Universitetet i Stavanger

DOES A CHANGE IN PERFORMANCE PAY HAVE AN EFFECT ON WORKERS' PRODUCTIVITY?

- An empirical analysis of a change in performance pay in Company X.

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Master's Thesis in Business Administration
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TITTEL:
Har en endring i prestasjonsbasert lønn en effekt på ansattes produktivitet?
- En empirisk analyse av en endring i prestasjonsbasert lønn i Bedrift X.

ENGELSK TITTEL:
Does a Change in Performance Pay have an Effect on Workers' Productivity?
- An empirical analysis of a change in performance pay in Company X.

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Abstract

This thesis investigates whether a change in performance payment (PP) have an effect on workers' productivity. During the period 2009 to 2015, dealer stores affiliated Company X experienced several PP changes. Some dealer stores experienced an increase in PP, other dealer stores experienced a reduction in PP, whereas some experienced no change. The dealer stores are divided into two groups, those dealer stores who experienced an increase in PP and those who did not. To analyze whether the change in PP affect workers' productivity, and to address the problem of endogeneity, I have conducted a natural experiment and a difference-in-differences method (DD), such that the differences between the two groups before and after the PP change are being compared.

The main analysis is based on the PP change that occurred in 2012, and results indicate a small positive effect, which is not statistically significant. Hence, there is no evidence to support the hypothesis that the change of PP leads to higher sales for the workers at those dealer stores who receives an increase in performance pay in 2012. A greater PP change occurred in 2014, and that effect is greater and statistically significant.

Preface

This thesis represent the final piece of a two-year master's degree in Business Administration at the University of Stavanger (UiS).

First, I want to thank my supervisor, Venke Furre Haaland, for her availability, advice and for her constructive criticism. Her advice and feedback has been very useful and greatly appreciated.

In addition, I would like to thank Company M and Company X for providing me with data and a great thank you to Glenn for providing me with organizational information and for giving me the opportunity to write about the interesting topic within human resource management. Thank you so much. Any errors or omissions in this paper are solely my own responsibility.

Stine Sund Ottersen

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1. Introduction

Incentives are the essence of economics, and the idea behind paying for performance is to motivate individuals to increase their effort, and hence their output (Lazear & Shaw, 2007; Prendergast, 1999). This paper will focus on the performance measurement and the power of incentives. I am analyzing workers' productivity, namely sales, and the objective is to investigate whether a change in performance payment (PP) in Company X, a Norwegian service company, has an effect on workers productivity.

My hypothesis addresses the issue of whether an increase in PP increases workers productivity. Workers, who experience an increase in performance payment, would have an incentive to increase output as it increases his or her pay, and thus the worker achieves additional benefit, so employers tend to respond strongly to incentives (Lazear & Gibbs, 2014). There are at least two important theories explaining why a change in performance payment should increase performance. Firstly, the performance payment can improve worker performance through direct incentive effects as workers expend more effort creating the outputs that are rewarded through pay (Lazear, 1986). A piece-rate worker chooses how much output to produce at the firm, and the more effort given, the greater his or her take-home salary. Because a piece-rate workers' salary depends strictly on how much the worker produce or sell, he or she would generally "work hard for the money". Hence, economists assume workers are motivated by monetary rewards, and they can be induced to expend greater effort in a task if those efforts are rewarded directly through performance payment (Bryson, Buraimo, & Simmons, 2011). Secondly, an increase in performance payment could improve workers productivity through worker sorting (Lazear, 1986). By offering performance based payment, firms may hire a better distribution of workers and keep their high producing workers from resigning (Prendergast, 1999). Based on theory, I predict that an increase in performance payment will improve performance. The assumption is that the increase will lead to greater motivation and effort, resulting in improved performance (Ariely, Gneezy, Loewenstein, & Mazar, 2009).

Naturally, economists have emphasized that some people are likely to work harder when there are tangible rewards. Personnel economics assumes that workers and firms are rational maximizing agents, seeking utility and profit where wages are determined by skills and productivity, which is in part determined by compensation through an incentive structure

(Lazear & Shaw, 2007). The productivity of workers differs, either because there are differences in ability across workers or because some workers put in more effort on the job than other workers do. However, the likelihood that performance related pay will encourage more effort raises some questions about identifying causal relationships carefully. Companies that use performance payment and have a high proportion of salary tied to performance may also be companies that have many workers who are in general more productive. Thus, we would observe a correlation between PP and productivity regardless of whether it really is a causal relationship. To examine the causal relationship between PP and productivity, I employ data from Company X.

Company X provide customers with service products mainly through dealer stores located in all of Norway's 19 counties. The dealer stores receives a performance payment (PP) from Company X for each service product sold (SALE2), and then the dealer stores pays their workers a piece rate (PP#2) for that same service product they sold (SALE2). Monthly sales per worker has been collected and analyzed over 84 consecutive periods, from 2009 to 2015. During this period, the dealer stores has experienced several changes in the PP, and the dealer stores has experienced the PP changes differently. Some dealer stores experienced an increase in the PP; other dealer stores experienced a decrease, whereas some experienced no change. For this reason, the workers at the dealer stores are ideal as a research objective for this study, in order to address my research question: '*Does a change in performance pay in Company X have an effect on workers' productivity?'* I am typically interested in whether or not the workers at the dealer stores who experienced an increase in performance pay in January 2012 became more productive compared to the workers at dealer stores that did not experience an increase.

For this analysis, I employ cross-sectional panel data from 119 dealer stores received from Company X. The data contains information about each worker's monthly sales for a total period of 84 months. Average monthly sales are the dependent variable that measures workers' productivity. The dealer stores are divided into two groups, those dealer stores who experienced an increase in performance payment (PP) is the treatment group, whereas those who experienced no change or a reduction in PP is in the control group. To analyze whether the change in PP has had an effect on the worker's productivity, and to address the problem of endogeneity, I have conducted a natural experiment and a difference-in-differences method (DD), such that the differences between the treated group and the control group prior and after the PP change are being compared. The increase or decrease in the difference becomes an estimate of the treatment effect, a difference in differences (DD) estimate. My analysis control for store- and time fixed effects, which eliminates effects that vary between stores, but not vary in time, and vice versa. I also control for individual fixed effects.

There are obviously various issues that may affect how well Company X perform, whether it is related to top management, uncontrollable factors such as consumer spending level, price levels, interest rates, rules and regulations and so on. There are also numerous reasons why workers are motivated in their job, but in this paper, I will focus on employee's behavioral aspects of performance and productivity related to performance payment.

There is a growing empirical literature investigating the effect of performance payment on performance, and the assumption is typically premised on the assumption that it matters for performance (Prendergast, 2015). A natural experiment conducted by Lazear (2000a), examining the behavior of 3000 workers in a large auto glass company, comparing person-specific data before and after a change in a pay scheme, found that the productivity of workers increased by 44% when they moved from hourly wages to piece-rate. Further, a study by Shearer (2004) confirms the existence of an incentive effect and reveals a 20% increase in worker productivity when workers are paid piece rates rather than fixed wages. On the other side, Kvaløy, Nieken, & Schötter (2015) and Deci & Lanzetta (1971) find that piece-rate pay could be damaging on performance if not accompanied by intrinsic factors such as motivational talk or feedback. Additionally, Frey & Jegen (2001) argue that performance payment likely reduces output as it harms intrinsic motivation, and the study by Ariely, Gneezy, Loewenstein, & Mazar (2009) find evidence of workers with very high reward levels had a detrimental effect on their performance.

Moreover, several studies support that one of the benefits with performance payment is the firm's opportunity to hire a better distribution of workers, to change the productivity of existing workers, and it might reduce resignations among the most productive workers (Cadena X., Schoar A., Christea A., & Delgado-Medrano H., 2011; Lazear, 2000a; Lazear & Shaw, 2007; Prendergast, 1999). Unlike Lazear (2000a) who investigated worker's productivity when moving from hourly wages to piece-rate, my experiment investigate worker's performance after an increase in performance payment. If my analysis shows that an increase in performance payment has a positive effect on workers performance, then this paper may contribute to the support of the existing literature declaring that monetary incentives affect performance.

The analysis of the PP change in 2012 suggest there is a treatment effect, but the regression results are inconclusive. My analysis suggest that increasing performance pay has very small but positive effect on productivity. The main results suggests a 2.64 percent increase in the average monthly sales for the treatment group compared to the monthly sales of the control group after the PP change. Notably, this relationship is not statistically different from zero. Thus, I cannot conclude whether the workers affiliated Company X was affected by the PP change or not. The rich data allow me to investigate if there are non-parallel trends in sales between treated and control dealer stores before the change in PP. Such non-parallel pre-trend could bias the estimation results. Results shows that the difference in sales between the treated and control groups before the change in PP are small in magnitude and not statistically different from zero. This suggest that the small and insignificant effect observed from the change in PP, is not biased by unparalleled pre-trends in sale.

In 2014, Company X induced an additional change in its PP. The results are large in magnitude and statistically different from zero. In contrast to the change in 2012, the stores could voluntarily implement a change in their PP in 2014. Thus, estimation results utilizing this change in PP has to be interpreted with caution.

The paper is organized in 7 sections. After the introduction, I present the incentive theory and the hypothesis. In chapter 3, I present background information on the firms, the performance payment scheme before the changes and the performance payment changes. This is followed by a presentation of the data in chapter 4 and the empirical strategy in chapter 5. Then, chapter 6 will present empirical results, and implications for the performance payment changes will be identified and discussed. Finally, I draw a conclusion based on the results and discuss final views on these.

2. Theory and Hypothesis

With an increasing globalized society, the business environment is becoming more demanding, firms are faced with higher competition, and therefore, it seems important to know what motivates workers to give higher effort at work. Theory suggests that monetary incentives make workers more motivated and therefore, a firm's payment scheme is an important tool for motivating employees to be more productive. According to Merchant & Van der Stede (2007), the performance management system will secure high productivity and motivation in the workplace if designed and implemented successfully. Consider a worker who takes an unobservable action a to sell output q. The production function might be linear and represented by $q = a + \varepsilon$, where ε is the noise term. The employer owns the output but contracts to share it with the worker by paying a wage Wt contingent on how many output sold at time t. Also, the wage contract might be linear, $Wt = s + bq$, where the intercept s is the base salary and the slope b is the piece rate (PP#2). The worker's pay off at time t is $Wt - c(a)$, the realized wage minus the disutility of action $c(a)$. The employer's payoff at time t is then, $q - Wt$, the realized output net of wages. The problem in question is 'does a change in Wt have an effect on workers' productivity, the number of output q sold?'

2.1 Why should we expect an effect on worker productivity by a change in the performance payment from Company X?

An incentive is something that encourages action and choice, considered as a motivator. Lazear (2000a) argues that workers respond to incentives, which is the cornerstone of the theory in personnel economics, as well as the underlying theory of this paper. The idea of the power of incentives closely relates to the understanding of rationality. One assumes that if an action provides greater benefit than another does, one would choose that action. A consequence of this assumption is that the proper use of incentives can control the actions of an economic operator. The individual will follow their preferences, in order to maximize its benefits and minimize costs. The idea behind incentive schemes is that wages linked to productivity and performance will encourage workers to work harder as their welfare depends on the result he or she produces. Internationally, performance based incentives is very common, and large multinational corporations use strong performance incentives. An increasing number of Norwegian companies induce performance pay (Bragelien, 2003). Salaries tied to performance is about to become the norm and not the exception. However, performance pay is not a new phenomenon in Norway, and have long been prevalent in many industries, particularly in agriculture and in the fisheries sectors.

Lazear (1986) argues that there are three important issues affecting a firm's choice of incentive scheme: inducing appropriate effort levels, sorting workers across jobs, and selecting quantity versus quality of output. These three factors affect workers productivity and directly relates to piece-rate pay. Moreover, piece rates are likely to be used over salaries when the cost of measuring output is low, the value of the alternative wage is high relative to average output at the current firm, workers are heterogeneous in ability levels, and when output is measured without too much error (Lazear, 2000a). This all applies well to the workers affiliated Company X, where the cost of measuring output is low and output is measured without too much error. In the following sub sections, I will explain three important issues related to the performance payment i.e. piece-rate compensation. First, I will talk about why a piece-rate compensation scheme affect worker effort. Secondly, I will explain the phenomena sorting, and thirdly I will discuss the quality vs. quantity when using piece-rate compensation.

2.1.1 Piece-rate Compensation and Worker Effort

The productivity of workers differs, either because there are differences in ability across workers or because some workers put in more effort on the job than other workers do. The most important reasons to tie pay to performance is to increase employee efforts and better align them with firm interest (Lazear & Gibbs, 2014). A piece-rate worker chooses how much output to produce at the firm and I assume that the worker chooses the level of effort that maximizes his or her utility. The more effort given, the greater his or her take-home salary, hence, the greater his or her utility (Borjas, 2013). Using a monetary incentive scheme, the employers are motivated to do something they would otherwise not be motivated to do. When looking at a worker affiliated Company X who works as a salesperson selling different products to customers. He or she receives a piece rate for each item sold, such that his or her payment ties directly to how many sales that he or she completes. The more the worker sells, the higher will the payment be. Therefore, piece work and incentive plans are known to be very effective for certain types of jobs (Bragelien, 2003). Economists assume workers are motivated by monetary rewards, and they can be induced to expend greater effort in a task if those efforts are rewarded directly through performance-related pay (Bryson et al., 2011).

Lazear (2000a) argues that workers respond to incentives. Incentive effects is the effect that compensation policies have on worker productivity (Paarsch & Shearer, 2000). In general, incentive schemes are distinguished between fixed pay and performance pay. Although there are several variations of the two, my study will focus on piece-rate as performance pay (PP), as the salary of workers affiliated Company X is highly affected on number of items sold. In this paper, the workers are salespersons paid on a strict commission basis and are therefore piece-rate workers. The distinguishable feature of a piece rate compared to a fixed pay such as salary is that, with piece rate, the worker's payment in a given period t, is related to output q, in that period (Lazear, 1986). If a worker is paid a piece rate, then his or her compensation in period t, is

$$Wt = \mathrm{f(qt)},$$

where qt is worker output in period t. Moreover, pay for performance is used in a wide variety of jobs, where workers are rewarded for their efforts based on observed measures of performance (Ariely et al., 2009). Variable pay provides incentives to put forth effort and, by paying based on output induces workers to sell more output (Lazear, 2000b). Therefore, the primary motivation behind a piece-rate scheme is according to Lazear (1986, 2000a) to induce workers motivation, whereas straight salaries which do not directly tie the agent's pay to their current-period performance, provides no direct incentives. According to economic theory, workers provides the minimum possible effort when fixed pay such as salary is used, as more effort will not give the worker additional benefit (Shearer, 2004). On the contrary, under a piece-rate incentive scheme, the worker has an incentive to increase output as it increases his or her pay, and give the worker additional benefit. Thus performance payment can improve worker performance through direct incentive effects as workers expend more effort creating the outputs that are rewarded through pay (Lazear, 1986). It is suggested that the incentive-earnings effect is in part a compensating differential for the greater risk borne by piece-rate workers and is in part a pure effort effect (Seiler, 1984). Productivity in piece-rate firms is according to Lazear (1986) higher than productivity in salary firms, but this does not imply that, if all salary firms were to pay piece rates, output would rise; the opposite is true.

Lazear & Shaw (2007) believes there is a possibility that paying for performance can induce people to work harder and Cadena et al. (2011) follows up stating that it is a given that paying on the basis of output, will induce workers to supply more output. Naturally, economists have also emphasized that some people are likely to work harder when there are tangible rewards. However, the likelihood that performance related pay will encourage more effort raises some questions about identifying causal relationships carefully as financial rewards is not the only thing that matters. Additionally, to motivate individuals to increase their effort and align the workers objectives in the direction of the firm's, require that the firm have knowledge about the incentive's effect on motivation and performance (Ariely et al., 2009).

2.1.2 Piece-rate Compensation and Sorting

Another mechanics in which an increase in piece-rate compensation could improve workers productivity is through worker sorting. Lazear & Shaw (2007) argues that some firms observe that piece rate pay induces the most productive workers to join the firm, as well as changing the productivity of existing workers. High ability workers have more to gain from a pay system which rewards them according to their performance (Lazear, 1986). Due to the fact that a piece rate allows higher ability workers to work harder and receive more from the job than an hourly wage does, high-ability workers often prefer piece rates (Lazear, 2000a). A worker who knows he or she is a high-ability worker and prefer to work at high levels of effort would more likely apply for a job that use a piece rate scheme than would a low-ability worker. Thus by offering performance based payment, firms may hire a better distribution of workers and keep their high productivity workers (Prendergast, 1999).

Moreover, pay that is mildly related to output can be very powerful in sorting workers and provide information. Pay that is related to effort, like salaries or hourly wages, is effective in generating incentives for a homogenous workforce, but does not do well in catering to worker differences (Lazear, 2000b). Consequently, heterogeneity, not power, is the primary reason for using variable pay schemes. If ability information is asymmetric, where workers have better information than firms about their output potential, then the least able workers work at the salary firms. The obvious implication is that, for a given occupation, firms that pay workers a straight salary have a lower-quality work force than have firms that pay piece rates. In other words, the low ability workers are the ones who are unwilling to bear the monitoring costs necessary to distinguish abilities (Lazear, 1986).

According to theory, those who believe that they will be most productive at the firm are more likely to apply for or stay at a job there. Similarly, workers affiliated Company X will according to theory, have greater motivation to invest in skills, because the return on skills will be higher as their performance is strongly tied to pay. In addition, high-ability workers are those who are more likely to apply for a job as a seller for Company X, and believes that he or she will sell many products receiving a higher payment than if paid an hourly rate.

2.1.3 Piece-rate Compensation and Quality vs. Quantity

Pay for performance have been praised for promoting achievements, but also criticized as a source of dysfunctional behavior in the workplace (Prendergast, 2015). A common form of distortion in performance measures involves quantity versus quality of production. If quantity is easily measured, quality is often very difficult to assess accurately and in a timely manner (Lazear & Gibbs, 2014). For these reasons, it is common for numeric performance measures to focus more on quantity of output rather than its quality and this can distort behavior and may lead to quality problems. Lazear (1986) argues that piece rates may sometimes induce the worker to produce too many low-quality goods and that salaries would avoid this problem. However, in more recent studies Lazear (1995, p. 24) also recognizes the tradeoff between quantity and quality with a piece rate but emphasizes that firms can overcome this problem stating that “there is always an appropriate compensation formula that will induce workers to put forth the right amount of effort towards quantity and quality”. The salary of the workers affiliated Company X, is closely tied to performance, and is partly based on the level of Customer Service Satisfaction (CSS) achieved. When a worker sell a product or service he or she want satisfied customers, which will increase the probability of them returning and thus buy more products in the future. A satisfied returning customer and the probability of future sales have higher value to the worker than to get that one sale, and no future sales. The worker will receive a bonus for the satisfied customer and for the sale. Thus, the CSS bonus ensures that the quantity sold by the workers will not be at the expense of the quality (i.e. the customer’s satisfaction).

2.2 Ratchet Effect

Some workers are high ability workers who perform very well, in fact so well that the managers believe that the job is too easy and wonder if they should reduce the piece rate to keep more of the revenue. This is according to theory a bad idea, and may result in lower motivation and quits. The ratchet effect occurs when a worker underperforms, and workers in piece-rate firms fear this well-known effect. The ratchet effect is caused by the ratchet: doing more now reduces future rewards (Brown, Miller, & Thornton, 1994). When workers produce more in one period, the firm’s managers might interpret the high level of production or sales as evidence that the job was easier than they thought, and they are paying their workers too much (Borjas, 2013). Consequently, the piece-rate is lowered in the next period, and the workers have to work even harder just to keep even. The ratchet effect discourages workers from accepting piece-rate jobs, as well as adopting more efficient production techniques (Borjas, 2013, p. 469). Fehr & Falk (1999) state that employers may be reluctant to give wage cuts because they are concerned that workers will behave reciprocal and punish the employer for giving them lower pay. Also, recent research shows that credible promises by the firm of not cutting the piece rates can be very effective as it can induce the workers to become more efficient and to outperform competitors (Borjas, 2013). Company X reduced their piece rate paid to some of the dealer stores. However, the dealer stores most likely did not reduce the piece rate paid to their workers[1], as it would most likely demotivate their workers to sell more. Thus, the dealer stores took the loss themselves, as the loss of reduced sales would most likely be greater if they reduced their workers piece rate.

Motivation and Performance

The understanding of what motivates workers are important in the process of figuring out whether an incentive system is productive. More importantly is to figure out if monetary reward is motivating the workers to be more productive. When workers feel that their manager treats them kindly, they will respond by being kind to the manager. This means that if the manager gives them a higher pay, the workers will respond by giving higher effort at work (Dufwenberg & Kirchsteiger, 2000). According to Fehr, Gachter & Kirchsteiger (1997), both workers and firms act reciprocal if they have the opportunity to do so. The theory in Akerlof and Yellen'study (1990) states that workers care about fair wages and that they respond to their wages by giving more or less effort. The idea is that workers give more effort, and therefore are more productive when receiving a wage that is considered fair, and contrary when receiving an unfair wage.

[1] The issue is discussed with managers at some of the dealer stores that did not experience an increase in PP.

2.3 Hypothesis

My hypothesis addresses the issue of whether an increase in performance payment increases workers productivity. A worker would according to theory outlined in Section 2.1, have an incentive to increase output if the piece rate increases, as it increases his or her pay, and the worker achieves additional benefit. Based on this, I predict that an increase in performance pay will increase workers productivity.

Hypothesis

> *H0: The change of PP has no impact on sales.*
> *H1: The change of PP leads to higher sales for the workers at those dealer stores who receives an increase in performance pay.*

According to basic utility maximizing theory, individuals will not expose themselves to costs (in terms of effort) unless it leads to increased utility. This is because performance is directly related to compensation, which means that additional effort accrues additional benefit.

2.4 Empirical Literature

The enormous literature on this subject is typically premised on the assumption that performance payment matters for performance (Prendergast, 2015). Several well-known studies address how incentive pay affects performance by using data on individual firms. However, some of these exercises are narrowly focused, as they are not performed in a setting where the incentives are exogenously changed, thus the change in productivity may only be reflected by the sorting of workers. Additionally, there is a lot of disagreement among existing studies. However, some recent evidence that take endogeneity into account, suggest that performance payment can induce workers motivation and increase performance (Ariely et al., 2009). The Standford professor Edward Lazear (2000a) examined the behavior of 3000 workers in a large auto glass company, Safelite Glass Corporation, over a 19-month period. He compared person-specific data before and after a change in a pay scheme, a very clean body of information on which to base an analysis of performance pay incentives. Lazear found that workers productivity increased by 44% when moving from hourly wages to piece-rates, even though they had a minimum wage per hour guaranteed. A given worker received about 10% increase in pay, as a consequence of the switch to piece rates. He explained that the productivity gain be split into two components. About half of the increase was due to incentive effects, whereas the other half was due to the firm’s ability to attract the most productive workers and the possible reduction in quits among those. Lazear also emphasize that moving to a piece-rate pay increases the variance in output, such that workers that are more ambitious have more incentive to differentiate themselves when piece-rates are used rather than with hourly wages. Safelite was able to retain its high-quality workers and recruit other high quality workers, because the payment of these employees increased (even for the same effort). His evidence and conclusions are unambiguous in such that workers respond to prices just as economic theory predicts (Lazear, 2000a). Cadena, Schoar, Cristea & Delegado-Medrano (2011) empirical results support Lazear's findings. Kruse (1993) also find large positive effects of pay for performance. Kruse (1993) uses new data from a survey of 500 U.S. public companies, and panel data on corporate performance, to examine the relationship between productivity measures and the adoption of profit sharing. He found a productivity increase of 4-5%.

Further, a study by Shearer (2004) confirms the existence of an incentive effect and reveals a 20% increase in worker productivity when workers are paid piece rates rather than fixed wages in his experiment within a tree-planting firm in Canada. Shearer's research confirms the previous natural experiment results obtained in Lazear (2000a) and Paarsch and Shearer (2000). The results of Paarsch and Shearer (2000) confirm the presence of an incentive effect; that is, workers are more productive under piece rates than under fixed wages. However, they did not conclude that piece rates are better than fixed wages. Moreover, a study done by Prendergast (1999) shows that one third of an increase in performance arise from sorting, from attracting better workers. Even if the studies mentioned is not a measure of a change in an existing performance payment, it is suggesting that incentive effects have an effect on performance. Thus, I find the existing literature of importance to compare my findings.

On the other side, Kvaløy, Nieken, & Schötter (2015) and Deci & Lanzetta (1971) find that piece-rate pay could be damaging on performance if not accompanied by intrinsic factors such as motivational talk or feedback. Kvaløy et al. (2015) proclaims that monetary rewards occasionally induce worse performance and referred to what psychologists call a "hidden cost of reward". The term refers to crowding out intrinsic motivation by discouraging confidence in their own abilities. If the agent perceives the principal's behavior as kind, he or she value the principal's payoff positively. On the contrary, if the agent perceives the principal's behavior as hostile, he or she value the principal's payoff negatively and may reduce performance. In order to overcome this issue, Kvaløy et al. (2015) conducted a lab experiment and based on their results they advised that motivational efforts such as motivational talk could work as a complement and enhance the effect of monetary incentives.

Research that find little evidence of incentives mattering includes the field experiment of Mellstrom & Johannesson (2008), who find little evidence of blood donation responding to marginal incentives. Also, based on numerous field and lab experiments, Frey & Jegen's (2001) survey argue that performance payment likely reduces output as it harms intrinsic motivation. The Motivation Crowding Effects suggests that monetary incentives or punishments may undermine external intervention, and under different identifiable conditions strengthen intrinsic motivation (Frey & Jegen, 2001, p. 589). Lastly, the lab experiment done by Ariely et al.,(2009) find evidence of workers with very high reward levels had a detrimental effect on their performance. Psychological research support this evidence, suggesting that excessive rewards can, in some cases; result in a decline in performance.

3. Background

In this chapter, I will present the firm-specific background of the dealer stores, Company M and Company X, then I will describe the performance payment scheme of workers affiliated Company X, and finally I will explain the changes that workers at the dealer stores has experienced over the period analyzed.

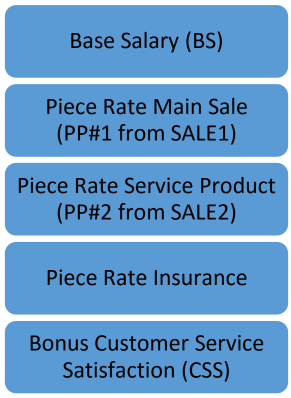
Company M is a Norwegian company that provides products to customers through a dealer network consisting of over 100 dealer stores located in all if Norway's 19 counties. Company X is a service company in Norway that provide services to customers through the same dealer stores as Company M, or provided directly through their company website. When one of Company X's service products is sold through a dealer store, the dealer store receives performance payment (PP) based on the service products sold, and the dealer store then pays its worker in the form of a piece rate or a commission (PP#2). During the period analyzed the dealer stores has experienced different changes in the performance payment scheme, and for this reason, the workers at the dealer stores are ideal as a research objective for this study, in order to address the problem statement: *Does a change in performance pay in Company X have an effect on workers' productivity?*

Performance (i.e. number of sales) has high impact on firm's (Company X's, and the dealer store's) value, thus it is according to Lazear & Gibbs (2014), strong incentive to use performance pay to increase worker's efforts and to better align them with firm interests. The dealer stores hire full-time employees, part-time employees and extra helpers. The payment scheme used at the individual dealer stores varies with the position held. In this paper, I will focus on the workers at each individual dealer store and I will exclude all the workers who are not sellers, such as managers etc. Therefore, when referring to the workers or employees of the firm, I am only referring to the salespeople employed at the dealer stores. Most of the dealer stores pay the workers a base salary, bonuses and piece-rates, which I will explain in detail in the following section.

3.1 Payment Scheme for Workers Affiliated Company X

Each worker sell several products and services supplied from several companies, and a typical worker's salary consist of the components summarized in Figure 3.1 and further explained below. All payments are in Norwegian Kroner (NOK).

Figure 3.1: Salary Components



Note: Salary components of the workers affiliated Company X, working at the dealer stores located in Norway.

Firstly, most of the dealer stores pay their workers a monthly Base Salary (BS), a fixed component that does not depend on output. The BS provides some insurance to the employees against bad luck and other uncontrollable factors. Secondly, the dealer stores pay their workers a performance payment, namely a piece rate (PP#1) for each main item sold, which I will refer to as SALE1 in this paper:

$$PP\#1 * SALE1 = Total\ Performance\ Pay\ for\ the\ numbers\ of\ SALE1\ sold$$

Thirdly, the dealer stores receives a performance payment (PP) from Company X for all service products that the workers sell. The dealer stores receives payment for the sales every four months, and calculations are based on signed contracts with customers conducted by the individual workers at the dealer stores. Further, the dealer stores pay their workers a performance payment, namely a piece rate (PP#2) for each service sold or added to the main sale (SALE1), which I will refer to as SALE2 in this paper:

$$PP\#2 * SALE2 = Total\ Performance\ Pay\ for\ the\ numbers\ of\ SALE2\ sold$$

Fourthly, the dealer stores use performance payment to reward their workers for selling insurance contracts as well, and lastly the dealer stores pay a bonus for levels of CSS achievements to reduce the risk of encouraging the workers to unethical behavior or to sell a high quantity of products with low quality.

The base salary (BS), the piece rates and the bonus (CSS) are not necessary paid all together. The dealer stores have different payment schemes, such that the payment of BS might be on a monthly basis at some dealer stores, while the piece rates might be paid monthly in advance or monthly in arrears and some dealers pay it as a larger pot quarterly. Some of the dealer stores might pay their workers when the customer has signed the contract, while others pay on delivery date of the main sale (SALE1). The two dates may differ by as much as a year. However, the worker's motivation to sell the products is on the contract date, and the worker sells the main product (SALE1), then he or she also sells the service product (SALE2). The workers may at any point in time, calculate his or her future payment, as they know how many sales they have made, when they will receive the payment, and the piece rate for each product sold. The share of the worker's monthly salary that is tied to performance is high, thus theory explained in Section 2.1.1 suggest that they have strong incentives to increase sales as their take home salary increases.

Each worker's main objective is to sell as many main products (SALE1) receiving a piece rate, PP#1, per product sold. When the seller has made this sale, he or she can sell several additional products (as listed in Figure 3.1). This will usually happen at the contract date of SALE1. Importantly, the seller has to sell the main product (SALE1) to be able to sell the additional products. In other words, unless SALE1 is sold, SALE2 will not be sold either. On the other hand, SALE1 may be sold without selling any other products. During the period in which I am analyzing, the PP#1 has been constant, while most of the dealer stores has experienced a change in the performance payment (PP) for SALE2 and thus I am assuming that some workers has experienced a change in PP#2.

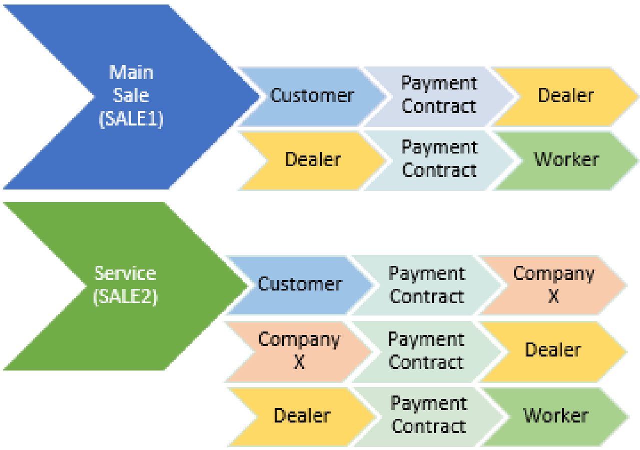
3.2 Performance Pay Scheme (PP) Before the Changes

All the dealer stores cooperates with Company X, and for each service product (SALE2) that a worker at a dealer store sells, the dealer store receives performance payment (PP), namely a piece rate or a commission from Company X. Dealer agreements between each dealer and Company X specifies the PP that the dealer stores are to receive for their sales. All the dealer stores have individual dealer agreements. Each dealer agreement are confidential and the performance payment that each dealer store receives should not be communicated to either the workers at the dealer stores, nor to other dealer stores. Thus, the workers do not know what the dealer stores receive in performance payment for the service products (SALE2) they sell.

Further, the dealer stores pay their workers a constant piece rate (PP#2) for the service products (SALE2) they sell.

The process of a sales transaction at a dealer store is as follows: At the contract date of SALE1, a worker ask the customer whether he or she want to buy a service product, SALE2. If the customer want the service, an additional contract is written with the customer, the dealer and Company X. A sales transaction is shown in Figure 3.2.

Figure 3.2: Sales Transaction



Note: An overview of a sales transaction at a dealer store.

When a service product (SALE2) is sold, it may not necessary result in a performance payment to the dealer store nor to the worker (PP#2). The dealer agreements determines the applicable framework for receiving performance payment for a sale, and the worker must follow a framework in order to achieve the PP#2. It is easy for Company X and for the managers at the dealer store to check whether the worker is eligible for the PP#2 for the service sale (SALE2) he or she sold. When a worker sell a main product (SALE1), he or she log on a system with a worker identity and registers the product, the brand of the product and whether the customer want the additional service (SALE2). Sales are traceable regarding who is to receive commission for a specific sale and each worker is able to keep track of his or her sales and of future performance payments.

3.3 The Performance Payment (PP) Changes

In January 2012, April 2013, May 2014 and in January 2015, Company X made changes in the performance payment to the dealer stores. All the changes happened simultaneously;

however, the dealer stores experienced the changes differently. Dealer stores in Group #1 experienced an increase in the piece rate; dealer stores in Group #2 experienced a reduction in the piece rate, while dealer stores in Group #3 experienced no change, as summarized in Figure 3.3. Note that the groups is not consistent over the period. In other words, Group#1 has not the same composition of dealer stores in say January 2012 and in April 2013 and so on. Group#1 represents those dealer stores who experienced an increase in performance payment at the specific month and so on.

Figure 3.3: Performance Payment Changes

	January 2012	April 2013	May 2014	January 2015
Group #1	89	115	70	27
Group #2	22	0	47	0
Group #3	8	4	2	92
TOTAL	**119**	**119**	**119**	**119**

	January 2012	April 2013	May 2014	January 2015
Group #1	74,8 %	96,6 %	58,8 %	22,7 %
Group #2	18,5 %	0,0 %	39,5 %	0,0 %
Group #3	6,7 %	3,4 %	1,7 %	77,3 %
TOTAL	**100 %**	**100 %**	**100 %**	**100 %**

Note: Dealer groups experiencing either an increase in PP, a decrease in PP or no change in PP. Dealer stores in Group #1 experienced an increase in PP; dealer stores in Group #2 experienced a decrease in PP, whereas dealer stores in Group #3 experienced no change in PP in the specified periods.

- In January 2012, Company X wrote new dealer agreements with each dealer store. Most dealer stores experienced an increase in the performance payment (PP) for SALE2, while about 19 percent experienced a reduction, and only about 7 percent out of 119 dealer stores experienced no change. All dealer stores were obliged to sign the new agreements in order to sell the service products, SALE2. The dealer stores were not able to choose whether to sign the new agreements; therefore, this reform is the focus of my analysis.
- In April 2013, all the dealer stores signed new agreements, and none was worse off. While only four individual dealer stores experienced no change.
- In May 2014, most dealer stores signed a new dealer agreement. The PP change was substantially larger and the dealer stores had the option of signing. However, this agreement was different from previous agreements. Those who signed received a larger performance payment, while those who did not sign the new agreement received less. Hence, it may not be random which dealer stores who signed the new dealer agreements. The dealer stores that believed they were more productive might be those who decided to

sign the new agreements. This reform is the alternative analysis in this paper, further investigated and explained in the Empirical Results, Section 6.

- In January 2015, many dealer stores felt their total performance payment dropped due to not signing the agreement in May 2014. Therefore, some additional dealer stores reconsidered and signed an identical agreement as offered in May 2014. As a result, those who signed the new agreement received an increase in performance payment, but no dealer stores experienced a reduction.

Table 3.1 illustrates the changes of the three worker groups for the PP change that occurred in January 2012. The dealer stores experienced the changes differently. The numbers and the monetary change in Table 3.1 are fictive and are for illustrating the changes only.

Table 3.1: Example of PP Change

Group #1	January 2009 - December 2011	January 2012
SALE1	1	1
SALE2	1	1
PP#1	kr 1 000	kr 1 000
PP#2	kr 500	kr 700

Group #2	January 2009 - December 2011	January 2012
SALE1	1	1
SALE2	1	1
PP#1	kr 1 000	kr 1 000
PP#2	kr 500	kr 400

Group #3	January 2009 - December 2011	January 2012
SALE1	1	1
SALE2	1	1
PP#1	kr 1 000	kr 1 000
PP#2	kr 500	kr 500

Note: A fictive monetary example for the three worker groups who experienced a change in PP in January 2012.

In this paragraph, I am referring to Table 3.1. The dealer stores pay their workers a fixed piece rate (PP#2) for each service product (SALE2) they sell. When I am analyzing the data, I am thus assuming that the dealer stores who experienced an increase in PP (Group #1) also increased their workers piece rate (PP#2) as illustrated. The dealer stores in Group #3 experienced no change in PP, and I am therefor assuming that those dealer stores did not change their workers piece rate (PP#2) in that period either. Dealer stores in Group #2 on the other hand, experienced a decrease in PP, but as managers at some of the dealer stores have communicated, I am reluctant to believe that the dealer stores in this group reduced their workers piece rate (PP#2) even though Table 3.1I am illustrating this change. The dealer stores would most likely demotivate their workers to sell SALE2 if they reduced their piece rate (PP#2). As explained in more detail in Section 2.2, Fehr & Falk (1999) amongst others believe that employers may be reluctant to give wage cuts because they are concerned that workers will behave reciprocal and punish the employer for giving them lower pay. If the dealer stores reduced the PP#2, the workers might become demotivated to sell SALE2, and the dealer stores would not only receive less PP, but they would reduce their sales as well. Therefore, if PP#2 remains unchanged, the total loss would most likely be smaller. In this thesis, I am interested in the individual worker's performance (i.e. their monthly sales) before and after the change of PP.

4. Data

I employ a large set of panel data received from Company X, to control for unobserved explanatory variables and to analyze change over time (Hardy & Bryman, 2004). The data set consists of over 300 000 observations of the main sale (SALE1) and the service product (SALE2) divided by 1243 individual workers and include 119 individual dealer stores[2]. The work environment is male dominant consisting of approximately 93% men, and 7% females. The total sample data expands from January 2009 to December 2015, a period of 84 consecutive months. I utilize the long time series I have available to perform a main analysis, specification analyses in the form of a placebo analysis and an alternative reform analysis, and to perform sub-sample analyses with treatment and control groups of similar characteristics.

Figure 4.1 shows an upward trend for both SALE1 and SALE2 over the period analyzed. The first change in PP occurred in January 2012, the second in April 2013, the third in May 2014, and the fourth in January 2015, illustrated by the vertical lines.

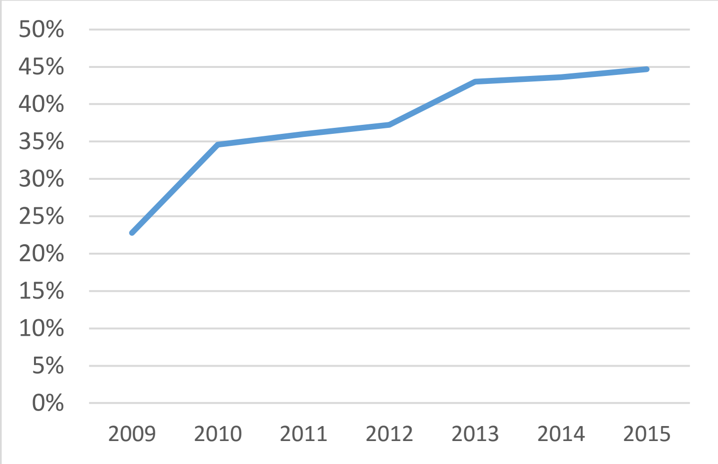
Figure 4.1: Number of SALE1 and SALE2 and the PP Changes

Note: Number of SALE1 and SALE2 and the four PP changes over the years 2009-2015.

Figure 4.2 illustrates the percentage share of SALE2 compared to SALE1 over the period analyzed. This trend is upward sloping, indicating an increase in the sale of SALE2 compared to the sale of SALE1.

[2] The original data set consisted of 1256 individual workers and 122 dealer stores, though I have removed incomplete observations from 2016 for this analysis.

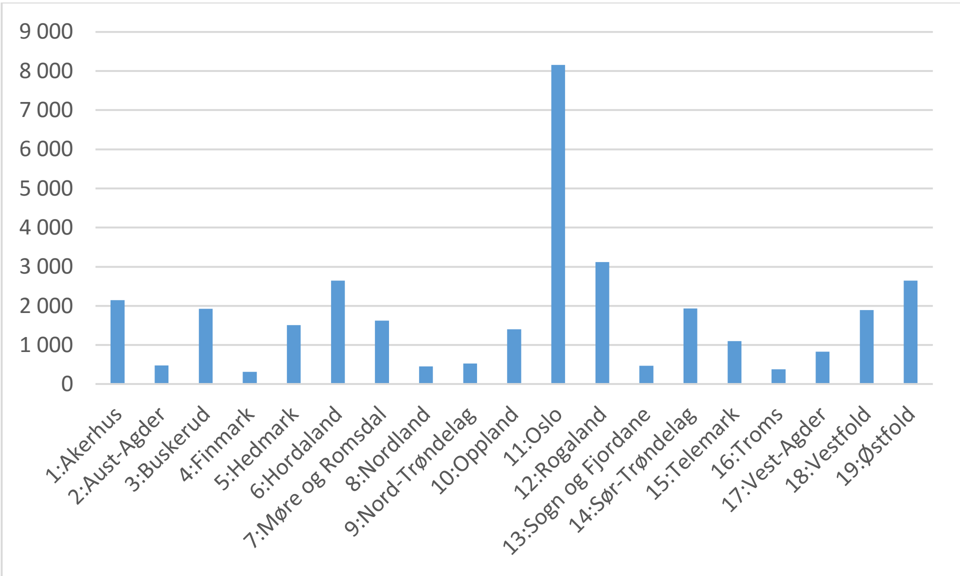
Figure 4.2: Percentage Share of SALE2 Compared to SALE1 Between Years 2009-2015.



Note: The number of SALE2 divided by the number of SALE1 over the period analyzed, 2009-2015.

In Figure 4.3, the observations in the dataset is divided by county, and we can clearly observe that dealer stores located in Oslo has the highest sales of SALE2 compared to other counties.

Figure 4.3: Number of SALE2 per County (1-19)



Note: Number of SALE2 per county in the period 2009-2015.

When a worker sells a main product (SALE1), he or she logs on a system using a worker identity number, registers the product (SALE1), its brand (1-4), and whether the customer is buying the additional service product (SALE2). The dealer store, Company X and Company M can trace who made the sales and who is to receive commission for the sales. Data used in this analysis originates from two sources. First, monthly sales data for the main sales (SALE1) from all the dealer stores has been collected from Company M's reporting system. Second, monthly sales data for the service products (SALE2) from all the dealer stores has been collected from Company X's reporting system. When a SALE2 contract is written, it is registered in both Company M's and Company X's system. Company X has matched and reconciled the data sets into one set of data. The system of both Company M and Company X is subject to financial reporting and is also subject to satisfy legal requirements. The system is subject to an annual inspection by auditors that verify that their IT-systems and internal controls are satisfactory, which increases the reliability of the provided data. The key variable of performance is monthly sales, reported as net variables. This means that the contracts are sealed and complete. Monthly sales are the key dependent variable used to analyze the treatment effect.

For each monthly observation of the main (SALE1), there is information about what year and month the contract has been written in, the worker's identity (anonymized) and gender, the dealer store's identity (anonymized) and location (county), what brand SALE1 is, whether the customer bought the service product (SALE2), and whether the dealer store received commission for SALE2 or not. The variables analyzed are summarized in Figure 4.4.

Figure 4.4: Data Set Variables

Contract Month and Year	Jan 2009-Dec 2015
Dealer Store Identity (random)	200-321
Worker Identity (random)	1-1256
Gender	female/male
County	1-19
Monthly Main Sale, SALE1	number of sales
Monthly Service Product, SALE2	number of sales
Brand	1-4
Comission SALE2	yes/no

Note: For each monthly observation of the main (SALE1), there is information about what year and month the contract has been written, the worker's identity (anonymized) and gender, the dealer store's identity (anonymized) and location (county), what brand SALE1 is, whether the customer bought the service product (SALE2), and whether the dealer store received commission for SALE2 or not.

A worker might have been employed at several dealer stores during the time period analyzed. Some workers may sell one brand, while some sell several or all four brands. All dealer stores that made sales during the period analyzed is included in the data set. Further, the data set consists of observations with missing worker identities, therefore I will run some regressions to test whether my results is affected by the missing information.

4.1 Definition of the Control Group and the Treatment Group

In order to carry out the analysis, I have conducted one control group and one treatment group. Personally, I went through all of the signed dealer store agreements at Company X, and registered the performance payment deals in a spreadsheet. Further, I checked that there was no inconsistency in the corporate system between the signed dealer agreements and the actual payments that the dealer stores received. Based on the data received from Company X, I have identified those dealer stores who experienced an increase in PP, a reduction in PP and those who experienced no change, and then I divided the dealer stores into the following two groups:

- The dealer stores who experienced an increase in PP in January 2012, identified as Group#1 in Figure 3.3, is the *treatment group*.
- The *control group* is the dealer stores who experienced a reduction or no change in PP in January 2012. The most ideal would be to observe the dealer stores who experienced no change, but the sample is too small. The dealer stores, who experienced a decrease in PP, did not experience a great reduction, rather a non-significant change compared to those dealer stores who experienced an increase in PP. Therefore, I have lumped together the dealer stores who experienced a reduction and no change in PP. The control group, identified as Group#2 and Group#3 in Figure 3.3.

Even though there have been several changes in the period analyzed, the focus is on the change in January 2012. This gives an exogenous increase in PP for the treatment stores, as the dealer stores had no choice whether to sign the new agreements or not. Additionally, I will present analyses regarding the PP change that occurred in May 2014.

From the Summary Statistics in Appendix 1, Table 0.2, we can observe the workers average monthly sales of SALE1 and SALE2 by treatment group, control group, brand, county and gender. The average monthly sales of SALE2 per month per worker increased for both groups. One can observe that the treatment group has the largest share of observations compared to the control group, as there was more dealer stores that experienced an increase in PP than did not. When measuring the size of a dealer store, I count numbers of individual workers who has made any sales during a month. The average dealer store increased for both groups after the PP change in January 2012.

5. Empirical Strategy

Does a change in performance pay in Company X have an effect on workers' Productivity?

5.1 Difference-In-Difference Method

The change of PP is an exogenous event that changes the workers' pay, thus I am conducting a natural experiment (Wooldridge, 2013). The natural experiment approach attempts to find a naturally occurring comparison group that can mimic the properties of the control group in the properly designed experiment, also known as difference-in-differences[3]. This is because it is usually implemented by comparing the difference in average behavior before and after a change for one eligible group with the difference in average behavior before and after contrast from a comparison group (Blundell & Dias, 2009). To analyze whether the change in PP has had an effect on the worker's productivity, I therefore use difference-in-differences method (DD) where a control group who is not affected by the change and a treatment group that is thought to be affected by the change is compared. The two groups arise naturally from the particular change unlike a true experiment, in which treatment and control groups are random and explicitly chosen (Wooldridge, 2013).

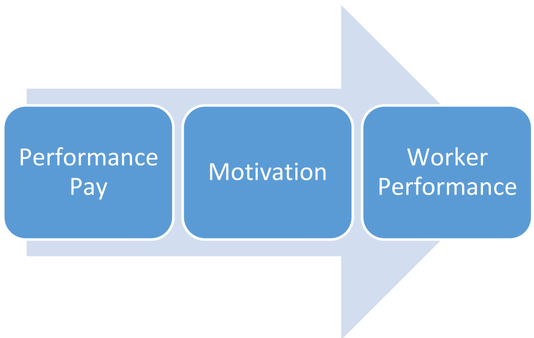
Difference-in-differences (DD) estimation also has its limitations. When the interventions are as good as random, conditional on time and group fixed effects, it is appropriate with a DD estimation. Thus, much of the debate around the validity of a DD estimate typically revolves around the possible endogeneity of the interventions themselves (Bertrand, Duflo, & Mullainathan, 2004, p. 250). If my analysis was based entirely on those dealer stores who received an increase in PP, the treatment group, I could observe tendencies of increased sales, which could falsely lead me to believe that the increase in PP would increase workers productivity. Firstly, when comparing dealer stores with high PP against those with lower PP, a problem could be systematic differences between the groups. This could be an explanation to the differences in the monthly sales between the two groups. Secondly, when comparing dealer stores over time, I look at the change in PP that affect all the dealer stores. The problem that could occur is that changes in monthly sales might have been caused by macroeconomic events happening at the same time as the change in PP. I would observe tendencies of

[3] The DD idea was first used to study the effects of minimum wages by Obenauer and von der Nienburg (1915), writing for the U.S. Bureau of Labor Statistics (Angrist, Angrist, & Pischke, 2009).

increased sales, which could falsely lead me to believe that the increase in PP caused the increase in worker productivity. In my DD approach, I account for these type of problems by adding a control group (no change in PP) and include time fixed effect.

To control for systematic differences between the treatment and control group and differences over time, I need two different sets of data, one before the change and one after the change (Wooldridge, 2013, p. 441). Thus, my sample is broken down into four different groups: the control group prior to the PP change, the control group post the PP change, the treatment group prior to the PP change, and the treatment group post the PP change. More formally, I wish to evaluate the impact of a change in performance payment on an outcome, $Sales_{i,t}$, namely the productivity of workers measured in monthly sales. There are two groups indexed by treatment status, that is, when the dummy variable $treatment_{i,t}$ takes the value 1, the dealer store is in the treatment group and when it takes the value 0, the dealer store is in the control group. Definitions of the control and treatment group are explicitly explained in Section 4.1. I have panel data and observe individuals in say two time periods, t = 0,1 where 0 indicates the time period before the change in PP and 1 indicates the time period after the change in PP, which is also represented by the dummy variable, $post$. The change in PP may have affected the workers productivity level. The model of study and the relationship between those variables are illustrated in Figure 5.1.

Figure 5.1: Model of Study



Note: The model of study and the relationship between the variables.

Before I explain the empirical model in detail, I will give a short explanation of what the Difference-in-Differences (DD) estimate shows. The first change in PP, which my main analysis is based on, occurred in January 2012. I will therefore compare the average monthly sales before and after the PP change in January 2012 for both the treatment group and for the control group. In other words, I want to calculate the difference between the difference between average monthly sales of the treatment group and control group before January 2012 and the difference between average monthly sales of the treatment and control group after January 2012: the difference in the differences. Average monthly sales per worker in the treatment group is denoted TSales, whereas average monthly sales per worker in the control group is denoted CSales. Without other factors in the regression, β2 will be the difference-in-differences (DD) coefficient and the causal effect of interest:

$$\beta 2 = (TSales_1 - CSales_1) - (TSales_0 - CSales_0)$$

The first part in the estimate,$(TSales_1 - CSales_1)$, is the difference in average monthly sale (SALE2) per worker between our treatment group and control group after the PP change in January 2012. The second part in the estimate,$(TSales_0 - CSales_0)$, is the corresponding difference before the PP change in January 2012. β2 is the DD coefficient, and is the focus of my research. It is also known as the average treatment effect as it measures the effect of the "treatment" on the average outcome of $Sales_{i,t}$ (Wooldridge, 2013). If the change of PP has a positive effect on the average monthly sale per worker in the treatment group (hypothesis) or a negative effect for the workers in the control group, the β2 coefficient will be positive. In other words, the coefficient, β2, is the impact on average monthly sales of those in the treatment group compared to those in the control group post the PP change, measured in numbers. Further, a positive β2 coefficient means either that the workers in the treatment group have a larger positively change in average monthly sales after the change in PP compared to the workers in the control group, or that the control group has a larger negatively change in average monthly sales compared to the treatment group. I estimate the following regression using OLS[4]:

$$Sales_{i,t} = \beta 0 + \beta 1 treatment_{i,t} + \beta 2 treat * post_{i,t} + \beta 3 county_i + \beta 4 month_t + \beta 5 year_t + \varepsilon_{i,t}$$

For this research, $Sales_{i,t}$ represents the continuous dependent variable, average monthly sale of SALE2, for one worker at a dealer store i at a specific time, t. The time-series dimension, t, represent time, and represent monthly data (Hardy & Bryman, 2004, p. 333). $treatment_{i,t}$ is

[4] "Ordinary Least Squares (OLS): A method for estimating the parameters of a multiple linear regression model. The ordinary least squares estimates are obtained by minimizing the sum of squared residuals" (Wooldridge, 2013, p. 848)

a dummy variable that takes the value 1 if the dealer store is in the treatment group and 0 if the dealer store is in the control group. $treat * post_{i,t}$ is an interaction term between treatment dummy variable and post dummy variable. $post$ is a dummy variable for whether there was a PP change, i.e. the dummy variable takes the value 1 if there has been a change in PP and 0 if there has not been a PP change. $\beta 0$ is the intersection term, the coefficient β1 represents the difference between the treatment group compared to the control group on monthly sales measured in numbers prior to the PP change. The coefficient $\beta 2$ is the difference in the change of those in the treatment group compared to those in the control group post the change on monthly sales, also measured in numbers. Finally, $\varepsilon_{i,t}$ is the disturbance in the regression output, or the idiosyncratic error.

The fixed effects that I have included in the main model are county, month and year represented by $\beta 3, \beta 4$ and $\beta 5$ respectively. The dealer stores' location does not vary over time, but may affect the workers' average monthly sales, thus I include county fixed effects in the main model. Dealer stores that are located in certain areas might be affected by unobserved factors specific to that location, such as regulations, climate, weather, easier access to customers or other macroeconomic events. Further, I use month and year fixed effects to control for unobserved factors that varies over time but not between dealer stores. The month and year fixed effects include factors that can affect sales and varies over time but not between the dealer stores or the individual workers. Monthly and yearly effects that might affect sales is national or regional campaigns, competitions, launching of new products and more. The main analysis use year 2011 as pre-treatment period and 2012 as the post-treatment period to exclude events that may affect the results. In further analyses, I expand the period analyzed which I will explain later.

The growth in the Norwegian economy in 2011 and 2012 was mainly driven by an increase in private consumption as a consequence of low interest rates, growth in real wages and increased government transfers (Olsen, 2011, 2012). Low interest rates over the period analyzed, has contributed to the lower interest payable of households and firms which has increased their disposable income. The improvement the economy faced over this period would only represent a problem regarding the identification if the treated dealer stores are located in areas where conditions improved most, while the control dealer stores are in areas where there are minor changes. The inclusion of county fixed effects will minimize this problem. However, there might still be considerable variation within counties where treated dealer stores can be located in counties with for example a larger drop in the unemployment rate. I will therefore include unemployment rate as a control variable in one model.

Other covariates that I have added to the model to control for robustness are dealer store size (population) and individual fixed effects. The dealer stores might have different sizes, which does not vary over time, but may affect the worker's monthly sales. Thus, I add the population covariate in one model to control for dealer size. The population variable is measured before the PP change in January 2012, and is not endogenous (i.e. not affected by the PP change). Additionally, Individual fixed effects may affect worker's monthly sales, which vary between the individuals, but does not vary over time. The individual fixed effects may include characteristics such as ability, effort and work experience. The longer the worker has been in the industry, the higher the monthly sales may the worker potentially have. He or she might have earned a customer base with loyal customers, hence reflected in his or her monthly sales. I therefore add worker identity as a covariate in one model to control for individual fixed effect, as an additional robustness analysis. I would like to control for work experience, but the data set has no information on this variable.

Cluster sampling is a case where cross section observations are correlated, and in performing my statistical analysis, I have obtained robust variance estimates that adjusts for within-cluster correlation in my main analysis (Wooldridge, 2002). All single observations are clustered on county, which means that all observations in a county over the analyzed period are clustered into one group. While I allow the units to be correlated within each cluster, I assume independence across clusters. Other problems that may occur is serial correlation due to the idiosyncratic error being correlated with the clusters. In addition, heteroscedasticity could be present as a result of varying cross-sectional sizes (different county and/or dealer sizes) which may lead to different variation of the treatment effect, thus I use robust standard errors.

The estimation of the equation above is in practice subject to a serial correlation problem (Bertrand et al., 2004). Three factors make serial correlation an important issue in the context of my DD estimate. Firstly, DD estimation relies on long time series. The panel data in my main analysis consists of 24 periods, whereas the whole data set consisting of 84 periods. Secondly, the dependent variable might be highly positively serially correlated. Thirdly, the treat*post variable may changes very little within a county over time. These three factors reinforce each other so that the standard error of $\beta 2$, the DD coefficient, could be underestimated. To combat the extent of this problem, I will apply placebo analyses, where treated groups and years are chosen at random.

5.2 Testing the Hypothesis

When testing the hypothesis I need to differentiate between those who are in the control group and those who are in the treatment group, as defined and explained in the above section. The PP change may affect the number of sales and when $\beta 1$ is negative, this indicates that those in the control group have a higher monthly sale than those in the treatment group prior to the PP change. On the other hand, when $\beta 1$ is positive, the treatment group has a higher monthly sale than the control group prior to the PP change. Whether the worker is in the treatment group after the PP change is represented by the interaction term treated*post. Which is the interaction between the treated dummy variable and the post dummy variable. $\beta 2$ is an important coefficient and is the DD estimate. The treatment group is experiencing a positive change in PP and I am interested in whether the monthly sales is affected by this change compared to the control group, which experienced a negative or no change in PP. To do this I am comparing the differences in monthly sales for the control group and the treatment group before and after the treatment. Moreover, if the coefficient $\beta 2$ is positive, it indicates that the gap in monthly sales between the control group and the treatment group has increased compared to the difference prior to the PP change. On the other hand, if $\beta 2$ is negative, it means that the gap has decreased.

5.3 Identifying Assumptions

The DD approach can be a powerful tool in measuring the average effect of the PP change on the treated dealer stores (Blundell & Dias, 2009). Further, it does this by removing unobservable individual effects and common macro effects by relying on two critically important assumptions:

Assumption 1: the first identifying assumption requires that the treatment and control group have a common time trend absent the change of PP.

An example of a common time effect is seasonal changes in demand. Because seasonal changes in demand will affect both the treatment group and control group equally, it means that the DD method will eliminate such time effects. I therefore assume that time trends for all workers are the same absent of the PP change. That is, the change in monthly sales would have been the same for both the control group and the treatment group if there were no PP changes. Including year fixed effects in the regression model eliminates the common trend for all observations. If however, workers in the treatment group had an increasing trend in monthly sales prior to the PP change, whereas the workers in the control group did not experience the same increasing trend, Assumption 1 is violated. An explanation for these non-common trends prior to the PP change could be dealer store location, difference in ability, worker experience, macroeconomic conditions and more. The treated dealer stores might be located in areas where it is easier to sell SALE2 products for some reason, such as location in larger cities with increasing consumer spending, decreasing unemployment rates, or other macroeconomic conditions. Another problem might be if more productive workers are entering treated dealer stores and not the control dealer stores. In order to test whether the treatment and control group have a common time trend absent the change of PP, I will use data on multiple years prior to the PP change and perform a placebo analysis in Section 6.3.1.

As illustrated in Figure 5.2, the trend in the sale of SALE2 for treated dealer stores and control dealer stores are parallel and have a certain time trend prior to the PP change. If the PP change did not occur, Assumption 1 states that the trend of the treated dealer store and the control store should be the same, represented by the red dotted line. The red dotted line represents the continuing trend line of the treated dealer stores if the change of PP did not occur, and Assumption 1 holds. The black line above the red dotted line represents the period after the PP change and the treated dealer stores' new trend in the sale of SALE2 if the PP change caused a treatment effect.

Figure 5.2: Causal Effects in the DD model

Note: Assumption 1: The trend of the control and treated stores would be equal if the change in January 2012 did not occur. The trend in the sale of SALE2 for treated dealer stores and control dealer stores are parallel and have a certain time trend prior to the PP change. The red dotted line represents the continuing trend line of the treated dealer stores if the change of PP did not occur, and the Assumption 1 holds. The black line above the red dotted line represents the period after the PP change and the treated dealer stores' new trend in the sale of SALE2 if the PP change caused a treatment effect.

Assumption 2: the composition of the treatment group and the control group is constant before and after the PP change.

No systematic composition changes within groups and I have to follow the same treatment group and control group before and after the PP change (Blundell & Dias, 2009). The control group and the treatment group does not have to include the same individuals over the whole period analyzed, however the composition of the groups need to be the same.

Using panel data in this thesis, I am following the same individuals throughout the period. However, some workers start working or quit during the period analyzed. In addition, some individuals change employer and might quit at one dealer, while starting at another during the period analyzed. Therefore, it may be different compositions of the treatment group and control group before and after the PP change. To test the validity of Assumption 2, I will include individual fixed effects. If the estimates changes when adding this variable, it may result in a violation of Assumption 2, and I may not have successfully managed to control for omitted variable bias due to changes in the composition of the treatment group and the control group.

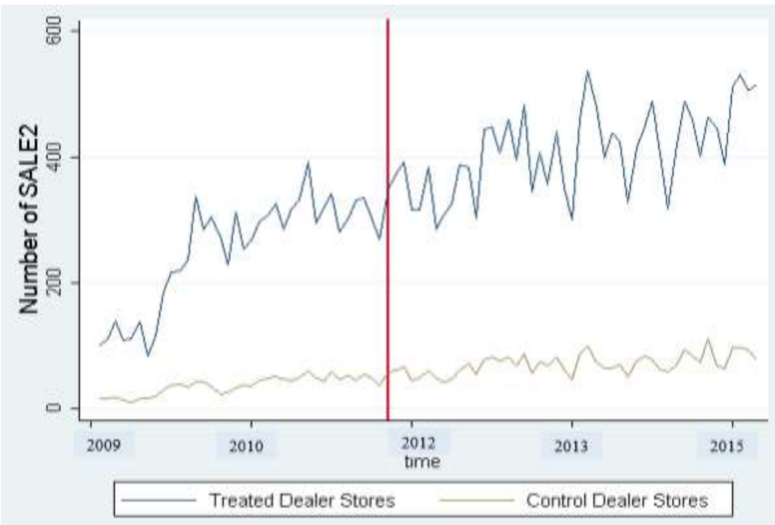
6. Empirical Results

This chapter aims to present the empirical results of the quantitative approach undertaken in this research paper. I consider the impact that the PP change had on workers' monthly sale of SALE2, whether the workers at the dealer stores who experienced an increase in PP, became more productive compared to those workers at the dealer stores who experienced a reduction or no change in PP. The descriptive results and the main analysis is based on PP change that occurred in January 2012 and the years 2011 and 2012. Presented in Table 0.2 in Appendix 1 is descriptive statistics for the entire period (2009 to 2015). Further, please refer to Table 0.1 in Appendix 1 for a detailed description of variable names used in the following sections.

6.1 Descriptive Results

Figure 6.1 illustrate monthly sales of SALE2 over the entire period for all the dealer stores, divided by treatment (i.e. blue line) and control group (i.e. brown line), and the first PP change that occurred in January 2012 (i.e. red vertical line). Figure 0.1 in Appendix 1 presents the average monthly sales of SALE2 for each county for the whole period analyzed, where Oslo has the highest and Nordland has the lowest average monthly sale of SALE2.

Figure 6.1: Number of SALE2



Note: Number of SALE2 by Company X over the period 2009 to 2015, divided by treatment group and control group. The blue line represent the number of monthly sales of SALE2 made by workers in the treatment group whereas the brown line represent the number of monthly sale of SALE2 made by workers in the control group. The vertical red line represent the PP change that occurred in January 2012.

First, I want to investigate whether there are non-parallel trends in SALE2 between the treated and control dealer stores before the PP change. Such non-parallel pre-trend could bias the estimation results. Based on Figure 6.1, it seems that there is a strong increase in number of SALE2 in late 2009 for the treated dealer stores. An explanation for this increase may be that large dealer stores entered the data set in that period, which increased number of SALE2 for the treated dealer stores. However, in the period used as the pre-change period in the main analysis, 2011, the trend for both groups seem to be similar in nature. Further, I expect a stronger increase in the number of SALE2 for the treatment group compared to the control group post the PP change in January 2012, represented by the area to the right of the vertical red line in Figure 6.1. It appears to be a stronger increase in number of SALE2 by the treatment group post the PP change, compared to the control group. However, due to large seasonal variation in SALE2, it is not possible to identify any clear trends with this figure. Analytical models that are more sophisticated are therefore necessary to analyze the effect of the PP change.

The number of dealer stores that experienced an increase in PP is greater than those that did not, as explained in Section 3.3, which is the main explanation for the gap between the number of SALE2 for the treatment group and the control group. This is also observable in the Summary Statistics, Table 6.1 below. The table presents the outcome variables average monthly sale of SALE1 and SALE2. Further, the table presents the developments in the control variables, dividing SALE2 by brands and dealer store location (i.e. counties), size of dealer stores (population) and share of males, which gives an indication whether there are compositional changes. A key element to notice from Table 6.1 is that the treatment group increased the average number of SALE2 after the PP change, whereas for the control group it decreased. An interesting observation is that the average number of SALE1 for the treatment group decreased after the PP change, whereas for the control group it increased. This observation is interesting as those dealer stores in the control group experienced a decrease or no change in PP, hence their focus might have been on the sale of SALE1 rather than the sale of SALE2 in that particular year. Based on conversations with managers at some of the dealer stores, I assume that those dealer stores that experienced a reduction or no change in PP, did not reduce the workers piece-rate (PP#2). A reason why they refused to decrease the PP#2 may be the Ratchet Effect further explained in Section 2.2. Hence, the decrease in SALE2 is most likely, not a consequence of a reduction in PP. Theory outlined in Section 2.1.1 suggests that the productivity of workers differs, either because there are differences in ability across workers or because some workers put in more effort on the job than other workers do. Assuming that the workers chooses the level of effort that maximizes his or her utility, the effort by the workers in the treatment group should be greater than the effort chosen by those in the control group, as those treated receives a higher take-home-salary for the same effort (Borjas, 2013).

The Population covariate shown in Table 6.1 indicate that the mean size of the treated group is larger than the mean size of the control group. The treated dealer stores could have a larger workforce, and at the same time a more productive workforce than the control dealer stores. The treated dealer stores also increased their average size in terms of workers after the PP change, whereas the control dealer stores decreased their average size. This might play a role in why there has been a change in the average number of monthly sales of both SALE1 and SALE2. Workers have individual abilities, and if high ability workers in the control group quit during the period analyzed, this could negatively affect the average monthly sales of SALE2. Additionally, if a more productive workforce were entering the treated dealer stores, this could increase the average monthly sales of SALE2. According to theory, those who believe that they will be most productive at the firm are more likely to apply for or stay at a job there. Hence, a dealer store that are paying a higher piece rate for same effort level will more likely keep their existing workers and attract a more productive workforce through sorting, as mentioned in Section 2.1.2. An issue is thus the possibility of a violation of Assumption 2, which states that the composition of the treatment group and the control group is constant before and after the PP change.

Table 6.1: Summary Statistics 2011 to 2012

Summary Statistics for year 2011 and 2012, before and after the PP change that occurred in January 2012. Control variables are mean monthly observations with standard errors in parentheses.

	Before January 2012			After January 2012		
	Treated	Control	Difference	Treated	Control	Difference
Outcome Variables						
SALE2 (number of obs)	3 821	596	3 225	4 126	669	3 457
Mean	2.324	1.9446	0.3794	2.3589	1.9342	0.4247
Sd	(1.8832)	(1.7847)	0.0985	(1.9276)	(1.488)	0.4396
SALE1 (number of obs)	10 216	2 056	8 160	10 679	2 199	8 480
Mean	3.6472	3.4864	0.1608	3.5726	3.3192	0.2534
Sd	(3.5185)	(9.6624)	-6.1435	(3.4723)	(6.7851)	3.3128
Control Variables						
Population	11.8327	6.6094	5.2233	11.8754	6.4125	5.4629
	(7.3866)	(2.8950)	4.4916	(6.8315)	(2.5981)	4.2334
Share of Male	93%	96%	-3%	93%	97%	-4%
Brand (1-4):						
1	2.4114	2	0.4114	2.2687	1.8537	0.415
	(1.7796)	(1.3457)	0.4339	(1.5657)	(1.3064)	0.2593
2	2.4058	2.1708	0.2278	2.4449	2.1667	0.2782
	(1.8777)	(2.2021)	-0.3244	(1.9181)	(1.7305)	0.1876
3	2.223	1.6948	0.5282	2.3656	1.791	0.5746
	(2.0703)	(1.2542)	0.8161	(2.2198)	(1.3083)	0.9115
4	2.0593	1.6047	0.4546	2.1365	1.5204	0.6161
	(1.6442)	(1.2298)	0.4144	(1.9027)	(0.8523)	1.0504
County (1-19):						
1	1.9204	0	1.9204	1.9842	0	1.9842
	(1.1977)	0	1.1977	(1.3559)	0	1.3559
2	0	1.8986	-1.8986	0	2.0299	-2.0299
	0	(1.7162)	1.7162	0	(1.4972)	1.4972
3	2.4489	1.6548	0.7941	2.2913	1.7667	0.5246
	(1.9206)	(0.8849)	1.0357	(2.0233)	(1.2812)	0.7421
4	2.1538	1.0833	1.0705	1.7391	1.2692	0.4699
	(1.4884)	(0.2823)	1.2061	(0.8643)	(0.5335)	0.3308
5	1.85	0	1.85	1.8382	0	1.8382
	(1.1936)	0	1.1936	(1.5751)	0	1.5751
6	2.4712	1.4815	0.9897	2.528	1.3333	1.1947
	(1.9126)	(0.753)	1.1596	(2.025)	(0.5401)	1.4849
7	2.1162	1.2571	0.8591	2.0972	1.8462	0.251
	(1.7134)	(0.6108)	1.1026	(1.4863)	(0.9608)	0.5255
8	1.2667	1.3077	-0.041	1.2321	1.6667	-0.4346
	(0.4577)	(0.6304)	-0.1727	(0.5718)	(1.1435)	-0.5717
9	1.68	1.4	0.28	1.9444	1.4211	0.5233
	(0.9355)	(0.7071)	0.2284	(1.2946)	(0.9016)	0.393
10	1.7885	1.5455	0.243	1.825	1.25	0.575
	(1.3127)	(1.0357)	0.277	(1.2418)	(0.463)	0.7788
11	2.4795	0	2.4795	2.5719	1.6	0.9719
	(1.8738)	0	1.8738	(1.9688)	(0.5477)	1.4211
12	2.6501	0	2.6501	2.6451	0	2.6451
	(2.2937)	0	2.2937	(2.1514)	0	2.1514
13	1.2703	0	1.2703	1.3387	0	1.3387
	(0.7321)	0	0.7321	(0.7670)	0	0.7670
14	2.6809	1.8364	0.8445	2.6952	2.0625	0.6327
	(2.2738)	(1.3846)	0.8892	(2.0711)	(1.4787)	0.5924
15	2.1818	1.7619	0.4199	2.5789	1.9277	0.6512
	(1.6817)	(1.1875)	0.4942	(2.3681)	(1.2176)	1.1505
16	3.2143	1	2.2143	1.2917	1.5	-0.2083
	(4.9017)	0	4.9017	(0.6241)	(0.7071)	-0.083
17	1.519	0	1.519	2.1538	0	2.1538
	(0.9722)	0	0.9722	(1.801)	0	1.801
18	2.4902	0	2.4902	2.7574	1	1.7574
	(2.0232)	0	2.0232	(2.1784)	0	2.1784
19	2.506	2.6471	-0.1411	2.426	2.2634	0.1626
	(1.9392)	(2.564)	-0.6248	(2.572)	(1.9299)	0.6421

Note: This Summary Statistics table reports descriptive statistics for the change in PP in January 2012, before and after the change. Description of the variables and codes are in Appendix 1.

Observations from Table 6.1, gives me an indication of the treatment effect as explained in detail in Section 5.1. The β2 coefficient in my regression model is the impact of those in the treatment group post the change on monthly sales of SALE2, also measured in numbers compared to those in the control group. I will illustrate this with calculations using statistics from Table 6.1, to give an indication of the treatment effect without using control variables. Monthly sales per worker in the treatment group is denoted TSales, whereas monthly sales per worker in the control group is denoted CSales. The Difference-in-Difference (DD) estimate is as follows:

$\beta 2 = (TSales_1 - CSales_1) - (TSales_0 - CSales_0)$

$\beta 2 = (2.3589 - 1.9342) - (2.324 - 1.9446) = 0.4247 - 0.3794$

$\underline{\beta 2 = 0{,}0453}$

The positive β2 estimate indicates that the change in PP has a positive effect on the average monthly sale per worker in the treatment group (Hypothesis) or a negative effect for the workers in the control group. The change represents a 1.95 percent increase in monthly sales of the treated dealer stores compared to the control dealer stores after the PP change.

By only looking at the descriptive statistics for the period 2011 and 2012 and the presented graphs, I am able to suggest, thus without including any control variables, that the treatment has made an impact on the treated worker's average sale of SALE2 due to the positive DD estimate calculated above. The increased sales effect may be explained by an increased productivity or more productive workers in the treated dealer stores compared to the control dealer stores as discussed in theory Section 2. This will be further analyzed in the Main Analysis in Table 6.2, using models that are more sophisticated and to test the hypothesis presented in Section 5.2.

6.2 Main Analysis and Test of Hypothesis

The main analysis presented in Table 6.2 investigates if an increase in PP increase workers performance in terms of higher sales. To measure the impact that the PP change have on sales I use the basic specifications explained in detail in Section 5 for the period January 2011 to December 2012. If the increase in PP had a positive effect on the treatment group (Hypothesis), I expect to see a positive and significant DD coefficient, implying that the sales of SALE2 in treated dealer stores has increased more than the sales of SALE2 in control dealer stores, as supported by theory. Theory outlined in Section 2.1.1 suggests that efforts tied directly to performance payment may induce workers to expend greater effort in that task. A worker has an incentive to increase output as it increases his or her pay, and gives the worker additional benefit (Lazear, 1986).

Table 6.2: Main Analysis

Main analysis for the PP change in January 2012: The effect of treatment on monthly sales of SALE2. Period analyzed 2011 and 2012. Dependent variable: Monthly sales

	(1)	(2)	(3)	(4)	(5)	(6)[5]
treated	0.378***	0.436***	0.436**	0.378*	0.379*	0.489**
	(0.0785)	(0.0837)	(0.204)	(0.187)	(0.194)	(0.212)
treated*post	0.0448	0.0600	0.0600	0.0554	0.0528	0.0364
	(0.102)	(0.0998)	(0.135)	(0.135)	(0.146)	(0.112)
Fixed effects:						
Year	Yes	Yes	Yes	Yes	Yes	Yes
Month	Yes	Yes	Yes	Yes	Yes	Yes
County	No	Yes	Yes	Yes	Yes	Yes
Dealer size	No	No	No	Yes	Yes	No
Unemployment Rate	No	No	No	No	Yes	No
Individual	No	No	No	No	No	Yes
R^2	0.0198	0.0500	0.0500	0.0525	0.0525	0.2340
Adjusted R^2			0.0467	0.0490	0.0490	0.1692
Observations	9 212	9 212	9 212	9 212	9 212	9 212

*Note: Robust standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01. This table reports the treatment effect on monthly sales of SALE2. treated is an independent variable that equals 1 if the individual is in the treatment group and 0 if the individual is in the control group. treated represents the difference in monthly sales of SALE2 between the treatment group and the control group before the change of PP. treated*post is the interaction between the treated variable and post variable. The post variable represents the period after the PP change, and equals 1 if the change has occurred and 0 otherwise. Therefore, treated*post shows the DD estimate, which shows how much the difference between the treatment and control group's monthly sale of SALE2 changes after the change of PP. All the Models (1), (2), (3), (4), (5) and (6) include year and month as control variables. Model (4) and (5) include dealer size as an additional control variable. Model (5) include unemployment rate as an additional control variable. Model (1) and (2) are estimated using the robust option in Stata. Model (3), (4) and (5) are estimated using the robust cluster option in Stata, where standard errors are clustered on county. Model (6) are clustered at individual level.*

Model 1 from Table 6.2 presents the first empirical result of the average treatment effect, the change in monthly sales after the change of the PP in January 2012. This regression includes robust standard errors and time fixed effects, such as month and year. The robust standard errors are asymptotically valid when the regression residuals are heteroskedastic, thus improve on old-fashioned errors (Angrist et al., 2009). The month and year fixed effects include factors that can affect sales and varies over time but not between the dealer stores or the individual workers, such effects might be national policies, federal regulations, national campaigns and more. The size of the treated coefficient from this regression, 0.378, is positive

[5] There are missing worker identities in 899 observations in the data. I have run the main analysis with and without these observations. Model 1-5 is not sensitive to the exclusion of these observations, but Model 6 is very sensitive as the model includes individual fixed effects, also the standard errors became very large. I will run all further regressions using the complete dataset, as the models' sensitivity is not large enough to exclude the observations.

and significant at 0.1% level, and is equivalent of 16.27 percent higher sales of the treatment group compared to the control group prior to the PP change. The DD coefficient size from this regression is very small, which shows that after the PP change, the difference between the treatment and control groups' average monthly sale increases by 0.0448, equivalent of a 1.97 percent increase. These results indicates a very small treatment effect, but the standard errors are large and hence, I cannot reject H0 – the effect is not statistically different from zero. The estimated treatment effect is very small compared to other studies. Lazear (2000a) found a 44 percent increase in worker productivity when they moved from hourly wages to piece rates, and Shearer (2004) reveals a 20 percent increase in worker productivity when workers are paid piece rates rather than fixed wages. Although their experiments are not identical to mine, it illustrates the magnitude of the effect of worker's performance.

As described in the empirical strategy, Section 5, compositional changes in the two groups could bias the coefficient in Model 1. I account for such possible observable changes in the composition of the treated and control group. In the following models, I add covariates sequentially in order to investigate whether differential trends or observed changes in the composition of the groups affect my coefficients. Even if I am able to control for observable factors, unobservable factors may also represent a problem that I am not able to control. However, comparing Model 1 with the other models it does not seem that the coefficient on DD is particularly sensitive to the inclusion of covariates until Model 4, where adding the dealer size covariate seem to be of importance[6].

In addition to the time fixed effects from Model 1, I have added the covariate county in Model 2 to see if the regression is robust to this inclusion. The independent variables in Model 1 might be too modest in explaining the average monthly sales of a worker, and hence dealer store location might affect sales. Some of the dealer stores are located in larger cities with easier access to a customer base or easier access to customers with a higher spending level. The treated coefficient is still positive and statistically significant at 0.1% level suggesting 19.18 percent higher sales of the treated group compared to the control group prior to the PP change when controlling for location. The DD coefficient size from this regression equals 0.06, representing a 2.64 percent increase in monthly sales for the treatment group compared

[6] The dealer size covariate (population) is measured prior to the PP change, hence it is not endogenous (i.e. affected by the change).

to the average monthly sales of the control group after the change of PP. When including county as a control variable, the DD coefficient increases and its standard errors decreases compared to the results in Model 1, indicating that there are compositional changes within the groups that I was not able to control for in the first regression.

With this panel data set, two problems could potentially occur. First, correlations within the clusters and correlations between the errors in different periods, serial correlation, could potentially occur (Wooldridge, 2013). Secondly, because of a variation in dealer store sizes, heteroscedasticity could be present and may lead to different variation in the treatment effect. To correct for heteroscedasticity, I have used robust standard errors. Additionally, to correct for both heteroscedasticity and serial correlations I have used clustered standard errors. In Model 3, standard errors are clustered on county and account for heteroscedasticity and non-independence of residuals across counties. The data from the 19 counties in Norway form a cluster sample, where each county is a cluster. I assume workers in the same counties might have tendencies of correlated productivity, as they are subject to similar environmental and locational influences. This correlation is called the clustering problem (Angrist et al., 2009). Because the outcomes within a cluster are likely to be correlated, allowing for an unobserved cluster effect is typically important (Wooldridge, 2013, p. 482). By using the cluster option to report all standard errors so that they are valid, in large cross sections, with any kind of serial correlation or heteroscedasticity. The treated coefficient and the DD coefficient in Model 3 is equal to the coefficients in Model 2, but their standard errors has changed. The corrected standard errors are substantially larger than the robust standard errors in Model 1 and 2. The standard errors in Model 2 have initially been estimated under the false assumption that they are not serial correlated. Thus, these large standard errors make it even harder for me to suggest that the PP change had an effect of those treated compared to those in the control group.

Further, a closely related problem is the correlation over time in the data sets commonly used to implement differences-in-differences (DD) estimation strategies (Angrist et al., 2009). The Stata’s cluster option may not be very good, as the asymptotic approximation relevant for clustered or serially correlated data relies on a large number of clusters or time series observations. To check whether I have a serial correlation problem, I have calculated the average sale of SALE2 in year 2011 and in 2012, and run the regression from Model 1, but without the month covariate. The treated coefficient is marginally larger and the standard errors does not change by more than 0.0007. Further, the DD coefficient increases only marginally and the standard error does not change. This indicates that there is not a problem of serial correlation over time in the data set. Results are not reported in the table.

In Model 4, the dealer store size in terms of number of workers is included as control variable as well as I cluster on counties. I do this to see if the regression is robust to this inclusion and it is interesting to check whether the size of the dealer store also affect monthly sales in the groups. The treated dealer stores are on average larger than the control dealer stores, and this might be a reason why they also have higher average monthly sales as mentioned in the previous section. A change in PP may affect the number of employees. When the PP increases, the firm are able to hire more employees and they may even be more productive. The larger dealer stores, which are mainly in the treatment group, might expand more than the smaller dealer stores after the PP change, thus the dealer size would increase. They might expand, due to their increased attractiveness as an employer. Further, if they manage to employ a more productive workforce, it can result in increased productivity. I cannot see this in my analysis, as the DD coefficient is decreasing from Model 3 to Model 4, and there is no change in the standard error. If this explanation is important, I would therefore expect that the treat*post coefficient would be lower when including individual fixed effect. If Model 4 was my main model, a positive effect may be explained by increased productivity or by sorting, through the hiring of a more productive workforce as theory and Lazear's (1986) research suggests. An important reason for adding the dealer size covariate is the possibility that large dealer stores has a different development in sales also prior to the PP change (Assumption 1). Dealer stores that are trying to expand may introduce a PP change as they expect an increased customer base. Consequently, it may be easier to sell more rather than it being a causation from the PP increase. Another important reason to add the covariate is the possibility that there are a larger share of large dealer stores in 2011 than in 2012 (Assumption 2). The treated coefficient is still positive, 0.378, and significantly different from zero at 10% level, suggesting a 16.63 percent difference in monthly sales between the two groups prior to the P change. The DD coefficient is small, 0.0554, and represents a 2.44 percent increased difference in average monthly sales between the treatment and control group after the PP change, but is not significant.

Model 5 from Table 6.2 is equal to Model 4, except for the inclusion of the covariate unemployment rate for each county. I have done this to control for macroeconomic conditions, which may affect people’s spending level in the different counties. The fact that the results are marginally different from the results in Model 4, and that the standard errors are slightly larger suggests that the inclusion of the unemployment rate covariate has little or no effect on monthly sale of SALE2, beyond the county covariate effect.

Model 6 from Table 6.2, includes individual fixed effects. Standard errors are clustered on individual workers and account for heteroscedasticity and non-independence of residuals across individuals. The DD coefficient is still very small and positive, 0.0364. The effect suggests a 1.6 percent increase in the difference between the two groups post the PP change, which may be due to increased productivity of existing workers or an increase in the hiring of high productive workers. However, the DD coefficient is nonsignificant; hence, the H0 is not rejected.

The increase in PP in January 2012 may have affected the monthly sales of the treated dealer stores, but the DD coefficients in all the models are not statistically different from zero. Thus, I am not able to say whether the PP change affected workers productivity, and I cannot reject the H0.

6.3 Specification Analysis

In the specification analysis presented in Table 6.3, I have used Model 3 from the main analysis in Table 6.2 as the reference model.

Table 6.3: Specification Analysis

Specification analysis of the PP change in January 2012, the alternative reform in May 2014 and Placebo effects. Dependent variable: Monthly sales

	(1)	(2)	(3)	(4)
treated	0.322*	0.347	0.438***	0.344**
	(0.173)	(0.206)	(0.100)	(0.177)
treated*post	0.157			
	(0.115)			
treated*post2			0.189***	
			(0.064)	
yr2009_treated		-0.187		
		(0.141)		
yr2010_treated		0.0321		
		(0.0848)		
yr2012_treated		0.0687		
		(0.132)		
yr2013_treated		0.0132		
		(0.143)		
yr2014_treated		0.338**		0.331***
		(0.16)		(0.0873)
yr2015_treated		0.0947		0.0928
		(0.171)		(0.101)
Fixed effects:				
Year	Yes	Yes	Yes	Yes
Month	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
R^2	0.0458	0.0462	0.0435	0.0433
Adjusted R^2	0.0447	0.0450	0.0418	0.0415
Periods observed	Jan2009-Dec2015	Jan2009-Dec2015	Jan2013-Dec2015	Jan2013-Dec2015
Observations	33 479	33 479	18 276	18 276

*Note: Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. This table reports the treatment effect on monthly sales of SALE2. treated is an independent variable that equals 1 if the individual is in the treatment group and 0 if the individual is in the control group. treated represents the difference in monthly sales of SALE2 between the treatment group and the control group before the change of PP. treated*post is the interaction between the treated variable and post variable. The post variable represents the period after the PP change in January 2012, and equals 1 if the change has occurred and 0 otherwise. Therefore, treated*post shows the DD estimate, which shows how much the difference between the treatment and control group's monthly sale of SALE2 changes after the change of PP. treated*post2 is the interaction term between the treated variable and the post variable in the alternative reform, i.e. the PP change that occurred in May 2014. Thus, the post2 variable represents the period after the PP change in May 2014, and equals 1 if the change has occurred and 0 otherwise. The interaction terms yr*treated represents placebo analysis using 2011 as reference year in Model 2. The interaction term shows how the difference in monthly sales between the two groups have changed compared to the reference year, 2011. In Model 4, using the alternative reform, yr*treated represents how the difference in monthly sales between the two groups have changed compared to the reference year, 2013. All the Models (1), (2), (3) and (4) include year, month and dealer location (county) as control variables and are estimated using the robust cluster option in Stata, clustering on county level.*

6.3.1 Placebo Effect

The workers affiliated Company X increased their sales of SALE2 during the period 2009 to 2015. This increase was not necessarily the same for all dealer stores, and I imagine that the increase in sales differed among workers across time and across dealer stores. By thoroughly exploring pre-trends in monthly sales and by performing a placebo test presented in Table 6.3, I investigate whether there are non-parallel trends in sale between treated and control dealer stores before the change in PP. Such non-parallel pre-trend could bias the estimation results (a violation of Assumption 1).

Model 1 is the reference model and is compared to Model 2. Model 2 from Table 6.3 represents the Placebo Analysis for the PP change in January 2012. A full set of dummies is included for each of the two kinds of groups and all periods, as well as all pairwise interactions. The interaction term between year and treated shows how the difference in monthly sales between the two groups have changed within different years using year 2011 as reference year. If there is a common time trend prior to the change in PP for the treatment and control group, I am expecting to see the DD coefficient for all the years prior to the PP change to be very small and not significantly different from zero. The yr2009_treated coefficient in Model 2 is small and negative, whereas the coefficient for yr2010_treated is positive and insignificant. Results shows that the difference in the sale between the treated and control dealer stores before the change in PP are small in magnitude and not statistically different from zero. This suggest that the small and insignificant effect we observe from the PP change is not biased by unparalleled pre-trends in sale.

6.3.2 Alternative Reform – PP Change in May 2014

In May 2014, another major change in PP affected dealer stores and workers affiliated Company X. The size of the PP change in May 2014 was substantially larger and in contrast to the 2012 change, the dealer stores could voluntary implement a change in their PP in 2014. If a dealer store decided to use Company X as their main service provider, that dealer store would with certainty, receive a higher PP for each sale of SALE2, whereas if the dealer store decided not to use Company X as their main service provider, the dealer store would with certainty, receive a lower PP for each sale of SALE2. The specification analysis presented in Table 6.3 helps me to investigate whether the PP change increased worker performance of those in the treatment group (i.e. those who received an increased PP in May 2014). To measure the direct impact that the PP change have on sales, I use Model 3 from the main analysis in Table 6.2 as reference model. If the increase in PP in May 2014 had a positive effect on the treatment group, I would expect to see a positive and significant DD coefficient, treated*post2.

Model 3 from Table 6.3 shows the empirical result of the treatment effect, the change in PP. The observed data expands from January 2013 to December 2015. The treatment dummy variable takes value 1 when the dealer store is in the treatment group, zero otherwise. The treated*post2 is an interaction term between treatment dummy variable and post2 dummy variable, and shows how the difference in monthly sales between the two groups have changed after the PP change in May 2014, which corresponds to the DD coefficient. Post2 is a dummy variable for whether there was a change in May 2014, i.e. the dummy variable takes the value 1 if there has been a change in PP and 0 otherwise. The size of the treated coefficient, 0.438, is positive and significantly different from zero at 1% level, suggesting that the monthly sales of the treated dealer stores are in fact higher than the monthly sales of the control dealer stores before the PP change that occurred in May 2014. The coefficient size of the DD estimate from this regression is small but positive, 0.189, and represent actual number of increased sales of those who experienced an increase in PP in May 2014, after the change occurred. The DD coefficient is statistically significant at 1% level, indicating that the result is in fact different from zero. Hence, the DD coefficient clearly indicates a treatment effect and shows that after the PP change in May 2014, the difference in monthly sales between the treatment group and the control group increased by 0.189. This represents 8.39 percent increase in monthly sales of those treated, and is in line with theory.

In Model 4 from Table 6.3, I have further investigated the results from Model 3. I have included the interaction term year*treated which shows how the difference in average monthly sales between the treated and control dealer stores have changed compared to the reference year 2013. The coefficient of yr2014*treated is positive and significant at 1% level, indicating that the PP change in May 2014 increased the difference in monthly sales between the treatment and the control group by 0.331 compared to year 2013. The effect is large and the DD coefficient clearly stands out representing a 14.70 percent increase in average monthly sales of those treated. The coefficient of Year 2015*treated is positive, 0.0928, but not statistically different from zero. In January 2015, a new PP change occurred for some dealer stores, as further explained in Section 3.3, hence it is difficult to see whether there is an effect of the PP change of May 2014 in that year.

Model 3 and Model 4 indicates that there is a treatment effect of the change in PP, which occurred in May 2014. Model 4 clearly shows a large treatment effect which stands out. Even so, these results should be red with caution. As mentioned in Section 5.1, difference-in-difference estimation is appropriate when the interventions are as good as random. In the alternative reform that happened in May 2014, the PP change may therefore not be random, as the dealer stores had the choice whether or not to sign the new dealer agreements. Maybe the dealer stores with the highest productivity are those who chose to sign the new agreement, whereas those who were least productive chose not to sign. Thus, estimation results utilizing this change in PP has to be interpret with caution.

6.4 Sub Sample Analyses

In my Sub-Sample Analyses, I want to find similar control and treatment groups to run regressions.

Table 6.4: Sub-Sample Analysis

The treatment effect on monthly sales with different sample restrictions. The period analyzed is year 2011 and 2012. Dependent variable: Monthly sales

	(1)	(2)
treatment	0.0911	0.443***
	(0.0966)	(0.0846)
treat_post	0.174	0.0432
	(0.113)	(0.103)
Fixed effects:		
Year	Yes	Yes
Month	Yes	Yes
County	Yes	Yes
Individuals:		
Treatment	296	492
Control	120	133
R^2	0.0725	0.0539
Sub-Sample	Small dealer stores	All dealer stores, except those located in Oslo
Observations	3 829	7 000

*Note: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table reports the treatment effect on monthly sales of SALE2. treated is an independent variable that equals 1 if the individual is in the treatment group and 0 if the individual is in the control group. treated represents the difference in monthly sales of SALE2 between the treatment group and the control group before the change of PP in January 2012. treated*post is the interaction between the treated variable and post variable. The post variable represents the period after the PP change in January 2012, and equals 1 if the change has occurred and 0 otherwise. Therefore, treated*post is the DD estimate, which shows how much the difference between the treatment and control group's monthly sale of SALE2 changes after the change of PP. Model (1) and (2) include year, month and location covariates. Model (1) represents small dealer stores and are estimated using the robust option in Stata. Model (2) represents dealer stores that are located in counties except Oslo and are estimated using the robust option in Stata.*

The samples are unevenly distributed, and I have a larger share of workers in the treatment group. In Model 1, there are 296 individuals in the treatment group and 120 in the control group and in Model 2 there are 492 individuals in the treatment group and 133 individuals in the control group. Model 1 from Table 6.4 represents dealer stores that are smaller than the average dealer store in terms of number of workers. Dealer stores classified ass small are similar between the two groups, and therefore I have included them in my analysis. Large dealer stores is not part of the analysis, as there are too few large dealer stores in the control group. Model 2 represents all dealer stores except for those located in Oslo. I wanted to exclude dealer stores in all large cities to analyze dealer stores with a similar sales marked. However, I do not have enough information to do so; therefore, I am analyzing the results by excluding Norway's largest city.

I can see that the gap between the groups after the PP change in January 2012 is positive, with a DD coefficient of 0.174 for Model 1 and a DD coefficient of 0.0432 for Model 2. Even though the small dealer stores stand out with a large DD coefficient, none of the DD coefficients are significant, and the standard errors are large. Thus, I am not able to suggest that there are a treatment effect, and H0 is not rejected. The DD coefficient from Model 1 corresponds to a 7.66 percent increase in the difference in monthly sale between the two groups, and this is quite different from my previous result of 1.99 percent in the main analysis. The DD coefficient from Model 2 corresponds to a 1.90 percent increase in the difference in monthly sale between the two groups.

In Appendix 1, Table 0.4 I have made yet another sub-sample analysis using the same groupings as in Table 6.4. Based on my results, there is not much evidence to suggest that there is an effect of the PP change in January 2012 even when grouping similar dealer stores. There are no strong significant differences in the years before or after the PP change. This suggests that pre-trend in SALE2 is common and most likely is Assumption 1 not violated. Thus, the small and insignificant coefficients in Table 6.4 is valid and not biased by non-parallel trends in SALE2.

7. Discussion and Conclusion

In this paper, my focus is on the performance measurement and the power of incentives. I have analyzed workers productivity in order to investigate whether a change in performance payment (PP) in Company X, a Norwegian service company, have an effect on workers productivity. During the period 2009 to 2015, dealer stores affiliated Company X experienced several PP changes. Some dealer stores experienced an increase in PP, some dealer stores experienced a decrease, whereas others experienced no change. To explore whether the PP change had an effect on workers' productivity I measured the workers' average monthly sale before and after the changes. I was interested in whether the increase in PP had a positive effect on the treated dealer stores where the workers would increase their productivity in terms of average monthly sales (hypothesis). To test the hypothesis an to analyze the effect I used panel data received from Company X and a difference-in-differences (DD) method, such that the differences between the treated group and the control group prior and after the PP change were compared. I expected to see a positive and significant DD coefficient in my regression analysis, implying that the average monthly sales of SALE2 of those in the treated group increased more than the average monthly sales of SALE2 of those in the control group.

First, the descriptive analysis suggested, thus without including any control variables, that the PP change that occurred in January 2012 made a positive impact on the treated workers' average monthly sale of SALE2. However, the regression findings were inconclusive. I did not find any evidence to support my analysis from the first PP change that occurred in January 2012. Thus, I cannot say whether the workers affiliated Company X was affected by that PP change or not. The main analyses showed positive DD coefficients, so these results indicated a treatment effect of approximately 2 percent, but the standard errors were so large and hence, I was not able to suggest that the effect was statistically different from zero. The effect was a little larger for the small dealer stores compared to the other dealer stores, a 7.66 percent increase, but the results are still not statistically significant. In the specification analysis, I investigated whether there was non-parallel trends in sales between treated and control dealer stores before the change in PP. Such non-parallel pre-trend could bias the estimation results. Results shows that the difference in sales between the treated and control groups before the change in PP are small in magnitude and not statistically different from zero. This suggest that the small and insignificant effect observed from the change in PP, is not biased by unparalleled pre-trends in sale.

Moreover, I find supportive indications that the PP change that occurred in May 2014, increased sales. The results are large in magnitude and statistically different from zero. The regression results suggests a 8.39 and 14.70 percent increase in average monthly sales of those treated compared to those in the control group. Thus, the result of my hypothesis is that the null hypothesis is rejected for this particular change, and there are statistically supportive indications of a positive treatment effect on sales. However, in contrast to the change in 2012, the stores could voluntarily implement a change in their PP in 2012. Thus, estimation results utilizing this change in PP has to be interpreted with caution.

One reason why I did not find any evidence to support my hypothesis could be that the PP change that occurred in January 2012 was too small to make an impact on workers motivation. Another drawback with my analysis is that I do not know for sure if the dealer stores increased all their workers' piece rate, hence some workers may not have been introduced to an increased payment. This make it difficult to know with certainty which employees experienced a reduction in pay, which employees experienced an increase in pay and which employees did not have a change in pay. This can have contaminated my results and I might have put wrong workers into wrong groups, for example I could potentially have put a worker who experienced no change into the treatment group instead of the control group and vice versa.

For a further investigation of workers' motivation after an increase in performance payment, Company X could conduct a field experiment where they give workers at some dealer stores a direct piece rate for each SALE2 they make without paying through the dealer store. These workers would receive a piece rate from the dealer and a piece rate from Company X. Thus, Company X would know for sure that these workers would get an increased payment. To test whether these workers would be more motivated to sell SALE2 by this increased payment, I suggest that they conduct a difference-in-differences method to see if the differences in average monthly sales of SALE2 between the workers who receives an increase in pay and those who do not increases. These results would be more accurate in explaining the effect of an increased performance payment.

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Appendix 1

Table 0.1: Description of Variables.

Variable/Term	**Description**
SALE2	Number of SALE2, a service product provided by Company X and sold through dealer stores
SALE1	Number of SALE1, a main product provided by Company M and sold through dealer stores
Population	Size of dealer store in terms of number of employees (measured before the PP change in January 2012)
Large dealer store	Number of employees in a month is above the average (measured before the PP change in January 2012)
Small dealer store	Number of employees in a month is below the average (measured before the PP change in January 2012)
Male	Dummy variable. If 1, the worker is a male, if 0, the worker is a female
Brand (1-4)	Brand of SALE1 can be either 1, 2, 3 or 4.
County (1-19)	Counties in Norway (1-19): 1: Akershus, 2: Aust-Agder, 3: Buskerud, 4: Finnmark, 5: Hedmark, 6: Hordaland, 7: Møre og Romsdal, 8: Nordland, 9: Nord-Trøndelag, 10: Oppland, 11: Oslo, 12: Rogaland, 13: Sogn og Fjordane, 14: Sør-Trøndelag, 15: Telemark, 16: Troms, 17: Vest-Agder, 18: Vestfold, 19: Østfold

Table 0.2: Summary Statistics 2009 to 2015

Summary Statistics for period 2009 to 2015, before and after the PP change that occurred in January 2012. Presented are number of observations, mean monthly observations and standard errors are in parentheses.

	Before January 2012			After January 2012		
	Treated	Control	Difference	Treated	Control	Difference
Outcome Variables						
SALE2	9 070	1 338	7 732	19 659	3 412	16 247
	2.1959	1.8303	0.3656	2.4362	1.9798	0.4564
	(1.8050)	(1.537)	0.268	(2.1702)	(1.6315)	0.5387
SALE1	27 098	5 555	21 543	45 106	9 510	35 596
	3.6752	3.4268	0.2484	3.4979	3.1655	0.3324
	(3.624)	(6.489)	-2.865	(3.3529)	(4.5007)	-1.1478
Control Variables						
Population	10.8205	5.9647	4.8558	11.9563	7.1872	4.7691
	(6.4438)	(2.6133)	3.8305	(6.6350)	(3.2982)	3.3368
Share of male	93%	97%	-4%	94%	98%	-4%
Brand (1-4):						
1	1 801	188	1 613	3 541	420	3 121
	2.2015	1.7766	0.4249	2.5210	2.081	0.44
	(1.6121)	(1.3256)	0.2865	(2.0215)	(1.6256)	0.3959
2	4 278	645	3 633	9 818	1 501	8 317
	2.3107	1.9736	0.3371	2.4955	2.0673	0.4282
	(1.9256)	(1.8172)	0.1084	(2.2280)	(1.8181)	0.4099
3	1 915	309	1 606	3 903	900	3003
	2.0757	1.754	0.3217	2.29	1.9044	0.3856
	(1.8423)	(1.2238)	0.6185	(2.1318)	(1.4884)	0.6434
4	1 076	196	880	2 413	595	1 818
	1.9442	1.5306	0.4136	2.2984	1.795	0.5034
	(1.4825)	(1.0348)	0.4477	(2.1831)	(1.2877)	0.8954
County (1-19):						
1	729	0	729	1 415	0	1 415
	1.8848	0	1.8848	2.0106	0	2.0106
	(1.269)	0	1.269	(1.5508)	0	1.5508
2	0	125	-125	0	357	-357
	0	1.672	-1.672	0	2.084	-2.084
	0	(1.4072)	-1.4072	0	(1.5447)	-1.5447
3	284	209	75	1 044	387	657
	2.1444	1.4976	0.6468	2.1973	1.6615	0.5358
	(1.9382)	(0.8667)	1.0715	(1.938)	(1.2244)	0.7136
4	63	46	17	102	105	-3
	1.7778	1.1739	0.6039	1.6471	1.3333	0.3141
	(1.2882)	(0.4374)	0.8508	(0.8861)	(0.8734)	0.0127
5	522	0	522	985	0	985
	1.7490	0	1.7490	1.9228	0	1.9228
	(1.1153)	0	1.1153	(1.5108)	0	1.5108
6	730	57	673	1 714	140	1574
	2.4438	1.4737	0.9701	2.5309	1.45	1.0809
	(1.8593)	(0.7816)	1.0777	(2.3206)	(0.7804)	1.5402
7	424	71	353	985	140	845
	1.9505	1.31	0.6405	2.3005	1.8214	0.4791
	(1.5513)	(0.6457)	0.9056	(2.0721)	(1.1397)	0.9324
8	37	37	0	233	147	86
	1.1892	1.3514	-0.1622	1.3906	1.7143	-0.3237
	(0.3971)	(0.7156)	-0.3186	(1.4255)	(1.205)	0.2205
9	102	52	50	295	74	221
	1.5588	1.2115	0.3473	1.7051	1.3784	0.3267
	(0.9073)	(0.5364)	0.3709	(1.1117)	(0.8553)	0.2564
10	441	25	416	893	43	850
	1.7256	1.36	0.3656	1.8891	1.2093	0.6798
	(1.2208)	(0.7572)	0.4636	(1.3716)	(0.4659)	0.9057
11	2751	0	2751	5 273	133	5140

	2.36	0	2.36	2.7948	2.1955	0.5993
	(1.9052)	0	1.9052	(2.4367)	(1.811)	0.6257
12	1 029	0	1 029	2 090	0	2 090
	2.4344	0	2.4344	2.555	0	2.555
	(2.009)	0	2.009	(2.2499)	0	2.2499
13	87	0	87	385	0	385
	1.23	0	1.23	1.5662	0	1.5662
	(0.6043)	0	0.6043	(1.3155)	0	1.3155
14	453	125	328	1 013	343	670
	2.4437	1.68	0.7637	2.5874	2.0437	0.5437
	(1.9821)	1.1188	0.8633	(2.2776)	(1.5675)	0.7101
15	116	146	-30	396	438	-42
	2.1552	1.904	0.2512	2.697	1.9315	0.7655
	(2.1972)	(1.3561)	0.8411	2.4493	(1.3564)	1.0836
16	146	4	142	205	27	178
	2.1027	1	1.1027	2.2927	2.5185	-0.2258
	(2.4878)	0	2.4878	(2.2649)	(2.6072)	-0.3423
17	152	0	152	677	0	677
	1.4934	0	1.4934	2.288	0	2.288
	(1.352)	0	1.352	(1.8688)	0	1.8688
18	597	0	597	1 269	27	1 242
	2.3668	0	2.3668	2.7029	1.963	0.7399
	(1.969)	0	1.969	(2.4051)	(1.344)	1.0611
19	407	441	-34	737	1061	-324
	2.2752	2.3968	-0.1216	2.46	2.2752	0.1848
	(1.9407)	(2.1114)	0.1707	(2.2321)	(2.0465)	0.1856

Note: Descriptive statistics for the change in PP in January 2012, both before and after the change. Descriptive statistics for monthly sales are reported for the treatment group, control group and the difference between the two groups for the whole period 2009 to 2015. County(1-19): 1:Akershus, 2:Aust-Agder, 3:Buskerud, 4:Finnmark, 5:Hedmark, 6:Hordaland, 7:Møre og Romsdal, 8:Nordland, 9:Nord-Trøndelag, 10:Oppland, 11:Oslo, 12:Rogaland, 13:Sogn og Fjordane, 14:Sør-Trøndelag, 15:Telemark, 16:Troms, 17:Vest-Agder, 18:Vestfold, 19:Østfold

Table 0.3: Summary Statistics 2011 to 2012 (including number of observations)

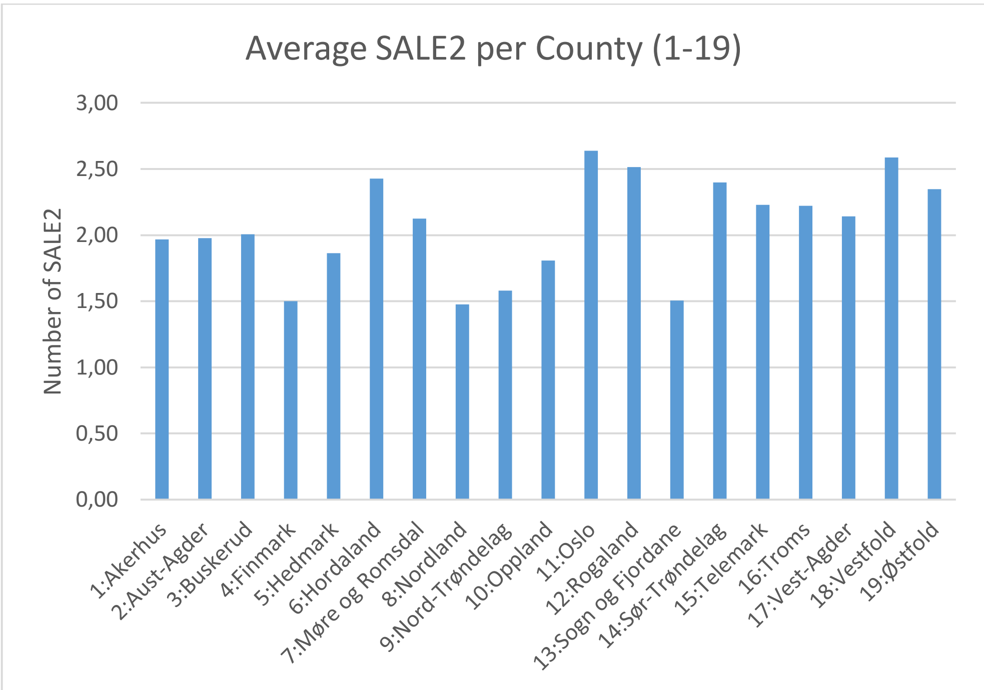
Summary Statistics (2011/2012) before and after the PP change in January 2012. Presented are number of observations, mean monthly observations and standard errors are in parentheses.

	Before January 2012			After January 2012		
	Treated	Control	Change	Treated	Control	Change
Outcome Variables						
SALE2	3 821	596	3 225	4 126	669	3 457
	2.324	1.9446	0.3794	2.3589	1.9342	0.4247
	(1.8832)	(1.7847)	0.0985	(1.9276)	(1.488)	0.4396
SALE1	10 216	2 056	8 160	10 679	2 199	8 480
	3.6472	3.4864	0.1608	3.5726	3.3192	0.2534
	(3.5185)	(9.6624)	-6.1435	(3.4723)	(6.7851)	3.3128
Control Variables						
Population	11.8327	6.6094	5.2233	11.8754	6.4125	5.4629
	(7.3866)	(2.8950)	4.4916	(6.8315)	(2.5981)	4.2334
Share of Male	93%	96%	-3%	93%	97%	-4%
Brand (1-4):						
1	717	75	642	763	82	681
	2.4114	2	0.4114	2.2687	1.8537	0.415
	(1.7796)	(1.3457)	0.4339	(1.5657)	(1.3064)	0.2593
2	1 779	281	1 498	2 005	312	1 693
	2.4058	2.1708	0.2278	2.4449	2.1667	0.2782
	(1.8777)	(2.2021)	-0.3244	(1.9181)	(1.7305)	0.1876
3	870	154	716	867	177	690
	2.223	1.6948	0.5282	2.3656	1.791	0.5746
	(2.0703)	(1.2542)	0.8161	(2.2198)	(1.3083)	0.9115
4	455	86	369	491	98	393
	2.0593	1.6047	0.4546	2.1365	1.5204	0.6161
	(1.6442)	(1.2298)	0.4144	(1.9027)	(0.8523)	1.0504
County (1-19):						
1	314	0	314	317	0	317
	1.9204	0	1.9204	1.9842	0	1.9842
	(1.1977)	0	1.1977	(1.3559)	0	1.3559
2	0	69	-69	0	67	-67
	0	1.8986	-1.8986	0	2.0299	-2.0299
	0	(1.7162)	1.7162	0	(1.4972)	1.4972
3	176	84	92	230	90	140
	2.4489	1.6548	0.7941	2.2913	1.7667	0.5246
	(1.9206)	(0.8849)	1.0357	(2.0233)	(1.2812)	0.7421
4	26	24	2	23	26	-3
	2.1538	1.0833	1.0705	1.7391	1.2692	0.4699
	(1.4884)	(0.2823)	1.2061	(0.8643)	(0.5335)	0.3308
5	200	0	200	204	0	204
	1.85	0	1.85	1.8382	0	1.8382
	(1.1936)	0	1.1936	(1.5751)	0	1.5751
6	295	27	268	322	33	289
	2.4712	1.4815	0.9897	2.528	1.3333	1.1947
	(1.9126)	(0.753)	1.1596	(2.025)	(0.5401)	1.4849
7	198	35	163	216	39	177
	2.1162	1.2571	0.8591	2.0972	1.8462	0.251
	(1.7134)	(0.6108)	1.1026	(1.4863)	(0.9608)	0.5255
8	15	13	2	56	27	29
	1.2667	1.3077	-0.041	1.2321	1.6667	-0.4346
	(0.4577)	(0.6304)	-0.1727	(0.5718)	(1.1435)	-0.5717
9	50	25	25	54	19	35
	1.68	1.4	0.28	1.9444	1.4211	0.5233
	(0.9355)	(0.7071)	0.2284	(1.2946)	(0.9016)	0.393
10	208	11	197	200	8	192
	1.7885	1.5455	0.243	1.825	1.25	0.575

	(1.3127)	(1.0357)	0.277	(1.2418)	(0.463)	0.7788
11	1 095	0	1 095	1 112	5	1 107
	2.4795	0	2.4795	2.5719	1.6	0.9719
	(1.8738)	0	1.8738	(1.9688)	(0.5477)	1.4211
12	423	0	423	417	0	417
	2.6501	0	2.6501	2.6451	0	2.6451
	(2.2937)	0	2.2937	(2.1514)	0	2.1514
13	37	0	37	62	0	62
	1.2703	0	1.2703	1.3387	0	1.3387
	(0.7321)	0	0.7321	(0.7670)	0	0.7670
14	188	55	133	210	64	-44
	2.6809	1.8364	0.8445	2.6952	2.0625	0.6327
	(2.2738)	(1.3846)	0.8892	(2.0711)	(1.4787)	0.5924
15	66	63	3	95	83	12
	2.1818	1.7619	0.4199	2.5789	1.9277	0.6512
	(1.6817)	(1.1875)	0.4942	(2.3681)	(1.2176)	1.1505
16	28	3	25	24	2	22
	3.2143	1	2.2143	1.2917	1.5	-0.2083
	(4.9017)	0	4.9017	(0.6241)	(0.7071)	-0.083
17	79	0	79	143	0	143
	1.519	0	1.519	2.1538	0	2.1538
	(0.9722)	0	0.9722	(1.801)	0	1.801
18	255	0	255	272	1	271
	2.4902	0	2.4902	2.7574	1	1.7574
	(2.0232)	0	2.0232	(2.1784)	0	2.1784
19	168	187	-19	169	205	-36
	2.506	2.6471	-0.1411	2.426	2.2634	0.1626
	(1.9392)	(2.564)	-0.6248	(2.572)	(1.9299)	0.6421

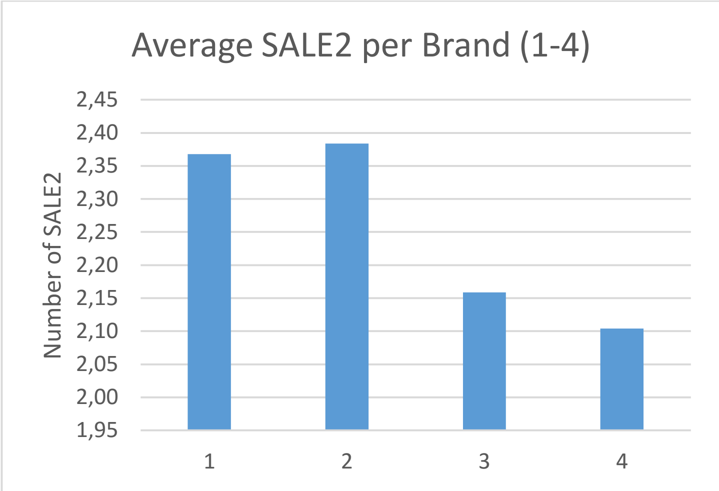
Note: Descriptive statistics for the change in PP in January 2012, both before and after the change. Descriptive statistics for monthly sales are reported for the treatment group, control group and the difference between the two groups for the period 2011 to 2012. County (1-19): 1:Akershus, 2:Aust-Agder, 3:Buskerud, 4:Finnmark, 5:Hedmark, 6:Hordaland, 7:Møre og Romsdal, 8:Nordland, 9:Nord-Trøndelag, 10:Oppland, 11:Oslo, 12:Rogaland, 13:Sogn og Fjordane, 14:Sør-Trøndelag, 15:Telemark, 16:Troms, 17:Vest-Agder, 18:Vestfold, 19:Østfold

Figure 0.1: Average SALE2 per County



Note: Average SALE2 by Company X in each of Norway's 19 Counties.

Figure 0.2: Average SALE2 per Brand



Note: Average SALE2 by Company X per Brand (1-4). Brand number 2 has the highest average monthly sale of SALE2.

Table 0.4: Sub-Sample Analysis

The treatment effect on monthly sales with different sample restrictions.
Dependent variable: Monthly sales

	(1)	(2)
treatment	0.0754	0.401*
	(0.236)	(0.200)
yr2009_treatment	-0.0200	-0.173
	(0.178)	(0.142)
yr2010_treatment	0.159	0.000418
	(0.108)	(0.0858)
yr2012_treatment	0.197	0.0444
	(0.220)	(0.131)
yr2013_treatment	0.0899	-0.0343
	(0.129)	(0.140)
yr2014_treatment	0.312	0.222
	(0.205)	(0.132)
yr2015_treatment	0.302	-0.0253
	(0.204)	(0.147)
Fixed effects:		
Year	Yes	Yes
Month	Yes	Yes
County	Yes	Yes
Individuals:		
Treatment	657	827
Control	185	581
R^2	0.0518	0.0432
Adjusted R^2	0.0490	0.0416
Sub-Sample	Small dealer stores	All dealer stores, except those located in Oslo
Observations	14 368	25 384

*Note: Robust standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01. This table reports the treatment effect on monthly sales of SALE2. treated is an independent variable that equals 1 if the individual is in the treatment group and 0 if the individual is in the control group. treated represents the difference in monthly sales of SALE2 between the treatment group and the control group before the change of PP in January 2012. The interaction terms yr*treated represents placebo analysis using 2011 as reference year. The year dummy variable takes value 1 when a dealer store made sales in that year, 0 otherwise. The interaction term therefore shows how the difference in monthly sales between the two groups have changed compared to the reference year, 2011. Model (1) and (2) include year, month and location covariates. Model (1) represents small dealer stores and are estimated using the robust option in Stata. Model (2) represents dealer stores that are located in counties except Oslo and are estimated using the robust option in Stata.*