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Missing at Work – Sickness-related Absence and Subsequent Job Mobility

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Abstract

Economists often interpret absenteeism as an indicator of effort. Using data from the German Socio-Economic Panel (SOEP) study, this paper offers a comprehensive discussion of this view by analysing various forms of job mobility. The evidence reveals a significantly negative (positive) link between sickness-related absence and the probability of a subsequent promotion (dismissal). In line with the interpretation of absenteeism as a proxy for effort, instrumental variable analyses suggest no causal impact of absence behaviour on the likelihood of such career events when variation in illness-related absence is triggered exogenously. We observe no consistent gender differences in the link between absence and subsequent career events.

Keywords: dismissal, gender difference, German Socio-Economic Panel (SOEP), instrumental variables, job mobility, promotion, sickness-related absence

JEL-classification: J 16, J 22, J 63, M 51

Note: We are grateful to Nicolas Ziebarth for providing background information on industry-specific bargaining contracts.

1. Introduction

Should I avoid calling in sick to better my chances for promotion and to reduce the likelihood of losing my job? While correlates and determinants of absence have been looked at comprehensively (see Brown and Sessions (1996) and Treble and Barmby (2011) for surveys), the potential significance of absenteeism for worker careers has received relatively little attention. This is surprising as the consequences of absenteeism are arguably the more relevant aspect, both from a firm's point of view and from an individual's perspective. Despite this lack of evidence, researchers regularly interpret sickness-related absence as an indicator of employee effort or performance (e.g. Flabbi and Ichino 2001, Audas et al. 2004, Ichino and Maggi 2000, Ichino and Riphahn 2005, Hesselius et al. 2009, Cornelißen et al. 2011, and Block et al. 2014). If this view is justified, one should expect a negative link between absenteeism and future career prospects.

A review of the existing literature (see Section 2) reveals that the evidence on the relationship between absence behaviour and job mobility is limited. Only a handful of authors specifically look at this link (e.g. Judiesch and Lyness 1999). There remain open questions, such as with respect to the role of health. In addition, comparisons with alternative effort proxies like hours worked (see e.g. Bell and Freeman 2001) are rarely possible. Moreover, evidence obtained from firm data lacks generalizability and this data usually contains only limited information with respect to individual characteristics. The latter aspect applies even more to register data. In order to establish causal relationships, which are of primary interest when looking at the consequences of absence, researchers may resort to (quasi-) experimental settings. Alternatively, respective analyses have to make use of plausible instruments, which are not always available. These features of extant contributions indicate that the research on the labour market consequences of absence behaviour has to be complemented by evidence based on representative household panel data with extensive information on individuals.

The present study pursues such an approach. In particular, we enquire whether absence behaviour affects the probability that an employee experiences an incident of firm-based job mobility. We provide evidence for Germany, the largest economy in Europe and fourth-largest in the world, that so far has received rather little attention regarding job mobility and the consequences of sickness-related absence in particular. To analyse the relationship between absence behaviour and job mobility, we use data from the German Socio-Economic Panel for the period 1994-2011. This household panel contains a great deal of information on individual employees, e.g. regarding their health status, and allows us to follow their career

within the same organisation. Therefore, we can avoid the problems of interpretation often associated with firm or register data. Finally, we can employ several plausible instruments in order to examine whether sickness-related absence is a causal trigger for job mobility. To the best of our knowledge, our paper is the first to provide (causal) evidence on the link between sickness-related absence and multiple forms of subsequent career events using representative household panel data.

There are various reasons why absence behaviour can be related to firm-based job mobility. Firms are most likely to promote those employees who have performed well in their current jobs. If employees are ill or shirk and, therefore, miss work, employers may view absence as an indicator of lower productivity and also of lower motivation. Moreover, firms are likely to dismiss those employees who are believed to have the lowest productivity. In Germany, the latter aspect may also be relevant in view of extensive employment protection regulations. The Protection Against Dismissal Act ("Kündigungsschutzgesetz") stipulates that (severe) illness is one cause that can justify dismissing an employee. With respect to quits or resignations, another form of career move, higher absence levels may indicate an employee's dissatisfaction with the current job or a mismatch between abilities and job demands. In both cases, one would expect the resignation rate to correlate positively with absence rates. Finally, there may also be a link between the probability of a transfer, defined as all instances of within-firm job mobility other than a promotion, and absence behaviour.

In our empirical analysis, we find robust evidence for a negative (positive) relationship between the duration of absence of an employee and the probability of being promoted (dismissed) in the next year. The evidence comes from pooled and fixed-effects regression analyses. We detect no robust link between absence and resignations or transfers within the same firm. Furthermore, we observe no gender differences in the relationship between absence behaviour and promotions and find only in a subsample analysis that the link between sickness-related absence and dismissals may be more pronounced for males. Finally, we analyse whether the observed correlations can be interpreted causally. Our instrumental variable (IV) analysis exploits exogenously triggered variations in absence, implying that motivation, job performance, and other factors influencing absence are likely to be constant. In this case, we find no significant effects of absence on the likelihood of subsequent promotions and dismissals.

Our findings have wider implications: First, we are able to consider a variety of health indicators in ways that previous studies using firm or register data could not take into account.

We find that the relationship between absence behaviour and job mobility is generally unaffected by the in- or exclusion of these health measures. Second, although we do not employ a causal identification for health effects, the minor role of health in estimating job mobility suggests that this factor does not generally constitute an important career determinant.[1] This insight is both interesting and important, as absence from work strongly correlates with people's health. Third, the predictive power of sickness-related absence is even more impressive in comparison to the alternative effort proxy of hours worked, which has no significance, and to the other alternative of job satisfaction. Similar to the evidence on quits (e.g. Clark 2001, Shields and Wheatley Price 2002, Green 2010), we observe a strongly negative link between satisfaction with work and the probability of both dismissal and promotion, the latter of which might be somewhat surprising.[2] Most importantly, the significant signal effect of sickness absence is not altered by consideration of any of these variables. We conclude that the observed link between absenteeism and job mobility most likely results from unobserved differences in worker effort, given that our findings remain unchanged in fixed-effects specifications.

Finally, there is substantial evidence that absence rates are gender specific (see e.g. Barmby et al. 2002). Based on these findings, Ichino and Moretti (2009) argue that sickness-related absence can contribute to an understanding of the overall gender gap in labour market outcomes, a claim that has not gone undisputed (e.g. Herrmann and Rockoff 2012). One argument in this context is that absenteeism may constitute a different effort signal for women when absence corresponds with family responsibilities (see Hansen 2000, Markussen 2012). As pointed out by Booth et al. (2003) in their analysis of promotions in British survey data, most firm-based investigations lack the generalizability that is strongly required when gender differences are investigated.[3] With fresh evidence from representative household data, we contribute to this debate by showing rather little gender differences and thereby questioning some of previous findings and considerations.

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[1] Several studies on labour market outcomes suggest a strong role of people's health status. Note that in order to establish causal identification, researchers mostly focus on severe health shocks (see e.g. Moller Dano 2005, Campolieti and Krashinsky 2006, Crichton et al. 2011, Halla and Zweimüller 2013).

[2] These findings complement recent research on people's job satisfaction at the end of their tenure (see Chadi and Hetschko 2014), as it seems that individuals are dissatisfied with the old job, independent of the type of mobility that subsequently follows.

[3] There is a large body of literature on the effect of gender differences on the probability for certain forms of job mobility. For promotions, the evidence is mixed. In some studies, women experience promotions more often than men (e.g. Hersch and Viscusi 1996), while in others the opposite result is obtained (e.g. Pekkarinen and Vartiainen 2006). Johnston and Lee's (2012) study contains a review of the literature. For job losses, the evidence typically indicates a higher chance of being dismissed for men (e.g. Campbell 1997, Wilkins and Wooden 2013, with the exception of Goerke and Pannenberg 2011).

We discuss the relevant literature in more detail in Section 2. Section 3 describes the data and the procedure to identify incidents of job mobility. In Section 4, we present our main results from pooled and fixed-effects specifications and also analyse potential gender differences. Section 5 contains the discussion of causality based on findings from IV-specifications. In both sections, we present our main results in tables, while the outcomes from various sensitivity analyses are reported verbally. A full documentation of these robustness checks is available from the authors upon request. Finally, Section 6 concludes.

2. Previous Research

There are some studies which have examined directly how sickness-related absence is linked to future job mobility and career events. Moreover, a number of investigations have focussed on related aspects, such as income changes and employment prospects, and, hence, provide indirect evidence with regard to the issue of interest here. We commence our description of the previous literature with analyses based on firm-specific data.

Judiesch and Lynnes (1999) analyse career success, utilising data from the United States on more than 10,000 managers from a large multinational financial services organization. They examine the relationship between leaves of absence for family-and sickness-related reasons and subsequent promotions and salary developments. If managers took more than one leave, the promotion probability and the salary increase were significantly lower than for managers with fewer or no absence spells, both for males and females. Ichino and Moretti (2009) investigate whether there is a relationship between women's menstrual cycle and absenteeism, employing data from a large Italian bank. If absenteeism by women is less informative with respect to work effort than by men, the consequences of absence behaviour may differ. In line with this conjecture, the authors observe a link between absence and the probability of being promoted that is weaker for women. They obtain similar findings with regard to wages. Finally, Pfeifer (2010) bases his analysis on personnel records of about 1,700 employees in a medium-sized German company. He distinguishes between blue- and white-collar workers and finds that the duration of absence within the last three months correlates with lower promotion probabilities for both groups of employees. However, there are no effects discernible for more distant absence spells and there is no information on gender differences.

Two further studies interpret absence as indicator of productivity and effort, respectively, and also provide relevant evidence. Flabbi and Ichino (2001) utilise the same data set which Ichino and Moretti (2009) employ and investigate the relationship between an individual's productivity and the probability of being promoted and the magnitude of wage increases.

They consider various productivity indicators, one of them being the number of absence periods per year of seniority. Audas et al. (2004) employ information derived from personnel records of a large British financial sector firm and analyse promotions. As an effort indicator, the authors use the absence rate of employees, relative to the rate of comparable colleagues. Both Audas et al. (2004) and Flabbi and Ichino (2001) find a negative correlation between absence and being promoted. In addition, the latter also observe a negative effect on wages.

In sum, studies based on firm data suggest that absence and the probability of being promoted are correlated negatively. None of the studies discusses in detail or establishes causal relationships.

In addition to firm-based investigations, there are some analyses on income effects that provide indirect evidence on job mobility and absenteeism, usually employing more comprehensive data sets and aiming at causal conclusions. Hansen (2000) matches register data to information from the Swedish Household Income Survey and shows that contemporaneous wages by females are slightly but significantly reduced by illness-related absence, employing an instrumental variable approach. Andersen (2010) uses register data from Denmark and employs a legal change relating to the reimbursement of sick leave payments as instrument. She finds that absence spells lasting at least two weeks negatively affect income in subsequent years. The most recent study by Markussen (2012) also analyses register data and finds that absence reduces subsequent earnings and the probability of future employment in Norway. The effects are estimated in an IV-setting and identified by an indicator of the leniency of doctors who have to certify the illness of an employee as instrument. While the decline in wages is more pronounced for males than for females, the opposite is true with respect to the employment probability. Consequently, the available investigations for Scandinavian countries suggest that absence can causally reduce income. Since promotions often result in wage increases, the findings based on representative data sets are in line with the firm-level evidence. In contrast, however, Herrmann and Rockoff (2013) observe no significant relationship between sickness-related absence and earnings in US survey data when considering important differences in individual job characteristics.

A more consistent picture than with regard to income is found in the research on how absenteeism relates to subsequent unemployment. The link appears to be positive in Swedish (Hesselius 2007) and Italian registry data (Scoppa and Vuri 2014). While neither of the studies focuses on causal relationships, the latter explicitly considers absenteeism as a proxy for employee shirking.

An important aspect of the studies based on register data is that consequences of absenteeism cannot necessarily be associated with the firm in which the absence spell occurred. More precisely, future wage and employment outcomes may be the result of responses within the firm in which the periods of absence occurred or be due to a change of the employer, respectively, to a job loss. Hence, the aftereffects of absence for internal firm mobility on the one hand and external mobility on the other cannot be distinguished clearly. Studies based on firm data can circumvent this problem to some extent, although the findings then only apply to a particular institution. Therefore, the extant literature, irrespective of the data used, only provides indications of the relationship between absence behaviour and firm-based job mobility.

3. Empirical Framework

3.1. Data

Our data comes from the German Socio-Economic Panel (SOEP) study, an annual panel survey that provides representative information on the people in Germany (see Wagner et al. 2007). Given the longitudinal structure, we can link data on absence behaviour for a certain year to events in terms of job mobility in subsequent years.

Our dependent variable is a dummy variable which indicates whether a job change and what kind of job mobility have occurred. The SOEP questionnaire includes two sets of relevant questions. One deals with whether the respondent has recently started a new job, the other with a potential termination of the employment contract. Based on the responses to these questions, we can differentiate between firm internal and external job mobility. Further, we divide incidents of the former type of job mobility into promotions and transfers. The former occurs if a respondent's job change is associated with a higher occupational rank. To define rank changes, we generally follow an approach pioneered by Lluis (2005) in a study on wages and internal firm internal mobility in Germany. Broadly speaking, by comparing the occupational status of a respondent prior to and subsequent to the job move within the firm, we can identify the nature of the occupational change. In particular, given five different occupational rank categories which can be derived from the SOEP data, we define a move to a higher occupational category as a promotion. Additionally, we classify a movement from one job to another in the same occupational category as a promotion if there is an above average wage increase. We outline the details of this classification procedure and discuss additional plausibility analyses in Appendix B. All internal job changes other than *promotions* are defined as *transfers*. They are fundamentally different from promotions in that they are

unlikely to constitute a career advancement.[4] Turning to external job mobility, the SOEP questionnaire directly enquires about the reason for the termination of the employment contract. From the list provided, the most commonly reported categories are *resignations* and *dismissals*.[5]

Turning to our main independent variable, the SOEP observes each employed individual's annual number of sickness-related absence days based on the following question: "How many days off work did you have in [the last year] because of illness?"[6] Furthermore, respondents are asked whether they were continuously absent for more than six weeks. Both questions refer to the year preceding the interview. The information is not detailed further to calculate the number of absence spells and their respective durations. For our main analyses, we follow previous studies, such as Audas et al. (2004), by applying a transformation of the absence information into an absence rate. More precisely, we divide the number of reported absence days by 250 as an approximation of the upper limit in a year (see Winkelmann 1999).[7]

In addition to the absence rate, we include a set of explanatory variables which can be broadly grouped into personal, job and firm characteristics. Personal characteristics include gender, age (age squared), education (in years), the aggregate duration of previous employment and unemployment spells (in years), whether the respondent owns a house or flat, its size (in square meters), a subjective measure of housing conditions, how many household members there are, whether children under the age of 16 are living in the household, whether the respondent has a partner and is married, equivalent household income as well as net earnings of the respondent (in Euros), the federal state ("Bundesland") where the respondent lives and the regional unemployment rate. Furthermore, we include indicators of subjective health (measured on a 5-point scale) and of health and job satisfaction (measured on an 11-point Likert scale) in some specifications. Job characteristics are incorporated by means of a tenure indicator (in years), dummy variables specifying whether the respondent is a blue- or white-

[4] Therefore, we also classify the movement to a lower occupational category as a transfer. Such demotions happen very rarely, as frequently shown (see e.g. Baker et al. 1994, Treble et al. 2001, van der Klaauw and Dias da Silva 2011), with the potential exception of firms prior to their bankruptcy (Dohmen et al. 2004).

[5] Other forms of job terminations are not regularly included in the SOEP and are either rare (such as a mutual agreement between employer and employee to dissolve the employment contract) or do not suggest a relationship to sickness-related absence (such as a plant closure).

[6] Note that this is not the wording provided to non-German speaking researchers by the SOEP. We find our translation (which is the same as in Ziebarth and Karlsson 2010) to be closer to the German original, as it implies no verbal encouragement to only report absence days that were genuinely resulting from illness. When being asked about days off work because of illness (which corresponds perfectly to the actual German formulation, i.e. "wegen Krankheit"), one may also report absence that results from shirking.

[7] This number approximates the maximum number of workdays per year in Germany (see IAB 2014). The division of the main independent variable by a fixed number obviously does not affect results, but simplifies the quantitative interpretation of findings.

collar worker or a civil servant and has a second job, the number of actual work hours and the occupational rank according to the SOEP-classification (see Appendix B). Firm characteristics include dummy variables for working in the public sector, denoting the size of the firm (0-19, 20-199, 200-1999, and 2000 or more employees), and the industry (NACE-1 level).

3.2 Sample restrictions

In our empirical analysis, we compare individuals who experience a promotion, a transfer, a quit or a dismissal to respondents who retain their job. To assess the relationship between absence and job mobility, we have to ensure that the respondent worked in the firm under consideration for the entire year for which we have information on absence. For respondents who exhibit an incident of firm internal job mobility in the next year, we oblige subjects to report no other form of job change in the two following interviews and an increase in tenure over this period. Furthermore, we ensure that repeated job mobility events for the same person do not affect the analysis by excluding all observations from individuals who experienced the same type of job mobility another time. Otherwise, observed absence could constitute both post- as well as pre-mobility behaviour.[8]

With respect to the control group, we require that each person reports a "no" when asked about a recent job change in *two* subsequent interviews. Note that this is necessary as SOEP respondents may report job changes in the subsequent year in both the next and the next but one interview (see Appendix B). We additionally adjust via the tenure information so that individuals are excluded from this reference group as soon as their tenure does not increase in concurrence with the statement that no job change occurred.

Turning to our main independent variable, people who suffer from extreme health problems are likely to be very different from those who are more or less healthy. Accordingly, we exclude all disabled persons from our main sample. We also drop individuals who reported a severe health problem, defined as having experienced either at least one sickness-related absence spell of more than six weeks duration or more than 50 days of absence per annum in total. The latter restriction implies that the maximum absence rate amounts to 0.2.

We restrict our empirical analysis to dependent, full-time employees and exclude individuals in vocational training and subsidised or other irregular forms of employment. To further

[8] The findings do not change qualitatively when we alter the treatment of respondents with multiple incidents of job mobility and (1) include all observations with more than one instance of job mobility of the same type, (2) exclude the post-mobility information for each respondent entirely, irrespective of the number of incidents of job mobility, or (3) drop persons with multiple job mobility events completely.

increase comparability, we require respondents to have a minimum of 35 contractual work hours (if such a number is specified in the contract) and to have actually worked at least 35 hours per week (including overtime). Furthermore, we impose an upper age limit of 58 years, to rule out effects resulting from early retirement.[9] Finally, we define the time range from 1994 to 2009 as our period of investigation, which is primarily determined by the availability of both absence and health variables.[10] Data from 2010 and 2011 is reserved to allow us the look "into the future" and thus the analysis of subsequent job mobility.

3.3 Outline of analysis

Our presentation of results in Section 4 starts with a descriptive illustration of the sample and average statistics. A graphical analysis of averages in sickness-related absence periods prior to job mobility follows. In order to consider the different background in the individuals' work and private life, we implement regression analyses based on the available SOEP data. To show relationships between sickness-related absence and various forms of subsequent job mobility, we first report the results from a multinomial logit model. This method allows having several categorical outcomes and thereby brings together all mobility types of interest in one analysis. In the further course of our investigation, we take a closer look at two specific events, promotions and dismissals, by implementing linear probability model regressions. This allows us to consider fixed individual effects and eases interpretation. In line with other studies on absence effects in earnings, we then establish an instrumental variable approach in order to discuss the question of causality. We document in detail the findings from our main specifications in in the main text and report verbally the central results from a variety of robustness checks. As indicated at the end of Section 1, a complete description of theses robustness analyses is available upon request.

4. Results

4.1 Descriptive Findings

Our sample consists of 46179 observations and 2171 instances of job mobility, as documented in Table 1. A complete illustration of all control variables with respect to average statistics is provided in the Appendix Table A1. Dismissals and quits are the most numerous events, while firm internal job mobility takes place less frequently. Comparing the distribution of respondents across the five ranks for those employees who do not change their job and those

[9] Our results do not change when we include employees up to 63 years of age.
[10] Moreover, the SOEP questionnaire changed substantially in 1994. This particularly affected the questions concerning job changes and terminations.

who quit it, we can discern virtually no difference. However, employees who are promoted or transferred within the firm are more likely to have already attained ranks 3 or 4, while employees who are dismissed more often belong to lower ranks. We also observe some promotions from rank 5, given that it can also be identified by a rank-preserving job change and a simultaneous above-average wage increase.

Turning to the individual absence behaviour, we note from Table 1 that the probability of being absent at least one day in a given year is higher for individuals who quit their job, relative to the control group of those who stay with their employer and the individuals who experience other forms of job mobility. Overall, however, for the career events examined here, a differentiation of cases according to the incidence of absence does not offer important evidence.[11] We can, furthermore, observe from Table 1 that employees exhibiting job mobility are characterised by a higher number of annual absence days than those who retain their job. The exception is employees who are promoted. They miss more than one day less per annum due to sickness than the group of stably employed and two or more days less relative to employees who exhibit other types of job mobility. This suggests that sickness absence and job mobility may be related and that the nature of this relation can depend on the type of job mobility.

----- Table 1 about here-----

Further inspection of Table 1 indicates that individuals exhibiting instances of job mobility do not differ strongly with respect to their (subjective evaluation of) health or actual working hours. Employees who are promoted are, inter alia, younger, less likely to be married and to have children. According to Table A1, they also have shorter employment experience and are more likely to work in large firms than employees who do not change their job. Furthermore, employees who leave the firm have lower income, are less likely to work in the public sector or be a civil servant ("Beamter"), are more likely to work in a small firm and to be blue-collar workers than the stably employed. Finally, individuals who are dismissed have the longest unemployment experience and also the lowest income level of all groups.

In sum, the descriptive statistics show that the various groups of employees do not only differ in their absence behaviour, but also with respect to other personal, job and firm

[11] In additional regression analyses, the binary incidence indicator is not found to be a strong predictor of subsequent job mobility.

characteristics. This motivates controlling for these and other differences in order to identify how absence behaviour relates to different types of job mobility.

Before moving on to the regression results, we can present further descriptive evidence indicating the temporal relationship between sickness-related absence and subsequent job mobility. Figure 1 depicts the absence rate in the same firm for four years prior to an incident of job mobility separately for all four types of changes.[12]

----- Figure 1 about here-----

In the case of promotions, the absence rate decreases in the three years prior to the job change. The decrease is particularly pronounced in the last year and the absence rate in the year prior to the promotion is also significantly lower than in the two previous years. The picture for dismissals is basically the opposite, in that the absence rate gradually increases in the years before a dismissal occurs. The temporal development of absence behaviour for employees who are subsequently transferred within the firm or who quit their job depicts no clear temporal pattern.

4.2 Multinomial Logit

The first set of main results comes from a multinomial logit analysis. Our underlying assumption is that an employee may leave the current job by either being promoted or transferred within the same firm, by being dismissed or by quitting the job. Table 2 depicts the estimated coefficients of the variable of main interest, i.e., the absence rate.

----- Table 2 about here-----

Panel A presents the results for the main sample. Accordingly, the absence rate relates positively to the likelihood of being dismissed and negatively to the probability of being promoted. This confirms that the descriptive findings are not driven by observable characteristics. Furthermore, the estimates reveal no relationship between transfers and absence behaviour. The analysis also indicates that a higher sickness-related absence rate is associated with a higher probability of quitting.

Table 2 also contains findings for a subsample of employees who are at least in their fourth year of tenure and are without job change for the last three years (Panel B). On the basis of

[12] Note that the number of observations is smaller than those reported in Table 1, as we additionally require that each included individual report no job change four years in a row.

this seniority-based subsample, which excludes newcomers, it is possible to ascertain whether the relationship between absence behaviour and job mobility is the same for employees whose effort behaviour is probably well-known to colleagues and management. Eliminating newcomers from the sample does not affect the findings with regard to promotions and dismissals. The relationship between absence and quitting behaviour, however, is sensitive to the exclusion of individuals who are in their first years of working for the firm under consideration.

We subsequently focus on promotions and dismissals. We do so for two reasons: First, resignations and transfers may both constitute career advancements and deteriorations and are, hence, difficult to interpret. Second, separate preliminary analyses for resignations and transfers have confirmed the findings from the multinomial logit approach. In particular, we are not able to detect a consistent relationship between absence and these two kinds of job mobility.

4.3 Promotions

In this sub-section, we present findings from linear probability models for pooled and fixed-effects specifications relating to promotions. Table 3 contains the estimated coefficients for the main variable of interest, two measures of the respondent's health and two further indicators of job effort. In the Appendix Table A2, we document the complete results for specification (3) of Table 3, for pooled and fixed-effects regressions.

----- Table 3 about here-----

The results in Table 3 show a significant link between absenteeism and promotion probability. This finding is not sensitive to the consideration of individual job characteristics and other observable information on individuals. Our preferred specification (3) also takes differences in workers' health into account. The results indicate that an increase in the absence rate by one percentage point, which is equivalent to 2.5 days more sickness-related absence per annum, reduces the probability of being promoted by slightly more than 0.05%. Given an overall promotion probability of about 0.7% per year in our sample, an increase in the absence rate of two percentage points, or one more week of sickness-related absence, reduces the promotion probability by an economically significant magnitude 16% ($\approx$ 0.11/0.7). Remarkably, the results of the fixed-effects regressions (Panel B) are qualitatively and quantitatively very similar to those of the pooled regressions (Panel A).

Another interesting finding is that the measures of health are not correlated with the promotion probability. This suggests that, first, the health status of an employee does not alter the probability of such type of job change. Second, health does not affect the role of sickness-related absence in predicting career advancement.

These findings are corroborated by further estimations in which we additionally include two arguably more objective measures of an individual's health status, namely the number of visits to a doctor and of nights spent in hospital in the year prior to the promotion. The estimated coefficients for the absence rate retain their sign, magnitude and significance. The same is true when we extend the sample to also include disabled employees. As soon as we include those with severe health problems by relaxing the restrictions on absence spells and total amount of annual absence, however, the estimated coefficients of the absence rate shrink in size and become statistically insignificant. Absence spells may indeed be informative with respect to health status and not effort for long-term absentees, in contrast to employees with shorter spells. For employees for whom absence primarily conveys information about the health status, we thus observe no relationship between absence behaviour and career advancements within the firm.

In addition, the last column of Table 3 shows that actual hours worked are not correlated with the promotion probability. If hours worked are interpreted as an indicator of effort, then the results suggest that such effort is not conducive to being promoted. In contrast, employees characterised by lower job satisfaction are more likely to be promoted. Assuming that effort is generally higher prior to a promotion this observation rather speaks against an interpretation according to which job satisfaction is a good indicator of the performance level in this particular situation.

Following the implicit suggestion resulting from Figure 1, namely that the correlation between sickness-related absence and job mobility may take time to materialise, we have also estimated specification (3) in Table 3 with lagged indicators of health and the lagged absence rate as additional covariates. It turns out that the estimated coefficient on the absence rate is basically unaffected, relative to that depicted in Table 3, while the estimated coefficient of the lagged absence rate is not significantly different from zero. This suggests that the information conveyed by sickness absence with respect to promotions may be short-lived, which is consistent with firm-based evidence (see Pfeifer 2010).

Finally, we have checked the robustness of our findings further with regard to the sample of employees. Thus far, we have excluded part-time employees to enhance the comparability of respondents and have included employees who are relatively new in the firm. However, the relationship between promotions and sickness absence may depend on working time and the length of the employment spell. In consequence, in additional estimations we either included part-time employees or excluded all newcomers, in line with the procedure outlined in the previous sub-section. The estimated coefficients and their significance levels are basically the same as depicted for the main sample in Table 3.

In sum, we find robust evidence of a sizeable negative relationship between sickness-related absence and the probability of being promoted in Germany. Since controlling for various indicators of health does not alter these results, we can conclude that this relationship does not arise because of an employee's health status. The only exception we find relates to employees with severe health problems for whom absence is no longer related to promotions. Since we obtain qualitatively and, in most cases, also quantitatively very similar results for pooled and fixed-effects specifications, time-invariant employee characteristics, such as someone's general attitude towards work, are not determining the findings.

4.4 Dismissals

In this sub-section, we present findings for dismissals, once again originating from linear probability models for pooled and fixed-effects specification. Table 4 contains a selection of the estimated coefficients. In the Appendix Table A2, we document the complete results for specification (3) of Table 3, both for pooled and fixed-effects regressions.

----- Table 4 about here-----

The results in Table 4 show a significant link between absenteeism and dismissal probability, irrespective of whether individual job characteristics and other observable information concerning individuals are accounted for or not. Our preferred specification (3) also takes differences in workers' health into consideration. The results show that an increase in the absence rate by one percentage point raises the probability of being dismissed by slightly more than 0.11% in the cross-sectional model (Panel A) and by 0.08% in the fixed-effects specification (Panel B). Given an overall dismissal probability of about 1.7% per year in our sample, an increase in the absence rate of two percentage points is equivalent to a rise in the probability of a dismissal by almost 10% ($\approx 0.16/1.7$), based on the fixed-effects specification.

Apart from the effects' magnitude, there are no substantial differences between the results of the pooled regressions compared to those of the fixed-effects regressions.

Once again, and with one exception (see Table 4, Panel A, column (4)), subjective measures of health are not correlated with job mobility. As it is true in the case of promotions, including the two more objective measures of health, the number of visits to a doctor and of nights spent in hospital, does not affect the results depicted in Table 4. Accordingly, the findings for dismissals are consistent with the interpretation developed when looking at promotions, namely that absence rates are not primarily an indicator of career-relevant differences in employees' health. In contrast to the findings for promotions, we continue to find statistically significant relations between the absence rate and the dismissal probability in the pooled and the fixed-effects specification when including disabled employees and those with severe health problems in the sample. This suggests that employees who are absent from work for sickness-related reasons for a sizeable duration face a substantially higher probability of losing their job than employees without such absence spell. With regard to dismissals, absence may, hence, be interpreted as an unconstrained indicator of effort.

In addition, Table 4 shows that employees characterised by higher job satisfaction are less likely to be dismissed and that actual hours worked are not correlated with the dismissal probability. Taking into account that promotions are also negatively associated with job satisfaction, our findings indicate that higher job satisfaction may be conducive to a general reduction in job mobility. Furthermore, working more hours has an effect neither on the probability of upward internal mobility nor of losing the job. Accordingly, such quantitative effort does not signal having better job prospects. Finally, when including lagged measures of absence into the specifications depicted in Table 4 as additional covariates, the estimated coefficients on the absence rate are basically unchanged and the estimated coefficient on the lagged absence rate is (weakly) significantly positive in the pooled model but not in the fixed-effects specification.

Finally, we have checked the robustness of our results with respect to the sample. Including part-time employees increases the estimated coefficients in the pooled and the fixed-effects specification in comparison to the estimations reported in Table 4 for the main sample, without altering the standard errors. When excluding firm newcomers from the sample, the relationship between absence behaviour and the dismissal probability loses significance in the fixed-effects specification. This last finding suggests that absence is only weakly linked to the

probability of a dismissal, once an employee has worked within a firm for a sufficiently long period.

All in all, we can present convincing evidence for a negative relationship between the duration of sickness-related absence and the probability of being dismissed within the next year. As in the case of promotions, we find no evidence that time-invariant employee characteristics govern the results.

4.5 Gender Differences

It is widely documented that absence rates for females are higher than for males. Moreover, our above review of the literature indicates that also the consequences of absence may vary with gender, although previous contributions do not provide a consistent picture. We investigate this issue by adding an interaction term (absence rate x female) to our preferred specification (3) in Tables 3 and 4. Results are depicted in Table 5 for our basic sample and the seniority-based subsample, from which we have excluded all employees who are newly employed in the firm (as defined in the above analysis illustrated in Table 2). We only present results from linear probability models using pooled data, since fixed effects specifications lead to the same conclusions regarding the role of gender differences.

----- Table 5 about here-----

Inspection of Table 5 clarifies that the relationship between sickness-related absence and the probability of a promotion does not differ according to gender. Even the probability of being promoted itself does not vary with gender (see also Table A2 in the Appendix). The picture with respect to dismissals is more blurred. While we observe no gender differences for the encompassing sample, the findings in column (4) for the seniority-based subsample indicate that the duration of sickness absence is not correlated with the dismissal probability for those females whose employment spell has lasted more than three years. Interestingly, the basic gender difference in the likelihood of being dismissed disappears for the smaller sample. For the main sample, we observe that females are in general less likely to be dismissed, which conforms to findings in the previous literature (see footnote 4).

5. Discussion of Causality

5.1 Instruments

Our research setting allows us to exploit several instruments as exogenous sources of variations in absenteeism. Hence, we can ascertain whether absence is causal for job mobility

or represents a hard-to-observe determinant of promotions and dismissals, such as may be the case with respect to effort. The first idea is to use the sick-pay legislation reform of 1996, which has attracted researchers' interest (Puhani and Sonderhof 2010, Ziebarth and Karlsson 2010, Goerke and Pannenberg 2012), as it constitutes one of the rare cases of a quasi-experimental setting with respect to absence behaviour in the German labour market. Through a reduction of statutory sick pay in parts of the private sector, individual absences went down slightly but significantly. For the second instrument, we exploit information on work accidents. The SOEP questionnaires of the 1990s contain this specific question, which gives researchers the opportunity to exploit an arguably coincidental event. Therefore, in the first part of our IV analysis we focus on the years of 1994 to 1999. We exclude data from the year of 1996 in the course of the first IV analysis because the sick-pay reform took place during that year. This step provides a clearer distinction between control and treatment groups and is in line with the above-mentioned studies. Additionally, we increase observation numbers by including people who work part-time into our sample.[13] This step is motivated by the results of this particular sensitivity check (see Section 4).

The availability of data for Germany's regional policy regions (ROR) allows conducting some further instrumental variable analyses. In fact, by using regional indicators that researchers can obtain upon request from the SOEP, the data sample for the whole of Germany can be broken down into 96 ROR. These clusters have an average population of slightly less than one million inhabitants, and on average a hundred SOEP interviewees live in each one. At such a regional level, differences in the individual survey data can be linked to local circumstances that are potentially relevant for people's sickness-related absence behavior. First, we view the number of hospital beds in a region (ROR) as an exogenous source for differences in absences from work. The intuition of how this regional indicator affects our variable of interest is straightforward. The more beds there are, the more likely it is that individuals abstain from going back to work because they are offered to stay in a hospital. Second, we focus on the number of inhabitants per doctor. For each ROR, we can calculate the ratio between the number of citizens and the total number of doctors in that region.[14] Accordingly, the lower the ratio, the more likely it is that an employee will find a doctor who is willing to issue a certificate confirming the inability to work. A different intuition suggests a positive link between sickness absence and the citizen-doctor ratio, if the latter is interpreted as a

[13] We require both contractual and actual work hours to be at least 15.
[14] The data to generate these regional indicators comes from the Federal Institute for Research on Building, Urban Affairs and Spatial Development.

quantitative indicator of medical support, which in turn may affect the number of times people actually have illness-related reasons to abstain from work. Hence, the relationship to absenteeism is unclear a priori. Most important for the IV analysis is the expectation that regional differences in medical support can lead to exogenously triggered differences in sickness absence rates.

5.2 IV Results

We depict the findings for the variables of interest for the first set of IV-specifications in Table 6. Our results do not suggest that there is a causal effect of sickness absence itself on the probability of being promoted or dismissed in the future. This finding is the same throughout the IV analysis, independent of variations in the approach. Sufficiently large F statistics and failed rejections of orthogonality assumptions according to the overidentification tests back up our empirical procedure. The same analysis on a sample of only full-time working individuals yields identical conclusions with regard to both outcomes.

----- Table 6 about here-----

The main insight from the second part of our IV analysis is the same for both types of job mobility. Neither for the probability of a promotion nor a dismissal do we observe a significant effect from absence rates, when these are manipulated by exogenous circumstances. The strongest effect on absence rates comes from the number of hospital beds, while the regional citizens-doctors relationship has only little significance. In consequence, the F statistics go down when using both instruments, but overidentification tests are possible. In either way, the findings are the same.

----- Table 7 about here-----

Considering fixed-individual effects in the IV analysis also leads to no other findings. While F statistics become small due to little variation in the regional characteristics, the first part of the IV analysis offers this opportunity. The advantage of the second part of the IV analysis is the underlying sample size, which allows a meaningful subgroup analysis when using only the more relevant instrument of hospital beds. Hence, we separate the data according to gender and repeat the analysis shown in Specifications (1) and (3) for each subsample. We observe no significant effect for either of the two subgroups.

The IV analysis shows that when we disentangle absence due to illness from effort-related aspects of worker behaviour, and try to isolate the effect of the former, the link between the variable of interest and labour market outcomes disappears. Those individuals who increase or decrease absence in response to exogenous variation do not experience career events with different probabilities. However, this does not imply that abstaining from work is without repercussions. The strong link in all our previous analyses (Section 4) suggests that absenteeism in general expresses some form of career-relevant behaviour pattern. Furthermore, the above-mentioned evidence on implications for future earnings levels (e.g. Markussen 2012) suggest that absence rates may be more than a signal or proxy for de facto career determinants, such as one's attitude towards work. Future research has to find out whether this is because wages can respond more flexibly or whether some form of labour market-specifics are at play.

6. Conclusion

Motivated by an increasing body of research that assumes sickness-related absence to be a proxy for workers' effort, this paper constitutes the first analysis of a representative household data set to analyse how absenteeism relates to future career events. We can substantiate previous findings from firm data for one of the largest and frequently used panel data sets, that is, the German Socio-Economic Panel (SOEP) study. Lower absence rates go along with higher probabilities of being promoted in the subsequent year and a reduction in the likelihood of being dismissed. These main findings are robust to the consideration of fixed individual effects and to a whole variety of sensitivity checks. As a rare exception, we find that absentees with long-term sickness spells do not experience lower promotion probabilities. Contrary to most existing studies, we can exploit comprehensive information on people's individual health levels. We find that this factor plays only a marginal role for career advancement itself and accordingly does not impair the interpretation of absence data. Moreover, the strong signal that comes from absenteeism is particular impressive when comparing the results to alternative proxies for career-relevant attributes of individual behaviour. Variations in hours worked are unrelated to subsequent career events, while lower job satisfaction is generally associated with higher subsequent job mobility.

A second motivation for our paper stems from studies that propose a causal link between illness-related absence and future earnings. Using multiple instruments for the analysis of job mobility, we do not obtain evidence supporting the notion that additional absenteeism itself

makes a big difference for an individual's career. Assuming that effort and other hard to observe but career-relevant factors are constant, additional sickness-related time off work does not causally reduce chances of being promoted or increase the probability of being dismissed. Whether the earnings effect of absenteeism is a particular Scandinavian phenomenon or other aspects are at play has to be clarified in future contributions.

Finally, we observe no consistent evidence of gender differences in the link between sickness absence and subsequent career events. This means that the signal character of being absent from work for career advancement is rather similar across the genders in our investigation. Given that females typically go do the doctor and report sick more often than males, this finding for representative data is interesting and contrasts with previous evidence. Future research should find out whether our insights extrapolate to other nation-wide data sets and clarify the role of potential country- and culture-specific aspects in the implications when the individual employee is missing at work.

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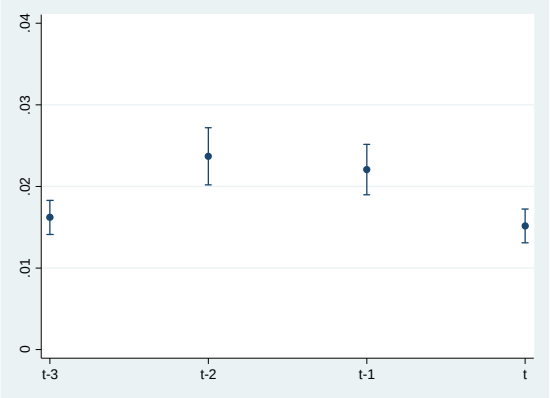
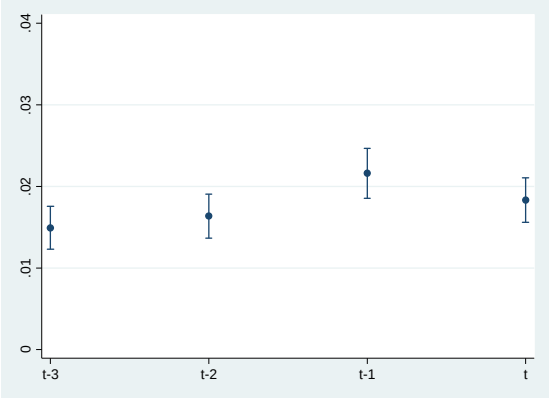
Figure 1 Average sickness-related absence rate in the years prior to job mobility

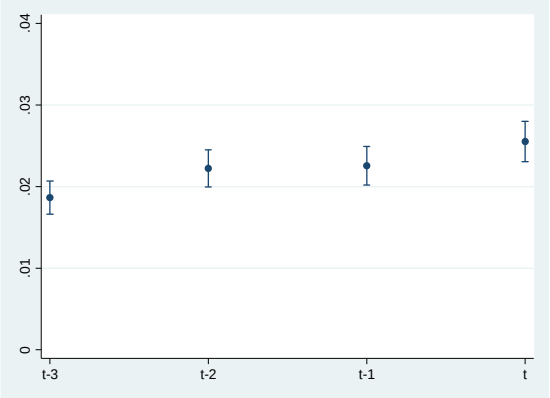
A. Promotion in t+1



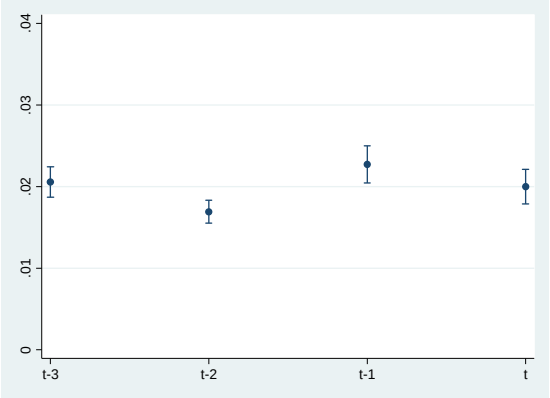
B. Transfer in t+1



C. Dismissal in t+1



D. Quit in t+1



Notes: Individuals are only included when they meet the sample criteria (see Section 3) for all four time points. Observation numbers are 106 (promotions), 98 (transfers), 197 (dismissals), and 219 (quits). Weighted averages in absence rates are plotted along with the 95% confidence intervals.

Table 1 Main data sample (selection of statistics)

Data:	all	(0) None	(1) Promotion	(2) Transfer	(3) Dismissal	(4) Quit
Female	0.29	0.29	0.35	0.36	0.27	0.35
Age	41.17	41.37	35.40	39.67	40.14	34.77
Education years	12.25	12.23	13.80	13.42	11.77	12.71
Children	0.67	0.67	0.54	0.68	0.66	0.68
Partner	0.82	0.82	0.74	0.83	0.80	0.82
Married	0.62	0.63	0.44	0.55	0.59	0.52
Tenure	12.58	12.81	9.50	12.38	7.62	6.57
Rank 1 (low autonomy)	0.12	0.12	0.04	0.03	0.20	0.10
Rank 2	0.27	0.27	0.14	0.10	0.39	0.25
Rank 3	0.33	0.33	0.37	0.45	0.24	0.35
Rank 4	0.24	0.24	0.42	0.33	0.15	0.26
Rank 5 (high autonomy)	0.04	0.04	0.02	0.09	0.02	0.03
Subjective health	2.64	2.64	2.74	2.55	2.61	2.79
Health satisfaction	7.10	7.10	7.12	6.87	6.94	7.36
Job satisfaction	7.09	7.12	6.72	6.94	6.45	6.63
Actual work hours	43.35	43.30	43.91	43.65	44.28	45.01
Sickness-related absence						
Incidence	0.56	0.55	0.55	0.58	0.56	0.64
Days	5.41	5.37	4.17	6.14	7.01	6.33
Rate	0.02	0.02	0.02	0.02	0.03	0.03
N	46179	44008	313	243	758	857

Source: The SOEP data is for the years 1994 to 2009 (with additional data from waves 2010 and 2011).
Notes: Mean statistics are displayed. Complete statistics of all control variables are in Table A1.

Table 2 Sickness absence and subsequent job mobility (multinomial logit model**)**

A) Main sample

Specification:	(1)			
	Promotion	Transfer	Dismissal	Quit
Absence rate	-7.298**	3.925	6.169***	5.093***
	(3.013)	(3.316)	(1.277)	(1.382)
Year dummies	Yes			
Set of controls	Yes			
N	46179			
Log likelihood	-33939315			

B) No newcomers

Specification:	(1)			
	Promotion	Transfer	Dismissal	Quit
Absence rate	-8.334**	2.373	6.082***	3.277
	(3.461)	(3.624)	(1.624)	(1.994)
Year dummies	Yes			
Set of controls	Yes			
N	33276			
Log likelihood	-19948339			

Source: The SOEP data is for the years 1994 to 2009 (with additional data from waves 2010 and 2011).
Notes: Individuals subsequently experience a promotion, a transfer, a dismissal, they quit or they stay in the firm without job change. Control variables include gender, age, age squared, education years, employment experience, unemployment experience, log equivalent household income, home ownership, housing conditions, living area, household members, children, partnership, married, side job, tenure, tenure squared, public sector, industry sector, firm size, log net earnings, white-collar, civil servant, hierarchical rank, regional unemployment as well as state dummies. Robust standard errors are in parentheses. Sample weights are used. The data set analysed in Panel B includes only individuals who are at least in their fourth year of tenure and are without job change for the last three years. Levels of significance are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 Sickness absence and subsequent promotion (linear probability models)

A) Pooled regressions

Specification:	(1)	(2)	(3)	(4)
Absence rate	-0.046***	-0.047***	-0.054***	-0.055***
	(0.017)	(0.017)	(0.020)	(0.020)
Subjective health			0.001	0.001
			(0.001)	(0.001)
Health satisfaction			-0.009	-0.003
			(0.006)	(0.005)
Job satisfaction				-0.013**
				(0.005)
Actual work hours				0.001
				(0.001)
Year dummies	Yes	Yes	Yes	Yes
Set of controls	No	Yes	Yes	Yes
N	44321	44321	44321	44321
R^2	0.002	0.015	0.015	0.016

B) Fixed-effects regressions

Specification:	(1)	(2)	(3)	(4)
Absence rate	-0.053**	-0.054**	-0.054**	-0.055**
	(0.027)	(0.026)	(0.026)	(0.026)
Subjective health			0.001	0.001
			(0.001)	(0.001)
Health satisfaction			-0.003	0.003
			(0.007)	(0.007)
Job satisfaction				-0.018***
				(0.006)
Actual work hours				0.000
				(0.002)
Year dummies	Yes	Yes	Yes	Yes
Set of controls	No	Yes	Yes	Yes
N	44321	44321	44321	44321
R^2	0.002	0.012	0.012	0.012

Source: The SOEP data is for the years 1994 to 2009 (with additional data from waves 2010 and 2011).
Notes: Individuals subsequently experience a promotion or they stay in the firm without job change. Control variables are the same as listed in Table 2 (excluding gender and linear age in fixed-effects regressions of Panel B). Robust standard errors are in parentheses. Sample weights are used. Levels of significance are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 Sickness absence and subsequent dismissal (linear probability models)

A) Pooled regressions

Specification:	(1)	(2)	(3)	(4)
Absence rate	0.104***	0.114***	0.115***	0.111***
	(0.028)	(0.028)	(0.029)	(0.029)
Subjective health			0.001	0.001
			(0.002)	(0.002)
Health satisfaction			-0.003	0.013*
			(0.007)	(0.007)
Job satisfaction				-0.033***
				(0.006)
Actual work hours				0.001
				(0.002)
Year dummies	Yes	Yes	Yes	Yes
Set of controls	No	Yes	Yes	Yes
N	44,766	44,766	44,766	44,766
R^2	0.003	0.026	0.026	0.028

B) Fixed-effects regressions

Specification:	(1)	(2)	(3)	(4)
Absence rate	0.089***	0.080***	0.079***	0.078***
	(0.029)	(0.028)	(0.028)	(0.028)
Subjective health			-0.001	-0.001
			(0.001)	(0.001)
Health satisfaction			-0.002	0.007
			(0.006)	(0.006)
Job satisfaction				-0.025***
				(0.006)
Actual work hours				0.001
				(0.002)
Year dummies	Yes	Yes	Yes	Yes
Set of controls	No	Yes	Yes	Yes
N	44,766	44,766	44,766	44,766
R^2	0.003	0.033	0.033	0.034

Source: The SOEP data is for the years 1994 to 2009 (with additional data from waves 2010 and 2011).
Notes: Individuals subsequently experience a dismissal or they stay in the firm without job change. Control variables are the same as listed in Table 2 (excluding gender and linear age in fixed-effects regressions of Panel B). Robust standard errors are in parentheses. Sample weights are used. Levels of significance are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 Sickness absence and subsequent job mobility (gender differences)

Specification:	(1)	(2)	(3)	(4)
Dependent variable:	**Probability of promotion**		**Probability of dismissal**	
Female	0.001	-0.003	-0.004**	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Absence rate	-0.044**	-0.046**	0.126***	0.126***
	(0.020)	(0.020)	(0.036)	(0.038)
IA: Absence rate	-0.029	-0.002	-0.033	-0.114**
X female	(0.037)	(0.030)	(0.056)	(0.053)
Method	LPM Pooled	LPM Pooled	LPM Pooled	LPM Pooled
Year dummies	Yes	Yes	Yes	Yes
Set of controls	Yes	Yes	Yes	Yes
Health controls	Yes	Yes	Yes	Yes
Newcomers	Included	Excluded	Included	Excluded
N	44321	32246	44766	32470
R^2	0.015	0.013	0.026	0.019

Source: The SOEP data is for the years 1994 to 2009 (with additional data from waves 2010 and 2011). *Notes*: Absence rates are de-meaned. In Specifications 1 and 2 (3 and 4), individuals either subsequently experience a promotion (dismissal) or they stay in the firm without job change. Control variables are the same as listed in Table 2. Health controls include variables for subjective health and health satisfaction. Robust standard errors are in parentheses. Sample weights are used. The data set analysed in Specifications 2 and 4 includes only individuals who are at least in their fourth years of tenure and are without job change for the last three years. Levels of significance are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6 Sickness absence and subsequent job mobility (instrumental variable analysis I)

1st stage				
Specification:	(1)	(2)	(3)	(4)
Dependent variable:	**Absence rate**			
Subjective Health	-0.005***	-0.005***	-0.005***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Health satisfaction	-0.012***	-0.012***	-0.013***	-0.013***
	(0.003)	(0.003)	(0.003)	(0.003)
IV1: Sick-pay reform	-0.003**	-0.003**	-0.003**	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
IV2: Work accident	0.023***	0.024***	0.023***	0.023***
	(0.002)	(0.002)	(0.002)	(0.002)
2nd stage				
Specification:	(1)	(2)	(3)	(4)
Dependent variable:	**Probability of promotion**		**Probability of dismissal**	
Absence rate	0.089	0.126	0.131	0.250
	(0.226)	(0.268)	(0.331)	(0.382)
Year dummies	Yes	Yes	Yes	Yes
Set of controls	Yes	Yes	Yes	Yes
Health controls	Yes	Yes	Yes	Yes
Year 1996	Included	Excluded	Included	Excluded
N	12629	10095	12814	10248
Wald F statistic	70.073	54.111	71.225	54.300
Hansen J statistic (p-value)	0.895	0.847	0.364	0.298

Source: The SOEP data is for the years 1994 to 1998 (with additional data from waves 1999 and 2000). *Notes*: In Specifications 1 and 2 (3 and 4), individuals either subsequently experience a promotion (dismissal) or they stay in the firm without job change (second stage). On the first stage, the dependent variable is the absence rate. Control variables on both stages are the same as listed in Table 2. Health controls include variables for subjective health and health satisfaction. Robust standard errors are in parentheses. Sample weights are used. The data set analysed in Specifications 2 and 4 excludes all observations from the survey wave of 1996. Levels of significance are $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7 Sickness absence and subsequent job mobility (instrumental variable analysis II)

1st stage				
Specification:	(1)	(2)	(3)	(4)
Dependent variable:	**Absence rate**			
Subjective health	-0.005***	-0.005***	-0.005***	-0.005***
	(0.000)	(0.000)	(0.000)	(0.000)
Health satisfaction	-0.017***	-0.017***	-0.017***	-0.017***
	(0.002)	(0.002)	(0.002)	(0.002)
Regional indicator:	0.828***	0.973***	0.845***	0.992***
Hospital beds per citizen	(0.195)	(0.203)	(0.195)	(0.203)
Regional indicator:		0.044*		0.045*
Citizens (in 10') per doctor		(0.026)		(0.026)
2nd stage				
Specification:	(1)	(2)	(3)	(4)
Dependent variable:	**Probability of promotion**		**Probability of dismissal**	
Absence rate	0.054	0.024	-0.533	-0.647
	(0.710)	(0.631)	(1.074)	(0.910)
Year dummies	Yes	Yes	Yes	Yes
Set of controls	Yes	Yes	Yes	Yes
Health controls	Yes	Yes	Yes	Yes
N	35555	35555	35859	35859
Wald F statistic	17.962	11.520	18.759	11.912
Hansen J statistic (p-value)		0.925		0.790

Source: The SOEP data is for the years 1998 to 2009 (with additional data from waves 2010 and 2011). *Notes*: In Specifications 1 and 2 (3 and 4), individuals either subsequently experience a promotion (dismissal) or they stay in the firm without job change (second stage). On the first stage, the dependent variable is the absence rate. Control variables on both stages are the same as listed in Table 2. Health controls include variables for subjective health and health satisfaction. Robust standard errors are in parentheses. Sample weights are used. Levels of significance are $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix A

Table A1 Main data sample (complete statistics)

Data:	all	(0) None	(1) Promotion	(2) Transfer	(3) Dismissal	(4) Quit
Female	0.29	0.29	0.35	0.36	0.27	0.35
Age	41.17	41.37	35.40	39.67	40.14	34.77
Education years	12.25	12.23	13.80	13.42	11.77	12.71
Employment experience	19.22	19.44	13.04	17.12	18.24	12.32
Unemployment experience	0.29	0.29	0.13	0.10	0.70	0.34
Equivalent household income	1877.3	1880.6	1994.3	2136.5	1624.5	1788.8
Home owner	0.47	0.48	0.38	0.47	0.33	0.37
Housing cond. top	0.69	0.69	0.68	0.69	0.59	0.63
Housing cond. fair	0.29	0.28	0.30	0.28	0.38	0.34
Housing cond. bad	0.03	0.03	0.02	0.03	0.03	0.03
Living area	98.24	98.59	90.64	102.63	87.38	91.45
Household members	2.74	2.74	2.32	2.64	2.75	2.64
Children	0.67	0.67	0.54	0.68	0.66	0.68
Partner	0.82	0.82	0.74	0.83	0.80	0.82
Married	0.62	0.63	0.44	0.55	0.59	0.52
Side job	0.07	0.07	0.07	0.09	0.07	0.09
Tenure	12.58	12.81	9.50	12.38	7.62	6.57
Public sector	0.28	0.29	0.45	0.50	0.07	0.14
Agriculture, Energy, Mining	0.03	0.03	0.02	0.01	0.04	0.01
Manufacturing	0.25	0.26	0.16	0.17	0.24	0.27
Construction	0.17	0.16	0.09	0.15	0.28	0.15
Trade	0.11	0.10	0.08	0.03	0.17	0.17
Transport	0.06	0.06	0.07	0.07	0.04	0.06
Bank, Insurance	0.05	0.05	0.11	0.09	0.02	0.03
Services	0.10	0.09	0.10	0.08	0.13	0.15
Small company	0.16	0.16	0.03	0.04	0.38	0.25
Medium company	0.29	0.29	0.14	0.17	0.36	0.37
Large company	0.26	0.27	0.28	0.22	0.17	0.21
Big company	0.28	0.28	0.55	0.57	0.09	0.17
Net earnings	1770.3	1778.0	1813.2	2053.9	1418.6	1565.5
Blue-collar	0.36	0.36	0.14	0.14	0.58	0.34
White-collar	0.55	0.55	0.63	0.57	0.41	0.64
Civil servants	0.09	0.09	0.23	0.28	0.00	0.02
Rank 1 (low autonomy)	0.12	0.12	0.04	0.03	0.20	0.10
Rank 2	0.27	0.27	0.14	0.10	0.39	0.25
Rank 3	0.33	0.33	0.37	0.45	0.24	0.35
Rank 4	0.24	0.24	0.42	0.33	0.15	0.26
Rank 5 (high autonomy)	0.04	0.04	0.02	0.09	0.02	0.03
Regional unemployment rate	10.97	10.94	11.26	11.34	13.09	10.64
N	46179	44008	313	243	758	857

Source: The SOEP data is for the years 1994 to 2009 (with additional data from waves 2010 and 2011).
Notes: Mean statistics are displayed.

Table A2 Sickness absence and subsequent job mobility (complete results**)**

Specification:	(1)	(2)	(3)	(4)
Dependent variable:	**Probability of promotion**		**Probability of dismissal**	
Female	0.001		-0.004**	
	(0.002)		(0.002)	
Age	-0.001**		-0.001	
	(0.001)		(0.001)	
Age squared	0.000	-0.000	0.000*	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Education	0.002***	-0.000	-0.000	-0.001
years	(0.000)	(0.002)	(0.000)	(0.001)
Employment	0.000	0.002	0.000	-0.006*
experience	(0.000)	(0.003)	(0.000)	(0.003)
Unemployment	-0.000	-0.002	0.004**	-0.156***
experience	(0.000)	(0.005)	(0.002)	(0.022)
Log equivalent	0.001	0.000	-0.001	0.008*
household income	(0.002)	(0.004)	(0.003)	(0.004)
Home ownership	-0.001	0.005	-0.004**	-0.006**
	(0.001)	(0.004)	(0.002)	(0.003)
Fair housing	0.000	-0.003*	0.002	-0.001
Conditions	(0.002)	(0.002)	(0.002)	(0.002)
Bad housing	-0.002	0.004	-0.008*	-0.008*
Conditions	(0.004)	(0.005)	(0.004)	(0.004)
Living area	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Household	-0.002**	-0.005***	-0.000	-0.001
Members	(0.001)	(0.002)	(0.001)	(0.002)
Children	0.001	0.000	0.001	0.002
	(0.001)	(0.002)	(0.001)	(0.002)
Partnership	-0.002	-0.003	-0.001	-0.004
	(0.003)	(0.004)	(0.003)	(0.004)
Married	-0.000	0.005	-0.001	0.006*
	(0.002)	(0.006)	(0.002)	(0.003)
Side job	-0.001	-0.001	0.002	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)
Tenure	-0.000	-0.000	-0.002***	0.004***
	(0.000)	(0.001)	(0.000)	(0.001)
Tenure squared	-0.000	0.000	0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Public sector	-0.000	-0.003	-0.012***	-0.007**
	(0.002)	(0.005)	(0.002)	(0.003)

Continued on the next page

Manufacturing	-0.002	-0.001	-0.002	-0.005
	(0.003)	(0.004)	(0.003)	(0.008)
Construction	-0.002	-0.001	0.007**	-0.004
	(0.002)	(0.004)	(0.003)	(0.008)
Trade	-0.000	0.001	0.003	-0.005
	(0.003)	(0.005)	(0.004)	(0.009)
Transport	-0.000	0.001	-0.002	-0.003
	(0.003)	(0.006)	(0.003)	(0.012)
Bank and Insurance	0.005	-0.006	0.001	-0.007
	(0.004)	(0.008)	(0.003)	(0.013)
Services	-0.001	0.003	0.002	-0.002
	(0.003)	(0.004)	(0.004)	(0.006)
Medium company	0.003**	0.003*	-0.014***	-0.010
	(0.001)	(0.002)	(0.003)	(0.006)
Large company	0.007***	0.001	-0.018***	-0.009
	(0.002)	(0.003)	(0.003)	(0.006)
Big company	0.013***	-0.009**	-0.020***	-0.011*
	(0.002)	(0.004)	(0.003)	(0.007)
Log net earnings	-0.004	-0.020***	-0.005	-0.004
	(0.003)	(0.005)	(0.004)	(0.006)
White-collar	-0.001	-0.004	-0.006**	0.000
	(0.002)	(0.003)	(0.003)	(0.004)
Civil servant	0.011**	0.008	-0.001	-0.003
	(0.005)	(0.010)	(0.003)	(0.009)
Rank 2	-0.002	-0.002	-0.002	-0.001
	(0.002)	(0.003)	(0.004)	(0.005)
Rank 3	-0.001	-0.003	-0.001	-0.006
	(0.002)	(0.004)	(0.004)	(0.006)
Rank 4	0.004	-0.010**	0.001	-0.009
	(0.003)	(0.005)	(0.005)	(0.007)
Rank 5	-0.009**	-0.036***	0.002	-0.007
	(0.004)	(0.010)	(0.006)	(0.009)
Regional unemployment	0.001	0.002	-0.001	0.002*
	(0.001)	(0.001)	(0.001)	(0.001)
Absence rate	-0.054***	-0.054**	0.115***	0.079***
	(0.020)	(0.026)	(0.029)	(0.028)
Method	Pooled	Fixed-effects	Pooled	Fixed-effects
Year dummies	Yes	Yes	Yes	Yes
State dummies	Yes	Yes	Yes	Yes
Subjective health	Yes	Yes	Yes	Yes
Health satisfaction	Yes	Yes	Yes	Yes
N	44321	42265	44766	42646
R^2	0.015	0.012	0.026	0.033

Source: The SOEP data is for the years 1994 to 2009 (with additional data from waves 2010 and 2011).
Notes: In Specifications 1 and 2 (3 and 4), individuals either subsequently experience a promotion (dismissal) or they stay in the firm without job change. Robust standard errors are in parentheses. Sample weights are used. Levels of significance are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix B

In this appendix, we first describe the relevant parts of the SOEP questionnaire. Subsequently, we clarify how we identify a job change, classify a rank change and treat repeated alterations of job ranks. Finally, we provide evidence which supports the validity of our classification approach.

SOEP Questionnaire

The SOEP questionnaire, inter alia, contains a question asking whether the respondent currently has a job. Suppose the question is asked in wave t + 1. If the answer is positive, the respondent is next asked whether he/ she has taken up work or changed jobs (in the same company) since December 31 of the year t – 1. If the response is "Yes", the month of the respective job change in year t or year t + 1 has to be provided and whether it occurred within the firm or involved a change of employer. A crucial implication of this design structure is that for those job changes that took place in t + 1 but after the annual interview, the event is reported only in wave t + 2. This forces us to consider always the two subsequent survey waves and thus a multiple-year window. Also note that absence information in wave t + 1 relates to year t. A challenging situation arises if the job change occurred in year t + 1 prior to the annual interview in that year. In this case, respondents have to provide information on this job change twice, as the design of the questionnaire requires them to report on it in waves t + 1 and t + 2. Only by being aware of these technical issues is it possible to identify the correct wave in which an individual reports on the characteristics of a new job for the first time.

Somewhat later in the questionnaire, people have to provide information about the date when they commenced work with the current employer. The questions relating to the end of an employment relationship are structurally the same as those concerning job changes. First, the respondent is asked whether her/ her employment relationship has ended since December 31 of year t – 1. Then the exact month of the termination and its cause have to be stated.

Job Change

From the information provided in the SOEP questionnaire we can straightforwardly derive whether a respondent has experienced an incident of internal job mobility, has been dismissed or has resigned either in year t or year t + 1. For internal job changes, we next have to determine if they also involved a rank change.

Rank Change

In every SOEP questionnaire, people have to provide information on their occupational status in their current (main) job. The question provides five broad categories, namely blue-collar worker, white-collar employee, civil servant, self-employed and being in vocational training. We focus on the first three groups of individuals. They are further sub-divided into five, six, and four sub-categories, respectively. The SOEP provides a generated variable called "autonomy in occupational activity" (variable name: *autono*) that is based upon these sub-categories. For each employed individual, the variable offers a value on a 5-point scale that ranges from "low" (1) to "high" (5). We use this measure of autonomy as an indicator of occupational rank because it reflects both differences within the groups of blue-collar workers, white-collar employees, and civil servant and distinctions across these three groups.

Table B1 provides information on the resulting occupational categories. It also contains the classification employed by Lluis (2005) who analysed wages and intra-firm mobility for Germany. Relative to Lluis (2005), our classification is more differentiated and also provides a categorisation of white-collar foreman (Industry and works foreman in a salaried position).

Table B1 Hierarchical positions

	SOEP *autono* levels	Lluis (2005) ranks	Net earnings (average) in Euros
Blue-collar worker			
Untrained worker	1	1	1181.89
Trained worker ("angelernt")	1	1	1371.96
Trained and employed as skilled worker	2	1	1448.15
Foreman ("Vorarbeiter")	3	2	1681.16
Master craftsman ("Meister")	4	3	1791.37
White-collar worker			
Industry and works foreman in a salaried position	4	-	1228.00
Employee with simple duties, without training/education certificate	2	1	1269.35
Employee with simple duties, with training/education certificate	2	1	1248.40
Employee with qualified duties (e.g. executive officer, bookkeeper, technical draftsman)	3	2	1596.10
Employee with highly qualified duties or managerial function (e.g. scientist, attorney, head of department)	4	3	2461.07
Employee with extensive managerial duties (e.g. managing director, manager, head of a large firm or concern)	5	4	3810.55
Civil servant (including judges and professional soldiers)			
Lower level	2	1	1626.05
Middle level	3	2	1883.14
Upper level	4	3	2453.97
Executive level	5	4	3371.32

Source: Own calculations SOEP 1994 – 2009, Lluis (2005, p. 759)

We define a promotion as follows: First, if the respondent's occupational category was higher in the new job than in the previous one, he/ she is considered to be promoted. Second, if the rank according to Table B1 did not change, we compared the net labour income in the old job to the respective income in the new job. Because job changes of the following year are often reported in the next SOEP interview but one, we compare income levels over a two-year period. The average increase in net income in our sample is about 6.3 %. All cases of job changes that go along without rank change but with a net labour income greater than this threshold value are defined as promotions. These cases add to those defined as hierarchical promotions. Meanwhile, a transfer occurs if a respondent changes jobs and the new jobs is characterised either by a lower rank than the previous one, which is a hierarchical demotion, or by the same rank and no above average wage increase, using the same threshold.

Association Between Job Mobility and Sickness-related Absence

Having identified a job change or a termination of the employment contract, for example, in period t + 1, we combine this information with data on absence in period t. In order to ensure that the absence information refers to one pre-mobility job only, we, first, require respondents to be beyond their first year of working for the current firm and to be without recent job change experience. This restriction rules out that the absence data relate to another employer. Second, we have to take care of instances of repeated job mobility. In such cases, observed absence behaviour prior to the second job move may be contemporaneous to or trailing the first job move.

For the sake of completeness, we briefly describe the procedure in regard to special and rare situations when several job mobility events take place within the subsequent time window. Our guiding motive is to leave out all those problematic cases which would require rather arbitrary decisions in order to incorporate them into the analysis. First, we exclude cases when a worker experiences both promotion and demotion following each other closely. Similarly, we exclude cases of resignations and dismissals that are directly followed by re-employment at the same employer, which we identify using the tenure information. Third, we indirectly drop all firm internal job moves that are followed by an immediate job termination, which results from our prerequisite to be observed in the same firm and to report on correspondingly increased tenure in both subsequent wave interviews (see Section 3.2). Finally, when both a dismissal and a resignation (quit) take place in the subsequent period, we include the individual observation but consider only the first event reported.

Validity of Identification Approach

Descriptive statistics on people's subsequent earnings underline the accuracy of our rank-based identification strategy. While average wages for the stably employed increase by 6.3% over the two-year time window, promotions and demotions according to comparisons between the new and the old job's rank are above, respectively below, this level and thus in line with expectations. The narrow definition of promotions (observation number: 107) based on rank increase relates to an average increase of 14.8% in net earnings. Using the Lluis (2005) definition, we observe a 15.0% average increase (observation number: 94). Narrowly defined demotions are rare (observation numbers: 51/ 54). In contrast to Lluis (2005), we observe wage increases that are below and not above the average (4.8%/ 4.1%). This observation enhances confidence in our idea to consider information from multiple SOEP waves to determine rank-based job changes.

In additional regressions, we replace our extended definition of promotions (that includes lateral moves with above-average increases) by the narrow definitions based on rank increase only. For both classifications (see Table B1), our preferred one and the one employed by Lluis (2005), we find significant effects of absence rates on promotion probabilities in both pooled and fixed-effects specifications.

Another way to check the validity of identified internal job moves is to look at the subjective comparisons of old and new job by the interviewees. SOEP data that is available until 2007 allows checking whether employees report improvements or deteriorations with regard to several aspects of work. For the type of work, for instance, a majority of 62.8% (respectively 71.4%) among those being promoted according to the main (respectively narrow, i.e. rank-based) definition report improvements. This contrasts strongly with those cases of rank-based demotions, for which only 20% report improvements. We observe similar evidence in favour of our identification strategy when we continue this analysis for other aspects of work that job changers are asked to assess in comparison to their previous job.

Finally, we can also use the information on expectations that is available biennially in the SOEP starting in 1999 to establish further confirmation of accuracy in identified job moves. Additional analyses for all four cases of job mobility (including dismissals and resignations as firm exits) reveal that each future event is predicted by self-reported expectations observed one or two years in advance. For instance, when respondents report a higher chance of being promoted in the near future, a promotion based on our definition did indeed take place with a significantly higher probability. In case of a transfer, the significance is weak, which however does not surprise, as the event is unlikely to be known in advance by many employees. In contrast, individuals are probably better informed when e.g. their promotion is forthcoming.

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