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Does sickness absence influence survey participation? A registry-based analysis.

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ABSTRACT

Objectives: Selective participation can bias results in epidemiological surveys. The importance of health status is often suggested as a possible explanation for nonparticipation but few empirical studies exist. In a population-based study, explicitly focussed on sickness absence, health and work, we examined whether a history of high levels of sickness absence was associated with nonparticipation.

Design: The study is based on data from official sickness absence registers from participants, nonparticipants and the total target population of the baseline survey of HAP (the Health Assets Project).

Setting: HAP is a population-based cohort study in the Västra Götaland region in South Western Sweden.

Participants: HAP included a random sample (*n*=7984) and two samples with recent sickness absence (employees (*n*=6140) and nonemployees (*n*=990)), extracted from the same overall general working-age population.

Primary outcome measures: We examined differences in participation rates between samples (2008), and differences in previous sickness absence (2001-2008) between participants and nonparticipants or target population within samples.

Results: No substantial differences in participation by sickness absence were found. Yet, participants had statistically significant less sickness absence during some of the follow-up years than nonparticipants and target population. Other factors than sickness absence were more important in explaining differences in participation, e.g. participants were more likely to be women, older, born in the Nordic countries, married and have higher incomes than nonparticipants.

Conclusion: Though specifically addressing sickness absence, having such experience did not add any substantial layer to selective participation in the present survey. Detailed measures are needed to gain a better understanding for health selection in health-related surveys such as those addressing sickness

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absence, for instance in order to discriminate between selection due to ability or motivation for participation.

Strengths and limitation of this study

- Selective participation by history of sickness absence was examined employing official registries of sickness absence across eight years. Such health data has rarely been applied in former studies on survey representativeness.
- The use of registries yielded complete and unbiased data from participants and nonparticipants alike.
- Data from a population-based survey was employed, increasing generalizability of the findings concerning selective participation.
- Both recent and more distant sickness absence were included as predictors for participation, which may provide evidence on representativeness of participants concerning both recent time and recurrent or prolonged sickness absence.
- The study does not investigate mechanisms for survey participation, which also are important to clarify to provide decision support for how to best approach potential participants.

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INTRODUCTION

Sickness absence is a major challenge and policy development requires high quality and unbiased data. In sickness absence research, surveys and cohort studies remain important to gain better understanding of variations in level, causes, consequences and mechanisms of sickness absence across social groups and gender. A crux of any survey is to ensure sample representativeness; if participants are different than nonparticipants in the variables of interest, estimates may suffer from bias.(1) The declining participation rates in epidemiological surveys observed across Western countries the last 30 years are therefore worrying.(2, 3) Registry data can circumvent issues regarding participation, but often lack the required depth of information for sickness absence research to move forward. Consequently, knowledge about selective survey participation and in particular concerning the key variable, sickness absence, is needed to provide researchers with decision support in how to contact participants and, perhaps more importantly, to evaluate the accuracy of results from such surveys.

In surveys across topics, demographic factors such as female gender, being married and higher socio-economic position are consistently found to predict survey participation,(4-8) whereas the evidence regarding age groups and ethnicity are less conclusive.(9) Existing evidence further suggest health selection whereby participants have better general(5, 7, 10, 11) and mental health,(12) are less likely to be on(5-7) or at risk for disability pension award,(10) and also have a higher life expectancy(13) than nonparticipants. Studies of health status and survey participation have mostly examined rare health-related events (such as hospitalization), or severe or long-lasting illness (like disability pension award and mortality). Barriers and selection mechanisms may be different in these cases than for sickness absence, which is common in the entire population, fluctuate, and in the majority of cases concerns common musculoskeletal and mental illnesses. Sickness absence is moreover a measure of

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health that reflect aspects related to functional and working ability, which might be more relevant than diagnoses in explaining survey behaviour.

If and how sickness absence predict for survey participation is uncertain. Linkages to administrative registries are expedient, as they enable unbiased and complete data from participants and nonparticipants.(14) Of the few studies having employed such data, some have found that participants have lower sickness absence rates than nonparticipants, in line with health selection to survey participation.(4, 15-17) Others have found this among men only,(8) or report weak(18) or no(6) association between sickness absence and survey participation. The unequivocal findings may relate to variations in measures and follow-up time, but also complex selection mechanisms involving reachability, ability and motivation to participate.(19)

Concerning motivation, it is commonly proposed that people will be more prone to participate if the survey topic is relevant for them personally.(e.g. 19, 20, 21, 22) In interviews with participants and responding nonparticipants, perceived value or personal gain of contributing to advances in research in the topic have been highlighted as decisive.(21, 23) Following this line of thought, studies addressing sickness absence should lead to increased inclusion of current and previous sickness absentees. Direct measures of relevance is difficult to obtain in representative samples of study participants, and a feasible compromise is to match characteristics of sampled individuals and the core topic of the survey, and infer topic relevance via these characteristics.(24) Based on this approach, personal relevance selection is found through randomized controlled designs,(25) observed by the general experience that cases are easier to recruit than controls in case-control studies(9) and relating to consent giving in medical record follow-ups.(26)

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Only one study has addressed personal relevance selection in surveys on sickness absence specifically(15) in which, contrasting to the personal relevance hypothesis, participants were found to have *less* sickness absence than nonparticipants. Due to a small study population from one company only, the finding might not be generalizable to a general population-context.

Taken together, it remains empirically unsettled whether sickness absence history influence survey participation, and in particular in surveys where sickness absence is the main topic. The general decreasing participation rates call for studies that can provide a basis for how to approach potential participants in the future. In the current study we analysed associations between registered sickness absence and survey participation in a large population-based survey-linkage study that explicitly focussed on sickness absence (the Health Assets Project, HAP). HAP started in 2008 with the main aim to compare workers with sickness absence experiences to those without such experience concerning health, work life and family affairs. To this end, a unique feature of HAP was the use of a "case-control" sampling technique, sampling two cohorts with a recent, new sickness absence episode (employees and nonemployees) in addition to a random population cohort (not recent sick-listed "controls"), all extracted from working age population of the Västra Götaland region in Sweden. This technique has e.g. enabled studies of differences in individual and structural factors between sick-listed and non-sick-listed(e.g. 27, 28) and predictors of return to work.(e.g. 29, 30) The data collection included links to official registries covering demographics and sickness absence days per year across nine years (2001-2009), extracted at an individual level for participants and aggregate level for the target populations for each of the three cohorts. This specific design allowed for examining our research aim through the following research questions:

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1. Were the participation rates higher in the two samples with a recent, new episode of sickness absence (employees and nonemployees) than in the random population sample?
2. Within each of the three samples, respectively, did participants have more sickness absence days annually in the years preceding the survey (2001-2008) than nonparticipants or the target population?
3. Within each of the three samples, respectively, were the proportions of individuals with registered sickness absence annually in the years preceding the survey (towards 2001) higher among participants than nonparticipants or the target population?

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METHODS

The present study is based on registry data from participants, nonparticipants and the target population of the baseline survey of HAP 2008. Figure 1 depicts the sampling procedure in HAP and which components that are compared in the current study.

Target population and samples in HAP

The study base in HAP was the working age population (19-64 years old) in Västra Götaland in Sweden, a region with both urban and rural areas comprising 17% of the Swedish population. In Sweden, all inhabitants are covered by the national sickness insurance. For employees, the employer covers the first 14 days of a sickness absence episode (except one qualifying day); thereafter benefits are granted from the Social Insurance Agency (SIA). Non-employed (e.g. self-employed, unemployed and students) can apply through self-report for benefit from SIA for sickness absence beyond one day. With help from SIA and Statistics Sweden the following three samples were extracted from the study base to obtain groups with and without recent sickness absence (see also Figure 1 and (27) for more details):

1) a recent sick-listed sample of *employees* (employee-sample), of which the target population consisted of all employed individuals with a *new* sickness absence episode > 14 days during 18.02-15.04.2008 (*n*=12,543); 2) a recent sick-listed sample of *non-employees* (nonemployee-sample), where the target population included all other insured with a *new* sickness absence episode >1 day during 18.02-01.04.2008 (*n*=5,004); and 3) a *random population* sample (population-sample, *n*=7,984). A negative coordination was performed to ensure non-overlapping samples; thus the population-sample included no cases with *new* registrations of sickness absence *during* inclusion. As the survey ideally should be conducted as close as possible to the current absence episode, the sampling frame only included those *registered* in SIA by 15.04.2008 (*n*=6,140 in employee-sample and *n*=4,240 in nonemployee-

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sample). In the employee-sample, the total sampling frame was invited to participate (n=6,140), whereas a random sample of the nonemployee sampling frame was invited (n=990).

Data collection. Eligible participants were invited through a postal survey, sent out 15th and 25th of April 2008 with two reminders (i.e. up till two months after onset of the registered sickness absence episode for the two sick-listed samples). The invitation letter included a description of the study aim, data collection procedures, contact details, and information that withdrawal from the study was possible at any time. It was explicitly stated that the SIA would not have access to information on participation status and that participation would not affect the invitee's sickness allowance. Participants gave informed consent to link survey data to official registry data on socio-demographic factors, sickness absence and employment status. Similar registry data were collected at an aggregate level for the target populations.

In the following, the registry data employed in the current study will be described in more detail, including amendments made to enable comparisons between the individual level data (participants) and aggregate level data (nonparticipants and target population).

Data source and measures on demographic variables

Regarding demographic variables, aggregate data from all *invited* were extracted from Statistics Sweden: *Gender* (male, female), *age group* (19-30, 31-50, 51-64), *country of birth* (Nordic, others), *marital status* (married, not married), and *gross income in intervals* (SEK ≤ 149 000, 150 000-299 000, ≥ 300 000).

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Data sources on registered sickness absence

Data on sickness absence benefit granted from SIA during the years 2001-2008 was extracted from the "Longitudinal integrated database for sickness insurance and labour market research (LISA)". The data included annual number of reimbursed sickness absence days (including sickness absence, rehabilitation and work injury allowance[1]). Data on participants were available at an individual level and data on the target populations at an aggregate level distributed by gender and age groups (employee-sample and nonemployee-sample: age groups 19-30, 31-50, 51-64; population-sample: age groups 20-29, 30-39, 40-49, 50-59 for data on sickness absence days and 16-19, 20-24, 25-29, 30-34, 35-39, 40-44, 45-49, 50-54, 55-59 for data on sickness absence cases).

To achieve appropriate comparison groups, the following accommodations were made: Firstly, as the data from the target populations for employee-sample and nonemployee-sample included those *granted* reimbursement, we excluded participants with no registered sickness absence days in 2008 from the participant groups. Secondly, to approximate nonparticipation groups, we subtracted participants in the employee-sample and nonemployee-sample from their respective target populations. Finally, we handled problems with age-related left censoring back in time (towards 2001) by only including those aged 31-64 in 2008 in the employee-sample and nonemployee-sample. In the population-sample, to correspond to available official statistics, we included those aged 20-59 per calendar year when comparing sickness absence days, and those aged 16-59 per calendar year when comparing sickness absence cases.

[1] The Västra Götaland general population statistics did not include work injury allowance, but this is regarded trivial for the analyses due to small numbers.

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Measures on registered sickness absence

1. Participation rates between samples. As a first crude step we examined whether participation rate in the two samples with a recent registered sickness absence episode (employee-sample and nonemployee-sample) differed from that in the population-sample.

2. Days with registered sickness absence, annually. We compared mean number of registered sickness absence days per year (2001-2008) between participants and nonparticipants (employee-sample and nonemployee-sample) or the target population (population-sample).

3. Proportions with previous sickness absence, annually. Finally, we compared the proportion of individuals with registered sickness absence per year between participants and nonparticipants (employee-sample and nonemployee-sample, 2001-2007) or the target population (population-sample, 2001-2008).

Statistical analyses

The data were analysed using Microsoft Excel 2010 and Stata 12. Differences in demographic characteristics between participants and nonparticipants in each of the three sub-samples were examined as relative proportions and chi-squared tests for aggregate data. Regarding sickness absence, we first compared participation rates with 95% CIs between the samples and performed chi-squared tests for aggregate data. Secondly, we performed one-sample mean-comparison t-test to examine differences in mean number of sickness absence days per year from 2001 till 2008 between participants and their comparison groups in each sample, respectively. To account for gender and age differences between the comparisons groups, we calculated means weighted for the distribution in the respective participant groups. Finally, to compare proportions with registered sickness absence per year, gender-stratified odds ratios

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(95% CIs) were calculated comparing participants and their comparison groups in each sample, respectively.

Ethical considerations

The HAP study was approved by the Ethics Committee at University of Gothenburg (registration number 039-08) and conducted in accordance with the latest version of the Helsinki protocol. The aggregated data on the target populations were based on group level official data records, which are available for research purposes.

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RESULTS

Demographic characteristics of participants and nonparticipants

Table 1 displays demographic characteristics and differences between participants and invited nonparticipants in the three samples. Participants were more likely than nonparticipants to be women, older, born in the Nordic countries, married and have higher incomes in both the population-sample and the recent sick-listed employee-sample. The demographic distribution was more even between participants and nonparticipants in the nonemployee-sample, though participants were more likely than nonparticipants to be women and to be born in the Nordic countries.

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Table 1. Demographic distribution and differences between participants and invited nonparticipants in the three samples included in the Health Assets Project.

	Random population sample			Recent sick-listed employee sample			Recent sick-listed nonemployee sample		
	Participants	Invited nonparticipants		Participants	Invited nonparticipants		Participants	Invited nonparticipants	
	n (%)	n (%)	Difference [a]	n (%)	n (%)	Difference [a]	n (%)	n (%)	Difference [a]
Gender			χ^2=143.9, df=1, p<0.001			χ^2=81.9, df=1, p<0.001			χ^2=9.5, df=1, p=0.002
Women	2234 (55.5)	1664 (42.1)		2196 (66.3)	1558 (55.1)		325 (65.3)	274 (55.7)	
Men	1793 (44.5)	2293 (57.9)		1114 (33.7)	1272 (44.9)		173 (34.7)	218 (44.3)	
Age group			χ^2=129.8, df=2, p<0.001			χ^2=121.4, df=2, p<0.001			χ^2=2.4, df=2, p=0.295
19-30	830 (20.6)	1175 (29.7)		380 (11.5)	516 (18.2)		114 (22.9)	116 (23.6)	
31-50	1803 (44.8)	1799 (45.5)		1479 (44.7)	1428 (50.5)		257 (51.6)	271 (55.1)	
51-64	1394 (34.6)	983 (24.8)		1451 (43.8)	886 (31.3)		127 (25.5)	105 (21.3)	
Country of birth			χ^2=138.4, df=1, p<0.001			χ^2=6.1, df=1, p=0.014			χ^2=6.6, df=1, p=0.010
Nordic	3642 (90.4)	3216 (81.3)		2985 (90.2)	2497 (88.2)		444 (89.2)	411 (83.5)	
Others	385 (9.6)	741 (18.7)		325 (9.8)	333 (11.8)		54 (10.8)	81 (16.5)	
Marital status			χ^2=175.2, df=1, p<0.001			χ^2=66.0, df=1, p<0.001			χ^2=2.1, df=1, p=0.146
Married	1877 (46.6)	1414 (35.7)		1705 (51.5)	1164 (41.1)		220 (44.4)	240 (48.8)	
Not married	2150 (53.4)	2543 (64.3)		1605 (48.5)	1666 (58.9)		278 (55.8)	252 (51.2)	
Income (SEK)			χ^2=179.7, df=2, p<0.001			χ^2=37.1, df=2, p<0.001			χ^2=3.4, df=2, p=0.181
≤ 149 000	987 (24.5)	1496 (37.8)		329 (9.9)	405 (14.3)		178 (35.7)	204 (41.5)	
150 000 – 299 000	1920 (47.7)	1678 (42.4)		2219 (67.0)	1892 (66.9)		254 (51.0)	229 (46.5)	
≥300 000	1120 (27.8)	783 (19.8)		762 (23.0)	533 (18.8)		66 (13.3)	59 (12.0)	

[a]Differences examined using Chi-square tests for aggregate data.

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Differences in participation rates between samples

The participation rate was 3.5 percentage points higher in the employee-sample (53.9%, 95%CI 52.7-55.2) than in the population-sample (50.4%, 95%CI 49.3-51.5) (chi^2=16.75, df=1 p<0.001). The participation rate among the nonemployee-sample (50.3%, 95%CI 47.2-53.5) was similar to the population-sample (chi^2=0.00, df=1 p=0.936).

Differences in survey participation by registered sickness absence days within samples

Overall, there were no substantial differences in registered sickness absence between participants and their comparison groups across the three samples. Participants in the population-sample had statistically lower mean number of sickness absence days per year than the corresponding level in the population in the years 2001-2003. Weighted for gender and age distribution among participants, the differences were statistically significant through 2001-2008, except 2007. Yet, the raw differences in annual mean number of registered sickness absence days only ranged from 1.7-5.3 days (Table 2). The same tendency was found in the employee-sample, however only statistically significant in the years 2001-2003 and 2007, weighted for gender and age distribution (Table 2). Participants in nonemployee-sample had, by contrast, higher mean number of sickness absence days per year than nonparticipants in 2008 and 2007, gender and age weighted (Table 2).

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Table 2. Differences in mean days of registered sickness absence, annually 2001-2008, between the participants and comparison groups within each of the three samples included in HAP

	Participants			Target population[a] or nonparticipants[b]	
Year	n	mean days	95% CI	mean days crude (raw difference[d])	mean days weighted[c] (raw difference[d])
Random population sample[a]					
2008	3379	8.5	7.0-9.9	9.6 (1.1)	10.2 (1.7)*
2007	3426	11.9	10.2-13.7	12.2 (0.3)	13.1 (1.2)
2006	3451	12.7	10.9-14.5	14.0 (1.3)	15.0 (2.3)*
2005	3477	14.2	12.4-16.1	15.7 (1.5)	16.8 (2.6)**
2004	3519	16.2	14.2-18.2	17.7 (1.5)	19.0 (2.8)**
2003	3538	17.6	15.4-19.8	20.4 (2.8)*	21.9 (4.3)**
2002	3468	17.0	14.9-19.2	21.0 (4.0)**	22.2 (5.2)**
2001	3384	14.9	12.9-16.8	19.3 (4.4)**	20.2 (5.3)**
Recent sick-listed employee sample[b]					
2008	2676	81.8	78.3-85.3	78.3 (-3.5)	78.8 (-3.0)
2007	2676	20.3	18.2-22.5	22.5 (2.2)*	23.0 (2.7)*
2006	2672	29.4	26.5-32.7	30.3 (0.9)	31.3 (1.9)
2005	2666	34.3	31.0-37.5	33.7 (-0.6)	35.1 (0.8)
2004	2661	33.2	29.9-26.4	34.6 (1.4)	36.3 (3.1)
2003	2658	32.0	28.8-35.3	35.8 (3.8)*	37.7 (5.7)**
2002	2650	30.4	27.4-33.4	31.9 (1.5)	33.9 (3.5)*
2001	2644	24.4	21.7-27.1	26.6 (2.2)	28.4 (4.0)**
Recent sick-listed nonemployee sample[b]					
2008	277	68.3	57.9-78.7	55.7 (-12.6)*	56.8 (-11.5)*
2007	277	49.5	37.7-61.4	32.8 (-16.7)**	32.9 (-16.6)**
2006	276	47.0	35.0-58.9	36.1 (-11.5)	35.9 (-11.1)
2005	275	47.6	35.8-59.4	39.1 (-0.8)	39.4 (-8.2)
2004	275	39.9	28.8-51.0	40.7 (4.6)	41.2 (1.3)
2003	275	36.1	25.9-46.3	41.5 (4.1)	42.1 (6.0)
2002	273	37.4	26.7-48.1	35.6 (-1.8)	36.1 (-1.3)
2001	272	27.3	18.9-35.8	27.3 (0.0)	27.7 (0.4)

Note: 95%CI=confidence interval. *p<.05; **p<.01. Differences in means examined employing one-sample t-tests.

[a] Participants aged 20-59 in the respective calendar years are compared to the corresponding age groups in the Västra Götaland population (target population).

[b] Nonparticipants comprise all individuals granted benefit by the "Social Insurance Agency" (SIA) for a new spell of sickness absence during the inclusion period (target population), excluding participants. Only participants with ≥1 day of registered sickness absence in 2008 are included to achieve equal inclusion criterion

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as for the nonparticipation group. Only age group 30-64 included to avoid age-related left censoring when going back in time towards 2001.

[c] Weighted for gender and age distribution among HAP participants.

[d] Raw difference= Mean days nonparticipants or target population – mean days participants.

Differences in participation by proportions with registered sickness absence within samples

Regarding individuals with registered sickness absence per year, the proportions were overall lower among participants than nonparticipants or the target population. In the population-sample, compared to the target population, participants had statistically significant lower odds for having had an episode of sickness absence only in 2001 and 2003 for women, and 2001, 2002 and 2003 for men (ORs ranging from .84-.91 for women and .76-.80 for men, see table 3). In the employee-sample, compared to nonparticipants, participants had statistically significant lower odds for having had an episode of sickness absence at most of the comparisons per years from 2001 to 2007 (ORs ranging from .87-.95 for women and .77-.88 for men, see table 3).The corresponding comparisons in nonemployee-sample resulted in small and generally non-significant differences, and in opposing directions for men and women (Table 3).

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Table 3. Gender-stratified proportions and odds ratio (95%CI) for participants in each cohort compared to nonparticipants or target population for having had at least one registered sickness absence each year one to seven years prior to the HAP survey

	Women				Men			
	Part.	Target pop.[a] or nonpart.[b]	Difference		Part.	Target pop.[a] or nonpart.[b]	Difference	
	%	%	OR	95% CI	%	%	OR	95% CI
Random population sample[a]								
2008	10.5	11.7	.89	.77-1.03	7.4	7.1	1.05	.86-1.28
2007	13.7	13.5	1.01	.89-1.15	7.5	8.1	.93	.76-1.12
2006	14.3	15.0	.95	.84-1.08	8.2	8.9	.91	.75-1.09
2005	15.4	16.1	.95	.84-1.07	9.2	9.4	.97	.82-1.15
2004	14.9	16.2	.90	.80-1.02	8.6	9.3	.92	.77-1.10
2003	15.9	18.4	.84	.75-.95**	8.4	10.7	.76	.64-.91**
2002	18.8	20.2	.91	.82-1.02	9.9	12.1	.80	.68-.94**
2001	17.5	19.7	.84	.76-.95**	9.5	11.7	.80	.67-.94**
Recent sick-listed employee sample[b]								
2008	100.0	100.0	-	-	100.0	100.0	-	-
2007	29.5	32.6	.87	.77-.98*	26.8	29.5	.87	.74-1.04
2006	31.5	34.2	.88	.79-.99*	26.2	28.8	.88	.74-1.04
2005	32.6	33.8	.95	.84-1.07	23.1	28.2	.77	.64-.92**
2004	28.8	32.3	.85	.75-.95**	20.6	25.6	.75	.63-.91**
2003	30.0	33.7	.84	.75-.95**	22.8	26.4	.82	.68-.98*
2002	31.7	34.5	.88	.78-.99*	21.3	25.9	.77	.64-.93**
2001	29.3	31.0	.92	.81-1.04	20.0	24.4	.77	.64-.93**
Recent sick-listed nonemployee sample[b]								
2008	100.0	100.0	-	-	100.0	100.0	-	-
2007	52.4	53.4	.96	.69-1.34	54.0	49.2	1.21	0.81-1.81
2006	40.2	46.8	.77	.55-1.07	52.7	42.3	1.52	1.02-2.28*
2005	37.2	42.9	.79	.56-1.11	47.8	38.5	1.46	0.92-2.19
2004	33.5	40.3	.75	.52-1.05	36.0	36.1	1.00	.65-1.51
2003	34.8	40.8	.77	.54-1.09	38.7	36.2	1.12	0.73-1.69
2002	35.0	38.1	.87	.62-1.23	44.6	32.9	1.64	1.08-2.47*
2001	32.1	32.6	.98	.68-1.39	36.4	30.5	1.32	0.86-2.02

Note: OR = Odds ratio. CI = Confidence intervals.

a Participants aged 16-59 in the respective calendar years are compared to the corresponding age groups in the Västra Götaland population (target population).

[c] Only participants with ≥1 day of registered sickness absence in 2008 are included to achieve equal inclusion criterion as for the nonparticipation group. Nonparticipants comprise all individuals granted benefit by the "Social Insurance Agency" (SIA) for a new spell of sickness absence during the inclusion period (target population), excluding participants. Only age group 31-64 included avoiding age-related left censoring (towards 2001).

*p<.05, **p<.01

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DISCUSSION

Main results

No substantial selection by recent or previous sickness absence was found in the HAP study, which specifically invited people to a survey on sickness absence, health and work. Yet, participants had overall somewhat less registered sickness absence in the past than nonparticipants and the target population. Secondary findings harmonize with commonly observed differences in socio-demographic characteristics as participants were more likely than nonparticipants to be women, older, born in the Nordic countries, married and have higher incomes.

Strengths and limitations

The main strengths of this study were chiefly related to our application of objective registry data on sickness absence from participants and nonparticipants. Firstly, this enabled investigating selection effects by sickness absence, which has rarely been achievable in prior research and restricted in many countries by lack of available registries. No study has had length of follow-up as applied in the current study. Many nonparticipation analyses on health variables are based on supplementary surveys of "participating nonparticipants", willing to complete a shortened version of the survey, with the inherent risk of partly reproducing the same nonparticipation bias.(31) Secondly, the use of registries reduced common methodological problems such as recall bias and missing responses.(14) Thirdly, as the registry data are based on financial reimbursement from the SIA, they are considered to be accurate and reliable. Finally, long-term follow-up is a particular advantage when examining selection by sickness absence, as sickness absence on the one hand is common, with a one-year cumulative incidence of 11.3% in the working population in Western Sweden in

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2008,(32) and on the other hand in some cases is prolonged and recurrent. Thus, the findings might inform representativeness of participants regarding both present time and prolonged or recurrent cases. Additionally, most studies on sickness absence as predictor for survey participation have employed specific occupational (4, 8, 15, 17) or diagnostic groups.(33).Including a population-based sample increases generalizability of the findings.

Despite considerable advantages in applying registries in research, the quality and accuracy of an analysis rest on the information available. Firstly, some participants had either no days but one or more case of registered absence or vice versa, whereas it was uncertain as to whether there were corresponding cases among nonparticipants, due to the use of aggregate level data for this group. This uncertainty might have produced noise in the analyses. Our results were however quite robust across alternative analyses of the data, strengthening our confidence in the observed findings.

Secondly, the skewed distribution of sickness absence days makes median calculations more appropriate than means.(34) The use of aggregate data on the target populations precluded calculating median values and standard deviation estimates for the comparison groups. The one-sample t-test was considered a valid approach based on the data available as the t-test is very robust for comparing means, and that the distribution of means, according to "the central limit theorem", will approximate a normal distribution when the sample size increases, even when the distribution in the population is non-normal.(35) That said, interpreting the mean values by themselves can be problematic when the distribution of the data is skewed. Though means of sickness absence days arguably is fairly meaningful, interpretations of results should focus more on the differences in means between groups than the mean values themselves.

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Thirdly, due to the fluctuating nature of sickness absence and lag in registry administration, our comparison groups for research question 1 were inevitable somewhat overlapping concerning sickness absence status. The population-sample naturally included some on-going cases and some cases with onset *after* inclusion (6.7% of the population-sample participants self-reported being currently sickness-absent). Nevertheless, as the employee-sample and nonemployee-sample *all* had recent sickness absence, the comparisons were regarded appropriate. As for the within samples comparisons, nonparticipants in the sick-listed samples comprised the respective target populations minus participants. These target populations also included some non-invited individuals due to registration in SIA after the predefined inclusion period. Lagged registration in SIA is in general slightly skewed.(36) A sensitivity analysis revealed however no differences in outcome between those invited in the first and second round in the employee-sample, with late registrations presumably overrepresented in the latter, indicating fairly comparable sickness absence histories between the invited and non-invited (numbers not shown).

Finally, we only had access to a limited amount of variables characterising the nonparticipation group. Hence we cannot rule out an impact from residual confounding, especially from socio-economic factors, (8, 18) on our results. The registry data did neither include information on medico-legal cause or specific timing of the sickness absence episodes beyond number of registered days per year.

Interpretation of the findings

Selection effects by topic relevance are assumed to be a particular statistical concern as associations are more prone to be biased if selection has to do with the key statistics.(1, 9, 24) Empirical tests of this assumption has thus far not found consequential impact on survey

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estimates analysing associations,(1) in line with most available studies on nonparticipation bias.(5, 9, 10) Prevalence estimates are notably more vulnerable for selection bias. Levels of registered sickness absence among participants did not diverge substantially from the target populations in HAP, and selection by sickness absence is thus not likely to be any substantial source of bias in this particular survey.

Some differences in participation by history of sickness absence were nonetheless observed. As to the mechanisms involved, we did not have the data to address all nuances, but the findings might shed light on some aspects to be addressed in more detail in future studies. Personal relevance by recent or previous sickness absence seemed *not* to be a prominent selection mechanism for this survey. Notably, the participation rate was slightly higher in the recent sickness-listed employee-sample than in the population-sample. This could be interpreted as a "recency effect" of personal relevance selection, as the finding contrasted the results regarding more distant sickness absence. The employee-sample nevertheless also included more women than the population-sample, and as women tend to participate more than men (9) this might have contributed to the observed result. The absolute difference of 3.5% may also be considered of little practical importance. Results for the nonemployee-sample diverged somewhat from the two other samples as well. This might be explained by numerous factors relevant for this sample, such as absence registration schemes, huge heterogeneity of this subsample containing both students and self-employed people, and lastly the small size of this sample.

The overall finding in this study was more compatible with a reduced health and functional capacity among nonparticipants, as we found somewhat less previous sickness absence among those who participated than those who did not. According to the "health selection hypothesis" illness precludes participation in research. (5, 7, 10) Several and potentially opposing mechanisms may have contributed to this finding, including reachability,

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motivation and ability to participate. Naturally, sickness absence can plain and simple entail reduced ability to participate due to poor health, fatigue, motivation or hospitalization, even though the person under normal circumstances would be inclined to participate. Social inequalities are besides related to both sickness absence (37) and differential participation. (7, 8) Barriers and facilitators for survey participation across social groups are not well understood, but may involve both structural barriers and differences in norms and perceived social value of research.(9, 38, 39) Some barriers could be specific to sickness absence: Firstly, "oversurveying" is suggested to contribute explaining falling participation rates in general.(9) Recurrent or long-term sickness absences requires repeated assessments of work capacity to be eligible for sickness insurance, and being approached with yet another questionnaire might not have been welcomed by some of those invited. We do not know anything about partial participation e.g. persons who start to answer the questionnaire, which was rather substantial, but gave up due to tiredness or lack of motivation. Secondly, sensitive questionnaire items decrease participation rates.(25) Stigma and shame related to some diagnoses such as mental illnesses (9, 31) or to the sickness absence status per se (40) could thus have made some more hesitant to participate. In concert with this interpretation, an epidemiological survey on mental health found participant to have fewer psychotropic prescriptions than nonparticipants, albeit using more medical services for somatic disorders.(31) The assurance of confidentiality in the invitation letter, hereunder that the questionnaire was not related to the employer or SIA, probably partly counteracted this,(25) but how much is not easily quantifiable. Diagnoses may also have yielded differences in personal relevance motivation, as the survey overall was more directed towards mental than physical aspects of work, health and sickness absence. In sum, a more direct and specified measure of perceived relevance and attitude toward the topic, albeit challenging to obtain,

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could in theory have discriminated better between individual motivations and barriers for participation.

Conclusion

Selective participation remains a challenge in epidemiological surveys, yet again demonstrated by demographic differences between participants and nonparticipants in the HAP survey. Sickness absence did not seem to add any substantial layer to the selection, based on several registry based comparisons in the present study. Registry data is a substantial resource for increasing knowledge on selective participation. Detailed measures are needed to gain a better understanding for health selection in health-related surveys such as those addressing sickness absence, for instance in order to discriminate between selection due to ability or motivation for survey participation.

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Competing interests

None declared.

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Authors' contribution

MK, JL, GH, SØ and KH designed the study. MK analysed the data, wrote the first draft and main revisions of the manuscript. All authors contributed in interpretation of the data and critical revision of the manuscript, and approved of the final version of the manuscript.

Data sharing

No additional data available.

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FIGURE LEGEND

*Fig 1***.** Flow chart of inclusion procedures in the Health Assets Project (HAP).

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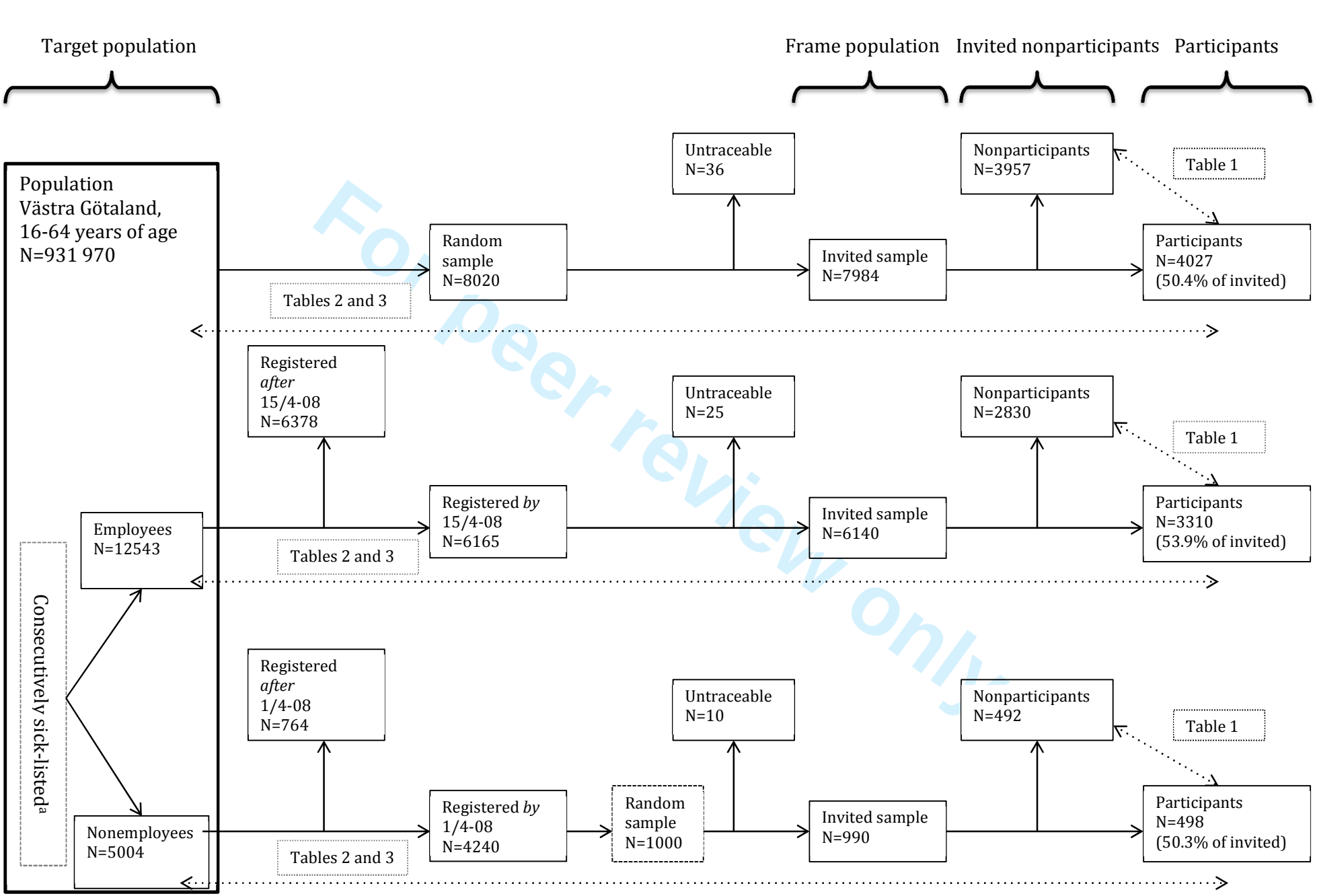

[a]Individuals granted benefit by the "Social Insurance Agency" for a new episode of sickness absence during 18/2-15/4-2008 (emplyees) and 18/2-1/4-2008(nonemployees), respectively

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STROBE 2007 (v4) checklist of items to be included in reports of observational studies in epidemiology*

Checklist for cohort, case-control, and cross-sectional studies (combined)

Section/Topic	Item #	Recommendation	Reported on page #
Title and abstract	1	(*a*) Indicate the study's design with a commonly used term in the title or the abstract	1,2
		(*b*) Provide in the abstract an informative and balanced summary of what was done and what was found	2
Introduction			
Background/rationale	2	Explain the scientific background and rationale for the investigation being reported	4-6
Objectives	3	State specific objectives, including any pre-specified hypotheses	7
Methods			
Study design	4	Present key elements of study design early in the paper	8
Setting	5	Describe the setting, locations, and relevant dates, including periods of recruitment, exposure, follow-up, and data collection	8-9
Participants	6	(*a*) *Cohort study*—Give the eligibility criteria, and the sources and methods of selection of participants. Describe methods of follow-up *Case-control study*—Give the eligibility criteria, and the sources and methods of case ascertainment and control selection. Give the rationale for the choice of cases and controls *Cross-sectional study*—Give the eligibility criteria, and the sources and methods of selection of participants	8
		(*b*) *Cohort study*—For matched studies, give matching criteria and number of exposed and unexposed *Case-control study*—For matched studies, give matching criteria and the number of controls per case	8-9
Variables	7	Clearly define all outcomes, exposures, predictors, potential confounders, and effect modifiers. Give diagnostic criteria, if applicable	9-10
Data sources/ measurement	8*	For each variable of interest, give sources of data and details of methods of assessment (measurement). Describe comparability of assessment methods if there is more than one group	9-10
Bias	9	Describe any efforts to address potential sources of bias	21
Study size	10	Explain how the study size was arrived at	8-9
Quantitative variables	11	Explain how quantitative variables were handled in the analyses. If applicable, describe which groupings were chosen and why	9-10 and 11-12
Statistical methods	12	(*a*) Describe all statistical methods, including those used to control for confounding	11-12
		(*b*) Describe any methods used to examine subgroups and interactions	12
		(*c*) Explain how missing data were addressed	Tables
		(*d*) *Cohort study*—If applicable, explain how loss to follow-up was addressed *Case-control study*—If applicable, explain how matching of cases and controls was addressed	N/A

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		Cross-sectional study—If applicable, describe analytical methods taking account of sampling strategy	
		(*e*) Describe any sensitivity analyses	20,21
Results			
Participants	13*	(a) Report numbers of individuals at each stage of study—eg numbers potentially eligible, examined for eligibility, confirmed eligible, included in the study, completing follow-up, and analysed	8-9
		(b) Give reasons for non-participation at each stage	8-9
		(c) Consider use of a flow diagram	Figure 1
Descriptive data	14*	(a) Give characteristics of study participants (eg demographic, clinical, social) and information on exposures and potential confounders	8-9, 21
		(b) Indicate number of participants with missing data for each variable of interest	Tables
		(c) *Cohort study*—Summarise follow-up time (eg, average and total amount)	11
Outcome data	15*	*Cohort study*—Report numbers of outcome events or summary measures over time	Tables
		Case-control study—Report numbers in each exposure category, or summary measures of exposure	
		Cross-sectional study—Report numbers of outcome events or summary measures	
Main results	16	(*a*) Give unadjusted estimates and, if applicable, confounder-adjusted estimates and their precision (eg, 95% confidence interval). Make clear which confounders were adjusted for and why they were included	Tables
		(*b*) Report category boundaries when continuous variables were categorized	
		(*c*) If relevant, consider translating estimates of relative risk into absolute risk for a meaningful time period	
Other analyses	17	Report other analyses done—eg analyses of subgroups and interactions, and sensitivity analyses	20,21
Discussion			
Key results	18	Summarise key results with reference to study objectives	19
Limitations	19	Discuss limitations of the study, taking into account sources of potential bias or imprecision. Discuss both direction and magnitude of any potential bias	19-21
Interpretation	20	Give a cautious overall interpretation of results considering objectives, limitations, multiplicity of analyses, results from similar studies, and other relevant evidence	21-24
Generalisability	21	Discuss the generalisability (external validity) of the study results	20
Other information			
Funding	22	Give the source of funding and the role of the funders for the present study and, if applicable, for the original study on which the present article is based	24

*Give information separately for cases and controls in case-control studies and, if applicable, for exposed and unexposed groups in cohort and cross-sectional studies.
Note: An Explanation and Elaboration article discusses each checklist item and gives methodological background and published examples of transparent reporting. The STROBE checklist is best used in conjunction with this article (freely available on the Web sites of PLoS Medicine at http://www.plosmedicine.org/, Annals of Internal Medicine at http://www.annals.org/, and Epidemiology at http://www.epidem.com/). Information on the STROBE Initiative is available at www.strobe-statement.org.

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A registry-based analysis of participator representativeness: A source of concern for sickness absence research?

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A registry-based analysis of participator representativeness: A source of concern for sickness absence research?

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ABSTRACT

Objectives: Selective participation can bias results in epidemiological surveys. The importance of health status is often suggested as a possible explanation for nonparticipation but few empirical studies exist. In a population-based study, explicitly focussed on sickness absence, health and work, we examined whether a history of high levels of sickness absence was associated with nonparticipation.

Design: The study is based on data from official sickness absence registers from participants, nonparticipants and the total target population of the baseline survey of HAP (the Health Assets Project).

Setting: HAP is a population-based cohort study in the Västra Götaland region in South Western Sweden.

Participants: HAP included a random population cohort (*n*=7984) and two cohorts with recent sickness absence (employees (*n*=6140) and nonemployees (*n*=990)), extracted from the same overall general working-age population.

Primary outcome measures: We examined differences in participation rates between cohorts (2008), and differences in previous sickness absence (2001-2008) between participants (individual-level data) and nonparticipants or target population (group-level data) within cohorts.

Results: Participants had statistically significant less registered sickness absence in the past than nonparticipants and the target population for some, but not all, of the years analysed. Yet, these differences were not of substantial size. Other factors than sickness absence were more important in explaining differences in participation, whereby participants were more likely to be women, older, born in the Nordic countries, married and have higher incomes than nonparticipants.

Conclusion: Though specifically addressing sickness absence, having such experience did not add any substantial layer to selective participation in the present survey. Detailed measures are needed to gain a better understanding for health selection in health-related surveys such as those addressing sickness

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absence, for instance in order to discriminate between selection due to ability or motivation for participation.

Strengths and limitation of this study

- Selective participation by history of sickness absence was examined employing official registries of sickness absence across eight years. Such health data has rarely been applied in former studies on survey representativeness.
- The sickness absence data on participants, nonparticipants and the target population alike are based on all reimbursements from the Social Insurance Agency, and are not self-reported, which is a strength with regard common methodological problems such as attrition and recall bias.
- As data from a population-based survey was employed, the observed results may reflect general tendencies concerning selective survey participation.
- Both recent and more distant sickness absence were included as predictors for participation, which may provide evidence on representativeness of participants concerning both recent time and recurrent or prolonged sickness absence.
- The study does not investigate mechanisms driving an association between sickness absence and survey participation, such as obstacles or motivations, which also are important to clarify to provide decision support for how to best approach potential participants.

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INTRODUCTION

Sickness absence is a major challenge and policy development requires high quality and unbiased data. In sickness absence research, surveys and cohort studies remain important to gain better understanding of variations in level, causes, consequences and mechanisms of sickness absence across social groups and gender. A crux of any survey is to ensure sample representativeness; if participants are different than nonparticipants in the variables of interest, estimates may suffer from bias.(1, 2) The declining participation rates in epidemiological surveys observed across Western countries the last 30 years are therefore worrying.(3, 4) Registry data can circumvent issues regarding participation, but often lack the required depth of information for sickness absence research to move forward. Consequently, knowledge about selective survey participation and in particular concerning the key variable, sickness absence, is needed to provide researchers with decision support in how to contact participants and, perhaps more importantly, to evaluate the accuracy of results from such surveys.

In surveys across topics, demographic factors such as female gender, being married and higher socio-economic position are consistently found to predict survey participation,(5-9) whereas the evidence regarding age groups and ethnicity are less conclusive.(10) Existing evidence further suggest health selection whereby participants have better general(6, 8, 11, 12) and mental health,(13) are less likely to be on(6-8) or at risk for disability pension award,(11) and also have a higher life expectancy(14) than nonparticipants. Studies of health status and survey participation have mostly examined rare health-related events (such as hospitalization), or severe or long-lasting illness (like disability pension award and mortality). Barriers and selection mechanisms may be different in these cases than for sickness absence, which is common in the entire population, fluctuate, and in the majority of cases concerns common musculoskeletal and mental illnesses. Sickness absence is moreover a measure of

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health that reflect aspects related to functional and working ability, which might be more relevant than diagnoses in explaining survey behaviour.

If and how sickness absence predict for survey participation is uncertain. Linkages to administrative registries are expedient, as they enable unbiased and complete data from participants and nonparticipants.(15) Of the few studies having employed such data, some have found that participants have lower sickness absence rates than nonparticipants, in line with health selection to survey participation.(5, 16-18) Others have found this among men only,(9) or report weak(19) or no(7) association between sickness absence and survey participation. The unequivocal findings may relate to variations in measures and follow-up time, but also complex selection mechanisms involving reachability, ability and motivation to participate.(20)

Concerning motivation, it is commonly proposed that people will be more prone to participate if the survey topic is relevant for them personally.(e.g. 20, 21, 22, 23) In interviews with participants and responding nonparticipants, perceived value or personal gain of contributing to advances in research in the topic have been highlighted as decisive.(22, 24) Following this line of thought, studies addressing sickness absence should lead to increased inclusion of current and previous sickness absentees. Direct measures of relevance is difficult to obtain in representative samples of study participants, and a feasible compromise is to match characteristics of sampled individuals and the core topic of the survey, and infer topic relevance via these characteristics.(25) Based on this approach, personal relevance selection is found through randomized controlled designs,(26) observed by the general experience that cases are easier to recruit than controls in case-control studies(10) and relating to consent giving in medical record follow-ups.(27)

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Only one study has addressed personal relevance selection in surveys on sickness absence specifically(16) in which, contrasting to the personal relevance hypothesis, participants were found to have *less* sickness absence than nonparticipants. Due to a small study population from one company only, the finding might not be generalizable to a general population-context.

Taken together, it remains empirically unsettled whether sickness absence history influence survey participation, and in particular in surveys where sickness absence is the main topic. The general decreasing participation rates call for studies that can provide a basis for how to approach potential participants in the future. In the current study we analysed associations between registered sickness absence and survey participation in a large population-based survey-linkage study that explicitly focussed on sickness absence (the Health Assets Project, HAP). HAP started in 2008 with the main aim to compare workers with sickness absence experiences to those without such experience concerning health, work life and family affairs. To this end, a unique feature of HAP was the use of a "case-control" sampling technique, sampling two cohorts with a recent, new sickness absence episode (employees and nonemployees) in addition to a random population cohort (not recent sick-listed "controls"), all extracted from working age population of the Västra Götaland region in Sweden. This technique has e.g. enabled studies of differences in individual and structural factors between sick-listed and non-sick-listed(e.g. 28, 29) and predictors of return to work.(e.g. 30, 31) The data collection included links to official registries covering demographics and sickness absence days per year across nine years (2001-2009), extracted at an individual level for participants and group level for the target populations for each of the three cohorts. This specific design allowed for examining our research aim through the following research questions:

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1. Were the participation rates higher in the two cohorts with a recent, new episode of sickness absence (employees and nonemployees) than in the random population cohort?
2. Within each of the three cohorts, respectively, did participants have more sickness absence days annually in the years preceding the survey (2001-2008) than nonparticipants or the target population?
3. Within each of the three cohorts, respectively, were the proportions of individuals with registered sickness absence annually in the years preceding the survey (towards 2001) higher among participants than nonparticipants or the target population?

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METHODS

The present study is based on registry data from participants, nonparticipants and the target population of the baseline survey of HAP 2008. Figure 1 depicts the sampling procedure in HAP, which components that are compared and data available for each component in the current study.

Target population and cohorts in HAP

The study base in HAP was the working age population (19-64 years old) in Västra Götaland in Sweden, a region with both urban and rural areas comprising 17% of the Swedish population. In Sweden, all inhabitants are covered by the national sickness insurance. For employees, the employer covers the first 14 days of a sickness absence episode (except one qualifying day); thereafter benefits are granted from the Social Insurance Agency (SIA). Non-employed (e.g. self-employed, unemployed and students) can apply through self-report for benefit from SIA for sickness absence beyond one day. SIA thus have registries of all covered sickness absence beyond 14 days for employees and beyond 1 day for non-employees. With help from SIA and Statistics Sweden the following three cohorts were extracted from the study base to obtain groups with and without recent sickness absence (see also Figure 1 and (28) for more details):

1) A recent sick-listed cohort of *employees* (employee-cohort), of which the target population consisted of all employed individuals with a *new* sickness absence episode > 14 days during 18.02-15.04.2008 (*n*=12,543) and 2) a recent sick-listed cohort of *non-employees* (nonemployee- cohort), where the target population included all other insured with a *new* sickness absence episode >1 day during 18.02-01.04.2008 (*n*=5,004). The sampling frame for these cohorts only included those *registered* in SIA by 15.04.2008 (*n*=6,140 in employee-cohort and *n*=4,240 in nonemployee-cohort), as the survey ideally should be conducted as

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close as possible to the current absence episode. In the employee-cohort, the total sampling frame was invited to participate (n=6,140), whereas a random sample of the nonemployee-sampling frame was invited (n=990). Finally, a 3) *random population* cohort (population-cohort, n=7,984) was invited. A negative coordination was performed to ensure non-overlapping cohorts; thus the population-cohort included no cases with *new* registrations of sickness absence *during* inclusion.

Data collection. Eligible participants were invited through a postal survey, sent out 15th and 25th of April 2008 with two reminders (i.e. up till two months after onset of the registered sickness absence episode for the two sick-listed cohorts). The invitation letter included a description of the study aim, data collection procedures, contact details, and information that withdrawal from the study was possible at any time. It was explicitly stated that the SIA would not have access to information on participation status and that participation would not affect the invitee's sickness allowance. Participants gave informed consent to link survey data to official registry data on socio-demographic factors, sickness absence and employment status. For this study we extracted the corresponding registry data for the each of the three cohorts' target populations, which are officially available at a grouped level.

In the following, the registry data employed in the current study will be described in more detail, including amendments made to enable comparisons between the individual level data (participants) and group level data (nonparticipants and target population).

Data source and measures on demographic variables

Regarding demographic variables, group-level data from all *invited* were extracted from Statistics Sweden: *Participation* (yes, no)*, gender* (male, female), *age group* (19-30, 31-50,

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51-64), *country of birth* (Nordic, others), *marital status* (married, not married (includes co-habitants), and *gross income in intervals* (SEK ≤ 149 000, 150 000-299 000, ≥ 300 000).

Data sources on registered sickness absence

Data on sickness absence benefit granted from SIA during the years 2001-2008 was extracted from the "Longitudinal integrated database for sickness insurance and labour market research (LISA)". The data included annual number of reimbursed sickness absence days (including sickness absence, rehabilitation and work injury allowance[1]). Data on participants were available at an individual level and data on the target populations at a group level, distributed by gender and age groups (employee-cohort and nonemployee-cohort: age groups 19-30, 31-50, 51-64; Västra Götaland population: age groups 20-29, 30-39, 40-49, 50-59 for data on sickness absence days and 16-19, 20-24, 25-29, 30-34, 35-39, 40-44, 45-49, 50-54, 55-59 for data on sickness absence cases).

To achieve appropriate comparison groups, the following accommodations were made: Firstly, as the data from the target populations for employee-cohort and nonemployee-cohort included those *granted* reimbursement, we excluded participants with no registered sickness absence days in 2008 from the participant groups. Secondly, to approximate nonparticipation groups, we subtracted participants in the employee-cohort and nonemployee-cohort from their respective target populations. Finally, we handled problems with age-related left censoring back in time (towards 2001) by only including those aged 31-64 in 2008 in the employee-cohort and nonemployee-cohort. In the population-cohort , to correspond to available official statistics, we included participants aged 20-59 per calendar year when

[1] The Västra Götaland general population statistics did not include work injury allowance, but this is regarded negligible for the analyses due to small numbers.

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comparing sickness absence days, and participants aged 16-59 per calendar year when comparing sickness absence cases.

Measures on registered sickness absence

1. Participation rates between cohorts. As a first crude step we examined whether participation rate in the two cohorts with a recent registered sickness absence episode (employee-cohort and nonemployee-cohort) differed from that in the population-cohort.

2. Days with registered sickness absence, annually. We compared mean number of registered sickness absence days per year (2001-2008) between participants and nonparticipants (employee-cohort and nonemployee-cohort) and the target population (all three cohorts).

3. Proportions with previous sickness absence, annually. Finally, we compared the proportion of individuals with registered sickness absence per year between participants and nonparticipants (employee-cohort and nonemployee-cohort, 2001-2007) or the target population (population-cohort, 2001-2008).

Statistical analyses

The data were analysed using Microsoft Excel 2010 and Stata 12. Differences in participation rate and distribution across demographic characteristics between participants and nonparticipants in each of the three cohorts were examined as relative proportions and chi-squared tests for group-level data. Regarding sickness absence, we first compared participation rates with 95% CIs between the cohorts and performed chi-squared tests for group-level data. Secondly, we performed one-sample mean-comparison t-test to examine

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differences in mean number of sickness absence days per year, and in total, from 2001 till 2008 between participants and their comparison groups in each cohort, respectively. To account for gender and age differences between the comparisons groups, we calculated means weighted for the distribution in the respective participant groups. Finally, to compare proportions with registered sickness absence per year, gender-stratified odds ratios (95% CIs) were calculated comparing participants and their comparison groups in each cohort, respectively.

Ethical considerations

The HAP study was approved by the Ethics Committee at University of Gothenburg (registration number 039-08) and conducted in accordance with the latest version of the Helsinki protocol. The group-level data on the target populations were based on official data records, which are available for research purposes.

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RESULTS

Demographic characteristics of participants and nonparticipants

Table 1 displays demographic characteristics and participation rates across groups between participants and invited nonparticipants in the three cohort. Participants were more likely than nonparticipants to be women, older, born in the Nordic countries, married and have higher incomes in both the population-cohort and the recent sick-listed employee-cohort. The demographic distribution was more even between participants and nonparticipants in the nonemployee-cohort, though participants were more likely than nonparticipants to be women and to be born in the Nordic countries.

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Table 1. Demographic distribution and participation rates across groups between participants and invited nonparticipants in the three cohort included in the Health Assets Project.

	Random population cohort				Recent sick-listed employee cohort				Recent sick-listed nonemployee cohort			
	Participants	Invited nonparticipants	Participation rate		Participants	Invited nonparticipants	Participation rate		Participants	Invited nonparticipants	Participation rate	
	n (%)	n (%)	%	Difference [a]	n (%)	n (%)	%	Difference [a]	n (%)	n (%)	%	Difference [a]
Total	4027	3957	50.4		3310	2830	53.9		498	492	50.3	
Gender				χ^2=143.9				χ^2=81.9				χ^2=9.5
Women	2234 (55.5)	1664 (42.1)	57.3	df=1	2196 (66.3)	1558 (55.1)	58.5	df=1	325 (65.3)	274 (55.7)	54.3	df=1
Men	1793 (44.5)	2293 (57.9)	43.9	p<0.001	1114 (33.7)	1272 (44.9)	46.7	p<0.001	173 (34.7)	218 (44.3)	44.2	p=0.002
Age group				χ^2=129.8				χ^2=121.4				χ^2=2.4
19-30	830 (20.6)	1175 (29.7)	41.4	df=2	380 (11.5)	516 (18.2)	42.4	df=2	114 (22.9)	116 (23.6)	49.6	df=2
31-50	1803 (44.8)	1799 (45.5)	50.1	p<0.001	1479 (44.7)	1428 (50.5)	50.9	p<0.001	257 (51.6)	271 (55.1)	48.7	p=0.295
51-64	1394 (34.6)	983 (24.8)	58.6		1451 (43.8)	886 (31.3)	62.1		127 (25.5)	105 (21.3)	54.7	
Country of birth				χ^2=138.4				χ^2=6.1				χ^2=6.6
Nordic	3642 (90.4)	3216 (81.3)	53.1	df=1	2985 (90.2)	2497 (88.2)	54.5	df=1	444 (89.2)	411 (83.5)	51.9	df=1
Others	385 (9.6)	741 (18.7)	34.2	p<0.001	325 (9.8)	333 (11.8)	49.4	p=0.014	54 (10.8)	81 (16.5)	40.0	p=0.010
Marital status				χ^2=175.2				χ^2=66.0				χ^2=2.1
Married	1877 (46.6)	1414 (35.7)	57.0	df=1	1705 (51.5)	1164 (41.1)	59.4	df=1	220 (44.4)	240 (48.8)	47.8	df=1
Not married	2150 (53.4)	2543 (64.3)	45.8	p<0.001	1605 (48.5)	1666 (58.9)	49.1	p<0.001	278 (55.8)	252 (51.2)	52.5	p=0.146
Income (SEK)				χ^2=179.7,				χ^2=37.1				χ^2=3.4
≤ 149 000	987 (24.5)	1496 (37.8)	39.8	df=2	329 (9.9)	405 (14.3)	44.8	df=2	178 (35.7)	204 (41.5)	46.6	df=2
150 000 – 299 000	1920 (47.7)	1678 (42.4)	53.4	p<0.001	2219 (67.0)	1892 (66.9)	54.0	p<0.001	254 (51.0)	229 (46.5)	52.6	p=0.181
≥300 000	1120 (27.8)	783 (19.8)	58.9		762 (23.0)	533 (18.8)	58.8		66 (13.3)	59 (12.0)	52.8	

[a]Differences examined using Chi-square tests for group-level data.

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Differences in participation rates between cohorts

The participation rate was 3.5 percentage points higher in the employee-cohort (53.9%, 95%CI 52.7-55.2) than in the population-cohort (50.4%, 95%CI 49.3-51.5) (chi^2=16.75, df=1 p<0.001). The participation rate among the nonemployee-cohort (50.3%, 95%CI 47.2-53.5) was similar to the population-cohort (chi^2=0.00, df=1 p=0.936). As detailed in table 1, there were overall more variations in participation rates across demographic groups within cohorts than between the cohorts.

Differences in mean days of registered sickness absence days between participants and comparison groups, within cohorts

Overall, there were no substantial differences in registered sickness absence between participants and their comparison groups across the three cohorts. Participants in the population-cohort had lower mean number of sickness absence days per year than the corresponding level in the population in the years 2001-2003. Weighted for gender and age distribution among participants, the differences were statistically significant through 2001-2008, except 2007. Yet, the raw differences in annual mean number of registered sickness absence days only ranged from 1.7-5.3 days (Table 2). The same tendency was found in the employee-cohort, however only statistically significant when comparing participants to nonparticipants in the years 2001-2003 and 2007, weighted for gender and age distribution (Table 2). Participants in nonemployee-cohort had, by contrast, higher mean number of sickness absence days per year than both nonparticipants and the target population in 2008 and 2007, gender and age weighted (Table 2).

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Table 2. Differences in mean days of registered sickness absence, annually 2001-2008, between the participants and comparison groups within each of the three cohorts included in HAP

	Participants			Nonparticipants		Target population	
Year	n	mean days	95% CI	mean days crude (raw difference[d])	mean days weighted[c] (raw difference[d])	mean days crude (raw difference[d])	mean days weighted[c] (raw difference[d])
Random population cohort[a]							
2008	3379	8.5	7.0-9.9	-	-	9.6 (1.1)	10.2 (1.7)*
2007	3426	11.9	10.2-13.7	-	-	12.2 (0.3)	13.1 (1.2)
2006	3451	12.7	10.9-14.5	-	-	14.0 (1.3)	15.0 (2.3)*
2005	3477	14.2	12.4-16.1	-	-	15.7 (1.5)	16.8 (2.6)**
2004	3519	16.2	14.2-18.2	-	-	17.7 (1.5)	19.0 (2.8)**
2003	3538	17.6	15.4-19.8	-	-	20.4 (2.8)*	21.9 (4.3)**
2002	3468	17.0	14.9-19.2	-	-	21.0 (4.0)**	22.2 (5.2)**
2001	3384	14.9	12.9-16.8	-	-	19.3 (4.4)**	20.2 (5.3)**
Recent sick-listed employee cohort[b]							
2008	2676	81.8	78.3-85.3	78.3 (-3.5)	78.8 (-3.0)	79.2 (-2.6)	79.7 (-2.1)
2007	2676	20.3	18.2-22.5	22.5 (2.2)*	23.0 (2.7)*	22.0 (1.7)	22.3 (2.0)
2006	2672	29.4	26.5-32.7	30.3 (0.9)	31.3 (1.9)	30.1 (0.7)	30.9 (1.5)
2005	2666	34.3	31.0-37.5	33.7 (-0.6)	35.1 (0.8)	33.8 (-0.5)	34.9 (0.6)
2004	2661	33.2	29.9-26.4	34.6 (1.4)	36.3 (3.1)	34.2 (1.0)	35.4 (2.2)
2003	2658	32.0	28.8-35.3	35.8 (3.8)*	37.7 (5.7)**	34.8 (2.8)	36.0* (3.8)
2002	2650	30.4	27.4-33.4	31.9 (1.5)	33.9 (3.5)*	31.4 (1.0)	32.7 (2.3)
2001	2644	24.4	21.7-27.1	26.6 (2.2)	28.4 (4.0)**	26.0 (1.6)	27.1 (2.7)
Total	2639	287.2	273.2-301.2	293.8 (6.6)	304.3 (17.1)	291.5 (4.3)*	299.0 (11.8)
Recent sick-listed nonemployee cohort[b]							

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2008	277	68.3	57.9-78.7	55.7 (-12.6)*	56.8 (-11.5)*	56.6 (-11.1)*	57.6 (-10.7)*
2007	277	49.5	37.7-61.4	32.8 (-16.7)**	32.9 (-16.6)**	33.9 (-15.6)*	34.2 (-15.3)*
2006	276	47.0	35.0-58.9	36.1 (-11.5)	35.9 (-11.1)	36.8 (-10.2)	36.8 (-10.2)
2005	275	47.6	35.8-59.4	39.1 (-0.8)	39.4 (-8.2)	39.6 (-8.0)	39.8 (-7-8)
2004	275	39.9	28.8-51.0	40.7 (0.8)	41.2 (1.3)	40.6 (0.7)	40.9 (1.0)
2003	275	36.1	25.9-46.3	41.5 (4.1)	42.1 (6.0)	41.1 (5.0)	41.5 (5.4)
2002	273	37.4	26.7-48.1	35.6 (-1.8)	36.1 (-1.3)	35.7 (-1.7)	36.1 (-1.3)
2001	272	27.3	18.9-35.8	27.3 (0.0)	27.7 (0.4)	27.3 (0.0)	27.7 (0.4)
Total	271	358.2	304.7-411.7	308.8 (-45.4)	311.7 (-46.5)	311.7 (-46.5)	314.6 (-43.6)

Note: 95%CI=confidence interval. *p<.05; **p<.01. Differences in means examined employing one-sample t-tests.

[a] Participants aged 20-59 in the respective calendar years are compared to the corresponding age groups in the Västra Götaland population (target population).

[b] Only age group 31-64 (per 2008) included to avoid age-related left censoring when going back in time towards 2001.Among participants, only those with ≥1 day of registered sickness absence in 2008 are included to achieve equal inclusion criterion as for the nonparticipation group. Nonparticipants comprise all individuals granted benefit by the "Social Insurance Agency" (SIA) for a new spell of sickness absence during the inclusion period (target population), excluding participants.

[c] Weighted for gender and age distribution among HAP participants.

[d] Raw difference= Mean days nonparticipants or target population – mean days participants.

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Differences in proportions with registered sickness absence between participants and comparison groups, within cohorts

Regarding individuals with registered sickness absence per year, the proportions were overall lower among participants than nonparticipants or the target population. In the population-cohort, compared to the target population, participants had statistically significant lower odds for having had an episode of sickness absence only in 2001 and 2003 for women, and 2001, 2002 and 2003 for men (ORs ranging from .84-.91 for women and .76-.80 for men, see table 3). In the employee-cohort, compared to nonparticipants, participants had statistically significant lower odds for having had an episode of sickness absence at most of the comparisons per years from 2001 to 2007 (ORs ranging from .87-.95 for women and .77-.88 for men, see table 3).The corresponding comparisons in nonemployee-cohort resulted in small and generally non-significant differences, and in opposing directions for men and women (Table 3).

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Table 3. Gender-stratified proportions and odds ratio (95%CI) for participants in each cohort compared to nonparticipants or target population for having had at least one registered sickness absence each year one to seven years prior to the HAP survey

	Women				Men			
	Part.	Target pop.[a] or nonpart.[b]	Difference		Part.	Target pop.[a] or nonpart.[b]	Difference	
	%	%	OR	95% CI	%	%	OR	95% CI
Random population cohort[a]								
2008	10.5	11.7	.89	.77-1.03	7.4	7.1	1.05	.86-1.28
2007	13.7	13.5	1.01	.89-1.15	7.5	8.1	.93	.76-1.12
2006	14.3	15.0	.95	.84-1.08	8.2	8.9	.91	.75-1.09
2005	15.4	16.1	.95	.84-1.07	9.2	9.4	.97	.82-1.15
2004	14.9	16.2	.90	.80-1.02	8.6	9.3	.92	.77-1.10
2003	15.9	18.4	.84	.75-.95**	8.4	10.7	.76	.64-.91**
2002	18.8	20.2	.91	.82-1.02	9.9	12.1	.80	.68-.94**
2001	17.5	19.7	.84	.76-.95**	9.5	11.7	.80	.67-.94**
Recent sick-listed employee cohort[b]								
2008	100.0	100.0	-	-	100.0	100.0	-	-
2007	29.5	32.6	.87	.77-.98*	26.8	29.5	.87	.74-1.04
2006	31.5	34.2	.88	.79-.99*	26.2	28.8	.88	.74-1.04
2005	32.6	33.8	.95	.84-1.07	23.1	28.2	.77	.64-.92**
2004	28.8	32.3	.85	.75-.95**	20.6	25.6	.75	.63-.91**
2003	30.0	33.7	.84	.75-.95**	22.8	26.4	.82	.68-.98*
2002	31.7	34.5	.88	.78-.99*	21.3	25.9	.77	.64-.93**
2001	29.3	31.0	.92	.81-1.04	20.0	24.4	.77	.64-.93**
Recent sick-listed nonemployee cohort[b]								
2008	100.0	100.0	-	-	100.0	100.0	-	-
2007	52.4	53.4	.96	.69-1.34	54.0	49.2	1.21	0.81-1.81
2006	40.2	46.8	.77	.55-1.07	52.7	42.3	1.52	1.02-2.28*
2005	37.2	42.9	.79	.56-1.11	47.8	38.5	1.46	0.92-2.19
2004	33.5	40.3	.75	.52-1.05	36.0	36.1	1.00	.65-1.51
2003	34.8	40.8	.77	.54-1.09	38.7	36.2	1.12	0.73-1.69
2002	35.0	38.1	.87	.62-1.23	44.6	32.9	1.64	1.08-2.47*
2001	32.1	32.6	.98	.68-1.39	36.4	30.5	1.32	0.86-2.02

Note: OR = Odds ratio. CI = Confidence intervals.

a Participants aged 16-59 in the respective calendar years are compared to the corresponding age groups in the Västra Götaland population (target population).

[c] Only participants with ≥1 day of registered sickness absence in 2008 are included to achieve equal inclusion criterion as for the nonparticipation group. Nonparticipants comprise all individuals granted benefit by the "Social Insurance Agency" (SIA) for a new spell of sickness absence during the inclusion period (target population), excluding participants. Only age group 31-64 included avoiding age-related left censoring (towards 2001).

*p<.05, **p<.01

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DISCUSSION

Main results

Participants in the HAP study, which specifically invited people to a survey on sickness absence, health and work, had less registered sickness absence in the past than nonparticipants and the target population in some, but not all of the years analysed. The differences found in sickness absence were moreover not of substantial size. Secondary findings harmonize with commonly observed differences in socio-demographic characteristics as participants were more likely than nonparticipants to be women, older, born in the Nordic countries, married and have higher incomes.

Strengths and limitations

The main strengths of this study were chiefly related to our application of objective registry data on sickness absence from participants and target population. Firstly, this enabled investigating selection effects by sickness absence, which has rarely been achievable in prior research and restricted in many countries by lack of available registries. The current study examined sickness absence history across more years than in previous studies. Many nonparticipation analyses on health variables are based on supplementary surveys of "participating nonparticipants", willing to complete a shortened version of the survey, with the inherent risk of partly reproducing the same nonparticipation bias.(32) Secondly, the use of registries reduced common methodological problems such as recall bias and missing responses.(15) Thirdly, as the registry data are based on financial reimbursement from the SIA, they are considered to be accurate and reliable. Finally, examining sickness absence several years before the survey is a particular advantage when studying selection by sickness absence, as the phenomenon on the one hand is common, with a one-year cumulative

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incidence of 11.3% in the working population in Western Sweden in 2008,(33) and on the other hand in some cases is prolonged and recurrent. Thus, the findings might inform representativeness of participants regarding both present time and prolonged or recurrent cases. Additionally, most studies on sickness absence as predictor for survey participation have employed specific occupational (5, 9, 16, 18) or diagnostic groups.(34). These groups may have specific distributions of sickness absence and demography, population, making the observed results not necessarily applicable to other groups. As the current study examined population-based cohorts, the results may to a greater extent be regarded as general tendencies.Despite considerable advantages in applying registries in research, the quality and accuracy of an analysis rest on the information available. Firstly, some participants had either no days but one or more episode of registered absence or vice versa, whereas it was uncertain as to whether there were corresponding cases among nonparticipants, due to the use of group-level data. This uncertainty might have produced noise in the analyses. Our results were however quite robust across alternative analyses of the data, strengthening our confidence in the observed findings.

Secondly, the skewed distribution of sickness absence days makes median calculations more appropriate than means.(35) The use of group-level data on the target populations precluded calculating median values and standard deviation estimates for the comparison groups. The one-sample t-test was considered a valid approach based on the data available as the t-test is very robust for comparing means, and that the distribution of means, according to "the central limit theorem", will approximate a normal distribution when the sample size increases, even when the distribution in the population is non-normal.(36) That said, interpreting the mean values by themselves can be problematic when the distribution of the data is skewed. Though means of sickness absence days arguably is fairly meaningful,

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interpretations of results should focus more on the differences in means between groups than the mean values themselves.

Thirdly, due to the fluctuating nature of sickness absence and lag in registry administration, our comparison groups for research question 1 were inevitable somewhat overlapping concerning sickness absence status. The population-cohort naturally included some on-going cases and some cases with onset *after* inclusion (6.7% of the population-cohort participants self-reported being currently sickness-absent). Nevertheless, as the employee-cohort and nonemployee-cohort *all* had recent sickness absence, the comparisons were regarded appropriate. As for the within cohorts comparisons, nonparticipants in the sick-listed cohorts comprised the respective target populations minus participants. These target populations also included some non-invited individuals due to registration in SIA after the predefined inclusion period. Lagged registration in SIA is in general slightly skewed.(37) A sensitivity analysis revealed however no differences in outcome between those invited in the first and second round in the employee-cohort, with late registrations presumably overrepresented in the latter, indicating fairly comparable sickness absence histories between the invited and non-invited (numbers not shown).

Finally, we only had access to a limited amount of variables characterising the nonparticipation group. Hence we cannot rule out an impact from residual confounding, especially from socio-economic factors,(9, 19) on our results. The data available on income, country of birth and marital status were retrieved separately from the sickness absence data, precluding the possibility for making statistical adjustments. The registry data did neither include information on medico-legal cause or specific timing of the sickness absence episodes beyond number of registered days per year, precluding some analyses on how sickness absence might influence survey participation.

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Interpretation of the findings

Selection effects by topic relevance are assumed to be a particular statistical concern as associations are more prone to be biased if selection has to do with the key statistics.(1, 10, 25) Empirical tests of this assumption has thus far not found consequential impact on survey estimates analysing associations,(1) in line with most (6, 10, 11, 38), though not all,(12, 39) available studies on nonparticipation bias. Prevalence estimates are notably more vulnerable for selection bias. Levels of registered sickness absence among participants did not diverge substantially from the target populations in HAP, and selection by sickness absence is thus not likely to be any substantial source of bias in this particular survey.

As described in the introduction, selection mechanisms in surveys are complex and involve reachability, ability and motivation to participate. Sickness absence-related motivators and barriers may have influenced participation in opposite direction, as will be elaborated on in the following, in concert contributing to the finding for relatively sickness absence histories between participants and non-participants. The study design did not allow for addressing these nuances directly, but the observed results might shed light on some aspects to be addressed in more detail in future studies. Personal relevance by recent or previous sickness absence seemed *not* to be a prominent selection mechanism for this survey. Notably, the participation rate was slightly higher in the recent sickness-listed employee-cohort than in the population-cohort. This could be interpreted as a "recency effect" of personal relevance selection, as the finding contrasted the results regarding more distant sickness absence. The employee-cohort nevertheless also included more women than the population-cohort, and as women tend to participate more than men (10) this might have contributed to the observed result. The absolute difference of 3.5% may also be considered of little practical importance. Results for

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the nonemployee-cohort diverged somewhat from the two other cohort as well. This might be explained by numerous factors relevant for this cohort, such as absence registration schemes, huge heterogeneity including both students and self-employed people, and lastly the small size of this sample.

The overall finding in this study was more compatible with a reduced health and functional capacity among nonparticipants, as we found somewhat less previous sickness absence among those who participated than those who did not. According to the "health selection hypothesis" illness precludes participation in research. (6, 8, 11) Several and potentially opposing mechanisms may have contributed to this finding. Naturally, current or recent sickness absence can plain and simple entail reduced ability to participate due to poor health, fatigue, motivation or hospitalization, even though the person under normal circumstances would be inclined to participate. Social inequalities are besides related to both sickness absence (40) and differential participation. (8, 9) Barriers and facilitators for survey participation across social groups are not well understood, but may involve both structural barriers and differences in norms and perceived social value of research.(10, 41, 42) Some barriers could be specific to sickness absence: Firstly, "oversurveying" is suggested to contribute explaining falling participation rates in general.(10) Recurrent or long-term sickness absences requires repeated assessments of work capacity to be eligible for sickness insurance, and being approached with yet another questionnaire might not have been welcomed by some of those invited. We do not know anything about partial participation e.g. persons who start to answer the questionnaire, which was rather substantial, but gave up due to tiredness or lack of motivation. Secondly, sensitive questionnaire items decrease participation rates.(26) Stigma and shame related to some diagnoses such as mental illnesses (10, 32) or to the sickness absence status per se (43) could thus have made some more hesitant to participate. In concert with this interpretation, an epidemiological survey on mental health

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found participant to have fewer psychotropic prescriptions than nonparticipants, albeit using more medical services for somatic disorders.(32) The assurance of confidentiality in the invitation letter, hereunder that the questionnaire was not related to the employer or SIA, probably partly counteracted this,(26) but how much is not easily quantifiable. Diagnoses may also have yielded differences in personal relevance motivation, as the survey overall was more directed towards mental than physical aspects of work, health and sickness absence. In sum, a more direct and specified measure of perceived relevance and attitude toward the topic, albeit challenging to obtain, could in theory have discriminated better between individual motivations and barriers for participation.

Conclusion

Selective participation remains a challenge in epidemiological surveys, yet again demonstrated by demographic differences between participants and nonparticipants in the HAP survey. Sickness absence did not seem to add any substantial layer to the selection, based on several registry based comparisons in the present study. Registry data is a crucial resource for increasing knowledge on selective participation. Detailed measures are needed to gain a better understanding for health selection in health-related surveys such as those addressing sickness absence, for instance in order to discriminate between selection due to ability or motivation for survey participation. Until such studies are performed, the overall findings of the current study did not give rise to much concern about representativeness of survey participants regarding sickness absence history.

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Competing interests

None declared.

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Authors' contribution

MK, JL, GH, SØ and KH designed the study. MK analysed the data, wrote the first draft and main revisions of the manuscript. All authors contributed in interpretation of the data and critical revision of the manuscript, and approved of the final version of the manuscript.

Data sharing

No additional data available.

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FIGURE LEGEND

*Fig 1***.** Flow chart of inclusion procedures in the Health Assets Project (HAP).

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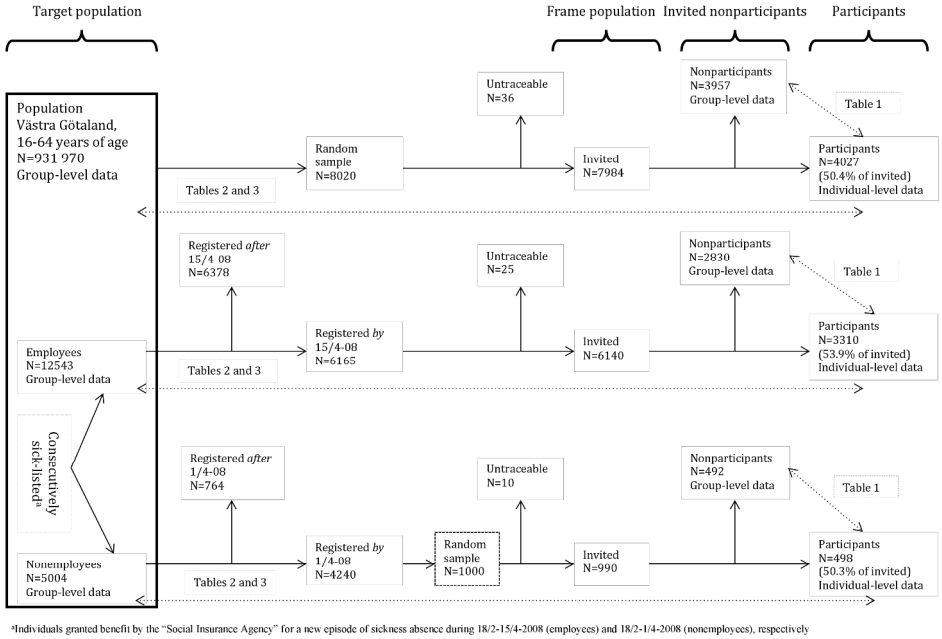

Fig 1. Flow chart of inclusion procedures in the Health Assets Project (HAP).
Figure 1
297x209mm (300 x 300 DPI)

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STROBE 2007 (v4) checklist of items to be included in reports of observational studies in epidemiology*

Checklist for cohort, case-control, and cross-sectional studies (combined)

Section/Topic	Item #	Recommendation	Reported on page #
Title and abstract	1	(*a*) Indicate the study's design with a commonly used term in the title or the abstract	1,2
		(*b*) Provide in the abstract an informative and balanced summary of what was done and what was found	2
Introduction			
Background/rationale	2	Explain the scientific background and rationale for the investigation being reported	4-6
Objectives	3	State specific objectives, including any pre-specified hypotheses	7
Methods			
Study design	4	Present key elements of study design early in the paper	8
Setting	5	Describe the setting, locations, and relevant dates, including periods of recruitment, exposure, follow-up, and data collection	8-9
Participants	6	(*a*) *Cohort study*—Give the eligibility criteria, and the sources and methods of selection of participants. Describe methods of follow-up *Case-control study*—Give the eligibility criteria, and the sources and methods of case ascertainment and control selection. Give the rationale for the choice of cases and controls *Cross-sectional study*—Give the eligibility criteria, and the sources and methods of selection of participants	8
		(*b*) *Cohort study*—For matched studies, give matching criteria and number of exposed and unexposed *Case-control study*—For matched studies, give matching criteria and the number of controls per case	8-9
Variables	7	Clearly define all outcomes, exposures, predictors, potential confounders, and effect modifiers. Give diagnostic criteria, if applicable	9-10
Data sources/ measurement	8*	For each variable of interest, give sources of data and details of methods of assessment (measurement). Describe comparability of assessment methods if there is more than one group	9-10
Bias	9	Describe any efforts to address potential sources of bias	21
Study size	10	Explain how the study size was arrived at	8-9
Quantitative variables	11	Explain how quantitative variables were handled in the analyses. If applicable, describe which groupings were chosen and why	9-10 and 11-12
Statistical methods	12	(*a*) Describe all statistical methods, including those used to control for confounding	11-12
		(*b*) Describe any methods used to examine subgroups and interactions	12
		(*c*) Explain how missing data were addressed	Tables
		(*d*) *Cohort study*—If applicable, explain how loss to follow-up was addressed *Case-control study*—If applicable, explain how matching of cases and controls was addressed	N/A

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		Cross-sectional study—If applicable, describe analytical methods taking account of sampling strategy	
		(*e*) Describe any sensitivity analyses	20,21
Results			
Participants	13*	(a) Report numbers of individuals at each stage of study—eg numbers potentially eligible, examined for eligibility, confirmed eligible, included in the study, completing follow-up, and analysed	8-9
		(b) Give reasons for non-participation at each stage	8-9
		(c) Consider use of a flow diagram	Figure 1
Descriptive data	14*	(a) Give characteristics of study participants (eg demographic, clinical, social) and information on exposures and potential confounders	8-9, 21
		(b) Indicate number of participants with missing data for each variable of interest	Tables
		(c) *Cohort study*—Summarise follow-up time (eg, average and total amount)	11
Outcome data	15*	*Cohort study*—Report numbers of outcome events or summary measures over time	Tables
		Case-control study—Report numbers in each exposure category, or summary measures of exposure	
		Cross-sectional study—Report numbers of outcome events or summary measures	
Main results	16	(*a*) Give unadjusted estimates and, if applicable, confounder-adjusted estimates and their precision (eg, 95% confidence interval). Make clear which confounders were adjusted for and why they were included	Tables
		(*b*) Report category boundaries when continuous variables were categorized	
		(*c*) If relevant, consider translating estimates of relative risk into absolute risk for a meaningful time period	
Other analyses	17	Report other analyses done—eg analyses of subgroups and interactions, and sensitivity analyses	20,21
Discussion			
Key results	18	Summarise key results with reference to study objectives	19
Limitations	19	Discuss limitations of the study, taking into account sources of potential bias or imprecision. Discuss both direction and magnitude of any potential bias	19-21
Interpretation	20	Give a cautious overall interpretation of results considering objectives, limitations, multiplicity of analyses, results from similar studies, and other relevant evidence	21-24
Generalisability	21	Discuss the generalisability (external validity) of the study results	20
Other information			
Funding	22	Give the source of funding and the role of the funders for the present study and, if applicable, for the original study on which the present article is based	24

*Give information separately for cases and controls in case-control studies and, if applicable, for exposed and unexposed groups in cohort and cross-sectional studies.

Note: An Explanation and Elaboration article discusses each checklist item and gives methodological background and published examples of transparent reporting. The STROBE checklist is best used in conjunction with this article (freely available on the Web sites of PLoS Medicine at http://www.plosmedicine.org/, Annals of Internal Medicine at http://www.annals.org/, and Epidemiology at http://www.epidem.com/). Information on the STROBE Initiative is available at www.strobe-statement.org.

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