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Excess use of Temporary Parental Benefit

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Patrik Hesselius
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Excess use of Temporary Parental Benefit[1]

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Summary

In this report we examine the excess use of Temporary Parental Benefit for parents who need to stay home from work when their children are sick. This study is based on a randomized experiment that took place during the spring 2006. The method used is rather new and more ambitious than those used in similar studies in the past. One advantage with this more elaborate technique is that a larger part of the veiled excessive use can be discovered. The result points to that as much as 22.5 percent of the costs for this social insurance are due to excess use. There are significant gender differences; women's excess use amounts to 19 percent of their total use while the corresponding figure for men is 28 percent.

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1 Introduction

Of the Swedish public social security systems – the Social Insurance Agency, the unemployment insurance and the local social service – together expends up to 500 billion SEK yearly in social allowances and benefits.[1] This is equivalent to one sixth of the national GDP. The importance to secure that the right benefit goes to the right individual at the right point in time is therefore considerable. This report accounts for the result from a study that aims to measure possible excess use of one of Sweden's benefit system, i.e. the Temporary Parental Benefit.

To measure the excess use of this benefit, we have used a fairly innovative technique, *the information method*. Instead of the usual randomized monitoring of benefit use, certain individuals have been informed in advance that they are subjects to increased monitoring. The individuals that have been informed about this inspection are randomly chosen from the population that is entitled to this benefit. Since these individuals receive information about their (potential) future withdrawal being exposed to monitoring, their average withdrawals are expected to decrease. In other words, we expect that those who were using the benefit excessively now adjust their patterns of withdrawal, at least temporarily, to be in line with what the rules prescribes. Since the information concerning increased inspections was not sent out to everyone entitled to the benefit, one could expect a difference in the withdrawal pattern, between the group who got and those who did not get the information. This difference can be treated as an indirect measure of excess use. One important condition is of course that those who use the benefit excessively see "the threat" of increased monitoring as credible.

Actual monitoring of the withdrawals has been carried out during the period, just as the letter informed. The results of the actual monitoring provides an indirect measure of the efficiency of the information letter; if everyone who received the letter discontinues their potential excess use of the benefit, no one will get caught when monitored. Jointly, the change in withdrawal patterns (the information effect) and the result from the monitoring in itself can be used to obtain a full measure of general excess use of the benefit.

[1] $1 ≈ 7 SEK.

The Temporary Parental Benefit constitutes, for a number of reasons, a suitable choice for this type of study. One reason is that this benefit has been explored in earlier studies, in terms of excess use. Therefore, it is possible to compare the results from this study with earlier results in order to compare the different methods. In addition, the formal rules tied to this particular benefit are relatively uncomplicated and easy to grasp; the main condition is of course that the child in question has to be home sick and the caretaker has to be home from work. In case one or both of those conditions fail, it would be hard to claim unawareness of the rules tied to the benefit. Our evaluation is therefore that in almost all cases of excess use, the person involved knows that he or she is breaking the rules. And this constitutes a most central quality in order for the results, stemming from the information method, to be easily interpretable.

This project was commissioned by the Government appointed delegation against incorrect payments in the social security system (the FUT-Delegation). It has been carried out in collaboration with IFAU and the Swedish Social Insurance Agency (Försäkringskassan).

1.1 Briefly on the Temporary Parental Benefit and the limits of the study.

The Temporary parental benefit contains three separate benefits; income insurance when a parent or caretaker has to stay at home from work in order to look after a sick child (we denote this with its Swedish abbreviation VAB from now on), a number of days in connection to child birth (paternity leave), and "contact days", when parents can visit their children's school or participate in parental education. The VAB-Benefit gives a parent right to economic compensation when he or she has to take time from work to take care of a child, that has not yet reached an age of 12, who is sick or infectious, or if the regular caretaker of the child is sick or infectious.[2] A parent is also eligible to compensation if he or she has to take time off from work to go to the doctor or child care centre with the child. VAB-benefits will not be paid when a parent receives sick pay, sick insurance benefit, is on vacation, is doing military service or is in custody in the correctional system.

[2] Parents can also draw VAB-Benefit for children who are above 12 and below the age of 16 if the child needs special care or supervision.

VAB-benefit can be drawn for 60 days for each child and year. After these have been taken in use, economic compensation can be drawn for another 60 days, though not due to the ordinary caretaker's illness (RFV, 2001).

The cost of Temporary Parental Benefits was 3.8 billion SEK in 2005. The VAB-benefit made out the absolute major part of this amount, 84 percent. The paternity leave benefit accounted for 16 percent of the payments and the "contact days" for 0.1 percent. The compensation level for the VAB-Benefit is at present 80 percent of the benefit determining income. The part of the income (wage income) that is over 10 price base amount (the "cap") is not compensated.[3] The compensation is paid in relation to the time of work the parent has to give up, whole days or some parts - ¾, ½, ¼ or 1/8 day. Counted in whole days, VAB-Benefits accounted for 5 million days in 2005. The average compensation was 741 SEK a day.

It is within the VAB part of the Temporary Parental Benefit that the risk of excess use is the highest, as the withdrawals that account for the other parts are more restricted. For children under 12 years no certificate from a doctor is required before the 8th day, while for children over the age of 12 it is requested from the 1st day. The potential excess use can therefore be expected to be concentrated among caretakers with children below the age of 12. A natural focus for study is therefore to investigate the excess use of VAB concerning children from 1-11 years old. From here on it is only this part of the benefit that will be assessed and we will simply denote this constrained part of the benefit by VAB.

1.2 The purpose

The purpose of this study is to evaluate and work out a more precise measure of the excess use of the Temporary Parental Benefit, VAB. The two essential measurements we will focus on are the total fraction of incorrect payments stemming from excess use of the benefit and the closely related measure of total share of all benefited net days stemming from excess use of the benefit. We will also calculate an aggregated measure on how much the total excess use of the VAB-Benefit amounts to in SEK.

It is further of great importance to discern how the excess use varies between different groups and regions. One objective of the study is to identify

[3] The cap in sickness- and parental insurance was raised on the 1st of July 2006 from 7.5 price base amount to 10. The cap/ceiling for 2006 is 397 000 SEK.

different types of individuals that, to a larger extent than others, use the benefit excessively. This aims at mapping out certain risk groups where intensified monitoring could, for efficiency reasons, be needed in the future.

1.3 Briefly about the results

The study shows that the excess use of the benefit is considerably more substantial than earlier evidence suggests. In the Social Insurance Agency's earlier investigations, the excess use of the benefit has been estimated to 6-10 percent of the total payments (and approximately the same figures in terms of number of days on benefit). The corresponding figure in our study amounts to 22.5 percent. When translated into SEK this estimate implies that more than 650 million SEK per year could be attributed to excess use.[4]

There are two prime explanations for why we find a higher figure in this study compared to earlier findings. Firstly, the method we use differ from previous studies – we see clear evidence that the information method discovers a larger part of the excess use than studies based on randomized inspections do.

As inspections can never be a hundred percent certain – there are always ways to use the benefit excessively that are impossible, for all practical purposes, to discover – estimates based only on random monitoring thus has an intrinsic tendency to underestimate the true excess use. The information method then offers a potential alternative. Since this study is based on the individuals' own (measurable) response, we are able to appreciate a larger part of the excess use and in this way get a more liable estimate. Secondly, the inspections carried out in this study are more ambitious than was the case in earlier studies. An important factor in the inspections has been to check whether the child in question had been present at their daycare center or school at the time their parent or caretaker used the benefit. This margin has, due to limited resources and other practical problems, not been thoroughly monitored in earlier studies. One more important aspect, that distinguishes this study's monitoring from earlier ones, lies in the timing; a shorter time has passed between the use of VAB and the actual monitoring in this study, than has been

[4] The total cost of the benefit amounted to 3.3 billion SEK 2005. However, this study only concerns the VAB-benefits attributed to children between 1-11 years old. Data from the Social Insurance Agency for the year 2004 shows that this group accounts for about 90 percent of the total VAB-days.

the case in earlier ones. Since the monitoring comes closer in time, it is also reasonable that the quality of the monitoring will be higher.

We found that men use the VAB-Benefit excessively to a larger extent than women: 28 percent compared to 19 percent. Men thus account for 50 percent more excess use than women given a total withdrawn amount; in other words, the risk that a given VAB-Krona could constitute excess use is 50 percent higher if it is handed out to a man instead of a woman. However, since the larger part of the VAB-Payments goes to women, the gender difference is mitigated when we instead examine the total excess use of the benefit in SEK. We find that practically the same total amount of excess use can be attributed to women as to men.

The study finds that the information method is fruitful for the study of excess use with regards to this specific benefit as well as other similar public beneficiary systems. For example, we find no signs indicating that the receivers of the information letters have reacted with excessive caution in their withdrawal of the benefit. However, far from every one that received the letter seem to have taken its content seriously. A little more than half of the estimated total excess use stems from the individuals' reactions to the letter. The rest is given by the actual monitoring, i.e. the fraction of payments that fail despite the received letter.

2 Context and concepts

In 2005 the government decided to take joint measures to secure the legitimacy of the welfare state's safety nets. This led to an appointment of a delegation with the task to investigate incorrect payments from the social security system (the FUT-delegation). One of the main tasks for this delegation is to function as a joint association for measures against incorrect use of the public social security systems.

The scope of the FUT-delegation covers all personal benefits and insurances in the public social security system. During 2005 the figures of such beneficiary arrangements reached an amount of nearly 500 billion SEK. The different kinds of benefits vary significantly – only the benefits administrated by the Swedish Social Insurance Agency amounts to 47 different categories. In addition to this there is the unemployment insurance system, the means tested social allowance, study allowances, etc.

The overall assignment for the delegation is to measure the extent of incorrect payments from the social security systems. Reliable estimates of this have not earlier been made on an aggregated level. Since the different kinds of benefit systems vary substantially, the incorrect payments have to be evaluated individually, before the extent of incorrectness could be calculated and aggregated to an overall measure. It is also reasonable to assume that the share of incorrect transfers varies in the different systems depending on a number of factors, such as the design of regulations, monitoring possibilities, etc.

2.1 Briefly about the concepts[5]

In this study we will use the concept *excess use* of benefit. This term covers all withdrawals, caused by the recipient, that are not in line with the intentions of the insurance. This covers what in daily language is called "cheating" and also what in legal terms goes under the label "suspected fraud". However, the wider concept of "incorrect transfers" (or payments) is not covered, as they also contain errors based on pure mistakes and blunders made by administration staff.[6] One limitation however, is that it is not possible in any way to question if a caretakers evaluation of the child's health status is correct or not. If a caretaker decides to stay home from work to take care of a sick child and withdraws benefit from the temporary parental system, this withdrawal is automatically seen as correct. Accordingly, this study does not concern excess use in this somewhat subjective dimension. What the study aims to capture is rather the deliberate excess use of the benefit that that does not relate to the health status of the child.

Throughout, the study's focus is on the individual caretaker and his/hers withdrawals from the VAB-Benefit – it is not the household units as a whole that constitute the subjects in this study. In Section 7.3, however, we have performed a sensitivity analysis where the spouse or joint caretaker to the chosen caretaker is studied. Information about the children is only used for the purpose of defining the population eligible for the benefit.

[5] For a more elaborate discussion of concepts relating to excess use we refer to the publication: *Från risk till mål och resultat* (FUT-Delegationen, 2005).
[6] Unintended mistakes do not lead to excess use in case withdrawals have been made incorrectly for day X instead of correctly for day Y, while the total amount is still correct.

3 Previous studies

3.1 On the VAB-Benefit

The National Social Insurance Agency performs continuous monitoring of the use of their different public insurances and benefits. During the past few years some systematic evaluations have been executed concerning whether the utilization of the social security schemes is consistent with its respective intentions. One of the most examined social security category is the VAB-Benefit. Below follows a brief summary on the results from the three most recent evaluations.

During 2002 the Swedish Social Insurance Agency performed random monitoring of withdrawals of VAB-Benefit during the month of June.[7] The total number of withdrawals during this month amounted to 171 298 and from those a randomized selection of 3 517 withdrawals was made. In the initial inspection of 3 370 withdrawals, 9.9 percent failed. The inspections mostly consisted of confirming the individual's absence from work and, when possible, the child's absence from pre-school and school.[8] In a deepened study of the initially failed cases it was shown that a number of those could be considered as correct.[9] After the deepened study only 6.4 percent of the withdrawals remained incorrect.

In the subsequent study the Swedish Social Insurance Agency performed, during the summer of 2004, a directed monitoring of VAB-Benefit receivers.[10] Parents with more than 10 cases during 2003 constituted the target for further investigation and of those it was 12 900 who had at least one case during the summer of 2004. From those a random selection of 940 individuals was made for closer monitoring. The monitoring consisted mainly of checking whether the parent had been present at work when claiming benefit. The results from this study showed that 39 percent of the parents had, on at least one occasion,

[7] See RFV (2002) and (2003) for a more detailed description of the study.
[8] The answering frequency from pre-schools and schools was very low in this study. It was shown to be very difficult, during the period of the investigation in July to September, to get information about the child's attendance during the month of June.
[9] This was mainly because the employers changed information plus a small share of incorrect information of dates from the recipients.
[10] See RFV (2004a) for a more detailed presentation of the study.

given incorrect information that lead to overcompensation. The incorrect payments accounted for 8.5 percent of the days compensated.

The Social Insurance Agency executed one additional investigation on the VAB-Benefit during the autumn of 2005. Individuals were divided into different groups contingent on their prior withdrawal pattern in order to examine whether the distribution of the incorrect payments differed depending on their historic utilization of the benefit. In total, 934 parents were selected by stratification from the three groups, and among these, 931 individuals were actually examined. The monitoring consisted of verification with employers and unemployment benefit funds. The share of parents whom had given incorrect information, in the group of parents accounting for 1-5 cases of withdrawals during the previous year, was 16 percent; the corresponding share in the group of parents accounting for 5-10 cases was about 29 percent; and the group of parents with more than 10 withdrawals a fraction of 41 percent had at some occasion given incorrect information. The total number of cases examined corresponded to 13 700 days of VAB-Benefit, from which 870 days failed when monitored. In the three different groups the distribution of incorrectly compensated days was fairly even: 6.9 percent incorrect compensations in the group with the least amount of cases, 6.1 percent in the middle group and 6.4 percent in the group with the highest amount of cases. The distribution was somewhat more uneven in terms of share of compensation in SEK. The corresponding figures were 6.8 percent fail rate of the payments to the low-consumption group and 5.3 percent fail rate for the group with more than 10 cases of VAB-Benefit. In 71 percent of the incorrect cases, more than one whole day of compensation had been paid incorrectly. The most usual cause for error, according to the investigation, was that a parent had been at work during the period corresponding with when he/she had applied for VAB-Benefit. No controls were made towards schools or pre-schools.

3.2 Previous studies where information letters have been used

We have only found a few examples of studies where methods similar to the one used here have been applied. These are briefly described below. We may also note that we have not found any earlier study evaluating the benefits of the method compared to alternative methods (in our case traditional random moni-

toring). To our knowledge this study is the first to examine the pros and cons of the method in both theory and practice.[11]

3.2.1 Tax evasion, Minnesota

An extensive experiment was carried out in 1995 by the local tax authority in Minnesota to test different strategies to improve voluntary compliance with state income tax. One of the tested strategies was to send out information letters notifying increased monitoring of tax returns, during the time before the tax returns should be handed to the authorities. This information letter was sent to one group which was to be representative of the population as a whole, as well as to one other group that was expected to be a risk group regarding tax evasion (tax payers with business or farm income). In total about 47 000 taxpayers were randomly selected to participate in this experiment; from those 1 724 individuals received an information letter. The letter informed the tax payer that he/she had been randomly selected for a study where the number of examinations of income tax returns would be increased. The information letter further declared that the income tax returns for both the state- as well as the federal tax would be subject to examination, and that if there was any misconducts discovered, further information was to be claimed paired with a more profound inspection. In that case, a more thorough investigation of earlier year's tax returns was a potential outcome.

The measurement used to estimate the tax payers´ willingness to pay taxes consisted of changes in declared income and changes in paid taxes between the years 1993 and 1994. These changes were compared with changes within the group which did not receive an information letter.

Low- and average income earners who received the letter about increased monitoring, reported higher incomes and paid more tax than what was the case in the representative control group. The increase was larger for those tax payers who had farm or business income. No statistically significant effects were discovered for high income earners. (Coleman, 1996)

Slemrod et al (2001) interprets the induced increase in declared taxes as a potential measure of tax evasion. This is an intuitive interpretation and it is indeed the key identification strategy used in the information method.

[11] For the theoretical description of the method, see Section 0. A more elaborate scientific description of the method can be found in Engström and Hesselius (2007).

3.2.2 Tax evasion, Great Britain

In Great Britain the tax authority carried out an experiment with information letters to entrepreneurs with a turnover of less than £ 15 000 in 2002. Five different letters were sent with varying messages. These different messages were:

i) "We can provide you with advice and support"
ii) "Paying the right tax increases public spending on hospitals, schools, etc."
iii) A warning: "We are increasing inquiries – your return may be chosen."
iv) As in letter 4 – but adding that "we charge financial penalties"
v) "Your 2001 Return has already been selected for audit".

Some of the entrepreneurs in the selected group did not receive any information letter, and thereby constituted the control group. Compared to the control group the entrepreneurs who received information letters, with the message (iii), (iv) and (v), significantly increased net surplus. The share of tax returns was examined in the group (iii) and (iv) as well as the control group. The result from this examination showed that the paid tax became more accurate when it was preceded by an information letter. (OECD, 2004 and Hasseldine et al, 2007).

3.2.3 The "Paternity Leave Letter"

In 2003 the Swedish Social Insurance Agency sent out a circular to about 300 000 fathers reminding them about their remaining paternity leave days. These letters were directed to fathers of children less than 7 years old, who had used less than 40 percent of their parental benefit and had at least 25 days remaining. The control group consisted of fathers who received the letter six months later. The group of fathers who received the letter six months earlier made use of 1.5 more parental benefit days on average compared to the control group. This corresponds to 25 percent of the days that fathers in the treatment group withdrew in total during this period. The "paternity leave letter" thus affected the share of parental benefit days withdrawn by men (RFV 2004).

3.2.4 Pregnant smokers

In a study carried out by Sexton & Hebel (1984) mothers' smoking habits influence on the fetus was examined. A circular, on the hazards of smoking while pregnant, was sent to the half of 900 pregnant women in an early state of their pregnancy. The share of smokers measured in the 8th month of pregnancy

was significantly lower in the group receiving the letter. On average, the birth weight was 92 g higher and the length 0.5 cm longer for the babies whose mothers received the letter. Through a follow up questionnaire it was found that those who received the letter smoked on average 6.4 cigarettes less a day compared to the control group. This study thus showed evidence for clear effects of information, as well as the hazards of smoking during pregnancy.

4 Method and implementation

To estimate the average excess use of the VAB-Benefit, an extensive randomized experiment has been carried out. The authors have been responsible for the design of the experiment and the analysis of the results. The Social Insurance Agency has been responsible for the practical execution of the experiment, such as sending out letters and information brochures as well as the actual monitoring.

The experiment can roughly be described as follows. First, a randomized selection of the eligible individuals was selected to be included in a treatment group. These individuals were informed through circulars notifying them that they were included in a randomized group whose use of the VAB-Benefit was to be exposed to closer monitoring for a period of time. The individuals in the treatment group were not informed whether they actually were to be inspected or how substantial the risk for this was.

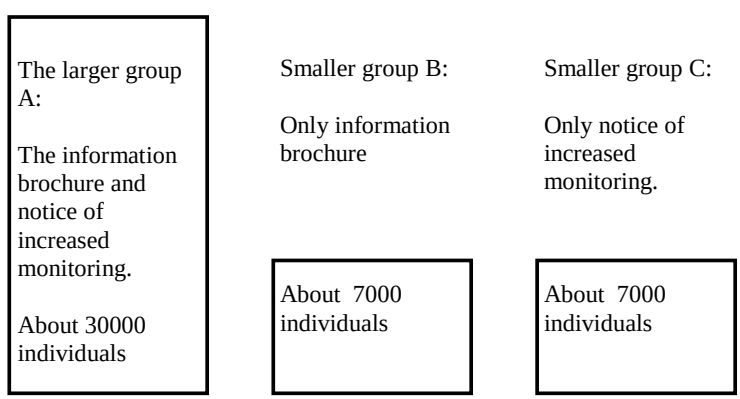

Figure 1. The three treatment groups

To get as much information as possible from the experiment, we separated the treatment group into three distinct groups, two small (B and C) and one large (A). The larger group received a letter informing the receiver that she had been selected randomly into a group which would be exposed to intensified monitoring along with a small pamphlet explaining the rules of the benefit.[12]

In the two smaller groups we varied the treatment according to the following (see Figure 1): (i) group B received only the brief information pamphlet and no added information of them being targeted for intensified monitoring; (ii) group C received only notice that they are subject to increased monitoring, and no accompanying pamphlet. This design aims at capturing: (i) the effect of the information of the rules in itself, and (ii) the effect of knowledge about intensified monitoring while the individual acts on her own idea about the set of rules regarding the benefit.

The treatment groups most central for the estimate of excess use are group A and group C (denoted AC), since those individuals have received information about increased monitoring and therefore can be expected to adjust their behavior in line with what the rules of the benefit prescribe. It is uncertain what effect may be expected from only receiving the informative brochure. Among those who were not actively aware of the benefit's existence, one would expect an increase in withdrawals. On the other hand, some individuals may perceive the pamphlet as an indirect notice of increased monitoring, in which case we would expect the same kind of effects as in group A and C.

The remaining population we denote D. It consists of the nearly 1.3 million individuals entitled to the benefit, but who are not part of any treatment group. This group (D) constitutes what in similar cases usually is called "control group". A control group represents a sort of reference not undergoing the treatment the researcher wants to measure the effects from (similar to placebo groups in medical experiments). To avoid misunderstandings, however, we will avoid the term "control group" in this study since it could be close at hand to interpret this as individuals who *will* be monitored. Instead we will use the term "remaining population" and "untreated population" as synonymous references to group D.

[12] See Appendix 1 for the design of the letter and appendix 2 for the information brochure.

4.1 Circulars

Letters and information brochures to the selected caretakers in the different treatment groups were sent out on the 24th of March. The majority of those reached its receivers four days later on the 28th. The period of the experiment started on the 29th of March and carried on until the 31st of May. The individuals who received the letter were informed about the increased monitoring procedure and that they had been selected by random into a group which was targeted for intensified monitoring; see Appendix 1 for the exact design of the letter.

The Social Insurance Agency also established a temporary call-center and a special e-mail box to where those who received the letters (group A and C) could turn with questions. To those individuals who only received the brochure describing the set of rules of the benefit (group B), an information sheet was also attached. In this sheet it was told that the Social Insurance Agency wanted to inform parents about the regulations associated with the VAB-Benefit (see Appendix 2).

4.2 Implementation of monitoring

From those who were selected to the three treatment groups, a certain share (about 30 percent) had their withdrawals from the VAB-Benefit examined. Each regional Social Insurance Agency was responsible for examining a number of cases. The cases were brought to the local agencies in three separate waves. One reason for this sequential process was that the administrators should not be overloaded with work all at once. But the main reason was to minimize the elapsed period between withdrawal and monitoring.

The chosen individuals' withdrawals were closely examined through contacts with employers and pre-schools/schools, and also crosschecked against the unemployment benefits etc. The absence during the compensated time was checked with the employer along with the grounds for absence (care for sick child, vacation, compensatory leave, work allocation, or else). Through the contact with school/pre-schools the child's absence status was checked for the period when compensation had been withdrawn, as well as the reason for the child's absence.

4.3 Description of the method

To understand the information method we picture a universal benefit with some possibility for excessive use. Let X percent of all compensated days stem from

excess use. The aim is to identify the size of X with highest possible precision. The randomization makes the selected treatment group representative for the entire population (those who are eligible for the benefit in question). The situation is described in Figure 2.

Since the individuals in the treatment group have been informed about the intensified monitoring of their withdrawals, they may be assumed to adjust their use in accordance with the systems' regulations, during the experimental period. In a best case scenario, individuals who earlier used the benefit excessively – and only those individuals – will lower their use and now only make legitimate withdrawals of the benefit. In this way we could get an approximate estimate of the true level of excess use by comparing the average level of withdrawals in the treated group with the rest of the population (see Figure 3).

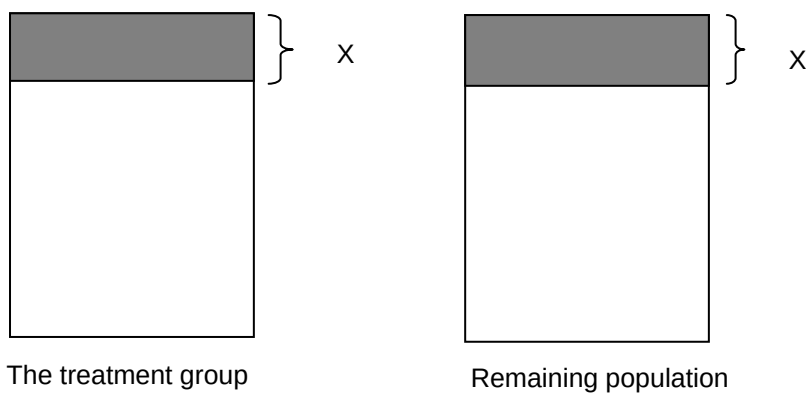

Figure 2. The number of days compensated by individual before the information on intensified inspections.

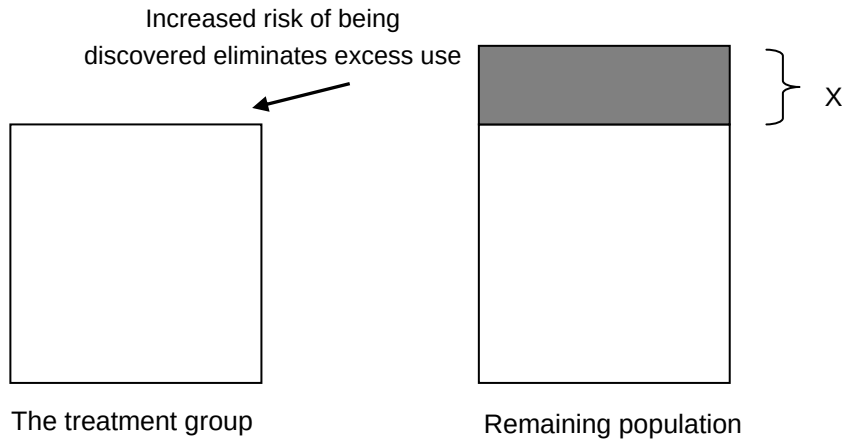

Figure 3. The ideal case - the whole amount of excess use may be identified.

The scenario described above is of course utopian. In practice there are a number of possible reactions to the letter and some of those may complicate the interpretation of differences in withdrawals in the treatment group compared to the rest of the population. We will revisit such issues in the following section.

It is of great importance to point out that the strategy described above is not based on that individuals finally will be caught in the monitoring process. Instead we aim at, through the use of information about increased monitoring, induce individuals to adjust their use of the benefit to be in line with the regulations. Since the selection process of individuals into the treatment groups is random, it is thereafter possible to identify the aggregate level of excess use. It should also be noted that each individual in the treatment group, who fails in the inspections, will be an indicator of to what degree the identification procedure described above will underestimate the true level of excess use; if a treated individual's withdrawal fails in the monitoring process it means that she has not adjusted her use of the benefit in line with it's rules and intensions, despite the fact that she was informed about her exposure to increased monitoring.

4.4 The advantages and disadvantages of the information method in this study

In order to get more tangible we will now turn to the VAB-Benefit explicitly. There are many ways in which this benefit could be used excessively, for example:

(i) *To escape the qualifying period in the individual sick-pay.* The individual sick-pay has a qualifying period of one day at each new sickness spell. After this day the compensation level in the sick-pay and the VAB-Benefit is equal (with some exceptions).[13] The cost for the individual of staying home due to own illness is therefore higher than when she pretends to be home taking care of a sick child.

(ii) *Additional income through the parent being at work while at the same time claiming VAB-Benefit.* When staying at home to take care of a sick child, this is reported directly to the Social Insurance Agency. There is no automatic inspection executed in order to confirm the absence status.

(iii) *Extra income through working in the black market.* The parent usually works in the white sector; when the opportunity to work on the black market occurs, the parent can get both compensation for lost income, by means of the VAB-Benefit, and extra income through temporary work.

(iv) *The child is attendant at school or pre-school while VAB-Benefit is claimed.* The parent may be home from work, due to any reason, while letting the child attend school or pre-school.

(v) *To be home with healthy child.* A parent takes time off work using the VAB-Benefit compensation, while at the same time keeps the child absent from school/pre-school.

To appreciate the advantages of the information method, we will first briefly describe how the usual random monitoring procedure may partially fail in capturing the types of misuse described above.

[13] The compensation level when sick-pay is paid by the employer is based on the salary and not on the temporary disability benefit founded income. A person who's salary is above a certain ceiling in the disability or illness benefit will also receive compensation for the part of the salary that exceeds this ceiling during the sick-pay period (the first 14 days). She thus has a higher compensation during this period than what she would receive from the VAB-Benefit.

4.4.1 Traditional random monitoring

Traditional random monitoring simply consists of random selection of a number of withdrawals that will be closely investigated. The legitimacies of withdrawals, with regards to the above types of excessive use, are possible to examine to a varying degree:

i) *To escape the qualifying period in the individual sick-pay.* Inspection is possible only if the child usually attends school or pre-school and the caretaker does not keep the child at home.

ii) *Additional income through the parent being at work while at the same time claiming VAB-Benefit.* This is theoretically possible to monitor through contacts with the employer. However, the employer may be loyal to the employee, which would make monitoring harder. If inspections are carried out after a long period of time, there is also a risk that the employer no longer keeps the attendance list. While a parent works, the child usually attends school or pre-school, in which case their attendance lists could be checked. However, in practice, the attendance lists are not usually kept for long, so it may not always be possible to obtain the necessary information.

iii) *Extra income through working in the black market.* Monitoring could be possible through the child's presence in school and pre-school. Direct investigations of black market work would be very complicated and costly.

iv) *The child is attendant at school or pre-school while VAB-Benefit is claimed.* Monitoring could be possible through the child's presence in school and pre-school.

v) *To be home with healthy child.* This form of excess use of the benefit is practically impossible to reach through monitoring.

The crucial point is that inspections never, with a realistic budget and authorized directives, can be a hundred percent reliable. Even if an individual's withdrawal does not fail in the monitoring process, we cannot be sure that it indeed was made in accordance with the benefit's rules and intentions. The lesson to learn from this line of reasoning is that the random monitoring procedure has an intrinsic tendency to underestimate the level of excess use of the benefit.

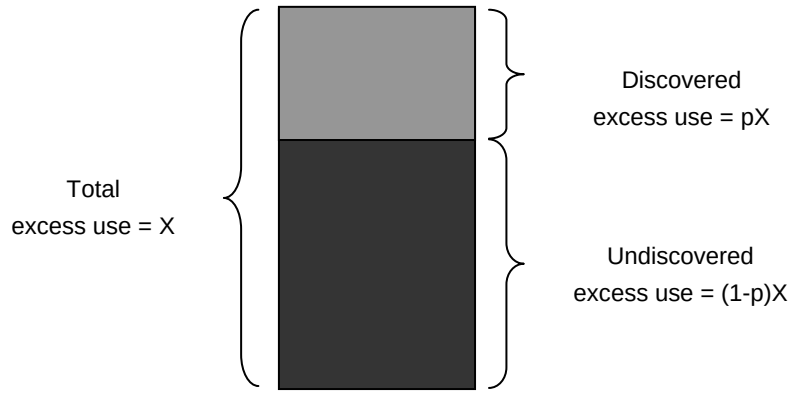

Figure 4. The spot traditional random monitoring strategy underestimates the level of excess use.

Let us say that an incorrect withdrawal of the benefit is discovered with the likelihood of p. The situation can then be described as in Figure 4. Random monitoring will consequently underestimate the total excess use of the VAB-Benefit: the estimate of X will have expected value pX and the expected underestimation will accordingly amount to (1-p)X. At this point it is also important to point out that from our position as investigators, we do not know the likelihood (p) to get caught, given that the individual uses the benefit excessively and is actually monitored; if we knew this it would be straight-forward to scale up the estimate pX with a factor of 1/p and thereby get an accurate expected value estimate of the true level of excess use.

4.4.2 The Information Method

How, then, are we going to get past these problems by using the information method, in combination with random monitoring, to estimate X? As earlier noticed, the identification of X is not primarily based on the actual share of withdrawals that fails when monitored; instead, the individuals own response to the information letter is crucial. So what does this response consist of? We can imagine a number of scenarios:

i) An individual who earlier has been negligent in her declarations on the exact dates will now be more careful.

ii) An individual who has consciously used the benefit excessively stops doing so from fear of being caught and being reported to the police and inspected retroactively.
iii) The individual did not use the benefit excessively before, and consequently does not do so after receiving the letter either – there will be no effect on use at all.
iv) The individual did not use the benefit excessively before, but now she becomes anxious and does not take the full compensation that she is entitled to.
v) The individual does not even open the letter; alternatively, she does not take the warning of increased monitoring seriously, and therefore continues to use the benefit excessively. In this category we also include the strategy to switch from one form of excess use to another form that is harder to detect through monitoring; for example, the individual could switch from staying home from work due to own illness, while the child attends school or pre-school, to the hardly detectable "staying home with a healthy child".
vi) The individual has not been actively aware of the existence of the benefit and therefore initially under-utilizes the benefit. Information about the set of rules – also in the case information about increased monitoring was attached (group A) – can then lead to new found knowledge about the VAB-Benefit and thus increased use.

In the ideal case the individual responds in line with scenario i), ii) or iii). This represents the result described in association with Figure 3 above. If the individuals react only in accordance to these three alternatives our estimate of X, in terms of expected value, coincides with the true value of X, the average excess use of the benefit. It should also be noted that, in this case, none of the individuals exposed to inspection, will "get caught" in the monitoring process.

It is, however, possible that certain individuals respond in accordance with scenario iv), v) or vi) above. Such responses cause some problems in estimating the value of X – reactions corresponding with iv) induce an overestimation of X while responses in line with scenario v) or vi) tend to underestimate X. It is therefore of great importance to avoid such reactions, as far as practically possible. Below, we describe how these aspects have been taken into consideration in the design of the experiment.

Scenario iv) *The individual did not use the benefit excessively before, but now she becomes anxious and does not take the full compensation that she is entitled to.*

This type of response is very serious, as it could lead to the information method overestimating the average excess use of the VAB-Benefit. Below follows a number of strategies that we have used to minimize the risk of inducing this type of response.

- The tone in the letter is deliberately "kind"; those who know that they have not been using the benefit incorrectly should not have to worry about the intensified monitoring.[14] When it comes to this particular benefit, the absolute majority of excess use is probably deliberate, i.e. you know when you use the benefit correctly and when you do not. This contrasts with e.g. unemployment insurance or sickness insurance where the definition of excess use is considerably fuzzier.
- In the letter it is clearly declared that the selection of individuals chosen for intensified monitoring was completely random; they have not been selected due to any wrongdoing in the past.
- The letter was accompanied with a comprehensible pamphlet explaining the, relatively simple, rules of the benefit (group A).
- A call center was set up by the Social Insurance Agency to which worried benefit recipients could turn with questions about the letter.

To appreciate the extent of the problem we have also studied how the withdrawals of the other caretaker were influenced when the partner received the letter. If the child truly was considered sick, i.e. the use was correct, the withdrawals of the other caretaker would be a likely substitute to own withdrawals – which would not be the case in most cases of excessive use of the benefit (for example concerning the caretakers own illness, presence at work and working in the black sector. An over-reaction to the letter is then likely to lead to increased withdrawals of the partner. If we find such a reaction we have an indication of certain individuals indeed reacting according to scenario iv).

One further strategy, that due to time limitations has not been included in this study, would be to complement the information letters with interviews

[14] See appendix 1 for the exact design of the letter

and/or questionnaires to gather information on the individuals' thoughts concerning the letter.

Scenario v): The letter is ignored or not taken seriously.
This reaction provides a potential underestimation of X, i.e. it represents an opposite problem to the one described above. Note firstly that if we consider the extreme case, when no one opens the letter, we would have a situation identical to the random monitoring procedure. The full measure of excess use would be based on the monitoring and our only loss is that we sent out all these letters in vain. The worst that could happen from this scenario is thus that we do not get anything additional out of using the information method. But for every individual we do influence to react in line with scenario ii), our estimation of X becomes better. It is therefore of great importance to reduce the response according to scenario v) as far as possible. Below follows the strategies utilized for this purpose:

- It is clearly stated in the letter that the inspections concerns *future* withdrawals between date a and b. If the individual believes that the inspection already have started he or she may react as if the "damage already has been done", and thus fail to stop excessively using the benefit.
- Some key phrases in the letters are forceful. Those underline distinctively that that it will be specifically Your withdrawals that will undergo intensified monitoring, and that the present letter was not sent out to everyone.
- The letter also points to the fact that this monitoring is special and out of the ordinary. This served the purpose of bringing further credibility to the risk of getting monitored.

In this context we would like to stress that, on a long-term basis, the main strategy to avoid reactions in accordance with scenario v) is to invest in good monitoring quality. If the intensified monitoring, that was referred to in the letter, fails to appear, or if the actual inspections are too indulgent, the usage of the method in subsequent studies could be undermined.

Scenario vi): The individual "discovers" the benefit as a result of the information letter.

This reaction is very tricky to prevent. But, the design of the experiment, with the arrangement into groups A, B and C, aims to bring an indication of the extent of the problem. In group B we try to refine the effect of receiving only the information about the benefit, while group C only receives the letter declaring intensified monitoring without the additional pamphlet. However, it is principally impossible to inform about increased monitoring of a benefit without at the same time informing about the very existence of the benefit. Even so, it seems reasonable to presume that a reaction in accordance with scenario vi) would be most distinct in group B and least distinct in group C, which could give us an approximate picture of how extensive our problems linked with this type of reaction are.

5 Data

5.1 Selection

Our population is defined by those who are eligible for the benefit – in principle every caretaker, with a few exceptions, with children in the age 1-11 years old.[15] This means that the population approximately consists of all Swedish caretakers of children born between 1st of June 1994 and 20th of March 2005. The selection into treatment groups were then determined by random birthdates of the caretakers. As a consequence of this selection procedure both caretakers in a few families would receive any of the three treatments. To avoid such incidences we could have included only one of the caretakers of a child in a treatment group. This would, however, complicate the selection procedure considerably, and in part it would have been impossible to implement with the present data. There are two reasons for this. First, if the child has had more than two caretakers during her lifetime, we cannot know which of those that are currently registered as caretakers. Second, we do not know whether the two caretakers live together and indeed live with the child. To avoid excluding single parents, and to be able to replicate the selection easily, we let both caretakers be included in the treatment groups in these (relatively few) cases.

[15] The individuals who were unregistered, taken into custody, doing military service, caretakers with protected identity and persons missing from the tsunami-catastrophe were not included in the treatment group or remaining population.

This occurred for 1078 out of 64 284 children whose one or both parents is included in group A, B or C.

As a part of the sensitivity analyses we will, as mentioned above, study cross effects among separate caretakers (see Section 7.3); what happens with the withdrawals made by one of the caretakers when the other caretaker is selected to the treatment group (A, B or C)? This analysis is done on a somewhat limited population, since we want it to be two caretakers in as close relationship as possible – the ideal being the traditional nuclear family. However, we do not have the information on whether the caretakers of the child live together or not. In the cases when a child's caretakers have changed, we do not know who the current caretakers are. For this reason we have defined our somewhat limited population from the observations where the children only have had two caretakers during their lifetime and when those and the child were registered in the same parish and when none of the caretakers have children in the age of 1 to 11 years together with another partner. We also exclude all "families" where both caretakers have had any kind of treatment (A, B or C). Given this selection of population we have a remaining 83.6 percent of the origin, which is close to official numbers from Statistics Sweden for the year 2004 that shows that 78 percent of the children in the age 1 to 11 years old lives with their original parents.[16]

5.2 Descriptive statistics

Table 1 shows the number of individuals, age and gender distribution in the different groups. In total 42 249 individuals were included in some treatment group – from those 28 543 were in group A, 7051 in group B and 6 655 in group C. The remaining population (group D) contains 1 272 993 individuals. It should be noted that there are no statistical significant gender and age differences when comparing the different treatment groups (A, B and C) to the remaining population (D). Since the confidence interval for the share of women, as well as average age, in the treatment groups (Table 1) cover the value of these variables in the remaining population (D), it follows that there are no significant differences in share of women or in age average. Neither are there any significant differences between these variables when comparing the treatment groups in pairs.

[16] It is natural that we get a somewhat higher number than Statistics Sweden, since we include those parents who are separated but still liver in the same parish.

In this study we have used a reference period against which the different groups' withdrawals may be compared (see Figure 5 and Figure 6 below for the withdrawals made by the whole population during the reference period as well as the experiment period). We decided to use the six months that preceded the experiment period, minus a "buffer month" in between the reference period and the actual experiment period. The reason for using a buffer month is that some people linger with their applications for VAB-Benefit for a few weeks. When an individual, in the end of March, receives the letter about intensified monitoring there is thus a possibility that she will reduce earlier withdrawals retroactively. This could have the effect that we underestimate the effect of the letter. Our reference period was thus made out of the 5 months between 05-10-01 and 06-02-28.[17]

If the random selection process has been correct, the expected withdrawal patterns within the different groups are identical. According to the confidence intervals given in Table 2, that presents the average fraction of men and women that made withdrawals, as well as the average number of VAB-Benefit days during the reference period, there were no significant differences between the groups A, B and the remaining population. But for the small group C, the average withdrawal is significantly larger than it was in group D. One explanation for this could be pure chance: we may have got an unusually high average need of the benefit by the caretakers in group C. When considering a 95 percent confidence level, pure chance would make us falsely reject the hypothesis that the underlying parameters are equal, in one out of 20 cases.[18] If we instead look at group A and C jointly (the AC group), the significant differences between all remaining groups (AC, B and D) disappear.

[17] An alternative would have been to instead use the period previous year (2005) corresponding to the experiment period (2006). But since the population eligible for the benefit changes over time, we found it more suitable to let the reference period be relatively close in time to the experiment period.

[18] It should be noted that the difference does not disappear when we cut the top of the withdrawal distribution, which means that it is not due to a small number of extreme cases, "outliers", producing the higher number of withdrawals in group C. We have compared the C-group's constitution – concerning a number of variables such as age, gender, profession etc – with the constitutions of the other groups without finding any statistically significant deviations. We therefore conclude that the random selection has been correctly carried out and that the C-group, simply by chance, has been assigned a larger fraction of individuals with a somewhat increased demand for VAB-Benefit.

Table 1. Descriptive statistics for the treatment groups (A, B and C) and the remaining population (D).

Group	Number of individuals	Share of women	Average age
A	28 543	0.516 [0.511; 0.522]	38.26 [38.18; 38.33]
B	7 051	0.512 [0.500; 0.524]	38.24 [38.09; 38.40]
C	6 655	0.514 [0.502; 0.526]	38.26 [38.10; 38.43]
D	1 272 993	0.515 [0.514; 0.516]	38.29 [38.28; 38.30]

Note: 95 percent confidence interval in parenthesis.

Table 2. Withdrawal patterns during the reference period (2005-10-01 until 2006-02-28).

Group	Percent share of women withdrawing VAB-Benefit on an average day	Percent share of men withdrawing VAB-Benefit on an average day	Percent of all individuals withdrawing VAB-Benefit on an average day
A	1.28 [1.23; 1.32]	0.81 [0.77; 0.84]	1.05 [1.02; 1.08]
B	1.31 [1.23; 1.40]	0.76 [0.70; 0.83]	1.04 [0.99; 1.10]
C	1.40 [1.31; 1.50]	0.89 [0.81; 0.96]	1.15 [1.09; 1.21]
D	1.30 [1.29; 1.31]	0.79 [0.78; 0.79]	1.05 [1.05; 1.06]

Note: 95 percent confidence interval in parenthesis.

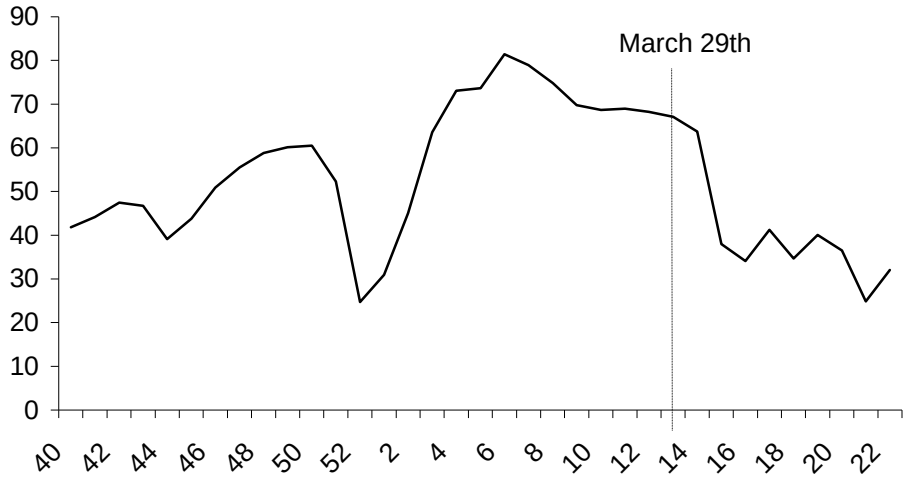

Figure 5. Withdrawal amount of VAB-Benefit in SEK per individual and week, for the whole population (A+B+C+D) with children in the age's 1-11 years old (from the week 40/2005 until the week 22/2006).

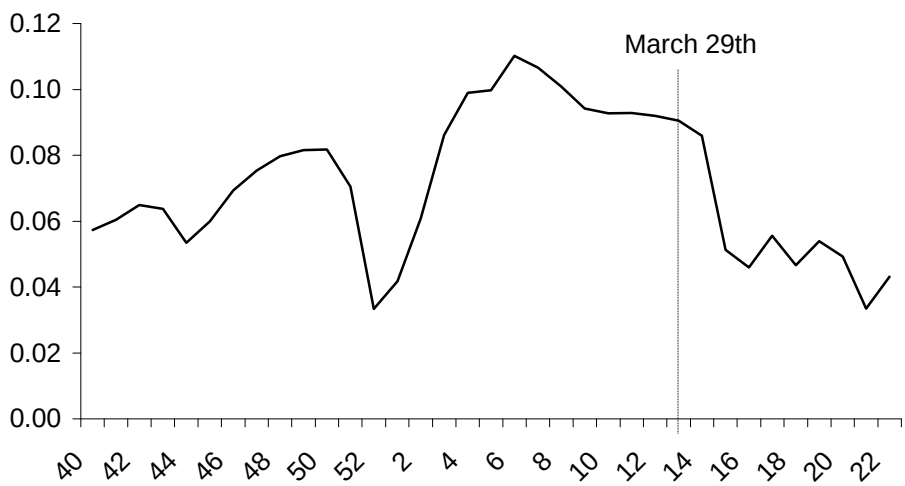

Figure 6. Compensated net days of VAB-Benefit per individual and week for the whole population (A+B+C+D) with children in the age's of 1-11 years old (from week 40/2005 until week22/2006).

6 Statistical method[19]

Our estimate of the total excess use of the VAB-Benefit will be composed of two components: it is partly made out of the potential decrease in withdrawals induced by the letters, and partly of the results of monitoring in itself. The statistical analyses behind the two measures are separate. We will therefore account for the underlying analysis of each component separately. Finally, we will describe the potential caveats of aggregating the two separate measures into a total measure of excess use. We will here only focus on the measure of the relative number of SEK stemming from excess use of the VAB-Benefit. However, the principle is identical when instead considering the measure related to the fraction of VAB-days stemming from excess use.

6.1 The letter effect

When estimating the relative effect of the treatment (the letter) we have used a so called ”difference-in-difference”-estimator (DiD) instead if the simpler “difference”-estimator (D-estimator). Both methods are unbiased in a randomized experiment. But under certain conditions the DiD-estimator is more efficient than the D-estimator. If the individual specific variation is high, efficiency gains could be made through the use of the DiD-estimator. Moreover, if there is an individual specific variation, a D-estimator could be seriously biased, ex post, if the randomization accidentally has given a sample that differs significantly from the population. In our example there is reason to believe that this is the case in the C-group. If the D-estimator is implemented on the C-group we may therefore get a biased result; in our case we measure no significant effect of the letter in the C-group when using the D-estimator. This is due to the average withdrawal level being unusually high in this group. However, for the AC-group as a whole we only get a marginally different result if we use the D- instead of the DiD-estimator. A very brief technical explanation of the DiD-estimator is presented below. The D-estimator is generated, in technical terms, from the DiD-estimator by setting all recipients’ withdrawals during the reference period to 0 (or any other constant). The

[19] The technically unfamiliar/uninterested may preferably skip this section.

difference between the estimators thus lies in that the simple D-estimator is not using any information on earlier withdrawal patterns.

We consider two separate periods of time (top-index *t*), period 0 and period 1, which refer to the reference period (0) and the experiment period (1). Assume that one individual in group $g = (C,T)$ – where *C* specifies the comparison group and *T* specifies the treatment group – has the following withdrawal pattern:

$$U_{gi}^{t} = \alpha^{t} + I_{g} + \delta_{g}^{t}\tau + \varepsilon_{i}^{t},$$

where U_{g}^{t} refers to the withdrawal of the benefit, α^{t} is a time specific component and I_{g} is an individual specific component. The third component, $\delta_{g}^{t}\tau$ refers to the treatment effect; δ_{g}^{t} is an indicator variable taking value of one if $g = T$ and $t = 1$, and zero otherwise, while τ is the size of the treatment effect which consequently is the parameter we want to estimate. At last ε_{i}^{t} refers to an error term (with the expected value of zero) and which variance could potentially vary over time.

Consider now the difference in withdrawals of the benefit, between the treatment period and the reference period, for an individual (*i*) in the treatment group:

$$\Delta U_{Ti} = \left(\alpha^{1} + I_{T} + \tau + \varepsilon_{i}^{1}\right) - \left(\alpha^{0} + I_{T} + \varepsilon_{i}^{0}\right) =$$
$$\left(\alpha^{1} + \varepsilon_{i}^{1}\right) - \left(\alpha^{0} + \varepsilon_{i}^{0}\right) + \tau,$$

and note how the individually specific term cancels out, herein lies the virtue of the DiD-estimator compared to a simple D-estimator. When using the D-estimator the individual effect cancels out only after we apply the expectation operator on the difference between the groups. By using information on earlier withdrawals, we can eliminate the time constant individual effect already on the individual level, which potentially could reduce the variance in our estimate.

Consider now the difference in withdrawals of benefit between treatment period and reference period for an individual (*k*) in the untreated comparison group:

$$\Delta U_{Ck} = \left(\alpha^{1} + I_{C} + \varepsilon_{k}^{1}\right) - \left(\alpha^{0} + I_{C} + \varepsilon_{k}^{0}\right) =$$
$$\left(\alpha^{1} + \varepsilon_{k}^{1}\right) - \left(\alpha^{0} + \varepsilon_{k}^{0}\right).$$

Finally, we obtain the absolute treatment effect by taking the difference of the expected values of the above given differences:

$$E\left(\Delta U_T\right) - E\left(\Delta U_C\right) = \tau\,.$$

Trough estimation of $E\left(\Delta U_T\right)$ and $E\left(\Delta U_C\right)$ from data of withdrawals we can thus obtain an unbiased estimation of τ according to:

$$\hat{\tau} = \hat{E}\left(\Delta U_T\right) - \hat{E}\left(\Delta U_C\right).$$

We proceed, in order to estimate the relative treatment effect ($\hat{\tau}_{rel}$), by dividing the difference in differences with the average use of VAB-Benefit by the comparison group during the experiment period, i.e.:

$$\hat{\tau}_{rel} = \frac{\hat{E}\left(\Delta U_T\right) - \hat{E}\left(\Delta U_C\right)}{\hat{E}\left(U_C^1\right)} = \frac{\hat{\tau}}{\hat{E}\left(U_C^1\right)}.$$

Thus, the estimation contains of a quota between an estimated absolute treatment effect and the estimated total withdrawal of the comparison group during the treatment period. Since the denominator is a stochastic variable there are certain problems associated with the calculation of the variance of $\hat{\tau}_{rel}$.[20] A standard solution to this problem is to use the so called Delta method, which gives an approximately correct variance for $\hat{\tau}_{rel}$.[21] The variance measures we present below are calculated with this approximate method, according to the following expression:

$$\widehat{\mathrm{var}}\left(\hat{\tau}_{rel}\right) = \left(\frac{1}{\hat{E}\left(U_C^1\right)}\right)^2 \widehat{\mathrm{var}}\left(\hat{\tau}\right) + \left(-\frac{\hat{\tau}}{\hat{E}\left(U_C^1\right)^2}\right)^2 \widehat{\mathrm{var}}\left(\hat{E}\left(U_C^1\right)\right)$$

$$+2\,\frac{1}{\hat{E}\left(U_C^1\right)}\left(-\frac{\hat{\tau}}{\hat{E}\left(U_C^1\right)^2}\right)\widehat{\mathrm{cov}}\left(\hat{\tau}, \hat{E}\left(U_C^1\right)\right).$$

Since the withdrawals of the treatment group can be expected to be independent of the withdrawals of the remaining population, there should be no significant co-variance between the measures and the last term could thus be

[20] Note that we see the whole remaining population as independent random draws from an infinite population.

[21] The Delta method is based on the first order of Taylor-approximation and is described in several standard textbooks; see e.g. Greene (2003).

set to zero. It is worth noticing that the variance measure for $\hat{\tau}_{rel}$ does not change much when using the Delta-method, compared to the more naïve approach where $\hat{E}\left(U_C^1\right)$ is treated as constant.

6.2 The monitoring component

The relative share of SEK stemming from incorrect payments is estimated as:

$$\hat{K}_{rel} = \frac{\sum F_i / n}{\sum U_i / n} = \frac{\bar{F}}{\bar{U}},$$

where F_i is incorrectly paid to individual i, U_i is the total amount paid to individual i, and n is the number of inspected individuals.[22] Hence, the estimation contains of a quota between the mean value of the individuals incorrect compensations $\left(\bar{F}\right)$ and the mean value for the paid amount $\left(\bar{U}\right)$. Since the estimation contains of stochastic variables in both the numerator and the denominator we have some problems with the variance estimate for $\hat{K}_{rel}$ as well. The Delta-method gives in this case:

$$\widehat{\text{var}}\left(\hat{K}_{rel}\right) = \left(\frac{1}{\bar{U}}\right)^2 \widehat{\text{var}}\left(\bar{F}\right) + \left(-\frac{\bar{F}}{\bar{U}^2}\right)^2 \widehat{\text{var}}\left(\bar{U}\right) + 2\left(\frac{1}{\bar{U}}\right)\left(-\frac{\bar{F}}{\bar{U}^2}\right)\widehat{\text{cov}}\left(\bar{F},\bar{U}\right).$$

In this case we cannot exclude the co-variance term, since it may possibly be a correlation between incorrect paid amount and total paid amount. Also in this case we should mention that the difference in variance estimates is very small when using the Delta method compared to the more naïve approach in which $\bar{U}$ is treated as a constant.

6.3 Total measure

The effect from the letter described above in Section 6.1 corresponds to a measure on how individuals react to information about intensified monitoring.

[22] When it comes to the monitoring component, all withdrawals refer to the treatment group during the experiment period, why we suppress the sub-index (for group) and top-index (for time) in this sub-section.

Decreased withdrawals (i.e. a negative relative effect) are interpreted as a decrease in excess use. The partial estimate of excess use, the information induced estimate, is thus given by,

$$\hat{\phi}_{rel} = -\hat{\tau}_{rel}$$

The total measure on excessive use is principally the sum of the information estimate ($\hat{\phi}_{rel}$) and the share of payments that get caught through monitoring ($\hat{K}_{rel}$). For minor (a few percent) effects this is quite accurate. But when we consider larger effects such approximation is not satisfactory. One example illustrates the logic: assume that the information effect gives an estimate of excess use of 50 percent (i.e. paid amount decrease by 50 percent due to the letters) and, furthermore, let 50 percent of the remaining paid amount fail in the monitoring process. If we simply sum up these components flat we get a total effect of 100 percent, which obviously cannot be accurate. The true estimate would in this example rather arrive at 75 percent. When we sum up the two percentage rates flat we are summing up apples and pears, as they concern two different reference levels. We therefore have to weight the share of failing payments by using the "letter-effect-adjusted" level of withdrawals before we can sum up these two different measures.

According to this principle our total estimation ($\hat{T}_{rel}$) will be,

$$\hat{T}_{rel} = \hat{\phi}_{rel} + (1 - \hat{\phi}_{rel})\hat{K}_{rel}$$

Here as well we are facing some problems with the calculation of variance measures. The variance for the first term we already have. The other term variance can be obtained using the Delta-method. There is however potential covariance between $\hat{\tau}_{rel}$ and $\hat{K}_{rel}$, as well as between the two main terms, that are difficult to estimate.

The equation above can be rewritten as:

$$\hat{T}_{rel} = 1 - \left(1 - \hat{\phi}_{rel}\right)\left(1 - \hat{K}_{rel}\right).$$

Through variable substitution and the Delta-method we get:

$$\begin{aligned}\widehat{\text{var}}\left(\hat{T}_{rel}\right) &= \widehat{\text{var}}\left(1 - \left(1 - \hat{\phi}_{rel}\right)\left(1 - \hat{K}_{rel}\right)\right) = \widehat{\text{var}}\left(\left(1 - \hat{\phi}_{rel}\right)\left(1 - \hat{K}_{rel}\right)\right) \\ &= \left(1 - \hat{K}_{rel}\right)^2 \widehat{\text{var}}\left(\hat{\phi}_{rel}\right) + \left(1 - \hat{\phi}_{rel}\right)^2 \widehat{\text{var}}\left(\hat{K}_{rel}\right) + \\ &\quad + 2\left(1 - \hat{\phi}_{rel}\right)\left(1 - \hat{K}_{rel}\right)\widehat{\text{cov}}\left(\hat{\phi}_{rel}, \hat{K}_{rel}\right)\end{aligned}$$

In this expression a co-variance remains between $\hat{\phi}_{rel}$ and $\hat{K}_{rel}$ which we are unable to estimate in this case. However, the co-variance ought to be negative; a large letter effect would lead to fewer being caught in the monitoring process. This means that the last is likely to be negative. To estimate the variance assuming independence between the included components therefore leads us to overestimate the variance, which means that we are on the safe side.

7 Results

In this section the results of the study will be presented. We will start with the main results of the estimate of total excess use based on the information effect and the outcome of monitoring. In Section 7.2 the outcome from the monitoring is presented in depth. We will there present the distribution between different reasons why the withdrawals failed. In Section 7.3 we will study the behavior of the other caretaker when the partner is treated. An overreaction to the letter – the caretaker stops using the benefit even though she is fully eligible to it – would tend to increase the withdrawals of the partner since this constitutes a close substitute when using the benefit correctly. In Section 7.4 we present the results separately for a number of subgroups.

7.1 The main result

From the description of the method in Section 0 as well as the statistical method in Section 6 it was made clear that the estimation of the total excess use of the VAB-Benefit will be composed of two main estimates: in part the potential reduction of withdrawals that may follow from the letters, and in part the result from the actual monitoring. The results from both of these components and the aggregate estimate of excess use are presented in Table 3.

The main analysis in the entire result section will concern only the AC-group, i.e. the ones who received the letter with information about intensified monitoring (the AC-group consists of group A and C which constitutes our proper treatment group).

To reach the effect of the letter/information we compare the changed withdrawals in the separate groups during the separate time periods – the experiment period and the reference period. As shown in Figure 7 and 8, the withdrawal pattern during the reference period shows the same shape in the AC-group as in the D-group. As we then entry into the experiment period an

apparent relative decrease in withdrawals occur for the treated group, AC; this is the effect of the letter.[23] A clear sign of the weekly based figures being a bit shaky (high variance) is the substantial temporary increase in withdrawals of the AC-group for the 20th week. However, when we calculate the effect of the letter we use the average for the whole experiment period (the reference period respectively) and this considerably lowers the variance compared to a single week's figures. In the first column of Table 3 the effect is translated into numbers. The calculations show that the letter effect for group AC brings the withdrawals down by about 13 percent (13.2 percent when counted in amount of SEK and 12.9 percent when counted in days). The effect is statistically significant irrespective of confidence level.

Concerning the effect of the information pamphlet, explaining the rules of the VAB-Benefit (the B-group), the results are more uncertain. The point estimates, at 4.4 and 3.3 percents increase respectively, compared to the D-group, indicate that the individuals "discovers" the benefit to some degree. On average we thus find no disciplinary effect as a result of only information about the benefit. However, the increase in withdrawals in the B-group is far from statistically significant on traditional levels. Our conclusion is therefore that the experiment does not give a clear cut answer to the pure effect of information.

In the second column of Table 3 the results from the actual monitoring are presented. The withdrawals were checked through contacts with schools/pre-schools (day care centers) and employer/unemployment benefit funds etc. Even though the individuals in the A and C groups had been informed about the increased monitoring in advance, we find that both groups, in particular the A group, fail to substantial degrees in the monitoring process. In the A-group 11.8 percent of the paid amounts failed and respectively 11.4 percent of the days compensated. For the C-group the corresponding numbers are 6.9 and 7.2 percent respectively. In Section 7.2 the results of monitoring are described in depth.

[23] Please note that what appears to be a decreased number of withdrawals in the treatment group begins already a few weeks before the experiment period. This does not have to be a coincident. In Section 5.2 we presented the need of a buffer-month in between the reference period and the experiment period. The reason for this is that many people wait for a few weeks before sending their claims for the benefit. Thus, we can get a retroactive decrease of withdrawals in the treatment group.

Table 3. The main results of the study: letter effect, results from monitoring and total estimate of overuse.

	Group	Letter effect	Failing rate when monitored	Total estimate of excess use
Amounts	A	-12.9% [-16.6%; -9.3%]	11.8% [9.8%; 13.7%]	23.2% [19.6%; 26.8%]
	B	4.4% [-3.4%; 12.1%]	16% [11.9%; 20.2%]	
	C	-14.1% [-22.2%; -6%]	6.9% [4.4%; 9.4%]	20.0% [12.2%; 27.9%]
	AC	-13.2% [-16.5%; -9.8%]	10.7% [9.1%; 12.3%]	22.5% [19.2%; 25.8%]
Net days	A	-12.8% [-16.5%; -9.1%]	11.4% [9.5%; 13.2%]	22.7% [19.1%; 26.4%]
	B	3.3% [-4.3%; 11%]	15.8% [11.2%; 20.5%]	
	C	-13.5% [-21.6%; -5.4%]	7.2% [4.6%; 9.8%]	19.7% [11.9%; 27.6%]
	AC	-12.9% [-16.3%; -9.5%]	10.5% [8.9%; 12.0%]	22.1% [18.7%; 25.4%]

Note: 95 percent confidence interval in parenthesis.

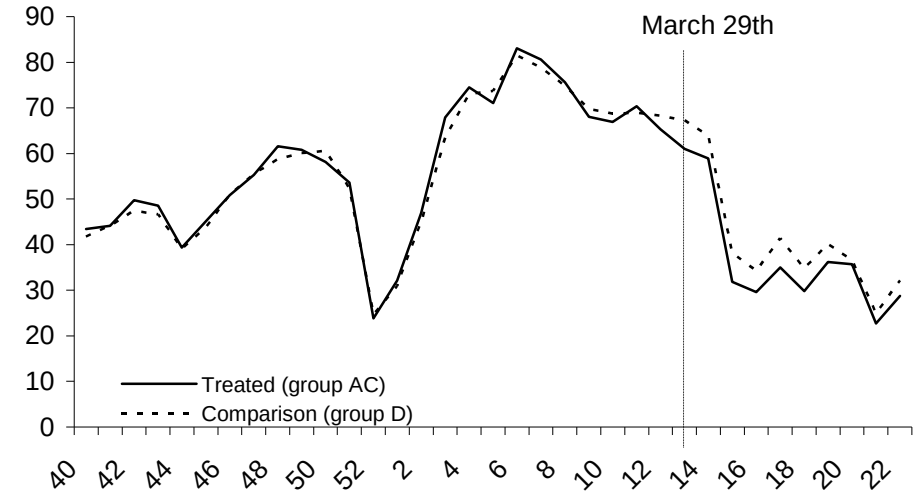

Figure 7. VAB-Benefit withdrawn in SEK per individual and week, for the AC-group and the D-group (from week 40/2005 until week 22/2006).

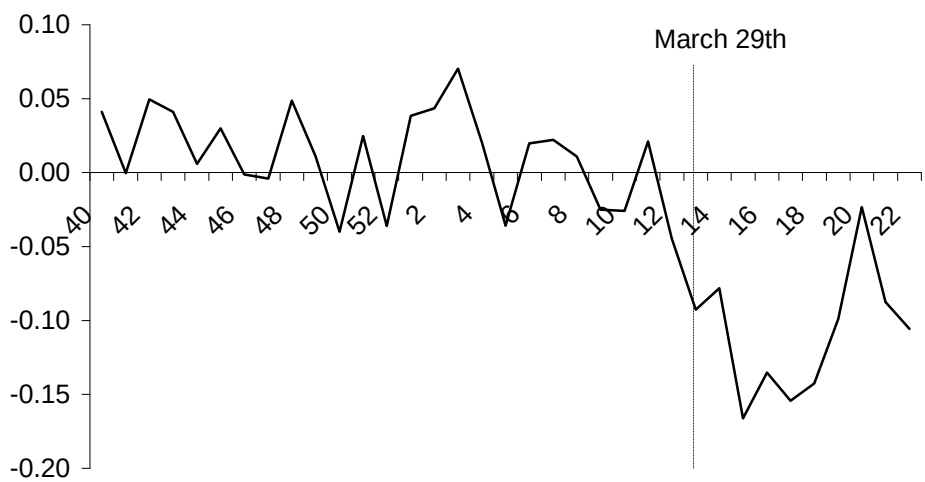

Figure 8. Relative difference between weekly withdrawn amount in the AC group compared to the D-group ((AC-D)/D) (from week 40/2005 until week 22/2006).

The third column of Table 3 shows the total estimates of excess use. These are approximately given by the sum of the estimates in column 2 and 3 in Table 3. However, these figures are not simply addable since they refer to different basic levels of withdrawals. The total estimates of excess use are therefore slightly below the sum of the two figures in column 2 and 3 (see Section 6.3 for a more elaborate discussion).

Regardless of whether we measure in amount or in days our estimation of the share of withdrawals stemming from excess use is more than 22 percent (22.5 percent of paid amount and 22.1 percent of the compensated days). These numbers indicate that more than a fifth of every withdrawn SEK (or day) was unnecessary. Expressed in total SEK these numbers indicate that the Social Insurance Agency, with regards to this single benefit, hands out more than 650 million SEK in one year to recipients not eligible for compensation.[24]

7.2 A closer look at the results of monitoring

In this section we will present the results of monitoring in greater detail. Table 4 below shows the total number of individuals, the share of individuals who made withdrawals from the benefit during the experiment phase and the number of actually monitored individuals in each treatment group. About 17-18 percent of the individuals included in one of the treatment groups made withdrawals from the VAB-Benefit during the experiment period. Among these about 30 percent had their withdrawals monitored. The monitoring was carried out so that, in each round (three rounds in total), a number of random individuals with a positive withdrawal of the benefit during the respective period, were selected. Thereafter, every withdrawal made by this individual during the sub-period was closely monitored. The monitoring consisted of checking against the employer, the child's attendance in school/pre-school and checking for "double payment" from any other benefit system such as unemployment benefits etc. The results from the monitoring are presented in Table 5 and 6 below.

From Table 5 it is shown that monitoring, at least in some dimension, has been possible in more than 99 percent of the cases selected. As in earlier

[24] The whole amount of the benefit turnover was 3.3 billion SEK in the year 2005. However, this study only concerns the benefit payments made for children in the ages of 1-11 years old. Data from the Swedish Social Insurance Agency for the year 2004 shows that this group accounts for more than 90 percent of all withdrawn benefit days.

monitoring studies, carried out by the Social Insurance Agency on the VAB-Benefit, the cases where information from the employer has not been attainable is regarded as missing values even if information is attainable from e.g. the child's school. By the same logic the observation is not regarded as missing if an answer is possible to get from the employer, even if information regarding some other monitoring margin is missing. Among the observations that are not regarded as missing, an answer from the school/pre-school has been attainable in 87 percent of the cases (see Table 6).

In Table 7 we present the particular reasons for failure. The most frequent reason – 5.9 (AC) and 8.4 (B) percent respectively in the monitored cases – was that the child in question had been present in school/pre-school during the period when the parent claimed VAB-Benefit. The second most frequent reason – 5.9 (AC) and 7.7 (B) percent respectively in the monitored cases – was that the parent actually was at work or was salaried during the days or hours when she claimed VAB-Benefit. Other causes for failure were that the parent was not supposed to be at work during the period, received sick pay (or were facing a qualifying day), or in a few cases, was on vacation or received unemployment benefits. Examples of "other causes" for failure are that an inaccurate yearly work time was reported or the income determining compensation level was incorrect, that the parent received rehabilitation assistance or no longer was employed by the stated employer.

Note that in Table 3 above, the results are presented on the basis of paid amount and compensated days, while we in this section present the results in terms of whole VAB-Benefit spells. A spell often consists of a number of compensated days. For a spell to fail, the whole amount withdrawn need not be incorrect; if, for example, an individual has used the benefit during 4 consecutive days, and the monitoring shows that she only was absent from work for 3 days, it is only the amount withdrawn the incorrect day that is categorized as excess use (Table 3), while the whole 4-day spell is characterized as incorrect in Table 7.

Table 4. The number of individuals in the three treatment groups, the number with compensation during the experiment period and the number of monitored individuals.

Group	Number of individuals	Share using VAB-Benefit during the experiment period	Numbers of monitored individuals
A	28 543	17.1	1228
B	7 051	18.4	322
C	6 655	18.2	335
Total	42 249	17.5	1885
AC	35 198	17.3	1563

Table 5. The results of the monitoring.

Was the VAB-Benefit spell correct?	Number of spells	Percent	Percent of cases with responses
No response	15	0.6%	
Yes	1986	84.8%	85.3%
No	342	14.6%	14.7%
Total	2343		

Table 6. Was it possible to receive answers from school/pre-school?

Response	Number of spells	Percent
Response possible	2037	86.9%
Responses not possible	189	8.1%
No information	117	5.0%

Table 7. Spells that failed when monitored: group AC and B.

Reason for failure	Number of spells in group AC	Percent of total spells in group AC	Number of spells in group B	Percent of total spells in group B
The parent was absent from work, while the child was present at school/pre-school	115	5.9	34	8.4
The parent was at work/received salary during the period of the VAB-Benefit spell	115	5.9	31	7.7
The parent was not supposed to be working	8	0.4	4	1.0
The parent received sick-pay	7	0.4	1	0.3
The parent received unemployment benefit	2	0.1	-	-
The parent was on vacation	-	-	1	0.3
Other reasons	19	1.0	5	1.2
Total numbers of failed spells	266	13.7	76	18.8
Total numbers of monitored spells	1938		405	

7.3 Did the individuals who received the letters overreact?

One risk to be concerned with when informing about increased monitoring is that the individuals exposed to the information will overreact, i.e. that individuals who did not use the benefit excessively decrease their correct use of the benefit. As described in Section 0 a number of measures have been taken in this experiment in order to reduce the risk of such overreaction. But there is still no way to be entirely sure that there is no substantial overreaction, why this subsection is devoted to gathering evidence on the seriousness of the problem in this particular study.

The schools and pre-schools do not usually admit sick children, due to the risk of infection as well as the increased workload. Since the sickness status of the child is an often binding constraint, a parent who overreacts to the letter must often find a substitute caretaker. An obvious candidate is the partner, since the letter is very clear on that it is only "Your" individual use of the VAB-Benefit that will be subject to increased monitoring. Even if grandparents who are no longer working could step in, the partner must in many cases be the most obvious choice. Since we have data on the partners, we can examine whether an overreaction is at hand by studying the implicit reaction of the partner of the ones exposed to the treatment (in particular group A and C). Since the effect on the partner is likely to be larger if the two caretakers actually live together we try to limit the sample to the traditional nuclear family. How this was done was described in the data section (Section 5.1).

The figures below (Figure 9 and 10) illustrate the qualitative results. The group of people that is indirectly treated, i.e. whose partner was in the AC group, is denoted the DAC group. Furthermore we denote the new remaining population as the DD group. Figure 9 shows that the difference between the withdrawals in the DD and the DAC group is very modest.

In Table 8 the graphic analysis has been transferred into numbers. Note first that the effect of the letter among the "families" is not particularly different from the treatment effect in the full population. In the AC group we estimate an effect of the letter at -13.7 percent, which should be compared to an effect of -13.2 percent in the full population. The effect of only sending out an informative brochure (group B), is positive in the "family"- population as well, and somewhat higher compared with the total population. The effect in group B, however, is still not statistically different from zero.

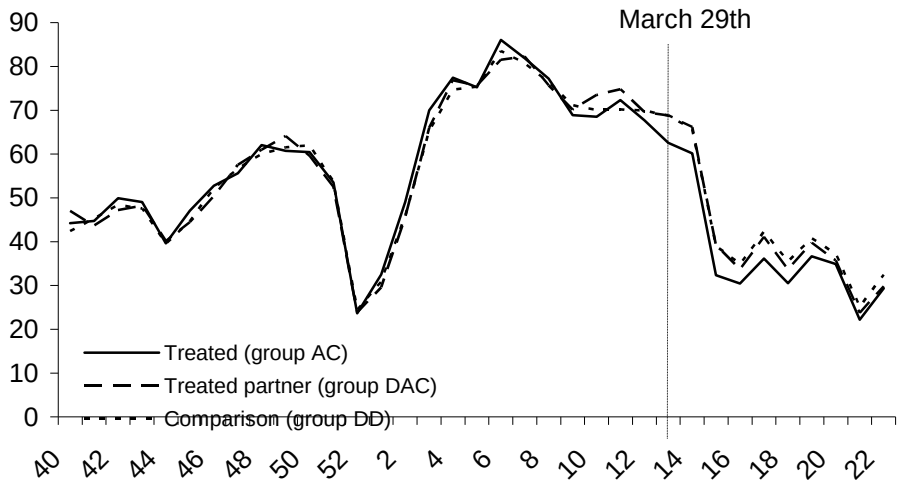

Figure 9. Withdrawal amount of VAB-Benefit in SEK per individual and week (from week 40/2005 until week 22/2006), divided into the AC group, the DAC group and the DD group.

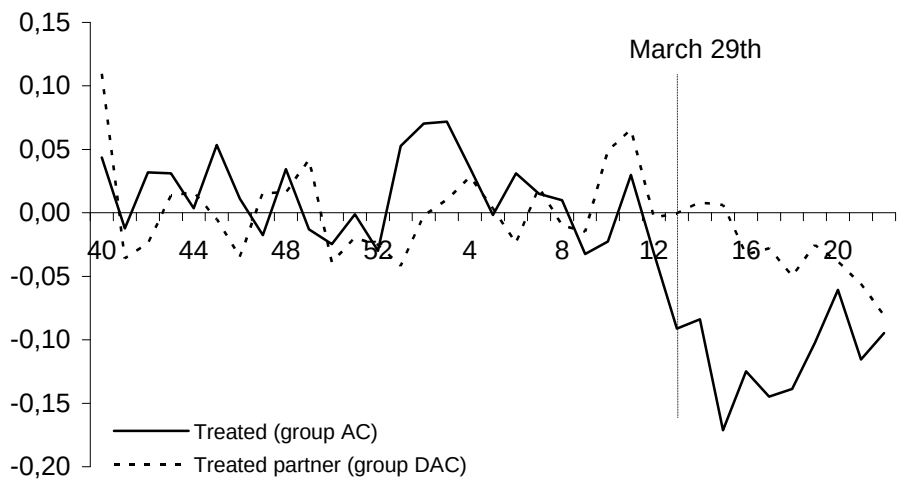

Figure 10. Relative difference between weekly withdrawn amount in the AC group, and the DAC group, compared to the DD-group ((AC-DD)/DD and (DAC-DD)/DD) (from week 40/2005 until week 22/2006).

Table 8. The letter effect on the receiver and its partner.

Group	Effect of letter	Effect on partner from letter
A	-13.5% [-17.6%; -9.5%]	-2.6% [-6.7%; 1.4%]
B	6.6% [-2.0%; 15.2%]	2.6% [-5.9%; 11.2%]
C	-14.1% [-23.4%; -4.8%]	-2.5% [-11.9%; 7.0%]
AC	-13.7% [-17.4%; -9.9%]	-2.6% [-6.4%; 1.1%]

Note: 95 percent confidence interval in parenthesis.

If we instead examine how the partner reacts to the letter, we find no sign of substitution effect. The measured partner effect of the letter in group A and C are decreased withdrawals by about 2.5 percent, while the partner effect in the B group is an increase of 2.6 percent. However, these results are highly insignificant.

Even if the major part of the potential overreacting families would use another substitute than the partner (friend, relatives, parents or letting the sick child attend school/pre-school), we would at least expect a minor increase in the partners use of the benefit. However, if there are two counteracting effects, one substitution and one disciplinary (if some interpret the letter as the whole family being monitored) we can get a net result that does not show any effect in aggregate. It is principally possible that what we measure is the sum of a positive effect (when you let the partner take care of the sick child) and a negative contamination effect (the disciplinary effect contaminates the partner). However, two counteracting effects tend to increase the variance of the variable in question. A comparison of the distribution of the differences in paid amount between the AC group and the D group does not show such pattern. All this taken into consideration, we feel safe to conclude that, in this particular study, there is not a substantial problem with overreaction in response to the letters.

7.4 The effects on different subgroups

An important purpose of this study is to separate different subgroups that are particularly prone to excess use of the VAB-Benefit. To this purpose we here present the results for a number of different subgroups.

There are two reasons why the results in this section should be interpreted with caution. Firstly, the statistical certainty of an estimate decreases generally as the group we are studying gets smaller. Secondly, the results cannot be interpreted as direct causal effects. A simple example illustrates the logic. Assume that we find individuals living in region X particularly prone to excess use of the benefit. In this kind of partial analysis we will not answer the, more ambitious, question about what quality of the inhabitants in region X that causes this behavior. It can possibly depend on a number of factors such as age structure, distribution of incomes, educational level or the labor market in the region. Thus, we cannot conclude that an individual, given the other characteristics (gender, age, income etc.) can be expected to be more inclined to excess use, just because she lives in region X.

We confine our presentation to cover only groups based on gender, sector, educational level, region, age and income. We find only very small differences between excess use measured in compensated days and amount. For simplicity we will therefore limit the presentation to only the results measured in withdrawn amount. Furthermore, only the results of the main treatment group, AC, will be presented.

The results in this section can be summarized with two main findings. Firstly, we find significant differences between the sexes; men engage in excess use to a higher extent than women. This should be interpreted in the sense that a given amount of benefit paid out to a man is likelier to stem from excess use, than if it was paid out to a woman. Secondly, we find that excess use decreases with education level. The estimates for each category are presented in Table 9.

7.4.1 Gender

Excess use of the benefit by men is estimated to 28 percent of the amount withdrawn by men, while the same measure for women is about 19 percent. The difference is statistically significant. As also shown in the table, practically the whole difference stems from men reacting stronger to the letter; the effect of the letter is about twice as big for men than for women.

Table 9. Subgroup results.

	Effect of letter	Share failing when monitored	Total estimate of excess use
Gender			
Men	-18.9% [2.9%]	11.3% [1.5%]	28.0% [2.8%]
Women	-9.5% [2.1%]	10.3% [1.0%]	18.9% [2.1%]
Sector			
Private	-15.2% [2.2%]	11.1% [1.0%]	24.6% [2.2%]
Public	-11.0% [2.8%]	9.1% [1.5%]	19.2% [2.8%]
Education level			
Compulsory school	-27.9% [6.5%]	18.7% [3.8%]	41.3% [6.0%]
High-school	-18.7% [2.3%]	9.8% [1.0%]	26.7% [2.3%]
Post high-school	-1.4% [2.7%]	9.9% [1.3%]	11.2% [2.8%]
Region			
Metropolitan area	-12.4% [2.7%]	14.4% [1.3%]	25.0% [2.6%]
Major population centre	-14.1% [2.9%]	7.4% [1.1%]	20.5% [2.8%]
Rural area	-12.9% [3.4%]	8.5% [1.9%]	20.2% [3.5%]
Age			
- 33	-13.3% [3.0%]	9.2% [1.3%]	21.2% [2.9%]
34 - 37	-11.5% [3.2%]	11.7% [1.7%]	21.9% [3.2%]
38 - 41	-18.4% [3.6%]	10.7% [1.8%]	27.1% [3.5%]
42 -	-8.7% [4.2%]	11.7% [1.8%]	19.4% [4.0%]
Income group			
Men below median	-16.7% [4.0%]	10.6% [1.8%]	25.5% [3.9%]
Men above median	-21.0% [4.2%]	11.9% [2.2%]	30.4% [4.1%]
Women below median	-12.2% [3.1%]	9.0% [1.4%]	20.1% [3.1%]
Women above median	-7.5% [2.8%]	11.2% [1.3%]	17.9% [2.8%]

Note: Standard errors in parenthesis.

7.4.2 Sector

When studying how excess use differs between individuals in private and public sector, no statistically significant results are found. The tendency, based on the point estimates, is that individuals in the private sector have a higher level of excess use than people in the public sector: 24.6 percent compared to 19.2 percent. Most of the difference stems from the effect of the letter. The sectoral affiliation is determined by the sector in which the individual draw the main income during 2004.

7.4.3 Level of education

When dividing the population into different educational categories we find that the excess use decreases with level of education. In the group with only nine-year compulsory school the excess use is estimated to 41.3 percent, while the corresponding number for the group with at most high-school education is 26.7 percent. However, the group with the lowest education level is relatively small, both in numbers of individuals as well as in share of withdrawals. When turning to the excess use of the group with the highest education, post high-school, we find a substantial decrease in excess use. The excess use is estimated to 11.2 percent in the group with the highest education. All three estimates are significantly different from each other.

7.4.4 Region

When it comes to regional differences, the pattern is more ambiguous. We find no significant differences between the three different regional types: big cities[25], major population centers[26] and others. The point estimates indicate that excess use of the benefit is highest in the big cities and lowest in major population centers, but there are no statistically significant differences.

[25] Refers to Stockholm/Södertälje A-region, Göteborg A-region and Malmö/Lund/Trelleborg A-region – see Statistics Sweden (SCB) (2003).

[26] Refers to the Statistics Sweden classification of rural districts in H-regions. Medium-sized cities are rural districts with more than 90 000 inhabitant within 30 kilometre radius from the city's centre (See SCB, 2003).

7.4.5 Age

The variation in excess use over different age categories is not clear cut. There is a tendency to a reversed U-shape, i.e. lower excess use among the youngest and oldest and a bit higher in the middle. The heterogeneity in the estimates stems from the effect of the letter. The highest response, 18.4 percent is obtained in the age groups of 38-41 years old, and the smallest response, 8.7 percent is found in the age group of 42 years and older. However, there are no significant differences in the total estimates of excess use.

7.4.6 Income

Since income represents a variable with considerable gender differences, we found it appropriate to do separate income analysis. We thus classify women and men separately as high- and low income earners. Men with an income over the average are classified as high income earners and the remaining men as low income earners. Women are classified correspondingly. The income year used is the taxed earnings in 2004.

The reactions to the letter is relative disparate. The strongest reaction, estimated to 21.0 percent, is obtained from men with high income. Women with high incomes show the weakest reaction to the letter; they decrease their withdrawals by 7.5 percent due to the letter.

The tendency for the total estimates of excess use is that men who are high income earners have a higher estimate of excess use than low income men. The inverted pattern is obtained for women; women with high incomes have a lower estimate of excess use than low income women have. However, no comparisons within the separate sexes are statistically significant. The tendencies regarding incomes should therefore be interpreted with great caution.

8 Summary

To measure the excess use of the VAB-Benefit (a benefit for staying home from work with a sick child), we have here used a relatively new method, the information method. As a complement to the traditional random monitoring approach, some individuals are notified in advance that they are to be exposed to intensified monitoring. Those individuals were randomly chosen from the population eligible for the benefit. When an individual receive notice that his or her future (potential) withdrawal of the benefit will be subject to intensified monitoring, we expect this person to decrease his or her potential excess use of the benefit. In essence, the potential decrease in benefit withdrawal, caused by the letter, is through aggregation interpreted as excess use. Moreover, the actual monitoring is executed, as promised in the letter, which adds an additional component to the estimate of total excess use.

The study shows that the excess use of the VAB-Benefit is far more substantial than earlier studies have shown. In earlier studies of this benefit, carried out by the Social Insurance Agency, it was found that about 6-10 percent of the total withdrawn amount could be attributed to excess use. We estimate that the excess use amounts to 22.5 percent of the total withdrawn amount. This point estimate implies that more than 650 million SEK yearly are unnecessarily paid out through this benefit alone.

We also find that men engage in excess use of the benefit to a larger extent than women do; the estimates are 28 percent for men and 19 percent for women. According to these estimates the risk of a given VAB-Benefit krona to stem from excess use is about 50 percent higher if it is withdrawn by a man than by a woman. Expressed as share of the total VAB-Benefit payments this gender difference is, however, neutralized, due to the fact that women use the benefit to a larger extent than men do. We find that approximately the same share of the total VAB-Benefit kronor, stemming from excess use, are withdrawn by men and women.

Since monitoring can never be a hundred percent reliable – there are always ways to engage in excess use of a benefit that is impossible, for all practical purposes, to discover – estimates based on random monitoring have an intrinsic tendency to underestimate the excess use. The information method offers a potential alternative. Since this method is partly also based on the individuals

own response to knowledge of increased monitoring, we may capture a larger part of the general excess use of the benefit. This study has therefore arrived at substantially larger estimates of excess use than earlier studies have. A further reason why this study arrives at higher figures than before is that, for the first time, monitoring through contacts with the children's schools/pre-schools has been executed extensively; according to the rules of the benefit, the child (obviously) has to be home from school/pre-school when the caretaker uses the benefit. In earlier studies of the benefit, this margin has not been monitored extensively. We find that many withdrawals fail due to this particular margin, i.e. many withdrawals have been made when the child actually was attending school/pre-school. One additional important aspect, that distinguishes the monitoring process in this study from earlier studies, is that there has elapsed a considerably shorter time between the withdrawal and the actual monitoring; it is very reasonable to assume that the quality of the monitoring increases as this time-span decreases.

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Appendix 1: The letter to treatment group A and C

Monitoring of temporary parental benefit

You have been selected for special monitoring concerning temporary parental benefit. The monitoring concerns claims and compensations for temporary parental benefit during the period of the 29th of March to the 30th of May 2006. The Swedish Social Insurance Agency is going to carry out inspections by e.g. contacting employers, day care and schools.

The fact that you have been selected has nothing to do with earlier withdrawals of temporary parental benefit. You may not necessarily have applied for temporary parental benefit in the past. We have used a method of random selection. The reason for You being selected is thus not that you are suspected of cheating. It is equally probable for all having children in the ages of 1-11 to be selected for intensified monitoring.

The increased inspection represents a part of a bigger work to counteract cheating and offences against public compensations.

If you have questions on this, please feel free to contact us. Please use our e-mailbox: kontroll.tfp@forsakringskassan.se. If you prefer to talk to us, the phone number is 020 – 45 00 56.

If you wish to learn more about the Swedish Social Insurance Agency and temporary parental benefit, you may visit our web site, www.forsakringskassan.se.

The Swedish Social Insurance Agency
Försäkringskassan

Appendix 2: Information letter to the treatment group B

Information to you who have children in the age of 1 to 11

As a way to secure that the correct compensation is paid to the right individual for the right occasion, the social insurance sends out information on care for sick child compensation (VAB). The reason that you receive this information is that you have children (child) in these ages.

In the attached leaflet you can read about the rules you have to fulfill to being eligible for compensation.

The leaflet gives information on the VAB benefit, but should not be regarded as words of an act. If you wish to learn more you may get into contact with the Social Insurance Agency or visit our web site, www.forsakringskassan.se.

Appendix 3: The information on the regulations sent out to Group A and B

Temporary parental benefit, care for sick child

The right to care for sick child compensation

You, who have children in the ages below 12, are eligible to the VAB-Benefit. Besides biological parents, also adoptive parents are affected by this, as well as:

- A person living together with the parent
- A person who by admission of the social welfare board takes care of a child for the purpose of adopting it.
- A juridical caretaker who, without being a parent for the child have custody of the same.
- A person who takes care of a child permanently in his or her home (foster parent).
- Individuals living together and are or have been married or have had joint children.

Under what circumstances are you eligible to VAB-Benefit?

VAB-Benefit may be paid out to the parent who has to be absent from work to take care of a sick child in the home for any of the following reasons:

- The child is ill or infectious.
- The regular caretaker is ill or infectious. The regular caretaker is the person who usually takes care of the child when you are working, such as a parent working in the home, the other parent, child minder or relative.
- The other parent has to visit the doctor with another child of the family. The condition is that the child is under the age of 12 and in some cases under the age of 16.
- To visit the child welfare centre or the public preventive medicine care, such as the dentist or children's psychiatry and youth welfare.

VAB-Benefit can also be paid when a parent to a sick child or a child with a functional disorder visits an institution, such as rehabilitation, special school or alike or attend a course arranged by hospital staff. If care allowance has been paid for the child, VAB-Benefit would not be paid for the same treatment and supervision.

Certificate for the child

If the child is sick/ill infectious for more than 7 days a certificate from the doctor or nurse will be needed from the 8^{th} day. The first day is counted from the first day compensated for care taking. Also the days when you don't receive benefit will be counted to the first unattested 7 days. The calculation of the period when no certification is needed would not be affected by the parents for instance replacing each other in the care taking of the child.

For the regular caretaker

If the regular caretaker is ill this would be confirmed through a certificate or stated opinion from a doctor from the 8th day.

Number of compensation days

The parents can be compensated during maximum 60 days per child and year. When these 60 days are exhausted, compensation can be paid for additional maximum 60 days per child and year. The additional 60 days cannot be claimed due to the regular caretakers illness or infectiousness.

Compensation to another person when the parent is working

The right to VAB-Benefit can be signed over to another person, who, in the parents place, stays home from work to take care of the child. This means that the parent continues to work and that another insured is absent work in order to take care of the child (in the caretakers place). The right to VAB-Benefit can be signed over when the child is sick or infectious or when the regular caretaker, for example the child minder, is sick or infectious.

Compensation to another person – when the parent falls ill

When a parent is ill and receives sick pay or sickness benefit the Social Insurance Agency can decide that another person, who is absent from work in order to take care of someone else's sick child, can receive VAB-Benefit compensation (in the parents place). That is to say that if you were well and would have been eligible to the VAB-Benefit in the claim for benefit in the situation it concerns. VAB-Benefit with regards to children younger than 240 days can only be paid if the child is hospitalized or when the supervision of the child is permanently arranged in day care and you as a parent has to stay at home from your work in order to take care of the child. VAB-Benefit can also be paid when the child is in the final phase of a care period and is nursed in the home if the alternative would be continuous hospital treatment.

Exchange of parental benefit into VAB-Benefit

If the child is taken care of in a hospital, parental benefit can be e exchanged for VAB-Benefit. This also applies both for the child it concerns as well as older siblings.

Special needs

In some cases VAB-Benefit can be paid for a sick or disabled child who's younger than 240 days. This applies among others to when a parent visits an institution, for example rehabilitation, special school or alike, or attends a course arranged by a medical institution. The same applies for children 240 days or older when the parent exchanges parental benefit to VAB-Benefit.

Compensation

The VAB-Benefit is 80 percent of the sickness benefit based income. The compensation can be withdrawn for a whole, three quarters, a half, one quarter or one eighth of a day. A day with three quarter, a half, one quarter or one eighth compensation is counted as corresponding share of a day. If you are employed, you receive hour- or day based compensation, i.e. 80 percent of the sickness benefit based income divided by the work in one year expressed in hours or days. If you collect income from other type of employment you will receive calendar day calculated compensation, i.e. 80 percent of the sickness benefit based income divided by 365. This applies even if you exchange parental benefit to VAB-Benefit. Your compensation is calendar day calculated also for a day when you are wholly or partially unemployed. If you are wholly unemployed you receive compensation only for days when you loose jobseeker's allowance.

Application

You have to make an application to the Social Insurance Agency on at latest the very same day you wish to begin the period of compensation. You can also apply for the benefit at the web www.forsakringskassan.se, service phone 020-524 524 or phone your insurance office. Some employers automatically file an application for you. It is therefore important that you learn how this is done at your work place.

More information

This folder shall not be regarded as words of an act in this matter. If you want to learn more about the VAB-Benefit and the parental insurance you may visit our web site www.forsakringskassan.se, or get into touch with the Social Insurance Agency.

Obligations

What happens when you receive too much in compensation?

If you receives compensation that you’re not eligible to, you will normally have to pay pack the incorrect sum. This implies even if it’s not your fault that the compensation was incorrect.

It is felonious to cheat

At the Social Insurance Agency we regard the matter of cheating seriously. By “cheating” we mean that someone deliberately tries to get round the rules in order to get hold of compensation. What happens when you cheat, for example by reporting incorrect information or by neglect reporting changed conditions? Well, the one who is cheating always will be hold accountable and held responsible and furthermore runs the risk of being punished by paying fine or to serve jail time.

Always report changes

You are always obliged to report changed conditions significant for the eligibility to compensation.

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