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Department of Economics
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Backlash: Undesirable Effects of Female Economic Empowerment

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Backlash: Undesirable Effects of Female Economic Empowerment[☆]

Sanna Ericsson¹

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Abstract

This paper estimates the effect of female economic empowerment on domestic violence. I use individual level data from high-quality Swedish administrative registers on women's earnings and hospital visits relating to assault. With this third-party reported violence measure I overcome the issue of selective under-reporting of violence. I proxy female economic empowerment with a measure of women's potential earnings, caused by local changes in female-specific labour demand. This measure reflects the outside option of the marriage, and captures earnings variation that is not endogenous to domestic violence. I show that, even while keeping the earnings of husbands constant, the causal effect of increasing women's potential earnings on domestic violence is positive and substantial. In addition, I show that increasing women's potential earnings increase the husbands' risk of destructive behaviour, such as stress, anxiety, substance abuse and assault. Taken together, these results indicate that improving women's relative economic position triggers a male backlash response, even in presumably gender-equal Sweden.

Keywords: domestic violence, potential earnings, household bargaining, male backlash, local labour demand

JEL codes: D13, I12, J12, J16, Z13.

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1. Introduction

Domestic violence is a major issue for public health, productivity and gender equality. Globally, one in three women will experience violence from a partner at some point during their lifetime (García-Moreno et al., 2013), and the global cost of total intimate partner violence is estimated to be 5.2% of world GDP (Hoeffler, 2017). Female economic empowerment is often cited as one of the most effective ways to combat domestic violence, but the theoretical predictions diverge. Models of household bargaining predict that a better relative economic position of the wife improves the outside option of the marriage, and, as a result, reduces violence (Farmer and Tiefenthaler, 1997; Aizer, 2010).¹ In contrast, models of male backlash predict that an improved relative economic position of the wife increases violence, as it violates traditional gender norms and redefines the power relationship between the spouses, which could trigger a violent backlash response from the husband (Macmillan and Gartner, 1999).²

Investigating the relationship between female economic empowerment and domestic violence offers several empirical challenges. First, domestic violence is a sensitive topic that is prone to selective under-reporting (Ellsberg et al., 2001). Self-reported measures might not be representative of actual violence, but rather, the selection in who reports a violent incident. As the probability of reporting a violent incident is likely to increase with empowerment (Iyer et al., 2012), it is important to distinguish between changes in violence and changes in reporting behaviour.

Second, analysing aggregate measures carries a risk of ecological fallacy conclusions.³ This issue becomes especially important with a relatively rare outcome such as domestic violence, for which data sparsity can lead to aggregate measures that do not properly represent the underlying distribution of violence.

Third, an important threat to identification is earnings endogeneity, as re-

¹An improved economic position raises the threat point of the wife by improving her outside option of the marriage. The outside option is the situation she would face in case of a marriage dissolution. A higher threat point has a negative effect on violence, both indirectly, through more women leaving abusive spouses, and directly, through the deterrent effect of the threat of leaving.

²An improved economic position of the wife redefines the relationship of power between the spouses and violates traditional gender norms, which could increase violence as a way for the husband to take back authority over his wife.

³An ecological fallacy occurs when we make inference about individuals based on inference about the groups to which the individuals belong.

alised earnings likely reflect unobserved individual characteristics, which could be an outcome of, or correlate with, violence. Furthermore, the outside option of a marriage is not determined by a woman's realised earnings, but rather by the earnings potential she would face in case of a marriage dissolution (Aizer, 2010). The relative level of empowerment within a marriage depends on the outside option, and, therefore, on potential earnings rather than realised earnings. Furthermore, assortative matching will create selective marriages that are functions of earnings and of the underlying propensity for both perpetrating violence and for staying in a violent relationship (Pollak, 2004). This makes relative earnings a problematic measure of economic empowerment within a household.

In this paper I study the effect of female economic empowerment on domestic violence, and I overcome the previously mentioned empirical challenges. I use high-quality Swedish administrative data, which enables me to observe both earnings and violence on an individual level. I measure domestic violence using hospital visits for assault, which I derive from third-party reported hospital records. A unique feature of my data is that detailed information on hospital visits for accidents allows me to investigate possible misreporting at the hospital, and this way, I am able to conclude that my study do not suffer from bias of non-random misreporting. Furthermore, individual level data excludes the risk of ecological fallacies.

To overcome the empirical challenges of earnings endogeneity, I derive a measure of women's potential earnings, which captures exogenous variation in female economic empowerment caused by only local demand changes for female labour. I exploit the fact that women and men tend to sort into different industries and create gender-specific measures of prevailing local earnings potential for women. This measure captures earnings variation that is not endogenous to domestic violence, and it provides a more accurate representation of the outside option of a marriage.

I show that the causal effect of increasing women's potential earnings, while keeping the earnings of their husbands constant, increases the risk of assault. The effect is substantial in magnitude, and does not depend on which spouse earns more than the other. Thus, my results are in line with the predictions of male backlash theory, as they show that an improved relative economic position for the wife increases the risk that she experiences assault. As further support for the backlash mechanism, I show that increasing women's potential earnings, while

keeping the earnings of their husbands constant, increase the risk of husbands' destructive behaviour, such as visiting a hospital for reasons related to depression, anxiety, substance abuse and assault.

The richness of my data allows me to conduct a detailed heterogeneity analysis, where I show that the effect of women's potential earnings differ depending on the age and education level of the woman. For the youngest women the effect of increased potential earnings is negative, but after the age of 40 the effect is consistently positive. Likewise, for the women with no more than high school education potential earnings reduce the risk of assault, but for women of higher education levels potential earnings increase the risk. These results indicate that the women for whom a change in potential earnings actually affects the credibility of their threat of leaving, such as for the younger women or for the least educated; where the outside option may be binding, the results are in line with the predictions of bargaining power theory. But for the women who may have the economic possibility to leave, but still do not, the effects are in line with male backlash theory. In line with this reasoning, I show that the backlash effect increase also with the duration of the marriage.

My paper relates to the growing and diverse literature on female empowerment and domestic violence. Several well-identified empirical studies, mostly from developed countries, find support for the bargaining power hypothesis, i.e. that female empowerment reduces domestic violence. [Brassiolo \(2016\)](#) and [Stevenson and Wolfers \(2006\)](#) show that the introduction of unilateral divorce laws in Spain and the US led to large reductions in domestic violence, and [Anderberg et al. \(2016\)](#) find that spousal abuse varies negatively with male unemployment but positively with female unemployment.⁴

In contrast, several studies, mostly from developing countries, find support for the male backlash theory, i.e. that female empowerment increases violence. Recent studies show that female employment varies positively with spousal abuse in many developing countries ([Heath, 2014](#); [Cools and Kotsadam, 2017](#); [Bhalotra et al., 2018](#)). [Chin \(2012\)](#) and [Guarnieri, Rainer et al. \(2018\)](#) show further support for a backlash response, by using rainfall shocks or historic institutional

⁴In a developing country context, [La Mattina \(2017\)](#) shows that Rwandan women who married after the genocide in 1994 experience more domestic violence, consistent with the hypothesis that a shortage of men led to reductions in women's bargaining power in the marriage market.

differences to identify exogenous variation in women’s employment opportunities.

My paper also relates to a strand of the literature that connects domestic violence prevalence to the concept of gender identity. The gender identity model in economics was introduced by [Akerlof and Kranton \(2000\)](#). The connection to relative earnings and domestic violence was first established by [Atkinson, Greenstein, and Lang \(2005\)](#). They construct a measure of “traditionalism” using couples level of agreement with various statements,⁵ and find that the relative earnings of wives is only positively correlated with violence if the wives are married to traditional husbands. More recently, [Svec and Andic \(2018\)](#) show that women who have higher earnings than their partners are more at risk of experiencing domestic violence in Peru, and [Alonso-Borrego and Carrasco \(2017\)](#) find that a woman’s employment only reduces violence when her partner is also employed. [Tur-Prats \(2017\)](#) finds that male backlash responses to female relative employment only exists for couples who live in areas that historically contained families with more traditional gender norms.⁶

Finally, a recent strand of the literature finds that domestic violence can be triggered by negative emotional cues or psychological stress ([Card and Dahl, 2011](#); [Cesur and Sabia, 2016](#); [Beland and Brent, 2018](#)). Losing relative economic power to one’s wife could possibly cause such stress, which is what I show when I investigate the mechanisms of my results.

The paper closest to mine is [Aizer \(2010\)](#), who studies the effect of changes in the gender wage gap on aggregate measures of female hospitalisations for assault in Californian counties. She exploits demand-driven exogenous variation in the gender wage gap and finds that narrowing the wage gap leads to reduced levels of female hospital admissions for assault, a relationship that is consistent with the bargaining power hypothesis. My results stand in contrast to those of [Aizer \(2010\)](#), as I find a positive effect of women’s potential earnings on domestic violence. However, my heterogeneity analysis reveal a negative relationship

⁵In the economics literature traditional gender norms are usually measured by how much couples agree with statements such as “A mother can work full-time when she has a child under the age of 5”, “It is much better for everyone if the man earns the main living and the woman takes care of the home and family”, or “If a woman earns more money than her husband, it is almost certain to cause problems” (e.g. [Atkinson, Greenstein, and Lang, 2005](#); [Bertrand, Kamenica, and Pan, 2015](#)).

⁶In addition, macro-level evidence shows that domestic violence prevalence is higher in countries with more traditional gender norms ([Heise and Kotsadam, 2015](#); [González, Rodríguez-Planas et al., 2018](#))

between potential earnings and assault for the lowest educated women, which are the only ones included in the study of Aizer (2010). Furthermore, I replicate Aizer’s study on Swedish data and I find a negative relationship between the aggregate gender wage gap and municipality-level of assault (both hospitalizations and police reports), whereas I still find a positive effect on the individual level with an outcome measure that is as close as possible to Aizer’s. Taken together, these results indicate that an aggregate study may mask what is really happening on the individual level, and possibly result in an ecological fallacy conclusion.

My contributions to the literature are threefold. First, my study is the first to use a close to objective measure of violence from individual level data. Related to this, my study is also the first to investigate possible misreporting at the hospital, which allows me to conclude that my study is not suffering from reporting bias.

Second, I show that the effect of increased potential earnings on domestic violence differs sharply for different subgroups of the population. This way, I show that both effects in line with the bargaining power hypothesis and effects in line with the theory of male backlash can co-exist, depending on the subgroup of the population and, speculatively, on how credible their threat of leaving an abusive spouse is.

Third, I investigate the mechanisms behind the positive effect of women’s potential earnings on domestic violence. By estimating the effect of women’s potential earnings on various measures of husbands’ destructive behaviour, I show that the mechanisms are in line with a male backlash response to an improved relative economic position of the wife. This result is especially interesting to find in a developed, and presumably gender-equal, country like Sweden.

The remainder of the paper is organised as follows. In Section 2, I introduce the data and the dependent and independent variables; Section 3 outlines the empirical strategy; Section 4 presents the results; Section 5 ensures that they are robust; and Section 6 concludes.

2. Data

My dataset comes from the Swedish Interdisciplinary Panel (SIP). It is a two-generational dataset covering all individuals born in Sweden between 1973 and 1995 and their parents, both whose outcomes I observe during 2001–2011. I acquire the indicator of violence exposure from individual level hospital records,

which cover both in-patient and out-patient visits. The in-patient records refer to all hospital visits that last for at least one over-night stay at the facility. The out-patient records do not cover primary care, but contain all other contacts with specialised care providers. Most importantly for my study, they contain all contact with medical doctors that takes place in emergency rooms. Compared to the data used by [Aizer \(2010\)](#), my data contain a wider array of hospital visits, including less severe cases that do not require overnight hospitalisation.⁷

My dependent variable is a binary indicator of visiting a hospital for an injury caused by assault during the current year.⁸ I identify cases of assault using ICD-10 diagnosis codes, reported by medical personnel at the moment of the visit.⁹ In order to isolate assaults that are likely of a domestic nature, I use only assaults that took place at home or in unspecified locations.¹⁰ Measurement error due to violence committed by someone other than an intimate partner is unlikely, as the most common perpetrator of female assault is someone close to the victim ([Frenzel, 2014](#)).¹¹ Figure 1 shows the average number of hospital visits for assault that occurred at home or in unspecified locations, and the average number of filed police reports for domestic assaults, for each year of my study. Both measures depict a positive time trend, indicating that domestic violence has increased during the first decade of the 21st century.

My final dataset is an unbalanced panel consisting of married women of working age, i.e. 20–65 years old.¹² The final dataset contains 7,965,166 observations spanning over 1,046,867 individuals. Table 1 shows the summary statistics. The

⁷As a robustness check I perform my analysis using the outcome measure closest to [Aizer \(2010\)](#), see column (4) of Table 8, and the results do not change.

⁸Some women visit the hospital multiple times a year for assault-related injuries, but these incidences are rare. However, my results are robust to using a count variable instead of a binary indicator.

⁹Each ICD-10 code contains a capital letter denoting the broad category of the diagnosis, followed by a sequence of digits. If the diagnosis was externally caused it contains a similar code denoting the circumstances of the external cause; this is where I can isolate cases of assault. Codes X85–Y09 denote assault as the cause of injury, both physical and sexual. The fourth letter of the code denotes where it took place.

¹⁰Assaults taking place, e.g. in workplaces, bars, or public events are, therefore, not included in my main sample. Unspecified location is kept as discussions with medical personnel at Swedish emergency rooms revealed that this notation is often used to save time during stressful situations. However, my results are robust to including only the assaults that took place at home, see column (2) of Table 7.

¹¹In 72% of physical assaults against women the perpetrator is someone close to the victim, a number which is likely understated as the probability of making a police report is lower when the perpetrator is someone close.

¹²I exclude same-sex married couples.

women are on average 46 years old, and 8% of them will get a divorce within the time frame of my study. As I restrict the sample based on marriage, a large fraction of my sample consists of the parental generation, who are sampled simply because they are parents to someone born between 1973 and 1995, therefore, the fraction of women who have kids is unusually high. Each year, on average 0.39 women per 1,000 visit a hospital due to assault.¹³ Most women in my sample only visits a hospital for assault once during my time frame, which contradicts the notion that domestic violence is often a recurring event.¹⁴ However, my data contain mainly aggravated assault and, therefore, only the tip of the iceberg of true violence numbers, as any minor assaults that do not result in a hospital visit are not captured in my study.

2.1. Selection

Selection in the probability of visiting a hospital, given an assault, is unlikely to be a large issue for my results. [Frenzel \(2014\)](#) show that the number of assaulted women who visit a hospital is substantially higher than that of those who file a police report, which implies that women seek medical care out of need and severity of injury rather than by choice.¹⁵ Selection due to income constraints is unlikely as visiting a hospital or health care unit in Sweden is free, except for a small fee of, at most, 1150 SEK (about 110 Euro) per year.¹⁶

However, the risk of experiencing an assault is not randomly distributed among the population. Rather, I expect it to correlate with various socio-demographic measures. Figure 2 shows that the risk of assault significantly

¹³Average assault without the marriage restriction 0.76 per 1,000 women. Compared to [Aizer \(2010\)](#) my average number of assaults, including every hospital visit, is about four times as common as what she finds in California, which is reasonable as Swedish hospital records contain a wider array of visit types. My numbers for in-patient hospitalisations only are similar to [Aizer \(2010\)](#).

¹⁴Over 20% of the women who reported to have been subjected to aggravated assault in 2012 stated that violence occurred several times a week; another 20% experienced violence several times a month; and the remaining women stated that that violence occurred between one and a few times a year ([Frenzel, 2014](#)).

¹⁵The number of women who suffered from physical intimate partner violence in 2012, who later filed a police report, was only 4%. In contrast, 12% of the assaulted women, and 29% of those who suffered from aggravated assault, contacted health care services.

¹⁶Visits are confidential and everyone working in a hospital is bound to professional secrecy by law. However, there are some exceptions. If a child is believed to be in danger, hospital staff are obligated to report this to the appropriate authority. If a crime with minimum punishment of one year in prison has been committed, staff have the option of reporting this to the police. Thus, according to Swedish law, hospital staff have the possibility to report cases of aggravated assault, but it is unlikely that they would do so against a patient's will.

decreases with age, education level, earnings and household income. These correlations highlight the importance of adjusting flexibly for different socio-demographic characteristics when investigating the determinants of domestic violence.

Another important, but unobserved, factor is selective marriage matches. Pollak (2004) argues that growing up in a violent home makes men more prone to violence and women more prone to staying in a violent marriage. Moreover, men and women who grew up in violent homes are more likely to marry each other, which makes assortative matching in the marriage market a crucial determinant of domestic violence. As men and women are likely to marry assortatively based on earnings as well, this selection could bias my results if it is not accounted for.

3. Empirical strategy

3.1. Descriptive relationship between relative earnings and domestic violence

As a descriptive exercise, I begin by defining female economic empowerment as spouses' relative earnings, which I measure as wives' share of half of the household labour earnings, as follows:

$$RelativeEarnings_{it} = \frac{WifesEarnings_{it}}{(WifesEarnings_{it} + HusbandsEarnings_{it})/2} - 1 \quad (1)$$

The measure spans between -1 and 1, where -1 means that the husband earns 100% of the household earnings, 0 implies perfect income equality and 1 means that the wife earns 100% of the household earnings. I measure earnings as labour income, and as the measure is derived from individual tax registers, it contains very little measurement error.¹⁷

The richness of my data allows me to model the relationship between spouses' relative earnings and domestic violence non-parametrically, without imposing any assumptions on its functional form.¹⁸ In addition, I estimate the descriptive relationship parametrically, using a linear probability model. I sequentially add demographic controls and indicators to adjust for any observed characteristics,

¹⁷Statistics Sweden estimated that the Swedish informal labour market in 2008 was about 3% of GDP.

¹⁸I use kernel-weighted local linear scatterplot smoothing for the estimations.

and I allow the effect of changes in relative earnings to differ depending on which spouse earns more than the other. I estimate the following equation:

$$\begin{aligned}
Assault_{it} = \alpha + \beta RelativeEarnings_{it} + \gamma RelativeEarnings_{it} \times H \leq W_{it} \\
+ H \leq W_{it} + Z'_{it}\delta + X_i + \epsilon_{it} \quad (2)
\end{aligned}$$

Where $H \leq W_{it}$ is a binary variable that takes the value of one if wives earn an equal wage to, or more than, their husbands. X_i contains indicators for age group, household income quantiles, municipality of residence, year, cohort, and education level of both spouses. Z_{it} contains linear controls for spousal age span and municipality-level annual filed police reports for assaults against women. The coefficients of interest are the respective sums of β and γ , which capture the differential effects along the relative earnings distribution.

Second, to account for unobserved heterogeneity not captured by the linear model (e.g. selective marriage matches), I exploit the panel dimension of my data and introduce individual fixed effects. I estimate the following model:

$$\begin{aligned}
Assault_{it} = \beta RelativeEarnings_{it} + \gamma RelativeEarnings_{it} \times H \leq W_{it} \\
+ H \leq W_{it} + Z'_{it}\delta + \eta_i + \epsilon_{it} \quad (3)
\end{aligned}$$

Where η_i , the individual fixed effects parameter, captures any unobserved heterogeneity that is time-invariant. This model is more restrictive as it only utilises the within-variation over time for identification, thus holding constant all marriage matches and other time-invariant determinants of relative earnings and domestic violence. However, a fixed effect model cannot fully account for the endogeneity of earnings, as any time-varying unobserved heterogeneity or reverse causality will not be captured by η_i .

3.2. Identifying the effect of female economic empowerment: women's potential earnings

The most important threat to identification is endogenous earnings. Realised earnings likely reflect underlying characteristics of the individual, which could be a function of underlying violence (abused women are less productive) or un-

observables that might correlate with violence. Furthermore, the outside option of a marriage is not determined by a woman’s realised earnings, but rather, by the earnings potential she would face in case of a marriage dissolution (Aizer, 2010).¹⁹ The relative level of economic empowerment within a marriage depends on the outside option, and, therefore, on spouses’ earnings potential rather than realised earnings.

To account for earnings endogeneity, and for the fact that theory predicts that potential, rather than actual, earnings determine the bargaining power of a woman, I construct a measure of prevailing female earnings that reflects only the exogenous demand for female labour. I exploit the fact that men and women tend to sort differentially across industries (for example women are overrepresented in the health and service sector, whereas men sort into manufacturing and construction) and that these industries experience different wage growth over time. This approach builds on previous work by Bartik (1991), Aizer (2010), Bertrand, Kamenica, and Pan (2015) and Lindo, Schaller, and Hansen (2018), and isolates gender-specific variation in earnings, driven only by changes in local labour demand. Thus, it is a measure of women’s local earnings potential. I allow the measure to vary by age and education, to take into account that wages are usually set depending on education level and that they tend to increase with experience. I construct the measure of women’s potential earnings as:

$$PotentialEarnings_{maet} = \sum_j \gamma_{maej,2000}^f \times w_{eajt,-m}^f \quad (4)$$

Where f denotes female, m municipality of employment, a age group, e education group, j industry, and t year. $\gamma_{maej,2000}^f$ is the share of women, in a given age and education group, who work in industry j in municipality m in the base year 2000. This proportion is fixed, so changes in earnings do not reflect selective sorting across industries. Variable $w_{eajt,-m}^f$ is the average national annual earnings in year t in industry j for women of a given age and education group, excluding municipality m . Consequently, the potential earnings of a woman, of a given age and education level, is higher the larger the share of similar women in her municipality who are employed in industries with high national wage growth is.

¹⁹Women’s earnings at the threat point determine the bargaining power, and earnings at the bargaining equilibrium do not necessarily equal earnings at the threat point. Pollak (2005) provides an example of a married woman who does not work (zero wages) at the cooperative equilibrium but who would work in the event of the dissolution of the marriage.

I test the validity of the potential earnings proxy in two ways. First, one advantage of my data is that I can ensure that the potential earnings measure is correlated with realised, individual and relative, earnings. The first six columns of Table 2 show the estimated correlations between wives' and husbands' potential earnings and realised wives', husbands' and relative earnings. Reassuringly, the estimated correlation between wives' potential and realised earnings and is positive, sizeable and precise, which shows that potential earnings is an appropriate proxy for capturing earnings variation of women.

Second, a correlation between wives' (or husbands') potential earnings and realised earnings of the other spouse implies that the potential earnings measure is a more general measure of labour market shifts, and do not only capture gender-specific earnings shifts as it should. Column (2) of Table 2 shows that wives' potential earnings are, as they should, uncorrelated with the realised earnings of husbands. However, column (4) shows that husbands' potential earnings are positively correlated with wives' realised earnings, which implies that husbands' potential earnings are a less appropriate measure of earnings shifts for men only. Taken together, these validity tests show that women's potential earnings is an appropriate measure to capture female earnings variation that is not endogenous to domestic violence, but that husbands' potential earnings (and therefore also potential relative earnings) are more problematic.²⁰

To capture the causal effect of women's potential earnings, as a proxy for relative economic empowerment, I estimate the following model:

$$\begin{aligned}
 Assault_{it} = & \alpha + \beta PotentialEarnings_{it} + \gamma PotentialEarnings_{it} \times H \leq W_{it} \\
 & + H \leq W_{it} + \delta HusbandsEarnings_{it} + Z'_{it}\theta + X_i + \epsilon_{it} \quad (5)
 \end{aligned}$$

Again, $H \leq W_{it}$ is a binary variable that takes the value of one if wives earn

²⁰Finally, following [Goldsmith-Pinkham, Sorkin, and Swift \(2018\)](#), I verify that no single industry contributes the majority of the identifying variation in potential earnings. Appendix Figure A1 shows the distribution of Rotemberg weights. Each weight corresponds to the misspecification elasticity of each industry-period pair, and measures how sensitive the parameter estimate is to each instrument. Although this test is mainly required for using industry shares as instruments, it is still reassuring that, if they were to be used as instruments, Appendix Figure A1 shows that the identifying variation would be dispersed among several different industries. The industry contributing the largest share of the identifying variation is the telecommunication sector, followed by the financial sector.

an equal wage to, or more than, their husbands. X_i contains indicators for age group, household income quantiles, municipality of residence, year, cohort, and education level of both spouses. Z_{it} contains linear controls for spousal age span and municipality-level annual filed police reports for assaults against women. I keep the earnings of the husbands constant, to identify variation in relative female economic empowerment that is driven solely by changes in local labour demand for women. I allow the effect to differ depending on which spouse earns more than the other. The coefficients of interest are the respective sums of β and γ , which capture the differential effect of women’s potential earnings, along the realised relative earnings distribution.

The identifying assumption is that women’s potential earnings is as good as random, conditional on observables. Assuming that unobservable characteristics relating to domestic violence do not deviate from the *municipality * agegroup * educationgroup* trend when its economic conditions deviate from the trend, this approach will uncover the causal effect of women’s earnings potentials.

The estimated effect of women’s potential earnings will consist of two mechanisms; first, as column (3) of Table 2 shows, women’s potential earnings varies positively with realised relative earnings, which means the measure is partly capturing the effect of increasing relative earnings. Second, regardless of realised earnings, an increase in potential earnings implies an improved outside option of the marriage, which may also have an effect on domestic violence. I cannot distinguish between these two mechanisms, but as they both imply increased relative economic empowerment for wives there is no need to. However, this means that using potential earnings as an instrument to capture exogenous variation in only relative earnings is not appropriate.²¹

4. Results

4.1. Descriptive relationship between relative earnings and domestic violence

Figure 3 shows the non-parametric relationship between relative earnings and hospital visits for assault. Figure 4 shows the same relationship, but excluding those with zero earnings. Both Figures show that the relationship is U-shaped;

²¹A full IV analysis requires strong additional identifying assumptions, such as an exclusion restriction, which is an inherently untestable assumption that I believe is hard to argue for in this setting. For this reason, I focus only on the reduced form model using potential earnings as a proxy rather than an instrument.

such that higher resource inequality is associated with higher levels of assault. Consequently, the effect of a change in relative earnings differs depending on whether the husband or wife earns more than the other. An increase in the wife’s relative earnings when the husband still earns more than her is associated with lower levels of violence. However, increases in relative earnings are positively associated with violence when the wife earns more than the husband.

Table 3 shows that this descriptive relationship holds in parametric estimations. The first three columns show the results for the linear probability model, which allows for variation both between and within couples, whereas the fourth column includes individual fixed effects and controls for everything time-invariant that varies across couples. Column (3) shows the preferred specification for the linear probability model. When I allow the slope of the relationship to differ depending on which spouse earns more than the other, the estimates reproduce the results of Figure 3 and 4. The coefficients show the effect of increasing relative earnings with one standard deviation (0.53). For couples in which the wife earns more than the husband, this increase is associated with a 0.046 increase in the risk of assault, which corresponds to a change of about 18% of the mean. In contrast, column (4) shows that when individual fixed effects are added to the model, the point estimates indicate an overall positive relationship between relative earnings and assault, however, they are less precisely estimated.

4.2. The effect of women’s potential earnings on hospital visits for assault

Table 4 shows the effect of women’s potential earnings on the risk of visiting a hospital for assault. All specifications are keeping the earnings of the husband constant, to identify variation in relative economic empowerment that is driven only by exogenous changes in local demand for female labour. Column (3) shows the preferred specification.²² The coefficients are positive, statistically significant and large in magnitude. A one standard deviation increase in women’s potential earnings (46’ SEK) increases the risk of assault by 0.246, which corresponds to an effect size almost 100% of the mean.²³ Furthermore, the effect is positive on both intervals of relative earnings, which, in line with the fixed effects results, shows that the causal effect of increased potential earnings on domestic violence

²²Individual fixed effects are not appropriate for this model, as the earnings proxy varies at group level.

²³Equivalent, an effect size of 0.246 corresponds to about 1.7% of the standard deviation in hospital visits for assault.

is positive overall, and does not depend on which spouse earns the most.

My results are in line with the predictions of male backlash theory, as I show that increases in women's potential earnings, as a proxy for increasing relative economic empowerment, almost doubles the risk of a hospital visit for assault, compared to the mean value. According to the theory of male backlash, the main driver of the backlash response is the violation of traditional gender norms and the stress and anxiety this causes for the husband. Unfortunately administrative data do not allow me to investigate the gender norm channel directly, but, in order to investigate the mechanisms behind my results further, I test whether there is a relationship between women's potential earnings and various measures of husbands' destructive behaviour. Table 5 shows that increasing women's potential earnings, while keeping the earnings of their husbands constant, increases the risk of the husband visiting a hospital for reasons related to depression, anxiety and stress (i.e. mental instability), substance abuse, and for himself having been assaulted. Interestingly, I find a very small, and only marginally significant, effect for husbands' assault that took place at home or in unspecified locations (my measure of domestic violence). These results indicate that the increased assault risk of husbands is for assaults taking place in other places such as bars, sports arenas, public places etc. I also show that women's potential earnings increase the risk of divorce, although, this effect is likely to be endogenous to domestic violence, as assault is a strong predictor of divorce.

4.3. Heterogeneity in the effect of women's potential earnings

The response to an improved relative economic position may differ depending on the socio-demographic characteristics of the woman. For example, a threat of leaving a spouse may be more credible for younger women who are in a newer relationship, than for those who have already stayed in a possibly toxic relationship for a long time. Figure 5 shows how the effect of increasing women's potential earnings differ by the age of the woman. Interestingly, the effect is negative for the younger women, but it grows with age and stabilises at a positive level from around age 40 and onwards. Although the youngest age group is a quite small (about 6% of the total sample) and selective sample of those women who married when they were young, the negative estimate indicates that the effect of an im-

proved relative economic position for the wife differs with age.²⁴ Speculatively, the mechanism behind these differences could be the credibility of a threat of leaving, which is likely to be higher for young women but declines with the time she chooses to remain in the marriage. In support of this argument, Figure 6 allows the effect to differ by length of the marriage.²⁵ The Figure shows that potential earnings has no effect on the risk of assault for the youngest marriages, but that the backlash effect grows steadily with marriage duration.

Education and income level may also influence spouses' possible responses to increased potential earnings. Figure 7 show that the women with no more than compulsory schooling are less likely to experience assault as their potential earnings increase, whereas the the assault risk is increasing with potential earnings for women with higher education levels. One possible explanation is that the outside option is binding for the lowest educated, as their labour market opportunities are fewer. Thus, once the outside option improves, their threat of leaving abusive spouses becomes more credible as they now may have the economic possibility to break up the marriage. In contrast, women with higher education levels most likely have the possibility to support themselves economically, hence, staying with an abusive spouse is more of a choice than a necessity. In line with this reasoning, Figure 8 show that women with a realised income in the bottom quintile experience a smaller positive effect of increased potential earnings. Figure 9 show that the effect does not differ by total household income.

Finally, the effect of increasing women's potential earnings may differ depending on civil status. My main result contain only married women, as those are the only ones where I can be sure that they have an intimate partner. However, being in a relationship without being married, either domestic or living apart, is very common in Sweden. To ensure that my results are not driven by a selective population of those who marry, I investigate whether the effect differs by civil status. For this reason, I use a larger sample of all women in my dataset, regardless of marriage status. That the perpetrator of an assault is an intimate partner becomes a stronger assumption to make for this population, as I cannot observe whether they have an intimate partner or not. However, as most

²⁴Furthermore, Appendix Figure A3 show that these heterogeneous effects exists also on a sample that contains all women, including the non-married, indicating that a selective group of those who marry young are not driving the heterogeneity.

²⁵Unfortunately, I can only observe marriage dates after 1968. Everyone who got married earlier than 1968 receives 1968 as their year of marriage.

female assault are done by an intimate partner, I believe measurement error is still small. Figure 10 show that increasing women’s potential earnings increase the risk of assault, regardless of whether the woman is non-married, married or divorced (although the effect is smaller in magnitude for the divorced).²⁶

5. Robustness

Even though the hospital visits for assault are reported by medical personnel, there is still the concern that women at the hospital might not truthfully state the causes of their injuries. If this misreporting is non-random and related to potential earnings, it would invalidate my results as I cannot distinguish between a reduction in assaults and a switch in reporting behaviour. I address this concern by investigating the relationship between potential earnings and hospital visits for accidents, as a misreported assault would likely be coded as an accident. To isolate those accidents that are most likely to be hidden assaults, I only use accidents that happened at home or in unspecified locations, with a similar main diagnosis to that of an assault. Column (1) in Table 6 shows that potential earnings has a positive effect, although smaller compared to the main results, on the risk of visiting a hospital for these types of accidents, which implies that some misreporting might take place. For this reason, I replicate my main analysis using an outcome measure that consists of both assaults and similar accidents (thereby capturing all assaults, including the misreported ones). Reassuringly, the estimates in column (2) depict the same relationship as the main results of Table 4, but are of larger magnitude. Thus, if anything, possible misreporting attenuates my results, and I can conclude that it does not pose a threat to my study. Finally, my results are not due to the low frequency of assaults or data sparsity at the tails of the potential earnings distribution. Column (3) shows the results of a placebo test in which I show that no relationship exists between appendix complications, which are about as common as assaults in my sample, and women’s potential earnings.

My results hold for using only assault that took place at home as the measure

²⁶Instead of controlling for husbands’ earnings linearly, I include indicators for husbands earnings decile, or for not having a husband. Appendix Figure A2 show the risk of assault by husbands’ income decile, and Appendix Table A1 show the effect of women’s potential earnings for the larger sample that includes also those women who are not married. Appendix Figures A3 and A4 show that the same pattern of heterogeneous effects by age and education level exists also for this sample.

of domestic violence, and are not sensitive to excluding the unemployed. I control for non-random attrition via divorce by estimating the model on a balanced sample where I include only those couples who are observed every year of my study. The results for this subgroup are slightly weaker than the main results, as would be expected since many of the “low-quality” marriages opt out of my full dataset over time. Furthermore, my results are robust to controlling even more flexibly for household income using decile fixed effects and a linear trend, and to excluding the top 1% and zero-earners from my sample. Table 7 shows a summary of all these robustness checks.

5.1. Replication of Aizer (2010) and evidence using police reports

My findings differ from those of [Aizer \(2010\)](#), who finds a significant negative effect of a reduction in the gender wage gap on aggregate levels of hospitalisations for assault. One possible reason for the difference between my results and those of [Aizer \(2010\)](#) is that she restricts her dataset to contain only hospital visits by women with no more than high school education. My heterogeneity analysis shows that increasing potential earnings for the lowest educated women reduces their assault risk. Thus, my results do not contradict those of [Aizer \(2010\)](#) if I restrict my sample to contain a similar study population as hers. In contrast, my study contributes to [Aizer \(2010\)](#), as I show that the effect of an increased relative economic position differs depending on women’s education level and age.

Nevertheless, I replicate her findings using a dataset and model specification that mimics hers as closely as possible.²⁷ Table 8 shows the results of this replication exercise, where column (1) shows the results from using her model with the outcome measure most similar to hers (all overnight hospitalisations for assault), column (2) contains her model with the outcome measure of my study (hospital visits for assaults that took place at home or in unspecified locations), column (3) contains an aggregate version of my model using police reports for domestic violence as the dependent variable, and column (4) show the results using my individual level model with the outcome variable of [Aizer \(2010\)](#). The results show that I can replicate Aizer’s findings on aggregate Swedish data (containing women of all education levels), but that our results differ if the analysis is done on an individual level dataset. The difference in our results

²⁷I aggregate my data to municipality level, and construct a measure of the potential gender wage gap that is as close to hers as possible.

are not due to any difference in our preferred outcome; the estimates in column (2) are still negative and the estimates in column (4) are positive. Thus, using the model and aggregate data set-up of [Aizer \(2010\)](#) yields negative results for both hers and my outcome measure, and for police reports, whereas my model and individual level data yields positive results for both my outcome and that of [Aizer \(2010\)](#). These results indicate that an effect measured on an aggregate level can mask what is really happening on the individual level. Making inference about individuals based on the aggregate result of my model would result in an ecological fallacy.

6. Conclusion

This paper estimates the effect of female economic empowerment on domestic violence, using high-quality data from Swedish administrative registers. I measure domestic violence using individual hospital visits for assault. Distinguishing violence changes from reporting behaviour is of utter importance with an outcome as sensitive as domestic violence, especially as the propensity to report a violent incident is likely increasing with empowerment. A unique feature of my data is that rich information on hospital visits for accidents allows me to investigate possible misreporting at the hospital, which allows me to conclude that non-random misreporting does not pose a threat to the validity of my results.

Descriptively, I show that the relationship between spouses' realised relative earnings and risk of assault is U-shaped. Violence is increasing with earnings inequalities in both directions, and the sign of the association differs depending on which spouse is the main breadwinner. The U-shaped relationship is an interesting finding, but it does not depict a causal relationship. As spouses select into marriage in an assortative way, which may be a function of earnings as well as both underlying propensity for violence and for staying in a violent relationship, the characteristics of who marries whom may be partly driving the relationship between relative earnings and domestic violence. Furthermore, the outside option of a marriage is determined by earnings potentials rather than realised earnings, and individual earnings may be endogenous with respect to violence and unobserved characteristics that correlates with violence.

To account for these empirical challenges, I proxy women's relative economic empowerment with a demand-driven measure of women's potential earnings, which reflects the outside option of the marriage and captures earnings variation

that is not endogenous to domestic violence. In all estimations I keep the earnings of the husbands constant, in order to identify variation in the wives' relative economic empowerment that is driven only by changes in female labour demand. This way, I show that increasing women's potential earnings has a causal positive effect on the risk of domestic violence, regardless of which spouse earns more than the other. In addition, I show that women's potential earnings increase the husbands' risk of visiting a hospital for stress, anxiety, substance abuse and assault. Thus, the mechanisms are in line with a male backlash response to an improved relative economic position of married women. This result is perhaps in contrast to a priori expectations of what we should find in a gender-equal country like Sweden, and it indicates that traditional gender norms may play an important role in determining the relationship between female economic empowerment and domestic violence.

The richness of my data allows me to conduct a detailed heterogeneity analysis, where I show that the backlash response to women's potential earnings is increasing with age of the woman and duration of the marriage. Likewise, for the women with no more than high school education increases in potential earnings reduce the risk of assault, but for women of higher education levels potential earnings increase the risk. These results indicate that the women for whom a change in potential earnings actually affects the credibility of their threat of leaving, such as for the younger women or for the least educated; where the outside option may be binding, the results are in line with the predictions of bargaining power theory. But for the women who may have the economic possibility to leave, but still do not, the effects are in line with male backlash theory. An important implication of these results is that women from less vulnerable groups still experience a high risk of a backlash response to an improved relative economic position within the marriage.

More research is needed in this area as domestic violence is a significant issue for public health. My effect sizes are substantial, and suggest a high risk of a backlash response as women improve their relative economic position within a marriage. Furthermore, I show that the risk of backlash is higher for older women and highly educated women, which are demographic groups that are usually overlooked in the discussion on how to best combat domestic violence. Policy makers should consider how to best reach these women, who may already have the economic opportunity to leave their abusive spouse but who still choose to remain in the relationship. Furthermore, along with providing shelter and legal

support for assaulted women, policy makers should consider policies targeted to promote less traditional gender norms in boys and girls, preferably early in their lives.

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Table 1
Summary statistics

	Socio-demographic variables
Age	46.08 (9.98)
Wife older	0.17 (0.38)
Have children	1.00 (0.04)
Have university degree	0.38 (0.49)
Ever get divorce	0.08 (0.27)
	Hospital visits per year and 1000 people
Pr(hospital visit for assault at home or unspec. place)	0.25 (15.77)
Pr(hospital visit for similar accidents)	0.65 (25.54)
Pr(hospital visit for appendix complications)	0.88 (29.67)
Total hospital visit for any assault	0.39 (32.17)
Total in-patient hospital visit for any assault	0.05 (7.55)
	Earnings and income, in 1000
Employed	0.95 (0.23)
Wives' yearly earnings	190.65 (133.95)
Husbands' yearly earnings	290.68 (258.80)
Household yearly income	549.29 (304.05)
Relative earnings	-0.15 (0.53)
	Potential earnings, in 1000
Women's potential earnings	198.99 (46.38)
Husbands' potential earnings	291.34 (72.99)
Potential relative earnings	-0.18 (0.11)
Observations	7,965,166
No. couples	1,046,867

Notes: The table reports means and standard deviations (in parentheses) of key variables. All earnings and income measures are inflation-adjusted and reported in 2000 levels.

Figure 1
Assaults over time: mean hospital visits and police reports per 1000 women

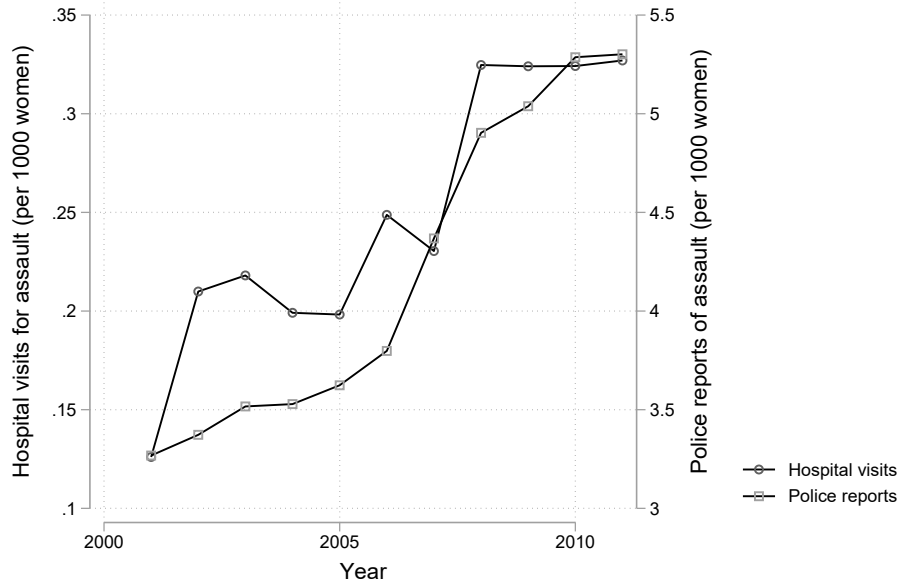


Figure 2
Mean hospital visits for assault by age, education and income

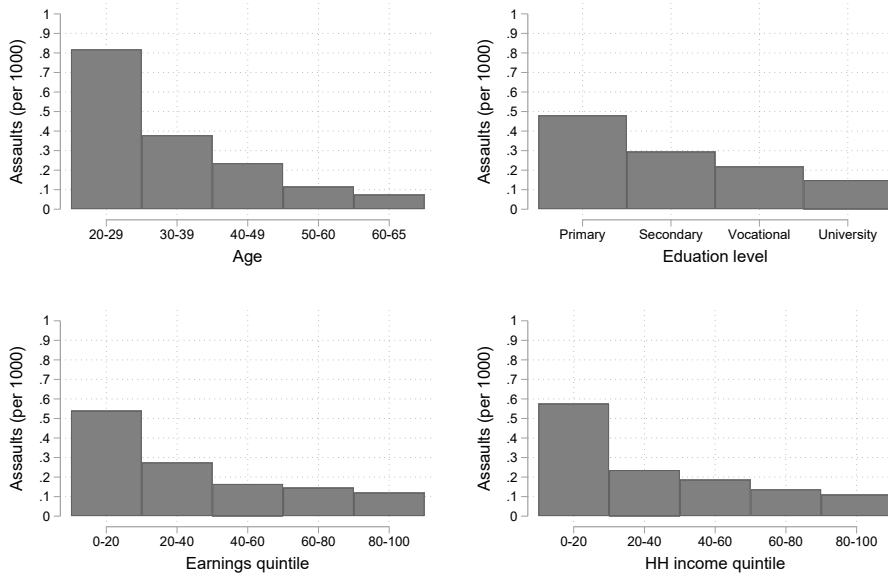


Table 2
Relationship between potential earnings and realised earnings

	Correlation with realised				Cross effects?	
	Wives'	Husbands'	Relative	Wives'	Husbands'	Relative
Women's potential earnings	0.163*** (0.007)	0.003 (0.005)	0.069*** (0.005)			0.116*** (0.005)
Husbands' potential earnings				0.018*** (0.003)	0.112*** (0.005)	-0.108*** (0.003)
Indicators						
Basic FE	Yes	Yes	Yes	Yes	Yes	Yes
Detailed FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.35	0.37	0.26	0.35	0.37	0.07
Observations	7,965,166	7,965,166	7,965,166	7,965,166	7,965,166	7,965,166

Notes: The table reports correlations between potential earnings and realised earnings. The effect is measured as a change of one standard deviation. Basic FE: Municipality, year, age group, education group and cohort. Detailed FE: basic FE plus household income quintile and spouse's education group. Controls are municipality level of police reports, spousal age span and husbands' earnings. Standard errors in parentheses are clustered at the *mun * edugroup * agegroup* level. * p<0.05, ** p<0.01, *** p<0.001.

Figure 3
Descriptive relationship between relative earnings and assault

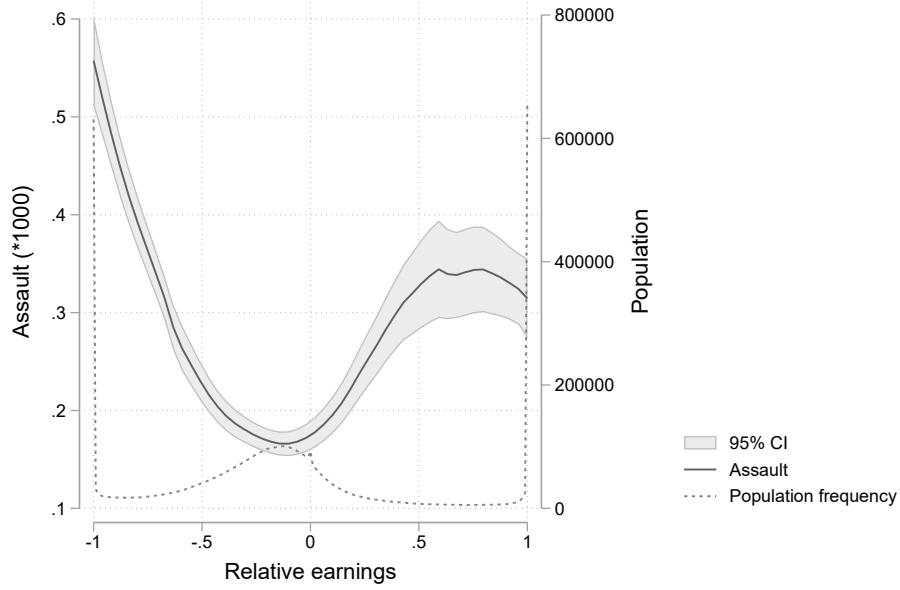
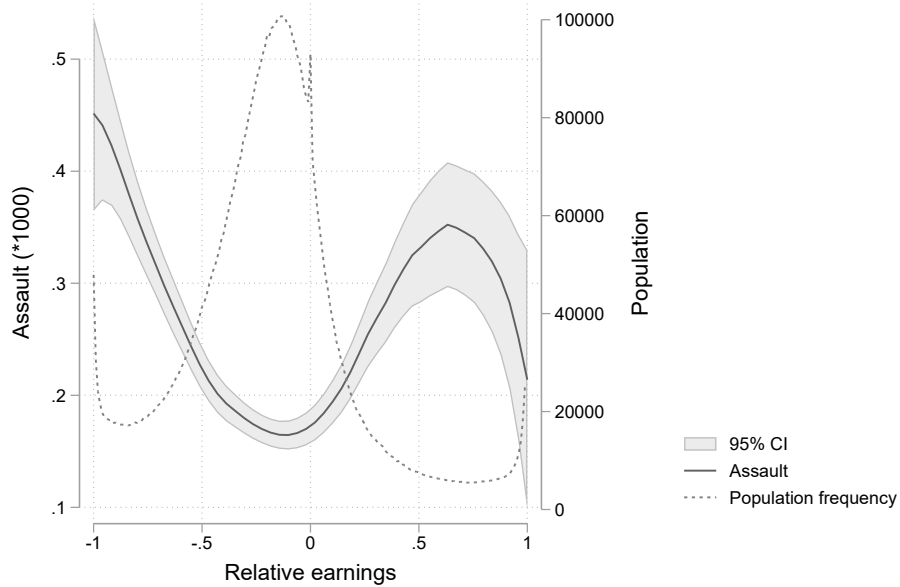


Figure 4
Descriptive relationship between relative earnings and assault: dual-earners only



Notes: Figure 3 plots the descriptive relationship between female hospital visits for assault and relative earnings of spouses. Relative earnings of -1 means the husband is making 100% of the household labour income, and relative earnings of 1 means the wife is making 100% of the household labour income. Figure 4 plots the same relationship, for only dual-earner couples. The dotted line show the frequency distribution of relative earnings. For ease of interpretation, assault prevalence is measured per 1000 women.

Table 3
Descriptive relationship between relative earnings and assault

	(1)	(2)	(3)	(4)
Relative earnings	-0.003 (0.007)	-0.022** (0.008)	-0.022** (0.008)	0.016 (0.014)
With intervals				
Husband > Wife	-0.152*** (0.014)	-0.111*** (0.014)	-0.111*** (0.014)	0.029 (0.023)
Husband ≤ Wife	0.104*** (0.010)	0.045*** (0.011)	0.046*** (0.011)	0.006 (0.019)
Observations	7,965,166	7,965,166	7,965,166	7,894,612
No. couples	1,046,867	1,046,867	1,046,867	976,313
Mean	0.25	0.25	0.25	0.24
Indicators				
Basic FE	Yes	Yes	Yes	No
Detailed FE	No	Yes	Yes	No
ID FE	No	No	No	Yes
Controls	No	No	Yes	Yes

Notes: The dependent variable is an indicator of a hospital visit for assault in the current year. The associations are estimated for a one standard deviation increase in relative earnings (0.53). The top panel reports baseline estimates of the effect of a change in relative earnings on assault. The bottom panel allows the effect to differ along two intervals: [-1, 0) and [0, 1]. Basic FE: Municipality, year, age group, education group and cohort. Detailed FE: basic FE plus household income quintile and spouse's education group. Controls are municipality level of police reports and spousal age span. Standard errors in parentheses are clustered at the *mun*edugroup*agegroup* level. * p<0.05, ** p<0.01, *** p<0.001.

Table 4
Effect of women's potential earnings on assault

	(1)	(2)	(3)
Women's pot. earnings	0.244*** (0.029)	0.245*** (0.029)	0.246*** (0.029)
With intervals			
Husband > Wife	0.251*** (0.031)	0.247*** (0.030)	0.248*** (0.030)
Husband ≤ Wife	0.233*** (0.029)	0.242*** (0.029)	0.244*** (0.029)
Observations	7,965,166	7,965,166	7,965,166
No. couples	1,046,867	1,046,867	1,046,867
Mean	0.25	0.25	0.25
Indicators			
Basic FE	Yes	Yes	Yes
Detailed FE	No	Yes	Yes
Controls	No	No	Yes

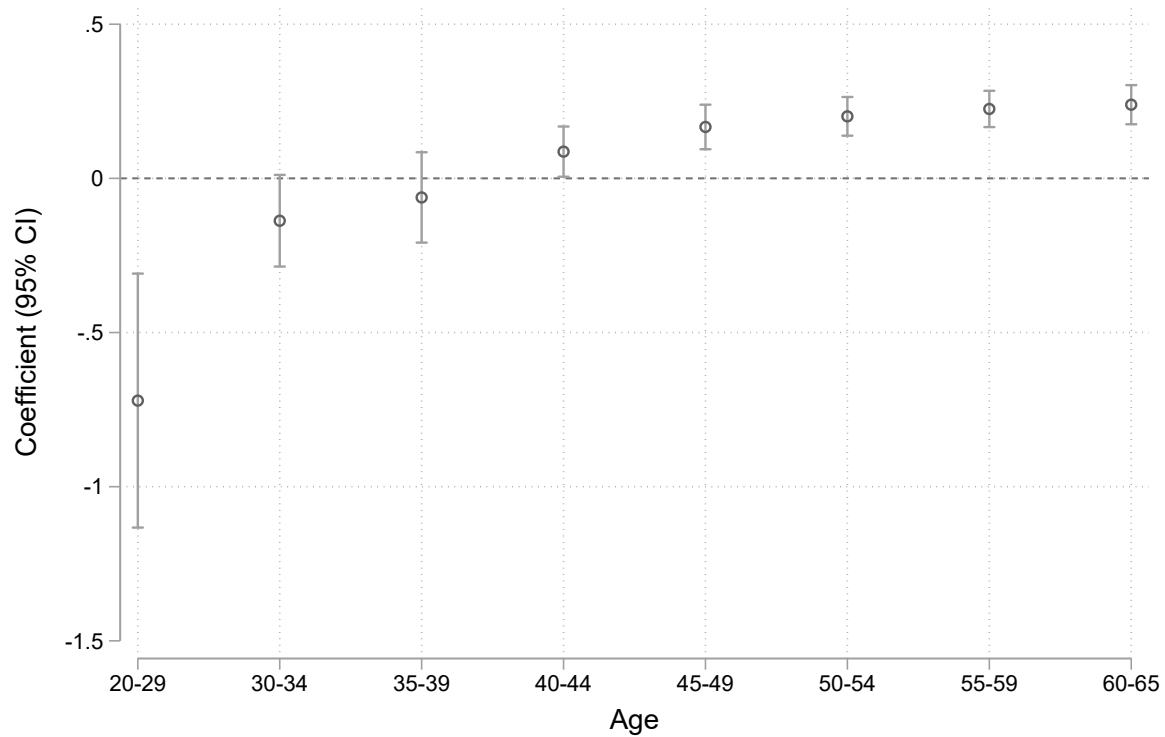
Notes: The dependent variable is an indicator of a hospital visit for assault in the current year. The top panel reports baseline estimates of the effect of a one standard deviation increase women's potential earnings (46' SEK) on assault. The bottom panel allows the effect to differ along two intervals of relative earnings: [-1, 0) and [0, 1]. Basic FE: Municipality, year, age group, education group and cohort. Detailed FE: basic FE plus household income quintile and spouse's education group. Controls are municipality level of police reports, spousal age span and husbands' earnings. Standard errors in parentheses are clustered at the *mun * edugroup * agegroup* level. * p<0.05, ** p<0.01, *** p<0.001.

Table 5**Backlash: effect of women's potential earnings on husbands' destructive behaviour**

	Mental inst.	Substance abuse	Any assault	HU assault	Divorce
Women's pot. earnings	1.914*** (0.166)	0.321*** (0.063)	0.443*** (0.050)	0.025* (0.012)	7.446*** (0.347)
Observations	7,965,166	7,965,166	7,965,166	7,965,166	7,965,166
No. couples	1,046,867	1,046,867	1,046,867	1,046,867	1,046,867
Mean	5.89	1.22	0.50	0.43	17.41
Indicators					
Basic FE	Yes	Yes	Yes	Yes	Yes
Detailed FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

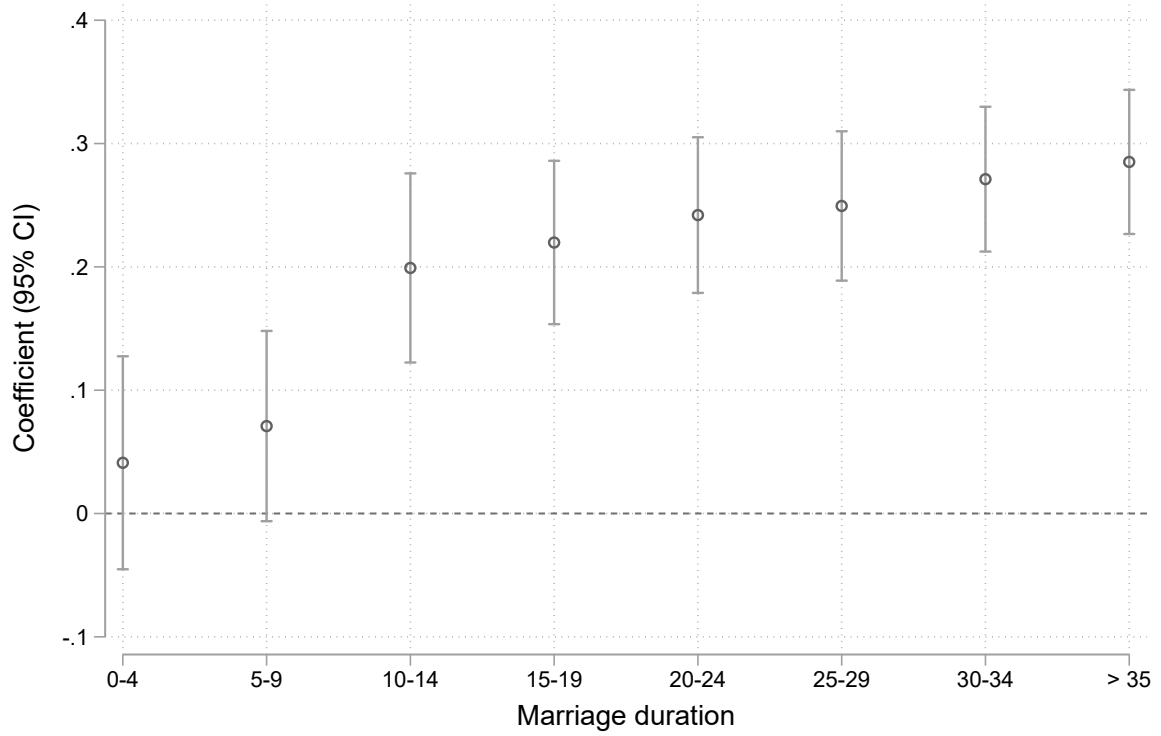
Notes: The dependent variable is an indicator of husbands' hospital visit for stress, depression or anxiety (1), substance abuse (2), any assault (3), assaults that took place at home or in unspecified locations (4), and couples risk of divorce (5). The table reports estimates of the effect of a one standard deviation increase women's potential earnings (46' SEK) on assault. Basic FE: Municipality, year, age group, education group and cohort. Detailed FE: basic FE plus household income quintile and spouse's education group. Controls are municipality level of police reports, spousal age span and husbands' earnings. Standard errors in parentheses are clustered at the *mun * edugroup * agegroup* level. * p<0.05, ** p<0.01, *** p<0.001.

Figure 5
Heterogeneity by age



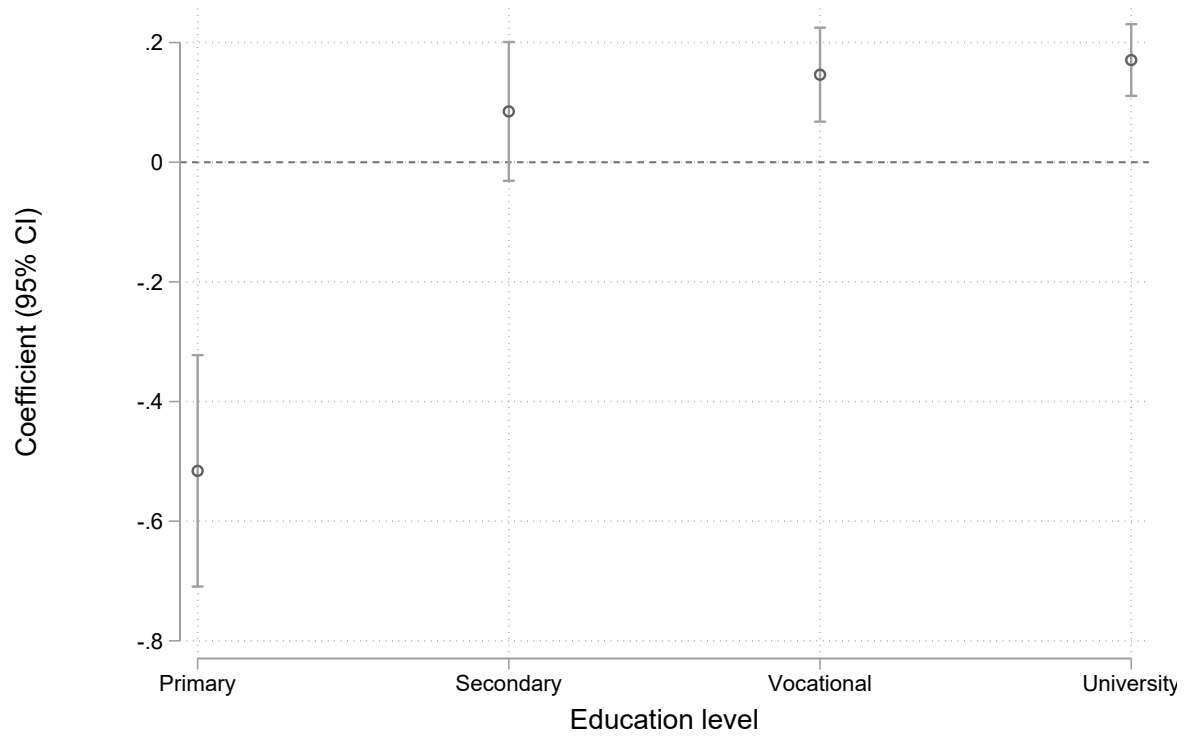
Notes: The Figure plots the effect of a one standard deviation increase women's potential earnings (46' SEK) on assault, when allowing the effect to differ by age group of the woman. The model controls for indicators for municipality, year, age group, education group, cohort, household income quintile and spouse's education group. Controls are municipality level of police reports, spousal age span and husbands' earnings. The Figure plots 95% confidence intervals.

Figure 6
Heterogeneity by length of marriage



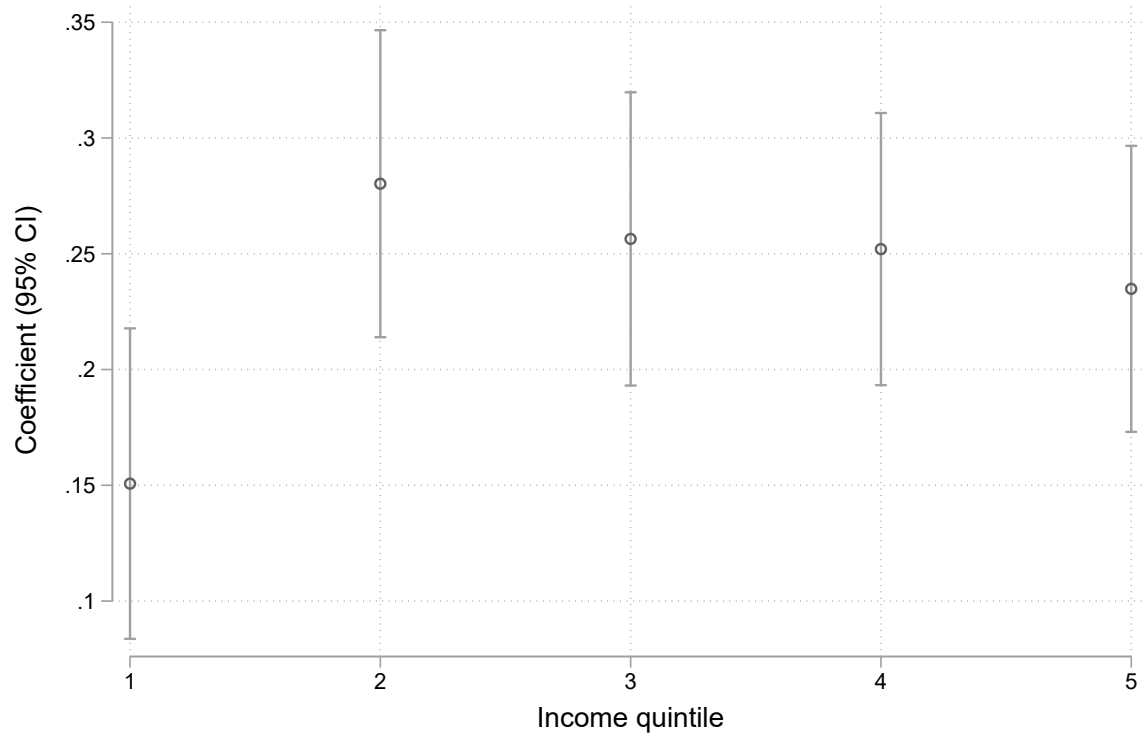
Notes: The Figure plots the effect of a one standard deviation increase women's potential earnings (46' SEK) on assault, when allowing the effect to differ by how long the couple have been married. The model controls for indicators for municipality, year, age group, education group, cohort, household income quintile and spouse's education group. Controls are municipality level of police reports, spousal age span and husbands' earnings. The Figure plots 95% confidence intervals.

Figure 7
Heterogeneity by education level



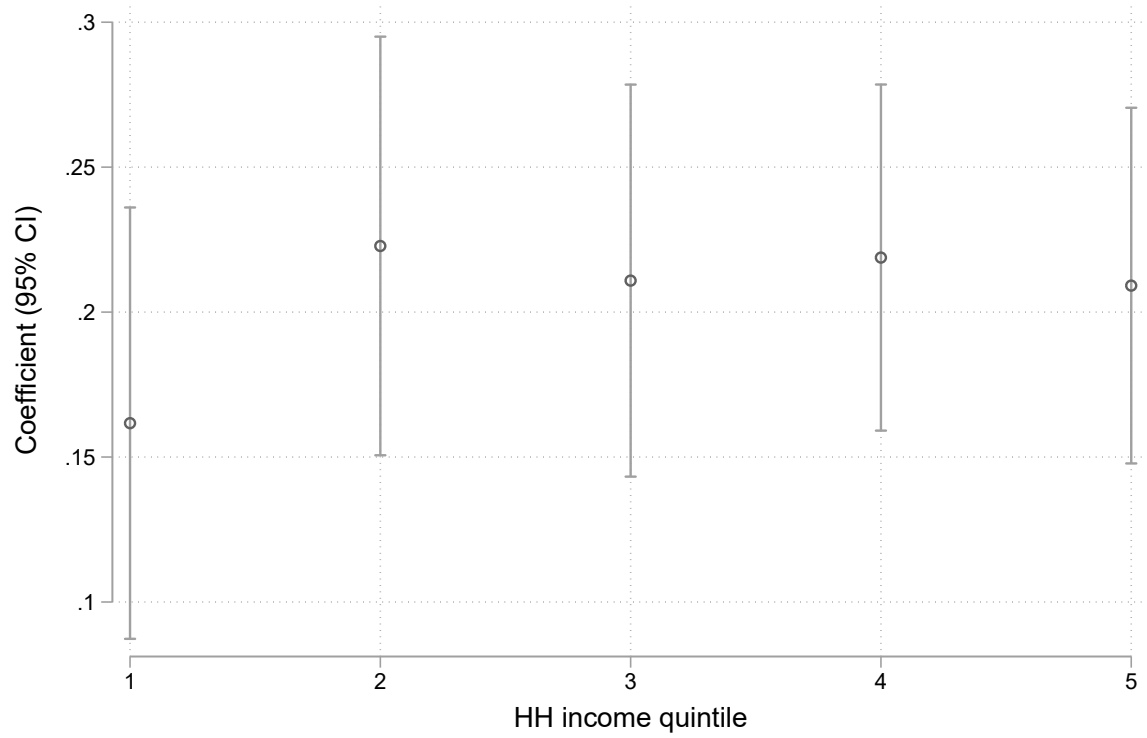
Notes: The Figure plots the effect of a one standard deviation increase women's potential earnings (46' SEK) on assault, when allowing the effect to differ by education group of the woman. The model controls for indicators for municipality, year, age group, education group, cohort, household income quintile and spouse's education group. Controls are municipality level of police reports, spousal age span and husbands' earnings. The Figure plots 95% confidence intervals.

Figure 8
Heterogeneity by income level



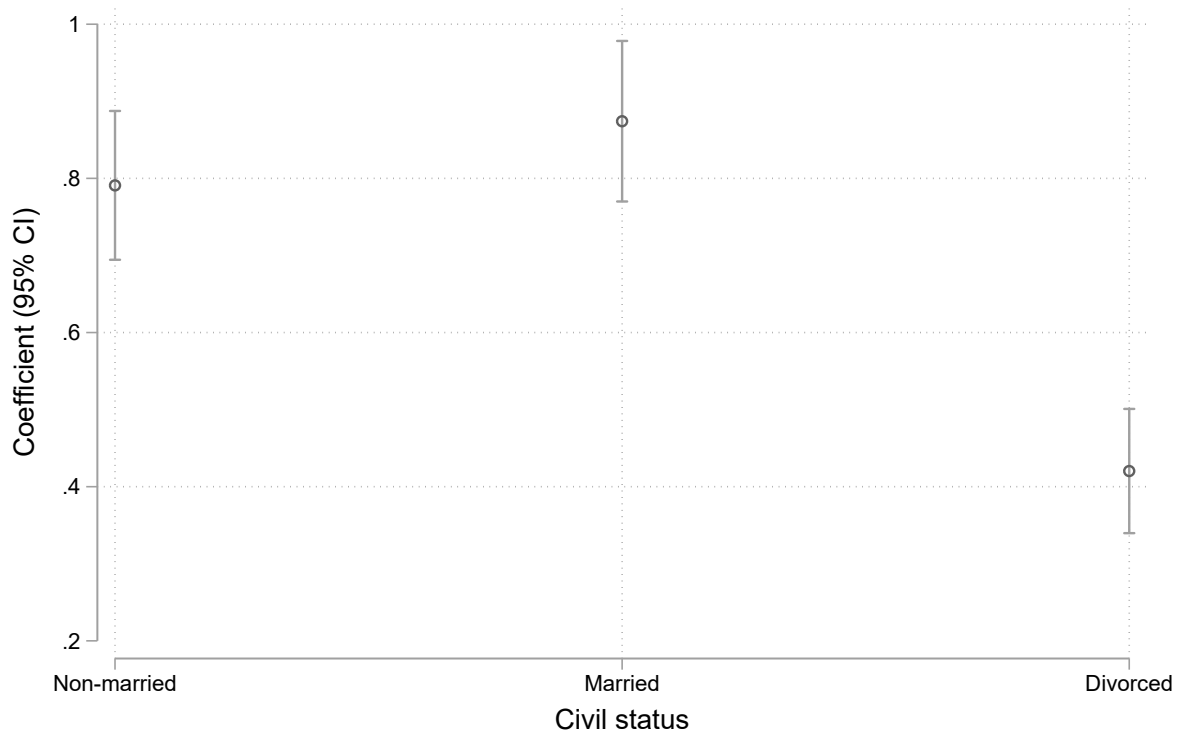
Notes: The Figure plots the effect of a one standard deviation increase women's potential earnings (46' SEK) on assault, when allowing the effect to differ by income quintile of the woman. The model controls for indicators for municipality, year, age group, education group, cohort, household income quintile and spouse's education group. Controls are municipality level of police reports, spousal age span and husbands' earnings. The Figure plots 95% confidence intervals.

Figure 9
Heterogeneity by household income



Notes: The Figure plots the effect of a one standard deviation increase women's potential earnings (46' SEK) on assault, when allowing the effect to differ by household income quintile. The model controls for indicators for municipality, year, age group, education group, cohort, household income quintile and spouse's education group. Controls are municipality level of police reports, spousal age span and husbands' earnings. The Figure plots 95% confidence intervals.

Figure 10
Heterogeneity by civil status



Notes: The Figure plots the effect of a one standard deviation increase women's potential earnings (46' SEK) on assault, when allowing the effect to differ by civil status of the woman. The model controls for indicators for municipality, year, age group, education group, cohort, household income quintile and spouse's education group. Controls are municipality level of police reports, spousal age span and husbands' earnings. The Figure plots 95% confidence intervals. The sample used for this model is larger than the main sample, as it also includes non-married and divorced women.

Table 6
Placebo tests: accidents and appendix complications

	(1)	(2)	(3)
	Accidents	Accidents + assaults	Appendix
Women's pot. earnings	0.113*** (0.032)	0.359*** (0.040)	-0.044 (0.040)
Observations	7,965,166	7,965,166	7,965,166
No. couples	1,046,867	1,046,867	1,046,867
Mean	0.65	0.90	0.88
	Indicators		
Detailed FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: The dependent variable in column (1) an indicator for visiting a hospital for an accident with similar characteristics as the assaults in the current year. The dependent variable of column (2) is these accidents and assaults combined. Column (3) show the effect of hospital visits for appendix complications, as a placebo test. The table reports estimates of the effect of a one standard deviation increase women's potential earnings (46' SEK) on assault. Basic FE: Municipality, year, age group, education group and cohort. Detailed FE: basic FE plus household income quintile and spouse's education group. Controls are municipality level of police reports, spousal age span and husbands' earnings. Standard errors in parentheses are clustered at the *mun * edugroup * agegroup* level. * p<0.05, ** p<0.01, *** p<0.001.

Table 7
Robustness checks

	Main effects	At home	Excl. unempl.	Balanced	Finer HH FE	Excl. zero & top 1%
Women's pot. earnings	0.246*** (0.029)	0.040*** (0.011)	0.219*** (0.031)	0.062** (0.020)	0.243*** (0.028)	0.221*** (0.031)
Observations	7,965,166	7,965,166	6,604,002	4,589,827	7,965,166	6,597,080
No. couples	1,046,867	1,046,867	989,798	417,257	1,046,867	960,078
Mean	0.25	0.06	0.20	0.11	0.25	0.21

Indicators

Detailed FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is female hospital visits for assault. Column (1) show the main effects. Column (2) show the effects for hospital visits for assaults that took place only at home. Column(3) excludes all couples where either the wife or the husband received any unemployment benefits during the current year. Column (4) reports the effect on a balanced sample of couples who I can observe full the full 11 years. Column (5) includes decile fixed effects and linear trends of household income. Column (7) excludes zero-earners and the top 1 % of the earnings distribution. The table reports estimates of the effect of a one standard deviation increase women's potential earnings (46' SEK) on assault. Basic FE: Municipality, year, age group, education group and cohort. Detailed FE: basic FE plus household income quintile and spouse's education group. Controls are municipality level of police reports, spousal age span and husbands' earnings. Standard errors in parentheses are clustered at the *mun * edugroup * agegroup* level. * p<0.05, ** p<0.01, *** p<0.001.

Table 8
Replication of Aizer (2010)

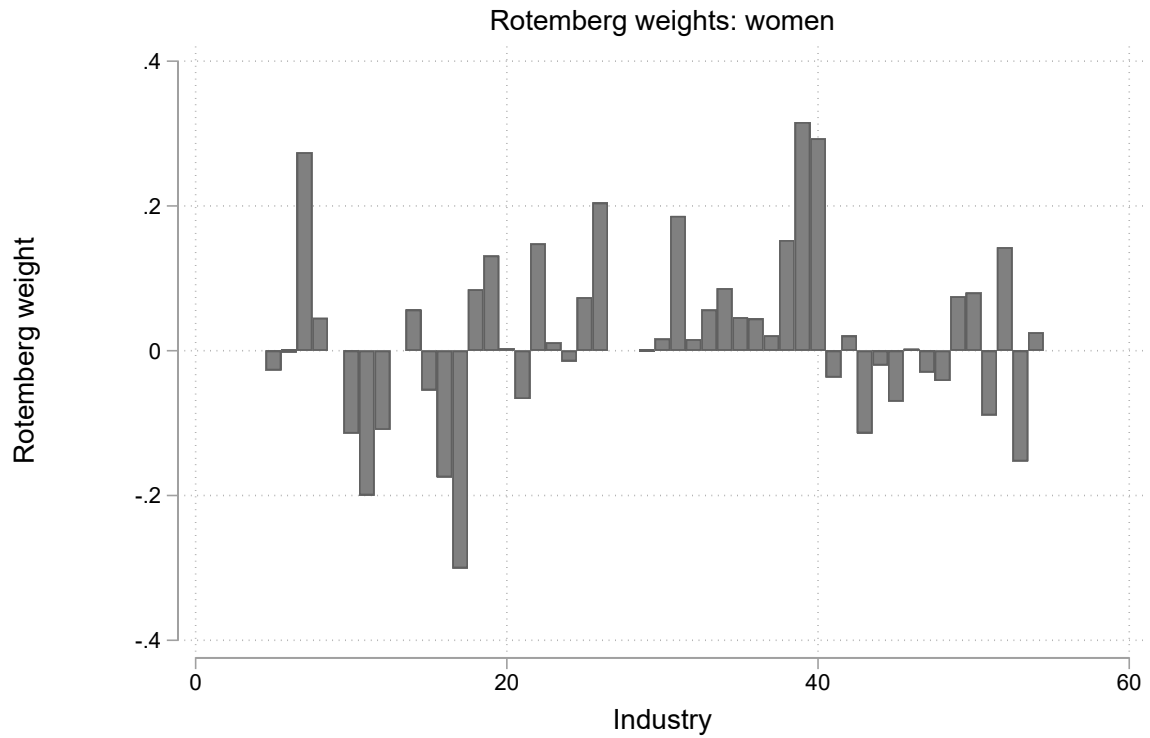
	Hospitalizations	Hosp. visits	Police reports	Hospitalizations
Pot. rel. earnings	-0.162** (0.050)	-0.086 (0.170)		
Women's pot. earnings			-0.167 (0.233)	0.031** (0.011)
Observations	790	790	2,869	7,965,166
Mean	0.11	0.96	0.75	0.05

	Indicators			
Municipality FE	Yes	Yes	Yes	See T4
Year FE	Yes	Yes	Yes	See T4
Controls	Yes	Yes	Yes	See T4

Notes: The dependent variable is the natural log of female inpatient hospitalizations for assault (1), natural log of female hospital visits for assaults that took place at home or in unspecified location (2), and number of police reports filed for domestic abuse (3), per municipality and year. The dependent variable in column (4) is an indicator of overnight hospitalizations for assault in the current year. Potential relative earnings are calculated following Aizer (2010). The effect is estimated for a one standard deviation change. All regressions include fixed effects for municipality and year. Controls for column (1)-(3) are natural log of municipality mean income, natural log of municipality population, natural log of female homicides and lagged natural log of female hospitalizations or police reports. Municipalities with a female population below 10,000 are excluded from the analysis in column (1) and (2), following Aizer (2010). Standard errors in parentheses are clustered at municipality level. Controls for column (4) are the same as in column (3) of Table 4. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

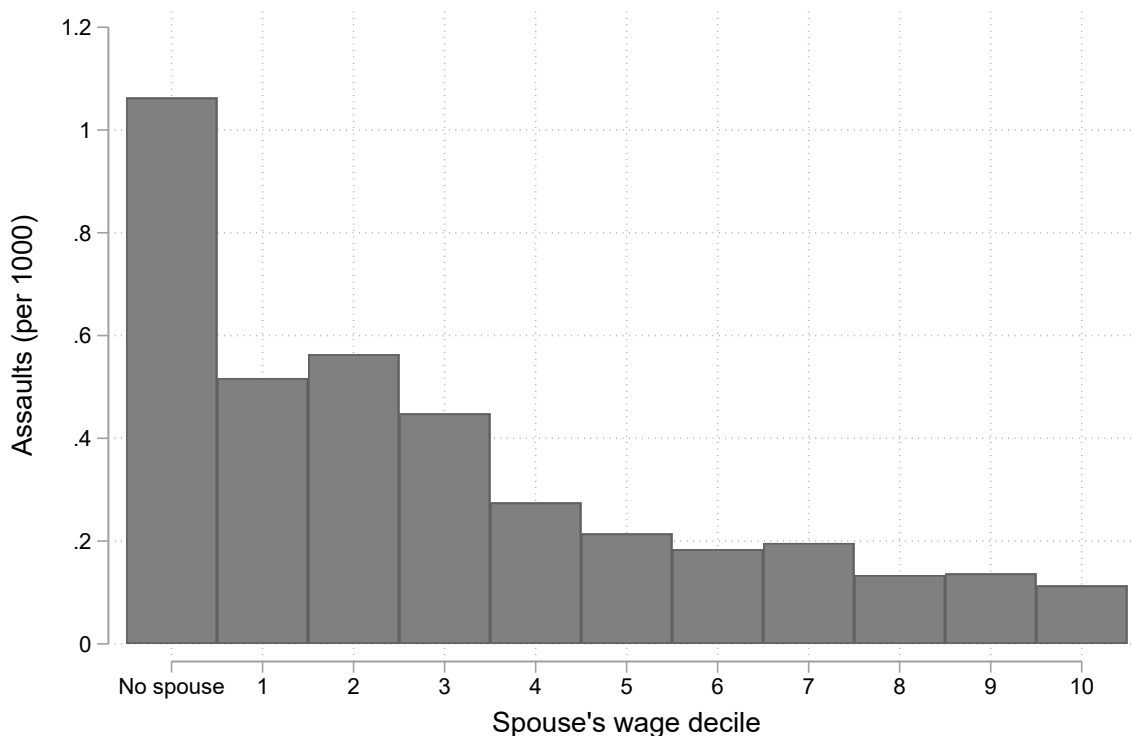
Appendix A

Figure A1
Rotemberg weights for each industry



Notes: The figure plots the Rotemberg weights for each industry, which quantifies the contribution of each industry to the identification. These are constructed using the Stata "bartik weight" command, as outlined in the supplemental material of Goldsmith-Pinkham et al (2018). The endogenous variable is relative earnings, and the outcome is hospital visits for assault. By construction, the weights sum to one.

Figure A2
Assault risk by spouse's wage decile: all women



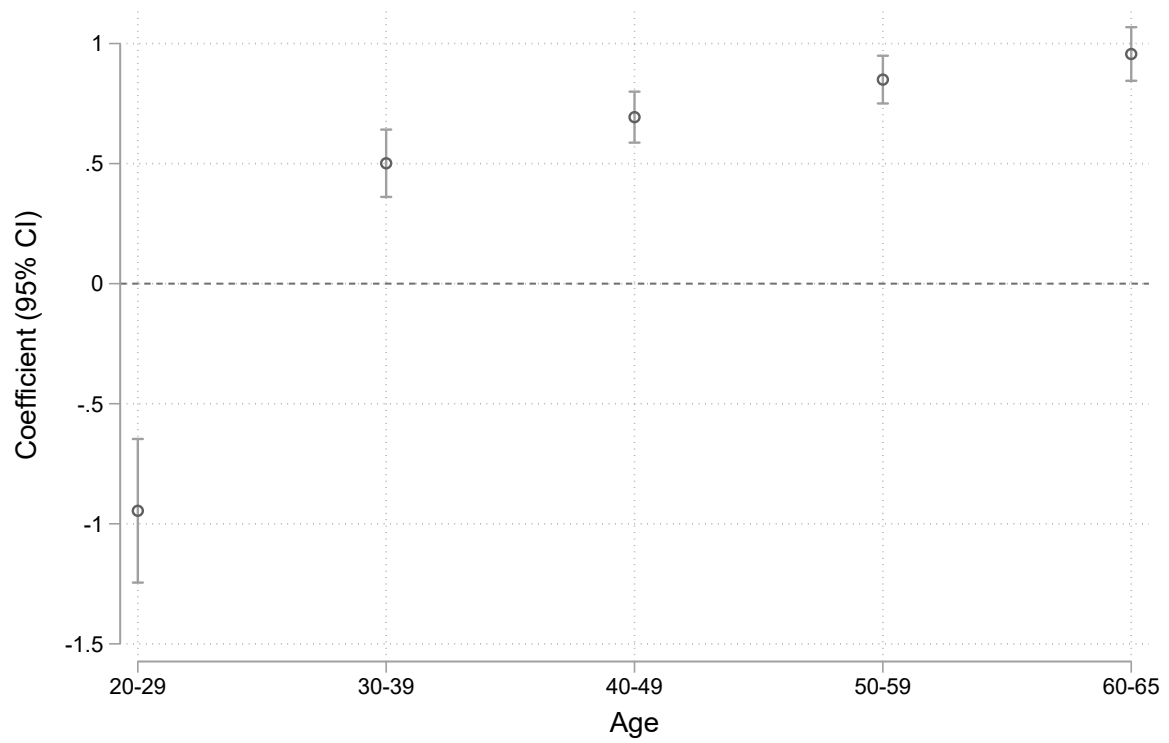
Notes: The Figure plots mean risk of visiting a hospital for assault (that took place at home or in unspecified locations) by husbands' income decile, and for unmarried women.

Table A1
Effect of women's potential earnings on assault: all women

	(1)	(2)	(3)
Women's pot. earnings	0.705*** (0.047)	0.781*** (0.047)	0.782*** (0.047)
Observations	21491632	21491157	21491157
No. couples	2,422,648	2,422,648	2,422,648
Mean	0.76	0.76	0.76
	Indicators		
Basic FE	Yes	Yes	Yes
Detailed FE	No	Yes	Yes
Controls	No	No	Yes

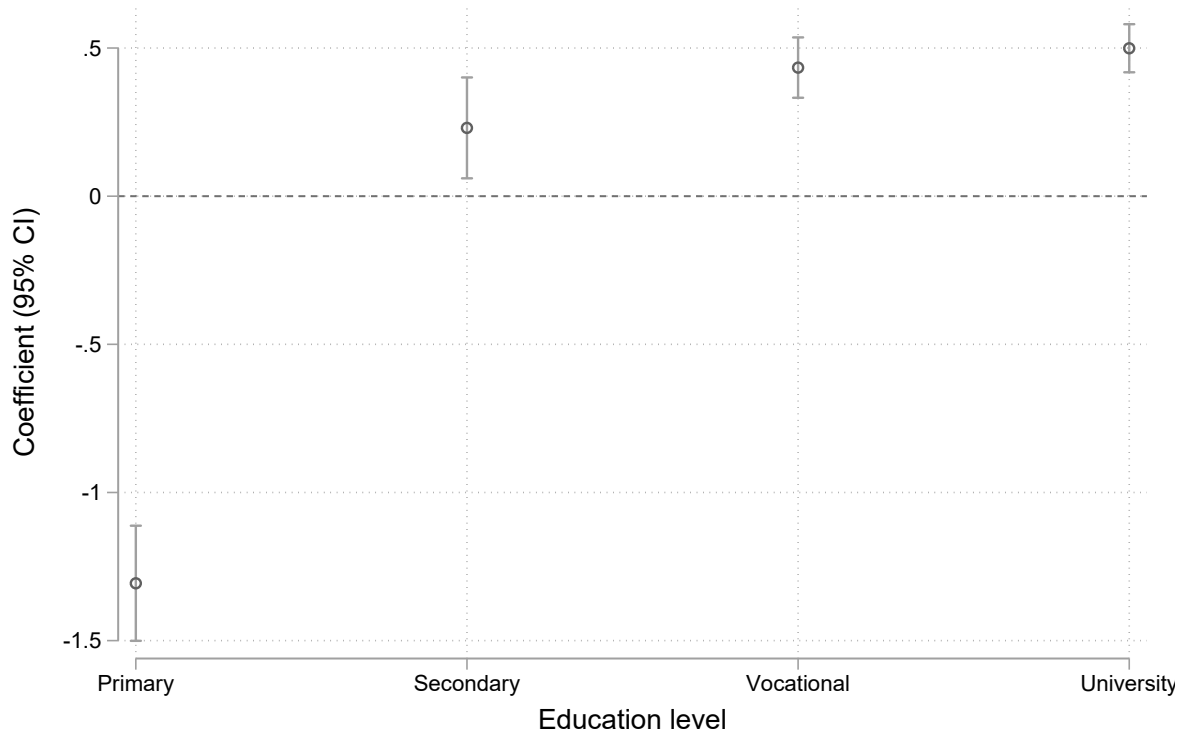
Notes: The dependent variable is an indicator of a hospital visit for assault in the current year. The top panel reports baseline estimates of the effect of a one standard deviation increase women's potential earnings on assault. The bottom panel allows the effect to differ along two intervals of relative earnings: [-1, 0) and [0, 1]. Basic FE: Municipality, year, age group, education group and cohort. Detailed FE: basic FE plus household income quintile and spouse's education group. Controls are municipality level of police reports and spousal age span and indicators for husbands' income decile or for being unmarried. Standard errors in parentheses are clustered at the *mun * edugroup * agegroup* level. * p<0.05, ** p<0.01, *** p<0.001.

Figure A3
Heterogeneity by age: all women



Notes: The Figure plots the effect of a one standard deviation increase women's potential earnings (46' SEK) on assault, when allowing the effect to differ by age group of the woman. The model controls for indicators for municipality, year, age group, education group, cohort, household income quintile and spouse's education group. Controls are municipality level of police reports, spousal age span and husbands' earnings. The Figure plots 95% confidence intervals. The sample used for this model is larger than the main sample, as it also includes non-married and divorced women.

Figure A4
Heterogeneity by education level: all women



Notes: The Figure plots the effect of a one standard deviation increase women's potential earnings (46' SEK) on assault, when allowing the effect to differ by education group of the woman. The model controls for indicators for municipality, year, age group, education group, cohort, household income quintile and spouse's education group. Controls are municipality level of police reports, spousal age span and husbands' earnings. The Figure plots 95% confidence intervals. The sample used for this model is larger than the main sample, as it also includes non-married and divorced women.