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Effort and Selection Effects of Performance Pay in Knowledge Creation

Erina Ytsma
MIT
Sloan - IDE
eytsma@mit.edu

Abstract

This paper studies the effect of performance pay on knowledge creation through effort and selection effects, using the introduction of performance pay in German academia as a natural experiment. I estimate the pure effort effect in a diff-in-diff framework comparing the productivity of cohorts that started their first tenured position just before the reform, and consequently do not receive performance pay, with those starting their first tenured position after the reform, and therefore do receive performance pay. I find a significant and economically large effort effect, amounting to a 35% increase in academic productivity relative to the pre-reform productivity in the control group. I estimate the selection effect by analysing the rate at which academics of different productivity levels switch to the performance pay scheme and find that more productive academics are more likely to select into performance pay.

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JEL classification: J33, M52, O31

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1   Introduction

Universities constitute an important economic sector, both in terms of direct value and exports (McCormack et al. 2014), and regarding basic and applied research in an economy (Lach and Schankerman 2008). In turn, investments in research and education at universities have been shown to come with considerable localised spillovers (Kantor and Whalley 2014, Agrawal 2001) and act as a catalyst for innovation at a national level (Aghion et al. 2010, Acemoglu et al. 2006). It is therefore important to understand factors determining the performance of universities; from general management practices (McCormack et al. 2014) to specific incentives like inventor royalty shares (Lach and Schankerman 2008, 2004) and external factors such as a university's autonomy and the amount of competition it faces (Aghion et al. 2010). This paper aims to add to this literature by zooming in to the level of individual scientists and studying the effort and selection effect of performance pay in academia. I use the introduction of performance pay in German academia as a natural experiment, and find that performance pay increases effort by 35% and that more productive academics self-select into the pay scheme.

The academic pay reform that I exploit as a natural experiment was passed by German parliament in 2002 and introduced a new professorial salary scheme that comprises a basic wage plus performance related bonuses. These bonuses are distributed partly through on-the-job tournaments, partly as, effectively, piece-rate pay. The new performance related pay scheme replaced the old pay scheme in which professorial salaries increased with age and wages were thus effectively flat (Handel 2005). German states had until 1 January 2005 to implement the reform within their respective jurisdiction (Detmer and Preissler 2005). Any appointment after the implementation of the reform necessarily falls under the new performance pay scheme. Furthermore, any renegotiation of an existing contract after the reform moves a professor from the old, age-related system to the new, performance pay scheme (Detmer and Preissler 2004).

In order to estimate the effort effect of performance pay in academia, I use a new data set comprising affiliations, productivity measures and related information of the universe of academics in Germany and I exploit the fact that academics who make tenure just before the reform fall under the age-related pay scheme, while those who make tenure just after the reform are paid according to the performance pay scheme. If the timing of the tenure decision is exogenous, any difference in productivity from before to after the reform between academics who make tenure just before the reform and those who tenure directly after the reform can be interpreted as the causal effect of performance pay on effort. I estimate this effort effect in a difference-in-differences framework, with academics who make tenure directly after the reform constituting the treatment group and academics who make tenure just before comprising the control group. I find that performance pay increases effort by 35%. Roughly two-thirds (23%) of this effort effect can be ascribed to the tournament component of the performance pay scheme, while the remaining third (12%) is caused by the piece-rate component. Given that the pay reform was mandated to be budget-neutral, the increase in research productivity due to this effort effect came at no extra cost, and is therefore a pure research productivity gain. I provide evidence of the validity of the identifying parallel trends assumption and exogeneity of the timing of the tenure decision through tests of pre-existing trends and placebo difference-in-differences.

The second part of the paper is devoted to estimation of the selection effect of performance pay in academia. Academics who hold a tenured position when the reform is implemented switch from the age-related pay system to the performance pay system when they change their affiliation or position. Because pay no longer increases with age in the performance pay system but only with productivity, I expect more productive professors to be more likely to select into the performance pay scheme by changing position or affiliation. Hazard rate and survival function analyses confirm that more productive academics are indeed

more likely to switch to the performance pay scheme, so the selection effect goes in the same direction as the effort effect to increase research output.

As mentioned above, this paper aims to contribute to the literature on university governance (cf. Haeck and Verboven (2012), Aghion et al. (2010), Belenzon and Schankerman (2009), Lach and Schankerman (2008, 2004)) in particular, and to the literature on the organisation of knowledge creation (Grigoriou and Rothaermel 2014, Phelps et al. 2012, Jones 2009, Wuchty et al. 2007, Singh 2005, Audretsch and Feldman 2004, 1996, Jaffe et al. 1993)) more generally. By studying the effort and selection effects of performance pay in academia, this paper studies one of the key components of human resource management, incentive systems, (cf. Lazear and Oyer (2012), Bloom and Reenen (2011), Oyer and Schaefer (2011), Lazear and Shaw (2007)) in relation to university governance, thus building on and adding to the vast literature on incentives in organizations as well as that on university management and the organisation of knowledge creation.

Within the body of literature on incentives, numerous papers have examined how incentives affect worker productivity empirically, and many of those report significant positive effort effects of higher powered incentive schemes, both in the field (e.g. Muralidharan and Sundararaman (2011), Shi (2010), Lavy (2009), Bandiera et al. (2005), Shearer (2004), Lazear (2000)) and in the lab (i.a. Hossain and List (2012), Boly (2011), Dohmen and Falk (2011), Carpenter et al. (2010), Freeman and Gelber (2010), Bellemare et al. (2010), Ariely et al. (2009), Dickinson and Villeval (2008), Dickinson (1999)). The kinds of higher-powered incentive schemes that have been shown to have positive effort effects are several; from piece-rate pay (cf. Dohmen and Falk (2011), Shi (2010), Bellemare et al. (2010), Ariely et al. (2009), Bandiera et al. (2005), Shearer (2004), Lazear (2000), Dickinson (1999)) or bonus pay (i.a. Hossain and List (2012), Muralidharan and Sundararaman (2011), Lavy (2009)) to tournament schemes (cf. Carpenter et al. (2010), Freeman and Gelber (2010), Harbring et al. (2004)) and monitoring regimes (e.g. Boly (2011), Dickinson and Villeval (2008)). It is not a given that performance pay schemes would increase academic effort too, since academics are thought to be intrinsically motivated (McCormack et al. 2014), and extrinsic incentives might crowd out this intrinsic motivation (Dickinson and Villeval 2008, Besley and Ghatak 2005). The empirical results in this paper however provide evidence of a positive effort effect of both a piece-rate component and a tournament component of performance pay in academia.

Though some papers have studied the effort effect of performance pay in education, either by estimating the effect of teacher incentives (cf. Muralidharan and Sundararaman (2011), Glewwe et al. (2010)) or student incentives (i.a. Bettinger (2012), Leuven et al. (2011), Angrist and Lavy (2009), Angrist et al. (2009)), this paper is, to the best of my knowledge, one of only few to study the effort effect of performance pay on academics, particularly with respect to research productivity. Lach and Schankerman (2008, 2004) also study the effect of incentives on research productivity, but they measure research productivity at the university instead of individual academic level, and therefore cannot estimate the effort effect directly.

Within the body of literature on incentive schemes, a number of papers study sorting into pay schemes. Dohmen and Falk (2011) and Lazear (2000) for instance study the selection effect of piece-rate schemes for workers in a lab experiment and windshield installers in the field, respectively, and find that higher productivity workers self-select into the higher-powered pay scheme. Leuven et al. (2011) report a similar finding for selection in tournament schemes for students. This paper provides evidence that performance pay also has a positive and significant selection effect on academics. The selection effect thus reinforces the effort effect, both contributing to a greater productivity under performance pay.

The paper is structured as follows: the next section provides an institutional background regarding the academic pay reform and the academic sector in Germany. The empirical analysis makes up section 3, with

the first part focusing on the effort effect and the second part on the selection effect of performance pay. Section 4 concludes.

2   Institutional Background

The German academic pay reform that I exploit as a natural experiment in this paper introduced a new pay scheme ("W-pay") under which professors can earn performance related bonuses on top of a basic wage (BMBF 2002). Before the reform, professors were paid according to an age-related pay scheme ("C-pay") in which pay increased every two years until the age of 49 (Hochschullehrerbund 2009, Oeffentlicher-Dienst 2004, Expertenkommission 2000). For all but the youngest professors, the basic wage of the performance pay scheme is lower than the age-related wages (Hochschullehrerbund 2009, Oeffentlicher-Dienst 2004), but the total pay under the performance pay scheme can exceed that under the age-related pay scheme if an academic is paid large performance bonuses (Detmer and Preissler 2006).

The federal law introducing the new professorial pay scheme was passed by Germany's parliament in February 2002. The law required all states to implement the reform within their respective jurisdiction latest 1 January 2005 and only three states (Bremen, Niedersachsen and Rheinland-Pfalz) did so before the end of 2004 (Detmer and Preissler 2005). Consequently any new or renegotiated professorial contract entered into as of 1 January 2005 falls under the performance pay scheme. Importantly, this means that, once a professor switches to performance pay by renegotiating his current contract or changing position or affiliation after 2004, he can never go back to age-related pay (Detmer and Preissler 2004).

The academic pay reform was mandated to be cost-neutral. In particular, the average professorial pay at the federal and state level was to remain at the respective pre-reform levels (benchmark year 2001) (BMBF 2002)[1]. The law does allow for the benchmark professorial pay average to be exceeded by, on average, 2% per year, though not exceeding 10% in total[2] (BMBF 2002). The budget-neutrality stipulation was explicitly introduced to prevent the reform leading to cost-cutting or a cost explosion (Detmer and Preissler 2006, Handel 2005). Because the basic wage in the performance pay system is lower than most of the age-related wages, the cost neutrality requirement guarantees that whatever is saved on basic wage payments, is paid as bonuses in the performance pay scheme scheme. Handel (2005) calculates that, with a pre-reform professorial pay average of 71.000 euro at universities, about 26% of total professorial pay for university professors is available for performance pay bonuses[3]. In many states, the state's ministry of education implements the cost-neutrality requirement by calculating university-specific professorial pay averages that are used as benchmark professorial pay average for the respective university (Handel 2005).

The pay reform only applies to public institutions of higher education. In this paper, I restrict attention to public universities only, of which there are 89 (*Hochschulkompass* 2014)[4]. I focus on universities because I will use research output as a measure of productivity and other higher education institutions, such as universities of applied sciences ("Fachhochschulen"), are more applied and faculty therefore publish much less BMBF (2002).

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[1] The law does allow states to raise their target average professorial pay level to - at most - the highest average professorial pay at the state or federal level.

[2] As long as the state's budget allows.

[3] For this calculation, Handel (2005) uses 2001/2002 data and assumes that the ratio of W2 to W3 professors at universities will be about the same as that of C3 to C4, namely 46:54.

[4] Number of public universities reported by *Hochschulkompass* (2014) on 31 August 2014. The set of public universities that I consider for the empirical part of the paper is slightly different, because some institutions became universities only recently (the Hochschule Geisenheim University became a university on 1 January 2013 and the Hochschule für Film und Fernsehen Potsdam only in July 2014 for instance (*Historie 1872 bis heute* 2014, *Filmuniversitaet* 2014)) and are therefore not included in this set.

2.1   Performance Pay (W-Pay)

There are three basic pay levels in the performance pay scheme: W1 pays a basic monthly wage of 3405.34 euro, W2 3890.03 euro and W3 4723.60[5] (Detmer and Preissler 2006, Oeffentlicher-Dienst 2004). As mentioned above, professors can earn performance bonuses on top of these basic wages in the W-pay scheme (BMBF 2002). It is important to note that universities themselves decide how to award these performance bonuses [6], though the federal law governing the pay reform provides ground rules. Performance bonuses can be awarded on the basis of three grounds: as attraction or retention bonus, for on-the-job performance, and for taking on management roles or tasks (BMBF 2002).

The first kind of bonus, the attraction or retention bonus, is determined as part of contract (re)negotiations and generally awarded on the basis of a professor's qualifications and past achievements and performance, taking into account applicant pool quality and labour market tightness (Detmer and Preissler 2005). Many states give universities the option to award attraction or retention bonuses on a permanent basis, for a fixed term (initially) or even as a one-off payment (Detmer and Preissler 2004, 2005). State laws and university statutes generally do not impose a maximum on the amount paid as attraction or retention bonus.

Secondly, the pay reform introduced bonuses for on-the-job performance. These bonuses can be awarded for performance in research, art, teaching, mentoring and supervision (BMBF 2002). To asses research performance for instance, universities take into account the number and kind of publications, research prizes, patents and external research grants, while for instance exceptional teaching evaluations can serve as evidence of special teaching achievements (Detmer and Preissler 2005). Most state laws stipulate that the performance for which on-the-job bonuses are awarded must have been effected over multiple years, and allow universities to award these bonuses either for a fixed term (with the option to renew) or permanently (Handel 2005).

Most universities pay on-the-job bonuses through what is essentially a promotion tournament (the so-called "Stufenmodell") (Harbring et al. 2004, Kräkel 2006, Lünstroth 2011). This system comprises a number of performance or hierarchy levels, each of which is associated with a bonus (Lünstroth 2011). Generally speaking, universities announce at the beginning of a year either both the number of levels and associated bonus pay or only the number of promotions to higher levels (if the associated bonus pay is specified in the university's statutes) to be awarded in that year (Lünstroth 2011). There is subtantial variation in the number of pay levels across universities; the number of levels ranges from 2 (e.g. Augsburg and Erfurt University) to 10 (University of Trier), and the associated pay from 90 (Technical University of Berlin) to 2500 euro per month (e.g. Bielefeld and Bremen University) (Lünstroth 2011).

Some universities pay on-the-job bonuses through a relative performance pay system which is essentially a Japanese style (J-type) tournament ("Leistungspunkte Modell") (Kräkel 2003, Lünstroth 2011). In this system, a university announces the total amount available for on-the-job performance bonuses in a given year and awards professors points for achievements in research and education. Each academic then receives a share of the total "prize pot" that is equal to his relative performance that year as performance bonus.

The third kind of bonus in the performance pay scheme comprises pay supplements for taking on management tasks or roles (BMBF 2002). These bonuses are paid for the duration of the task or role only and generally range somewhere between 200 and 600 euro (for the dean) per month .

Finally, the reform also introduced the option for professors to extract a pay supplement from third-party

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[5] These were the basic wage levels under the performance pay scheme in former West-German states as of 1 August 2004. The corresponding basic wage levels in former East-German states were 92.5% of the West-German rates (Detmer and Preissler 2006).

[6] See Handel (2005) for a comprehensive overview of how much discretion higher education institutes have regarding hiring and pay decision after the reform in the different German states.

awarded funds for research or teaching projects for the duration of such projects (BMBF 2002)[7]. Some states restrict the amount of these supplements to not exceed the basic wage of the professor (Detmer and Preissler 2005).

Only tenured professors can earn bonuses in the performance pay scheme - junior professors (the German equivalent of assistant professors) can only earn a (non-pensionable) supplement of 260 euro per month upon positive evaluation[8] (Detmer and Preissler 2005). For comparison, tenured professors can earn performance bonuses up to a total amount of 5241,48 euro per month [9] - more than the highest basic wage in the performance pay scheme - or more in special circumstances[10](BMBF 2002, Detmer and Preissler 2005). Consequently, I restrict attention to tenured professors when analyzing effort and selection effects of the pay reform in this paper.

There are two tenured professorial ranks in Germany; the equivalent of an associate professorship ("ausserordentliche (or a.o.) Professur"), and the equivalent of a full professorship ("ordentliche (o.) Professur") (*Research and Academic Jobs in Germany* 2011). Aspiring academics have to complete a PhD, as well as, traditionally, a post-doctoral qualification ("habilitation") in order to qualify for a professorship. During the habilitation academics work as part of the research group of a full professor and write a postdoctoral thesis Fitzenberger and Schulze (2014), Pritchard (2006). In 2002, together with the announcement of the pay reform, the German federal government introduced the equivalent of assistant professorships ("Juniorprofessur"), to supersede the habilitation[11](Pritchard 2006). Junior professors enjoy greater independence than academics completing a habilitation (Fitzenberger and Schulze 2014).

2.2 Comparison With Age-Related Pay (C-Pay)

There are four pay levels in the age-related pay scheme (C1-C4); university professors are generally awarded C3 or C4 pay. The monthly salary in these levels increases every two years by roughly 170 euro[12], from the age of 21 to the age of 49 (Hochschullehrerbund 2009, Oeffentlicher-Dienst 2004, Expertenkommission 2000). In contrast, the basic wage in the W-pay scheme does not vary with age, and the level is such that professors earn a higher before-bonus wage in the performance pay scheme at first, but once they get older, they would earn a higher basic wage in the C-pay system. Figure 1 depicts the monthly wage by age for the several pay levels in the performance pay and age-related pay schemes. As is clear from this figure, the crossing point of these basic wage schedules depends on the specific (offered) pay level of an academic[13]; the age-related wage starts to exceed the basic wage in the performance pay scheme either at age 33[14] or at age 43[15] (Oeffentlicher-Dienst 2004, Handel 2005).

Before the pay reform, professors in the highest pay level of the age-related pay scheme (C4) could earn bonuses when they received offers after their first appointment as C4-professor. These bonuses were standardised to be around 650 euro for the second C4-offer, and about 730 euro for the third C4-offer from

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[7] These supplements were intended to motivate professors to take on activities as part of their academic job that they may have otherwise performed on the side (Handel 2005)

[8] In special cases, junior professors can earn an additional supplement per month, which is not to exceed 10% of a junior professor's basic wage (Detmer and Preissler 2005).

[9] This limit is set at the difference between the basic wage of W3 and B10 (another, non-professorial pay scheme), which was 5241.48 on 1 August 2004 (Detmer and Preissler 2005)

[10] If the academic already earns bonuses that exceed this limit and a higher bonus is necessary to attract the academic from or prevent him from wandering off to another German university (BMBF 2002).

[11] The complete abolition of the habilitation was however declared unconstitutional and consequently retracted (Pritchard 2006).

[12] Using pay tables valid as of August 2004 (Hochschullehrerbund 2009)

[13] C3 or C4 in the age-related system; W2 or W3 in the performance pay system.

[14] When comparing W2 with C3 or W3 with C4

[15] When comparing W3 with C3, a transition which is allowed in most states (Handel 2005).

another university, and roughly 75% of this if a counter-offer of the home university was accepted (Detmer and Preissler 2006, Preissler 2006, Dilger 2013). By comparison, the average attraction and retention bonus in the W-pay system had already grown to 1187 euro in 2006, and the average on-the-job performance bonus to 1649 euro (BMI 2007). Furthermore, only a small fraction of professors qualified for and received bonuses under the age-related pay system. Handel (2005) for instance calculates, using data from the Ministry of Science and Culture in Niedersachsen, that only about 16.5% of professors received attraction or retention bonuses in the age-related pay system. In contrast, any tenured professor in the performance pay system can receive bonuses, and already in 2006 about 77% of professors in the performance pay scheme did receive bonuses (BMI 2007). Consequently, only about 3.55% of the total professorial pay volume was spent on attraction and retention bonuses in the age-related system, before the reform (Handel (2005), using data from Expertenkommission (2000)), while an estimated 26% of the professorial pay volume was available for performance bonuses under the performance pay scheme immediately after the reform(Handel 2005). Combined with the fact that, at most ages, the basic wage is lower in the performance pay system than in the age-related system, this means that a larger portion of professorial pay depends on performance and there is a greater spread in professorial pay in the W-pay system. The W-pay system therefore offers higher powered performance incentives than the old, age-related pay system.

3 Empirical Analysis

3.1 Data Description

In order to study effort and selection effects of performance pay in knowledge creation, I use a new panel data set that encompasses the affiliations and productivity measures of the universe of academics in German academia for the years 1999-2013. This individual level panel encompasses 55132 academics who held a tenured position at a German public university at some point between 1999 and 2013. The data set provides information regarding an academic's affiliation; reporting whether his position is tenured, and whether he is affiliated with a public university in a given year. Furthermore, the data set contains a variable for the impact factor-weighted[16] number of publications of an academic in a given year and the average number of weighted publications in the previous six years. The data set also provides the year in which an academic obtained his postdoctoral qualification, as well as the year a person started working in academia. Finally, there is a gender variable, birth year variable and, if applicable, year of passing.

As discussed above, I restrict attention to academics who held a tenured position at a German public university between 1999 and 2013, because the reform only changes the pay schemes of academics at public higher education institutions, and performance bonuses can be earned in tenured positions only. I discard higher education institutions other than universities, because I focus on research output, and the research output of universities is incomparable to that of other higher education institutions.

I draw from three main input data sets to construct the individual panel data set that I use in this paper; Kuerschners Deutscher Gelehrten Kalender, Forschung & Lehre Magazine and ISI Web of Science. Kuerschners Deutscher Gelehrten Kalender (hereafter: DGK) is a comprehensive encyclopedia of academics affiliated with German universities (*Kuerschners Deutscher Gelehrten Kalender Online* 2013, *Kuerschners Deutscher Gelehrten Kalender* 2006, 2008). I use DGK as a register of the universe of academics affiliated with German universities and extract academics' personal information (full name, birth date, year of passing,

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[16] A publication is weighted by the impact factor of the journal in the publication year, where the impact factors are taken from *Journal Citation Report* (2000-2012).

gender) as well as professional information (academic affiliation at different points in time, start year of academic career in Germany, end year of academic career in Germany, self-reported information on career history) from it. I supplement the information in DGK regarding the timing of affiliation changes and the obtainment of postdoctoral qualifications with information from Forschung & Lehre Magazine (hereafter: FuL) (*Forschung und Lehre* 1999-2013). Finally, I extract publication records for the academics in my data set for the years 1993-2013 from the ISI Web of Science database to construct measures of research productivity. My preferred measures of productivity are based on weighted publication records, where I weigh each publication by the two-year impact factor of the journal in which it is published. I use the impact factors from the ISI journal citation report (JCR) of the year of publication[17].

I match academics across the three input data sets on the basis of last name, initials and field. Doing so yields an 83% match rate of academics who FuL reports as having a tenured affiliation with a German university to academics listed in DGK. Differences in the spelling of names, typos and erroneous information regarding affiliation changes in FuL mostly explain the 17% that I cannot match. I have direct information on the timing of half the affiliation changes [18] in my panel data set from FuL. For the other half of affiliation changes, DGK provides the year of change in 23% of the cases[19] and I infer the timing of the remaining affiliation changes from academics' affiliations listed in DGK at different points in time, the year they passed their habilitation as well as the start and end year of their academic career in Germany recorded in DGK.

3.2   Descriptive Statistics

Table 1, Panel A reports a few summary statistics for the individual level panel used for the effort effect analysis below. The main variable of interest is the weighted number of publications of academics in a given year. As the summary statistics in the table show, academics at public universities are on average more productive than academics at other higher education institutions (compare rows 1 and 2), while tenured professors at public universities are more productive than nontenured academics at public universities (compare rows 2 and 3). Academics who are in the early stages of their academic career and manage to obtain a tenured position at a public university are more productive still (cf. row 4). Row 4 shows the weighted number of publications of academics who make tenure at a public university either just before the reform, in 2003 or 2004, or directly after, in 2005 or 2006. The former cohort comprises 2193 academics, the latter 1524, and I restrict attention to these cohorts to estimate the effort effect of performance pay as explained below[20].

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[17] Because I have ISI JCR data for the years 2000-2013 only, I use the average of the impact factors from JCR 2000 through JCR 2004 to weigh publications before 2000.

[18] Where at least one of the affiliations concerns a tenured position at a German university.

[19] This is self-reported career information and hence may introduce bias in my data set. I therefore use the information regarding affiliation changes provided in FuL whenever available. Reassuringly though, a consistency check revealed that the information in DGK regarding the timing of affiliation changes differs from that in FuL for only 5% of the individuals who change a (tenured) affiliation at least once.

[20] To be exact, I restrict attention to academics who make tenure in 2003 or 2004, respectively 2005 or 2006, and who are affiliated with a public university at some point in the post-reform period. This also includes academics who spent some of the post-reform period at another higher education institution. As the summary statistics show, academics at higher education institutions other than universities publish less on average, and if they are at a private university, they are not affected by the pay reform. The productivity of academics not at public universities should therefore change less in response to the reform, and the below estimates of the effort effect are a lower bound. The estimates of the effort effect reported below are robust to restricting attention to academics who were affiliated with public universities throughout the post-reform period, and, as expected, larger.

3.3   Effort Effect

In order to identify the pure effort effect of the introduction of performance pay in German academia, I use the fact that any contract for a professorial position at a public university in Germany signed or renegotiated as of 1 January 2005 necessarily falls under the performance pay scheme, whereas any contract signed before this date falls under the old, age-related pay scheme[21]. Accordingly, academics who start their first tenured affiliation before 2005 continue to fall under the age-related pay scheme[22], whereas academics who make tenure after 2004 switch to the performance pay scheme upon making tenure. If the timing of the tenure decision is exogenous, the performance incentives that first-time tenured affiliates face are exogenous as well. I can then identify the pure effort effect of performance pay on academic productivity by comparing the change in productivity from before to after the pay reform of academics who start their first tenured affiliation before 2005 (the control group) with the change in productivity of academics who start their first tenured affiliation as of 1 January 2005 (the treatment group)[23]. However, it is not ex ante obvious whether the timing of the tenure decision is exogenous. In particular, academics could try to speed up the process in order to avoid the performance pay system[24]. I provide a number of tests that yield no evidence of this below.

Table 2 shows the unconditional means of academic productivity for the treatment and control group and before and after reform periods separately [25]. I use a two-year window before and after the reform to define the treatment and control group in order to abstract from seniority effects. Thus the treatment group consists of academics who first made tenure in 2005 or 2006, while the control group consists of academics who first made tenure in 2003 or 2004. Academic productivity is defined here as the number of impact-factor weighted publications of academic i in field f in year $t + x_f$, where x_f denotes the average publication lag in field f. The average publication lags are taken from ? and differ across fields. I have data for 6 years before the reform (1999-2004) and 9 years after the reform (2005-2013).

The difference in means estimate in Table 2, Panel A, column 3, row 1 shows that there is no significant difference in productivity between the treatment and control group before the reform. After the reform however, the average productivity in the treatment group is significantly larger than that in the control group (cf. Column 3, row 2). Moreover, the difference between these two differences is positive and significant (cf. column 3, row 3). If assignment to the treatment and control group was indeed exogenous, so that, absent the reform, the treatment group's productivity would have followed the same trend as that of the control group, this difference-in-differences estimate is an estimate of the causal effect of performance pay on academic effort. This effort effect is economically large; amounting to a 35% increase in academic productivity relative to the pre-reform productivity in the control group. In what follows, I perform several checks of the validity of this estimate of the effort effect.

The differences in means in columns 1 and 2 of row 3 in Panel A provide a first such check. Reassuringly,

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[21] With the exception of Bremen, Niedersachsen and Rheinland-Pfalz, who introduced performance pay before this deadline (in 2003 and 2004, respectively) (Detmer and Preissler 2005). Note that using 2005 as uniform before-after cut-off yields a conservative measure of the effort effect, since some of the control group is in fact already treated before this time.

[22] They would be promoted to a higher pay grade in the age-related pay scheme when making tenure (Detmer and Preissler 2004).

[23] Note that any other events that might occur around the time of the reform do not affect the validity of interpreting the diff-in-diff estimate as the causal effort effect of performance pay as long as those events do not affect the pre- and post-reform cohorts differentially. To the best of my knowledge there are no events that do so.

[24] Note that an academic's attempt to delay the tenure decision would not be rational and therefore not much of a concern, since he would delay earning the higher pay associated with a tenured position, while he can always opt into the performance pay scheme after 2005 if so preferred.

[25] The means and differences in means are estimated through a pooled OLS regression of the lagged, weighted number of publications of academic i in year t on a constant, a treatment dummy, a post dummy (that is 1 for the years after the reform (2005-2013), and 0 before, so that I estimate persistent shifts in productivity from before to after the reform) and a treatment*post interaction term. I estimated standard errors, clustered by individual academic.

the average productivity of both the control group and the treatment group increases from before to after the reform. Hence the positive difference-in-differences estimate in column 3, row 3 really comes from a greater increase in the average productivity of the treatment group than the control group instead of a (larger) decrease in productivity in the control group. The fact that the average productivity of the control group also increases after the reform is consistent with a positive average treatment effect. This aligns both with increased effort from academics who still fall under the age-related pay scheme to up their chances of receiving a lucrative job offer in the performance pay scheme, and with greater positive spillover effects through increased positive assortative matching[26] (Ytsma 2016).

In order to test the identifying assumption that the assignment to treatment and control is exogenous, I report the results of a placebo difference-in-differences estimation in Table 2, Panel B. Here, the placebo treatment group consists of academics who first made tenure in '03/'04 (the control group in Panel A), and the placebo control group consists of academics who first made tenure in '01/'02. If academics were able to influence the tenure clock, I would expect lower productivity academics to try to move up the tenure decision if it would allow them to stay in the age-related pay system. If not just the level but also the growth rate of academic productivity is smaller for lower productivity academics, this would decrease the growth in average productivity of the control group used above (academics first tenured in '03/'04) from before to after the reform. Consequently, the change in average productivity of academics who first tenured just before the reform (in '03/'04) would then be smaller positive (or larger negative) than the change in average productivity of an earlier cohort (academics who first made tenure in '01/'02). As the placebo difference-in-differences estimate in column 3, row 3 shows however, I do not find evidence of such selection into the '03/'04 first tenure cohort. The average productivity of both the placebo treatment and control group increases from before to after the reform (cf. columns 1 and 2, row 3), and this increase is even slightly larger for the placebo treatment group than the placebo control group, though not significantly so.

3.3.1 Baseline Difference-in-Differences

As a next step I estimate the effort effect in a parametric difference-in-differences model. In particular, I estimate the following equation as a Fixed Effects model (at the individual level) in order to estimate the effort effect of the introduction of performance pay:

$$Y_{i,f,t-x_f} = \alpha_i + \beta_1 t + \beta_2 post'05 + \beta_3 post'05 * Treatment_i + u_{i,t} \tag{1}$$

The corresponding estimation results are shown in Table 3. The dependent variable, $Y_{i,f,t-x_f}$, denotes the lagged number of impact factor weighted publications of academic i in field f in year $t-x_f$, where x_f denotes the average publication lag in field f as before. The variable $post'05$ is 1 as of 2005 and 0 beforehand. The $Treatment$ variable is 1 for academics who start their first tenured affiliation at a public university in 2005 or 2006, and 0 otherwise. I restrict the sample to include only those academics who start their first tenured affiliation at a public university in 2003, 2004, 2005 or 2006. The $post'05 * Treatment_i$ interaction term is therefore a difference-in-differences estimate of the effort effect of performance pay in knowledge creation, with academics who start their first tenured affiliation at a public university in 2003 or 2004 as the control group. Robust standard errors, clustered at the individual level are reported throughout.

Column 1a in Table 3 reports the results from the baseline regression without linear time trend t, column b shows the results from the baseline regression with linear time trend. The $post'05 * Treatment_i$ interaction

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[26] If the academic production function is supermodular.

term is always positive and significant, and in the same order of magnitude of the $post'05$ coefficient estimate. The estimate of the interaction term implies that the academics starting their first tenured affiliation under the performance pay system (the treatment group) produce on average about one and a half weighted publication more than academics who started their first tenured affiliation under the age-related pay system in every year after the reform. I thus find evidence of an effort effect that is highly significant and economically large.

The $post'05$ coefficient estimate in Column 1a is also positive and significant, and economically large. Including the time trend however turns the $post'05$ coefficient negative and significant. This could be caused by mean reversion after promotion (here: making tenure), as argued in Lazear (2004). The linear trend itself is positive and significant, providing evidence that the number of weighted publications increases over time throughout the entire sample period. All in all, this suggests academics produce more as they grow older (at least at the beginning of their academic career), and implies I find no evidence that the reform increases the productivity of the control group as well.

3.3.2   Pre-Existing Trends

To further validate the identification strategy, I test for pre-existing trends. It could be that the positive and significant $post'05 * Treatment_i$ interaction term in the baseline simply reflects pre-existing trend differences between the treatment and control group. As a first pass, I therefore estimate the following simple difference-in-differences regression equation including a full set of treatment dummy * year dummy interactions:

$$Y_{i,f,t-x_f} = \alpha_i + \sum_{yr=2001}^{2013} (\beta_{1,yr} yr + \beta_{2,yr} yr * Treatment_i) + u_{i,t} \tag{2}$$

Here, yr is a year dummy[27], all other variables are as specified above and all regressions contain individual fixed effects. As for the baseline regression 1, the sample is restricted to academics who started their first tenured affiliation at a German public university in 2003, 2004, 2005 or 2006, so that - as before - $Treatment$ is 1 if an academic starts his first tenured affiliation in 2005 or 2006 and 0 if he starts his first tenured affiliation in 2003 or 2004 (the control group). The point estimates and 90% confidence intervals of the corresponding interaction terms are plotted in panel a of figure 2^{28}. These interaction terms give the year-on-year productivity differences between the treatment and control group. The figure shows no sign of a pre-existing trend: the estimates of the year-on-year differences first become positive and significant in 2005 only. They subsequently remain positive and significant for most of the post-reform period, so for most of the post-reform period, the productivity of the treatment group is statistically significantly larger than that of the control group.

As an alternative test for pre-existing trends, I augment the baseline model in equation 1 with three placebo $post$ variables - $post\_t - 3$, $post\_t - 2$, $post\_t - 1$ - and their interactions with the $Treatment$ variable[29]. These placebo $post$ variables are 1 as of 2002, 2003 and 2004, respectively, and 0 beforehand. The estimation results in Table 3, columns 2a and 2b confirm that there is no evidence of a consistent pre-existing trend, since the interactions of the placebo $post$ variables with the $Treatment$ variable are not all positive and significant. Reassuringly, including the pre-trend terms does not render the $post * Treatment_i$ interaction term insignificant either, though the size of the coefficient does decrease.

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[27] Note that I include year dummies here (e.g. the 2005 dummy is 1 in 2005 only, and 0 otherwise) and not post variables as in the baseline model in equation 1 (the post variable is 1 as of 2005 and 0 before).

[28] Corresponding tabulated estimation results are available from the author upon request.

[29] Where, as in the baseline model (1), the $Treatment$ variable is 1 for academics who start their first tenured affiliation at a public university in 2005 or 2006, and 0 otherwise.

Furthermore, I find evidence of an announcement effect in these regressions, because the $post\_t - 3 *$ $Treatment_i$ interaction term is positive and significant. This means that academics who would tenure after the performance pay reform comes into effect (as of 2005) and hence would get paid under the performance pay scheme once tenured, step up their game the moment the reform is announced (in 2002), so as to up their chances of earning bonuses in the future. The effect amounts to about half a weighted publication more per year since the announcement of the reform; a 12% increase in academic productivity relative to the unconditional pre-reform productivity in the control group[30]. Given that only the, effectively, piece-rate component of the performance pay regime - namely the prospect of attraction bonuses, which are linked to (past) performance - takes effect from the moment the reform is announced, this 12% increase in productivity can be interpreted as the effort effect of the piece-rate component only. This means that the effort effect of the tournament component of the performance pay scheme brings about a 23% increase in productivity. The effort effect of competitive (tournament) pay is thus almost twice as large as the effort effect of piece-rate pay.

3.3.3 Placebo Experiment

As a final set of tests of the identification strategy, I perform the same regressions for a placebo treatment and control group. In Table 4 columns 1a and 1b, I show the results of a placebo difference-in-differences baseline regression (equation 1) with the placebo treatment group comprising academics starting their first tenured affiliation in 2003 or 2004 (the control group in the baseline regression in Table 3) and the placebo control group made up of academics starting their first tenured affiliation in 2001 or 2002. Given that both these groups fall under the age-related pay system, there should be no differential effort effect once the reform gets implemented. The fact that the $post * Placebo\_Treat_i$ interaction term is not significant is in line with this.

Figure 2b depicts the confidence intervals of the interaction terms in equation 2 when the sample is restricted to academics who started their first tenured affiliation at a German public university in 2001, 2002, 2003 or 2004, so that $Treatment$ is 1 if an academic starts his first tenured affiliation in 2003 or 2004 (the placebo treatment group) and 0 if he starts his first tenured affiliation in 2001 or 2002 (the placebo control group). In line with the results in Table 4, columns 1a and 1b, most of the year-on-year productivity differences between placebo treatment and placebo control are not significantly different from zero in the post reform (2005-2013) period, again allaying concerns about endogenous selection into the treatment group.

Figure 2b also shows that the three interaction terms in the years surrounding tenure of the treatment group (2003, 2004, 2005) are positive and significant, This is to be expected, since academics need a good publication record to make tenure, and will therefore make every effort to publish (more) when their tenure clock is running out. Taking into account that some publications counting towards tenure may not actually have been published but only accepted by the time the tenure decision is made, allows for a relative upswing in publications by newly tenured academics the year after they make tenure. Such a surge in productivity around the time academics first make tenure would however only bias the baseline difference-in-differences estimates of the effort effect (Table 3) if it causes a permanent increase in the level of productivity from the moment of first tenure. In order to test whether this is the case, I finally estimate the placebo difference-in-differences model augmented with three placebo $post$ variables - $post\_t - 3$, $post\_t - 2$, $post\_t - 1$ - and their interactions with the $Placebo - Treatment$ variable. The corresponding estimation results in Table 4, columns 3 and 4 show that none of the interactions are positive and significant, so I find no evidence of

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[30] Cf. Table 2, column 2, row 1.

a permanent upswing in the productivity level from the moment an academic makes tenure. Furthermore, these results provide further evidence that there are no signs of endogenous selection into treatment and control group either[31].

3.4   Selection Effect

Apart from academics who tenure after the reform, and therefore necessarily fall under the performance pay scheme, academics who already have a tenured affiliation before 2005 can also select into the performance pay scheme by changing affiliation or position, or by opting into the pay scheme while retaining the same position. I do not have information on the latter, though Detmer and Preissler (2005) report that only a small number of professors chose to opt into the W-pay scheme in their current position. I do however observe professors changing affiliation or position and, consequently, changing into the performance pay scheme. I exploit this information to analyse the selection effect of the reform. Given that a professor's pay increases with performance through bonuses in the performance pay scheme, but no longer with age as in the age-related pay system, I expect more productive academics to be more likely to select into the performance pay scheme. I test this hypothesis through hazard rate and survival function analysis in this section.

For this purpose, I derive survival data from the individual panel data set, in which I focus on academics switching from the age-related pay scheme to the performance pay scheme by renegotiating the contract of their current tenured position[32] or by changing to another tenured position, possibly at another university. In order to abstract from academics entering the performance pay scheme because they make tenure after the reform, I restrict attention to academics who already hold a tenured affiliation before 2005. I further restrict the sample to academics with a tenured affiliation at a public university after the reform, since only these academics have the choice (i.e. are "at risk") of switching pay scheme. There are 37571 such academics and I observe a total of 3376 switches in a total of 248107 periods (years) that these academics can switch from the age-related to the performance pay system (cf. Table 1, Panel B).

Panels a and b of figure 3 show the Epanechnikov kernel density estimates of the hazard function for switches from age-related to performance pay for academics whose average productivity falls in the top decile or bottom 90% of the average productivity distribution[33]. Panel a uses contemporaneous average productivity measures, while Panel b employs the average productivity data from 2005. Because an academic's average productivity is calculated as the average number of impact factor weighted publications in years t-6 through t-1, the average productivity measures in 2005 abstract from the effort effect of the performance pay reform. In both figures, the hazard rate for switching to the performance pay scheme is clearly greater for top decile academics throughout, so higher productivity academics are more likely to sort into the performance pay scheme. The Kaplan-Meier estimates of the survival functions of staying in the old, age-related scheme in panels c and d of figure 3 show the same result. A log-rank test of the equality of the survival functions

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[31] Specifically, if lower productivity academics were able to speed up their tenure clock and if their productivity would slow down more than the average mean reversion in the placebo control group upon making tenure, the $post\_t - 2 * Placebo\_Treat_i$ interaction term or $post\_t - 1 * Placebo\_Treat_i$ interaction term would be negative and significant. If, on the other hand, higher productivity academics were able to speed up their tenure clock and if their productivity would slow down less than the average mean reversion in the placebo control group, the $post\_t - 2 * Placebo\_Treat_i$ interaction term or $post\_t - 1 * Placebo\_Treat_i$ interaction term would be positive and significant. I do not find evidence of either type of selection.

[32] I assume that, whenever an academic receives an offer, he either accepts and changes position, or rejects and renegotiates his current contract. In either case, the academic switches to the new performance pay scheme if the change or renegotiation happens after the reform. If there are academics who do not at least renegotiate their contract when they receive an offer, these academics are more likely to be of a lower productivity type, and including them in the pool of switchers would reduce the estimate of the selection effect I find.

[33] Because only academics who already had a tenured affiliation before the reform can switch from age-related to performance pay by changing affiliation or position, only these changes are considered here.

of top decile academics and bottom 90% academics rejects the equality of the survival functions at the 1% significance level[34].

I estimate the selection effect of the introduction of performance pay parametrically by estimating the following Weibull proportional hazard model:

$$\lambda_{i,t} = \rho * exp\left[\beta_0 + \beta_1 \bar{y}_{i,t} + \beta_2 age_{i,t} + u_{i,t}\right] * t^{\rho-1} \tag{3}$$

The results of the estimation of this Weibull model are presented in Table 5. The model estimates the hazard ratio for academics switching from age-related to performance pay[35]. In columns 1a and 2a of Table 5, I only use the average productivity of academic i in year t: $\bar{y}_{i,t}$ (the average weighted number of publications of academic i in years $t-6$ through $t-1$) as explanatory variable. In columns 1a and b, I use a contemporaneous measure of average productivity, while in columns 2a and 2b, an academic's average productivity in 2005 is used to abstract from effort effects. In columns 1b and 2b, I also include an age variable. This age variable is calculated using an academic's reported birth year whenever available[36], and equal to a synthetic age otherwise. I calculate synthetic birth years by subtracting the average age at habilitation or career start of academics for whom I do have a birth year[37] from the year of habilitation or career start of the academics for whom I do not have a birth year. Synthetic age is then equal to the age imputed using the synthetic birth year for academics for whom I do not know the actual birth year.

The coefficient estimate of the average productivity of an academic, $\bar{y}_{i,t}$, is positive and significant throughout, suggesting that more productive academics are more likely to select into the performance pay scheme. The coefficient estimate in column 1a implies that one extra impact factor weighted publication per year on average increases the probability of selecting into the performance pay scheme by 0.3%. Adding age as covariate increases the size of this coefficient to 0.5%. This is not surprising, given the strong negative correlation between age and average productivity. Indeed, one extra year of age is associated with a 9% decrease in the probability that an academic will select into the performance pay scheme. Accordingly, after controlling for age, the Weibull parameter ρ changes from being a precisely estimated zero - indicating hazard does not change over time - to being positive and significant, in line with increasing hazard over time. This means that academics are more likely to switch to the performance pay system the longer this system has been around, perhaps because any uncertainty regarding the practical implications of the new pay scheme is reduced as time goes by. The results are robust to using a contemporaneous average productivity measure in columns 2a and 2b[38].

4   Conclusion

This paper studies the effort and selection effect of performance pay in academia and provides empirical evidence that academics significantly increase effort in response to performance pay and that higher productivity academics are more likely to select into performance pay. In order to do so, I use the introduction of performance pay in German academia in 2002 as a natural experiment and employ a newly constructed data set encompassing information regarding research productivity and affiliations of the universe of German

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[34] The log-rank test returns a Chi-squared statistic of 167.38 (p-value 0.0000) when using the contemporaneous average productivity measure and a Chi-squared statistic of 101.15 (p-value 0.0000) when using the 2005 average productivity measure.

[35] While being younger than 66 and hence not retired.

[36] I have birth year data for 43.3% of academics in the data set. In order to prevent selection bias, I supplement the birth year data with synthetic birth year data in the analyses.

[37] The average age at habilitation or career start is 38.

[38] Estimation of equivalent Cox proportional hazard models yield similar results (results available upon request)

academics. Before the reform, academics were all paid according to an age-related pay scheme, in which the effectively flat wage increases with age. In contrast, in the performance pay scheme implemented after the reform, academics earn a basic wage that does not increase with age and is lower than the basic wage in the age-related pay scheme for most ages. On top of this basic wage however, academics can now earn bonuses that are partly distributed through on-the-job performance tournaments and partly through an, effectively, piece-rate scheme.

In order to estimate the effort effect, the paper exploits the fact that academics who make tenure just before the reform fall under the old, age-related pay scheme, while academics who make tenure directly after the reform are paid according to the performance pay scheme. If the timing of the tenure decision is exogenous, the difference in the change in productivity from before to after the reform between the cohort making tenure just before the reform and the cohort making tenure directly after, can be interpreted as the causal effect of performance pay on academic effort. I estimate this differential change in a difference-in-differences framework and find an effort effect that amounts to a 35% increase in productivity. Because the pay reform was required to be budget-neutral, this increase in research productivity represents a net gain, as it comes at no extra cost. About one-third of this effort effect is caused by the piece-rate component of the performance pay scheme, with the remaining two-thirds instigated by the on-the-job performance bonuses. A placebo difference-in-differences estimation shows that the identifying assumption of an exogenous tenure decision is plausible, as there is no evidence of academics speeding up their tenure process. Furthermore, I find no evidence of pre-existing trends, which lends support to the identifying parallel-trends assumption of the difference-in-differences framework.

I estimate the selection effect into performance pay though hazard rate and survival function analysis, where I use the fact that any tenured professor who changes affiliation or position after the reform automatically switches to the performance pay scheme. Because pay in the performance pay scheme only increases with performance, and no longer with age, I expect to find that more productive academics are more likely to switch to performance pay. This selection effect is borne out by the analysis indeed, which adds another route through which performance pay can increase research productivity.

By studying the effort and selection effect of performance pay in academia, the paper aims to contribute to and form a bridge between the literature on university governance and incentives in organizations. Given the economic importance of the academic sector, both in terms of direct economic value as well as for innovation and growth, it is crucial to understand the factors that determine the performance of universities. The literature on incentives in organisations provides ample evidence that incentives can significantly increase performance, and this paper shows that incentives can improve academic performance too.

There are several steps than can be taken next. The current paper focuses on research productivity of academics, but it would be very interesting and equally relevant to study the effect of performance pay on the educational performance of academics. Furthermore, it would be interesting to see if and when extrinsic motivation starts to crowd out academics' intrinsic motivation. Another important question is how performance pay affects the selection of candidate-academics. Other possible impacts of performance pay, such as on collaboration and network formation would make for exciting research avenues too.

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Fig. 1

The figure above shows the monthly wages (in euros) by age for the various pay levels in the age-related (C) and performance pay (W) schemes. The depicted wages were valid as of 1 August 2004 in former West-German states; the corresponding monhtly wages in former East-German states were 92.5% of these (Detmer and Preissler 2006). Data source: Oeffentlicher-Dienst (2004).

Fig. 2: Confidence Intervals of Year-on-Year Productivity Differences Between Treatment and Control Group

(a)

(b)
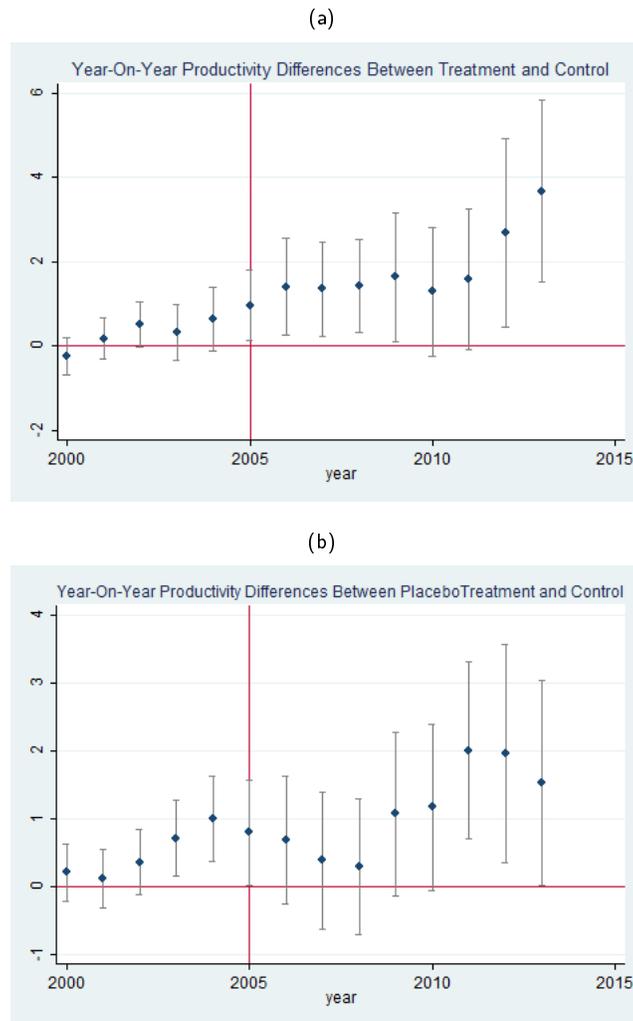
The figures depict the coefficient estimates and corresponding 90% confidence intervals of the interactions of a treatment dummy and year dummies in a regression of weighted publications in t on year dummies and year*treatment interactions, controlling for individual fixed effects. Refer to text for specification.

Fig. 3: Smoothed Hazard and Kaplan-Meier Survival Estimates of Switch to Performance Pay

(a)

(b)
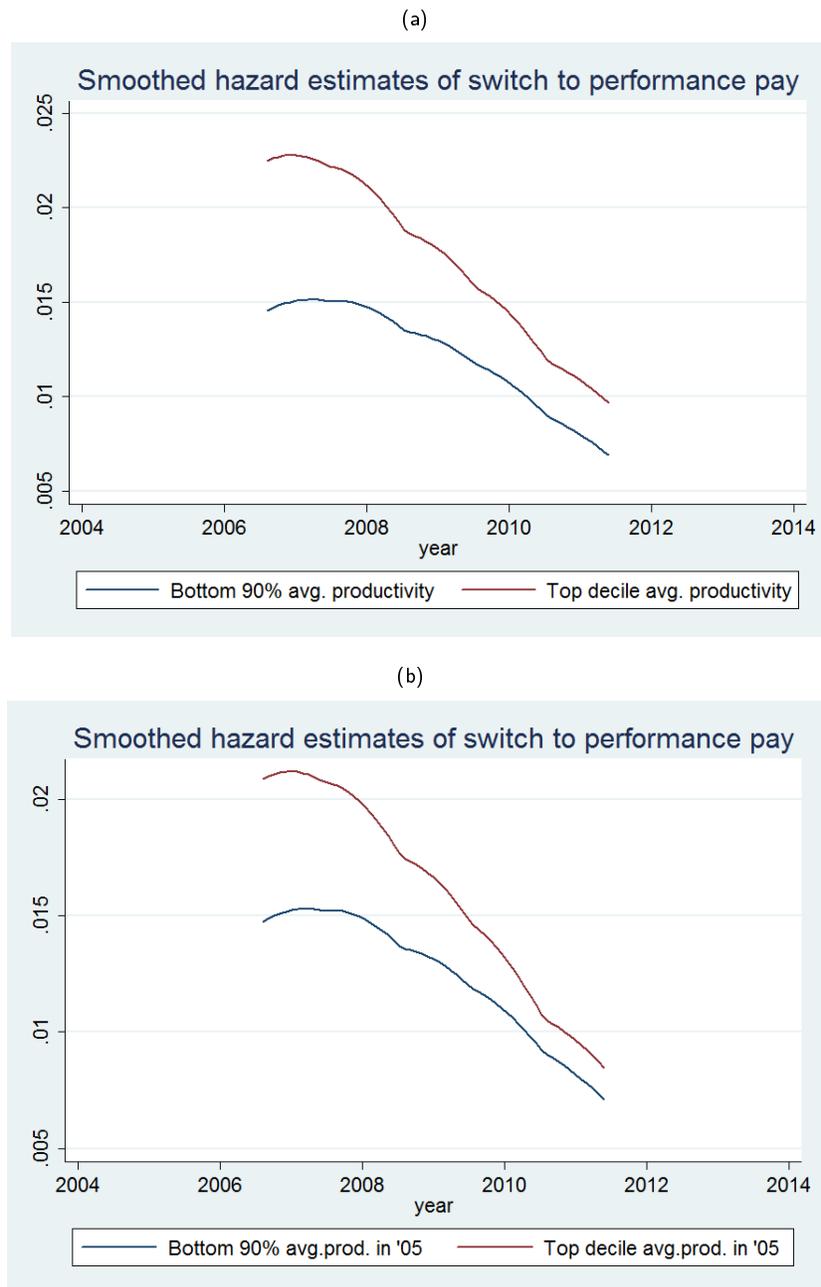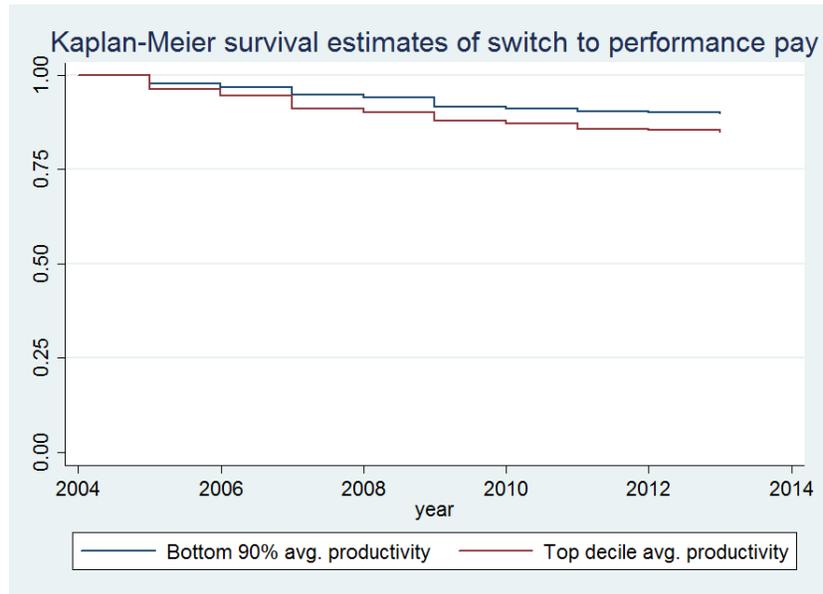
(c)
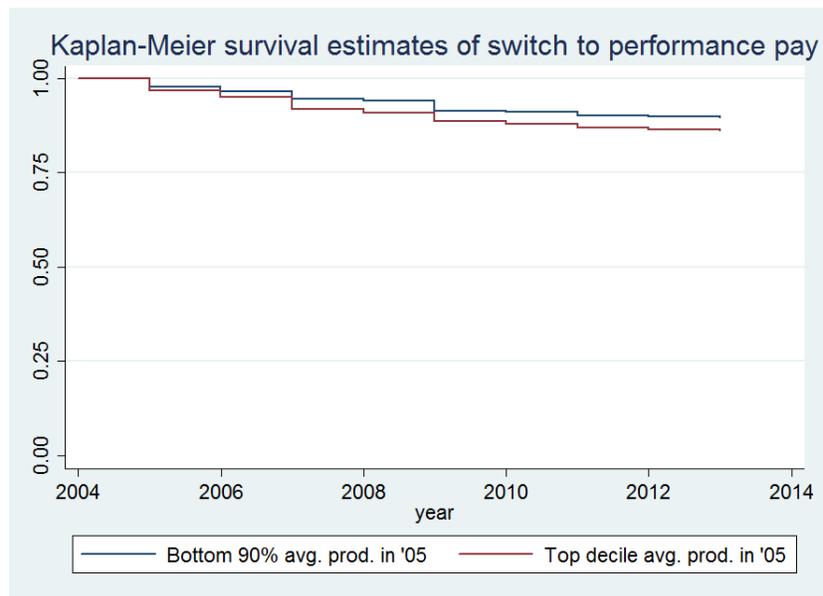
(d)

Figures a and b depict the Epanechnikov kernel-density estimates of the hazard function and figures c and d depict the Kaplan-Meier estimates of the survival function for switching to the performance pay scheme for academics in the top decile and bottom 90% of the average productivity distribution. Refer to text for specification.

Tab. 1: Summary Statistics

	Mean	Std	Min	Max	Median
Panel A: Effort Effect Analysis					
Weighted Publications (All academics, 1999-13)	3.783	16.709	0	1298.662	0
Weighted Publications (At public uni post'05, 1999-13)	4.737	18.910	0	1298.662	0
Weighted Publications (Tenured at public uni post'05, 1999-13)	5.235	22.038	0	1298.662	0
Weighted Publications (1st Tenure at public uni 03-06, 1999-13)	7.333	26.594	0	705.255	0
Panel B: Selection Effect Regressions	Total	Mean	Min	Median	Max
Switches	3376	0.090	0	0	1
Time at risk	248107	6.604	1	9	9

Notes: The unit of observation is academic i. For the baseline effort effect estimations, the sample is restricted to academics who started their first tenured affiliation at a German public university in 2003, 2004, 2005 or 2006. For the selection effect estimations, the sample is restricted to academics who made tenure before 2005.

Tab. 2: Unconditional Difference-in-Differences: Means of Weighted Number of Publications by First Tenure Cohort and Before/After Reform Period

(a) Experiment of Interest

	Treatment group (first tenured '05/'06)	Control group (first tenured '03/'04)	Difference
Before reform ('99-'04)	4.693	4.362	0.331
	(0.377)	(0.256)	(0.455)
After reform ('05-'13)	10.667	8.794	**1.873****
	(0.796)	(0.525)	**(0.953)**
Difference	**5.974*****	**4.432*****	**1.542****
	(0.540)	**(0.360)**	**(0.649)**

(b) Placebo Experiment

	Treatment group (first tenured '03/'04)	Control group (first tenured '01/'02)	Difference
Before reform ('99-'04)	4.362	4.196	0.166
	(0.256)	(0.236)	(0.348)
After reform ('05-'13)	8.794	7.931	0.863
	(0.525)	(0.509)	(0.731)
Difference	**4.432*****	**3.734*****	0.697
	(0.360)	**(0.362)**	(0.511)

Notes: * denotes significance at 10%, ** at 5% and *** at 1%. The table shows the means, differences in means and corresponding standard errors of the impact factor weighted number of publications of the (placebo) treatment and control group before and after the performance pay reform. Refer to text for specification.

Tab. 3: Baseline Diff-in-Diff and Pre-Trend Test

Dep. Var.: Weighted Number of Publications	Baseline		Pre-trend Test	
	1_a	1_b	2_a	2_b
Linear Time Trend		0.721***		0.780***
		(0.056)		(0.069)
Post_t-3 (1 if year$\geq$ 2002,0 otherwise)			0.548***	-1.012***
			(0.176)	(0.225)
Post_t-2 (1 if year$\geq$ 2003,0 otherwise)			0.822***	0.042
			(0.189)	(0.196)
Post_t-1 (1 if year$\geq$ 2004,0 otherwise)			0.297	-0.483**
			(0.192)	(0.205)
Post (1 if year$\geq$ 2005,0 otherwise)	4.432***	-0.975**	3.362***	-0.538
	(0.360)	(0.411)	(0.343)	(0.372)
Post_t-3 * Treat. (1st tenured '05/'06)			0.543**	0.543**
			(0.275)	(0.275)
Post_t-2 * Treatment			-0.186	-0.186
			(0.328)	(0.328)
Post_t-1 * Treatment			0.301	0.301
			(0.357)	(0.357)
Post * Treatment	1.542**	1.542**	1.144*	1.144*
	(0.649)	(0.649)	(0.635)	(0.635)
N	3717	3717	3717	3717

Notes: * denotes significance at 10%, ** at 5% and *** at 1%. Refer to text for specification.

Tab. 4: Placebo Diff-in-Diff and Pre-Trend Test

Dep. Var.: Weighted Number of Publications	Trend Break DiD		Pre-trend Test	
	1_a	1_b	2_a	2_b
Linear Time Trend		0.539***		0.594***
		(0.041)		(0.050)
Post_t-3 (1 if year$\geq$ 2002,0 otherwise)			0.299*	-0.889***
			(0.165)	(0.201)
Post_t-2 (1 if year$\geq$ 2003,0 otherwise)			0.468**	-0.126
			(0.206)	(0.206)
Post_t-1 (1 if year$\geq$ 2004,0 otherwise)			0.017	-0.577***
			(0.186)	(0.198)
Post (1 if year$\geq$ 2005,0 otherwise)	3.734***	-0.305	3.259***	0.290
	(0.362)	(0.389)	(0.335)	(0.344)
Post_t-3 * Placebo-Treat. (1st tenured '03/'04)			0.249	0.249
			(0.241)	(0.241)
Post_t-2 * Placebo-Treatment			0.354	0.354
			(0.279)	(0.279)
Post_t-1 * Placebo-Treatment			0.281	0.281
			(0.267)	(0.267)
Post * Placebo-Treatment	0.697	0.697	0.103	0.103
	(0.511)	(0.511)	(0.479)	(0.479)
N	4270	4270	4270	4270

Notes: * denotes significance at 10%, ** at 5% and *** at 1%. Refer to text for specification.

Tab. 5: Proportional Hazard Model

	1_a	1_b	2_a	2_b
Average Productivity	**0.003***	**0.005***	**0.003***	**0.005***
	(0.001)	**(0.001)**	**(0.001)**	**(0.001)**
Age		**-0.092***		**-0.092***
		(0.003)		**(0.003)**
Constant	**-4.304***	-0.015	**-4.304***	-0.015
	(0.027)	(0.160)	**(0.027)**	(0.160)
$ln\,(\rho)$	-0.006	**0.145***	-0.003	**0.148***
	(0.010)	**(0.011)**	(0.010)	**(0.011)**
N	37571	37562	37571	37562

Notes: * denotes significance at 10%, ** at 5% and *** at 1%. Refer to text for specification.