

Temporary contracts and employee effort

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Abstract

Temporary contracts provide employers with a tool to screen potential new employees and have been shown to provide “stepping stones” into permanent employment for workers. For both reasons, workers on temporary contracts have an incentive to provide more effort than permanent employees. Using indicators for unpaid overtime work and absences taken from the Swiss Labor Force Survey (SLFS), we present evidence that temporary workers indeed provide higher effort than permanent employees: Their probability of working unpaid overtime exceeds that of permanently employed workers by 60%. We show the heterogeneity of this effect across different types of temporary contracts, investigate differences between men and women, and discuss the relevance of endogenous selection into temporary employment.

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1. Introduction

An important characteristic of employment contracts is whether they are permanent or temporary. Since temporary employment as a share of total employment recently has risen in a number of countries, it has created increased scientific attention (cf. [OECD, 2002](#); [Booth et al., 2002a](#)). However, researchers mostly address questions relating either to the macroeconomic impact of temporary employment on unemployment and labor turnover,¹

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¹ See, e.g., [OECD \(1999\)](#), [Cahuc and Postel-Vinay \(2002\)](#), [Dolado et al. \(2002\)](#), [Holmlund and Storie \(2002\)](#), and [Blanchard and Landier \(2002\)](#).

or to differences of job (e.g., wage, training) and worker characteristics in permanent versus temporary contracts.²

This appears to be the first study to investigate whether there are measurable behavioral responses connected to holding permanent versus temporary employment contracts. The incentives behind such responses are hypothesized to derive from the character of temporary jobs as potential “stepping stones” to generally preferable permanent jobs, a role that is confirmed in empirical studies:³ If temporary employment can offer access to desirable permanent contracts, temporary employees have an incentive to display high levels of effort.

The issue of behavioral responses to temporary contracts is related to an empirical literature which confirms contract-related incentives in a variety of other areas. The studies look at effort responses to probation and employment protection, analyze the response of absenteeism to sick-leave regulations, and describe responses to monitoring intensity.⁴

Using data from the Swiss Labor Force Survey (SLFS), we compare the behavior of individuals in permanent and temporary contracts. The SLFS—a rotating panel data set—contains information on workers’ current contract and on effort indicators such as absenteeism and unpaid overtime work. We investigate the overall evidence for incentive effects of temporary contracts as well as their heterogeneity across effort indicators and worker groups. Particular attention is devoted to possible gender differences in the response to contractual incentives as well as to the potential bias related to non-random selection into temporary employment.

This study adds to the literature in several regards. First, it continues the micro-level analysis of temporary contracts initiated by Booth et al. (2002b). That study examined whether temporary contracts deserve to be considered as “stepping stones.” The authors confirm for the United Kingdom that a large proportion of workers on temporary contracts move on to permanent contracts with higher wages and fringe benefits. They also show that high effort among temporary workers is positively correlated with the probability of career advancement.⁵

Second, we provide empirical evidence of the extent of contract-related incentive effects, an issue neglected in prior discussions. We carefully describe the behavior of individuals employed in different types of temporary contracts to clarify that there are important heterogeneities. Finally, we present evidence for the interesting case of Switzerland, a country similar to the United Kingdom and the United States in that employment protection is limited.

Our results show that temporarily employed individuals are significantly more likely to provide unpaid overtime work. On average, their overtime work propensity exceeds that of permanently employed workers by 60%. The findings are robust to changes in sample and specifications, and yield no clear differences in the behavioral responses of males and females. We investigate the relevance of endogenous selection into temporary employment and describe the heterogeneity of our findings across different types of temporary employment.

² See, e.g., Morris and Vekker (2001), OECD (2002), and Segal and Sullivan (1997).

³ A summary of that literature is provided by OECD, 2002, pp.160–161.

⁴ See, e.g., Ichino and Riphahn (2001), Barmby et al. (1991), and Nagin et al. (2002).

⁵ Hagen (2003) also confirms the stepping stone hypothesis using German data.

2. Institutions, prior evidence, and hypotheses

Compared to other developed countries, the labor market in Switzerland is left quite unregulated. Employment protection in permanent contracts is weak, as even after 8 years of tenure layoff notice periods are only 2 months, reaching the maximum of at least 3 months after 9 years of tenure.⁶ Given the limited employment protection for permanent contracts, it may be less likely in Switzerland compared to other countries that temporary contracts are used as buffers against cyclical fluctuations by employers. Temporary contracts can extend over many years without limitations, and only must grant a layoff notice period of 6 months when they exceed a duration of 10 years. If temporary contracts are used to circumvent the stricter regulations of permanent contracts the temporary contracts will be looked at as permanent contracts. However, given the very limited extent of regulation for permanent contracts in Switzerland, there is little reason to use temporary contracts to avoid the additional regulation.⁷

Since the incentive effect of temporary contracts on worker effort has rarely been investigated before, we review related studies and then discuss our hypotheses. [Guadalupe \(2003\)](#) seems to be the first to address the different types of incentives following from permanent versus temporary contracts: While she shows that temporary contracts cause significantly higher accident rates, we investigate contract effects on employee effort. Generally, the effect of contract-related incentives on employee behavior has found attention in the empirical literature on the role of employment protection as well as in research on sick-pay-related moral hazard.

The studies evaluating the incentive effects of employment protection utilize institutional regulations to identify the relevant effects. [Ichino and Riphahn \(2001\)](#) find strong increases in absenteeism among Italian bank employees when probation periods end and employment protection sets in. [Riphahn and Thalmaier \(2001\)](#) confirm similar absenteeism responses to the end of probationary contracts among German employees. [Jimeno and Toharia \(1996\)](#) investigate the effect of low employment protection in Spanish temporary employment on absenteeism: Those without employment protection provide significantly higher levels of effort compared to workers in secure permanent contracts.

The hypothesis that sick-pay provides incentives for opportunistic behavior has been confirmed in a broader literature. [Barmby et al. \(1991\)](#) show that sick-pay regulations yield clear employee responses. [Johansson and Palme \(1996, 2002\)](#) describe the decline in absenteeism among Swedish men after a reduction of sick-pay, and [Barmby \(2002\)](#) finds that workers reduce absences more when sick-pay is low relative to earnings.

Our study analyses effort responses which are not necessarily driven by labor market regulations but are related to the possibly purely market-driven duration of contracts. These can be evaluated by comparing the behavior of workers on temporary and

⁶ The [OECD \(1999\)](#) provides a ranking of the overall strictness of employment protection for regular employment for the late 1980s and the late 1990s. Out of 27 countries, Switzerland ranks fifth (after the United States, the United Kingdom, Canada, and Australia) in both periods, rendering it one of the countries with the lowest employment protection.

⁷ In his textbook on Swiss labor laws, [Rehbinder \(2002\)](#) even points out that the liberal dismissal rules do not meet the more restrictive requirements of the European Union and similarly contradict Agreement 158 of the International Labor Organization, which requires stricter employee protection.

permanent contracts. As long as temporary employment contracts are less attractive than permanent ones temporary workers have an incentive to provide high levels of effort.⁸ A first reason is that firms tend to use temporary employment to screen potential permanent employees prior to committing to a binding contract. So if temporary workers want to obtain a permanent employment contract with typically higher wages, they need to pass this employer screening.

Second, there is considerable direct evidence that temporary contracts serve as stepping stones into more attractive employment contracts—also with other than the current employer. Booth et al. (2002b) confirm for their UK sample that over a period of 7 years, about 38% of all workers observed on fixed-term contracts move on to permanent employment after the fixed-term contract expires.⁹ Interestingly, the authors confirm a positive correlation between a worker's effort as measured by the number of unpaid hours of overtime work, and the probability of moving on to a permanent contract.

However, these incentive mechanisms may not characterize all types of temporary contracts in the same way. Booth et al. (2002b) distinguish jobs that are temporary by nature (seasonal and casual employment) from those that could just as well be filled by permanent employees (fixed term contracts). Employees in these contract categories differ significantly in their transition rates to permanent employment, in the long-run wage effects of past temporary employment, as well as in wages and job satisfaction.

Another interesting issue concerns the difference between male and female temporary workers. Booth et al. (2002b) argue—similar to Lazear and Rosen (1990)—that women may seek temporary employment because this better matches their high propensity to move on to non-market employment. Therefore, women may more frequently self-select in non-screening types of temporary employment than men. This would suggest that women in temporary employment *ceteris paribus* may provide less effort than men as men are more likely to seek career advancement.

Additional aspects may affect behavioral differences between the sexes: Discrimination might influence the selection of individuals into fixed term versus permanent jobs. If for given ability men are more likely to find permanent employment, then—on average—women in temporary employment should be of higher ability. If high ability is correlated with a career orientation then these high ability women may respond to incentives and provide additional effort.¹⁰ In this case, women in temporary employment *ceteris paribus* may provide more effort than men. However, the conclusion that the experience of discrimination results in the subsequent provision of high effort may not be convincing: If high ability has not been rewarded in the past high effort now may be in vein as well.

⁸ OECD (2002) estimates show significant wage penalties for temporary employment in regressions that control for individual and job characteristics, separately for 13 European countries. This finding is representative of the literature. We investigated the wage difference between permanent and temporary workers in Switzerland and confirmed the disadvantage of temporary workers independent of the chosen empirical specification. In a linear random effects estimation, the wage disadvantage of otherwise identical workers with temporary contracts amounted to 9% (without random effect controls even 15%).

⁹ In our data, 26% of those temporarily employed in 1 year move on to permanent employment in the next period.

¹⁰ Booth et al. (2002c) also discuss active discrimination on the part of the employer as an explanation for differences between the sexes.

These are three gender-specific mechanisms where the first and the last should cause lower and the second higher effort among women compared to men. Below, we investigate the empirical evidence on this question.

Overall, our empirical analysis seeks to test the following hypotheses:

H1. Workers in temporary employment provide more effort compared to workers in permanent contracts.

H2. Temporary workers on different types of temporary contracts show different levels of effort.

H3. Males and females provide different levels of effort in response to temporary employment incentives.

3. Data and empirical strategy

3.1. Data set and sample

The data for our analysis are taken from six annual waves (1996–2001) of the SLFS. The SLFS is a telephone survey among randomly chosen individuals aged 15 and above. It does not cover foreign workers without a permanent residence permit. The typical annual survey samples 16,000–18,000 households. The SLFS is a rotating panel where every individual is interviewed up to five times. The questionnaires cover socio-demographic indicators, employees' type of contract, and measures of effort.

Pooling the observations from six annual surveys yields 103,005 observations. We restrict our sample to non-self-employed individuals who are in full-time employment at the time of the annual interview and who are not in an apprenticeship or in military service. We drop individuals of retirement age (men above age 65, women above 63) and a few observations with missing values on key variables. Our sample covers 33,945 person-year observations, representing 10,497 different male and 5411 different female workers, with on average 2.1 annual observations per person.

3.2. Key dependent and explanatory variables

We consider two effort indicators as dependent variables, which are established in this literature: The first measures whether the worker provides unpaid overtime hours,¹¹ the second describes whether a person missed work for the entire week prior to the survey due to illness, accident, personal, or family matters, or "other" reasons.¹² Both measures are binary variables.

¹¹ Respondents are asked whether it happens frequently or at least sometimes that they have to work longer hours than spelled out in their contract. If they agree a subsequent question asks whether this is typically remunerated financially, in leisure, or not at all. Those indicating that overtime work is typically not remunerated provide unpaid overtime work.

¹² Similar indicators were used by Booth et al. (2002b) in the case of overtime and Jimeno and Toharia (1996) for absences.

The key explanatory variable indicates whether the individual works on a temporary contract. Of all employees in our sample, 4.4% represents those who are temporarily employed. This figure differs from the about 10% share presented by OECD (2002) for Switzerland; the difference is most likely due to the different selection of the samples.¹³ Based on the sampling rules applied by OECD (2002), Switzerland's temporary employment share is at the OECD average. While some countries experienced severe fluctuations in the share of their temporary employment over time, the Swiss figure remained relatively stable during the 6 years of our data between a maximum of 4.9% in 1996 and the lowest value of 3.7% in 2000.

Based on the SLFS information on the type of temporary contract, we distinguish five contract types. The contract types describe (i) seasonal and casual employment, (ii) public sector sponsored employment programs, (iii) internships, and (iv) “advanced temporary contracts” combining temporary substitute workers, fixed term project work, probationary employment, temporary employment agency workers, and “other” temporary contracts.¹⁴ Public sector sponsored programs are designed to reintegrate previously unemployed workers in the labor market. Internships are temporary contracts with typically low or no pay where the intern intends to learn about the workings of a company or agency. Contract type (v) describes observations with missing contract type information. We expect that the incentive to provide effort is highest among those with the highest probability of company screening, i.e., categories (iii) and (iv), and lowest among groups (i) and (ii) where workers are either unlikely to reapply for employment or where promotions are not possible.

Table 1 presents the characteristics of individuals employed in permanent and temporary contracts. It yields differences in a variety of dimensions: Temporary work is more frequent among the young, among females, non-married, and foreign workers. The educational patterns are nonlinear in that temporary workers are more likely to be in the lowest and the highest educational categories compared to those employed permanently. Despite conditioning on full-time employment, we find a higher number of weekly hours worked (contractual and overtime) for temporary workers. The distribution of workers across firm size categories does not seem to differ by contract type, but permanent workers have much longer tenure.¹⁵

Given the availability of panel data for some of our observations, it is interesting to measure the probability of remaining in temporary employment over time. The period-to-period probability of leaving temporary employment for a permanent job amounts to 26% in our sample. The reverse probability of leaving permanent employment for a temporary job is low at 0.9%.¹⁶

Given that permanent and temporary contracts differ in a number of dimensions, it is interesting to describe effort levels by contract type. The first row in Panel A of Table 2

¹³ The OECD (2002, Chart 3.1) measures include part-time workers, apprentices, and self-employed workers, and do not seem to impose age restrictions.

¹⁴ Varying formulations of the contract categories across the six annual questionnaires did not allow us to describe these contract types more finely in a consistent manner over time.

¹⁵ The large share of temporary workers with tenure beyond 5 years is surprising. However, there are no clear patterns in the data suggesting that this is a special group.

¹⁶ Both figures represent lower bounds of the true shares because we do not observe the transitions of those individuals who drop out of the survey due to panel rotation.

Table 1

Worker characteristics by type of contract (in percent of column total)

	Permanent	Temporary
<i>Sociodemographic characteristics</i>		
Age group 16–25	9.63	27.66
Age group 26–40	47.54	46.48
Age group 41–65	42.83	25.85
Female	30.48	42.33
Married	49.70	31.75
Foreigner	16.15	22.37
Health problems	1.35	1.21
Education—none or basic	12.17	15.00
Education—medium	55.79	37.31
Education—advanced school	5.75	13.26
Education—advanced vocational training	17.13	7.57
Education—academic	9.15	26.86
<i>Employment characteristics</i>		
Regular weekly working hours	41.81	42.39
Average weekly paid overtime hours (last calendar year)	2.42	1.84
Average weekly unpaid overtime hours (last calendar year)	5.85	6.76
Firm size, 1–11 employees	28.57	29.81
Firm size, 20–99 employees	29.41	24.45
Firm size, >99 employees	40.30	39.99
Firm size, missing information	1.71	5.76
Tenure, <5 years	40.12	78.83
Tenure, 5–15 years	36.40	12.32
Tenure, >15 years	23.49	8.84
Number of observations	32,452	1,493

indicates that 20.6% of all those employed on permanent contracts and 27.7 of those on temporary contracts have worked unpaid overtime. The first row in Panel B shows that the absence rate among those in permanent contracts exceeds that among temporary workers

Table 2

Unpaid overtime work and absenteeism by type of contract and sex (in percent)

	Contract		Type of temporary contract				
	Permanent	Temporary	Seasonal and casual	Public program	Internship	Advanced	Missing information
<i>(A) Probability of providing unpaid overtime work (in percent)</i>							
All	20.62	27.66	17.62	8.99	22.33	32.71	32.43
Males	21.94	30.43	14.52	7.02	32.22	35.90	34.09
Females	17.62	23.89	22.09	12.50	15.20	27.86	30.00
<i>(B) Probability of absence in the week prior to the survey (in percent)</i>							
All	1.15	0.80	0.95	2.25	0.47	0.55	2.70
Males	1.06	0.58	0.81	1.75	0.00	0.55	0.00
Females	1.35	1.11	1.16	3.13	0.80	0.56	6.67
Number of observations	32,452	1493	210	89	215	905	74

Table 3
Descriptive statistics of explanatory variables

Variable	Male		Female	
	Mean	Standard deviation	Mean	Standard deviation
<i>Key explanatory variables</i>				
Temporary contract (0/1)	0.037	0.188	0.060	0.238
Temporary contract—seasonal and casual (0/1)	0.005	0.073	0.008	0.090
Temporary contract—public program (0/1)	0.002	0.049	0.003	0.055
Temporary contract—internship (0/1)	0.004	0.062	0.012	0.108
Temporary contract—advanced (0/1)	0.023	0.151	0.034	0.181
Temporary contract—missing information (0/1)	0.002	0.043	0.003	0.053
<i>Sociodemographic characteristics</i>				
Age in years	40.211	10.862	36.930	11.471
Age group 16–25 (0/1)	0.073	0.260	0.174	0.379
Age group 26–40 (0/1)	0.479	0.450	0.467	0.499
Age group 41–65 (0/1)	0.448	0.497	0.359	0.480
Married (0/1)	0.597	0.491	0.250	0.433
Foreigner (0/1)	0.163	0.369	0.167	0.373
Health problems ^a (0/1)	0.014	0.117	0.012	0.111
Education—none or basic (0/1)	0.103	0.304	0.167	0.373
Education—medium (0/1)	0.536	0.499	0.580	0.494
Education—advanced school (0/1)	0.043	0.203	0.100	0.301
Education—advanced vocational training (0/1)	0.207	0.405	0.077	0.267
Education—academic (0/1)	0.110	0.314	0.074	0.262
<i>Employment characteristics</i>				
Firm size, 1–11 (0/1)	0.272	0.445	0.318	0.466
Firm size, 20–99 (0/1)	0.304	0.460	0.265	0.442
Firm size, >99 (0/1)	0.411	0.492	0.384	0.486
Firm size, missing information (0/1)	0.013	0.113	0.032	0.177
Tenure (in years)	10.495	9.715	7.552	7.849
Tenure, <5 years (0/1)	0.379	0.485	0.507	0.500
Tenure, 5–15 years (0/1)	0.361	0.480	0.337	0.473
Tenure, >15 years (0/1)	0.261	0.439	0.157	0.363
Number of observations	23,423		10,522	

^a Descriptive statistics are calculated based on non-missing indicators only. The missing values for the first three surveys, when the question was not asked, were set to zero.

by about 44%. Thus, the aggregate measures confirm our expectation of higher effort among temporary employees. These differences are similar when we evaluate the evidence by sex, where generally men appear to provide higher levels of effort than women.

The last five columns of Table 2 describe mean effort by type of temporary employment contract. For both indicators, we find the highest effort levels among those in contract categories (iii) and (iv).¹⁷ Also when disaggregating by sex effort seems to be lowest among those in public programs.

¹⁷ The high effort levels of workers in the last column of Table 2 with missing contract type information are surprising, but may be correlated with the high educational level in this group (figures not presented).

3.3. Empirical strategy

The purpose of our analysis is to test the hypotheses derived in Section 2. Hypothesis 1 (H1) claims that workers in temporary contracts provide more effort than those permanently employed. This is tested by regressing our effort outcomes on an indicator of temporary employment in random effects probit models. To ensure that the measured outcomes are not due to composition effects, the model considers control variables describing the individual worker (age, sex, marital status, nationality, health, level of education), the job (tenure, firm size, industry, occupation), and survey year effects. Table 3 provides descriptive statistics of the explanatory variables. We provide the results of random effects probit estimates when the control for individual-specific unobserved heterogeneity improved the model, and discuss standard probit results with robust standard errors otherwise.

In step 2 of the analysis, we test H2, which proposes that effort responses of temporary workers should vary by type of contract. We expect higher effort among interns and those on advanced temporary contracts. This is formally tested by substituting a set of type of contract indicators for the overall indicator of temporary employment in our models.

The last step of the analysis investigates differences in responses to incentives by gender. We first add interactions between the temporary contract and female sex indicators and then discuss more refined estimation approaches.

4. Results

The results of the “baseline” regressions for the overtime and absence indicators are presented in Table 4. The random effects control significantly improved the model fit in the overtime model, only. Overall, the regression results are robust to the choice of the estimator.

The temporary contract indicator yields the expected coefficient in the model for overtime work: Individuals on temporary jobs are significantly more likely to work unpaid overtime hours than individuals on permanent contracts. In fact, controlling for the other variables the difference in the predicted probabilities even exceeds that presented in Table 2: The regression suggests that holding everything else constant the probability of overtime work under a temporary contract exceeds that under a permanent contract by 60.49% (see bottom row and notes of Table 4). This compares to a 33.85% difference in the unadjusted data in Table 2.¹⁸

The other results indicate that the propensity to work unpaid overtime increases significantly with age and education, it is higher for males, Swiss nationals, for married individuals, and for those free of grave health shocks.¹⁹ Also, overtime is significantly

¹⁸ The SLFS offers information on the average number of unpaid overtime hours worked during the last calendar year. To test the robustness of our results, we ran linear regressions on this outcome for the subsample of workers with at least 1 year of tenure, i.e., those who were employed under the same contract during the observation period: The results confirm that the temporarily employed provide significantly more unpaid hours of overtime work than those on permanent contracts.

¹⁹ The measure for a health shock is available only for the 1998–2001 surveys and describes whether a worker has ever experienced a health based work absence of more than half a year.

Table 4

Probit estimates of the determinants of effort choice

	Unpaid overtime work		Absence	
	Coefficient	Standard error	Coefficient	Standard error
Temporary contract	0.241	0.065***	−0.132	0.114
<i>Sociodemographic characteristics</i>				
Age group 26–40	0.008	0.056	0.051	0.079
Age group 41–65	0.320	0.063***	0.246	0.083***
Female	−0.285	0.042***	0.167	0.053***
Married	0.186	0.036***	0.044	0.047
Education—medium	0.291	0.063***	−0.182	0.054***
Education—advanced school	1.049	0.086***	−0.284	0.112**
Education—advanced vocational training	0.984	0.072***	−0.361	0.081***
Education—academic	1.538	0.080***	−0.595	0.130***
Health problems	−0.231	0.131*	1.058	0.082***
Foreigner	−0.298	0.050***	0.164	0.051***
<i>Employment characteristics</i>				
Tenure (in years/10)	−0.016	0.044	−0.068	0.060
Tenure ² (in years ² /100)	2.326	1.249*	1.301	1.647
Firm size, 20–99 employees	−0.144	0.038***	0.035	0.053
Firm size, >99 employees	−0.328	0.039***	−0.007	0.055
Firm size missing information	−0.150	0.105	0.109	0.128
<i>Fixed effect controls</i>				
Occupation dummies (10)	Yes	—***	Yes	—***
Industry dummies (5)	Yes	—***	Yes	—**
Yearly dummies (5)	Yes	—	Yes	—
Constant	Yes	—***	Yes	—***
Rho	0.628	0.011***	—	—
Estimator	Random effects	Probit, robust		
	probit	standard errors		
Log likelihood	−13,427.2	−1926.7		
Predicted effect of temporary contract	60.49	−30.94		
Number of observations	33,945	33,915		

The predicted effect of holding a temporary employment contract reflects the ratio of the outcome probability when the temporary contract indicator is set to 1 relative to when it is set to 0, minus 1:

$$[\text{Pr}(\text{Effort high}|x, \text{Temp}=1)/\text{Pr}(\text{Effort high}|x, \text{Temp}=0)] - 1.$$

The asterisks for the fixed effects controls indicate the joint significance of these measures.

There are only 33,915 observations for the absence model, as 30 observations were dropped due to collinearity.

* Represents statistical significance at the 10% level.

** Represents statistical significance at the 5% level.

*** Represents statistical significance at the 1% level.

and positively correlated with tenure and negatively with firm size, and it varies significantly by occupation and industry.

In contrast to the estimation of overtime work, the model for absences does not yield a statistically significant difference by contract status. While the negative coefficient indicates a lower absence probability for those on temporary contracts, the coefficient is insignificant. This finding is robust across specifications and estimators, and remains unchanged when more detailed indicators and interactions of the temporary work indicator are considered.²⁰

This same result of no significant difference in absence rates of temporary and permanent workers was found for the case of Spain by Jimeno and Toharia (1996). However, they could show that composition effects determined this outcome: Since temporarily employed workers were employed in industries and occupations with higher accident rates, these accident related absences affected the overall comparison between permanent and temporary workers. When the authors controlled for occupation and/or industry specific accident rates, the overall absence rates showed significant differences between permanent and temporary employees. In order to test whether such compositional effects possibly explain the lack of a significant difference in the absence rates of permanent and temporary workers in our case as well, we obtained industry-specific accident rates for Switzerland.²¹ The regression of absence probabilities in Table 4 was then reestimated considering controls for industry-specific accident rates. This additional set of controls—even though significant predictors of absence probabilities—did not affect the coefficient estimate for temporary contracts. Independent of whether we considered the industry and occupational controls with the accident rates, there was no significant difference in the absence propensity of temporary and permanent workers in our data.²²

We conclude that in our data temporary workers differ from permanently employed colleagues with respect to unpaid overtime work but not regarding absences. This can be interpreted to mean that either temporary workers do not make a special effort to avoid absences or permanent workers miss work already at very low levels. By international comparison, Swiss absenteeism rates are indeed very low. Barnby et al. (2002) compare absence rates for nine European countries: Swiss workers are the least likely to miss work. The Swiss absence rate of 1.8 is far below the international average of 3.2. This suggests

²⁰ Given the very small share of absence events in the data the imprecise estimate may be due to the small number of observations in the temporary contract group (only 12 temporary workers experienced a work absence). Since logit and probit models generate different results when there is an extreme split in the dependent variable, we estimated the models with logit estimators as well. The outcome of an insignificant difference for the absence outcome between temporary and permanent contract workers remains robust.

²¹ The “Swiss collecting agency for the statistics of the accident insurance” provides information on accident rates (number of accidents per 1000 insured workers) by industries covered by private and public accident insurances. We obtained the data for the year 1999—about the middle of the period we are investigating—and grouped them in 16 industries, which could then be matched to our data.

²² Guadalupe (2003) shows that differences in the accidents between temporary and permanent workers may not only be due to composition effects across industries and occupations, but that there may be structural reasons why temporarily employed workers suffer more accidents than permanent employees in the same industry and occupation. Since we do not have contract type specific accident rates available for Switzerland we cannot control for such effects. This might be the reason why we find no difference in the absence rates by contract type.

that in Switzerland, absenteeism is not a dimension in which temporary workers can signal their motivation. We therefore focus our further analysis on the overtime work indicator for which Hypothesis 1 cannot be rejected.

4.1. Robustness tests

In this section, we discuss tests of the robustness of our results to changes in sample and specification. One could, for example, imagine that there are selection effects taking place over the course of the temporary contract. If the temporary contract serves as a screening tool for the employer then it seems plausible that workers who represent bad matches are dismissed earlier than those who are good matches. In that case, one might expect different effort levels of temporary workers depending on their tenure in the temporary contract: Those with longer tenure might be a positive selection. To investigate whether the effort effect varies with tenure we estimate the temporary contract coefficient when using only subsets of employees. The results are summarized in Table 5 (rows 2 and 3) and suggest positive and significant effects for temporary employees with short tenure under 1 or under 3 years. However, based on the simulations in the last column of Table 5 the contract-related effect seems indeed to be larger for those temporary workers with longer tenure. This may be due to match selection as mentioned above but could also be related to the term structure of alternative types of temporary contracts (cf. Section 4.2).

In row 4 of Table 5, we provide the results of an estimation where only individuals with at least 1 year of tenure are considered. The rationale is that we may observe workers on permanent contracts who were recently hired and are still in the probationary period of their contract. During probation, they can be fired easily with only 1 week's notice, and, thus, they are subject to similar behavioral incentive mechanisms as workers on temporary

Table 5
Robustness tests of the role of contract type for unpaid overtime work: coefficient estimates in random effects probit estimates for different subsamples and specifications

	Coefficient	Standard error	Predicted effect of temporary contract
1. Baseline results from Table 4: Temporary contract ($N=33,945$)	0.241	0.065***	60.49
2. Subsample with tenure less than 1 year: Temporary contract ($N=4683$)	0.162	0.085*	32.41
3. Subsample with tenure less than 3 years: Temporary contract ($N=10,412$)	0.236	0.078***	54.03
4. Subsample with tenure more than 1 year: Temporary contract ($N=29,262$)	0.306	0.090***	84.73
5. Sample as in Table 4, model without occupation and industry controls: Temporary contract ($N=33,945$)	0.197	0.067***	47.10

All models control for the same set of covariates as in Table 4.

* Represents statistical significance at the 10% level.

*** Represents statistical significance at the 1% level.

contracts.²³ If workers on probation are among the permanent employees this may cause a downward bias for the effect of temporary contracts. By dropping observations with less than 1 year of tenure, we circumvent this problem and indeed obtain a clear and sizeable effect of temporary contract incentives exceeding the baseline result.

A plausible objection to our specification in Table 4 could be that the selection into occupations and industries which are controlled for in Table 4 are endogenous. Therefore, we reestimated the model in Table 4, this time omitting occupation and industry effects. Row 5 in Table 5 yields that the coefficient of the temporary contract indicator remains highly significant and positive when the specification of the model is changed. Overall, our results are highly robust to changes in sample and specification.

4.2. Heterogeneity of effort responses

Next, we investigate the heterogeneity of effort responses across different types of temporary employment. The estimation results are presented in Table 6. The categorical indicators of the type of contract are jointly significant at the 1% level. The coefficients confirm our expectations in that interns and workers on advanced temporary contracts are more likely to provide extra effort with a highly significant effect for the latter. The predicted effects indicate an increase in the probability of overtime work by 2.3 and 4.6 percentage points for interns and “advanced” temporary workers, respectively. These are sizeable effects given a baseline probability of overtime work of 6.5%.²⁴ Even when controlling for covariates, workers on public programs are less likely to work overtime.²⁵ Thus, Hypothesis 2 cannot be rejected; there is significant heterogeneity in the effort response of temporary workers.

4.3. Differences by gender

Finally, we investigate potential differences between the sexes as suggested by Hypothesis 3. The baseline model for unpaid overtime work shows that among all workers, females are less likely to provide unpaid overtime work than men (see Table 4). We are interested in the differences between temporary contracts and reestimated the model after adding an interaction for temporary contract and female sex. The result (see Table 7, column 1) yields an imprecisely estimated coefficient of small magnitude. Thus, we find no significant difference between the sexes.

In Section 3, we discussed alternative hypotheses regarding the differences in male–female effort responses to working on temporary contracts. If both factors causing higher and lower effort are effective, the net effect might be that females do not differ from males.

²³ For more detailed analyses of the incentive effects of probation periods, see Ichino and Riphahn (2001) or Riphahn and Thalmeier (2001).

²⁴ The results of higher effort among those on “advanced” temporary jobs was confirmed also in robustness tests where we investigated the average number of unpaid overtime hours worked as an outcome measure for those workers with at least 1 year of tenure. See footnote 18 above.

²⁵ Individuals on Public Program Contracts are somewhat less likely to provide extra effort even though Gerfin and Lechner (2002) find that these programs are the “success story” among Swiss employment programs.

Table 6

Random effects probit estimates of the determinants of unpaid overtime work: heterogeneous effects of temporary contracts

	Coefficient	Standard error
Permanent contract (reference)	—	—
Temporary contract—seasonal and casual	0.212	0.176
Temporary contract—public program	−0.473	0.291
Temporary contract—internship	0.160	0.162
Temporary contract—Advanced	0.296	0.078***
Temporary contract—missing information	0.487	0.241**
Rho	0.627	0.011***
Estimator	Random effects probit	
Log likelihood	−13,423.0	
P value for test of joint significance of presented coefficient	0.001	
Number of observations	33,945	

The model controls for the same set of covariates as in Table 4.

** Represents statistical significance at the 5% level.

*** Represents statistical significance at the 1% level.

To investigate this possibility, we estimate refined models with additional interaction terms.

In column (2) of Table 7, we present the effect of interactions of the type of temporary contract and female sex. These coefficients are jointly significant at the 10% level and show that the direction of the effort effect for women runs counter to the effect estimated for men and thereby counter to H2: Female interns are significantly less likely to provide unpaid overtime work compared to male interns. Female seasonal and casual workers are significantly more likely to work overtime compared to their male colleagues. This weakly suggests that women respond less to career incentives than men.

A possible explanation for this gender difference may be that female workers accumulate less overtime in general because due to family obligations their time constraints are particularly binding (cf. Table 2). We investigated whether this affects the effort responses to temporary work by controlling for interactions with marital status and even an indicator of whether there are children to care for in the household.²⁶ These estimations did not yield statistically significant results and we cannot reject the hypothesis that in our sample, the family background of women has no effect on their response to incentives.²⁷

²⁶ The data does not allow us to guarantee that the women in our sample are the mother of the children, but it is likely to be true in most cases.

²⁷ We also investigated whether women are on average more likely to end up in temporary employment than men with the same characteristics. If the selection mechanism works different for the two sexes this may affect our results. However, the sex indicator has no significant impact on the probability of being temporarily employed, neither overall nor for the different types of contracts when looked at separately.

Table 7

Random effects probit estimates of the determinants of unpaid overtime work: differences across the sexes

	(1)		(2)	
	Coefficient	Standard error	Coefficient	Standard error
Temporary contract	0.278	0.083***	—	—
Female	−0.279	0.043***	−0.280	0.043***
Temporary contract × Female	−0.092	0.128	—	—
Temporary contract—seasonal and casual	—	—	−0.076	0.250
Temporary contract—public program	—	—	−0.750	0.374**
Temporary contract—internship	—	—	0.492	0.228**
Temporary contract—advanced	—	—	0.360	0.098***
Temporary Contract—missing information	—	—	0.483	0.312
Temporary contract—seasonal and casual × Female	—	—	0.607	0.354*
Temporary contract—public program × Female	—	—	0.784	0.599
Temporary contract—internship × Female	—	—	−0.664	0.323**
Temporary contract—advanced × Female	—	—	−0.167	0.158
Temporary contract—missing information × Female	—	—	0.007	0.491
Rho	0.628	0.011***	0.627	0.011***
Estimator	Random effects probit		Random effects probit	
Log likelihood	−13,426.9		−13,417.9	
P value for test of joint significance of interaction effects	—		0.072	
Number of observations	33,945		33,945	

The models control for the same set of regressors as in the specifications in Table 4.

* Represents statistical significance at the 10% level.

** Represents statistical significance at the 5% level.

*** Represents statistical significance at the 1% level.

4.4. Endogenous selection into temporary contracts

A potentially important issue not addressed so far concerns the exogeneity of the temporary contract indicator itself. It is possible that those who end up in temporary employment are not a random draw from the population and that this selection affects the estimates. Given the detailed controls for human capital and job characteristics in our model it is unlikely that a selection based on *observable* characteristics causes a bias in our estimation.

However, the results may be biased if *unobservable* factors determine the selection into temporary contracts and if these are correlated with our dependent variable. On the one hand, temporary employees might be a “negative” selection of workers with lower ability or motivation, accepting the wage penalty connected to this type of employment. In that case, we would expect lower levels of effort compared to an average worker, and our estimates would underestimate the true effort response.

Alternatively, one might argue that temporarily employed workers are a “positive” selection of workers with below average risk-aversion who accept temporary wage penalties and the challenge of active screening (e.g., as trainees) to qualify for better positions and in expectation of later rewards. In this case, our estimates would exaggerate the true effect.

If unobserved characteristics cause selection into temporary employment, the panel nature of our data helps to investigate their impacts: If temporary workers as a group differ systematically from permanent employees, there should be a noticeable difference in behavior of those temporary workers who just became permanent compared to those who were permanently employed throughout. We investigate this hypothesis by adding an indicator to the baseline model of Table 4 which describes whether a person who is currently permanently employed was a temporary employee in the preceding period.

The results are presented in Table 8 (column 1). The overall temporary contract coefficient is hardly affected by the additional control and the lagged indicator of prior temporary employment status has an insignificant positive coefficient. Thus, once previously temporarily employed workers are on a permanent contract their effort does not differ significantly from that provided by other permanently employed workers. This provides evidence against endogenous selection into temporary employment.

One could alternatively argue that high effort among temporary workers is driven by a group of “positively selected” high performers. The results in Table 8(1) yield that those who later work on permanent jobs still provide (insignificantly) more effort than other

Table 8
Random effects probit estimates of the determinants of unpaid overtime work in period t : dynamic effects of temporary contracts

	(1)		(2)	
	Coefficient	Standard error	Coefficient	Standard error
Reference: permanent contract in $t-1$ and in t	–	–	–	–
Temporary contract in t	0.215	0.112*	–	–
Permanent contract in t and temporary contract in $t-1$	0.127	0.123	–	–
Reference: permanent contract in t	–	–	–	–
Temporary contract in t and temporary contract in $t+1$	–	–	0.646	0.159***
Temporary contract in t and permanent contract in $t+1$	–	–	0.215	0.119*
Rho	0.667	0.014***	0.665	0.015***
Estimator	Random effects probit		Random effects probit	
Log likelihood	–7074.4		–7085.2	
Number of observations	18,037		18,037	

The models control for the same set of regressors as in the specifications in Table 4.

Due to controls for lagged and lead variables, the number of observations declined compared to prior estimations.

* Represents statistical significance at the 10% level.

*** Represents statistical significance at the 1% level.

permanent employees. To investigate the relevance of potential “positive selection” for our results, we additionally compare the effort of temporary workers who do and do not reach permanent jobs in the next period. We expect that the latter would be a less “positively selected” group for whom—if selection is important—the effort response to temporary contracts should be lower. The results in column 2 of Table 8 show highly significant positive coefficients even for those temporary workers who are not moving up to permanent jobs in the next period. Given that the estimated effect is even larger for those who are not moving up than for the “high achievers” who are subsequently employed permanently, this provides further evidence against the suggestion of endogenous selection into temporary employment.

We performed two additional tests for the endogeneity of temporary employment. First, to investigate the potential biasing impact of considering an endogenous “temporary work” indicator on the other coefficients in our model, we reestimated model 1 in Table 4 excluding the temporary contract variable. The marginal effects of the other variables remained largely unchanged, which is unlikely to occur if temporary work were endogenous. Second, the problem could, in principle, be addressed by using a fixed effects regression. However, the fixed effects logit estimator involves the loss of a sizeable number of observations, in our case, 27,141 of 33,945 observations. With the fixed effects estimator we again obtain a positive coefficient on the “temporary work” indicator. This positive effect—even though imprecisely estimated—again supports the conclusion that the positive correlation between “temporary work” and worker effort is not due to endogeneity.

5. Summary

We analyze the behavioral correlates of temporary versus fixed term contracts based on six waves of data from the Swiss Labor Force Survey. We hypothesize that the disadvantages of working on a temporary contract provide implicit incentives for temporary workers to provide high effort in order to increase their probability of shifting to a permanent contract. To test this hypothesis, we compare the effort of temporary and permanent employees. Landers et al. (1996) and Booth et al. (2003) discuss the relevance of promotion incentives for permanent workers. The higher such incentives for permanent employees, the less likely we will find the incentive effects of temporary contracts.

However, our results suggest that temporary workers provide significantly more effort as they have a 60% higher probability of working unpaid overtime than employees with permanent contracts. Thus, even though permanent employees may be subject to their own incentive mechanisms, the average temporarily employed worker works even harder. Among the temporarily employed effort differs depending on the type of contract: Those in positions with potential for “upward mobility” are significantly more likely to supply high levels of effort.

Interestingly, there was no measurable effect of temporary employment on absenteeism, an indicator typically applied in this literature. We interpret this result in combination with international evidence as indicative of a low overall level of opportunistic absences in Switzerland, which may well be related to the low level of employment protection in this

country. As a consequence, temporary employees may not be able to signal their characteristics to the employer by low absence rates.

A comparison between males and females yields no significant differences in their overall responses to contract-related incentives in Switzerland. There are only weak indicators for a stronger response of males to such incentives. This similarity of the incentive response of the two sexes would be interesting to examine with different data and for other countries as it deviates from the findings for the United Kingdom (Booth et al., 2002b).

Our results support the predictions of an incentive-based model of employee signaling behavior in situations of asymmetric information. This can be interpreted either as a behavioral response of those on permanent contracts to the benefits provided by their position, or as its mirror image, the response of temporary workers to the lack of such benefits and their incentive to obtain them.

The former interpretation stresses moral hazard in the behavior of permanent employees who do not keep up with the effort levels of temporary workers. However, given the nature of our effort indicator, i.e., unpaid overtime work, which describes not just the absence of shirking but also the presence of effort above and beyond contractual requirements, the moral hazard interpretation is not convincing. Instead, we prefer to emphasize the extra effort of temporary workers who invest in signaling activities. Such behavior—when observed among managers—is considered as an indicator of ‘career concerns’ which are “concerns about the effects of current performance on future compensation” (Gibbons and Murphy, 1992, p. 468). The career concerns concept appears appropriate for the situation of temporary employees whose future depends—just as in the case of managers—on being able to establish a reputation with current or potential future employers.

Our evidence supports the hypothesis that asymmetric information on employee characteristics provides effective incentives for temporary employees to invest in signaling behavior. Such behavior should vary with the relative benefit of permanent over temporary employment. Therefore, the effort response among temporary workers in Switzerland may constitute a lower bound for the response to be expected in countries with more protective labor market regulations or higher wage penalties for temporary jobs.

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