenever quotas are implemented. However, we do not find evidence for a significant distortion effect on evaluations provided by non-affirmed raters.

Table 3.2: Regression analysis peer-reviews provided by rater type

Dependent variable:	Ex-ante equal		Ex-ante unequal	
Peer-review	(1)	(2)	(3)	(4)
	Affirmed rater	Non-affirmed rater	Affirmed rater	Non-affirmed rater
Quota	0.217	-0.806***	1.028***	-0.094
	(0.306)	(0.282)	(0.344)	(0.407)
Affirmed ratee	-0.595***	0.137	-0.701***	0.111
	(0.190)	(0.088)	(0.198)	(0.178)
Disadvantaged ratee			0.003	0.072
			(0.118)	(0.145)
Independent rating	0.346***	0.339***	0.347***	0.400***
	(0.032)	(0.027)	(0.044)	(0.036)
Constant	0.690***	0.747***	0.101	0.179
	(0.236)	(0.210)	(0.333)	(0.300)
Observations	2,160	2,160	1,404	1,404
Number of participants	320	320	312	312
Number of groups	40	40	39	39

Notes: Two-way error component linear model, allowing for creator and evaluator random effects. Separate models for affirmed and non-affirmed raters.
The dependent variable is peer-review received. Independent variables: Quota (dummy equal to one in treatments with a quota), Affirmed ratee (dummy equal to one for affirmed ratees in the treatments with a quota), Disadvantaged ratee (dummy equal to one for disadvantaged ratees in all treatments involving less working time for disadvantaged type), Independent Rating (continuousvariable with the mean evaluation of independent raters who are blind to treatments). In the ex-ante equal setting, all raters from the *Baseline_equal* serve as a reference group for both affirmed and non-affirmed raters. In the ex-ante unequal, disadvantaged raters from the *Baseline_unequal* treatment serve as the reference group for affirmed raters, while the non-disadvantaged individuals from the *Baseline_unequal* treatment serve as reference group for non-affirmed raters in the *Quota_unequa*l treatment.
In all specifications robust standard errors are clustered by matching groups of eight participants.
Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

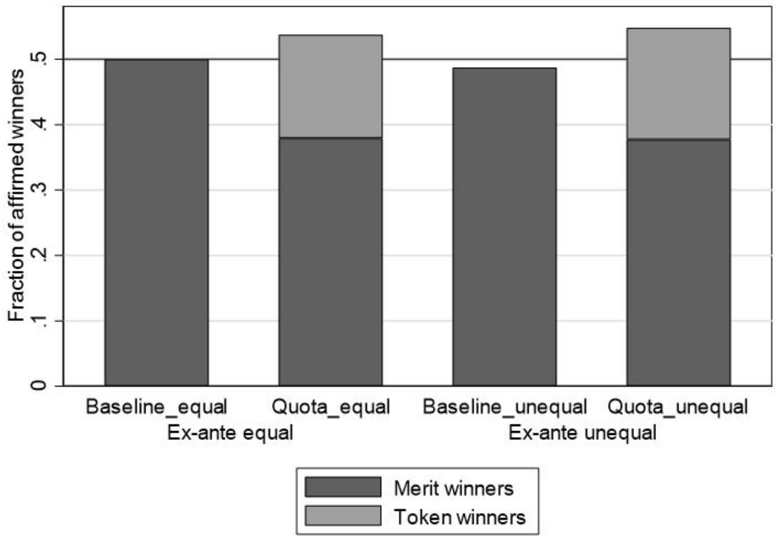
Specifications (3) and (4) provide the results for the ex-ante unequal setting. In specification (3), we focus on affirmed raters. With respect to the level effect, we find that affirmed raters provide significantly more favorable peer-reviews, i.e., around 1.0 point more favorable evaluations compared to the baseline, whenever a quota is implemented. We also find evidence for a large and significant distortion effect, where affirmed raters provide of around 0.7 points less favorable peer-reviews to other affirmed peers as compared to non-affirmed peers. As presented in specification (4), we do not find evidence for any systematic biases in peer-reviews provided by non-affirmed raters when a quota is introduced in the ex-ante unequal setting.

Thus, it seems that level effects depend on the setting in which a quota is implemented, while distortion effects are fully driven by reviews provided by affirmed raters and seem to be independent of the setting. As a robustness check for this interpretation, we provide the results from regression analyses in which we additionally control for perceived procedural fairness in table 3.8 in the appendix to this chapter. We find that adding this control substantially reduces observed level effects but has basically no effect on observed distortion effects.[12]

3.3.2 Tokenization of Affirmed Winners

In figure 3.4, we display the fraction of tournament winners from the affirmed group by treatments. We additionally differentiate between "merit winners" – winners of the affirmed type who are among the two participants with the highest peer-review – and "token winners" – affirmed winners who are not among the two performers with the highest peer-review, but win because of the quota intervention. We find that in both the ex-ante equal and the ex-ante unequal settings, the introduction of the quota significantly increases the overall number of tournament winners from the affirmed type (Mann-Whitney U-test, $p<0.01$ and $p=0.08$, correspondingly). In both the *Baseline_equal* and the *Baseline_unequal* treatments, by design, all winners are "merit winners". In the *Quota_equal* and the *Quota_unequal* treatments, in absence of side-effects of quotas, the fraction of "merit winners" would be unaffected by the introduction of a quota and the fraction of "token winners" would correspond to the increase in the representation of affirmed types among tournament winners. However, in our experiment we observe substantial distortions in peer-reviews. Thus, we find that quotas lead to a decrease in the fraction of affirmed "merit winners" (Whitney U-test, $p\leq0.01$ for both settings). As a consequence, the fraction of "token winners" is larger than the actual increase in the representation of affirmed types among tournament winners. Therefore affirmed types appear to be in the need of a quota to win the tournament, whereas a large fraction of these "token winners" would also have been among the winners without a quota intervention.

[12]In table 3.9 in the appendix to this chapter, we provide further analysis splitting the sample into male and female raters. We find that distortion effects are independent of gender. With respect to level effects, we observe some gender differences, where affirmed females raters provide more favorable peer-reviews when a quota is implemented.



Notes: In the ex-ante equal setting, one random type from the *Baseline_equal* serves as reference group for affirmed types in the *Quota_equal* treatment. In the ex-ante unequal setting, disadvantaged types from the *Baseline_unequal* treatment serve as reference group for affirmed and disadvantaged types in the *Quota_unequal* treatment.

Figure 3.4: Fraction of affirmed winners

3.3.3 Spillover Effects of Quotas

Lastly, we are interested in spillover effects of quotas on behavior in a dictator game that does not involve any quota regulation. Therefore, we conduct a dictator experiment (Forsythe et al. 1994) after the main experiment. Besides the fact that we inform dictators about the receiver's type from the previous experiment, this experiment is unrelated to the main experiment. The instructions for this experiment are given after completion of the main experiment (see the appendix to this chapter for the experimental instructions). In this experiment, dictators allocate 2 euros between themselves and an anonymous recipient. Dictators are informed about the type of the recipient (yellow or green) from the previous tournament experiment. We repeat the dictator game four times with random rematching, so that each participant acts twice as a dictator and twice as a recipient. In both roles (dictator and recipient), each participant is matched to both a yellow and a green type player. We randomized the sequence of the four rounds. Only one of the rounds is randomly chosen for payment, and no feedback is given between the rounds.

In table 3.3, we show the results from a linear regression analysis with the amount sent in the dictator game as the dependent variable and a quota dummy and dummies for affirmed and if applicable, disadvantaged receiver as independent variables. Specification (1) and (2) show the results for the ex-ante equal setting, and specification (3) and (4) for the ex-ante unequal setting. For each setting, we analyze dictator behavior for the previously affirmed and non-affirmed group separately.

Table 3.3: Regression analysis dictator game

Dependent variable:	Ex-ante equal		Ex-ante unequal	
Peer-review	(1)	(2)	(3)	(4)
	Affirmed dictator	Non-affirmed dictator	Affirmed dictator	Non-affirmed dictator
Quota	0.081	-0.029	0.078	0.010
	(0.060)	(0.055)	(0.058)	(0.064)
Affirmed receiver	-0.067**	-0.106***	-0.064*	-0.017
	(0.027)	(0.027)	(0.033)	(0.037)
Disadvantaged receiver			0.088***	-0.004
			(0.026)	(0.022)
Constant	0.324***	0.324***	0.211***	0.295***
	(0.032)	(0.032)	(0.040)	(0.045)
Observations	480	480	312	312
Number of participants	240	240	156	156
Number of groups	40	40	39	39

Notes: Ordinary-least squares linear model. Separate models for affirmed and non-affirmed raters. The dependent variable is the amount sent in the dictator game.
Independent variables: Quota (dummy equal to one in treatments with a quota), Affirmed receiver (dummy equal to one if receiver was affirmed in the previous experiment), Disadvantaged receiver (dummy equal to one if receiver was disadvantaged in the previous experiment).
In the ex-ante equal setting, all dictators from the *Baseline_equal* serve as a reference group for both affirmed and non-affirmed dictators. In the ex-ante unequal, the dictatators that were disadvantaged in the previous *Baseline_unequal* treatment serve as the reference group for affirmed dictators, while the dictators that were non-disadvantaged in the previous experiment serve as reference group for non-affirmed dictators in the *Quota_unequal* treatment.
In all specifications robust standard errors are clustered by matching groups of eight participants.
Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Throughout all specifications, we do not find any significant effect of facing a quota in the previous experiment on the overall level of giving. However, we do observe distortion effects where participants, who were affirmed in the previous experiment, receive significantly lower contributions in the subsequent dictator game. Independent of the setting, we find that a quota in the previous experiment leads to lower giving by dictators, who were previously affirmed, to previously affirmed receivers of around 0.07 and 0.06 euros respectively. In both treatments, this amounts to 22% less compared to the average in the corresponding baseline. For dictators who were non-affirmed in the previous experiment, we find that distortions in dictator giving depend on the setting of the previous experiment. Dictators who faced the ex-ante equal setting in the previous experiment, give around 0.11 euros less to receivers who were affirmed in the previous experiment as compared to receivers who were non-affirmed, i.e., around 34% less compared to the *Baseline_equal* treatment. Dictators who faced the ex-ante unequal setting in the previous experiment, do not discriminate based on the type of the receiver in the previous experiment.

3.4 Conclusion

In this chapter, we provide evidence for biases in subjective performance evaluations which arise due to the introduction of a quota. We find that quotas have a significant effect on the overall level of peer-reviews provided and that this effect varies with the setting in which quotas are implemented. In our experimental study, the average level of peer-review does not have an impact on tournament outcomes. Thus, level effects do not counteract procedural inequalities arising due to the implementation of a quota. Nevertheless, we observe that the effect of a quota on the level of peer-reviews provided is sensitive to the setting in which quotas are implemented. We can show that this difference is mainly driven by differences in perceived procedural fairness due to the introduction of a quota. One explanation for this finding is that costs of providing biased reviews depend on perceived procedural fairness. This interpretation is in line with previous findings on the effect of perceived fairness on unethical behavior (see e.g., Greenberg 1990; Schweitzer and Gibson 2008; Houser, Vetter, and Winter 2012). In real-life work situations, such level effects may have additional motivational implications (Heilman and Barocas Alcott 2001; Unzueta, Gutiérrez, and Ghavami 2010) and may be problematic whenever peer-reviews elicited under a quota regime are compared to reviews elicited absent of a quota regime.

We additionally show that quotas lead to systematic distortions in peer-reviews, where affirmed individuals receive less favorable peer-reviews as compared to non-affirmed individuals whenever quotas are implemented. These distortions seem to be unrelated to ex-ante procedural fairness. Given that distortions are fully driven by peer-reviews provided by affirmed types, it seems that distortions are the consequence of enhanced competition among affirmed individuals whenever quotas are implemented. While a quota compensates for the potential distortions in prize assignment due to biased peer-reviews, quotas may have a long-term negative impact on the career chances of affirmed individuals. Distortions make affirmed types appear to be less able and in the need of a quota even when they are not. As peer-reviews are widely used in domains where objective performance measures are lacking, such distortions can have far-reaching consequences on career-relevant opportunities of affirmed individuals. In practice, the same performance evaluations are often used for multiple managerial decisions (Edwards and Ewen 1996; Bracken 2001; L. K. Johnson 2004). Distortions in performance evaluations arising due to a quota may therefore negatively affect the career opportunities of affirmed individuals in contexts which go beyond the scope of the quota regulation. Additionally, distorted peer-reviews may serve as an anchor for future performance evaluations (Tversky and Kahneman 1974; Thorsteinson et al. 2008). Furthermore, performance evaluations affect self-image and motivation (Unzueta, Gutiérrez, and Ghavami 2010; Heilman and Barocas Alcott 2001; Leslie, Mayer, and Kravitz 2014). Relatively unfavorable peer-reviews may therefore reduce the future productivity of affirmed individuals and may discourage them from pursuing relevant career paths (Leibbrandt and List 2018; Leibbrandt, Wang, and Foo 2018).

In addition, we study spillover effects of quotas on altruistic behavior in a non-competitive context that is not regulated by a quota. In line with previous research on spillover effects of quotas (Kölle 2017; Maggian and Montinari 2017; R. Banerjee, Gupta, and Villeval 2018), we do not find evidence of an effect of quotas on the overall level of altruistic behavior. However, the implementation of a quota in the previous experiment substantially reduces altruistic behavior among individuals who were affirmed in the previous experiment. Proponents of affirmative action argue that by increasing the representation of a minority group, this also fosters cooperative networks amongst them and thus acts as a catalyzer. Our findings, however, suggest that quota interventions may undermine the effectiveness with respect to the establishment of social networks (Athey, Avery, and Zemsky 2000; Bertrand, S. Black, et al. 2019; Buckles 2019).

Overall, our research provides an important contribution to better understand the mechanisms behind a quota intervention. While quotas are a popular form of affirmative action and have been rolled out in many countries, most of the literature has focused on the positive effects of quota interventions with respect to tournament entry and gender. Our project enriches the discussion of quota interventions by exploring distortions in subjective peer-reviews as an important channel that could mitigate the positive effects of quotas.

3.5 Appendix to Chapter 3

Experimental Instructions - Main Experiment

Welcome to this experiment!

Please carefully read through the following instructions. If you have a question, please raise your hand. We will then come to your desk and answer your question.

All of your decisions are anonymous. Communication with other participants is not permitted for the duration of the experiment. We would like to ask you to switch off your mobile phone and place it in your bag.

You will receive a show-up fee of 4 euros for your participation. You can earn additional money in the following experiment.

Instructions

Experimental Procedure

- This experiment consists of **multiple rounds**.
  - Initially, you will take part in a test round that is not relevant for your payment.
  - After that, 3 experimental rounds will be conducted.
- At the beginning of the experiment, you will be randomly assigned a type.
  - You are either **type "green"** or **type "yellow".**
  - You can recognize your type based on the color of the frame of your display.
  - This type assignment remains constant for the entire experiment.
- In each round, you will be randomly assigned to a group of four participants.
  - Each group consists of 2 type "green" and 2 type "yellow" participants.
  - You will be assigned to a new group in each round.
  - However, your type (green or yellow) remains constant for the entire experiment.

Your Task

- Your task is to **illustrate an object using given materials**.
  - Group members of both types are provided with the same materials to illustrate the object (see images below)
    - The materials provided to type "yellow" members are pictured in the bottom left (yellow frame).
    - The materials provided to type "green" members are pictured in the bottom right (green frame).
  - The object that you are supposed to illustrate will be displayed on your screen.
  - You will illustrate a different object in each round.
  - All members of your group have to illustrate the same object in the respective rounds.

  Baseline_equal and Quota_equal Treatments:

  - You have **5 minutes** time available in each round.

  Baseline_unequal and Quota_unequal Treatments:

  - The time available to illustrate the object in each round is limited
    - **Type "yellow" ("green")** group members have 2 minutes and 30 seconds available in each round.
    - **Type "green" ("yellow")** group members have 5 minutes available in each round.

Rating the Illustrations

- Each group member rates the other 3 group members' illustrations
- The color of the frame of the respective illustrations indicates the type (green or yellow) to which the group member who produced the illustration belongs.
- The rating is conducted on a scale from **0.0 to 10.0 points**.
    - 0.0 points correspond to the worst rating.
    - 10.0 points correspond to the best rating.
    - Please always specify **exactly one decimal place** (please use a dot as a decimal sign).
- The sum of the points awarded equals the final rating.
    - For each illustration, this **final rating** lies **between 0.0 and 30.0 points**.

Payment

- At the end of the experiment, **one of the three experimental rounds** will be randomly **chosen**.
    - Only this round is relevant for the payment.
- Based on the final rating and the assigned type, exactly **two prizes** with the amount of **16 euros** each will be paid out to **two** different group members.

Baseline_equal and Baseline_unequal Treatments:

- The group member with the highest final rating **among all** group members of **both types** receives a prize.
- The group member with the highest final rating among the **remaining three** group members of **both types** receives a prize.
- The other two group members do not receive a prize.
- This means that **at most two prizes** are awarded to **type "green"** group members.

- This means that **at most two prizes** are awarded to **type "yellow"** group members.

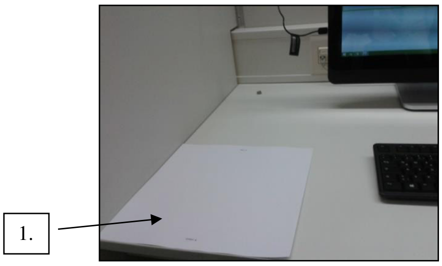
Quota_equal and Quota_unequal Treatments:

- The **type "green" ("yellow")** group member with the higher final rating between **the two** type "green" ("yellow") group members receives a prize.
- The group member with the highest final rating among the **remaining three** group members of **both types** receives a prize.
- The other two group members do not receive a prize.
- This means that **at least one prize and at most two prizes** are awarded to **type "green" ("yellow")** group members.
- This means that at **most one prize** are awarded to the **type "yellow" ("green")** group members.

Procedure

Please proceed with the illustration of the object as follows:

1. Illustrate the object in the marked area using the provided materials.



2. Take a photo of the illustrated object by clicking on “take photo.” If the photo meets your expectations, save this photo by clicking on “save photo.” If a photo does not meet your expectations, you can delete it by clicking on “delete photo.”
3. You can take and save as many photos as you wish during the time available. You cannot take or save any additional photos after time has expired.
4. After time has expired, you have the opportunity to choose one of your saved photos. Only this photo will be rated by the other group members in the next step. None of the other photos will be rated.
5. Note that only previously saved photos can be chosen.

Please note the following when illustrating the objects:

- **Only** use the provided materials.
- For each illustration, you may use all materials or a selection of materials.
- Place the object only **within** the area marked with the piece of paper (only this area will be captured by the camera)
- Pay attention to the **direction** of your illustration (the piece of paper is labeled with “top” and “bottom”).
- Pay attention that **your hands** are **not visible** in the marked area.
- Keep the **unused materials outside** of the marked area.
- Please **do not write or draw** on the piece of paper representing the marked area.
- Pay attention to the **time limit** for the task; after time has expired you **cannot** take or save any **new photos**.

Post-experimental Questionnaire

Questionnaire

Please, answer the following questions while we prepare the payment.
Thank you!

<table>
<tr><td>1. Please, indicate your gender:</td><td>female
☐</td><td>male
☐</td></tr>
<tr><td>2. How old are you?</td><td colspan="2"></td></tr>
<tr><td>3. How many siblings do you have?</td><td colspan="2"></td></tr>
<tr><td>4. Do you have a red-green colorblindness?</td><td>Yes
☐</td><td>No
☐</td></tr>
<tr><td colspan="3">Please answer the following questions using the provided scale.</td></tr>
<tr><td colspan="3">5. How satisfied are you with the experiment overall?</td></tr>
<tr><td colspan="3">Not satisfied at all ☐ ☐ ☐ ☐ ☐ Very satisfied</td></tr>
<tr><td colspan="3">6. How much would you like to participate in an experiment like this one again?</td></tr>
<tr><td colspan="3">Not at all ☐ ☐ ☐ ☐ ☐ Very much</td></tr>
<tr><td colspan="3">7. How likely is it that you would recommend participating in an experiment like this one to a friend?</td></tr>
<tr><td colspan="3">Not likely at all ☐ ☐ ☐ ☐ ☐ Very likely</td></tr>
</table>

8. How fair did you consider the payment procedure of this experiment?				
Not fair at all				Very fair
☐	☐	☐	☐	☐
9. Did you feel disadvantaged or advantaged by the payment procedure of this experiment?				
Very disadvantaged				Very advantaged
☐	☐	☐	☐	☐
10. How justified did you consider this disadvantage or advantage?				
Not justified at all				Very justified
☐	☐	☐	☐	☐
11. How much fun did you have solving the task?				
No fun at all				A lot of fun
☐	☐	☐	☐	☐
12. How creative are you?				
Not creative at all				Very creative
☐	☐	☐	☐	☐
13. How difficult did you find the task?				
Not difficult at all				Very difficult
☐	☐	☐	☐	☐

14. How well, do you think, you performed compared to the other participants in this room?

Far below average				Far above average
☐	☐	☐	☐	☐

15. In how many rounds do you think you received the highest or second highest final rating?

0	1	2	3
☐	☐	☐	☐

16. In how many of these three rounds do you think you received a prize?

0	1	2	3
☐	☐	☐	☐

17. How likely is it, do you think, that the other group members rated your illustration according to its quality?

Not likely at all				Very likely
☐	☐	☐	☐	☐

18. How do you see yourself: Are you someone who is willing to take risks or do you try to avoid them?

Not willing at all to take risks				Very willing to take risks
☐	☐	☐	☐	☐

Please indicate for the following questions how much the respective statements apply to you.				
19. I get upset when someone is better off for no reason.				
Doesn't apply at all				Fully applies
☐	☐	☐	☐	☐
20. I feel guilty when I am better off for no reason.				
Doesn't apply at all				Fully applies
☐	☐	☐	☐	☐
21. If someone harms me on purpose, I will try to take revenge even when I have to bear the costs.				
Doesn't apply at all				Fully applies
☐	☐	☐	☐	☐
22. If someone does me a favor, I am happy to return it.				
Doesn't apply at all				Fully applies
☐	☐	☐	☐	☐
23. I like to compete with others.				
Doesn't apply at all				Fully applies
☐	☐	☐	☐	☐
24. It is important to me to be the best.				
Doesn't apply at all				Fully applies
☐	☐	☐	☐	☐

Summary Statistics

Table 3.4: Summary statistics

	Baseline_equal	*Quota_equal*		*Baseline_unequal*		*Quota_unequal*	
	All	Affirmed	Non-affirmed	Disadvantaged	Non-disadvantaged	Affirmed	Non-affirmed
Peer-review received	2.88	2.27	2.83	2.37	2.38	2.37	3.02
	(0.77)	(0.70)	(0.67)	(0.92)	(1.00)	(0.89)	(0.78)
Independent rating received	6.28	5.98	6.16	5.71	6.21	5.46	6.18
	(0.38)	(0.62)	(0.56)	(0.70)	(0.59)	(0.69)	(0.74)
Independent rating received (lab rater)	4.54	4.51	4.51	n.a.	n.a.	n.a.	n.a.
	(0.47)	(0.77)	(0.74)				
Procedural fairness perception	2.39	2.26	1.81	2.11	2.42	2.55	2.24
	(0.40)	(0.42)	(0.50)	(0.48)	(0.54)	(0.54)	(0.53)
Female	0.56	0.66	0.59	0.48	0.63	0.63	0.65
	(0.20)	(0.30)	(0.23)	(0.27)	(0.22)	(0.23)	(0.23)

Notes: Means with standard errors on group level in parentheses. Evaluation scale for procedural fairness perception: 1 – very unfair to 5 – very fair

Additional Regressions

Table 3.5: Regression analysis peer-reviews provided by rater type with lab raters as control for independent ratings

Dependent variable:		Ex-ante equal	
Peer-review	(1)	(2)	(3)
	All	Affirmed rater	Non-affirmed rater
Quota	-0.210	0.036	-0.876***
	(0.224)	(0.278)	(0.313)
Affirmed ratee	-0.565***	-0.659***	0.075
	(0.145)	(0.194)	(0.085)
Independent rating	0.427***	0.470***	0.421***
	(0.044)	(0.053)	(0.048)
Constant	0.940***	0.749***	0.968***
	(0.232)	(0.262)	(0.241)
Observations	2,880	2,160	2,160
Number of participants	320	320	320
Number of groups	40	40	40

Notes: Two-way error component linear model, allowing for creator and evaluator random effects. Separate models for affirmed and non-affirmed raters. The dependent variable is peer-review received. Independent variables: Quota (dummy equal to one in treatments with a quota), Affirmed ratee (dummy equal to one for affirmed ratees in the treatments with a quota), Independent Rating (lab rater) (continuous variable with the mean evaluation of independent raters who are blind to treatments). All individuals from the *Baseline_equal* serve as a reference group for both affirmed and non-affirmed raters. In all specifications robust standard errors are clustered by matching groups of eight participants.
Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.6: Regression analysis peer-reviews provided – all treatments combined

Dependent variable: Peer-review	(1) All	(2) Affirmed rater	(3) Non-affirmed rater
Quota_equal	-0.012	0.389	-0.801***
	(0.241)	(0.299)	(0.281)
Quota_equal x affirmed ratee	-0.498***	-0.595***	0.142
	(0.142)	(0.188)	(0.089)
Unequal	-0.470	-0.602*	-0.193
	(0.296)	(0.314)	(0.319)
Unequal x disadvantaged ratee	0.169	0.003	0.055
	(0.117)	(0.116)	(0.142)
Quota_unequal	0.650**	1.028***	-0.099
	(0.296)	(0.342)	(0.402)
Quota_unequal x affirmed ratee	-0.549***	-0.701***	0.103
	(0.198)	(0.196)	(0.177)
Independent rating	0.362***	0.347***	0.365***
	(0.018)	(0.026)	(0.022)
Constant	0.603***	0.700***	0.580***
	(0.195)	(0.218)	(0.200)
Observations	5,688	3,564	3,564
Number of participants	632	316	316
Number of groups	79	79	79

Notes: Two-way error component linear model, allowing for creator and evaluator random effects. Models including all settings and treatments and either all or only affirmed or non-affirmed raters. The dependent variable is peer-review received. Independent variables: *Quota_equal* (dummy equal to one in Quota_equal treatment), *Quota_equal* x Affirmed ratee (dummy equal to one for affirmed ratee in the *Quota_equal* treatment), Unequal (dummy equal to one for ex-ante unequal treatment), Unequal x Disadvantaged ratee (dummy equal to one for disadvantaged ratee in ex-ante unequal treatment), *Quota_unequa*l (dummy equal to one in *Quota_unequal* treatment), *Quota_unequal* x Affirmed ratee (dummy equal to one for affirmed ratee in the *Quota_unequal* treatment), Independent Rating (continuous variable with the mean evaluation of independent raters who are blind to treatments). In all specifications robust standard errors are clustered by matching groups of eight participants. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.7: Regression analysis independent ratings

Dependent variable: Independent ratings	(1) Ex-ante equal	(2) Ex-ante unequal
Quota	-0.125	-0.029
	(0.150)	(0.211)
Affirmed ratee	-0.177	-0.229
	(0.149)	(0.238)
Disadvantaged ratee		-0.495***
		(0.183)
Constant	6.281***	6.209***
	(0.084)	(0.130)
Observations	960	936
Number of participants	320	3212
Number of groups	40	40

Notes: Ordinary least squares linear model. Models include either ex-ante equal or ex-ante unequal settings. The dependent variable is independent rating received. Independent variables: Quota (dummy equal to one in treatments with a quota), Affirmed ratee (dummy equal to one for affirmed ratee in the treatments with a quota), Disadvantaged ratee (dummy equal to one for disadvantaged ratee in all treatments involving less working time for disadvantaged type). In all specifications robust standard errors are clustered by matching groups of eight participants. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.8: Regression analysis peer-reviews provided by rater type with control for perceived procedural fairness

Dependent variable:	Ex-ante equal		Ex-ante unequal	
Peer-review	(1)	(2)	(3)	(4)
	Affirmed rater	Non-affirmed rater	Affirmed rater	Non-affirmed rater
Quota	0.335	-0.315	0.619*	-0.009
	(0.282)	(0.275)	(0.353)	(0.400)
Affirmed ratee	-0.595***	0.137	-0.702***	0.112
	(0.190)	(0.088)	(0.199)	(0.178)
Disadvantaged ratee			0.000	0.073
			(0.117)	(0.144)
Independent rating	0.346***	0.342***	0.342***	0.401***
	(0.031)	(0.025)	(0.042)	(0.035)
Fairness perception of rater	0.838***	0.861***	0.931***	0.485***
	(0.140)	(0.138)	(0.235)	(0.150)
Constant	-1.291***	-1.327***	-1.835***	-0.981**
	(0.380)	(0.392)	(0.529)	(0.436)
Observations	2,160	2,160	1,404	1,404
Number of participants	320	320	312	312
Number of groups	40	40	39	39

Notes: Two-way error component linear model, allowing for creator and evaluator random effects. Separate models for affirmed and non-affirmed raters.
The dependent variable is peer-review received. Independent variables: Quota (dummy equal to one in treatments with a quota), Affirmed ratee (dummy equal to one for affirmed ratees in the treatments with a quota), Disadvantaged ratee (dummy equal to one for disadvantaged ratees in all treatments involving less working time for disadvantaged type), Independent Rating (continuousvariable with the mean evaluation of independent raters who are blind to treatments), Fairness perception of rater (continuous variable with the rater's evaluation of fairness as elicited in the post-experimental questionnaire). In the ex-ante equal setting, all raters from the *Baseline_equal* serve as a reference group for both affirmed and non-affirmed raters.
In the ex-ante unequal, disadvantaged raters from the *Baseline_unequal* treatment serve as the reference group for affirmed raters, while the non-disadvantaged individuals from the *Baseline_unequal* treatment serve as reference group for non-affirmed raters in the *Quota_unequa*l treatment. In all specifications robust standard errors are clustered by matching groups of eight participants.
Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.9: Regression analysis peer-reviews provided by rater type – gender differences

Dependent variable:	Ex-ante equal				Ex-ante unequal			
	Affirmed rater		Non-affirmed rater		Affirmed rater		Non-affirmed rater	
Peer-review	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Female	Male	Female	Male	Female	Male	Female	Male
Quota	0.976**	-0.259	-0.711*	-0.777*	2.052***	-0.659	0.148	-0.704
	(0.409)	(0.472)	(0.405)	(0.415)	(0.542)	(0.568)	(0.486)	(0.724)
Affirmed ratee	-0.562***	-0.654*	0.113	0.183*	-0.708***	-0.638	0.118	0.091
	(0.193)	(0.338)	(0.123)	(0.103)	(0.226)	(0.406)	(0.220)	(0.311)
Disadvantaged ratee					-0.021	0.034	0.062	0.032
					(0.126)	(0.227)	(0.176)	(0.191)
Independent rating	0.395***	0.310***	0.367***	0.316***	0.411***	0.317***	0.429***	0.314***
	(0.041)	(0.053)	(0.038)	(0.047)	(0.071)	(0.055)	(0.049)	(0.058)
Constant	0.508	0.355	0.690**	0.318	-0.445	0.604*	0.095	0.189
	(0.350)	(0.331)	(0.328)	(0.303)	(0.536)	(0.351)	(0.381)	(0.579)
Observations	1,278	882	1,224	936	774	630	891	513
Number of participants	142	98	136	104	86	70	99	57
Number of groups	39	35	40	39	37	35	39	33

Notes: Two-way error component linear model, allowing for creator and evaluator random effects. Separate models for affirmed and non-affirmed, and female and male raters. The dependent variable is peer-review received. Independent variables: Quota (dummy equal to one in treatments with a quota), Affirmed ratee (dummy equal to one for affirmed ratees in the treatments with a quota), Disadvantaged ratee (dummy equal to one for disadvantaged ratees in all treatments involving less working time for disadvantaged type), Independent Rating (continuousvariable with the mean evaluation of independent raters who are blind to treatments). In the ex-ante equal setting, all raters from the *Baseline_equal* serve as a reference group for both affirmed and non-affirmed raters. In the ex-ante unequal, disadvantaged raters from the *Baseline_unequal* treatment serve as the reference group for affirmed raters, while the non-disadvantaged individuals from the *Baseline_unequal* treatment serve as reference group for non-affirmed raters in the *Quota_unequal* treatment. In all specifications robust standard errors are clustered by matching groups of eight participants. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Experimental Instructions - Dictator Experiment

Instructions

Welcome to this part of the experiment! Please carefully read through the following instructions. If you have questions, please raise your hand. We will then come over to you and answer your question. As before, all of your decisions are anonymous. Communication with other participants is not permitted for the duration of the experiment.

The Experiment

This experiment consists of four rounds. In each round you will be **anonymously assigned** to another participant and also **assigned** a role. At the beginning of each round, you will learn the other participant's type (green or yellow) as well as your role (active or passive) in this round. Assignment to the player types (green or yellow) corresponds with the assignment from the first experiment. **The roles will be newly assigned in each round**.

- The **active** participant has an **endowment of 2.00 euros** in each round.
- The **passive** participant has an **endowment of 0.00 euro** in each round.
- The **active** participant **chooses** how many euros (in 0.10 euro increments) he/she would like to **transfer to** the **passive** participant.

A total of **4 rounds** will be conducted in this experiment. The assignment of roles (active and passive) varies between each round, and you will be assigned to a different participant in each round.

At the end of the experiment, **one round** will be **randomly** determined to be relevant for the payment.

Payment

- The active participant's payment equals 2.00 euros minus the amount transferred in the round relevant for the payment, i.e., **the active participant's payment = 2.00 euros – transferred amount**.
- The passive participant's payment equals 0.00 euro plus the amount transferred in the round relevant for the payment, i.e., **the passive participant's payment = 0.00 euro + transferred amount**.

Chapter 4

The Hidden Cost of Training[1]

"Human capital itself is not enough in order to make workers productive, because skills are embodied in humans."

Jörn-Steffen Pischke (2005)

4.1 Introduction

Skilled labor is an important driver of innovation and thus economic growth (R. Lucas 1988; S. Black and Lynch 1996; Fernald and Jones 2014). Especially in developed economies, tasks are becoming more skill-intensive (Acemoglu and Pischke 1999; Autor and Handel 2013). One way for employers to boost labor productivity is to invest in training to enhance the individual's skill set, i.e., human capital, and thereby increase the productivity potential of the respective employee (G. Becker 1962). A recently published training industry report surveying a representative sample of organizations with 100 or more employees in the United States, finds that training expenditures increased to a total of 90.6 billion US Dollars (Training Magazine 2017). OECD (2011) estimates that an individual spends on average 715 hours in job-related non-formal education over the course of a working life.

Previous research has tried to pin down the returns of training both for the employer in form of productivity gains, and for the employee in form of higher wages. However, as training can take many different forms and is mostly a result of non-exogenous processes, estimating its effects poses an ambitious research endeavour (Heckman 2000; Pischke 2005). Thus, it is not surprising that depending on the estimation strategy and the specific dataset, the estimated training effects vary widely (see Bassanini et al. 2007, for an overview). With respect to the productivity effect of training, the literature is relatively scarce since productivity is inherently hard to measure and often not comparable across

[1] This chapter is based upon Petters (2019)

different organizations. Therefore, this strand of literature mostly relies on firm-level data and estimates productivity gains ranging between 5 and 20% (Mincer 1962; Holzer, R. Block, et al. 1993; Dearden, Reed, and Van Reenen 2000; De Grip and Sauermann 2011; Adhvaryu, Kala, and Nyshadham 2018). With respect to the effect of training on wages, the literature finds very high effect sizes as well as insignificant and close to zero effects (Lynch 1992; Booth 1993; Veum 1995; Blundell et al. 1999; Parent 1999; Pischke 2001; Frazis and Loewenstein 2005). However, selection bias and the resulting heterogeneity between trained and untrained workers might lead to overestimating the training effects. Those studies with cleaner identification strategies consistently find lower effect sizes (Blundell et al. 1999; Leuven and Oosterbeek 2004; Leuven and Oosterbeek 2008; Adhvaryu, Kala, and Nyshadham 2018).

In this chapter, we formalize a theoretical framework that acknowledges the fact that the production function of human labor is more complex compared to other input factors as it might be subject to behavioral responses to the environment. We argue that, additional to the effect of training on the skill level of an employee (G. Becker 1964), training might also affect wage expectations of the respective employee.[2] In other words, training increases the "fair wage" as coined by Akerlof and Yellen (1990). According to their fair wage-effort hypothesis, the employee evaluates his actual wage against this "fair wage", which if below results in a feeling of being underpaid and unfairly treated. To compensate for this perceived loss, Akerlof and Yellen (1990) predict a reduction of employee's effort. When we understand labor productivity as a function of skills and effort, and apply our argument based on the fair wage-effort hypothesis, training can, ceteris paribus, generate two effects affecting productivity in different directions: a direct positive effect on skills, and an indirect negative effect on effort, which works through a shift in wage expectations. However, which of the two countervailing effects of training dominates, ulimately poses an empirical question. Additionally, the specific size of the two effects might vary between individuals and with the specific environment in which training is provided.

We use field and experimental data to study the relationship between training participation and fair wage expectations as well as the behavioral consequences following from this. First, we use an extensive matched employer-employee dataset on vocational training from Germany to provide first evidence on the positive relationship between training participation and wage expectations. Second, in order to explore the different behavioral channels, we develop a novel experimental design which allows us to induce our treatment variations in a controlled environment and to specifically measure the variables of interest. We apply an employer-employee gift-exchange setting, in which employees work for a fixed wage on a real-effort decoding task for the benefit of the employer. In a two-by-two design we exogenously assign training and wage increases and capture how employees respond with respect to measures for the fair wage, effort and overall productivity. The experimental results strengthen the hypothesized

[2] Our proposed mechanism is in line with the findings by Bolton and Werner (2016), who show that higher productivity leads to higher entitlements and thus wage demands (also compare Gächter and Riedl 2005). Similarly we argue that the increase in skills related to training might come along with a feeling of being entitled to a higher wage which results in adapted wage expectations.

relationship between training participation and higher fair wage expectations. Furthermore, we find that training can be ineffective in increasing overall productivity. Even though training increases the skills and thus productivity potential in our experiment, the employee's potential might not be realized because of lower efforts on the intensive and extensive margin. Additional analyses of heterogenous effects suggest that fairness considerations are the driving mechanism for this result.

Numerous studies have emphasized the importance of fairness in economic decision making in general (Kahneman, Knetsch, and Thaler 1986; Fehr and Schmidt 1999; Bolton and Ockenfels 2000) and reciprocal behavior in particular (see Fehr, Goette, and Zehnder 2009, for an overview). Mas (2006) for example presents field evidence from police officers in New Jersey showing that the distance from the subjective fair wage affects police performance. Ockenfels, Sliwka, and Werner (2014) find similar results for managers in a large company when bonus payments fall short of expectations. While Cohn, Fehr, and Goette (2015) show in a field experiment that only workers that felt underpaid before react with increased efforts after having received a pay raise. The results obtained by Abeler et al. (2010) in a laboratory study imply that only equity-based wages related to individual productivity are being perceived as fair. If this fairness norm is violated by paying equal wages, they observe substantially lower efforts. Similarly, Breza, Kaur, and Shamdasani (2018) show in a field experiment with Indian manufacturing workers that pay disparity among a group of seemingly similar co-workers results in lower output, higher absenteeism and lower group cohesion. This effect vanishes once these differences in wages are justified by differences in productivity.

This project contributes to the existing literature in several ways. We present a theoretical framework as well as empirical evidence, using both field and experimental data, for a behavioral mechanism that might impair the returns to training. In particular, we show that skill-enhancing training leads to higher wages being perceived as fair, which if not paid accordingly results in a reduction of effort. Therefore, efficiency gains related to training might not necessarily translate into higher productivity. As a result, the effectiveness of training might be underestimated by firms and institutions and thus lead to lower training investments.

4.2 Theoretical Framework and Hypotheses

To illustrate the main idea, we develop a theoretical framework which includes training as an additional factor in the productivity of labor function. We model productivity of labor Y_L as a function of effort e and skills s: $Y_L = F(e, s)$ which is increasing in both arguments, i.e., $\frac{\partial Y_L}{\partial e} > 0$ and $\frac{\partial Y_L}{\partial s} > 0$. Training then serves as a mean to increase the current skill level of the trained employee, i.e., $s(t)$ with $\frac{\partial s}{\partial t} > 0$.

In line with the fair wage-effort-hypothesis as stated by Akerlof and Yellen (1990), we model effort as a function of the wage w relative to some fair wage w^*. The wage which is considered as fair, however, might as well depend on the acquired skill level as it is directly related to the employee's productivity potential, i.e., $w^*(s(t))$ with $\frac{\partial w^*}{\partial s} > 0$. Hypothesis 1 follows.

Hypothesis 1: Training participation increases expectations towards a fair wage.

Following Akerlof and Yellen (1990), we assume that $e = min(\frac{w}{w^*(s(t))}, 1)$ with $\frac{\partial e}{\partial w} \geq 0, \frac{\partial e}{\partial w^*} \leq 0$. Thus, if the actual wage falls below the fair wage, effort is adjusted accordingly and only a fraction of "normal effort" (normalized to 1) is provided. Given Hypothesis 1, our model also predicts a negative effort response related to training participation (if the actual wage lies below what would be considered fair).

Hypothesis 2: Training participation decreases effort provision.

As in the standard model, our model integrates a gift-exchange setting and thus predicts that a higher wage evokes more reciprocal behavior by the employee in the form of increased effort (in case "nomal effort" was not yet provided).

Hypothesis 3: A higher wage increases effort provision.

When training is combined with a higher wage, the effect on effort is ambiguous since both affect effort provision in different directions.

Integrating the adapted skill and effort functions, we can derive a productivity of labor function which takes training as an additional argument:

$$Y_L = F(e(min(\frac{w}{w^*(s(t))}, 1)), s(t))$$

This revised productivity of labor function indicates that training has two potentially countervailing effects. On the one hand, training has a direct effect on skills, which in turn should positively affect overall productivity of labor, i.e., $\frac{\partial F}{\partial s}\frac{\partial s}{\partial t} > 0$. On the other hand, training has an effect on the fair wage, which indirectly affects productivity of labor negatively through its effect on effort, i.e., $\frac{\partial F}{\partial e}\frac{\partial e}{\partial w^*}\frac{\partial w^*}{\partial s}\frac{\partial s}{\partial t} \leq 0$.

In a first step, we study the relationship between training and wage expectations (Hypothesis 1) using an extensive field dataset. This hypothesis constitutes the core novelty of our approach and serves as the underlying assumption for our revised version of the productivity of labor function. In a next step, we test all hypotheses derived from our theoretical framework (Hypotheses 1-3) in a laboratory experiment. The experimental setting allows us to exogenously assign trainig and wages and to investigate the specific mechanisms with respect to skills, effort, and overall productivity in a controlled environment.

4.3 Evidence from Field Data

We begin by presenting evidence of the relationship between training participation and future wage expectations based on a linked employer-employee survey dataset. We use the German dataset WeLL (Further training as a part of lifelong learning), which was collected by the Research Data Centre of the Federal Employment Agency at the Institute for Employment Research (FDZ) in four consecutive years from 2007 to 2010. The questionnaire specifically focuses on employees' training activities (see Bender, Fertig, et al. 2009, for detailed information on the dataset). Furthermore, we link this dataset to administrative records of the Institute for Employment Research (IAB) to be able to control for observed wages, employment experience and tenure.[3] We limit the sample to full-time workers covered by social security, for whom we have information on all variables used in our estimations. This leaves us with a total of 2,274 employees employed in 143 different establishments[4] and a total of 3,592 observations.

The main outcome variable to address the first part of our research question relates to the survey question: *"Assuming you are employed in 12 months from now. What is the minimum/maximum monthly income (taxes and social security contritbutions already deducted), you will earn in total given your current assessment?"*. The mean of the minimum and the maximum monthly expected income stated in this questions serves as our proxy for fair wage expectations. General training participation is captured with the following survey question: *"Did you particpate in any seminars, lectures, courses or trainings of professional development within the time span of January [previous year] through today?"*.

To account for the potential unobserved heterogeneity between the group of trained and untrained employees and thus potentially biased estimates, we create an alternative control group following previous approaches in the training literature (Leuven and Oosterbeek 2008; Dietz and Zwick 2016). We use the information of already scheduled training activities that have been cancelled because of reasons beyond the employees' control, i.e., cancellations by the organizer or because of a job at work with high priority. With this, we address

[3]Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the GESIS – Leibniz Institute for the Social Sciences as well as remote data access.

[4]The sample selection followed a two-step procedure. First, the sample of establishments was drawn from the IAB Establishment Panel and stratified according to establishment size, sector, location, further training activities and investment activities. Second, a sample of employees where drawn from the total of employees employed in these establishments. Therefore, the sample is not representative of German establishments in general, but for the purpose of studying in-firm further training activities focuses on establishments which support employee training.

the selection bias into training by using a control group which intended to participate in training, but was not able to because of exogenous reasons. Table 4.5 in the appendix to this chapter provides summary statistics for all variables used in our estimations.

To analyze the relationship between wage expectations and training participation, we regress expected monthly income in Euros on a training dummy, which constitutes our main explanatory variable (see table 4.1). The dummy variable equals one if the employee participated in training and zero if the employee had scheduled a training but could not participate because of exogenous reasons. We estimate our results using ordinary least squares regressions. In specification (1), we only include year fixed effects to control for the economic situation in that year. Specification (2) additionally controls for employee characteristics, which capture age, education, occupational status, firm tenure and labor market experience. We follow a lagged dependent variable approach in specification (3) and include mean expected monthly income in Euros in the previous year as an additional variable, in order to control for unobserved factors which might affect wage expectations such as, for example, general career aspirations. Specification (4) additionally includes establishment characteristics and establishment fixed effects.

Table 4.1 gives an overview of the regression results. In specification (1), we find a highly significant and sizeable positive relationship between training participation and expected monthly income. Employees who have been trained in the previous year on average expect to earn a monthly income which is around 300 Euros higher compared to employees in our control group. Specification (2), however, shows that even though we apply the restriction of exogenous non-participation in already scheduled training to the control group, the two groups still differ with respect to individual characteristics. When we include individual controls, the coefficient substantially drops, but remains highly statistically significant. Including the wage expectations from the previous year as an additional control reduces the estimated coefficient further to around 51 Euros (compare specification (3)). Also in the last specification, which contains establishment fixed effects, we find a significant effect of training participation on the expected monthly income. The average difference in expected earnings between training participants and exogenous non-participants estimated in specification (4) amounts to around 53 Euros. Given that employees in the dataset on average stated an expected monthly income of around 2,434 Euros, this equals an expected increased pay of 2.2% following training participation.

In line with our hypothesis, we find that employees adjust their expectations about future earnings when they participated in training. Thus, the field results provide empirical support for Hypothesis 1.

Table 4.1: Training and wage expectations

Dependent variable: Mean expected monthly income in Euros	(1)	(2)	(3)	(4)
Training	299.890***	115.846***	50.948**	53.300**
	(49.591)	(39.322)	(23.928)	(25.330)
Lagged mean expected monthly income in Euros			0.672***	0.624***
			(0.126)	(0.136)
Constant	2,167.732***	-462.113	-89.300	-279.803
	(49.833)	(420.094)	(191.392)	(262.775)
Employee controls	No	Yes	Yes	Yes
Establishment controls & FE	No	No	No	Yes
Observations	3,592	3,592	3,592	3,592
# of clusters	2,274	2,274	2,274	2,274
R-squared	0.009	0.364	0.719	0.736

Notes: Linear regression with robust standard errors clustered on individuals in parentheses. All specifications include year fixed effects. Employee controls include: gender, age, age (squared), secondary education, occupational status, tenure, tenure (squared), labor market experience, labor market experience (squared). Establishment controls include: establishment location (east/west), establishment size, industry.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

4.4 Experimental Evidence

While our results from the WeLL dataset provide support for the prediction that training participation affects fair wage expectations, factors such as the great variety of contents and types of trainings and the potentially very different reasons behind training participation (career incentives, outside options, new job requirements, job security) could bias our estimates from field data. Even though we use an alternative control group of exogenous non-participants to address selection into training, we can not fully eliminate any potential endogeneity concerns. Additionally, to test all of our research hypotheses related to the specific behavioral mechanisms behind training participation, we need reliable and comparable productivity measures with respect to the task addressed within the training, as well as measures for skills and effort. Therefore, we designed a laboratory experiment in which we, as a first step, aim to replicate our results from the field and, as a second step, test the hypothesized mechanisms in a controlled environment.

4.4.1 Experimental Design

We introduce a novel experimental design to study the causal effect of training participation on the perceived fair wage and its consequences for effort and final productivity outcomes in a gift-exchange setting. In the experiment, we exogenously vary whether an employee (i) participates in a skill-enhancing training and whether he (ii) receives a wage increase afterwards. In addition to productivity measures, we elicit a measure of the norm with respect to fair wages in order to be able to analyze whether this is the relevant channel for the hypothesized effects.

The experiment consists of five incentivized parts and a questionnaire after the conclusion of the experiment. Upon arrival, participants are randomly seated and receive general instructions about the experiment (see the appendix to this chapter for the experimental instructions), which are also read aloud by the experimenter. In order to make sure that the general conditions of the experiment are well understood, participants have to pass a number of control questions before being able to start the main part of the experiment. The experiment is framed as an employer-employee setting in which four employees are matched to one employer. Employees are paid a fixed wage and asked to work on a real-effort decoding task which benefits the employer. The employer only plays a limited role in the experiment, however, she is the one who implements our treatment variations by determining which two of the four employees are trained and which two of the four employees receive a wage increase.

The chronological structure of the experiment is as follows (see figure 4.1 for an overview of the general structure of the experiment). Employees start with working for ten minutes on the real-effort task (first working phase). Subsequently, the first fair wage norm elicitation takes place. At the same time, the employer determines whether the respective employee will participate in training or can enjoy free time and use the internet for ten minutes. After the conclusion of this phase, the second fair wage norm elicitation takes place. Again, simultaneously the employer determines the respective wage levels. Following this, the employees are informed about their wage for the second working phase and the second working phase starts, which again lasts for ten minutes.

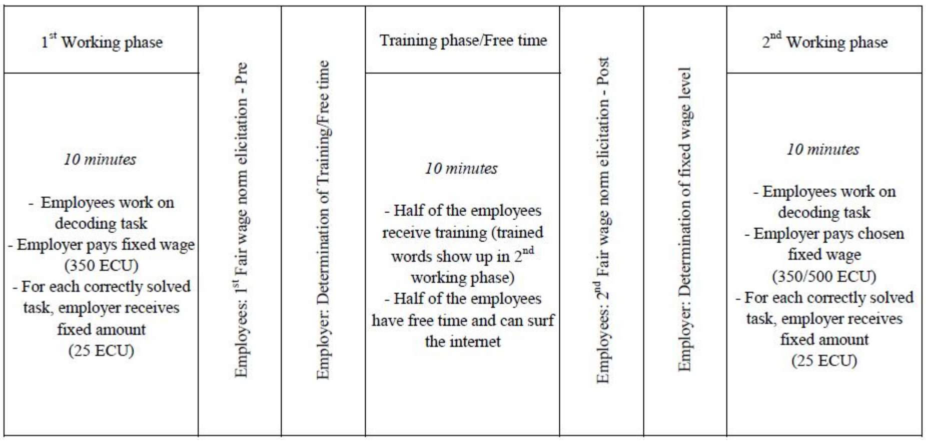


Figure 4.1: Experimental structure

The experiment was conducted in February and June 2018 at the Cologne Laboratory for Economic Research (CLER). A total of 480 subjects (96 employers and 384 employees) took part in 16 experimental sessions. During the experiment, earnings were denoted in experimental currency units (ECU) and only converted into Euros at the end of the experiment (exchange rate: 100 ECU = 1 Euro). Average earnings for participation in the experiment amounted to 17.70 Euros for an approximate total duration of 75 minutes. Participants were recruited through the online recruitment software ORSEE (Greiner 2015) and the experiment was programmed using Java and oTree software (D. L. Chen, Schonger, and Wickens 2016).

Real-Effort Decoding Task

In each of the two working phases, employees work for ten minutes on a real-effort decoding task similar to the encoding task used by Erkal, Gangadharan, and Nikiforakis (2011). Employees are given a seven-digit sequence of numbers and a decoding table that enables them to decode the sequence into a meaningful word. For an examplary screen see figure 4.2, which shows the seven-digit sequence of 26 29 6 11 20 5. Given the decoding table in the lower part of the screen, we find that 26 = S, 29 = T, 6 = A, 11 = I, 20 = O, and 5 = N, and can derive that the corresponding word is “station”. All employees are presented the same sequences of numbers and decoding tables in the same predefined order. Once a word is correctly decoded and the solution is sent, the next screen with a new decoding task appears. For each working phase the employees receive a fixed wage, which amounts to 350 ECU in the first working phase, and 350 ECU or 500 ECU respectively in the second working phase. At the same time the employer receives 25 ECU for each correctly decoded word by her employees. Employees are informed at all times of their respective fixed wage for the current working phase, the number of correctly decoded words and the resulting payoff they generated for their respective employer (see figure 4.2 in the appendix to this chapter). It is common information that neither the employer nor the employees receive information about the (other) employees' performance at any point in the experiment. Therefore, we can rule out any social comparison or reputation effects as an explanation of our treatment effects. Since the experiment resembles a gift-exchange setting, the employees can freely choose how much time they spend on working on the task itself and how much time they take for each task during the working phases of 10 minutes. But in any case they have to sit in front of the screen and are required not to engage in any other unrelated activities, such as using their phone or reading.

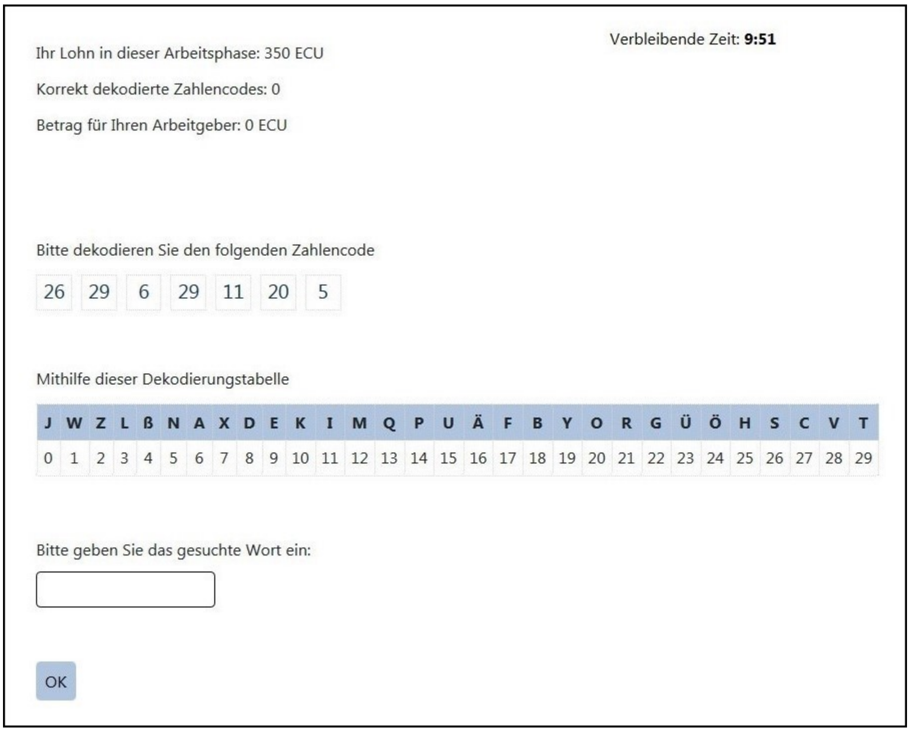


Figure 4.2: Screenshot real-effort decoding task

Fair Wage Norm Elicitation

We use a mechanism adapted from Krupka and Weber (2013) to elicit the social norm with respect to the fair wage. We ask employees for their expected fairness response of comparable employees, i.e., employees that have passed through the same phases as they did. For this, employees have to rate each of the respective wage levels for the second working phase on a scale of "very unfair", "somewhat unfair", "somewhat fair" to "very fair" (see figure 4.4 in the appendix to this chapter for an exemplary screen). This fairness measurement is elicited for 16 different wage levels between "less than 50 ECU " and "more than 700 ECU". We use the wage level at which employees change their evaluation from "somewhat fair" to "very fair" as a proxy for the fair wage in our later analyses.[5] The elicitation is incentivized with a small additional bonus of 50 ECU that employees receive if they correctly stated the modal fairness response for a randomly chosen wage level.[6] We elicit this measure twice. The first elicitation takes place after the first working phase is concluded. Given that our randomization into treatments worked, we should not find any differences between treatments. Therefore, the first measurement mainly serves as an additional control variable to reduce noise as it is highly correlated with the second elicitation. The second elicitation takes place after the training phase is concluded and before the wage for the second working phase is announced.

Training/Free Time

Between the two working phases, employees either participated in a training phase (called "trained employees" in the following) or enjoyed free time in which they were given the possibility to surf the internet (called "untrained employees" in the following). In the training phase, employees were shown similar screens as before, but in this phase a short animation showed them the decoding of the numbers into letters up to the point where the correct solution was visible (see figure 4.5 in the appendix to this chapter for an exemplary screen). Then the animation stopped and employees were asked to type in the correct word themselves. If this was not done within 15 seconds, the next screen with a new task was shown. It was common information for employees that took part in the training phase, that every second decoding task in the second working phase will consist of a task that was already practiced in the training phase. Since the solutions to the decoding tasks were actual words, trained employees should be able to remember those words and therefore decode the respective words faster. Thus, the training should enable the trained employees to decode more words in the second working phase compared to untrained employees.

Treatments

Our treatment variations consist of whether an employee participates in training and whether he receives a wage increase in the second working phase (which is only announced after the second fair wage norm elicitation). Unlike in most experimental studies, our treatments take place simultaneously within one session and the assignment of employees to treatment groups is implemented over the course of the experiment (see figure 4.1 for the chronological structure of the experiment). Both the training decision as well as the decision on the wage level for the second working phase are determined by the employer. As the employer

[5]This means that we potentially lack this measurement for agents that never evaluated any wage level as "very fair". However using alternative measures as robustness checks, e.g., the mean fairness evaluation or the wage level that was first considered as "somewhat fair", does not change the results.

[6]While the social norm with respect to the fair wage offers the convenience of being incentivizable, it might at the same time be only a noisy measure for the individual fairness perception. To account for this, we also elicited the non-incentivized version in a pilot study (N=21) and found that both measures are highly correlated (Pearson-correlation: 0.66, $p<0.01$). Therefore, we henceforth use the social norm as a proxy for individual fairness perceptions.

has no information about the performance or any other characteristics of her employees and can only choose between two (for her) arbitrary options, i.e., giving training to employee 1 and 2 or employee 3 and 4 and giving a wage increase to employee 1 and 3 or employee 2 and 4, the treatments are exogenous. Hence, treatments are randomly assigned while at the same time the chosen procedure ensures that subjects in the role of the employees perceive that employers have an impact on their training participation and wages.

Figure 4.3 summarizes the four different treatments and the number of independent observations from employees[7], which form the data basis for the following analysis. Treatment group T0 forms our control group, which receives no training and no wage increase in the second working phase, i.e., 350 ECU as in the first working phase. Employees in treatment group T1 receive no training, but a wage increase in the second working phase, i.e., 500 ECU, while in treatment group T2 they receive training, but no wage increase in the second working phase, i.e., 350 ECU as in the first working phase. Finally, when in treatment group T3, employees receive both training and a wage increase in the second working phase, i.e., 500 ECU.

	No Wage Increase	Wage Increase
No Training	**T0** No Training & No Wage Increase N=93	**T1** No Training & Wage Increase N=95
Training	**T2** Training & No Wage Increase N=94	**T3** Training & Wage Increase N=93

Figure 4.3: Treatments

After the experiment is concluded, subjects in the role of employees are paid their respective wages for both working phases as well as any additional bonuses resulting from the fair wage norm elicitations. Only one of the two working phases is randomly chosen for the payout of subjects in the role of the employer in order to avoid employees hedging working efforts between the two working phases. On top of their generated payments, all subjects receive a show-up fee for participation in the experiment.

4.4.2 Results

In this section, we present evidence from our experimental data to test the predictions from our theoretical framework. First, we focus on the effect of training

[7]Nine observations had to be discarded because of technical problems during the experiment.

participation on fair wage expectations (Hypothesis 1). Second, we study the effect of training and wages on our measure of effort (Hypotheses 2 and 3). In our analyses, we additionally study the role of fairness perceptions by analyzing heterogenous effects on subgroups with different fair wage expectations. Finally, we analyze how training participation affects realized productivity.

Fair Wage Norm (Hypothesis 1)

We use the elicited fair wage norm as a proxy for fair wage expectations. Descriptively we find that untrained employees report on average a fair wage norm of about 482 ECU for the second working phase (see table 4.7 in the appendix to this chapter for the complete summary statistics). Trained employees state a significantly higher level of 530 ECU as fair ($p<0.01$, Mann-Whitney test, two-sided). In table 4.2, we present the corresponding regression analysis with the fair wage norm as elicited in the second elicitation as the dependent variable and a training dummy as the independent variable. At the point of the second fair wage norm elicitation our treatment variations with respect to wages have not become effective yet, which is why we group both treatments with training (T2 and T3) together and compare them against the baseline of no training (T0 and T1). In specification (1) we only control for the fair wage norm as elicited before the training or free time respectively, specification (2) includes additional control variables such as session fixed effects, gender and measures for envy, competitiveness, guilt and reciprocity as elicited from the post-experimental questionnaire.

We find that employees that received the training state a significantly higher fair wage norm, which exceeds that of untrained employees by around 50 ECU, i.e., 10%. The result is robust to including additional control variables.

Result 1: Trained employees state significantly higher fair wage expectations compared to untrained employees.

The findings support the conjecture that an increased productivity potential as a result of skill-enhancing training[8] comes along with higher expectations of what consitutes a fair wage.[9] Therefore, the results we found in our field data

[8] See table 4.8 in the appendix to this chapter for the effect of training on the time needed to correctly decode a word as a proxy for skills. While, to some extent, this measure also captures effort on the intensive margin, our training intervention constitutes a skill shock only for those words that have been shown in the previous training phase. We find that those employees that received training (T2 and T3) are significantly faster in decoding the trained words. However, only for employees that received a wage increase in addition to training (T3), this also leads to an overall decrease in average decoding time.

[9] Given the previous literature on entitlements related to higher productivity potential (Gächter and Riedl 2005; Bolton and Werner 2016) and the salience of the productivity effect of our training intervention (i.e., the words shown in the training are known to appear in the second working phase again), we focus on this channel. However, we acknowledge that additional factors, such as the (perceived) investment of time and effort within the training, might play a role as well by affecting the size of the former effect. We find suggestive evidence for this relationship. For example, it seems to be of relevance how enjoyable agents found the training. Those who reported to have enjoyed participating in the training more, stated a significantly lower fair wage norm after the training compared to agents who did not enjoy the training to the same extent ($p<0.1$, t-test, two-sided).

can also be replicated in a laboratory setting which provides further evidence in favor of Hypothesis 1.

Table 4.2: Fair wage norm

Dependent variable: Fair wage norm	(1)	(2)
Training	47.934***	49.237***
	(13.069)	(13.045)
Fair wage norm - pre training/free time	0.788***	0.779***
	(0.053)	(0.061)
Constant	58.852**	71.389
	(29.521)	(43.787)
Additional controls	No	Yes
Observations	356	352
R-squared	0.379	0.403

Notes: Linear ordinary least squares regression with robust standard errors in parentheses. Additional control variables include: session fixed effects, gender, questionnaire measures for envy, competitiveness, guilt, and reciprocity.
*** p<0.01, ** p<0.05, * p<0.1.

Time Invested (Hypotheses 2 and 3)

We analyze the time invested as a proxy for effort on the extensive margin. Since time working on the task is not contracted in our experimental setting, employees can freely choose for how long they are willing to work on the task within the time frame of ten minutes. Table 4.3 shows a linear regression with the time invested, i.e., the point in time at which the respective employee decoded his last word, as the dependent variable and dummies for each treatment group as independent variables. We also control for the time invested in the first working phase. As before, we show specifications without and with additional controls for the fair wage norm as elicited before the training/free time, session fixed effects, gender and measures for envy, competitiveness, guilt and reciprocity as elicited from the post-experimental questionnaire. In specification (1) and (2), we analyze all employees in one model. According to the theoretical framework, however, effort provision is determined by whether or not the wage falls below what is perceived as fair. For this reason, we additionally analyze heterogenous effects of our treatment interventions by splitting the sample into subgroups of employees that stated a fair wage norm below or equal to 500 ECU (specifications (3) and (4)) and employees that stated a fair wage norm of above 500 ECU (specifications (5) and (6)). 500 ECU corresponds to the wage level employees were paid when they received a wage increase.[10]

[10]We split the sample by this fixed cutoff of 500 ECU (instead of using whether the actual wage falls below the stated fair wage norm) in order to create a more comparable control group (i.e., employees that perceive 350 ECU as fair might be inherently different, e.g., very

Overall (specifications (1) and (2)), we only find suggestive evidence ($p<0.15$, t-test, two-sided) for a negative effect of training on the time invested for the group of employees who only received training (T2). These employees reduce the time invested to work on the task by around 0.4 minutes (24 seconds), which is significantly less compared to employees that did not receive training but a wage increase (T1) ($p<0.05/0.07$, Wald test, two-sided). We do not find any significant effects for employees that received only a wage increase (T1), nor for employees that received training combined with a wage increase (T3). When we, however, split the sample according to whether the stated fair wage norm lies below (or is equal to) or above 500 ECU, we find strong heterogeneities between the two groups.

For the group of employees who stated a fair wage norm below or equal to 500 ECU (specification (3) and (4)), we find no significant negative effects of training on effort provision. A wage increase that pays a wage that the respective employee stated as fair (T1), seems to have a (weakly) positive effect on effort provision.

When we, however, focus on the group of employees that are paid below what they stated as fair, a wage increase (T1) has no significant on the time invested to work on the task. With respect to training, we find a significant negative effect on effort provision - even when employees received a wage increase (T2 and T3). In both treatments employees significantly reduce the time invested to work on the task by around 0.7 minutes (42 seconds).[11]

Result 2: Only if employees are paid below what they perceive as fair, training reduces effort provision.

Our results show that our predicted effects highly depend on whether the respective employee perceives the paid wage as fair. Only for employees that stated a fair wage norm below or equal to 500 ECU, we find suggestive evidence for a positive effort effect of a wage increase as outlined in Hypothesis 3. For this group, we also do not find any negative effort response to training. However, for the group of employees with a fair wage norm above 500 ECU, which means they fall below their fair wage in all treatments, we find a negative effect of training on effort provision, which provides support for Hypothesis 2.

altruistic, compared to employees that perceive 500 ECU as fair). Additionally, with this we divide the data into more similar subgroups with respect to sample size. The alternative specifications, however, provide qualitatively similar results (see table 4.10 in the appendix to this chapter).

[11] These results are also similar to findings reported in Sauermann (2019). In the field experiment with agents in a call center, trained agents increase their performance on a single task, i.e., the handling time of a customer call. However, at the same time they significantly decrease the number of hours worked and show a higher number of absence days. These results, while interpreted in a different manner, are in line with the theoretical framework and experimental results presented in this chapter.

Table 4.3: Time invested

Dependent variable:	All		Fair wage norm $\leq$ 500 ECU		Fair wage norm > 500 ECU	
Time invested (min.)	(1)	(2)	(3)	(4)	(5)	(6)
T1 - No Training & Wage Increase	0.118	0.021	0.458*	0.447	-0.274	-0.419
	(0.201)	(0.198)	(0.267)	(0.295)	(0.268)	(0.301)
T2 - Training & No Wage Increase	-0.362	-0.410	-0.126	-0.226	-0.655*	-0.685*
	(0.249)	(0.250)	(0.316)	(0.269)	(0.371)	(0.403)
T3 - Training & Wage Increase	-0.222	-0.290	0.274	0.319	-0.659**	-0.717**
	(0.231)	(0.241)	(0.271)	(0.286)	(0.326)	(0.333)
Time invested (min.) 1st working phase	0.862***	0.863***	0.742***	0.772***	0.930***	0.935***
	(0.096)	(0.095)	(0.192)	(0.183)	(0.100)	(0.101)
Constant	1.161	0.575	2.168	1.955	0.712	-0.014
	(0.927)	(1.124)	(1.845)	(1.685)	(1.018)	(1.536)
Additional controls	No	Yes	No	Yes	No	Yes
Observations	375	370	174	172	201	198
R-squared	0.472	0.490	0.441	0.501	0.497	0.519

Notes: Linear ordinary least squares regression with robust standard errors in parentheses. Additional control variables include:
Fair wage norm - pre training/free time, session fixed effects, gender, questionnaire measures for envy, competitiveness, guilt, and reciprocity.
Specifications (1) and (2) include all employees, specifications (3) and (4) the subgroup of employees that stated a fair wage norm $\leq$500 ECU,
and specifications (5) and (6) the subgroup of employees that stated a fair wage norm > 500 ECU. *** p<0.01, ** p<0.05, * p<0.1.

Decoded Words

In this section, we study the effect of training and wage increase on the overall productivity of labor. In the context of our experimental task, the number of decoded words constitutes our measure of productivity. As before, we use a linear regression analysis with the number of decoded words in the second working phase as the dependent variable and treatment group dummies as independent variables. Since the number of decoded words in the first working phase is highly predictive of the number of words decoded in the second working phase, we control for this in all specifications. As in the previous analyses, we include further control variables such as the fair wage norm elicited before the training/free time, session fixed effects, gender and measures for envy, competitiveness, guilt and reciprocity as elicited from the post-experimental questionnaire in a second stage. Again, we first analyze all observations in one model (specification (1) and (2)), and then study potential heterogenous effects by splitting the sample into employees that state a fair wage norm below (or equal to) 500 ECU (specification (3) and (4)) and above 500 ECU (specification (5) and (6)). Table 4.4 gives an overview of the results.

Overall, we find that both treatment groups that received a wage increase, i.e., T1 and T3, are significantly more productive compared to the baseline group T0, which neither participated in training nor received a wage increase. These employees decode on average 1.6 (1.7) and 1.9 (2.0) respectively more compared to the baseline. This corresponds to an overall productivity increase of 6.1% (6.4%) and 7.2% (7.6%). Training alone (T2), however, does not lead to a significant increase in the number of decoded words. Also, the difference in productivity between T1 and T3 is not statistically significant ($p>0.74$, Wald test, two-tailed).

Once we split the sample, we find that the overall positive productivity effects are solely driven by the group of employees which state a fair wage norm below or equal to 500 ECU (specification (3) and (4)). In this group, we find that employees who receive a wage increase only (T1) decode on average 2.5 (2.7) words more, which amounts to an increase of 9.5% (10.2%) compared to the baseline. Additionally, we find suggestive evidence that, for this group, also training alone (T2) has a positive productivity effect, showing that trained employees on average decode 2.3 (1.7) words more, which corresponds to a productivity increase of about 8.7% (6.4%) compared to the baseline. For employees who received training combined with a wage increase that pays a wage they consider as fair (T3), the positive productivity effect is especially pronounced. This group decodes on average 4.0 (4.4) words more compared to the baseline, which constitutes an increase in productivity of 15.2% (16.7%). Additionally, we find suggestive evidence that the combination of training with a wage increase is also more effective than either training or a wage increase alone ($p<0.19$ for comparison T1 and T3 and $p<0.07$ for comparison T2 and T3, Wald test, two-tailed).

For employees that are paid below what they stated as a fair wage norm, we do not find any significant productivity effects of the wage increase, the training, nor the combination of both (specification (5) and (6)).[12]

[12] Even though we also find a training effect with respect to the decoding time of trained words for this group of employees (see table 4.9 in the appendix to this chapter).

Result 3: Only if employees perceive their wage as fair, training also leads to higher productivity.

We interpret the findings as follows. It seems that the distance between the perceived fair wage and the actual wage is of high importance for determining whether or not the respective employee is willing to release his productivity potential. If this gap is (almost) closed, both higher wages and training are effective means to increase productivity. Paying a higher wage targets the effort channel of labor productivity by evoking reciprocal behavior on the side of the employee. Training an employee focuses on the skill channel of productivity of labor as it increases the employee's productivity potential. By paying a wage that is perceived as fair (even after being trained), the employer can prevent a negative effort response and thus effectively increase productivity through the skill channel. When, however, the employee's pay falls below what he perceives as fair, neither training nor higher wages are effective for increasing labor productivity.

Table 4.4: Decoded words

Dependent variable: # decoded words	All		Fair wage norm ≤ 500 ECU		Fair wage norm > 500 ECU	
	(1)	(2)	(3)	(4)	(5)	(6)
T1 - No Training & Wage Increase	1.613**	1.699**	2.508**	2.653**	0.504	0.875
	(0.794)	(0.817)	(1.054)	(1.172)	(1.202)	(1.381)
T2 - Training & No Wage Increase	0.558	0.583	2.253*	1.685	-1.007	-1.111
	(0.956)	(0.959)	(1.205)	(1.236)	(1.485)	(1.580)
T3 - Training & Wage Increase	1.850**	1.960**	4.018***	4.376***	0.132	0.110
	(0.851)	(0.872)	(1.091)	(1.174)	(1.296)	(1.323)
# decoded words 1st working phase	1.094***	1.073***	1.043***	1.106***	1.136***	1.166***
	(0.045)	(0.052)	(0.087)	(0.093)	(0.048)	(0.060)
Constant	1.657	4.024	2.476	10.821**	1.195	2.698
	(1.184)	(3.199)	(2.294)	(4.836)	(1.305)	(4.488)
Additional controls	No	Yes	No	Yes	No	Yes
Observations	375	364	174	170	201	194
R-squared	0.652	0.670	0.639	0.682	0.673	0.687

Notes: Linear ordinary least squares regression with robust standard errors in parentheses. Additional control variables include: Fair wage norm - pre training/free time, session fixed effects, gender, questionnaire measures for envy, competitiveness, guilt, and reciprocity. Specifications (1) and (2) include all employees, specifications (3) and (4) the subgroup of employees that stated a fair wage norm ≤500 ECU, and specifications (5) and (6) the subgroup of employees that stated a fair wage norm > 500 ECU. *** p<0.01, ** p<0.05, * p<0.1.

4.5 Conclusion

Much research has been devoted to examining the effect of training on productivity and wages. We add to this literature by challenging the approach to consider the two effects independently from each other. Instead, we propose an additional behavioral mechanism according to which the wage the employee considers fair is shifted as a response to training. In turn, these increased wage expectations might affect the productivity-enhancing effect of training if the higher productivity potential is not compensated appropriately.

We find support for the hypothesized relationship between training participation and fair wage expectations both in field and laboratory settings. Additionally, our experimental results give insight into the specific behavioral mechanisms following training participation. While training can be effective in increasing the skills and thus productivity potential of an employee, our results indicate that this does not translate into productivity gains per se. Instead, a reduction of the time invested in working on the task, i.e., effort on the extensive margin, hinders the realization of the full productivity potential.[13] Only when combined with a wage increase that is considered fair will higher skills also lead to higher productivity.

It seems that, as suggested by the theoretical framework, only when the gap between the fair wage and the actual wage is closed, the employee is willing to release his productivity potential. Otherwise the increased skills might be substituted for effort. This implies that employers who want to effectively turn training investments into higher productivity, need to let their employees participate in the gains from training in a way that agents perceive as fair. Previous literature on the relationship between training and wages (compare section 4.1), however, suggests that employees in many cases benefit from training, i.e., increased wages, only to a very limited extent. Thus, firms might not be aware of the relationship between training and wage expectations. Not sharing the potential gains could leave a significant fraction of output potential in an economy untapped.

Our research adds an important behavioral factor to the cost-benefit analysis of firms and institutions when deciding whether or not to invest in training. We find empirical evidence for a trade-off between an increased skill level and higher wage demands, which can lead to subsequent negative effort responses. Therefore, returns on investment for firms might be lower than expected and in turn might lead to lower human capital investments by firms.

[13] Withdrawing effort can take various forms in the work environment. Similar to what we observe in our experiment, employees could increase shirking by, for example, taking longer breaks, distracting themselves with private affairs or slacking off when doing their work tasks. An increased efficiency resulting from higher skills also allows employees to finish their regular workload faster and thus to work less overtime and leave the office earlier. This would therefore be another way to recoup their share of the gains from training and would not result in a productivity gain per se.

4.6 Appendix to Chapter 4

Summary Statistics WeLL

Table 4.5: Summary statistics WeLL

	All		Training participants		Ex. non-participants	
	Mean	SD	Mean	SD	Mean	SD
Mean expected monthly income in Euros	2,434	1,196	2,482	1,215	2,185	1,062
Monthly income in Euros	2,631	844	2,671	832	2,425	872
Age	46	9.33	45.9	9.33	46.7	9.33
Female (1/0)	0.27	0.44	0.27	0.45	0.22	0.41
Secondary Education:						
Certificate of Secondary Education (1/0)	0.15	0.36	0.13	0.34	0.25	0.44
General Cert. of Secondary Education (1/0)	0.40	0.49	0.40	0.49	0.41	0.49
Adv. Technical College Entrance Qual. (1/0)	0.43	0.50	0.45	0.50	0.32	0.47
Other (1/0)	0.02	0.13	0.02	0.13	-	-
Occupational status:						
Low-skilled worker (1/0)	0.11	0.31	0.09	0.29	0.20	0.40
Skilled worker (1/0)	0.21	0.41	0.20	0.40	0.28	0.45
Master craftsman (1/0)	0.03	0.16	0.03	0.16	-	-
Clerical worker (1/0)	0.66	0.48	0.69	0.46	0.49	0.50
Tenure (years)	14.2	8.56	14.1	8.58	14.6	8.43
Labor market experience (years)	19.4	8.05	19.3	8.07	20.1	7.94
Establishment located in East Germany (1/0)	0.42	0.49	0.42	0.49	0.38	0.49
Establishment size:						
100 - 199 employees	0.13	0.34	0.12	0.33	0.19	0.39
200 - 499 employees	0.24	0.43	0.23	0.42	0.27	0.45
500 - 1,999 employees	0.63	0.48	0.64	0.48	0.53	0.50
Industry:						
Manufacturing (1/0)	0.52	0.50	0.49	0.50	0.69	0.46
Service (1/0)	0.48	0.50	0.51	0.50	0.31	0.46
N	3,592		3,010		582	

Additional Regressions WeLL

Table 4.6: Training and wage expectations (alternative specification)

Dependent variable: Mean expected monthly income in Euros	(1)	(2)	(3)	(4)
Training	303.569***	92.953***	35.83	47.858*
	(38.157)	(29.745)	(26.091)	(27.203)
Lagged monthly income in Euros			0.858***	0.911***
			(0.038)	(0.055)
Constant	1,988.295***	-930.362***	272.693	744.345**
	(38.986)	(294.628)	(233.398)	(310.076)
Employee controls	No	Yes	Yes	Yes
Establishment controls & FE	No	No	No	Yes
Observations	7,681	7,681	7,681	7,681
# of clusters	4,322	4,322	4,322	4,322
R-squared	0.009	0.323	0.490	0.521

Notes: Linear regression with robust standard errors clustered on individuals in parentheses. All specifications include year fixed effects. Employee controls include: gender, age, age (squared), secondary education, occupational status, tenure, tenure (squared), labor market experience, labor market experience (squared). Establishment controls include: establishment location (east/west), establishment size, industry. *** p<0.01, ** p<0.05, * p<0.1.

Experimental Instructions

Instructions

Welcome to this experiment!

Please read the following instructions carefully. Do not hesitate to ask any questions. If you have any question, please, raise your hand. We will approach you and answer your question in private.

All your decisions remain anonymous. Communication with other participants during the whole experiment is not allowed. Please remove all items you brought with you from the table, switch off your phone and store the phone in your bag.

For showing up, you receive a show-up fee of 4 Euros. In the following experiment you can earn more money.

The money, you earn during the experiment, will be expressed in ECU (=Experimental Currency) and will be converted into Euros at the end of the experiment. The exchange rate is as follows, for **100 ECU** you will be paid **1 Euro** at the end of the experiment.

There are two different roles within this experiment: **Employer** and **Employees.** At the beginning of the experiment you will be randomly assigned to either one of these roles. The role assigned to you, will be shown on your screen at the beginning of this experiment and will **remain** the same for the **whole experiment**.

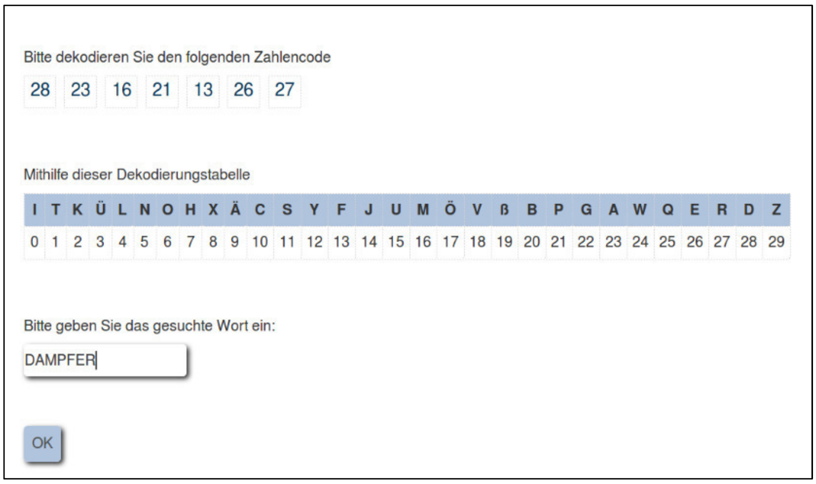
Every **employer** will be matched with **four employees**. You will neither during nor after the experiment know who was in the role of the employer or the employee, nor which employees were assigned to which employer.

Every **employee** participates in two working phases, with **each** having a **duration of 10 minutes**. During each working phase the employee is asked to work on a decoding task, where the employee is asked to translate numerical codes into words benefiting the employer.

Decoding task:

Within the decoding task **numerical codes** should be translated into **words**. To solve this task there is a **decoding table** beneath the numerical code, which **matches each number with a letter** (Please see example on page 2). With the aid of this decoding table the numerical code can be translated into a word.

Example:



In the decoding table it is shown, that 28=D, 23=A, 16=M, 21=P, 13=F, 26=E, 27=R. Therefore, the word that is searched for is **DAMPFER.**

Only if the numerical code is **correctly decoded**, you can approach the **next numerical code**. Each numerical code consists of a new numerical combination and each time there is a **new decoding table** shown. **Each word** only **appears once** during each working phase.

At the top on the left hand side of the window, the employee is able to see the **wage** for the particular working phase, the amount of **correctly decoded words**, as well as the **resulting amount of money** for the **employer**. At the top on the right hand side, the employee is able to see the **remaining time** for the particular working phase.

The **employer** will **neither during nor after the experiment be informed** about the amount of correctly decoded numerical codes by each employee.

Employees' pay-out

The particular employee receives a **fixed wage for each of the two working phases**. This wage will be paid to the employee by the **assigned employer independently of the number** of correctly coded words.

Employers' pay-out

One working phase will be randomly determined to be **relevant** for the employer's **pay-out.**

For **each numerical code**, that was **correctly decoded** by the particular employee in this **particular working phase**, the **employer** receives a **fixed amount of money**.

Screenshots Experiment

Bitte geben Sie nun **für jedes Lohnniveau** an, ob Sie denken, dass **mit Ihnen vergleichbare Arbeitnehmer**, d.h. Arbeitnehmer, die in diesem Experiment die gleichen Phasen wie Sie durchlaufen haben, das jeweilige Lohnniveau für **sehr unfair, etwas unfair, etwas fair** oder **sehr fair erachten.**

Lohn für die 2. Arbeitsphase entspricht...

	Sehr unfair	Etwas unfair	Etwas fair	Sehr fair
Weniger als 50 ECU				
50 ECU				
100 ECU				
150 ECU				
200 ECU				
250 ECU				
300 ECU				
350 ECU				
400 ECU				
450 ECU				
500 ECU				
550 ECU				
600 ECU				
650 ECU				
700 ECU				
mehr als 700 ECU				

weiter

Figure 4.4: Screenshot fair wage elicitation

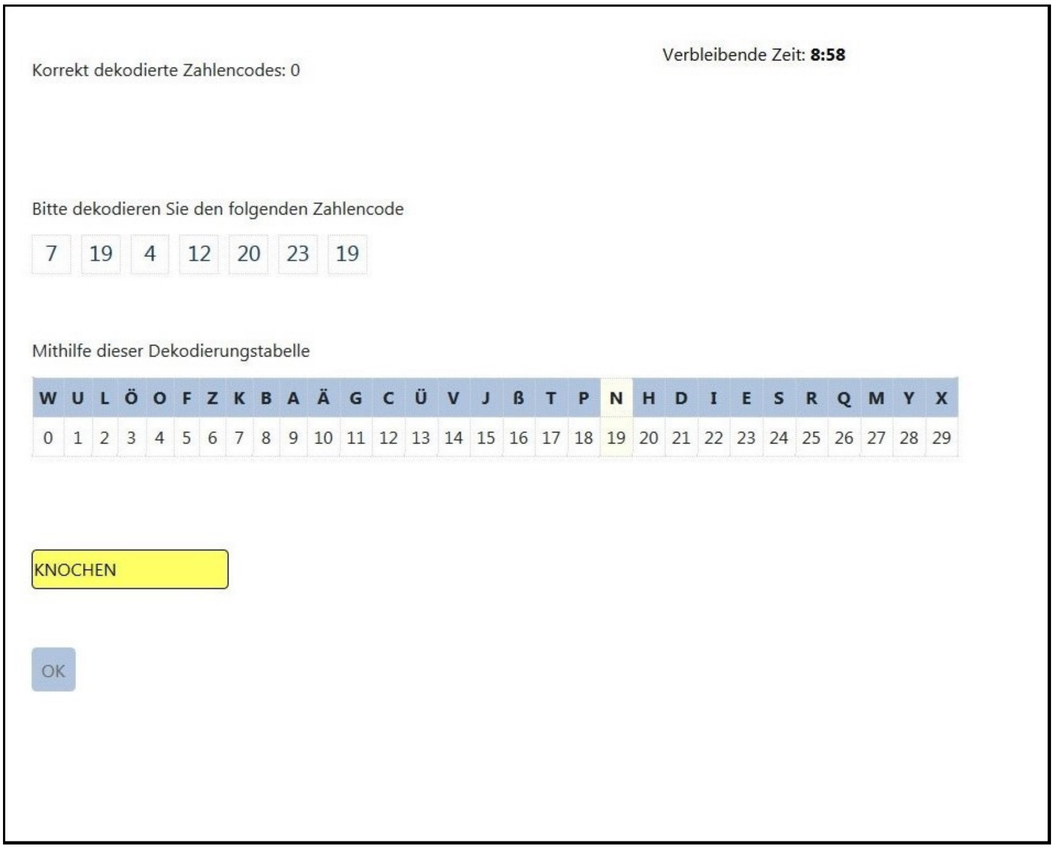


Figure 4.5: Screenshot training

Post-experimental Questionnaire

Cabin-No.: _____

Questionnaire

Please, answer the following questions while we prepare the pay-out.
Thank you!

1. What is your gender?	Female ☐	Male ☐
2. How old are you?		
3. How many siblings do you have?		

Please, answer the following questions using the given scale.

4. How satisfied are you with the experiment overall?

Not at all satisfied				Very satisfied
☐	☐	☐	☐	☐

5. How much would you like to participate again in an experiment like this one?

Not at all				Very much
☐	☐	☐	☐	☐

6. How likely is it that you would recommend to participate in an experiment like this one to a friend?

Not likely at all				Very much
☐	☐	☐	☐	☐

In case you were assigned to the role of an **employee**, please **answer** the **following questions**.

In case you were assigned to the role of an **employer**, we **do not need any further information**. Thank you very much for your participation.

7. How much did you enjoy the decoding task?				
Not at all				Very much
☐	☐	☐	☐	☐
8. How tedious did you find the decoding task?				
Not tedious at all				Very tedious
☐	☐	☐	☐	☐
9. How fair, do you think, was the wage in the **first working phase**?				
Not fair at all				Very fair
☐	☐	☐	☐	☐
10. Did you feel disadvantaged or advantaged by the wage in the **first working phase**?				
Very disadvantaged				Very advantaged
☐	☐	☐	☐	☐
11. What do you think, how was your performance in the **first working phase** compared to other participants in this room?				
Below average				Above average
☐	☐	☐	☐	☐
12. How fair, do you think, was the wage in the **second working phase**?				
Not fair at all				Very fair
☐	☐	☐	☐	☐

13. Did you feel disadvantaged or advantaged by the wage in the **second working phase**?				
Very disadvantaged				Very advantaged
☐	☐	☐	☐	☐
14. What do you think, how was your performance in the **second working phase** compared to other participants in this room?				
Below average				Above average
☐	☐	☐	☐	☐

15. Did you decode more, about the same or fewer numerical codes in the second working phase compared to the first working phase?		
More	About the same	Fewer
☐	☐	☐

16. For what reason did you decode more, about the same or fewer numerical codes in the second working phase compared to the first working phase? *For the success of this study, it is very crucial, that we understand why you decoded more, about the same or fewer numerical codes in the second working phase. Please, answer as precise and detailed as possible. Thank you very much.*
Answer:

Did you participate in a training phase before the second working phase?

Yes	No
☐	☐
	In case the answer is no, please skip this page and continue on the next page.

17. How much did you enjoy the training phase?

Not at all				Very much
☐	☐	☐	☐	☐

18. How tedious did you find the training phase?

Not tedious at all				Very tedious
☐	☐	☐	☐	☐

19. Do you believe that the training has enabled you to correctly decode more numerical codes in the second working phase?

Not at all				Very much
☐	☐	☐	☐	☐

20. Do you believe that your employer had an advantage in the second working phase, because you participated in the training phase?

Not at all				Very much
☐	☐	☐	☐	☐

21. Why (not)?

Answer:

22. Are you of the opinion that the participation in the training phase should go along with a higher wage in the second working phase?

Not at all				Very much
☐	☐	☐	☐	☐

For the following questions please indicate to what extend the statements apply to you.				
1. It annoys me when others are undeservedly better off than I am.				
Does not apply at all				Applies very much
☐	☐	☐	☐	☐
2. I feel guilty when I am undeservedly better off than others.				
Does not apply at all				Applies very much
☐	☐	☐	☐	☐
3. If someone harms me on purpose, I will try to repay that person with the same coin, even if it costs me something.				
Does not apply at all				Applies very much
☐	☐	☐	☐	☐
4. If someone does me a favour, I am willing to reciprocate it.				
Does not apply at all				Applies very much
☐	☐	☐	☐	☐
5. I like to compete with others.				
Does not apply at all				Applies very much
☐	☐	☐	☐	☐
6. Generally, it is important to me to be the best.				
Does not apply at all				Applies very much
☐	☐	☐	☐	☐

If you have any further comments on this experiment, do not hesitate to note it here:

Thank you very much for your time and participation!

Summary Statistics Experiment

Table 4.7: Summary statistics experiment

First working phase

	T0			T1			T2			T3		
	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD
Fair wage in ECU	92	521	136	92	541	116	93	531	139	92	539	106
Average decoding time (min.)	92	.442	.192	95	.475	.271	93	.462	.269	92	.442	.225
Minutes worked	93	9.07	1.85	95	9.12	1.73	94	9.01	1.78	93	9.14	1.61
# decoded words	93	22.6	7.01	95	22.3	7.96	94	22.7	8.19	93	22.8	6.82
# decoded words/minute worked	92	2.47	.572	95	2.41	.682	93	2.48	.694	92	2.47	.549

	T0			T1			T2			T3		
	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD
Fair wage in ECU	86	468	175	90	496	158	90	518	155	93	541	119
Average decoding time (min.)	91	.424	.422	93	.376	.217	92	.442	.551	89	.324	.064
Minutes worked	93	8.98	2.07	95	9.14	1.81	94	8.56	2.58	93	8.82	2.27
# decoded words	93	26.4	9.56	95	27.7	8.87	94	27.1	12.3	93	28.5	9.72
# decoded words/minute worked	91	2.89	.782	93	2.99	.747	92	3.09	1.01	89	3.21	.692

Explanation of treatments: T0 - No Training & No Wage Increase, T1 - No Training & Wage Increase,
T2 - Training & No Wage Increase, T3 - Training & Wage Increase

Additional Regressions Experiment

Table 4.8: Decoding time

Dependent variable:	Trained words		Untrained words		Overall	
Decoding time (min.)	(1)	(2)	(3)	(4)	(5)	(6)
T1 - No Training & Wage Increase	-0.010	-0.006	-0.013	-0.006	-0.011	-0.006
	(0.012)	(0.013)	(0.012)	(0.012)	(0.011)	(0.011)
T2 - Training & No Wage Increase	-0.050***	-0.046***	0.011	0.017	-0.019	-0.014
	(0.013)	(0.013)	(0.014)	(0.015)	(0.012)	(0.013)
T3 - Training & Wage Increase	-0.055***	-0.050***	-0.004	0.008	-0.029***	-0.021**
	(0.011)	(0.012)	(0.011)	(0.010)	(0.010)	(0.010)
Mean decoding time (min.)	-0.016**	-0.015**	-0.041**	-0.030**	-0.029**	-0.023**
1st working phase	(0.007)	(0.007)	(0.016)	(0.013)	(0.011)	(0.009)
Constant	0.530***	0.568***	0.777***	0.723***	0.682***	0.665***
	(0.071)	(0.077)	(0.164)	(0.133)	(0.117)	(0.099)
Additional controls	No	Yes	No	Yes	No	Yes
Observations	5,056	4,930	5,221	5,088	10,277	10,018
# clusters	362	352	365	355	365	355
R-squared	0.064	0.075	0.058	0.063	0.058	0.065

Notes: Linear regression with robust standard errors clustered on indvidual in parentheses. All specifications include word fixed effects. Additional control variables include: Fair wage norm - pre training/free time, session fixed effects, gender, questionnaire measures for envy, competitiveness, guilt, and reciprocity. *** p<0.01, ** p<0.05, * p<0.1.

Table 4.9: Decoding time - heterogenous effects

	Fair wage norm $\leq$ 500 ECU					
Dependent variable:	Trained words		Untrained words		Overall	
Decoding time (min.)	(1)	(2)	(3)	(4)	(5)	(6)
T1 - No Training & Wage Increase	0.009	0.018	0.000	0.014	0.005	0.016
	(0.016)	(0.019)	(0.015)	(0.021)	(0.014)	(0.018)
T2 - Training & No Wage Increase	-0.055***	-0.047***	0.011	0.008	-0.022	-0.019
	(0.015)	(0.016)	(0.016)	(0.017)	(0.014)	(0.015)
T3 - Training & Wage Increase	-0.052***	-0.044***	0.010	0.017	-0.021	-0.013
	(0.014)	(0.015)	(0.015)	(0.017)	(0.014)	(0.015)
Mean decoding time (min.)	-0.007	-0.007	-0.014	-0.012	-0.010	-0.009
1st working phase	(0.008)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)
Constant	0.422***	0.515***	0.486***	0.662***	0.467***	0.602***
	(0.074)	(0.099)	(0.085)	(0.142)	(0.077)	(0.110)
Additional controls	No	Yes	No	Yes	No	Yes
Observations	2,425	2,373	2,501	2,447	4,926	4,820
# of clusters	172	168	172	168	172	168
R-squared	0.099	0.117	0.051	0.088	0.067	0.095

	Fair wage norm > 500 ECU					
Dependent variable:	Trained words		Untrained words		Overall	
Decoding time (min.)	(1)	(2)	(3)	(4)	(5)	(6)
T1 - No Training & Wage Increase	-0.028	-0.035	-0.022	-0.031	-0.025	-0.033*
	(0.018)	(0.021)	(0.018)	(0.019)	(0.016)	(0.018)
T2 - Training & No Wage Increase	-0.048**	-0.047**	0.009	0.023	-0.019	-0.011
	(0.021)	(0.024)	(0.022)	(0.024)	(0.019)	(0.021)
T3 - Training & Wage Increase	-0.060***	-0.059***	-0.012	-0.005	-0.036**	-0.032**
	(0.017)	(0.019)	(0.015)	(0.015)	(0.014)	(0.015)
Mean decoding time (min.)	-0.028***	-0.023**	-0.073***	-0.048**	-0.052***	-0.036**
1st working phase	(0.008)	(0.011)	(0.024)	(0.024)	(0.015)	(0.015)
Constant	0.660***	0.700***	1.102***	0.866***	0.928***	0.812***
	(0.086)	(0.115)	(0.250)	(0.246)	(0.168)	(0.169)
Additional controls	No	Yes	No	Yes	No	Yes
Observations	2,631	2,557	2,720	2,641	5,351	5,198
# of clusters	190	184	193	187	193	187
R-squared	0.058	0.073	0.084	0.076	0.071	0.071

Notes: Linear regression with robust standard errors clustered on indvidiual in parentheses. All specifications include word fixed effects. Additional control variables include: Fair wage norm - pre training/free time, session fixed effects, gender, questionnaire measures for envy, competitiveness, guilt, and reciprocity. *** p<0.01, ** p<0.05, * p<0.1.

Table 4.10: Time invested - heterogenous effects (alternative specification)

Dependent variable:	Fair wage norm ≤ actual wage		Fair wage norm > actual wage	
Time invested (min.)	(1)	(2)	(3)	(4)
T1 - No Training & Wage Increase	0.372	0.295	-0.238	-0.311
	(0.387)	(0.358)	(0.254)	(0.270)
T2 - Training & No Wage Increase	-0.475	-0.495	-0.455	-0.506
	(0.640)	(0.615)	(0.290)	(0.310)
T3 - Training & Wage Increase	0.142	0.061	-0.562*	-0.596*
	(0.406)	(0.332)	(0.314)	(0.335)
Time invested (min.) 1st working phase	0.550**	0.574**	0.940***	0.961***
	(0.262)	(0.220)	(0.094)	(0.093)
Constant	4.041	3.501	0.517	0.292
	(2.542)	(2.333)	(0.958)	(1.419)
Additional controls	No	Yes	No	Yes
Observations	118	117	241	238
R-squared	0.318	0.452	0.497	0.528

Notes: Linear regression with robust standard errors in parentheses.
Subgroup analysis for employees that stated a fair wage norm ≤actual wage or > actual wage respectively.
Additional control variables include: Fair wage norm - pre training/free time, session fixed effects, gender, questionnaire measures for envy, competitiveness, guilt, and reciprocity. *** p<0.01, ** p<0.05, * p<0.1.

Table 4.11: Decoded words - heterogenous effects (alternative specification)

Dependent variable:	Fair wage norm $\leq$ actual wage		Fair wage norm > actual wage	
# Decoded words	(1)	(2)	(3)	(4)
T1 - No Training & Wage Increase	1.596	1.316	1.159	1.672
	(1.540)	(1.546)	(1.009)	(1.227)
T2 - Training & No Wage Increase	-0.227	-2.314	0.591	0.834
	(2.338)	(2.929)	(1.101)	(1.218)
T3 - Training & Wage Increase	3.101*	2.447	0.916	1.038
	(1.579)	(1.630)	(1.082)	(1.195)
# decoded words 1st working phase	0.973***	1.027***	1.147***	1.166***
	(0.119)	(0.121)	(0.043)	(0.053)
Constant	4.946	11.495	0.158	4.296
	(3.111)	(7.303)	(1.195)	(4.402)
Additional controls	No	Yes	No	Yes
Observations	118	115	241	237
R-squared	0.581	0.664	0.680	0.692

Notes: Linear regression with robust standard errors in parentheses.

Subgroup analysis for employees that stated a fair wage norm $\leq$actual wage or > actual wage respectively.

Additional control variables include: Fair wage norm - pre training/free time, session fixed effects, gender, questionnaire measures for envy, competitiveness, guilt, and reciprocity. *** p<0.01, ** p<0.05, * p<0.1.

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