The Price of Violence

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Abstract: This paper estimates the long term impact of assault on health. The connection between assault and ill-health in women is well-documented, but less is known for men. For both genders there is a lack of causal evidence. Using unique and rich administrative data for the whole Swedish population, a matching strategy is implemented to link violence victimization to mortality, work status, and sickness insurance uptake. Assaulted are compared to non-assaulted who were statistically indistinguishable from the assaulted prior to the assault. The results suggest that assault increases the risk of death during the next eight years with four percentage points, to an estimated cost of 19,300 USD per assault and year. Assault reduces the probability to work with 6-11 percentage points, and increases the number of days with sickness insurance uptake with about 20 per year for women, and 7 per year for men. Labor income losses are estimated to 2,430 USD per assault and year. While at first they are almost fully covered by the state, at the end of the study period about half of the economic costs are carried by the victimized individual. There is little sign of reduction of the consequences of assault during the 5-8 years that constitute the follow-up period. Taken together, the findings suggest that calculations of the societal costs of violent crime have been underestimated, as neither the full loss of lives nor the reduction in work capacity of victims have been sufficiently taken into account.

INTRODUCTION

Although most people believe violent crime to be a substantial public health issue, we do not know the magnitude of its effects on health. Some victims in addition to the direct physical injury face long term general health problems and emotional issues, with in turn economic consequences for both individual and society. The total extent and cost of consequences driven by violence are difficult to identify, as assault is not an entirely random event (see e.g. Kilpatrick et al, 1997). There exist a handful of studies taking the non-randomness of violence victimization seriously in studying health effects (Aizer, 2011; Ehrensaft et al, 2006; Kilpatrick et al, 1997; Lindhorst & Oxford, 2008; Stevenson & Wolfers 2006). This literature supports a causal link from violent assault to psychiatric morbidity at least in women, although the methodological frameworks are not robust enough to allow for reliable estimates of magnitude.

In this paper, the long-term impact of violence victimization on health is estimated. Specifically, I focus on the effects on excess mortality, suicidality, work status, sickness insurance uptake, and income. The study is based on large-scale and high-quality longitudinal register data on hospitalizations, income and transactions, and family structure for the entire population of Sweden. The data allows me to follow people for several years before an assault, comparing assaulted to individuals who were their statistical twins prior to the event in order to remove selection bias. Propensity score matching (Rosenbaum & Rubin, 1983) is especially pervasive in cases such as this where there exist a broad set of covariates corresponding to several years of the individual's pre-exposure history (see e.g. Deheija & Waba, 1999).

The estimated effect of violence victimization on mortality is a magnitude higher than the baseline death rate, exposing assaulted men and women to about five percent risk of dying during the eight years following an assault, during which time matched controls have a death rate of half a percent. The effects on suicide risk are as well a magnitude higher than the baseline rate in matched controls, supporting a causal link from violence exposure to severe psychiatric morbidity in both men and women. The effects of assault on work status amounts to an almost 10 percentage point reduction in the probability to work. The effects on sickness insurance uptake are estimated to about 8 days yearly for men, more for women, and more for those who work in the year prior to assault than for those who do not. The effects are highly

persistent over time. In fact, at the end of the 5-8 years post-assault years I am able to observe, none of the subgroups followed is free of the effects of assault on any of the outcomes studied. I calculate the costs due to lost statistical lives to 19,300 USD per assault and year, and the costs due to reduced income to 2,430 USD per assault and year

Calculating the burden of violent crime is useful in a number of important respects. The results give a comprehensive view of the impact of assault on health, which could be used by policy-makers in order to improve our understanding of how society could better aid assault victims. Second, information about the cost of violent crime can be used to inform resource allocation decisions. Calculations of the societal costs of violence will be underestimated if reduced health and decreased labor market participation of its victims are not taken into account[1]. Such estimates have previously neither been able to reliably identify the full and long-term effects on the traumatized person, nor how these effects might differ between victims.

In the next section I review the previous literature linking violence victimization to ill-health, and discuss how the identification problem has, and has not, been dealt with. I then present the data, before introducing the evaluation problem. As the last stage of the methodology, I discuss under which conditions matching is a successful identification strategy, describe how matching has been implemented in this study, and present indicators of match quality. Next I give the graphical evidence and report estimation results. The paper ends with a discussion.

BACKGROUND

In this section I briefly discuss the findings on long term health implications of violent crime on its victims, and to which extent it can be attributed to a causal effect. Violent crime in Sweden is to a large part separable by the sex of the victim (Häll, 2004) [2]. Women are typically either victims to sexual assault or assault by a sexual partner or former sexual partner, and men are physically assaulted outside their homes (Daly & Wilson, 1989;

[1] For example, the Swedish National Board of Health and Welfare calculates the cost of one rape to 6,120 USD while the British Home Office comes to the conclusion that the first year after a rape costs 76,500 USD, more than 10 times more. These (huge) differences in estimates are due to whether immaterial costs are taken into account or not Vylder, S. D. 2010. Vad kostar våldtäkter? Samhällsekonomiska konstader för sexuellt våld. *In:* Heimer, G. & Hermelin, A.-M. T. (eds.) *Antologi. Sju perspektiv på våldtäkt.* Uppsala: Uppsala university.

[2] In workplace violence is to some extent a third, and gender neutral category of typical assaults, at least according to self reported incidents in victimization surveys (Häll, 2004). However, workplace violence rarely results in injury or distress enough to result in inpatient care, which can be seen from professions at risk for workplace violence not being overrepresented in the inpatient care data (see Table 2).

Lundgren et al, 2001). I begin by using the sex of the victim to structure a review of the literature on the association between health problems and violence victimization. Next we move the focus to the literature aiming to identify a causal effect of violent crime on health.

Association of violence exposure and ill-health

Violence victimization in women is associated with a number of common physical health problems. As shown by previous studies, these include gynaecological problems, chronic pelvic pain (Campbell, 2002; McCaulley et al., 1995), high rates of visits to health services, high rates of serious and chronic illness and a large number of days in bed (Campbell, 2002; Campbell & Lewandowski, 1997; Taft, Vogt, Mechanic, & Resick, 2007).

The most prevalent mental health disorders associated with violence exposure in women are depression and post traumatic stress syndrome, PTSD (Golding, 1999). PTSD is diagnosed when an individual has been exposed to a traumatic event that both presents actual or threatened death or serious injury to oneself or others, and elicits intense fear, helplessness, or horror. Symptoms include reexperiencing the traumatic event, and results in avoidance and hyperarousal. Rates of lifetime PTSD in response to rape exeed 30 percent (Resnick et al, 1993) and one in ten female rape victims suffer current PTSD (Kilpatrick & Acierno, 2003). Female violence victims are also more likely to attempt suicide, suffer from sleep and eating disorders and social dysfunction (Bergman & Brismar, 1991; Humphreys & Lee, 2005; Kaslow et al., 2002; Olshen, McVeigh, Wunsch-Hitzig & Rickert, 2007). In addition, they present an exacerbation of psychotic symptoms (Neria et al., 2005) and increased rates of substance abuse (Becker & Duffy, 2002; Bergman, Larsson, Brismar, & Klang, 1989; Golding, 1999; Jones, Hughes, & Unterstaller, 2001; McCauley et al., 1995; Ratner, 1993).

We do not know much about the general health of heterosexual male victims of violent crime, but there are some studies on their mental well being. Men sometimes do develop PTSD in response to physical injury (Michaels et al, 2000). However, the most common precipitant of PTSD in North American men is not assault but rather combat experience and witnessed violence (Kilpatrick & Acierno, 2003). Overall, women report worse post-trauma quality-of-life than men, independent of cause of injury and injury severity (Holbrook, 2004). While

women develop PTSD in response to physical or sexual assault at about the same rate, men rarely report PTSD in response to physical assault and rarely are sexually assaulted. [3]

Identification of long term effects

Many studies using cross-sectional data fully contribute the association between assault and health problems to a causal effect of assault (e.g. Eadie, 2008; Sutherland et al, 2002; Talley, 1998). This is problematic as unobserved factors may affect both the likelihood of violence victimization and health outcomes (selection bias), and health may in itself affect the likelihood of victimization (reverse causality).

In evidence of sorting, several studies have reported that a lack of economic resources puts the individual at risk for violence exposure (Aizer, 2010; Andrews et al., 2000, Ehrensaft et al., 2004, Ehrensaft et al., 2003, Nilsson & Estrada, 2006). Poor and rich people differ in baseline health, implying that part of the association of health and violence victimization is driven by the lower economic resources of the victimized group. As an example of reverse causality, psychological problems can manifest themselves in risk seeking behaviors or substance abuse which in turn increases risk taking. That such a link exists between ill-health and violence exposure has found some support from longitudinal studies, indicating a causal effect of substance abuse on assault incidence (Kilpatrick et al, 1997). Therefore, while the strong and robust association between violence exposure and ill-health suggests that assault has long-term negative consequences far beyond the direct effects, conclusions should remain tentative until more methodologically-sound evidence is available.

A few studies have employed longitudinal data in studying the effect of violent crime on the victim's subsequent life situation. Ehrensaft et al (2006) follow a representative birth cohort prospectively and ask whether domestic violence generates an increased risk of psychiatric disorders in young adults, and whether this holds for both women and men. Lindhorst and Oxford (2008) ask specifically about the link from domestic violence to depressive symptoms in adolescent mothers. Kilpatrick et al. (1997) test competing hypotheses about the direction of relationships between assault and substance abuse in women. These three studies all use a lagged dependent variable regression and come to the conclusion that violence victimization

[3]Tthere is an ongoing debate about to what extent these gender differences in self reported (mental) health are driven by more objective differences in (psychological) well-being (for a discussion on differences in men's and women's reports of subjective health, see Paxon & Case, 2004).

increases psychiatric morbidity in (young) women[4]. In addition, the introduction of unilateral divorce in the United States has been shown to reduce suicide in women, arguably mediated by a reduction in domestic violence (Stevenson & Wolfers, 2006). This result indicates that domestic violence exposure can lead to severe depression. Another finding supporting this claim is that the extremely high rates of depression in battered women have been found to decline over time once the abuse has ceased, and the severity and duration of violence is associated with the prevalence and severity of depression (Golding, 1999), indicating a causal impact of battering on short term depression.

DATASET

The empirical analysis is based on micro data from four administrative registers. The register LOUISE/LISA (A longitudinal database on education, income, and employment) is collected and maintained by Statistics Sweden. It covers the period 1990−2009 and includes all individuals from 16 years of age that were registered in Sweden by December 31:st the year in question. It contains annual information on a wide range of educational, demographic and economic characteristics. To access to several years of high-quality data both before and after an assault, the study is restricted to assaults taking place during the five years from 1998 to 2002. The sample consists of all individuals who were residents in Sweden and 20-54 years of age at least one of these years. The time restriction allows the selection of covariates four years prior to, and up to eight years following, and assault. I further require that selected individuals have had residency in Sweden at least one year prior to the year of interest, and that they have not previously been treated for injuries due to an assault in a Swedish hospital.[5]

Violence exposure

Assault is defined through the National Inpatient Care Register.[6] It covers all inpatient medical contacts at public hospitals from 1987 through 1996, and from 1997 all inpatient medical contacts regardless of operating form.[7] Starting in 2002, the register also covers

[4]These studies are the most convincing in the field. However, in case the true model includes any unobserved, constant factors driving both health behaviors and assault-risk, this specification is likely to bias estimates upwards (compare e.g. Angrist & Pischke, 2009, p. 246 and note that assaulted are have higher pre-assault psychiatric disorders and substance-use than non-assaulted). It would be interesting to see the fixed-effects estimates as well.

[5] After 1987 that is, before that we have no information on medical care.

[6] Administrative data on hospitalizations and mortality are collected and maintained by the National Board of Health and Welfare.

[7] Until 1994, all medical care in Sweden was operated by public agents.

outpatient medical contacts in specialized care. The diagnoses, made by physicians, are classified according to the World Health Organization's International Statistical Classification of Diseases and Related Health Problems, ICD[8]. In the case that an injury leads up to a hospital admission, the external cause of the injury is noted. To be a victim of violence, *assaulted*, in this study is defined as having received an external cause of injury code denoting assault. The severity of the assault is defined using the injury severity score (ISS) (Baker et al, 1974).[9] Table 1 gives an overview of diagnose codes denoting assault, diagnose codes relevant for the ISS, and the ISS categories used in this study.

Dependent variables

Mortality is defined using the Cause of Death Register. It includes death date and cause for all who died during one calendar year and were registered in Sweden at the time of death, regardless of whether the death occurred inside or outside the country. I have access to records up to and including 2010. *Suicide* is defined as a death with an indication of self harm on the cause of death variable.[10]

The measure of sickness insurance uptake, *sick leave*, equals the total number of days on either sickness insurance or disability benefits. Sickness insurance covers loss of income in case of work absence due to illness or disability. All workers (employed and unemployed) are covered by public sickness insurance (with the first day sick uncompensated). For unemployed sick two or more days in a row, and employed sick beyond two weeks, the Social Insurance Agency is responsible for benefit payments. The Social Insurance Agency's records cover all disbursements made by the agency and are available to me from 1990 to 2008. Disability benefits[11] are always provided through the Social Insurance Agency and can be granted either fully or partially (25, 50, or 75 percent).

[8] ICD is a four digit coding of diseases and signs, symptoms, abnormal findings, complaints, and external causes of injury or diseases. For both the Cause of Death Registry and the Inpatient Care Registry, diagnoses have been recorded according to ICD-9 from 1987 to 1996/7, and from 1997/8 according to ICD-10. By 1998, diagnosing according to ICD-10 was implemented in all Swedish hospitals.

[9] The main injury in each of six body regions is classified according to its relative severity on an integer scale from 1-6. The ISS is defined as the squared injury severity of the three most severely injured body regions. Translation from ICD to ISS follows MacKenzie et al. (1989). The specific coding from ICD-10 to injury severity applied here was possible using data from the European Center for Injury Prevention (2006).

[10] values X60 – X84 in the ICD-10 coding system.

[11] With the term *disability benefits* I denote reimbursements for individuals with long-term reduced work capacity. The terms for this part of the Swedish social insurance used by the Social Insurance Agency are *activity compensation* (up to age 30) and *sickness compensation* (age 30-64)

From LOUISE/LISA the following outcome variables are drawn. *Work status* is defined as *working* if the individual was working at least one hour a week in November that year. All individuals who do not fit this criteria are defined as *not working*, irrespective of whether they are in the labor force of not. *Income* is defined as net income from work and capital. *Disposable income* is measured using the universe of net income from work and capital combined with net social benefits and transfers. All numbers are given in year 2010's prices.

Covariates

A *hospital visit* corresponds to an inpatient medical contact.[12] A *psychiatric diagnose* is defined as having made a hospital visit resulting in a diagnose referring to a mental or behavioral disorder[13] at some point since year 1987. *Years of schooling* is constructed by transforming the highest reported education into the average number of years needed to accomplish it. *Newly Separated* is defined as a shift from cohabiting/being married to living alone during the last two years. *Risk occupation* is defined as having an occupation with close client contact, as those occupations are most exposed to workplace violence: teacher, social worker, medical staff, police, or prison ward.

Descriptive statistics

Table 2 displays pre-assault descriptive statistics for assaulted and the general population. All assaulted in the sample are selected; variable values correspond to the year prior to the assault. For each year 100,000 non-assaulted of each sex are randomly drawn from LOUISE/LISA; variable values correspond to the year prior to the selection year. The final sample includes 1536 assaulted women, 5345 assaulted men, and above 2 million non-assaulted of each sex.[14]

Prior to the assault, assaulted differed from the reference population on almost every aspect studied here, and they differed not by a few percentage points, but often by several times the likelihood of the reference population. Looking first at the family situation, I recognize the two groups of commonly assaulted from survey studies, single young men and single mothers, often recently separated from their partners. Assaulted women were half as likely to be married as the average individual, and assaulted men a third as likely. Instead the assaulted

[12] Contacts relating to child delivery are not included.
[13] ICD-10 code beginning with an F.
[14] The exact number of cases and controls cannot be stated, because the identification strategy regards only information up to the year of assault. It is discussed in detail in the section on risk set matching.

were twice as likely as the reference population to recently have broken up, either from a spouse or from a cohabiting partner with whom they have children. Assaulted men were half as likely to have small children as the reference population, but if they did, they were twice as likely to be single fathers. Still, single parenthood is a more central characteristic among the assaulted women, with single motherhood twice as likely among assaulted as among women on average, and single parenting being more common overall in women than in men.

Moving to some general variables, we find that assaulted men have received one year less schooling than the average person, and were much less likely to work in the year prior to the assault. Of both men and women in the reference population, more than 75 percent are working. In assaulted, this was the case for only 40 percent of the women, and 50 percent of the men. Assaulted had lower disposable income than the reference population, with the income difference most pronounced in men. While men in general have higher income than women, assaulted men make significantly less than assaulted women. By analyzing the pre-assault proportions of individuals with an at-risk occupation, we see that these individuals are not particularly overrepresented[15]. This indicates that such client related assaults seldom causes injuries or distress severe enough to result in inpatient care.[16]

Turning to pre-assault health status, the most prominent difference is the percentage of the population with a history involving hospitalization for a mental health problem, a strong indicator of psychosocial morbidity. Only 4 percent of the general population classifies into these criteria of a mental health problem. Moving to the assaulted, the increase in prevalence is between 460 and 760 percent as almost one in four men, and one in three women has received such psychiatric diagnose. Assaulted are more likely to have received in-patient care of any sorts, indicating a higher proportion with serious health problems. Focusing on work related health, the results again indicate that assaulted have worse general health than the reference population. Prior to being assaulted, assaulted received sickness benefits twice as often as the reference population, and were more than twice as likely to receive disability insurance during the year prior to the assault. Assaulted women received sickness insurance on average every third work day, and assaulted men every six days.

[15] Here they are underrepresented, as assaulted are less likely to work at all than non-assaulted. Looking specifically at employed (results not shown) risk occupation is however still not an overrepresented trait.
[16] We do not say that such violence cannot have consequences, but that it is not part of what defines assault in our study. We thus conclude that the sex separation of the analysis to a large extent defines the assault characteristics of the data analyzed.

THE EVALUATION PROBLEM

Selection bias

To be able to discuss causality in a non-experimental setting I use the potential outcome framework, with two potential outcomes. Y_{i1} is the outcome of individual *i* after an assault, and Y_{i0} is the outcome of the same individual, in the case that person was not assaulted. Unfortunately, I can observe either Y_1 or Y_0, but never both. The actually observed outcome for any individual *i* can be written as a function of the potential outcomes and the assault $Y_i = Y_{i1}D_i + (1 - D_i)Y_{i0}$, where D_i is a binary assault indicator which takes the value 1 if individual *i* has been assaulted and the value 0 otherwise. As we never observe both Y_1 and Y_0 for the same individual we have a missing data problem, and as we do not have experimental data, we cannot be sure that the data is missing at random.

The difference between individual *i*'s potential outcomes $\Delta_i = Y_{i1} - Y_{i0}$ can be interpreted as the causal effect of becoming assaulted for individual *i* . This holds under what is called the Stable Unit Treatment Value Assumption (SUTVA). That is, we need to assume that assault affects only the outcome of the individual assaulted, with no network effects, and no equilibrium effects. For feasibility, focus is on aggregate effects. Specifically I want to estimate the impact of assault on those who have been assaulted. This parameter is called the average treatment effect on the treated (ATT)[17]. It is given by

$$\mathrm{ATT} = \mathrm{E}(\Delta_i|D_i = 1) = \mathrm{E}(Y_{i1}|D_i = 1) - \mathrm{E}(Y_{i0}|D_i = 1).$$

Using non-assaulted as a comparison group to estimate the effects of assault would fail to identify the ATT, since assaulted and non-assaulted are selected groups with potentially different outcomes even in the absence of assault,

$$\mathrm{E}(Y_{i1}|D_i = 1) - \mathrm{E}(Y_{i0}|D_i = 0) = \mathrm{ATT} + [\mathrm{E}(Y_{i0}|D_i = 1) - \mathrm{E}(Y_{i0}|D_i = 0)].$$

The additional term to the right captures the selection bias from a naïve estimation of the ATT, consisting of the difference between those who are and are not assaulted, in the absence of assault.

[17] Actually, we will focus on a slightly different estimator: The average effect on the treated of treatment now instead of later. It will be explained in detail in the section on risk set matching.

The matching strategy

If we assume that selection occurs on (only) observed characteristics, I could use a matching estimator to remove the bias term in the equation above. The basic idea of matching is to select from a large group of non-assaulted those individuals who are similar to the assaulted in all relevant (observable) characteristics, i.e. those characteristics that influence the risk of assault and the outcomes simultaneously.

I use propensity score matching to choose a comparison group as similar as possible to the cases in terms of observable characteristics. The propensity score $p(X_i)$ is defined as the probability of assault victimization for individual *i*, and summarizes the information in the observed covariates X_i into a single probability. The ATT can be identified through matching if, for a given value of the *X* vector, the distribution of the (counterfactual) outcome Y_0 in the assaulted group is the same as the (observed) distribution of Y_0 in the non-assaulted group. For assaulted and non-assaulted with the same propensity score, the distributions of the covariates *X* are the same (Rosenbaum & Rubin, 1983). In symbols, unconfoundedness for the comparison group given the propensity score can be expressed as

$$Y_{i0} \perp D_i | p(X_i),$$

where $\perp$ denotes independence. Under the unconfoundedness assumption, the missing counterfactual mean for the assaulted is given by the outcomes of non-assaulted matches:

$$E(Y_{i0}|p(X_i), D_i = 1) = E\big(Y_{i0}\big|p(X_i)\big) = E(Y_{i0}|p(X_i), D_i = 0).$$

For estimation, both $E(Y_{i1}|p(X_i), D = 1)$ and $E(Y_0|p(X), D = 0)$ need to be well defined simultaneously. That is, there has to exist non-assaulted with the same covariate distribution as that of the assaulted. This is often called the assumption of weak overlap and can be formally expressed as

$$Pr(D_i = 1|X) < 1, \forall X.$$

These assumptions are sufficient for identification of the ATT, the parameter of interest. It holds for the full population of assaulted as well as for persons with certain characteristics of X. In this study, the subgroups of interest are based on injury severity, sex and work status prior to assault. The ATT at some point X=x, where x is a particular realization of X is defined as follows,

$$\mathrm{ATT}(\mathrm{X_i} = \mathrm{x}) = \mathrm{E}(\Delta|\mathrm{X_i} = \mathrm{x}, \mathrm{D_i} = 1) = \mathrm{E}(\mathrm{Y_1}|\mathrm{X_i} = \mathrm{x}, \mathrm{D_i} = 1) - \mathrm{E}(\mathrm{Y_0}|\mathrm{X_i} = \mathrm{x}, \mathrm{D_i} = 1).$$

MATCHING IN PRACTICE

In the following section the implementation and justification of the matching estimator are discussed in the context of this particular study. I begin by introducing the estimator implemented in this study. Following that, I present the specification of the propensity score estimation and the matching algorithm. Finally I discuss whether the assumptions made in the previous section are reasonable by examining the common support and match quality.

Risk set matching

In this analysis a generalization of propensity score matching called risk set matching is employed (Li et al. 2001). Risk set matching generalizes the propensity score by making it time dependent. The purpose is to avoid information loss in the case when the treatment, in this case the assault, is received at different times, and time-varying covariates are available. The counterfactual expectation of assaulted, had they not been assaulted, is constructed as

$$\mathrm{E}(\mathrm{Y_0}|\mathrm{p}(\mathrm{X_t}), \mathrm{D_t} = 1) = \mathrm{E}\big(\mathrm{Y_0}\big|\mathrm{p}(\mathrm{X_t})\big) = \mathrm{E}(\mathrm{Y_0}|\mathrm{p}(\mathrm{X_t}), \mathrm{D_t} = 0).$$

$\mathrm{D_t}$ is an indicator for assaulted at *t* and $\mathrm{p}(\mathrm{X_t})$ the time dependent propensity score, a function of pre-assault covariates measured up to *t*. This allows for matching assaulted at *t* with individuals who have approximately the same probability of assault but have not been assaulted at or up to *t*. This specification of the risk of assault can be estimated through a standard logistic regression model for each year separately. Its dependent variable is whether individual *i* is assaulted or not in year *t* and the predictor variables are covariate values measured up to *t*

$$p(X_{it}) = Pr(\mathrm{D_{it}} = 1|\mathrm{D_{it-1}} = 0, X_{it}) = \frac{1}{1+e^{\alpha_t+\beta_t X_{it}}},$$

where X_{it} is a vector measured up to year *t*, which may include time varying characteristics, and α_t is a time specific constant. The time index of β_t signifies that the impact X_{it} is allowed to vary between years.

The risk set algorithm is implemented as follows. In year *t+1* assaulted in year *t* are removed together with their matches, and the procedure is repeated on the remaining population. With this way of handling multiple treatment times, a person who later on becomes assaulted can

be included in the study as a control instead of as a case. Viewed in relationship to a standard ATT, a risk set estimator is biased towards zero, with the ATT as an upper bound.[18]

Estimating and matching on the propensity score

While failure to include covariates that influence both risk of assault and work related health in the specification of the propensity score will bias the results, inclusion of irrelevant variables inflates the variance. Only variables that simultaneously influence the assault risk and the outcome variable should thus be included in the matching procedure. Theoretical arguments and previous empirical research are argued to be key to covariate selection (see, e.g. Smith & Todd, 2005), but recent evidence point to data driven covariate selection techniques to potentially do better than theoretically based strategies (De Luna et al 2011).

We use previous empirical findings to select potentially important variables, while the final choice of specification is selected using a stepwise procedure. Previous studies in economics and criminality on the consequences of poverty and inequality on assault point to the inclusion of socio-demographic and health related variables. Feminist theory as well as empirical findings highlight the importance of family situation (e.g. Häll, 2004; Ornstein & Rickne, forthcoming). Regarding the first category I use age, years of schooling, work status and disposable income. The second class (health related variables) is especially important as indications of bad pre-assault health of assault victims is central to worries about confounding and reverse causality. In this category I use pre-assault values of the number of days on sickness insurance and whether the individual has received any disability benefits. In addition I include whether the individual has made any hospital visits, and whether she has a received a psychiatric diagnose. The third category includes marital/cohabiting status, recent separation from partner, existence of young children, and custody. To be able to take trends in life-situation and health into account, I utilize variables from four pre-assault years. For variables concerning mental health and inpatient care I am able to use information dating back to 1987.

The propensity score is estimated separately for men and women, and separately for each assault year. First several functions[19] of the abovementioned variables are included in the

[18] The control group for risk set estimators is at risk of assault later during the study period or evaluated years. The correct interpretation of our estimator of the assault impact is therefore as the *effect on the assaulted of assault now instead of possibly later*. In our final sample, 18 of the female controls and 145 of the male controls are assaulted during the study period, i.e. would have been defined as cases had the matching strategy conditioned on future information.

model. I then stepwise exclude variables which are not significant (5%) in any of the five years of the study period. If a variable relating to a year further back in time is included, that variable is kept in the model all later years. I match on the propensity score, and in addition match exactly on sex and work status in the year prior to assault.[20] After matching, covariate balance is evaluated, and the procedure is repeated with more weight on poorly balanced variables. I stop when the originally included over-time averages are balanced. Propensity score specifications can be found in the Appendix, Table A1-A2.

Plausibility of assumptions

First, I check the region of common support. For matching to be a feasible strategy, there has to exist non-assaulted who are similar to the assaulted in terms of their assault probability. It is a common finding that the distribution for non-assaulted is highly skewed to the left. The region of interest is therefore the right tail, i.e. the highest values of the propensity score. In this case the size of the available control group is large enough to overlap the assaulted on the right side of the distribution of the propensity score with good margin. Overlap graphs are very similar for the 20 match groups (separated by assault year, sex, and work status in the year prior to the assault). To give a picture of the overlap, I show the right tail distribution of working women assaulted in 1998 in Figure A1.[21]

Since this is not not using an exact matching procedure, we need to check the ability of the matching algorithm to balance the relevant covariates in cases and controls. In Table 3, match quality is presented separately for men and women. We can see that assaulted cannot be distinguished from the matched comparison group on any of the over-time averages, with the exception that assaulted men have a slightly increased risk of having received a psychiatric diagnose.[22] Note from Table 2 that this was far from the case with the unselected population.

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[19] Depending on the characteristics of the original variable some of the following are included: squared and cubic forms, dichotomizations, categorizations, and log(x+1). In addition, we include variables relating to the previous four years: linear trends, positive vs. negative trend, "not once", and "at least once "

[20] I focus on bias reduction over variance reduction, and implement nearest neighbor matching with replacement, using five controls and a tight caliper of 0.00045. Nearest-neighbor matching uses the assaulted and a specified number of her closest neighbors, the fewer the more variance and less bias. Matching with a caliper restricts the available matches of individual *i* to individuals with a propensity score within that caliper of *i´*s propensity score. For relation, the standard deviation of the propensity score is 0.0042 for men, and 0.0024 for women.

[21] Five observations are unsupported. Their propensity scores are shown in the Appendix, Table A3. For four of these observations, there exist non-assaulted in the same match group with higher values on the propensity score. The lack of suitable matches is caused by the choice of a quite strict caliper in the balancing between bias reduction and common support.

[22] In Table 2 and 3 over-time summary statistics of pre-assault characteristics are shown. For the variables psychiatric diagnose and hospital visit, while the summary variables covering the likelihood over time is well

INFERENCE

As matching – hopefully – removes selection bias, estimating effect size in a matched subsample is straightforward. I estimate the ATT as follows:

For each assaulted individual *i*, let $j = 1, \dots, 5$ be the five closest matches. The potential outcome $\hat{Y}_{i0}$ is constructed as the average of the outcome of these matches. The estimator of the ATT is defined as the difference between the estimated potential outcome of the assaulted after assault, and the constructed potential outcome of the assaulted after no assault.

$$\hat{Y}_{i0} = \frac{1}{5}\sum_{j=1}^{5} Y_{j0}.$$

$$\widehat{ATT} = \frac{1}{N}\sum_{i=1}^{N}(Y_{i1} - \hat{Y}_{i0}).$$

Matching gives asymptotically unbiased results under SUTVA and the unconfoundedness assumption. A further refinement is to use bias adjustment (see for example Abadie & Imbens, 2011). The bias adjusted estimator is defined following Abadie and Imbens (2004). First, $\hat{X}_{i0}$ is constructed by averaging over the matches in the same way as with $\hat{Y}_{i0}$ above. Now, define μ_{i0} as the regression prediction of the constructed potential outcomes on (a subset of) their pre-assault characteristics. The potential outcomes correcting for the regression predictions are adjusted using the respective pre-assault characteristics of assaulted and non-assaulted. $\hat{X}_{i0} = \frac{1}{5}\sum_{j=1}^{5} X_{j0}$, $\mu_{i0}(\hat{X}_{i0}) = E(\hat{Y}_{i0}|\hat{X}_{i0})$,

$$\tilde{Y}_{i0} = \mu_{i0}(\hat{X}_{i0}), \qquad \tilde{Y}_{i1} = \mu_{i0}(X_{i1}).$$

The bias adjusted effect estimate is defined as the mean difference between these bias adjusted outcomes

$$\widehat{ATT} = \frac{1}{N}\sum_{i=1}^{N}(\tilde{Y}_{i1} - \tilde{Y}_{i0}).$$

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balanced, in each specific pre-assault year assaulted differ significantly from controls. To amend this potential bias source, we adjust extensively for these characteristics in the estimation of ATT. For the remainder of the characteristics originally included in the propensity score specification, covariate balance is achieved in *each* pre-assault year. For the variables work status, sick leave, and income, this is visible in Figure 2-5, and 9-11.

Throughout this study, match values are adjusted using age, any psychiatric diagnose, the number of hospital visits and the average number of days on sick leave during the last four years prior to the assault, and any hospital visit and cohabiting status one year prior to the assault.

To assess whether post assault differences are larger than what is likely to be random noise, both variation in the matched sample and the matching in itself needs to be taken into account. The variance of the matching estimators implemented here has been derived in Abadie and Imbens (2006). I calculate the variance of the population ATT assuming homogenous effects (within sex and prior work status) and homoscedasticity of the outcome variable.[23]

MORTALITY

Mortality is important to the analysis not only because the severity of the outcome, but in addition because impact on mortality causes nonrandom censoring on other outcomes. In case an assault reduces the probability of survival, it will be specifically individuals with high sickness absence and low labor force participation who will be most at risk, biasing the estimates downwards. Note that assaults resulting in immediate death are excluded from the sample, as these individuals are not part of the population at the end of the assault year. Focus of the analysis is on mortality beyond what is driven directly by physical injuries.

Visualizing the death hazard of the full sample in Figure 1, we see that while all individuals are most likely not to die, there is a marked difference between assaulted and controls. Assaulted have a higher risk of dying than matched controls, and as this difference does not diminish over time, the discrepancy in the probability of being dead increases steadily over the eight years following an assault. Table 4 reports the baseline mortality and bias adjusted estimates of excess mortality of assaulted during the eight year follow-up period. The risk of dying during this period is about 0.5 percent for matched controls. The risk of dying for assaulted is one magnitude larger, 3.6 percentage points higher for women, and 4.8 for men.

The premature loss of lives due to assault can be monetized using the value of a statistical life in traffic investment calculations, estimated from the amount individuals on average are willing to pay for risk reductions (Hultkrantz & Svensson, 2012). A statistical life in Sweden is estimated to 22,328,999 SEK in year 2010's prices (ASEK, 2012), about 3,396,000 USD.

[23] In addition, I assume that the propensity score is known. Specifically, I use equation (8) and (10) in (Abadie et al, 2004). It is unclear which effect estimating on an estimated propensity score has on the standard errors for ATT estimators (Abadie and Imbens, 2009, p. 8).

The average yearly increase in mortality due to assault equals 0.57 percentage points. As there is no sign of reduction in excess mortality over time I calculate the additional cost due to lost statistical lives per assault and year, it amounts to 126,949 SEK, or about 19,300 USD.

To analyze the role of psychological trauma as a mediator from assault to death I investigate the effect of assault on suicidality. As expected in a sample with such high proportion of psychiatric problems, the baseline suicide rate of matched controls is several times that of the general population. Suicide rates of assaulted however, are 0.7 percentage points higher than those of matched controls, and about 0.8 percent of assaulted men and women in my sample chose to end their lives within eight years of the assault. About 15 percent of the increase in mortality is driven by an increase in suicides. Note that while suicide is a far more common cause of death among men than women among the matched controls as well as in the general population, among assaulted that is not the case.

LABOR MARKET EFFECTS

In this section I report the impact of violence on victims' subsequent probability to work and their number of days on sick leave. The results are provided first graphically then in regression tables to give more detailed information and the possibility to compare ATT estimates with the bias adjusted ATT, as well as including effect sizes in relative terms.

In figure 2, the likelihood to work is assessed for women and men respectively. Focusing first on the pre-assault patterns, we see that the propensity to work is balanced for all pre-assault years, and that it has a positive trend for both assaulted and matches. Moving over to the post-assault years, we note a strong and lasting impact of assault reducing the probability to work with about 10 percentage points for women and 7 percentage points for men. The plausibility of these results hinges on us trusting that the pre-assault positive trend in work status would continue in absence of assault, as it does for the matches while the assaulted make a distinct break from the previous direction. The effects seem to be larger for women than for men and for neither sex is there any indication that the effects are disappearing over time. In Table 5 we can follow the impact of assault on work status in detail. Column 1 and 4 show the simple ATT estimates. In the specifications shown in column 2 and 5 the bias adjusted results from graph 2 are reproduced. Building 95% confidence intervals around the bias adjusted estimates, we see that during the eight consecutive years after an assault it reduces the probability to work between 7.0 and 11.2 percentage points for women, and between 5.9 and

8.6 percentage points for men. Finally in column 3 and 6 the original ATT estimates are given as relative effects. While the effects in absolute terms are larger for women than for men, as men are more often working, the relative effects are about equal for men and women, amounting to a decrease in work probability of between 16 and 24 percent.

In figure 3 the outcome variable is sick leave. Each year prior to the assault, sick leave is balanced between assaulted and controls although only over-time averages are included in the propensity score specification. Prior to the assault, sick leave is increasing for both men and women. After the assault year, matches continue on the slowly rising trend but assaulted make a trend break and increase their sick leave drastically. The increase amounts to about 20 days for women and 10 for men. Detailed estimates are given in Table 6. Column 1 and 4 present the simple ATT estimates while Column 2 and 5 present the bias adjusted ATT estimates given in the figure. The latter are in the interval of 16.6 to 24.3 days per year for women, with signs of diminishing effects. For men there is a large jump from the short term effect of about 13.5 days per year to the longer term effects of 6.6-8.3 days yearly. Women on average are more often on sick leave than men, and this is true among the assaulted as well. Still, although the short term relative effects are of similar size for men and women, about 22 percent, the slower effect reduction in women lead to larger relative effects for women from 2 years post-assault and onwards.

DIRECT EFFECTS OF PHYSICAL VIOLENCE

Physical violence injures the victim in a very concrete and direct way. While I do not analyze the cases with the most severe violence (and thus leading to death), some individuals in the sample are likely to have sustained irreparable damages, leading to long term disability and withdrawal from the labor force. The next question is to which extent the behavior of these individuals can explain the large effects seen in the section above.

The Abbreviated Injury Scale (AIS) describes injuries on a 6-point numerical scale in terms of threat to life and tissue damage. An AIS 1 (minor) injury does not pose a threat to survival, whereas survival is highly uncertain in the case of an AIS 5 (critical) injury. The Injury Severity Score (ISS) provides a numerical scale (from 1 to 75) combining the square of the three injuries with the highest severity in three different body regions to measure the overall severity. An ISS score of 75 is, for all intents and purposes, non survivable (Baker et al 1971), and ISS scores from 16 and upwards are typically classified as major trauma.

In Figure 4 and 5 we see that the majority of individuals did not suffer a major physical injury from the assault analyzed. Instead, 92 percent of women and 86 percent of men incurred injuries corresponding to ISS scores below 7 (mild injury). To give an understanding of what a specific ISS scores corresponds to in the assaulted population, the main diagnoses in the sample are surveyed. Note that individuals with different ISS scores might have the same main diagnose, only different secondary diagnoses. Around 60 percent of both men and women with an ISS score denoting a mild injury have concussion as their main diagnose, followed by fractured facial bones at below 10 percent.[24] Among those with ISS scores of 8-15 (moderate injury), collapsed lungs[25] is the most common diagnose, followed closely by concussion, each inflicting around 20 percent of injured. Of the men and women with an ISS score of 16 or higher (major injury), more than 70 percent had a serious brain injury as their main diagnose.[26]

In Figure 6 the negative effects of assault on probability of work status are presented by injury severity and sex. In both men and women, the effect of injury severity is in the expected direction. In women however, effects are quite large irrespective of injury severity. Turning to the impact of injury severity on sick leave shown in Figure 7, we again see the expected direction of effect magnitudes in both men and women, although to a lower degree in women. In men, ISS score seem able to explain almost the full effect of assault on sick leave. In fact, effect magnitudes of less than 10 days yearly on average for the majority of assaulted men with mild injuries hide the yearly increase in sick leave of almost 40 days for the unfortunate men inflicted with severe injuries. In women, the results point to there being something else than injury severity driving the large effects of violence victimization on health.

HETEROGENOUS EFFECTS

We have now seen the persistent effects of assault on work status and sick leave for men and women separately. In this section, I will further investigate potential heterogeneity of effects. In particular, I focus on the role of work status prior to assault. To work in one year might correlate negatively with an individual's likelihood to respond to adverse events by either temporarily not working or altogether withdrawing from the labor market. In that case, we would see stronger vulnerability to assault in those who were previously not working. A

[24] Icd-code s06.0 denotes concussion, s02.3 fractured orbital floor, and s02.4 fractured jaw bone.
[25] Icd-code s27.0
[26] The three most common main diagnoses in this group was s06.5 (subdural hemorrhage), s06.2 (diffuse brain injury), s06.4 (epidural hemorrhage), and s06.3 (focal brain injury).

potential bias in the other direction is the fact that the outcomes are censored for those with bad pre-assault work related health. Given the specifications of the outcome variables, one cannot work less than not at all, and not be more ill than on sick leave every day of the year. An adverse event such as assault will not trigger any response in an individual whose potential outcome in the absence of assault was to be on full time sick leave.

Figure 8 and Table 7 give the bias adjusted estimates on the probability of working. There does not seem to be much heterogeneity in terms of pre-assault work status. Turning to sick leave in Figure 9 and Table 8, we find very different results. This outcome turns out to be remarkably heterogeneous. Men and women who were not working prior to the assault year increase their sick days with on average 8 days yearly in the short term. Men and women who did work prior to the assault instead respond to assault with respectively 18 and 45 extra days sick the following year. Looking on impact over time we see that the impact of assault has not disappeared after five years but working men and women still show an excess sick leave of 28 extra days yearly for women and 12 extra days for men.[27]

INCOME EFFECTS

While the consequences of violence victimization hit the assaulted individual the hardest, society can mediate the costs, through for example sickness insurance and disability benefits, as could be seen in previous sections. First, I use income to calculate some measure of lost productivity due to assault. While income is not a standard measure of productivity, it captures the value of the time lost working of the affected subgroup. Second, I compare the income effects with the effects on disposable income, to analyze how the economical consequences of assault are distributed between the individual victim and the state.

The income effects can be seen in Figure 10 and Table 9, left side. In the four years prior to assault, income is balanced over assaulted and controls and increasing. Note that income is not included in the specification of the propensity score estimation. Post assault, assaulted have made a trend break. The bias adjusted effect estimates for the whole group amounts to an income reduction per individual and year by on average 18,600 SEK ($2,830), with no signs of effects diminishing over time.

[27] Over this period, the type of sickness insurance uptake changes in the expected direction from the more temporary sickness benefits to the more long term disability benefits (results not shown).

To which extent is this economic consequence carried by the individual? Turning to the variable disposable income on the right side of Figure 10 and Table 9, we instead get a measure of the individual's income plus transactions, such as social insurance transfers. Disposable income is well balanced over assaulted and matched controls during all four pre-assault years, although only an over-time average is used for matching. After the assault, assaulted have reduced their disposable income significantly in relation to matched controls. However, the reduction is much smaller than it was for income. In the short run, almost the full cost of the income loss of violence victims seems to be covered by the state. After 8 years however, at the end of the follow up period, the victims and the state share these costs equally

DISCUSSION

Violent crime is a general public health concern, but the full consequences to victims are far from known. I have estimated the long-term impact of violence victimization on health using Swedish administrative records from 1987 to 2010.

The findings provide evidence that violence victimization increases both mortality and morbidity and that the effects remain several years after the event. All estimations are derived from comparing only individuals who are identical in numerous ways and all results survive after the inclusion of regression controls.

Victimized individuals are about 0.57 percentage points more likely that matched controls to die each year following an assault, and in the eight years of follow up, assault increased suicide risk with 0.7 percentage points, effects almost a magnitude higher than the baseline rates. Violence victimization results in a reduction of work and an increase in sick leave regardless of sex. With a few exceptions, the estimated average effects are consistent over time, meaning that there is no indication of diminishing effects or that the effects should be limited to the follow up period. In absolute and relative measures, those working when assaulted are affected the most.

Studying the effects of the physical injury, we could see that it had important explanatory power mainly in men, and some explanatory power in women. From the review of diagnoses corresponding to the different ISS scores in the section on injury severity, it became clear that broad categories of traumatic injuries receive similar ISS scores. Such a rough measure of

injury severity as ISS is unlikely to explain all variation in consequences. However, in men, ISS scores did explain a large share of the variation in work status, and the vast majority of the variation in sick leave. In women, lightly injured assault victims presented surprisingly large effects. For both men and women, the explanatory power of injury was larger for sick leave than for work status.

To the best of my knowledge, I am the first to employ a thorough causal analysis in estimating the impact of assault on any outcome. These results have important policy implications, as they suggest that the cost of violent crime have been severely underestimated. Existing calculations of the costs of violent crime neither take excess mortality into account, nor assume a lasting reduction in work related health. Using the value of a statistical life in traffic safety calculations, I estimate the cost of the differential long-term mortality to $19,300 per assault and year, with no sign of reduction during the eight years I was able to follow. One way to measure the lost productivity is to look at the cost of the work lost due to assault, i.e. the lost income of the victimized individuals. This measure of productivity loss amounts to $2,830 per assault and year, again with no sign of reduction during the eight years I was able to follow. In the Swedish case these costs at first affect the state rather than the individual, likely through increased social insurance uptake.

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FIGURES AND TABLES

Table 1: Coding systems defining assault status and injury severity.

Trauma	Coding	Inclusion criteria
Assault	ICD-10	X85 – Y05, Y08 – Y09
Injury	ICD-10	S00 – T61
Injury severity	ISS	0-7: Mild, 8-15: Moderate, 16-75: Severe

Table 2. Pre-assault characteristics of assaulted and a random sample non-assaulted.

	Women		Men	
	Unselected sample	Assaulted	Unselected sample	Assaulted
Risk occupation (%)	35.8	22.3***	7.6	5.1
	(0.2)	(1.06)	(0.0)	(0.3)
Married (%)	42.9	21.0***	36.3	10.9***
	(0.2)	(1.04)	(0.1)	(0.4)
Newly separated (%)	7.3	14.0***	7.4	13.2***
	(0.1)	(0.89)	(0.0)	(0.5)
Child <4	15.3	13.8	13.1	6.6***
	(0.2)	(0.88)	(0.0)	(0.3)
Single parent	10.8	25.9***	2.4	4.2***
	(0.1)	(1.12)	(0.0)	(0.3)
Age	37.7	35.9***	37.4	32.6***
	(0.045)	(0.253)	(0.014)	(0.139)
Years of schooling	12.4	11.1***	12.09	11.097***
	(0.01)	(0.053)	(0.003)	(0.025)
Working (%)	75.2	38.3***	78.8	50.8***
	(0.2)	(1.24)	(0.1)	(0.7)
Real income + transactions	140,000	123,000***	167,000	105,000***
(mean last 4 years, SEK)	(280)	(1290)	(295)	(957)
Mental problem diagnose	4.0	34.48***	4.1	22.5***
(since 1987, %)	(0.1)	(1.21)	(0.0)	(0.6)
In-care patient (since	55.8	81.6***	35.4	58.6***
1987, %)	(0.2)	(0.99)	(0.1)	(0.7)
Sickness absence (mean	14.1	33.5***	8.0	17.1***
last 4 years)	(0.19)	(1.77)	(0.046)	(0.677)
Any DI (last 4 years, %)	5.0	16.49***	3.4	8.0***
	(0.1)	(0.947)	(0.0)	(0.4)
Days on sickleave or DI	27	81.7***	17.8	40.0***
(last 4 years)	(0.37)	(3.43)	(0.095)	(1.339)
Observations	500,000	1 536	500,000	5345

Note: Standard errors in parentheses. Stars on the assaulted indicate whether they differed significantly from the reference population prior to the assault: *** p<0.01, ** p<0.05, * p<0.1. Real income has year 2000 as base.

Table 3. Pre-assault characteristics of assaulted and matched controls.

	Women		Men	
	Matched controls	Assaulted	Matched controls	Assaulted
Age	36.1	35.9	32.5	32.6
	(0.25)	(0.25)	(0.14)	(0.14)
Years of schooling	11.1	11.1	11.1	11.1
	(0.052)	(0.053)	(0.025)	(0.025)
Married (%)	21.6	21.1	10.8	10.9
	(1.1)	(1.0)	(0.4)	(0.4)
Newly separated (%)	12.2	14.0	13.0	13.2
	(0.8)	(0.9)	(0.5)	(0.5)
Child <4	15.2	13.8	6.9	6.6
	(0.9)	(0.9)	(0.4)	(0.3)
Single parent	26.1	25.9	4.2	4.2
	(1.1)	(1.1)	(0.3)	(0.3)
Sickness absence (mean last 4 years)	31.4	33.6	16.0	17.1
	(1.71)	(1.77)	(0.65)	(0.68)
Any DI (last 4 years, %)	16.6	16.5	8.2	8.0
	(1.0)	(0.9)	(0.4)	(0.4)
Days on sickleave or DI (last 4 years)	79.4	79.8	40.4	39.9
	(3.34)	(3.29)	(1.37)	(1.33)
In-care patient (%)	81.2	81.6	60.0	58.6
	(1.0)	(1.0)	(0.7)	(0.7)
Mental problem diagnose (%)	32.9	34.5	21.1	22.5*
	(1.2)	(1.2)	(0.6)	(0.6)
Working (%)	38.5	38.3	51.0	50.8
	(1.2)	(1.2)	(0.7)	(0.7)
Real income + transactions (mean last 4 years, SEK)	127,000	123,000	105,000	105,000
	(4,579)	(1,285)	(752)	(977)
Risk occupation (%)	21.1	22.4	5.3	5.1
	(1.0)	(1.1)	(0.3)	(0.3)
No. of observations	7,662	1,532	25,731	5,205

Note: Standard errors in parentheses. Stars on the assaulted indicate whether they differed significantly from the reference population prior to the assault: *** p<0.01, ** p<0.05, * p<0.1. Real income has year 2000 as base.

Figure 1: Cumulative death hazard for assaulted and matched controls.

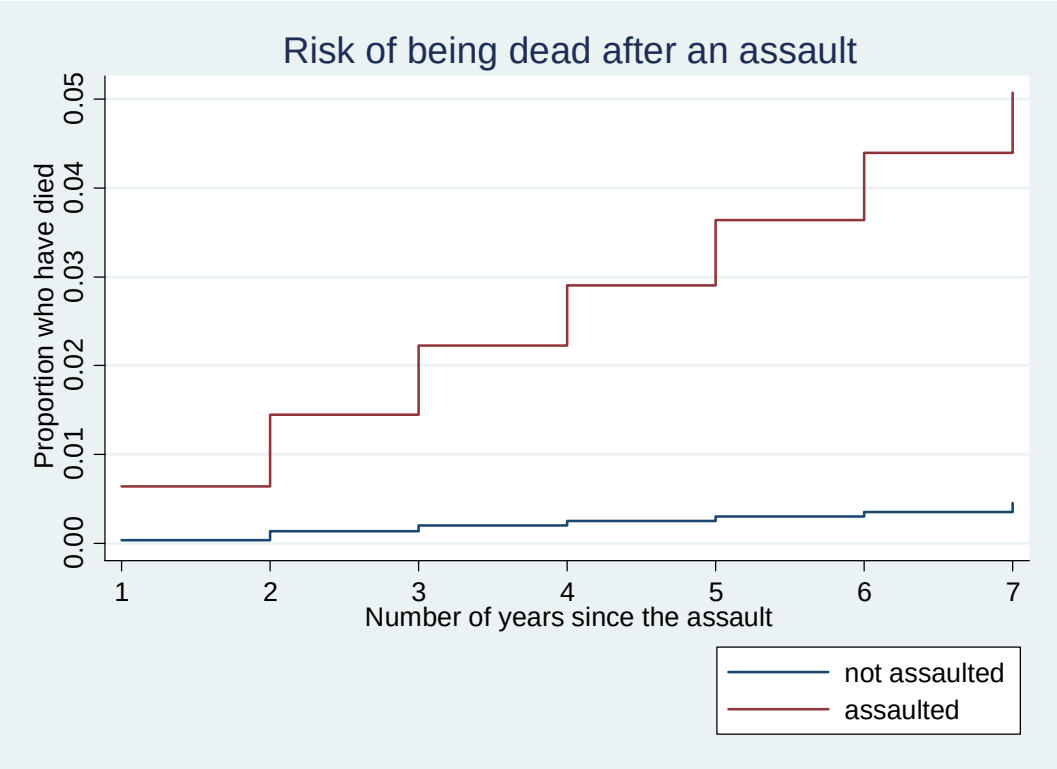
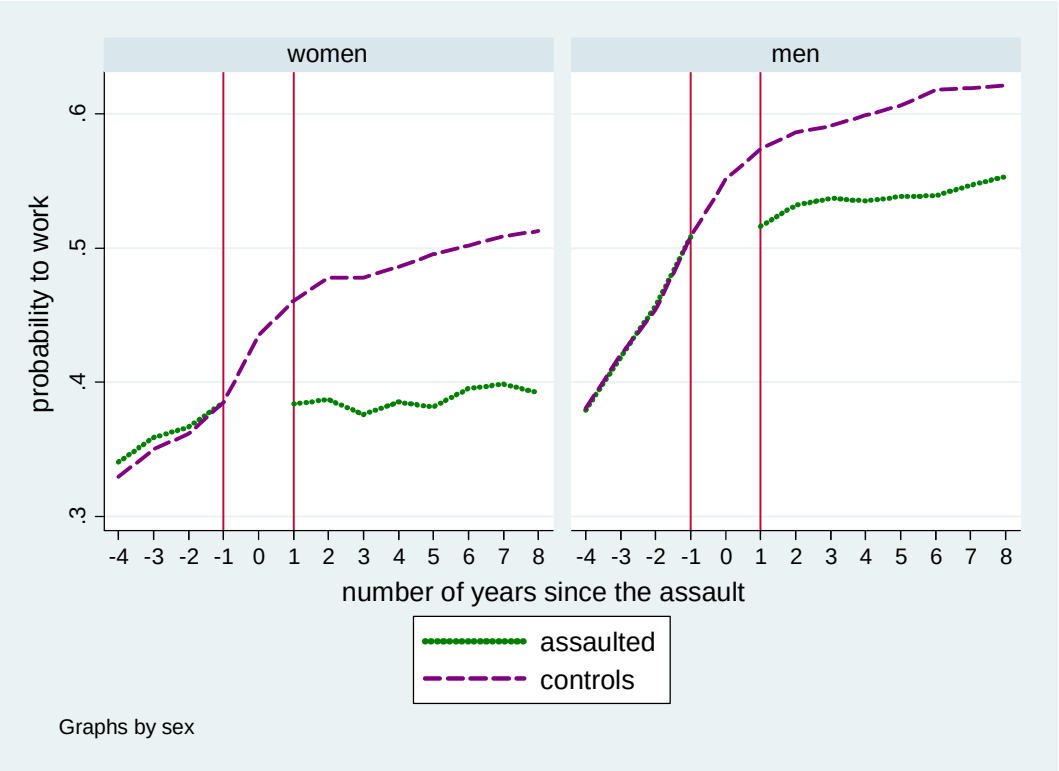


Table 4: Baseline and differential mortality and suicide risk following an assault, by sex.

	Women	Men
Mortality - ATT	3.561***	4.838***
	(0.091)	(0.067)
Mortality – baseline risk	0.575	0.477
Suicide - ATT	0.709***	0.752***
	(0.018)	(0.010)
Suicide – baseline risk	0.078	0.127

Note: Results are reported in percentage points. All ATT estimates are bias adjusted. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure 2: Probability to work over time for assaulted and matched controls.



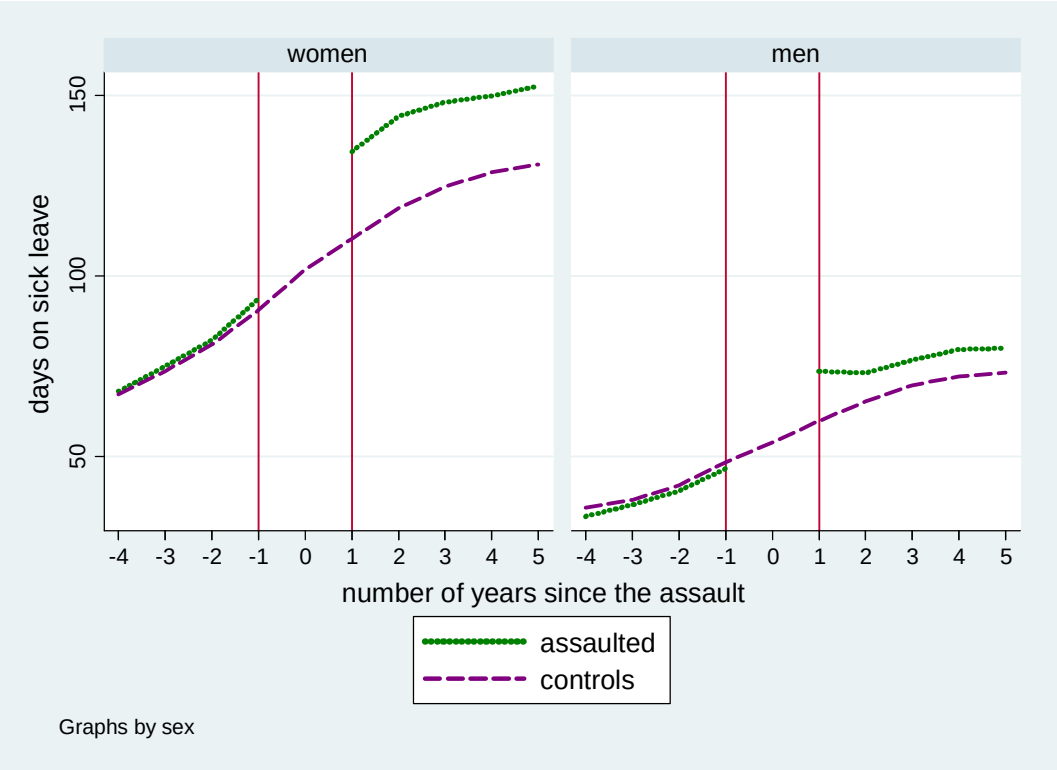
Note: Post assault match values are bias adjusted. The year of assault is set to 0 and no data on assaulted is used from year 0.

Table 5: Negative effects of assault on work status, by sex.

Years post assault	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
1	8.389***	7.390***	15.715	6.934***	6.590***	16.627
	(0.214)	(0.189)	.	(0.096)	(0.091)	.
2	9.771***	8.760***	18.882	6.609***	6.209***	16.282
	(0.249)	(0.224)	.	(0.092)	(0.086)	.
3	10.961***	10.019***	21.255	6.457***	6.045***	15.934
	(0.280)	(0.256)	.	(0.090)	(0.084)	.
4	10.487***	9.562***	20.490	7.362***	6.887***	18.400
	(0.268)	(0.244)	.	(0.102)	(0.095)	.
5	11.549***	10.636***	22.795	7.873***	7.387***	20.076
	(0.295)	(0.271)	.	(0.109)	(0.102)	.
6	10.641***	9.722***	21.241	8.938***	8.417***	23.392
	(0.272)	(0.248)	.	(0.124)	(0.117)	.
7	10.962***	9.951***	22.149	7.962***	7.412***	20.766
	(0.280)	(0.254)	.	(0.110)	(0.103)	.
8	11.685***	10.689***	23.632	7.605***	7.037***	20.008
	(0.298)	(0.273)	.	(0.105)	(0.098)	.

Note: Separate specifications in each cell. Standard ATT estimates (in percentage points) in column (1) and (4). Bias adjusted estimates in column (2) and (5). Relative effects (in percent) in column (3) and (6). Relative effects calculated as the ATT estimate divided by the average match value (relative risk). Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure 3: Sick leave over time for assaulted and matched controls.



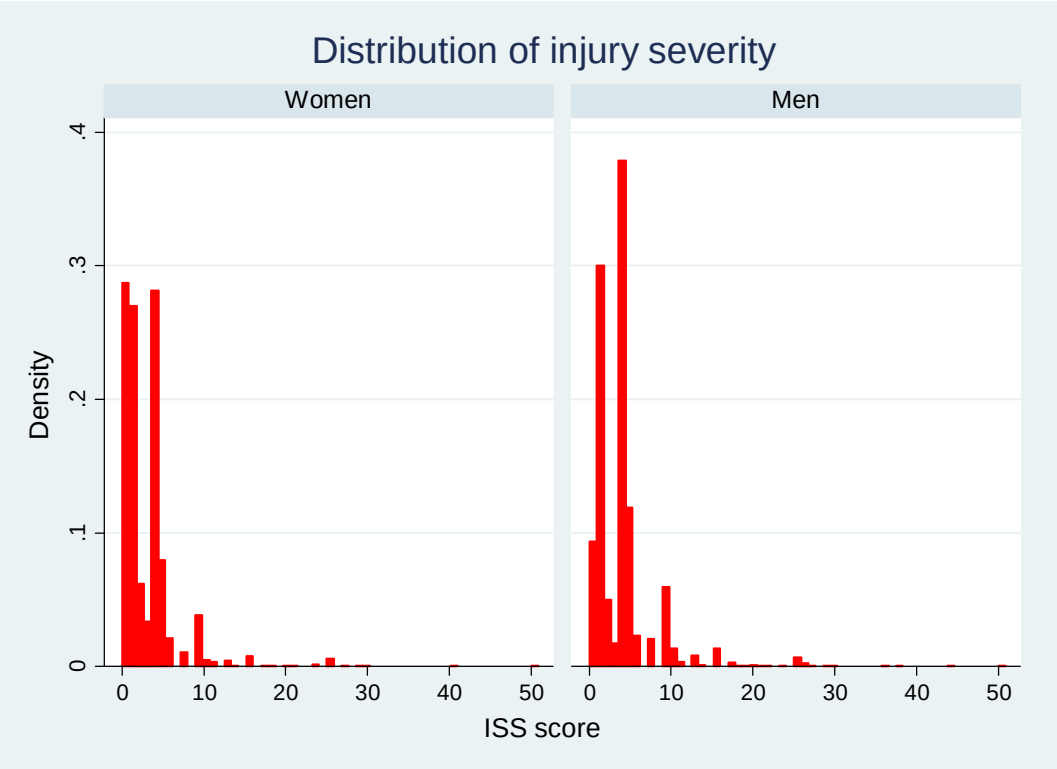
Note: Post assault match values are bias adjusted. The year of assault is set to 0 and no data on assaulted is used from year 0.

Table 6: Effects of assault on days on sick leave, by sex.

Years post assault	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
1	24.964***	22.344***	22.763	13.508***	13.454***	22.392
	(0.637)	(0.570)	.	(0.187)	(0.187)	.
2	25.636***	23.252***	21.748	8.398***	8.111***	12.820
	(0.654)	(0.593)	.	(0.116)	(0.112)	.
3	22.405***	19.818***	18.092	7.803***	7.295***	11.304
	(0.572)	(0.506)	.	(0.108)	(0.101)	.
4	21.007***	18.657***	16.561	8.196***	7.829***	11.459
	(0.536)	(0.476)	.	(0.114)	(0.109)	.
5	19.842***	17.536***	15.347	7.157***	6.774***	9.925
	(0.506)	(0.447)	.	(0.099)	(0.094)	.

Note: Separate specifications in each cell. Standard ATT estimates (in percentage points) in column (1) and (4). Bias adjusted estimates in column (2) and (5). Relative effects (in percent) in column (3) and (6). Relative effects calculated as the ATT estimate divided by the average match value (relative risk). Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure 4: Distribution of injury severity (ISS-scores), by sex.



Note: The ISS scale range from 0 (no injury) to 75 (almost certain prediction of death). Assaults in this sample (where survival is necessary for inclusion) result in injuries ranging from ISS values between 0 and 50, where 16 and higher is considered major trauma.

Figure 5: Distribution of injury severity over severity categories, by sex.

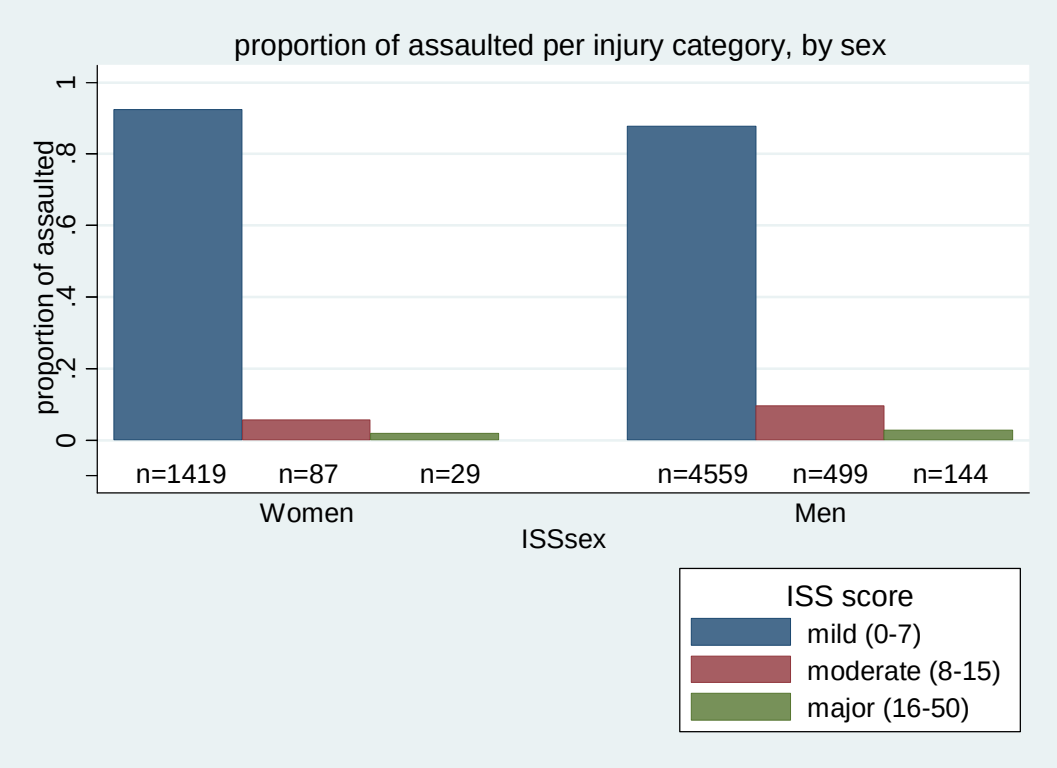
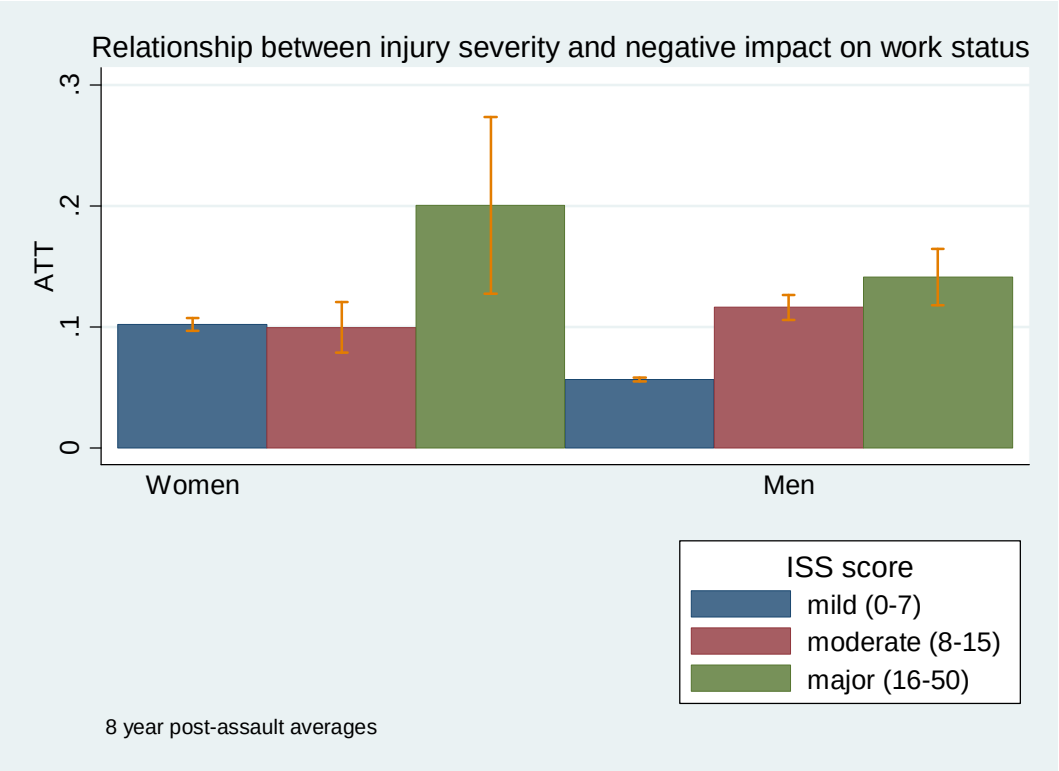
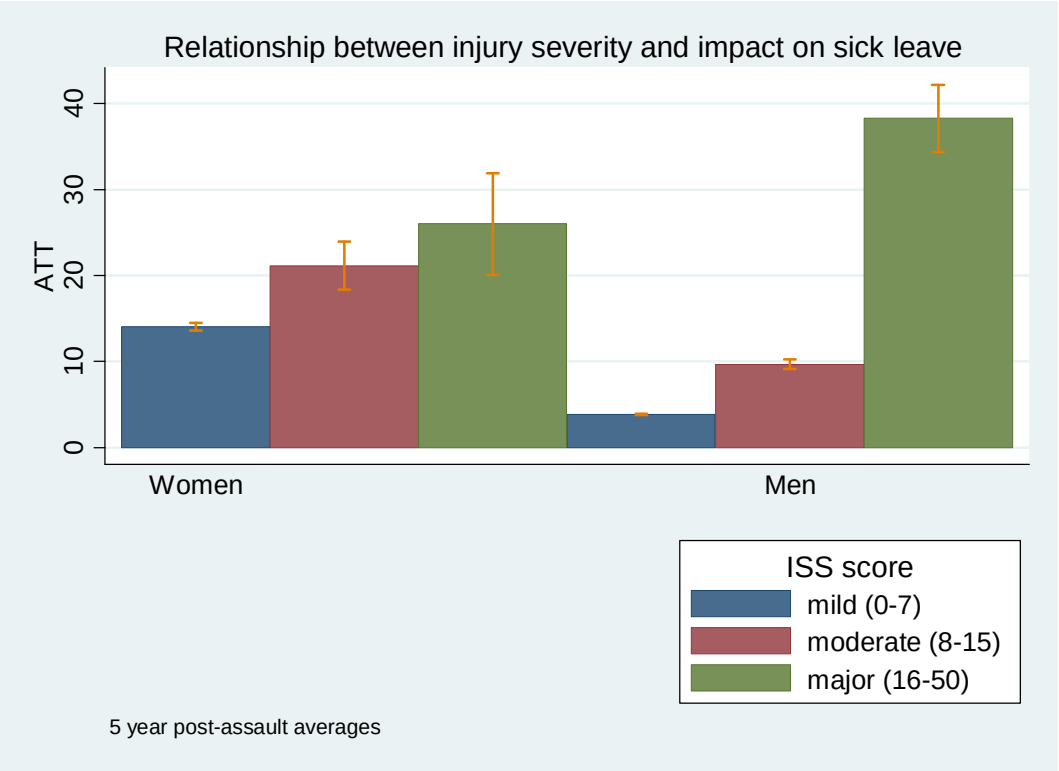


Figure 7: Negative effects of assault on work status, by injury severity and sex.



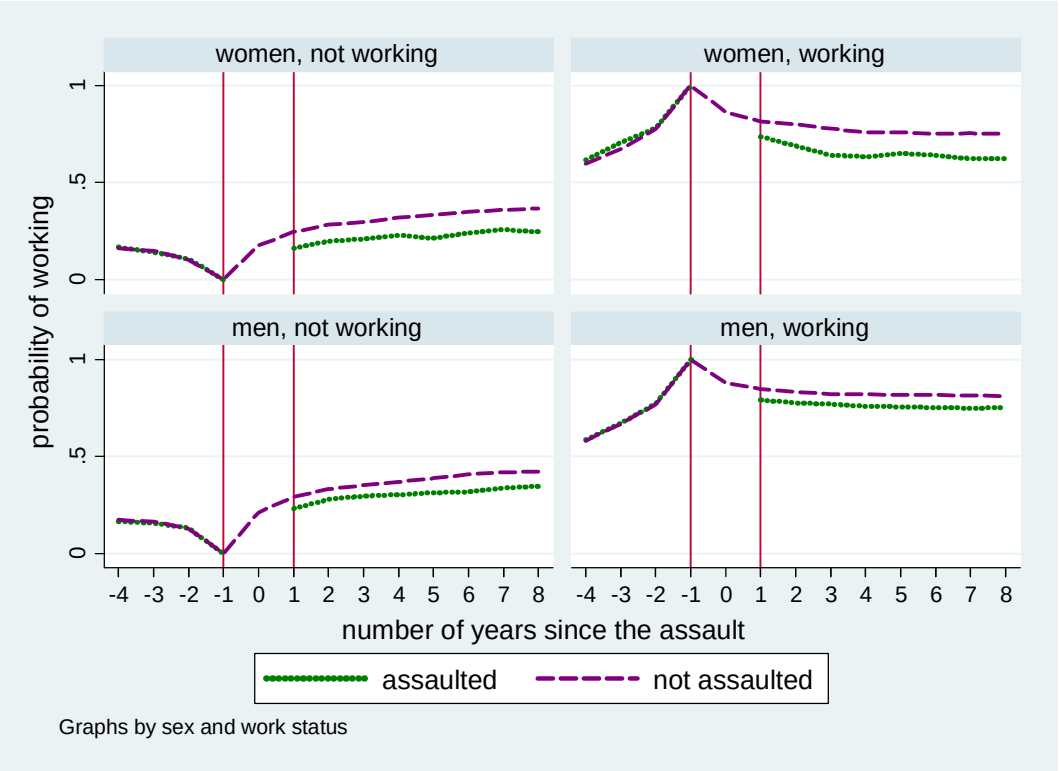
Note: The y-axis represent the reduction in probability to work of assaulted compared to matched controls, averaged over the 8 first years following the assault.

Figure 8: Effects of assault on sick leave, by injury severity and sex.



Note: The y-axis represent the increase in days on sick leave of assaulted compared to matched controls, averaged over the 5 first years following the assault.

Figure 9: Probability of working over time, by sex and work status in the year prior to assault.



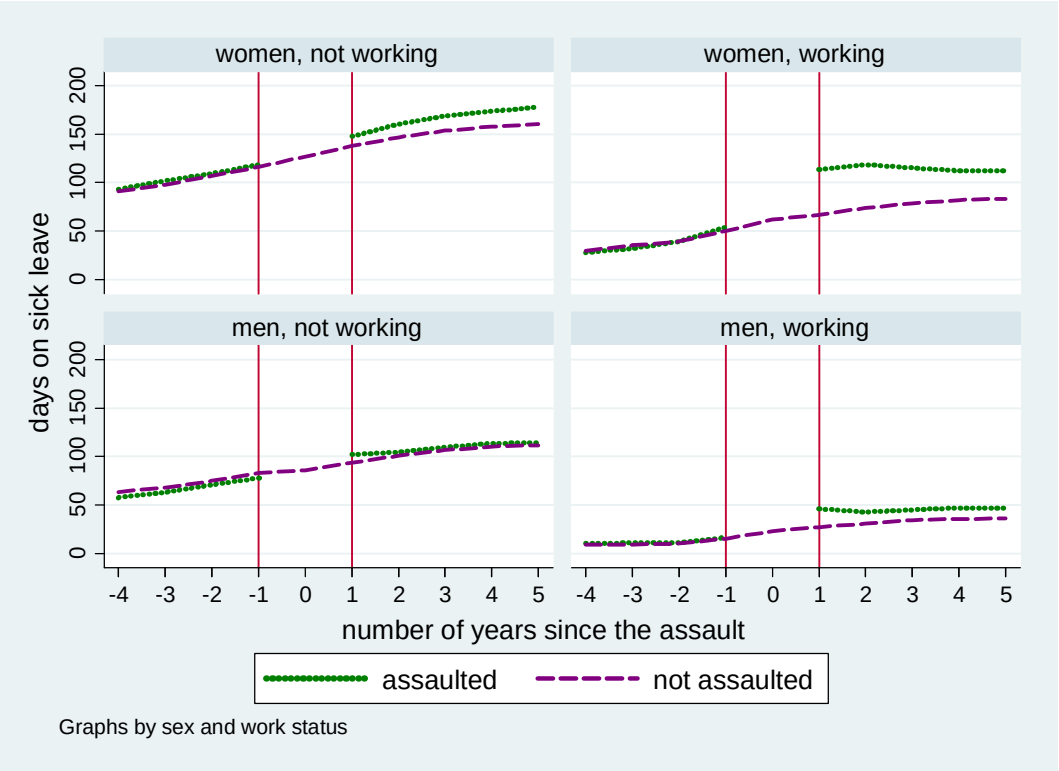
Note: Post assault match values are bias adjusted. The year of assault is set to 0 and no data on assaulted is used from year 0.

Table 7: Effects of assault on probability of not working, by sex and work status in the year prior to assault.

Years post assault	(1) Women, not working	(2) Women, working	(3) Men, not working	(4) Men, working
1	8.340***	7.856***	5.945***	5.534***
	(0.272)	(0.324)	(0.118)	(0.108)
2	8.530***	10.998***	5.303***	5.520***
	(0.278)	(0.453)	(0.105)	(0.107)
3	8.698***	13.538***	5.561***	5.212***
	(0.283)	(0.558)	(0.110)	(0.101)
4	9.026***	12.615***	6.575***	6.151***
	(0.294)	(0.520)	(0.130)	(0.120)
5	12.220***	10.840***	7.389***	6.091***
	(0.398)	(0.447)	(0.146)	(0.118)
6	10.777***	11.172***	9.060***	6.698***
	(0.351)	(0.460)	(0.179)	(0.130)
7	10.170***	13.048***	8.068***	6.377***
	(0.331)	(0.537)	(0.159)	(0.124)
8	12.112***	12.629***	7.429***	6.041***
	(0.395)	(0.520)	(0.147)	(0.117)

Note: Each cell represents one specification. Results are reported in percentage points. All ATT estimates are bias adjusted. Standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Figure 10: Sick leave over time, by sex and work status in the year prior to assault.



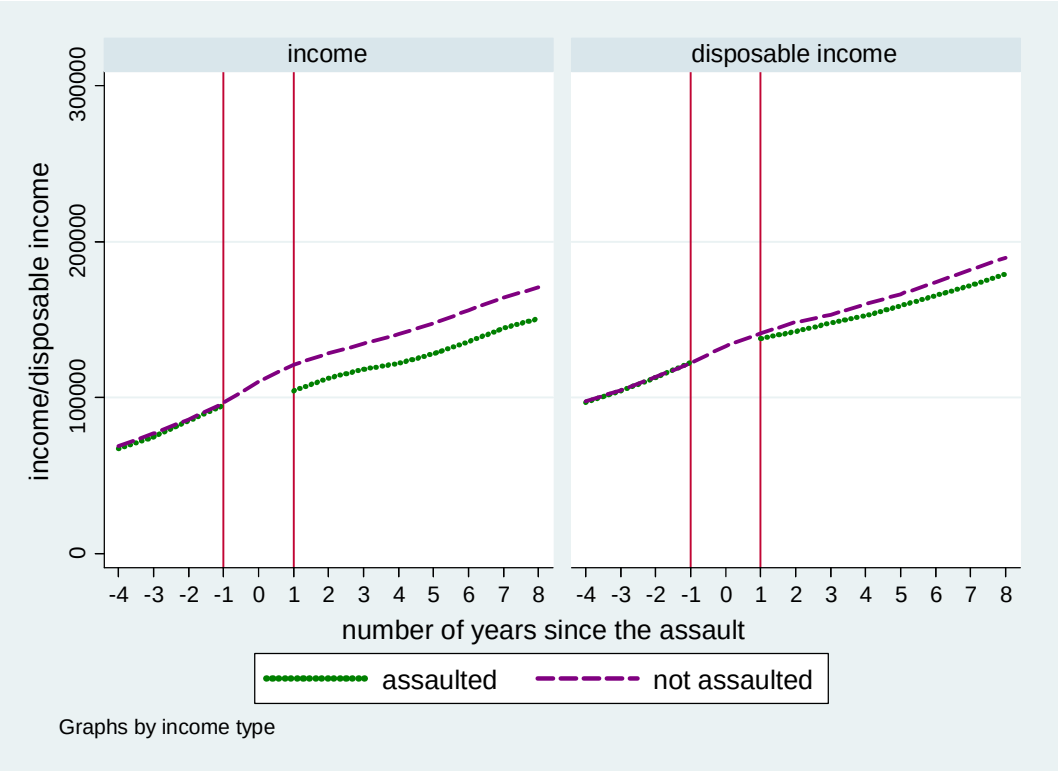
Note: Post assault match values are bias adjusted. The year of assault is set to 0 and no data on assaulted is used from year 0.

Table 8: Effects of assault on sick leave, by sex and work status in the year prior to assault.

Years post assault	(1) Women, not working	(2) Women, working	(3) Men, not working	(4) Men, working
1	8.274***	45.265***	8.502***	18.067***
	(0.269)	(1.866)	(0.168)	(0.352)
2	11.685***	42.514***	4.512***	11.460***
	(0.379)	(1.753)	(0.089)	(0.223)
3	11.457***	34.770***	3.572***	11.284***
	(0.372)	(1.433)	(0.071)	(0.220)
4	13.138***	29.488***	3.370***	12.583***
	(0.427)	(1.216)	(0.067)	(0.245)
5	12.335***	28.265***	2.168***	11.879***
	(0.401)	(1.165)	(0.043)	(0.231)

Note: Each cell represents one specification. Results are reported in percentage points. All ATT estimates are bias adjusted. Standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Figure 11: Income and disposable income over time for assaulted and matched controls.



Note: Post assault match values are bias adjusted. The year of assault is set to 0 and no data on assaulted is used from year 0. Real income/disposable income reported with year 2010 as base.

Table 9: Effects of assault on income and disposable income.

Years post assault	(1) Income	(2) Disposable income
1	-16,634***	-3,376***
	(200)	(41)
2	-15,954***	-6,035***
	(189)	(72)
3	-16,289***	-5,316***
	(190)	(62)
4	-18,758***	-7,355***
	(217)	(85)
5	-19,284***	-7,475***
	(220)	(85)
6	-20;254***	-8,649***
	(228)	(98)
7	-19,545***	-9,958***
	(217)	(111)
	-20,052***	-10,476***
	(221)	(115)

Note: Each cell represents one specification. All estimates are bias adjusted. Real income/disposable income reported with year 2010 as base. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX

Table A1: Specification of propensity score estimation for women 1998-2002.

VARIABLES	(1)	(2)	(3)	(4)	(5)
no. children <11 years	**1.809**	-0.284	**0.976**	**1.126**	-0.485
	(0.567)	(0.463)	(0.476)	(0.557)	(0.542)
children^2	-0.099	0.074	0.062	-0.071	**0.112**
	(0.086)	(0.044)	(0.045)	(0.079)	(0.033)
age below 30	**-0.395**	0.042	-0.115	-0.210	**-0.527**
	(0.194)	(0.181)	(0.200)	(0.195)	(0.211)
Children*age	**-0.058**	-0.004	**-0.036**	**-0.040**	-0.003
	(0.017)	(0.013)	(0.014)	(0.015)	(0.015)
working, 1 year prior	0.410	-0.549	0.375	**0.759**	**0.984**
	(0.344)	(0.311)	(0.305)	(0.300)	(0.306)
children*working	**-1.914**	-0.094	-0.541	-1.328	0.174
	(0.938)	(0.734)	(0.781)	(0.802)	(0.814)
Children*working*age	**0.062**	0.008	0.017	0.045	-0.001
	(0.028)	(0.021)	(0.024)	(0.024)	(0.023)
Single, any last 4 years	**1.138**	**0.931**	**1.256**	**0.868**	**0.982**
	(0.179)	(0.158)	(0.185)	(0.166)	(0.174)
single with child	**0.340**	0.128	0.131	0.153	**0.327**
	(0.172)	(0.165)	(0.169)	(0.162)	(0.159)
years of schooling	**0.983**	-0.225	0.721	0.130	-0.113
	(0.428)	(0.197)	(0.381)	(0.191)	(0.184)
(years of schooling)^2	**-0.046**	0.005	-0.032	-0.007	0.002
	(0.019)	(0.009)	(0.017)	(0.008)	(0.008)
Swedish with at least 1 Swedish parent	0.408	0.153	**1.818**	0.542	**1.401**
	(0.811)	(0.598)	(0.780)	(0.602)	(0.604)
Swedish*(years of schooling)	-0.103	-0.063	**-0.224**	-0.097	**-0.155**
	(0.073)	(0.054)	(0.070)	(0.052)	(0.053)
psychiatric diagnose during hospital visit, since 1987	0.265	0.258	0.514	0.467	0.421
	(0.330)	(0.294)	(0.295)	(0.257)	(0.266)
psychiatric diagnose, last 4 years	0.524	-0.392	**0.744**	**0.643**	0.680
	(0.372)	(0.270)	(0.323)	(0.305)	(0.296)
psychiatric diagnose, last year	0.522	**0.666**	0.397	0.385	0.090
	(0.304)	(0.281)	(0.274)	(0.269)	(0.264)
psychiatric diagnose on at least 2 separate hospital visits, since 1987	0.860	-1.772	0.149	0.388	-0.556
	(0.843)	(1.302)	(0.869)	(0.765)	(0.911)
no. of hospital visits resulting in a psychiatric diagnose, since 1987	-0.009	0.007	-0.014	-0.014	-0.000
	(0.017)	(0.013)	(0.014)	(0.012)	(0.010)
no. of hospital visits, last 4 years	**0.033**	**0.049**	**0.058**	**0.065**	**0.072**
	(0.010)	(0.009)	(0.011)	(0.011)	(0.011)
categorical variable over number of hospital visits	**0.497**	**0.479**	**0.350**	**0.502**	**0.482**
	(0.076)	(0.071)	(0.073)	(0.069)	(0.073)
any hospital visit, since 1987	**-0.589**	**-0.734**	-0.435	**-0.712**	**-1.383**
	(0.238)	(0.225)	(0.243)	(0.246)	(0.249)
any hospital visit, 1 year prior	-0.114	-0.253	-0.047	-0.148	0.207
	(0.191)	(0.181)	(0.190)	(0.184)	(0.180)
any hospital visit, 2 years prior	**-0.322**	-0.264	**-0.414**	**-0.601**	**-0.731**
	(0.152)	(0.139)	(0.152)	(0.152)	(0.159)
any hospital visit, 3 years prior	**-0.684**	**-0.740**	**-0.519**	**-0.539**	**-0.396**
	(0.163)	(0.151)	(0.156)	(0.152)	(0.155)
any hospital visit, 4 years prior	-0.264	**-0.501**	**-0.415**	**-0.400**	**-0.676**

	(0.150)	(0.144)	(0.153)	(0.148)	(0.159)
(psychiatric diagnose) (number of	0.309	**1.314**	0.479	0.434	0.589
hospital visits), last 4 years	(0.438)	(0.341)	(0.389)	(0.349)	(0.345)
any sick leave with duration at least	**0.290**	0.217	0.211	**0.442**	**0.358**
15 days, last 4 years	(0.142)	(0.135)	(0.143)	(0.135)	(0.142)
any disability insurance, last 4 years	-0.038	**-0.884**	-0.580	-0.262	-0.404
	(0.380)	(0.335)	(0.363)	(0.326)	(0.308)
average sickness benefit uptake, last	-0.001	0.000	-0.000	**-0.001**	-0.000
4 years	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
(working, 1 year prior) (average	**0.001**	0.000	0.000	0.000	**0.001**
sickness benefit uptake, last 4 years)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
3:rd quartile on disposable income,	-0.052	0.105	0.410	**0.672**	0.164
1 year prior					
	(0.249)	(0.233)	(0.233)	(0.212)	(0.259)
working*(3:rd quartile on	-0.212	-0.042	**-0.742**	-0.404	0.118
disposable income), 1 year prior	(0.347)	(0.306)	(0.343)	(0.306)	(0.327)
working last 4 years	0.220	**-0.727**	**-0.616**	**-0.959**	0.359
	(0.335)	(0.285)	(0.299)	(0.279)	(0.305)
employed but not working, any of	-0.229	-0.185	**-0.383**	**-0.457**	0.192
last 4 years	(0.157)	(0.145)	(0.150)	(0.142)	(0.163)
working 1 year prior	**-1.131**	-0.203	-0.331	**-0.815**	**-1.057**
	(0.313)	(0.245)	(0.265)	(0.254)	(0.261)
working 2 years prior	-0.197	-0.066	-0.268	-0.331	**-0.379**
	(0.222)	(0.191)	(0.191)	(0.177)	(0.192)
working 3 years prior	-0.016	-0.337	-0.081	-0.185	-0.072
	(0.210)	(0.194)	(0.204)	(0.180)	(0.194)
working 4 years prior	**-0.649**	-0.142	-0.219	-0.010	**-0.516**
	(0.215)	(0.178)	(0.197)	(0.179)	(0.204)
Constant	**-13.037**	**-6.149**	**-12.264**	**-8.273**	**-6.700**
	(2.387)	(1.092)	(2.191)	(1.151)	(1.105)
pseudo R2	0.1246	0.124	0.1345	0.138	0.140
observations	2,019,318	2,004,305	1,988,039	1,972,721	1,959,635
Assault year	1998	1999	2000	2001	2002

Note: Logit regression of assault on variables specified above. Significant (95%) values are **bold faced**.

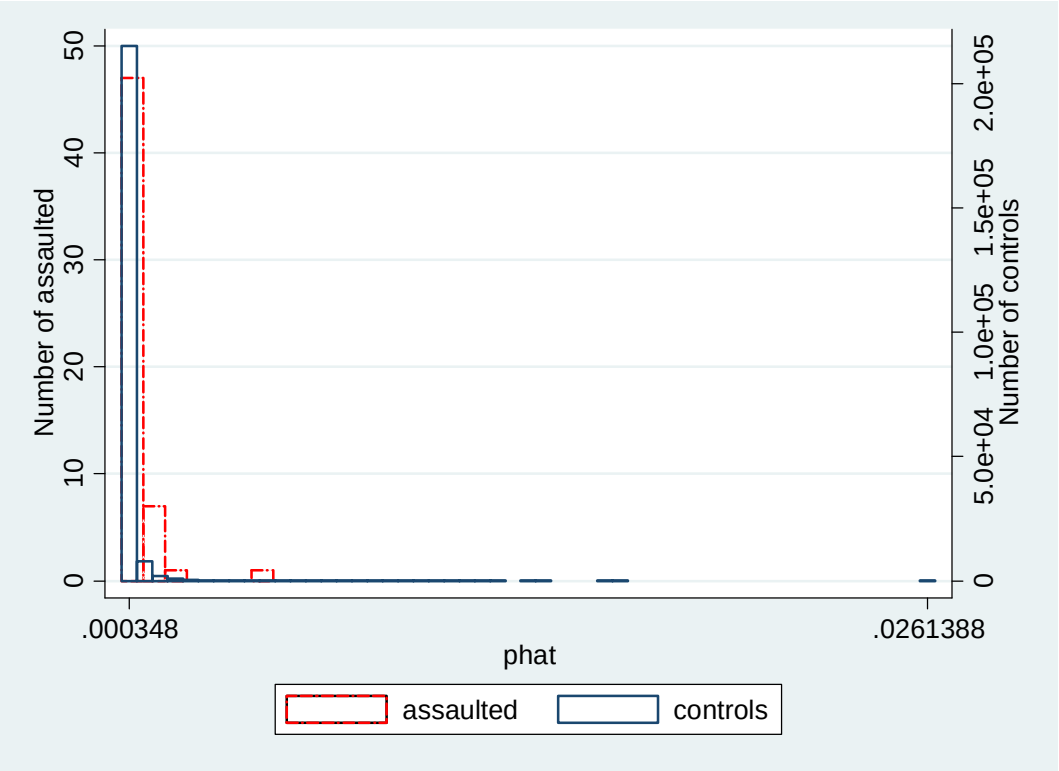
Table A2: Specification of propensity score estimation for men 1998-2002

VARIABLES	(1)	(2)	(3)	(4)	(5)
no. children <11 years	0.010	0.446	0.165	0.411	-0.018
	(0.249)	(0.257)	(0.265)	(0.237)	(0.276)
children^2	**0.076**	-0.012	0.016	**0.102**	0.060
	(0.030)	(0.048)	(0.046)	(0.027)	(0.045)
children age	-0.007	-0.010	-0.006	**-0.020**	-0.007
	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)
married	**-0.429**	**-0.354**	**-0.652**	-0.180	-0.216
	(0.124)	(0.123)	(0.128)	(0.125)	(0.123)
single, any last 4 years	**0.348**	-0.032	**0.452**	**0.499**	**0.386**
	(0.140)	(0.154)	(0.133)	(0.137)	(0.136)
single, 1 year prior	0.200	**0.664**	0.153	0.263	0.192
	(0.141)	(0.157)	(0.133)	(0.136)	(0.137)
age	**-0.031**	**-0.034**	**-0.038**	**-0.040**	**-0.046**
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
separated during last 2 years	**0.412**	0.048	0.251	0.278	0.008
	(0.157)	(0.168)	(0.158)	(0.167)	(0.187)
(age below 30)*(separated last 2 years)	**-0.387**	-0.315	**-0.551**	**-0.508**	**-0.439**
	(0.185)	(0.193)	(0.184)	(0.189)	(0.213)
years of schooling	**0.447**	**0.479**	0.124	**0.346**	**0.250**
	(0.216)	(0.161)	(0.205)	(0.121)	(0.115)
(years of schooling)^2	**-0.025**	**-0.028**	-0.012	**-0.019**	**-0.017**
	(0.010)	(0.008)	(0.009)	(0.005)	(0.005)
Swedish with at least 1 Swedish parent	**-0.577**	**-0.581**	**-0.570**	**-0.389**	**-0.459**
	(0.071)	(0.070)	(0.069)	(0.073)	(0.073)
any hospital visit resulting in a psychiatric diagnose, since 1987	-0.576	**-0.906**	**-0.980**	-0.533	**-1.291**
	(0.353)	(0.334)	(0.322)	(0.318)	(0.345)
psychiatric diagnose, last 4 years	-0.345	**-0.568**	**-0.602**	-0.307	**-0.889**
	(0.289)	(0.246)	(0.247)	(0.263)	(0.272)
psychiatric diagnose, 1 year prior	0.376	-0.173	-0.134	0.222	0.285
	(0.203)	(0.201)	(0.203)	(0.209)	(0.221)
psychiatric diagnose, 2 years prior	**0.982**	**0.935**	**0.763**	**1.013**	**0.922**
	(0.220)	(0.217)	(0.218)	(0.226)	(0.243)
psychiatric diagnose, 3 years prior	**0.956**	0.226	**0.548**	**0.773**	**1.026**
	(0.220)	(0.217)	(0.223)	(0.231)	(0.251)
psychiatric diagnose, 4 years prior	**0.899**	**0.862**	**1.105**	**0.464**	**1.068**
	(0.219)	(0.222)	(0.230)	(0.231)	(0.255)
psychiatric diagnose, at least 2 occasions, since 1987	**-0.546**	0.299	0.039	-0.289	-0.309
	(0.262)	(0.245)	(0.266)	(0.277)	(0.307)
(psychiatric diagnose, since 1987)*(separated during last 2 years)	0.015	0.343	0.393	0.297	**0.522**
	(0.240)	(0.228)	(0.219)	(0.226)	(0.258)
(psychiatric diagnose, since 1987)*age	**0.024**	**0.025**	**0.025**	0.020	**0.040**
	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)
no. of hospital visits, since 1987	**-0.030**	-0.019	**-0.024**	**-0.029**	-0.020
	(0.014)	(0.011)	(0.012)	(0.012)	(0.011)
no. of hospital visits, last 4 years	**0.120**	**0.101**	**0.164**	**0.189**	**0.168**
	(0.022)	(0.021)	(0.021)	(0.026)	(0.028)
no. of hospital visits, 1 year prior	-0.033	0.026	**-0.128**	**-0.128**	**-0.200**
	(0.042)	(0.036)	(0.049)	(0.045)	(0.067)
categorical variable over the	**0.394**	**0.369**	**0.341**	**0.439**	**0.331**

number of hospital visits	(0.053)	(0.049)	(0.048)	(0.048)	(0.050)
any hospital visit, since 1987	**-1.264**	**-1.220**	**-1.384**	**-1.821**	**-1.961**
	(0.111)	(0.107)	(0.104)	(0.109)	(0.109)
any hospital visit, 1 year prior	0.040	-0.034	0.129	-0.071	**0.435**
	(0.136)	(0.127)	(0.132)	(0.141)	(0.140)
any hospital visit, 2 years prior	**-1.185**	**-1.132**	**-1.111**	**-1.390**	**-1.242**
	(0.151)	(0.141)	(0.141)	(0.154)	(0.158)
any hospital visit, 3 years prior	**-1.255**	**-0.896**	**-1.029**	**-1.321**	**-1.353**
	(0.153)	(0.136)	(0.141)	(0.157)	(0.165)
any hospital visit, 4 years prior	**-1.117**	**-1.427**	**-1.554**	**-1.090**	**-1.440**
	(0.146)	(0.149)	(0.154)	(0.147)	(0.167)
(psychiatric diagnose, since 1987)*(any hospital visit, last 4 years)	**0.766**	**1.385**	**1.268**	**1.024**	**1.216**
	(0.267)	(0.226)	(0.216)	(0.229)	(0.224)
any sick leave with duration at least 15 days, last 4 years	**0.313**	**0.331**	**0.366**	**0.365**	**0.408**
	(0.076)	(0.075)	(0.074)	(0.076)	(0.074)
any disability insurance, last 4 years	**-0.361**	**-0.570**	**-0.559**	**-0.708**	**-0.581**
	(0.142)	(0.143)	(0.146)	(0.152)	(0.146)
total sickness insurance uptake corresponding to at least part time, last 4 years	-0.000	-0.000	**-0.000**	**-0.000**	**-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
mean income, last 4 years	**-0.000**	**-0.000**	**-0.000**	**-0.000**	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
3:rd income quartile, 1 year prior	-0.014	-0.025	-0.200	-0.123	**-0.334**
	(0.169)	(0.162)	(0.163)	(0.167)	(0.148)
(income, last 4 years)*(mental problem, since 1987)	-0.000	0.000	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
working, last 4 years	-0.285	**-0.349**	-0.227	-0.096	-0.263
	(0.157)	(0.155)	(0.150)	(0.161)	(0.155)
working 1 year prior	**-0.545**	-0.228	**-0.461**	**-0.323**	**-0.419**
	(0.132)	(0.123)	(0.124)	(0.124)	(0.130)
working 2 years prior	0.059	**-0.230**	-0.015	-0.145	**-0.226**
	(0.105)	(0.104)	(0.097)	(0.096)	(0.097)
working 3 years prior	-0.140	-0.089	0.077	-0.110	0.022
	(0.101)	(0.104)	(0.100)	(0.099)	(0.097)
working 4 years prior	0.034	-0.030	-0.068	**-0.337**	**-0.249**
	(0.102)	(0.101)	(0.105)	(0.118)	(0.110)
working*(age below 30)	0.240	0.215	0.288	0.253	0.277
	(0.123)	(0.118)	(0.112)	(0.114)	(0.115)
employed but not working, 1 year prior	**-0.457**	-0.083	**-0.239**	-0.213	-0.217
	(0.110)	(0.111)	(0.112)	(0.116)	(0.118)
employed but not working, any of last 4 years	0.054	-0.122	-0.109	0.013	-0.041
	(0.092)	(0.093)	(0.092)	(0.094)	(0.099)
Constant	**-6.207**	**-6.032**	**-3.695**	**-5.342**	**-4.041**
	(1.208)	(0.869)	(1.142)	(0.715)	(0.668)
pseudo R^2	0.151	0.153	0.161	0.168	0.168
observations	2,089,181	2,071,204	2,053,839	2,037,438	2,024,016
Assault year	1998	1999	2000	2001	2002

Note: Logit regression of assault on variables specified above. Significant (95%) values are **bold faced**.

Figure A2: Overlap graph



Note: The right tail of the propensity score distribution of assaulted and potential matches. The sample includes all working women with of age 20-54 in 1998.

Table A3: Propensity score of assaulted without matches

Assault year	Women, not working	Women, working	Men, not working	Men, working
1998				
1999				0.064
2000			0.411	
2001	0.096			0.279
2002	0.606			

Note: A caliper is set to 0.00045.