Are workers with a long commute less productive?

An empirical analysis of absenteeism

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Abstract. We hypothesize, and test for, a negative effect of the length of the worker’s commute on worker’s productivity, by examining whether the commute has a positive effect on worker’s absenteeism. We identify this effect using employer-induced changes in commuting distance. Our estimates for Germany indicate that commuting distance induces absenteeism with an elasticity of about 0.07. On average, absenteeism would be about 16 percent less if all workers would have a negligible commute. These results are consistent with extended urban efficiency wage models.

Keywords: absenteeism, commuting, productivity. JEL code R23; J22; J24

1. Introduction

The detrimental effect of commuting distance on worker’s productivity may concern employers, as there are many claims that workers with long commuting distances are more absent and arrive late for work (RCI, 2001). This issue cannot be analysed within standard urban economic models. In the standard urban economic model, the labour market is relatively undeveloped, as it is assumed that the labour market is fully competitive and productivity is given, so that unemployment is absent. This is at odds with most of the labour market literature, which allows for unemployment. One of the most important labour economic theories accounting for unemployment and productivity differences is efficiency wage theory, which incorporates workers’ shirking behaviour (e.g. reducing work effort, absenteeism) and wage setting by employers (Cahuc and Zylberberg, 2004). Hence, a logical step forward is to combine urban economic models with efficiency wage theory, the so-called urban efficiency wage models. A range of urban efficiency wage models have been developed over the last years: for an overview, see the recent book by Zenou (2009).

1 In the efficiency wage model, initially developed by Shapiro and Stiglitz (1984), workers may shirk (i.e. reduce effort levels) and employers observe shirking with a certain probability. Workers who are caught shirking are fired and become unemployed. The worker's shirking decision depends on the level of unemployment (which determines the re-employment probability) and the wage. Given this setup, employers optimally determine the wage level, and unemployment is endogenously determined.
In urban efficiency wage models, workers' shirking behaviour and employers' wage setting is combined with an explicit spatial housing market with endogenous house prices. Hence, urban efficiency wage models allow one to study the relationship between workers' shirking behaviour, which negatively affects productivity, and the length of the commute taking into account the endogenous choice of residence location (Zenou and Smith, 1995; Zenou, 2002; Brueckner and Zenou, 2003; Ross and Zenou, 2008; Zenou, 2008, 2009). Importantly, this contrasts with a large literature in urban economics which assumes that workers' productivity is independent of their commute. We aim to test this assumption using data on absenteeism. Absenteeism, the number of days absent from work for sickness reasons during a certain period, is closely related to shirking behaviour, as argued by Barmby et al. (1994), and therefore a reasonable (inverse) measure of worker’s productivity. Gibbons and Machin (2006) emphasize that there is a lack of evidence regarding the effect of commuting on absenteeism.

The urban efficiency wage literature employs two distinct behavioural mechanisms to analyse how the length of the commute may affect worker’s absenteeism. The first mechanism is that there is an involuntary relationship between workers' effort level and commuting (a relationship which is not under the control of workers). So, one may argue that workers with long commutes are more likely to fall ill, for example, due to increased fatigue (Koslowsky et al., 1995), and are therefore more likely absent for sickness reasons than workers with short commutes. In this case, involuntary absenteeism is a positive function of the length of the commute (see Zenou, 2002).

The second mechanism is based on a voluntary relationship and receives much more attention in the literature (e.g. Ehrenberg, 1970). The idea is that the worker's utility derived from a job depends on the length of the commute, because the worker's net wage (the wage minus monetary commuting costs) and leisure time are reduced when commuting costs
respectively commuting time increase.\(^2\) The worker's shirking decision depends then on the length of the commute and local unemployment rate, which both depend on the chosen residence location. One important question is then whether (and how), in equilibrium, the length of the commute affects workers' shirking behaviour (so given endogenously determined wages, the urban unemployment rate and house prices).\(^3\)

The answer to this question depends on the ability of employers to observe workers' commuting costs (Ross and Zenou, 2008). One may assume that employers observe workers' commuting costs, so employers are able to prevent shirking by varying wages. An alternative assumption is that employers do not observe these costs. This seems a useful assumption for two reasons. First, it seems unrealistic that employers perfectly observe these costs, so some of these costs will be unobserved. Second, employers may not have the legal means to adapt the wage as a function of residence location (after hiring), so they behave myopically with respect to commuting costs. When commuting costs are not fully observed, shirking behavior will vary with the length of the commute, and the nature of this relationship will depend on the exact form of the worker's utility function. When leisure time and work effort are substitutes\(^4\), then the relationship between shirking and the length of the commute is positive: see Ross and Zenou (2008, proposition 1). When they are complements\(^5\), the relationship is negative. The empirical results by Ross and Zenou (2008), which is the only previous study we are aware of that attempts to test the predictions of urban efficiency wage models but relies on information from spatial variation in wages and unemployment rates, support the assumption that leisure time and work effort are substitutes and therefore that absenteeism and

\(^2\) So, workers with a longer commute might be considered to have a lower cost of absence.

\(^3\) Wages are set conditional on a job based on the workers' commuting costs.

\(^4\) This is a reasonable assumption, for example, because lower leisure at home may imply that the worker has less time for rest and, as a result, the benefit from taking leisure while at work rises, see Zenou (2009, p. 225).

\(^5\) If someone's leisure time at home is reduced, social life may suffer substantially which, in turn, reduces the benefits derived from leisure at home. The decline in quality of social life is likely to reduce overall demand for personal time. As a result, the benefit from being at home falls because a substantial amount of time at home is already available for relaxation, see Zenou (2009, p. 225–226).
the length of the commute are positively related. This study does not employ a direct measure of shirking or productivity.

The above discussion implies that there are two behavioural mechanisms (an involuntary and a voluntary one) that support the idea that worker's absenteeism depends positively on the endogenously chosen commuting. As far as we know, there are no convincing direct empirical tests of this idea.\(^6\) To test the causal effect of commuting on absenteeism, as implied by urban efficiency wage theory, we take into account that residence location is endogenously chosen with respect to shirking. We address this endogeneity issue by including *residence fixed effects* (as well as worker and job fixed effects), so the effect of commuting on absenteeism is identified using differences in the workplace location keeping residence location constant.\(^7\) In urban models with *one* central business district, there is a one-to-one relationship between residence location and the length of the commute, so that given residence fixed effects, the effect of the length of the commute on absenteeism cannot be identified. In a more realistic setting with multiple workplace locations, commuters do not commute to a central location: therefore one may control for the worker's residence location and still observe variation in commuting distances, but it is then important to deal with the endogeneity of commuting distance due to endogenous choice of the workplace location. We address this issue by employing changes in commuting distance that are employer-induced, which guarantees that commuting distance is exogenous.

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\(^6\) However, there are several studies with suggestive evidence that relies on descriptive statistics (see Koslowski et al., 1995). Allen (1981) examines this issue but ignores endogeneity of residence location and uses cross-section data.

\(^7\) See Gutiérrez-i-Puigarnau and van Ommeren (2010) who use the same idea in the context of labour supply and commuting.
2. Empirical approach

2.1 Main empirical results

We use nine waves of the 1999–2007 German Socio-Economic Panel (GSOEP) survey (which includes about 48,000 observations) to study the effect of commuting on absenteeism. Absenteeism refers to the number of days reported absent during the year before the interview date. Commuting distance, our main explanatory variable of interest, is measured at the interview date.

We use a combination of residence, job and workers' fixed effects to estimate the effect of distance on absenteeism. To use worker fixed effects is recommended in the absenteeism literature to deal with workers' unobserved heterogeneity (Dionne and Dostie, 2007). To use job and residence fixed effects is not standard, but is useful as explained later on to use variation in commuting distance that is due to exogenous workplace relocations. Thus, the effect of distance on absenteeism is identified using the worker's change in reported distance on the worker's change in absenteeism (within-worker variation in absenteeism is quite large and equals to 35% of all variation). When the worker's commuting distance has changed during the year before the interview (due to a workplace move), the commuting distance reported at the interview date will only apply to part of the year for which absenteeism is reported, which creates a measurement error and therefore a bias in the estimates. In Appendix B, we show that given fixed effects, the bias in the estimated effect of distance is substantial, and given stationarity assumptions, equal to 50% of the unbiased effect. We remove this bias by excluding observations of workers for which the commuting distance changed during the year before the interview date (while keeping the other

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8 Workers who are absent are immediately obliged to inform their employers about their sick leave, but are obliged to obtain a doctor’s certificate from the fourth day of the sickness spell.

9 For the first two waves, information about distance is not available if the worker's workplace is in the residence municipality, so we have imputed a value of 5 km. Sensitivity analyses show that results are insensitive to the imputed value as well as whether these waves are included.

10 Ose (2005) estimates the effect of observed firm relocations on absenteeism, whereas our study focuses on the effect of the change in the commuting distance induced by a workplace relocation.
observations of these workers). This is unlikely to create a selection bias, because the use of worker fixed effects implies that worker-specific time-invariant selection effects are controlled for. Furthermore, the descriptive statistics for the full and selected sample are almost identical, also suggesting that sample selection is not an issue. We also ignore a small number of observations with zero commutes. We are then left with 16,762 annual observations for 7,104 workers.

We will now focus on the dependent variable of interest: the annual number of days absent. The mean number of days absent is 7.85, with a standard deviation of 19.14 (the full distribution can be found in Appendix A, Figure 1). For 43% of the observations, the worker has not been absent at all during the whole year. The number of days absent is a count variable (0, 1, 2, 3, etc), and we estimate fixed-effects negative binomial regression models. We include a large number of (time-varying) explanatory variables including commuting distance, year dummies, annual working hours, presence of children, wage, region dummies, firm size and industry dummies (see also Barmby et al., 1991; Barmby, 2002, and Barmby et al., 2002). The mean commuting distance is 15.6 km (the full distribution can be found in Appendix A, Figure 2). We have experimented with several functional forms for distance. The main results are hardly sensitive to the exact form chosen. We report log-linear specifications of commuting distance. Furthermore, we include a number of subjective and objective health indicators. These indicators include a self-reported description of current health (very good, good, satisfactory, poor, very bad), and objective indicators: number of trips to the doctor in the last three months and number of nights admitted to a hospital in the previous year. For ease of interpretation, we have annualised the doctor trip data.

11 If distance changes in year \( t \), then year \( t \) is dropped. If the distance does not change for years \( t - 1 \) and \( t + 1 \), then the coefficient of distance is identified by comparing the distance change between \( t - 1 \) and \( t + 1 \) with the change in absenteeism between years \( t - 1 \) and \( t + 1 \).
12 In Germany, workers who are absent for less than six weeks (30 uninterrupted working days) keep the same wage. Durations of absenteeism that exceed this threshold occur infrequently. In our data, only 3% of all workers are absent uninterruptedly for six weeks.
For the negative binomial model with panel data, for each observation which belongs to group $i$ at time $t$ holds that $\log E(A_i) = \beta X_{it} + \alpha_i$, where $\log E(A_i)$ denotes the logarithm of the expected number of days absent, $X_{it}$ are the explanatory variables and $\beta$ refers to parameters to be estimated. The definition of group $i$ is essential to our identification strategy, because we aim to use changes in commuting distance that are *employer-induced* and therefore exogenous (because firms typically ignore idiosyncratic preferences of workers, see Zax, 1991). In the survey analysed here, there is no information whether firms move their workplace. However, by using job, residence and worker fixed effects – thus group $i$ only includes observations of one worker with the same residence and the same job at the same employer – we infer that changes in commuting distance are caused by a (presumably exogenous) relocation of the workplace by the firm. So, we observe changes in commuting distance when the worker does not change residence and at the same time keeps the same job at the same employer. Hence, observations of the same worker belong to different groups if the worker moves residence or changes job. In our data set, we have 7,104 workers and 1,280 (residential or job) moves. The total number of groups is therefore 8,384 ($7,104 + 1,280$).

Our approach is based on exogenous changes in distance for workers who do not change job or residence, while remaining with the same employer. This may create a selection bias because the worker’s reaction to an employer-induced workplace relocation is endogenous (e.g. the worker may move residence or job, see Zax, 1991; Zax and Kain, 1996). This does not imply that our estimates are invalid, but it means that the estimates only hold for the selected group of workers who do not move residence or job when the workplace is relocated (see Angrist et al., 2000). Hence, the generality of our estimates depends on the proportion of workers who change job or residence as their employer relocates.

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13 Workplace relocation as a source of change in commuting distance is quite common. About 7–8% of Dutch firms are each year involved in relocation decisions (Weltevreden et al., 2007).

14 In our sample, about 10% of changes in commuting distance are employer-induced using this approach.

15 In Great Britain, in each year, 0.5% of workers state that they change residence because of an employer-induced workplace move (National Statistics, 2002).
their workplace. German job and residential moving rates are among the lowest in the world. In our data, the sum of these rates is only 7%, which suggests that our results hold quite generally.\footnote{By comparison, for the US, the sum of these rates is about 35%.
}

The estimation of non-linear models is usually not trivial, especially when it involves two or three-way fixed effects. Importantly, our definition of group $i$ implies that we deal with a one-way model, although we deal with several types of fixed effects. In principle, any three-way model is encompassed by a one-way model. For example, a balanced panel data set of $N$ workers that all move job $m$ times and move residence $r$ times within the period of observation is encompassed by a one-way model with $N(m + 1)(r + 1)$ fixed effects, whereas the three-way model contains $N(1 + m + r)$ fixed effects, so $Nrm$ less fixed effects. To estimate the one-way fixed-effects model is less efficient (if the three-way specification is the correct model), but the loss in efficiency is small when $m$ or $r$ is small. In our application, both $m$ and $r$ are small, so the loss in efficiency is practically negligible.

Estimation of the one-way fixed-effects negative binomial is straightforward when using a conditional maximum likelihood method, which circumvents the problem of estimating the fixed effects (in fact, the fixed effects are not identified). This method is applicable in a limited number of parametric non-linear models (main example is the logit model, but it excludes, for example, the probit model). In case of a count model, this method involves the maximization of the log likelihood conditional on the sum of the number of counts during the whole period of observation.

As can be seen from Table 1, the effect of commuting distance on the number of days absent is positive and statistically significant (at the 1% level). This result is consistent with Ross and Zenou (2008), where a positive relationship is an outcome of the theoretical model, as well as with Zenou (2002), where a positive relation between absenteeism and commuting
is assumed. The point estimate, and therefore the elasticity, is 0.0742 (the s.e. equals 0.0127). This indicates, for example, that the (expected) number of days absent is 12% higher for workers with a (one-way) commuting distance of 50 km than for workers with a distance of 10 km. Commuting distance is used in many studies as a proxy for commuting time as well as monetary costs. In the context of absenteeism, it is relevant to observe that the reduction in monetary costs of commuting while staying at home is small, as a large part of the monetary costs of commuting are fixed (e.g. purchase of car, rail discount cards, etc). This seems to imply that the effect is identified mainly through the time component of commuting costs.

To understand the magnitude of the effect, let us focus now on a hypothetical firm that actively starts to redline workers who do not live within 1 km of the workplace, such that after a certain time all workers will live at about 1 km from the firm (in the spirit of Zenou, 2002). In this case, the average logarithm of commuting distance, which is 2.12 in our data set, falls to about 0 km and absenteeism within this firm will fall by about 16% \((0.0742 \times 2.12)\) – on average, by 1.2 working days per year. Clearly, the results are not only statistically but also economically significant.

Strictly speaking we cannot exclude the possibility that commuting causes a decrease in (physical or mental) health which is unobserved and not captured by our health indicators and therefore increases absenteeism (e.g. a minor flu may neither affect the number of doctor visits nor affect subjective health measures). However, our results are insensitive to the inclusion of these health indicators (see last column of Table 1), so it is plausible that the effect of distance is also orthogonal to unobserved health indicators. This suggests that the effect identified is predominantly through voluntary absenteeism.\(^\text{18}\)

\(^{17}\) Note that such a recruitment rule is only hypothetical as it is counterproductive to the firm, because such a rule strongly reduces the supply of workers.

\(^{18}\) We have also investigated the effect of distance on health indicators, but we do not find that a longer commute leads to a deterioration of health (in contrast to claims by Koslowsky et al., 1995), in line with the above interpretation.
2.2 Sensitivity analyses

We have subjected the results to a number of sensitivity analyses. For example, we have estimated the same models as discussed above for males and females separately as the effect of distance may be gender-specific (see Vistnes, 1997). We find that the estimated effects of distance for males and females are almost exactly the same, indicating that the distance-effect identified is not gender specific. Further, we have examined the effect of possible ‘outliers’ of the dependent variable. This may be important for two reasons. First, a well-known feature of count models is that estimates are not consistent given random measurement error in the dependent variable (Winkelman, 2003). One can imagine that measurement error may be particularly large for workers with a large number of days absent. Second, workers who are absent for more than 30 working days may receive a wage reduction (this applies to about 3% of the workers). Hence, we have estimated the same models selecting only observations for which absenteeism is less than 20 days. For this sample, which contains about 95% of the full sample, the results are almost identical to the ones reported above. This indicates that the results are robust, and not due to a few outliers, and that unobserved wage reductions due to long absenteeism do not affect our estimates. Furthermore, it appears that controlling for explanatory variables does not appear to be essential for the estimated effect of distance.

We have also analysed the effect of interactions of distance with health indicators. This is relevant as one may imagine that the marginal costs of the commute are higher for unhealthy workers or for workers that visit doctors more frequently. We do not find any evidence that the interactions of distance with health indicators have an effect on absenteeism. This strongly suggests that workers’ marginal costs of commuting do not depend on the workers’ health.

We re-estimated the negative binomial model without any fixed effects. We find a much lower estimate of distance (0.0227) which is even statistically insignificant at a
common significance level of 5% (s.e. is 0.0134). Hence, cross-section estimation of the
effect of commuting on absenteeism negatively biases the results. The most plausible
explanation for this bias is that workers with unobserved positive attitudes to work are more
likely to accept jobs at long distances and are also less likely to be absent. Fixed-effects
estimators address this issue. Models without job and residence fixed effects, imply slightly
lower estimates for the effect of distance, suggesting that local unemployment, which is
controlled for by the residence fixed effects, reduces the effect of distance on absenteeism.
Recall that we have estimated models on a selective sample of workers to avoid a bias that
may occur as absenteeism is measured over a period, whereas distance is measured at a point
in time. To see the importance of this selection, we have also estimated models on the full
sample. For this sample the coefficient of commuting distance is about 30 to 60% lower than
the ones reported here (the exact percentage depends on the specification of distance),
consistent with our theoretical claim that the bias is about 50%. Finally, we have re-estimated
the model using alternative estimation approaches (Poisson), regressions based on samples
without ‘outliers’ (including only workers absent for less than 10 days per year), but the
implied elasticities are roughly the same.

3. Conclusion

A common assumption in the urban economic literature is that private costs of commuting are
fully borne by the worker and do not affect the worker’s productivity. This assumption is
challenged by urban efficiency wage theories that allow that worker’s work effort is a function
of the length of the commute. In particular, Zenou (2002) assumes that workers involuntarily
provide less work effort due to larger commutes. In addition, Ross and Zenou (2008)
demonstrate that if shirking and leisure time are substitutes in the worker’s utility function,
then one may expect a positive effect of commuting on shirking. Despite the growing
theoretical body of work, there is only one study, by Ross and Zenou (2008), which provides empirical support in this regard. To be more precise, Ross and Zenou (2008) demonstrate, as predicted by theory, that unemployment and the length of the commute are positively related. Further, they demonstrate that wages vary across commutes, which is consistent with firms being able to partially observe commutes and minimize shirking by setting wages conditional on the length of the commute. We employ a more direct measure of shirking, absenteeism, and focus on the effect of commuting distance on absenteeism. The effect identified is based on changes in distance that are employer-induced (we observe changes in commuting distance when the worker does not change residence and keeps the same job at the same employer) and therefore exogenous.

Our results indicate that, ceteris paribus, commuting distance has a strong positive effect on absenteeism, with an elasticity of about 0.07. In the hypothetical case that all workers in the economy have a negligible commute, absenteeism would be about 16% lower, roughly one day per year, so the results are economically relevant. Our favoured interpretation is that the effect identified is predominantly through an effect of the time component of commuting costs on voluntary absenteeism, in line with Ross and Zenou (2008), but we cannot completely exclude the possibility that some of the effect is through an effect on health and therefore on involuntary absenteeism, as argued by Zenou (2002).

References


RCI. (2001), Recruitment Confidence Index, December 2000, Cranfield School of Management, United Kingdom


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Notes: Standard errors in parentheses. Fixed effects are induced for workers, residences and jobs. Hence, groups are defined such that a group includes only observations of one worker with the same residence and job.
Appendix A: Figures

Figure 1. Frequency Distribution of Number of Days Absent

Figure 2. Frequency Distribution of Distance
Appendix B: Bias in the estimate of the effect of commuting distance

Suppose one has observations at discrete time intervals (e.g. \(t-1, t, t+1\)). We denote \(A_{t,t-1}\) as the worker’s number of days absent between time \(t-1\) and \(t\), so for a period of length one. At \(t\) the worker has a commute of \(d_t\); at \(t-1\) the worker has a commute of \(d_{t-1}\). If \(d_t \neq d_{t-1}\), then the worker has changed commuting distance within the period from \(t-1\) until \(t\). Distance \(D_{t,t-1}\) denotes the weighted average of distances \(d_t\) and \(d_{t-1}\), so:

\[
D_{t,t-1} = \lambda_t d_t + (1 - \lambda_t) d_{t-1},
\]

(A1)

where \(0 \leq \lambda_t \leq 1\) and \(\lambda_t\) measures the proportion of the period that the distance is equal to \(d_t\). For example, when the worker does not change distance between \(t-1\) and \(t\), then \(\lambda_t = 1\) and \(D_{t,t-1} = d_t\). It is assumed that \(d_t\) and \(d_{t-1}\) are observed, but \(\lambda_t\) and \(D_{t,t-1}\) are not observed. We denote absenteeism between \(t-1\) and \(t\) as \(A_t\). The relationship between \(A_{t,t-1}\) and distance \(D_{t,t-1}\), is assumed to be linear, so:

\[
A_{t,t-1} = \alpha + \beta D_{t,t-1} + u_t,
\]

(A2)

where \(\alpha, \beta\) are parameters to be estimated and \(u_t\) is random error. Suppose now that the worker’s distance changes between \(t-1\) and \(t\) (e.g. due to a residence move), but remains the same between \(t-2\) and \(t-1\), so \(D_{t-1,t-2} = d_{t-1}\). A fixed-effects estimator is then based on the following expression:

\[
A_{t,t-1} - A_{t-1,t-2} = \lambda_t \beta (d_t - d_{t-1}) + u_t - u_{t-1}.
\]

(A3)

It follows that if one uses \(d_t\) and \(d_{t-1}\) (which are observed) instead of \(D_{t,t-1}\) (which is unobserved), then the estimated value of \(\hat{\beta}\) equals \(\lambda_t \beta\), so \(\hat{\beta} \leq \beta\). The weighting variable \(\lambda_t\) differs per observation. Given uniform distribution of \(\lambda_t\), \(\hat{\beta} = 0.5 \beta\). This result also holds for the reversed situation, when there is no change in distance between \(t-1\) and \(t\), but a change between \(t-1\) and \(t-2\).