The Effect of Health Care Expenditure on Sickness Absence

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Abstract

Based on data from a panel of the Swedish municipalities during 1993-2004, the effects of public health care expenditure on absence from work due to sickness or disability were studied using an instrumental variable method. Public health care expenditure had no significant effect on absence due to sickness or disability and the standard errors were small enough to rule out all but a minimal effect. The same result was obtained when separate estimates were done for men and women and for absence due to sickness and disability.

Key words: health care expenditure; sick leave; disability; worker absenteeism; dynamic panel data models; endogeneity

JEL classification: H51; I12; J22

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1 Introduction

Rising health care costs in most industrialized countries have increased the importance of evaluating the effects of health care expenditure. In the past few decades, assessing its effects on health outcomes has become a central question in the context of health care cost containment in most developed countries (Nixon and Ulmann, 2006). From a Swedish perspective it is especially interesting to estimate the relationship between health care expenditure and absenteeism, since according to OECD (2005) "Sweden's single biggest economic problem is the high number of people absent from work due to sickness or disability".[1]

The effects of health care programs on the absence of specific patient groups have been studied previously.[2] However, to my knowledge, the effect of aggregated health care expenditure on absence has never been studied. The purpose of the present study was to estimate how aggregated public expenditure on health care affects absence from work due to sickness or disability.

The literature on absence includes estimations of the effects of individuals' health status: Paringer (1983) found perceived health status to be an important predictor of hours lost from work, which was supported by Primoff Vistnes (1997), who also reported statistically significant effects of obesity and smoking on the likelihood for women's absence. The literature also provides massive support for that economic incentives affect absence, for example the following four studies using Swedish data; Johansson and Brännäs (1998), Johansson and Palme (2002, 2005) and Henrekson and Persson (2004).[3]

Whether increased aggregated expenditure on health care or increased access to health care actually improves the health status of the population in industrialized countries is still an open question. Higher expenditure on health care could lead to better health among the population by reducing waiting times for medical care or improving procedures. But according to Nixon and Ulmann's (2006) review of the literature on health care expenditures and health outcomes, cross-country studies have found limited or no relationship between health care expenditure and mortality rates. On the other hand, Crémieux et al. (1999)

[1] According to Statistics Sweden and The Swedish Social Insurance Agency, 5.2 percent of employee working hours in Sweden were lost due to sickness absence in 2004; at the same time 8.1 percent of the population aged 16 to 64 were on disability pension.

[2] Absence will be used throughout this paper to mean absence from work due to either sickness or disability.

[3] Brown and Sessions (1996) review the literature on absence more broadly.

found that higher health care expenditure among Canadian provinces reduced male and female infant mortality and increased life expectancy. They explained the different results by the inherent heterogeneity associated with cross-country studies. Lichtenberg (2004) analyzed time-series of life expectancy in the United States and found that both public health care expenditure and research and development expenditure on pharmaceuticals had positive effects. Aakvik and Holmås (2006) found no effect of the total number of general practitioners per capita on mortality rates in Norwegian municipalities, but found a negative effect of the number of contracted general practitioners. Brook et al. (1983) reported on the Rand Health Insurance Experiment, a controlled trial in the United States where families were randomly assigned insurance plans. One group received all their medical care free of charge and, as a consequence, used more than the other groups. Despite this, the only statistically significant effects were improvements in health for those with poor vision and for low-income persons with high blood pressure. However, the study included only people aged 14 to 61 who were free of disability that precluded work.

Except for the rare occasions when a randomized controlled trial is preformed, determining the effect of health care expenditure and access to health care is complicated by the context in which decisions regarding health care are taken. Health care expenditure is partly determined by the perceived need for it, which in turn may be affected by absence. Therefore an instrumental variable estimator was used in this study in an attempt to determine the causal link between health care expenditure and absence.

Most previous studies have evaluated the effect of health care expenditure on mortality rates and life expectancy. Therefore, this paper contributes to the literature by examining the effect on absence due to sickness or disability, which can be expected to be correlated with individuals' health related quality of life. The analysis was based on municipality-level data from Sweden. As demonstrated by Granlund (2007), the results may also help determining the sign of a vertical fiscal externality that arises when a lower level of government provides health care, the central government provides a sickness benefit and both levels tax labor income. Health care is more likely to be over-provided by the local governments the smaller effect health care expenditure has on absence. The article also explains why the lower level of government has a weak incentive to reduce absence. In practice, this may result in a relatively small share of total health care expenditure being focused on reducing absence in a country like Sweden. The results from research in this field may also inform policy makers in their decision regarding the level of expenditure on health care. The main finding in this paper is that health care expenditure had no statistically significant effect on absence and that, under all circumstances, the possible effect was small.

The next section outlines the theory, while section 3 presents the empirical analysis. The data are discussed in section 3.1 and the empirical specification in section 3.2, while section 3.3 contains the results. Finally, in section 4 the paper's conclusions are presented.

2 Theoretical outline

To later be able to specify the empirical model, we need an absence function for municipalities which includes the health status of the population as well as a health production function. To derive the first of these, we first have to analyze what determines whether individuals will prefer to be absent from work.

An individual's instantaneous utility can be expressed as $u_t = u(c_t) - f(h_t, j_t) - g(s_{i,t-1}, s_{-i,t-1})$, where c_t is consumption at time t; effort (f) depends on health status (h_t) and work conditions (j_t); and the disutility of absence ($-g$) depends on one's own prior period absence ($s_{i,t-1}$) and that of others ($s_{-i,t-1}$). It is assumed that effort decreases with good health and work conditions, while the disutility of absence decreases with both one's own and others' prior period absence. This represents internal habit formation, that one might be more likely to be absent this period for a given health and other variables if one was absent the last period, and external habit formation, that high previous absence of others in one's surroundings might reduce the social cost of current absence. Naturally, for absent individuals the effort is zero and for those working the disutility of absence is zero.

For simplicity let's assume that individuals have no access to capital markets and only choose whether to work or be absent. When an individual works $c_t = w_t(1 - \tau_t)$, where w is labor income and τ is the tax rate on labor income. For individuals absent from work $c_t = B_t(1 - \tau_t)$, where B is a sickness benefit. Assuming myopic behavior, i.e. that individuals neglect the effect of current absence on their future utility, individual's will prefer to be absent if

$$u(B_t(1-\tau_t)) - g(s_{i,t-1}, s_{-i,t-1}) > u(w_t(1-\tau_t)) - f(h_t, j_t). \qquad (1)$$

Medical doctors and sickness insurance officials are usually involved in deciding whether a sickness benefit will be allowed. According to the Swedish regulations during the study-period, individuals were entitled to sickness benefits if their capacity to work was sufficiently reduced due to poor health and a doctor's certificate was usually required for sick leave extending one week.[4] For long-term sick leave, the capacity to perform other assignments would also be taken into consideration.[5] Nevertheless, also the variables B, τ, $s_{i,t-1}$, $s_{-i,t-1}$ and w can be expected to affect absence. First, since doctors and insurance officials cannot observe ability to work perfectly, they must rely in part on information provided by the individual, which may be affected by the individual's incentives. Second, doctors may also consider what their patients prefer, and while insurance officials can deny sickness benefits to individuals with a doctor's certificate, they mostly follow the doctor's recommendation.[6] The absence function for municipalities can therefore be written

$$\mathbf{s_t} = s(\mathbf{B_t}(1-\boldsymbol{\tau}_\mathbf{t}), \mathbf{w_t}(1-\boldsymbol{\tau}_\mathbf{t}), \mathbf{s_{t-1}}, \mathbf{j_t}, \mathbf{h_t}), \qquad (2)$$

where $\mathbf{B}, \boldsymbol{\tau}, \mathbf{w}, \mathbf{s}, \mathbf{j}, \mathbf{h}$ are in bold to indicate that they describe the situation for all individuals of working age in a municipality.

The aggregated health status in a municipality at period t can be written

$$\mathbf{h_t} = h(\mathbf{h_{t-1}}, \mathbf{e_t}, \mathbf{r_t}, \mathbf{X_t}), \qquad (3)$$

where $\mathbf{e}$ is public health care expenditure per capita; $\mathbf{r}$ represents the health risks that individuals are exposed to, for example at their workplaces; and $\mathbf{X}$ denotes demographic characteristics, like age and gender.

[4] Since October 1, 1995, a doctor's certificate has been required to receive benefits after the seventh calendar day of absence. Before that, the Swedish Social Insurance Agency could require a doctor's certificate for sick leave lasting over four weeks, and also earlier in some cases (Proposition 1994/95:147 and Law 1995:508).

[5] In certain circumstances, also the individual's age and education etc. were allowed to influence the judgment regarding capacity to work (Law 1996:1543).

[6] Shortell (1998) discusses physicians multiple accountabilities. Based on a survey of 4,200 physicians active in Sweden (response rate 58 percent), Arrelöv (2006) similarly reports that 65 percent of physicians consider the patient's motivation for returning to work when assessing the extent of allowable sick leave.

3 Empirical analysis

3.1 Data description

The present study was based on yearly municipality-level data on absence for the period 1993 to 2004. There were 286 municipalities in 1993, yielding 3,432 observations.[7] Data on public health care expenditure are primarily available at county level, where responsibility for health care provision lies. There were 23 counties in 1993, shrinking to 20 in 2004. In addition there were three municipalities (Malmö, Göteborg and Gotland) which did not belong to any county but provided health care themselves in 1993. By 2004 only Gotland remained in this category.

Table 1 (below) gives descriptive statistics of the variables used in this study while Table A1 in Appendix A defines them and gives data sources. Figure 1 and 2 (also in Appendix A) provide box plots for two of the most central variables in this study. The first six variables describe absence from work and cover all employees and self-employed in Sweden, since they were all automatically insured in the social insurance system. *Sickness* is the average number of days of absence from work due to sickness during a year for insured individuals in the ages of 16 to 64. *Disability* and *Rehab* are the corresponding numbers of days on disability/early retirement pension and days of absence due to rehabilitation, respectively, and absence, s, is the sum of these three variables. The original absence data lacked information for some observations and did not include days compensated by employers, which over the study-period changed between the first 14, 21, and 28 days of each absence spell. In Appendix B it is described how data from other sources were used to adjust the absence variables to always correspond to absence from the 15th day of each spell, as well as how missing data in these variables were handled.

The most common reasons for both sickness and disability absence were illness in locomotion organs and mental illness. Those whose capacity to work was expected to be sufficiently reduced for a long time could receive disability/early retirement pension, usually preceded by a long period of sick leave (Riksförsäkringsverket, 2004, and Law, 1962:381). During the study-period, the compensation levels in the social insurance system ranged from 75 to 90 percent

[7] By 2004 there were 290 municipalities, but data from the new municipalities were aggregated according to 1993 boundaries.

of the income from the second day of absence, but with a cap at a certain level of income. At first less than 10 percent of the insured were affected by this cap, but by 2004 22 percent were (Henrekson and Persson, 2004, and the Swedish Social Insurance Agency).

Public health care expenditure (e) was defined as each county's per capita operating costs on health care, excluding expenditure on dental care and pharmaceuticals. Of this roughly 2 percent was patients' co-payments for public health care. During the study-period total health care expenditure constituted 7.5-8.5 percent of Sweden's GDP, of which 11-15 percent was for pharmaceuticals and 8-10 percent for dental care. Pharmaceuticals were excluded from the study because they were paid by the central government until 1998, and dental care was excluded since it might have a quite different effect on absence compared to other health care services. Public expenditure accounted for approximately 95 percent of the total non-dental, non-pharmaceutical health care expenditure in Sweden.[8] The variable (e) of course includes expenditure on the entire population (not just those of working age), but adjustments for variations in county age-composition were made using microdata of health care consumption. Appendix C describes how this was done in order to create a variable describing age-adjusted per capita public health care expenditure, denoted $eadj$.

w is the average labor income of the non-absent population of working age (16 to 64 years of age) in each municipality. τ^M and τ^C are the proportional municipality and county income tax rates, respectively, and τ^{MC} is the sum of them.

The variables $Women$ to $Pop6064$ describe the shares in the population of working age which belong to each demographic group. $El.School$, $HighSchool$ and $University$ denote the shares of the working age population with different educational levels, described in Table A1 in Appendix A. $SocM$ and $SocC$ denote the fraction of each municipality and county parliament, respectively, represented by socialist parties. Finally, $PolmajM$ and $PolmajC$ are dummy variables which take the value 1 if either one of the two traditional Swedish political blocks has own majority in the municipality and county parliament, respectively.

[8] These figures are the result of own calculations based on data obtained from The National Board on Health and Welfare.

Table 1. Descriptive statistics

	1994**		2004		1993-2004	
Variable	Mean	Std.dv.	Mean	Std.dv.	Mean	Std.dv.
$Sickness$	10.46	1.84	15.55	3.30	13.44	4.08
$Disability$	29.08	8.06	31.26	7.20	28.72	7.40
$Rehab$	1.18	0.49	1.01	0.46	0.89	0.45
s	40.72	9.23	47.82	9.14	43.05	9.55
s_{women}	45.97	10.06	58.39	11.00	51.18	11.50
s_{men}	35.97	9.02	37.88	8.20	36.03	8.44
e^*	10.84	1.25	15.76	0.92	12.86	2.11
$eadj^*$	10.68	1.02	15.56	0.81	12.72	2.04
w	159.70	18.25	215.62	23.65	186.39	29.58
τ^m	19.24	1.80	21.45	1.26	20.70	1.72
τ^{c*}	11.09	1.34	10.43	0.68	10.24	1.11
τ^{mc}	30.34	1.12	31.84	0.91	30.94	1.21
$Women$	0.49	0.01	0.49	0.01	0.49	0.01
$Pop1639$	0.50	0.02	0.45	0.04	0.47	0.04
$Pop4049$	0.24	0.01	0.21	0.01	0.23	0.02
$Pop5054$	0.10	0.01	0.11	0.01	0.12	0.01
$Pop5559$	0.08	0.01	0.13	0.01	0.10	0.02
$Pop6064$	0.08	0.01	0.10	0.01	0.09	0.01
$El.School$	0.33	0.06	0.25	0.04	0.30	0.06
$HighSchool$	0.59	0.03	0.63	0.03	0.61	0.04
$University$	0.07	0.04	0.12	0.05	0.09	0.05
$SocM$	0.51	0.12	0.47	0.11	0.48	0.12
$SocC^*$	0.45	0.07	0.49	0.04	0.50	0.06
$PolmajM$	0.73	0.47	0.71	0.45	0.72	0.45
$PolmajC^*$	0.93	0.24	0.44	0.50	0.66	0.48

*Indicates that the variable is measured at county-level instead of municipality-level.
** Descriptive statistics are reported for 1994 since data on s^{women} and s^{men} were not available for 1993.

3.2 Empirical specification

The empirical specification of the municipal absence function, i.e. equation (2), can be written

$$\begin{aligned} s_{it} &= \boldsymbol{\beta}_1 w_{it}(1-\tau_{it}^{mc}) + \beta_2 s_{i,t-1} \\ &\quad + \sum_{l=1}^{2} \eta_l Edu_{lit} + \boldsymbol{\beta}_3 h_{it} + y_t + \mu_i + \varepsilon_{it}. \end{aligned} \tag{4}$$

$w_{it}(1-\tau_{it}^{mc})$, the net labor income in municipality i at time t, was included to capture the monetary incentive of remaining at work for the marginal worker. In equation (2) absence was also affected by the sickness benefit net of taxes, but this was left out from the specification since sickness benefits are a function of labor income and since observed values of it to a higher degree than the observed values of w_{it} depend on the composition of those on absence. Hence, a relatively high fraction of the variation in B_{it} does not correspond with variation in the monetary incentives to remain at work for the marginal worker.

The educational variables, Edu_{lit}, $(l = 1, 2)$, were used as proxies for work conditions. These variables may also capture the effect caused by that employment contracts differ among different type of jobs in respect to stipulated number of hours and flexible hours. Effects of contracts have been highlighted as major explanations to absence in previous economic literature (see e.g. Brown and Sessions, 1996). Year-specific fixed effects (y_t) were included to capture "national variables" such as business cycle effects on absence. Municipality-specific fixed effects (μ_i) were included to capture time invariant heterogeneity among the municipalities which might be correlated with the regressors. The other two variables, $s_{i,t-1}$ and h_{it}, were motivated in the theoretical outline.

The empirical specification of the municipal health production function, i.e. equation (3), can be written

$$\begin{aligned} h_{it} &= h_{i,t-1} + \gamma_2 eadj_{it} + \sum_{l=1}^{2} \delta_l Edu_{lit} + \sum_{n=1}^{4} \zeta_n \Delta Pop_{nit} \\ &\quad + \sum_{n=1}^{4} \kappa_n Pop_{nit} + \gamma_3 Women_{it} - \delta_t, \end{aligned} \tag{5}$$

where $h_{i,t-1}$ denotes lagged health status and $eadj$ is age-adjusted health care expenditure. Since lagged health status was included as an explanatory variable, the purpose of the other explanatory variables was to capture changes in the health status, rather than the level of it. As such, Edu_{lit}, $(l = 1, 2)$ were used as proxies for the health risks that people in the municipalities are exposed to during the year at their work place. $\Delta Pop_{nit} = Pop_{nit} - Pop_{ni,t-1}$, $(n = 1, 2...4)$ describe the change in the age-composition of the population in working age, whereas Pop_{nit}, $(n = 1, 2...4)$ and $Women_{it}$ describe the demographic composition of the population in working age. The demographic variables might enter the equation in differences since demographic groups might differ in health status and that changes in these variables therefore lead to changes in the population's health status. Demographic variables might enter in levels since demographic groups might have different development of their health status over time.[9] δ_t denotes depreciation of the health status and was allowed to vary over time but not over municipalities. Not allowing the depreciation of the health status to vary over municipalities in other ways than that captured by the demographic and the educational variables was of course a restriction. This restriction was imposed since health status is hard to measure and that it therefore is difficult to estimate how the depreciation depends on the level of this variable.

Differentiating equation (5) and substituting it into a differentiated version of equation (4) yields

$$\begin{aligned}\Delta s_{it} \quad = \quad & \boldsymbol{\beta}_1 \Delta(w_{it}(1-\tau_{it}^{mc})) + \boldsymbol{\beta}_2 \Delta s_{i,t-1} \sum_{l=1}^{2} \eta_l \Delta Edu_{lit} + \\ & + \boldsymbol{\beta}_3 \{\gamma_2 eadj_{it} + \sum_{l=1}^{2} \delta_l Edu_{lit} + \sum_{n=1}^{4} \zeta_n \Delta Pop_{nit} \\ & + \sum_{n=1}^{4} \kappa_n Pop_{nit} + \gamma_3 Women_{it}\} + Y_t + \Delta\varepsilon_{it}, \qquad (6)\end{aligned}$$

where $Y_t = y_t - y_{t-1} - \boldsymbol{\beta}_3 \delta_t$. This equation, on which all empirical specifications will be based, shows how the effect of health care expenditure on absence can be estimated without having to include proxies for health status. The new error term, $\Delta\varepsilon_{it} = \varepsilon_{it} - \varepsilon_{i,t-1}$, is by construction correlated with the lagged dependent variable, $\Delta s_{i,t-1} = s_{i,t-1} - s_{i,t-2}$, which is then endogenous. Also expenditure on health care might be endogenous since, ceteris paribus, an increase in absence might cause the counties to increase it. Beyond that, a negative health

[9] $\Delta WomenWA_{it}$ was left out since it is has no statistically significant effect on absence.

shock, not captured by any of the other explanatory variables of equation (6), might cause an increase in both absence and health care expenditure. However, the endogeneity problem is probably reduced since the dependent variable was measured at the municipal level, whereas health care expenditure was decided at the county level. Since county tax revenue was used to finance health care the tax rate might also be endogenous, and if the labor income of the marginal non-absent individual differs from the average labor income of the non-absent population, w_{it} will be endogenous as well.

The endogeneity problem was addressed by instrumenting $\Delta s_{i,t-1}$, $eadj_{it}$ and $\Delta(w_{it}(1-\tau_{it}^{mc}))$ with the closest lags uncorrelated with the new error term, namely $\Delta s_{i,t-2}$, $eadj_{i,t-1}$, and $\Delta(w_{i,t-2}(1-\tau_{i,t-2}^{mc}))$; and $\Delta PolmajM$, $\Delta PolmajC$, $\Delta SocM$, and $\Delta SocC$ were included as additional instruments.[10,11] These four last instruments are expected to correlate with the tax and expenditure decisions of the municipal and county governments and were primarily included to avoid problems with weak instruments for $\Delta(w_{i,t}(1-\tau_{i,t}^{mc}))$; but $\Delta PolmajC$ and $\Delta SocC$ could also be expected to strengthen the instrument set for health care expenditure.[12]

Based on the theoretical outline it is reasonable to expect $\Delta(w_{it}(1-\tau_{it}^{mc}))$ to have a negative impact on absence. Descriptive statistics from Sweden states that younger and better educated individuals were less likely to be absent, which formed my expectation about the coefficients for the differenced educational and demographic variables. During the period under study, absence has increased much more for women than for men, more for individuals aged 50 to 59 than for others and the absence has decrease for individuals aged 60 to 64. This formed my expectations for the coefficient for $Women_{it}$ and the three highest age-groups in levels, whereas I had no prior expectation regarding the remaining age-group and Edu_{lit} , $(l = 1, 2)$. Previous absence was expected to have a positive influence on current absence, as explained in the theoretical outline. Lastly, public health care expenditure was anticipated to have negative or no impact on absence. As reported in the introduction, some previous research

[10] These instruments will be uncorrelated with the new error term if ε_{it} and $\varepsilon_{i,t-1}$ are uncorrelated, which is likely since the lagged dependent variable is included in the model. ε_{it} and $\varepsilon_{i,t-1}$ are correlated if $\Delta\varepsilon_{it}$ and $\Delta\varepsilon_{i,t-2}$ are correlated, which can be tested.

[11] In the static specifications, $\Delta s_{i,t-2}$ was not included.

[12] Similar variables were used by Aronsson et al. (2000) in a regression of municipal tax base, but they used a Herfindal-Index of political fragmentation instead of $\Delta PolmajM$ and $\Delta PolmajC$ as here.

has reported no or very limited effects of aggregated health care expenditure on the health status of the population. Moral hazard problems is one set of explanations to why public health care expenditure might have no, or very limited, effect on the health status of the population and therefore also on health related work absence.

3.3 Results

Table 2 presents the estimation results for absence. All instrumental variable estimations were done using a two-step feasible generalized method of moments estimator, which is efficient in the presence of heteroskedacity and serial correlation.[13] First, the baseline specification (labeled IV) is presented where $eadj$, $\Delta s_{i,t-1}$ and $\Delta(w(1-\tau^{mc}))$ were instrumented and then four other specifications are presented to serve as comparisons. In the OLS specification all regressors were treated as exogenous and in the IV-small specification the education and demographic variables were left out. In the IV-e specification e was included instead of its age-adjusted version and the IV-static specification is a static version of the first one. The omitted education- and age-groups are $El.School$ and $Pop1639$. The derivatives $ds^*/deadj|_{h_{t-1}}$ and $ds^*/de|_{h_{t-1}}$ indicate the long run effects of health care expenditure on absence. The next six statistics, which describe the relevance and validity of the instruments, are discussed in Appendix D, where I conclude that the instruments are valid and reasonable relevant.[14]

[13] Greene (2003) describes the estimator in chapter 18.

[14] The estimations are based on 2,547 or 2,554 observations, since the use of lags and the first-difference transformation reduced the number of possible observations with 3*286, and since 27 (20 in the OLS specification) were lost due to lack of data on health care expenditure. To facilitate comparison, the OLS estimation was performed for the same years as the IV estimations.

Table 2. Estimation results, first difference of absence

	IV	OLS	IV-small	IV-e	IV-static
eadj or *e*	0.02	0.01	-0.03	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
Δs_{t-1}	0.26**	0.21***	0.37***	0.26**	
	(0.11)	(0.03)	(0.09)	(0.11)	
$\Delta(w(1-\tau^{mc}))$	-0.03	0.06***	-0.06	-0.03	-0.02
	(0.04)	(0.01)	(0.04)	(0.04)	(0.04)
$HighSchool$	-0.03	0.61		-0.22	0.44
	(0.85)	(0.82)		(0.88)	(0.93)
$University$	-3.04***	-3.56***		-3.13***	-4.13***
	(0.94)	(0.76)		(0.95)	(0.91)
$\Delta HighSchool$	-20.37**	-20.87**		-20.34**	-22.30**
	(9.31)	(8.88)		(9.28)	(8.96)
$\Delta University$	-48.52***	-60.11***		-48.63***	-51.91***
	(14.11)	(12.58)		(14.08)	(13.87)
$Women$	15.87***	17.16***		16.29***	19.92***
	(4.16)	(3.79)		(4.24)	(4.24)
$Pop4049$	-1.65	-1.56		-1.71	-0.40
	(2.70)	(2.71)		(2.69)	(2.93)
$Pop5054$	-1.46	-4.34		-1.42	-2.23
	(5.62)	(5.57)		(5.62)	(5.98)
$Pop5559$	12.73**	12.93**		12.70**	15.67***
	(5.52)	(5.46)		(5.51)	(5.91)
$Pop6064$	-4.07	-4.59		-4.45	-4.77
	(3.97)	(3.92)		(4.00)	(4.46)
$\Delta Pop4049$	34.24***	34.84***		34.22***	31.21***
	(11.48)	(11.51)		(11.44)	(11.99)
$\Delta Pop5054$	58.44***	63.80***		58.22***	61.87***
	(14.49)	(14.29)		(14.47)	(15.34)
$\Delta Pop5559$	52.92***	56.81***		52.92***	65.20***
	(15.50)	(14.35)		(15.51)	(14.98)
$\Delta Pop6064$	61.74***	70.09***		62.27***	81.85***
	(16.74)	(15.01)		(16.80)	(15.97)
$ds^*/deadj\|_{h_{t-1}}$	0.02	0.02	-0.05	0.04	
or $ds^*/de\|_{h_{t-1}}$	(0.04)	(0.04)	(0.05)	(0.06)	
Cragg-Donald	22.01		29.26	22.17	42.94
eadj or *e*: Shea	0.72		0.81	0.63	0.74
Δs_{t-1}: Shea	0.06		0.08	0.06	
$\Delta(w(1\text{-}\tau^{mc}))$: Shea	0.09		0.09	0.09	0.09
Hansen J	0.46		0.43	0.71	0.97
Serial corr. 2	0.62	0.86	0.97	0.64	0.21
Adj. R^2	0.74	0.74	0.72	0.74	0.73
Sample size	2547	2554	2547	2547	2547

The regressions include year specific effects. Robust standard errors are shown in parentheses. The Asterisks ***, ** and * denote significance at the 1, 5 and 10 percent levels.

In a simultaneous test of whether $eadj$, $\Delta s_{i,t-1}$ and $\Delta(w_{it}(1-\tau_{it}^{mc}))$ can be treated as exogenous, the null hypotheses of exogeneity could be rejected at the 10 percent level, which supports the use of instrumental variable estimators.[15] The education and age variables in levels and in first-differences, as well as the year-specific fixed effects were included since the null hypothesis of no effect could be rejected at the 10 percent level in group-wise F-tests. $\Delta Women_{it}$ and variables describing the share of the work force in various sectors were not included in the final regressions since these variables had no statistically significant effects. That $\Delta Women_{it}$ had no effect is surprising since women in Sweden are known to be absent more than men, but this is probably explained by low variation in gender-composition over time. Including $\Delta((w_{it}-B_{it}^{s})(1-\tau_{it}^{mc}))$ instead of $\Delta(w_{it}(1-\tau_{it}^{mc}))$, including lagged values of $eadj$, or estimating with two-stage least squares instead of using the generalized method of moments estimators, did not change the general results.[16,17]

Table 2 shows that health care expenditure had no statistically significant effect on absence and the estimated standard errors are small enough to rule out all but a minimal effect.[18] The difference in the estimated coefficients between in the IV estimations and the OLS specifications are negligible. This might be explained by absence being measured at the municipal level whereas health care expenditure was decided at the county level, or by the county councils' weak incentive to respond to changes in absence. Using unadjusted health

[15] The endogeneity test is based on the difference of two Hansen-Sargan statistics and is robust against heteroscedasticity.

[16] Robust standard errors are reported since a Pagan-Hall test indicates heteroscedasticity in all specifications. For the OLS specification, a White-Koenker test was used instead.

[17] Previous literature (e.g. Henrekson and Persson, 2004) have found statistically significant effects of current unemployment and labor force participation rates on absence. Here, national variations in these variables were captured by the year-specific fixed effects, while time-invariant heterogeneity in these variables was wiped out by the first-difference transformation. Including these variables, or their lags, directly into the model did not change the general results. Whether non-working individuals are unemployed, not part of the labor force, or absent, is probably affected by variables not included in the model, making labor-force participation and unemployment endogenous. Due to this and the difficulty of finding strong instruments for two additional endogenous regressors, those variables were not included in the final specification.

[18] For the baseline specification, the 99 percent confidence interval for health care expenditure reaches down to -0.07. In percentage terms a coefficient of -0.07 translates to that a 10 percent increase in health care expenditure would only reduce absence by approximately 0.21 percent.

care expenditure (IV-e) instead of age-adjusted (IV and others) also made little difference, perhaps because there was little heterogeneity in the changes in age-composition across counties.

The effect of health care expenditure on absence is of course heterogeneous and depends on what the money is spent on. Although the purpose of this study was to estimate the effect of aggregated public health care expenditure, i.e. to estimate the average effect, such heterogeneity might cause a problem when estimating the effect with IV methods (Heckman et al., 2006). Here, the problem would arise if the marginal expenditure identified by the instrumental variables were non-representative in terms of their effect on absence. Different instrument variables would then result in different parameters being estimated. To judge whether this is a serious problem in the present study, the baseline estimation was performed with numerous combinations of instrumental variables, which all gave similar results.[19]

The small effect of health care expenditure on absence might be explained partly by moral hazard problems; that is, individuals might reduce their personal investments in health, when public health care expenditure is increased. For example, people might exercise less and eat more unhealthy food when they have access to better health care. These moral hazard problems can also be one explanation to why several previous studies (see e.g. Aakvik and Holmås, 2006, or Nixon and Ulmann, 2006) have found no or limited effect of health care expenditure on the health statues of the population. It may also be that variations in health care expenditure in industrialized countries such as Sweden

[19] Some instrument combinations were found to be weak or invalid (the criteria used here were Cragg-Donald>10 and Shea>0.04 for all endogenous regressors, and Hansen J>0.10), but 30 reasonably good combinations remained, including using changes in Herfindal-Indexes of political fragmentation instead of $\Delta PolmajM$ and $\Delta PolmajC$, and including additional instruments such as $\Delta s_{i,t-3}$. In one case, when $eadj_{i,t-1}$, $\Delta PolmajM$, $\Delta PolmajC$, $\Delta SocM$ and $\Delta SocC$, were replaced by a variable describing the counties' financial resources, the lag of that variable, and a variable describing the share of Health Care Party members in the county government in the prior year, the coefficient for $eadj_{it}$ was negative and statistically significant at the 10 percent level, (coeff.= -0.06, std.err=0.03). In the other cases the general results held.

Since Arellano (1989) recommended using levels instead of differences as instruments for the lagged dependent variable, $s_{i,t-2}$ was also tested as an instrument instead of $\Delta s_{i,t-2}$. However, it was found to be weak so $\Delta s_{i,t-2}$ was used instead. The final choice of instruments was based on the values for the baseline specification of the instrument statistics, discussed in Appendix D.

have less to do with curing and more to do with caring (Newhouse, 1977). That is, health care expenditure on the margin might be spent so that the patients' comforts increase but without leading to quicker recoveries. Other contributing explanations could be migration of sick individuals to counties with higher health care expenditure and vice versa, low efficiency in public health care, or perhaps weak correlation between total health care expenditure and that directed to the working age population, or even a weak connection between health and absence.

In all specifications lagged absence was significant at the 5 percent level, implying persistence. The estimated coefficient is lower in the OLS specification, which was expected since $\Delta\varepsilon_{it}$ and $\Delta\varepsilon_{i,t-1}$ will be negatively correlated, at least if ε_{it} and $\varepsilon_{i,t-1}$ are uncorrelated.

The first difference of average after tax labor income was only significant in the OLS specification. A probable explanation to the positive estimate in that specification is that a reduction in absence lessens average labor income since the marginal non-absent individual likely has a lower labor income compared to the average in the non-absent population. If this relationship varies across municipalities, that could also account for the non-significant estimates in the other specifications. The limited effect of net income could also result from this variable having opposite substitution and income effects on the demand for absence. The coefficients might also be affected by the impact that net income has on absence through its effect on investments in health capital.

The results indicate that university graduates had a better absence development than others, and show that the coefficients for the differenced educational variables have the expected sign and relative size. Based on these estimates no conclusion can be drawn whether higher education was correlated with lower absence since those with higher education were exposed to fewer health risks at work, had better health and health development of other reasons, or had occupations that reduced their need for absence for a given health.[20] Of course, these variables might also capture characteristics that affect the absence of individuals belonging to other educational groups.

The share of women had a statistically significant positive effect, which was expected since absence increased much faster for women than for men during the study-period, partly caused by more psychological problems such as stress

[20] Grossman (2000) discusses possible explanations for the correlation between education and health on an individual level.

reactions and anxiety (Riksförsäkringsverket, 2004). Those 55 to 59 was according to the estimations, the age-group with the worst absence development. The coefficients for the differenced age variables all have the expected signs. That the coefficients for $\Delta Pop5559$ and $\Delta Pop6064$ are not even higher reflects the lower labor force participation rate in these age-groups.

Table A2 (in Appendix A) shows that the result regarding the effect of health care expenditure prevailed when absence were estimated separately for women and men. However, the estimates for the lagged dependent variable are not reliable because of weak instruments, especially for men, and this might also affect the other estimates in the dynamic IV specifications. The OLS estimations provide a possible explanation for the weak instruments for men's lagged absence, namely that the persistence in absence was relatively weak for men which results in the second lag of the dependent variable being a weak instrument for the first lag.

Absence because of sickness and disability were also estimated separately (Table E1. in Appendix E). Because of interaction between *Sickness* and *Disability* (discussed in Appendix E), both $\Delta Sickness_{i,t-1}$ and $\Delta Disability_{i,t-1}$ were instrumented with their lags and included in each estimation. The results do not allow us to reject the null hypotheses that health care expenditure has no effect on either *Sickness* or *Disability* and the estimated standard errors are small enough to rule out all but minimal effects. However, these results must be taken with caution since the instruments for $\Delta Sickness_{i,t-1}$ are weak, and since second-order serial correlation casts doubt on the validity of most instruments for the dynamic IV specifications and for the static specification for *Sickness*. (However, the p-values of the Hansen J statistic suggest that the instruments are valid for the *Sickness* specifications.)

4 Discussion

The effect of public health care expenditure on absence due to sickness or disability in Sweden was analyzed using an instrumental variable estimator for a dynamic panel model. Public health care expenditure was found to have no statistically significant effect on absence. This result is robust against changes in model specification and also held when separate estimations were conducted for women and men, and for absence due to sickness and disability. The standard errors were small enough to rule out all but a minimal effect of health care expenditure.

This result increases the likelihood that general health care is over-provided in Sweden, according to the model by Granlund (2007). However, health care aimed at reducing absence might still be under-provided. One possible explanation of the small effect on absence is that the correlation between expenditure on health care spent on the working population and total expenditure on health care was weak. It could also be that variation in health care expenditure had less to do with curing and more to do with caring, meaning that health care expenditure on the margin was spent so that the patients' comforts increase without leading to quicker recoveries (Newhouse, 1977). The Swedish counties have weak incentive to reduce absence, which supports either of these explanations. Due to these reasons, it should be stressed that it was the average effect of public health care expenditure on absence that was estimated, not the maximal (or potential) effect. Another set of explanation of the results is moral hazard problems, i.e. people might reduce their personal investments in health when public health care expenditure rises.

The paper relates to the literature studying the effects of access to health care or health care expenditure on health outcomes and the findings give no support for that health care expenditure on an aggregated level improves the health status of the population. However, the results from this paper might also be explained by a weak connection between health and absence. For example, the generous Swedish sickness insurance system might induce also the relatively healthy to report sick. A topic for future research could thus be to investigate the effect of health care expenditure in this sample on other health measures, e.g. on mortality rates. More research is also needed on the relationship between subcategories of health care and absence, for example, pharmaceutical expenditure. Access to health care, measured by for example waiting times, might also have an effect on absence independent of health care expenditure in general and thus be worthy of investigation in itself.

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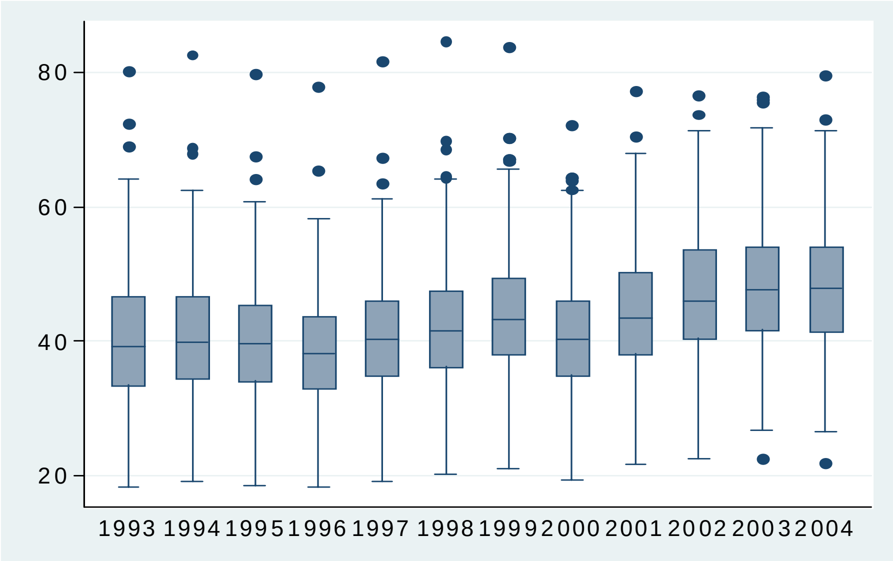
Appendix A: Tables and figures

Table A1. Data definitions and data sources.

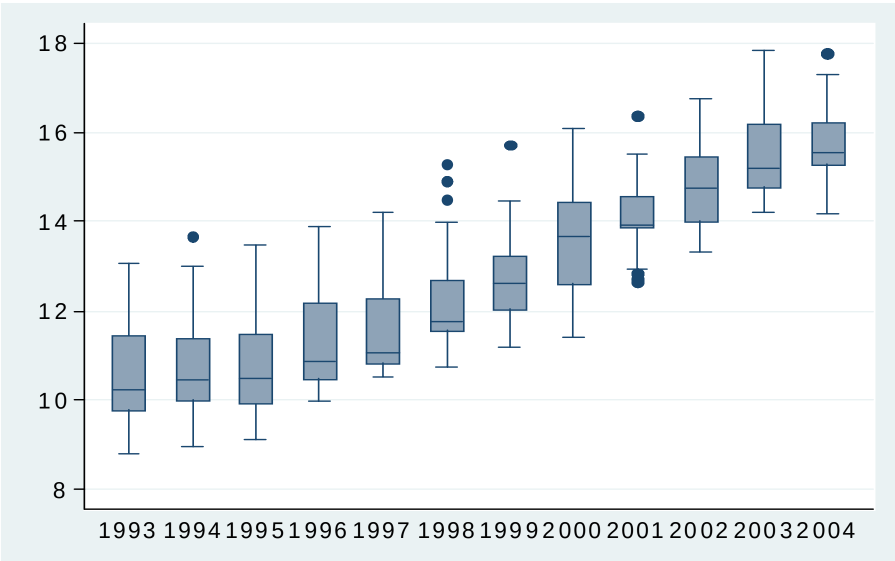
Variable	Definition	Source
Sickness	Average number of days of absence from work due to sickness for insured aged 16 to 64*	SSIA, etc.
Disability	Average number of days of absence from work due to early retirement pension/disability for insured aged 16 to 64	SSIA
Rehab	Average number of days of absence from work due to rehabilitation for insured aged 16 to 64	SSIA
s	Absence: sum of Sickness, Disability and Rehab*	SSIA, etc.
s^{women}	Absence for women	SSIA, etc.
s^{men}	Absence for men	SSIA, etc.
e	Non-dental, non-pharmaceutical, public operating cost for health care, thousands of SEK** per capita	FCC
eadj	Age-adjusted version of e (see Appendix C)	FCC, etc.
w	[Average income from work for those aged 16 to 64 (excluding sickness and disability benefits), thousands of SEK**] / [1-absence rate], where absence rate=s/365* (insured aged 16 to 64)/(population aged 16 to 64)	SCB,SSIA
τ^M	Proportional municipality income tax rate	SCB
τ^C	Proportional county income tax rate	SCB
τ^{MC}	Sum of τ^M and τ^C	SCB
$Women$	Share of women in the population aged 16 to 64	SCB
$Pop1639$	Share aged 16 to 39 of the population aged 16 to 64	SCB
Pop....	Pop4049-Pop6064 have corresponding definitions	SCB
$El.School$	Share of the population aged 16 to 64 who's highest education was elementary school	SCB
$HighSchool$	Share of the population aged 16 to 64 who's highest education was high school or less than tree years after high school	SCB
$University$	Share of the population aged 16 to 64 with three years or longer education after high school	SCB
SocM	Share of Social Democrats and Left Party members in municipal government	SCB
SocC	Share of Social Democrats and Left Party members in county government	SCB
$PolmajM$	Dummy variable which takes the value 1 if either of the two traditional Swedish political blocks has a majority in the municipality government***	SCB
PolmajC	Dummy variable which takes the value 1 if either of the two traditional Swedish political blocks has a majority in the county government***	SCB

The data sources are Swedish Social Insurance Agency, SSIA, The Federation of Swedish County Councils, FCC, and Statistics Sweden, SCB. All monetary variables are deflated by CPI and expressed in 2004 years prices. *Only absence from the 15th day of each spell is included. **On 21 December 2006, USD/SEK=6.81. ***The two political blocks consist of the Social Democratic Party and the Left Party; and the Moderate Party, the Liberal Party, the Christian Democrats and the Centre Party.



Note: The boxes include observations from the 25th to 75th percentiles. The whiskers are 1.5 times the length of the boxes or equal the distance from the box to the minimum or maximum values, whichever is smallest. The dots indicate outside values.

Figure A1. Box plot for absence, *s*



See note to Figure A1.

Figure A2. Box plot for age-adjusted health care expenditure, *eadj*

Table A2. Estimation results, first-difference of women's and men's absences

	Women			Men		
	IV	OLS	IV-static	IV	OLS	IV-static
$eadj$	0.05	0.05	0.06	-0.04	-0.01	-0.04
	(0.05)	(0.04)	(0.06)	(0.04)	(0.03)	(0.04)
Δs^*_{t-1}	0.08	0.19***		0.20	0.08**	
	(0.12)	(0.03)		(0.22)	(0.03)	
$\Delta(w(1-\tau^{mc}))$	-0.04	0.07***	-0.04	-0.02	0.03**	-0.01
	(0.05)	(0.02)	(0.05)	(0.04)	(0.01)	(0.03)
$HighSchool^*$	3.26*	2.91**	3.76**	-1.17	-1.18*	-1.27*
	(1.68)	(1.45)	(1.56)	(0.72)	(0.70)	(0.76)
$University^*$	-4.47***	-4.66***	-4.77***	-0.47	-1.04*	-0.76
	(1.13)	(0.99)	(1.12)	(0.66)	(0.54)	(0.59)
$\Delta HighSchool^*$	-16.81	-18.62*	-17.20*	-7.91	-8.11	-11.10
	(10.40)	(10.31)	(10.45)	(8.19)	(7.29)	(7.36)
$\Delta University^*$	-12.74	-28.09*	-12.32	-47.24***	-55.13***	-54.77***
	(16.89)	(15.17)	(16.87)	(14.44)	(11.01)	(11.85)
$Pop4049^*$	-0.23	-1.19	0.13	-4.34*	-4.02	-4.15
	(3.68)	(3.51)	(3.78)	(2.49)	(2.55)	(2.61)
$Pop5054^*$	-3.94	-5.68	-3.91	5.23	4.34	5.63
	(5.85)	(5.59)	(6.05)	(4.53)	(4.53)	(4.61)
$Pop5559^*$	28.26***	24.56***	30.16***	0.83	0.39	0.20
	(6.39)	(5.79)	(6.29)	(4.69)	(4.64)	(4.68)
$Pop6064^*$	-11.14**	-11.60**	-11.70**	0.47	-0.34	1.53
	(5.40)	(5.00)	(5.66)	(3.94)	(3.97)	(4.06)
$\Delta Pop4049^*$	24.63*	27.43**	24.23*	30.76***	28.39***	28.36***
	(13.53)	(13.50)	(13.76)	(9.91)	(9.71)	(9.75)
$\Delta Pop5054^*$	50.42***	52.40***	52.67***	29.39**	29.53**	26.76**
	(16.58)	(16.38)	(16.90)	(12.60)	(12.08)	(12.26)
$\Delta Pop5559^*$	44.81**	46.97***	48.40***	32.29**	38.13***	40.60***
	(17.92)	(17.38)	(17.93)	(14.90)	(12.01)	(12.16)
$\Delta Pop6064^*$	80.05***	80.77***	85.63***	62.84***	72.70***	75.11***
	(18.03)	(17.31)	(17.84)	(18.31)	(12.72)	(13.09)
$ds^*/deadj\|^*_{h_{t-1}}$	0.06	0.07		-0.05	-0.01	
	(0.06)	(0.05)		(0.05)	(0.04)	
Cragg-Donald	14.54		42.33	6.02		44.81
$eadj$ or e: Shea	0.72		0.74	0.75		0.75
Δs^*_{t-1}: Shea	0.04			0.02		
$\Delta(w(1\text{-}\tau^{mc}))$:Shea	0.10		0.10	0.11		0.11
Hansen J	0.88		0.93	0.90		0.80
Serial corr. 2	0.18	0.94	0.25	0.31	0.26	0.34
Adj. R^2	0.67	0.68	0.67	0.62	0.63	0.63
Sample size	2261	2268	2261	2261	2268	2261

* indicates that the variable is gender-specific. Also, see notes to Table 2.

Appendix B: Missing data and changes in the absence variables

Data on the absence variables were missing for some of the municipalities in 1997, 1998 and 1999. Instead, aggregated data were reported for two (or sometimes three or four) municipalities in the same county. For six municipalities, data for all three years were missing, and instead the aggregated data for three pairs of municipalities were reported. For 37 municipalities, data for 1998 and 1999 were missing, and instead the aggregated data for 12 pairs of municipalities, and three groups of three, and one group of four municipalities were reported. For seven municipalities, data for 1999 were missing, and instead the aggregated data for one group of three and one group of four municipalities were reported. Thus in total, 99 observations lacked data on the absence variables.

The absolute number of days of absence (*days*) as well the number of insured for each group of municipalities is known. I assumed that the *days* were divided among the municipalities in each group in proportion to their share of *days* before and after the years of missing data. That is

$$days_{iM} = \frac{days_{iB} + days_{iT}}{days_{pB} + days_{pT}} days_{pM},$$

where i indicates the ith municipality and p indicates the group of municipalities to which it belongs. B indicates the last year before, T the first year after, and M the years of missing data. This assumption was sufficient to get an estimate of *days* for each municipality for which data were missing for only one year. The estimated *days* was then divided by the number of insured, estimated in the same way, to obtain an estimate of s for each municipality.

Using the maximum entropy method (Wilson, 1970), *days* for those lacking data for two or three years was estimated as

$$days_{it} = \frac{days_{iM} * days_{pt}}{days_{pM}}.$$

The number of insured was estimated in the same way, and then an estimate of s was calculated for each municipality each year. *Sickness*, *Disability*, and *Rehab* were each estimated in this way, which is the least biased estimate possible with the information given (Jaynes, 1957).

For 1999, data on the absence variables were missing, and instead data for October 1998 to September 1999 were reported. I used the numbers reported inflated with 1/8 of the change from 1998 to 2000, which assumes that changes for the fourth quarter were of the same magnitude as the average change for the other three quarters from 1998 to 2000, and that half of this change occurred each year.

Estimations excluding the observations with missing data, and using the average of the values for 1998 and 2000 instead of the calculated values for 1999, give the same general results for the baseline specification.

As mentioned earlier, the absence variables include only days compensated by the Swedish Social Insurance Agency, not days compensated by employers. During 1992-1996 and April 1998-June 2003 employees were compensated by the employer for the first 14 days of absence; during January 1997-March 1998 for the first 28 days; and during July 2003-December 2004 for the first 21 days.[21] Following Henrekson and Persson (2004), this was addressed using data from The Confederation of Swedish Enterprise.

Their data cover a reasonably representative sample of 2,500 private sector establishments and 220,000 employees. Absence from work due to sickness was categorized by the length of the absence spells and separate figures were reported for nine Swedish regions.[22] Assuming that the absence pattern was the same for all municipalities belonging to the same region, and the same for the public sector as for the private sector, the original data for *Sickness* (and s) were adjusted to give absences from the 15th day of absence for all years. This was done by multiplying the original *Sickness*-variable by the percentage of work time lost due to sickness absence from the 15th day of absence divided by the percentage of work time lost due to sickness that was covered by the Swedish Social Insurance Agency.[23,24] The variable s was then created by adding

[21] The self-employed were allowed to choose among insurance plans which differed in when they began to reimburse for lost income due to sickness. One plan stipulated that the self-employed were reimbursed first from the 31st day, but this should have a small effect, if any, on the estimations.

[22] The data for 1997 and 1998 were published in SAF (1998) and SAF (1999), respectively. Data for 2003 and 2004 were provided directly by the company, Löneanalyser AB.

[23] From the data it is not possible to directly identify the shares of work-hours lost due to absence during days 15 to 21; instead the shares for days 15 to 20 are reported, which were multiplied by 7/6. Unfortunately this causes a slight overestimation of absence for the years 2003 and 2004, because of less absence the 21st day compared to the average for days 15-20. The absence patterns for the first and second halves of 2003 were assumed identical in order to adjust the data.

[24] For 1997 *Sickness* was multiplied by a factor ranging from 1.12 to 1.23 depending on the

Disability and *Rehab* to the adjusted version of *Sickness*. s^{women} and s^{men} were created similarly. Table 1 reports adjusted versions of these variables.

Appendix C: Age-adjustment of health care expenditure

The age-adjustment was based on an index of age dependent health care consumption produced by Statistics Sweden, which was used to calculate intergovernmental equalization grants for counties until 1995.[25]

For somatic short-term health care, psychiatric care, and geriatric care, the national average of treatment days for the age-groups 0-14, 15-44, 45-64, 65-79 and 80- were provided each year by the National Board on Health and Welfare.

The average number of physician consultations in primary health care and hospital connected health care were provided by the County Council of Skåne for 1991-1993 (covering only the county of Malmöhus, which became a part of Skåne in 1998) and for 2001-2004. To avoid regional differences, only data from former Malmöhus were used for the last period as well. These sources were used since no national figures were available. For 1991-1993 the data were reported for the same five age-groups as above, but for 2001-2004 eight age-groups were used: 0-4, 5-14, 15-24, 25-44, 45-64, 65-74, 75-84, and 85-. Based on this data, percentage changes each year were estimated for the two consultations types for the age-groups 0-14, 15-44, 45-64, and 65-. Then the number of consultations in each of the eight age-groups were calculated for 1993-2000 using the estimated percentage changes for the group 0-14 for the age-groups 0-4 and 5-14, etc.

Using these figures and population data, the expected number of treatment days and physician consultations per inhabitant were calculated for each county, each year, for each type of health care. These numbers were then divided by the national average to obtain indexes, which were aggregated using each health care category's national cost-share each year as weight. The cost-shares were

region. The corresponding figures for 1998, 2003, and 2004 are 1.03 to 1.05; 1.01 to 1.04; and 1.01 to 1.07, respectively. For all other years the factor was of course 1.

[25] Two other methods have been used by Statistics Sweden since 1995, the first based on a regression of health care expenditure on a number of macro-variables. This method was not used here since it can capture not only differences in need for health care but also differences in preference and resources. The second method was not used here since it depends on the number of patients with particular diagnoses, which is likely to be endogenous.

calculated from data obtained from Statistics Sweden for the years 1993-2003. The average cost-shares were; somatic short-term health care (0.5); psychiatric care (0.08); geriatric care (0.05); physician consultations in primary health care (0.19) and physician consultations in hospital connected health care (0.18). These five categories accounted for approximately 95 percent of non-dental non-pharmaceutical health care consumption. Finally, age-adjusted health care expenditure ($eadj$) was calculated by dividing health care expenditure (e) with the appropriate index for each observation.

Appendix D: Relevance and validity of the instruments

Table 2 reports the Cragg-Donald weak identification statistic, which is the smallest eigenvalue of the matrix analog to the F-statistic from the first-stage regressions. Since the models include several endogenous variables, this statistic is reported instead of the F-statistic. Based on the tabulation by Stock and Yogo (2005) of critical values for the Cragg-Donald weak identification statistic, the instrument is judged to be strong if the statistic is above 13.95 in the dynamic or 15.72 in the static specification.[26] The statistic is well above these values in all specifications.

As a complement to this test, Shea's (1997) partial R-squared measure of instrument relevance for models with multiple endogenous variables is reported. Shea did not provide any critical values, but mentioned 0.05 as an example when the instrument set is not very relevant for an endogenous regressor, and noted that low relevance increases asymptotic standard errors and increases the inconsistency of the estimates whenever instruments are not perfectly exogenous. The Shea values in Table 2 are above 0.05 for all endogenous regressors, especially for $eadj$, but are quite close (0.06) for Δs_{t-1} in two specifications. Together the Cragg-Donald and Shea statistics indicate that the instruments

[26] The critical values used are those for a maximum bias of 0.05 relative to OLS for a two-stage least squares (TSLS) estimator. No critical values are provided for a general means of moments (GMM) estimator, and these values are only approximate for the GMM estimator, since it diverges somewhat from the TSLS estimator because of heteroscedasticity and autocorrelation. But at least the test indicates strong instruments for the TSLS estimator, which, as discussed in the text, gives similar results.

are, at least, reasonably relevant for all the endogenous regressors.

The Hansen J statistic is the p-value of the Hansen test of overidentifying restrictions, where the joint null hypothesis is that the instruments are valid, i.e., uncorrelated with the error term. This test is consistent in the presence of heteroskedasticity and serial correlation, and supports the exogeneity of the instruments used.

Serial corr. 2 reports the p-value of a t-test of serial correlation of the second order. This test was conducted since the exogeneity assumptions for two of the four instruments were based on the assumption of no second-order serial correlation. The test can be viewed as complementary to the Hansen J test, which would also indicate that the instruments were invalid if second-order serial correlation were too strong. For all specifications, no statistically significant second-order serial correlation was found. Thus, both Hansen J and the Serial corr. 2 test support the assumption that the instruments are valid for all specifications reported in Table 2.

Appendix E: Sickness and disability

The basic transitions between the three mutually exclusive states that an individual can occupy are illustrated in Figure E1.[27] The illustrated flows are those that are driven by other factors than economic incentives, health status and work characteristics. The vertical arrows illustrate the effects of habit formation. The diagonal arrow from $Sickness_{t-1}$ to $Disability_t$ illustrates the flow caused by information increasing over time regarding the expected duration of individual's reduced work capacity. That is, this arrow shows the flow between the two states caused by better predictions about the individual's future work capacity, conditioned on the observed present one.

The flow from $Work_{t-1}$ to $Sickness_t$ illustrates the inflow to absence that is affected by previous absence in the municipality, through its effect on the disutility of absence.[28] One possibility is that this habit formation is not specific

[27] *Rehab* is of minor importance (Table 1) so no separate analysis was done with it as dependent variable.

[28] It is less likely that there is a direct flow from work to disability of this reason and no such arrow is therefore drawn. It is however possible, that for example individuals with long-lasting small reduction in work capacity go directly from work to disability, as a consequence of external habit formation.

to the type of non-work state. For example, a high rate of people on disability pension in a municipality might also reduce the social cost of being on sick leave, resulting in a correlation between $Disability_{t-1}$ and $Sickness_t$, even if no one actually moved in this direction, simply because a high rate of *Disability* caused a flow from *Work* to *Sickness*. Table E1 presents the estimation results for *Sickness* and *Disability*, which are discussed briefly in the text.

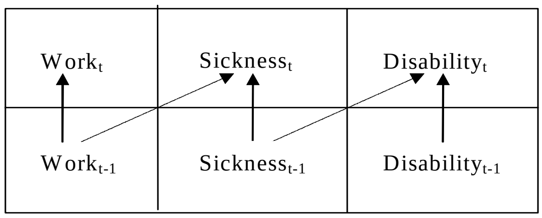


Figure E1. Interactions between *Work*, *Sickness*, and *Disability*

Table E1. Estimation results, first difference of *Sickness* & *Disability*

	Sickness			*Disability*		
	IV	OLS	IV-static	IV	OLS	IV-static
$eadj$	0.05	0.04*	0.03	-0.04	-0.01	0.01
	(0.04)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)
$\Delta Sick._{t-1}$	-0.23	0.10***		0.53***	0.05**	
	(0.16)	(0.03)		(0.13)	(0.02)	
$\Delta Dis._{t-1}$	-0.22**	-0.10***		0.47***	0.34***	
	(0.11)	(0.03)		(0.09)	(0.03)	
$\Delta(w(1-\tau^{mc}))$	-0.04	0.00	-0.05	0.01	0.06***	0.03
	(0.04)	(0.01)	(0.03)	(0.03)	(0.01)	(0.03)
$HighSchool$	1.62*	1.07	1.14	-1.53**	-0.33	-0.57
	(0.90)	(0.72)	(0.77)	(0.73)	(0.63)	(0.74)
$University$	-0.87	-0.46	0.10	-1.90**	-3.08***	-4.15***
	(1.00)	(0.70)	(0.76)	(0.83)	(0.61)	(0.72)
$\Delta HighSchool$	3.83	5.09	5.90	-17.05**	-20.64***	-22.68***
	(8.14)	(8.01)	(7.99)	(7.50)	(6.99)	(6.87)
$\Delta University$	-20.13	-22.48*	-15.84	-19.93*	-31.62***	-31.14***
	(12.75)	(11.51)	(11.98)	(11.77)	(9.62)	(10.49)
$Women$	10.17**	8.51**	6.72*	5.24	7.62***	12.47***
	(4.43)	(3.44)	(3.60)	(3.47)	(2.86)	(3.31)
$Pop4049$	1.09	-0.66	-0.32	-2.71	-0.96	-0.40
	(3.00)	(2.58)	(2.72)	(2.27)	(1.99)	(2.24)
$Pop5054$	-4.99	-3.15	-3.12	2.24	-1.25	-0.02
	(6.10)	(5.07)	(5.45)	(5.21)	(4.19)	(4.63)
$Pop5559$	14.04**	10.22**	11.54**	0.86	2.79	4.78
	(5.89)	(5.19)	(5.37)	(4.93)	(4.38)	(4.88)
$Pop6064$	0.01	-0.87	-0.01	-5.91*	-3.54	-5.40
	(4.61)	(3.96)	(4.06)	(3.54)	(3.13)	(3.48)
$\Delta Pop4049$	13.38	17.93*	17.22	21.35**	15.54*	13.44
	(11.98)	(10.49)	(10.86)	(10.57)	(9.04)	(9.41)
$\Delta Pop5054$	36.49**	33.00**	32.47**	20.56*	25.87**	27.57**
	(14.61)	(13.05)	(13.42)	(11.96)	(10.70)	(11.50)
$\Delta Pop5559$	35.74**	24.99*	24.56*	19.53	32.26***	40.99***
	(15.92)	(13.99)	(13.97)	(13.72)	(11.63)	(11.79)
$\Delta Pop6064$	20.10	9.19	2.28	42.71***	58.25***	77.57***
	(17.39)	(14.31)	(14.31)	(14.05)	(11.12)	(11.87)
$\mathrm{d}s^*/\mathrm{d}eadj\|_{h_{t-1}}^{\boxtimes}$	0.04	0.05*		-0.07	-0.02	
	(0.03)	(0.03)		(0.06)	(0.03)	
Cragg-Donald	9.92		42.94	9.92		42.94
$eadj$ or e: Shea	0.69		0.74	0.69		0.74
$\Delta Sick._{t-1}$:Shea	0.03			0.03		
$\Delta Dis._{t-1}$:Shea	0.09			0.09		
$\Delta(w(1\text{-}\tau^{mc}))$:Shea	0.09		0.09	0.09		0.09
Hansen J	0.27		0.37	0.00		0.44
Serial corr. 2	0.00	0.02	0.02	0.00	0.00	0.52
Adj. R^2	0.59	0.62	0.61	0.70	0.77	0.74
Sample size	2547	2554	2547	2547	2554	2547

$^{\boxtimes}$ $\mathrm{d}s^*/\mathrm{deadj}|_{h_{t-1}}$ states the long run effect of eadj on Sickness and Disability, respectively excluding the effect that goes through the other variable. Also, see notes in Table 2.