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Katja Görlitz
Marcus Tamm

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Participation – Evidence from a
Randomized Field Experiment**

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Information, Financial Aid and Training Participation – Evidence from a Randomized Field Experiment

Abstract

To increase employee participation in training activities, the German government introduced a large-scale training voucher program in 2008 that reduces training fees by half. Based on a randomized field experiment, this paper analyzes whether providing information about the existence and the conditions of the training voucher had an effect on actual training activities of employees. Because the voucher was newly introduced, only one-fourth of the eligible employees knew the voucher exists at the time of the experiment. The information intervention informed a random sample of eligible employees by telephone about the program details and conditions. The results indicate that the information significantly increased treated individuals' knowledge of the program but had no effect on voucher take-up or participation in training activities. Additional descriptive analyses suggest that the reasons for these zero effects are that the demand for self-financed training is low and that liquidity constraints do not discourage many employees from training participation.

JEL Classification: I22, D83, H52

Keywords: Training participation; voucher; financial aid; randomized field experiment; information treatment

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1. Introduction

In recent years, many European countries have introduced financial aid programs for training participation of employees (OECD 2004).[1] These programs subsidize individual monetary costs for training fees. The political objective of providing financial aid is to increase self-financed training activities of the employees. Increasing participation in lifelong learning is a political aim that was formulated by the strategic framework for European cooperation in education and training 'ET 2020'. While many studies have been concerned with training for the unemployed as part of active labor market policies (see e.g. Card et al. 2010 for an overview), much less is known about the effectiveness of financial aid to increase training participation of the employed population.

This study analyzes whether a newly introduced voucher program in Germany that reduces individual training costs by half has the potential to increase employees' training participation. In a randomized field experiment, a treatment group of eligible employees were given detailed information by telephone about the voucher, its conditions and how to obtain it. Both the treatment and the control group were drawn from a representative sample of eligible employees and they were not aware of participating in an experiment at any time. At the time of the first interview, only one-fourth of the eligible employees knew of the existence of the voucher program which could be the reason why in the overall population the number of issued vouchers was low shortly after voucher introduction. Comparing the treatment groups' voucher take-up in a follow-up survey one year later with the corresponding outcomes of the control group reveals whether it is the information constraint or whether it is the voucher itself that is responsible for low take-up rates. If the reasons for low voucher take-up are due to the design and the conditions of the voucher, the voucher will not be effective in raising training levels.

Our paper is related to different strands of the literature. We contribute to the literature being concerned with the effects of financial aid for educational attainment that was most often investigated with respect to schooling and tertiary education (see e.g. Deming and Dynarski 2010 for a review). With respect to financial aid for training, Schwerdt et al. (2012) and Hidalgo et al. (2014) show that training levels can be slightly increased by training vouchers that reimburse a fixed amount of the training costs which allows individuals to participate in training without bearing any of the training costs. In contrast to such vouchers, many of the voucher programs introduced at the European level rather finance a fixed share of the costs and require self-financing of the remaining training costs. Thus, our findings have important implications from a public policy point of view.

Because our field experiment comprises an information treatment about the voucher, we also contribute to the literature analyzing the role of information about financial aid on aid take-up and educational choice.[2] In particular, Dinkelman and Martínez (2014) show that informing 8th grade students about how to finance higher education increases the probability that they will

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[1] Examples include Austria, Belgium, Germany, Italy, the Netherlands, the UK and Switzerland.

[2] The literature also analyzes e.g. the role of information regarding the returns to education on educational decisions (see, e.g., Jensen 2010, Oreopoulos and Dunn 2013) and aspirations (McGuigan et al. 2012) as well as the role of information about school quality on school choice and student achievement (Hastings and Weinstein 2008). For our research question, however, the literature providing information about financial aid is more relevant.

enroll in a college preparatory high school and reduces absence from school. Bettinger et al. (2012) provide evidence that financial aid information alone does not increase college attendance unless applicants are given assistance with filling out a lengthy application form. Additionally, Booij et al. (2011) reveal that informing students about loans does not affect their behavior with respect to loan take-up. Our analysis examines adult education investment decisions that are undertaken after having entered the labor market. To our knowledge, we are the first to relieve the information constraints on training subsidies for the group of employees. Training decisions differ from decisions regarding college enrollment because they are less costly, much shorter in duration, occur more frequently and involve much lower opportunity costs because training usually occurs while employed, thus requiring no break in employment.

The main results indicate no significant effect of the information intervention on training activities measured at the extensive and intensive margin, i.e. by training incidence and the number of courses attended. This is because the intervention did not affect voucher take-up of the treatment group. We can rule out that this insignificant effect is due to an ineffective information treatment because the intervention did increase program knowledge one year after the treatment by a significant 8 to 9 percentage points, representing a 30-percent knowledge increase compared with the control group. The insignificant effect on voucher take-up can also not be attributed to small samples sizes (3,110 individuals participated in both panel waves) and limited statistical inference because power calculations reveal that an effect size for voucher take-up of as little as 0.6 percentage points would have turned statistically significant. We conclude that other reasons than the lack of information were responsible for not taking up the voucher and, thus, for not increasing training of the eligible employees.

Based on descriptive analyses, we additionally discuss reasons for why the voucher was ineffective in raising training quotas. This is another novel aspect of our paper that is particularly relevant for policy. We suggest that the non-existent intervention effect on voucher take-up is because reducing training costs by half does not increase the demand for self-financed training. This might be the case because most employees are able to satisfy their training needs with financial support of their employers.[3] Furthermore, non-participants in training report as most important reasons for non-participation to have no training demand, no time for training participation or low expected training returns. And the small share of employees who could not participate in training because of liquidity constraints are often not able to bear the remaining half of the training costs after redeeming the voucher. We suggest that our conclusions apply to other voucher programs which have a similar design as the German voucher. Policy makers planning to introduce training vouchers should put effort into analyzing the specific reasons for non-participation in training before designing and introducing a voucher.

The remainder of the paper is organized as follows. Section 2 presents the voucher program and section 3 describes the data, the experimental design and the empirical strategy. Section 4 documents the main results, section 5 discusses them in detail and section 6 concludes.

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[3] Training fully or partly financed by firms were not eligible for the voucher.

2. Background: The voucher program

The training voucher program *Bildungsprämie* (henceforth, the "BP voucher") was implemented in Germany in December 2008. In 2010 and 2011, the years of interest in this paper, the voucher reduced direct training costs by up to 50% for a maximum subsidy of 500 Euro and was targeted at employees.[4] Direct costs cover fees for participation in training courses that are charged by the providers of training. The voucher could be used for training at the vast majority of German training providers. The goals of the program were to increase the participation of employees in training activities, to enable them to individually finance lifelong learning activities and not only to participate in training financed by their employer. Training that was partly or fully financed by the employer was excluded from the subsidy. Note that employers are generally the major source of financing for training, not only in Germany but in Europe in general (Bassanini et al. 2007).

Eligibility for the BP voucher was pegged to several criteria. First, the voucher was only available for employees and self-employed workers with low or medium income. The income thresholds for taxable household income were 25,600 Euro per year for singles and 51,200 Euro for married couples. Approximately two-thirds of all employees (around 25 million) meet these income criteria. The unemployed were not eligible for the program but instead had access to active labor market programs. Second, only work-related training could be co-financed with the voucher and the voucher could not be used for training that had started before the voucher was issued or for training that was offered by the employer of the applicant. Third, the direct training costs that remained after deducting the voucher had to be borne by the applicants themselves; i.e., the voucher could not be combined with employer support or other public subsidies. Vouchers were issued in person at one of the 500 counselling offices located all over Germany. Counselling served the purpose of verifying the eligibility criteria and recording the training content on the voucher. The number of vouchers issued per person was restricted to one per year. When booking a course at a training provider, the voucher reduced individuals' fees immediately. Training providers were reimbursed by a governmental agency after submitting the voucher to the agency.

Next, we present results from administrative data regarding all vouchers and voucher users (RWI et al. 2012). The number of vouchers issued per year increased from approximately 63,000 in 2010 to 95,000 in 2011. With respect to the number of eligible employees, the share of program users equals 0.4 percent in 2011 (95,000/25,000,000=0.4 percent). The average redemption rate of vouchers was 78%. RWI et al. (2012) further show that the program users were a highly selective group and not representative for the sample of eligible employees. For example, 74% of program users were women compared with the corresponding share of 53 percent among the group of eligible employees. Approximately 17% were working as physical therapists, although their share within the group of eligible employees is less than 1 percent. The likely reason for this discrepancy is that women working in the health sector (including physical therapists) participate more frequently in self-financed training (with no financial involvement on the part of their employer), i.e., they participate more often in training that

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[4] The maximum voucher subsidy is high compared to the average costs paid by German training participants who had to incur training costs in 2012, which is on average 615 Euro (median: 230 Euro, see Behringer et al. 2013).

could potentially be financed by the voucher. In conclusion, the voucher was particularly attractive for a certain group of employees who sponsor training on their own with no employer involvement.

3. Experimental design, data and empirical strategy

3.1 Experimental design and data

The information intervention was implemented for a representative sample of eligible employees. The target population was drawn from administrative records of the social security system containing the income biography of employees, excluding self-employees and civil servants. Thus, the effect of the intervention is only representative of the sample of salaried employees, who, however, account for the vast majority of individuals eligible for the BP voucher (almost 90 percent of eligible employees are salaried and not self-employed or civil servants).[5] Eligible employees were initially interviewed by telephone in 2010, and the follow-up survey was administered approximately one year later in 2011. To avoid interviewees becoming aware of taking part in an experiment and to rule out unit nonresponse being systematically related to respondents' previous training participation or financial aid utilization, the telephone survey was labeled "Employment today and tomorrow". The information intervention was conducted at the end of the first interview, after general information on socio-demographics, employment aspects and previous training participation was collected. The second wave updated this general information, including information on the outcome variables of interest for the main analyses. At no time were respondents informed that they were part of an experiment.

The information intervention was randomly assigned to a 50-percent sample of the eligible survey respondents.[6] The intervention informed interviewees about the BP voucher, how to apply for and use it. The information was provided interactively; interviewees were asked to rate the attractiveness of the program and its conditions. They were also asked to indicate which sources they would use to inform themselves about the program and one of the answer categories contained the exact URL of the program webpage. In total, six questions were posed, as shown in Appendix A-2. The interactive method of informing about the program was chosen to support respondents' concentration and motivation as opposed to an intervention that provided information with no further involvement by the respondents. Overall, the intervention took approximately 5 minutes of the interview. The control group did not receive any such information as part of the interview.

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[5] A more detailed description of the sampling design of the survey can be found in Appendix A-1.
[6] The random assignment occurred at the level of the interviewees, not at the level of the interviewers. The interviewers did not know at the beginning of each interview whether they were talking to an individual from the treatment or the control group. This is because the CATI-questionnaires were completely same for both groups until the end of the interview where the sample split was randomly assigned among those who met the income criteria of the program and the intervention was only provided to the treatment group. Thus, we can rule out that interviewer-effects systematically correlate with treatment status. In the follow-up survey, interviewers were also unaware about the treatment status of individuals.

Six months after the first interview, all participants in the telephone interview received a letter for panel maintenance expressing gratitude for participation in the first interview and reminding them of the second interview to come. The letter also served the purpose of reminding the treatment group about the BP voucher, in particular, by an enclosed flyer whose content differed by treatment status. While the flyer informed the control group about first-wave results on changes in respondents' work life (how often respondents were confronted with new tasks in the workplace and how well they managed to cope with them), the treatment groups' flyer informed them about employees' training participation and the voucher program, including the URL of the program webpage. This information is important because 88 percent of the treated individuals stated in the first interview that they would use the internet as a source to obtain information about the program. The flyers for the treatment and the control group can be found in Appendices A-3 and A-4, respectively.

In both panel waves, all respondents were asked about their general knowledge about financial aid programs in the area of education and training. For each of four programs that are available for funding vocational education, college or employee training in Germany, the interviewees were asked whether they knew that the program existed. The specific question reads as follows: "I will now read out a list of public programs that provide financial support to enable participation in training and in education more generally. Please tell me whether you have heard about the respective program before or used it." The BP voucher was one of the four programs being presented to the respondents in randomized order. Individuals who knew of the BP voucher were also asked about previous program utilization. Of course, in the first interview, the treatment group was asked about program knowledge before the information intervention was conducted.

In the first wave, 5,019 interviews were conducted with eligible employees, of whom 2,501 received the information treatment and 2,518 did not. In the second wave, the sample consisted of 1,516 treated individuals and 1,594 individuals from the control group. The attrition rate between the first and the second interview was similar for the treatment (39%) and the control group (37%). Table 1 displays t-tests on the similarity of the characteristics between the treatment and control groups, documenting that the average characteristics for survey respondents in the first wave and for respondents in both waves are always balanced. None of the considered characteristics differs significantly by treatment status, indicating that the treatment and the control group are balanced.[7] Note that the balancing tests hold not only for the vast majority of socio-demographic and educational variables but also for the level of knowledge about the voucher program before the information treatment occurred. In the first wave, only one-fifth of the respondents in the treatment and the control group knew that the BP voucher program existed (22-23 percent), which means that more than three-quarters of eligible employees had never heard about the program two years after its introduction.

To provide further evidence on the quality of the randomization of the experiment, multivariate regressions were run to test the balancing of the treatment and the control group

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[7] We also tested whether the socio-demographic and educational variables are similar for those who drop out and are not interviewed in the second wave. The sample of dropouts is also balanced by treatment status (see Table B-1 in the Appendix).

and to test whether panel attrition is unrelated to the treatment status. Differential attrition was tested by regressing panel dropout on treatment status and individual control variables, revealing no statistically significant influence of the treatment on attrition (see column 1 of Table B-2).[8] Furthermore, we regressed the treatment indicator on individual characteristics to determine whether the characteristics are jointly significant. Columns 2 and 3 of Table B-2 show that the F-test of joint significance of the individual characteristics is insignificant for the first wave sample (p-value: 0.7203) and for the sample of individuals responding in both panel waves (p-value: 0.6546).

Table 1 is of further interest to describe the average characteristics of eligible employees. Women's share is 53 percent among eligible individuals. This figure is plausible because eligibility is tied to the level of income, which tends on average to be lower for women. Approximately 40 percent of eligible employees participated in training during the 12 months before the first interview and around one-quarter reported having training intentions for the coming 12 months; in particular, they indicated on a four-item scale that they intended to participate in training "for sure". Additional analyses combining first and second wave information reveal that the training intentions measured in the first wave are good predictors of actual participation because more than 75 percent of the individuals realized their plans in the second wave. Table 1 also shows that slightly more than one-fourth of eligible employees reported being liquidity constrained in the first interview. The dummy variable for liquidity constraints are based on a question asking respondents, how easily they could replace a good of daily use if it gets broken.[9] Individuals responding that they would not be able to replace this good were considered constrained. Although this question is of a subjective nature, it is more appropriate than household income because it also encompasses differences in household size and fixed-payment obligations (either for consumption or loans). A similar question for liquidity constraints was also used by Dohmen et al. (2010).

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[8] To test whether attrition is affected by treatment status within subsets of the sample, we also estimated a model including a full set of interactions between the treatment indicator and the individual control variables (results not shown in the tables). None of the interaction terms is statistically significant from which we conclude that dropout was not affected by the treatment status, not even for individuals with particular characteristics.

[9] The wording of the question is the following: "Assume that a basic commodity of daily use such as a television set or a sofa gets broken. How easily would you be able to afford 250 Euro to replace the commodity without having to borrow the money? 'Very easily', 'rather easily', 'rather difficult' or 'not at all able'".

Table 1. Summary statistics and balancing tests

	Eligible employees in the first wave					Eligible employees in the first and second wave				
	Treatment group Mean	Control group Mean	Difference	t-stat	Observations	Treatment group Mean	Control group Mean	Difference	t-stat	Observations
Socio-demographics and training measured in the first wave										
Female	0.537	0.525	0.012	0.78	5,019	0.543	0.517	0.025	1.25	3,110
Age < 35 years	0.243	0.246	-0.003	-0.26	4,996	0.206	0.197	0.010	0.63	3,100
Age (35 to < 45 years)	0.264	0.245	0.019	1.36	4,996	0.264	0.245	0.019	1.07	3,100
Age (45 to < 55 years)	0.326	0.338	-0.013	-0.83	4,996	0.349	0.374	-0.025	-1.28	3,100
Age (> 55 years)	0.167	0.170	-0.003	-0.23	4,996	0.181	0.185	-0.003	-0.21	3,100
Migration background	0.208	0.221	-0.013	-1.04	5,015	0.185	0.193	-0.008	-0.53	3,108
Cohabiting with partner	0.772	0.774	-0.002	-0.15	5,019	0.801	0.802	-0.001	-0.07	3,110
Children	0.388	0.371	0.017	1.12	5,019	0.399	0.372	0.027	1.38	3,110
East Germany	0.237	0.246	-0.010	-0.72	5,019	0.225	0.242	-0.017	-1.01	3,110
College	0.178	0.186	-0.008	-0.62	5,003	0.204	0.201	0.003	0.19	3,105
Vocational education	0.750	0.747	0.003	0.22	5,003	0.744	0.747	-0.003	-0.14	3,105
Compulsory education	0.072	0.067	0.005	0.65	5,003	0.052	0.053	-0.001	-0.08	3,105
Training participation in previous year	0.407	0.402	0.005	0.31	5,009	0.425	0.424	0.001	0.04	3,103
Future training plans (next 12 months)	0.236	0.254	-0.017	-1.25	5,004	0.255	0.270	-0.016	-0.86	3,103
Liquidity constraints	0.289	0.275	0.014	1.00	4,976	0.260	0.245	0.015	0.87	3,090
Program knowledge measured in first wave, before providing the information treatment										
Knowledge about the *Bildungsprämie*	0.229	0.227	0.002	0.14	5,007	0.2230	0.2255	-0.003	-0.15	3,104

Notes: The table shows summary statistics separately for the treatment and the control group for variables measured in the first wave, i.e., before the information intervention was provided. Difference shows the differences in the means by treatment status and the t-stats indicate their significance. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

3.2 Empirical framework

Because of the random assignment of the treatment, a comparison of individuals' outcomes between the treatment and control group identifies the causal effects of the information intervention. The intention-to-treat (*ITT*) effects that we estimate answer the question how the outcomes of interest would evolve, if our information treatment were provided to all eligible employees. The outcomes are estimated by the following regression:

$$Y_i = \beta_0 + \beta_1 Treat_i + \gamma' X_i + \varepsilon_i \tag{1}$$

where Y_i are the second-wave outcomes of individual *i*, in particular program knowledge, program take-up and training activities measured as binary variable (training incidence) and as the number of courses attended (training intensity). The training questions refer to work-related classroom training such as courses, seminars or lectures in the time between the first and second interview. The variable *Treat* indicates individuals' assignment to the treatment (1) or to the control group (0). The vector *X* includes control variables such as socio-demographics and educational attainment and ε is the idiosyncratic error term.

For outcomes for which there is pre-treatment information available, fixed effects regressions can additionally be estimated by the following model:

$$Y_{it} = \beta_0 + \beta_1 Treat_{it} + \gamma' X_{it} + \alpha_i + \omega_{it} \tag{2}$$

where Y_{it} represents the outcomes program knowledge and the binary training indicator of individual *i* at time *t* (with t=1, 2). As *Treat* equals one for the treatment group in t=2 and zero otherwise, it identifies the difference-in-differences effect. The vector *X* remains as described above, although only time-varying control variables can be incorporated. The variable α_i represents the individual-specific intercept controlling for time-invariant heterogeneity, which was found to matter for training decisions (Pischke 2001, Görlitz 2011). Motivated, talented or ambitious individuals participate more intensively in training. The idiosyncratic error term is indicated by ω_{it}. The standard errors account for clustering at the individual level. If equations (1) and (2) return similar effects in terms of size and significance, this would be an indication that the treatment and the control group are also balanced with respect to time-invariant unobservables or that time-invariant unobservables do not alter the results.

4. Results

Intention-to-treat estimates

Table 2 documents the effects of the information treatment on outcomes that are measured one year after providing the intervention. The first column summarizes the effects on individuals' program knowledge, in which the upper and lower panels present estimates of equations (1) and (2), respectively. The average knowledge about the program in the control group is 28 percent in the second interview; that is, three years after the introduction of the program, more than 70 percent of the eligible employees are still unaware of its existence. The treatment group experienced an average knowledge increase caused by the information

intervention by a statistically significant 8.4 to 9.0 percentage points. The period between the first and second interview is one year, which is a long time compared with other studies that measure a change of knowledge between 3 and 7 months after administering the treatment (Dinkelmann and Martinez 2011, Booij et al. 2011, Jensen 2010). Thus, the knowledge increase in our experiment should be interpreted as a long-term effect. The short-term knowledge increase might be higher because people tend to forget information, particularly if it appears to be of little relevance to them.[10]

Column (2) displays the results of the information treatment on actual program take-up. The intervention did not increase program take-up of the treatment group on any reasonable statistically significant level. Importantly, the reason for the statistically insignificant result cannot be attributed to sample size issues. Power calculations reveal that even a small increase in program take-up of 0.6 percentage points would have been sufficient to provide statistically significant results at the 5-percent level given the sample size and the control group mean. Relating the 0.6 to the average knowledge increase of the treatment group shows that the coefficient on program take-up would have become statistically significant if only one out of 15 individuals (=0.6 ppt /9.0 ppt) who learned about the program by the intervention had obtained a training voucher. Furthermore, note that the share of voucher recipients in the control group is approximately the same (0.5 percent) as the share of voucher recipients in the entire population of eligible individuals (0.4 percent; see the second section).

Although there is no effect on program take-up, an effect of the intervention on individuals' training activities could emerge, if the information affected training through channels other than reducing training costs by utilizing the voucher. For instance, learning that the government sponsors training might increase employees' training motivation and intention by showing how necessary public agents consider training. Alternatively, the intervention could have prompted more information about the program being gathered on the internet, ultimately resulting in use of another public program subsidizing training. Although there are few financial aid programs in Germany, the BP voucher clearly being the most far-reaching, there are alternative voucher programs that finance training in a few federal states.[11] The effect of the information treatment on training activities is documented in columns (3) and (4) of Table 2. There are no statistically significant effects of the intervention on the training activities of the treatment group. Additionally, the size of the point estimates is small compared with the second-wave control group mean in the case of both the cross-sectional model and the fixed effects model (see first row of Table 2). As a robustness check, the Appendix Table B-3 shows the equivalent results when re-estimating all regressions excluding the covariates, which leaves the main results unaffected.

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[10] Already in 1885 Hermann Ebbinghaus showed that the ability to recover information from memory declines when the retention interval increases, specifically if information is meaningless for an individual.
[11] See Görlitz (2010) for an evaluation of a state-specific German voucher program that not only allowed access to employees but also to firms. In this aspect, the program differs from the BP voucher program.

Table 2: Effects of the information treatment on program knowledge, take-up and training

	Outcomes measured one year following the intervention			
	Program knowledge	Program take-up	Training incidence in previous year	Training intensity in previous year
	(1)	(2)	(3)	(4)
Control group mean	0.278	0.005	0.445	1.111
Cross-sectional model				
Effect of information intervention	0.084 ***	0.004	0.010	0.087
	(0.019)	(0.003)	(0.018)	(0.071)
Control variables	Yes	Yes	Yes	Yes
Observations	3,056	3,052	3,058	3,058
Fixed effects model				
Effect of information intervention	0.090 ***		0.007	
	(0.021)		(0.022)	
Control variables	Yes		Yes	
Individual fixed effect	Yes		Yes	
Observations	6,102		6,116	

Notes: The cross-sectional models regress the second-wave outcomes on a treatment indicator and control variables measured in the first interview (i.e., gender, age, migrant, partner, children, East Germany, education dummies, previous training participation, training intentions and liquidity constraints). The fixed effects models include as covariates children and education dummies. When omitting the control variables, the effects are virtually the same (see Table B-3 in the Appendix). Standard errors are robust (in the cross-sectional models) or account for clustering at the individual level (in the fixed effects models). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Heterogeneity of treatment effects

Because the reasons for non-participation in training might be manifold and because the voucher only affects financial reasons or cost-benefit considerations, one would not necessarily expect homogeneous treatment effects from the information intervention in the group of treated individuals. Instead, the highest possible effects should be observed for individuals with training intentions or with liquidity constraints. Table 3 summarizes the results of subgroup analyses, particularly cross-sectional regressions accounting for heterogeneity in several dimensions. Individuals with definite training intentions in the first interview are considerably more likely to have remembered the information about the voucher program until the second interview. The information treatment increased the knowledge of these individuals by 13 percentage points (p-value: 0.001), nearly two times that of individuals with no intentions (7 percentage points). This result is evidence that the knowledge increase was more pronounced for individuals most inclined to participate in training, i.e., those for whom the information was more relevant. Nevertheless, the group with training intentions did not take up the voucher more often; nor did their training activities increase significantly.

Table 3: Heterogeneous effects of the intervention

	Outcomes measured one year following the experiment			
	Program knowledge	Program take-up	Training in-cidence in pre-vious year	Training in-tensity in pre-vious year
	(1)	(2)	(3)	(4)
Training intentions				
Effect of information intervention (a)	0.069 ***	0.004	0.021	0.094
	(0.021)	(0.004)	(0.021)	(0.070)
Treatment × training inten-tions measured in the 1st wave (b)	0.058	-0.001	-0.044	-0.028
	(0.045)	(0.007)	(0.038)	(0.198)
p-value of Wald test of (a)+(b)	0.001 ***	0.543	0.487	0.722
Observations	3,056	3,052	3,058	3,058
Liquidity constraints				
Effect of information intervention (c)	0.084 ***	0.007	0.020	0.104
	(0.022)	(0.004)	(0.021)	(0.088)
Treatment × liquidity constraints (d)	0.003	-0.010	-0.039	-0.068
	(0.042)	(0.006)	(0.038)	(0.145)
p-value of Wald test of (c)+(d)	0.015 **	0.482	0.550	0.754
Observations	3,056	3,052	3,058	3,058
Financial burden main reason for previous non-participation in training				
Effect of information intervention (e)	0.078 ***	0.004	0.014	0.091
	(0.019)	(0.003)	(0.018)	(0.075)
Treatment × financial burden as reason for non-participation (f)	0.123	-0.002	-0.081	-0.084
	(0.080)	(0.004)	(0.067)	(0.156)
p-value of Wald test of (e)+(f)	0.009 ***	0.209	0.299	0.956
Observations	3,045	3,041	3,047	3,047

Notes: The heterogeneous effects are estimated based on the cross-sectional model including an additional interaction term between the treatment indicator and the first-wave training intentions. Also, interactions between the treatment dummy and a measure of liquidity constraints is considered. Last, the treatment variable is interacted with an indicator for previous non-participants in training who mention financial costs as their main reason for non-participation. When omitting the control variables, the effects are virtually the same (see Appendix Table B-3). All regressions are estimated by OLS with robust standard errors. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The second dimension of heterogeneity, i.e., the liquidity constraints, is derived from the theoretical consideration that the cost component of the training investments is a larger obstacle for liquidity constrained individuals. The results reveal that the joint effect of the information together with its interaction term with liquidity constraints differs significantly from zero in the regression using program knowledge as the dependent variable (p-value: 0.015) but the magnitude of the knowledge differences between constrained and non-constrained individuals is modest. Furthermore, program take-up and training activities also remain unaffected by the intervention for the group of constrained individuals. A third dimension of heterogeneity is analyzed by using a question posed in the first wave to non-participants of training requesting whether the main reason for non-participation was the financial cost.[12] This question combines the general willingness to participate in training with liquidity constraints and best approximates the target group of the voucher program. Their knowledge increased by as much as 20 percentage points, suggesting once more that remembering the information is not random but rather systematically related to the relevance of the information to the individual.[13] However, once again, there are no statistically significant treatment effects on program take-up or training.

IV estimates

The previously estimated *ITT* effects answer the question of what would happen to training levels, if the information treatment – as it was implemented in the experiment – were provided to all eligible employees. We would further like to understand the effect of knowing that the program exists on training investments. Local average treatment effects (LATE) are estimated based on an IV model where in the second stage the training outcomes are regressed on program knowledge and controls. The intervention is used for instrumenting program knowledge, which is a strong instrument (in the first stage regression the F-statistic of the instrument is 20).[14] As mentioned, there could be an effect of the intervention on training participation apart from program take-up, for example, by an increase in individuals' general motivation for training or the use of other voucher programs for financing. Table 4 shows the IV regression results estimating the effect of program knowledge on the extensive and intensive margin of training participation. They document that knowing that the program exists does not affect training incidence or intensity at any statistically significant level.

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[12] Section 5 provides a more detailed description and analysis of the reasons for non-participation in training.

[13] In line with this finding, the levels-of-processing theory in the psychological literature on cognition argues that information individuals are likely to remember best is information that is actively processed for meaning (e.g. Smith and Kosslyn 2006). If information is relevant for an individual himself such processing and elaboration on its meaning is more likely.

[14] Although we would also like to provide LATE effects of program take-up on training, for instance, to assess the size of the deadweight loss of the program, these effects cannot be identified empirically because of a weak first stage (see Table 2, column 2 for an insignificant effect of the intervention on program take-up; the F-statistic of the excluded instrument in the first stage is 1.67, which is strong evidence of a weak first stage). A weak first-stage returns biased second-stage estimates (Staiger and Stock 1997).

Table 4. Effect of knowing about the existence of the program on training activities

	First stage	Second stage	
	Program knowledge	Training incidence in previous year	Training intensity in previous year
	(1)	(2)	(3)
Effect of information intervention	0.084 ***		
	(0.019)		
Effect of program knowledge		0.122	1.025
		(0.207)	(0.865)
Control variables	Yes	Yes	Yes
R^2	0.0323	0.2510	0.1516
F-test of excluded instruments	20.21 ***		
Observations	3,055	3,055	3,055

Notes: The table shows 2SLS estimates. The first column indicates the first-stage estimates. The difference in the number of observations compared with Table 2 comes from restricting the sample to observations with complete information on program knowledge, treatment status, control variables, training incidence and training intensity. All regressions are estimated by linear models with robust standard errors. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robustness of the results

One shortcoming of the previous analysis is that the reference period for measuring effects on training is the period between the first and the second interview, which covers only approximately one year, and this might be a rather short time period for detecting treatment effects. For instance, individuals participate in training because they perceive their skills have depreciated. Something like this does not necessarily occur frequently and, thus, training demands could also arise after a period of more than 12 months. To shed light on this issue, we use the second-wave information on training intentions for the next 12 months as an additional outcome variable while acknowledging that this outcome must be interpreted with care due to the subjective nature of the question. The first two columns of Table 5 summarize the ITT effects of the intervention on future training plans. Both the cross-sectional and the fixed effects regression return results that do not differ significantly from zero. The last two columns show the results of knowing about the program on future training plans, where the knowledge was instrumented with the information experiment. As before, there is no significant effect of program knowledge on future training plans, which is indicative evidence that the intervention and the increase in program knowledge as a result of it also have no effect on training activities in the medium term.

Table 5. Effect of the information treatment and of program knowledge on future training plans

	ITT results		2SLS results	
	OLS	FE	First stage	Second stage
	(1)	(2)	(3)	(4)
Effect of information intervention	0.006	0.022	0.083 ***	
	(0.015)	(0.019)	(0.019)	
Effect of program knowledge				0.069
				(0.185)
Control variables	Yes	Yes	Yes	Yes
R^2	0.2296	0.0018	0.0322	0.2245
F-test of excluded instruments			19.69 ***	
Observations	3,051	6,102	3,048	3,048

Notes: The dependent variable is future training plans in the next 12 months, measured in the second interview. ITT effects indicate the effects of regressing future training plans on treatment status in addition to controls. In the cross-sectional model in column (1), the control variables include first-wave information on gender, age, migrant, partner, children, East Germany, education dummies, previous training participation, training intentions and liquidity constraints. The fixed effects model in column (2) includes as covariates children and education dummies. Columns (3) and (4) document the first- and second-stage results of IV regressions, estimating the effect of program knowledge on future training plans. The instrument for program knowledge is the indicator for the information treatment. All regressions are estimated by linear models with robust standard errors (OLS and IV models) or with standard errors clustered at the individual level (fixed effects model). Significance levels: * $p <$ 0.10, ** $p < 0.05$, *** $p < 0.01$.

Another concern with the results could be that the time between the interviews did not always amount to exactly twelve months. Although the aim was to schedule the second interview 12 months after the first, this timing could not always be achieved because not all respondents were reachable by phone during the targeted time. The share of interviews that could be scheduled exactly 12 months later is 46 percent, which is almost the same as the share of interviews conducted 11 or 13 months after the first interview (48 percent). Only 5 percent of the second-wave interviews were administered 9, 10 or 14 months after the first one. Re-estimating the main results by additionally controlling for the time between the interviews (not shown in the tables) leaves OLS and IV results virtually unchanged (e.g., for program knowledge and take-up, training activities and future training plans). This result is in line with further descriptive analysis revealing that the treatment and the control groups are balanced with respect to the time between the interviews (t-stat: 0.71).

Finally, the previous analyses applied linear regression models, although the dependent variables are inherently binary or count data. However, the main results are robust to estimating the effects based on non-linear regression models (not shown in the tables). In particular, we re-estimated the ITT effects using probit models for program knowledge, program take-up, training incidence and future training intentions as well as using a negative binomial model for the number of training courses. The sign, magnitude and significance are similar to the main results. Additionally, a fixed effects logit model confirms the difference-in-differences results, which were estimated by linear fixed effects in the main specification.

5. Discussion

Our main results raise several questions, most importantly why the information about the program did not increase voucher take-up and, as a consequence, training. This section offers answers that are based on further descriptive analyses of the survey questions. As part of the information intervention, treated individuals were asked to assess the conditions of the voucher (see Appendix A-2 for the survey questions). Overall, the responses show that the treatment group is positive toward the idea of government financial support for training. Almost 96 percent view government training incentives in the form of BP vouchers as very good or good. Treated individuals were additionally asked whether they would be willing to participate in a 30-minute counselling session to obtain the voucher and how long they would be willing to travel to the counselling office. Two-thirds state that they would participate in counselling even when traveling takes 30 minutes or more. Individuals were also asked whether they could imagine participating in training using the BP voucher during the next 12 months and 34 percent stated that they could imagine doing so. In summary, we conclude that visiting a counselling office is not an obstacle for voucher take-up for most of the eligible employees and one-third of the respondents could imagine to use the voucher in the future. Why, then, have there been no effects of the information treatment on voucher take-up?

The purpose of the BP voucher is to relax the liquidity constraints of individuals who could not afford to finance training. To gauge whether financial restrictions have the potential to discourage employees from participation, individuals were asked in the first interview about their reasons for non-participation in the past. Specifically, non-participants in the first interview were presented a list of potential reasons of which they were asked to identify the most relevant reasons that applied to them. In the treatment and the control group alike, 15 percent of all non-participants identified the financial burden as the main reason for non-participation. This corresponds to only 5 percent of all employees eligible for the BP voucher, which we assess as being a rather small sub-group. More importantly, because voucher utilization requires self-financing of the other half of the training costs, this sub-group with low financial liquidity might not be able to afford paying the other half of the training costs that remains after deducting the voucher. Indeed, as many as 58 percent indicated at the time of the first interview that they would not be able to replace a good used daily if broken, which differs greatly from the mean of 24 percent of the entire treatment group. In summary, there is only a small group of eligible employees for whom liquidity constraints are the main reason for non-participation in training and who could afford paying the other half of the costs.

Note that 85 percent of the non-participants reported other main reasons for previous non-participation in training activities. In particular, 23 percent of the non-participants stated that they had no need for training, 19 percent mentioned health- and family-related reasons, 19 percent had no time for training, 10 percent expected low returns, 7 percent did not know which training would be useful and 7 percent reported that the training supply was insufficient. As the BP voucher is designed especially to remove financial barriers for non-participation, it is not effective to increase training participation for individuals who have other reasons for non-participation.

Another issue that we want to discuss is why we do not observe deadweight losses for those individuals with training participation in the time period between the interviews. Since almost half of the treatment group have participated in training between the first and the second wave, we would have expected to observe voucher take-up among this group of individuals to co-finance training that they would have attended even in the absence of the voucher. To determine why there is no deadweight loss, the following descriptive analysis identifies all courses that could have potentially been financed by the voucher. As mentioned previously, most training courses in Germany are at least partly employer financed, excluding them from voucher eligibility as the BP voucher exclusively subsidizes self-financed training; courses financed or provided by the employer are not covered. In the second interview, individuals report whether their training activities between the interviews involved costs for training fees and whether employers defrayed at least some of these costs. Since most training of employees is fully or partly financed by employers, the share of individuals with training participation in at least one course that involves self-financing without any co-financing by employers is only 5 percent of all treated individuals. Furthermore, vouchers cannot be applied to training that is publicly financed by another financial aid program. Individuals were also asked whether they participated in training while employed or while unemployed of whom only the former could be subsidized by the BP voucher. Restricting the sample of eligible employees to training participants who additionally fulfill these criteria leads to an overall share of individuals who could have potentially used the voucher of less than 4 percent.

When this group is further reduced to those willing to participate in the counselling despite a travel time of 30 minutes or more, the share falls to 2 percent. In absolute numbers, this share corresponds to only 28 individuals in the treatment group. 45 percent of these individuals have borne costs of less than 100 Euro and approximately 25 percent costs between 100 and 200 Euro.[15] Further deleting those individuals whose self-financed training costs were below 200 Euro leaves only 9 individuals for whom the voucher could have been an attractive option. In conclusion, we observe only a small number of eligible employees whose training activities between the first and the second interview could potentially have been subsidized by the voucher, mainly because most courses were fully or partly financed by employers. This is the main reason for not finding evidence of any deadweight losses as a consequence of the intervention. Note also that our results imply that within this group of training participants, the voucher did not lead to an increased demand for training supplementing the employer-sponsored courses. We suggest as one reason that the overall training needs of employees are already satisfied by employer-financed courses.

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[15] Using administrative data on voucher recipients, RWI et al. (2012) show that among the total population of voucher recipients from 2010, only 3 percent of voucher users participated in training with total costs of less than 100 Euro and 7 percent participated in training that cost between 100 and 200 Euro. In turn, 90 percent of the voucher recipients used the voucher for training that cost more than 200 Euro, suggesting that the likelihood of voucher take-up increases with the total amount of training costs.

6. Conclusion

This paper analyzes the effects of providing information about a training voucher program on voucher take-up and training participation. The voucher decreases a substantial amount of the training costs for employees. The analysis is conducted based on a field experiment with random assignment of the treatment group. The results suggest that the information intervention increased the treatment groups' program knowledge significantly compared with the control group. However, relieving the information constraint had no effect on voucher take-up or training activities. These results indicate that increasing eligible employees' general program knowledge, e.g., by allocating more resources to public relations work, would neither increase program take-up nor raise training levels. Because increased training levels by means of the voucher require voucher take-up at first, the results also indicate that the voucher is ineffective to increase the training participation quota of the employed population at large.

Descriptive results indicate that the reason for the inability of the voucher to stimulate training is that the financial burden is seldom the main reason for not participating in training. Many employees participate in employer-financed training with no personal financial involvement. The majority of non-participants in training report other reasons for previous non-participation such as no time, no training demands or low expected training returns. Only 5 percent of the eligible employees report the financial burden as being responsible for previous non-participation. However, for many of this small sub-group, bearing half of the training costs seems to be an obstacle for voucher take-up, as this particular group is characterized by particularly scarce financial resources. Comparing our results with those from Schwerdt et al. (2012) and Hidaldo et al. (2014) who analyze vouchers that do not necessarily require a financial contributions of the individuals and who find positive effects on voucher take-up and training suggest that paying half of the training costs might be an obstacle for increasing training levels for employees with limited financial resources.

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Appendix A

Appendix A-1. Sampling design of data

As described in section 2, the BP voucher is available to individuals with low taxable household income, if they are employed, self-employed or on maternity/parental leave. Because there is no single source of data from which to draw a sample of these individuals and for reasons of cost efficiency, the survey primarily focused on salaried employees. The survey sample was drawn from administrative records covering all employees contributing to the social security system. Defining the eligibility status of the target population, which depends on taxable household income, is not straightforward based on the information covered in the administrative records because they only include individuals' labor income. Information on marital status, partner's income and income from sources other than labor are missing. Therefore, it was instead necessary to ask all relevant questions in the survey and then decide about eligibility during the interview. To minimize the number of interviews with non-eligible individuals, the survey sample was drawn from all individuals who were employed in December 2008 and was stratified by gender and labor income; i.e., individuals with low labor income were sampled with a higher probability than individuals with high labor income. In the analysis, the stratification is accounted for by using sample weights. Note that the main results for program knowledge, program take-up and training activities are similar when using an unweighted sample (results not shown).

In the first wave in 2010, 6,075 employees were interviewed (corresponding to a response rate of 35 percent), of whom 5,019 employees were identified as eligible for the program. Eighty-nine percent of the respondents declared their willingness to be contacted for a follow-up interview. In the second wave, respondents who had declared their agreement and were eligible for the program, were contacted again. Overall, 3,110 interviews were conducted successfully, which corresponds to a response rate of 71 percent.

Appendix A-2: Questions in the experiment

Q1 "I would like to talk to you about the Bildungsprämie program. The Bildungsprämie is a voucher program whereby the government finances half of the costs incurred when you participate in training for vocational reasons. More specifically, for training costs up to 1000 Euro, the government reimburses 50 Percent of the cost. For example, if a person participates in training at a cost of 800 Euro, 400 Euro are reimbursed. How do you personally rate the idea of the government providing financial support for training participation?" (very good/partly good/partly not so good/not good at all)

Q2 "You can obtain information on the Bildungsprämie from various sources. There is a webpage 'www.bildungsprämie.info' and a toll-free hotline. You can also order a flyer that is sent to you by mail or visit one of the counselling offices or the roadshow. Assuming you would like to get more information on the Bildungsprämie, which of the sources of information would you probably use?" (yes/no; webpage, toll-free hotline, flyer, counselling office, roadshow)

Q3 "To obtain the Bildungsprämie voucher, you have to apply for it in a counselling office. Counselling takes approximately 30 minutes and verifies that you meet the eligibility criteria. If you do, the voucher is issued. Would you be willing to take part in such counselling to obtain financial support?" (yes/no)

Q4 "During counselling, individual advice on training topics that might be helpful or information on particular courses are available (if demanded by the applicant). Assuming that you would like to participate in training, do you feel sufficiently informed about training opportunities or do you require more information?" (yes, I require more information/no, I do not require more information)

Q5 "Currently, there are approximately 400 counselling offices across Germany. What is the most amount of time you would be willing to spend travelling to a counselling office?" (hours, minutes)

Q6 "Can you imagine participating in training within the next 12 months and using the Bildungsprämie voucher to benefit from the financial support?" (yes/no)

Appendix A-3. The information flyer for the treatment group

GIB rwi infas

Employment today and tomorrow

A cooperation of the research institutes:

infas
infas Institut für angewandte Sozialwissenschaft GmbH, Bonn

rwi
Rheinisch-Westfälisches Institut für Wirtschaftsforschung, Essen

GIB
Gesellschaft für Innovationsforschung und Beratung mbH, Berlin

Funded by

Bundesministerium für Bildung und Forschung

ESF Europäischer Sozialfonds für Deutschland

Europäische Union

Why we still need your support:

Since our last survey in 2010, a lot has happened. The economy for instance has regained its strength after the financial crisis in 2010. What changed for you?

For us it is important to learn, what job-related changes you have experienced since your last interview. Maybe your job tasks or employer have changed. Maybe you even find yourself in a phase of occupational re-orientation. Or perhaps the past year did not bring much change for you. All this information is needed to describe the development of the working world. In order to get a complete picture of the situation, it is particularly important that all previous respondents continue to participate in the survey.

Hence, we would kindly like to ask you to keep supporting our study by taking part in the second wave of the survey. In 2011 we will contact you again in order to ask for a phone interview.

If you have any further questions regarding the study, you can easily reach us by phone. Phone number: _____ (toll-free)

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GIB RWI infas

What is it about?

The occupational requirements demanded from each one of us, are highly different depending on the job held or the former job experience. Also changes in work-related aspects or occupational requirements occur differently. Some face regular changes; for others rather little is changing.

In order to gain a complete picture on the changes in occupational life, last year infas started to question selected people on their employment history, current occupation and working environment.

In total, roughly 6.000 individuals took part in the study "Employment today and tomorrow", conducted in 2010.

You also belonged to those individuals. We would like to take the opportunity and thank you for your participation in the study!

With this information sheet we would like to present you with some of the results from the first wave of the survey. The study will be continued in 2011. Therefore, we would kindly like to ask you to support our study with a second interview.

Who participated so far?

In 2010, infas questioned about 6.000 employed men and women in all of Germany for this study.

Many employed people are regularly confronted with new tasks. Consequently, 70% report that their tasks change frequently or sometimes. The other 30% rarely or never faces new challenges at work.

In light of the above, work-related training is of great relevance since new skills are learned and old ones are refreshed. More than half of the respondents (58%) have participated in training over the past two years. However, there are also reasons not to participate in training activities. For instance, 35% of non-participants state that the financial burden of training is simply too high.

What do you think about the funding program "Bildungsprämie"?

| 3% | 35% | 62% |

not good good very good

To financially support training participation, there are many different state funded programs. For instance we asked in the interview, whether the state funding program "Bildungsprämie" was known to the respondent. This program awards a grant of 50% of the training costs, up to a maximum of 500 euros.

Almost all respondents found the program "Bildungsprämie" to be either very good or good (see left figure).

Which source of information do you find attractive in order to inform yourself about the Bildungsprämie?

website www.bildungspraemie.info — 87%
information material (e.g. handouts) — 67%
personal counseling — 52%
information events — 48%
toll-free hotline — 42%
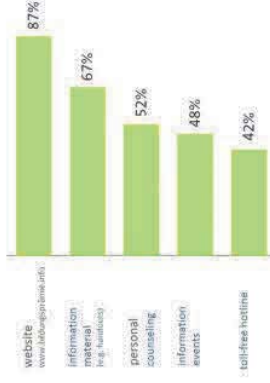
In order to inform themselves about the conditions, almost 90% of the respondents would use the website. Two third would consult handouts. About half of the respondents would like to participate in a personal counseling. Another 42% would use a toll-free hotline (see right figure). These results show what importance the internet has gained as a source of information.

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Appendix A-4. The information flyer for the control group

Why we still need your support:

Since our last survey in 2010, a lot has happened. The economy for instance has regained its strength after the financial crisis in 2010. What changed for you?

For us it is important to learn, what job-related changes you have experienced since your last interview. Maybe your job tasks or employer have changed. Maybe you even find yourself in a phase of occupational re-orientation. Or perhaps the past year did not bring much change for you. All this information is needed to describe the development of the working world. In order to get a complete picture of the situation, it is particularly important that all previous respondents continue to participate in the survey.

Hence, we would kindly like to ask you to keep supporting our study by taking part in the second wave of the survey. In 2011 we will contact you again in order to ask for a phone interview.

If you have any further questions regarding the study, you can easily reach us by phone.
Phone number: (toll-free)

A cooperation of the research institutes:

infas
infas Institut für angewandte
Sozialwissenschaft GmbH, Bonn

rwi
Rheinisch-Westfälisches
Institut für Wirtschaftsforschung, Essen

GIB
Gesellschaft für Innovationsforschung
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für Deutschland

EUROPÄISCHE UNION

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Employment today
and tomorrow

continued from previous page...

What is it about?

The occupational requirements demanded from each one of us, are highly different depending on the job held or the former job experience. Also changes in work-related aspects or occupational requirements occur very differently. Some face regular changes; for others rather little is changing.

In order to gain a complete picture of the changes in occupational life, last year infas started to question selected people on their employment history, current occupation and working environment.

In total, roughly 6.000 individuals took part in the study "Employment today and tomorrow", conducted in 2010.

You also belonged to those individuals. We would like to take the opportunity and thank you for your participation in the study!
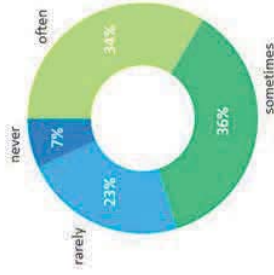
With this information sheet we would like to present you with some of the results from the first wave of the survey. The study will be continued in 2011. Therefore, we would kindly like to ask you to support our study with a second interview.

Who participated so far?

In 2010, infas questioned 6.000 employed men and women in all of Germany for this study. 84% of the employees are in full-time employment and 16% are in part-time employment. On average the respondents are 43 years old.

Many employees are regularly confronted with new challenges. Consequently, 34% report that their job tasks change often. Another 36% sometimes deal with new tasks. However, one out of three employees rarely or never faces new challenges at work (see figure). Men more frequently report to be confronted with new tasks than women.

How often are you confronted with new tasks?

From the survey-data we also receive positive news, since most respondents feel that they measure up to the tasks they are confronted with at work (see figure)

Do you feel that you measure up to your tasks at work?

All in all, 70 % of the respondents are satisfied with their work. However, the job satisfaction varies considerably by gender. While three out of four women are satisfied with their job, only 64% of men are satisfied.

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Appendix B. Additional tables

Table B-1. Summary statistics and balancing of panel dropouts

	Eligible employees in the first wave with dropout in second wave				
	Treatment group Mean	Control group Mean	Difference	t-stat	Obser-vations
Socio-demographics and training measured in the first wave					
Female	0.528	0.538	-0.010	-0.39	1 909
Age < 35 years	0.301	0.334	-0.033	-1.41	1 896
Age (35 to < 45 years)	0.265	0.246	0.019	0.84	1 896
Age (45 to < 55 years)	0.289	0.276	0.013	0.57	1 896
Age (> 55 years)	0.145	0.145	0.001	0.03	1 896
Migration background	0.246	0.271	-0.026	-1.16	1 907
Cohabiting with partner	0.724	0.724	0.001	0.03	1 909
Children	0.370	0.369	0.002	0.07	1 909
East Germany	0.255	0.254	0.001	0.06	1 909
College	0.136	0.160	-0.024	-1.23	1 898
Vocational education	0.759	0.747	0.012	0.54	1 898
Compulsory education	0.104	0.093	0.012	0.80	1 898
Training participation in previous year	0.378	0.363	0.014	0.57	1 906
Future training plans (next 12 months)	0.207	0.225	-0.018	-0.82	1 901
Liquidity constraints	0.335	0.327	0.008	0.35	1 886
Program knowledge measured in first wave, before providing the information treatment					
Knowledge about the *Bildungsprämie*	0.2380	0.2292	0.009	0.41	1 903

Notes: The table shows summary statistics of individuals dropping out of the panel after the first interview separately for the treatment and the control group. Difference shows the differences in the means by treatment status and the t-stats indicate their significance. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table B-2. Differential attrition and balancing tests based on multivariate regressions

	Dependent variables		
	Panel dropout	Treatment indicator (1-yes, 0-no)	
		First-wave sample	Both-wave sample
	(1)	(2)	(3)
Information intervention	0.020		
	(0.015)		
Female	0.002	0.015	0.030
	(0.015)	(0.016)	(0.021)
Age < 35 years	Reference Group	Reference Group	Reference Group
Age (35 to < 45 years)	-0.090 ***	0.014	-0.006
	(0.022)	(0.023)	(0.031)
Age (45 to < 55 years)	-0.143 ***	-0.007	-0.034
	(0.021)	(0.022)	(0.028)
Age (> 55 years)	-0.147 ***	0.002	-0.015
	(0.026)	(0.027)	(0.035)
Migration background	0.059 ***	-0.021	-0.020
	(0.019)	(0.020)	(0.026)
Cohabiting with partner	-0.044 **	-0.004	-0.002
	(0.012)	(0.020)	(0.026)
Children	-0.020	0.017	0.025
	(0.018)	(0.019)	(0.024)
East Germany	0.037 **	-0.017	-0.031
	(0.018)	(0.019)	(0.024)
College	-0.165 ***	-0.028	0.016
	(0.034)	(0.036)	(0.048)
Vocational education	-0.107 ***	-0.022	0.004
	(0.030)	(0.030)	(0.043)
Compulsory education	Reference Group	Reference Group	Reference Group
Training participation in previous year	-0.022	0.013	0.004
	(0.017)	(0.018)	(0.022)
Training planned in next 12 months	-0.027	-0.030	-0.028
	(0.019)	(0.020)	(0.025)
Liquidity constraints	0.062 ***	0.017	0.021
	(0.017)	(0.035)	(0.023)
R^2	0.0413	0.0024	0.0040
F-statistic	13.27	0.74	0.81
p-value of F-test	0.000 ***	0.720	0.655
Observations	4,914	4,914	3,059

Note: The dependent variables are indicated in the column headings. Covariates are measured in the first wave. All results were estimated using the linear probability model. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B-3. Re-estimation of main results excluding all control variables

	Outcomes measured one year following the experiment			
	Program knowledge	Program take-up	Training incidence in previous year	Training intensity in previous year
	(1)	(2)	(3)	(4)
Panel A: Cross-sectional model				
Effect of information intervention	0.084 ***	0.004	0.005	0.061
	(0.019)	(0.003)	(0.020)	(0.079)
Observations	3,107	3,103	3,109	3,109
Panel B: Fixed effects model				
Effect of information intervention	0.086 ***		0.005	
	(0.021)		(0.021)	
Observations	6,211		6,212	
Panel C: Heterogeneous effects				
Training intentions				
Effect of information intervention (a)	0.068 ***	0.004	0.018	0.075
	(0.021)	(0.004)	(0.022)	(0.073)
Treatment × training intentions measured in the 1st wave (b)	0.066	0.000	-0.019	0.039
	(0.045)	(0.007)	(0.040)	(0.203)
p-value of Wald test of (a)+(b)	0.001 ***	0.507	0.961	0.546
Observations	3,100	3,096	3,102	3,102
Liquidity constraints				
Effect of information intervention (c)	0.088 ***	0.007	0.016	0.093
	(0.022)	(0.004)	(0.024)	(0.096)
Treatment × liquidity constraints (d)	-0.009	-0.011	-0.034	-0.087
	(0.042)	(0.006)	(0.044)	(0.160)
p-value of Wald test of (c)+(d)	0.027 **	0.431	0.626	0.961
Observations	3,087	3,083	3,089	3,089
Financial burden main reason for previous non-participation in training				
Effect of information intervention (e)	0.079 ***	0.004	0.010	0.069
	(0.019)	(0.003)	(0.021)	(0.082)
Treatment × financial burden as reason for non-participation (f)	0.104	-0.004	-0.097	-0.162
	(0.082)	(0.003)	(0.068)	(0.154)
p-value of Wald test of (e)+(f)	0.021 **	-	0.183	0.478
Observations	3,095	3,091	3,097	3,097

Notes: The cross-sectional models regress the second-wave outcomes on a treatment indicator. The heterogeneous effects are estimated based on the cross-sectional model, including an additional interaction term between the treatment indicator and, first, the training intentions in the first wave, second, a measure of liquidity constraints and, third, an indicator for previous non-participants in training who mention financial costs as the main reason for non-participation. The fixed effects model includes no covariates apart from the treatment indicator. Standard errors are robust (in the cross-sectional models) or account for clustering at the individual level (in the fixed effects models). Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.