A Pay Change and Its Long-term Consequences

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This version: November 2019

Abstract:
We investigate the consequences of an unexpected pay change in a personnel search firm. Top management's decision to adjust the pay of some divisions' workers to the pay of other divisions resulted in a quasi-experiment increasing the fixed wage but decreasing the slope of the bonus function. Consultants' output in the affected divisions decreased by around 30%, and attrition increased. Based on data from the firm's management information system, we document that efforts decreased by the same order of magnitude, and so did absenteeism. We provide evidence that workers' spot reaction to the flatter slope of the bonus function is unable to explain these effects. Rather, the findings support the fair wage hypothesis and relational contracting. Observing the effects over a period of more than three years, we show long-term negative reciprocity of those affected, but no negative selection effects. Consultants who enter after the pay change are equally productive as the ones who were in the firm when the pay change occurred. Nonetheless, the pay change would have been profitable for management only for a horizon of ten years or more.

Keywords: incentives, pay for performance, insider econometrics, fair wage, relational contracts, organizational economics, wage rigidity

JEL Codes: M2, M5, J3

Miriam Krueger: miriam.krueger@bundesbank.de; Guido Friebel: gfriebel@wiwi.uni-frankfurt.de. This paper is based on Chapter 1 of Miriam Krueger's PhD thesis (2015). We are grateful for the comments of seminar participants in UC Berkeley, Cambridge, Cologne, Essex, Goethe U Frankfurt, Kellog, MIT Org Econ, Rotterdam, Rotman Toronto, Tilburg, the Relational Contracting Conference in Madrid 2016, the German Economic Association meeting in Augsburg 2016, the EEA in Geneva 2016, and the COPE in Cologne 2014. We would like to thank the partners of our study firm, and Yvonne Klemm for excellent assistance with the data. Any opinions

expressed in this paper represent the authors' personal opinions and do not necessarily reflect the views of the Deutsche Bundesbank or its staff.

Introduction

Finding the right compensation scheme for a firm's workers is an intricate challenge for management. In particular, in early stages of a firm's operations, management lacks precise knowledge about how workers will react to fixed and variable wage components and how workers' actions translate into profit.[1] Over time, management gains knowledge about the link between compensation and outputs and may be tempted to adjust the compensation scheme, in order to save on the costs of incentivizing workers' effort and attracting productive workers. Such adjustments, meant to reduce workers' rents can be costly, though. Workers may consider such pay changes as unfair (Akerlof, 1980; Akerlof & Yellen, 1988, 1990; Kahneman, Knetsch, & Thaler, 1986) and react by withdrawing effort or leaving the firm; these reactions can also be part of an optimal punishment strategy in a relational contract (Gibbons & Roberts, 2012). Timothey Bewley summarized the belief about the potentially drastic consequences of pay changes as follows: *"a firm would lose more money from the adverse effects of cutting pay than it would gain from lower wages and salaries. What restrained employers from cutting pay was the belief that doing so hurts morale and increases labor turnover"* (Bewley, 1998 pp. 475-476).[2]

Despite the general agreement among economists that pay cuts can be prohibitively costly, firms *do* change their compensation scheme, but the real-world evidence on the effects is scarce. We here present such evidence from a unique data set. Top management of a global personnel search company decided to change the pay of some divisions' workers in Germany. For historical reasons, pay had differed across divisions, and top management decided to standardize the compensation system across divisions. The workers in some divisions experienced a cut in bonuses, and an increase in fixed wages, while the system in the other divisions remained unchanged.

This setting allows for a particularly useful quasi-experiment to estimate the effects of a pay cut on worker efforts and outputs. First, the change came as a surprise, the decision came from global top management and was not influenced by management in Germany. Also, it was announced only shortly before the pay change went live. Second, workers do not interact across divisions, minimizing the risk of spillovers. Third, workers are used to volatile income streams

[1] Cf. the literature on specific knowledge (Jensen & Meckling, 1992; Raith, 2008)

[2] Campbell III and Kamlani (1997) provide an overview of the theoretical literature, and Bewley (2007) an overview of the empirical work related to wage rigidity.

and operate in an individualistic organizational culture, such that our findings are likely to be lower bound estimates for the effect of a pay cut.

We observe all offices in Germany before and after October 2009 when the compensation structure was harmonized across divisions over a period of almost four years. The adjustment caused a permanent reduction in pay, for the more productive employees, while for some less productive employees, pay increased.

We find strong negative effects in employees' output in the realm of 30% and more. The pay change also resulted in increased turnover of the more productive employees and in more absenteeism. Furthermore, because the management information system (MIS) registers the employees' activities, we can document the precise mechanisms of the output fall: the affected employees reduce effort along different dimensions of their activities such as client meetings, vacancies and candidates identified. Absence of the affected workers also increases.

While these results support the beliefs about the negative effects of a pay change in the by now classical literature, there is a surprise, though. We find no evidence that new entrants after the pay change are less productive than the people they replace. Hence, new entrants are unaffected by potential long-term cultural effects of mistrust in the respective divisions. Moreover, it appears that the initial high bonuses were not necessary to attract high-quality workers, i.e., we find no evidence for sorting of workers (Lazear, 2000, Lazear & Shaw, 2007). Indeed, top management's belief that the difference in the steepness of the bonus slopes contributed little to incentivizing and inducing sorting of workers was an important factor in their decision to adjust the pay of the "treated" division in the first place (although the pay change was framed in a different way, see the next section). The absence of sorting effects can be explained by the fact that most hires have no experience in the job. Hence, they may have little if any private information about their productivity in the job.

We also investigate whether the reduction in performance is a rational reaction to flatter compensation curve and find that this cannot explain our observations. This complements the results in Jayaraman et al (2016) who look at a similar experiment in a different cultural context. In their work, upon a wage rise, they observe an initial reaction of employees that could be interpreted as an indication of workers' reciprocal motivation, but after a few months, the effort response of workers seems to be entirely in line with a standard incentive model. The long-lived negative reactions in our case square well with the results of a large literature in laboratory settings and short-term field experiments (some of them reviewed in Camerer & Weber, 2012, and, more recently, Kube et al, 2013, Cohn et al, 2014a and b, Heinz et al, 2019) indicating that negative reciprocity tends to produce stronger reactions than positive reciprocity.

Our paper, however, is the first that can establish the long-lived nature of negative reciprocity. We observe people over a span of almost four years, and it is only when the employees who experienced the pay change have left the firm that the negative effect of the treatment on output vanishes. We hence believe to fill an important gap about the real-world effects of reciprocity at the workplace. Among the advantages of this paper is the quasi-experiment triggered by headquarters from outside the country we look at, and that individual panel data is observable on a monthly basis over a time span of several years. We hence observe the development of the effects on individuals and can disentangle post-treatment changes related to a decrease in productivity and post-treatment changes related to workforce changes such as an increase in turnover and adverse selection. Since output and employee activities are recorded, clear individual profit and effort measures of each employee are observable. Another reason why this personnel search firm is attractive for being studied is that employees work on their own, do comparable jobs, and that there are no risks of spillovers, because employees do not interact across divisions.

Most of the papers from the field are not experimental; Greenberg (1990) looks as employee theft as a response to pay changes, and Lee and Rupp (2007) interpret delays of planes as pilots' reactions to pay cuts (but do not find much evidence). Although he does not look at pay cut, Mas (2006) is also related: he looks at the reactions to the outcomes of wage negotiations that do not fulfill police officers reference points.[3]

Despite the persistence of negative reciprocity that we document, it is not clear at all whether the pay change was, on balance, profitable or not. There is a "relatively" short-term loss due to a lower generated revenue associated with the pay change (in the realm of 24 months), but there is a long-term gain, because the firm succeeds in hiring equally productive new workers at lower cost. A back-of-the-envelope calculation however shows that the horizon of management would need to be more than ten years for the pay change to be profitable, even at very optimistic assumptions about interest rates and firm growth.[4]

It is interesting to contrast our paper with the subsequent work of Sandvik et al (2018) about a pay cut in a call center. They find similar effects in attrition, but not in effort, and no evidence for a behavioral channel. It is conceivable that this is owing to different contexts, because in our setting, employees are given substantial discretion about their effort choices[5]

[3] Smith (2015) studies the effect of wage cuts on employee satisfaction using British household data.

[4] Moreover, there are important factors that we cannot price like (i) negative effects on the talent pool for team leaders and management positions, and (ii) potential reputational losses for the firm.

[5] Note that the presence of the MIS in which activities and efforts are registered does not mean that workers efforts are controlled. Ultimately, employees in our firm were retained and rewarded conditional on output, not input. Information about their activities was mainly used for training purposes and a feedback mechanism.

and they have an average tenure of 18 month and career concerns, while in Sandvik et al. (2018), call centers, the firm exerts much more control over employees' work, and their horizon is shorter, potentially providing less bite for a story like ours that emphasizes the impact of frustration on effort and attrition. Moreover, they look at a much shorter period of observation, the setting appears more as a spot relation than a long-term employment relation. Finally, we benefit of a setting in which treatment occurs across divisions, lending additional support to our identification strategy. Another interesting paper that is related is Coviello et al (2020) who document counter-productive actions of workers after a pay cut.

Before describing the setting, and discussing our findings, we would like to emphasize that our paper provides evidence for a very important assumption in many macro models, namely, downward wage rigidity. Going back to, at least, McLaughlin (1994) and Kahn (1997), most of the literature (recently reviewed by Faia and Penzone, 2019) has analyzed the institutional roots of downward rigidity. In many countries, though, white collar workers do not enjoy so much protection as blue collar work does. Our workers are not unionized and they are used to income volatility. Their fixed wage is even increased by the pay change, and yet, the effects are very large. Still, in line with the fair wage hypothesis, they seem to despise their firm's decision to cut bonuses and react as predicted in Hart and Moore (2008) and Halonen-Akatwijuka and Hart (2013) who argue that contracts act as reference points, deviations from which are penalized by shading. Our evidence hence also supports the assumptions of these theories of incomplete contracting.

1. The Firm, Work and Workers

Our study firm is a personnel search firm operating in many countries and with a focus on middle management and specialist vacancies in different industries. After entering the German market, the firm grew rapidly, opening around 18 offices in the biggest German cities; in each office, all workers belong to the same division. In total there are four divisions. Even though the divisions belong to the same parent company, offices were managed separately. Company headquarters is located outside Germany and supervises the German divisions; during the time of the pay change, it also maintained a few common support services for offices all over Europe. Table 1 presents an overview of the firm's German operations, covering the entire span of observation from December 2008 to September 2012: the average division includes 58 employees in non-management positions who we will frequently refer to as "consultants"; the average office includes 25 consultants. We do not consider support staff and managers, because they were not affected by the change and we have no performance data on them.

Consultants work individually with firms (the "clients") and "candidates" to fill existing vacancies with matching workers. Upon successfully matching a candidate and a job, around 20 to 30 percent of the annual income of the new job holder is paid by the client to the consulting firm. Within the divisions, some of the staff deals with permanent placements and some with placing freelancers into projects (temporary placements). This distinction has some implications on the consultants' pay structure, as explained in the next section. Around 68 percent of the consultants deal with permanent placements, 32 percent with freelancers. This has some consequences on revenues, but for the purpose of this paper, it is irrelevant. Consultants work for one division only, and there are no movements between the divisions on the consultant level.

Table 1: Overview, Consultants and Pay

	Mean	St. Dev.
Number of consultants per division	57.6	[27.72]
Number of consultants per office	25.41	[14.52]
Tenure in months	17.79	[10.80]
Percentage fixed pay per consultants	0.63	[0.03]
Male (percentage)	0.53	[0.5]
Year of birth	1980.48	[13.65]
German (percentage)	0.85	[0.35]
College degree (percentage)	0.96	[0.20]

Notes: This sample includes 572 employees (Junior Consultants, Consultants, Senior Consultants) and covers a time period of 46 months. For accounting reasons, a "month" can vary between 4 and 5 weeks. The unit of observation is an employee-month. Tenure shows the tenure in the last observed month in the sample. The full sample contains 8,936 employee-month observations.

Because the firm only promotes from within, consultants are normally hired after completion of their bachelor's degree, and most have no experience in the job. Consultants' jobs are similar across the divisions, varying only by industry and regions. Management positions differ because they involve training and directing consultants. Promotions to a management position occur after 30 months on average; further career advances to positions of regional manager or in various support functions are possible. Consultants who leave the firm mostly stay in the personnel search business or related sales jobs. Average tenure is 18 months.

Output depends on the individual consultant's effort to fill a vacancy but is also determined by seasonal, sectoral and regional variation. Effort entails different activities: meetings with clients, finding new vacancies and new candidates. These activities are meticulously measured through a management information system (MIS). Consultants are supervised by managers who also provide on-the-job training. Consultants' output is recorded

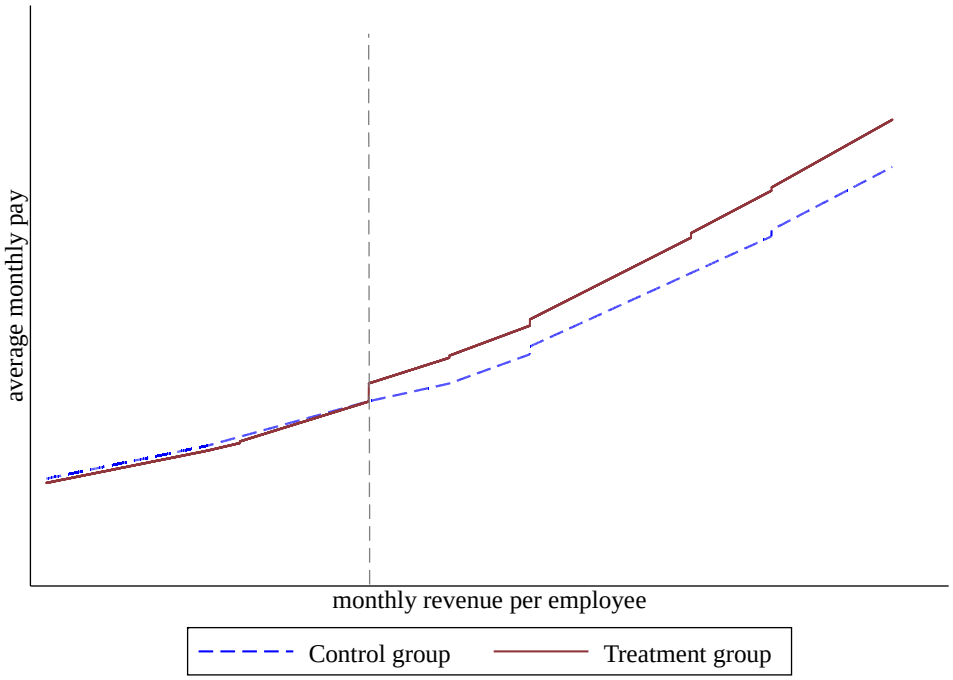
electronically through the MIS introduced to all German offices at the end of 2008. Since activities and output are checked by the managers and the billing department on a regular basis, there are little measurement errors. Records are visible for the management of the same division. Consultants get to know their colleagues' performance in the same division because performance scores show the relative performance of employees within their division. Additionally, a score shows the relative performance of the own division compared to the ones of other offices of the same division in Germany, Europe and worldwide. Records of other divisions' employees are usually not visible, except for upper management. In line with this highly transparent working environment, and a competitive corporate culture, all offices are open spaced.[6]

2. Compensation and the Change in the Compensation Scheme

Consultants' pay consists of a fixed and a variable component, the latter representing on average 1/3 of total monthly compensation (as can be seen in Table 1). Fixed pay on the non-management level is on average around 2500 Euro per month. Consultants receive a commission conditioned on the pay the firm receives from clients, that we will refer to as "revenue". The precise computation of this commission is somewhat intricate because it depends on the wage of the candidate the client hired, the timing of employment contracts between clients and candidates, and the distinction between permanent and free-lance positions. On average, around ten percent of the revenue that accrues to the firm is directly paid to the responsible consultant, but the scheme of the commission is convex (see Figure 1).

Figure 1: Compensation Schemes before and after the Change

[6] Typically, offices are supervised by regional managers whereby within the offices different teams are supervised by one team manager each who also coaches each consultant by setting targets for output and activities.



Notes: Figure 1 shows how performance (monthly generated revenue) relates to average monthly pay per consultant for the treatment and the control group before the pay change. The vertical line marks the threshold, roughly 25,000 Euro, beyond which the pay function is steeper in the treatment group compared to the control group before the pay change.

Revenue depends on the hired candidates' annual wage and accrues one month after the new hire begins to work for the client. Consequently, if a consultant has placed more than one candidate who happen to start working in the same month, and given the convex nature of the commission, the consultant's commission is higher than if the starting date of the new hires were to be distributed evenly across a number of months. While a consultant's effort affects the number of candidates placed, they have little if any influence on the starting date of a contract between client and candidate because they are not involved in the negotiations. The distinction between permanent and free-lance contracts is also pay-off relevant: While permanent contracts involve one payment only, free-lance contracts involve a stream of monthly commissions over the duration of the free-lancer/client relationship. Finally, there is a quarterly bonus paid out only to top performers and very rarely (only 1.7 percent of the commission payments are quarterly bonuses).

Before the pay change in October 2009, divisions differed in their compensation structure. Group A, the "treatment group" includes two divisions and group B, the "control group" also consists of two divisions. Consultants in the control group incurred no change, while the compensation of the treatment group had slightly lower fixed wages, but higher commission rates. Figure 1 shows how cumulated monthly revenue maps into consultants' compensation before the pay change. Most importantly, in the control group, fixed wages for

junior consultants were on average 10 percent higher than in the treatment group, and for Consultants and Senior Consultants the respective figures were (7.2 percent, 2.5 percent). More importantly, for total expected pay, the slope of the commission curve was substantially steeper in the treatment group, from around 25.000 Euro revenue onwards, and at an increasing rate.

From October 1st, 2009, the compensation scheme of the treatment group was adjusted to the one of the control group. As a consequence, the average expected pay of consultants in the treatment group -- keeping revenue before and after the pay change constant -- decreased by 3.5 percent. The pay change was first announced in August 2009, and became effective on the 1st October. Top management in the headquarters communicated as a main reason for the change a strategic reorientation of the firm's operations, going along with the need to harmonize HR policies across the four divisions: *"the current scheme puts too much emphasis on short-term goals, rather than long-term career opportunities; it doesn't reward long-term achievers as well as it should. The new scheme puts all brands in line with each other, making mobility and recruitment more effective."*

Meetings were held in each office of the affected divisions and it was pointed out that the change would potentially lead to a pay cut for consultants. However, career perspectives in the restructured firm, and across divisions were also highlighted. Consultants in the treatment group were asked to sign amendments to their contracts. While this created a good amount of discussions and perturbance, according to our knowledge, all consultants signed. It may appear somehow surprising that nobody resisted the firm's demand for the contract change, because the pay stipulations were an explicit component of the initial contract. However, consultants may have expected adverse consequences of not signing and hence shied away from challenging the pay change in the firm or in court.

Evidently, the realized pay decrease would depend on consultants' expectations about future revenues, given their own performance record. Indeed, interviews in the firm revealed that when the new compensation scheme was announced, consultants in the treatment group calculated the pay change depending on their past revenue. When we do the same, it becomes clear that roughly half of the consultants rationally expected cuts in pay, while the other half, because of the increase in fixed wages, would even stand to benefit from the change. The largest effect when holding constant the performance at the level prior to the pay change would have been 5.4% for the most senior consultant level. However, it turned out that because of the effort and output reduction we document below, consultants in the treatment group experienced a wage decrease of 19% on average.

3. Data and Research Design

Figure 2 depicts the structure of our data. In total, the data cover 572 consultants with an average of 17.79 months tenure in the firm, and hence 8,936 observations. Our results are derived from regressions using data on all employees, and from fixed-effect regressions of the 128 employees who entered the firm before the pay change, dropped out of the sample after the pay change, and were either working for the treated divisions or for the control division. In the graph, this is represented by the arrow in the middle of the upper panel, while the respective arrow in the lower panel represents the 77 "treated" individuals. Consequently, there are 51 non-treated individuals.

We use a standard diff-in-diff design; in the robustness section, we also present an analysis that collapses pre- and post-treatment period to avoid potential problems of serial correlation (Bertrand, Duflo, & Mullainathan, 2004).

Figure 2: Data Structure

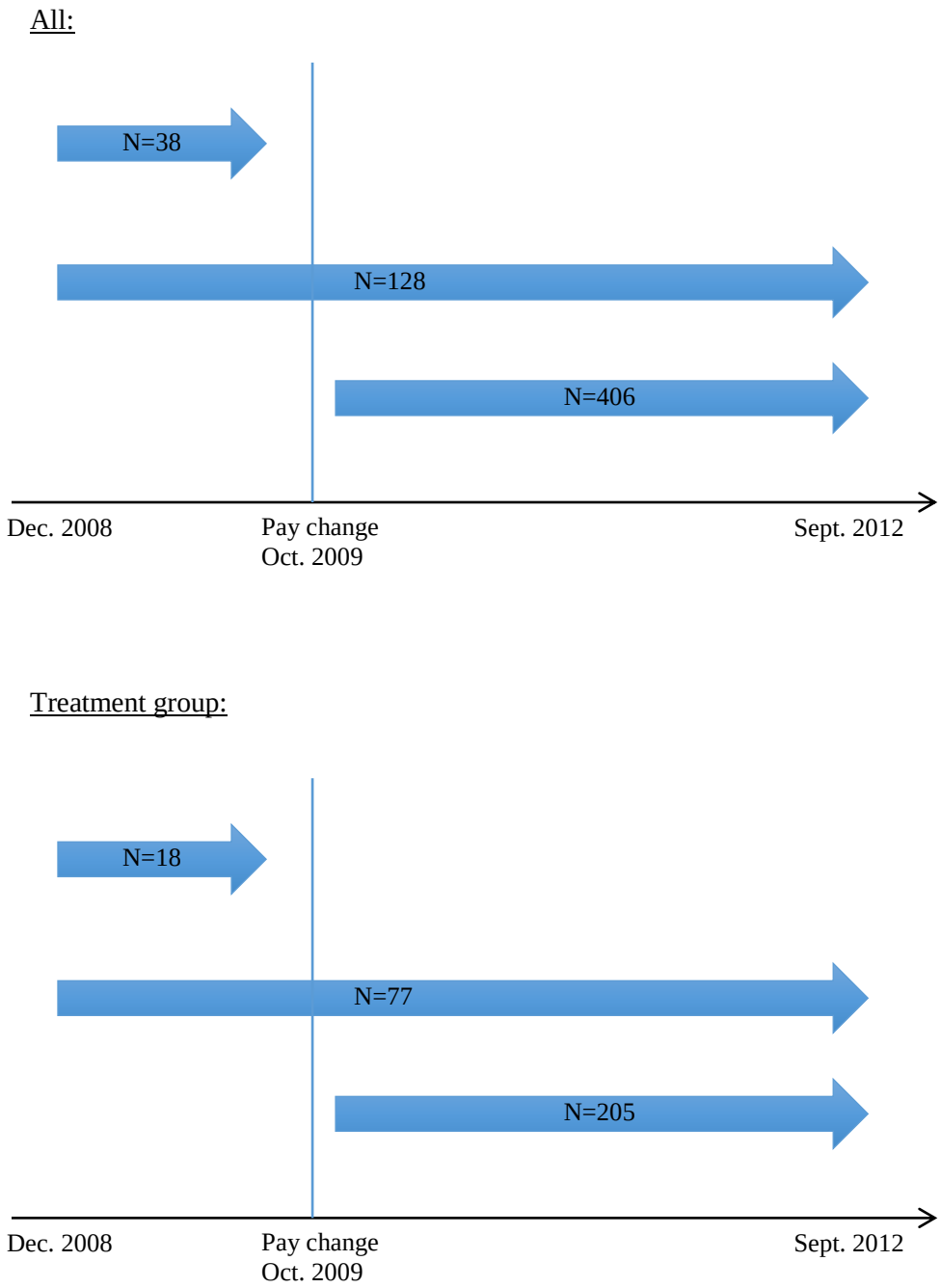


Table 2 presents an overview over the personal characteristics of the consultants in treatment and control divisions, and their location. Table 3 presents statistics on the activities of the consultants as recorded by the firm's MIS. The MIS provides detailed information about consultants' output, in terms of number of placements, and total revenue, but also activities on the job. Collapsing the data on consultant level, statistically significant differences exist concerning placements and revenue, and "candidates found". In all categories, the treatment group exhibits larger numbers. There are sectoral differences between the two groups (for

which we will control in the regressions). Looking at the largest sector of the firm's activities, IT, we find that except for "candidates found", the differences disappear. While our empirical strategy is robust to pre-treatment differences that are not related to dynamics in outcome variables, we will later control both for pre-treatment trends and for sector-time fixed effects.

Table 2: Personnel Characteristics (Dec. 2008 - Sept. 2009)

	Treatment Group		Control Group		Difference	
	Mean	St. Dev.	Mean	St. Dev.	Diff_Mean	St. error
Tenure in months	15.85	9.88	14.80	10.00	1.05	1.56
Male (percentage)	0.57	0.50	0.49	0.50	0.08	0.08
Year of birth	1978.40	14.47	1979.51	13.87	-1.11	2.23
German (percentage)	0.80	0.40	0.87	0.34	-0.07	0.06
College degree (percentage)	1.00	0.00	0.97	0.16	0.03	0.02
Group: Temporary placements (percentage)	0.34	0.48	0.29	0.46	0.04	0.07
City:						
1	0.50	0.50	0.45	0.50	0.04	0.08
2	0.41	0.50	0.45	0.50	-0.04	0.08
Other cities	0.10	0.29	0.10	0.30	-0.00	0.05
	95		71			

Notes: This sample includes employees (Junior Consultants, Consultants, Senior Consultants) in the pretreatment months (Dec. 2008 - Sept. 2009). The data is collapsed on consultant level.

Table 3: Activities

	Full sample (Dec. 2008 - Sept. 2009)						Sector: IT (Dec. 2008 - Sept. 2009)					
	Treatment group		Control group		Difference		Treatment group		Control group		Difference	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Error	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Error
Placements	0.65	0.48	0.52	0.41	0.13	0.07	0.65	0.40	0.53	0.41	0.12	0.08
Revenue	9198.67	7715.70	7362.86	6615.47	1835.81	1139.96	8681.55	6463.49	7120.80	6040.16	1560.74	1291.28
Vacancies found	4.30	2.61	4.00	3.84	0.30	0.50	3.92	2.49	3.83	3.24	0.09	0.61
Meetings scheduled	2.47	1.82	2.71	1.74	-0.25	0.28	2.84	1.72	2.66	1.69	0.18	0.35
Candidates found	7.25	5.30	4.01	4.55	3.24	0.78	6.46	4.01	3.62	4.25	2.84	0.86
Absenteeism (days)	2.51	1.93	2.93	2.38	-0.42	0.34	2.63	1.44	2.93	2.09	-0.30	0.38
Exit probability	0.05	0.16	0.07	0.17	-0.02	0.03	0.06	0.17	0.06	0.12	0.00	0.03
Number of consultants	95		71				43		51			

Notes: This sample includes employees (Junior Consultants, Consultants, Senior Consultants) in the pretreatment months (Dec. 2008 - Sept. 2009). The data is collapsed on consultant level. Sample IT includes only employees working in the IT sector.

We use a standard difference-in-difference design:

(1) $$y_{ijt} = \alpha + \gamma_j + \lambda_t + \beta D_{jt} + \delta X_{ijt} + \varepsilon_{ijt.}$$

Here γ_j is an indicator function that has the value 1 for the treatment group; λ_t is an indicator function for post-treatment period. D_{jt} is the indicator for the change in the compensation scheme, i.e. the interaction of the two effects. To balance pre- and post-treatment period duration and to reflect employees' average tenure, we consider an event window of 20 months around the date of the pay change. Nevertheless, we also look at the entire three-year post-treatment period to observe the long-term development of the effect. Standard errors are clustered on individual level to control for serial correlation problems (Bertrand et al., 2004).

We also run regressions with individual fixed-effects γ_i, and D_{it} represents the time-treatment effect on the individual level:

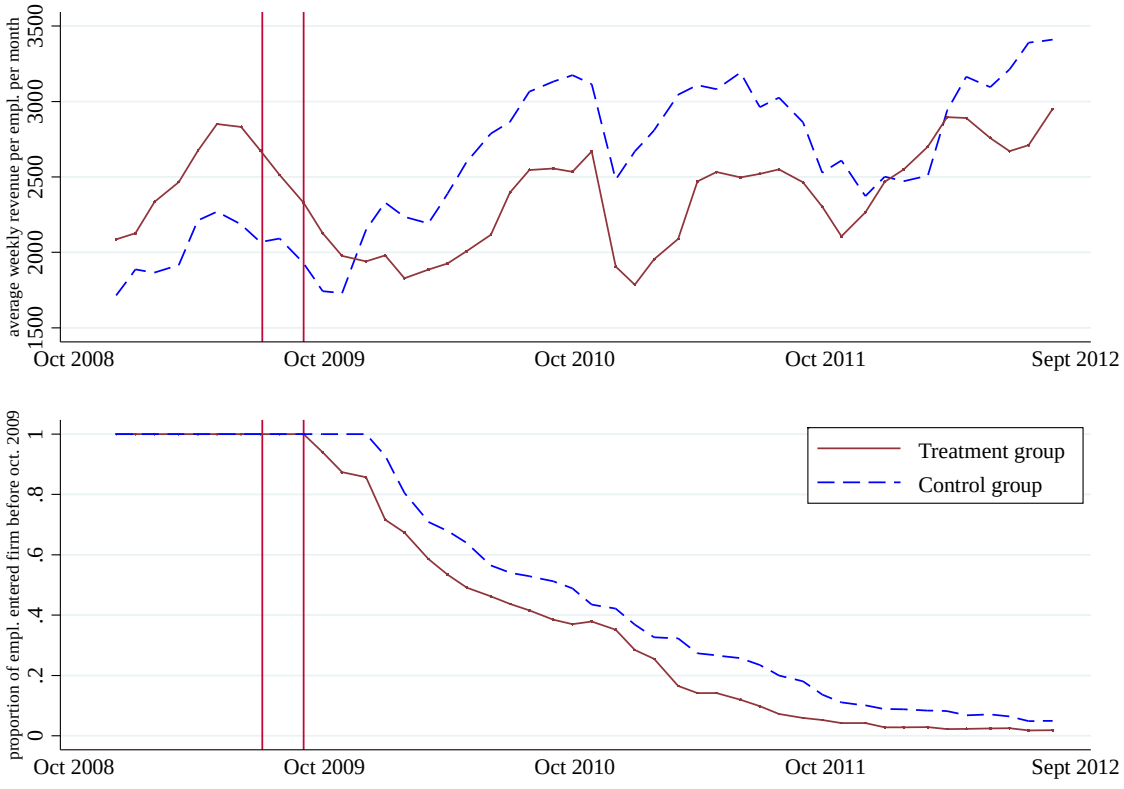
(2) $$y_{ijt} = \alpha + \gamma_i + \lambda_t + \beta D_{it} + \delta X_{it} + \varepsilon_{it}$$

Because this specification controls for all time-invariant person effects, the comparison of the two regressions allows to shed light on composition effects in the treatment group that are triggered by exit of treated individuals and replacement by new recruits, similar to Lazear (2000).

3. Effects of the Pay Change on Revenue, Deals and Retention

Figures 3 (a) and (b) depict the development of revenues and employee attrition over the span of observation (December 2008 to September 2012) for all workers who entered the firm before the treatment and stayed at least until the moment the pay change was introduced.

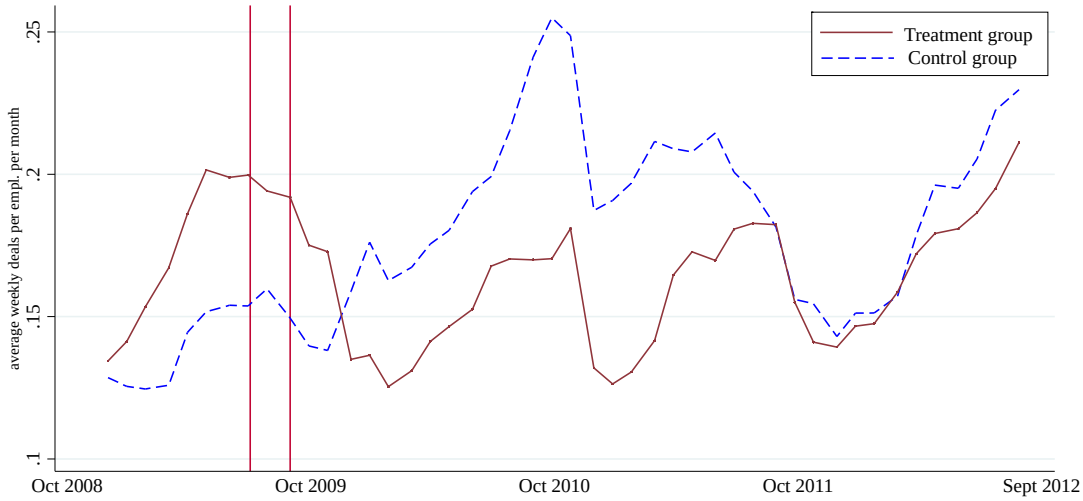
Figure 3: (a) Revenues, (b) Retention



Notes: The upper figure depicts 5-month moving averages of weekly generated revenue (without contract extensions) per month. Data cover the time from December 2008 to September 2012. The lower figure shows retention within treatment and control group. The vertical axis represents the proportion of employees who entered the firm before October 2009 and are still in the firm at different points of time.

The first vertical line indicates the date of the pay change announcement, while the second line indicates the date at which the new pay system was introduced. Pre-treatment trends appear parallel in the control and the treatment groups for revenues (for attrition, however, they are identical by definition, because we only look at employees entering before and exiting after the pay change).[7] The revenue trends are diverging after the treatment, they remain parallel because of seasonal effects, but revenues are markedly higher in the control group (while the opposite is true before the treatment). Figure 3 (b) complements this picture by showing that attrition was higher in the treatment than in the control group. When almost all employees of the two groups had left the firm or were promoted to managers, differences in revenues seem to disappear. Looking at a Figure 4, a similar graph that depicts the number of deals rather than revenue, conveys the same impression.

Figure 4: Deals



Notes: Lines show 5-month moving averages of weekly generated deals (without contract extensions) per month. Further explanations in the note to Figure 3.

Table 4 presents the results of the difference-in-difference regression (specification 1) using data for a total of 282 employees who either entered in a time window of ten months before the treatment or 10 months after the treatment. We are using these windows to have balanced pre- and post- treatment periods (in Table 6, we provide regression results for the entire data set). We present the results for both revenue and deals (placements). Column 1

[7] Note, however, that in Table 3, the exit probability, i.e. attrition of employees in both groups is also identical when all employees are considered, including the ones leaving before the pay change.

shows a strong effect of the after pay cut*treatment group interaction effect of an average of 3600 € (the pre-treatment monthly average revenue is around 9,000 €), column 3 presents the same regression for number of deals. Column 2 (5, for deals) shows the results when tenure and tenure squared as control variables are introduced; while these variables are important in explaining revenues (the R-squared more than doubles), the point estimate of the after pay cut* treatment group variable stays at the same level. Adding separate trends for treatment and control groups also does not change the main results (not reported here, available on request). Because control and treatment groups are not fully balanced between sectors of activities, we also control for year*quarter*sector fixed effects (in column 3 and 6) that absorb the time dependent influences of different markets. The point estimate is somewhat reduced. The second set of regressions uses deals as left-hand side variables and produces similar results.

Table 4: Difference-in-Difference Estimates of Employee Output

	Revenue/1000 per month per employee			Deals per month per employee		
	(1)	(2)	(3)	(4)	(5)	(6)
After pay cut indicator	1.88	2.11*	2.36	0.16	0.17	0.22
	(1.29)	(1.26)	(3.00)	(0.13)	(0.12)	(0.38)
Treatment group indicator	1.96*	0.90	0.69	0.14	0.04	0.03
	(1.15)	(1.08)	(1.14)	(0.11)	(0.11)	(0.12)
After pay cut *treatment group	-3.58***	-3.12***	-2.59**	-0.35***	-0.31***	-0.26**
	(1.24)	(1.18)	(1.29)	(0.12)	(0.11)	(0.13)
Tenure (months)		1.01***	1.00***		0.07***	0.07***
		(0.10)	(0.10)		(0.01)	(0.01)
Tenure^2 (months)		-0.02***	-0.02***		-0.00***	-0.00***
		(0.00)	(0.00)		(0.00)	(0.00)
City, sector and demographic controls	YES	YES	YES	YES	YES	YES
Year*quarter fixed effects	YES	YES	NO	YES	YES	NO
Year*quarter*sector fixed effects	NO	NO	YES	NO	NO	YES
N	3019	3019	3019	3019	3019	3019
R-sq	0.03	0.10	0.11	0.11	0.19	0.20

Notes: This sample includes 282 employees (Junior Consultants, Consultants, Senior Consultants) ten months before and ten months after the implementation of the compensation change. All regressions include controls for month lengths, training days, and if the employee deals with freelancers or permanent placements. City refers to the location of the office a consultant is located, sector to the main sector of activity. Dependent variables are revenue/1000 and placements per month per employee (contract extensions for freelancers included). Standard errors in parentheses clustered on individual level. * p<0.10, ** p<0.05, *** p<0.01

The preceding regressions were based on data for all employees. Table 5 presents fixed-effect regressions on those employees that experienced the pay cut. Because consultants do not rotate over different offices or sectors of activities, using person-fixed effects precludes the use

of city and sector fixed effects in Table 4. Because we find results in the same order of magnitude for both revenue and deals in both regressions, sorting effects of the different bonus slopes do not seem to be important. There are two, potentially related, reasons for this observation: first, most recruits have no or very little experience in the job. Second, in another research project with the same firm, we found that, fresh recruits from another cohort entering the firm have very high levels of overconfidence. The average recruit believes that 85% of the recruits in the same cohort will perform less well than themselves. High confidence levels are negatively correlated with performance, indicating that people have little information about their productivity in the firm.

Table 5: Difference-in-Difference Estimates of Output Including Employees Who Entered Before October 2009

	Revenue/1000 per month per employee		Deals per month per employee	
	(1)	(2)	(3)	(4)
After pay cut indicator	2.76**	1.08	0.23*	0.00
	(1.34)	(1.43)	(0.13)	(0.12)
After pay cut *treatment group	-3.69***	-3.59***	-0.30**	-0.29**
	(1.36)	(1.36)	(0.13)	(0.13)
Tenure (months)		2.42**		0.30***
		(1.04)		(0.11)
Tenure^2 (months)		-0.01***		-0.00
		(0.00)		(0.00)
Individual fixed effects	YES	YES	YES	YES
Year*quarter fixed effects	YES	YES	YES	YES
N	2166	2166	2166	2166
R-sq	0.02	0.03	0.03	0.04

Notes: This sample includes 128 employees (Junior Consultants, Consultants, Senior Consultants) who entered the firm before October 2009 ten months before and ten months after the implementation of the compensation change. All regressions include controls for month lengths and training days. Dependent variables are revenue/1000 and placements per month per employee (contract extensions for freelancers included). Standard errors in parentheses clustered on individual level. * p<0.10, ** p<0.05, *** p<0.01

We now focus on the question of long-term consequences of the bonus cut. Table 6 presents the results of regressions in which we interact the treatment with three dummies representing different time spans of one year each after the treatment. Controlling for a battery of fixed effects, we find that for revenue the treatment effects is negative over two years, while, seemingly, the treatment effect disappears in the third year. However, when controlling (in columns 2 and 4) for consultants' entering the firm after the treatment and adding an interaction term for those of these new entrants who worked in the previously treated divisions, we find that revenues for the remaining consultants that experienced the treatment are lower than for the others (while there is no significant effect on number of deals). We interpret this as evidence for the effect at least being persistent over two years after the treatment, and would carefully argue that there is, even after three years, a certain negative effect. One should notice, however,

there are only a small number of individuals who experienced the treatment and are still working as a consultant in the firm (as depicted in Figure 3).

Table 6: Difference-in-Difference Estimates of Eemployee Output; Development over Three Years of Post-treatment Period

	Revenue/1000 per month per employee		Deals per month per employee	
	(1)	(2)	(3)	(4)
Treatment group indicator	0.38	0.35	0.02	0.01
	(1.17)	(1.17)	(0.12)	(0.12)
Oct. 2009-Sept. 2010* treatment group	-2.92**	-3.72**	-0.29**	-0.30**
	(1.35)	(1.49)	(0.13)	(0.15)
Oct. 2010-Sept. 2011* treatment group	-2.88*	-4.84**	-0.17	-0.20
	(1.72)	(2.15)	(0.16)	(0.23)
Oct. 2011-Sept. 2012* treatment group	-1.49	-4.09*	-0.01	-0.04
	(1.61)	(2.16)	(0.14)	(0.22)
Tenure (months)	1.07***	1.09***	0.08***	0.09***
	(0.08)	(0.08)	(0.01)	(0.01)
Tenure^2 (months)	-0.02***	-0.02***	-0.00***	-0.00***
	(0.00)	(0.00)	(0.00)	(0.00)
Entered after Oct 2009 indicator		-1.11		0.07
		(1.54)		(0.13)
Entered after Oct 2009*treatment group		2.85*		0.04
		(1.68)		0.01
City, sector and demographic controls	YES	YES	YES	YES
Year*quarter*sector fixed effects	YES	YES	YES	YES
N	8923	8923	8923	8923
R-sq	0.10	0.10	0.19	0.19

Notes: This sample includes 569 employees (Junior Consultants, Consultants, Senior Consultants) ten months before and 36 months after the implementation of the compensation change. All regressions include controls for month lengths, training days, and if the employee deals with freelancers or permanent placements. City refers to the location of the office a consultant is located, sector to the main sector of activity. Dependent variables are revenue/1000 and placements per month per employee (contract extensions for freelancers included). Standard errors in parentheses clustered on individual level. * p<0.10, ** p<0.05, *** p<0.01

How is separation affected by the treatment? Table 7 summarizes the results of a duration analysis in which the separation rate is the dependent variable. The interaction term shows that the treated consultants are more likely to leave.

Table 7: Cox Proportional Hazard Model: Employee Separation Rates

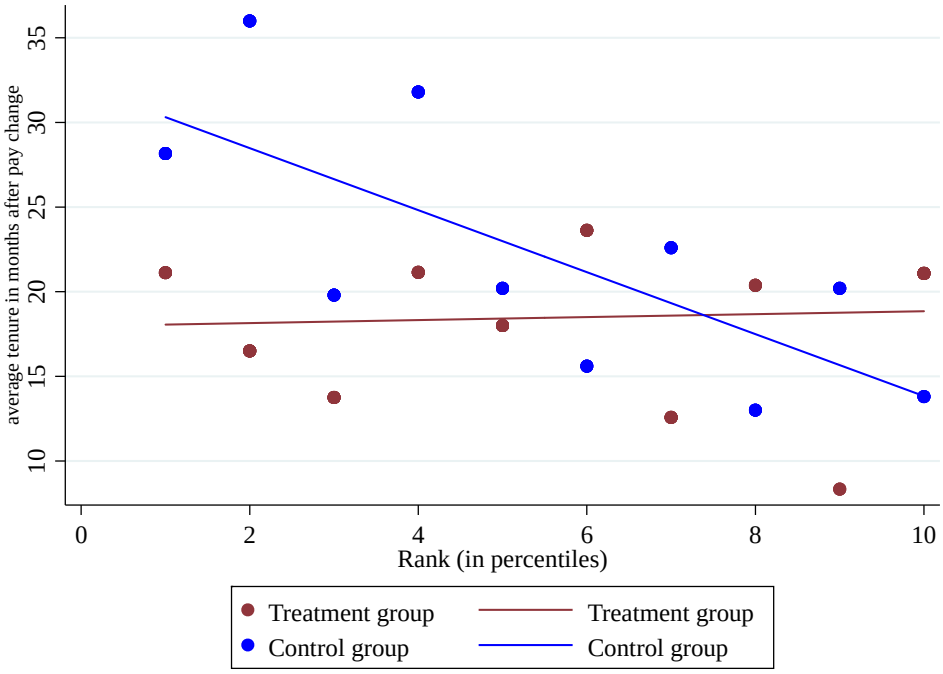
	(1)	(2)
After pay cut indicator	0.69**	0.81**
	(0.34)	(0.36)
Treatment group indicator	-0.38	-0.16
	(0.36)	(0.36)
After pay cut *treatment group	0.77*	0.70*
	(0.42)	(0.42)
City, sector and demographic controls	NO	YES
N	295	292

Notes: This sample includes 166 employees (Junior Consultants, Consultants, Senior Consultants). City refers to the location of the office a consultant is located, sector to the main sector of activity. Observations are split into before and after pay cut time intervals. The results are reported as coefficients rather than hazard rates. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01

An interesting question is what type of consultants is driving this result. Figure 5 plots separately, for treatment and control group, the tenure profile by sales rank of consultants. The first percentile represents the consultants with the highest performance. While in the control

group tenure increases in performance, this is not the case in the treatment group. For illustrative purposes, the graph also provides a simple linear fit of the observations in the two groups.

Figure 5: Average Tenure by Revenue Rank



Notes: Average tenure after the pay change refers to the time period in months from the pay change in October 2009 until the exit period or the last observed period of the dataset (September 2012). The rank (in percentiles) refers to the generated revenue/1000 (contract extensions for freelancers included) within treatment or control group before the pay change. The first percentile represents the consultants with the highest performance.

6. Effect on consultants' activities

It is a particular advantage of our data that the MIS records the most important activities of the consultants. These are job vacancies found, meetings held, and candidates found. The system also recorded consultants' days of absence. Table 8 collects the results of a fixed effect regression on these four direct measures of consultants' behavior. The picture seems to be very much consistent with the regressions in section 5: treated consultants have 1.4 less vacancies found (the average in the pre-treatment period being 4.3), they have 0.74 less meetings with clients (average pre-treatment 2.5), they find 2 candidates less (average pre-treatment 7.3) and they roughly take 0.8 more days of absence (average pre-treatment 2.5). Notice that these effects are around 1/3 for each of the activities, which is commensurate to the effects on revenues and deals. We have run the same regressions on a 36-month window around the event, and find quite similar results (Table A1 in the Appendix).

Table 8: Difference-in-Difference Estimates of Employee Activities Including Employees Who Entered Before October 2009; -10 to +10 Months Event-Time Window

	Vacancies found		Meetings scheduled		Candidates found		Absenteeism (days)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After pay cut indicator	0.76*	0.52	0.28	0.36	0.40	0.60	0.14	-0.23
	(0.42)	(0.45)	(0.35)	(0.38)	(0.59)	(0.61)	(0.45)	(0.45)
After pay cut *treatment group	-1.37***	-1.36***	-0.74**	-0.72**	-1.97***	-1.97***	0.77**	0.80**
	(0.47)	(0.47)	(0.30)	(0.30)	(0.63)	(0.64)	(0.37)	(0.36)
Tenure (months)		0.32**		-0.05		-0.25		0.53**
		(0.15)		(0.16)		(0.19)		(0.23)
Tenure^2 (months)		-0.00		-0.00**		-0.00		-0.00***
		(0.00)		(0.00)		(0.00)		(0.00)
Individual fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Year*quarter fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
N	2166	2166	2166	2166	2166	2166	2166	2166
R-sq	0.07	0.07	0.06	0.07	0.11	0.11	0.03	0.04

Notes: This sample includes 128 employees (Junior Consultants, Consultants, Senior Consultants) who entered the firm before October 2009 ten months before and ten months after the implementation of the compensation change. All regressions include controls for month lengths, training days,. Dependent variables are meetings scheduled, vacancies and candidates found and absenteeism (days) per month per employee. Standard errors in parentheses clustered on individual level. * p<0.10, ** p<0.05, *** p<0.01

7. Robustness

In Table A2 in the Appendix we collect numerous robustness checks on the regressions presented so far. In column (1 and 5) we collapse each consultant's data into one pre- and one post-treatment observation, to deal with serial correlation concerns, and as recommended by Bertrand et al. (2004). In column (2 and 6) we exclude the period between announcement and implementation of the pay change to deal with the possibility that consultants may have reacted even before the pay change affected their wage. Column (3 and 7) is particularly interesting to appreciate the validity of the data on activities. Excluding the observations of consultants leaving in the first 10 months after the pay change (i.e. until 09/2010) we find little changes in the effect of the treatment on performance. This indicates that the effort response documented is not simply owing to the fact that consultants who anticipate leaving soon reduce their activities. Column (4 and 8) has observations for consultants active in the IT sector only, one of the biggest sectors of activity in the firm; comparing only those consultants deals with potential heterogeneity across sectors.

8. Mechanisms

A possible interpretation of our results goes as follows. When the firm decides to reduce the bonus rate, consultants *rationally* adjust their behavior by reducing their effort, which translates into lower output. The magnitudes we discussed in the preceding sections do not seem to be consistent with such an interpretation, but the data allow to do some additional analyses. We

carry out a difference-in-difference analysis on entrants in treatment and control divisions before the pay change, in which the treatment group had higher incentives, compared to entrants after the pay change, when all consultants worked with the same compensation scheme. Crucially, for this analysis we exclude observations for the period after the pay change of those consultants who experienced the pay change.

Figure 6: Data Structure

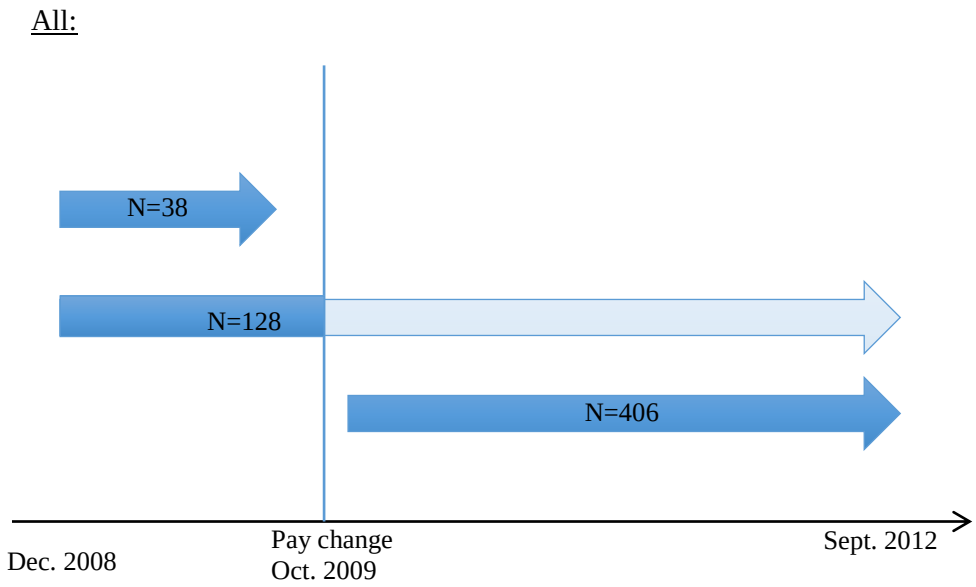


Figure 6 illustrates which data from sub-groups we use: The first group (in the upper left corner, N = 38) consists of consultants who entered and left the firm before the pay change. The second group consists of consultants who entered the firm before the pay change and left it afterwards (lower left corner, N = 128). Out of this group, 77 were affected by the pay change. We only use the data of these 128 consultants up to the moment the pay change was introduced (hence we exclude the lighter-colored arrow). The third group consists of consultants who entered the firm only after the pay change, all of whom work under the same compensation contract. For this third group, we only consider those entering after the pay change.

If the reduction of the bonus per se had a substantial effect in explaining our main results, one should expect a strong negative coefficient of being in the treatment group in a difference-in-difference analysis of the type carried out before on the full data set. However, Table 9 shows that there is no such effect. In particular, when controlling for sectors interacted with time, it is reduced remarkably and far from any conventional level of statistical significance. Note that in Section 5, when comparing fixed effect regressions with OLS, we did not find evidence for sorting. We cannot entirely exclude sorting effects, but see these results

as support to the interpretation of our main regression results that there is more going on than a simple reaction of effort supply to a changed incentive slope.

Table 9: Difference-in-Difference Estimates: Effect of Pay Change on New Entrants

	Revenue/1000 per month per employee		Deals per month per employee	
	(1)	(2)	(6)	(7)
After pay cut indicator	1.80	1.88	-0.09	-0.29
	(1.61)	(2.94)	(0.15)	(0.31)
Treatment group indicator	0.92	0.39	0.10	0.02
	(1.03)	(1.15)	(0.11)	(0.12)
After pay cut *treatment group	-2.04	-1.47	-0.17	-0.07
	(1.25)	(1.45)	(0.12)	(0.13)
Tenure (months)	1.57***	1.55***	0.12***	0.12***
	(0.10)	(0.10)	(0.01)	(0.01)
Tenure^2 (months)	-0.04***	-0.04***	-0.00***	-0.00***
	(0.00)	(0.00)	(0.00)	(0.00)
City, sector and demographic controls	YES	YES	YES	YES
Year*quarter fixed effects	YES	NO	YES	NO
Year*quarter*sector fixed effects	NO	YES	NO	YES
N	7057	7057	7057	7057
R-sq	0.11	0.11	0.17	0.18

Notes: This sample includes 567 employees (Junior Consultants, Consultants, Senior Consultants) ten months before (December 2008 to September 2009) and ten months after the implementation of the compensation change (December 2011 to September 2012). All regressions include controls for month lenghts, training days, and if the employee deals with freelancers or permanent placements. City refers to the location of the office a consultant is located, sector to the main sector of activity. Dependent variables are revenue/1000 and placements per month per employee (contract extensions for freelancers included). Standard errors in parentheses clustered on individual level. * p<0.10, ** p<0.05, *** p<0.01

According to the fair wage hypothesis (Akerlof & Yellen, 1990) workers who perceive wage cuts as unfair react by withholding effort and leaving with a higher probability. What we find is in line with this, and it is also in line with the view by Hart and Moore (2008) and Halonen-Akatwijuka and Hart (2013) that contracts act as reference points, deviations from which are penalized by shading. Our results are supportive for the view brought forward by a large literature that shows in laboratory settings that negative reciprocity can have strong effects, but our setting offers the unique opportunity to show that this holds in the field over a long period of time.

We cannot exclude either that the reactions we document constitute a rational penalty in the framework of a relational contract between one worker and the firm (cite). In particular, higher attrition of the concerned workers is compatible with a grim-trigger strategy, in which the relationship is dissolved after the deviation of the principal. However, we do not find any evidence for multi-lateral relational contracting (reference). Here, deviations of the firm from paying a promised bonus would not only be penalized by the affected worker, but also by his or her colleagues who have not been negatively affected, or by workers entering the firm afterwards who would provide lower effort because the firm has lost reputation of maintaining bonus payments in the future. Indeed Table 5 shows that new entrants into the treated divisions,

if anything are more productive than new entrants in the non-treated divisions showing that there is no collective penalty from the treated divisions; neither is there a cultural spillover. Table 9 is also in support of this interpretation: new entrants who came in after the pay change have no differential performance between treated and non-treated divisions.

9. Evaluating the Pay Change

The effects of the pay change are large, personnel turnover increased in the treated group and productivity dropped in the realm of one third of the otherwise-to-be-expected sales in the treatment divisions. However, a priori, we cannot exclude that the pay change was a profitable project for the firm, given the long-term perspective of owners. Not only were the costs of the pay change drastic, but quite short-lived because they disappeared with the attrition of the consultants affected. A back-of-the-envelope calculation reveals (details in the Appendix) INSERT COMPUTATIONS that the pay cut's positive wage effects (for the firm) would outweigh the negative revenue effects after, roughly, ten years. In that regard it is informative that the firm's ownership structure has not substantially changed since its inception.

Crucial for this calculation and somehow surprising is that the firm was able to recruit productive workers in the aftermath. The new consultants entering after the pay change were "cheaper", but they seem equally good as those who they replaced. This may not have come as a surprise for the company's management because in the pre-treatment period the performance of consultants was quite similar in the two groups despite the difference in bonuses.

It seems puzzling that we find no evidence for negative sorting after the pay change, but most rookies know little about their suitability for the job. In another, more recent study with the firm (Friebel et al, 2019), we found very high levels of overconfidence of fresh recruits REFERENCE. Upon entry, many consultants take a rather long period before concluding their first placement, and many leave even before such a placement happens. There hence seems to be a good amount of potential employees' uncertainty about their match with the job. Additionally, even after the pay change, the firm still offered quite competitive packages compared to the rest of the market.

Concluding, we would like to make clear what we see as the main contribution of the paper. One should not take for granted that firms shy away from cutting wages because they are afraid of worker reactions. The picture we have tried to convey is quite nuanced: the work culture in our firm is highly individualistic, with a high personnel turnover and in which only the affected workers react. It is hence likely that what we found is a lower bound of the likely effects that insider econometricians could find if similar data sets as the one we used were available. A particular challenge for other studies, and forces that would increase the costs of a

pay change beyond what we find would be to price two effects we could not pick up with our data: the potential loss of managerial human capital and firm reputation.

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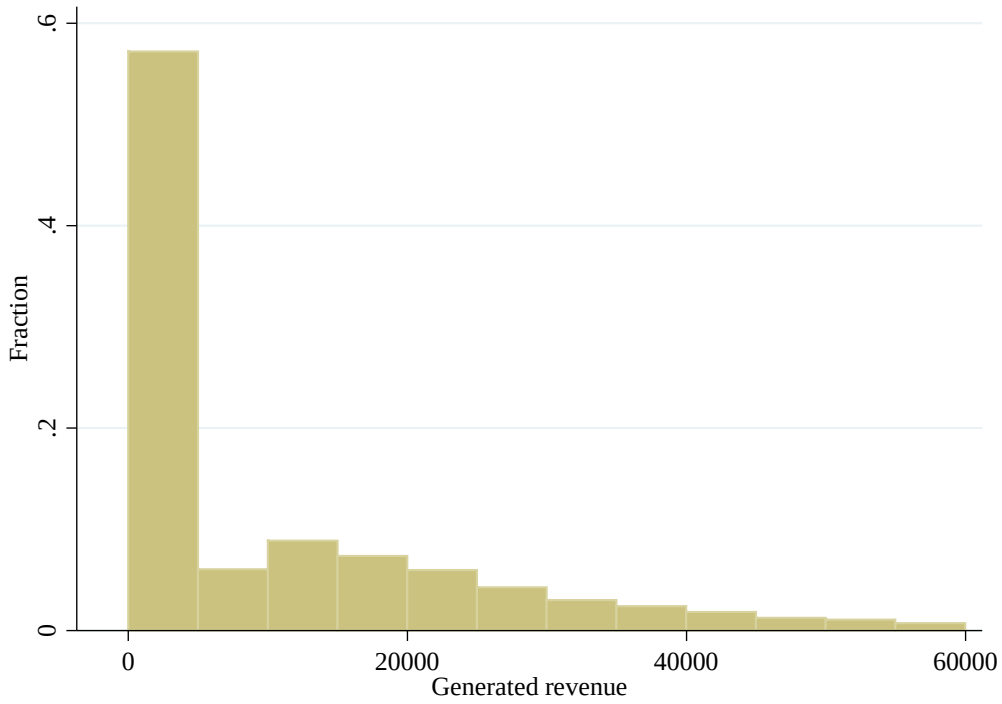
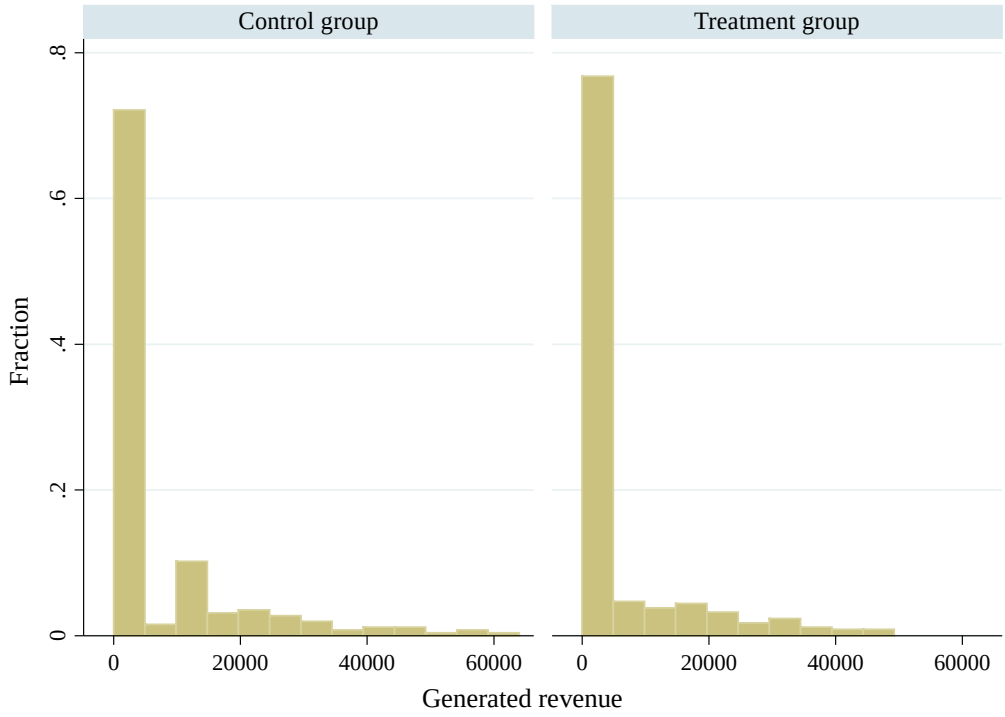
Appendix

Figure A1: Distribution of Revenue



Notes: The distribution of revenue refers to the entire observed time period in months (December 2008 to September 2012).

Figure A2: Distribution of Revenue, New Entrants by Group



Notes: The distribution of revenue refers to the first year after the pay change and employees who entered the firm after the pay change.

Table A1: Difference-in-Difference Estimates of Employee Activities Including Employees Who Entered Before October 2009; -10 to +36 Months Event-Time Window

	Vacancies found		Meetings scheduled		Candidates found		Absenteeism (days)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After pay cut indicator	0.74*	0.54	0.54	0.58	0.97	1.24*	-0.54	-1.03*
	(0.41)	(0.43)	(0.34)	(0.37)	(0.62)	(0.64)	(0.51)	(0.53)
After pay cut *treatment group	-1.22**	-1.25***	-1.10***	-1.12***	-2.66***	-2.64***	1.53***	1.49***
	(0.47)	(0.47)	(0.29)	(0.29)	(0.68)	(0.67)	(0.40)	(0.38)
Tenure (months)		0.32**		-0.01		-0.39**		0.74***
		(0.14)		(0.16)		(0.16)		(0.28)
Tenure^2 (months)		-0.00		-0.00		0.00		-0.00
		(0.00)		(0.00)		(0.00)		(0.00)
Individual fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Year*quarter fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
N	2908	2908	2908	2908	2908	2908	2908	2908
R-sq	0.11	0.11	0.08	0.08	0.15	0.15	0.04	0.04

Notes: This sample includes 128 employees (Junior Consultants, Consultants, Senior Consultants) who entered the firm before October 2009 ten months before and ten months after the implementation of the compensation change. All regressions include controls for month lengths, training days. Dependent variables are meetings scheduled, vacancies and candidates found and absenteeism (days) per month per employee. Standard errors in parentheses clustered on individual level. * p<0.10, ** p<0.05, *** p<0.01

Table A2: Robustness of Event Study Estimates of Employee Output; -10 to +10 months Event-Time Window

	Revenue/1000 per month per employee				Placements per month per employee			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After pay cut indicator	-2.63	-3.43**	2.03	0.20	0.04	-0.05	0.06	-0.10
	(3.57)	(1.65)	(1.69)	(1.88)	(0.37)	(0.15)	(0.15)	(0.16)
After pay cut *treatment group	-3.30**	-2.92*	-3.95**	-3.78**	-0.26*	-0.29*	-0.32**	-0.44**
	(1.34)	(1.57)	(1.54)	(1.67)	(0.13)	(0.15)	(0.15)	(0.18)
Tenure (months)	1.10**	1.06***	2.59**	3.46**	0.04	0.07***	0.32**	0.52***
	(0.44)	(0.24)	(1.27)	(1.52)	(0.05)	(0.02)	(0.13)	(0.17)
Tenure^2 (months)	-0.01**	-0.01***	-0.01***	-0.01*	-0.00	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Collapse pre- and post-period	YES	NO	NO	NO	YES	NO	NO	NO
Exclude 092009/102009	NO	YES	NO	NO	NO	YES	NO	NO
Exclude employees left before 092010	NO	NO	YES	NO	NO	NO	YES	NO
IT	NO	NO	NO	YES	NO	NO	NO	YES
Individual fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Year fixed effects	NO	YES	NO	NO	NO	YES	NO	NO
Year*quarter fixed effects	NO	NO	YES	YES	NO	NO	YES	YES
Number of employees in sample	128	128	92	69	128	128	92	69
N	256	1914	1663	1187	256	1914	1663	1187
R-sq	0.17	0.03	0.04	0.03	0.13	0.04	0.05	0.05

Notes: This sample includes employees (Junior Consultants, Consultants, Senior Consultants) who entered the firm before October 2009 ten months before and ten months after the implementation of the compensation change. All regressions include controls for month lengths and training days. Dependent variables are revenue/1000 and placements per month per employee (contract extensions for freelancers included). Standard errors in parentheses clustered on individual level. * p<0.10, ** p<0.05, *** p<0.01