SWEDISH SOCIAL INSURANCE INSPECTORATE

Equilibrium effects of monitoring

Evidence from a quasi experiment in the Swedish temporary parental benefit program

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Abstract

Monitoring and screening have been shown to be empirically important in reducing the use of social insurance benefits. Most previous studies have focused on ex-post effects of monitoring but *ex ante* effects from monitoring could potentially be even more important. This paper contributes to this literature by empirically studying whether monitoring in the Swedish temporary parental benefit program affects future take-up rates. We estimate the effects of being randomly selected into monitoring on the take-up of temporary parental benefits during the following year. The results suggest that parents who are selected into monitoring reduce their future benefit take-up.

1 Introduction

In order to reduce the moral hazard in social insurance programs benefits are not in general paid out unconditionally, that is, without monitoring and screening the eligibility. Given that screening and monitoring is costly an important empirical question is how this enforcement should be made optimally. The theoretical literature on law enforcement agents show that sanctions should be large when monitoring is costly. This theoretical prediction is however of limited value for policy given that there exists type II errors in any insurance (i.e., the situation when individuals are wrongly condemned), together with a public opinion against too hard punishments.

In many programs, e.g., unemployment insurance (UI) and sickness insurance (SI) programs, it is easy to show theoretically that there are both ex ante and ex post effect of monitoring and sanctions. There is quite strong empirical support of ex post effects of especially benefit sanctions in the UI literature. There is a sharp increase in the exit rate from unemployment to employment when individuals are sanctioned. There is also, by now, plenty of evidence of the effects of monitoring in the SI program (cf. Hägglund (2010), D'Amuri (2011), Hartman et al (2013)). The general result is that time limits and screening/monitoring reduce time on sickness benefits.

Empirical evidence on ex ante or deterrence[1] effects is harder to arrive at. Boone and van Ours (2006) calibrate their theoretical model to Dutch data and show by using simulation that the strength of the deterrence effect depends on the monitoring intensity. If the monitoring intensity is high, the deterrence effect could be very important. There are results of "threat effects" of active labour market training (ALMT) programs (see Graversen and Larsen (2013) for an updated review of the empirical literature as well as a reanalysis of the result in Geerdsen (2006)). That is, individuals being offered an ALMT program have a faster exit rate from unemployment before the date when supposed to enter into the program than those not offered a program. As the offer can be seen as screening the unemployed individual's motivation this result gives some support that deterrence effects in the UI program could be important.

In the "threat effects" literature the individuals know with certainty when to enter an ALMT program. This differs from the theoretical framework analyzing ex ante effects in which the probability of being detected is assumed to be known but less than one. It is thus interesting to empirically study deterrence effects in programs with small probabilities of being detected which is the situation studied in this paper.

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[1] A potential ex ante effect from monitoring is from deterring people from taking-up the insurance, hence, the ex ante effect is in the following also denoted a deterrence effect.

An additional question is the consequences on take-up rates when the individuals do not have full information of the detection probability. If individuals update their probability of being detected misusing the insurance when being monitored this would reduce the take-up probability later on given that there is a deterrence effect.[2] This would result in a form of equilibrium effect from monitoring. In contrast, if the detection probability was known the individual’s future take-up of the insurance would not be affected by being monitored today even if a deterrence effect exists. Furthermore if there is no deterrence effect then the updating of probabilities of detection will not affect future take up either.

There is some support of this type of equilibrium effect through the updating of probabilities of being monitored in the SI literature. Johansson and Lindahl (2013) find that screening affects future take-up rates of sickness benefits. de Jong et al., (2011) finds that that the increased screening in the SI program reduce the applications to the disability insurance program. This effect is denoted self screening in de Jong et al. (2011).

This paper studies the existence of an equilibrium effect of monitoring or self screening by using data from the Swedish temporary parental benefit program. The program is well suited to test for an equilibrium effect on insurance programs as the frequency of using the insurance is high. In addition we have the advantage that the SSIA picks out parents randomly to be monitored which enables us to estimate the effects of being randomly assigned to be monitored on future (up to one year after the assignment) temporary parental leave payments. If individuals do not know the probability of detection but make inferences about the probability of the detection by using heuristic rules, e.g. based on their own experience then they are likely to "overestimate" the probability of detection some time period after being monitored even when the level of monitoring is constant, which is the case for the program in the studied period.

The temporary parental benefit compensates for the loss of earnings when staying at home from work to take care of a sick child (below the age of 12). In the SI program the employer pays the sickness benefits the first fourteen days and there is one waiting day before receiving the compensation, while there is none in the temporary parental benefit program. Both the employer period and the waiting day gives monetary incentives of using the insurance for the parents own illness.[3] In order to claim benefits from the insurance parents simply report that the child is ill to the Swedish Social Insurance Agency (SSIA). This implies that parents could claim for benefits for own illness but also as a means of increasing incomes, that is, they can work and let the child attend day care or school during the claimed benefit period. The sanctions when misusing the system are small or not existing and the degree of monitoring or screening is low (around 5 percent of all claims). It is hence very likely that any finding of an economic significant equilibrium effect could be carried over to other programs with less lenient sanctions and to programs with more monitoring.

[2] This prediction builds on the idea of bounded rationality, that is, we humans make decisions based on probabilities that are estimated using simple heuristic rules (cf. Kahneman (2003)).

[3] Since the cap in the temporary insurance is lower than the cap in the sickness insurance the incentives differ across individuals. The incentives may be the opposite for high income individuals who expect to be long term absent.

We find that individuals assigned to be monitored on average reduce their later take up rate. We interpret the result as individuals after being monitored adjust their assessment of the probability of monitoring which then affects later take-up. The empirical design does not allow us to estimate the magnitude of the deterrence effect from monitoring. It is however possible to show that the present level of monitoring save cost for the government just because it changes the perceptions of the level of monitoring within the program.

Methodologically the paper by Kleven et al. (2011) is close to our study. In their study effects on future tax evasion from being assigned to be audited the year before is estimated. They find evidence of substantial effects on self reported income the year after. Another highly relevant study is Engström et al. (2007). Based on a randomised design they sent out "threat of monitoring letters" to parents eligible for the Swedish temporary parental benefit.[4] The results show that the parents who received the "threat of monitoring" letter decreased their use of the benefit by 13 per cent.[5] This paper, thus, shows that individuals respond on information of (increased) monitoring. However, as it is not custom for authorities to inform individuals on future monitoring this may not be an estimate of an ex ante or deterrence effect and the paper have been criticised in Sweden for overstating the effect of monitoring. Our results however seems to be in accordance with theirs.

The rest of the paper is arranged as follows. Section 2 describes the Swedish temporary parental benefit. Section 3 describes the data. The analysis is given in section 4 and section 5, finally, concludes.

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[4] These letters are similar to "threat of audit" letters in the tax evasion literature (cf., Coleman (1996), Slemerod et al (2001) and Hasseldine et al. (2007) and Kleven et al. (2011).

[5] Persson (2011) shows that the increased monitoring also had spill-over effects on parent's own sickness absence. The parents who were informed about the increased monitoring were more likely to be absent from work due to own sickness. The increase in the parent's own sickness absence corresponded to 43 per cent of the decrease in the take-up of temporary parental benefit.

2 Temporary parental leave insurance

Temporary parental benefit is available for all parents who need to stay at home from work to take care of a sick child aged less than 12 years.[6] Temporary parental benefit can also be received if the person who usually looks after the child is ill or when the parent needs to take the child to a doctor or a dentist. The benefit can be paid out for whole days or a fraction of a day if the parent has not been absent from work the whole day. The benefit can be paid out a maximum of 120 days[7] per year and child without any waiting period. After seven days in every benefit spell a medical certificate on the child's illness is required. However, most benefit spells are short, usually only one or two days. In 2012, the parents who used the benefit claimed on average 7.2 gross benefit days per year. About 46 per cent of the parents who were likely to be entitled to the benefit if their child was ill actually claimed any benefit (53 per cent of the mothers and 38 per cent of the fathers).

The temporary parental benefit compensates 77.6 per cent of the foregone earnings up to a monthly wage of SEK 27,750 (EUR 3,011) in 2014. In 2011, approximately 62 per cent of the fathers and 27 per cent of the mothers eligible to the benefit had an income exceeding the benefit cap. The benefit cannot be received if the parent is receiving sick-pay or other social insurance benefits, e.g., unemployment benefits or parental benefits.

The parent needs to notify the SSIA on the first absence day in order to use the benefit. The parent can then later on apply for the benefit. The SSIA checks the information the parent provides when applying for the benefit. All applications are automatically checked against the information previously registered at the SSIA, for example parent's income and whether the parent is receiving any other social insurance benefits. The SSIA also exchanges information with other authorities and matches data from, among others, the National Board of Student Aid, the unemployment funds and the Swedish Tax Agency in order to detect cases of benefit fraud.

Since 2006 the SSIA has carried out controls that the parent has not worked and that the child has been absent from the day care or school during the benefit period. Between July 2008 and December 2012 the child's absence was monitored with an absence certificate that the day care or school had to sign before the parent sent the certificate to the SSIA. The

[6] In some cases it is also possible to receive temporary parental benefit for children older than 12 years. Special rules also apply for children under the age of 8 months.

[7] However, unlimited number of benefit days can be paid out if the child is seriously ill. In those cases, a doctor's certificate is required from the first benefit day. The benefit spells for children who are seriously ill are excluded from the analysis in this study.

absence certificate was required in nearly all the applications. The absence certificate was abolished in January 2013. Usually about 5 per cent of the applications are selected for monitoring. However, during some months, usually in the summer when the use of the benefit decreases, no applications have been selected for monitoring (see figure 1 for details).

The parent is contacted only when the information from the employer, day care or school differs from the information the parent has given on the application. Hence, the parents are often only aware of being monitored when some error has been detected in the application. It is also possible that the employer, the day care or the school informs the parent about the monitoring. We have no information on how often this occurs.

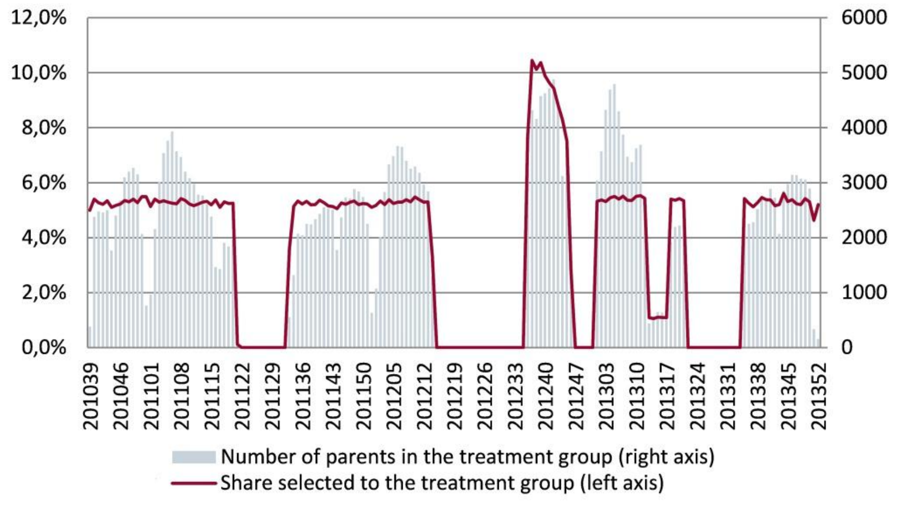
It should also be stressed that by far all discrepancies that are discovered in the monitoring are conscious misuse and many discrepancies are not even real errors. In 7 per cent of the monitored applications some type of error or discrepancy is detected. It should also be noted that the monitoring aims to detect whether the parent has worked or the child has been present at child care or school, not to detect other possible types of misuse. For example it is not possible to detect if a parent is staying at home with a healthy child a few days. During the first week of the benefit spell no medical certificate on the child’s health status is required. The same rule applies also for receiving sickness benefits. However in the sickness insurance program the employer pays the sickness benefits the first fourteen days and there is one waiting day before receiving the compensation.

3 Data and descriptive statistics

The analysis is based on register data from the SSIA. The population consists of parents with children aged 1 to 11 years who applied for the temporary parental benefit between October 1 2010 and December 31 2013. The SSIA's treatment sample is randomized on the applications and not on the parents. Therefore, parents who use the benefit more often have a higher probability to be monitored over time. In order to deal with this problem we select all parents in a given week (starting in October 1 2010) whose parental benefit claim was chosen to be monitored and select a random sample of parents (two comparison parents are chosen to each treated) [8] in the same week who were not selected to be monitored. [9]

We have matched information on the socio-economic background of the parents to the data. Furthermore the number of benefit claims and paid gross and net benefit days for the period October 1 2009 to March 4 2014 is added. The SSIA's register includes a rich set of socio-economic background variables, such as gender, age, marital status, education level, income, and the number and ages of the children. We also have information on the parent's sickness and parental benefit spells.

Figure 1. The number and share of applications selected to monitoring each week



[8] We select only two matches to the treated to make the analyses sample manageable.

[9] Since the SSIA's treatment assignment is not done on weekly basis a few (<1 per cent of the claims) selected parents have more than one benefit claim during the same week. For convenience we removed these observations from the estimation sample. Sensitivity analyses show that the results are not sensitive to this sample selection.

Figure 1 shows the number and the share of benefit applications selected to monitoring each week between October 1 2010 and December 31 2013. Between 40 and 4,900 applications are assigned to treatment each monitoring week. For most weeks, this corresponds to about 5 per cent of the applications. During some weeks, however, no applications have been selected for monitoring. In autumn 2012 about 10 per cent of the applications were selected for monitoring.

Table 1 shows the descriptive statistics for the treatment and the comparison groups. It seems convincing that the treatment assignment is random. The treatment and the comparison groups are very similar to each other. There are, however, small differences in the age of the parent, the number of children and the age of the youngest child between the treatment and the comparison group.

The sum of temporary parental benefit days 52 weeks before the monitoring assignment does not differ between the groups. On average the parents in the treatment and comparison groups got paid for approximately 9.2 gross benefit days during the 52 weeks before monitoring. After monitoring the treatment group uses less benefit days compared to the comparison group.[10] The benefit take-up for the treatment group 52 weeks after the monitoring assignment is 0.11 gross benefit days lower than for the comparison group. This can be interpreted as a causal effect of monitoring assignment. More precise analysis is carried out in the next chapter to verify this effect.

[10] Both groups have lower level of benefit take-up in the period after. One reason for this is that the follow-up period is shorter than 52 weeks for parents selected for the treatment and comparison groups after February 2013.

Table 1. Descriptive statistics

Variable	Comparison group Mean	Comparison group 95% CI	Treatment group Mean	Treatment group 95% CI
Sum of gross benefit days during 52 weeks preceding the treatment assignment	9.170	[9.148–9.192]	9.189	[9.157–9.220]
Sum of gross benefit days during 52 weeks following the treatment assignment	8.240*	[8.218–8.261]	8.132*	[8.101–8.162]
Female	0.638	[0.637–0.639]	0.637	[0.636–0.639]
Married	0.556	[0.555–0.557]	0.556	[0.554–0.558]
Unmarried	0.384	[0.382–0.385]	0.384	[0.382–0.386]
Divorced	0.059	[0.058–0.060]	0.058	[0.058–0.059]
Widow/er	0.002	[0.001–0.002]	0.002	[0.002–0.002]
Lower secondary education	0.057	[0.056–0.057]	0.056	[0.055–0.057]
Upper secondary education	0.450	[0.448–0.451]	0.449	[0.447–0.450]
Post-secondary education	0.494	[0.493–0.495]	0.496	[0.494–0.497]
Age of the parent*	37.322	[37.318–37.34]	37.254	[37.23–37.27]
Number of children*	1.795	[1.793–1.797]	1.804	[1.802–1.807]
Youngest child < 1 years	0.011	[0.011–0.011]	0.012	[0.011–0.012]
Youngest child 1-3 years*	0.537	[0.536–0.538]	0.543	[0.542–0.545]
Youngest child 4-6 years*	0.277	[0.276–0.278]	0.274	[0.273–0.276]
Youngest child 7-11 years*	0.175	[0.174–0.176]	0.171	[0.169–0.172]
Born abroad	0.120	[0.119–0.120]	0.119	[0.118–0.120]
Yearly income/1000	308.8	[308.0–309.1]	308.2	[307.8–308.6]
Parental benefit days 1year before monitoring	5.476	[5.396–5.556]	5.428	[5.314–5.541]
Parental benefit days 1 year after monitoring	4.110	[4.043– 4.176]	4.129	[4.034–4.224]
Sickness benefit days 1 year before monitoring	1.715	[1.670–1.759]	1.691	[1.628–1.755]
Sickness benefit days 1 year after monitoring	1.306	[1.268–1.344]	1.295	[1.241–1.349]

Note: The difference between the treatment and the comparison group is statistically significant on 5 per cent level is denoted with *. We have in addition tested for differences across counties. No statistically significant regional difference was found.

4 Analysis

Using ordinary least squares we estimate the following linear regression model

$$y_{i(t+k)} = \alpha_k + \beta_k D_{it} + \mu_t + \varepsilon_{i(t+k)}, k = -52, \dots, 52 \quad (1)$$

Here $y_{i(t+k)}$ is the number of benefit days for parent i in week t+k, α_k are intercepts, D is a dummy variable: it is zero but takes value one if the parent i was assigned to treatment in week t, and $\varepsilon_{i(t+k)}$ is the error term. β_k, $k = -52, \dots, 52$, are the estimated difference in the paid benefit days between the treatment and the comparison group. The model is estimated separately for benefit take-up each week from 52 weeks to 1 week before the monitoring took place and from 1 week to 52 weeks after the monitoring took place, in total for 104 weeks. The first 52 coefficients, $k< 0$ should not differ from zero if the quasi experiment is well designed. The following 52 coefficients, $k > 0$, should be negative if individuals are deterred from using the insurance by the new information of the probability of being monitored. Fixed effects for the treatment week, μ_t, are added to correct for the large seasonal and yearly variation in the benefit take-up rates and variation in the intensity of monitoring. However, dropping fixed effects for the treatment week does not affect the estimation results.

The question is how we should interpret β_k, $k>0$? There are two concerns with the given quasi experimental design.

The parent is not contacted if no errors or signs of misuse are detected; it is possible that many of the monitored parents do not actually know that they have been monitored.[11] According to the SSIA's database errors or discrepancies are detected in 7 per cent of the monitored applications. The parents can however also find out about the monitoring from their employer or from the day care or school, but it is reasonable to assume that by far all parents know that they have been monitored. As the behavioral effect only emerges if the parents know about monitoring the estimated effect will most likely be biased downwards. If we assume that only 10 per cent of the monitored parents were aware of the monitoring, the real effect would be ten times higher than the estimated effect.

The second concern is that also the controls may be affected by the (potential) monitoring of the treated. Monitored parents can tell other

[11] A second, highly related, problem is that we do not know how careful the caseworker at the SSIA has monitored the application. The caseworker should register whether the application is monitored but this registration is often missing.

parents, their partners for example, about the monitoring and this might affect the benefit claims of the comparison group.[12] If such spill-over effects exist, the estimated effect is biased downwards.

4.1 Results

Figure 2 shows the estimated differences in benefit days between the treatment and the comparison group each week from 52 weeks before the treatment assignment to 52 weeks after the treatment assignment. The estimated difference between the treatment and the comparison group is zero and statistically insignificant before the monitoring assignment takes place. After monitoring the benefit take-up decreases in the treatment group compared to the comparison group. The figure shows that the monitoring effect appears after two weeks, which is expected since the actual monitoring takes place when the parent applies for the benefit, not when the parent is selected for monitoring.[13] Monitoring decreases the benefit take-up by roughly 2 per cent during the first two months. The figure shows that the effect of monitoring decreases over time and the effect on benefit days is statistically significant up to roughly four months after the monitoring.[14]

In order to summarize the results as well as to study effect heterogeneity we also estimate yearly effects using a difference in difference framework. Here the outcome is total temporary parental benefits one year before and one year after being assigned to treatment or control a given week. In the estimation we also add control variables. The yearly estimates are given in table 2.

From this table we can see that monitoring decreases the use of the benefit by about 0.13 gross benefit days, which corresponds to approximately 1.4 per cent decrease in the benefit days. Similar results are also achieved when measuring the benefit take-up as net benefit days. The results are not sensitive to excluding parents with extremely few or many benefit days before the monitoring. Adding controls for parent’s individual characteristics or excluding the fixed effects for the monitoring week does not alter the results (see row one and two in Table 2).

Effects for different subgroups are estimated with less precision. There are no differences between men and women. We can however see that parents with lower education react stronger to monitoring than parents with a higher level of education.

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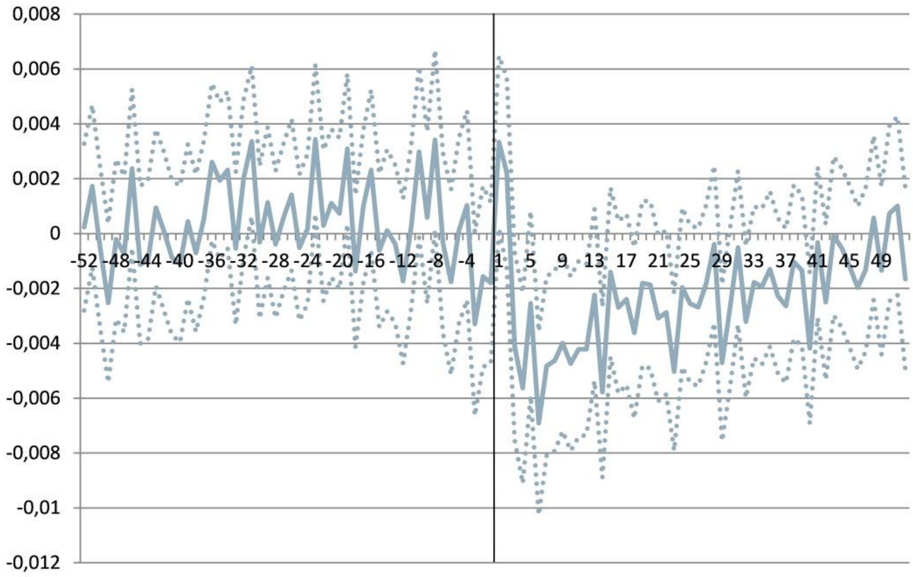
[12] This means that the treatment also affect the non-treated parents. This implies that the no interference assumption in the stable unit treatment value assumption, SUTVA, is not valid (see Rubin 1978). Empirical evidence suggests that such spill-over effects exist when individuals are exposed to treatment in the sickness insurance (see e.g. Hesselius et al. 2013).

[13] The monitoring assignment is done on the first day of absence when the parent notifies the SSIA. The benefit application can be sent in later. Between October 2010 and December 2013 the parents applied for the benefit on average 17 days after reporting the child’s illness to the SSIA.

[14] It seems thus, as the monitoring does not have a permanent effect on the behavior of the monitored parents. However, one should note that our design does not allow us to identify weather individuals preferences are stable. One reason is that also the controls are affected by the monitoring by e.g. being colleague, relative or a neighbor. More importantly, 25 per cent of our selected controls are selected for monitoring in our follow up period.

Figure 2. The effect of monitoring on weekly gross benefit days



Note: The solid line shows the estimated weekly difference in gross benefit days between the treatment and the comparison group. Randomization to the comparison group has been done on a weekly basis. The dotted lines show the estimated 95 per cent confidence interval. Standard errors are robust against heteroskedasticity and clustered on the parent. The vertical line shows the timing of the treatment assignment.

Table 2. The deterrence effect one year after assignment

	Effect of monitoring (gross benefit days)	*Effect of monitoring (per cent)*	*Number of parents in the treatment group*	*Number of parents in the comparison group*
All parents	−0,127*** (0,027)	−1,4***	313,908	632,817
All, controls for parent's background	−0,126*** (0,026)	−1,2***	311,931	628,776
Mothers	−0,131*** (0,036)	−1,3***	200,066	403,561
Fathers	−0,121*** (0,038)	−1,6***	113,842	229,256
Basic education	−0,481*** (0,156)	−4,3***	17,534	35,750
Upper-secondary education	−0,173*** (0,043)	−1,8***	140,548	283,962
Post-secondary education	−0,043 (0,034)	−0,5	155,272	311,967

Note: The effect is estimated with differences-in-differences model. Benefit days are measured during one year before and after the treatment assignment. Robust standard errors in parentheses. The model controlling for the parent's background include gender, polynomial of income, age, age of the youngest child, number of children, education level, marital status, born abroad and county. *** p<0.01, ** p<0.05, * p<0.1.

The effect is not very large on the individual level, but it is still of large economic importance. The government's total expenditure on temporary parental benefit in 2013 was about SEK 5.78 billion (EUR 0.66 billion). About 101,000 applications were selected for monitoring in 2013 and each benefit day is on average worth SEK 910. If we assume that monitoring decreases the paid benefit days by 1.4 per cent, the expenditure on temporary parental benefit was about SEK 12 million (EUR 1.3 million) lower than without the monitoring.[15] A rough estimate is that the monitoring system costs between SEK 3 (EUR 0.33) and 9 (EUR 1) million per year, depending on whether only labour costs or the total administrative costs for the SSIA are included.[16] The effect of monitoring on the benefit expenditure exceeds the costs of monitoring. This applies even though the calculations do not take into consideration that the monitoring has a direct effect in itself.

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[15] The average number of days on temporary parental benefits is 9.2 per year and the average daily cost is 910 SEK. Thus, given an effect estimate of -.014, the benefits from 101,000 controls is SEK 11.8 million (= -0.014*9.2*SEK 910*101,000).

[16] The calculation is based on 101,000 monitored applications, which means that we assume that the SSIA monitors all the applications that are selected for monitoring, which is probably an overestimation. The total administrative costs include besides direct labour costs even general overhead costs for the SSIA (IT, office rent etc.).

5 Conclusion

Monitoring and screening have been shown to be empirically important in reducing the (ex post) moral hazard in social insurance programs. It is easy to show theoretically that ex ante or deterrence effects of the monitoring could also be important in reducing the take up rate of the programs. The empirical support of ex ante or, when it comes to monitoring, deterrence effect are however, basically non existing. In the tax evasion literature Kleven et al. (2011) have shown that the deterrence effect on self reported income on audits the year before could be substantial. This results support, in general, the results from "threat of audit" letters in the same literature (see Coleman (1996), Slemerod et al (2001) and Hasseldine et al (2007) and Kleven et al. (2011)).

Our result supports the results in Engström et al. (2007) who sent out "threat of monitoring letters" in the Swedish temporary family insurance. Their results showed that the parents who received the "threat of monitoring" decreased their use of the benefit by 13 per cent. Our results show that individuals respond on information of (increased) screening. The advantage with our study is that we use the normal SSIA monitoring routine. This routine has, furthermore, been stable over a longer period. One result from our study is that individuals on average update the risk of being detected of misusing the insurance. This means that there exists an equilibrium effect from monitoring. Our intent-to-treat estimates show on average a 1.4 per cent decrease in the benefit days one year after assignment to monitoring. Given that only around 7 per cent of the parents are directly contacted (they could however also receive information on the monitoring from employers or day care) the estimate in Engström et al. (2007) is not unreasonable high.

The consequences of misusing the parental leave system are very mild and the degree of monitoring is quite low. In addition, our intent-to-treat estimate is most likely biased toward zero. Even so, we found an economic significant effect of monitoring on later take up rates. This means that there exists deterrence effects that are larger and more important in programs with larger sanctions and in programs with a higher degree of monitoring.

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