Institute for Fiscal Studies

Abi Adams
Kristiina Huttenen
Emily Nix
Ning Zhang

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The economic impacts of rape

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The Economic Impacts of Rape*

Abi Adams,† Kristiina Huttunen,‡ Emily Nix,§ and Ning Zhang¶

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Abstract

We estimate the economic impacts of rape using population-wide Finnish police reports and hospital admissions. Over the five years following a rape, victims' employment falls by 6 percentage points and annual earnings decline by €1,500, alongside a 6 percentage point increase in anti-depressant use, relative to otherwise observationally equivalent women. These effects persist for at least a decade and correspond to a 15% decline in employment, 17% earnings loss, and 28% rise in anti-depressant use relative to pre-assault baselines. Rape also generates spillover effects to victims' networks: female schoolmates and victims' parents experience significant mental health deterioration.

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†Northwestern University, abigail.adams@northwestern.edu

‡Aalto University, kristiina.huttunen@aalto.fi

§University of Southern California Marshall School of Business, enix@usc.edu

¶The Chinese University of Hong Kong, ning.zhang@cuhk.edu.hk

1 Introduction

Rape is common. One in five American women report experiencing a rape or attempted rape since age 15 (Smith *et al.*, 2018). Rape is a deeply traumatic crime. Anecdotal accounts and small-scale surveys suggest that sexual violence can lead to severe economic and mental deterioration for survivors, and these negative effects could also spill over to their families and peers.[1] In the United States, 94% of rape survivors report PTSD symptoms and 33% contemplate suicide (Rothbaum *et al.*, 1992; Kilpatrick, 1992). The UK Crime Survey (2021) found that 21% of female rape victims reported taking time off work, and 5% reported losing their job or leaving work altogether (ONS, 2021). As such, sexual violence could have profound implications for aggregate female well-being and gender inequality, given its high prevalence (Blau and Kahn, 2013; Goldin, 2014; Bertrand, 2018).

The existing quantitative literature provides important but incomplete evidence on the impact of rape.[2] Much of this work has focused on the direct health and legal costs associated with sexual assault (Hunt *et al.*, 2017; Aboal *et al.*, 2016; Hunt *et al.*, 2019; Tennessee *et al.*, 2017; Byrnes *et al.*, 2012; Dolan *et al.*, 2005; Cohen *et al.*, 2004). When labor market effects are considered, the dominant approach is to estimate the value of self-reported days of work lost in the aftermath of an assault (Peterson *et al.*, 2017, 2018; Lovell *et al.*, 2021; McCollister *et al.*, 2010; Miller *et al.*, 2021; Klietz *et al.*, 2010; Yang *et al.*, 2014). For example, Peterson *et al.* (2017) assumes that 3.6 productive work days are missed following a rape in their calculation of the economic burden of sexual assault.[3] This strategy can only capture the immediate impact of an assault on productivity, neglecting the potential for longer-term scarring on survivors' economic trajectories. There are currently no estimates that allow for the potential for persistent impacts of the trauma of rape on employment stability or earnings growth in the long run.

In this paper, we harness detailed Finnish administrative data to provide new insights into

[1] We use the terms "survivor" and "victim" to refer to individuals who have been raped, as both terms are widely employed in academic and policy literatures.

[2] See Peterson *et al.* (2024) for a review of public health estimates of violence costs, including sexual violence.

[3] Peterson *et al.* (2017) also allows for the health and substance abuse consequences of rape to impose separate productivity losses. In total, they estimate a cost of $122,461 per rape.

the economic impacts of rape on victims and their social networks. We use Finnish administrative records from both police reports and hospital admissions to identify sexual assaults.[4] We identify rapes from police reports through detailed crime codes and from hospital records using diagnosis codes that include items for rape examinations and sexual assault. These administrative records include unique identifying numbers that allow us to perfectly link victims to their economic outcomes in administrative tax records. Given the traumatic nature of rape, we supplement the information on labor market outcomes with linkages to prescription data to capture the mental health effects of rape. The vast majority (94 percent) of rape victims that we identify in administrative records are female. Thus, the majority of our analysis focuses on female victims.

We find that rape victims are young: 41% of victims are below the age of 21 when they are assaulted. Victims older than 21 are less likely to have completed college, more likely to have been prescribed anti-depressants, and have lower earnings and employment in the year before an assault than non-victimized women. The parents of younger victims (aged younger than 21 at the time of an assault) are less likely to be cohabiting, more likely to be on anti-depressants, and have lower household earnings than the parents of non-victimized adolescent girls. We find that the majority of sexual assaults identified in our administrative sources are reported to the police. Only 9% of the victims we can identify appear only in hospital admission data. Amongst the set of victims we can identify, we find that older women and those with no history of substance abuse are more likely to report an assault to the police.

Identification of the causal impact of rape on victims' outcomes is difficult for at least four reasons. First, victims might be on (permanently) different life trajectories, which could generate persistent differences in economic outcomes compared with non-victims, even in the absence of a sexual assault. Second, victimization might be linked to other external life shocks that separately negatively impact economic outcomes. For example, victims may be more likely to experience a job loss, which makes them more vulnerable to assault and separately reduces their economic outcomes. Third, victims are often very young. These young victims are either still at school or have very recently left, and so their pre-event economic outcomes are not observed. Based on

[4]We received ethical approval for this study (Aalto University decision: D/343/03.04/2025). Statistics Finland employs strict data protection safeguards to preserve individual anonymity.

these concerns, we divide our main analysis of the impacts on victims into two groups: "older" victims (aged 21 or older at the time of assault) for whom we can estimate more rigorous event study specifications using pre- versus post-crime labor market outcomes, and "younger" victims (aged 16-20 years old at the time of assault) for whom identification of labor market effects is more challenging given the absence of pre-event economic outcomes. Finally, we can only estimate the impacts of sexual assault in cases that appear in administrative records. As many victims do not report to the police (Daly and Bouhours, 2010), and may not seek medical treatment, this raises concerns about reporting bias.

For older victims, we estimate the impacts of rape using several complementary research designs. Our primary approach employs a matched-control event study to account for persistent observed differences in pre-assault economic and mental health trajectories. We find that rape causes a sharp and sustained deterioration in victims' economic outcomes: employment falls by 6 percentage points (p.p.) and earnings decline by €1,500 relative to matched controls on average over the five years following the assault. These effects correspond to a 15% decline in employment and 17% fall in earnings relative to pre-assault baselines. These effects are highly persistent. Neither employment nor earnings recover relative to the control group in the long run. Employment rates among victimized women remain approximately 9 p.p. below those of their matched controls a decade after being assaulted, and their annual earnings are €3,000 lower. These results indicate that rape fundamentally alters women's economic trajectories, generating long-run scarring rather than a temporary, transitory shock.[5]

We examine heterogeneity in the impacts of rape across victim characteristics and institutional contexts. The adverse effects on economic outcomes are smaller for victims in a cohabiting relationship and for those with a college degree. We find suggestive evidence that victimization costs are lower in areas with a higher share of female police officers and a more effective criminal justice response, indicating that institutional features may help mitigate the economic consequences of rape (Miller and Segal, 2019; Daly and Bouhours, 2010).

[5]We note that we cannot fully disentangle the impacts of rape itself from the potential secondary victimization impacts from negative interactions with police. However, the fact that we observe similar victimization impacts for the cases we identify in hospital records that were not reported to the police suggests that this is highly unlikely to be the driver of our results (Appendix Table A6).

We also explore the impact of rape on a broader set of non-labor market outcomes. Relative to matched controls, prescriptions for anti-depressants increase by 9 p.p. immediately after the rape and remain 5 p.p. higher five years later. The suicide rate for rape victims following an assault is 180% higher than their matched controls. We find that relationship formation is significantly reduced amongst rape victims: survivors are 8 p.p. less likely to be in a cohabiting relationship five years following an assault than their matched controls.

We perform several exercises to verify the robustness of our results to the primary identification challenges. We first examine whether victims experience other life shocks close to the assault that could reduce their economic outcomes and make them more vulnerable to rape. We estimate large and significant negative impacts of rape, even for women with no change in relationship status, employment, anti-depressant prescriptions, and no observable substance abuse in the years before the assault. We also leverage variation in the timing of victimization to check whether the magnitudes of our primary results are confounded by unobserved differences between victims and non-victimized women. We compare the evolution of victims' outcomes who were assaulted at age t to victims who are born in the same year but who were victimized at age $t+4$ in a "future victims" design (Fadlon and Nielsen, 2021; Fadlon *et al.*, 2025). This exercise also reveals large negative economic impacts of rape that are statistically indistinguishable from our main estimates. Unobserved differences between victims and (otherwise observationally identical) non-victims do not, therefore, play a significant role in driving our results.

To assess the external validity of our results, we address two key concerns: under-reporting of sexual assault and the expanding legal definition of rape to include less physically violent assaults. We do so by: (i) comparing cases reported to police with those observed only in hospital data, (ii) reweighting our estimates to align with victimization survey demographics, (iii) examining differences between more and less violent assaults, and (iv) analyzing related crimes against women not prosecuted as sexual assaults. We find little difference between police- and hospital-only cases, and reweighting has minimal impact on results. While effects are smaller for less violent cases, they remain substantial and statistically significant.

We do not observe good measures of younger victims' pre-assault economic outcomes. We

therefore estimate a matched first differences exercise for those victimized before age 21. Specifically, we compare the age-25 outcomes of young girls who experience a rape to young girls with the same (pre-event) GPA and mental health outcomes and who come from observationally similar families in terms of parents' education and income, but who do not experience a reported rape. We find that young victims who experienced a rape in adolescence are 10 p.p. less likely to graduate college, 14 p.p. less likely to be employed, and 6 p.p. more likely to be prescribed anti-depressants at age 25 compared to young women with similar pre-rape characteristics. The decline in college completion rates is particularly pronounced amongst the most academically able teenage victims.

Finally, we examine spillovers in the impact of sexual assaults on others in victims' social networks. We can identify the school that young victims were enrolled in at age 15 and observe anti-depressant prescriptions for all ages. We compare the mental health outcomes of female and male school peers before and after one of their classmates is assaulted, with adolescents enrolled in otherwise similar schools, but where no girls are victimized. We find a 0.2 increase in the number of female classmates taking anti-depressant prescriptions and no effect for male classmates. This can be interpreted as one additional girl being on anti-depressants in every five classes in which an adolescent girl is assaulted. The null effect for male peers is reassuring evidence against a school-level shock causing both a rape and a deterioration in mental health outcomes, given gender-homophily in childhood friendship networks (Mehta and Strough, 2009). We also examine the impacts of rape on the mothers and fathers of victims.[6] Using an analogous matched control event study specification to that estimated for older victims, we find that rape victims' mothers and fathers both experience significant negative impacts on their mental health outcomes. In particular, mothers' and fathers' anti-depressant prescriptions increase by 2.1 p.p. and 1.3 p.p., respectively, within 5 years after their daughters' rape.

Our findings make three main contributions to the literature. First, this paper contributes to the economics literature on gender-based violence. An active recent literature explores the impacts of gender-based violence on victims, largely focused on workplace harassment and do-

[6]We consider both younger and older victims for this analysis.

mestic violence (Sanin, 2021; Bhalotra *et al.*, 2021; Bindler and Ketel, 2022a; Bhuller *et al.*, 2024; Folke and Rickne, 2022; Karimi *et al.*, 2023; Chang *et al.*, 2023; Adams-Prassl *et al.*, 2024; Adams *et al.*, 2024a). Much of this literature builds on the groundbreaking work of Bindler and Ketel (2022a), who use Dutch administrative data to provide the first labor market impacts of victimization more generally.[7] In related work, Ornstein (2017) uses Swedish administrative data to examine the effect of violent crime on health and labor market outcomes of men and women, but does not focus on sexual assault.

Our analysis is complementary: we focus on a single, prevalent form of gender-based violence and tailor the empirical design to the distinctive age profile, timing, and long-run consequences of rape victimization. We show that sexual assault causes significant economic costs to victims despite it not being tied directly to a work or a cohabiting family setting. Our estimates emphasize the importance of prior work documenting triggers of sexual violence, including the regulation and solicitation of sex work (Cunningham and Shah, 2018; Ciacci and Sviatschi, 2022; Cunningham *et al.*, 2024), pornography spread by the internet (Bhuller *et al.*, 2013), clashes of gender norms (Guarnieri and Tur-Prats, 2023), "party culture" and alcohol consumption (Lindo *et al.*, 2018; Zimmerman and Benson, 2007; Topper, 2023), and sex offender registries (Agan, 2011).

Second, we contribute to the broader interdisciplinary literature on sexual assault across economics, public health, criminology, and law. Existing studies document the severe psychological consequences of rape, including elevated risks of post-traumatic stress disorder, depression, suicide, and work disruption (Resick, 1993; Potter *et al.*, 2018; Peterson *et al.*, 2017; Molstad *et al.*, 2023; Rothbaum *et al.*, 1992; Sabia *et al.*, 2013; Rees and Sabia, 2013). However, much of this research relies on survey-based or small-scale data, limiting the ability to measure long-run impacts or to address pre-assault differences in observed and unobserved characteristics of women who are and are not victimized. We advance this literature by leveraging population-wide administrative records and employing complementary identification strategies to credibly isolate the causal effects of rape. In contrast to prior work emphasizing short-run disruptions, we provide the first

[7] An earlier working-paper version reported preliminary estimates for sexual assault. The authors noted that these results were difficult to interpret due to pre-trends and imprecision, and they were not pursued further in the published version.

causal evidence on the longer-term scarring effects of rape by tracking the economic and health trajectories of victims relative to otherwise comparable women who are not victimized for up to a decade post-assault.

Third, we contribute to the literature estimating spillover impacts of crime on non-victims. Several papers assess the impact of local crime rates on the well-being of non-victims using either direct measures of health outcomes (Cornaglia *et al.*, 2014; Dustmann and Fasani, 2016; Currie *et al.*, 2022) or house prices via a revealed preference argument (Gibbons, 2004; Linden and Rockoff, 2008). Direct spillovers of physical violence on non-victimized household members have been previously documented. Bindler and Ketel (2022b) finds that the earnings of cohabiting partners also fall when their partners are subject to violent threats. Bhuller *et al.* (2024) find that children's mental health falls when their mother is subject to a domestic violence incident. We show that rape negatively affects the labor market and mental health outcomes of victims' parents. Our primary contribution to this literature is to show that negative spillovers extend beyond household members to schoolmates in the case of rape. This complements Carrell *et al.* (2018), who find that domestic violence has a significant effect on the earnings of peers of children in households where domestic violence occurs. Our findings on spillovers within families also suggest caution in interpreting contemporaneous between-siblings designs that have been used to assess the impact of sexual violence on outcomes (Rees and Sabia, 2013; Sabia *et al.*, 2013), which may not fully account for within-family transmission of trauma.[8]

2 Data and Descriptive Statistics

2.1 Identifying Rape

We identify the complete set of rapes recorded in Finnish administrative data, including police reports, inpatient hospital admissions, and outpatient hospital admissions from 2006 to 2019. We link these records to administrative data on employment, earnings, demographics, education, health, and family networks.

[8]We note that the between-siblings designs in Rees and Sabia (2013) and Sabia *et al.* (2013) were also not powered to detect the impacts of rape.

Police reports A police report is the first step in any investigation and occurs before any formal charge or court hearing. Reports can be filed online or in person at a police station. After an investigation, a suspect is charged only if the prosecutor considers that there is sufficient evidence to secure a conviction. After this step, a court case may take place. We use police reports rather than court records because we cannot perfectly link court records to wider registry data at the case level, as victim identifiers are not consistently recorded in the Finnish court data we obtained. However, in our view, it is optimal to use police reports rather than convictions. First, 30% of reports in our data do not identify a perpetrator and therefore cannot be prosecuted. They would be omitted if we restricted to cases that resulted in convictions. Further, a wide literature in criminology and sociology has found that the criminal justice system often fails to prosecute perpetrators of rape even when the perpetrator is known; case attrition rates between reporting and a court case are high for reasons that are typically unrelated to the veracity of the underlying claim (Daly and Bouhours, 2010; Hohl and Stanko, 2015). While focusing on police reports runs the risk of including false reports, evidence suggests this is a more minor concern. The review in Lisak *et al.* (2010) suggests a false reporting rate of between 2-10%. Their analysis of all sexual assault reports at a major US university found a 6% false allegation rate. Kelly *et al.* (2005) find a false reporting rate of 3% in their study of sexual assault police reports in the UK.

Police reports include a unique case number for each crime, information on the date and type of the crime, and basic demographics of the victim and suspect. There can be multiple charges associated with a given case (e.g., rape and menace). We focus on the total number of cases, not the total number of charges. The police indicate which is the most serious charge for a given case, and this is the crime type we use to identify rape and sexual assault.[9]

We classify an incident as "rape" if it is labeled with one of the following crime codes listed under Chapter 20 of the Finnish Criminal Code that defines sexual offenses: Aggravated Rape, Rape, Sexual Abuse, Coercion into a Sexual Act, and Sexual Harassment. We provide the full definitions of these crimes in Appendix B. We also classify an incident as rape if one of this set of crimes was attempted, i.e., Attempted Aggravated Rape, Attempted Rape, Attempted Sexual

[9] Appendix Figure A3 gives the distribution of the time between a sexual assault occurring and the date it is reported to the police. Almost 80% of rapes are reported in the year of occurrence.

Abuse, and Attempted Coercion into a Sexual Act. For our main results, we collectively refer to all of these crime types as "Rape". Appendix Figure A1 reports the distribution of crime codes among the 14,793 rapes we identify from the police data. 50% of the cases are flagged as Rape or Aggravated Rape. 41% of cases are Sexual Abuse, Coercion into a Sexual Act, or Sexual Harassment. The remaining cases are those where one of the primary crimes was attempted, e.g., Attempted Rape or other attempted sexual crimes. We demonstrate the robustness of our results to focusing on the sub-sample of cases labeled as Rape or Aggravated Rape in Section 3.1.

The police data includes unique national identification numbers (IDs) for both the victim and perpetrators when their identities are known. 99% of perpetrators are male, and we drop the cases involving a female perpetrator (see Figure A3). Appendix Figure A9 gives the relationship between victims and perpetrators, if any, that we can identify from administrative sources. For almost 30% of police reports, the perpetrator ID is missing. For just under 10% of cases, we identify the perpetrators as cohabiting partners, schoolmates, or extended families of the victims.[10] The remaining cases have a perpetrator ID from the police records, but we cannot explicitly link the perpetrator to the victim. We caution that the absence of a known relationship in the data does not necessarily indicate that the perpetrator is a stranger to the victim. For example, we cannot observe dating relationships that do not lead to cohabitation, so date rape could result in a perpetrator and victim whose relationship is not identifiable in our data, despite the perpetrator being known to the victim.[11]

Hospital diagnosis records We complement the police data with rape cases that we can identify through administrative data on hospital admissions. Finnish hospitals use the International Classification of Diseases (ICD) codes, specifically ICD-10, to classify diagnoses for both inpatient and outpatient visits. The hospital diagnosis data includes information on the type of visit, date of diagnosis, and ICD-10 codes assigned by healthcare professionals. These records allow us to

[10]Note that marital rape was not outlawed in Finland until 1994. The low share (less than 5%) of perpetrators who are cohabiting partners is consistent with Adams *et al.* (2024a) who find that for domestic violence cases, less than 5% of cases involve sexual crimes.

[11]Therefore, many of these cases could be intimate partner violence, even if we cannot identify them as such in our data. However, our results are also consistent with findings in psychology and criminology that stranger rapes are more likely to be reported to the police (Campbell *et al.*, 2001; Wolitzky-Taylor *et al.*, 2011).

observe instances of sexual violence that may not be reported to the police but are documented in medical settings.

To identify rape victims in hospital records, we use the following ICD-10 codes: Sexual assault by bodily force (Y05); Sexual abuse, confirmed rape or sexual assault (T74.2); Encounter for examination and observation following alleged rape (Z04.4). We present the share of each ICD code by inpatient and outpatient admission in Appendix Figure A2. The majority of identified cases are from inpatient records (86%). 85% of inpatient cases are for rape examinations. Outpatient visits identify the minority of rapes recorded in the hospital data (14%). Of the outpatient visits, 55% are for sexual abuse and 35% are for rape examinations.

Other register data After identifying the set of rape cases, we use the unique victim IDs recorded in both police reports and hospital admissions records to perfectly link victims from the police data to the Finnish Linked Employer-Employee Data (FLEED) and FOLK data, as well as data on anti-depressant prescriptions from Kanta and substance abuse hospitalizations.

FLEED is a population register data containing demographic characteristics, education, annual income, and employment. It covers all persons residing in Finland aged 15-70 for the years 1988-2016. FOLK is identical, but it also covers the years after 2016. The Kanta prescription data consists of every prescription filled by individuals in Finland through the public health system for the years 2000-2022. We focus on prescriptions for anti-depressants as anecdotal accounts from rape survivors indicate that these events likely have severe impacts on mental health and result in PTSD and depression. We use the hospital inpatient administrative records to identify hospitalizations for conditions involving alcohol and substance abuse.[12]

After merging the police and hospital data with our rich administrative data on individual outcomes and creating the sample of rape crimes, we construct the labor market and mental health trajectories of victims before and after a rape. We focus on employment, earnings, and anti-depressant prescriptions. Employment measures whether and in which plant an individual was working in December of each calendar year. Income consists of all labor income reported to

[12]See Appendix B for the ACT codes (prescription data) and the ICD-10 codes (hospitalization data).

the tax authorities and measured in December of each year (salary and wage earnings, as well as any self-employment income, but excluding benefits). Anti-depressant prescriptions include the number and kind of anti-depressant prescribed.

2.2 Descriptive Facts

We focus on rape cases involving female victims in the main text. This represents the overwhelming majority of cases; 92% of cases where victims are 21-65 that we can identify through administrative police and hospital data sources involve a female victim. In Table A8, we provide descriptive statistics on male victims.[13]

Figure 1 gives the age distribution of female victims recorded in the police or hospital data. 41% of female victims are younger than 21 years old at the time of the crime. This trend aligns with survey evidence suggesting that many women experience rape during their teenage years or in their twenties.[14]. We do not observe good measures of pre-assault economic characteristics for younger victims. We will therefore split our sample into those who were victimized before age 21 ("younger" victims) and those victimized between age 21 and age 65 ("older" victims) for the rest of our analysis. We exclude the small number of victims aged 65 years and older, given our focus on economic impacts.

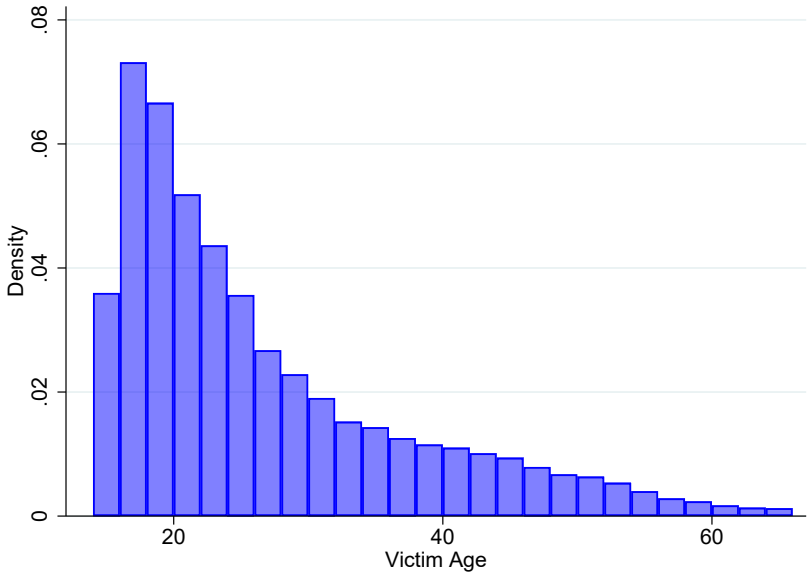
Older Victims Table 1 Panel A reports the characteristics of older victims in the year before they are assaulted.[15] Column 1 provides statistics for the full set of victims identified through police and hospital records. Column 2 gives the characteristics of the subset of cases reported to the police. Column 3 gives the characteristics of victims who appear in the hospital data with a diagnosis code for rape but with no police report for rape in the same year. Column 4 provides the equivalent statistics for women aged 21-65 who are not identified as victims of rape in the

[13]We focus on female victims throughout the main text for three reasons. First, they comprise the majority of cases. Second, their age distribution and other aspects of their cases differ substantially from those of sexual assaults with male victims, suggesting male and female victims should not be grouped together. Last, rape is globally viewed as a gendered crime, with links to broader social issues of gender inequality and power imbalance (MacKinnon, 1989; Brownmiller, 1993).

[14]https://www.cdc.gov/violenceprevention/sexualviolence/fastfact.html; Bindler and Ketel (2022b)

[15]For rapes identified from police reports, we take the year of assault from the date of the reported incident. For rapes identified from hospital records, we use the date of the hospital admission.

Figure 1: Age Distribution of Female Rape Victims



Notes: Figure gives a histogram of the age distribution with 2-year age bins for the population of rape cases involving a female victim identified from police reports and hospital records between 2006-2019. The sample size is 19870.

police or hospital data.

Comparing older victims to other Finnish women aged 21-65, we find that victims are younger and have lower educational attainment. Victims have a higher rate of anti-depressant usage and are more likely to have a history of substance abuse than the average Finnish woman who isn't victimized. Victims have lower economic outcomes than other women in the year before an assault; they are less likely to be employed, and have lower earnings in the year before victimization. However, while there are differences in the level of employment and earnings between older victims and non-victims, we do not find any evidence of poor pre-trends in economic outcomes before an assault (see Appendix Figure A5, which we discuss in more detail below).

Younger Victims The majority of younger victims are still in school and have not yet entered the labor market at the time of the sexual assault. Since their labor market outcomes are realized only after victimization, we also report the demographic characteristics of their parents in Table 1 Panel B. We distinguish between the characteristics of mothers and fathers in Appendix Table A1. We again report the characteristics of victims (Column 1), split between those recorded to the

police (Column 2) and those only present in hospital data (Column 3), and those of non-victimized girls of similar age (Column 4).

We find that younger victims have lower GPAs and are more likely to receive anti-depressant prescriptions. They also tend to come from families where parents are less likely to cohabit, more likely to use anti-depressants, and have lower education levels, employment rates, and earnings. Figure A4 presents the full distribution of GPA (measured at age 15) for younger victims and non-victimized adolescent girls. While victims have lower GPAs on average, there is substantial overlap in the GPA distributions of the two groups.

Table 1: Summary Statistics

	All	Police Reported	Hospital Only	Non-Victim
	(1)	(2)	(3)	(4)
Panel A: Older Victims				
Age	32.40	32.58	31.02	43.83
College	0.12	0.12	0.14	0.27
High School	0.55	0.55	0.56	0.58
Dropouts	0.33	0.34	0.30	0.14
Anti-Depressant	0.34	0.34	0.35	0.11
Employed	0.44	0.44	0.47	0.72
Earnings	10207	10188	10351	22562
+ Earnings	17103	17171	16619	27636
Any Violent Crime	0.05	0.05	0.04	0.00
Substance Abuse	0.10	0.10	0.11	0.00
Cohabitation	0.32	0.33	0.29	0.65
Speak Finnish	0.90	0.90	0.91	0.89
Observations	10596	9334	1262	1556985
Panel B: Younger Victims & Parents				
Age	17.83	17.78	19.20	18.01
Student	0.63	0.64	0.40	0.64
Has GPA	0.39	0.39	0.52	0.45
GPA	7.37	7.37	7.38	7.89
Anti-Depressant	0.21	0.21	0.25	0.04
Substance Abuse	0.02	0.01	0.05	0.00
Parents Cohabit	0.42	0.42	0.40	0.63
Hhld. College	0.14	0.14	0.17	0.21
Hhld. Earnings	50589	50298	57710	66547
Hhld. Emp	0.68	0.68	0.70	0.81
Parent Anti-Depressant	0.25	0.25	0.22	0.17
Parent Any Substance	0.03	0.03	0.04	0.01
Parents Any Violent Crime	0.04	0.04	0.04	0.01
Observations	5682	5459	223	1953623

Notes: Table reports the summary statistics for overall female victims (column 1), female victims identified from the police records (column 2), and female victims identified from hospital ICD codes only (column 3) when the victims are 21-65 years old at the time of crime in Panel A. We also report statistics for women aged 21-65 years old in 2014 who have not committed or experienced violent crimes in columns 4. Panel B reports summary statistics for younger victims, i.e. girls who experience a rape before 21 years old, and their parents. Column 4 in panel B reports summary statistics of same-aged non-victim girls in 2014. All variables in all columns are measured at t-1. Earnings consist of all labor market earnings (the sum of wage, salary, and self-employment earnings) deflated to 2013 Euros. "+ Earnings" is earnings excluding zeros. "Substance Abuse" is the proportion hospitalized for alcohol or drug abuse. "Parents Cohabit" is a dummy equal to 1 if the mother and father cohabited with each other and 0 otherwise. "Hhld. College" and "Hhld. Emp" are the average college level and employment status between mothers and fathers; "Hhld. Earnings" is the total earnings of mothers' and fathers'; "Parent Anti-Depressant", "Parent Any Substance", and "Parent Any Violent Crime" is a dummy equal to 1 if either parent has anti-depressant prescription, substance abuse, or violent crime records, respectively.

Reporting Comparing Columns 2 and 3 of Table 1 reveals that the majority of rape cases we identify in the administrative data sources are identified through police reports. For older victims, this relationship is not mechanical. There is no mandatory reporting of sexual assault by health authorities in Finland for victims aged 18 or over.

Just 12% of our older sample is present only in hospital admissions data and cannot be linked to a police report in the same year. Those identified only through hospital records are younger compared to victims who can be identified through police reports. Turning to younger victims, Table 1 Panel (b) shows that only 4% of younger victims are present only in the hospital data. This group is 1.4 years older on average than cases with a police report, suggesting that police reporting rates are likely higher for younger victims because of the mandatory reporting requirement for health authorities.

To examine the characteristics of individuals who appear in police reports more formally, we estimate a linear probability model in which the dependent variable is an indicator for whether the case is recorded in police data. Figure A6 reports the resulting coefficients. We find that women younger than 33 years old (the median age among older victims) and those with a prior history of substance abuse hospitalization are less likely to report to the police. Prior economic outcomes (employment and education) are not significantly associated with reporting behavior among the set of victims we can identify in administrative records.

In our analysis on the impact of rape below, we control for differences in pre-assault characteristics using a matching procedure to identify observationally equivalent individuals for each case. We also assess the robustness of our findings by analyzing whether there is a statistically significant difference in the estimated impact of rape between cases that are reported to the police and those that only appear in hospital records.

Nonetheless, our baseline estimates of the prevalence of rape from administrative sources are likely to substantially understate the true incidence. Victimization surveys suggest that many cases go unreported to police (European Institute for Crime Prevention & Control, 2009; EU Agency for Fundamental Rights, 2015), and the majority of our documented cases come from

police reports.[16] To address concerns about the relevance of our main estimates for the broader set of cases (including those not reported to police), in Section 3.3, we use responses to a victimization survey in Finland to re-weight our main estimates to account for differences in the characteristics of women who record that they reported to the police versus those who did not report to the police. We also note that Despite inherent limitations in the measurement of rape and selective reporting into administrative data, our analysis represents an essential first step in understanding the broader economic impacts of rape.

3 Impacts on Older Victims

3.1 Labor Market Outcomes

We document raw employment and earnings dynamics for older victims before and after rape in the solid lines of Figure 2. Panel I (a) shows the average employment rate for rape victims in the five years before and after a rape. Victims' economic outcomes are improving in the years before being assaulted. Their employment rates increase by 9 percentage points in the five years leading up to the rape. Appendix Figure A5 replicates the average employment and earnings of female victims in the five years before an assault, but also includes earnings and employment of all women in Finland (weighted to have the same age-education distribution). We again see that the pre-trends in economic outcomes of victims and non-victims are identical, with no evidence of differences in earnings or employment trends before the rape occurs.[17]

This growth in employment and earnings leading up to the event provides a stark contrast to the abrupt halt and then deterioration in these same outcomes after the rape occurs. Figure 2 shows that employment falls in absolute terms by 3 p.p. in the three years following a rape. Victims' earnings growth also reduces substantially; while in the five years before being raped, victims' earnings grew by 34%, in the five years after being assaulted, earnings growth stalls.

While these descriptive results are suggestive, two concerns immediately arise. First, if vic-

[16] For example, it is estimated that 70% of rapes are not reported to the police in the U.S. (Wieberneit *et al.*, 2024).

[17] This remains true when we normalize relative to three years before rape in Panels (c) and (d) of Appendix Figure A5. However, we note that there are significant differences in the levels of earnings between victimized women and the average non-victimized woman.

tims' employment and earnings growth had continued on the same trajectory absent the rape, then we would severely understate the impacts of rape if we only compare raw means around the assault. Second, descriptive statistics in Table 1 show that women who experience a rape have lower employment, earnings, and education before the rape compared with other women in Finland. If perpetrators target more vulnerable women who would have experienced worse economic outcomes irrespective of the assault, we risk overstating the negative effects of rape.

We harness a series of complementary empirical strategies to overcome these identification challenges. Our primary approach estimates a matched-control event study design. This allows us to control for persistent observed differences in pre-assault economic trajectories by comparing changes in survivors' outcomes before and after the rape to those of observationally equivalent women who do not report a rape. This empirical strategy ensures that we do not confound persistent observed differences between victims and non-victims with the effect of rape. It does not, however, rule out that victims experience idiosyncratic life shocks that lead both to greater vulnerability and worse economic outcomes, nor does it account for potential unobserved differences in the economic trajectories of victimized and non-victimized women.

In Section 3.2, we demonstrate the robustness of our primary results to these remaining concerns. We show that our main conclusions hold for women who do not experience job loss, relationship breakup, mental health crises, or substance abuse crises immediately preceding an assault, which may separately lead to a deterioration in earnings. We also leverage variation in the age of victimization to account for the possibility of persistent unobserved differences in economic trajectories. Specifically, we contrast survivors' outcomes before and after rape with those of other victims before they are victimized in a "future victims" identification strategy.

Main Empirical Strategy Starting with our main matched event study approach, we identify women who are observationally identical to victims according to their key economic and demographic characteristics before they experience a rape. We restrict to victims we can follow five years before and five years after the event for most of the analysis.[18] We perfectly match the age

[18]This implies that we cannot analyze the impacts of rapes occurring after 2014.

group, college degree, employment status, cohabitation status, and whether a woman takes anti-depressant prescriptions in the year before rape (year -1).[19] Within these groups, we match on a wider set of covariates based on an estimated propensity score. To do so, we estimate a linear probability model, where the dependent variable is a dummy capturing whether an individual is a victim and with controls including a woman's age, high school graduate dummy, as well as an anti-depressant prescription dummy, hospitalization for substance abuse (either alcohol or drug abuse), earnings group, fertility, cohabitation, and employment status in the four years before rape.[20] Using this estimated propensity score, we identify a victim's three nearest neighbor matches. Appendix Table A2 shows that the characteristics of victims and their matched controls are essentially identical in the year before victimization. Figure 2 (a) and (b) show that employment and earnings trends for the treatment and control groups are nearly identical in the five years before rape.

With matched control and treatment observations in hand, we estimate the following regression model:

$$Y_{it} = \sum_{j=-5}^{5} \left(\delta_j D_{i,j} + \alpha_{m(i),j}\right) + \gamma_t + \epsilon_{it} \tag{1}$$

where Y_{it} represents the outcome of interest for woman i in year t. $D_{i,j}$ is an indicator variable for the treatment (being a victim of rape) in the year j since the assault. $\alpha_{m(i),j}$ give the set of match-by-time from rape fixed effects.[21] δ_j are the coefficients of interest, identifying the effects of a rape relative to the matched counterfactual without a rape. Given the inclusion of $\alpha_{m(i),j}$, δ_j is identified from variation between victims and their matched controls in the time period of interest. Additionally, we include year fixed effects, γ_t. Standard errors are clustered at the match level.[22]

Figure 2 Panels (c) and (d) depict the event study coefficients from Equation 1 for victims

[19] We require that a victim's nearest neighbor match is in the same quartile of the age distribution.

[20] We use a linear probability model rather than a logit model to preserve as many observations as possible, because the logit regression will drop observations when the model fails to converge. Matching victims to controls in the same year provides control women with a pseudo-event year.

[21] We include match pair ID by time-since-rape fixed effects to ensure that we are comparing outcomes between treated individuals and their matched controls in the same year relative to event year.

[22] Comparisons always occur between treated (police report for rape) and never-treated (no police report for rape) individuals to address concerns of bias in event-study estimates (Goodman-Bacon, 2021; Sun and Abraham, 2020).

aged 21 and older at the time of the rape. We observe a decline of approximately 6 p.p. in the employment rate of rape victims in the first year after the incident relative to their matched controls, which increases to around 8 p.p. decline in employment by five years later. There are similarly significant impacts on earnings, with victims experiencing a €1,453 fall in annual earnings on average over the five years following an assault. This corresponds to a 17% reduction compared with average earnings in the five years before the incident (see Table 2).[23]

Appendix Figure A7 presents event-study estimates from Equation 1 up to ten years post-rape for victims assaulted between 2006 and 2009, for whom long-run outcomes are observed. Neither employment nor earnings recover relative to the control group in the long run. Employment rates among victimized women remain approximately 9 percentage points below those of their matched controls even at the end of the sample period, and annual earnings are about €3,000 lower. These persistent gaps indicate that rape fundamentally alters women's economic trajectories rather than resulting in a temporary, transitory shock.

Comparison to Alternative Crimes To put the size of these effects in context, we compare them to the victimization costs of non-sexual crimes. Ideally, we would be able to disentangle the effects of other violent crimes from sexual crimes. However, because many violent crimes against women can involve sexual elements without meeting the legal definition of rape in Finland, comparisons between rape and other violent crimes against women are likely to suffer from substantial misclassification bias. For this reason, we benchmark the impacts of rape against property crimes, which are far less likely to involve sexual violence and therefore provide a cleaner non-sexual crime comparison group.

We take two different approaches. First, we simply estimate the victimization cost of property crimes for female victims using the same matched-control event study design described above (Equation 1), where we select a matched control for each property crime victim who is observationally identical except that she did not experience a property crime. Appendix Figure A11

[23] We note that we cannot fully disentangle the impacts of rape itself from the potential secondary victimization impacts from negative interactions with police. However, the fact that we find similar victimization impacts for the cases we identify in hospital records that are not reported to the police suggests that this is highly unlikely to be the driver of our results (Appendix Table A6).

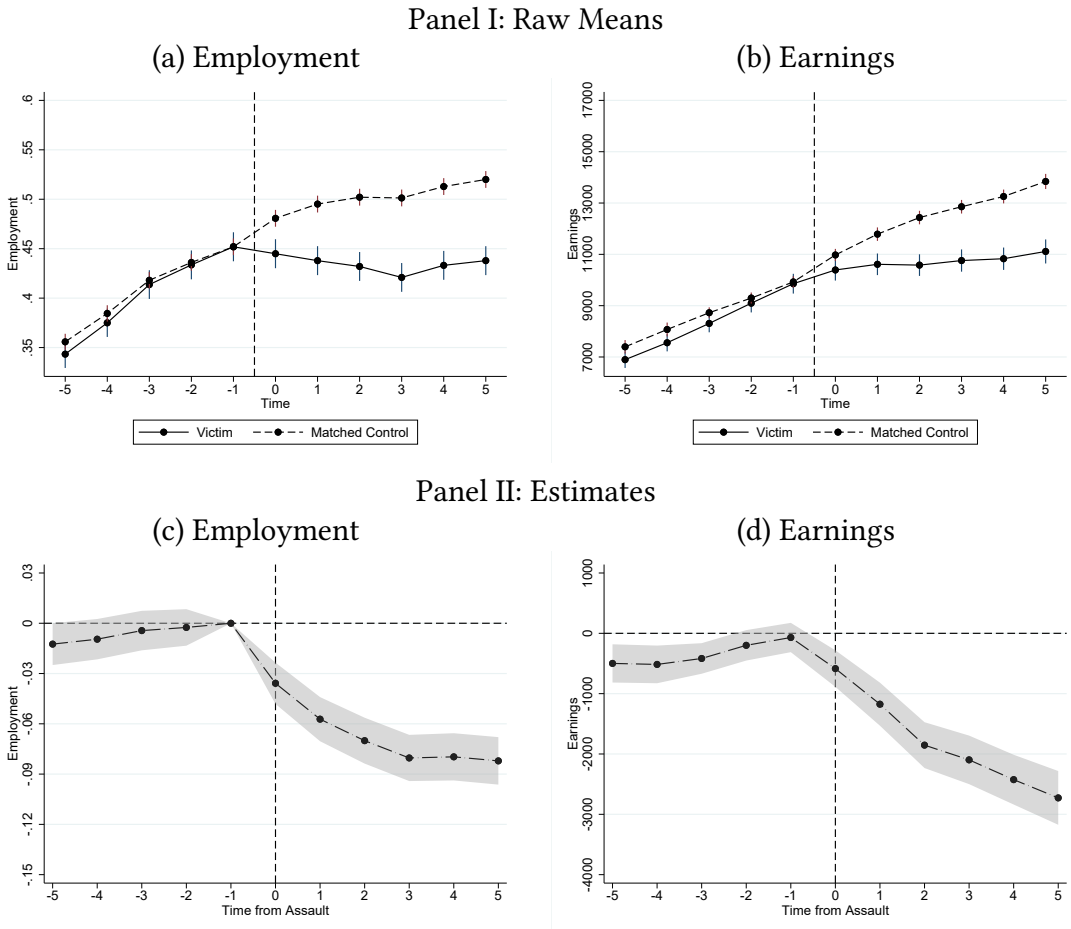
shows that in the five years following a property crime, there is little change in employment for victims of property crime relative to their matched controls. This is in sharp contrast to the large employment impact of rape on women.

However, this approach does not deal with the fact that women who experience property crimes may be very different from women who experience rape.[24] This may have an impact on their ability to recover from any crime, including rape. As a complementary strategy, we select a matched control observation for rape victims from the sample of women experiencing a property crime. This directly provides an alternative "non-sexual crime" counterfactual for sexual assault victims instead of having the matched control observations experience no crime at $t = 0$ as in our main specification. Using this approach, we observe that rape victims experience a 5.7 p.p. larger fall in employment in the five years following an assault compared to observationally equivalent women who experience a property crime.

We also note that the effects of sexual assault are similar in size or larger than other significant events estimated in previous literature, such as the child penalty, job displacement, burnout, and workplace injury. See Appendix Table A3 for direct comparisons to these other estimates. Many of these other events have robust social support mechanisms in place to offset their large impacts. For example, unemployment insurance is offered to address job loss, and family leave is provided to address the impacts of children. Our estimates, which suggest comparably large impacts of rape, could be used to justify similarly robust social safety system responses to sexual violence.

[24]For example, Table 1 documented that women who experience rape are lower income and more likely to be on anti-depressants compared to all other women in Finland.

Figure 2: Impact of Rape on Victim Employment and Earnings



Notes: Figure Panels (a) and (b) report descriptive statistics for employment and earnings before and after a rape occurs for victims and their matched controls. The sample consists of all rapes reported to police and recorded in hospitals in Finland from 2006-2014 where victims are 21 or older at the time of the assault. Panels (c) and (d) report estimated impacts of a rape relative to matched controls (equation 1). Year 0 denotes the year in which the rape occurs. Employment indicates whether an individual was employed during the last week of the year (the reference week). Earnings consist of all labor market earnings (the sum of wage, salary, and self-employment earnings) and is deflated to 2013 Euros. We observe employment and earnings for all years, unless the individual has died or moved out of Finland. Standard errors are clustered at the match level. The sample size is 194524 for employment results and 192508 for earnings results.

Heterogeneity by Individual Characteristics and Quality of Police Response Appendix Table A4 explores heterogeneity in the impacts of rape based on a victim's age, education, employment status, cohabitation status, and anti-depressant use in the year prior to the assault. We find no significant differences in the impacts of rape based on age, prior anti-depressant use, or employment status.

Victims with a college degree experience a smaller decline in employment, significant at the 5% level. This pattern is consistent with stronger baseline labor market attachment among college graduates,[25] which may make them less vulnerable to complete job separation following a traumatic event. At the same time, college-educated victims suffer earnings losses that are similar in absolute magnitude to those of non-college-educated victims, potentially reflecting reductions in hours worked or slower career progression. Yet, as college graduates have higher pre-assault earnings, these losses translate into a smaller proportional decline: about 3% relative to baseline earnings, compared to 19% for victims without a college degree.[26] Additionally, we find a larger negative effect of rape among those who were single in the year prior to the assault, suggesting that having a partner may mitigate some of the adverse effects of victimization.

The impact of rape may also vary with the characteristics of the assault itself. In Section 3.3, we examined heterogeneity by the crime code used to classify the assault (e.g., rape versus attempted rape) as part of a discussion of the external validity of our results in light of recent expansions in the legal definition of sexual assault in many OECD countries. Here, we also examine heterogeneity with respect to whether the perpetrator is identified (within the subset of cases with a police report). The criminology literature has previously found suggestive evidence that "stranger rapes" are typically more hostile and more likely to be surprise attacks (see Greathouse *et al.* (2015) for a review). We find that the impact of rape is slightly larger for cases with an unknown perpetrator in Appendix Figure A10: employment rates and earnings fall by 7.2 p.p and €1,870 on average per year over the five years after an assault for women victimized by an unknown perpetrator compared to 5.6 p.p and €1,360 for those with a known perpetrator.

[25] The average employment rate for women with a college degree in Finland is 84%.

[26] Average earnings of victims with and without a college degree in the years before an assault were €22,489 and €7930 respectively.

Finally, we examine heterogeneity in victim impacts as a function of local police responsiveness and criminal justice quality. Criminal justice systems around the world have long been criticized for their ineffectiveness in addressing sexual crimes (Edwards *et al.*, 2011; Gregory and Lees, 1996). Daly and Bouhours (2010) document the high attrition rate of rape cases in judicial systems worldwide. Finland is no exception: a 2019 Amnesty International Report remarked on the "high levels of impunity for sexual violence in Finland and the state's failures to respond to sex crimes".[27]

We examine heterogeneity in victimization costs by the share of female officers in the police force, as existing literature has shown that gender composition of the police can have a differential impact on gender-based violence (Miller and Segal, 2019), and also the quality of the local criminal justice response to rape. We use the lagged local "clearance rate" as a measure of criminal justice quality. This is defined as the ratio of the number of rape cases that proceed to court over the number of rape cases reported to police in municipality d and year t:[28]

$$CR_{d,t} = \frac{RapeCourtCases_{d,t}}{RapePoliceReports_{d,t}} \tag{2}$$

We use lagged clearance rates to avoid a victim's own case from entering the construction of the clearance rate, which could mechanically induce correlation between clearance rates and victim outcomes.[29] A "leave-one-out" construction of the clearance rate is not feasible because police and court records cannot yet be perfectly linked at the individual level in our data.

Appendix Table A5 reports the results. Victims residing in municipalities with lower rape clearance rates experience substantially worse labor-market outcomes following an assault. In below-median clearance municipalities, post-assault employment declines are 3 p.p. larger, and annual earnings are approximately €700 lower per year in the five years following an assault.

[27] See: Amnesty Report Summary.

[28] Appendix Figure A22 gives the distribution of rape clearance rates across municipalities and time periods. The average clearance rate is 23%. There is substantial variation in clearance rates: the 25th percentile is 18%, and the 75th percentile is 26%. Our measure of clearance rates aggregates court and police cases to the municipality. This may introduce measurement error, as court and police jurisdictions do not always align perfectly with one another or with municipal borders.

[29] For example, more severe cases could be both more likely to proceed to court and more likely to generate large adverse impacts.

We also find that a higher share of female police officers is associated with improved victim outcomes: in regions where the share of female officers is above the national median, the adverse impacts on victims' employment and earnings are reduced by 4.3 p.p. and 1000 €, respectively. While we caution against over-interpreting these results since clearance rates and police gender composition are not randomly assigned across municipalities and are imperfect proxies for criminal justice quality, the findings suggest that more effective prosecution of rape cases and greater female representation in policing may reduce the economic burden of sexual crimes for victims.

There could be several mechanisms underlying these results, ranging from victims feeling safer when perpetrators are punished to more responsive policing practices that reduce secondary victimization. Regardless of the precise channel, the findings suggest that criminal justice institutions can play an integral role in facilitating victim recovery.

3.2 Robustness

We examine the robustness of our results to the two main threats to identification that are not addressed by our primary empirical strategy: (i) victims may experience shocks that make them more vulnerable to assault and have an independent impact on their economic trajectories; (ii) there may be unobserved differences between victims and non-victims that affect counterfactual trajectories.

Negative Life Shocks Survivors may experience negative life shocks shortly before victimization that independently affect both subsequent economic outcomes and perpetrators' opportunity to commit violence. For example, job loss or relationship dissolution can alter daily routines and patterns of exposure in ways that perpetrators exploit. Similarly, periods of acute stress, mental health crises, or substance use may coincide with declines in labor market attachment while also increasing perpetrators' ability to target individuals who are temporarily more isolated.

To address these concerns, we take two approaches. First, we observe if these life events predict victimization when comparing victims and their matched controls. If these events have little predictive power when comparing victims and matched controls, this suggests that our

matching exercise already does a good job of addressing this concern. In Appendix Figure A12, we report results from a linear probability model where we regress victimization status on mental health prescriptions, employment, cohabitation, and substance abuse in the three years prior to an assault.[30] For completeness, we also include all other variables in Table 1. While victims have very different economic, mental health, and cohabitation trajectories compared to the population of non-victimized women, all estimates are very close to zero, and most are statistically insignificant when comparing victims and their matched controls.

More directly related to our concern about shocks, and not levels, being important determinants of both risk of victimization and labor market outcomes, in Appendix Table A7, we construct a series of possible "shocks." These consist of becoming unemployed (job loss), becoming single out of cohabitation (breakup), becoming depressed (starting on anti-depressants), and being hospitalized for substance abuse, all just before the rape occurs. We find that becoming single, becoming depressed, and being hospitalized for substance abuse are all predictive of victimization relative to the full population of women in Finland. However, there is no significant predictive value when we compare victims to their matched controls. Together, these results suggest that our main analysis effectively addresses this concern.

To address any remaining concerns, we use the same matched DiD strategy to examine how potential life crises may affect the magnitude of our estimated effects. Specifically, we compare the impact of rape on all victims, including those who have experienced negative life events between one (year -1) and two years (year -2) before a rape, to victims who have not experienced each of these life shocks. Note that these life shocks can separately impact economic outcomes (our identification concern) but can also make it harder to recover from an assault. The second possibility implies that omitting victims who have experienced each of these life shocks could lead us to underestimate the true impacts of rape. For example, someone who experiences a mental health crisis might be more vulnerable to sexual assault and also experience larger negative economic impacts because poor mental health makes it harder to recover from the assault. Nonetheless, it is important to determine whether our findings remain or change when we omit

[30]The year of assault is set equal to 2014 for non-victimized women, the middle of our full sample period.

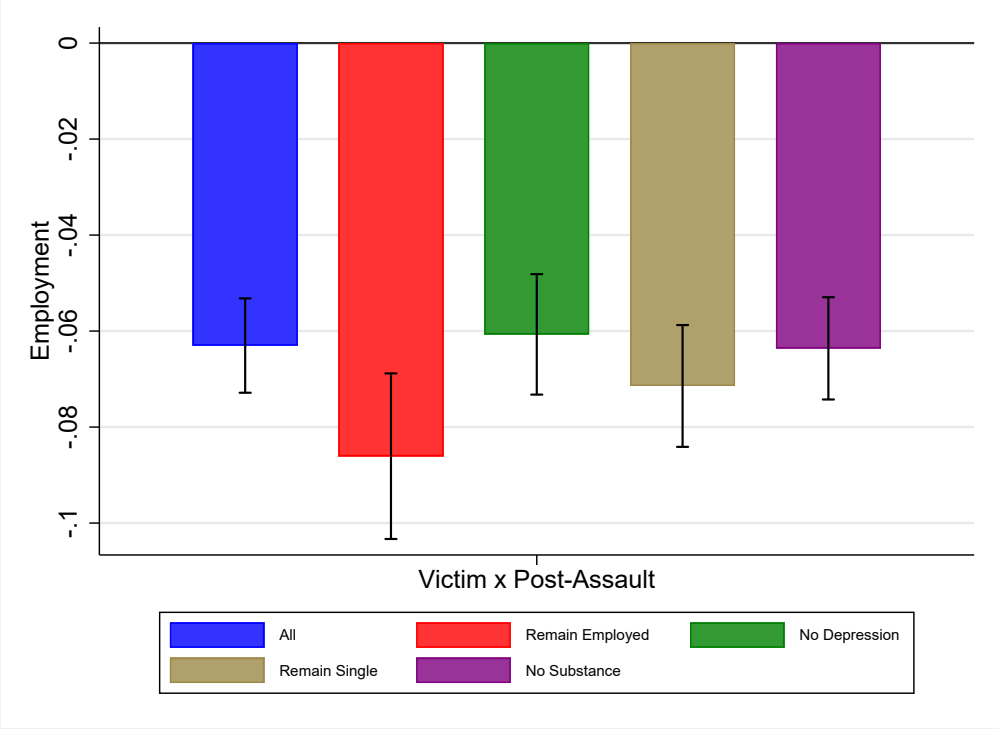
victims who experience each of these possible life shocks from the analysis.

Figure 3 presents results. The first bar reports our main DiD estimate for the employment effect of rape for all victims. The second bar restricts to those who do not experience a job loss before the rape. The third bar restricts to victims with no anti-depressant prescriptions in the two years before a rape. The fourth bar restricts to victims who remain single in the two years before the rape. The fifth bar reports impacts for those who have no contact with the health system for substance abuse in the two years before a rape. We still find significant and considerable negative effects for victims who have not experienced a job loss, who have no history of anti-depressant prescriptions, who remain single, and who do not have any observed problem with substance abuse. These results suggest that rape has large negative effects on victims' employment, which cannot be fully explained by other possible negative life events occurring around the same time as the rape.

Unobservable Differences Between Victims & Non-Victims A valid concern is that victims may share unobservable factors that make them more vulnerable to rape and cause differences in the evolution of their economic outcomes. To address this concern, we leverage variation in the timing of victimization, using "future victims" as counterfactual controls (Bhuller *et al.*, 2024; Fadlon and Nielsen, 2021; Fadlon *et al.*, 2025). Formally, we compare outcomes at ages $t+1, t+2, t+3$ for those who were victimized at age t (treatment) to those who are victimized at age $t+4$ (the control group). We use age $t+4$ as a control group as a compromise between wanting our counterfactual observations to be as similar as possible to those treated at age t, while also ensuring we observe victims' outcomes for several years after an assault. That is, we compare victims born in the same year, with the main difference being that one is victimized at a slightly earlier age, while the other is victimized later. A cost of using $t+4$ as a control group is that we are only able to analyze victims' outcomes for the three years after an assault, before the "control" group's outcomes are contaminated.[31]

[31] We prefer our primary matched DiD identification strategy to this future victim approach for two interrelated reasons. First, the age distribution of victims is skewed: even within our "older" sample, many victims are relatively young (Figure 1). Second, a central contribution of our paper is to estimate the long-run effects of rape, whereas much of the existing literature has focused on short-run direct productivity costs. The future-victim design thus

Figure 3: Impact of Rape on Victim Employment, Robustness to Life Shocks



Notes: Figure reports estimates of δ_j obtained using Equation (1) where we collapse into a pre- and post-period to recover difference-in-differences (DiD) estimates. The first column reports our main estimates. The next four columns report the impacts of rape on employment where we omit victims who experienced the following life event between one and two years before the rape in turn: 1) job loss, 2) mental health deterioration (victims who start mental health prescriptions), 3) end of cohabitation, 4) substance abuse. The estimates use the matched control for the victim to identify effects 5 years before and 5 years after the victim experienced a rape. The sample consists of all rapes reported to police and recorded in hospitals in Finland from 2006-2014 for female victims 21 years or older at the time of the assault. Employment indicates whether an individual was employed during the last week of the year (the reference week). We observe employment for all years unless the individual has died or moved out of Finland. The sample size is 194524 for all results.

We follow Fadlon and Nielsen (2021) and Fadlon *et al.* (2025) to estimate the following specification:

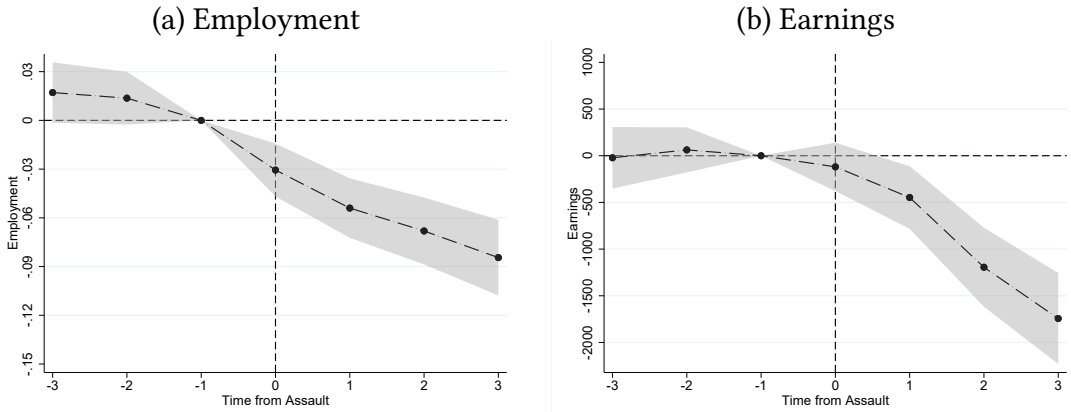
$$Y_{it} = \sum_{j \neq -1} (\delta_j I_j \times Treat_i + \gamma_j I_j) + \alpha_i \times Treat_i + \lambda X_{it} + \epsilon_{it} \tag{3}$$

where I_j denotes indicators for years relative to the event (the actual event among the treatment group and the placebo event among the control group); $Treat_i$ is an indicator if i is in the treatment group; X_{it} includes year fixed effects, a quadratic in age, and interactions of age and average earnings in the three years prior to victimization (or placebo victimization for the control group). As victims can appear in both the treatment and control group, their individual fixed effect is interacted with treatment status. We cluster standard errors at the individual level throughout. δ_j gives the primary coefficients of interest.

Figure 4 (a) and (b) give our results for employment and earnings, respectively. Appendix Figure A8 reports the raw mean comparison between victims and future victims over the analysis time horizon (i.e. periods $t = -3$ to $t = 3$ for the "treated" victims and outcomes corresponding to $t = -4$ to $t = -1$ for "control" victims). Future victims' employment rates and earnings increase continuously over the period, while victims experience a significant drop in their economic outcomes right after rape. Under this alternative empirical design, we estimate that employment decreases 9 percentage points 3 years after rape, similar to our main estimates. We estimate that victims' earnings are €1,774 lower three years after a rape using this future victim design compared to €2,028 in our main specification. The consistency of short-run results across these alternative empirical designs suggests that unobserved differences between victims and (otherwise observationally identical) non-victims do not, therefore, play a significant role in driving our main results.

faces a tension: comparison groups must be similar in age to ensure comparability, yet be victimized at sufficiently different ages to avoid contamination of the control group. This limitation makes it unsuitable for estimating long-run outcomes.

Figure 4: Impact of Rape on Older Victims: Using Future Victims as Controls



Notes: Panel I (a) and (b) report estimated impacts of a rape using the future victim to identify effects 3 years before and 3 years after rape, estimating equation 3 for employment and earnings respectively. Year 0 denotes the year at which the rape occurs for the victim. Employment indicates whether an individual was employed during the last week of the year (the reference week). Earnings consist of all labor market earnings (the sum of wage, salary, and self-employment earnings) and is deflated to 2013 Euros. We observe employment for all years unless the individual has died or moved out of Finland. The sample size is 115269.

3.3 Reporting & External Validity

Many cases of sexual assault are not reported. The legal definition of rape has also been expanded in many countries in recent years to put less emphasis on physical violence. Given this context, we probe the external validity of our estimates by: (i) examining heterogeneity in the impact of rape for cases that are reported to the police versus those that appear only in the hospital data; (ii) reweighting our main estimates to account for differences in the observed characteristics of victims in the administrative data and in victimization survey data; (iii) examining heterogeneity in effect sizes for more and less violent forms of sexual assault within our sample;[32] (iv) estimating the impact of crimes against women that are perpetrated by rapists but not prosecuted as sexual assaults.

Police Reported versus Hospital Only and Survey Cases We re-estimate our main results using the subset of rape cases that can only be identified from hospital ICD codes (i.e., they are not reported to the police). Appendix Table A6 shows that we continue to find large economic

[32]Victimization surveys suggest that approximately 10% of physical assaults are reported to the police in Finland, with lower reporting rates for crimes considered less serious by the victim (EU Agency for Fundamental Rights, 2015).

impacts for unreported rapes. Employment falls by 6.6 p.p. compared to matched controls in the five years following an assault (relative to a pre-assault employment rate of 0.45 in the five years before an assault). We similarly continue to find a large impact on earnings, which fall €739 per year relative to matched controls. However, this effect is noisy given the small sample size.

We additionally use a gender-based violence survey conducted in Finland in 2021 to re-weight our administrative sample so that the distribution of victim characteristics more closely matches that of all rape victims, including those who did not report to the police (Attila *et al.*, 2023).[33] The survey includes 7,575 effective respondents, of whom 32% report having experienced sexual violence since age 15. A key limitation of this exercise is that the survey measures respondents' current characteristics (e.g., age, education, and employment status), while eliciting information on experiences of sexual violence since age 15.[34] As a result, the survey does not allow us to observe victims' characteristics at the time of assault, which is the relevant comparison for assessing selection into reporting. We are unable to fully resolve this mismatch because we were not permitted to link the survey and administrative data, and we do not know the exact timing of victimization from the survey questions.

With this significant caveat in mind, when we re-weight our sample of police-reported and hospital-identified victims to match the survey distribution of age, education, and employment status (measured at the time of the survey), we obtain estimates that are very similar to our main results. The reweighted employment effect is a 7 p.p. decline relative to matched controls, compared to 6.2 p.p. in the baseline specification. The corresponding reweighted earnings effect is a €1,659 decline per year, compared to a baseline estimate of €1,453.

Legal Definition of Rape and External Validity The legal definition of what constitutes rape has evolved in many OECD countries in recent years to acknowledge the importance of consent during sexual activity. Finland used a coercion-based definition of rape during our study period. Chapter 20 of the Finnish Criminal Code defined rape as occurring when either:

[33] We were not permitted to directly link the victimization survey data to administrative records for this project.

[34] The survey asks about domestic violence in the previous five years but does not record this time period in the case of sexual violence.

- A person who forces another into sexual intercourse by the use or threat of violence directed against a person.
- A person takes advantage of the fact that another person, due to unconsciousness, illness, disability, state of fear or other state of helplessness, is unable to defend themselves or to formulate or express their will, and has sexual intercourse with them.

In practice, the thresholds for a "helpless state" and "expression of will", i.e., the threshold for the second condition, were set very high over our time period. An Amnesty International representative stated in 2019: "In many ways, Finland is a model country for gender equality, but its antiquated rape laws are badly failing women. The definition of rape in Finnish law, which is based on physical violence and incapacity, is outdated".[35] Many other countries during this period also set a high bar for an assault to be categorized as rape. For example, 31 out of 38 OECD countries still used coercion-based definitions of rape during the period we study (post-2006) (Uhnoo *et al.*, 2024).

In 2023, new legislation came into effect in Finland that defined rape by lack of consent, whether or not force was involved.[36] As such, our main estimation sample will consist of cases that are more likely to be physically violent and will not necessarily reflect the impacts of cases that could be classified as rape, given a modern definition that includes a consideration of consent. We first examine heterogeneity in the impact of rape across crime types in our sample that are likely to be more/less physically violent. Our definition of sexual assault includes assaults recorded as sexual abuse and coercion into sexual acts, in addition to those directly labeled rape (Section 2.1). We find slightly higher victimization costs when we restrict to the subset of assaults recorded as "rape" and "aggravated rape" (Appendix Table A6). These are likely to be the most physically violent assaults in our sample. However, the impact of the wider set of crimes encompassed within our main definition of sexual assault remains substantial.

In the Finnish police records, we find that 40% of men who perpetrate at least one rape commit other crimes against female victims. Those cases are not recorded as rape in our police data, but

[35] https://www.amnesty.org/en/latest/news/2019/09/finland-international-body-condemns-outdated-definition-of-rape-as-amnesty-calls-for-rape-law-reform/

[36] See the discussion at the Finnish Ministry of Justice portal and Chapter 20 of the Finnish Criminal Code.

may share similarities with the rape cases we identified using the official definition of rape. This section presents descriptive facts and estimated effects for the sample of women who are attacked by a known rape perpetrator, but where the crime is not categorized as rape. We consider this an imperfect but suggestive exercise to probe the external validity of our main estimates to a broader definition of sexual assault.

Appendix Figure A13 plots the distribution of the officially reported type of crime experienced by other female victims of rapists. Assault, sexual abuse of children, menace, and petty assaults are the most common crimes recorded in the police data for these other female victims of rapists.[37] This distribution of crimes, and particularly the high rate of assault, suggests that these other crimes committed by perpetrators could have been sexual assaults but would not have been classified as such under the legal definitions of rape in Finland during the years we study. Appendix Table A2 Column 3 reports summary statistics on the demographic characteristics of other female victims of rapists, but where the crime was not a rape. We find that these victims are similar to the main estimation sample of rape victims. While they are slightly older, have a lower probability of taking anti-depressants, are more likely to be employed, and have higher earnings, the magnitudes of these differences between the two groups of victims are generally small.

We apply the same matched difference-in-differences identification strategy to examine the impact of these violent crimes perpetrated by rapists on other women. The last two columns of Table 2 report the matched DiD results. We find a large and significant negative impact on female victims' employment and earnings. The point estimates indicate that compared to their matched controls, the employment rate of rapists' other female victims drops by 4.2 p.p. and earnings drop by €675 (5.4% relative to the baseline mean) within 5 years after the crime. The point estimate is smaller compared to the effect on rape victims (first two columns of Table 2) but still large in magnitude.

[37] For the rest of the analysis, we restrict to cases excluding sexual abuse of children.

Table 2: Impact on Rape Victims and Rapists' Other Female Victims

	Rape Victims		Other Victims of Rapists	
	Employed	Earnings	Employed	Earnings
	(1)	(2)	(3)	(4)
Victim x Post Assault	-0.0618***	-1453***	-0.0423***	-675**
	(0.0051)	(145)	(0.0075)	(221)
Victim	-0.0058	-340**	0.0010	-170
	(0.0033)	(111)	(0.0050)	(183)
Control Mean	0.409	8683	0.504	12550
Observations	194524	192508	84656	83892

Notes: Table reports estimated impacts of rape relative to matched controls. "Post assault" is a dummy equal to 1 in the year or an assault and any year after. Employment indicates whether an individual was employed during the last week of the year (the reference week). Earnings consist of all labor market earnings (the sum of wage, salary, and self-employment earnings) and are deflated to 2013 Euros. Standard errors are clustered at the match level. We observe employment for all years unless the individual has died or moved out of Finland. Control means refer to mean levels of the outcome in the control group in the years -5 to -1. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

3.4 Mental Health and Family Formation Outcomes

To understand the repercussions of rape beyond labor market outcomes, we estimate impacts on mental health and family formation. Anecdotal accounts and survey data indicate that experiencing rape causes immediate and sustained psychological damage to victims (Jina and Thomas, 2013). It is essential to understand the extent to which the trauma described by survivors materializes in large-scale data and the persistence of these impacts. Deteriorating mental health may also serve as a key mechanism by which economic outcomes deteriorate (Biasi *et al.*, 2021).

Figure 5 (a) reports matched event study impacts of experiencing a rape on whether the victim is prescribed any anti-depressant. The impact is large and immediate. Compared to their matched controls, older victims are 9 p.p. more likely to be prescribed anti-depressants in the first year after the rape, and this effect persists at 5 p.p. five years later. In the year before the assault, 34% of victims were prescribed anti-depressants, so these effects are substantial.

Reports indicate that rape victims are also at greater risk for suicide (Kilpatrick, 1992). We link our victims and their matched controls to the universe of death records in Finland. Mechanically, there are no differences in suicide between victims and their matched controls before the event, so we report the rates of suicide for victims in the years following a rape.[38] We find that the suicide rate for rape victims is 180% higher than their matched controls following an assault (Appendix Figure A14).

Experiencing sexual assault may make individuals less likely to date and form new relationships or cause them to break up with an existing partner. Moreover, rape may cause victims to experience trauma around sexual intimacy. Figure 5 Panels (b) and (c) report matched DiD effects of rape on cohabitation status and fertility. We find large negative impacts of rape on cohabitation: the cohabitation rate of victims is 8 p.p. lower compared to their matched control after a reported rape, and the effect persists for at least five years. We find no significant fertility impacts.

3.5 Impacts on Male Victims

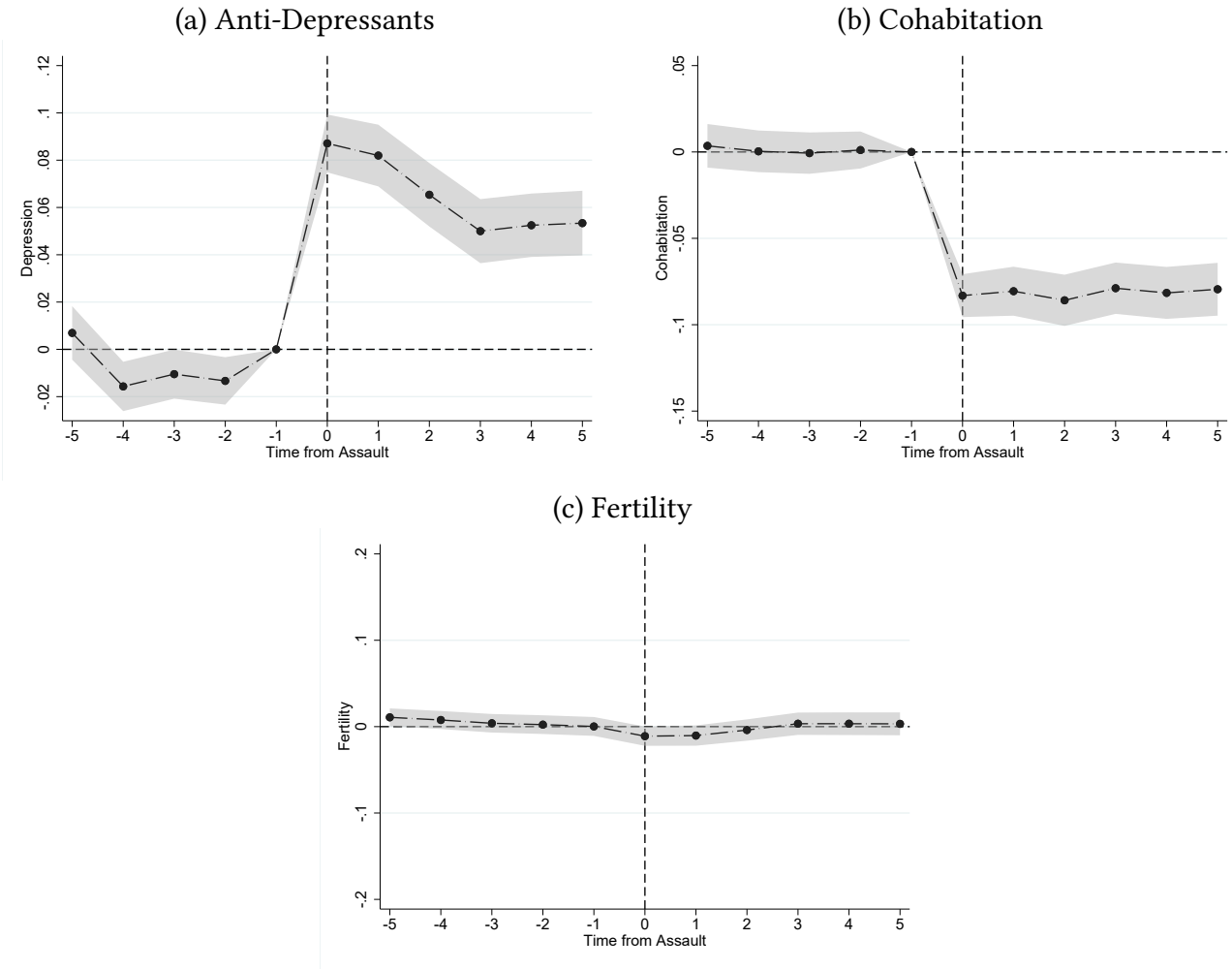
A minority of cases we identify through administrative police and hospital data sources, 8%, involve a male victim. We report summary statistics for this sample of male victims in Appendix Table A8. In the year before victimization, we find that male victims have lower education levels, higher rates of anti-depressant use, more records of violent crime, and more substance abuse issues compared to non-victim men in Finland. We also find evidence that male victims are less likely than female victims to report an assault to the police; a larger share of male cases are present only in hospital records compared to that for female victims (Appendix Figure A6).

Appendix Table A9 presents the estimated effects of rape on male victims' outcomes using an equivalent matched DiD approach to that used for female victims. We find that rape has a negative impact on male employment and earnings, though these effects are not statistically significant. However, we do find a significant 1.9 p.p. increase in anti-depressant prescriptions and a 2.1 p.p. increase in substance abuse as measured by prescription and hospitalization records following victimization. In addition, we find that rape has a 6 p.p. negative impact on the probability that

[38] Given that suicide is thankfully still rare, we use death records for all available post-assault years.

victims enter a cohabiting relationship. Together, these results suggest that sexual assault also has a lasting impact on men who are victimized.

Figure 5: Impact of Rape on Victim Non-Labor Market Outcomes



Notes: The figure reports estimated impacts of a rape in Finland using the matched control to identify effects 5 years before and 5 years after rape (estimating equation 1). Year 0 denotes the year at which the rape occurs. The sample consists of all rapes reported to police and recorded in hospitals in Finland from 2006-2014 where victims are 21 or older at the time of the assault. Panel (a) reports impacts on an indicator equal to one if the individual is prescribed an anti-depressant at any point in the year. Panel (b) reports impacts on cohabitation, a variable equal to 1 if the woman is cohabiting with a partner. Cohabitation is reported at the end of each year. Panel (c) reports impacts on fertility, which is a variable equal to 1 if the woman gives birth in a given year. Standard errors are clustered at the match level. The sample size is 194524.

4 Impacts on Younger Victims

For victims below age 21, we do not observe a long time-series of employment and earnings outcomes before a rape occurs, so we cannot identify the causal impacts of rape on labor market outcomes through event studies. Instead, we compare the age-25 outcomes of victims to same-age girls who are observationally equivalent on 1-digit primary school GPA, anti-depressant prescriptions, and their mothers' and fathers' earnings decile and education in the year before the rape occurs. Specifically, we estimate the following equation:

$$y_i = \beta Rape_i + \gamma_{g(i)} + \delta_{t(i)} + \epsilon_i \tag{4}$$

where y_i is an age-25 outcome, $Rape_i$ is a dummy equal to 1 if the girl has experienced a rape, $\gamma_{g(i)}$ is the set of match pair fixed effects, and $\delta_{t(i)}$ gives the year the victim turns 25 years old.

Table 3 presents estimates from Equation 4. By age 25, victims are almost 14 p.p. less likely to be employed and are 10 p.p. less likely to have completed college. To further explore these effects, we present heterogeneous results based on victims' pre-rape GPA and their parents' income in Appendix Figures A15 and A16. We find that the effect on college completion is larger for girls with higher GPAs, i.e., those who were most likely to go to college absent a rape. In contrast, we find that the employment effect is significant for all groups except girls with the highest GPAs. Therefore, it is the group of girls on the margin of attending college and the group of girls on the margin of employment who are most affected. When we examine heterogeneity by parental income, we find a consistent story: the effect on college completion is larger for girls from high-income families, whereas the employment effect is larger for those from low-income families.

We are not able to observe a sharp discontinuity in employment or earnings at the time of the rape since these girls are largely still in school. We therefore interpret these estimates as suggestive evidence of large economic costs of experiencing rape as an adolescent girl, while acknowledging that some portion of the estimates may capture pre-existing differences between victimized and non-victimized girls.[39]

[39]To assess the OLS estimation strategy for younger victims, we adopt a comparable approach for older victims

Table 3: Age-25 Outcomes of Victims Relative to Matched Controls

	Emp. (1)	College (2)	Depressed (3)	Cohabit (4)	Fertility (5)
Rape	-0.1375***	-0.1005***	0.0640***	-0.0455***	0.1339***
	(0.0131)	(0.0078)	(0.0106)	(0.0130)	(0.0127)
Observations	180275	180275	180275	180275	180275
Control Mean	0.705	0.287	0.101	0.526	0.227
Fixed effects					
Year	✓	✓	✓	✓	✓
Match	✓	✓	✓	✓	✓

Notes: Table reports the impact of experiencing a rape on different age-25 outcomes. Emp. is a dummy variable equal to 1 if the individual is employed at age 25. College is a dummy variable equal to 1 if the individual has a college degree (or higher) by the age of 25. Depressed is a dummy variable equal to 1 if the individual has a prescription for anti-depressants at age 25. Cohabitation is a dummy variable equal to 1 if the individual has a cohabiting partner at age 25. Fertility is a dummy variable equal to 1 if the individual has 1 or more children by age 25. Estimates are from equation 4, and include match pair fixed effects and controls for the year the victim turns 25.

Table 3 columns (3)-(5) report the impact of rape on age-25 mental health, cohabitation, and fertility for young victims. We find that adolescent girls who experience rape are 6.4 p.p. more likely to take anti-depressants compared to their matched controls at age 25. They are 4.5 p.p. less likely to be in a cohabiting relationship but 13 p.p. more likely to have children. There is a statistically significant 2 p.p. increase in fertility in the year following an assault, suggesting that a small part of the increase in completed fertility by age 25 may be a direct consequence of the rape itself.

We observe anti-depressant prescriptions at all ages and therefore have measures of mental health before and immediately after the rape occurs for young victims. This allows us to estimate matched event studies for this outcome for those victimized in adolescence. As anti-depressant prescriptions are rare before age 12, we identify victims' exact match on any-prescription usage in the three years before a rape, as well as their age, household income quartile, parents' college

by comparing outcomes within cells of age group, college education, employment status, cohabitation status, and anti-depressant usage one year before rape and estimating a simple OLS to compare the outcomes of victimized and non-victimized women in the fifth year after rape. We find a larger impact on employment (-17 percentage points reduction, standard error of 0.0048) and earnings (5,334 Euro reduction, standard error of 240) using the coarse matching and simple comparison between treatment and control.

completion, and pre-assault parental employment. Appendix Figure A17 shows that girls who experience a rape are 14 p.p. more likely to be on anti-depressants the year after the incident compared to their matched control in the year following a rape. While these impacts dissipate over the next few years, these girls are still almost 10 p.p. more likely to be on anti-depressants five years later, consistent with the longer-term age-25 mental health effects reported in Table 3. Last, we find that the suicide rate of young victims is 560% higher compared to their matched controls (Appendix Figure A19).

5 Spillovers on Peers and Families

A rape could also be traumatic for the victim's family and her peers. In this section, we explicitly examine these spillover effects to capture some of the indirect impacts of rape.

5.1 School Peers

Many victims (22%) are younger than 18 years old, the minimum school leaving age, when they are victimized.[40] Motivated by the evidence of significant peer effects in classrooms, we examine whether the school peers of young victims' are adversely affected by a sexual assault occurring within their social network (Hoxby, 2000; Carrell *et al.*, 2018).

We observe the school that a victim was enrolled in at age 15, but not their class.[41] We construct a victim's peer group as those who were enrolled in the same school and born in the same calendar year. We refer to this group as the victim's school cohort. Appendix Table A10 provides summary statistics on the characteristics of female students in school cohorts in which at least one female student was raped between ages 15-18 (upper secondary school), alongside summary statistics for female students in school cohorts in which no rapes are recorded. 28% of Finnish adolescent girls belong to a school cohort in which at least one of their female peers is raped before the age of 18. There are no significant differences in the family characteristics of non-

[40] 41% of victims are younger than 21 and comprise our younger victims sample (see Table 1), but in this section, we only focus on young victims who are still in secondary school so we can observe their school peers.

[41] The average secondary school class size in Finland is 20 pupils, so there will be multiple classes per cohort in larger schools. This will mean that our measure of a victim's peer group will be more attenuated in larger schools.

victimized peers and girls in cohorts where no rapes are reported. However, victimized school cohorts are significantly larger than non-victimized cohorts: 62 girls per victimized school cohort versus 53 girls in non-victimized cohorts.

We consider the outcomes of female and male students in the same school cohort as the victims who do not report an assault. We restrict our analysis to school cohorts that had 50 girls or fewer to ensure that the girls likely knew each other at the time of the assault.[42] This restriction involves dropping 49% of school cohorts. Our conclusions are robust for cohorts with 100 girls or fewer but are less precisely estimated (Appendix Figure A20); 99% of Finnish female school cohorts over our study period were smaller than 100.

Since economic outcomes are unavailable until older ages and we do not have access to high-frequency data on education outcomes, we focus on anti-depressant prescriptions. Our primary outcome measure is the number of adolescents in a school cohort prescribed anti-depressants. This allows us to capture if, for example, a victim's closest friend was strongly affected by the sexual assault, but other classmates (especially in larger cohorts) are not affected. We focus on victims younger than age 18 because they are likely in school before this age.

We estimate a matched event study specification and compare the evolution of mental health outcomes in school cohorts where a rape occurs to cohorts in different schools that are similar based on pre-assault cohort size and mental health outcomes, but where no rape is reported. Specifically, we exactly match on school cohort size in bands of 10 students and whether any student (beyond the victim) was prescribed anti-depressants in each of the three years before the assault.[43] With matched control and treatment cohorts in hand, we estimate the following regression model:

$$Y_{it} = \sum_{j=-3}^{5} \left(\delta_j D_{i,j} + \alpha_{m(i),j} \right) + \gamma_t + \beta_i + \epsilon_{it} \tag{5}$$

where Y_{it} gives the number of non-victimized students with an anti-depressant prescription in school cohort i in year t. $D_{i,j}$ is an indicator variable for treatment (a student in the cohort being

[42] See the last two columns of Appendix Table A10 for descriptive statistics at the peer level, and Appendix Table A11 for summary statistics collapsed to the average for the cohort.

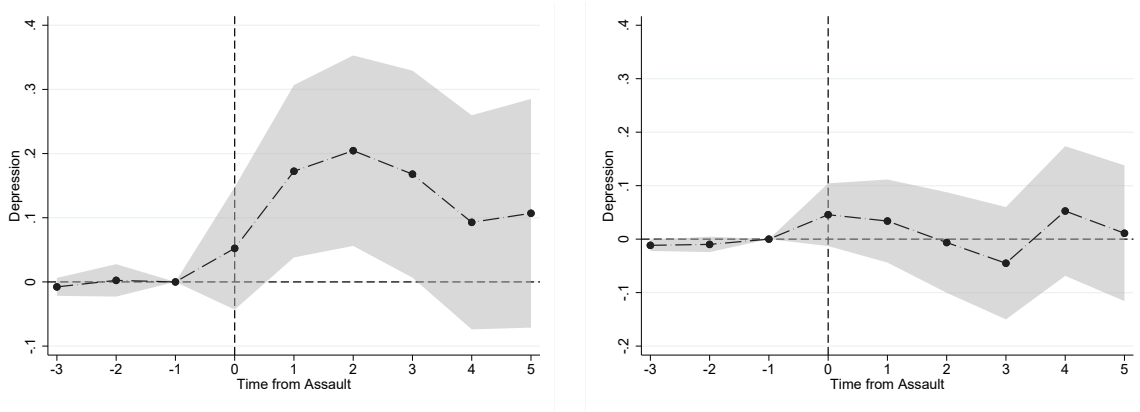
[43] We drop other age cohorts within the same school and do not use them as comparison groups, given the potential for spillover effects across cohorts within schools.

raped) in year j since the assault. $\alpha_{m(i),j}$ give the set of match-by-time from rape fixed effects. δ_j are the coefficients of interest, identifying the effects of being in a school cohort in which a rape occurs relative to the matched counterfactual without a rape. δ_j is identified by variation between victimized and non-victimized cohorts in the time period of interest. Additionally, we include year fixed effects, γ_t, and birth year fixed effects, β_i.

We report results from this exercise separately for female and male peers in Figure 6. We find evidence consistent with worsening mental health outcomes among the victim's female school peers. By the second and third year following the rape, there is a 0.2 increase in the number of a victim's female school peers who receive an anti-depressant prescription. Thus, in every five school cohorts in which a student is raped, on average one non-victimized girl starts to take anti-depressants. This effect size dissipates by a very small amount in years four and five after the rape. Panel (b) shows no significant effect of the rape on anti-depressant prescriptions among male peers. This is reassuring given that strong gender homophily in friendships among adolescents would suggest effects should be strongest for adolescent girls (Mehta and Strough, 2009). It also provides reassurance that some other school-wide shock is unlikely to be behind the negative effects we observe for female peers.

The deterioration in mental health among girls attending the same school as the victim may reflect peer spillovers, but it could also capture cases in which the perpetrator assaults another girl in the same cohort who does not report the incident. When we restrict the sample to cases in which the perpetrator was aged 18 or older (Appendix Figure A21), we continue to observe an increase in anti-depressant prescriptions among female peers. This provides suggestive evidence that the baseline effect is unlikely to be driven entirely by multiple assaults committed by the same adolescent perpetrator within a cohort. At the same time, our data do not allow us to fully disentangle peer spillovers from additional unreported victimization. Under either interpretation, the findings point to harms that extend beyond the officially recorded victim and underscore the importance of a robust policy response.

Figure 6: Impact of Rape on the Mental Health of Female and Male Schoolmates of the Victim



(a) Girls (b) Boys

Notes Figure reports the impact of a rape that results in a police report or hospital record on the number of anti-depressant prescriptions among the victim's female school cohort classmates in Panel (a) and male school cohort classmates in Panel (b). We focus on victims younger than age 18. The estimates use peers from observationally similar school cohorts but where no female peer reports a rape to the police, estimating equation 5. Standard errors are clustered at the match level. The sample size for panel a is 45207 and for panel b is 45774.

5.2 Parents

We also examine spillover effects within the family. The majority of our victims are not cohabiting at the time of an assault, so we focus on parents. We employ the same matched event-study strategy we estimated for older victims, but instead of examining victims' employment, earnings, and mental health, we focus on these same outcomes for their parents. We perfectly link victims to their parents using registry information. With these links, we then create a comprehensive panel data set for biological mothers and fathers of victims consisting of their age, education, violent crime reports, employment, and earnings history in the five years before the crime and the five years after. Among Finland's full population, we find matched controls that are observationally similar to the victims' parents, using the same matching strategy that we employed for older victims. With matched controls in hand, we estimate Equation 1 but with the outcomes of the victims' mothers and fathers as the variables of interest.

Table 4 illustrates the negative spillover impact of rape on the mental health of mothers and fathers estimated in our matched difference-in-differences framework. We find that mothers' and fathers' likelihood of taking anti-depressant prescriptions increases by 2.1 p.p. and 1.3 p.p., and the differences between mothers and fathers are significant at the 10% level (see Appendix Table

A12 for a formal test pooling the data). The mental health impacts are particularly large and robust relative to the control mean, with mental health prescriptions increasing by 14% and 13% for mothers and fathers, respectively. On the other hand, we find no significant impact on rape on parents' employment or earnings.

Table 4: Impact of Rape on Victims' Parents

	Mothers			Fathers		
	Employment (1)	Earnings (2)	Depressed (3)	Employment (4)	Earnings (5)	Depressed (6)
Rape	-0.0044	-61	0.0206***	-0.0036	-208	0.0130***
	(0.0037)	(161)	(0.0033)	(0.0040)	(239)	(0.0028)
Observations	292688	289400	292688	251592	248736	251592
Control Mean	0.673	20882	0.162	0.653	28536	0.092
Fixed effects						
Year	✓	✓	✓	✓	✓	✓
Match	✓	✓	✓	✓	✓	✓

Notes: Table reports the impact of a rape on the mothers' and fathers' outcomes in the five years after their daughter experiences a rape. Estimates use a matched control DiD research design to identify effects 5 years after versus 5 years before rape, estimating equation 1 but where we collapse to pre and post treatment and select matched controls for victims' mothers and fathers. Employment indicates whether an individual was employed during the last week of the year (the reference week). We observe employment for all years unless the individual has died or moved out of Finland. Depression indicates whether the parent received at least one anti-depressant prescription in that year. Standard errors are clustered at the match level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

6 Conclusion

This paper provides the first large-scale causal evidence on the long-run economic impacts of rape. Using linked administrative records from Finland, we demonstrate that sexual violence has persistent and substantial consequences for survivors' employment, earnings, and mental health. Victims experience sharp declines in labor market attachment and earnings, accompanied by sustained increases in anti-depressant use and elevated suicide risk. These effects extend far beyond the immediate aftermath of an assault, persisting for at least ten years and, for young victims, shaping their educational attainment, employment opportunities, and family formation outcomes well into adulthood.

Our analysis underscores that the costs of rape extend beyond survivors themselves. We find significant negative spillovers on the mental health of school peers and on the well-being of parents. Adolescents who share classrooms with victims are more likely to be prescribed antidepressants, and the mothers and fathers of survivors exhibit measurable declines in psychological well-being. These spillovers highlight the broader social toll of sexual violence.

By linking rape to long-term economic trajectories, our results complement prior work in public health, criminology, and law that has emphasized the psychological, social, and legal dimensions of sexual violence. Importantly, our study moves beyond short-run cost-of-crime exercises, which typically value lost workdays or health expenditures, by documenting persistent scarring effects that depress survivors' human capital accumulation, labor supply, and earnings potential. It is possible that the impacts of rape may be even larger in other contexts beyond Finland where social support is more limited, and women face greater social stigma following an assault.

Our findings highlight that sexual violence is not only a moral and human rights issue but also an economic one, with consequences for gender inequality in labor markets and for aggregate productivity. Our suggestive findings that victimization costs are lower in areas with higher clearance rates and a greater share of female police officers are consistent with the idea that more effective prosecution and gender representation in policing may help reduce the economic burden of sexual violence for victims. However, much more research is needed to identify the policies that can best support victims.

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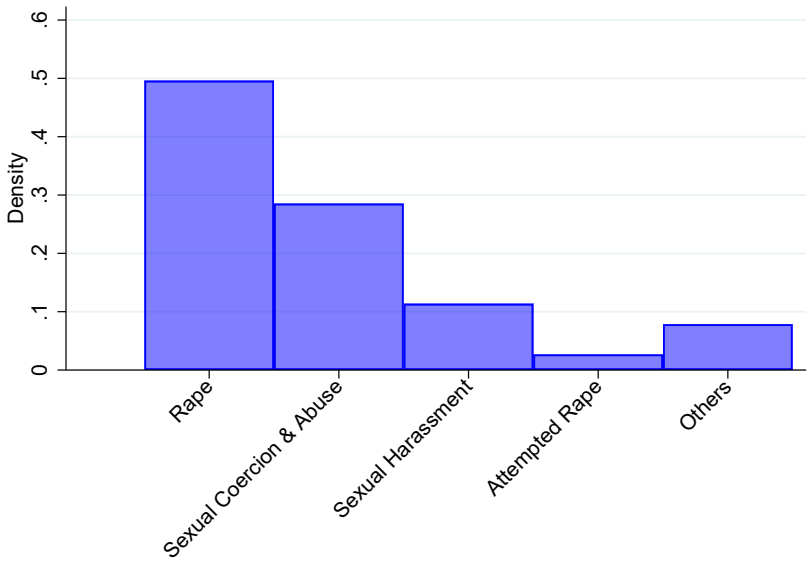
Online Appendix

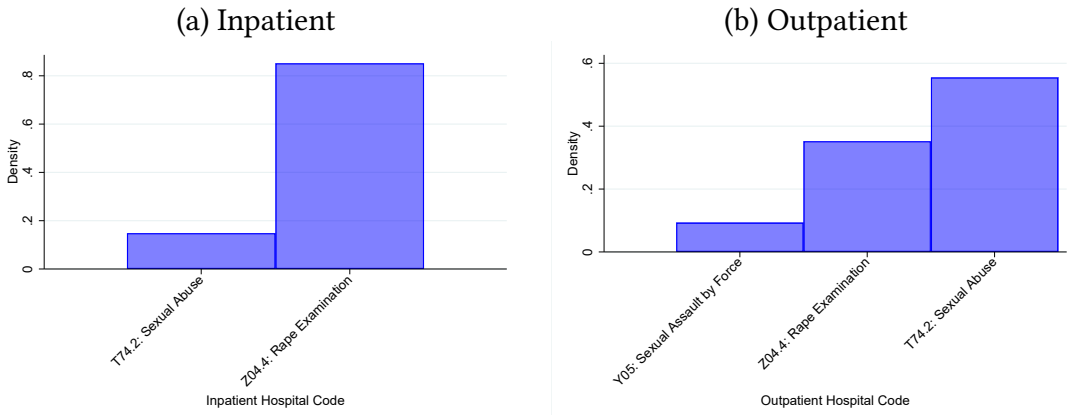
A Additional Figures and Tables

Figure A1: Types of Crimes in the Police Rape Sample



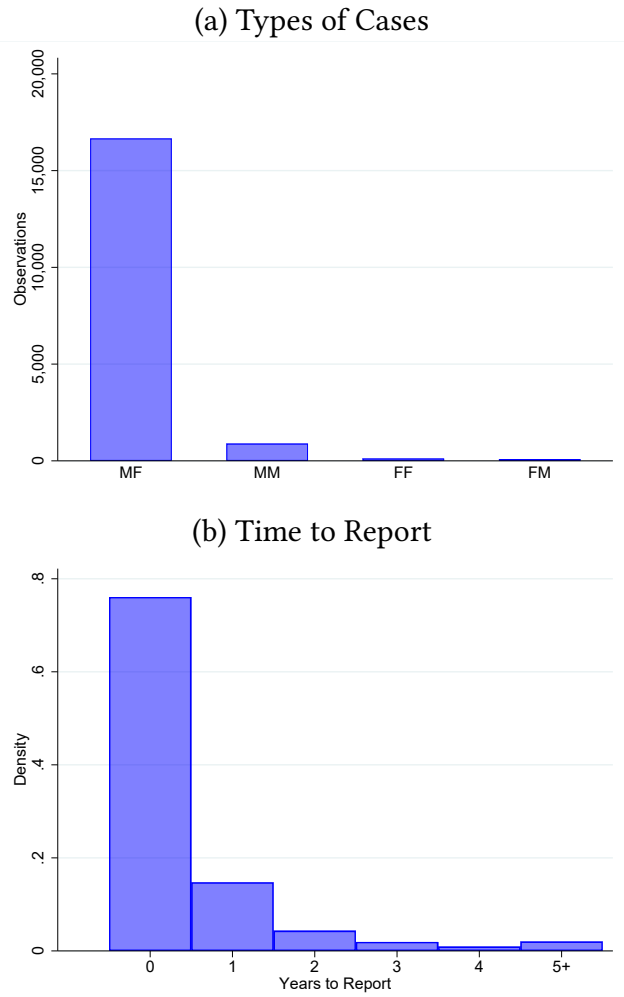
Notes Figure reports the distribution of the types of crimes experienced by women in our police reports sample.

Figure A2: ICD codes in the Hospital Rape Sample



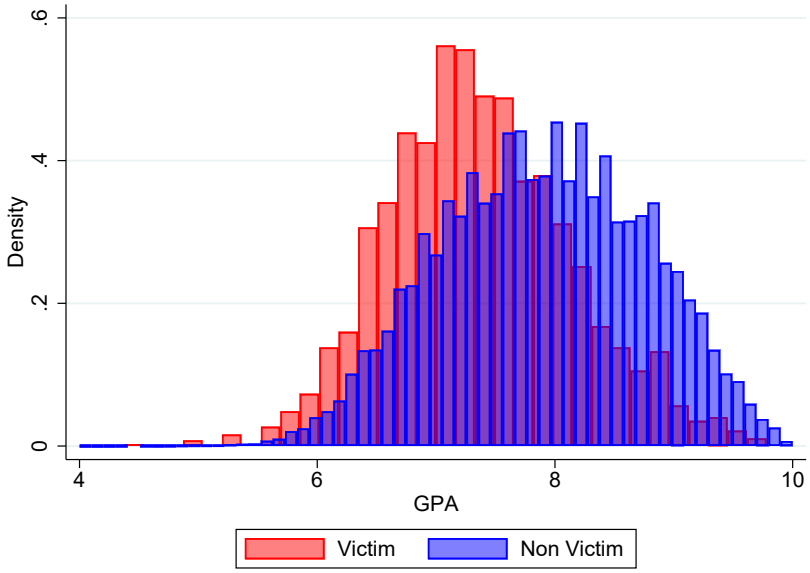
Notes Figure reports the distribution of the ICD codes of rape in our hospital records data from 2006-2019. Observations are at the individual-visit-diagnosis code level. For this figure, an individual can appear multiple times if they are assigned multiple ICD codes. For the later analysis, we collapse to the individual level.

Figure A3: Descriptive Statistics for Police-Reported Rape



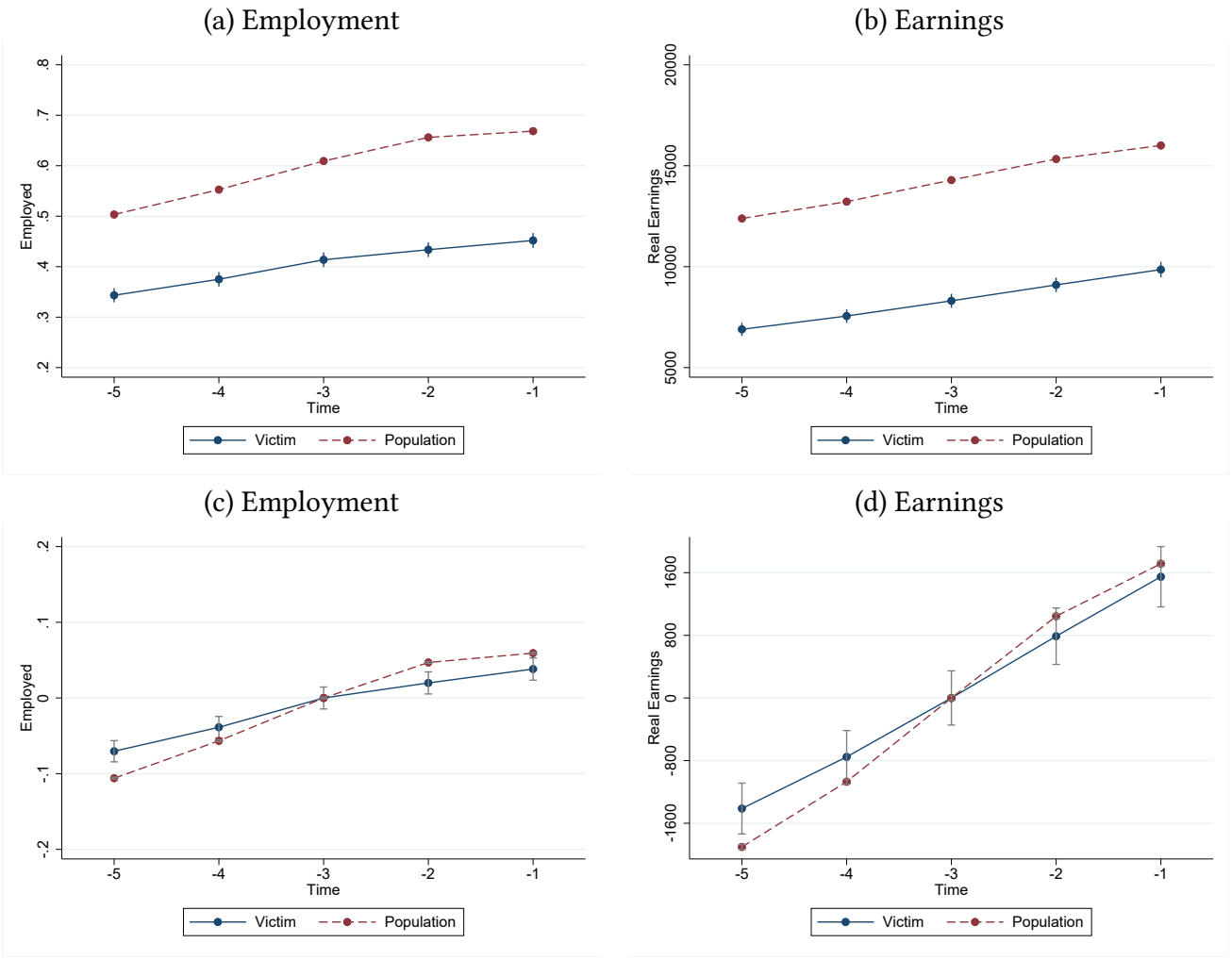
Notes: Figure gives statistics for the population of rape-flagged police reports between 2006-2019, where victims' were aged 14-65 years old at the time of assault. Panel (a) depicts the types of rape cases, with "MF" indicating cases with a male perpetrator and female victim, "MM" indicating cases with a male perpetrator and male victim, and so on. Panel (b) reports the number of years between the time of assault and of reporting to the police.

Figure A4: GPA Distribution for Women Who Experience Rape Before Age 21



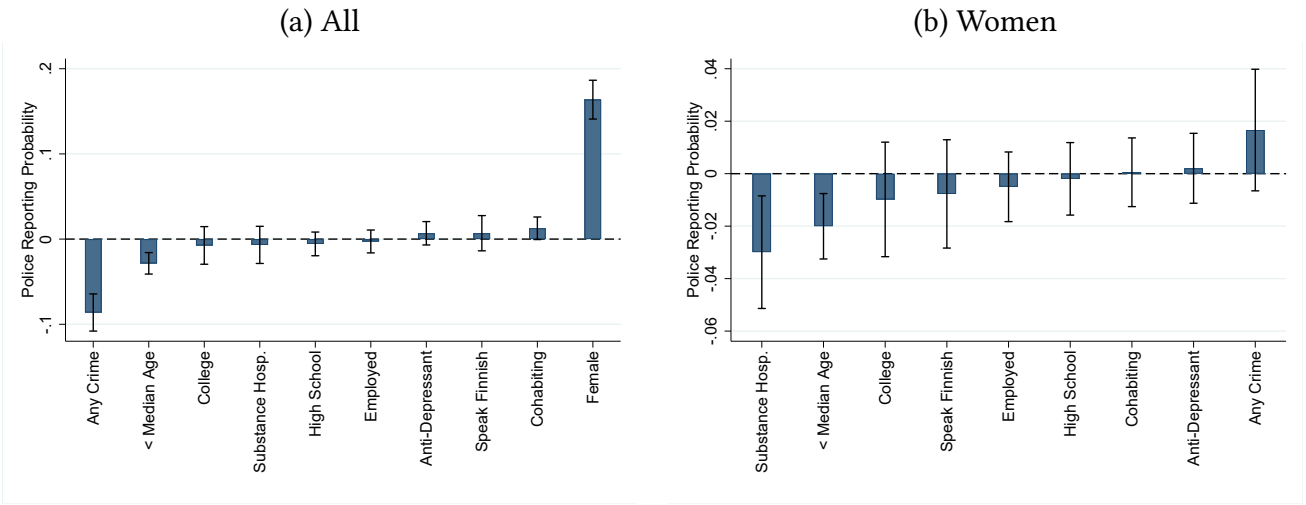
Notes Figure depicts the distribution of GPA measured at age 15 for younger victims in red, and the GPA measured at age 15 for non-victims of the same age (under 21) in blue. GPA is on a scale from 0 to 10, but all grades lower than 4 are consolidated to a 4. We observe GPA at age 15 for all students in Finland.

Figure A5: Employment and Earnings Trajectories Prior to Rape



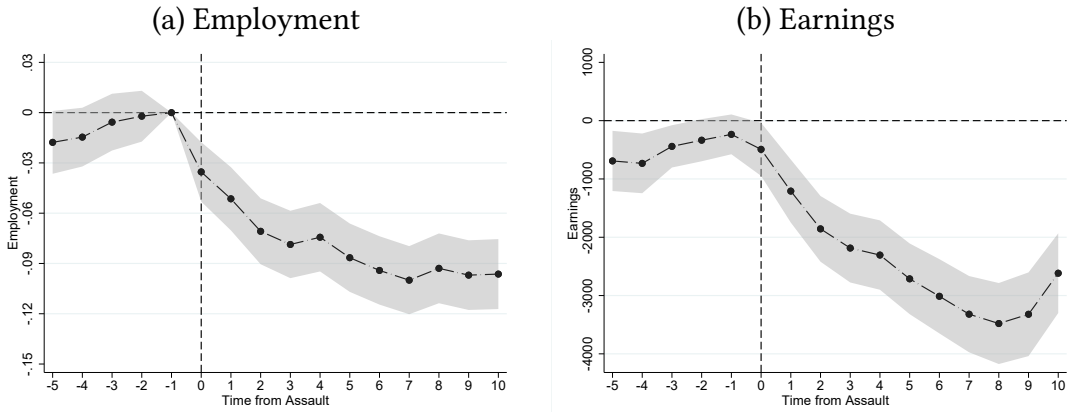
Notes Figure reports the employment and earnings in the five years prior to the violent incident for victims older than 21 years old, relative to all workers in Finland of the same gender, and of a similar age and education level. The x-axis measures years relative to the year in which a rape takes place for victims.

Figure A6: Reporting to Police



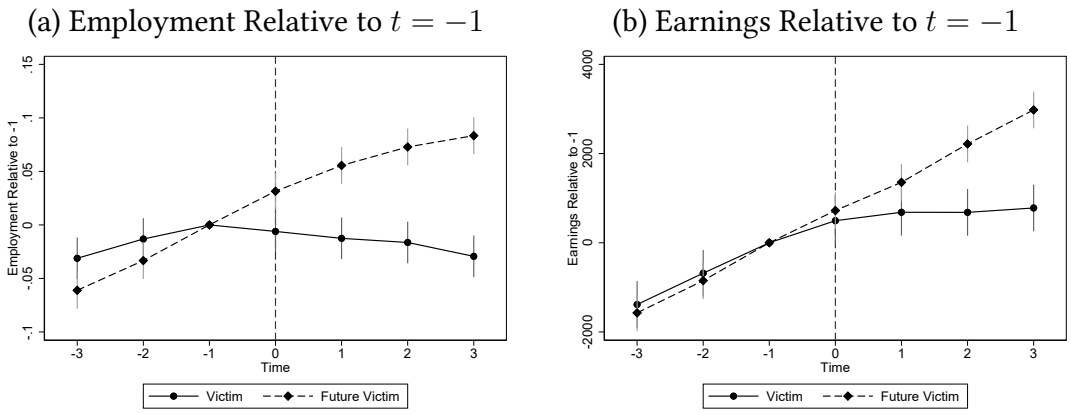
Notes Figure reports the linear probability models that regress the characteristics of victims on whether the case is reported to police. Panel (a) for both men's and women's samples and panel (b) for women only.

Figure A7: Impact of Rape on Older Victims: 10-Year Outcomes



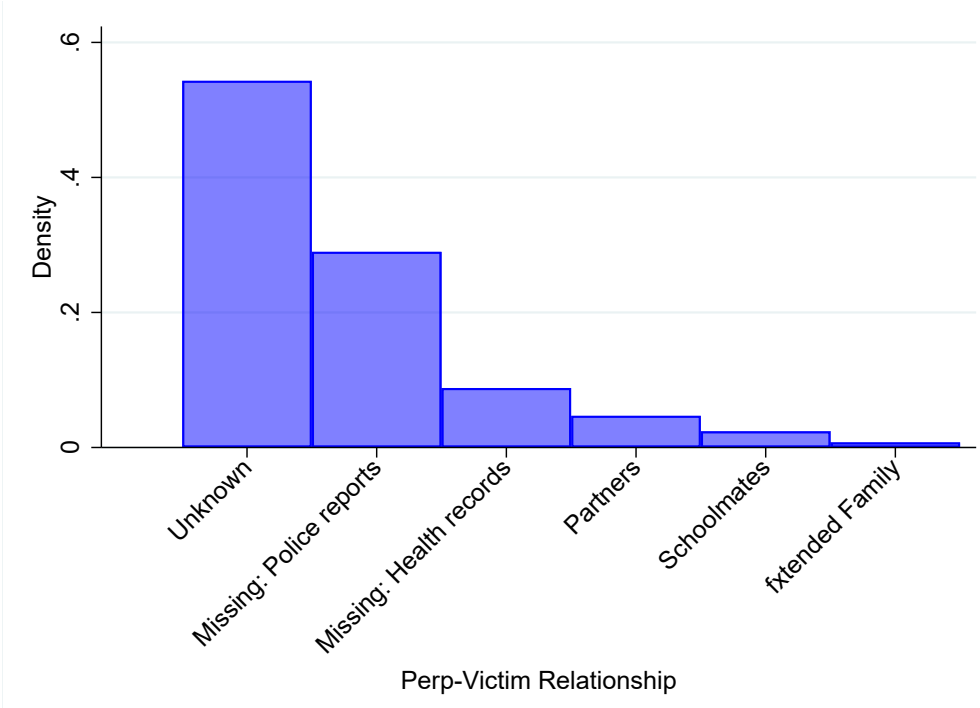
Notes The sample consists of all rapes reported to police and recorded in hospitals in Finland from 2006-2009 where victims are 21 or older at the time of the assault. Figure shows outcome of estimating equation 1 to identify effects 5 years before and 10 years after rape. Year 0 denotes the year at which the rape occurs. Employment indicates whether an individual was employed during the last week of the year (the reference week). Earnings consist of all labor market earnings (the sum of wage, salary, and self-employment earnings) and is deflated to 2013 Euros. We observe employment and earnings for all years unless the individual has died or moved out of Finland. Standard errors are clustered at the match level. The sample size is 134016 for both employment and earnings results.

Figure A8: Impact of Rape on Older Victims: Using Future Victims as Controls



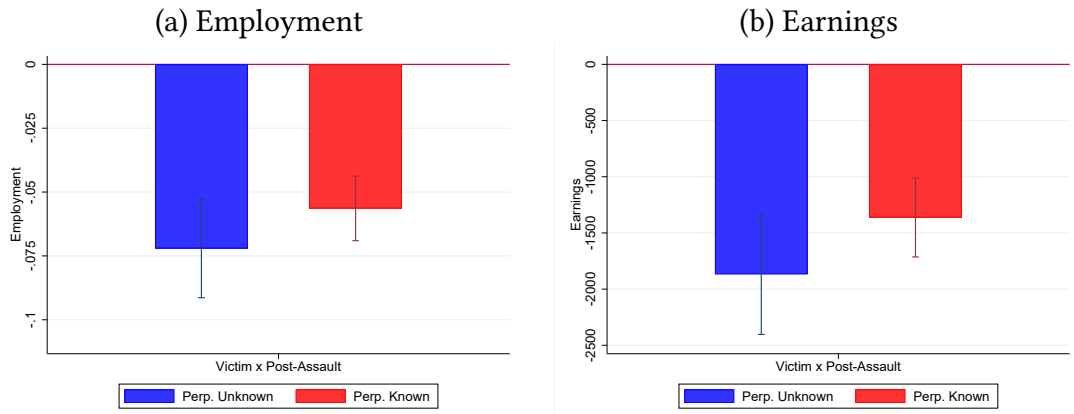
Notes Panel I (a) and (b) report average employment and earnings relative to $t = -1$ for victims (solid line) and future victims (dash line). Year 0 denotes the year at which the rape occurs for the victim and is four years before the future victim experiences a rape. Employment indicates whether an individual was employed during the last week of the year (the reference week). Earnings consist of all labor market earnings (the sum of wage, salary, and self-employment earnings) and is deflated to 2013 Euros. We observe employment for all years unless the individual has died or moved out of Finland. The sample size is 115269.

Figure A9: Relationship between Victims and Perpetrators



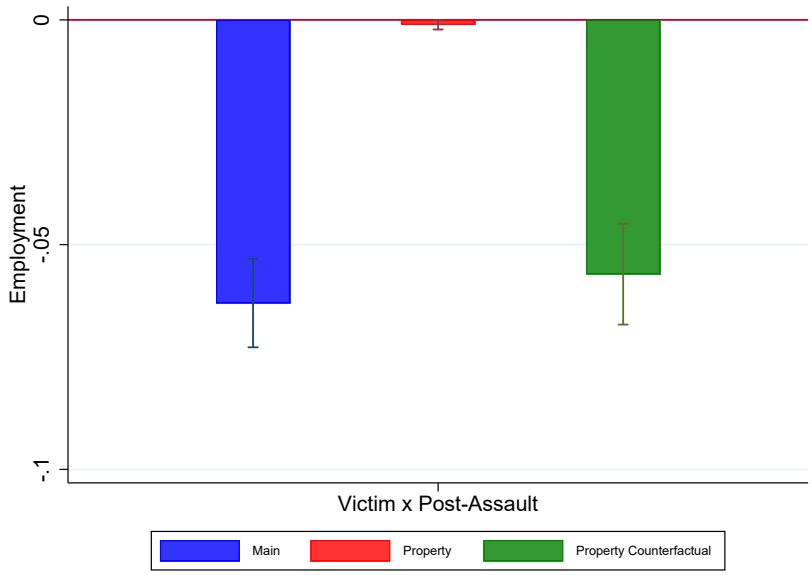
Notes Figure reports the relationship between victims and perpetrators. "Unknown" means victims and perpetrators relationship is not identified from the data, but does not preclude the possibility of a relationship we cannot observe such as dating; "Missing" means the perpetrator ID is missing from the police data or hospital records; "Partners" and "Schoolmates" refer to the cases when perpetrators are the partners or schoolmates (respectively) of the victims. "Extended family" refers to the case when perpetrators are related to the victims as: fathers, grandfathers, uncles, brothers or cousins.

Figure A10: Impact of Rape on Older Victims: By Known vs Unknown Perpetrators



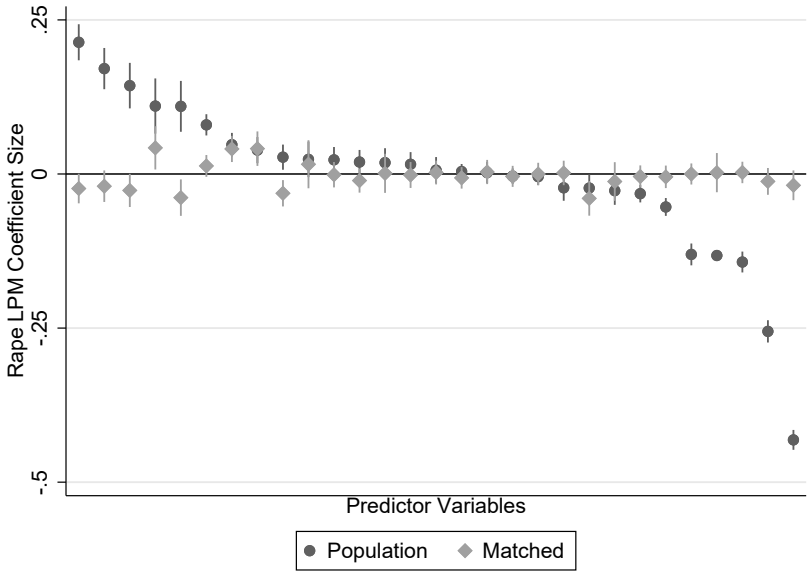
Notes Panel I (a) and (b) report effect on older victims for employment and earnings by whether perpetrator is known or unknown. Employment indicates whether an individual was employed during the last week of the year (the reference week). Earnings consist of all labor market earnings (the sum of wage, salary, and self-employment earnings) and is deflated to 2013 Euros. We observe employment for all years unless the individual has died or moved out of Finland.

Figure A11: Impact of Rape on Employment for Older Victims: Property Crimes Comparison



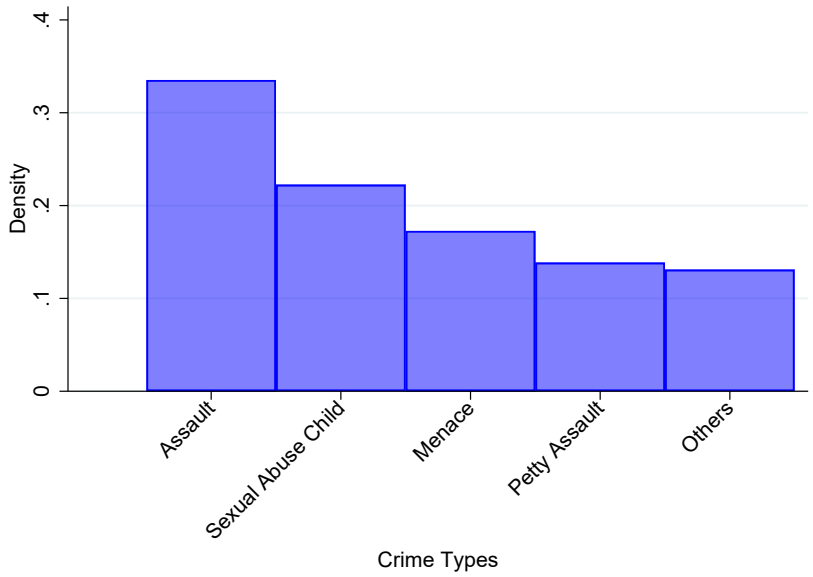
Notes First column in blue reports our main estimates of the impact of rape on employment, i.e. using the matched control to identify effects 5 years before and 5 years after cohabitation, estimating equation 1 but collapsed to a post and pre indicator. The middle column in red uses the exact same approach to estimate the impact of experiencing a property crime on employment. The last column in green estimates the impact of experiencing rape relative to an observationally identical woman who experiences a property crime, i.e., addressing compositional differences between the types of female victims who experience property crimes versus the type of female victims who experience rape. Standard errors are clustered at the match level. We observe employment for all years unless the individual has died or moved out of Finland.

Figure A12: Individual Characteristics and its Prediction of Rape



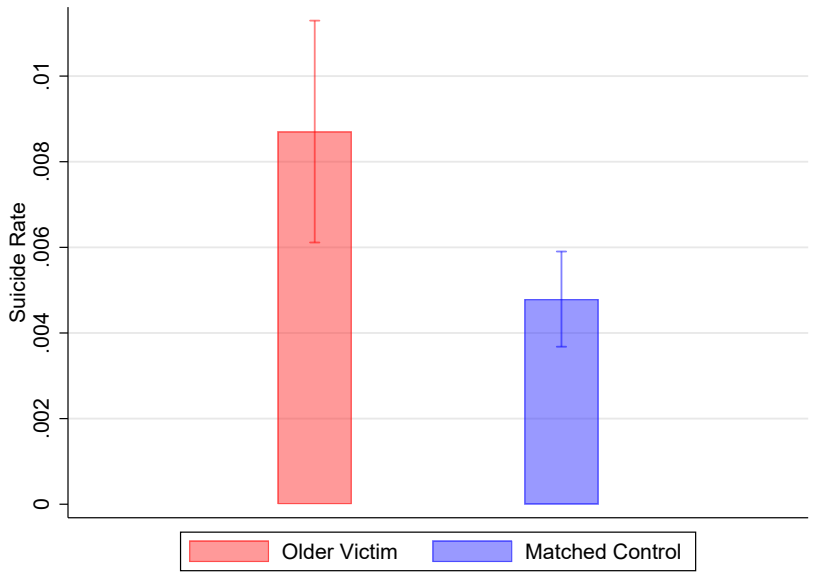
Notes Figure reports the coefficient values from a linear probability model of victimization status on employment, mental health, cohabitation, and substance abuse in the three years before assault and other characteristics reported in Table 1. The diamond markers give results for the sample of victims and their matched control observations. The dark markers give results for victims and the population of non-victimized women in 2014 (re-weighted to give the same relative hazard rate of victimization to the matched control sample).

Figure A13: Types of Crimes on Other Female Victims of Rapists



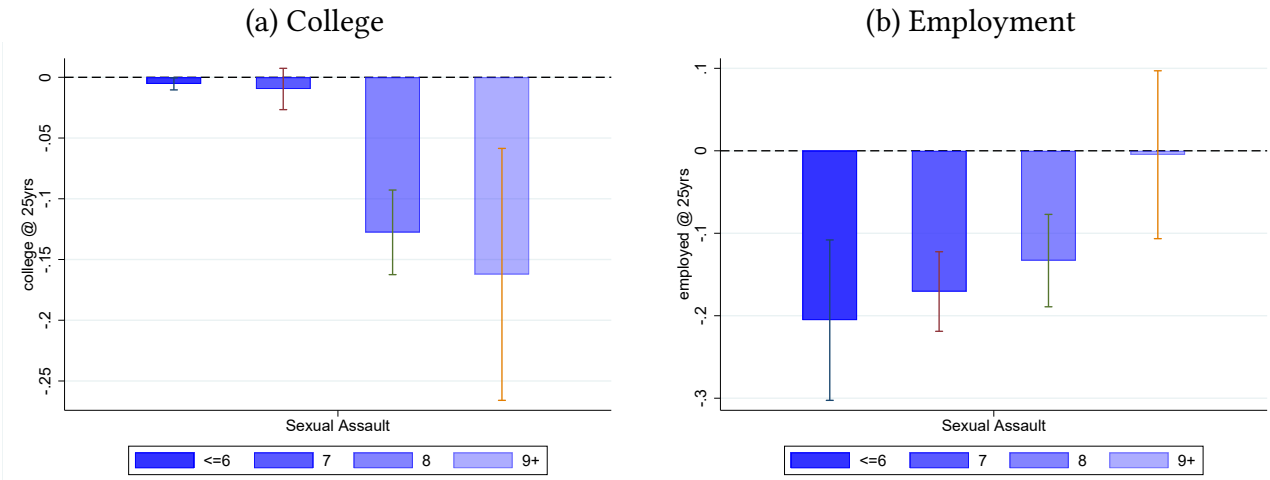
Notes Figure reports the distribution of the types of crimes experienced by other female victims of rapists. Other female victims of rapists are defined as those who are assaulted by a man who commits "Rape," "Attempted Rape," "Aggravated Rape," "Aggravated Attempted Rape," "Coercion into a Sexual Act," "Attempted "Coercion into a Sexual Act," "Sexual Abuse," "Attempted Sexual Abuse," or "Sexual Harassment" against another woman, but the crime they experience as a victim is not among this group of crimes we use to define rape for our main analysis.

Figure A14: Impact of Rape on Suicide for Older Victims



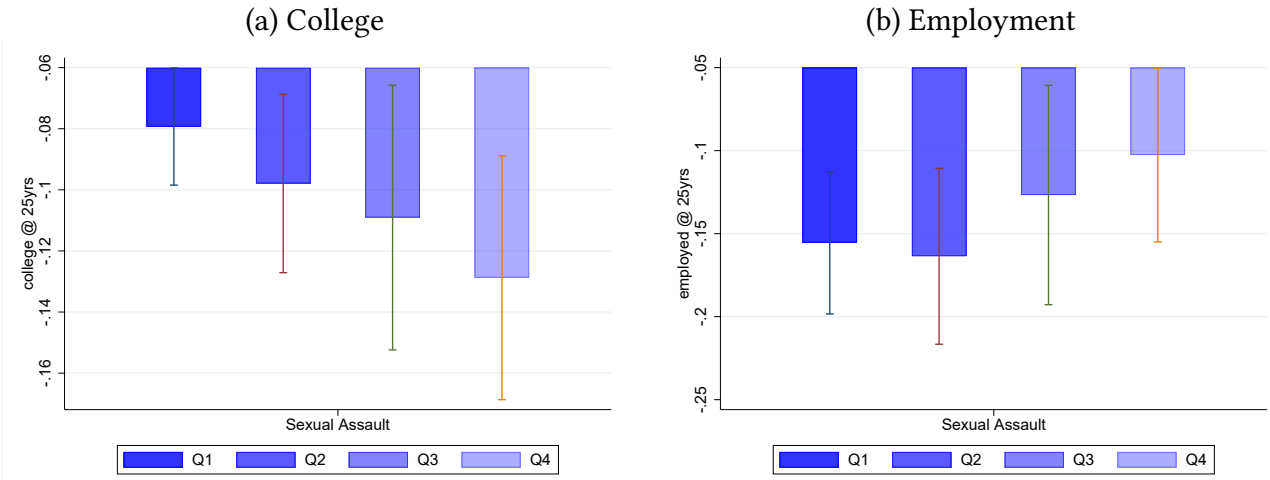
Notes Figure reports the mean suicide rate of victims and their matched controls. Estimates obtained by linking victims from the police data and their matched controls to the universe of death records in Finland. Mechanically there is no difference in suicide in the years before the assault, so we simply report the suicide rate for victims and their matched controls in the years we observe following an assault.

Figure A15: Heterogeneity by GPA



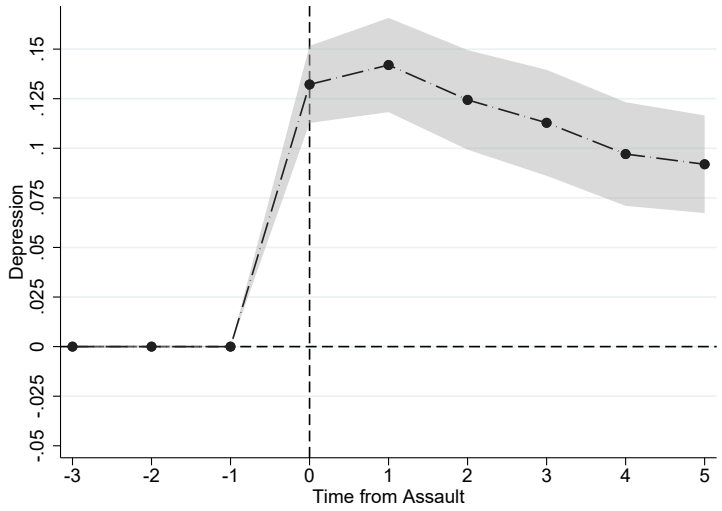
Notes Figure reports the heterogeneous impact of a rape on outcomes of victims younger than 21 measured at their 25 years old, by pre-rape GPA distribution. Outcomes including whether the individual completes a college and employment status.

Figure A16: Heterogeneity by Parent Income



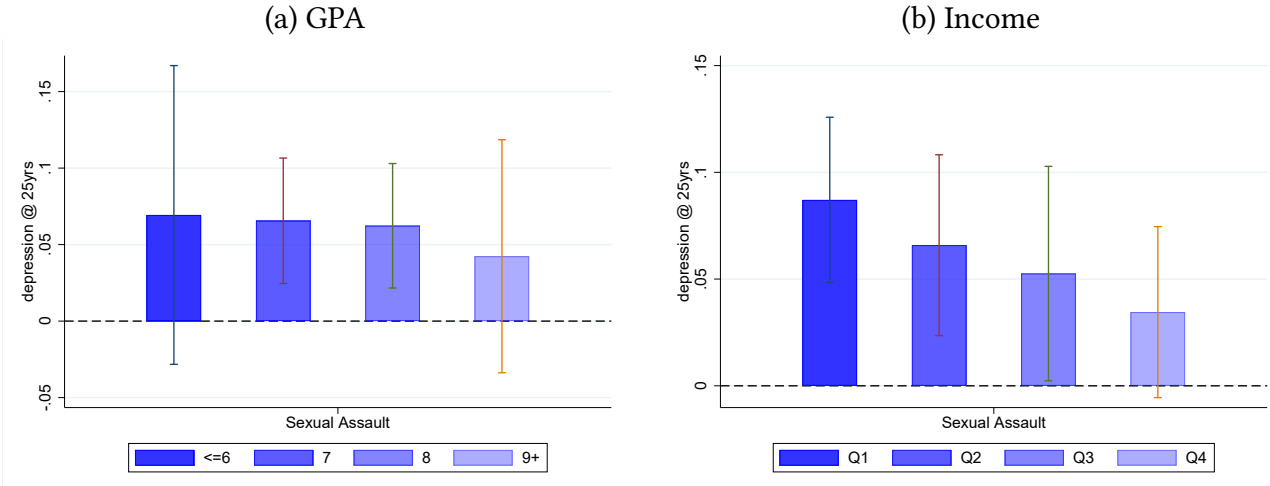
Notes Figure reports the heterogeneous impact of a rape on outcomes of victims younger than 21 measured at their 25 years old, by parental income distribution. Outcomes including whether the individual completes a college and employment status.

Figure A17: Impact of Rape on Younger Victim Mental Health



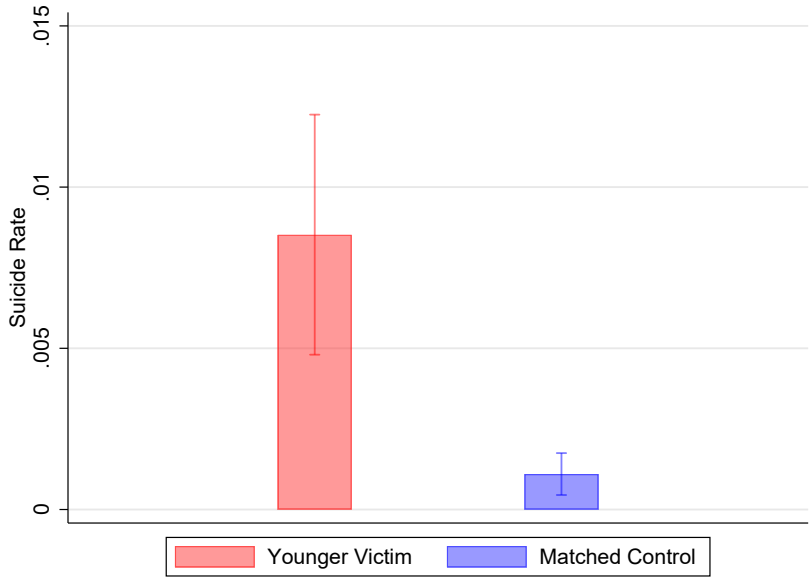
Notes Figure reports estimated impacts of a rape that is reported to the police and recorded in hospitals in Finland using the matched control to identify effects 5 years before and 5 years after rape (see text for details). Year 0 denotes the year at which the rape occurs. The sample consists of all rapes reported to police in Finland from 2006-2014 where the victim is under 21 at the time of rape. The outcome variable is an indicator equal to one if the individual is prescribed an anti-depressant at any point in the year. Standard errors are clustered at the match level.

Figure A18: Heterogeneity in Impact of Rape on Younger Victim Mental Health by GPA and Parental Income



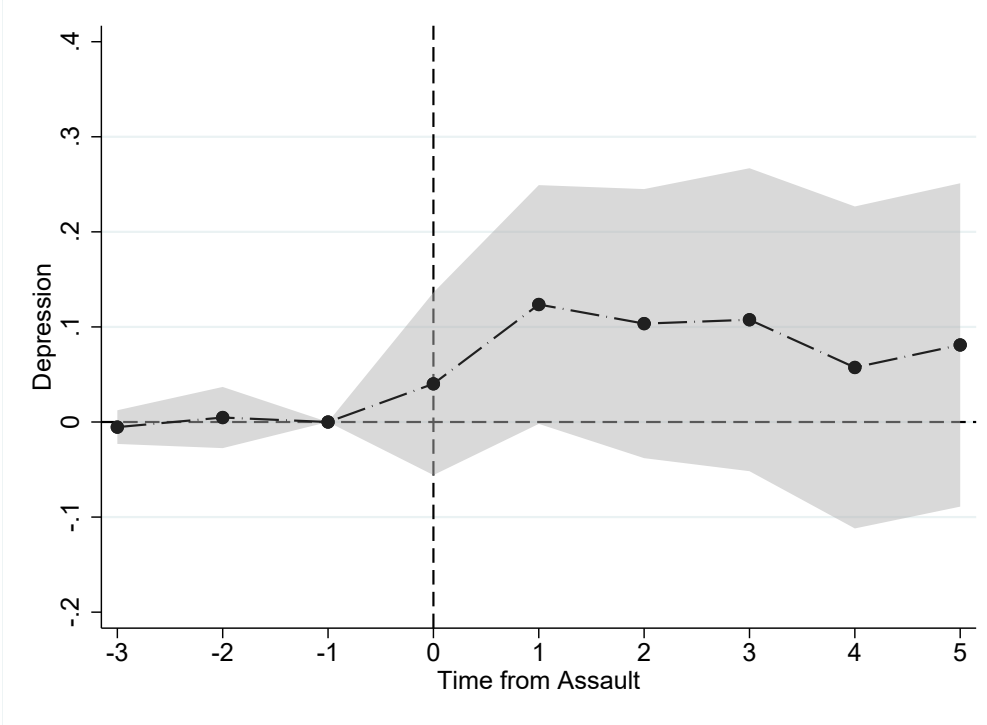
Notes Figure reports the heterogeneous impact of a rape on outcomes of victims younger than 21 measured at their 25 years old, by pre-rape GPA distribution and by parental income distribution. Outcomes including whether the individual takes anti-depressants at age 25.

Figure A19: Impact of Rape on Suicide for Younger Victims



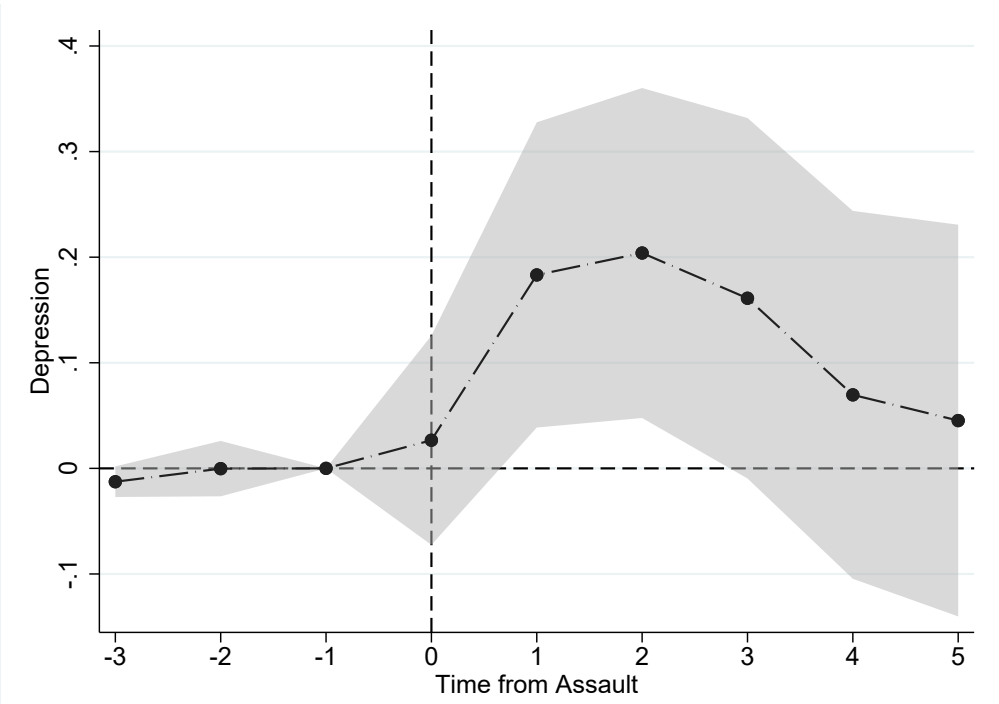
Notes Figure reports the mean suicide rate of victims and their matched control. Estimates obtained by linking victims from the police data and their matched controls to the universe of death records in Finland. Mechanically there is no difference in suicide in the years before the assault, so we simply report the suicide rate for victims and their matched controls in the years we observe following an assault.

Figure A20: Impact of Rape on the Mental Health of Female School Peers, 100 Girls Per Cohort



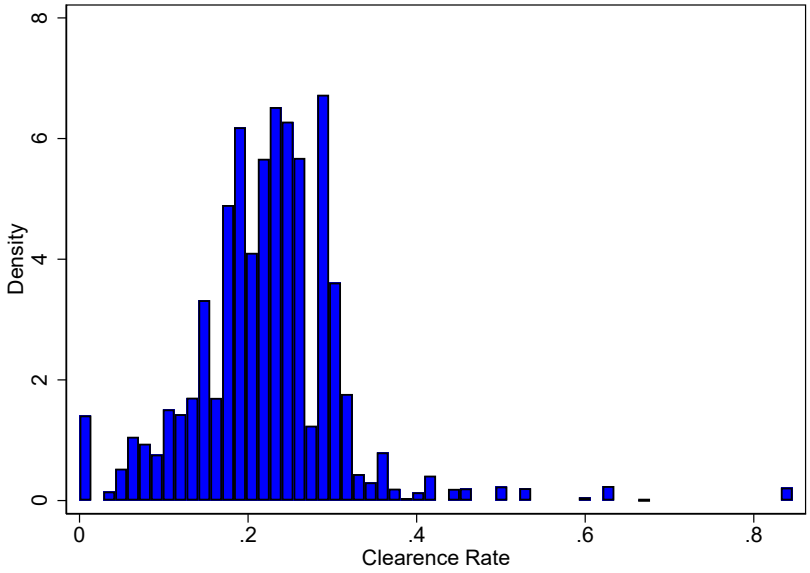
Notes Figure reports the impact of a rape on the number of anti-depressant prescriptions among the victim's female school cohort classmates. The estimates use peers from observationally similar school cohorts but where no female peer has an identified rape case, estimating equation 5. In contrast to estimates in the main text, we include all schools with 100 girls or fewer. Standard errors are clustered at the match level.

Figure A21: Impact of Rape on the Mental Health of Female School Peers, Perpetrator Older than 18



Notes Figure reports the impact of a rape on the number of anti-depressant prescriptions among the victim's female school cohort classmates, but excluding cases where the perpetrator is known and under 18. The estimates use peers from observationally similar school cohorts but where no female peer has an identified rape case, estimating equation 5. Standard errors are clustered at the match level.

Figure A22: Distribution of Rape Clearance Rates in Sample



Notes Figure reports the histogram of the rape clearance rate across municipalities and time periods.

Table A1: Summary Statistics for Young Victims' Parents

	Mum Victim	Mum NonVictim	Dad Victim	Dad NonVictim
Age	44.94	46.37	47.68	48.68
College	0.15	0.21	0.12	0.20
High School	0.64	0.66	0.56	0.58
Dropouts	0.21	0.13	0.32	0.22
Anti-Depressant	0.18	0.11	0.09	0.07
Employed at t-1	0.70	0.83	0.66	0.79
Earnings at t-1	22061	28353	28527	38194
+ Earnings at t-1	27999	32099	38630	44915
Prior Violent Crimes	0.04	0.01	0.22	0.05
Cohabitation	0.63	0.75	0.63	0.76
Substance Abuse	0.01	0.00	0.02	0.01
Observations	5682	1953623	5682	1953623

Notes: Table reports the summary statistics for younger victims' mothers and fathers respectively, and the mothers and fathers of non-victimized girls. All variables in all columns are measured at t-1. Earnings consist of all labor market earnings (the sum of wage, salary, and self-employment earnings) deflated to 2013 Euros. "+ Earnings" is earnings excluding zeros. "Substance Abuse" is the proportion hospitalized for alcohol or drug abuse. "Prior Violent Crime" is a dummy equal to 1 if a parent has a violent crime record.

Table A2: Balance Table

	Victims	Match Control	Rapist Other Female Victims	Match Control
	(1)	(2)	(3)	(4)
Age	32.71	32.90	39.23	39.38
Share college	0.05	0.05	0.02	0.02
Share high school	0.57	0.58	0.62	0.63
Share dropouts	0.39	0.38	0.36	0.35
Fertility	0.51	0.51	0.80	0.79
No. of Children	1.16	1.13	2.15	2.02
Employment	0.45	0.45	0.52	0.52
Earnings	9856	9926	13515	13575
+Earnings	16056	16392	20836	21074
Prior Crimes	0.24	0.07	0.31	0.07
Any Crime	0.07	0.02	0.10	0.02
Share depression	0.29	0.29	0.16	0.16
Cohabitation	0.33	0.33	0.45	0.45
Share substance abuse	0.07	0.09	0.05	0.07
Move across municipality	0.30	0.26	0.21	0.19
Speak Finnish	0.91	0.88	0.90	0.87
Observations	4421	13263	1924	5772

Notes: Table reports the summary statistics female rape victims in column (1), their matched control in column (2); In column (3) and (4) we report summary statistics for other female victims of rapists and their matched control, but for whom the crime code was not categorized as our rape definition in the paper. All variables in all columns are measured the year before the crime occurred.

Table A3: Effects of Adverse Events on Employment and Earnings

Event	Female		Male	
	Employment	Earnings	Employment	Earnings
Rape	-15%	-17%		
Child Penalty	-8%	-13%	0	-1%
Graduate in Recessions	-	-	0	-4%
Job Displacement	-14%	-22%	-11%	-16%
Burnout	-11%	-12%	-9%	-13%
Parental Death	-1%	-3%	-0.7%	-2%
Workplace Injury	-11%	-14%	-11%	-14%

Notes: The child penalty estimates from Adams *et al.* (2024b). The graduation in recession estimates for men are from Oreopoulos *et al.* (2012). The job displacement estimates are from Schmieder *et al.* (2023). The labor market effects of burnout estimates are from Nekoei *et al.* (2024). The parental death estimates are from Jensen and Zhang (2026). The workplace injury estimates are from Dworsky and Powell (2022), which do not distinguish male vs. female effects.

Table A4: Heterogeneous Impacts of Rape by Victim Characteristics

	(1)	(2)	(3)	(4)	(5)
Panel A:			Dependent Variable: Employment		
Victim	-0.0058	-0.0058	-0.0058	-0.0058	-0.0058
	(0.0033)	(0.0033)	(0.0033)	(0.0033)	(0.0033)
Victim x Post Assault	-0.0736***	-0.0637***	-0.0624***	-0.0423***	-0.0620***
	(0.0100)	(0.0053)	(0.0062)	(0.0086)	(0.0065)
Victim x Post Assault x 20s	0.0124				
	(0.0124)				
Victim x Post Assault x 40+	0.0211				
	(0.0135)				
Victim x Post Assault x College		0.0407*			
		(0.0196)			
Victim x Post Assault x Depress			0.0023		
			(0.0107)		
Victim x Post Assault x Single				-0.0292**	
				(0.0104)	
Victim x Post Assault x Employed					0.0005
					(0.0101)
Observations	194524	194524	194524	194524	194524
Panel B:			Dependent Variable: Earnings		
Victim	-340**	-340**	-340**	-340**	-340**
	(111)	(111)	(111)	(111)	(111)
Victim x Post Assault	-1597***	-1487***	-1600***	-977***	-1247***
	(347)	(145)	(183)	(277)	(169)
Victim x Post Assault x 20s	225				
	(415)				
Victim x Post Assault x 40+	119				
	(487)				
Victim x Post Assault x College		752			
		(1240)			
Victim x Post Assault x Depress			516		
			(327)		
Victim x Post Assault x Single				-713*	
				(351)	
Victim x Post Assault x Employed					-454
					(336)
Observations	192508	192508	192508	192508	192508

Notes: Table reports the heterogeneous impact of rape on victims' employment and earnings by age, education, anti-depressant, relationship status, and employment status before the rape. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A5: Heterogeneous Impact of Rape by Police Response

	(1) Employment	(2) Employment	(3) Earnings	(4) Earnings
Victim	-0.00161	-0.00161	-180.8	-180.8
	(0.00370)	(0.00370)	(111.8)	(111.8)
Victim x Post Assault	-0.0810***	-0.0846***	-1954.0***	-2064.9***
	(0.00746)	(0.00718)	(210.3)	(200.5)
Victim x Post Assault x Clearance	0.0329**		707.0*	
	(0.0104)		(323.7)	
Victim x Post Assault x Female Police		0.0430***		1008.0**
		(0.0105)		(333.7)
Observations	167541	167541	165560	165560

Notes: Table reports the heterogeneous effects by police responses. Columns 1 and 3 include an interaction between treatment and whether the lagged clearance rate in the municipality where the crime occurred is above median. The lagged municipality clearance rate is equal to the share of rape cases in a given municipality in the previous year that appeared in court. Columns 2 and 4 report the impact of having an above median number of female police in the municipality where the rape occurs, captured by "Victim $\times$ Post $\times$ Fem Police." Columns 1 and 2 report the impacts on employment, and columns 3 and 4 report the impacts on earnings. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A6: Older Victims Impact of Rape Robustness to Type of Report

	(1) Main	(2) Restricted Rape	(3) Hospital Only	(4) Police Only	(5) Reweight by Survey
Panel A:			Dependent Variable: Employment		
Victim x Post Assault	-0.0618***	-0.0831***	-0.0659***	-0.0614***	-0.0712***
	(0.0051)	(0.0077)	(0.0172)	(0.0054)	(0.0131)
Victim	-0.0058	-0.0057	0.0062	-0.0069*	0.0085
	(0.0033)	(0.0048)	(0.0116)	(0.0035)	(0.0057)
Observations	194524	89936	17424	177100	194084
Panel B:			Dependent Variable: Earnings		
Victim x Post Assault	-1453***	-1992***	-739	-1523***	-1659**
	(145)	(212)	(547)	(150)	(520)
Victim	-340**	-426	-210	-353	333
	(111)	(147)	(340)	(117)	(326)
Observations	192508	88696	17244	175264	192092

Notes: Table reports estimated impacts of a rape on employment using the matched control. Estimates restricted to victims who experience rape, attempted rape, aggravated rape, and attempted aggravated rape. Employment indicates whether an individual was employed during the last week of the year (the reference week). Standard errors are clustered at the match level. We observe employment for all years unless the individual has died or moved out of Finland. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A7: Impact of External Life Shocks on Rape

	Dependant Variable: Rape							
	Match	All	Match	All	Match	All	Match	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ Employment	-0.0038	0.0317**						
	(0.0114)	(0.0097)						
Δ Cohabitation			0.0046	0.1545***				
			(0.0122)	(0.0125)				
Δ Depression					0.0048	0.2394***		
					(0.0111)	(0.0121)		
Δ Substance							-0.0169	0.6254***
							(0.0144)	(0.0099)
Observations		19760	1732661	19760	1732661	19760	1732661	19760

Notes: Table reports reports the impact of external life shocks on rape from a linear probability model. The outcome variable is the incidence of rape, and the external shocks are dummy variables indicating whether the individual experiences a change in employment, cohabitation, anti-depressant prescription use, and hospitalization due to substance abuse between -2 and -1 years before rape. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A8: Summary Statistics

	All	Police Reported	Hospital Only	Non-Victim
	(1)	(2)	(3)	(4)
Age	37.70	38.20	36.70	43.35
College	0.10	0.11	0.09	0.21
High School	0.51	0.52	0.48	0.59
Dropouts	0.39	0.37	0.43	0.20
Anti-Depressant	0.14	0.14	0.13	0.07
Employed at t-1	0.49	0.51	0.43	0.69
Earnings at t-1	17016	18789	13501	28943
+ Earnings at t-1	27414	29815	22428	35838
Prior Violent Crimes	0.90	0.80	1.10	0.06
Any Violent Crime	0.10	0.08	0.12	0.01
Hospitalization Due to Substance	0.08	0.09	0.06	0.01
Cohabitation	0.37	0.41	0.29	0.61
Speak Finnish	0.89	0.91	0.82	0.89
Observations	907	603	304	1591435

Notes: Table reports the summary statistics for overall male victims (column 1), male victims identified from the police records (column 2), and male victims identified from hospital ICD codes only (column 3) when the victims are 21-65 years old at the time of crime. We also report statistics for men aged 21-65 years old in 2014 who have not committed or experienced violent crimes in columns 4. All variables in all columns are measured at t-1. Earnings consist of all labor market earnings (the sum of wage, salary, and self-employment earnings) deflated to 2013 Euros. "+ Earnings" is earnings excluding zeros. "Substance Abuse" is the proportion hospitalized for alcohol or drug abuse.

Table A9: Impact of Rape on Male Victims

	(1) Employment	(2) Earnings	(3) Mental Health	(4) Substance Abuse	(5) Cohabitation
Victim x Post Assault	-0.0160	-1009	0.0194*	0.0214***	-0.0607***
	(0.0107)	(525)	(0.0077)	(0.0045)	(0.0123)
Victim	-0.0098	-541.6103	0.0079	-0.0024	0.0437**
	(0.0059)	(657.9761)	(0.0069)	(0.0031)	(0.0152)
Observations	32505	32023	32505	32505	32505
Control Mean	0.69	27382	0.09	0.03	0.59

Notes: Table reports estimated impacts of a rape for male victims on employment, earnings, mental health (dummy equal to 1 if the individual is on an anti-depressant), substance abuse (dummy equal to 1 if the individual appears in the hospital data for substance abuse), and cohabitation status. Estimates use the matched control design described in the main text. Victims are those who experience rape, attempted rape, aggravated rape, or attempted aggravated rape. Employment indicates whether an individual was employed during the last week of the year (the reference week). Standard errors are clustered at the match level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A10: Summary Statistics for Female Students in Victimized and Non-Victimized School Cohorts

	All Cohorts			Cohorts with $\leq$ 50 Girls		Cohorts with $\leq$ 100 Girls	
	All	Victim Peers	Others	Victim Peers	Others	Victim Peers	Others
GPA	7.87	7.85	7.88	7.83	7.88	7.84	7.87
	(0.860)	(0.866)	(0.857)	(0.865)	(0.855)	(0.865)	(0.857)
Depr.	0.02	0.02	0.01	0.02	0.02	0.02	0.01
	(0.124)	(0.128)	(0.121)	(0.137)	(0.124)	(0.128)	(0.121)
College Mom	0.19	0.19	0.19	0.16	0.17	0.19	0.18
	(0.390)	(0.393)	(0.389)	(0.366)	(0.373)	(0.389)	(0.388)
College Dad	0.19	0.19	0.19	0.16	0.17	0.19	0.18
	(0.390)	(0.393)	(0.389)	(0.366)	(0.373)	(0.389)	(0.388)
HS Mom	0.69	0.68	0.69	0.70	0.70	0.69	0.69
	(0.463)	(0.465)	(0.462)	(0.460)	(0.460)	(0.464)	(0.462)
HS Dad	0.69	0.68	0.69	0.70	0.70	0.69	0.69
	(0.463)	(0.465)	(0.462)	(0.460)	(0.460)	(0.464)	(0.462)
Dropout Mom	0.12	0.13	0.12	0.14	0.14	0.13	0.12
	(0.331)	(0.332)	(0.330)	(0.351)	(0.344)	(0.334)	(0.331)
Dropout Dad	0.12	0.13	0.12	0.14	0.14	0.13	0.12
	(0.331)	(0.332)	(0.330)	(0.351)	(0.344)	(0.334)	(0.331)
Depr. Mom	0.11	0.11	0.10	0.11	0.10	0.11	0.10
	(0.308)	(0.312)	(0.306)	(0.312)	(0.303)	(0.312)	(0.306)
Depr. Dad	0.06	0.06	0.06	0.06	0.06	0.06	0.06
	(0.246)	(0.243)	(0.246)	(0.244)	(0.247)	(0.244)	(0.246)
Employed Mom	0.85	0.85	0.85	0.83	0.83	0.85	0.85
	(0.359)	(0.355)	(0.360)	(0.374)	(0.374)	(0.357)	(0.362)
Employed Dad	0.85	0.85	0.85	0.84	0.84	0.85	0.85
	(0.356)	(0.354)	(0.357)	(0.370)	(0.369)	(0.355)	(0.358)
Earnings Mom	27426	27881	27277	25857	26010	27689	27179
	(19923)	(20208)	(19822)	(19965)	(20176)	(20257)	(19854)
Earnings Dad	39498	40177	39284	37080	36992	39813	39165
	(38212)	(37437)	(38514)	(36681)	(37408)	(37019)	(38548)
Crimes Mom	0.01	0.01	0.01	0.01	0.01	0.01	0.01
	(0.162)	(0.162)	(0.161)	(0.191)	(0.173)	(0.160)	(0.164)
Crimes Dad	0.04	0.04	0.04	0.06	0.04	0.04	0.04
	(0.611)	(0.545)	(0.626)	(0.680)	(0.696)	(0.558)	(0.628)
Subst. Mom	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.0663)	(0.0634)	(0.0675)	(0.0665)	(0.0684)	(0.0642)	(0.0677)
Subst. Dad	0.01	0.01	0.01	0.01	0.01	0.01	0.01
	(0.103)	(0.106)	(0.102)	(0.113)	(0.106)	(0.107)	(0.102)
Cohort Size	55.11	62.33	52.52	36.18	31.83	58.76	50.43
	(24.52)	(23.58)	(24.33)	(10.23)	(12.36)	(20.00)	(21.91)
Observations	150204	39664	110209	12153	51236	36934	106450

Notes: Table reports the summary statistics for school cohort peers of younger female rape victims and other girls in schools in Finland. "Depr." is a dummy equal to 1 if the individual is prescribed anti-depressant medication.

Table A11: Summary Statistics for Cohorts with Victimized and Non-Victimized Peers

	All Cohorts			Cohorts with $\leq$ 50 Girls		Cohorts with $\leq$ 100 Girls	
	All	Victim Peers	Others	Victim Peers	Others	Victim Peers	Others
GPA	7.770	7.740	7.780	7.720	7.780	7.740	7.780
	(0.443)	(0.368)	(0.459)	(0.394)	(0.495)	(0.368)	(0.461)
Depressant	0.020	0.020	0.020	0.030	0.030	0.020	0.020
	(0.081)	(0.064)	(0.085)	(0.087)	(0.097)	(0.065)	(0.085)
College Mom	0.160	0.170	0.160	0.150	0.150	0.170	0.160
	(0.156)	(0.117)	(0.164)	(0.127)	(0.178)	(0.117)	(0.164)
College Dad	0.160	0.170	0.160	0.150	0.150	0.170	0.160
	(0.156)	(0.117)	(0.164)	(0.127)	(0.178)	(0.117)	(0.164)
HS Mom	0.680	0.680	0.680	0.680	0.670	0.680	0.680
	(0.208)	(0.138)	(0.221)	(0.169)	(0.250)	(0.140)	(0.222)
HS Dad	0.680	0.680	0.680	0.680	0.670	0.680	0.680
	(0.208)	(0.138)	(0.221)	(0.169)	(0.250)	(0.140)	(0.222)
Dropout Mom	0.160	0.150	0.160	0.170	0.180	0.150	0.160
	(0.188)	(0.129)	(0.199)	(0.165)	(0.226)	(0.130)	(0.200)
Dropout Dad	0.160	0.150	0.160	0.170	0.180	0.150	0.160
	(0.188)	(0.129)	(0.199)	(0.165)	(0.226)	(0.130)	(0.200)
Depr. Mom	0.110	0.110	0.110	0.120	0.110	0.110	0.110
	(0.130)	(0.086)	(0.138)	(0.111)	(0.158)	(0.087)	(0.139)
Depr. Dad	0.070	0.070	0.070	0.070	0.070	0.070	0.070
	(0.113)	(0.056)	(0.122)	(0.071)	(0.140)	(0.056)	(0.123)
Employed Mom	0.810	0.830	0.800	0.810	0.780	0.830	0.800
	(0.194)	(0.124)	(0.206)	(0.157)	(0.233)	(0.125)	(0.207)
Employed Dad	0.820	0.840	0.810	0.810	0.800	0.840	0.810
	(0.187)	(0.116)	(0.200)	(0.147)	(0.227)	(0.117)	(0.201)
Earnings Mom	25057	26501	24720	24536	23505	26383	24666
	(8762)	(6940)	(9104)	(7469)	(9726)	(6980)	(9121)
Earnings Dad	35675	37957	35141	34946	33074	37741	35066
	(14474)	(11413)	(15052)	(12111)	(15741)	(11462)	(15092)
Crimes Mom	0.010	0.010	0.010	0.010	0.010	0.010	0.010
	(0.135)	(0.028)	(0.150)	(0.034)	(0.173)	(0.028)	(0.150)
Crimes Dad	0.050	0.050	0.050	0.070	0.060	0.050	0.050
	(0.304)	(0.241)	(0.317)	(0.327)	(0.364)	(0.245)	(0.318)
Subst. Mom	0.010	0.000	0.010	0.000	0.010	0.000	0.010
	(0.033)	(0.012)	(0.036)	(0.015)	(0.041)	(0.012)	(0.036)
Subst. Dad	0.010	0.010	0.010	0.010	0.010	0.010	0.010
	(0.052)	(0.024)	(0.057)	(0.030)	(0.065)	(0.024)	(0.057)
Cohort Size	35.040	48.910	31.800	29.570	19.800	47.060	31.020
	(26.450)	(25.240)	(25.670)	(14.060)	(15.440)	(23.170)	(24.540)
Observations	4277	811	3466	424	2588	787	3432

Notes: Table reports the summary statistics for school cohort peers of younger female rape victims and other girls in schools in Finland, but collapsed to report the average for the cohort and the number of cohorts. "Depr." is a dummy equal to 1 if the individual is prescribed anti-depressant medication.

Table A12: Pooled Estimates for Parents

	Employment	Earnings	Depression
	(1)	(2)	(3)
Rape	-0.0036	-207.6874	0.0130***
	(0.0040)	(238)	(0.0028)
Rape*Mother	-0.0008	146	0.0076
	(0.0054)	(287)	(0.0043)
Observations	544280	538136	544280
Control Mean	0.664	24396	0.130
p-value for Rape*Mother	0.8755	0.6097	0.0791

Notes: Table reports estimated impacts of a rape on victim's parent's outcomes using data that pools both fathers and mothers and their matched controls. Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

B Data Appendix

Police Data We use administrative data from the Finnish police to identify all cases of "Rape," "Attempted Rape," "Aggravated Rape," and "Attempted Aggravated Rape." Second, we also include a smaller set of additional crimes that are very close to rape, specifically "Coercion into a Sexual Act," "Attempted Coercion into a Sexual Act," "Sexual Abuse," "Attempted Sexual Abuse," "Coercion into a sexual intercourse," and "Attempted Coercion into a sexual intercourse."

Sexual abuse is defined in Finland as cases "where the perpetrator, by abusing his or her position, entices a person in a weaker and more vulnerable position into sexual intercourse or to engage in another sexual act that essentially violates the person's right to sexual self-determination." Vulnerable groups consist of those institutionalized or, more commonly for the victims identified in this paper, those who are under the age of 18 and, due to either the perpetrator's position or the age gap between victim and perpetrator, are deemed incapable of sexual self-determination.

We also use this same data to identify property crimes, as we use women who experience property crimes as an alternative counterfactual group for women who experience sexual assault. Last, we use other victims of known rapists whose crimes were not categorized in the above group, who we can identify in the police data. This data set consists of every recorded offense in the police information system (PATJA) from 2006-2019. Within this data, we observe the unique case number, victim and perpetrator unique IDs (when available), information on the time/date of the crime, whether the case was referred to the prosecutor, and the type of offense at a 6-digit classification (see above for the offenses we include in our main analysis). Furthermore, we observe the age, gender, and location of the perpetrator and victim. We can perfectly link the victims in the police reports to the other registry data described below using their unique identifying numbers which are consistent across data sets. The data used in this project, including the police data, is not publicly available. Statistics Finland holds the data securely. All personally identifying information is replaced with a unique identifying number to protect individual information. Data is only linked when approved by the relevant agencies. In summary, this study was conducted in a way that respects the privacy and integrity of all individuals.

Labor Market and Demographics Data We gather information on victims' and their parents' labor market outcomes from Statistics Finland's FOLK and FLEED data sets. These are essentially the same data except that the FLEED data are used for 1988-2016, and the FOLK data provides coverage thereafter. These data will include anyone residing in Finland between the ages of 15 and 70. Every person in the FLEEd and FOLK data will have a unique identifying number which is repor ted in the data set (anonymized national person number). We use this number to perfectly link them to the police data (where we can see if they are either a victim or a perpetrator of a crime), and also to link labor market data across years. We observe a wealth of data in FLEED and FOLK, including but not limited to employment status at the end of the year, annual labor earnings, annual taxable income, school enrollment status, age, gender, spouse indicator, number of children, municipality of residence, marriage, and cohabitation status.

Health Data The health information comes from three different data sources. The information on substance and alcohol abuse is obtained from the HILMO Care register of the Finnish Institute of Health and Welfare (THL). These data have information on all inpatient or outpatient hospital visits for all Finnish patients from 2000-2022. We create indicators for whether a given individual was a patient in any hospital during a given year for alcohol or drug abuse related reasons using the following ICD10 codes: X45, Y90,Y91, F10, Z714, F714, R780, X44, X45, Y90,Y91 (for alcohol abuse), and F11-F16, F18-F19, F72, R78 (excluding R780), T40, and X44 (for drug abuse). Substance abuse indicates either drug or alcohol abuse.

The suicide rates come from cause-of-death statistics from Statistics Finland. These statistics report all deaths and their detailed causes according to Statistics Finland's 54-class cause-of-death coding that is based on the ICD10 classification. We identify suicides using the code 50, which corresponds to ICD10 classification codes (X60-X84, Y870).

Finally, information on anti-depressant prescriptions is obtained from the Kanta register of prescription medicines from KELA (Socian Insurance Institution). This data register contains information about all prescriptions prescribed by doctors in Finland (it does not include medicine ordered for patients in a hospital) and detailed information on the medicine category. We create

an indicator variable, "depression," which is equal to one if an individual has obtained a prescription for anti-depressants in Finland during a given year. anti-depressants are defined using the following ATC codes: N06AA01, N06AA02, N06AA03, N06AA04, N06AA05, N06AA06, N06AA07, N06AA08, N06AA09, N06AA10, N06AA11, N06AA12, N06AA13, N06AA14, N06AA15, N06AA16, N06AA17, N06AA18, N06AA19, N06AA21, N06AA23, N06AB02, N06AB03, N06AB04, N06AB05, N06AB06, N06AB07, N06AB08, N06AB09, N06AB10, N06AF01, N06AF02, N06AF03, N06AF04, N06AF05, N06AF06, N06AG02, N06AG03, N06AX01, N06AX02, N06AX03, N06AX04, N06AX05, N06AX06, N06AX07, N06AX08, N06AX09, N06AX10 N06AX11, N06AX12, N06AX13, N06AX14, N06AX15, N06AX16, N06AX17, N06AX18, N06AX19, N06AX21, N06AX22, N06AX23, N06AX24, N06AX25, N06AX26, N06AX27, N06AX28.