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The price of violence: Consequences of violent crime in Sweden

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The price of violence:
Consequences of violent crime in Sweden[a]

by

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Abstract

The connection between violence victimization and long term ill-health is well documented, but evidence is lacking on the causal effects of victimization beyond the time of the immediate injury. The aim of this study is to identify and estimate the longer term consequences of interpersonal violence on victims. Using rich administrative population data for Sweden, I compare individuals who visited a hospital in the years 1998 to 2002 due to assault with individuals who did not experience assault, but who were statistically indistinguishable from the cases of interest in the four years prior to the incident. The results suggest that violent crime has large and persistent effects on mortality, suicide, earnings, work status, disposable income, as well as on the number of days on sick leave. Specifically, an assault leading to a hospital visit is estimated to convey losses amounting to 1.4 million SEK per victimized woman and 1.5 million SEK per victimized man, whereof more than 80 percent result from excess mortality. Estimates on socio-economic outcomes show robustness against selection on unobserved characteristics. Estimates on mortality and suicides are very robust.

Keywords: suicide, mortality, domestic abuse, value of statistical life, violent crime
JEL-codes: I12, I14, J12, J17, K42

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1 Introduction

Victims of violent crime often sustain physical injuries or experience psychological distress. They are overrepresented among individuals with disabilities, depression, reproductive and physical health problems, high-risk sexual behavior, and alcohol and drug misuse (Campbell, 2002; Krogh et al., 2002; Garcia-Moreno et al., 2006). It is unclear, however, to which extent violence victimization leads to negative outcomes further in the future. In fact, there exists very little knowledge about the consequences of violence victimization beyond that directly attributable to the immediate injuries and psychological responses. This in turn limits our ability to assess the economic and social impact of policy directed at reducing violence. This study attempts to fill this gap by estimating the long-term effects of interpersonal violence on victims.

The present study addresses a number of limitations in the existing literature. First, most previous studies on the relationship between exposure to violence and implications for victims do not attempt to establish a causal relationship through either accounting for omitted variable bias or reverse causality. Second, the few studies that actually do consider endogeneity problems (Ehrensaft et al, 2006; Kilpatrick et al, 1997; Lindhorst & Oxford, 2008) are based on adjusting for pre-assault characteristics in small samples, and have limited ability to control for systematic differences in trend. Third, these studies use specific assault characteristics, outcomes, or target populations, resulting in estimates that are difficult to generalize to broader contexts.

To overcome these shortcomings, I identify assault victims in Sweden using hospital visits recorded as being due to assault. The data source covers the entire Swedish population and provides objective information regarding the severity of injuries. The hospital records are linked with comprehensive Swedish administrative registers. This rich information is then utilized to construct statistical twins in terms of pre-assault health, family situation, productivity, and demographics. Looking at assaults taking place in the years 1998 to 2002, I am able to ensure the similarity of assaulted and controls during a period of four years prior to exposure, and follow up results during the consecutive 8 years after an assault. In cases such as this, where there exists a broad set of covariates corresponding to several years of the individual's pre-exposure history, propensity score matching has previously worked especially well (see Deheija & Waba, 1999).

The choice of data source limits the study to violence which has resulted in the victim seeking hospital care, i.e. violence which the victim has experienced as being severe in some sense, may it be due to the physical injury or psychological trauma. In these cases, the physician has recorded "assault" in a specific column intended to provide information on the cause of injury, in addition to recording the medical symptoms. While some of these individuals have sustained no physical injuries at all, others have suffered a serious brain injury. A typical victim will, however, have had a concussion.

Prior to victimization, victims are shown to differ significantly from the general population on a broad range of characteristics, spanning work related measures, family structure, social insurance uptake, and health indicators. The full range and magnitude of differences between victims of violence and the general population cannot be attributed to the violence. The results in this study have implications for the interpretation of the results in previous studies linking violence victimization to health problems; they indicate the importance of a proper identification strategy for estimation of the causal link. In the rest of the study, to remove observable pre-assault differences, the victims of assault are compared with controls that are their statistical twins.

The results indicate that interpersonal violence severely reduces life expectancy. During the follow-up period of 8 years, the estimated cumulative effect of assault on mortality exceeds four percentage points for both men and women, a magnitude higher than the baseline death rate of half a percent. In traffic investment calculations, the value of a life in Sweden is monetized using the average individual's willingness to pay to increase her own safety. Using this measure, I calculate the cost due to increased mortality to 1,126,000 SEK per assault for women, and 1,332,000 SEK per assault for men. In terms of the capacity to work, victimization is found to result in lost productivity among survivors corresponding to 264,000 SEK for women, and 215,000 SEK for men, measured through cumulative gross earnings during the 8 year follow-up period. Total costs per assault to the victim thus sum to 1.4 million SEK for women, and 1.5 million SEK for men. Effects of violence exposure on the probability to work amounts to an 11 percentage point reduction for women, and a 7 percentage point reduction for men.

The results are separated by the sex of the victim, due to strong indications pointing to men and women experiencing different types of victimization. Men and women do, however, seem to respond somewhat similarly to violent trauma, with effect sizes of the

same order on all outcomes. Looking at heterogeneity by injury severity, effects are larger for individuals with moderate to major injuries. The average effects are, however, not far from those exhibited by those with less severe physical injuries.

Posttraumatic stress disorder (PTSD) has been thought to provide the link between violence victimization and somatic problems (Dutton et al., 2006). Two outcomes in the current study are selected specifically to address this hypothesis. Effects of assault on suicide risk sum to 0.6 percentage points for women, and 0.7 percentage points for men. Effects on suicides account for 14 percent of the effects on mortality. The large effects of violence victimization on suicide lend support to the theory that the results on mortality are partly driven by a causal link between the trauma of violence exposure and psychiatric illness. Similarly, part of the reduction in earnings and probability of working run through increases in sick leave uptake; these amount to 31 excess days annually for women, and 15 for men.

The effects are highly persistent over time. In fact, at the end of the observed eight year post-assault period, none of the subgroups followed appear to be free of the effects of assault on any of the outcomes studied. In many victims of traumatic events, PTSD persists over time, sometimes becoming chronic (Kessler et al., 1995). This might be one reason to why effect sizes do not diminish over time. Another potential explanation is the fact that the outcomes of interest in this study – especially mortality – are quite crude measures of health. This requires ill-health to have accumulated before they manifest. For earnings and work status, growth in the absolute size of the effect over time can be attributed to the combination of time-persistent relative effects and a general increase in counterfactual earnings as the population ages.

The development of the effects over time is also consistent with lacking effectiveness of the matching strategy. If the matched individuals differ systematically from those who are assaulted, the difference in life indicators between the assaulted individuals and their matches could be expected to increase in subsequent years irrespective of the assault. To make such an explanation less plausible, the matching has been constructed to ensure that controls are similar to assaulted in both levels and trend in terms of both external context and individual resources. In addition to this, a sensitivity analysis has been performed. To be able to see whether the effects are sensitive to selection bias due to correlation between unobserved factors and assault, I have calculated bounds for the amount of unobserved

selection bias that would be sufficient to overturn the results. The sensitivity analysis indicates that the effects on mortality and suicides are not sensitive to remaining selection bias. Assaulted women would need to have underlying unobserved characteristics making them 12 times more likely to die than controls for results on mortality to be spurious; assaulted men would need to be more than 9 times more likely to die. Looking at the sensitivity of the outcomes related to the capacity to work – earnings, work status, disposable income, and sick leave – the results are robust as well. The robustness regarding these outcomes is, however, more moderate, and these results could all be explained by an unobserved factor that would make the assaulted have a true assault risk 80 percent higher than that of their matched controls. Note that the reassuring results from the sensitivity analysis do not imply robustness of effect sizes. It is quite possible that some of the effects attributed to assault could instead be attributed to selection on unobserved characteristics. The results reported here should, thus, rather than be taken conclusively, be interpreted as an argument for the need for more research in this area.

This study has implications for policy decisions regarding public and domestic safety, specifically regarding the expected cost-effectiveness of potential reforms aiming at reducing violent crime. When comparing the results from this study with previous ones on the topic of costs of assault, it is evident that a majority of the consequences are indirect costs specifically befalling the victim of violence, rather than direct costs incurred by health care institutions or the legal system. While several previous cost calculations have tried to include consequences to the victim, this is a challenging task lacking a reliable identification strategy (see e.g. Dubourg, 2005; NCIPC, 2003; de Vylder, 2010). The Swedish National Board of Health and Welfare has previously estimated the costs of assault to the Swedish society to between 40 000 and 50 000 SEK per victimized woman (Socialstyrelsen, 2006). Those figures correspond to a mere three percent of the estimates calculated in this study. The definitions of assault as well as target groups differ between the reports, limiting comparability, but the contrast in magnitude is, none the less, striking. Socialstyrelsen (ibid.) explicitly disregards costs incurred due to long-term reduced health outcomes of victims, as these are not manifestly attributable to violence victimization. It is precisely these consequences that I have tried to capture here, and the results indicate that they form a key component for future cost calculations of violent crime.

The paper is laid out as follows. I first review the previous literature linking violence victimization to ill-health. Section 3 introduces the challenges to evaluation and the empirical strategy. Next the data is introduced, including descriptive statistics. Section 5 presents estimation of the propensity score, the quality of matches, and inference. The next section presents results on, first, premature death, and second, on the capacity to work. This is followed by a sensitivity analysis. The paper ends with a summary of the results.

2 Background

2.1 The demographics of violence victimization

Over the very long-term, interpersonal violence is declining (Pinker, 2011). During the shorter span of the last few decades however, OECD countries have exhibited stable levels (Estrada, 2006; Wittebrood & Junger, 2002). About 4 percent of the adult population in OECD countries report experiencing physical violence during a year (Van Wilsem, 2004). At face value, levels in Sweden are average with 7 percent of women and 5 percent of men reporting victimization to either physical or sexual violence in the last year (NCK, 2014). 46 percent of Swedish women and 38 percent of Swedish men report having ever been the victim of violence victimization (NCK, 2014). One in five women report being raped in their lifetime in North America (Breiding, 2014; Johnson, 1996), as well as in Sweden (NCK, 2014; Lundgren et al., 2001).[1]

Victimization rates are heterogeneous within countries. Teenagers and young adults report especially high rates of violence (Häll, 2004; NCK, 2014; Lundgren et al., 2001). A lack of economic resources is another key risk factor (Andrews et al., 2000, Ehrensaft et al., 2004; Ehrensaft et al., 2003; Stickley & Carlson, 2010), partly due to a causal effect of resource scarcity on violence exposure (Aizer, 2010). Another prominent pattern is the high adult victimization rates among former victims of child abuse (Lundgren et al., 2001; NCK, 2014).

Women and men have similar victimization rates, but the characteristics of violence differ greatly by gender. While women typically are victims of sexual assault or assault

[1] The level of violence is sensitive to questionnaire methodology. This is evident not least from the differences in levels and trends between different data sources (Aaltonen m.fl., 2012). Sexual violence and intimate partner violence – i.e. types of violence with predominantly female victims – seem to be especially sensitive to question formulation (Walby & Myhill, 2001), as well as directly undersampled by wordings specifically referencing physical violence (see e.g. the Swedish living conditions survey: Häll, 2004).

by current or previous sexual partner, men are generally physically assaulted outside their homes (SCB, 2011). Because of this, the sex of the victim is a good indicator of context and type of a violent event.

2.2 Linking violence exposure to ill-health

There is a large literature documenting a strong relation between a history of violence victimization and both psychiatric and somatic symptoms in women, with few studies including male victims. The extent to which the results in this section are generalizable to male victims of violence is discussed in section 2.3.

Two common *psychiatric* diagnoses in victims of violence is depression and post-traumatic stress syndrome, PTSD (Golding, 1999). Other psychiatric problems associated with violence exposure are psychotic symptoms, suicide attempts, sleeping and eating disorders, substance abuse, and social dysfunction (see e.g. Humphreys & Lee, 2005; Kaslow et al., 2002; Neria et al., 2005; Olshen et al., 2007). *Somatic* symptoms are largely gynecological and/or pain related (Campbell, 2002; McCauley et al., 1995). Previous experiences of violence are also associated with ill-health in general. Victims of violence more frequently visit healthcare, exhibit excess risk for serious and chronic illness, and spend an increased number of days in bed (Campbell, 2002; Campbell & Lewandowski, 1997; Taft, et al., 2007).

There are some results supporting a causal effect of violent crime on the victim's mental health. Kilpatrick et al. (1997) test competing hypotheses about the direction of relationships between assault and substance abuse in women. 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 about the link from domestic violence to depressive symptoms in adolescent mothers. These three studies all come to the conclusion that violence victimization increase psychiatric morbidity in (young) women. They are the most convincing in the field, using a before versus after comparison on several waves of self-report data. However, their small sample sizes preclude a rich set of controls. With a before-after design, in case there are other factors than the lagged outcome that affect both health and assault-risk, estimates are likely to be biased. In addition, while the choices of specific outcomes, assault characteristics, or target population are reasonable for the studies in themselves, they limit their usefulness

as basis for policy decisions. To the best of my knowledge, there exist no other high-quality studies of long-term consequences of assault on victims.

Related findings supporting a causal link from violence to psychiatric ill-health include the timing of symptoms and the effectiveness of treatment. The extremely high rates of depression exhibited by battered women have been found to decline over time once the abuse has ceased (Golding, 1999). The currently most effective treatment strategy for patients suffering from trauma related stress is to help the patient desensitize the traumatic event, so that it loses its stressful connotations (Powers et al., 2010; SBU, 2005). That a focus on the patient's relation to a previous traumatic event has been shown to be effective strengthens the claim that the event caused the psychological distress.

Few studies investigate the potential causal link in the opposite direction, from mental health to victimization risk. An exception is Kilpatrick et al. (1997), who report a positive effect of substance abuse on subsequent violence victimization.

2.3 The role of psychological trauma

Posttraumatic stress disorder (PTSD) has been hypothesized as the key mechanism linking violence victimization to somatic problems (Dutton et al., 2006). It is the psychological trauma caused by the violent event that is thought to generate consequences beyond immediate physical effects. PTSD is diagnosed when an individual, following exposure to a serious traumatic event, exhibit intense fear, helplessness, or horror (SBU, 2005; Herman, 1997). Symptoms include re-experiencing the traumatic event, and results in avoidance and hyperarousal (Brewin et al., 2003). The mediating effect of PTSD is thought to act through affecting the body's response to stress hormones (Dutton et al., 2006). The symptoms show a high level of persistence over time, with indications of a third of cases becoming chronic (Kessler et al., 1995).

Women develop PTSD in response to physical or sexual assault at about the same rate. About 30 percent of rape survivors report having ever experienced PTSD (Resnick et al., 1993; Nck, 2014), and about one in ten report ongoing PTSD symptoms (Kilpatrick & Acierno, 2003). Men rarely report PTSD in response to physical assault, but show the same elevation of both psychiatric and somatic symptoms as women in response to sexual

violence.[2] The most common type of violence afflicting adult males is physical violence perpetrated by a stranger. Among this group of violence victims the association with ill-health is weak (NCK, 2014). The most common precipitant of PTSD in North American men is instead combat experience and witnessed violence (Kilpatrick & Acierno, 2003).

Why does physical violence seem traumatic to women to a much higher extent than to men? One way of dividing intimate partner violence that has shown to be useful is by the level of controlling behaviors that characterize the violence. It is specifically domestic violence characterized by high levels of control that have been found to have a high propensity to be experienced as traumatic (Johnson, 2010). Men's victimization tends to have much shorter duration, as they are to a higher extent physically assaulted by acquaintances and strangers. An interpretation of this pattern of results is that psychological trauma may follow from a severe sense of loss of control, and the main categories of violence that induce this experience and take place in peaceful democracies are sexual violence and (controlling) domestic violence. Differences in PTSD across gender may thus reflect differences in type of violence and relationship to the perpetrator.

3 The identification problem

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_{i1} or Y_{i0}, 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

[2]There is an ongoing debate about to what extent sex differences in self-reported health are driven by more objective differences in well-being. For a discussion on differences in men's and women's reports of subjective health, see e.g. Paxon & Case (2004).

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 (see e.g. Heckman, 2005). For feasibility, focus is on aggregate effects. Specifically I want to estimate the impact of assault on those who have been assaulted, the average treatment effect on the treated (ATT).

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

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,

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

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.

3.1 Matching

Assuming that selection occurs on only observed characteristics, I 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 dimensions, i.e. 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 $\mathrm{p}(\mathrm{X_i})$ 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 $\mathrm{Y_0}$ 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). 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.

3.1.1 Risk set matching

When introducing time into the matching strategy, it is important to avoid using future information in the selection of either the assaulted or the match. This implies that controls must not be selected from the pool of never-treated, as "never" is a statement about the future. I want to base the selection of matches exclusively on information up until the moment before an assaulted individual was victimized. For this purpose I employ a generalization of propensity score matching called risk set matching (Li et al. 2001).[3] The strategy is to redefine the counterfactual expectation of interest to the outcome of victimized at time *t*, had they not been victimized at or up to *t*. It is constructed as

$$E(Y_{i0}|p(X_{it}), D_{it} = 1) = E\big(Y_{i0}\big|p(X_{it})\big) = E(Y_{i0}|p(X_{it}), D_{it} = 0).$$

D_{it} is an indicator for assaulted *i* at *t* and $p(X_{it})$ 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 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 period separately. I define the dependent variable as

[3] An analogous estimator has been introduced to economics by Sianesi (2004).

whether individual *i* is assaulted or not in year *t*. The vector of covariates, X_{it}, is measured up to but not including year *t*, and includes time varying characteristics. The time index of β_t signifies that the impact of X_{it} is allowed to vary between years.

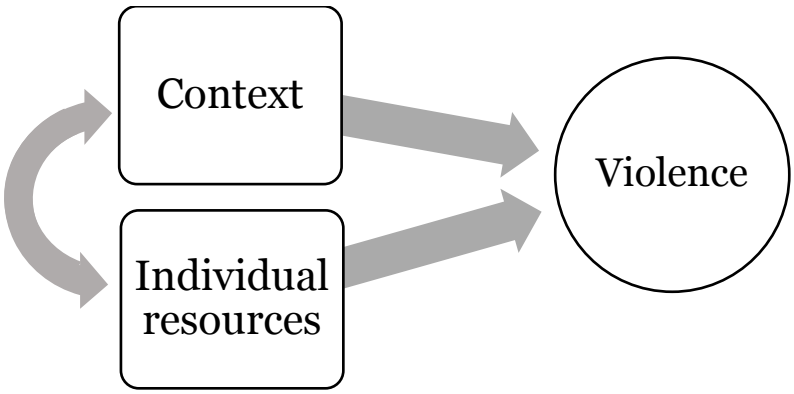
$$\mathrm{p(X_{it})} = Pr(\mathrm{D_{it}} = 1|\mathrm{D_{it-1}} = 0, X_{it}) = \frac{1}{1 + e^{\beta_t X_{it}}}$$

An individual who is assaulted in year *t* can enter the study in one of two ways: as an assaulted case in year t or as a not-yet-assaulted control for an assaulted in a year strictly prior to t. Viewed as an estimator of the standard ATT, a risk set estimator is biased towards zero, with the ATT as an upper bound.[4]

3.2 Modeling victimization risk

I model violence as a function of on one hand the individual's external context, and on the other hand individual resources influencing the ability and willingness to amend risk. While this is an oversimplification, it gives structure for categorizing indicators of risk. Here, I highlight key areas of differential risk of the two sorts. In section 5 I operationalize the model using Swedish administrative data, and in section 6.3 I discuss potential shortcomings in capturing the true assault risk.

Figure 1 Theoretical model of violence victimization risk factors



[4] The unbiased interpretation of the risk set estimator of the assault impact is therefore as the *effect on the assaulted of assault now instead of possibly later*. As the control group for risk set estimators is at risk of assault during the follow-up period, the outcome of the control group is a weighted average of the outcome under no assault for the assaulted and outcomes depending on the effects of assault. For a formal explanation, see Vikström (2014, p. 8). The size of the problem depends on the likelihood of assault among controls. In the final sample, 7 of the female controls and 30 of the male controls were assaulted during the study period, i.e. would have been defined as cases had the matching strategy conditioned on future information.

Both work and family are important risk differentials in serving the role of protective contexts for the majority of people, while presenting risk increases for some. The workplace presents risk of interpersonal violence primarily to occupations with close client contact (Arbetsmiljöverket, 2010; Estrada et al., 2010). Studies on domestic violence have identified the time following a separation as a time of increased risk for women (e.g. Daly & Wilson, 1988; Ornstein & Rickne, 2013). For men, cohabitation seems to function as a protecting factor, decreasing their risk of violence exposure outside of the home (Häll, 2004). In this case, the family might be better thought of as affecting the individual's interest in risk behaviors, rather than as a site for exposure in itself, indicating the need for an arrow from "Context" to "Individual resources" in Figure 1. This in addition underscores the importance of sex specific models of exposure. Age, as well as the existence of children, are as well likely to serve roles in shaping the individual's context. As the model regards Sweden, the context of the neighborhood has been found to play a negligible role in affecting the exposure to violence leading to hospitalizations (Nilsson & Estrada, 2007).

Health is an individual resource, and disease may reduce an individual's ability to engage in risk avoidance behaviors. Specifically, severe mental illness stand out as inflicting important impediments to risk avoidance (Goodman et al., 2001). In addition, disease makes the individual more dependent on others, affecting her risk of violence exposure through shaping her context, indicating the need for an arrow from "Individual resources" to "Context" in Figure 1. A lack of financial resources restricts the risk avoidance behaviors an individual can partake in, increasing risk of exposure (Nilsson & Estrada, 2007). The earnings potential of a woman relative her partner has been found to increase her ability to leave an abusive relationship (Aizer, 2010). Financial resources, as well as access to resources through education and social network, likely increase an individual's general ability to exit problematic situations. Personality as well as cognitive and non-cognitive ability are important parts of an individual's resources when it comes to handling risk and are difficult to capture fully through administrative data. However, several typically observed factors such as education, employment, income, and civil status can be viewed as proxies for general ability, while hospital records containing information on psychiatric problems indicate lack of ability.

4 Data

The data contain information on the entire Swedish population aged 16 and above each year from 1987 to 2010. I integrate registers from Statistics Sweden (with information on birth year, sex, education, income, disposable income, and work status, and including links between children and their parents), with registers from the Social Insurance Agency (with information on sickness absence and disability benefit uptake), and the National Board of Health and Welfare (which is responsible for the National Inpatient Care Register covering all hospital visits since 1987, including both hospitalizations and emergency department visits, with information on medical diagnose and cause of injury codes, as well as the Cause of Death Register).

To increase the number of sampled assaults while ensuring access to several years of high-quality data both before and after the event, the study is restricted to violence occurring in the five-year window from 1998 to 2002. The sample consists of all individuals who were residents in Sweden and 20-54 years old at least one of these years. The time restriction allows the selection of covariates four years prior to, and up to eight years following, an 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 in a Swedish hospital for injuries due to assault.[5] The final sample includes 1536 victimized women, 5345 victimized men, and about 2 million non-assaulted of each sex. [6]

4.1 Violence

The focus in this study is on non-lethal interpersonal violence, physical force exercised between individuals. Throughout the report the terms violence victimization, violent crime and assault are used interchangeably; operationalized as hospital visits resulting in an external cause of injury code denoting assault. Research on both risk factors and consequences of violent victimization has previously relied mainly on either self-report surveys or police statistics (Wittebrood & Junger, 2002). Hospital records have been used in few studies on violent crime (for Swedish examples, see Estrada, 2006; Kühlhorn & Grevholm, 2007), but has several advantages. The panel structure of the data allows me

[5] After 1987 that is, before that there is no information on hospitalizations. This sample restriction excludes about 7 percent of both men and women assaulted during the study period.
[6] The exact number of controls differs between years.

to follow people for several years before a violent event. The large scale of the data allows for detailed questions concerning effect magnitudes, for the analysis of important but rare outcomes such as suicides, as well as ensures that relevant comparison individuals exist even after a particularly rich matching specification. While response rates are likely to be low in groups with the highest risks of victimization, assaults leading to severe enough physical injury are almost certain to entail a hospital visit. Studies comparing different data sources covering the victimized population conclude that the choice of data source is vital to the violence that is captured, and that different sources are most in agreement concerning violent acts with severe physical consequences (Aaltonen et al., 2012; Walby & Myhill, 2001).

In the case that an injury leads up to a hospital admission, the external cause of the injury is noted by the physician. Violence victimization is defined as having made a hospital visit denoting *assault* as the external cause. Assault is defined as "injuries inflicted by another person with intent to injure or kill, by any means" (WHO, 2016). The identification of violence is thus contingent on a hospital visit, e.g. at an emergency department, followed by the patient's revelation that the cause of injury was assault, and the physician's recording of assault as the cause of injury. The choice of measurement thus minimizes the risk of inclusion of events which do not meet the criteria of violent crime. While it is specifically violence with physical injuries that is systematically captured, leading to the under-representation of sexual violence, patients may seek hospital care and disclose of the cause of their distress for several reasons, including psychological trauma or the intention to document evidence of assault.

The injury itself is described in the up to eight slots available for medical diagnoses. The diagnoses, made by physicians, are classified according to the World Health Organization's International Statistical Classification of Diseases and Related Health Problems, ICD[7]. Based on this qualitative medical data, I construct a quantitative measure of injury severity. The diagnoses are first classified in terms of injury specific severity. Specific injuries are then recoded first into local, body-part specific, injury severity measures, and then into a global measure, the Injury Severity Score, ISS (Baker et al.,

[7] 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.

1974).[8] The ISS provides a numerical scale (from 1 to 75) corresponding to the overall severity of the patient's injuries in terms of the level of threat to life and tissue damage. An ISS score of 75 is considered non-survivable (Baker et al. 1974), and ISS scores from 16 and upwards are typically classified as major physical trauma. Table 1 gives an overview of the external cause of injury codes denoting assault as well as the diagnose codes relevant for the ISS classification.

Table 1 Coding systems defining assault status and injury severity

Trauma characteristic	Source	Classification system	Inclusion criteria
Assault	external cause of injury	ICD-10	X85 – Y05, Y08 – Y09
Injury severity (ISS)	diagnoses	ICD-10	S00 – T61

Note: The incidence of assault is defined exclusively from the *external cause* of injury code. Injury severity is defined specifically for the assaults, and only based on physical injuries, neither on psychological distress nor disease in general.

To give an understanding of the injuries, the main diagnoses in the sample are surveyed.[9] The majority of individuals analyzed did not suffer an injury classified as major (Figure 1).[10] Instead, 92 percent of women and 86 percent of men incurred injuries corresponding to ISS scores below 8 (mild injury). 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.[11] Among those with ISS scores of 8–15 (moderate injury), collapsed lungs[12] is the most common diagnose, followed closely by concussion, each inflicting around 20 percent of injured. Of the men and women with an

[8] First, the main injury in each of six body regions is classified by combining all recorded diagnoses (the main diagnose and the eight positions allowing to record "other conditions") and ranking their severity on an integer scale from 1-6 in terms of threat to life and tissue damage. This is called the Abbreviated Injury Scale (AIS). 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 ISS is calculated by summing the square of the three injuries with the highest severity in three different body regions. Translation from ICD diagnoses to ISS follows MacKenzie et al. (1989). The specific coding from ICD-10 to injury severity applied here was possible using a table generously provided by the ECIP (2006).

[9] Note that individuals with different ISS scores might have the same main diagnose, only different secondary diagnoses.

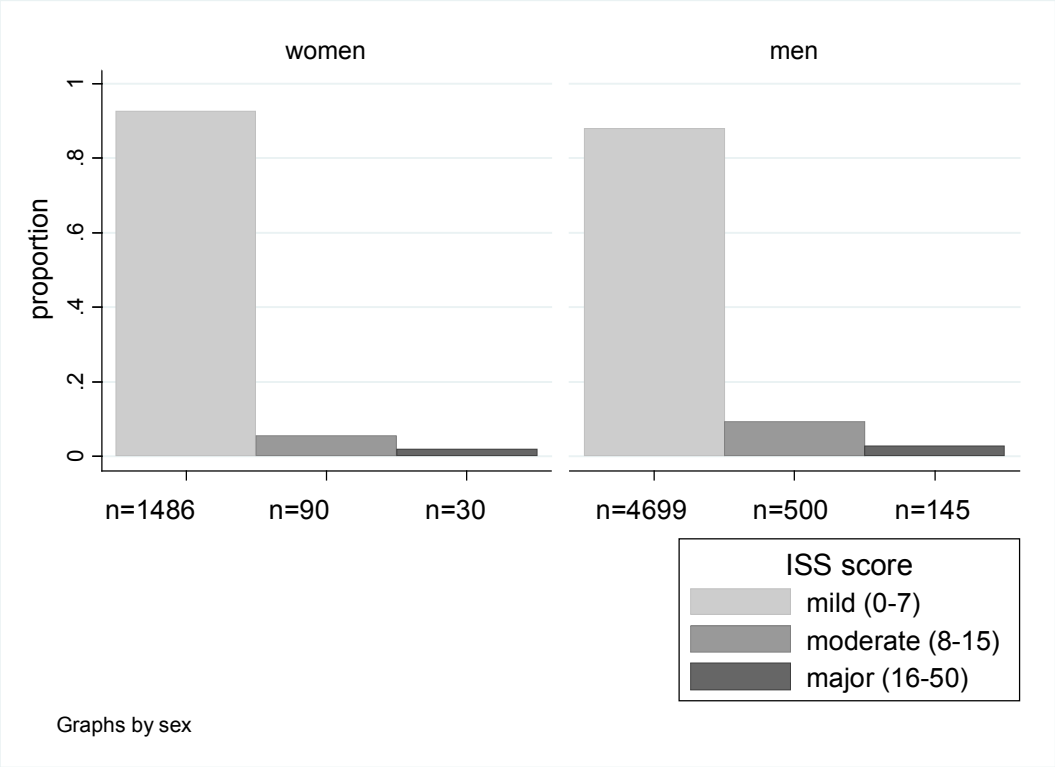
[10] The Abbreviated Injury Scale (AIS) describes injuries in each of six body regions on a 6-point numerical scale in terms of threat to life and tissue damage. An AIS 1 (minor) injury does not pose a threat to survival, whereas survival is highly uncertain in the case of an AIS 5 (critical) injury. 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. 1974), and ISS scores from 16 and upwards are typically classified as major trauma.

[11] Icd-code s06.0 denotes concussion; s02.3 fractured orbital floor; s02.4 fractured jaw bone.

[12] Icd-code s27.0

ISS score of 16 or higher (major injury) more than 70 percent had a serious brain injury as their main diagnose.[13]

Figure 2 Distribution of injury severity, by sex



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

4.2 Outcomes

To capture the long-term effects of violence victimization, two categories of outcomes are selected: mortality, and capacity to work. Year and cause of death for all registered in Sweden at the time of death are available until 2010. Premature loss of life is monetized based on the numbers used in Swedish traffic investment calculations. The value is estimated through the amount individuals state that they are willing to pay for risk reductions (Hultkrantz & Svensson, 2012). The Swedish transport administration recommends the value of a statistical life to be 24,000,000 SEK in 2014 (Trafikverket, 2014).[14] The value is recommended to be updated with changes in real earnings, as it is

[13] The most common main diagnoses in this group are s06.5 (subdural hemorrhage), s06.2 (diffuse brain injury), s06.4 (epidural hemorrhage), and s06.3 (focal brain injury).

[14] The monetization of smaller reductions in quality of life than death is less clear, and therefore not used in this study. A severe injury (resulting in some in-house hospital care) is recommended to count as 16.6% of a life, and a minor injury (needing only outpatient care) as 1%. There are no recommendations concerning the valuation of injuries with debilitating consequences.

dependent on the average individual's capacity to pay for risk reductions, and calculated to 25,152,240 SEK using year 2016 as base.[15]

The capacity to work is operationalized using individuals' gross earnings and probability to work. The measure *earnings* equals the value paid for an individual's labor, calculated by adding the payroll tax to her salary income. A somewhat cruder measure of the ability to work is the measure *work status*. 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 criterion are defined as not working, irrespective of whether they have an employer or not, or whether they are in the labor force of not.

To increase understanding of the consequences for crime victims, I look at some related outcomes. To estimate the extent to which the individual carries the financial consequences of her loss in earnings capacity, I look at effects on *disposable income*, defined as net earnings combined with net social benefits and transfers. To analyze the role played by psychological trauma in the path from non-lethal violence to premature death, differential mortality through *suicide* is looked at specifically.[16] For another way to capture morbidity, I count the *days on sick leave*, defined as the total number of days on either sickness insurance or disability benefits. All workers (employed and unemployed) are covered by public sickness insurance. For unemployed who report sick two or more days in a row, and employed who report sick beyond a two week period, the Social Insurance Agency is responsible for benefit payments.[17] Sick leave beyond 7 days requires a doctor's certificate. A persistent reduction in work capacity is reimbursed in the form of disability benefits.[18] The agency's records cover all disbursements and are available to me from 1990 to 2008.

4.3 Covariates

Key covariates in the matching specification are lagged versions of the outcomes earnings, work status, disposable income, and days on sick leave. These variables can all be viewed as both shaping the context the individual exists in, and simultaneously serving

[15] I use statistics on changes in nominal earnings (real earnings * inflation) from the Swedish National Mediation Office, and update the value given in 2014 with 104.801 percent to calculate the corresponding value with year 2016 as base.
[16] Suicide is denoted by values X60 – X84 in the ICD-10 coding system.
[17] To avoid effects of assault on work status to inflate effects on sick leave, I remove sick leave of less than two weeks during years when the individual has not been working. This however does not affect the results at all.
[18] *Disability benefits* are provided for sick leave due to a reduction in work capacity of at least 5 years. Disability benefits are always provided through the Social Insurance Agency and can be granted either fully or partially (25, 50, or 75 percent).

as indicators of individual resources. Although they do have a large overlap, they are likely to capture different aspects of the two themes. Thus, while earnings is a measure of individual productive capacity and thus clearly an indicator of resources, disposable income might be thought more of as a measure of an individual's context.

Other variables mainly capturing context are country of birth and family situation, such as whether the individual is married, has separated during the last four years, has a child, or lives alone with a child. Workplace characteristics are part of an individual's context as well. Violence at the workplace is comparatively common (Häll, 2004), and affects both men and women, typically in occupations with close client contact. To evaluate whether such events lead to hospitalizations and thus are part of the violence analyzed in this study, I use the Swedish Standard Classification of Occupations (SSYK) to construct a measure that captures risk of workplace victimization. *Risk occupation* is defined as an indicator variable, grouping those occupations most overrepresented in reporting workplace violence (Arbetsmiljöverket, 2010, p. 134): teacher, social worker, medical staff, police, and prison ward.

Indicators of resources are education and health. Education is operationalized as *years of schooling* by transforming the highest reported education into the average number of years needed to accomplish it. Health is a broad and important resource, and I construct several variables to capture different health dimensions: A *hospital visit* denotes any emergency or inpatient medical contact, excluding child delivery. A *psychiatric diagnose* is defined as having made a hospital visit resulting in a diagnose referring to a mental or behavioral disorder. It is further broken down into the following sub-categories: *substance abuse*, *psychosis*, *mood disorder*, *anxiety*, *personality disorder*. These categories are chosen to capture the majority of mental health problems, and specifically those that are likely to influence assault victimization risk through affecting risk-aversion and the ability to avoid dangerous situations. Table 2 gives the precise definitions of the mental health variables.

Table 2 Icd-codes defining psychiatric diagnoses

Variable	Inclusion criteria, Icd-9; –1997	Inclusion criteria, Icd-10; 1998 –
Psychiatric diagnose	290-319	F
Substance abuse	291, 292, 303-305	F10-F19
Psychosis	295, 297, 298	F20-F29
Mood disorder	296, 311	F30-F39
Anxiety	300	F40-F48
Personality disorder	301	F60

4.4 Descriptive statistics

Table 3 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 all 20–54 year old residents; variable values correspond to the year prior to the selection year.

Prior to the assault, assaulted differed from the reference population on almost every characteristic available in the data. Looking first at some general variables, we find that assaulted men have received one year less schooling than the average man, and that they were much less likely to work in the previous year. Of both men and women in the reference population, more than 75 percent were working. In assaulted, this was the case for only 37 percent of the women, and 50 percent of the men. Prior to the assault, assaulted had lower income than the reference population, and transfers comprised a larger share of their disposable income.

Analyzing the pre-assault proportions of individuals with an occupation that puts them at-risk for workplace violence shows that these individuals are not particularly overrepresented[19]. This indicates that such client related assaults seldom result in inpatient care. Since workplace violence is the only common type of victimization which typically affects both women and men, I conclude that the gender separation of the analysis to a large extent defines the characteristics of the violent events analyzed.

[19] Here they are *underrepresented*, as assaulted are less likely to work at all than non-assaulted. Looking specifically at those working (results not shown) risk occupation is however still not an overrepresented trait.

Table 3 Pre-assault characteristics of cases and a random sample from the population

	Women		Men	
	Unselected sample	Assaulted	Unselected sample	Assaulted
Age	37.5	35.8***	37.4	32.6***
	(0.014)	(0.248)	(0.014)	(0.135)
Years of schooling	12.3	11.1***	12.1	11.1***
	(0.003)	(0.053)	(0.003)	(0.025)
Working (%)	0.738	0.373***	0.781	0.499***
	(0.001)	(0.012)	(0.001)	(0.007)
Earnings	114,700	47,700***	170,000	72,700***
(mean last 4 years, SEK)	(133)	(1,689)	(222)	(1,220)
Disposable income (mean	122,000	107,400***	148,100	93,221***
last 4 years, SEK)	(124)	(1,366)	(287)	(983)
Risk occupation (%)	0.351	0.215***	0.076	0.050***
	(0.001)	(0.010)	(0.000)	(0.003)
Married (%)	0.430	0.228***	0.367	0.113***
	(0.001)	(0.010)	(0.001)	(0.004)
Newly separated (%)	0.077	0.144***	0.080	0.141***
	(0.000)	(0.009)	(0.000)	(0.005)
Child <7	0.313	0.295	0.265	0.122***
	(0.001)	(0.015)	(0.001)	
Single parent (%)	0.108	0.255***	0.024	0.042***
	(0.000)	(0.011)	(0.000)	(0.003)
In-care patient	0.614	0.819***	0.350	0.578***
(since 1987, %)	(0.001)	(0.010)	(0.001)	(0.007)
Psychiatric diagnose	0.036	0.324***	0.039	0.219***
(since 1987, %)	(0.000)	(0.012)	(0.000)	(0.006)
Substance abuse	0.015	0.231***	0.024	0.188***
(since 1987, %)	(0.000)	(0.011)	(0.000)	(0.005)
Psychosis	0.007	0.034***	0.007	0.022***
(since 1987, %)	(0.000)	(0.005)	(0.000)	(0.002)
Mood disorder	0.007	0.063***	0.005	0.026***
(since 1987, %)	(0.000)	(0.006)	(0.000)	(0.002)
Anxiety	0.010	0.122***	0.006	0.038***
(since 1987, %)	(0.000)	(0.008)	(0.000)	(0.003)
Personality disorder	0.004	0.065***	0.003	0.026***
(since 1987, %)	(0.000)	(0.006)	(0.000)	(0.002)
Days on sickness insurance	13.532	32.4***	7.88	16.9***
(mean, last 4 years)	(0.059)	(1.70)	(0.046)	(0.658)
Any Disablity benefits	0.048	0.160***	0.034	0.078***
(last 4 years, %)	(0.000)	(0.009)	(0.000)	(0.004)
Days on sick leave	27.055	78.9***	17.7	39.3***
(mean, last 4 years)	(0.115)	(3.30)	(0.095)	(1.30)
No. of 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. Earnings and disposable income in year 2016's values.

Moving to 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. 23 percent of assaulted women were married, compared to 43 percent of women

in the reference group. Among assaulted men, only 11 percent were married, compared to 37 percent in the reference group. Instead the assaulted 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, and still they, just as assaulted women, were twice as likely to be a single parent. However, single parenthood is a much more common characteristic among the assaulted women, as single parenting is more common overall in women than in men.

Turning to pre-assault health indicators, assaulted are more likely to have received in-patient care of any sorts, indicating a higher proportion with serious health problems. The most prominent difference however is the percentage of the population with a history involving hospitalization for a mental health problem, a strong sign of psychosocial morbidity. Less than 4 percent of the general population but one in three assaulted women and one in five assaulted men met these criteria of a mental health problem. Looking at more specific diagnose characteristics, the prevalence of substance abuse among assaulted stands out. 23 percent of assaulted women and 19 percent of assaulted men have received a diagnose denoting a substance abuse problem, compared to 1.5 percent and 2.4 percent among women and men in the reference population. Among women anxiety stands out as well, afflicting 12 percent of the assaulted. Focusing on sick leave, the results again indicate that assaulted have worse general health than the reference population. Prior to being assaulted, assaulted received sickness insurance on average twice as often as the reference population, and were more than twice as likely to receive disability benefits during the years prior to the assault.

5 Estimation

The exposure model is based on theoretical arguments and previous empirical research argued to be key to covariate selection (see, e.g. Smith & Todd, 2005). While failure to include covariates that influence both risk of assault and either mortality or productivity will bias the results, inclusion of irrelevant variables inflates the variance. There are indications that data driven covariate selection techniques might do better than theoretically based strategies (De Luna et al. 2011). Completely data driven strategies however fall into the problem of dimensionality, as there are infinitely many factors that

could potentially be constructed from the data available. Here, the compromise has been a theoretically guided, manually implemented estimation algorithm. While theory has guided variable construction, variable inclusion has followed a stepwise procedure. The algorithm is schematically laid out below.

1 The model of exposure outlined in section (3.2) is operationalized using the variables presented in section (4.3). For the matching algorithm to be specified, a set of variables needs to be defined on which balance is evaluated. I denote these variables *key balancing variables*. The set of key balancing variables is defined by the variable list presented in Table 3 and Table 4.
2 As evident from the literature review, the characteristics of violence differ by the sex of the victim. Therefore the propensity score is estimated separately for men and women. To implement risk set matching, i.e. to avoid matching on post-assault information, the propensity score is estimated separately for each assault year. I stepwise exclude variables which are not significant (5%) in any of the five years of the study period.
3 Using the selected specification, the propensity score in 1998 is estimated.
4 Each individual assaulted in 1998 is matched to five controls, who may or may not be assaulted in later years. Exact matching is employed for sex, work status and on any substance related hospital visit during the last four years. The first two to enable heterogeneity analysis, and the last as it has been brought up as especially important in the literature. Within these subgroups, matching is based on the propensity score for 1998.[20]
5 For matching to be a feasible strategy, there has to exist individuals who were not victims of violent crime although they were similar to crime victims in terms of their victimization probability. It is a common finding when looking at the propensity score, that the distribution for non-exposed 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 three magnitudes larger than the number

[20] I focus on bias reduction over variance reduction, and implement nearest neighbor matching with replacement, using up to 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, sacrificing overlap and variance reduction to reduce bias. For relation, the standard deviation of the propensity score is about 0.0043 for men, and 0.0032 for women.

of assaulted, so that there exist an abundance of non-exposed with extreme values, and lack of overlap is not a problem. [21]

6 Assaulted in 1998 and matched controls are removed from the population.

7 Step 3–5 is repeated sequentially for year 1999–2002.

8 After matching, covariate balance is evaluated using the key balancing variables. Balance is evaluated separately for men and women. The variable in the set of key balancing variables with largest significance on the difference between assaulted and matches is considered the most poorly balanced variable. Several functions of the poorly balanced variable are constructed and included in the model. Depending on the characteristics of the original variable some of the following are included: squared and cubic forms, count variables and dichotomizations. These functions are constructed to allow for effects other than linear. For example, it might not be the average income, but rather to not be poor that is important in the path from financial resources to risk reduction. In addition, the timing could, but might not, matter. I therefore include variables relating to each specific year from four years before the assault to the last year before the assault, variables capturing aspects of continuity over the observed pre-assault period, such as "not once during the last four years", as well as variables disregarding the specific timing, such as "at least once during the last four years", and "on average during the last four years".

9 Estimation is repeated including the newly constructed variables. All significant newly constructed variables are kept. If none is significant, the one with lowest p-value is kept. I then start over from step 3.

10 I stop when the key balancing variables no longer unambiguously improve. Final propensity score specifications are given in the Appendix, Table A1-A2.

The success of a matching strategy depends on whether it achieves balance on (observed) covariates between cases and controls. In Table 4, match quality is presented. We can see that assaulted cannot be distinguished from the matched comparison group on any of the

[21] To give an illustration, I show the right tail distribution of the propensity score of women working in 1998 in Figure A1. In total in this study, four observations are removed due to lack of support.

key balancing variables, with the exception that assaulted men have a slightly decreased risk of having visited a hospital.[22]

Table 4 Pre-victimization characteristics of cases and matched controls

	Women		Men	
	Matched controls	Assaulted	Matched controls	Assaulted
Age	36.0	35.8	32.6	32.6
	(0.112)	(0.248)	(0.062)	(0.137)
Years of schooling	11.2	11.1	11.1	11.1
	(0.024)	(0.053)	(0.011)	(0.026)
Working (%)	0.374	0.374	0.501	0.501
	(0.005)	(0.012)	(0.003)	(0.007)
Earnings	49,300	47,800	74,700	73,200
(mean last 4 years, SEK)	(760)	(1,690)	(555)	(1,241)
Disposable income	106,400	107,400	93,800	83,500
(mean last 4 years, SEK)	(621)	(1366)	(367)	(1,004)
Risk occupation (%)	0.201	0.215	0.055	0.049
	(0.005)	(0.010)	(0.001)	(0.003)
Married (%)	0.230	0.229	0.117	0.114
	(0.005)	(0.010)	(0.002)	(0.004)
Newly separated (%)	0.125	0.144	0.137	0.143
	(0.004)	(0.009)	(0.002)	(0.005)
Child <7	0.295	0.296	0.129	0.124
	(0.007)	(0.015)	(0.002)	(0.006)
Single parent (%)	0.249	0.255	0.042	0.041
	(0.005)	(0.011)	(0.001)	(0.003)
In-care patient (%)	0.819	0.818	0.599	0.580**
	(0.004)	(0.010)	(0.003)	(0.007)
Psychiatric diagnose (%)	0.314	0.323	0.217	0.216
	(0.005)	(0.012)	(0.003)	(0.006)
Substance abuse (%)	0.229	0.229	0.184	0.185
	(0.005)	(0.010)	(0.002)	(0.005)
Psychosis (%)	0.031	0.034	0.026	0.022
	(0.002)	(0.004)	(0.001)	(0.002)
Mood disorder (%)	0.065	0.062	0.026	0.026
	(0.003)	(0.006)	(0.001)	(0.002)
Anxiety (%)	0.107	0.120	0.039	0.037
	(0.003)	(0.008)	(0.001)	(0.003)
Personality disorder (%)	0.051	0.064	0.022	0.026
	(0.002)	(0.006)	(0.001)	(0.002)
Days on sickness insurance	31.657	32.4	16.4	17.0
(mean, last 4 years)	(0.780)	(1.700)	(0.296)	(0.671)
Any disability benefits	0.163	0.159	0.086	0.078
(last 4 years, %)	(0.004)	(0.009)	(0.002)	(0.004)
Days on sick leave	80.3	78.5	42.0	39.5
(mean, last 4 years)	(1.54)	(3.299)	(0.625)	(1.32)
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. Earnings and disposable income in year 2016's values.

[22] For the variables psychiatric diagnose and in-care patient, it is easy to construct functions – e.g. secondary terms or count variables as discussed under step 7 in the algorithm – where matches differ significantly from controls. To amend this potential bias source, I adjust for these characteristics in the estimations, see note 26 for details.

Balance on information left out of the propensity score specification lends support for the quality of matches. The final propensity score include pre-assault information on outcomes for both men and women, but most lagged outcomes are not significant and thus left out of the specification. Year-by-year balance on pre-assault values of the dependent variables is shown in Figures 7–11 in the results section. Information on marriage, mood disorder, and personality disorder is not used in the propensity score specification for women, and still show balance. For men, information on children, mood disorder, anxiety, and personality disorder is left out of the propensity score specification, and still show balance. This is additional evidence that the matching strategy has been technically successful in achieving balance on observed characteristics.

Even after matching successfully, assaulted might differ from the reference population on characteristics not available in administrative data. Information processing capacity for example, matters in quickly making accurate judgements on risk (Classen et al., 2005). I have tried to capture such variation through combining information on severe psychiatric illness from hospital records with indicators of general ability such as earnings and years of schooling. From this study, I cannot say whether this attempt has been successful. How problematic the potential of remaining bias is to the interpretation of the results can however be analyzed. The sensitivity of this study to bias from unobservable characteristics is evaluated in section (6.3).

5.1 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 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.[23] 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.[24]

6 Results

6.1 Premature death

Visualizing the cumulative death hazard[25] of women and men in Figure 3, we see that while all individuals are most likely not to die, there is a marked difference between violence victims and their matched controls. Victimized have a higher risk of death, and the discrepancy in the probability of being dead increases steadily over the eight years following an assault. Column 1 and 4 in Table 5 presents ATT estimates of effects of violence on mortality during each year of the eight year follow-up period. At the end of the follow-up period, the excess risk of premature death due to assault amounts to 4.5 percentage points for women, and 5.3 percentage points for men. Column 2 and 5 present the yearly death hazards. The average yearly increase in mortality due to assault are presented in the last row and amounts to 0.56 percentage points for women, and 0.66

[23] A further refinement is to use bias adjustment (see e.g. Abadie & Imbens, 2011). The bias adjusted estimator is defined following Abadie et al. (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.

$$\hat{X}_{i0} = \frac{1}{5}\sum_{j=1}^{5} X_{j0},\ \mu_{i0}(\hat{X}_{i0}) = E(\hat{Y}_{i0}|\hat{X}_{i0}),\ \tilde{Y}_{i0} = \mu_{i0}(\hat{X}_{i0}),\ \tilde{Y}_{i1} = \mu_{i0}(X_{i1}).$$

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

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

Throughout this study, match values have been adjusted using age, any psychiatric diagnose, the number of hospital visits as well as 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. As bias adjustment did not make any qualitative difference for any outcome, these results have been left out of the report.

[24] 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 an estimated propensity score has on the standard errors for ATT estimators (Abadie & Imbens, 2009, p. 8).

[25] Assaults resulting in immediate death are excluded from the sample, as the cost to the victim is directly observable. To aggregate total costs of assault, these cases should be included.

percentage points for men. Relative effects are shown in column 3 and column 6. They average to a 17 time increase in the risk of death for women, and a 10 time increase in the risk of death for men, over the follow-up period.

Figure 3 Cumulative death hazards for women and men

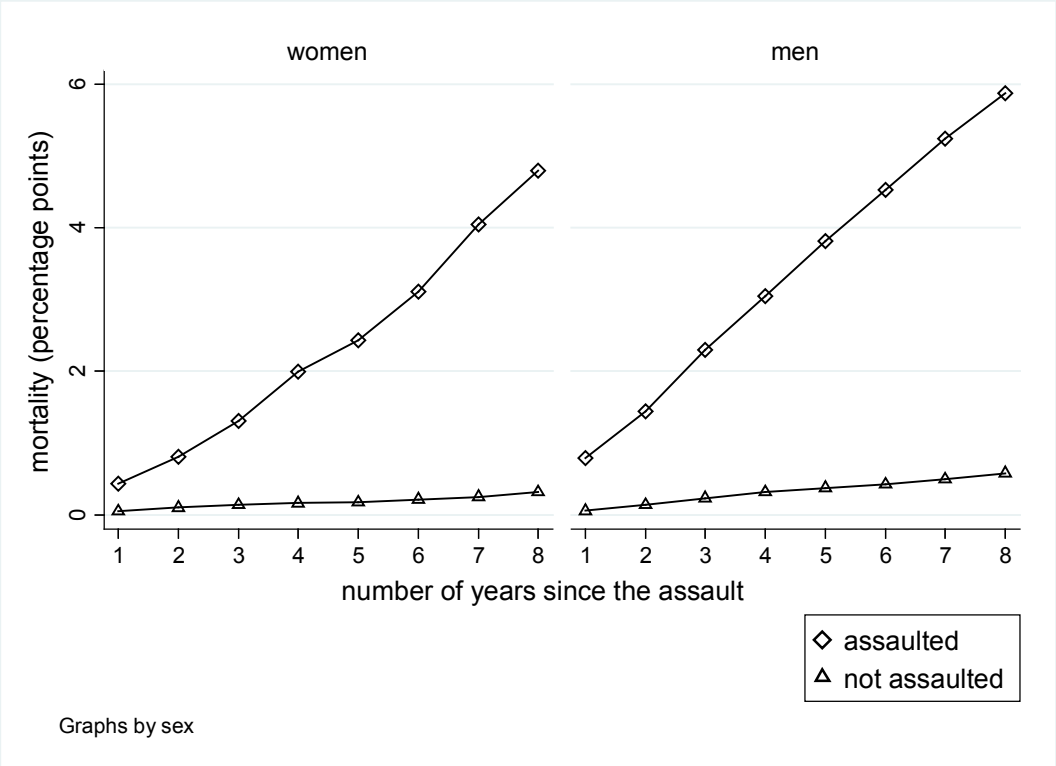


Table 5 Effects of assault on mortality

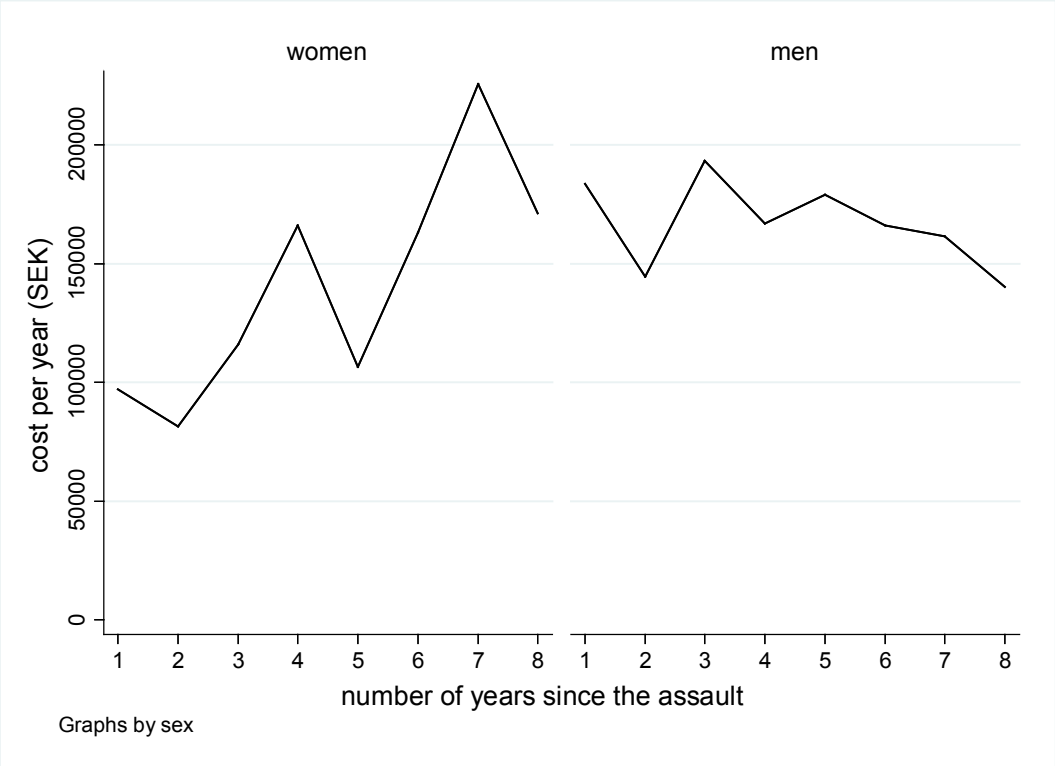
	ATT, women		Relative risk, women	ATT, men		Relative risk, men
	Cumulative, percentage points	Yearly, percentage points	Yearly, percent	Cumulative, percentage points	Yearly, percentage points	Yearly, percent
Years post	(1)	(2)	(3)	(4)	(5)	(6)
1	0.386*** (0.010)	0.386	7.8	0.730*** (0.010)	0.730	13.0
2	0.710*** (0.018)	0.324	6.5.	1.304*** (0.018)	0.574	7.1
3	1.171*** (0.029)	0.461	12.3	2.071*** (0.028)	0.768	8.2
4	1.831*** (0.046)	0.660	26.47	2.733*** (0.037)	0.664	7.7
5	2.254*** (0.056)	0.424	34.0	3.444*** (0.047)	0.722	12.7
6	2.902*** (0.072)	0.649	17.3	4.103*** (0.056)	0.660	12.6
7	3.798*** (0.095)	0.898	24.0	4.743*** (0.065)	0.643	9.0
8	4.477*** (0.112)	0.681	9.9	5.297*** (0.072)	0.558	6.7
Average		0.560	17.3		0.664	9.6

Note: Column (1) and (4) correspond to ATT effects in percentage points. Column (2) and (5) correspond to yearly effects, calculated on those alive at the beginning of that year, i.e. difference in yearly hazard rates of assaulted relative to controls. Relative effects, calculated as the ATT estimate divided by the average match value, are presented in column (3) and (6). Standard errors in parentheses. ***/ **/* = the estimates are significantly different from zero at the 1/5/10 percent level, respectively.

Next, I rescale estimates on mortality to estimates of cost, using the value of a statistical life in Sweden 2016: 25,152,240 SEK.[26] At the end of the eight year follow-up period, the accumulated cost due to premature mortality amounts to 1,126,000 SEK per victimized woman, and 1,332,000 SEK per victimized man. This corresponds to an average yearly cost of 141,000 SEK per assaulted woman and 167,000 SEK per assaulted man. Details are given in Table A 3 in the appendix. I plot the cost per assault in a given year over time in Figure 4 below. From the development of costs over time since the assault, it seems unlikely that the effects on mortality would abruptly cease past the eight year follow-up period. The estimate on long-term cost due to excess mortality should thus be viewed as a lower bound.

[26] The value of a statistical life is calculated by the Swedish transport administration and described in more detail in section (4.2).

Figure 4 Average cost due to excess mortality



Note: Costs are reported with year 2016 as base.

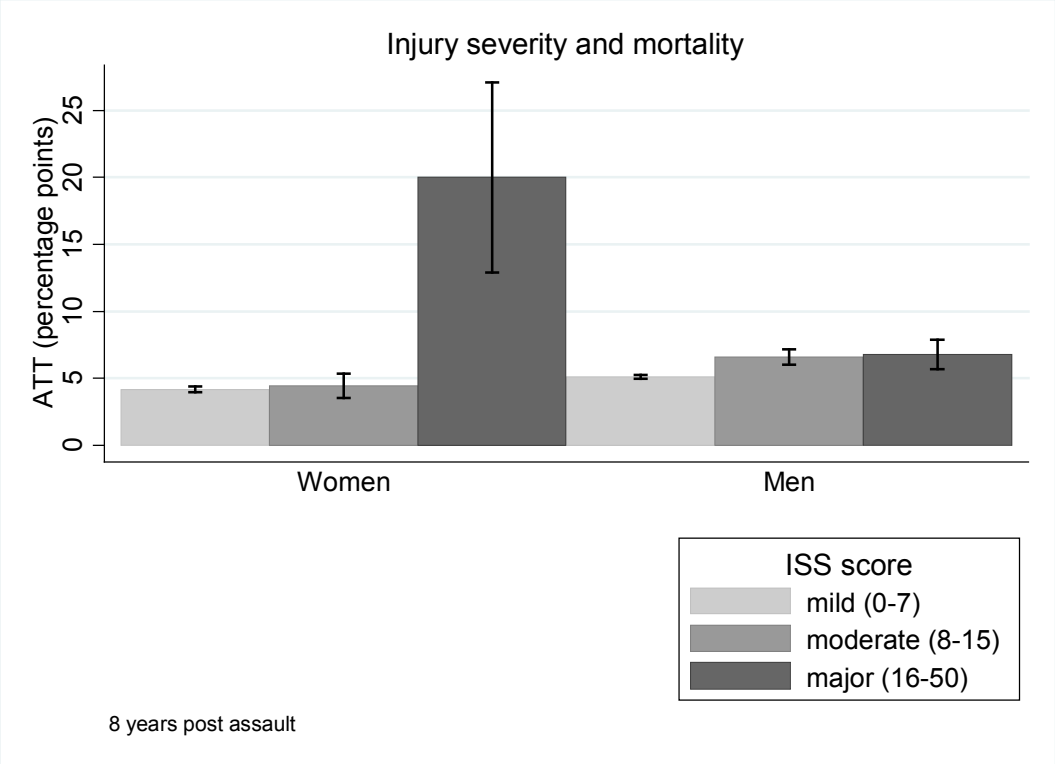
6.1.1 Heterogeneity

In this section, results are separated by the severity of the physical injury, and by work status prior to victimization. Heterogeneity with respect to the physical injury could potentially tell us something about the mechanism behind the consequences of assault, from the extent to which they are related to the physical consequences of the injury. It is however important to note that the injury severity might well be correlated with characteristics of the violence as well as of the victimized individual. Heterogeneity with respect to labor market attachment captures potential differences in vulnerability with respect to pre-assault situation.

Figure 5 presents results on mortality separated by injury severity (details are given in Table A 4). Women with little to moderate injuries exhibit an effect size of 4.2 and 4.4 percentage points, respectively, just below the average effect size of the whole group. Women who received a major injury incur an almost five times larger increase in their death hazard. Men exhibit some heterogeneity in the direction of the injury severity, but

of much smaller magnitude. Their difference is between those with less severe injury versus those with moderate to major injury.

Figure 5 Effects of assault on mortality, by sex and injury severity



In Figure 6 (and Table A 5), results are instead separated by pre-victimization work status. The largest absolute effects can be seen for individuals who did *not* work in the year prior to victimization – after eight years their excess mortality amounts to 5.8 percentage points for women, and 7.9 for men – which puts the largest costs on this group. However, it is those who were working prior to the assault who experience the largest relative effects. The discrepancy is in the same direction for both women and men, but largest for women, and is explained by the low counterfactual mortality of those working prior to the assault. After eight years, assaulted women who did not work exhibit a 12 times higher excess mortality than controls, while previously working women are 64 times more likely to have died than their matched controls. The corresponding numbers for men is an 8 time increase for those not working prior to victimization, and a 13 time increase for those working.

Figure 6 Effects of assault on mortality, by sex and work status

6.1.2 Potential mechanism: suicide

To investigate the role of psychological trauma as a potential mediator between non-lethal assault and death, I estimate the effect of victimization on suicides. Effects are shown in Table 6. The results indicate that 0.6 (0.7) percent of women (men) who have been the victim of a violent crime chose to end their lives within eight years after the event. Relative effect sizes amount to a 17 times increase in suicide probability for women, and a 9 times increase for men. Suicides account for about 14 percent of the effect of interpersonal violence on excess mortality.

Table 6 Effects of assault on suicides

ATT 8 years post assault, women		ATT 8 years post assault, men	
Cumulative, percentage points	Relative, percent	Cumulative, percentage points	Relative, percent
(1)	(2)	(3)	(4)
0.648***	17.3	0.704***	8.5
(0.016)		(0.010)	

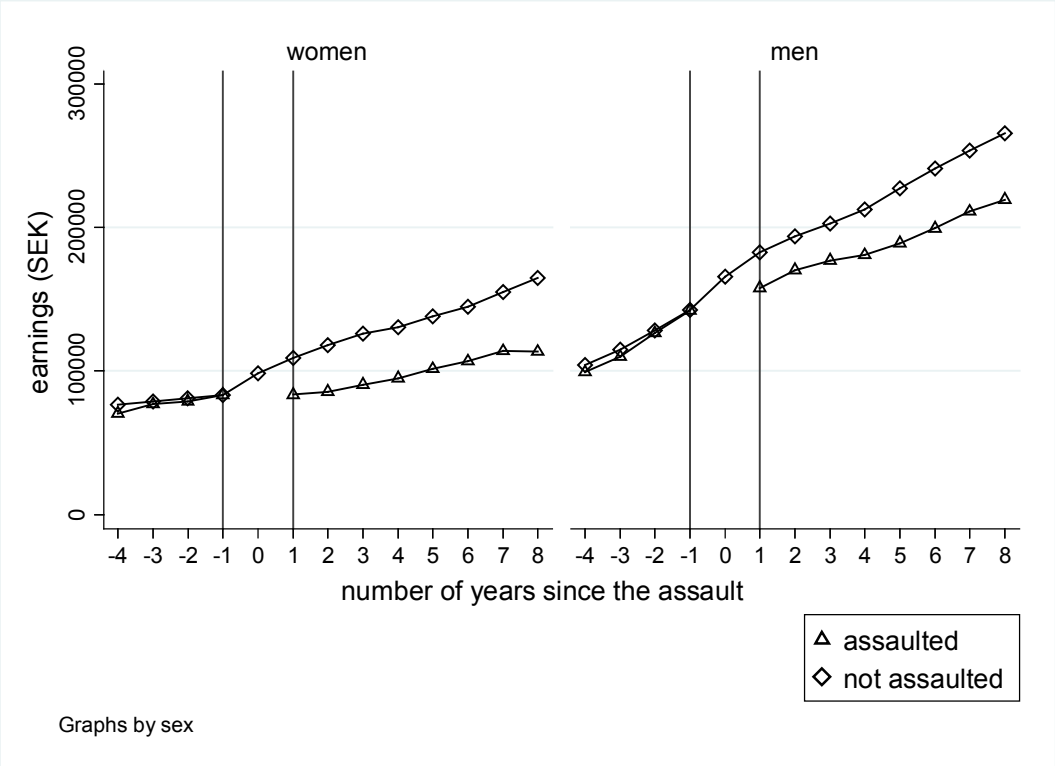
Note: Column (1) and (3) correspond to ATT effects in percentage points. Relative effects, calculated as the ATT estimate divided by the average match value, are presented in column (2) and (4). Standard errors in parentheses. ***/ **/* = the estimates are significantly different from zero at the 1/5/10 percent level, respectively.

6.2 Reduced capacity to work

Next, I look at effects on the capacity to work, measured through effects on earnings and work status. Yearly effects on earnings can be seen in Figure 7 and in Table 9 column 1 and 4. Before the assault, earnings are balanced over assaulted and controls. Afterwards, earnings take a downward shift among the assaulted compared to the controls. Absolute effects are of about the same size for women and men; the average effect size during the follow-up period amount to an annual reduction by 34,000 SEK per woman and 30,000 SEK per man during the eight year follow-up period. The total earnings loss over the studied eight years sum to 270,000 SEK per assault for women, and 244,000 SEK for men.

The total effect of assault on earnings runs through two paths: Through reduced productivity among assault survivors, through and the increased mortality described in the previous section. Reduced productivity among assault survivors is not the same as the value of the loss of their lives. In the previous section, a life was viewed as a good to the specific individual, valued by the amount the average individual pays to insure herself through the consumption of safety. In this section, the value of life comes instead from its function as a prerequisite for productive capacity. In column 2 and 5 of Table 7, effects through the second path are removed. Among assault survivors, earnings losses due to the assault amounts to 264,000 SEK per assaulted woman, and 215,000 SEK per assaulted man. By comparing column 1 and 2, and column 4 and 5, respectively, the share of the total effect on earnings which runs through the path of increased mortality can be calculated. It corresponds to 2 percent of the total effect for women, and 12 percent of the total effect for men. It is notable that the majority of the reduction in earnings is driven by a reduced capacity to work among assault survivors.

Figure 7 Earnings of assaulted and matched controls, by sex



Note: The year of violence victimization is set to 0 and no data on victimized individuals is used from this year. Earnings reported with year 2016 as base.

In column 3 and 6 of Table 9, relative effects are presented. As women earn less than men, similar absolute effect sizes correspond to larger relative effects for women. Female victims of assault reduce their subsequent earnings with on average 25 percent, while the corresponding figure for men equals 14 percent. Note that while the absolute effects are increasing over time, the relative effects are quite stable. During the study period, earnings increase annually for the whole population, as both cases and controls increase their earnings as their labor market experience increase with their age. Increasing counterfactual earnings is thus an important factor behind absolute effect sizes increasing over time.

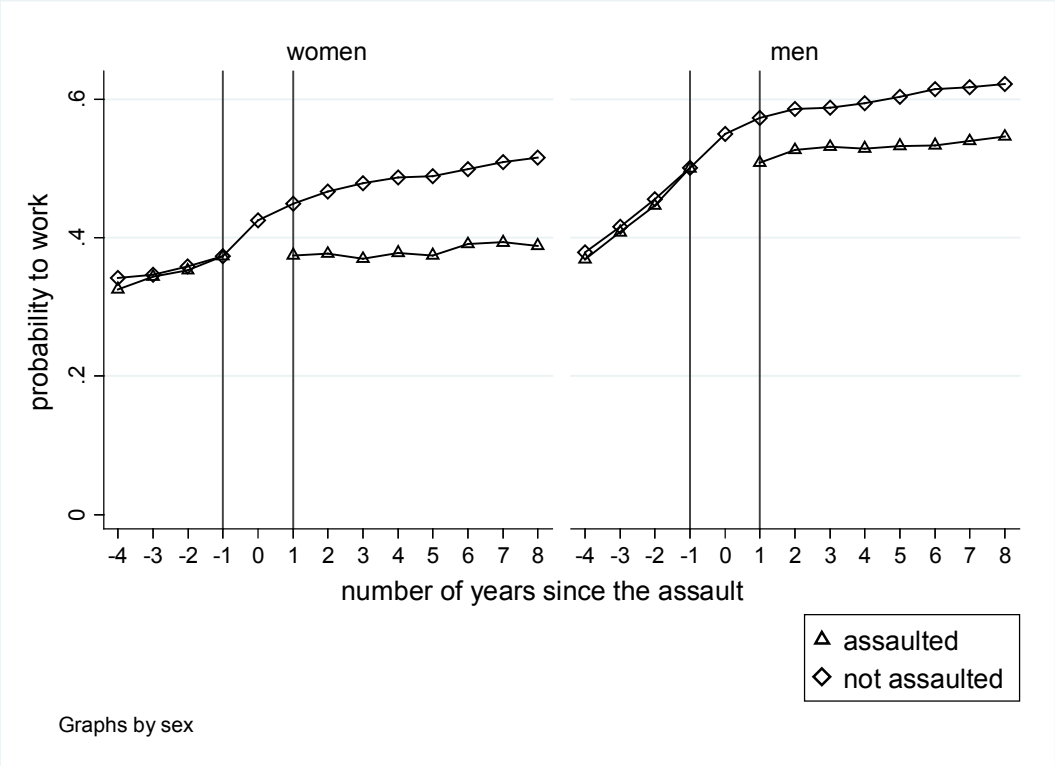
Table 7 ATT estimates of assault on earnings, by sex

	ATT, women			ATT, men		
	Yearly (SEK)	Yearly; effects due to mortality excluded (SEK)	Yearly (percent)	Yearly (SEK)	Yearly; effects due to mortality excluded (SEK)	Yearly (percent)
Years post	(1)	(2)	(3)	(4)	(5)	(6)
1	-24,850***	-24,786***	-22.8	-24,109***	-23,477***	-13.2
	(612)	(612)		(326)	(318)	
2	-30,956***	-30,688***	-26.3	-22,320***	-21,192***	-11.5
	(758)	(754)		(299)	(285)	
3	-33,122***	-33,056***	-26.2	-23,195***	-20,905***	-11.4
	(801)	(805)		(307)	(280)	
4	-32,671***	-32,311***	-25.1	-28,638***	-25,820***	-13.5
	(784)	(783)		(376)	(344)	
5	-32,842***	-32,217***	-23.8	-33,810***	-29,854***	-14.9
	(782)	(777)		(440)	(396)	
6	-33,486***	-32,483***	-23.2	-36,041***	-31,068***	-15.0
	(791)	(779)		(465)	(410)	
7	-36,140***	-34,543***	-23.3	-36,402***	-30,631***	-14.4
	(846)	(826)		(465)	(402)	
8	-45,647***	-44,068***	-27.7	-39,434***	-32,510***	-14.8
	(1,062)	(1,052)		(500)	(425)	
Cumulative	-269,714	-264,155		-243,949	-215,462	
Average	-33,714	-33,019	-24.8	-30,493	-26,933	-13.6

Note: Column (1) and (4) correspond to ATT estimates in SEK. The part of the ATT not accountable by excess mortality of assaulted is presented in column (2) and (5). Estimates are in year 2016's values. Relative effects (in percent) presented in column (3) and (6). Relative effects are calculated as the ATT estimate divided by the average match value (relative risk). Standard errors in parentheses. ***/ **/* = the estimates are significantly different from zero at the 1/5/10 percent level, respectively.

Effects of assault on the probability to work can be seen in Figure 8 (and Table A 6). Pre-assault outcomes are balanced. Absolute effects amount to about 11 percentage points for women, and 7 percentage points for men. In column 3 and 6, relative effects are shown. As women work less than men, similar absolute effect sizes correspond to larger relative effects for women. Female victims of assault reduce their subsequent probability to work with on average 22 percent, while the corresponding figure for men equals 12 percent. Both absolute and relative effects are increasing slightly over time.

Figure 8 Probability to work for assaulted and matched controls, by sex



Note: The year of violence victimization is set to 0 and no data on victimized individuals is used from this year. Dead individuals are coded as not working.

6.2.1 Heterogeneity

To try to cast some light on the path from violence victimization to reduced capacity to work, I separate results by first injury severity, and then pre-victimization employment. In Table 8, the results in terms of injury severity are shown. For both sexes, individuals having sustained an injury classified as minor show significant and non-negligible reductions in earnings. For women, there is no clear heterogeneity in terms of the physical injury, in contrast to the sizable effects on mortality exhibited by those who had received a major injury. Men instead exhibit a large heterogeneity. The effect size of men who had received a moderate to major injury is twice that of those with less severe injuries.

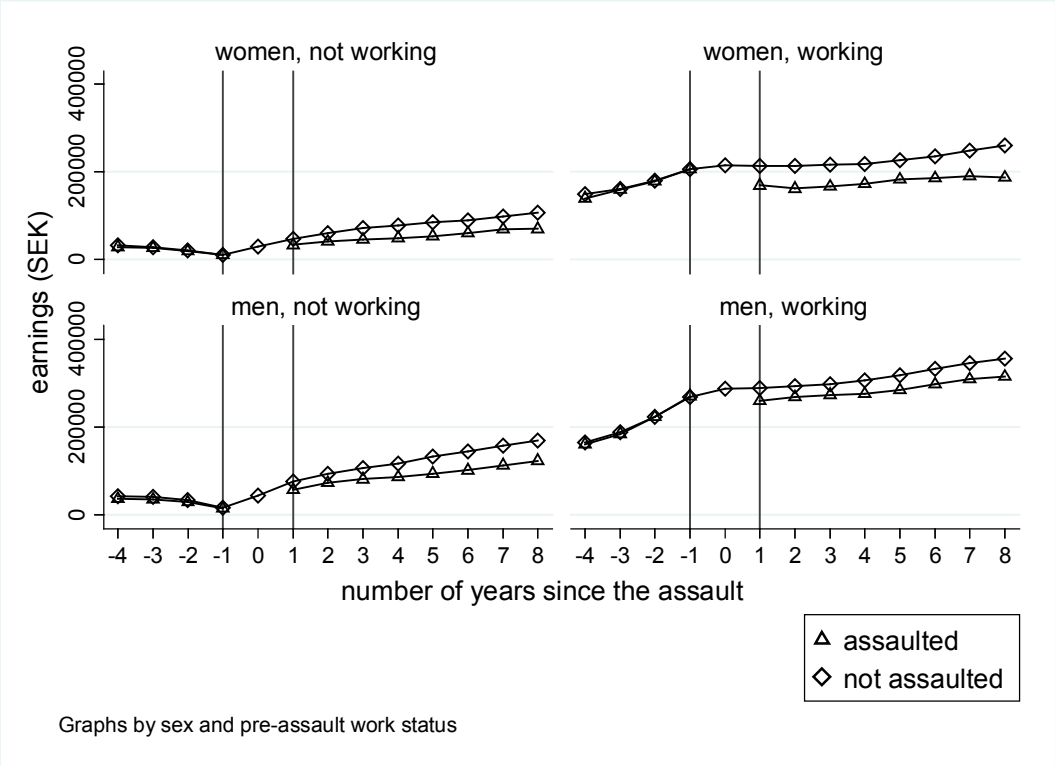
Table 8 Effects of violence on earnings, by sex and injury severity

	ATT 8 years post assault, women		ATT 8 years post assault, men	
	Cumulative, percentage points	Relative, percent	Cumulative, percentage points	Relative, percent
Injury severity	(1)	(2)	(3)	(4)
Mild (0–7)	-45,706*** (1,106)	-27.7	35,780*** (483)	-13.5
Moderate (8–15)	-40,133*** (3,945)	-24.3	62,807*** (2,606)	-23.6
Major (16–50)	-59,279*** (9,898)	-35.9	77,275*** (6,051)	-29.1

Note: Effects reported with year 2016 as base. Standard errors in parentheses. ***/ **/* = the estimates are significantly different from zero at the 1/5/10 percent level, respectively. Relative effects, calculated as the ATT estimate divided by the average match value, are presented in column (2) and (4). Dead individuals are assigned 0 earnings.

Heterogeneous estimates on earnings, separated by prior work status, are shown in Figure 9 (and Table A 7). Some of the individuals not working in the year prior to assault have a zero counterfactual capacity to work in subsequent years. An adverse event such as violence victimization will not trigger any effect on earnings in those individuals. Pre-assault employment is thus predicted to have mechanical effects on the absolute effect of assault on earnings. Women working prior to the assault have reduced their earnings with on average 69,000 SEK per year eight years after victimization, whereas the corresponding number is 32,000 SEK per year for women previously not working. The relative effects for women however, correspond to just below 30 percent, irrespective of their work status prior to the assault. Men show no heterogeneity in absolute effect sizes, but in relative: Men not working reduce their earnings with 24 percent due to assault, whereas working men reduce their earnings with 11 percent.

Figure 9 Earnings of assaulted and matched controls, by sex and work status

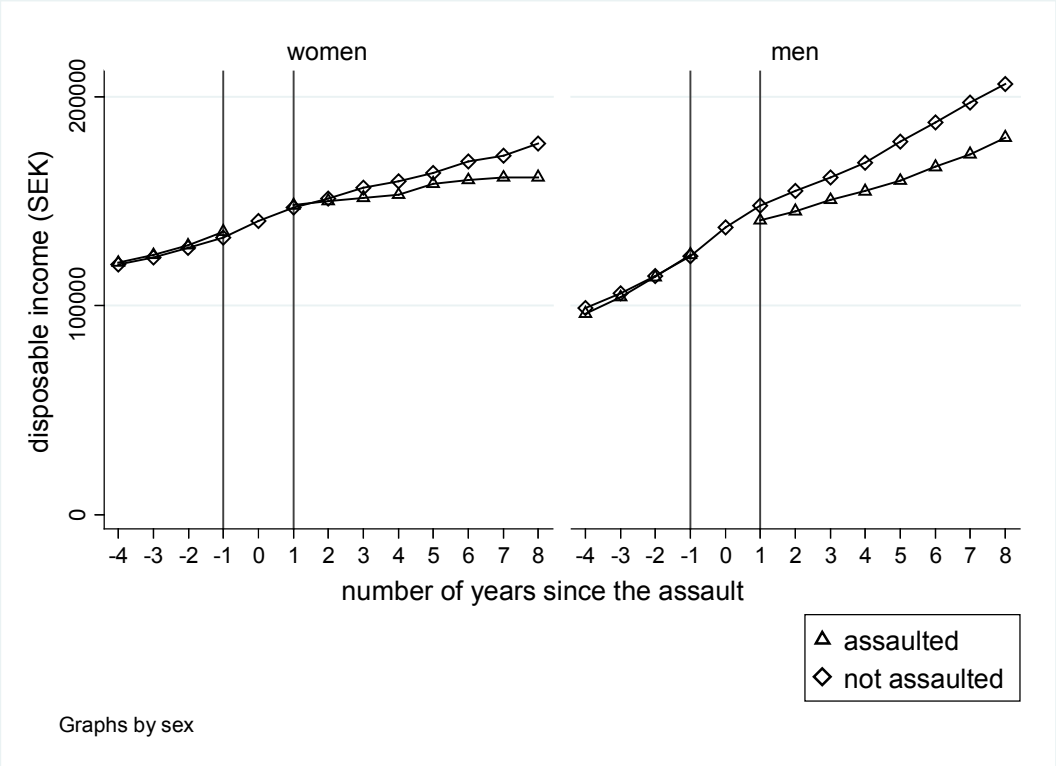


Note: The year of victimization is set to 0 and no data on victimized individuals is used from year 0. Gross earnings reported with year 2016 as base. Dead individuals are assigned 0 earnings.

6.2.2 Disposable income

Effects on disposable income can be related to effects on earnings and interpreted as the extent losses in earnings are carried by the individual. Results are presented in Figure 10 (and Table A 8). After the assault, assaulted reduce their disposable income significantly in relation to matched controls, but the reduction is considerably smaller than it is for earnings, especially for women. In the short run, most of the income lost due to violence is covered by the public, with the individual carrying a larger share of the cost each year. While the relative effects of victimization on earnings are stable, the effects on disposable income are increasing irrespective of whether it is an absolute or relative measure that is in focus. At the end of the follow-up period, violent crime has resulted in an 8 percent reduction in disposable income for female crime victims, and an 11 percent reduction in disposable income for male crime victims. At this time, losses in terms of disposable income have summed to 43,000 SEK for female violence victims, and 116,000 SEK for male victims of violence.

Figure 10 Disposable income of assaulted and matched controls, by sex



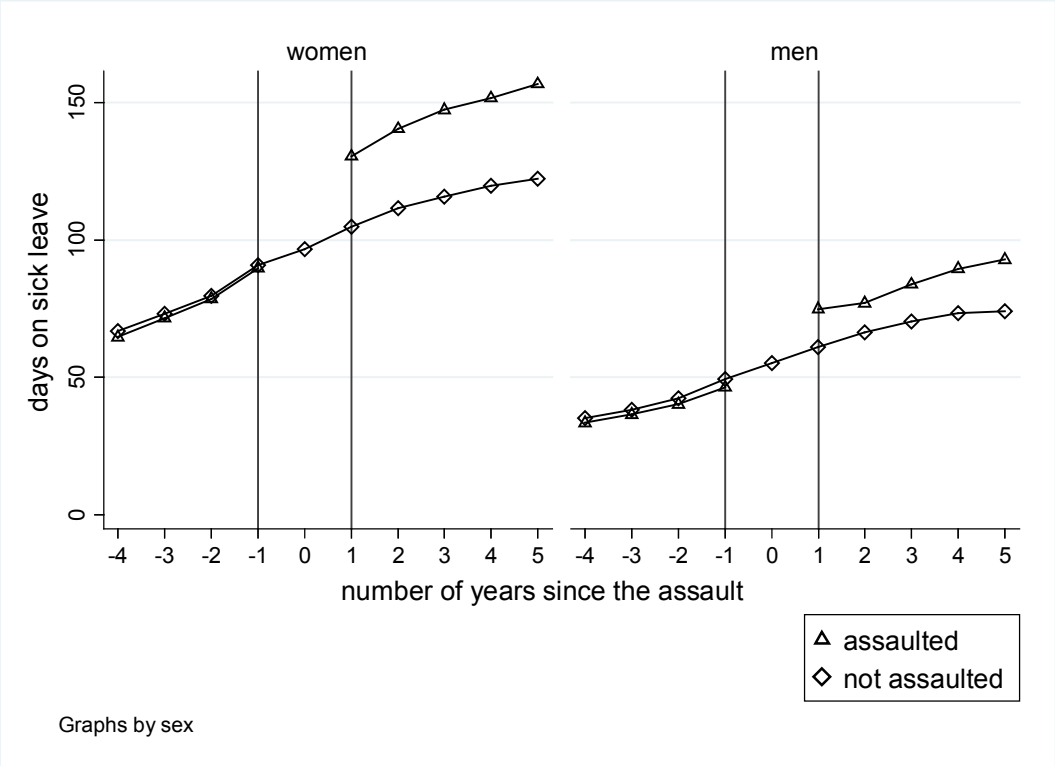
Note: The year of violence victimization is set to 0 and no data on victimized individuals is used from this year. Disposable income reported with year 2016 as base. Dead individuals are assigned 0 in income.

6.2.3 Potential mechanism: sick leave as an indicator of long-term ill-health

In this section, focus is on the role of sick leave, i.e. absences from work extending two weeks and certified by a physician. In Figure 11 (and Table A 9) the outcome variable is sick leave. Prior to the assault, sick leave is increasing for both men and women. After the violent event, matches continue on the slowly rising trend while crime victims make a break in levels and increase their sick leave drastically. The average effect amounts to 31 yearly excess days for women, and 15 yearly extra days for men.[27] Women on average are more often on sick leave than men, and this is true among the assaulted as well, so that relative effects are of more equal size, a 27 percent increase for women, and a 21 percent increase for men.

[27] Over the follow-up 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).

Figure 11 Sick leave of assaulted and matched controls, by sex



Note: The year of violence victimization is set to 0 and no data on victimized individuals is used from this year. Dead individuals are assigned 365 days of sick leave.

6.3 Sensitivity

In the absence of random assignment, victims of violence and controls may differ on characteristics not observable in the administrative registers. As a consequence, individuals who appear comparable may not be. In this section, I follow the framework developed by Rosenbaum (see e.g. Rosenbaum, 1991 for an overview) to investigate the robustness of the results to unmeasured characteristics that simultaneously affect violence victimization and outcomes.

The strategy in this type of sensitivity analysis is to manipulate the estimated odds of assault to see how the effects vary, i.e. asking how the unmeasured covariate would have to be distributed over cases and controls to alter the conclusions of the study. Consider two units *i* and *k* with the same observed characteristics $x_i = x_k$ and potentially different unobserved characteristics u_i and u_k. The probability that unit *i* receives treatment is $Pr(\mathrm{D_i} = 1 | X_i = x_i, U_i = u_i) = \pi_i$. In the results section, inference is calculated under the assumption that $u_i = u_k$. Another way to state this assumption is to write the odds ratio of *i* to *k* as bounded by a constant Γ, and note that inference in the results section is

made under the assumption $\Gamma = 1$. Selection bias in this setting means that $u_i \neq u_k$, so that it is no longer certain that π_i equals π_k, and thus no longer certain that the odds ratio of assault equals 1. In this section I analyze how changes in Γ – allowing for differences in the odds of assault between *i* and *k* – affects inferences about the impact of assault.

$$\frac{1}{\Gamma} \leq \frac{\pi_i(1-\pi_k)}{\pi_k(1-\pi_i)} \leq \Gamma$$

Each $\Gamma > 1$ returns an interval of p-values, reflecting uncertainty due to hidden bias. The length of the interval is increasing in Γ, and the value of Γ at which the interval becomes uninformative can be used as a measure of sensitivity to hidden bias. It is defined as the Rosenbaum bound and indicates the magnitude of unobserved bias that could be present without altering the conclusion of a test. I denote the Rosenbaum bound by Γ_{max} and define it as the maximum difference in assault risk between assaulted and controls that returns a p-value lower than 5 percent on a two-sided test on that outcome. The specific test used here to define Γ_{max} has been adapted for multiple controls by Rosenbaum (1988).[28] All variables have as well been analyzed using the sign-score test given in Rosenbaum (2002, pp. 121). While the exact bounds differ somewhat between the tests (in different directions depending on the outcome), the choice of test does not affect any of the qualitative results.

Starting with mortality and suicides (shown in Table 9), the results are found to be remarkably stable. Assaulted women would need to be more than 11.9 times more likely to be assaulted than matched controls for another factor to potentially explain their high post-assault mortality. Assaulted men would need to be more than 9.4 times more likely to be assaulted for their results to be overturned by the presence of a strong confounder. For deaths caused by suicide, the results are as well very robust. Assaulted women (men) would need to be more than 7.2 (6.1) times more likely to be assaulted than their matched controls for their increased suicide rate to potentially be caused by some other factor, which would in turn need to be a very good predictor of suicide. While this does not make it impossible for the increased mortality of assault survivors to be the effect of a spurious

[28] The exact test used is an m-test with Huber's weight function given in Rosenbaum (2007, section 4.2). It is based on permutation inference, heavily censors large differences, and has been implemented in the R-package 'sensitivitymv' (Rosenbaum, 2015). The reported sensitivity bounds are computed using the command senmv('data', gamma='Γ_{max}', trim=1).

correlation, these are unusually robust results. Such a strong relation is not easily explained away.[29]

Table 9 Sensitivity to hidden bias for mortality-related outcomes

Outcome	p-value for $\Gamma = 1$	(Γ_{max}, p-value)
All deaths, women	<0.0001	(11.9, 0.0496)
All deaths, men	<0.0001	(9.4, 0.0476)
Suicide, women	<0.0001	(7.2, 0.0492)
Suicide, men	<0.0001	(6.1, 0.0433)

Note: $\Gamma = 1$ corresponds to standard inference. The column to the right shows the maximum Γ for which the test has a p-value <0.05, and the corresponding p-value. The table is based on all deaths that had occurred 8 years post-assault.

Looking next to the socio-economic outcomes (shown in Table 10), they turn out to be much more sensitive to hidden bias. Although the results are significant by a large margin under the assumption of no hidden bias, they could potentially all be explained by other factors if assaulted women were more than 1.8 times – 80 percent – more likely, and assaulted men more than 1.7 times – 70 percent – more likely, to be assaulted than their respective matched controls.

Table 10 Sensitivity to hidden bias for socio-economic outcomes

Outcome	p-value for $\Gamma = 1$	(Γ_{max}, p-value)
Work, women	<0.0001	(1.7, 0.0232)
Work, men	<0.0001	(1.3, 0.0002)
Sick leave, women	<0.0001	(1.3, 0.0054)
Sick leave, men	0.0002	(1.0, 0.0002)
Earnings, women	<0.0001	(1.8, 0.0200)
Earnings, men	<0.0001	(1.4, 0.0012)
Disposable income, women	<0.0001	(1.3, 0.0194)
Disposable income, men	<0.0001	(1.6, 0.0359)

Note: $\Gamma = 1$ corresponds to standard inference. The column to the right shows the maximum for Γ for which the test has a p-value <0.05. The table is based on outcomes 8 years post assault (5 years post assault for sick leave), but results do not differ qualitatively between the years.

7 Summary

Violent crime is a general public health concern, but the full extent of the consequences are far from known. Previous cost analyses have therefore only included effects manifestly attributable to the violence, and typically, only those inflicted on society

[29] For comparison, this level of robustness to hidden bias exceeds that of the first formal implementation of a sensitivity analysis, calculated by Cornfield et al. (1959) for the correlations between smoking and lung cancer.

through medical and legal expenses. This is a first attempt to identify the costs affecting the victims of violence. The study is based on Swedish administrative records and defines crime victims as individuals who visited a hospital in 1998 to 2002 as a result of having been the victim of an assault. I utilize a propensity score matching method to construct a control group, identical on observed characteristics to those later exposed to violence during 4 years prior to victimization.

The findings provide evidence that violence victimization substantially increases mortality. I find that interpersonal violence leads to a yearly increase in mortality corresponding to more than 0.5 percentage points for both women and men. The results indicate that effects continue beyond the 8 year follow-up period. There exists a dose-response relationship between the severity of the injury and the resulting excess mortality, but also those who do not sustain a severe physical injury incur a significant increase in mortality. A hypothesized path to mortality is through increased psychiatric morbidity. Support for this is found through looking at suicides. About 14 percent of the effect of violence on mortality can be contributed to an increase in suicides. The sensitivity analysis shows that it is unlikely that the full effects on mortality are spurious. Female crime victims would need to have 12 times the risk of victimization as their matched controls for results to potentially be explained by a strong confounder. The corresponding figure for male victims is more than 9 times the victimization risk. This does not mean that results could not possibly be spurious, but it is an indication of reliability.

For survivors, assault is found to have large negative effects on productivity. Female crime victims reduce their earnings with on average 25 percent, and male crime victims with 14 percent. These effects are persistent over the eight year follow-up period. The productivity losses are partly carried by society, but it also has specific consequences for the victims' personal financial situations; these correspond to about 10 percent reductions in disposable income after 8 years. Reduced earnings are largely driven by reductions in the probability to work, where relative effects correspond to 22 percent for women, and 12 percent for men. For men, there exist a dose-response relationship between injury severity and decreased earnings. A likely mechanism behind the decrease in earnings is absence from work due to reduced work capacity. Following an assault, women increase their sick leave uptake with 31 days annually, and men increase theirs with 15 days per annum. The sensitivity analysis on outcomes relating to the capacity to work shows more

moderate robustness than the one on mortality. All such outcomes could be explained by a confounder if the true unobserved risk of victimization is in fact more than 80 percent higher for assaulted than that of their matches.

Using the recommended value of a statistical life in Swedish traffic investment calculations, costs due to increased mortality are calculated to 1,126,000 SEK per assault for women, and 1,332,000 SEK per assault for men. Costs due to lost productivity among survivors sum to 264,000 SEK for women and 215,000 SEK for men, measured in gross earnings. Combined costs during the 8 year follow-up period, thus, sum to 1.4 million SEK per assaulted woman, and 1.5 million SEK per assaulted man.

The study design aims to delineate consequences from assault from pre-existing ill-health among victims of violence. One of the findings has been that, prior to victimization, victims of violent crime have significantly worse health and lower earnings than the average person. Given this, it is difficult to exclude the possibility of remaining differences in unobserved differences between the assaulted individuals and their controls. The sensitivity analysis indicates that those unobserved differences would have to be considerable to explain away the existence of detrimental effects of assault, especially on mortality. However, this does not preclude the possibility of bias inflating effect sizes. Another problem, albeit in the opposite direction, is the restriction of the follow-up period. Neither theory nor empirical results indicate that consequences cease after eight years. These limitations, together with limitations of the scope of the analysis, and the reported results of large welfare costs of violent crime, all point to the need for further studies.

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Appendix

Table A 1 Specification of propensity score estimation for women 1998–2002

Variables	(1)	(2)	(3)	(4)	(5)
no. children <11 years	**-0.265**	**-0.153**	-0.038	**-0.297**	**-0.225**
	(0.090)	(0.076)	(0.079)	(0.080)	(0.085)
Living alone with child	**0.470**	0.033	0.119	0.228	0.287
	(0.161)	(0.155)	(0.158)	(0.151)	(0.151)
Single, any last 4 years	**1.121**	**0.917**	**1.298**	**0.872**	**1.038**
	(0.175)	(0.151)	(0.180)	(0.160)	(0.170)
Swedish born with at least 1 Swedish born parent	**0.981**	**1.209**	**1.830**	**0.917**	**1.944**
	(0.500)	(0.410)	(0.472)	(0.388)	(0.379)
Swedish*(years of schooling)	**-0.153**	**-0.160**	**-0.218**	**-0.125**	**-0.208**
	(0.044)	(0.036)	(0.042)	(0.032)	(0.032)
Any psychiatric diagnose, at least on 2 occasions, last 4 years	-0.059	0.274	-0.050	-0.451	-0.418
	(0.367)	(0.339)	(0.355)	(0.357)	(0.348)
Any psychiatric diagnose, last 4 years	-0.045	**-0.746**	0.548	0.360	0.427
	(0.415)	(0.295)	(0.353)	(0.344)	(0.359)
Any psychiatric diagnose, 1 year prior	-0.056	-0.618	-0.136	-0.041	-0.193
	(0.411)	(0.395)	(0.329)	(0.311)	(0.331)
Any psychiatric diagnose, 2 years prior	**0.674**	**0.616**	0.338	0.522	**0.734**
	(0.314)	(0.295)	(0.303)	(0.300)	(0.308)
Substance abuse, since 1987	**0.797**	**0.515**	**0.768**	**0.684**	**0.746**
	(0.268)	(0.212)	(0.219)	(0.209)	(0.210)
Anxiety, since 1987	-0.252	**0.565**	0.191	0.324	**0.444**
	(0.277)	(0.198)	(0.218)	(0.203)	(0.199)
Psychosis, since 1987	**-1.404**	**-0.899**	-0.327	-0.103	**-0.820**
	(0.479)	(0.361)	(0.307)	(0.281)	(0.351)
Substance abuse, 1 year prior	0.526	**1.182**	0.252	0.511	0.669*
	(0.424)	(0.408)	(0.364)	(0.342)	(0.354)
no. of hospital visits, last 4 years	0.015	0.025*	**0.035**	**0.042**	**0.051**
	(0.020)	(0.014)	(0.013)	(0.011)	(0.010)
At least 2 hospital visits, last 4 years	0.361	**0.903**	**0.482**	**0.645**	**0.857**
	(0.218)	(0.200)	(0.227)	(0.215)	(0.222)
At least 4 hospital visits, last 4 years	**0.781**	**0.584**	**0.849**	**0.872**	**1.023**
	(0.234)	(0.215)	(0.232)	(0.221)	(0.220)
At least 2 hospital visits, since 1987	**0.543**	**0.422**	**0.504**	**0.349**	**0.574**
	(0.190)	(0.180)	(0.186)	(0.167)	(0.193)
Any hospital visit, since 1987	0.040	-0.290	-0.196	0.076	**-1.026**
	(0.204)	(0.190)	(0.214)	(0.213)	(0.213)
Any hospital visit, 2 years prior	**-0.638**	**-0.570**	**-0.699**	**-0.854**	**-1.159**
	(0.175)	(0.162)	(0.188)	(0.187)	(0.199)
Any hospital visit, 3 years prior	**-0.604**	**-0.775**	**-0.500**	**-0.520**	**-0.451**
	(0.158)	(0.146)	(0.159)	(0.156)	(0.158)
Any hospital visit, 4 years prior	-0.169	**-0.444**	**-0.396**	**-0.341**	**-0.662**
	(0.149)	(0.139)	(0.153)	(0.152)	(0.162)
(any psychiatric diagnose since 1987) * (at least 2 hospital visits last 4 years)	**1.135**	-0.394	-0.308	-0.560	-0.380
	(0.516)	(0.311)	(0.339)	(0.316)	(0.345)
(any psychiatric diagnose since 1987) * (any hospital visit last 4 years)	-0.403	**1.456**	0.578	**0.700**	0.342
	(0.551)	(0.296)	(0.370)	(0.350)	(0.373)

Variables	(1)	(2)	(3)	(4)	(5)
any sick leave, last 4 years	**0.325**	0.223*	0.256	**0.336**	**0.328**
	(0.133)	(0.123)	(0.131)	(0.124)	(0.129)
any disability insurance, last 4 years	-0.146	-0.262	**-0.527**	**-0.565**	-0.101
	(0.187)	(0.170)	(0.195)	(0.178)	(0.172)
Working, any of last 4 years	**-0.626**	**-0.603**	**-0.481**	-0.330	**-0.679**
	(0.183)	(0.169)	(0.183)	(0.170)	(0.182)
Working, all of last 4 years	-0.197	**-0.901**	**-0.947**	**-1.210**	-0.155
	(0.247)	(0.219)	(0.227)	(0.217)	(0.217)
employed but not working, any of last 4 years	-0.091	**-0.269**	**-0.328**	**-0.293**	0.196
	(0.151)	(0.135)	(0.143)	(0.135)	(0.149)
Earnings, 1 year prior	**-0.000**	-0.000	-0.000	**-0.000**	**-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
3:rd quartile on disposable income, mean last 4 years	-0.221	0.286*	-0.027	**0.477**	0.297*
	(0.184)	(0.158)	(0.179)	(0.158)	(0.166)
(any psychiatric diagnose since 1987) * (Working, any of last 4 years)	0.085	**0.630**	**0.486**	0.278	**0.553**
	(0.281)	(0.226)	(0.239)	(0.228)	(0.227)
Constant	**-8.378**	**-7.778**	**-8.382**	**-8.048**	**-7.839**
	(0.242)	(0.212)	(0.255)	(0.247)	(0.244)
Observations (in thousands)	2,052	2,041	2,029	2,020	2,011
Assault year	1998	1999	2000	2001	2002
Pseudo R^2	0.122	0.128	0.131	0.131	0.140

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

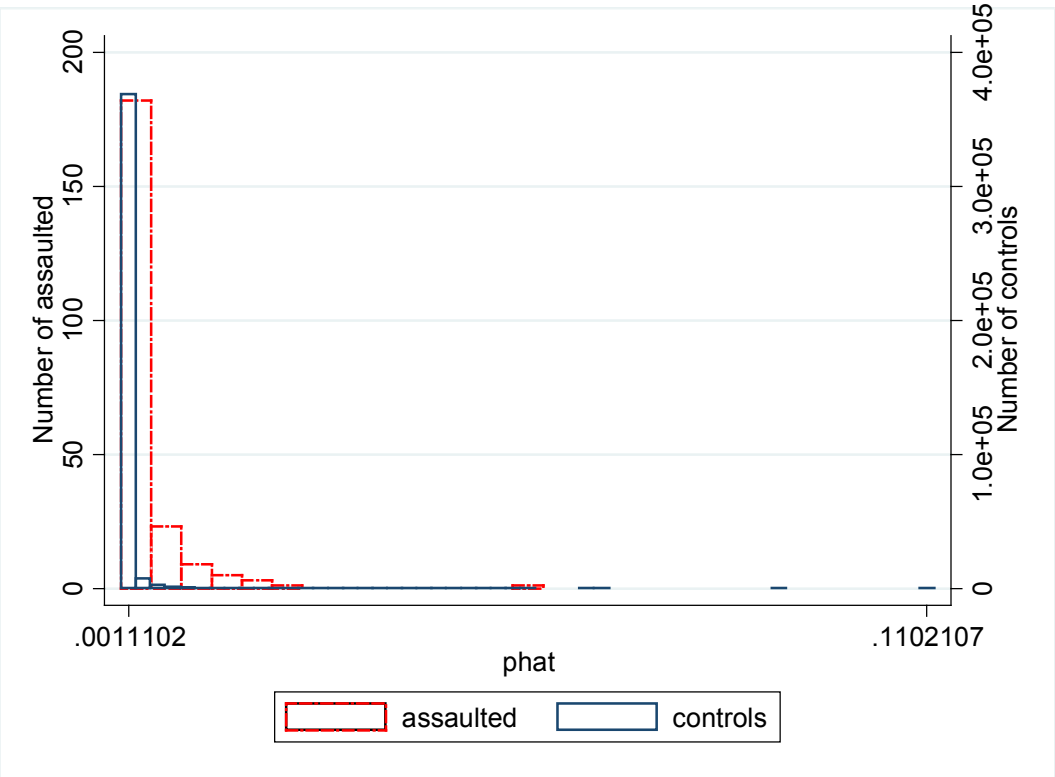
Table A 2 Specification of propensity score estimation for men 1998–2002

Variables	(1)	(2)	(3)	(4)	(5)
married	**-0.489**	**-0.337**	**-0.594**	**-0.243**	**-0.310**
	(0.119)	(0.118)	(0.119)	(0.118)	(0.117)
single, any last 4 years	**0.285**	-0.038	**0.478**	**0.424**	**0.276**
	(0.132)	(0.145)	(0.124)	(0.131)	(0.129)
single, 1 year prior	**0.275**	**0.629**	0.161	**0.357**	**0.338**
	(0.126)	(0.141)	(0.116)	(0.123)	(0.123)
age	**-0.033**	**-0.041**	**-0.041**	**-0.047**	**-0.050**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
(working, 1 year prior) * (age < 30)	0.227	0.137	**0.296**	0.194	**0.274**
	(0.120)	(0.115)	(0.109)	(0.111)	(0.112)
separated during last 2 years	**0.349**	0.084	**0.319**	**0.324**	0.125
	(0.134)	(0.137)	(0.126)	(0.135)	(0.149)
(single, any last 4 years) * (age < 30)	**-0.385**	-0.318	**-0.613**	**-0.513**	**-0.466**
	(0.173)	(0.177)	(0.165)	(0.168)	(0.187)
years of schooling	**0.505**	**0.540**	0.152	0.255	0.261
	(0.214)	(0.150)	(0.202)	(0.106)	(0.105)
(years of schooling)^2	**-0.027**	-0.031	-0.013	-0.015	-0.017
	(0.010)	(0.007)	(0.009)	(0.005)	(0.005)
Swedish born with at least 1 Swedish born parent	**-0.542**	-0.586	-0.563	-0.407	-0.466
	(0.070)	(0.069)	(0.068)	(0.071)	(0.072)
age * (any hospital visit resulting in a psychiatric diagnose, since 1987)	**0.019**	**0.026**	**0.027**	**0.021**	**0.040**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
any hospital visit resulting in a psychiatric diagnose, since 1987	**-0.842**	**-1.033**	**-1.254**	**-0.862**	**-1.461**
	(0.378)	(0.353)	(0.354)	(0.343)	(0.362)
any psychiatric diagnose, last 4 years	-0.283	**-0.663**	**-0.712**	-0.218	**-0.996**
	(0.300)	(0.249)	(0.249)	(0.264)	(0.273)
any psychiatric diagnose, 1 year prior	**0.365**	-0.118	-0.124	0.063	**0.462**
	(0.184)	(0.173)	(0.183)	(0.179)	(0.205)
any psychiatric diagnose, 2 years prior	**0.784**	**1.016**	**0.760**	**1.013**	**1.131**
	(0.223)	(0.214)	(0.212)	(0.230)	(0.246)
any psychiatric diagnose, 3 years prior	**0.658**	0.157	**0.494**	**0.616**	**1.004**
	(0.228)	(0.210)	(0.213)	(0.233)	(0.252)
any psychiatric diagnose, 4 years prior	**1.020**	**0.821**	**1.097**	0.446	**1.116**
	(0.230)	(0.226)	(0.230)	(0.230)	(0.259)
no. of hospital visits, last 4 years	**0.055**	**0.052**	**0.086**	**0.089**	**0.062**
	(0.014)	(0.013)	(0.012)	(0.013)	(0.014)
categorical variable of no. of hospital visits, last 4 years	**0.252**	**0.220**	**0.170**	**0.313**	**0.182**
	(0.055)	(0.052)	(0.050)	(0.048)	(0.050)
any hospital visit, since 1987	**-1.126**	**-1.068**	**-1.193**	**-1.700**	**-1.792**
	(0.113)	(0.109)	(0.105)	(0.108)	(0.109)
no. of hospital visits, 1 year prior	-0.077	-0.010	**-0.176**	**-0.140**	**-0.150**
	(0.045)	(0.037)	(0.048)	(0.044)	(0.055)
any hospital visit, 2 years prior	**-1.225**	**-1.276**	**-1.199**	**-1.438**	**-1.378**
	(0.149)	(0.145)	(0.137)	(0.158)	(0.159)
any hospital visit, 3 years prior	**-1.204**	**-0.925**	**-1.125**	**-1.332**	**-1.403**
	(0.151)	(0.136)	(0.138)	(0.159)	(0.163)

Variables	(1)	(2)	(3)	(4)	(5)
any hospital visit, 4 years prior	**-1.311**	**-1.440**	**-1.650**	**-1.174**	**-1.522**
	(0.152)	(0.151)	(0.154)	(0.150)	(0.167)
at least 2 hospital visits, last 4 years	**1.255**	**0.692**	**1.028**	**1.043**	**1.677**
	(0.194)	(0.202)	(0.198)	(0.213)	(0.212)
at least 4 hospital visits, last 4 years	**0.447**	**0.587**	**0.544**	**0.548**	**0.571**
	(0.188)	(0.171)	(0.174)	(0.186)	(0.206)
(any psychiatric diagnose since 1987) * (any hospital visit last 4 years)	**1.017**	**1.192**	**1.369**	**1.096**	**1.481**
	(0.298)	(0.243)	(0.241)	(0.246)	(0.238)
any sick leave, last 4 years	**0.313**	**0.261**	**0.300**	**0.329**	**0.374**
	(0.076)	(0.076)	(0.075)	(0.075)	(0.072)
any disability insurance, last 4 years	**-0.277**	**-0.375**	**-0.363**	**-0.590**	**-0.438**
	(0.133)	(0.134)	(0.134)	(0.143)	(0.135)
disposable income, mean last 4 years	**-0.000**	-0.000	**-0.000**	**-0.000**	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
earnings, 1 year prior	-0.000	**-0.000**	**-0.000**	**-0.000**	**-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
working, all of last 4 years	**-0.357**	**-0.362**	-0.099	**-0.432**	**-0.439**
	(0.113)	(0.108)	(0.102)	(0.101)	(0.098)
employed but not working, 1 year prior	**-0.406**	-0.164	**-0.265**	**-0.203**	**-0.227**
	(0.098)	(0.096)	(0.096)	(0.101)	(0.102)
working, 1 year prior	**-0.481**	-0.166	**-0.357**	-0.174	**-0.348**
	(0.137)	(0.128)	(0.125)	(0.126)	(0.130)
substance abuse, since 1987	**0.636**	**0.708**	**0.850**	**0.783**	**0.699**
	(0.202)	(0.190)	(0.200)	(0.191)	(0.193)
psychosis, since 1987	**-0.671**	**-0.741**	**-0.980**	**-0.732**	**-0.618**
	(0.228)	(0.228)	(0.255)	(0.232)	(0.229)
(working, any of last 4 years) * (at least 2 hospital visits last 4 years)	**-0.448**	0.039	0.184	**-0.405**	**-0.484**
	(0.170)	(0.157)	(0.158)	(0.176)	(0.185)
(any psychiatric diagnose since 1987) * (at least 2 hospital visits last 4 years)	**-1.169**	-0.201	**-0.485**	**-0.792**	**-1.451**
	(0.242)	(0.239)	(0.233)	(0.245)	(0.256)
constant	**-6.579**	**-6.307**	**-3.922**	**-4.711**	**-4.185**
	(1.196)	(0.796)	(1.122)	(0.615)	(0.599)
observations (in thousands)	2,119	2,104	2,089	2,077	2,067
assault year	1998	1999	2000	2001	2002
pseudo R^2	0.154	0.157	0.167	0.172	0.173

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

Figure A 1 Overlap



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

Table A 3 Costs due to premature mortality

years post	ATT, women		ATT, men	
	cumulative, SEK	yearly, SEK	cumulative, SEK	yearly, SEK
	(1)	(2)	(3)	(4)
1	97,100***	97,101	183,558***	183,559
	(2,424)		(2,510)	
2	178,540***	81,488	327,895***	144,486
	(4,457)		(4,485)	
3	294,434***	115,961	520,867***	193,279
	(7,350)		(7,124)	
4	460,445 ***	166,081	687,482***	167,052
	(11,495)		(9,404)	
5	566,943***	106,549	866,334***	179,219
	(13,127)		(11,850)	
6	676,889***	163,076	1,032,007***	166,087
	(14,154)		(14,116)	
7	955,346***	225,757	1,192,974***	161,647
	(23,850)		(16,318)	
8	1,126,056***	171,304	1,332,290***	140,228
	(28,112)		(18,224)	
average		140,914		166,944

Note: Column (1) and (3) correspond to cumulative ATT effects monetized with the cost of a statistical life in Sweden with year 2016 as base, in Swedish crowns. Column (2) and (4) correspond to yearly effects, calculated on those alive at the beginning of that year. Standard errors in parentheses. ***/ **/* = the estimates are significantly different from zero at the 1/5/10 percent level, respectively.

Table A 4 Effects of assault on mortality, by sex and injury severity

	ATT 8 years post assault, women		ATT 8 years post assault, men	
	Cumulative, percentage points	Relative, percent	Cumulative, percentage points	Relative, percent
Injury severity	(1)	(2)	(3)	(4)
Mild (0–7)	4.166*** (0.108)	13.1	5.113*** (0.075)	8.8
Moderate (8–15)	4.444*** (0.468)	14.0	6.600*** (0.295)	11.4
Major (16–50)	20.000*** (3.627)	63.0	6.759*** (0.561)	11.7

Note: Column (1) and (3) correspond to ATT estimates in percentage points. Standard errors in parentheses. ***/ **/* = the estimates are significantly different from zero at the 1/5/10 percent level, respectively. Relative effects, calculated as the ATT estimate divided by the average match value, are presented in column (2) and (5).

Table A 5 Effects of assault on mortality, by sex and work status

	ATT 8 years post assault, women		ATT 8 years post assault, men	
	Cumulative, percentage points	Relative, percent	Cumulative, percentage points	Relative, percent
Pre-assault work status	(1)	(2)	(3)	(4)
Not working	5.875*** (0.734)	12.1	7.862*** (0.983)	8.3
Working	2.133*** (0.267)	64.0	2.742*** (0.343)	13.1

Note: Column (1) and (3) correspond to ATT estimates in percentage points. Standard errors in parentheses. ***/ **/* = the estimates are significantly different from zero at the 1/5/10 percent level, respectively. Relative effects, calculated as the ATT estimate divided by the average match value, are presented in column (2) and (5).

Table A 6 Effects of assault on the probability to work, by sex

	Women			Men		
	Yearly (percentage points)	Yearly; effects due to mortality excluded (percentage points)	Yearly (percent)	Yearly (percentage points)	Yearly; effects due to mortality excluded (percentage points)	Yearly (percent)
Years post	(1)	(2)	(3)	(4)	(5)	(6)
1	-7.43*** (0.186)	-7.40*** (0.185)	-16.6	-6.50*** (0.089)	-6.27*** (0.086)	-11.3
2	-8.94*** (0.223)	-8.825*** (0.221)	-19.2	-5.91*** (0.081)	-5.51*** (0.076)	-10.1
3	-10.95*** (0.273)	-10.89*** (0.274)	-22.8	-5.68*** (0.078)	-4.94*** (0.068)	-9.6
4	-10.92*** (0.273)	-10.67*** (0.269)	-22.4	-6.59*** (0.090)	-5.63*** (0.078)	-11.1
5	-11.449*** (0.286)	-11.17*** (0.282)	-23.4	-7.08*** (0.097)	-5.86*** (0.081)	-11.7
6	-10.845*** (0.271)	-10.34*** (0.262)	-21.7	-8.17*** (0.112)	-6.65*** (0.093)	-13.3
7	-11.60*** (0.290)	-10.91*** (0.278)	-22.8	-7.78*** (0.106)	-6.09*** (0.086)	-12.6
8	-12.73*** (0.318)	-11.89*** (0.304	-24.6	-7.60*** (0.104)	-5.57*** (0.079)	-12.2
Average	-10.61	-10.27	-21.7	-6.91	-5.81	-11.5

Note: Column (1) and (3) correspond to ATT estimates in percentage points. Relative effects (in percent) presented in column (2) and (4). Relative effects are calculated as the ATT estimate divided by the average match value (relative risk). Dead individuals are assumed to have zero probability to work. Standard errors in parentheses. ***/ **/* = the estimates are significantly different from zero at the 1/5/10 percent level, respectively.

Table A 7 Effects of assault on gross earnings, by sex and work status

Pre-assault work status	ATT 8 years post assault, women		ATT 8 years post assault, men	
	(1)	(2)	(3)	(4)
Not working	-31,553*** (912)	29.8	-40,013*** (696)	23.6
Working	-69,277*** (2,709)	26.7	-38,857*** (716)	10.9

Note: Column (1) and (3) correspond to ATT estimates in SEK. Standard errors in parentheses. ***/ **/* = the estimates are significantly different from zero at the 1/5/10 percent level, respectively. Relative effects, calculated as the ATT estimate divided by the average match value, are presented in column (2) and (4).

Table A 8 ATT estimates of assault on disposable income, by sex

	Women		Men	
	Yearly (SEK)	Yearly (percent)	Yearly (SEK)	Yearly (percent)
Years post	(1)	(2)	(3)	(4)
1	1,225 ***[30]	0.8	-6,691***	-4.5
	(30)		(90)	
2	-824***	-0.5	-9,097***	-5.9
	(20)		(122)	
3	-4,025***	-2.6	-9,855***	-6.1
	(97)		(131)	
4	-5,941***	-3.7	-12,230***	-7.3
	(143)		(161)	
5	-3,909***	-2.4	-16,612***	-9.3
	(93)		(216)	
6	-7,366***	-4.4	-18,578***	-9.9
	(174)		(240)	
7	-8,628***	-5.0	-21,354***	-10.8
	(202)		(273)	
8	-13,548***	-7.6	-21,711***	-10.5
	(315)		(275)	
Cumulative	-43,015		-116,126	
Average	-5,376	-3.2	-14,515	-8,0

Note: Column (1) and (3) correspond to ATT estimates in SEK. Dead individuals are assumed to have a disposable income of zero. Estimates are reported with year 2016 as base. Standard errors in parentheses. ***/ **/* = the estimates are significantly different from zero at the 1/5/10 percent level, respectively. Relative effects (in percent), calculated as the ATT estimate divided by the average match value, are presented in column (2) and (4).

Table A 9 Effects of assault on days on sick leave, by sex

	Women		Men	
Years post	(1)	(2)	(3)	(4)
1	25.7***	24.6	13.7***	22.4
	(0.642)		(0.187)	
2	28.9***	25.9	10.7***	16.1
	(0.722)		(0.147)	
3	31.7***	27.4	13.6***	19.3
	(0.791)		(0.186)	
4	31.8***	26.6	16.1***	22.0
	(0.795)		(0.221)	
5	34.6***	28.3	18.6***	25.2
	(0.864)		(0.255)	
Average	30.6	26.6	14.5	21.0

Note: Column (1) and (3) correspond to ATT estimates in days. Dead individuals are assumed to have 365 days of sick leave each year. Standard errors in parentheses. ***/ **/* = the estimates are significantly different from zero at the 1/5/10 percent level, respectively. Relative effects (in percent), calculated as the ATT estimate divided by the average match value, are presented in column (2) and (4).

[30] The surprising direction of the one-year effect is driven solely by women not working prior to victimization, while women previously working have negative effects on disposable income all follow-up years. There seems to be an interesting study to be made looking at the effects of domestic violence on separation and shifts in *sources* of income.